May 4, 2005

The Honorable Charles Grassley
Chairman
Committee on Finance
United States Senate
Washington, D.C. 20515

Dear Mr. Chairman:

On behalf of the American Surgical Hospital Association (ASHA), I would like to formally announce our disagreement and disappointment with the report produced by Jean Mitchell, Ph.D. of the Georgetown Public Policy Institute titled: Effects of Physician-Owned Limited-Service Hospitals. The findings are incomplete and absolutely one-sided. I would like to express my disappointment on the apparent lack of input on this issue from the specialty hospital industry.

One of our main concerns, which is expressed in more detail below, is the flaw in study methodology. The methods used were not expressed. Sample sizes differ. ASHA contends that for these reasons and reasons listed below the report should be dismissed altogether.

ASHA would like to officially submit the following questions and concerns about the study performed by Dr. Mitchell:

1. Details of the study methodology are lacking. The study is not presented in a standard scholarly format. How did the sample size go from 1.9 million claims to 250,000? What criteria were used to select cases? Were attempts made to control for patient comorbidities? If so, which methods were employed (e.g., Charlson-Deyo, Elixhauser, etc.). These methodological choices can have a significant impact on the findings, and need to be discussed. What kind of study design was employed? Is it a panel of patients? How were outliers treated?

2. The claim that physicians are performing unnecessary surgery based on financial incentives is impossible in the current Oklahoma Workers' Compensation System. Surgery cannot be performed on a Workers' Compensation patient without prior review by a Workers' Compensation case manager and pre-authorization for surgery.

3. Dr. Mitchell's data set is limited and only includes worker's compensation claims submitted to an insurance company between January 1999 and December 2004. The claim in her report is that the company controls 40% of the Oklahoma workers compensation market. First, this makes her data set extremely narrow to a very particular group of patients and she even restricts her study more by choosing a restricted set within the class, namely claims

submitted by one company alone. Why wouldn't she collect data from multiple sources using a double blind study that is traditional in scientific studies to prove her thesis unless she had a predetermined outcome in mind?

4. Another fundamental flaw is in the analysis by claiming that the cases studies in the data set, namely workers compensation claims submitted by state employees and employees of small businesses, are somehow accurately reflected in data compiled by the Oklahoma Department of Labor (ODL) in its reporting of private sector occupational injuries. However, ODL reports injuries to state workers in a separate Public Sector Occupational Injury Report. When these numbers are reviewed, it is shown that the occupational injury incidence rate among Oklahoma public sector employees was 6.9 in 1999, 7.2 in 2000, 6.7 in 2001, 6.5 in 2002 and 6.4 in 2003. Using her data this shows a drop in the occupational injury incidence rate among public sector workers during the same time period of 13.6% or 1 less injury for every 200 employees during the reporting period. Her survey and that of ODL rely on public sector data and both fail to take into account ongoing treatment in their data compilations. Both surveys rely on data compiled regarding initial injuries reported during the reporting period. Neither survey reports on follow-up care or long-term pain management.

5. Dr. Mitchell's study alleges that increases in utilization rates for complex spinal fusion surgery are a result of physician ownership in specialty hospitals. However, her study is based on flawed data. Previous to October 1, 2002, this emergent technology had been coded to Diagnostic Related Group (DRG) 497 or DRG 498. Historically DRG 496 had been recognized only for 360-degree spinal fusions reflecting both posterior and anterior approaches. However, the Center for Medicare and Medicaid Services (CMS) added this code to recognize advances in instrumentation allowing surgeons to perform these fusions using a single approach rather than two incisions. Consequently, the increase reported by the economist in this study is actually a reflection of a coding change implemented by CMS, as opposed to an increase in the number of these procedures actually performed.

6. Dr. Mitchell's claims are based on insufficient information. The study was conducted by reviewing insurance claim form information only. There are clinical indications for both simple and complex spinal fusion surgery and the study did not review patient medical record information. It is impossible to determine whether a patient requires surgery or not based on insurance claim form information. This study makes serious allegations based on flawed data.

7. Workers' Compensation cases may have increased at Specialty Hospitals during this time frame due to the fact that as these new hospitals opened, they were locked out of lucrative managed care contracts, and Workers Compensation patients were one group that they could treat at their new facilities.

8. The study design, as we are able to infer from the scant details presented in the study report, is insufficient and incapable of supporting the conclusions reached by Dr. Mitchell. Again, this may be due to the lack of details provided in the report. Did the author estimate a multivariable regression? If so, was it cross sectional time series, panel data, etc.? There is no mention anywhere of a multivariable regression model. Thus, it would be impossible to correctly attribute increases in volume to the opening of a specialty hospital. There are many other potential factors contributing to changes in utilization.

9. The author fails to even provide a benchmark for the procedure-level utilization increases; for example, what was the national growth rate in complex spinal infusion surgery? In the early years of CT and MRI diffusion, growth rates differed markedly across counties nationwide because of varying rates of diffusion. As the new diagnostic methods proved themselves, they "fully diffused." How many of the procedures chosen for analysis by Dr. Mitchell might fit a diffusion story? There is no mention of it.

10. Suppose the authors conducted a rigorous multivariable statistical model, and suppose that they found statistically significant specialty-hospital utilization effects even after controlling for other factors likely to affect utilization trends and patterns. What percentage of the "specialty-hospital induced" utilization increases represent inappropriate care? This would be a difficult question to answer without some sort of retrospective chart review. But doesn't it seem like an important question? Should it not at least warrant a mention somewhere?

11. Throughout the report, the old (circa 1980s) "supplier induced demand" story is implied. Here the author is demonstrating a lack of knowledge of changes in the economics field over the past 20 years. While it is true that, to some extent, supply creates its own demand in health care markets, there is a large and growing body of research critical of that reasoning. Again, there may be some supplier-induced demand, but how do we know how much?

12. Self-referral is a complex issue. It is not *per se* a negative event, for two reasons. First, suppose we can rank the quality of physicians on a scale of 0-100. There is nothing to be gained by forcing a "90" physician to refer to a "70" physician. If a physician is of higher quality, we would expect better outcomes in a self-referral setting. We historically have left it up to physicians to assess these relative differences and refer accordingly. The CMS study conducted by RTI appears to have found evidence that physicians, even in markets with specialty hospitals, continue to abide by this professional referral ethic.

13. Self-referral is a variant of the classic "make it or buy it" question. How can you determine to what extent self-referral is motivated by the ownership interest per se or by the fact that practices using these services intensively

are more like than others to reduce the transactions costs of producing care in a timely and efficient manner by (in effect) 'making' rather than 'buying'? Don't you have a self-selection problem here?

14. There is no evidence to support the contention that physician self-referral to specialty hospitals has any adverse effect on patient or societal welfare. The literature on self-referral generally shows higher rates of service utilization associated with physician ownership of ancillary services. However, any inference of causality in this association is problematic at best, because those physicians most likely to use such ancillary services most intensively also have the most to gain from increased control over the availability of such services, independent of any incentive associated with a return on investment in the facility itself. Thus, it is extremely difficult to quantify the impact of the financial incentive associated with physician ownership *per se* on the volume of self-referrals.

15. The existence of an association between physician ownership of self-referral for *ancillary* services provides no evidence that ownership of acute care facilities would result in similar differences in utilization. The direct financial incentive for physician self-referral associated with physician investment in specialty hospitals is unlikely to play a major role in a physician's use of a specialty hospital, for four reasons: (1) the extent of investment for the vast majority of physicians with ownership interests in specialty hospitals is small compared to the extent on physician ownership of *ancillary* services; (2) there is no direct evidence that physician self-referral is motivated primarily or disproportionately by financial incentives associated with physician ownership; (3) there is no evidence that self-referrals result in worse outcomes than other types of referral; and (4) in the case of physician ownership of acute care facilities, it is likely that the magnitude of financial incentives is small relative to the more direct financial incentive associated with fee-for-service payment for physician services.

16. Nowhere does the report entertain the idea that patients might prefer specialty hospitals. Regardless of ones personal biases, it is clear that specialty hospitals offer a product that has a distinctly different look and feel compared to full service hospitals. The entire U.S. health care system is moving toward "consumer directed" care. As a recent FTC report emphasized, the increased competition associated with consumer-directed care is likely to have large and lasting effects on the efficiency of the U.S. health care system. Thus far, studies of the effects of competition in the health care industry are overwhelmingly positive. Recently, there have even been some studies showing a positive effect of competition on health care quality. All of this, of course, was discovered and embraced in non-health care industries 40 years ago.

17. The ambulatory surgery centers have proven that it makes sense to have the less complicated cases go to less intensive settings. If all that I need is knee surgery, why should I go to a facility that has a burn unit?

As you are fully aware, the Medicare Payment Advisory Commission and the Centers for Medicare and Medicaid have both officially released reports stating specialty hospitals do not cause harm to community hospitals. Furthermore, the General Accounting Office, the Federal Trade Commission and the Treasury Department are now finding serious problems with community hospitals and their management of their monopoly. More and more these days our nation's newspapers print expose after expose of hospitals abusing the public trust.

As you can plainly see, ASHA is deeply concerned about this report. What is more concerning is that there was not ample notification, if any at all, given that this hearing was taking place. Overall, the gravity of this report's errors, as mentioned above, are such that they would compromise its acceptance for publication in any peer reviewed academic journals. The departure from academic and scientific standards in the report raises questions as to whether the report may have been prepared, not only in haste, but also prepared with a predetermined bias and preconceived conclusions.

If you or any of your colleagues have any questions about specialty hospitals or the association's position on the findings presented by Dr. Mitchell, please do not hesitate to contact me or the association's Washington representative, Randy Fenninger, who can be reached at (202) 833-0007.

As always, ASHA looks forward to working with you on improvements to the healthcare system that will benefit all Americans.

Sincerely,

Jim Grant, President
The American Surgical Hospital Association