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COMMENTARY

Comment on Silber et al.: Investing in Postadmission Survival—A “Failure-to-Rescue” U.S. Population Health

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In 1938 Sir Allison Glover presented a paper to the Royal Academy of Medicine in England documenting significant variation between towns in tonsillectomy rates among school children (Glover 1938). By the 1970s regional variation had become the focus of concentrated scientific inquiry in the United States (Wennberg and Gittelsohn 1973) and, in 2009–2010, the center of U.S. health care reform debate. Short of directly implementing payment reforms to reduce variation, Section 1159 of the Affordable Health Care for Americans Act requires that the Institute of Medicine (IOM) conduct a study of geographic variation in volume and intensity of services and health care spending and make recommendations regarding payment reforms based on their findings. Among the decades of research findings that the IOM’s committee must integrate in this task is a new study by Jeffrey H. Silber, R. Kaestner, O. Even-Shoshan, Y. Wang, and L. J. Bressler in this Theme Issue of *Health Services Research*, “Aggressive Treatment Style and Surgical Outcomes” (Silber et al. 2010).

Silber and colleagues studied the ecologic relationship between a hospital’s intensity, as measured by the Dartmouth Atlas End-of-Life Expenditure Index, and rates of operative complications, failure-to-rescue from complications, and postadmission mortality among 4.5 million Medicare beneficiaries receiving general, orthopedic, and vascular surgery at 3,065 hospitals between 2000 and 2005. They find significant reductions in 30-day postadmission mortality at higher intensity hospitals that appear to operate not through a reduction in complication rates—these are the same across hospitals of varying intensity—but through a lower mortality rate among those with complications (i.e., lower failure-to-rescue rates). This survival benefit persists despite robust adjustment for hospital characteristics that might confound the relationship between hospital intensity and postadmission survival, including the availability of advanced technology, resident-to-bed ratio, and nurse-to-bed

ratio and multiple sensitivity analyses aimed at reducing potential selection effects, including restricting analyses to less discretionary procedures with more urgent timing.

Silber's findings, like my own recent paper (Barnato et al. 2010), seem at first glance to contradict the now widely accepted notion that high-intensity practice is inefficient (Fisher et al. 2003a, b). Indeed, based upon the sheer number of model specifications, sensitivity analyses, and electronic supplemental appendices in these two papers, it is apparent that the peer-reviewers themselves needed serious convincing. But reconciliation of these findings is relatively simple and has important implications for health care reform.

The ecologic analyses by Fisher and colleagues found no relationship between regional intensity and 1-year mortality and functional decline, that is, when using measures of *population health*. In contrast, Silber and colleagues and Barnato and colleagues reported a relationship between greater hospital intensity and lower 30-day postadmission mortality, that is, when using measures of *hospital quality*. Silber and colleagues found further that, conditional upon surviving this 30-day window, patients from lower and higher intensity hospitals were no more likely to live or die in the following year. Specifically, patients return to the same "baseline hazard" of death after surviving the first month after surgical treatment. This is a remarkable finding because it suggests that going to a higher intensity hospital is more likely to get you out alive, but it does not make you "healthier." That is, better hospital "quality" improves safety (e.g., survival conditional upon hospitalization), but it does not improve population health (e.g., longer life expectancy or slowed functional decline).

The study by Silber and colleagues, then, is a poster child for U.S. short-term health care priorities and the loss aversion bias that leads us to prioritize providing intensive medical treatment over assuring long-term public (i.e., population) health.

For the sake of argument, let us consider the hypothetical use of Medicare resources for providing access to higher intensity hospitals for elderly surgery patients versus for increasing influenza vaccination rates among the elderly. (Notice here I am not suggesting that we use the money to reduce U.S. disparities in educational and socioeconomic achievement, thereby improving life expectancy, but I am keeping the resources within the fully fungible CMS budget in this hypothetical exercise.) Imagine that we magically move

all Medicare surgical patients from lower intensity hospitals to higher intensity hospitals. Over the 5 years studied, this would have involved moving an average of 427 end-of-life patients (<http://www.DartmouthAtlas.org>) and 1,487 surgical patients per hospital, 37% (545), 55% (821), and 8% (122) of which would have been general, orthopedic, and vascular surgery patients, respectively (Silber, Rosenbaum, and Ross 2009). This would have saved 1.5 general surgery patients' lives ($545 \times 2.7/1,000$), 1.3 orthopedic surgery patients' lives ($820 \times 1.6/1,000$), and 0.8 vascular surgery patients' lives ($122 \times 6.7/1,000$). We would spend at least U.S.\$4,270,000 (U.S.\$10,000 $\times$ 427 end-of-life patients) more than we currently spend every 5 years for each hospital "magically moved." This amounts to U.S.\$1.2 million per surgical life saved.

Imagine instead that we spend that U.S.\$4,270,000 on an immunization vaccination campaign among the exact same population of Medicare patients. Even if we assumed the costs per additionally vaccinated elder were very high, say U.S.\$100 in direct incentive payment to the patient, we could vaccinate 42,700 75-year-olds, save 159 lives in a nonepidemic year, or 378 lives in an epidemic year and (now get this) save U.S.\$7.7 million (in 2005 dollars) through reductions in outpatient and inpatient treatment (Nichol et al. 1994).

The American public and politicians (as evidenced by the recent debate in passing the Affordable Health Care for Americans Act) are extremely reluctant to talk about cost effectiveness in making these decisions, but let's continue the example of improving surgical survival by magically moving Medicare surgical patients from the lower intensity hospitals to the higher intensity hospitals. The average age of the surgical patient saved is 76.4 years old (Silber et al. 2009) and therefore has approximately 10 more years of life expectancy (<http://www.cdc.gov/nchs/data/has/has09.pdf#listtables>). This translates to an incremental cost-effectiveness ratio (ICER) of U.S.\$118,658/life year (LY) gained, or a minimum of U.S.\$141,259/quality-adjusted life year (QALY) gained, assuming all of these 76.4 year olds had a mean utility of 0.84 (Gold et al. 1998). These ICERs (which are underestimates, since they only include the increased spending on end-of-life patients and do not include the likely higher cost of surgical treatment at the higher-intensity hospital) are above traditional thresholds for cost effectiveness and certainly not as fiscally responsible as *saving* U.S.\$7.7 million.

Cynics might wrinkle their noses at these simplistic back-of-the-envelope calculations, arguing that you can't just trade high-intensity practice with lower failure-to-rescue rates for higher immunization rates. But in reality we are

making just those kinds of trade-offs between high-intensity medical care and public health practices right now. By specializing in lots of (potentially life-saving) intensive treatment in the United States, we are failing to specialize in (potentially greater life-saving) population health interventions or other forms of lower intensity health care.

Chandra and Staiger (2007) have empirically demonstrated these “productivity spillovers” in the treatment of acute myocardial infarction (AMI) among Medicare patients. Regions that provide a lot of intensive surgical management to AMI patients are not as good at providing quality medical management. Regions that provide excellent quality medical management are not as good at providing intensive surgical management. On average the two types of regions have similar overall outcomes for AMI patients. Therefore, the higher intensity regions are “failing to rescue” some AMI patients with high-quality medical management and the lower intensity regions are “failing to rescue” other AMI patients with intensive surgical management. But the lower intensity regions spend less.

This is the commentary I have been waiting my entire career to write. The health services researcher in me applauds the rigorous analyses conducted by Silber and colleagues. Indeed, their findings are consistent with my own NIH-funded work in this area. However, the public health physician in me implores policy makers not to lose sight of the bigger picture. As my hypothetical hospital-intensity/immunization trade-off demonstrates, investing too heavily in improving postadmission survival would be a “failure-to-rescue” U.S. population health. This point is too important to get lost due to Americans’ reluctance to consider the costs of providing intensive health care to the neglect of alternative investments that can significantly improve the long-term health of the American public. In this light I might rewrite the conclusion of Silber and colleagues to say: “Although attempting to reduce postadmission mortality is a laudable goal, policy makers should be aware that investments in improving acute care will not increase U.S. population health and life expectancy, which continue to lag woefully behind other developed nations.”

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