Letters

Advancing the Health of Lesbian, Gay, and Bisexual Adults

To the Editor In an Original Investigation in a recent issue of *JAMA Internal Medicine*, Gonzalez et al,1 using national health surveillance data, observed that lesbian, gay, and bisexual (LGB) adults experience health disparities that warrant clinician attention. These findings were described, incorrectly, as “the first to capture the disparity in a population-based sample rather than a convenience or clinic-based sample.”2(p1352) In fact, these effects have been well documented, both in the United States1 and elsewhere,4 in population-based studies published in the scientific literature.

It is time to use this knowledge to implement changes in clinical services and research. Although many US health surveillance systems measure sexual orientation, this is not yet integrated in our electronic health records or public health reporting systems. Furthermore, these data systems are not designed to explain why these differences exist nor how systems should be redesigned to respond to the patient-centered needs of LGB individuals. To advance the health of the LGB population, science must build on what is known and move with the proper protections to collect sexual orientation data as a necessary aspect of health care services.

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In Reply We thank Cochran et al for their insightful comments, and we agree that our study1 on health disparities in the lesbian, gay, and bisexual (LGB) population adds to the mounting body of evidence that people who identify as LGB experience worse health outcomes compared with their heterosexual peers, potentially due to the stressors that LGB people face as a result of interpersonal and structural discrimination. As Cochran and Mays have previously noted, “research on LGB populations is still in its infancy.”2(p1352) Compared with health disparities research on racial and ethnic minority populations, other researchers have also noted that the work to resolve health disparities in the LGB population has barely begun.3 In our study we provide the broadest knowledge base on LGB health disparities to the readers of *JAMA Internal Medicine*, as it is imperative for clinicians to understand, accept, and address the health needs of LGB and transgender (LGBT) patients. We encourage *JAMA Internal Medicine* to continue publishing important findings documenting LGBT health disparities and best practices for eliminating those disparities. Doing so will help health care providers implement positive changes in their practice. Meanwhile, recent developments at the National Institutes of Health will help build the research capacity for LGBT health. Specifically, the National Institute of Minority Health recently designated sexual and gender minorities as a health disparity population for research purposes.4 This designation will broaden funding opportunities and research on LGBT populations, among other sexual and gender minorities. We commend these efforts and encourage high-impact journals, like *JAMA Internal Medicine*, to continue reporting new discoveries and advancements in LGBT health.

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Lingering Questions Concerning Intensive Care Unit Utilization

To the Editor The article in a recent issue of JAMA Internal Medicine by Drs Chang and Shapiro1 asks an important question: Do hospitals’ habits of costly and invasive intensive care utilization affect patient outcomes? The authors conclude that intensive care unit (ICU) utilization is not associated with hospital mortality and estimate that decreasing ICU overutilization may save millions of dollars. Before policy experts and hospital administrators sharpen their knives to trim the fat of ICU overutilization, the authors’ may add perspective to some unmentioned elements of reporting that may change the above conclusions.

First, was hospital mortality for in-hospital time or over another time period? Use of in-hospital mortality results in discharge bias that would meaningfully affect comparisons across hospitals.2 Similarly, were transferred patients excluded? Transfer is a marker for unmeasured disease severity and the cause of mortality shift to referral hospitals. These patients are typically excluded in such studies.2

Why were risk-adjustment models built using discharge diagnoses rather than “present on admission” comorbidities? This post facto assessment of risk undermines the goal of risk adjustment; hospitals with higher mortality rates also have higher rates of secondary complications, or complication cascades,3 so using discharge morbidity rather than present on admission comorbidity for risk adjustment may warp conclusions about overutilization or underutilization of intensive care.

Lingering questions may require us to reinterpret the investigation. Intensive care is not a therapy like aspirin for acute myocardial infarction—administered in uniform doses around the nation for a specific indication. Intensive care is titrated based on each hospital’s workload and capacity to achieve the result anticipated in a counterfactual group. Perhaps the lack of difference in mortality across wide rates of intensive care utilization demonstrates well-matched workload-capacity for intensive care utilization. By analogy, the dose of a vasoppressor may not be associated with a patient’s posttreatment blood pressure (everyone’s mean arterial pressure is over 65 mm Hg). But this lack of association should not be interpreted as lack of efficacy of a vasopressor to increase blood pressure.

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In Reply Dr Hyder raises interesting questions about the interpretation of our findings,1 and we are grateful for the opportunity to clarify. In our Original Investigation, we showed that hospitals that utilize intensive care units (ICUs) more frequently during admissions for diabetic ketoacidosis (DKA), pulmonary embolism (PE), upper gastrointestinal hemorrhage (UGIB), and congestive heart failure (CHF) were more likely to perform invasive procedures and have higher costs without improvements in hospital mortality.1 Dr Hyder suggests that hospital mortality may be affected by discharge bias if hospitals differentially discharge patients with high likelihoods of death to other facilities.2 We agree that using time-specific end points such as 30-day mortality reduces such bias. Unfortunately, only in-hospital mortality was available in the administrative data set used in our study. It is important to note, however, that Reineck et al2 showed that discharge bias generally results in lower in-hospital mortality among smaller hospitals, as they are more likely to discharge high-risk patients to other facilities.2 In our study, there was a greater proportion of smaller hospitals in the higher–ICU utilization group. As such, discharge bias would disproportionately decrease in-hospital mortality in the higher–ICU utilization group. If mortality was actually greater in the higher-utilization group, this would strengthen our conclusion regarding the dangers of ICU overutilization.

We did not exclude hospitalizations resulting in transfers to other acute-care hospitals. The frequency of transfers in higher– vs lower–ICU utilization hospitals were 0.8% and 0.9%,