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Uncovering Waste in US Healthcare:

Evidence from Ambulance Referral Patterns*

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Abstract

There is widespread agreement that the US healthcare system wastes as much as 5% of GDP, yet much less agreement on the source of the waste. This paper uses the effectively random assignment of patients to ambulance companies to generate comparisons across similar patients treated at different hospitals. We find that assignment to hospitals whose patients receive large amounts of care over the three months following a health emergency have only modestly better survival outcomes compared to hospitals whose patients receive less. Outcomes are related to different forms of spending. Patients assigned to hospitals with high levels of inpatient spending are more likely to survive to one year, while high levels of outpatient spending result in lower survival. In particular, we discovered that downstream spending at skilled nursing facilities (SNF) is a strong predictor of mortality. Our results highlight SNF admissions as a quality measure to complement the commonly used measure of hospital readmissions and suggest that in the search for waste in the US healthcare, post-acute SNF care is a prime candidate.

At the heart of healthcare spending reforms is the idea that 30% of spending in the US, or 5% of GDP, may be wasted (Fisher, Bynum, and Skinner 2009; Cutler 2010; Skinner and Fisher 2010).[1] This idea stems from the striking amount of geographic variation in treatment intensity that yields little apparent benefit in terms of patient health outcomes (E. S. Fisher et al. 2003a; E. S. Fisher et al. 2003b; Chandra and Skinner 2012). More broadly, the US is an outlier in terms of healthcare spending per capita at 40% more than the next highest-spending country in the OECD (OECD 2014), yet broad measures of health are not noticeably better in the US.

There is less evidence on the crucial question of which types of spending are unproductive. In part, this is due to concerns over selection bias. Those providers who spend the most on

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[1]Skinner and Fisher (2010) note that 20–30% is likely an underestimate and note, “At least three other groups have come to 30% waste estimates: the New England Healthcare Institute, McKinsey, and Thomson Reuters.”

care may be treating the sickest patients. Differences in unobservable characteristics may therefore bias results towards finding no effect of greater spending.

The main policy response has been a call to “pay for quality” rather than quantity, but the means to do so are not obvious. In particular, two approaches have received most of the attention: (1) capitating payments, either annually or for up to 90 days after a health episode, coupled with quality bonuses, and (2) hospital readmission penalties. Despite such policy emphasis, there is little causal evidence that spending levels over 90 days after an episode, or readmissions, are wasteful. While readmissions are costly, the optimal rate is not zero. Further, readmission risk competes with mortality risk: higher mortality can lower readmission rates.

The aim of this paper is to circumvent the problem of selection bias in order to identify potential sources of waste in healthcare spending around episodes of acute care. We focus on costly health emergencies where mortality is a common outcome that is unambiguous in terms of welfare comparisons. We further develop a framework based on earlier work that leverages the effective random assignment of patients to ambulance companies to provide plausibly exogenous variation in hospital choice (Doyle et al. 2015). A feature of the instrumental variables strategy that stems from ambulance company assignment is that each community provides its own experiment, with ambulance companies delivering patients to hospitals with different treatment patterns. This enables us to compare patients assigned to hospitals with different combinations of treatment intensities. Given the empirical strategy, our approach focuses on patients entering the hospital on an emergency basis. Using longitudinal Medicare claims data from 2002–2012, we observe treatment provided and paid for across institutional settings. This allows us to characterize each hospital with respect to the sources of spending that accrues to its patients for different types of care. In addition, the data are linked to vital statistics records that provide our key outcome: one-year mortality.

We have four primary findings. First, our causal framework largely corroborates the cross-sectional result that hospitals with higher total (inpatient and downstream) spending over the 90 day period after an initial hospitalization have only modestly better survival outcomes compared to those with lower spending levels. Second, we confirm the finding of Doyle et al. (2015) that patients assigned to hospitals with large average inpatient expenditures have lower mortality rates compared to patients assigned to less intensive hospitals. Third, we square these results by finding that patients assigned to hospitals with high average levels of downstream spending have substantially higher mortality rates compared to those treated in hospitals whose patients receive lower amounts of such care. Fourth, based on these findings we further investigate the type of post-acute spending accrued by discharged patients and conclude that the positive relationship between downstream spending and mortality is concentrated in hospitals whose discharged patients have high spending at Skilled Nursing Facilities (SNF). In a similar spirit to widely used readmission measures, SNF admission is expensive and we find it is a strong predictor of mortality. This suggests that SNF admission, or some combination of SNF and hospital admission, creates a stronger quality measure.

The results are also suggestive that in the search for waste in the US healthcare, post-acute SNF care is a prime candidate. This finding is consistent with recent work that points to post-acute care as a potential culprit for waste in the US system (Navathe et al. 2017; Baicker and Chernew 2017; Newhouse and Garber 2013). Such care is a major contributing factor to residual geographic variation in healthcare spending among the over-65 population (Newhouse and Garber 2013; Newhouse, Garber, and Graham 2013), a result found for the under-65 population as well (Franzini et al. 2014). Current reform proposals bundle payments to providers for 90-days after a hospitalization, and penalize for hospitals for readmissions that occur within 30 days of discharge. There is hope that such reforms will provide incentives to coordinate care across inpatient and outpatient institutions in a way that reduces costs and improves health. We discuss the implications of our findings for such proposals.

The remainder of this paper proceeds as follows. Section II provides background on payment reforms and the way hospitals are compared in current policy experiments. Section III describes the empirical framework, while Section IV describes the data. Section V presents the results for the impact of spending on outcomes. Section VI concludes and discusses policy implications.

1 Background

1.1 Provider Incentives

As noted in the introduction, there is a large literature documenting that, on average, high-spending hospitals and high-spending geographic areas do not have better outcomes compared to lower-spending ones. This raises the fundamental question: where is the unproductive spending in the US healthcare system?

In efforts to control healthcare spending, the main idea in current policy discussions is to remove the incentive to provide too much care created by a system that pays a fee for every service. Instead, proposals call for fixed, rather than marginal, payments and reward or penalize providers for their performance on quality measures to guard against sub-optimal care. For example, the Affordable Care Act promotes the formation of vertically-integrated providers, Accountable Care Organizations (ACOs), with the aim to coordinate care across different types of providers. A related approach (bundled payment) pays providers for an amount for a period of care after a hospitalization. Typically, bundled payments cover up to 90-days of care and vary at the level of the diagnosis.[2] Under both approaches the hope is that providers will know (or will learn) what types of care can be reduced or improved without harming patients.

This paper considers risk-standardized hospital-level measures of Medicare spending per beneficiary to all providers up to 90 days after acute episodes to mimic the measures used in policy discussions. There are a few advantages of this type of approach. First, there is reason to believe that coordination of care after a hospitalization is cost effective: it provides an incentive for better care transitions and reduces readmissions (Naylor et al. 2011). This junction of the US healthcare system is often the target of suspicion for a major source of

[2]In the Episode of Care Payment Demonstration project, a typical hospital would receive $21,000 for 90 days of care after a heart attack hospitalization. See, for example, Table 3-3 in http://www.medpac.gov/documents/reports/jun13/_ch03.pdf?sfvrsn=0

waste, as coordination across providers may be necessary to achieve the gains, yet is often not reimbursed by payers; indeed, in an effort to improve hospital quality the Centers for Medicare and Medicaid Services began publicly reporting similar hospital-level measures of 30-day Medicare Spending Per Beneficiary. Second, the care plan after an acute health problem provides a natural basis for reimbursement, as care transitions are more likely to be affected by treatment decisions compared to annual patient spending. Third, spending on such care is substantial. Cutler and Ghosh (2012) find that capping episode-based bundled payments to the median level across markets would yield nearly the same savings as capping an annual payment per beneficiary to its median level.

1.2 Ambulance Referral Patterns

The key ingredient of our approach is the recognition that the locus of treatment for emergency hospitalizations is, to a large extent, determined by pre-hospital factors, including ambulance transport decisions and patient location. Critically, areas are often served by multiple ambulance companies, and the assignment of the ambulance company to the patient is effectively random.[3]

In particular, patients are transported by different companies for two main reasons. First, large cities such as New York, Los Angeles and Chicago contract with private ambulance companies to work in conjunction with fire departments to provide Emergency Medical Services (EMS) (Johnson 2001). Chiang, David, and Housman (2006) found that of the top 10 cities with the highest population over age 65, 5 contracted with both public and private ambulance carriers, while 2 others contracted exclusively with private carriers. In a more recent 2010 survey covering 97 areas, 40 percent reported contracting with private ambulance companies and an additional 23 percent utilized hospital-based ambulance providers (Ragone 2012). In these communities served by multiple ambulance services, 911 systems often use software that assigns units based on a rotational dispatch mechanism; alternatively, they may position ambulances throughout an area and dispatch whichever ambulance is closest, then reshuffle the other available units to respond to the next call.

Second, in areas with a single ambulance company, neighboring companies provide service when the principal ambulance units are busy under so-called “mutual aid” agreements. Within a small area, then, the variation in the ambulance dispatched is either due to rotational assignment or one of the ambulance companies being engaged on another 911 call. Both sources appear plausibly exogenous with respect to the underlying health of a given patient.

In addition to plausibly exogenous assignment, ambulance companies are expected to have preferences for particular hospitals. In survey work described in Doyle et al. (2015), we found that paramedics have developed relationships with local emergency departments. For example, Skura (2001) studied ambulance assignment in the wake of a new system of competition between public and private ambulances in New York City. He found that patients living in the same ZIP code as public Health and Hospital Corporation hospitals were less than half as likely to be taken there when assigned a private, non-profit ambulance

[3]This section builds on our previous description in Doyle et al. 2015.

(29%) compared to when the dispatch system assigned them to an FDNY ambulance (64%). In most cases, the private ambulances were operated by non-profit hospitals and stationed near or even within those facilities, and they tended to take patients to their affiliated hospitals.

More broadly, transport assignment for emergencies is often determined by idiosyncratic preferences. Even in the case of trauma – for which there are often local triage protocols designed to determine assignment to designated trauma centers (Kahn et al. 2008) – “undertriage” of elderly patients is common. Between 33% and 44% of trauma center-eligible elderly and near-elderly (>55 years old) ambulance patients were treated at a non-trauma center in a recent study in Utah and California, for example (Staudenmayer et al. 2013)). As noted in one Institute of Medicine report, ambulance personnel often “lack the means to determine which hospitals can provide the best care to a patient” (Medicine 2010). This combination of exogenous assignment of ambulance companies, coupled with their preference for taking patients to certain hospitals, provides an empirical lens to compare similar patients who live nearby one another but visit different hospitals.

2 Empirical Framework

Our main regression of interest is the relationship between spending patterns accrued by patients assigned to particular hospitals, such as average 90-day spending among other patients, H, and outcomes such as mortality, M, for patient i with principal diagnosis $d(i)$ originating from a particular point of origin $o(i)$ (home, nursing home or elsewhere) within ZIP code $z(i)$ in year $t(i)$:

$$M_i = \beta_0 + \beta_1 H_i + \beta_2 X_i + \beta_3 A_i + \gamma_{d(i)} + \theta_{z(i),o(i)} + \lambda_{t(i)} + \varepsilon_i \quad (1)$$

where X_i is a vector of patient controls including age, race, and sex, and indicators for 17 common comorbidities controlled for when the Centers for Medicare and Medicaid Services (CMS) computes quality scores.

A_i represents a vector of ambulance characteristics including the payment to the company, which provides a useful summary of the treatment provided in the ambulance; indicators for distance traveled in miles; whether the transport has Advanced Life Support (e.g., paramedic) capabilities; whether the transport was coded as emergency (i.e., “lights and sirens”) transport; and whether the ambulance was paid through the outpatient system rather than the carrier system.

We cluster standard errors at the Hospital Service Area (HSA) level, as each local market may have its own assignment rules. In addition to one-year mortality as an outcome, we will also consider one-year Medicare spending downstream of the index event. The main outcome is binary and while there are limitations to imposing linearity, we prefer linear OLS and 2SLS in this setting given the enormous number of fixed effects included (57,157). Further, the mean outcome is far from zero, suggesting that the linear model is not particularly problematic.

Patient selection is likely to confound the structural equation (1), so we estimate it using two-stage least squares. To operationalize ambulance preferences, we calculate a set of instrumental variables based on the characteristics of hospitals where each ambulance company takes other patients – a leave-out mean approach that helps avoid weak instrument concerns (Kolesár et al. 2015; Stock, Wright, and Yogo 2012). For patient i assigned to ambulance $a(i)$, we calculate the average hospital measure H_j (e.g., average 90-day log spending) among the patients in our analysis sample for each ambulance company:

$$Z_{a(i)}=\left(\frac{1}{N_{a(i)}-1}\right)\left(\sum_{j\neq i}^{N_{a(i)}-1} H_j\right)$$

This measure is the ambulance company fixed effect in a model that predicts H_j, leaving out patient i.[4]

We use this instrument to estimate the first-stage relationship between hospital spending measures H and the instrument, Z: the hospital measure associated with the ambulance assigned to patient i:

$$H_i=\alpha_0+\alpha_1 Z_{a(i)}+\alpha_2 X_i+\alpha_3 A_i+\gamma_{d(i)}+\theta_{z,o(i)}+\lambda_{t(i)}+\nu_i \quad (2)$$

This regression, in other words, compares individuals who live in the same ZIP code and are picked up from similar location (e.g., at home), but who are assigned ambulance companies with different “preferences” across hospitals with different spending patterns.[5] A positive coefficient α_1 would indicate that ambulance company “preferences” are correlated with where the patient actually is admitted.

Doyle et al. (2015) focuses on inpatient spending at the time of the health shock and discusses at length potential limitations with this strategy and various specification checks that begin to address them. In particular, that study finds results that are robust to controls for both patient characteristics and the characteristics of pre-hospital care in the ambulance itself; results that are robust to the level of heterogeneity in the demographics of the ZIP codes, which suggests that within-ZIP differences in patient assignment is not driving the assignment; that the rate of admission to a hospital among emergent patients is not correlated with the ambulance level instrument, suggesting that selection into hospital admission conditional on ambulance use is not a concern; another check on the impact of differences in care across the ambulance companies is that impact of ambulance assignment on health outcomes occurs not in the first day but over longer horizons, which is suggestive that the health of patients upon entry to the hospital does not vary substantially across ambulance companies.

[4]The first-stage estimates below do not take into account the noise created when estimating this fixed effect, however we expect this to be a small adjustment given that the average number of observations used to calculate this fixed effect is over 485.

[5]A particular concern is that an ambulance affiliated with a nursing home will systematically pick up older, sicker patients. We hope to avoid this by using ZIP-by-origin fixed effects. Results are similar when we exclude the point-of-origin fixed effects, however.

One additional point to highlight in the context of the current paper is that our instrument only exogenously varies the hospital to which the patient is brought. As a result, when we examine the impact of post-acute care on patients, we are not measuring the causal impact of such care. Rather, we are measuring the impact of the patient going to a hospital that is associated with higher post-acute spending. We will return to this distinction below.

3 Data

3.1 Medicare Claims Data

The underlying data are claims paid by the Centers for Medicare and Medicaid Services (CMS) from 2001 to 2012. CMS reimburses ambulance companies using two systems captured by the Carrier file and the Outpatient claims file. We can access Carrier claims for a 20% random sample of beneficiaries, and 100% of outpatient claims. Most ambulance claims are paid via the Carrier claims, and we increase our sample by 6% by including the outpatient claims – claims that are affiliated with a hospital or other facility file. We link each ambulance patient's claims to her inpatient claims in the Medicare Provider Analysis and Review (MEDPAR) files, which records pertinent information on date of admission, primary and secondary diagnoses, procedures performed, and payments made by CMS. Diagnoses and procedures recorded in each patient's claims for the year leading up to the ambulance-linked admission are then mapped to Condition Codes (CC) to construct a set of comorbidity measures. We also link each ambulance patient to information on age, race, and gender. The claims data also include the ZIP code of the beneficiary, where official correspondence is sent; in principle, this could differ from the patient's home ZIP code.

The carrier data also include information about the ambulance visit. First, to control for the location where the patient is first contacted by the ambulance company, the data contain a patient origin variable that includes home, nursing home or other non-acute care facility, and scene of an accident. Second, the driving distance from the pick-up location to the hospital is recorded because Medicare reimburses ambulances in part based on distance traveled with the patient. The reimbursement made to the ambulance company also provides a summary of the amount of care provided prior to arriving at the hospital. We also observe whether the patient is assigned to a basic life support ambulance, which provides care administered by Emergency Medical Technicians, or advanced-life support care provided by more highly trained paramedics.

As described in more detail below, we also draw upon the Medicare claims in the construction of additional provider-level spending measures. Finally, vital statistics data that record when a patient dies are linked to these claims. This allows us to measure our main outcome, one-year mortality.

3.2 Sample Construction

We rely on a sample consisting of patients admitted to the hospital with "nondeferrable" conditions where selection into the healthcare system is largely unavoidable. Discretionary admissions see a marked decline on the weekend, but particularly serious emergencies do not. Following Dobkin (2003) and Card, Dobkin, and Maestas (2009), diagnoses whose weekend admission rates are closest to 2/7ths reflect a lack of discretion as to the timing of the hospital admission. Using our Medicare sample, we chose a cutoff of all conditions with a weekend admission rate that was as close or closer to 2/7ths as hip fracture, a condition commonly thought to require immediate care. In addition to these conditions, we also draw upon the set of non-discretionary emergency conditions based on an expert physician panel as reported in Mulcahy et al. (2013). Appendix Table A1 shows the distribution of admissions across these diagnostic categories, which include diagnoses such as acute myocardial infarction (heart attacks), strokes, and hip fractures. These are the types of diagnoses that are particularly costly and are candidates for episode-based bundled payments (Cutler and Ghosh 2012). Our condition set represents roughly 30% of all hospital admissions among Medicare beneficiaries in 2011, 75% of which arrived through the emergency room.[6] Among those whose index admission originates in the emergency room in our initial sample of non-deferrable patients, we calculate that 40% arrived via ambulance.

We further limit the sample to patients first observed in this ambulance-transport sample of diagnoses, and patients who have not been admitted to the hospital with a principal diagnosis for one of the non-deferrable conditions in the prior year. We also limit the sample to individuals in fee-for-service Medicare for at least one year after the index admission so that we can observe uncensored Medicare spending over that time period. To observe beneficiaries for at least one year and observed an uncensored measure of one-year mortality, our sample is restricted to patients who are admitted in the years 2002–2011 who are at least 66 years of age. Our final analytic sample is comprised of 1,575,273 patients.[7]

The analysis sample is restricted to relatively severe health shocks where there is relatively little choice but to seek treatment. The estimates of the effects of hospital types on mortality apply to these types of episodes. We caution against extrapolating our results to other sources of medical spending, such as most treatment for chronic disease; we discuss this point further in the conclusion.

3.3 Medicare Spending Per Beneficiary Measures

Each beneficiary in our analysis sample has a unique index event associated with the inpatient admission via an ambulance. For each hospital, we compute the average risk-standardized spending (total, inpatient and non-inpatient) for the index episode and over the following 90 days after admission for all other patients treated by the hospital in our analysis sample. Our inpatient spending measure includes all hospital facility payments as well as doctor payments for acute care services and all emergency department spending. The non-inpatient spending measure includes Medicare payment for Part B services not provided in a hospital, all outpatient care (except emergency room use), skilled nursing facilities, home

[6] Author tabulations of the 2011 National Inpatient Sample.

[7] This sample is larger than previous versions of this paper due to the addition of two new years of Medicare claims (through 2012), additional non-discretionary diagnosis ICD-9 codes based on Mulcahy et al. (2013)). In addition, we no longer restrict our sample to patients with no inpatient admissions within the last year. We found that our main results were robust when we relaxed this inclusion criteria to only restrict to patients with no admission for the principal diagnoses studied here within the last year, and results in a larger sample size.

health care, hospice, and durable medical equipment. The only Medicare reimbursement category not included in our data is pharmaceuticals provided outside of the hospital setting.

Each spending measure is constructed using a procedure similar to that used to construct the 30-day Medicare Spending Per Beneficiary quality measure for US hospitals, as well as the previous cross-sectional comparisons across hospitals and markets:[8]

1. Calculate Expected Episode Spending. We utilize an ordinary least squares regression (OLS) model that controls for age, race and gender.
2. Truncate and Normalize Predicted Values. The predicted values from the OLS regression model are truncated at the 0.5th percentile to reduce the influence of extreme predictions. Predicted values are then normalized so that average 90d spending is the same before and after truncation.
3. Calculate Residuals. We calculate the residuals for each patient episode as the difference between the observed 90-day spending and the (truncated) predicted value.
4. Exclude Outliers. For constructing the measure, we exclude all observations with residuals above the 99th percentile and below the 1st percentile.
5. Calculate The Spending Measure at the Hospital Level. The hospital-level spending measure is estimated as ratio of the average observed spending for the hospital to the average predicted spending (from (a)) for the hospital, multiplied by average 90-day spending in the sample.

This approach allows us to assess the impact in spending relative to the hospital's underlying mix of patients, e.g. to ask whether a hospital is high spending given its patient mix.

For all of our estimates below, each spending measure enters as a continuous measure that has been demeaned and scaled by 2 standard deviations to facilitate interpretation. Thus, all coefficients reflect a difference of between one standard deviation above vs. below the mean (i.e., coefficient values reflect comparisons between "low" vs. "high" spending hospitals).

3.4 Summary Statistics

Table 1 reports summary statistics for the analysis sample. The reliance on ambulance transports allows us to focus on patients who are less likely to decide whether or not to go to the hospital. This sample is slightly older (average age of 82) compared to all Medicare patients. 38% are male, and 90% are white. Common comorbidities measured in the year preceding (and including) the initial episode include hypertension (41%), chronic obstructive pulmonary disease (COPD, 21%), diabetes (20%), and pneumonia (19%).

The third column reports the standardized differences in means for the 90-day total spending measure: the difference in the mean of the covariate when the instrument is above versus below its median value computed from a regression model that controls for ZIP code by

[8]See the Medicare Spending Per Beneficiary methodology report (http://www.qualityforum.org/Projects/c-d/Cost_and_Resource_Project/2158.aspx) for full details.

patient-origin fixed effects, divided by the pooled standard deviation. Relative to commonly-used maximum standardized difference thresholds for assessing sample balance (e.g., a maximum of 0.25 standard deviations; see Rubin (2001)), these standardized differences are remarkably small across the wide range of control variables, consistent with the effective random assignment of ambulance companies to patients. A similar level of balance is found for 90-day spending on inpatient and non-patient categories as well.

4 Results

4.1 First Stage

Table 2 presents the main results. The main explanatory variable is the total 90-day spending measure for the hospital. The first column includes ZIP-by-patient origin and year fixed effects. The second column adds controls for patient characteristics, including principal diagnosis, demographics, co-morbidities, and pre-hospital care. Average 90 day spending for our hospitalized patients is $27,351, and a two standard deviation increase should be regarded as a $8,486 increase.[9] Inpatient spending constitutes the majority of spending, with an average of $15,876 and two standard deviations totaling $6,226. Non-inpatient spending over the 90 days after discharge averages $10,557, and two standard deviations totals $3,170.

With or without controls, the first-stage coefficient is 0.192. This means that an increase in the average 90-day spending measure for the hospitals where the ambulance company takes other patients by 1 (a 2 s.d. increase) is associated with a 0.192 increase in the 90-day spending measure where the patient is actually treated, an increase of 0.192*2 = 0.384 standard deviations. The fact that the relationship between the ambulance and hospital measures is not one-to-one is illuminating about the nature of the variation used in the instrumental-variables results. Consider an ambulance company that ordinarily treats patients in a geographic area and takes seriously-ill patients to a particular hospital. That ambulance company is then called in to a nearby area via a mutual aid agreement. The first-stage results suggest that this patient is much more likely to be transported back to the ambulance company's usual hospital, but at a lower rate than the rate at which it transports its usual patients.

4.2 90-day total spending

Panel B shows the results that relate 90-day total spending to one-year mortality. The OLS results show that patients who are sent to high 90-day spending hospitals have modestly lower mortality rates compared to hospitals with lower 90-day spending: a two standard-deviation increase in the measure is associated with a 1.7 percentage point reduction in the absolute risk of mortality compared to a mean of 43% in models with ZIP-by-patient origin, and year fixed effects. This coefficient falls to 1.1 percentage points when additional controls are included. That is, the OLS coefficient suggests that a two-standard deviation in area health care spending leads to a 2.6% reduction in mortality compared to the mean. This

[9] All dollar amounts are in 2012 dollars using the CPI-U and means and standard deviations for the main explanatory variables are reported in Table A2.

is consistent with the broader cross-sectional literature across markets, which as noted earlier finds at best a modest association between higher spending levels and lower mortality levels.

The instrumental variable estimates are reported in the third row. Here, in the model with baseline controls the point estimate is a 2.0 percentage-point reduction in mortality when patients are transported to hospitals with high spending levels compared to hospitals with lower 90-day total spending levels. The estimate with the full set of controls is almost identical.

While the IV estimates are somewhat larger in magnitude compared to the OLS estimates, our first conclusion is that the modest relationship between spending and mortality is not driven by patient selection. Our OLS and instrumental variables estimates both show only a small relationship between total 90-day spending and mortality. Replacing the dependent variable in our regression by total patient spending over 365 days, we can estimate the implied patient cost per life-year saved from this regression. We estimate this cost at more than $300,000 per life year, which is high relative to existing estimates of the value of a life year.

We view these results as corroborative of the cross-sectional results that hospitals with high levels of spending do not have improved outcomes compared to low-spending hospitals (Barnato et al. 2010; E. S. Fisher et al. 2003a; E. S. Fisher et al. 2003b; Skinner and Fisher 2010).

4.3 Inpatient vs. Non-inpatient Spending

Table 3 extends the analysis by disaggregating 90-day total spending into separate risk-standardized measures of 90-day inpatient and 90-day non-inpatient spending. We begin by showing the first stage estimates in Panel A, both with and without additional controls. The table shows our key coefficients of interest – the full specification is shown in Table A6.

Unsurprisingly, the first stage coefficient is stronger for inpatient care compared to post-acute non-inpatient care, as the direct assignment to hospital is more impacted by ambulance preferences than is the ultimate disposition of downstream care. Nevertheless, the first stage estimates are again highly statistically significant for both types of care.

Panel B shows OLS results, separately for inpatient and non-inpatient spending, first with no controls, then with controls included. The OLS estimates continue to show a lack of a relationship between spending levels and mortality. In a model with full controls, a two standard deviation increase in inpatient spending over the following 90 days is associated with a 1.0 percentage point reduction in mortality. For patients admitted to hospitals with high levels of non-inpatient spending over the subsequent 90 days, the estimated effect is very close to zero.

The magnitude of the estimates changes when we attempt to control for patient selection via instrumental variables. Patients admitted to hospitals with high levels of inpatient spending have substantially lower mortality rates: a point estimate of 4.7% in the model with baseline controls and 4.3% in the model with full controls. That is, mortality is about 10% lower than the mean for patients who are transported to high-spending hospitals.%[10]

In contrast, the instrumental variable estimates imply that patients admitted to hospitals with high levels of spending on non-inpatient downstream spending have significantly higher mortality rates. A two standard deviation increase in such spending is associated with a 5.5 percentage-point increase in mortality with baseline controls and a 4.6 percentage-point increase in a model with full controls. That is, we find that patients who are admitted to hospitals that typically have high post-acute spending are much more likely to die in the subsequent year.

This set of findings is striking, especially in light of an Institute of Medicine report that post-acute hospital spending is a major source of residual variation in US health care spending across areas (Newhouse, Garber, and Graham 2013). As in Doyle et al. (2015), hospitals with higher inpatient spending intensity achieve lower patient mortality. But as in the various studies cited earlier such as E. S. Fisher et al. (2003a) and Skinner and Fisher (2010) overall spending does not appear productive. Our results suggest a lack of productivity of spending after hospital discharge as a possible resolution of these discordant results.

4.4 A Closer Examination of Post-Acute Care

The fact that hospitals whose discharged patients have high spending on post-acute care also have increased mortality risk leads us to further explore the possible sources of inefficiency within this category. Table 4 explores these results in more detail.

The first question we address is whether the pattern of results holds when we estimate the inpatient and non-inpatient spending effects together: conditional on non-inpatient spending, for example, does inpatient spending continue to predict lower mortality?

Table 4 reports results when we include both measures of spending in the model. Panel A shows the first stages where two instrumental variables are used, reflecting the inpatient and non-inpatient spending levels of the hospitals where ambulances typically take other patients. We find that the ambulance measure for inpatient spending is highly predictive of inpatient spending itself, and likewise that the measure for post-acute spending is highly predictive of post-acute spending. We also find modest, negative cross-effects of both measures: patients transported by ambulance companies that take other patients to hospitals with higher levels of downstream, non-inpatient spending are treated at hospitals with lower inpatient treatment intensity, for example.

Panel B reports the OLS and IV results. When both measures are included simultaneously, the results are similar to the estimates in Table 3 where they are included separately. We

[10]Doyle et al. (2015) also find a substantial negative relationship between hospital spending at the initial admission and mortality. That paper focused on average log(spending) rather than the risk-standardized measure used here. Both spending measures answer related, but ultimately distinct, questions on the returns to Medicare spending overall vs. the returns to spending above and beyond what the case-mix would predict. The present measure is relevant to recent payment reform models that use risk-adjustment; the current results can also be more readily compared to the previous cross-sectional literature, which also risk adjusts. We considered separating our inpatient spending into pre-and post-discharge spending due to readmissions. However, post-discharge inpatient spending is so highly correlated with spending levels at the index admission that we cannot separately identify the two measures of resource intensity.

conclude that the inefficiency of post-acute spending is not driven solely by a negative correlation with productive inpatient spending.

Next, we considered each major post-acute spending category to examine the potential sources of post-discharge care related to higher mortality. The means reported in Table 5 show that Skilled Nursing Facilities (SNF) spending is the single largest category of post-acute spending, accounting for half of such spending and averaging over $5000. Home health, which is seen as a lower-cost substitute for SNF care, averages less thatn $2000 in this sample. The remaining “other” category includes physician office visits, labs, hospice, outpatient facilities and durable medical equipment and averages under $3000.[11]

The OLS estimates in Table 5 show that inpatient, home-health, and other spending measures are modestly, negatively correlated with mortality, while SNF spending is modestly, positively correlated with mortality. Again, these relationships could be confounded by non-random assignment of patients to this type of spending.

The IV estimates in Panel B of Table 5 show in fact that there is a very strong and positive relationship between being admitted to a hospital with high SNF spending and mortality, with a reciprocal relationship with home healthcare spending.[12][13] Treatment at a hospital with higher levels of other spending leads to higher mortality as well. When we include all of the measures in the same model, the pattern of results remains. The inpatient spending coefficient declines slightly, although this model is controlling for downstream SNF spending, which could reflect the productivity of the inpatient spending in a way that competes with the inpatient measure itself. In summary then, the results are consistent with hospitals whose patients accrue more inpatient and home healthcare spending are productive in terms of reducing patient mortality, but this is offset by the inefficiency of being admitted to a hospital with high SNF spending.

The positive relationship with SNF spending is striking. We have not, however, distinguished between two potential explanations for this relationship. The first is that SNF use itself is harmful. The second is that SNF use is not harmful, but instead that use of the SNF is a marker of poor hospital quality. Absent a direct instrument for SNF use, as opposed to an instrument that works through hospital assignment, we are unable to distinguish these views. Nevertheless, the results resolve a puzzle regarding different results in the cross-sectional and natural-experimental literatures about the spending-mortality relationship described in the introduction, and they suggest that if we are interested in testing reforms to reduce waste in the US healthcare system, post-acute care appears to be a particularly fruitful area to consider.

[11]We considered estimating risk-standardized hospital spending measures for each of these smaller categories, but found that doing so resulted in a preponderance (over 75%) of 0 mass in most categories, since only a small share of the sample ever used those services in the 90-days following discharge. We therefore elected to aggregate them into a residual category.

[12]See Table A3 for the first stages, which are similar to the first stages reported earlier

[13]We also considered an alternative measure risk-standardized measures of SNF utilization within 90 days and found similar results for mortality as shown using the 90-day SNF spending result in Table 5.

4.5 Extensions and Robustness

In this section, we consider several extensions and robustness checks of our results. First, we consider whether our findings for mortality extend to another measure of patient outcomes, hospital readmissions, which are usually taken as an indicator of poor initial hospital care. A disadvantage of this measure relative to our mortality indicator is the complication of competing risks: if hospitals with high inpatient spending keep you alive longer, that raises the opportunity to be readmitted. Nevertheless, so long as these competing risks are not too large, we should obtain similar results for this alternative quality measure.

In Appendix Table A4, we replicate our basic analysis using hospital readmission as a dependent variable. In fact, we find that our basic pattern of results persists: weak associations in OLS that become strong and typically significant (although not always) in IV. The negative effect of inpatient spending on readmissions (despite competing risks concerns), and the positive effect of non-inpatient spending, further confirms post-acute care as a likely source of waste in the health care system.

The effects of admission to different hospitals that vary in their treatment intensity will vary across different types of patients. Another way to explore the sources of the results, and consider such heterogeneous treatment effects, is to compare results across different types of patients. Table 6 reports results for different age and diagnosis categories.

When we break the sample into three age groups, we find similar point estimates for 65–74 and 75–84 year olds: IV point estimates for inpatient spending on the order of −0.06, and +0.06 for non-inpatient spending. The oldest category shows a smaller point estimate for inpatient spending (−0.029) and a somewhat larger point estimate for non-inpatient spending (0.065). While the point estimates are not statistically significantly different across the age groups, the oldest category has the largest SNF admission rate, consistent with focusing on SNF spending as a marker of quality or candidate for efficiency gains.

The next set of results compares patients across 5 major diagnosis categories. Again, the OLS point estimates are relatively small. The IV results for inpatient spending are similar to the main results for Circulatory conditions, including heart attacks and strokes, Respiratory conditions, and Injuries including hip fracture. Digestive conditions, including treatment for ulcers, has a larger point estimate (−0.100), but again the standard errors are larger when considering subgroups (s.e. = 0.0395). Inpatient spending for “Other” conditions has a small relationship with mortality; this is the only category where the inpatient spending-mortality relationship is not significant in the IV results.

In terms of non-inpatient spending, we find similar results compared to the overall results for Circulatory, Respiratory and Other diagnoses. Interestingly, Digestive conditions again behaves somewhat differently, with no relationship between non-inpatient spending levels at the hospital and mortality. This category has a relatively low SNF admission rate, perhaps dampening effects of downstream coordination. Meanwhile, for patients diagnosed with an Injury, admission to a hospital that has high downstream spending results in a higher mortality rate (coeff. = 0.072, s.e. = 0.03). This is suggestive that greater rehabilitation services after treatment for an injury is a marker for lower-quality care within the hospital.

Finally, Table 7 considers another source of heterogeneity in our results: differential impacts across hospital types. To address this, we have reestimated our model separately for teaching hospitals, non-profit non-teaching hospitals, and for-profit hospitals. Interestingly, we find that for each type of hospital we see the same pattern of offsetting reductions in mortality from inpatient spending and increases in mortality from non-inpatient spending. The magnitudes of these associations differ across hospital types, with the largest effects for teaching hospitals; the reason for these differencs is not clear, although they are generally not stastically distinguishable by hospital type.

The heterogeneity results are meant to explore the relationship between overall spending levels and mortality, as opposed to spending on a particular condition. One concern is that hospitals that are high-spending on average may not be high spending for a particular condition. This would result in a failure of the monotonicity condition that enables interpretation of the results as local average treatment effects: assignment to a “high-spending” ambulance company need not imply that the hospital is high-spending for the patient’s condition. As a further robustness check, we calculated the instrument at the hospital-major diagnosis level defined by 5 main categories of disease (circulatory, respiratory, injury, digestive, and all other) and found similar results: a coefficient of −0.074 (s.e.=0.011) for 90-day inpatient spending and 0.064 (s.e.=0.014) for non-inpatient spending.[14]

As the final exercise in this section, we also consider a robustness test of our findings. Our results focus only on spending (inpatient and non-inpatient) as a measure of intensity of care. But it is of course possible that spending may be correlated with other hospital characteristics. To partially address this point, we can run “horse race” regressions where we not only include spending, but also other key hospital characteristics that are likely associated with hospital quality. We can estimate these models by IV, instrumenting for the alternative hospital measures in the same way that we do for spending (by using ambulance preferences across these other measures).

We show the results of doing so in Table A5, for the three key measures of hospital volume, whether the hospital is a teaching hospital, and whether the hospital is for-profit. In fact, we find that in each case controlling for these measures does not much affect our main result. Of course, this is only a subset of possible measures we could use for this exercise. But the fact that the results are so robust to this subset is comforting.

5 Conclusions

One of the key challenges facing health policy makers is how best to redesign hospital reimbursement systems to reflect provider quality and reduce costs. In this paper we have sought to overcome selection bias and characterize the relationship between hospitals’ spending profiles and patient outcomes. Our findings are consistent with previous evidence of low returns to area-wide, total spending differences, yet high returns to hospital treatment

[14]One other concern may be that certain hospitals use post-acute services such as home health and SNFs as substitutes for longer lengths of stay. To address this, we estimated the correlation between SNF utilization and length of stay and found only modest (0.006) correlation between the two.

intensity: the resolution to this mystery is the potentially unproductive role played by post-acute care. Use of hospitals which are associated with higher spending on Skilled Nursing Facilities, in particular, appears to lead to both higher costs for Medicare and higher mortality rates for the elderly.

Our results provide somewhat subtle implications for spending reform efforts. On the one hand, overall 90 day spending does not seem closely related to patient outcomes, which suggests that bundled episode-based payments could be lowered relative to current fee for service levels. On the other hand, inpatient spending does appear highly productive, so lowering such bundled payments could penalize high performing hospitals if the reduced reimbursement is not properly allocated across the bundle participants (hospitals and post-acute care facilities).

Ideally, the formation of Accounatable Care Organizations could lead to the internalization of these tradeoffs, allowing for targeting spending towards productive inpatient care and strategies that improve the efficiency of post-acute care. Unfortunately, the early results for ACOs do not suggest that path is being followed to date: only 18% of ACOs are contracting with SNFs at this point (Colla et al. 2016). Rather than relying on ACOs to internalize these incentives, an alternative would be to penalize or reward hospitals based on the downstream spending of patients discharged from these hospitals. That is, policy makers could move beyond penalizing readmissions to penalizing other measures of care that reflect poor outcomes.

Of course, a limitation is that the results directly apply only to serious, emergent conditions. A high priority for future work is to find different ways to address patient selection in extending this type of analysis to a broader set of diagnoses. Still, the types of conditions studied here are costly and represent prime candidates for the type of episode-based bundled payments currently under discussion in payment reform models.

Another limitation is that spending on SNF care itself may be a marker for quality rather than a source of poor outcomes. As such, the results can guide quality measurement rather than prescribe a reduction in SNF use. Future research should test the returns different types of treatment intensity directly. For example, more randomized controlled trials that encourage the switch from SNF care to home health care, complete with monitoring to avoid subsequent hospitalizations, appear to present opportunities to improve health and lower costs.

The results suggest that the aim of ACOs should be to improve patient health such that SNF admission is not needed. A comprehensive set of quality measures that include utilization and health outcomes such as mortality can result in a new system that “pays for quality”.

Appendix

Table A1

Sample Characteristics and Balance: Principal Diagnosis

	Mean	Standardized Difference 1(*Instrument* > *Median*)
038 Septicemia	0.129	−0.029
162 Malignant neoplasm of trachea, bronchus, and lung	0.008	0.002
197 Secondary malignant neoplasm of respiratory and digestive systems	0.005	0.009
410 Acute myocardial infarction	0.077	0.020
431 Intracerebral hemorrhage	0.011	0.010
433 Occlusion and stenosis of precerebral arteries	0.008	0.004
434 Occlusion of cerebral arteries	0.070	−0.002
435 Transient cerebral ischemia	0.027	0.004
482 Other bacterial pneumonia	0.017	0.003
486 Pneumonia, organism unspecified	0.128	0.007
507 Pneumonitis due to solids and liquids	0.048	−0.002
518 Other diseases of lung	0.057	−0.013
530 Diseases of esophagus	0.011	0.006
531 Gastric ulcer	0.009	0.010
532 Duodenal ulcer	0.007	0.004
557 Vascular insufficiency of intestine	0.007	−0.002
558 Other and unspecified noninfectious gastroenteritis and colitis	0.008	−0.001
560 Intestinal obstruction without mention of hernia	0.024	−0.002
599 Other disorders of urethra and urinary tract	0.084	−0.012
728 Disorders of muscle, ligament, and fascia	0.006	−0.012
780 General symptoms	0.080	0.013
807 Fracture of rib(s), sternum, larynx, and trachea	0.005	−0.001
808 Fracture of pelvis	0.014	−0.001
820 Fracture of neck of femur	0.112	0.007
823 Fracture of tibia and fibula	0.004	0.003
824 Fracture of ankle	0.008	0.001
959 Injury, other and unspecified	0.002	0.001
965 Poisoning by analgesics, antipyretics, and antirheumatics	0.002	−0.008
969 Poisoning by psychotropic agents	0.002	−0.002
Other Non-Discretionary[†]	0.030	−0.002

Notes: N=1,575,273. Balance statistics report the standardized difference (i.e., the difference in means divided by the pooled standard deviation) based on splitting the sample on whether patients were admitted to a hospital profiled above or below the median for the total 90D risk-standardized spending measure. [†]Based on diagnoses used in Mulcahy, et al. *N Engl J Med* 2013; 368:2105–2112.

Source: 2002–2012 Medicare Part A and B Data

Table A2

Sample Characteristics: Hospital Measures

	Mean	Standard Deviation
Total 90D Spending	27,351	4,243
Inpatient Spending	15,876	3,113
Non-Inpatient Spending	10,557	1,585
SNF Spending	5,164	1,114
Home Health Spending	1,773	581
Other Post-Discharge Spending	2,700	446

Notes: N=1,575,273. All measures have been risk-standardized by age, race and gender. Other category includes non-inpatient physician spending, outpatient facility spending, rehab and long-term care hospital spending, durable medical equipment spending, and hospice spending. Subcategory spending may not add to total category spending due to outlier and sample adjustments in risk-standardization process.

Source: 2002–2012 Medicare Part A and B Data

Table A3

First Stage Estimates by 90D Spending Measure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
First Stage								
	90D-IP	90D-SNF	90D-HH	90D-OTH	90D-IP	90D-SNF	90D-HH	90D-OTH
Amb:90D Inpatient	0.251 (0.0052)				0.264 (0.0055)	−0.038 (0.0046)	0.005 (0.0062)	0.027 (0.0039)
Amb:90D SNF		0.143 (0.0042)			−0.028 (0.0028)	0.150 (0.0043)	−0.002 (0.0027)	−0.020 (0.0029)
Amb:90D Home Health			0.164 (0.0049)		−0.014 (0.0043)	−0.002 (0.0032)	0.161 (0.0052)	0.016 (0.0030)
Amb:90D Other				0.141 (0.0042)	−0.022 (0.0043)	−0.020 (0.0029)	0.025 (0.0027)	0.139 (0.0044)
Spending Mean	15,876	5,164	1,773	2,700				
Spending SD	3,113	1,114	581	446				

Notes: N=1,575,273. Outcome Mean=0.426. IP=inpatient; HH=home health; SNF=skilled nursing facility; OTH=other spending. Other category includes non-inpatient physician spending, outpatient facility spending, rehab and long-term care hospital spending, durable medical equipment spending, and hospice spending. All spending measures are trimmed of outliers and then risk-standardized by age, race and gender. Risk-standardized spending measures have been demeaned and scaled by 2 standard deviations. Thus, coefficients reflect a difference of ±1 standard deviations from the mean (i.e., “low” vs. “high” spending). Models include all patient and ambulance controls listed in Table 1, ZIP code × patient origin fixed effects, year fixed effects, and primary diagnosis fixed effects (see Table A1 for a full list of categories). Standard errors, clustered at the Health Service Area (HSA) level, are reported in parentheses.

Source: 2002–2012 Medicare Part A and B Data

Table A4

30-Day Readmission Outcomes: First Stage, OLS and 2SLS Estimates, by Risk-Standardized 90D Hospital Spending Measure

		90D Inpatient Spending		90D Non-Inpatient Spending	
		(1)	(2)	(3)	(4)
Panel A. First Stage					
	Ambulance Average 90D Inpatient Spending	0.250 (0.0052)	0.251 (0.0052)		
	Ambulance Average 90D Non-Inpatient Spending			0.136 (0.0045)	0.137 (0.0045)
Panel B. OLS					
	90D Inpatient Spending	0.002 (0.0016)	−0.003 (0.0015)		
	90D Non-Inpatient Spending			0.003 (0.0017)	0.002 (0.0016)
Panel C. 2SLS					
	90D Inpatient Spending	−0.021 (0.0063)	−0.030 (0.0063)		
	90D Non-Inpatient Spending			0.021 (0.0092)	0.009 (0.0090)
	Sample Size	1,575,273			
	Outcome Mean	0.426			
	Patient Controls	No	Yes	No	Yes

Notes: Each column reports model results based on hospital measures of total spending over 90 days after the index admission. All spending measures are trimmed of outliers and then risk-standardized by age, race and gender. Risk-standardized spending measures have been demeaned and scaled by 2 standard deviations. Thus, the reported coefficients reflect a difference of ±1 standard deviations from the mean (i.e., “low” vs. “high” spending). Means (SDs) for spending variables: 90D Inpatient Total = $15,876 (3,113); 90D Non-Inpatient Total = $10,557 (1,585). Models without patient controls include ZIP × patient origin fixed effects, as well as year and principal diagnosis controls (see Table A1 for a full list); models with patient controls adds all patient and ambulance controls as listed in Table 1, with age controls in 5-year bins. Standard errors, clustered at the Health Service Area (HSA) level, are reported in parentheses.

Source: 2002–2012 Medicare Part A and B Data

Table A5

One Year Mortality: OLS and 2SLS Estimates Across Different 90D Spending Measures and Hospital Characteristics

		(1)	(2)	(3)
Panel A. OLS				
		365D Mort.	365D Mort.	365D Mort.
	90D Inpatient	−0.001 (0.0020)	−0.007 (0.0022)	−0.010 (0.0019)
	90D Non-Inpatient	0.003 (0.0023)	−0.001 (0.0022)	−0.000 (0.0022)
	Volume	−0.010 (0.0017)		
	Teaching Hospital		−0.006 (0.0017)	

	(1)	(2)	(3)
For-Profit			0.004(0.0021)
Panel B. 2SLS			
90D Inpatient	−0.044 (0.0100)	−0.051 (0.0105)	−0.050 (0.0088)
90D Non-Inpatient	0.051 (0.0130)	0.052 (0.0129)	0.052 (0.0130)
Volume	−0.013 (0.0054)		
Teaching Hospital		0.001 (0.0065)	
For-Profit			0.010 (0.0083)
Sample Size			
Outcome Mean			

Notes: Each column reports model results based on a single regression utilizing hospital measures of total spending over 90 days after the index admission, as well as hospital characteristics as defined in the text. All spending measures are trimmed of outliers and then risk-standardized by age, race and gender. Risk-standardized spending measures have been demeaned and scaled by 2 standard deviations. Thus, the reported coefficients reflect a difference of ±1 standard deviations from the mean (i.e., "low" vs. "high" spending). Means (SDs) for spending variables: 90D Inpatient Total = $15,876 (3,113); 90D Non-Inpatient Total = $10,557 (1,585). Models without patient controls include ZIP × patient origin fixed effects, as well as year and principal diagnosis controls (see Table A1 for a full list); models with patient controls adds all patient and ambulance controls as listed in Table 1, with age controls in 5-year bins. Standard errors, clustered at the Health Service Area (HSA) level, are reported in parentheses.

Source: 2002–2012 Medicare Part A and B Data

Table A6

Full Regression Results for 90-Day Inpatient and Non-Inpatient Spending

Outcome: 365-Day Mortality	(1)	(2)	(3)
Risk-Standardized Spending Measures			
90D Inpatient Spending	−0.0497 (0.00928)	−0.0424 (0.00918)	
90D Non-Inpatient Spending	0.0596 (0.0139)		0.0538 (0.0136)
Year Controls			
year==2003	0.0382 (0.00192)	0.0384 (0.00189)	0.0383 (0.00189)
year==2004	0.0336 (0.00189)	0.0335 (0.00187)	0.0336 (0.00189)
year==2005	0.0263 (0.00188)	0.0257 (0.00186)	0.0265 (0.00186)
year==2006	0.0216 (0.00180)	0.0209 (0.00178)	0.0218 (0.00177)
year==2007	0.0156 (0.00183)	0.0146 (0.00180)	0.0157 (0.00181)
year==2008	0.0140 (0.00186)	0.0130 (0.00183)	0.0144 (0.00184)
year==2009	0.00714 (0.00181)	0.00623 (0.00178)	0.00742 (0.00179)

Outcome: 365-Day Mortality	**(1)**	**(2)**	**(3)**
year==2010	0.00598 (0.00179)	0.00515 (0.00177)	0.00617 (0.00178)
year==2011	0.00508 (0.00174)	0.00481 (0.00172)	0.00506 (0.00172)
Principal Diagnosis Controls			
idiag1	0.0427 (0.0206)	0.0397 (0.0204)	0.0415 (0.0207)
idiag2	0.379 (0.0207)	0.373 (0.0204)	0.378 (0.0207)
idiag3	0.377 (0.0208)	0.370 (0.0205)	0.375 (0.0208)
idiag4	−0.0709 (0.0207)	−0.0753 (0.0204)	−0.0730 (0.0207)
idiag5	0.217 (0.0210)	0.213 (0.0207)	0.212 (0.0211)
idiag6	−0.152 (0.0209)	−0.157 (0.0206)	−0.155 (0.0210)
idiag7	−0.0449 (0.0207)	−0.0506 (0.0205)	−0.0473 (0.0208)
idiag8	−0.285 (0.0208)	−0.291 (0.0205)	−0.286 (0.0208)
idiag9	−0.0161 (0.0211)	−0.0210 (0.0208)	−0.0168 (0.0211)
idiag10	−0.100 (0.0207)	−0.105 (0.0204)	−0.101 (0.0207)
idiag11	0.0771 (0.0207)	0.0723 (0.0204)	0.0759 (0.0207)
idiag12	0.0984 (0.0209)	0.0944 (0.0206)	0.0975 (0.0209)
idiag13	−0.244 (0.0208)	−0.250 (0.0206)	−0.247 (0.0209)
idiag14	−0.213 (0.0208)	−0.219 (0.0205)	−0.215 (0.0209)
idiag15	−0.169 (0.0206)	−0.173 (0.0204)	−0.171 (0.0207)
idiag16	−0.0360 (0.0548)	−0.0428 (0.0547)	−0.0408 (0.0546)
idiag17	−0.0832 (0.0209)	−0.0855 (0.0207)	−0.0844 (0.0210)
idiag18	−0.268 (0.0209)	−0.275 (0.0206)	−0.269 (0.0210)
idiag19	−0.161 (0.0208)	−0.165 (0.0205)	−0.162 (0.0208)
idiag20	−0.136 (0.0207)	−0.143 (0.0204)	−0.138 (0.0208)
idiag21	−0.382 (0.0399)	−0.392 (0.0399)	−0.381 (0.0403)
idiag22	−0.209 (0.0210)	−0.215 (0.0207)	−0.211 (0.0211)
idiag23	−0.266 (0.0206)	−0.273 (0.0204)	−0.268 (0.0207)
idiag24	0.0743 (0.0314)	0.0678 (0.0310)	0.0610 (0.0314)

Outcome: 365-Day Mortality	**(1)**	**(2)**	**(3)**
idiag25	−0.0572 (0.0222)	−0.0647 (0.0219)	−0.0668 (0.0223)
idiag26	−0.239 (0.0217)	−0.248 (0.0215)	−0.244 (0.0218)
idiag27	0.104 (0.0316)	0.0974 (0.0313)	0.0959 (0.0317)
idiag28	−0.00953 (0.0574)	−0.0167 (0.0571)	−0.0174 (0.0574)
idiag29	−0.191 (0.0209)	−0.198 (0.0206)	−0.194 (0.0210)
idiag30	0.0350 (0.0286)	0.0315 (0.0283)	0.0277 (0.0287)
idiag31	−0.220 (0.0209)	−0.226 (0.0206)	−0.223 (0.0210)
idiag32	−0.226 (0.0209)	−0.232 (0.0206)	−0.228 (0.0210)
idiag33	−0.0817 (0.223)	−0.0917 (0.221)	−0.0788 (0.226)
idiag34	−0.208 (0.0256)	−0.217 (0.0252)	−0.213 (0.0256)
idiag35	−0.260 (0.0292)	−0.264 (0.0290)	−0.264 (0.0291)
idiag36	−0.222 (0.0208)	−0.229 (0.0206)	−0.225 (0.0209)
idiag37	−0.290 (0.0221)	−0.297 (0.0219)	−0.293 (0.0221)
idiag38	−0.193 (0.0206)	−0.199 (0.0203)	−0.195 (0.0206)
idiag39	−0.236 (0.0213)	−0.242 (0.0210)	−0.238 (0.0214)
idiag40	−0.266 (0.0209)	−0.272 (0.0207)	−0.268 (0.0210)
idiag41	0.219 (0.0759)	0.208 (0.0744)	0.187 (0.0741)
idiag42	−0.0257 (0.201)	−0.0299 (0.198)	−0.0404 (0.207)
idiag43	−0.0403 (0.0815)	−0.0307 (0.0812)	−0.0505 (0.0811)
idiag44	−0.0457 (0.0974)	−0.0625 (0.0968)	−0.0628 (0.0983)
idiag45	−0.0256 (0.0471)	−0.0287 (0.0459)	−0.0429 (0.0467)
idiag46	0.601 (0.0553)	0.591 (0.0549)	0.574 (0.0519)
idiag47	0.336 (0.276)	0.340 (0.276)	0.331 (0.274)
idiag48	−0.0912 (0.0288)	−0.0965 (0.0288)	−0.0922 (0.0292)
idiag49	−0.219 (0.0761)	−0.231 (0.0727)	−0.228 (0.0760)
idiag50	−0.221 (0.0217)	−0.227 (0.0214)	−0.223 (0.0217)
idiag51	−0.177 (0.0221)	−0.182 (0.0218)	−0.179 (0.0221)

Outcome: 365-Day Mortality	(1)	(2)	(3)
idiag52	−0.238 (0.0217)	−0.243 (0.0214)	−0.240 (0.0217)
Demographic Controls			
ag70t74	0.0327 (0.00180)	0.0328 (0.00180)	0.0336 (0.00178)
ag75t79	0.0700 (0.00173)	0.0715 (0.00174)	0.0717 (0.00173)
ag80t84	0.116 (0.00172)	0.118 (0.00172)	0.118 (0.00171)
ag85t89	0.174 (0.00174)	0.177 (0.00173)	0.176 (0.00174)
ag90t94	0.244 (0.00191)	0.246 (0.00190)	0.247 (0.00191)
ag95p	0.318 (0.00287)	0.320 (0.00286)	0.321 (0.00287)
male	0.0706 (0.000969)	0.0674 (0.000973)	0.0698 (0.000956)
black	−0.0205 (0.00214)	−0.0124 (0.00212)	−0.0250 (0.00209)
race_other	−0.0358 (0.00312)	−0.0210 (0.00306)	−0.0402 (0.00308)
Comorbidity Controls			
como_hyper	−0.0300 (0.00110)	−0.0300 (0.00110)	−−0.0298 (0.00110)
como_stroke	0.00980 (0.00211)	0.00936 (0.00207)	0.00959 (0.00211)
como_cervas	−0.0113 (0.00164)	−0.0119 (0.00162)	−0.0111 (0.00163)
como_renal	0.0557 (0.00138)	0.0557 (0.00137)	0.0554 (0.00136)
como_dialysis	0.173 (0.00428)	0.171 (0.00418)	0.170 (0.00421)
como_COPD	0.0322 (0.00135)	0.0318 (0.00134)	0.0324 (0.00135)
como_pnuemo	0.0247 (0.00140)	0.0240 (0.00140)	0.0249 (0.00140)
como_diabetes	0.0108 (0.00132)	0.0106 (0.00131)	0.0109 (0.00132)
como_protein	0.0778 (0.00192)	0.0778 (0.00191)	0.0776 (0.00191)
como_dementia	0.0624 (0.00140)	0.0621 (0.00138)	0.0624 (0.00139)
como_FDLsDis	0.0170 (0.00178)	0.0163 (0.00175)	0.0166 (0.00178)
como_periph	0.0207 (0.00148)	0.0204 (0.00148)	0.0206 (0.00148)
como_metaCancer	0.274 (0.00246)	0.273 (0.00244)	0.274 (0.00246)
como_trauma	−0.00333 (0.00135)	−0.00374 (0.00135)	−0.00327 (0.00135)
como_subs	0.00294 (0.00186)	0.00300 (0.00186)	0.00283 (0.00186)

Outcome: 365-Day Mortality	**(1)**	**(2)**	**(3)**
como_mPsych	−0.0229 (0.00211)	−0.0227 (0.00210)	−0.0229 (0.00211)
como_cLiver	0.112 (0.00460)	0.112 (0.00457)	0.111 (0.00462)
card_hf	0.0608 (0.00133)	0.0608 (0.00132)	0.0606 (0.00133)
card_mi	0.0123 (0.00207)	0.0123 (0.00206)	0.0122 (0.00206)
card_ua	−0.0396 (0.00275)	−0.0391 (0.00272)	−0.0396 (0.00275)
card_cathero	−0.0108 (0.00124)	−0.0106 (0.00123)	−0.0108 (0.00123)
card_respFal	0.0149 (0.00172)	0.0146 (0.00171)	0.0148 (0.00172)
card_hyperhd	−0.0296 (0.00335)	−0.0302 (0.00334)	−0.0297 (0.00333)
card_valve	0.0263 (0.00163)	0.0262 (0.00162)	0.0262 (0.00163)
card_arrhythmia	0.0266 (0.00119)	0.0265 (0.00118)	0.0265 (0.00119)
Ambulance Controls			
amb miles	−0.000499 (0.0000835)	−0.000536 (0.0000840)	−0.000627 (0.0000851)
amb_als	0.0211 (0.00115)	0.0214 (0.00115)	0.0210 (0.00115)
amb_emergency	−0.0329 (0.00174)	−0.0333 (0.00172)	−0.0327 (0.00172)
amb_iv	−0.00325 (0.00203)	−0.00378 (0.00202)	−0.00316 (0.00202)
amb_intubate	0.135 (0.0136)	0.135 (0.0134)	0.133 (0.0135)
amb_op	−0.00921 (0.00322)	−0.00568 (0.00324)	−0.00856 (0.00319)
Constant	−0.00108 (0.000107)	0.000169 (0.0000831)	−0.00124 (0.000105)

Standard errors in parentheses

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Table 1

Sample Characteristics and Balance

	Mean	Standard Deviation	Standardized Difference 1(*Instrument* > *Median*)
Age	81.614	7.596	−0.029
Male	0.376	0.484	−0.002
Race: Black	0.073	0.264	0.017
Race: Other	0.031	0.174	0.006
Hypertension	0.409	0.490	−0.028
Stroke	0.068	0.253	0.004
Cerebrovascular Disease	0.101	0.300	−0.004
Renal Failure Disease	0.155	0.353	−0.053
Dialysis	0.015	0.122	−0.001
COPD	0.212	0.408	−0.008
Pneumonia	0.191	0.391	−0.015
Diabetes	0.196	0.396	−0.009
Protein Calorie Malnutrition	0.070	0.252	−0.014
Dementia	0.176	0.381	0.000
Paralysis	0.085	0.278	−0.002
Peripheral Vascular Disease	0.131	0.336	−0.010
Metastatic Cancer	0.034	0.181	0.000
Trauma	0.137	0.342	−0.012
Substance Abuse	0.067	0.247	−0.014
Major Psych. Disorder	0.056	0.229	−0.004
Chronic Liver Disease	0.010	0.097	0.000
Ambulance: Miles Traveled with Patient	7.051	8.216	0.006
Ambulance: Advanced Life Support	0.688	0.467	−0.041
Ambulance: Emergency Transport	0.912	0.293	−0.048
Ambulance: Outpatient File	0.072	0.242	−0.074
Ambulance: Payment	340.510	192.316	−0.108

Notes: N=1,575,273. Balance statistics report the standardized difference based on the difference in the mean of the covariate when the instrument is above versus below its median value computed from a regression model that controls for year and ZIP code × patient origin fixed effects, divided by the pooled standard deviation. Average age reported here, however in all regression models age controls are included as dummy variables for 5 year age bins starting at age 66.

Source: 2002–2012 Medicare Part A and B Data

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Table 2

One Year Mortality: First Stage, OLS and 2SLS Estimates, by Risk-Standardized 90D Hospital Spending Measure

		(1)	(2)
Panel A. First Stage			
	Ambulance Average Total 90D Spending	0.192 (0.0043)	0.192 (0.0042)
Panel B. OLS and 2SLS			
	OLS: Hospital Average Total 90D Spending	−0.017 (0.0023)	−0.011 (0.0021)
	2SLS: Hospital Average Total 90D Spending	−0.020 (0.0113)	−0.021 (0.0105)
	Sample Size	1,575,273	
	Outcome Mean	0.426	
	Patient Controls	No	Yes

Notes: Within each panel and regression type (OLS and 2SLS), each column reports model results based on hospital measures of total spending over 90 days after the index admission. Spending measures are trimmed of outliers and then risk-standardized by age, race and gender. Risk-standardized spending measures have been demeaned and scaled by 2 standard deviations. Thus, the reported coefficients reflect a difference of ±1 standard deviations from the mean (i.e., “low” vs. “high” spending). Means (SDs) for spending variables: 90D Total = $27,351 (4,243). Column (1) includes ZIP × patient origin fixed effects, as well as year and principal diagnosis controls (see Table A1 for a full list); Column (2) adds all patient and ambulance controls as listed in Table 1, with age controls in 5-year bins. Standard errors, clustered at the Health Service Area (HSA) level, are reported in parentheses.

Source: 2002–2012 Medicare Part A and B Data

Table 3

One Year Mortality: First Stage, OLS and 2SLS Estimates, by Risk-Standardized 90D Hospital Spending Measure

		90D Inpatient Spending		90D Non-Inpatient Spending	
		(1)	(2)	(3)	(4)
Panel A. First Stage					
	Ambulance Average 90D Inpatient Spending	0.250 (0.0052)	0.251 (0.0052)		
	Ambulance Average 90D Non-Inpatient Spending			0.136 (0.0045)	0.137 (0.0045)
Panel B. OLS					
	90D Inpatient Spending	−0.017 (0.0021)	−0.010 (0.0019)		
	90D Non-Inpatient Spending			0.002 (0.0023)	−0.001 (0.0022)
Panel C. 2SLS					
	90D Inpatient Spending	−0.047 (0.0096)	−0.043 (0.0087)		
	90D Non-Inpatient Spending			0.055 (0.0142)	0.046 (0.0127)
	Sample Size	1,575,273			
	Outcome Mean	0.426			
	Patient Controls	No	Yes	No	Yes

Notes: Each column reports model results based on hospital measures of total spending over 90 days after the index admission. All spending measures are trimmed of outliers and then risk-standardized by age, race and gender. Risk-standardized spending measures have been demeaned and scaled by 2 standard deviations. Thus, the reported coefficients reflect a difference of ±1 standard deviations from the mean (i.e., "low" vs. "high" spending). Means (SDs) for spending variables: 90D Inpatient Total = $15,876 (3,113); 90D Non-Inpatient Total = $10,557 (1,585). Models without patient controls include ZIP × patient origin fixed effects, as well as year and principal diagnosis controls (see Table A1 for a full list); models with patient controls adds all patient and ambulance controls as listed in Table 1, with age controls in 5-year bins. Standard errors, clustered at the Health Service Area (HSA) level, are reported in parentheses.

Source: 2002–2012 Medicare Part A and B Data

Table 4

One Year Mortality: OLS and 2SLS Estimates by 90D Spending Measure

		(1)	(2)	(3)	(4)
		90D Inpatient	90D Non-Inpatient	90D Inpatient	90D Non-Inpatient
Panel A. First Stage					
	Amb: 90D Inpatient Spending	0.265 (0.0055)	−0.009 (0.0036)	0.265 (0.0056)	−0.009 (0.0036)
	Amb: 90D Non-Inpatient Spending	−0.041 (0.0032)	0.139 (0.0048)	−0.042 (0.0032)	0.139 (0.0048)
		365D Mortality		365D Mortality	
Panel B. OLS					
	90D Inpatient Spending	−0.011 (0.0024))		−0.010 (0.0019)	
	90D Non-Inpatient Spending	0.004 (0.0026)		−0.000 (0.0022)	
Panel C. 2SLS					
	90D Inpatient Spending	−0.047 (0.0115)		−0.050 (0.0088)	
	90D Non-Inpatient Spending	0.058 (0.0190)		0.052 (0.0129)	
	Sample Size	1,575,273			
	Outcome Mean	0.426			
	Patient Controls	No	No	Yes	Yes

Notes: N=1,575,273. Outcome Mean=0.426. For Panels A and B, columns (1) and (3) report model results based on measures of spending that, when summed across types, equal total spending over 90 days beginning with the index admission. All spending measures are trimmed of outliers and then risk-standardized by age, race and gender. Risk-standardized spending measures have been demeaned and scaled by 2 standard deviations. Thus, coefficients reflect a difference of ±1 standard deviations from the mean (i.e., "low" vs. "high" spending). All models include ZIP code × patient origin fixed effects, year fixed effects, and primary diagnosis fixed effects (see Table A1 for a full list of categories). Models with full controls also include all patient and ambulance controls listed in Table 1. Standard errors, clustered at the Health Service Area (HSA) level, are reported in parentheses.

Source: 2002–2012 Medicare Part A and B Data

Table 5

One Year Mortality: OLS and 2SLS Estimates Across Different 90D Spending Measures

		(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. OLS								
	Outcome:	365D Mort.	365D Mort.	365D Mort.	365D Mort.	365D Mort.	Spending Mean	Spending SD
	90D Inpatient	−0.010 (0.0019)				−0.008 (0.0019)	15,876	3,113
	90D SNF		0.009 (0.0020)			0.007 (0.0021)	5,164	1,114
	90D Home Health			−0.011 (0.0020)		−0.008 (0.0021)	1,773	581
	90D Other				−0.010 (0.0019)	−0.007 (0.0021)	2,700	446
Panel B. 2SLS								
	90D Inpatient	−0.043 (0.0087)				−0.037 (0.0100)	15,876	3,113
	90D SNF		0.092 (0.0123)			0.072 (0.0121)	5,164	1,114
	90D Home Health			−0.085 (0.0115)		−0.068 (0.0121)	1,773	581
	90D Other				0.030 (0.0123)	0.048 (0.0134)	2,700	446
	Sample Size	1,575,273						
	Outcome Mean	0.426						

Notes: N=1,575,273. Outcome Mean=0.426. IP=inpatient; HH=home health; SNF=skilled nursing facility; OTH=other spending. Other category includes non-inpatient physician spending, outpatient facility spending, rehabilitation and long-term care hospital spending, durable medical equipment spending, and hospice spending. Columns (1)–(4) report model results based on measures of spending that, when summed across types, equal total spending over 90 days beginning with the index admission. Results in columns (1)–(4) are based on separate regressions for each spending measure, while those in column (5) report model results from a single regression with all spending measures included. All spending measures are trimmed of outliers and then risk-standardized by age, race and gender. Risk-standardized spending measures have been demeaned and scaled by 2 standard deviations. Thus, coefficients reflect a difference of ±1 standard deviations from the mean (i.e., “low” vs. “high” spending). Models include all patient and ambulance controls listed in Table 1, ZIP code × patient origin fixed effects, year fixed effects, and primary diagnosis fixed effects (see Table A1 for a full list of categories). Standard errors, clustered at the Health Service Area (HSA) level, are reported in parentheses.

Source: 2002–2012 Medicare Part A and B Data

Table 6

2SLS Estimates: One Year Mortality: Selected Subgroups

		OLS	2SLS	Sample Size	90D SNF Admission Rate
		(1)	(2)		
Age					
65–74	90D-IP	−0.015 (0.0036)	−0.050 (0.0188)	350,714	0.351
	90D-Non-IP	−0.004 (0.0042)	0.055 (0.0263)		
75–84	90D-IP	−0.010 (0.0027)	−0.060 (0.0132)	714,561	0.472
	90D-Non-IP	−0.005 (0.0033)	0.044 (0.0189)		
85+	90D-IP	−0.006 (0.0031)	−0.029 (0.0142)	670,134	0.593
	90D-Non-IP	−0.002 (0.0032)	0.065 (0.0182)		
Diagnosis Category					
Circulatory	90D-IP	−0.016 (0.0039)	−0.050 (0.0193)	340,535	0.382
	90D-Non-IP	−0.006 (0.0046)	0.064 (0.0297)		
Respiratory	90D-IP	−0.014 (0.0039)	−0.059 (0.0168)	430,311	0.477
	90D-Non-IP	−0.006 (0.0041)	0.042 (0.0223)		
Digestive	90D-IP	−0.024 (0.0075)	−0.100 (0.0395)	115,487	0.384
	90D-Non-IP	−0.007 (0.0081)	−0.027 (0.0551)		
Injury	90D-IP	−0.005 (0.0037)	−0.051 (0.0205)	314,114	0.711
	90D-Non-IP	0.006 (0.0044)	0.072 (0.0292)		
Other	90D-IP	−0.002 (0.0031)	−0.015 (0.0130)	534,962	0.474
	90D-Non-IP	−0.006 (0.0036)	0.056 (0.0194)		

Notes: All spending measures are trimmed of outliers and then risk-standardized by age, race and gender. Risk-standardized spending measures have been demeaned and scaled by 2 standard deviations. Thus, the reported coefficients reflect a difference of ±1 standard deviations from the mean (i.e., “low” vs. “high” spending). Means (SDs) for spending variables: 90D Inpatient Total = $15,876 (3,113); 90D Non-Inpatient Total = $10,557 (1,585). Models include ZIP × patient origin fixed effects, as well as year and principal diagnosis controls and all patient and ambulance controls as listed in Table 1. Standard errors, clustered at the Health Service Area (HSA) level, are reported in parentheses. The last column reports 90-day SNF utilization rates for each subgruop.

Source: 2002–2012 Medicare Part A and B Data

Table 7

2SLS Estimates: One Year Mortality: Selected Hospital Characteristics

		OLS (1)	2SLS (2)	Sample Size
Teaching Hosptial	90D-IP	−0.005 (0.0032)	−0.075 (0.0228)	700,226
	90D-Non-IP	−0.017 (0.0044)	0.121 (0.0426)	
Non-Profit, Non-Teaching	90D-IP	−0.012 (0.0044)	−0.023 (0.0201)	750,505
	90D-Non-IP	0.007 (0.0038)	0.042 (0.0209)	
For-Profit, Non-Teaching	90D-IP	−0.022 (0.0089)	−0.058 (0.0397)	212,552
	90D-Non-IP	0.001 (0.0070)	0.062 (0.0367)	

Notes: All spending measures are trimmed of outliers and then risk-standardized by age, race and gender. Risk-standardized spending measures have been demeaned and scaled by 2 standard deviations. Thus, the reported coefficients reflect a difference of ±1 standard deviations from the mean (i.e., “low” vs. “high” spending). Means (SDs) for spending variables: 90D Inpatient Total = $15,876 (3,113); 90D Non-Inpatient Total = $10,557 (1,585). Models include ZIP × patient origin fixed effects, as well as year and principal diagnosis controls and all patient and ambulance controls as listed in Table 1. Standard errors, clustered at the Health Service Area (HSA) level, are reported in parentheses. The last column reports 90-day SNF utilization rates for each subgruop.

Source: 2002–2012 Medicare Part A and B Data