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HEALTHCARE EXCEPTIONALISM? PRODUCTIVITY AND ALLOCATION IN THE U.S. HEALTHCARE SECTOR

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ABSTRACT

The conventional wisdom in health economics is that large differences in average productivity across hospitals are the result of idiosyncratic, institutional features of the healthcare sector which dull the role of market forces. Strikingly, however, we find that productivity dispersion in heart attack treatment across hospitals is, if anything, smaller than in narrowly defined manufacturing industries such as ready-mixed concrete. While this fact admits multiple interpretations, we also find evidence against the conventional wisdom that the healthcare sector does not operate like an industry subject to standard market forces. In particular, we find that hospitals that are more productive at treating heart attacks have higher market shares at a point in time and are more likely to expand over time. For example, a 10 percent increase in hospital productivity today is associated with about 4 percent more patients in 5 years. Taken together, these facts suggest that the healthcare sector may have more in common with "traditional" sectors than is often assumed.

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1. Introduction

A central observation about the U.S. healthcare sector is the existence of substantial differences in productivity across regions and across hospitals. For example, annual Medicare spending per capita ranges from $6,264 to $15,571 across geographic areas (Skinner, Gottlieb, and Carmichael 2011), yet health outcomes do not positively covary with these spending differentials (e.g. Fisher et al 2003a,b; Baicker and Chandra 2004; Chandra, Staiger, and Skinner 2010; Skinner 2011). Similar patterns have been documented across hospitals within geographic markets (e.g., Yasaitis et al 2009). These facts have in turn generated substantial academic interest in understanding the root causes of the underlying productivity dispersion and what can increase productivity at under-performing hospitals (e.g. Skinner, Staiger and Fisher 2006; Chandra and Staiger 2007; Staiger and Skinner 2009). Outside of academia, these "Dartmouth Atlas" facts have also attracted consider popular attention (see, for example, Gawande's 2009 *New Yorker* article) and were heavily cited by the Obama administration during the discussions leading up to the 2010 Affordable Care Act (e.g. Pear's 2009 *New York Times* article or Office of Management and Budget 2009).

The conventional wisdom in health economics is that the driving forces behind these large average productivity differences are various idiosyncratic, institutional features of the healthcare sector that effectively reduce competitive pressures on providers. Oft-cited culprits include uninformed consumers who lack knowledge of the quality and price differences across providers, generous health insurance that insulates consumers from the direct financial consequences of their healthcare consumption decisions, and public sector reimbursement that provides little incentive for productive efficiency by providers. These factors are widely believed to dull the basic disciplining force of demand-side competition that exists in most other sectors.

Echoing and advancing this view, Cutler (2010) notes:

> "There are two fundamental barriers to organizational innovation in healthcare. The first is the lack of good information on quality. Within a market, it is difficult to tell which providers are high quality and which are low quality… Difficulty measuring quality also *makes expansion of high-quality firms more difficult* [emphasis added]… The second barrier is the stagnant compensation system of public insurance plans."

In a similar vein, Skinner (2011) states in his overview article on regional variations in healthcare:

> "[low productivity producers are]…unlikely to be shaken out by normal competitive forces, given the patchwork of providers, consumers and third-party payers each of which faces inadequate incentives to improve quality or lower costs…"

This notion of "healthcare exceptionalism" has a long tradition in health economics. It dates back at least to the seminal article of Arrow (1963), which started the modern field of health economics by emphasizing key features of the health care industry that distinguish it from most other sectors and therefore warrant tailored study.

But when it comes to productivity dispersion, the ostensibly unique features of the healthcare sector stand alongside a large empirical literature outside of the health care sector that has documented extensively – almost without exception – enormous differences in average productivity across producers within narrowly defined industries (see Bartelsman and Doms (2000), Syverson (2011), and references therein). For example, on average within narrow US manufacturing (4-digit SIC) industries, the 90th productivity percentile plant creates almost twice as much output as the 10th percentile plant, given the same inputs (Syverson 2004a). This dispersion exists both within and across geographic markets (e.g. Syverson 2004a,b).

We estimate that productivity dispersion across hospitals in treating heart attacks is about the same order of magnitude as productivity dispersion within narrowly defined manufacturing industries. Figure 1 (whose construction we describe in much more detail later in the paper) shows, for example, that productivity dispersion across hospitals for heart attack treatment is slightly lower than productivity dispersion across ready-mixed concrete plants. Ready-mixed concrete is, like healthcare, a spatially differentiated good in that it is produced and consumed locally, but one in which the product is less differentiated, insurance does not dampen price sensitivity, and prices aren't set administratively. More generally, looking across 450 different narrowly defined (4-digit SIC code) manufacturing industries in the US, average within-industry productivity dispersion in manufacturing is quite similar to our estimates across hospitals for heart attack treatment (Syverson 2004a).

This finding is striking and, we believe, surprising. But, it admits multiple possible explanations. Productivity dispersion has been shown, both theoretically and empirically, to shrink with greater competition within and across industries (e.g. Syverson, 2004a,b; Martin 2008; Balasubramanian and Sivadasan 2009). However, we would not be comfortable drawing any direct inferences about the relative roles of competition in these two very different sectors from comparisons of their productivity dispersions.

Rather, these facts serve as a point of departure that motivates us to re-examine productivity and allocation within the healthcare sector using the analytical insights from the broader productivity literature. In particular, we draw on a long tradition of theoretical and empirical work in manufacturing examining whether higher productivity producers are systematically allocated greater market shares; in healthcare, the prevailing wisdom captured by the Cutler (2010) and Skinner (2011) quotations above is that these re-allocation forces are weak or non-existent.

Our findings suggest otherwise. Figures 2a and 2b (again discussed in more detail later in the paper) give a qualitative flavor of our results. They show that within a market-year, hospitals that have higher productivity for heart attack treatment tend to have greater market share (i.e., more heart attack patients) at a point in time (Figure 2a) and experience more growth in market share over time (Figure 2b). Quantitatively, we find that a 10 percent increase in hospital productivity is associated with about a 25 percent higher market share at a point in time and 4 percent more growth over the next 5 years.

A finding that the market allocates more market share to more productive firms at a point in time and over time is a robust characteristic of US manufacturing industries (Syverson 2011 provides a recent review) but is noticeably absent from manufacturing in less competitive settings such as Central and Eastern European countries at the beginning of their transition to a market economy (Bartelsman, Haltiwanger, and Scarpetta 2009), Chile prior to trade reforms (Pavcnik 2002), or the US steel industry in the 1960s (Collard-Wexler and de Loecker 2013). As a result, these allocation metrics are often interpreted as "signposts of competition." As in much of this previous work in manufacturing, we do not establish a causal link between competition and the signs of competition in the data. It could be that competitive market forces re-allocate market share to higher productivity hospitals, or it could be that higher productivity hospitals happen to have other features – such as beautiful lobbies or good managers – which separately increase demand. But whatever the driving force behind them, some force or forces in the healthcare sector lead it to evolve in a manner favorable to higher productivity producers. This finding puts US healthcare on a very different part of the map than, say, Romanian or Slovenian manufacturing in the early 1990s, where there appears to have been little (or even negative) correlation between a firm's productivity and its market share (Bartelsman et al, 2009). The results are particularly noteworthy given the context of heart attack treatments, where the acute nature of the condition might be expected to generate a smaller role for market forces in allocating patients to more productive hospitals than for less time-sensitive conditions such as cancer treatment, the management of chronic conditions, or elective procedures.

Taken together, our results suggest that healthcare may have more in common with "traditional" sectors than is commonly recognized in popular discussion and academic research. Continued efforts to understand productivity dispersion and uncover what may improve productivity in the US healthcare sector may therefore benefit from greater attention to the theoretical and empirical insights from the broader productivity literature. Naturally, the converse applies as well.

The rest of the paper proceeds as follows. Section 2 describes the analytical framework. Section 3 discusses our estimation of hospital productivity – the key empirical input to all our analyses. Section 4 presents our main results on the relationship between hospital productivity and market share. Section 5 discusses some questions of interpretation, including possible mechanisms behind the findings and various gauges of their magnitude. Section 6 shows that our main findings are robust to a variety of alternative specifications. A concluding section follows.

2. Analytical Approach: Static and Dynamic Allocation

Our primary empirical exercise examines the correlation between producer (i.e. hospital) productivity and market share at a point in time, and the correlation between producer productivity and growth in market share over time. These relationships have been analyzed in a variety of industries and countries as a proxy for the role of competition in these settings (e.g., Olley and Pakes 1996; Pavcnik 2002; Escribano and Guasch 2005; Bartelsman, Haltiwanger, and Scarpetta 2009; Collard-Wexler and De Loecker 2013). Intuitively, competitive forces exert pressure on low productivity firms, causing them to either become more efficient, shrink, or exit.

Models of such reallocation mechanisms among heterogeneous-productivity producers have found applications in a number of fields, including industrial organization, trade, and macroeconomics.[1] While these models differ considerably in their specifics, they share a common intuition: greater competition – as reflected in greater consumer willingness or ability to substitute to alternate producers – makes it more difficult for higher-cost (lower-productivity) firms to earn positive profits, since demand is more responsive to their cost and price differentials relative to other firms in the industry. As substitutability increases, purchases are reallocated to more productive firms, raising the correlation between productivity and market share at a point in time ("*static allocation*") and causing more productive firms to experience higher growth over time ("*dynamic allocation*"). Appendix A describes this archetypical mechanism slightly more formally.

For the static allocation analysis, we will use the following regression framework:

$$\ln\left(N_{h,t}\right) = \beta_0 + \beta_1 a_{h,t} + \gamma_{M,t} + \varepsilon_{h,t} \quad (1)$$

where $N_{h,t}$ is a measure of the market size of hospital h in year t, $\gamma_{M,t}$ are market-year fixed effects, and $a_{h,t}$ is our estimate of total factor productivity (which we refer to throughout as TFP) of hospital h in year t; we discuss in detail below how we estimate $a_{h,t}$. Thus β_1 reflects the static relationship between a hospital's TFP and its market share, within a hospital market-year. If the coefficient is positive, as has been found in many U.S. industries (e.g., Olley and Pakes 1996; Hortaçsu and Syverson 2007; Bartelsman, Haltiwanger and Scarpetta 2009), it indicates that higher productivity producers have a greater share of activity. If β_1 is zero or negative, as has been found for example in some former Soviet-bloc countries in the early 1990s (Bartelsman, Haltiwanger and Scarpetta 2009), in Chile prior to trade reforms (Pavcnik 2002), and in the U.S.

[1] See, for example, Ericson and Pakes (1995), Melitz (2003), and Asplund and Nocke (2006).

Steel industry circa 1960-70 (Collard-Wexler and De Loecker 2013), it indicates that less productive industry producers are the same size or larger than their high productivity counterparts and suggests that forces beyond standard competition are driving the allocation of market activity.[2]

The static allocation analysis in equation (1) can reflect the market's ability to reallocate activity from less productive hospitals to more productive ones. But it shows the outcome of this process rather than the process itself. To measure the actual dynamics of the market's selection and reallocation mechanisms, we employ two additional metrics.

Our first dynamic allocation metric examines the relationship between hospital TFP and its probability of closing. We will estimate:

$$I[exit_{h,t+1}] = \beta_0 + \beta_1 a_{h,t} + \gamma_{M,t} + \varepsilon_{h,t} \quad (2)$$

where $I[exit_{h,t+1}]$ is an indicator equal to one if hospital h exits at time $t+1$, and the right hand side variables are defined as in equation (1). Thus β_1 reflects the relationship between a hospital's TFP and its probability of exit, controlling for any changes in aggregate exit probabilities across market-years. A negative relationship between TFP and hospital exit is one of the most robust findings in the productivity literature (See Bartelsman and Doms 2000 and Syverson 2011 for surveys). It is indicative of a Darwinian selection process at work: less productive producers find it more difficult to survive.

Our second dynamic measure is the relationship between hospital TFP and future hospital

[2] A positive correlation between a hospital's productivity and the number of patients it treats is also consistent with increasing returns to scale, in which causality runs from scale to productivity rather than vice versa. This is a general issue for interpreting the static allocation measure in any industry. In the particular context of health care, the "volume-outcome" hypothesis conjectures that treating more patients improves provider performance. Not surprisingly, it has proven challenging to establish empirically whether an observed positive correlation between provider volume and outcomes is causal (see e.g. Epstein 2002 for a discussion of the interpretation difficulties in this literature). Moreover, it is harder to understand why scale economies would predict our "dynamic allocation" finding that *current* productivity predicts increases in the number of *future* patients.

growth. We will estimate:

$$\Delta_{h,t,t+1} = \beta_0 + \beta_1 a_{h,t} + \gamma_{M,t} + \varepsilon_{h,t} \tag{3}$$

where $\Delta_{h,t,t+1}$ is a measure of the hospital's growth rate (in terms of number of heart attack patients treated) between year t and $t+1$. A positive correlation between TFP and growth indicates that more productive hospitals see larger gains in patient traffic, and points to the operation of a selection and reallocation process. While not as robust as the negative TFP-exit relationship, there is widespread evidence in developed country manufacturing and retail that higher TFP producers experience growth in market shares (e.g. Scarpetta, Hemmings, Tressel, and Woo 2002; Disney, Haskel, and Heden 2003; and Foster, Haltiwanger, and Krizan 2006).

Regression equations (1) through (3) form the heart of our empirical analysis. They describe the associations between a hospital's productivity and market share and indicate whether forces exist that are favorable to the expansion of higher productivity producers. Although motivated by models in which competitive forces create these re-allocation pressures, the correlations are naturally not direct evidence of the impact of competition. After presenting our results, we discuss possible interpretations in light of other forces that may mimic the effects of competition.

3. Estimation of the Hospital Production Function.

The key empirical input for estimation of our analytical equations (1) through (3) is a measure of a producer's (i.e. hospital's) TFP. We estimate hospital TFP in the specific context of hospital treatment of heart attacks, analyzing the treatment and outcomes of about 3.5 million heart attack patients from 1993 through 2007. TFP is the amount of output a supplier can produce per unit input. In our setting, variation in TFP across hospitals reflects differences in patient survival (output) conditional on treatments (inputs) the patient receives. We describe the data and approach we use to estimate hospital TFP, and discuss key estimation challenges.

3.1 Setting: Heart Attack Treatments in US Hospitals

Heart attacks present an excellent setting for studying hospital productivity for a number of reasons. First, cardiovascular disease, of which heart attacks (acute myocardial infarctions, or AMIs) are the primary manifestation, is the leading cause of death in the United States. Second, the high post-AMI mortality (survival rates at one year are less than 70 percent in our Medicare population) provides an accurately measured outcome with a great deal of variation across hospitals. There is broad agreement that for AMIs, survival is the most important endpoint both clinically and in terms of patient preferences, and therefore a key measure of output, particularly in an elderly population.[3] Third, the emergency nature of heart attacks provides a setting in which the sorting of patients across providers is likely to be more limited than in many other healthcare settings, reducing empirical concerns arising from patients selecting into hospitals on the basis of their underlying health. At the same time, the reduced scope for sorting also makes the null hypothesis that higher productivity hospitals do not attract greater market share a particularly plausible one in this context. Finally, inputs are well measured and there exist rich data on the relevant health characteristics of the patients (called risk-adjusters) which can be used in the estimation. Not surprisingly, therefore, heart attacks have been the subject of considerable study in the medical and economics literature on the value of medical technology and the returns to medical spending (e.g. Cutler, McClellan, Newhouse and Remler, 1998; Cutler and McClellan, 2001; Skinner, Staiger and Fisher, 2006; Chandra and Staiger, 2007).

[3] Clinical trials for heart-attack therapies compare treatments by focusing on survival as the key outcome (see for example, Anderson et al., 2003), but this is not true for trials of treatments for more elective coronary conditions such as stable coronary disease where quality of life concerns make it more difficult to measure output. A review of over twenty-three trials for heart-attack treatments is provided by Keeley, Boura and Grines (2003).

3.2 The Hospital Production Function for AMI Patients

We posit a patient-level health production function of the following form:

$$y_p = A_{h,t}\left(\prod_k R_{p,k}^{\alpha_k} x_p\right)^{\mu} e^{\varepsilon_p} \tag{4}$$

where y_p is the number of post-AMI survival days of patient p treated at hospital h in year t, and x_p is a measure of hospital inputs used to treat this patient. All production functions relate outputs to inputs; our particular function uses patient survival days as a measure of output and a single (dollar-denominated) index of resources spent on the patient as inputs.[4] Because patients are inherently heterogeneous, survival may also depend on characteristics of the patient, which could potentially also be correlated with input choices. In addition, the marginal effect of inputs on survival may vary with patient characteristics. To capture both of these effects, we follow the literature and adjust inputs for a vector of observable patient-level risk factors, $R_{p,k}$, where k indexes the factors. The parameters α_k capture the influence of these risk factors on health. Thus the expression in the parentheses reflects risk-adjusted inputs on the patient. The parameter μ is the elasticity of survival days with respect to risk-adjusted inputs. Finally, the expression e^{ε_p} is a patient-level error term that accounts for random variations in health outcomes.

The key input into all of our analyses described in Section 2 is the logarithm of $A_{h,t}$, which we have previously called $a_{h,t}$. $A_{h,t}$ measures the (exponent of) total factor productivity (TFP) of hospital h in year t. It is common across all (risk-adjusted) patients in that hospital in that year.[5] Holding risk-adjusted inputs constant, differences in $A_{h,t}$ across hospitals produce

[4] This sort of single-input production function is unusual but convenient; one could reasonably interpret the single input as an index of the use of multiple inputs that go into producing health. In Appendix E we show the results are robust to the use of a multi-input production function instead.

[5] We allow hospital productivity to vary across years because it allows us to capture intertemporal variation in hospitals' efficiencies, and because it is consistent with standard practice in the broader productivity literature outside the healthcare sector. As we discuss below, we find that hospital productivity is highly persistent across years within our sample.

systematic differences in survival length. In other words, if it were possible to send a particular heart attack patient to two hospitals with different TFP levels, providing him the same level of inputs at both, the patient's expected survival would be greater in the higher TFP hospital than in the lower one.

The hospital production function model in (4) allows variation across providers in the marginal health product of inputs (i.e., $A_{h,t}\mu$ varies across hospital-years) but constrains them to have the same elasticity of output with respect to input (i.e., μ is common across hospitals). Our empirical specification therefore allows the "marginal return to inputs" curve to vary across hospitals, as suggested by Chandra and Staiger (2007) and Garber and Skinner (2008). Figure 3 provides a stylized illustration of our production function specification.

Taking logs, we have our main estimating equation for the hospital production function:

$$\ln(y_p) = \ln(A_{h,t}) + \mu \sum_k \alpha_k \ln(R_{p,k}) + \mu \ln(x_p) + \varepsilon_p \tag{5}$$

To estimate equation (5) we regress the log of patient survival days on a vector of risk factors ($R_{p,k}$), the inputs applied to each patient (x_p), and a set of hospital-year fixed effects. These hospital-year fixed effects are in turn our TFP estimates ($a_{h,t} \equiv \ln(A_{h,t})$) which we then use as inputs to estimate our main analytical equations (1) through (3).

3.3 Data and Measurement of Key Variables

Our primary dataset consists of all Medicare Part A (i.e., inpatient hospital) claims for all heart attacks (AMIs) in individuals age 66 and over in the United States from 1993 through 2007. We limit the sample to AMIs in patients who have not had an admission for an AMI in the prior year. We have information on mortality through 2008, so we can observe at least one year of post-AMI survival for all patients. In order to have enough data to estimate annual hospital productivity, we follow standard practice (e.g. Skinner and Staiger 2009) and eliminate any hospital-year with fewer than 5 heart attack patients that year. This restriction eliminates less than 1 percent of patients, but about 10 percent of hospital-years and 6 percent of hospitals; naturally the dropped hospitals are disproportionately small.

Tables 1a and 1b present some basic summary statistics on our sample. Our final sample consists of about 3.5 million heart attacks in 55,540 hospital-years and 5,346 unique hospitals. The average hospital-year has about 65 patients, but the median hospital-year has only 39 patients**.** We follow the literature in defining a hospital market (*M*) for an AMI as a Hospital Referral Region (HRR, see e.g. Chandra and Staiger 2007).[6] Our sample includes 304 HRRs, and on average they have about 12 hospitals in them. The Medicare claims data also include information on patient demographics (age, race and sex) and detailed information on co-morbidities (i.e. admissions for other conditions) during the prior year. We use this information as a basis of our risk adjusters $R_{p,k}$.

Our baseline output (survival) measure (y_p) is the number of days that the patient survives after receiving initial treatment, up through the first year. Survival includes the first day of treatment itself, so y_p is bounded from below at 1 and above at 367 days. As shown in Table 1, average survival through 1 year, censoring anyone who survives more than 1 year at 367 days of survival, is 268 days; about two-thirds of our sample survives past one year. We show below that our core results are robust to alternative time horizons for measuring output (i.e. 30 day or 5 year survival windows).

Our baseline input measure defines hospital factor inputs for a patient as the (dollar-

[6] The *Dartmouth Atlas of Healthcare* divides the United States into HRRs which are determined at the zip code level through an algorithm that reflects commuting patterns to major referral hospitals. HRRs, which are akin to empirically defined markets for healthcare, may cross state and county borders. A complete list of HRRs can be found at http://www.dartmouthatlas.org/. Since defining a market is not a straightforward undertaking, in Appendix D (Table A4) we also show that our results are robust to defining markets based on Hospital Service Areas (HSAs) instead; there are about 10 times as many HSAs as HRRs.

converted) sum of diagnostic-related group (or DRG) weights during the first 30 days following a heart attack. These DRG weights reflect the Centers for Medicare and Medicaid Services' (CMS's) assessment of the resources necessary to treat a patient as a function of the patient's comorbidities and procedures received. This approach is standard in the literature and ensures that we measure real services rendered to patients, purged of reimbursement (price) variation across geographic areas or hospitals (see e.g. Skinner and Staiger 2009, Gottlieb et al 2010). Appendix B gives a detailed description of our baseline input measure and the sources of variation that contribute to it.[7] About 15 percent of the variation is explained by indicator variables for whether the patient received one of two surgical procedures: bypass or stent.

On average, about $16,000 worth of hospital inputs are used on one of our patients in the 30 days following a heart attack, with a standard deviation of about $12,000. As is typical in healthcare, inputs are right skewed; the median is about $12,000 and the 90th percentile is nearly $32,000. We show below that our core results are generally robust across a wide range of alternative input measures, as well as across alternative time horizons for measuring inputs.

3.4 Estimation Challenges

Estimating productivity in any setting is conceptually straightforward but practically involves a number of measurement challenges (Syverson 2011). In addition to the measurement of output and inputs discussed above, we describe three other challenges to estimating the hospital production function: endogeneity of inputs, differences across hospitals in patient characteristics related to survival, and estimation error.

[7] As described in Appendix B, we make an adjustment to the prior literature's approach to account for the fact that some of CMS's DRGs are defined partly based on subsequent survival status. We purge our measure of this outcome-based variation in input measurement by assigning the relevant patients the average weight across the DRGs which distinguish otherwise similar treatments based on survival. We also discuss some of the challenges in measuring inputs in other settings (such as the handling of intermediate inputs or different qualities across workers) that we avoid here, as well as shared challenges such as the appropriate weighting of different inputs.

Endogeneity of Inputs. A general econometric concern that pervades production function estimation is the potential endogeneity of inputs. In a typical setting, productivity is the residual in a firm-level regression of outputs on inputs; therefore, the coefficient on inputs (μ in our setting) may be biased by a correlation between input choice and the residual (productivity). In our setting, however, because we observe production at the unit (patient) level, we can include hospital-year fixed effects, estimating μ solely from within-hospital-year variation. By identifying the coefficients on inputs only from variation within hospitals, we control for any tendency for hospitals with different productivity to use different amounts of inputs on average. Of course, any unobserved inputs that do not vary within the hospital (such as, for example, whether the hospital requires its staff to use checklists) will load onto our estimate of hospital productivity. This is not a problem per se; as in the productivity literature more broadly, we think of productivity as the component of output that cannot be explained by observed inputs.

However, our estimates will be biased if, within hospital-year, hospitals choose different observable input levels for patients who differ unobservably in their latent survival, or if their choice of unobservable inputs is correlated with observed inputs at the patient level. The sign of the bias of the estimate of μ is not obvious. Moreover, our focus is not on estimating μ. Our primary concern is what impact any bias in μ will have on our analysis of the relationship between estimated productivity and market share, which are the ultimate objects of interest for the analysis. We therefore evaluate below the robustness of our main results to imposing, rather than estimating, various values for the scale parameter μ. This method amounts to following the index number, or Solow residual, approach to measuring productivity in which factor elasticities are taken from auxiliary data such as factor cost shares. We are re-assured that our main results are quite insensitive to the choice of μ. This insensitivity also has an economic interpretation that we discuss below.

Differences Across Hospitals in Patient Characteristics. Even if μ is known and imposed based on auxiliary information, if patients at different hospitals differ on average in their unobserved survival probabilities, this variation will cause us to misestimate hospital productivity. As noted earlier, one of the reasons for the focus on heart attacks in the empirical literature is the belief that such patient sorting across hospitals may be less of an issue in an emergency setting. But this does not mean there is no potential for sorting; indeed, were there no mechanisms by which patients (or their surrogates) actively selected hospitals for AMI treatment, it would be difficult to view our re-allocation findings as consistent with a role for market forces.

Therefore, to try to minimize the impact of any unobserved patient health differences across hospitals, we follow the standard practice in the literature and include various risk adjusters ($R_{p,k}$) to control for observable patient characteristics that are related to health. In particular, our baseline specification controls for a full set of interactions between age (in five-year groupings), gender, and whether the patient is white, as well as various co-morbidities. Each co-morbidity is included as an indicator for whether the patient has been to the hospital for a specific condition in the year prior to the AMI admission. Table 1b shows that on average our patients are 78 years old (recall our sample is for the Medicare population), about half are female, and about 90 percent are white; it also presents the means for the 17 co-morbidities we include in our baseline specification. We show below that our main results are quite insensitive to using fewer or more (for a subsample of patients where they are available) risk adjusters.

Estimation Error in TFP Measures. The median hospital-year in our sample has less than 40 patients, and for 20 percent of our hospital-years we observe fewer than 15 patients. The consequence of a relatively small number of patients in some hospital-years, together with the stochastic nature of our outcome (survival), means that our key object of interest and input into all of our productivity metrics – hospital TFP, $a_{h,t}$ – may be estimated with error. Such estimation error will cause attenuation bias in our analysis of the relationship between market share and hospital productivity in equations (1) through (3).[8]

We therefore apply the standard shrinkage or "smoothing" techniques of the empirical Bayes literature (e.g. Morris, 1983) to adjust for estimation error in our estimates of hospital productivity.[9] Appendix C provides a detailed description of this procedure. The intuition behind it is that when a hospital's productivity is estimated to be far above (below) average, it is likely to be suffering from positive (negative) estimation error. Therefore, the expected level of productivity, given the estimated productivity, is a convex combination of the estimate and the mean of the underlying productivity process. The relative weight that the estimate gets in this convex combination varies inversely with the noise of the estimate (which is based on the standard error of the hospital-year fixed effect). In practice, as we show in Appendix C, our core finding that hospitals with higher estimated productivity get allocated more market share at a point in time and over time remains statistically significant without the empirical Bayes adjustment, although naturally the magnitude is attenuated. All the analyses of hospital TFP use the empirical Bayes adjustment unless explicitly noted.

3.5 Estimates of the Hospital Production Function

Table 2 presents our estimates of the "returns to scale" parameter (μ) from estimating

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[8] This small-sample problem is probably much less of an issue in more traditional settings for estimating productivity, since the number of units of output produced (the statistical analog of patients in our context) is much larger. Increasingly, however, the productivity literature is also trying to adjust for other sources of measurement error in output (e.g. Collard-Wexler, 2011, Dobbelaere and Mairesse, 2013).

[9] McClellan and Staiger (1999) introduced this approach into the healthcare literature when estimating quality differences across hospitals, and it has since been widely applied in the education literature for estimating and analyzing teacher or school value added measures (e.g. Kane and Staiger 2001, Jacob and Lefgren 2007).

equation (5). Column 1 presents our baseline estimates, which use our full set of risk adjusters. We estimate a coefficient on log patient inputs (μ) of 0.446 (standard error = 0.005), which suggests that every 1 percent increase in inputs per patient is associated with a 0.45 percent increase in survival days. A comparison of columns 1 through 3 indicates that our estimate of μ increases from 0.45 to 0.59 as we reduce the set of risk adjusters to just age, race and sex (column 2) or to nothing (column 3), with the age-race-sex risk adjustment accounting for most of the difference between the results with no risk adjusters and with all risk adjusters included. Our estimates of μ are in the middle of the (very wide) range of estimates that papers in this literature have produced.[10]

The key input into our productivity metrics is not our estimate of μ but rather our estimates of TFP, $a_{h,t}$. These objects are the hospital-year fixed effects from equation (5) and are the key right-hand-side variables in our estimating equations (1) through (3). We find a great deal of within-hospital persistence in productivity over time, with $a_{h,t}$ exhibiting an AR(1) coefficient of about 0.7.

As a validity check on whether our estimates are picking up differences in hospital productivity, we verify that these estimates correlate positively in the cross-section with observable and independently gathered hospital quality measures. This exercise is in the spirit of Bloom and Van Reenen (2007), who perform the reverse procedure: validating an observable measure of management quality by correlating it with estimates of firm level productivity.

[10] Skinner and Staiger (2009) note that various papers have used different right hand side specifications or sample periods to produce estimates of the "return to spending." They re-estimate many of these alternative specifications in a within-hospital linear probability model of an indicator for one year survival on one year inputs and produce estimates ranging from -0.015 to 0.122. In our data such linear probability models produce estimates of the "return to spending" of 0.072 to 0.100, depending on the risk adjusters. Within-hospital estimates of the return to input use tend to produce a positive relationship between inputs and survival, in contrast to the cross-region or cross-hospital comparisons that tend to find no or negative association between inputs and health-related outcomes. One parsimonious explanation for this difference would be if low productivity hospitals tended to compensate by using more inputs.

The results are summarized in Table 3, and several are presented graphically in Figure 4.[11] The first two columns of Table 3 show the correlation between our estimates of hospital TFP and two quality measures that were first collected by the Center for Medicare and Medicaid Services (CMS) in 2003; they have been publicly reported by the agency's "hospital compare" website (www.hospitalcompare.hhs.gov) since 2005. They are calculated by hospitals and submitted to CMS independently of the data that we use.

These measures are created to indicate the fraction of patients who received the treatment(s) that CMS determined were appropriate for their medical conditions. In the regressions, we convert them to z-scores by normalizing their means and variances to 0 and 1, respectively. In Table 3 column 1 we look at the hospital's z-score for beta blockers, which are inexpensive drugs that reduce the demands on the heart and are long-established as having important benefits for AMI patients after discharge. In column 2 we look at the z-score of a combined measure that sums across the number of patients who are given each of eight consensus AMI treatments and divides by the sum of patients appropriate for each of these treatments.[12] All of these measures have been studied in the literature and are considered indicative of good quality care (e.g. Higashi et al. 2007, Skinner and Staiger 2009, Jha et al. 2005, and cites therein). We use the measure in the first year it was collected to minimize the chance that hospitals responded to the reporting by changing the measure and thus reducing its

[11] Table 3 and Figure 4 examine regressions of our estimates of hospital TFP in 2003 on various hospital characteristics. We omit the EB correction for hospital TFP since classical measurement error on the left-hand side does not affect the consistency of a regression. The estimates of a hospital's 2003 TFP come from our full sample estimates of equation (2), but we use only a single year since most of the hospital characteristics are only available cross-sectionally. We choose 2003 estimates since that is the first year that the CMS quality measures are available.

[12] The eight measures are 1) given aspirin at arrival, 2) given aspirin at discharge, 3) given ACE inhibitor for left ventricular systolic dysfunction (LVSD), 4) given smoking cessation advice/counseling, 5) given beta blockers at arrival, 6) given beta blockers at discharge, 7) given fibrinolytic medication within 30 minutes of arrival, and 8) given percutaneous coronary intervention (PCI) within 90 minutes of arrival.

signal of quality.

In column 3 we use the Bloom et al. (2012) measure of hospital management quality.[13] It is based on a survey of management practices that were administered to a sample of approximately 300 hospitals in 2009 and 2010; a higher management z-score indicates closer conformance to management best practices. This measure of management quality has been found to be significantly negatively correlated with 30 day risk-adjusted mortality for patients in cardiac units (McConnell et al. 2013); outside the hospital sector, it has also been found to correlate positively and significantly with productivity, profitability, Tobin's Q, and firm survival (Bloom and Van Reenen 2007)

Reassuringly, the results indicate a positive correlation between these "external" measures of the quality of the hospital and our estimates of hospital productivity. For example, we estimate that a one standard deviation increase in the hospital's beta blockers score is associated with a 3 percent increase in hospital productivity. The results are statistically significant for the beta blockers and composite score; the results for the hospital management measure (which are available for only a very small subsample of our hospitals) are significant at the 10% level. We also find that teaching hospitals and urban hospitals have higher estimated productivity; estimated productivity is higher for non-profit hospitals than for for-profit or public hospitals.

4. Main Results: Static and Dynamic Allocation

Table 4 presents our central results on the static and dynamic allocation of patients across hospitals. In our discussion, we focus on column 1, which presents our baseline estimates based on the full set of risk adjusters (i.e. the same specification as shown in Table 2, column 1); the

[13] We are extremely grateful to Nick Bloom for providing us with these measures.

results are not sensitive to the choice of risk adjusters (columns 2 and 3).

The first row shows our static allocation analysis based on estimation of equation (1), examining the correlation between a hospital-year's productivity, $a_{h,t}$, and the logged number of heart attack patients it treats, $\ln(N_{h,t})$. Because we include market-year (HRR-year) fixed effects, this estimate is within market-year, relating a hospital's market share of heart attack patients to its TFP relative to other hospitals in its market-year. Our right-hand side measure of $a_{h,t}$ ($\equiv \ln(A_{h,t})$) is the estimate of productivity from estimation of the hospital production function in equation (5). We bootstrap the standard errors, clustering at the market level.

The results show a statistically significant positive relationship between productivity and market share, suggesting that within markets, more market share (patients) tends to be allocated to more productive hospitals at a point in time. In particular, our baseline estimate suggests that a 10 percent increase in a hospital's productivity is associated with about a 25 percent higher market share.[14] A visual presentation of the results is given in Figure 2a.

The second row shows our analysis of the TFP-exit relationship based on estimation of equation (2), which examines the within market-year relationship between a hospital-year's productivity $a_{h,t}$ and an indicator variable for whether the hospital "exits" next year. The regression's right-hand side and standard errors are calculated as in the static allocation analysis. We define the dependent variable $I[exit_{h,t+1}]$ equal to one if hospital h has less than 5 heart attack patients in each year from year $t+1$ to $t+5$.[15] We measure exit as the lack of more than 5 patients

[14] Because our sample is limited to hospital-years with at least 5 patients, there is a potential concern about selection on the dependent variable in the static analysis. (This is not a concern for the subsequent dynamic analysis). We explored the sensitivity of our static allocation results to an alternative, Tobit-style truncated regression and found that the static allocation results were slightly strengthened by this adjustment.

[15] There are a non-trivial number of hospital mergers over our time period. If hospital A merges with hospital B and physically shuts down, hospital A is coded as having 0 patients in subsequent years. If however, hospital A and B

in each of five subsequent years to try to ensure that we've captured a "permanent" reduction in volume, as opposed to measurement error stemming from idiosyncratic fluctuations in the number of patients that a hospital receives.

We find a statistically significant negative relationship between hospital productivity and subsequent exit. The baseline results suggest that a 10 percent increase in hospital productivity within a market-year is associated with a statistically significant decline in the probability of exit next year of about 0.3 percentage points (about an 8 percent decline relative to the baseline exit rate of 4.4 percent).

The bottom row of Table 4 shows our analysis of the TFP-growth relationship based on estimation of equation (3), which examines the within market-year relationship between a hospital-year's productivity ($a_{h,t}$) and its subsequent one-year growth. The right-hand side and standard errors are calculated as in the prior analyses. For our left-hand side measure of the hospital's one-year growth rate $\Delta_{h,t,t+1}$ we define

$$\Delta_{h,t,t+1} = \frac{N_{h,t+1} - N_{h,t}}{\frac{1}{2}(N_{h,t+1} + N_{h,t})} \qquad (6)$$

where $N_{h,t}$ is once again the number of heart attack patients treated by hospital h in year t. Our measure of the hospital's one-year growth rate thus divides the change in the number of patients between this year and next year by the average number of patients across these two years.[16] Again, the estimates are statistically significantly different from zero. The baseline results suggest that a 10 percent increase in hospital productivity within a market-year is associated with

both continue to exist physically and admit their own patients (e.g. Beth Israel and Deaconess), they continue to be coded as separate hospitals with each still assigned the AMI patients whom they admit.

[16] This monotonic transformation of the standard percentage growth rate metric bounds growth between -2 (exit) and +2 (growth from an initial level of 0). An attraction of this transformation is that it reduces the chance that the results are skewed by a few fast-growing but initially small hospitals that would have very large percentage growth rates. This growth rate transformation has been used in other contexts to avoid unnecessary skewness in the growth rate measure; see, for example, Davis, Haltiwanger, and Schuh (1996).

over a 1 percent increase in the number of patients the hospital treats in the next year.[17] Figure 2b gives a visual presentation of this relationship between hospital productivity and growth.

5. Interpretation and Discussion

5.1 Mechanisms

The above findings indicate that more productive hospitals have statistically significantly higher market share at a point in time and are more likely to increase that market share over time. These findings contrast with the conventional wisdom – summarized in the introductory quotations – that there is little in the healthcare sector to encourage the growth of higher productivity providers or weed out lower productivity ones. Our findings place US healthcare, at least qualitatively, in the same part of the spectrum as US manufacturing, and differentiate it from many less competitive manufacturing settings where these relationships have been found to not exist or even to have the opposite sign.

What mechanisms might act to allocate more patients to higher productivity hospitals in an emergency setting like heart attacks? A definitive answer is beyond the scope of this paper. However, we try in this section to present some initial, suggestive evidence.

We begin by examining whether the positive relationship between productivity and market share is primarily driven by patients choosing hospitals that, for a given amount of inputs, are more likely to produce high survival, or hospitals that, for a given amount of survival, use fewer inputs. Figures 5a and 5b therefore show the within market-year correlation, respectively, between risk-adjusted survival and market share (conditional on risk adjusted inputs) and

[17] Table 4 reports negative average annual growth; this is primarily due to the fact that our measure conditions on the hospital initially being in the market.

between risk-adjusted inputs and market share (conditional on risk adjusted survival).[18] The results suggest that the productivity-market share relationship is primarily driven by the relationship between risk-adjusted survival and market share. The positive correlation between risk-adjusted survival and market share (Figure 5a) is virtually the same as that between risk-adjusted productivity and market share in Figure 2a. The negative correlation between risk-adjusted inputs and market share (Figure 5b) is statistically significant but less than half the magnitude. These findings are consistent with patients and their surrogates primarily seeking out hospitals that achieve higher risk-adjusted survival (conditional on risk adjusted inputs) rather than seeking out ones that use fewer risk-adjusted inputs (conditional on risk-adjusted survival). In practice, we find that risk-adjusted survival and productivity are extremely highly correlated.

It is not immediately obvious how patients know which hospitals offer longer survival. This ambiguity is not unique to our study. Indeed, a long-standing question in the field – dating back at least to Arrow (1963) – is how patients can acquire information on provider quality. One possibility is some form of market-learning; hospitals acquire a reputation for good outcomes and this reputation spreads through physicians' professional networks and patients' social networks and influences patients, family members, physicians, and ambulance drivers to request treatment at hospitals that are better at producing survival. Indeed, in a related setting, Johnson (2011) finds that cardiac specialists who have higher risk-adjusted survival rates for their patients are less likely to stop practicing. She interprets this and related evidence as consistent with a model of market learning by the referring physician. Patients or their family members may also obtain such information themselves; there is some evidence, for example, that patients respond to

[18] As with our productivity estimates, we use an empirical Bayes correction to adjust our estimates of risk-adjusted survival and of risk-adjusted inputs for measurement error; our procedure accounts for the correlation in measurement error between these two objects.

provider report cards (e.g., Dranove et al. 2003 and Dranove and Sfekas (forthcoming)).

An alternative view, however, is that there is no scope for AMI patients or their surrogates to exercise choice over hospitals because in emergency situations all (or most) patients simply get taken to the nearest hospital. This hypothesis seems particularly natural given the famous McClellan et al. (1994) use of distance as an instrumental variable for which hospital treats a given AMI patient. With mechanical assignment of many patients to the nearest hospital, our static and dynamic allocation results could be produced spuriously if, for example, within a market, more densely populated (e.g. urban) areas have both higher productivity hospitals and faster population growth.

In practice, however, this type of strict mechanical allocation rule does not seem able to explain our findings. For one thing, we estimate that slightly over half of AMI patients go to a hospital that is *not* the closest one in their market; In other words, while the McClellan at al. (1994) distance instrument has a significant first stage with respect to hospital choice, its R^2 is far from 1. There is therefore scope for demand to affect patient allocation to hospitals in the AMI context. Moreover, when we produce a counterfactual allocation of patients by assigning each patient to his nearest hospital within an HRR instead of the one at which we observe treatment, our static and dynamic allocation results either substantially attenuate or actually reverse.[19]

Of course, the presence of active hospital choice by AMI patients or their surrogates does not establish that they are choosing on the basis of hospital productivity or risk-adjusted survival as in the speculative discussion of market learning above. It is possible that the correlation

[19] Specifically, the exit result reverses sign and is statistically insignificant; the growth result is less than 20 percent of the baseline estimate and is statistically insignificant; the static allocation result remains statistically significant but with a magnitude that is 20 percent of the baseline estimate; see Appendix Table A4 (Columns 1 vs 3) and Appendix D for more detail.

between productivity and market share reflects omitted factors that independently drive demand and correlate with productivity. For example, higher productivity hospitals might also have better non-health amenities like nicer lobbies, which would in turn influence hospital demand. Alternatively, high productivity hospitals could have better managers who improve both the production process and separately increase demand for the hospital.

As one highly imperfect and indirect way to gauge what may be driving the observed correlations between productivity and market share, we briefly examine how the magnitude of these static and dynamic relationships varies across hospitals and across markets. The results, which are shown in Appendix D (especially Table A5) are mixed. For example, within a market the allocation results are stronger for hospitals facing more competition for their patients (following Gaynor and Vogt's (2003) use of distance to nearest hospital as a proxy for hospital competition); however, at the market level there is no evidence that the allocation results are stronger for more competitive markets (following Syverson's (2004b)) use of population density as a proxy for market competition for a spatially differentiated product. More work is clearly needed to establish to what extent the allocation and re-allocation to more productive hospitals is a direct result of competition or the result of other factors that are correlated with both productivity and demand.

5.2 Magnitudes

For many economic and policy questions, the mechanism by which market share is allocated to higher productivity firms is quite important. However, the exact mechanism is less important for forecasting whether and to what extent the market is evolving in a manner that favors higher productivity firms. Here, the magnitude of the productivity-market share relationships we estimate becomes important.

To begin to try to shed some light on these magnitudes, we investigate how a hospital's productivity correlates with its within-market growth and exit over longer horizons than the one-year horizon examined in Table 4. Specifically, we re-estimate equations (2) and (3) replacing the dependent variables $I[exit_{h,t+1}]$ and $\Delta_{h,t,t+1}$ with $I[exit_{h,t+k}]$ and $\Delta_{h,t,t+k}$, respectively.

Table 5 shows the results. The first row shows the allocation relationships one year out (i.e. the results from Table 4, where k=1), and the subsequent rows show results up to 10 years out (k=10). The relationship between productivity and growth or exit strengthens in absolute value over time. For example, a 10% increase in hospital productivity is associated with about 1 percent more patients next year, 4 percent more patients in 5 years, and almost 6 percent more patients in ten years.[20]

As another way to provide a sense of magnitude, we calculate the market re-allocation associated with a standard deviation change of productivity. Our baseline estimate of the national standard deviation of hospital productivity is 0.17.[21] Thus a hospital that has one standard deviation higher productivity has about 40 percent higher market share at a point in time, and grows about 6 percent more over the next five years.

On the other hand, many other factors besides hospital productivity create the observed variation in market share. We estimate a partial R^2 on productivity in the static allocation regression (1) of about 5 percent, and in the growth regression (3) of about 0.06 percent. Of course, noise in our productivity estimate causes us to understate the ability of (precisely measured) productivity to explain market share.

[20] Because our data on growth and exit ends in 2007, as k rises, a smaller sample of hospital-years is available for these analyses. We verified that the finding that these relationships strengthen over time also holds (with quite similar magnitudes) if we restrict our sample to the hospital-years for which we observe at least 10 years of subsequent growth data (not shown).

[21] Appendix D (especially Table A6) presents the dispersion estimates and also shows that they are quantitatively stable across alternative sets of risk adjusters.

As a final way to provide a sense of the magnitudes of these relationships, we compare them to those in other industries. To do so, we produced estimates of the static and dynamic allocation analyses for the ready-mixed concrete industry, which produces a physically homogenous product. Details on the data, estimation and results can be found in Appendix D. Like healthcare, concrete is consumed and produced locally, so that spatial differentiation (i.e. physical distance) can be an important barrier to competition. Otherwise, however, concrete lacks many of the features deemed to be important impediments to competition in healthcare: prices are not set administratively, consumers are likely well informed about their choices, and they bear the financial consequences of their decisions.

Across all of our static and dynamic allocation measures, the results indicate a stronger (often an order of magnitude larger) relationship between producer productivity and market allocation for hospitals than for concrete plants. Likewise, Figure 1 shows that national productivity dispersion appears larger for concrete than for hospitals; we estimate a standard deviation of 0.25 in concrete, compared to 0.17 for hospitals.[22]

This comparative finding is not limited to concrete. The static and dynamic allocation analyses are not easily comparable to pre-existing estimates in other sectors. However, productivity dispersion in other U.S. manufacturing industries also tends to be similar to (indeed,

[22] We follow the tradition of the existing productivity literature and compute productivity dispersion metrics at a nationwide (within-year) level, even though the market for treating heart attacks is (like many of the manufacturing industries studied) plainly local. This standard practice arose in part because manufacturing industries, the focus of the previous literature, are often geographically broad. But the literature has also typically reported nationwide numbers even for those industries that are more locally oriented, such as ready-mix concrete (Syverson 2004b), in part because geographic differentiation is itself one of the possible causes of productivity dispersion within an industry. In practice, we find within-market year dispersion to be only slightly lower (standard deviation about 0.16) than our national dispersion estimate. Put another way, we estimate that about 88 percent of the within-year variation in hospital productivity is within (rather than across) markets. For concrete, we estimate that about 70 percent of the variation in productivity is within market.

somewhat larger than) our estimates for healthcare.[23]

We are not the first to perform such cross-industry comparisons in productivity dispersion. For example, looking across narrowly defined manufacturing industries, Syverson (2004a) finds that the extent of within-industry productivity dispersion is negatively correlated with proxies for the amount of substitutability or competition across firms within that industry. We caution, however, against drawing inferences about the extent of competition in such different settings as heart attack treatment and manufacturing from comparisons of productivity dispersion. Basic measurement differences – such as differences in the output definition (survival vs. revenue), how inputs are measured, and estimation error – raise real comparability concerns, albeit without creating a clear direction of bias.[24] Moreover, as noted earlier, the causal force behind reduced dispersion is unclear, and need not be competitive pressure.

Nonetheless, at a broad level, the comparison may serve as a useful benchmark against which to assess the quantitative relationships we have estimated for productivity and allocation

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[23] Compared to our estimate of a standard deviation of hospital productivity of 0.17, Foster, Haltiwanger and Syverson (2008) estimate an average within-industry standard deviation of productivity of 0.22 across a dozen manufacturing industries in the US selected for having physically homogeneous products (e.g. white pan bread, block ice, raw sugar cane, etc.); Bartelsman, Haltiwanger and Scarpetta (2009) estimate an average within-industry standard deviation of 0.39 across a broader range of manufacturing industries. Across 450 different narrowly defined (4-digit SIC code) US manufacturing industries, Syverson (2004a) estimates an average within-industry interquartile range of logged plant productivity of 0.29, compared to our estimate in Table A6 of 0.23 for hospitals. Although most of the work in productivity dispersion has focused on the manufacturing sector, the more limited work on productivity dispersion in service industries suggests that in general it is roughly similar to that found in manufacturing. For example, Fox and Smeets (2011) estimate productivity dispersion in four Danish service industries and four Danish manufacturing industries and find generally comparable estimates. Similarly, looking at 4-digit retail industries, Foster, Haltiwanger and Krizan (2006) estimate an average interquartile range for logged labor productivity which is comparable to Syverson (2004a)'s estimate of the interquartile range for logged labor productivity in manufacturing.

[24] To take but one example, the extent of measurement error in output – which would serve to attenuate estimates of the correlation between productivity and market share and to increase estimated dispersion – is likely different in healthcare than in manufacturing, although the sign of the difference is unclear. On the one hand, AMI survival is an accurately recorded account of output, in contrast to manufacturing revenue which could be reported with error and may confound output variation with price variation (see Foster, Haltiwanger, and Syverson, 2008 and 2012). On the other hand, in manufacturing industries output is more-or-less a deterministic function of inputs, while survival in our setting is stochastic. As discussed, we use the empirical Bayes "shrinkage" estimator to try to adjust for this stochastic element and the relatively small sample size within hospital-years.

in the US healthcare sector. They also seem inconsistent with the conventional wisdom that the variations in inputs across areas and hospitals without concomitant output gains are unique to healthcare and must therefore result from idiosyncratic features of the sector.

6. Robustness

We explored the robustness of our allocation and dispersion findings along a number of dimensions and were generally quite reassured by the results. Here, we briefly describe some of our robustness analysis concerning risk adjustment, measurement of inputs, measurement of output, and potential endogeneity of inputs. Appendix E presents the results in more detail.

6.1 Controls for Patient Health

A key concern is whether we have adequately controlled for patient characteristics that are correlated with both hospital choice and survival. We have already seen that our core results are robust to controlling for fewer observable characteristics than in our baseline specification; specifically all of our tables have shown results with no patient covariates and with only covariates for age/race/sex interactions, in addition to the "full" set of demographics and co-morbidities. In addition, for one year of our sample we have access to considerably richer data that are abstracted from patients' medical charts and contain many additionally relevant clinical characteristics such as test results and medical histories. We find that our results are not sensitive to including this more extensive set of controls (see Table A8).

6.2 Input Measure

We face several key choices with the construction of our input measure. One is how coarsely or finely to measure inputs. There is a tradeoff between our relatively coarse baseline measure of inputs (with its associated measurement error stemming from input variation that we do not capture) and more granular measures which suffer from potential survivorship bias (a patient cannot receive many procedures if she does not survive very long); we experimented with considerably more granular input measures based on the individual procedures received and the length of hospital stay. We also explored using these inputs directly in a multi-input production function rather than aggregating them to a single index as in our baseline approach. Finally, our baseline measure follows standard practice and defines inputs based only on hospital inpatient treatments, thereby excluding physician inputs – which may occur both inside and outside the hospital – and other outpatient inputs. We tried an alternative input measure that incorporates non-hospital inputs. Again there is a trade-off; some non-hospital inputs may be closely linked (or indeed part of) the care received in the hospital, while others may be quite distinct. These alternative input measures are each described in more detail in Appendix B and the results are presented in Table A9.[25]

6.3 Time Horizon

Another issue concerns the time horizon over which we measure inputs and outputs. Our baseline measures use a 30 day window for inputs and a 1 year window for output (survival days). We explored the robustness of our results to shorter and longer time horizons – 7 days and 1 year on the input side, and 30 days and 5 years on the output side. Again, there are tradeoffs in the length of time horizon.[26] We find our results are generally robust to these alternative input

[25] Estimation in more traditional settings must also deal with input measurement problems, including issues we do not confront here stemming from differential qualities across types of workers and capital, trying to capture the flow of capital services using measures of capital stocks, and intermediate inputs typically measured by expenditures rather than quantities. Additionally, and more directly to the issue here, these inputs must also be aggregated to a single-dimensional input index by weighting the individual inputs appropriately. The theoretically correct weights are the elasticities of output with respect to the respective inputs. Estimating these elasticities involves its own set of measurement challenges. Our approach in the hospital sector avoids many of these additional issues.

[26] On the input side, a shorter time horizon will miss some of the resources the patient receives, while a longer horizon creates greater scope for survival bias as well as treatments that are linked to providers other than the original hospital. On the output side, for our baseline measure we chose the relatively standard 1-year horizon since it seemed substantively more of interest than shorter-term (e.g. 30 day) survival. Analysis of a shorter horizon might capture aspects of hospital productivity that reflect only a slight postponement in death, and might not capture

and output horizon windows (Table A10).

6.4 Potential Endogeneity of Inputs

Finally, as noted earlier, a pervasive concern in the productivity literature is the potential endogeneity of inputs to producer productivity. This can bias the estimates of the returns to scale parameter μ. There is a wide range of estimates of this parameter in the literature (see e.g. Cutler et al. 1998, Fisher et al. 2003b, and Baicker and Chandra 2004) and uncertainty as to the "right" estimate. We are therefore reassured that our results are quite robust to imposing (rather than estimating) a range of "reasonable" values of μ and then calculating productivity under different imposed values (see table A11). The lack of sensitivity of our static and dynamic allocation results to alternative values of μ is consistent with the results in Figures 5a and 5b that the correlation between market share and estimated productivity is driven primarily by the correlation between market share and risk-adjusted survival.[27]

7. Conclusion

This paper has examined the relationship between productivity and market allocation in healthcare, specifically for hospital treatment of Medicare patients' heart attacks. We have done so by drawing on the insights of several decades of theoretical and empirical work in productivity more broadly. Qualitatively, we find that higher productivity hospitals have greater market share at a point in time and grow more over time. Quantitatively, a hospital with a one

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aspects that affect outcomes through long-term mechanisms such as the management of complications due to co-morbidities and the quality of the hospital's follow-up care. On the other hand, with a longer output horizon there is greater scope for the impact of non-hospital factors – such as patient compliance in terms of diet, smoking and medication, and the impact of doctor quality regardless of whether the doctor was associated with the initial hospital – on our productivity estimates.

[27] Referring back to the basic estimating equation for hospital productivity (equation (5)), the fact that the market share-productivity covariance is not sensitive to μ must mean that there is little variance in risk-adjusted inputs and/or a low covariance between risk-adjusted inputs and market share – otherwise, changes in the value of μ, which ties risk-adjusted input variation to our estimate of hospital's productivity levels, would change the correlation between estimated productivity and market share.

standard deviation higher productivity has about 40 percent higher market share at a point in time, and grows about 6 percent more over the next five years.

These relationships, which are driven primarily by the relationship between risk-adjusted survival and market share, mean that over time the healthcare market evolves in a manner favorable to higher productivity producers. This qualitative pattern is generally viewed by the broader productivity literature as an empirical sign of the workings of competition; it has been consistently found within manufacturing industries in the United States but not in less competitive settings such as post-Soviet Eastern block countries or Chile prior to trade reforms. Our more speculative quantitative comparisons between healthcare and manufacturing industries in the US suggest that, if anything, these re-allocation results are stronger, and dispersion similar or smaller, in healthcare.

Taken together, our qualitative and quantitative findings indicate that the healthcare sector may not be as idiosyncratic as the conventional wisdom has claimed. In this sense, our results are in the same spirit as Skinner and Staiger's (2007) finding of a common "innovativeness factor" across healthcare and other sectors within a geographic area; they found that areas of the country that were early adopters of hybrid corn in the 1930s and 1940s were also early adopters of beta blockers for heart attacks at the beginning of the current century.

Such findings suggest that, going forward, research on the determinants of productivity in the health care sector may benefit from more attention to the insights, both theoretical and empirical, from research about productivity and allocation in other industries. By the same token, insights from the health care sector may likewise be a useful laboratory for thinking about other industries. A recent series of papers by Bloom, Van Reenen and co-authors have begun to do just this, empirically investigating the role of such factors as management style and labor quality on hospital performance (usually survival rates; see Bloom et al., 2010; Propper and Van Reenen, 2010; Bloom et al., 2012; and McConnell et al., 2013). Moreover, in our healthcare setting as in the manufacturing setting more broadly, the estimated re-allocation relationships stop far short of indicating what economic or policy forces could be unleashed to create still greater reallocation to higher productivity producers. We see a great opportunity for further work that tries to estimate the causal impact of competition – or other factors – on allocation in healthcare and in manufacturing settings.

Of course, a given amount of re-allocation to higher productivity producers – or a given improvement in this re-allocation process – may be much more valuable in healthcare than in manufacturing, not to mention of greater consequence for public sector budgets. In this case, more than healthcare having different market dynamics, perhaps it is this feature of healthcare that makes it exceptional.

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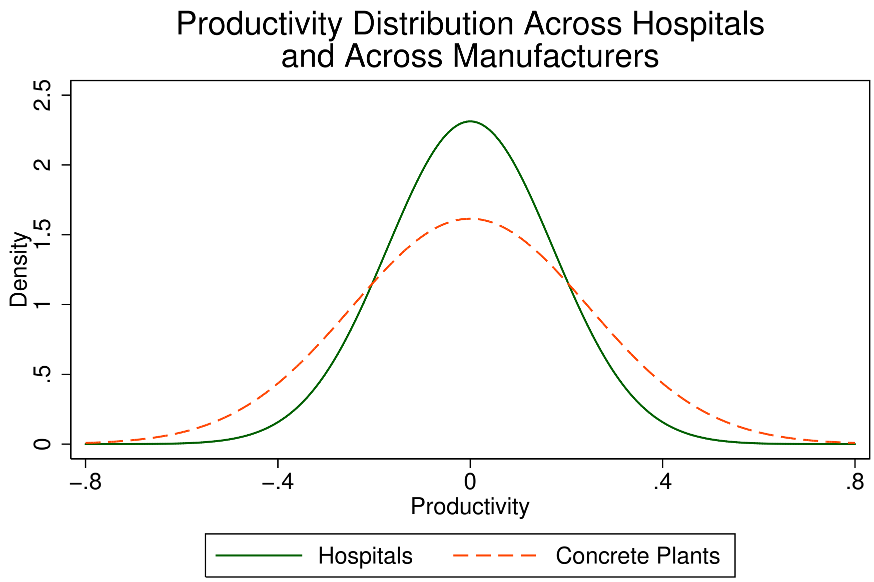


Figure shows estimated productivity dispersion across hospitals for heart attack treatments and across concrete plants for the production of ready−mixed concrete. We show the average within−year fitted normal density for each. Hospital productivity estimates (which reflect the hospital's ability to produce patient survival given a fixed set of inputs), are from our baseline specification (Table 2, column 1); concrete productivity estimates are from Table A7. See text for more details on the construction of these estimates.

Figure 1

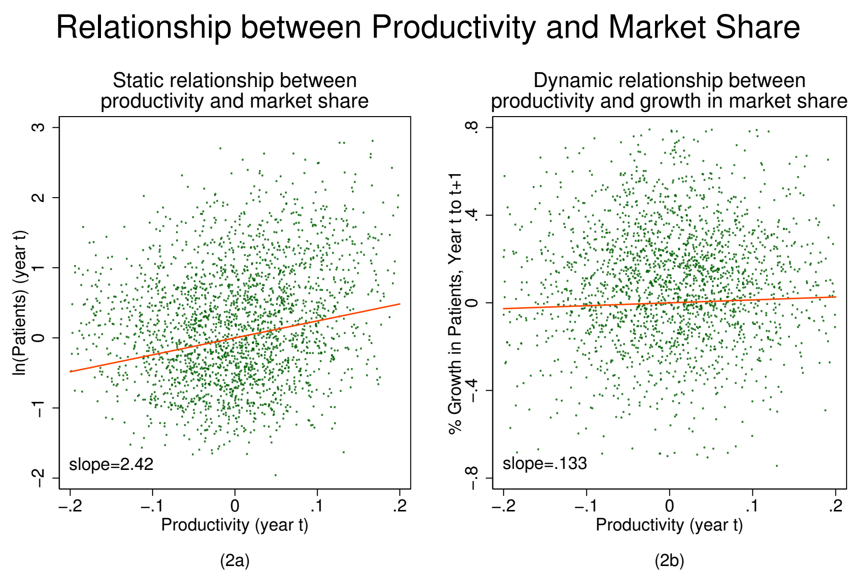
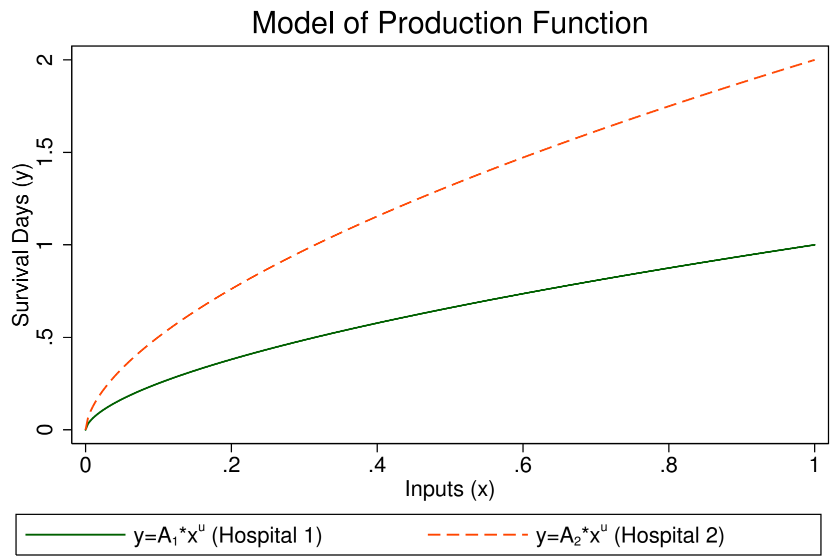


Figure shows relationship between a hospital−year's market share and productivity after partialing out market−year fixed effects. Figure 2a shows the static relationship between the hospital's log number of heart attack patients in year t and estimated productivity in year t; Figure 2b shows the dynamic relationship between the hospital's percent growth in heart attack patients between year t and t+1 (defined in equation 6) and estimated productivity in year t. Hospital productivity estimates (which reflect the a hospital's ability to produce patient survival given a fixed set of inputs) are from our baseline specification (Table 2, column 1). Figures show results for a random 5% of hospital−years, with hospital−years that have less than 11 patients suppressed form the scatter for confidentiality reasons. In addition, in Figure 2b for visual clarity the y−axis is restricted to the almost 95% of hospital−years with residual growth between −0.8 and 0.8. In both graphs, line shows the linear fit based on the whole sample (prior to any suppression).

Figure 2



Hospital 2 has higher productivity than hospital 1.

Figure 3

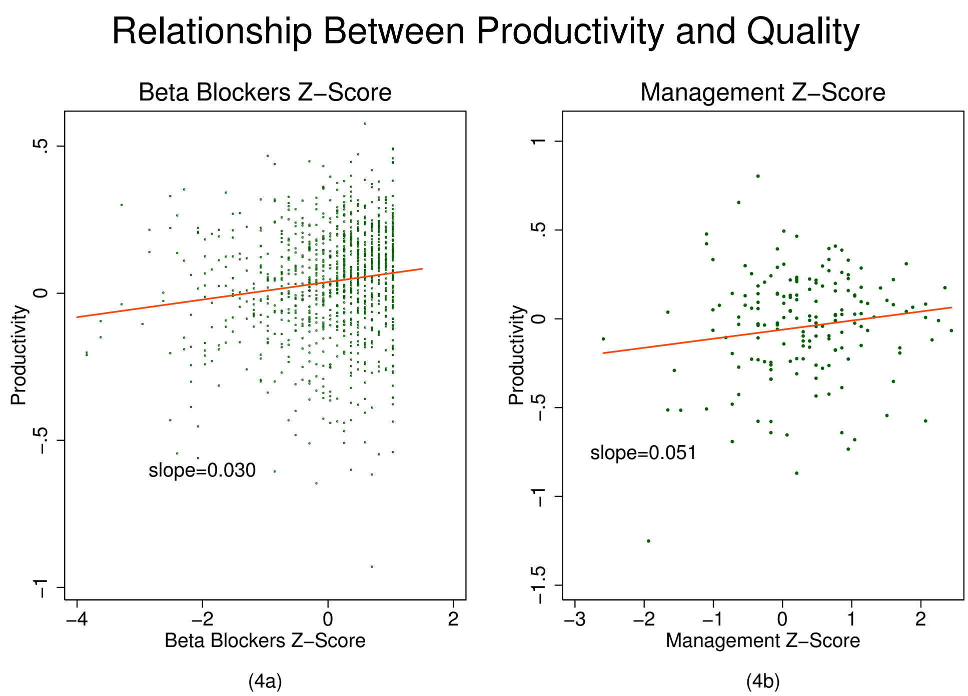


Figure plots the relationship between our estimate of 2003 hospital–year TFP (from our baseline specification in Table 2, column 1, but without the empirical Bayes adjustment) against specific observable measures of hospital quality. Left hand panel plots relationship between the hospital's TFP and its beta–blockers z–score in 2003 for the 1,045 hospitals where we observe both (6 hospitals with outlying z–scores are not shown). Right hand panel shows the relationship between the hospital's 2003 TFP and its management z–score for the 179 hospitals where we observe both. See text for more detail on both of these z–scores. Hospitals that have less than 11 patients in 2003 are suppressed from the scatter for confidentiality reasons. Line shows the linear fit based on the whole sample (prior to any suppression and removal of outliers).

Figure 4

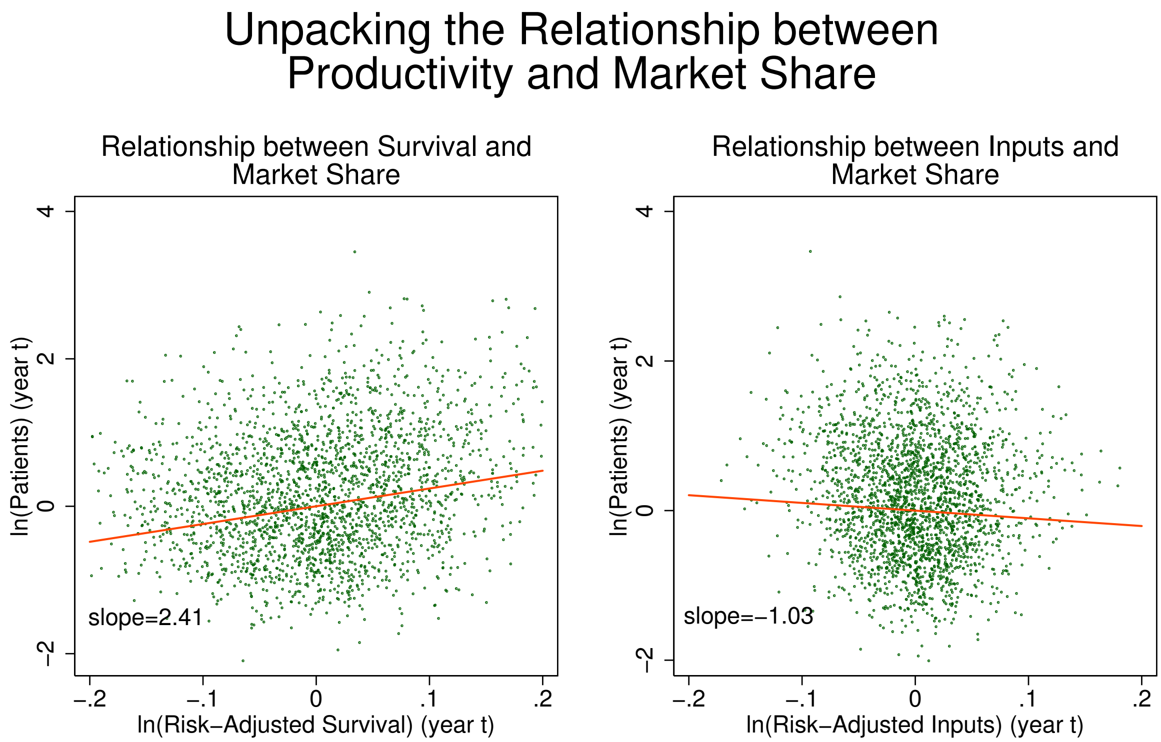


Figure shows relationship between a hospital−year's market share and risk−adjusted survival after partialing out market−year fixed effects and risk−adjusted inputs. Y−axis is the log number of heart attack patients in year t; x−axis is the hospital's risk−adjusted average log−survival in year t. Baseline risk adjusters (shown in Table 1b) are used. Figure shows results for a random 5% of hospital−years, with hospital−years that have less than 11 patients suppressed from the scatter for confidentiality reasons. For visual clarity, the x−axis is restricted to the 97% of hospital−years with residual survival between −0.2 and 0.2. Line shows the linear fit based on the whole sample (prior to any suppression or restriction).

(5a)

Figure shows relationship between a hospital−year's market share and risk−adjusted inputs after partialing out market−year fixed effects and risk−adjusted survival. Y−axis is the log number of heart attack patients in year t; x−axis is the hospital's risk−adjusted average log−input in year t. Baseline risk adjusters (shown in Table 1b) are used. Figure shows results for a random 5% of hospital−years, with hospital−years that have less than 11 patients suppressed from the scatter for confidentiality reasons. For visual clarity, the x−axis is restricted to the 99.9% of hospital−years with residual inputs between −0.2 and 0.2. Line shows the linear fit based on the whole sample (prior to any suppression or restriction).

(5b)

Figure 5

Table 1a - Hospital and market statistics

	(1) Mean	(2) SD	(3) Min	(4) Max
Hospital-Years (N=55,540)				
Patients	63.57	69.63	5	917
Market-Years (N=4,560)				
Patients	774.2	735.2	63	5,700
Hospitals	12.18	11.38	1	97

Note: The number of hospitals is 5,346.

Table 1b - Patient Summary Statistics

	(1) Mean	(2) SD
Outputs		
Survival (days; censored at 365)	268.1	149.4
Binary: Survival > 365 Days	0.660	0.474
Inputs		
Baseline (30 day) input measure ($)	15,996	12,172
Risk Adjusters		
Age	78.17	7.546
Female	0.507	0.500
White	0.906	0.291
Hypertension	0.207	0.405
Stroke	0.0232	0.150
Cerebovascular Disease	0.0398	0.195
Renal Failure	0.0521	0.222
Dialysis	0.00670	0.0816
COPD	0.0981	0.297
Pneumonia	0.0592	0.236
Diabetes	0.128	0.334
Protein Cal Malnut	0.0118	0.108
Dementia	0.0412	0.199
Paralysis/FD	0.0256	0.158
Periph Vasc Disease	0.0639	0.245
Metastatic Cancer	0.0117	0.107
Trauma	0.0392	0.194
Substance Abuse	0.0225	0.148
Major Psych Disorder	0.0138	0.117
Chronic Liver Disease	0.00281	0.0529

Note: The number of observations is 3,530,401.

Table 2 - Production Function Parameter Estimates

	(1)	(2)	(3)
Risk Adjustment:	Baseline	Age/Race/Sex	None
Parameter			
μ	0.446	0.481	0.589
	(0.00511)	(0.00523)	(0.00552)

Notes: N = 3,530,401 patients, 55,540 hospital-years, and 5,346 hospitals. Standard errors are bootstrapped with 300 replications and are clustered at the market level (304 markets). "Baseline" risk-adjustment includes a full set of interactions between age (in five year groupings), gender and whether the patient is white; it also includes indicators for the various co-morbidities shown in Table 1; column 2 excludes the co-morbidities and column 3 has no risk adjusters.

Table 3 - Relationship Between Hospital TFP and Hospital Covariates

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(Prod)	ln(Prod)	ln(Prod)	ln(Prod)	ln(Prod)	ln(Prod)
Beta-Blockers Z-Score	0.0299					
	(0.00667)					
Composite Z-Score		0.0299				
		(0.00676)				
Management Z-Score			0.0511			
			(0.0290)			
Teaching Hospital				0.0799		
				(0.0129)		
Urban					0.0696	
					(0.0160)	
For-Profit						0.0228
						(0.0266)
Non-Profit						0.101
						(0.0221)
Constant	0.0382	-0.0147	-0.0609	-0.0810	-0.102	-0.127
	(0.00577)	(0.00590)	(0.0248)	(0.00890)	(0.0144)	(0.0202)
Observations	1,045	2,183	179	3,361	3,361	3,363

Unit of observation is a hospital. Dependent variable is our estimate of 2003 hospital-year TFP from our baseline specification (Table 2, column 1) without empirical Bayes adjustment. Right hand side variables in columns 1 through 3 are z-scores for hospitals that reported the measure indicated. Data on beta-blockers and composite scores are from CMS Hospital Compare; beta-blockers score includes hospitals with at least 30 patients appropriate for the treatment, while composite score includes hospitals with a sum of at least 30 patients appropriate for each of the treatments within the score. Data on management score are based on a 2010 survey of management practices adminstered by Bloom et al. (2012); see text for more details. Right hand side variables in columns 4 through 6 are indicators for whether the hospital is a teaching hospital (Column 4), in an urban area (Column 5), or is a for-profit or non-profit entity (Column 6, public is the omitted category). Indicators for hospital characteristcs are coded from CMS Provider of Services and Impact files; we define a teaching hospital as one that has residents. Standard errors are bootstrapped with 300 replications and are clustered at the market level.

Table 4 - Main Results - Allocation Metrics

	(1)	(2)	(3)	(A)	(B)
Risk Adjustment:	All	Age/Race/Sex	None	DV Mean[a]	Observations
Static Allocation	2.418	2.496	2.618	3.641	55,540
	(0.0889)	(0.0851)	(0.0779)		
Dynamic Allocation					
Exit Regression	-0.0329	-0.0353	-0.0458	0.0438	40,379
	(0.00935)	(0.00884)	(0.00766)		
Growth Regression	0.133	0.154	0.201	-0.126	52,777
	(0.0225)	(0.0214)	(0.0184)		

Notes: "Static Allocation" reports the results from estimating the relationship between a hospital's log(patients) and TFP (i.e. productivity) within a market year given by equation (1). "Exit regression" reports the results from estimating the within-market relationship between a hospital "exit" as defined in the text and last year's productivity as given by equation (2). "Growth regression" reports the results from estimating the within-market relationship between a hospital's one-year percent growth and its base year productivity as defined in equation (3). Productivity is estimated based on the corresponding specifications from Table 2. Standard errors are bootstrapped with 300 replications and are clustered at the market level. Observations are hospital-years.

[a]"DV mean" reports the mean of the dependent variable for the regressions, which is ln(Patients) for the static allocation regression, 5-year exit for the exit regression, and 1-year growth for the growth regression. See text for more detailed definitions of dependent variables.

Table 5 - Dynamic Allocation Varying Time Horizons

	Growth from *t* to *t+k*				Exit in *t+k*			
Years (*k*)	Coeff	Std Err	Mean DV[a]	Obs	Coeff	Std Err	Mean DV[a]	Obs
1	0.133	(0.022)	-0.126	52,777	-0.033	(0.009)	0.044	40,379
2	0.207	(0.027)	-0.224	49,954	-0.056	(0.014)	0.077	36,864
3	0.270	(0.038)	-0.314	46,961	-0.085	(0.019)	0.108	33,163
4	0.345	(0.047)	-0.392	43,742	-0.122	(0.023)	0.137	29,338
5	0.365	(0.052)	-0.462	40,379	-0.147	(0.028)	0.166	25,359
6	0.397	(0.062)	-0.530	36,864	-0.165	(0.030)	0.195	21,320
7	0.477	(0.068)	-0.598	33,163	-0.203	(0.037)	0.226	17,226
8	0.526	(0.070)	-0.666	29,338	-0.224	(0.040)	0.255	13,050
9	0.573	(0.074)	-0.735	25,359	-0.242	(0.049)	0.284	8,761
10	0.587	(0.077)	-0.807	21,320	-0.212	(0.060)	0.313	4,412

These results report the coefficient and its standard error from the regressions of growth or exit on productivity, controlling for market-year fixed effects. These are modified versions of equations (2) and (3) where the time horizon over which growth or exit is measured is now *k* years rather than 1 year. Each row considers a different time horizon *k*. Longer horizons have smaller samples because data on growth ends in 2007 and data on exit ends in 2003. Standard errors are bootstrapped with 300 replications and are clustered at the market level.

[a] "Mean DV" refers to the mean of the dependent variable (growth or exit) in the sample over the time horizon indicated.

Appendix A: Analytical Framework

As mentioned in the text, models of reallocation mechanisms among heterogeneous-productivity producers have found applications in a number of fields, including industrial organization, trade, and macro-economics. While these models differ considerably in their specifics, they share an archetypal mechanism that connects the extent of competition in the market to the shape of the productivity distribution among market producers. We describe this central mechanism here.

Producers (indexed by i) earn profits which depend positively on their idiosyncratic productivity levels A_i – more productive firms earn higher profits due to their lower costs – and negatively on the number (or mass, in models with a continuum of firms) of producers in the industry N.[28] Hence $\pi_i = \pi(A_i, N)$, with $\partial\pi/\partial A_i > 0$ and $\partial\pi/\partial N < 0$. The monotonic relationship between productivity and profits implies that, for any given N, there is a critical cutoff productivity level $A^*(N)$ at which firm profits are zero. Only producers with productivity levels at or above $A^*(N)$ will operate in equilibrium.

The zero-profit cutoff productivity $A^*(N)$ is endogenously determined by a free entry condition, where ex-ante identical potential entrants consider whether to pay a sunk cost σ to take an idiosyncratic productivity draw from a known distribution, $G(\cdot)$ with upper bound $\bar{A}$. The expected value of entry, which equals zero by the free entry condition, is:

$$V^e = \int_{A^*(N)}^{\bar{A}} \pi(A, N) g(A) dA - \sigma = 0$$

The expected profits from entry depend upon the equilibrium number of entrants N in two ways. First, an increase in N shifts upward the zero-profit cutoff productivity level $A^*(N)$, reducing the probability that the entrant's productivity draw is high enough to earn nonnegative profits and thus making successful entry less likely. Second, a higher number of firms N also reduces the producer's profits if it does enter. Thus expected profits fall monotonically in N. In equilibrium, the number of firms choosing to pay the entry cost yields a number of entrants N that, through these two effects, exactly equates the expected profit from taking a productivity draw to the sunk entry cost.

The endogeneity of $A^*(N)$ means the industry productivity distribution observed in the data is determined in equilibrium. Specifically, it is a truncation of $G(\cdot)$, the underlying productivity distribution from which potential entrants take productivity draws, where the truncation point is $A^*(N)$. Changes in market primitives that shift the equilibrium location of $A^*(N)$ therefore shift the observed productivity distribution as well.

[28] Standard presentations of these models consider profit-maximizing firms. Although we keep this terminology to be more familiar relative to the existing literature, we note that in the context of hospitals, it might be more appropriate to consider firms as earning (and maximizing) "surplus" rather than "profits". This more general terminology recognizes that many hospitals are legally structured as nonprofits and does not affect the qualitative comparative statics. Nonprofit hospitals are often modeled in the literature as having an objective function that is a convex combination of profits and other objectives; therefore on the margin they should respond qualitatively the same way as for-profit hospitals to factors like competition. Moreover, even if a hospital's objective is not profit maximization, it is likely that for any given level of output(s) the hospital produces (in order to meet whatever outcomes are in its objective function), surplus will be larger if the hospital's costs are lower. In practice, a large empirical literature finds essentially no evidence of differential behavior across for-profit and non-profit hospitals, calling into question whether the non-profit label has any substantive meaning for behavioral responses (see Sloan 2000 for a recent review of this literature).

The primitive that we are interested in here is the extent of competition, as reflected in how easily consumers can (or how willing consumers are to) substitute to alternate producers. The specific mechanism through which primitives map into substitutability may vary, from changes in the differentiation of firms' products, to shifts in openness to trade, to movements in the size of transport costs. The particulars of the mechanism aren't important here; what matters are the effects on the equilibrium.

Higher substitutability has three effects that can be examined empirically. First, it makes it more difficult for higher-cost (lower-productivity) firms to earn positive profits, as demand is now more responsive to their cost and price differential relative to other firms in the industry.[29] In turn, the zero-profit cutoff productivity level $A^*(N)$ rises: the threshold for operation is greater than before. This truncates the equilibrium productivity distribution, reducing observed *productivity dispersion*.[30] Second, higher substitutability means that, among operating firms, market shares are more sensitive to productivity differences. Purchases are reallocated to more productive firms, raising the correlation between productivity and market share at a point in time ("*static allocation")*. Third, over time more productive firms are likely to grow in market share (*"dynamic allocation")*.[31]

[29] In the case of hospitals, this demand response can be manifested either directly in patients' choices in response to out-of-pocket costs, or indirectly through insurers' decisions to include the hospital in its covered network.

[30] This dispersion implication requires some additional regularity assumptions on the underlying productivity distribution. Most "standard" distributions exhibit declining second moments as they are truncated from below. The exponential distribution, however, is an example of one that does not. Nevertheless, if we assume the productivity distribution is bounded at the top (i.e., there is some maximum productivity level), as we do here, then all distributions will eventually exhibit decreased dispersion as they are truncated from below.

[31] The model just described is static, so the effects of changes in competition on equilibrium should be thought of as comparing two different markets or the same market across different long-run steady states. However, several of the models in the literature are explicitly dynamic and have similar predictions about the effect of competition on the productivity of entrants and growth of incumbents (e.g., Hopenhayn 1992, Asplund and Nocke 2006).

Appendix B: Measuring Inputs

Our baseline input measure (as well as many of the alternative measures discussed below) is derived from the formulas used to determine Medicare's Hospital (Part A) reimbursement. Some alternative measures also use information derived from the formulas used to determine Medicare's reimbursement of physicians and outpatient facilities (Part B). It is therefore useful to begin with a very brief overview of the key features of Medicare hospital reimbursement needed to understand the construction and composition of our baseline and alternative input measures. Considerably more detail can be found in CMS (2011).

The amount Medicare reimburses a hospital is determined by the patient's Diagnosis Related Group (DRG), national factors, and hospital-specific factors. A patient's DRG is a function of his principal diagnosis, procedures performed, and secondary complications and comorbidities. Some DRGs also depend on whether the patient died in the hospital.

Each DRG is assigned a (national) weight based on how much it costs to treat the nationwide average patient with that DRG; a national conversion factor is used to convert these DRG weights into dollar payments. The weights and the conversion factor are updated annually. The national rate is then adjusted for hospital-specific considerations. The major adjustments are due to geographic factors (e.g. the local wage rate) and characteristics of the hospital (such as whether it operates a resident training program or has a disproportionate share of patients on Medicare or SSI).

For most stays the hospital will receive payments solely based on the patient's DRG. However, in certain extraordinarily costly cases hospitals receive additional "outlier payments" covering 80 percent of costs beyond a threshold level. To compute costs, the hospital's billed charges are deflated by a hospital-specific cost-to-charge ratio. If a patient has a short stay and is transferred to another hospital, Medicare reduces payments to the transferring hospital but pays the receiving hospital as it would for a standard inpatient stay. For our purposes, we assign all inputs for the patient in the time horizon (30 days for our baseline measure) back to the initial hospital.

B1. Baseline Input Measure: Part A "Resources"

Our baseline input measure follows the approach of Gottlieb et al. (2010) and Skinner and Staiger (2009) to purge the "price" variation in the reimbursement formula from the "input" variation. Specifically, our starting point is the DRG weight (multiplied by a national conversion factor to convert it to a dollar metric) plus outlier payments (also in dollars). It does not reflect any variation in reimbursement prices across hospitals due to geographic factors or specific characteristics of the hospital.

According to this measure, the inputs a patient receives equal the sum of his converted DRG weights and outlier payments at all hospital stays in the 30 days following his AMI. Variation across patients in the input measure therefore comes from 3 sources: variation in the patient's DRG(s); whether there are (and the extent of) outlier payments; and the number of hospital stays during the 30 day window. We discuss each in turn.

Variation across Index Event DRGs

To give a sense of the nature and variation across DRGs, Table A1 lists the top 20 DRGs for the index event (initial AMI hospital stay), their patient share and their weights in 2000.[32] The top five DRGs account for over 90 percent of the index events, and the top 20 account for virtually 100 percent.

Looking within the top five we see substantial differences in weight based on whether an invasive procedure is performed. There are two separate DRGs for invasive procedures (#107, "Coronary Bypass with Cardiac Catheterization" and #116, "Other Permanent Cardiac Pacemaker Implant or PTCA with Coronary Artery Stent Implant") and they respectively have weights of 5.46 and 2.47. By contrast, the

[32] For presentation purposes, we limit Table A1 to one year because DRG weights and classifications change slightly from year to year.

other three DRGs in the top five are medical DRGs (i.e. do not involve invasive procedures) and have weights ranging from 1.11 to 1.51. For the year 2000, two dummies for these two surgical DRGs (bypass and stent) explain 15 percent of the total variation in our 30 day input measure.

Within the three most common medical DRGs, we see that there is variation for a medically treated AMI based on whether or not the patient died (#123), survived following a stay with major complications (#121) or survived following a stay without major complications (#122). This variation has, to our knowledge, not previously been noted by the large empirical literature on the relationship between inputs for heart attacks and subsequent survival which has used the variation in inputs stemming from survival. However, this source of variation in the standard input measure seems suspect: it partly causes in-hospital death – not inputs, per se – to explain survival, an association that must exist trivially.

Therefore, for these three DRGs that refer to the same diagnosis but differ on the basis of patient survival, we eliminate the variation in inputs across DRGs within this group at the hospital-year level. We assign each DRG the patient-weighted average of the different DRG weights. The averaging weights are equal to the share of patients in the DRG in that year. Almost three-quarters of hospital stays were grouped into DRGs that were affected by this fix.[33]

Variation from Outlier Payments

Approximately 8.2% of our patients trigger outlier payments due to unusually costly cases. These payments are triggered when a hospital's cost of treating a patient exceeds a national threshold. Conditional on receiving an outlier payment, the average outlier payment as a share of DRG reimbursement without outlier payments is 53.9; the standard deviation of outlier payments is 13,154.8. (All statistics calculated for patients in the year 2000.)

Variation Due to Number of Hospital Stays

Even ignoring outlier payments, the total variation coming from DRGs is in fact larger than that indicated in Table A1 because of the possibility of multiple (and potentially non AMI) hospital stays in the 30 days following the index event (AMI). Our baseline input measure is constructed for the 30 days following the initial AMI, meaning that it includes all hospital stays in these 30 days. On average, an AMI patient has 1.07 stays in this window. Conditional on having multiple stays, the average patient visits the hospital 2.07 times in the month following the AMI.

If a hospital stay straddles the end of the time window (e.g. a patient stays in the hospital for 10 days and is admitted on day 25 days following the heart attack), the inputs attributed to that hospital are reduced; in particular, we multiply our input measure by the share of days in the hospital that were inside

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[33] Note that this "fix" also purges the variation across the three most common medical DRGs in whether the patient had a major complication or not. Although the case in question is the only one where different DRGs are assigned based on patient survival, there are other cases where separate DRGs are assigned based on the presence of complicating conditions (CCs). For example, the 6th-ranked DRG #110, "Major Cardiovascular Procedures with CC" (weight 4.16) and the 18th-ranked DRG #111, "Major Cardiovascular Procedures without CC" (weight 2.23) differ only on this basis. It is a priori unclear to us whether we want to purge variation due to the presence of CCs. On the one hand, conditional on a rich set of patient risk adjusters, the presence of a CC may be a useful measure of the intensity of resources required to treat the condition; on the other hand, with imperfect risk adjusters, it may also capture correlates of mortality (our outcome of interest).

As noted, in practice our approach to purging mortality-based variation across DRGs also purges complications-based variation in the most common DRGs. We experimented with an alternative measure that purged variation due to CCs in all DRGs. The procedure took DRGs that were identical but for the CC requirement and assigned them the same DRG weight within each hospital-year. This DRG weight was a weighted average of the component DRG weights; the averaging weights were the shares of patients in each DRG in the hospital-year. For example, in 2000, DRGS #110 and #111 were assigned the same weight in each hospital-year. This correction affected only a few percent more patients and made no noticeable difference to our findings (results available on request).

the 30 day analysis window. We adjusted all DRGs (not just those associated with index events) to purge variation stemming from mortality in the manner described above.

Table A2 lists the top 20 DRGs across all stays in the 30 day window following the index event. The index events are included in this table. As expected, there is more variation across these DRGs.

Empirical Variation in Baseline Input Measure

The panels of Figure A1 show the variation in the input measures across patients for one year (2000). Figure A1a shows the variation in the DRG index events (using our "collapsed" DRG measure that purges mortality variation). Figure A1b shows the variation from the DRG index events plus outlier payments in the index event. Figure A1c shows the total 30 day variation (our baseline input measure), which adds in additional hospital stays (their DRGs and outlier payments) within the 30 days. As would be expected, the input distribution gets less "lumpy'' at each step.

B2. Alternative Input Measures

We confronted a number of choices in defining our baseline input measure. We therefore constructed several other alternative input measures. This section describes them.

Alternative Measures of Hospital Inputs

A central tension in our choice of input measurement is how coarse or detailed we make our input measure. The tradeoff is between the survival bias that can occur with finer input measures—since the longer a patient survives, the more can be done to a patient—and the measurement error which occurs at coarser definitions of inputs. Our baseline measure, following standard practice, is aggregated to a relatively high level, and may therefore measure inputs with a non-trivial amount of error.

We experimented with two alternative hospital-based input measures. One measures Part A spending rather than Part A inputs; it therefore includes variation in reimbursement rates stemming from hospital specific factors like geographic location or type of hospital. As shown in Figure A1d the distribution of Part A reimbursement is less "lumpy" than our baseline input measure; the correlation between the two is 0.90.

The other measure is designed to be more detailed than our baseline measure to reflect that fact that input use may vary substantially within the relatively coarse DRGs. We used data on the length of hospital stay and the procedures performed during the stay (up to six may be listed). Procedure codes are themselves available at different levels of granularity; there are 3 levels of CCS procedure codes ranging from the least granular level 1 to the most granular level 3; the much larger set of ICD-9 procedure codes is more granular still. The ICD-9 codes account for over 3878 possible procedures that may be performed on patients.

To reduce the dimensionality of the set of procedures, we use the following algorithm. We start with the coarsest set of procedures (level 1 CCS codes, of which there are 16) and move iteratively to the finest set of procedure codes (ICD-9). At each step we aggregate codes that are rare and disaggregate codes that are very common. Thus, beginning with CCS level 1 codes, we include indicators for level 1 procedures that were performed on less than 10% of patients; if the level 1 procedure was performed on 10% or more of patients, we disaggregate it by looking at CCS level 2 components.

In similar fashion, if the CCS level 2 procedures were performed on 1-10 percent of patients, we include an indicator for it. Within a level 1 code, all level 2 codes performed on less than 1 percent of patients are grouped together and included as one indicator. If the level 2 procedure was performed on 10 percent or more of patients, we disaggregate by looking at its level 3 components.

We follow the same process for level 3 components; when we disaggregate these codes we look at the component ICD-9 codes. If the ICD-9 code was performed on at least 1 percent of patients we include an indicator for it. Within a level 3 code, all ICD-9 codes that were performed on less than 1 percent of patients are grouped together and included as one indicator.

This algorithm results in 60 procedure indicators: 18 for ICD-9 codes, 6 for level 3 CCS codes, 22 for level 2 CCS codes and 14 for level 1 CCS codes.

Incorporating Non-Hospital Inputs

A limitation of our input measures thus far is that, following standard practice in the heart attack literature, they reflect only inpatient hospital inputs. Notably, they do not include physician inputs, which may occur in an inpatient or outpatient setting. They also do not include outpatient tests and procedures like MRIs.

Many of these inputs are directly related to the treatment of the AMI. For example, the work of physicians who treat the patient surgically or medically in the hospital is obviously an input that may bear on the patient's survival. Likewise, an MRI done in an outpatient facility that is closely affiliated with the hospital will inform treatment decisions and influence mortality.

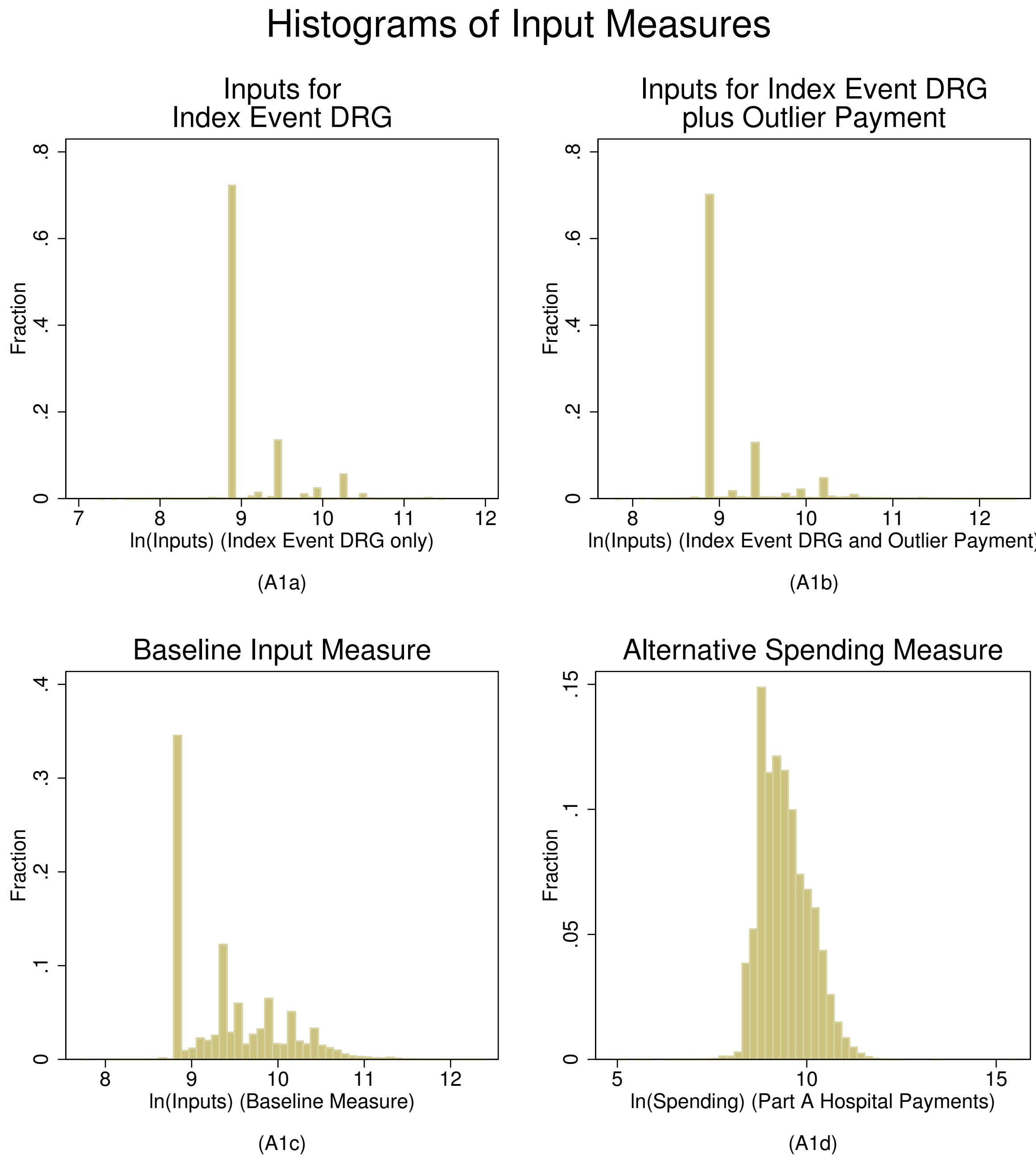
There are two reasons why we follow most of the literature on heart attacks and do not include inputs by physicians or outpatient facilities in our baseline measure. First, while some of these inputs are closely linked to the care received in the hospital, many of the payments reflect care that is independent of the hospital. In particular, doctor visits and outpatient diagnostic tests at long time horizons from the initial AMI admission may be less dependent on initial treatment decisions. The second reason is practical: data on much of these other input measures are only available for 20 percent of the sample and only since mid-2000, reducing the set of hospital-years in which we can observe at least 5 AMI patients by 70.0%.

Still, we sought to evaluate the sensitivity of our results to including physician and outpatient services. Medicare reimburses physicians based on their assessment of the "Relative Value Units" (RVUs) of the services the physician provided; the RVU of a service is intended to reflect the resources required to provide that service. The RVUs attributed to procedures are constant across geographic areas and practitioners, although Medicare makes further adjustments based on geography and provider type to derive reimbursement rates (see MedPAC [2010a] or Clemens and Gottlieb [2012] for more details). We construct our measure of physician inputs by summing all RVUs associated with the patient in the 30 days following his initial hospital admission. We multiply the RVUs by a national conversion factor to convert them to a dollar metric; the national conversion factor eliminates variation due to Medicare's geographic price adjustments.

Calculating outpatient contributions to the production function is significantly more complicated than calculating physician or inpatient contributions. While physician services and inpatient stays are each reimbursed using a single payment system that is designed to reflect resource utilization, different outpatient services are covered by different types of systems (MedPAC [2010b] provides more details). Some outpatient services are covered prospectively – although the payment groups are so fine that treatment decisions may be reimbursed at the margin. Providers are paid for other services according to a fee schedule that is geographically adjusted. Some services are reimbursed according to local prices.

For the portion of outpatient services covered prospectively, there is a series of classification groups (Ambulatory Payment Classification groups or APCs) which function analogously to DRGs. Each APC is given a weight that is based on its expected resource costs; we translate these weights into a dollar basis using a national conversion factor that is an analogous to the procedure we use to convert DRG weights. For services that are reimbursed on a fee schedule, we mimic the method used for physician inputs by applying the fee schedule prior to geographic adjustments.

These adjustments eliminate much of the variation in outpatient prices that is region- or provider-specific. Still, some payments, like those for certain prescription drugs and new technologies, do not have an associated national fee schedule and are included unadjusted.



Top row shows the component of our baseline input measure that is attributable to the patient’s "index event", or initial hospitalization for the AMI. Bottom row shows the distribution of the baseline input measure and compares it to an alternative measure that captures actual payments to hospitals. Specifically, Figure A1a shows the component of the baseline measure due to the patient’s index event DRG weight. Figure A1b adds index event outlier payments. Figure A1c, our baseline input measure, adds inputs (due to DRGs and outlier payments) from subsequent hospital stays within 30 days of the index event. Figure A1d shows the Part A (hospital−based) spending measure, an alternative input measure which incorporates the same hospital stays as the baseline measure but adds in geographic and hospital−specific price adjustments to capture actual Medicare payments to providers. See Appendix B for more details. All measures are in logarithms and are for the year 2000 only.

Figure A1

Table A1 - List of Top DRGs for Index Events (Initial Hospital Stays for the AMI Episode) in 2000

Rank	Number	DRG Name[a]	Weight	Share	Cum. Share
1	121	Circulatory Disorders with AMI and Major Complications, Discharged Alive	1.63	41.2%	41.2%
2	122	Circulatory Disorders with AMI, without Major Complications, Discharged Alive	1.11	20.9%	62.1%
3	116	Other Permanent Cardiac Pacemaker Implant or PTCA with Coronary Artery Stent Implant	2.47	13.0%	75.1%
4	123	Circulatory Disorders with AMI, Expired	1.51	10.9%	86.0%
5	107	Coronary Bypass with Cardiac Catheterization	5.46	5.4%	91.4%
6	110	Major Cardiovascular Procedures with CC	4.16	2.0%	93.4%
7	112	Percutaneous Cardiovascular Procedures	1.92	1.6%	95.0%
8	115	Permanent Cardiac Pacemaker Implant with AMI, Heart Failure or Shock, or AICD Lead or Generator Procedure	3.47	1.0%	96.0%
9	104	Cardiac Valve and Other Major Cardiothoracic Procedure with Cardiac Catheterization	7.24	0.8%	96.8%
10	483	Tracheostomy except for Face, Mouth, and Neck Diagnoses	16.12	0.5%	97.3%
11	106	Coronary Bypass with PTCA	7.33	0.4%	97.7%
12	109	Coronary Bypass without PTCA or Cardiac Catheterization	4.04	0.4%	98.1%
13	144	Other Circulatory System Diagnoses with CC	1.15	0.3%	98.4%
14	478	Other Vascular Procedures with CC	2.35	0.3%	98.7%
15	468	Extensive OR Procedure Unrelated to Principal Diagnosis	3.64	0.3%	99.0%
16	120	Other Circulatory System OR Procedures	2.01	0.2%	99.2%
17	108	Other Cardiothoracic Procedures	5.77	0.2%	99.4%
18	111	Major Cardiovascular Procedures without CC	2.23	0.1%	99.5%
19	477	Non-Extensive OR Procedure Unrelated to Principal Diagnosis	1.77	0.1%	99.6%
20	145	Other Circulatory System Diagnoses without CC	0.65	0.1%	99.7%

Notes: "Rank" refers to the share of patients with the DRG; "Number" refers to CMS's assigned number for that DRG; "Weight" is a CMS-assigned value that is designed to be proportional to the average cost of treatment and is used to determine reimbursement - the weights are set by CMS so that the average Medicare patient across all conditions has a weight of 1.

[a]Abbreviations: CC - Complicating Conditions, OR - Operating Room, PTCA - Percutaneous Transluminal Coronary Angioplasty.

Table A2 - List of Top DRGs for All Claims

Rank	Number	DRG Name[a]	Weight	Share	Cum. Share
1	121	Circulatory Disorders with AMI and Major Complications, Discharged Alive	1.63	15.1%	15.1%
2	127	Heart Failure and Shock	1.01	8.4%	23.5%
3	116	Other Permanent Cardiac Pacemaker Implant or PTCA with Coronary Artery Stent Implant	2.47	8.0%	31.5%
4	122	Circulatory Disorders with AMI, without Major Complications, Discharged Alive	1.11	7.3%	38.8%
5	123	Circulatory Disorders with AMI, Expired	1.51	3.8%	42.6%
6	132	Atherosclerosis with CC	0.67	2.8%	45.4%
7	107	Coronary Bypass with Cardiac Catheterization	5.46	2.7%	48.1%
8	462	Rehabilitation	1.36	2.7%	50.8%
9	89	Simple Pneumonia and Pleurisy, Age > 17, with CC	1.09	2.5%	53.3%
10	14	Specific Cerebrovascular Disorders Except TIA	1.19	1.9%	55.2%
11	88	Chronic Obstructive Pulmonary Disease	0.94	1.8%	57.0%
12	144	Other Circulatory System Diagnoses with CC	1.15	1.5%	58.5%
13	174	Gastrointestinal Hemorrhage with CC	1.00	1.2%	59.7%
14	112	Percutaneous Cardiovascular Procedures	1.92	1.2%	60.9%
15	124	Circulatory Disorders Except AMI, with Cardiac Cath and Complex Diagnosis	1.40	1.2%	62.1%
16	138	Cardiac Arrhythmia and Conduction Disorders with CC	0.82	1.2%	63.3%
17	143	Chest Pain	0.53	1.2%	64.5%
18	296	Nutritional and Miscelaneous Metabolic Disorders, Age > 17, with CC	0.86	1.2%	65.7%
19	109	Coronary Bypass without PTCA or Cardiac Catheterization	4.04	1.1%	66.8%
20	182	Esophagitis, Gastroenteritis, and Miscelaneous Digestive Disorders, Age > 17, with CC	0.78	1.1%	67.9%

Notes: "Rank" refers to the share of patients with the DRG; "Number" refers to CMS's assigned number for that DRG; "Weight" is a CMS-assigned value that is designed to be proportional to the average cost of treatment and is used to determine reimbursement - the weights are set by CMS so that the average Medicare patient across all conditions has a weight of 1.

[a]Abbreviations: CC - Complicating Conditions, OR - Operating Room, PTCA - Percutaneous Transluminal Coronary Angioplasty, TIA - Transient Ischemic Attack.

Appendix C: Empirical Bayes Adjustment

Introduction

In this appendix we describe the empirical Bayes (EB) procedure we use to adjust our estimates of hospital productivity for measurement error. This procedure is based on Morris (1983). For another example see Jacob and Lefgren (2007).

The exponentiated productivity of hospital h at time t is A_{ht} and its productivity is $a_{ht} = \ln(A_{ht})$. These objects are the "true" productivities and their distribution is the "underlying" distribution of productivity. We denote by $\hat{a}_{ht}$ the estimate of productivity; it equals productivity plus an error term η_{ht}:

$$\hat{a}_{ht} = a_{ht} + \eta_{ht}$$

The goal of the EB procedure is to adjust the estimates of productivity so that the presence of the error term does not introduce bias into our regressions, which use our estimate of productivity ($\hat{a}_{ht}$) as a key right hand side variable. The procedure adjusts the estimates by shrinking them toward the mean of the true, underlying productivity distribution.

True productivity is not observable, but we show in this appendix that its distribution is estimable. We also show how this shrinkage estimator fixes the attenuation bias that measurement error would otherwise introduce into our regressions.

Background on Empirical Bayes Procedure

Statistical Background

We start with an overview of the EB procedure assuming that all parameters of the distributions are known, and refer to the EB-adjusted estimated productivity as a_{ht}^{EB}. We then describe the feasible EB-adjusted estimate, which we denote $a_{ht}^{EB(f)}$.

Suppose that the estimated productivities are independently normally distributed around the true productivities with known variance π_{ht}^2:

$$\hat{a}_{ht}|a_{ht}, \pi_{ht}^2 \sim N\left(a_{ht}, \pi_{ht}^2\right) \text{ independently}$$

One can think of π_{ht}^2 as the variance of the measurement error of the estimate.

We also assume that the true productivities a_{ht} are independently normal with underlying mean $x_{ht}\beta_t$ (a known, year-specific linear function of the hospital-year's covariates) and underlying variance $\sigma_{a,t}^2$ (known and common across hospitals within a year).

The *prior distribution* of the productivity a_{ht} -- the distribution before conditioning on the estimated productivity -- is therefore:

$$a_{ht}|x_{ht}, \beta_t, \sigma_{a,t}^2 \sim N\left(x_{ht}\beta_t, \sigma_{a,t}^2\right) \text{ independently}$$

Conditioning on the estimated productivity $\hat{a}_{ht}$ produces the *posterior distribution* of a_{ht}:

$$a_{ht}|\hat{a}_{ht}, x_{ht}, \beta_t, \sigma^2_{a,t}, \pi^2_{ht} \sim N\left(a^{EB}_{ht}, \pi^2_{ht}\left(1 - B_{ht}\right)\right) \tag{A1}$$

a^{EB}_{ht} denotes the EB adjusted productivity. This object is the expected value of a_{ht} conditional on the estimated value $\hat{a}_{ht}$ and the parameters $\beta_t, \sigma^2_{a,t}$, and π^2_{ht} and is given by the formula:

$$\begin{aligned} a^{EB}_{ht} &= \left(1 - B_{ht}\right)\hat{a}_{ht} + B_{ht}x_{ht}\beta_t \\ B_{ht} &= \pi^2_{ht}/\left(\pi^2_{ht} + \sigma^2_{a,t}\right) \end{aligned}$$

The adjustment amounts to attenuating the estimate $\hat{a}_{ht}$ toward the mean $x_{ht}\beta_t$. As the variance of the measurement error π^2_{ht} rises, the EB correction increasingly disregards the value of the estimate and closes in on the mean.

Feasible Version of Procedure

This section describes how we implement the EB procedure when parameters must be estimated.

The productivity estimate $\hat{a}_{ht}$ is the estimated coefficient on a hospital-year fixed effect from equation (5). The regression that produces the estimated coefficient also yields a standard error for it -- an estimate of the standard deviation of the asymptotic distribution of $\hat{a}_{ht}$. We estimate π^2_{ht} by squaring the standard error and call this value $\hat{\pi}^2_{ht}$.

We estimate β_t and $\sigma^2_{a,t}$ using a method outlined in Morris (1983, section 5) which we reproduce here. Fix yearly estimates:

$$\begin{aligned} \hat{\beta}_t &:= \left(X_t' W_t X_t\right)^{-1} X_t' W_t A_t \\ \hat{\sigma}^2_{a,t} &= \max\left\{0, \frac{\sum_h W_{ht}\left\{\left(\frac{N_{ht}}{N_{ht}-N_x}\right)\left(\hat{a}_{ht} - x_{ht}\hat{\beta}_t\right)^2 - \hat{\pi}^2_{ht}\right\}}{\sum_h W_{ht}}\right\} \\ W_{ht} &:= \frac{1}{\hat{\pi}^2_{ht} + \hat{\sigma}^2_{a,t}} \end{aligned}$$

X_t is the stacked x_{ht} for year t, W_t is a diagonal matrix of the W_{ht} for year t, and A_t is the stacked $\hat{a}_{ht}$ for year t. N_{ht} is the number of hospitals, or equivalently the number of $\hat{a}_{ht}$, in year t. N_x is the number of regressors, i.e. the dimensionality of x_{ht}.

$\hat{\beta}_t$ is a WLS regression of the $\hat{a}_{ht}$ on x_{ht}. $\hat{\sigma}^2_{a,t}$ is the weighted average of the squared deviations of $\hat{a}_{ht}$ from $x_{ht}\hat{\beta}_t$ less the weighted average of $\hat{\pi}^2_{ht}$. The weights are W_{ht}, giving more weight to observations with less measurement error. The max operator ensures that $\hat{\sigma}^2_{a,t}$ is always nonnegative in finite samples.

$\hat{\beta}_t$ and $\hat{\sigma}^2_{a,t}$ are simultaneously determined in these equations, so for each year they are estimated by the following iterative procedure. We by fixing $W_{ht} = 1 \forall h$, then iterate the following to convergence:

1. Compute $\hat{\beta}_t$ and then a new estimate $\hat{\sigma}^2_{a,t}$
2. If $\hat{\sigma}^2_{a,t}$ has converged, exit. Otherwise, fix new weights W_{ht} and return to step 1

With a degrees of freedom correction, the (feasible) best estimate of the posterior mean $a_{ht}^{EB(f)}$ is:

$$\begin{aligned} a_{ht}^{EB(f)} &= \left(1-\hat{B}_{ht}\right)\hat{a}_{ht}+\hat{B}_{ht}x_{ht}\hat{\beta}_t \\ \hat{B}_{ht} &= \left(\frac{N_{ht}-N_x-2}{N_{ht}-N_x}\right)\left(\frac{\hat{\pi}^2_{ht}}{\hat{\pi}^2_{ht}+\hat{\sigma}^2_{a,t}}\right) \end{aligned}$$

The variance of productivity unconditional on covariates, called $\hat{\varsigma}^2_{a,t}$, is given by the following formula:

$$\begin{aligned} \hat{\varsigma}^2_{a,t} &= \max\left\{0, \frac{\sum_h W_{ht}\left\{\left(\frac{N_{ht}}{N_{ht}-1}\right)\left(\hat{a}_{ht}-\bar{A}_t\right)-\hat{\pi}^2_{ht}\right\}}{\sum_h W_{ht}}\right\} \\ \bar{A}_t &= \frac{\sum_h W_{ht}\hat{a}_{ht}}{\sum_h W_{ht}} \end{aligned}$$

Where $\bar{A}_t$ is the weighted mean productivity.

Implementation of Empirical Bayes Adjustment

We assume that the underlying mean of productivity is equal to a market-year fixed effect, i.e. $x_{ht}\beta_t = \tau_{M,t}$, where M indexes markets. Thus x_{ht} becomes a vector of 304 indicators for whether hospital h was in each of the 304 markets and β_t is a vector of the 304 market fixed effects for year t.

We perform the EB procedure separately year-by-year, producing estimates of the underlying market-year means $\hat{\beta}_t$ and year-specific conditional -- i.e. within-market -- variance $\hat{\sigma}^2_{a,t}$. Running the procedure also yields EB-adjusted estimated productivities $a_{ht}^{EB(f)}$ and also can be used to produce the unconditional -- i.e. national -- estimated variance $\hat{\varsigma}^2_{a,t}$, as described below.

Our procedure ensures that when the EB-adjusted productivities are used in our main regressions (equations (1) through (3) in the main text) which have market-year fixed effects, all regressors are orthogonal to the measurement error term.

Reported productivity metrics

Standard Deviation

To estimate the standard deviation of productivity using the EB adjusted values, we rely on the estimates of the yearly underlying national variance of productivity $\hat{\varsigma}^2_{a,t}$ that the procedure computes.[1]

[1]While it might seem natural to instead estimate the standard deviation of the EB-adjusted values, this would cause us to erroneously under-estimate dispersion. Underlying productivity is composed of a best prediction (the EB-adjusted

The root of these estimates is taken, forming $\hat{\varsigma}_{a,t}$. The yearly values are then averaged together.

The EB adjustment produces $\hat{\varsigma}^2_{a,t}$ by taking the weighted empirical variance of the $\hat{a}_{ht}$ and subtracting the weighted average squared standard error $\hat{\pi}^2_{ht}$. Hospital-years with larger standard errors receive lower weights. In effect, this process takes the variance of the noisy productivity estimates and subtracts off the variance due to measurement error.

90:10 and 75:25

We define the 90:10 ratio of productivity as $F^{-1}(0.9) - F^{-1}(0.1)$ and the 75:25 ratio as $F^{-1}(.75) - F^{-1}(.25)$ where F^{-1} is the inverse CDF of the productivity distribution. The 90:10 is the 90th percentile value of the distribution minus the 10th percentile value, and likewise for the 75:25. Exponentiating these ratios would produce the 90:10 ratio of the exponentiated productivity distribution (that is, an actual ratio: p90 / p10).

As with the standard deviation, it is not possible to estimate these ratios using the distribution of the $a^{EB(f)}_{ht}$. The EB correction does not produce a variable with the same asymptotic distribution as the underlying process. The procedure is only intended to estimate the parameters of an underlying normal distribution and correct for measurement error in regressions.

To estimate these ratios we use the inverse CDF of the underlying normal distribution that the EB procedure uncovers, so the yearly 90:10 and 75:25 are:

$$\begin{aligned} F^{-1}(0.9) - F^{-1}(0.1) &= \hat{\varsigma}_{a,t}\left[\Phi^{-1}(0.9) - \Phi^{-1}(0.1)\right] \\ F^{-1}(0.75) - F^{-1}(0.25) &= \hat{\varsigma}_{a,t}\left[\Phi^{-1}(0.75) - \Phi^{-1}(0.25)\right] \end{aligned}$$

Where $\Phi(\cdot)$ is the standard normal CDF.

Allocation Metrics (Patient, Growth, and Exit Regressions)

The allocation metrics use noisy estimates of productivity on the right-hand side of regressions, and rely on EB adjustment to correct for measurement error. Jacob and Lefgren (2007) show that with the adjustment, these regressions are estimated consistently.

Suppose that there is a relationship between growth g_{ht}, market-year fixed effects $\gamma_{M,t}$, and productivity a_{ht}:

$$g_{ht} = \gamma_{M,t} + \delta a_{ht} + \epsilon_{ht}$$

where $\mathbb{E}[\epsilon_{ht}|x_{ht}, a_{ht}] = 0$ (x_{ht} is a vector of indicators for the market-years -- the design matrix for the market-year fixed effects.) The left-hand side variable could alternatively be the number of patients or an indicator for hospital exit.

productivity) and the prediction error. These two components are orthogonal. The variance of true productivity is thus strictly greater than the variance of EB-adjusted productivity (see Jacob and Lefgren 2007).

Since we do not observe true productivity a_{ht}, we use the estimate $\hat{a}_{ht} = a_{ht} + \eta_{ht}$, where η_{ht} is measurement error. Then substituting into the equation:

$$g_{ht} = \gamma_{M,t} + \delta \hat{a}_{ht} + (\epsilon_{ht} - \delta \eta_{ht})$$

This regression produces a biased and inconsistent estimate of δ due to the correlation between $\hat{a}_{ht}$ and η_{ht} in the error term. We use the EB-adjusted productivity a_{ht}^{EB} to eliminate this correlation. Equation A1 implies:

$$\mathbb{E}\left[a_{ht} | \hat{a}_{ht}, x_{ht}, \beta_t, \sigma_{a,t}^2, \pi_{ht}^2\right] = a_{ht}^{EB}$$

We represent the prediction error of the EB procedure as v_{ht}:

$$a_{ht} = a_{ht}^{EB} + v_{ht}$$

By construction the prediction error is orthogonal to a_{ht}^{EB} and any regressor included in x_{ht} -- i.e. the market-year fixed effects:

$$\mathbb{E}\left[v_{ht} | a_{ht}^{EB}, x_{ht}, \beta_t, \sigma_{a,t}^2, \pi_{ht}^2\right] = 0$$

($\hat{a}_{ht}$ is replaced by a_{ht}^{EB} because given the parameters, knowing one determines the other)

The regression of g_{ht} on market-year effects and a_{ht}^{EB} adds only v_{ht} to the original error term ϵ_{ht}:

$$g_{ht} = \gamma_{M,t} + \delta a_{ht}^{EB} + (\epsilon_{ht} - \delta v_{ht})$$

Therefore there is no correlation between any of the regressors and the new error term. The consistency of δ follows.

Comparison of estimates

We run all of our regression analyses with the EB-adjusted productivities $a_{h,t}^{EB(f)}$ and calculate our dispersion metrics using the EB-adjusted dispersion estimates as described above. Table A3 explores the impact of the EB correction on our main results. The first column reproduces the EB-adjusted main results from Tables 2, 4, and A6. The second column shows the results without the EB correction.

To produce the uncorrected allocation metrics, we use the estimates $\hat{a}_{ht}$ rather than $a_{h,t}^{EB(f)}$ in our regressions. Due to measurement error in the estimates, the allocation metrics computed without the EB correction will be attenuated. We calculate the uncorrected dispersion metrics in the same manner as the corrected versions, but using uncorrected estimates of productivity. For example, to calculate the standard deviation, the empirical weighted standard deviation of the estimated productivities -- $SD(\hat{a}_{ht})$ -- is taken year-by-year, then averaged (we use the same weights that were used to calculate $\hat{\varsigma}_{h,t}^2$ so that the statistics are comparable.) Likewise, the 90:10 and 75:25 ratios are calculated by fitting a normal distribution to the estimated, uncorrected productivities and reporting the ratios implied by it (the ratios are calculated year-by-year, then averaged). Due to measurement error, the dispersion metrics computed without the EB correction will overstate the true dispersion.

The results show that the EB correction has a substantial effect on our baseline estimates, and moves them in the expected direction. Comparing our baseline (EB-adjusted) estimates in column 1 with the un-adjusted version in column 2, we see that the allocation results are substantially larger and the dispersion estimates are substantially lower with the correction. For example, we find that measurement error explains nearly half of the dispersion of the productivity estimates; without correcting for measurement error, these estimates have an average yearly standard deviation SD $(\hat{a}_{ht})$ of 0.293, while the EB procedure estimates that the underlying productivity process has an average yearly standard deviation $\hat{\varsigma}_{a,t}$ of 0.173.

A quantitatively large impact of the EB correction (i.e. a large amount of measurement error) is not surprising in light of results from other applications. For example, looking at estimates of teacher fixed effects in value added regressions, Jacob and Lefgren (2007) estimate a ratio of the unadjusted standard deviation to the EB-adjusted estimate of the standard deviation of about 1.3 to 1.6. We find ratios of about 1.7.

Table A3 - Sensitivity of Results to EB Adjustment

	(1)	(2)
EB Adjustment:	Yes	No
Parameter		
μ	0.446	0.446
	(0.00511)	(0.00511)
Static Allocation	2.418	0.440
	(0.0889)	(0.0182)
Dynamic Allocation		
Exit Regression	-0.0329	-0.0138
	(0.00935)	(0.00347)
Growth Regression	0.133	0.0373
	(0.0225)	(0.00759)
Dispersion		
90:10	0.442	0.751
	(0.0112)	(0.0136)
75:25	0.233	0.395
	(0.00590)	(0.00714)
Standard Deviation	0.173	0.293
	(0.00438)	(0.00530)

Notes: Column (1) is baseline specification. Column (2) shows results without the empirical Bayes adjustment. Standard errors are bootstrapped with 300 replications and are clustered at the market level.

Appendix D: Additional Results

Counterfactual allocation rule

An alternative explanation for our findings is that patients go the nearest hospital to treat their AMI, and it happens that areas with higher productivity hospitals are both higher in population density and higher in population growth. If this story held, a mechanical allocation rule that assigned patients to their nearest hospital would spuriously produce our static and dynamic allocation results. In practice, based on geocoding hospital addresses and patient zip codes to latitudes and longitudes, we estimate that less than half of our AMI patients go to the nearest hospital in their market. Moreover, we examined what the static and dynamic allocation results would look like if (counter-factually) each AMI patient did go to the nearest hospital within his market. We would be concerned if this mechanical rule produced similar static and dynamic allocation results, as that would suggest the result could be generated without any role for patient demand. In fact, as shown in Table A4 (column 3 vs. column 1), with this assignment rule the dynamic allocation results are either the wrong sign or an order of magnitude smaller (and not statistically significant) and the static allocation result declines to 20 percent of the baseline estimate.

Static and Dynamic Allocation For Different Hospitals and Markets

Appendix Table A5 looks at how the static and dynamic allocation results vary across different types of hospitals within a market, and how they vary across different markets. The results are mixed. Within a market, the allocation results are stronger for hospitals facing more competition for their patients (using distance to the nearest hospital as a proxy for competition as in Gaynor and Vogt, (2003)); the allocation relationships are also weaker for public (compared to private) hospitals. However, at the market level, there is no evidence that the allocation results are stronger in more competitive markets (using population density as a proxy for competition for a spatially differentiated product as in Syverson (2004b)); there is also no evidence that the allocation result is stronger in markets with more educated consumers.

Productivity Dispersion Across Hospitals

Appendix Table A6 shows our estimates of productivity dispersion across hospitals. The calculation of the metrics was described in Appendix C.

Static and Dynamic Allocation in Concrete and Health Care

We use data on ready-mixed concrete from the Census of Manufactures, which we have for every five years from 1972 – 1997. We observe approximately 2,500 ready-mixed concrete plants per data year; by way of comparison, we have approximately 3,700 hospitals per year. We use these data to estimate plants' physical total factor productivity levels. A plant's physical total factor productivity is the number of cubic yards of concrete it produces per unit input, where inputs are a weighted composite of labor, capital, and intermediates. The weights are the inputs' cost shares. These weights are theoretically correct, equaling the elasticities of output with respect to each input assuming cost minimization and no adjustment costs in inputs. Our market definition is the Bureau of Economic Analysis' Component Economic Areas, which are approximately 350 mutually exclusive and exhaustive groupings of economically interrelated U.S. counties. (See, e.g., Syverson 2004b for more details on productivity and market measurement in ready-mixed concrete.) To reduce the influence of outliers, we trim the top and bottom 1% of the industry's productivity distribution in each Census of Manufactures.

Table A7 reports the results. Across all of our static and dynamic allocation measures, the results indicate a stronger relationship between market allocation and producer productivity for hospitals than for concrete plants. The first row reports the results for static allocation. We estimate a slight variant of equation (1); as before, the specification regresses output on productivity (both measures are in logarithms) and market-year fixed effects. However, we now use lagged productivity on the right-hand side to facilitate comparisons between hospitals and concrete plants.[34] Strikingly, the correlation between output and lagged productivity is an order of magnitude larger in healthcare than in concrete.

The second row reports our exit analysis, based on equation (4) but modified to account for the fact that in concrete we only have data every five years; therefore, for purposes of comparability, we look at exit five years later for both hospitals and for concrete. However, comparability is limited by the fact that "exit" is defined quite differently in the two data sets.[35]

The final row reports our growth analysis. To make the analysis comparable across the two industries, for both we run the following regression:

$$\frac{N_{h,t+10}-N_{h,t+5}}{\frac{1}{2}\left(N_{h,t+10}-N_{h,t+5}\right)} = \beta_0 + \beta_1 a_{h,t} + \gamma_{Mt} + \varepsilon_{ht} \qquad \text{(A2)}$$

Here, "size" (N) is defined as the number of patients in hospitals or the amount of physical output for concrete plants.[36]

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[34] Due to how productivity is measured for concrete plants, regressing output on contemporaneous productivity would yield spuriously expanded coefficients: for concrete, output is effectively the numerator of the productivity measure. To fix the bias, we use the productivity measure from 5 years earlier on the right-hand side, rather than contemporaneous productivity. The lag is 5 years for both sectors because data for concrete plants is only available at that frequency.

[35] In the concrete data, exit is directly observed; in the hospital data we infer "exit" based on the hospital having less than 5 patients for five consecutive years. Therefore, for concrete we regress an indicator for whether the firm has exited at year *t+5* on productivity in year *t* (and market-year fixed effects). For hospitals, we regress an indicator for whether the hospital has less than five patients in every year from year *t+5* to year *t+9* on productivity in year *t* (and market-year fixed effects).

[36] In order to make the growth analysis comparable for hospitals and for concrete, this regression differs from our baseline growth regression (equation 3) in two ways. First, because the concrete data is only available every five years, it looks at growth between 5 year periods rather than 1 year periods. Second, it lags the productivity estimate on the right hand side back another time period. As in the static allocation metric, we do this because in manufacturing, our measure of size is output, which also enters the numerator of the productivity estimate; if there is mean reversion in output and we had $a_{h,t+5}$ on the right hand side instead, this would create a negative bias on the β_1 coefficient.

Table A4 - Tests of Robustness of Allocation Results

	(1)	(2)	(3)
Risk Adjustment:	Baseline	Smaller Market	Nearest Hospital
Static Allocation	2.418	2.816	0.449
	(0.0889)	(0.152)	(0.0685)
Dynamic Allocation			
Exit Regression	-0.0329	-0.0675	0.00407
	(0.00935)	(0.0189)	(0.00889)
Growth Regression	0.133	0.161	0.0219
	(0.0225)	(0.0446)	(0.0220)

Notes: The allocation results are produced by estimating the specifications given in the notes to Table 4. Column (1) repeats the baseline full risk adjustment results. Column (2) reports the results from running the same specification with the market defined as an HSA (Hospital Service Area; HSAs partition the baseline set of markets into approximately 10 times as many markets). Since the coefficients are identified by market-years with multiple hospitals, this reduces the effective number of observations by about half. Column (3) reports the baseline results but counterfactually calculates hospital size, growth, and exit by assigning all patients to the nearest hospital in their market, rather than the hospital at which they were actually treated. Standard errors are bootstrapped with 300 replications and are clustered at the market level.

Table A5 - Allocation Metrics By Hospital- and Market-Level Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	ln(Pats)	Growth	ln(Pats)	Growth	ln(Pats)	Growth	ln(Pats)	Growth	ln(Pats)	Growth
ln(Productivity)	2.418	0.133	2.371	0.143	2.280	0.174	3.644	0.171	3.104	0.202
	(0.0889)	(0.0225)	(0.0931)	(0.0236)	(0.119)	(0.0286)	(0.363)	(0.0883)	(0.357)	(0.0627)
× Government-Run Hospital			-0.341	-0.0802						
			(0.143)	(0.0369)						
× ln(Min Distance to Nearest Hospital)					-0.150	-0.0392				
					(0.0537)	(0.0118)				
× Share College+ in Market							-5.372	-0.166		
							(1.467)	(0.357)		
× ln(Pop/KM^2) in Market									-0.157	-0.0159
									(0.0763)	(0.0127)
Government-Run Hospital			-0.511	-0.0358						
			(0.0398)	(0.00816)						
ln(Min Distance to Nearest Hospital)					-0.273	-0.0186				
					(0.0155)	(0.00258)				
Observations	55,540	52,777	55,540	52,777	55,540	52,777	55,540	52,777	55,540	52,777

Notes: Columns (1) and (2) replicate our baseline static and dynamic allocation results from Table 4, column 1. Column (1) shows the static allocation relationship between a hosital-year's log(patients) and productivity within a market-year (see equation 1). Column (2) shows the dynamic allocation relationship (within a market-year) between a hospital's one year percent growth and its base year productivity (see equation 3). In the remaining columns these analyses are augmented to include the specified interactions with market- and hospital-level variables (as well as the main effect of these variables as indicated). Standard errors are bootstrapped with 300 replications and are clustered at the market level.

Government-Run is defined using the hospital control field in the CMS Provider of Services file. Min Distance is the distance between the hospital and the nearest hospital to it that treated an AMI patient in that year. Share College+ is defined as the share of the population in the hospital's market that had at least a bachelor's degree in the 2000 Census. Pop/KM^2 is the population density in the hospital's market according to the 2000 Census.

Table A6 - Productivity Dispersion across hospitals.

	(1)	(2)	(3)
Risk Adjustment:	All	Age/Race/Sex	None
90-10	0.442	0.469	0.521
	(0.0112)	(0.0117)	(0.0126)
75-25	0.233	0.247	0.274
	(0.00590)	(0.00614)	(0.00666)
Standard Deviation	0.173	0.183	0.203
	(0.00438)	(0.00455)	(0.00493)

Notes: Productivity is estimated based on the corresponding specification in Table 2. Dispersion measures in productivity are constructed nationally each year, and then averaged across years. The top row reports difference in productivity between the 90th percentile hospital and the 10th percentile hospital; the next row reports the difference in productivity between the 75th percentile and the 25th percentile hospital; the bottom row reports the estimated standard deviation of the productivity distribution. Standard errors are bootstrapped with 300 replications and are clustered at the market level.

Table A7 - Allocation Metrics: Concrete vs Hospitals

	Concrete			Hospitals		
Risk Adjustment:	Estimate	DV Mean	Sample (Approx)	Estimate	DV Mean	Sample
Static Allocation	0.299		5,500 plant-years	2.166	3.585	33,155 hospital-years
	(0.076)			(0.094)		
Dynamic Allocation						
Exit Regression	-0.066	0.20	12,400 plant-years	-0.147	0.17	25,359 hospital-years
	(0.018)			(0.028)		
Growth Regression	0.080	-0.075	2,600 plant-years	0.480	-0.62	18,569 hospital-years
	(0.069)			(0.069)		

Notes: Estimates for concrete are based on data from the quinquennial Census of Manufactures from 1972-1992. Estimates for hospitals are based on Medicare AMI patients from 1993-2007 and use our baseline specification (see Table 2, column 1). Standard errors are robust analytic (Concrete) or bootstrapped with 300 replications and clustered at the market level (Hospitals). See text for further details on metrics and data (described in more detail in Appendix D).

Appendix E: Robustness Analysis

Additional risk adjusters

For approximately one year of patients, we have access to even more detailed information on health than in the Medicare claims data. These data comes from the Cooperative Cardiovascular Project (CCP), which abstracted information from patient charts to create an extremely detailed dataset of clinically relevant characteristics, like test results and medical histories, for a nationally representative sample of Medicare AMI patients in 1994 and 1995. These data, which are described in more detail in Chandra and Staiger (2007), are considered superior to administrative data because of the much more specific and reliable information available on patient charts than in claims data. In Table A8 we re-run our analyses on this subset of the data and show that the results are not sensitive to adding this additional, more extensive, set of controls.

Column (7) shows the results for the CCP sample with the all the information abstracted from the patient chart. These results are very similar to results from the CCP data that use fewer risk adjustors (columns 8 and 9). Results with fewer risk adjustors in the CCP data (columns 8 and 9) look roughly similar to results in one year (1994) of Medicare claims data with the same risk adjustors (column 5 and 6), which are also roughly similar to the results on our full set of Medicare claims data (columns 1-3).

Alternative Input Measures

Appendix Table A9 explores the robustness of our results to alternative input measures; more detail on their construction is provided in Appendix B. Column 1 replicates our baseline results. As noted in Section 6, there is a tradeoff between our relatively coarse baseline measure of inputs (with its associated measurement error) and more granular measures which suffer from potential survivorship bias (a patient cannot have a lot of procedures done if he does not survive very long). Columns 2 and 3 explore the sensitivity of our estimates to more granular measures which use as inputs a series of approximately 60 indicators for whether various procedures were performed as well as a continuous variable measuring the log of the number of days in the hospital during our 30 day window.

We incorporate this more granular input measure in two different ways. In column 2 we explore a multi-input production function; specifically, we replace our single index measure with all of the procedure indicators as well as the log hospital days variable. In column 3 we return to a single-input production function but one that is based on this more granular input measure; we create the single input by regressing log hospital charges on these same procedure indicators and the log hospital days variable, as well as hospital-year fixed effects.[37] We use the coefficients from this regression – ignoring the hospital-year effects – to produce an estimate of predicted charges for each patient in our data. The correlation between this predicted log charges measure and our baseline log input measure is 0.77 (with actual log charges it would be 0.75). As would be expected from survivorship bias, the returns to scale coefficient μ in column 3 is substantially higher than that in our baseline column 1.

Yet another alternative approach to inputs is to measure Medicare reimbursement to the hospital for a patient, rather than the hospital's use of inputs per se. Like our baseline approach, this approach is also often used in the literature (e.g. Cutler et al., 1998, Skinner and Staiger 2009). Medicare reimbursement depends not just on the patient's DRGs (our baseline resource measure) but also characteristics of the hospital (such as whether it is a teaching hospital or whether it treats a

[37] Hospital "charges" are accounting charges for rooms and procedures and do not reflect transacted prices. They have been used in the literature as a convenient, price-weighted summary of treatment, albeit at somewhat artificial prices (Card et al., 2009, Finkelstein et al., 2012). The hospital-year fixed effects in the log charges regression eliminate variation across hospital-years in the charge-to-cost ratio (i.e. differential hospital markups of list prices above costs).

disproportionate share of low income patients) and its location (MedPAC 2011a). Part A Medicare spending per AMI patient is the standard measure used in the economics literature in studying the relationship between heart attack treatment and outcomes (e.g. Cutler et al. 1998, Skinner and Staiger 2009). The results in column 4 use this Medicare reimbursement measure; the returns to scale parameter μ is therefore interpreted here as the return to federal expenditures (in the form of post-AMI survival) rather than real inputs. The correlation between our baseline resources measure and the reimbursement measure is 0.90. The main results are all quite robust to this alternative measure.

A final input measure incorporates physician and outpatient inputs for the subsample of hospital years beginning in 2001 (see Appendix B for more details; our sample starts in 2001 because it is the first full year with data). Column 5 shows our baseline results limited to the sample where we can observe these other input measures; this cuts our sample of hospital-years substantially, by about 70 percent. Column 6 shows the results for this same "overlap" sample with our expanded input measure. For the overlap sample, the correlation between our baseline input measure and the expanded measure is 0.98.[38]

Looking across the columns, the basic qualitative findings concerning the role for competition in allocating more market demand to more productive firms both at a point in time and over time are quite robust to alternative input measures. In particular, the static allocation analysis and the growth analysis remain statistically significant in virtually all alternative specifications. The statistical significance of the exit-based regression results is more sensitive to the choice of input measure. Perhaps not surprisingly, the magnitudes of the static and dynamic allocation analyses vary somewhat across the specifications. The dispersion estimates are remarkably robust to alternative input measures.

Alternative Time Frames for Measuring Inputs and Outputs

Appendix Table A10 considers how our metrics are affected by alternative time windows for measuring survival and inputs. Our baseline specification looks at survival over 1 year and at inputs over 30 days. A shorter time horizon for inputs will miss some of the resources provided to the patient. There is also a practical limitation to very short horizons; we observe resources at the level of a hospital stay, not a hospital day or hour; 96% of hospital stays are at most 30 days long, but a measure like 7 day utilization would require arbitrary spreading of resources across the 7 days for the 33% of patients who spend more than 7 days in the hospital. Longer time horizons have their own limitations: issues of survival bias (the longer the patient lives, the more that can be done) and the fact that as time passes since the first incident, the treatments that are undertaken are increasingly linked to providers outside the original hospital. Columns 2 and 3 show, respectively, that the results are robust to a longer (one year) survival horizon and a shorter (7 day) survival horizon, rather than our baseline 30 day time frame.

In terms of the time horizon for outcomes, we choose a 1-year survival window because it is of more interest than short-term survival, which may reflect only a few days postponement of mortality. As a practical matter, censoring is also less prevalent at 1 year than at shorter horizons. Finally, another advantage of our 1-year window is that it will pick up aspects of hospital productivity that affect outcomes through longer-term mechanisms such as the management of complications due to co-morbidities like congestive heart failure or diabetes. Longer time windows will also better capture the quality of continuing care like the prescribing of statins and the follow up to make sure the patient is taking these medications. Such inputs are less likely to affect survival at much shorter horizons but can be quite important over longer intervals. On the other hand, the longer measurement horizon introduces greater scope for patient autonomy (e.g. in terms of changes in behavior such as diet and smoking, compliance with recommended medications, follow-up visits, etc.) and for the impact of doctors (regardless of which hospital the patient went to) or admissions to other hospitals to affect survival.

[38] This high correlation reflects the fact that outpatient resources are, on average, about one-fifth the size of the inpatient resources devoted to one of our patients; in addition there is a high (about two-thirds) correlation between outpatient and inpatient resources devoted to a patient.

Longer horizons may therefore attenuate differences across hospitals in measured productivity. Our results are robust to moving away from our baseline 1 year survival to 30 day survival (column 4) or to 5 year survival (column 6); the 5 year horizon requires that we limit the sample to heart attacks through 2003 so that we observe the patient for 5 subsequent years; column 5 shows our baseline 1 year survival measure on this sample.

Alternative Market Definition

Our analysis looks at within-market static allocation and dynamic re-allocation. The baseline results use a Hospital Referral Region (HRR) as the market definition. An alternative definition of the hospital market which is sometimes used is a Hospital Service Area (HSA). HSAs are partitions of HRRs; there are about 10 times as many HSAs as HRRs.[39] Table A4 shows that our core static and dynamic allocation results are robust – indeed, they become slightly larger in magnitude – when using this alternative market definition.

Imposing scale parameter μ

We evaluated the robustness of our main results to imposing, rather than estimating, various values for the scale parameter μ. This method amounts to following the index number, or Solow residual, approach to measuring productivity in which factor elasticities are taken from auxiliary data such as factor cost shares. We impose a μ of 0.1, 0.3, and 0.9. These results are shown in Table A11.

[39] For more information see http://www.dartmouthatlas.org

Table A8 - CCP

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dataset	Medicare Claims 1993-2007			Medicare Claims 1994			CCP 1994-1995		
Risk Adjustment	Baseline	Age/Race/Sex	None	Baseline	Age/Race/Sex	None	Entire Chart	Age/Race/Sex	None
Parameter									
μ	0.446	0.481	0.589	0.456	0.482	0.600	0.282	0.412	0.530
	(0.00511)	(0.00523)	(0.00552)	(0.00857)	(0.00869)	(0.00875)	(0.0074)	(0.0087)	(0.0091)
Static Allocation	2.418	2.496	2.618	2.560	2.540	2.332	1.942	2.104	2.118
	(0.0889)	(0.0851)	(0.0779)	(0.268)	(0.249)	(0.181)	(0.3362)	(0.3284)	(0.2910)
Dispersion									
90:10	0.442	0.469	0.521	0.447	0.465	0.523	0.366	0.401	0.424
	(0.0112)	(0.0117)	(0.0126)	(0.0221)	(0.0219)	(0.0221)	(0.0247)	(0.0267)	(0.0266)
75:25	0.233	0.247	0.274	0.235	0.245	0.275	0.193	0.211	0.223
	(0.00590)	(0.00614)	(0.00666)	(0.0116)	(0.0115)	(0.0116)	(0.0130)	(0.0141)	(0.0140)
Standard Deviation	0.173	0.183	0.203	0.174	0.181	0.204	0.143	0.156	0.166
	(0.00438)	(0.00455)	(0.00493)	(0.00861)	(0.00853)	(0.00861)	(0.0096)	(0.0104)	(0.0104)
Patients		3,530,401			244,070			136,434	
Hospitals		5,346			4,349			3,829	
Hospital-Years		55,540			4,349			3,829	

Notes: Columns 1-3 reproduce our main results from Tables 2, 4 and 6. Columns 4-6 perform the same analysis on a single year of our data (1994), and columns 7-9 show the analysis on the 1994-1995 CCP sample. The CCP sample is smaller than the year of Medicare claims because it only collected data for each region of the country for 8 months and because it excluded patients whose charts had been incorrectly coded as showing evidence of AMI. The CCP results using age/race/sex adjustment (column 8) look similar to our results for one year of data using age/race/sex adjustment (column 5). (We are unable to replicate our baseline set of covariates in the CCP data due to some differences in variable availability). In the CCP, we find that relative to age, race, and sex risk adjustment (column 8), using all information that was abstracted from the patient chart (column 7) slightly weakens the static allocation relationship and slightly reduces dispersion. Standard errors are bootstrapped with 300 replications and are clustered at the market level

Table A9 - Comparison of Input Measures

	(1)	(2)	(3)	(4)	(5)	(6)
Input Measure:	Baseline	Procedures	Fitted Chg	Spending	Baseline	Base+Part B
Sample:	Full	Full	Full	Full	With Part B Data	
Parameter						
μ	0.446		0.714	0.395	0.369	0.399
	(0.00511)		(0.00652)	(0.00508)	(0.00699)	(0.00715)
Static Allocation	2.418	1.497	0.972	1.749	2.326	2.232
	(0.0889)	(0.0879)	(0.0996)	(0.0834)	(0.233)	(0.232)
Dynamic Allocation						
Exit Regression	-0.0329	-0.0199	-0.00661	-0.0245	-0.0330	-0.0347
	(0.00935)	(0.0106)	(0.0106)	(0.00943)	(0.0450)	(0.0476)
Growth Regression	0.133	0.0611	-0.00515	0.0762	0.220	0.211
	(0.0225)	(0.0258)	(0.0263)	(0.0230)	(0.0762)	(0.0798)
Dispersion						
90:10	0.442	0.431	0.428	0.453	0.353	0.343
	(0.0112)	(0.00891)	(0.00908)	(0.0104)	(0.0229)	(0.0227)
75:25	0.233	0.227	0.225	0.239	0.186	0.180
	(0.00590)	(0.00469)	(0.00478)	(0.00545)	(0.0121)	(0.0120)
Standard Deviation	0.173	0.168	0.167	0.177	0.138	0.134
	(0.00438)	(0.00348)	(0.00354)	(0.00404)	(0.00895)	(0.00887)
Patients / 1000	3,530	3,530	3,530	3,525	271.3	271.3
Hospital-Years	55,540	55,540	55,540	55,529	15,039	15,039
Hospitals	5,346	5,346	5,346	5,346	3,092	3,092

Notes: Column (1) is baseline specification. All other columns use alternative input measures (described in more detail in Appendices B and E). Column 5 and 6 are limited to the sub-sample of approximately 30 percent of hospital-years for which we observe Part B physician and outpatient data for at least five AMI patients in that hospital-year; in column 6 our baseline input measure (which uses only Part A inputs) is expanded to include Part B inputs; see text for more details. Standard errors are bootstrapped with 300 replications and are clustered at the market level.

Table A10 - Comparison of Results with Varying Survival and Input Horizons

	(1)	(2)	(3)	(4)	(5)	(6)
Survival Horizon:	1 Year	1 Year	1 Year	30 Days	1 Year	5 Years
Input Window:	30 Days	1 Year	7 Days	30 Days	30 Days	30 Days
Sample Thru:	2007	2007	2007	2007	2003	2003
Parameter						
μ	0.446	0.790	0.172	0.292	0.451	0.585
	(0.00511)	(0.00504)	(0.00959)	(0.00243)	(0.00544)	(0.00791)
Static Allocation	2.418	2.694	2.421	3.992	2.347	2.047
	(0.0889)	(0.0955)	(0.0906)	(0.146)	(0.0938)	(0.0811)
Dynamic Allocation						
Exit Regression	-0.0329	-0.0317	-0.0372	-0.0660	-0.0221	-0.0201
	(0.00935)	(0.00969)	(0.00918)	(0.0173)	(0.0105)	(0.00815)
Growth Regression	0.133	0.138	0.147	0.213	0.101	0.101
	(0.0225)	(0.0230)	(0.0220)	(0.0409)	(0.0251)	(0.0189)
Dispersion						
90:10	0.442	0.422	0.450	0.224	0.446	0.583
	(0.0112)	(0.00981)	(0.0117)	(0.00626)	(0.0119)	(0.0146)
75:25	0.233	0.222	0.237	0.118	0.235	0.307
	(0.00590)	(0.00516)	(0.00617)	(0.00330)	(0.00628)	(0.00770)
Standard Deviation	0.173	0.164	0.175	0.0874	0.174	0.227
	(0.00438)	(0.00383)	(0.00457)	(0.00244)	(0.00465)	(0.00571)
Patients / 1000	3,530	3,530	3,530	3,530	2,702	2,702
Hospitals	5,346	5,346	5,346	5,346	5,180	5,180

Notes: Column (1) is baseline specification. In other columns the time horizon in which we measure survival and/or inputs is modified as indicated in the column headings. Standard errors are bootstrapped with 300 replications and are clustered at the market level.

Table A11 - Sensitivity of Results to μ

	(1)	(2)	(3)	(4)
Source of μ:	Estimated		Imposed	
Value of μ:	0.446	0.1	0.3	0.9
Static Allocation	2.418	2.358	2.399	2.278
	(0.0889)	(0.0883)	(0.0884)	(0.0835)
Dynamic Allocation				
Exit Regression	-0.0329	-0.0361	-0.0343	-0.0263
	(0.00935)	(0.00923)	(0.00930)	(0.00891)
Growth Regression	0.133	0.144	0.138	0.107
	(0.0225)	(0.0220)	(0.0223)	(0.0215)
Dispersion				
90:10	0.442	0.449	0.445	0.457
	(0.0112)	(0.0116)	(0.0114)	(0.0104)
75:25	0.233	0.237	0.234	0.241
	(0.00590)	(0.00611)	(0.00599)	(0.00549)
Standard Deviation	0.173	0.175	0.173	0.178
	(0.00438)	(0.00453)	(0.00444)	(0.00407)

Notes: Column (1) shows results based on estimation of our baseline specification (Table 2, column 1). In the other columns μ is imposed rather than estimated. Standard errors are bootstrapped with 300 replications and are clustered at the market level.

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