Can Pay Regulation Kill? Panel Data Evidence on the Effect of Labor Markets on Hospital Performance

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In many sectors, pay is regulated to be equal across heterogeneous geographical labor markets. When the competitive outside wage is higher than the regulated wage, there are likely to be falls in quality. We exploit panel data from the population of English hospitals in which regulated pay for nurses is essentially flat across the country. Higher outside wages significantly worsen hospital quality as measured by hospital deaths for emergency heart attacks. A 10 percent increase in the outside wage is associated with a 7 percent increase in death

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rates. Furthermore, the regulation increases aggregate death rates in the public health care system.

Economists have long warned of the unintended consequences of labor market regulation (e.g., Botero et al. 2004). Many rules rationalized by equity considerations can be harmful both to consumers and to those they are meant to help. There are many studies of labor quantity restrictions (e.g., firing costs) and labor price floors (e.g., minimum wages). One common but understudied form of regulation is one in which pay is mandated to be the same over a large geographical area that includes heterogeneous local labor markets. In the United States, the pay of postal workers, federal government employees, and many unionized workers is set in this way.[1] Centralized pay setting is the norm for physicians, nurses, and high school teachers in many other countries.

When the outside market wage is high, the regulated wage acts as a *pay ceiling*, and we would expect this to cause difficulties in recruitment and retention, especially of higher-quality workers, which in turn should lead to lower service quality. One contribution of this paper is to confirm this simple economic intuition and to show that centralized pay regulation has exactly this negative impact on consumers in a very stark setting—the market for nurses in the U.K. public hospital system. A second contribution is that we show how the effect is "convex" in that the negative effect of regulation on hospital quality is much stronger in the high-cost areas (where regulated wages are much lower than the outside wage) than the positive effect in the low-cost areas (where regulated wages are higher than the outside wage). Thus, the aggregate effect of the pay regulation is to increase aggregate death rates and strongly reduce social welfare.[2] We show how these effects can arise naturally in the context of a two-sector, two-region equilibrium model of an occupational labor market. The model also makes a third prediction that is consistent with the data: that performance should improve in the unregulated sector when outside wages are high.

Our design exploits the centralized pay setting for over a quarter of a million nurses in the U.K. National Health Service (NHS). Nurses account for over half of the clinical staff in hospitals, and their number and quality have been argued to be a key input into the production of

[1] Below we discuss the analysis of geographically flat pay for a U.S. auto manufacturer in Cappelli and Chauvin (1991), which is the closest precedent of our paper.

[2] We show in Sec. V that this is due to the lack of geographical variation in the regulated wage with market conditions rather than to the level of the regulated wage being "wrong" on average.

patient care.[3] The data we use are taken from a panel of all short-term general hospitals (called "acute" hospitals) in England. To measure performance we examine one important marker of hospital quality: the in-hospital death rate within 30 days of emergency admission for acute myocardial infarction (AMI), commonly called "heart attacks."[4]

One advantage of this setting is the very rigid national pay-setting structure for nurses. Pay for nurses (and physicians) in NHS hospitals, which provide almost all hospital care in the United Kingdom, is set by a central review body that sets pay scales in which there is very limited regional variation (see, e.g., Review Body for Nursing Staff, Midwives, Health Visitors and Professions Allied to Medicine 2003). The variation that exists does not fully reflect the wage differentials in the external labor markets in which the staff are employed. Regional pay differences are considerable in the U.K. private sector even after we control for human capital characteristics and other factors.[5] We would therefore expect to see differences between inside and outside wages reflected in staffing difficulties that manifest themselves in the lower performance of hospitals operating in high outside wage labor markets.[6]

A second advantage of our design is that hospital and patient selection (the concern that observed hospital death rates may partially reflect unobserved patient attributes) is less likely to affect our study than if we used U.S. data. Our institutional setting is one in which there is almost no choice of hospitals by patients and minimal incentives for hospitals to select patients. Medical care is free at the point of use in the United Kingdom. In the period we study neither patients nor their family physicians chose hospitals for the emergency treatment of heart attacks. In fact, even for nonemergency care, patients had little say over where they went for treatment. In contrast to the United States, U.K. hospitals also had little incentive to select low-risk and low-cost patients. For much of the period we study they were not required to publish any outcome data (e.g., their mortality rates); nor were the costs of an individual patient easily identified since full public insurance meant that hospital financial systems were not designed to record costs at the patient level. Nevertheless, we carefully assess the evidence for possible selec-

[3] For example, California mandated nurse to patient ratios in all California hospitals from January 2004.

[4] Examples of the use of AMI death rates to proxy for hospital quality include Kessler and McClellan (2000), Gaynor (2004), and Propper, Burgess, and Green (2004, 2008) for the United Kingdom. The advantages of this measure are discussed below.

[5] See Bulman (2002). As in the United States (e.g., Borjas 2002), the cross-sectional dispersion of U.K. public-sector pay is much lower than in the private sector (e.g., Disney and Gosling 1998).

[6] There is other evidence that falling U.K. public-sector wages relative to the private sector have led to a decline in the quality of the public-sector workforce (Nickell and Quintini 2002).

tivity biases through the use of hospital fixed effects, comorbidity measures, and analysis of business cycle influences.

Our paper is connected to several literatures. First, labor economists have long been interested in the impact of labor market changes on firm performance. Theories of "efficiency wages," for example, suggest that improvements in the labor market outside the firm's boundaries could lead to decreased productivity within a firm because there may be more shirking (Shapiro and Stiglitz 1984), a loss of high-quality workers (Weiss 1980), or perceptions of inequity (e.g., Akerlof 1982; Mas 2006). It is difficult to test these ideas in an unregulated labor market. Where pay is set by regulation, however, there is a wedge between inside and outside wages that enables identification of the impact of external labor markets on firm outcomes. So we can effectively use regulation to generate exogenous variation in factor prices.[7]

The closest antecedent to our paper is Cappelli and Chauvin (1991), which shows that higher outside wages increase shirking (as measured by the dismissal rate) in a U.S. auto manufacturer. As in our paper, the authors exploit the fact that the union contract stipulates the same pay rates across diverse metropolitan areas. Our paper is very complementary with Cappelli and Chauvin's, but in addition to studying a different country, industry, and time period, we go beyond their research in two major ways. First, we show that the effect of the outside wage is heterogeneous across regions in such a way that the aggregate death rate rises as a result of the regulation. We present a simple cost-benefit analysis that suggests that the regulation reduces welfare. In general, it is more likely that the government-imposed pay regulation of the type we analyze will be suboptimal compared to a voluntary agreement between two parties (the auto company and the union in their paper). Second, Cappelli and Chauvin have a cross section of 78 plants, whereas we have a much larger panel of hospitals. We are therefore able to control for plant- and area-specific fixed effects that could bias a purely cross-sectional analysis.

We also connect to a second literature in industrial organization on productivity dispersion. We document large differences in performance across hospitals, just as has been observed for firms in other sectors (e.g., Foster, Haltiwanger, and Syverson 2008). We argue that one reason

[7] Cawley, Grabowski, and Hirth (2006) find that higher outside wages are associated with worse health outcomes in U.S. nursing homes. In their paper, the mechanism is that stronger external labor markets lead to higher inside wages and, therefore, a substitution away from nursing care toward labor-saving medical interventions. In our paper, by contrast, we show that even when inside wages are held fixed, higher outside wages reduce hospital quality. In the United States, where nurse wages are not regulated, increasing factor prices move hospitals up the labor demand curve rather than the labor supply curve.

for this heterogeneity in the U.K. context is the effect of regulated wages on outcomes.

Finally, our study relates to the literature on the impact of local economic conditions on health. Such studies focus on how economic conditions affect the demand for health by changing people's wealth or stress levels. For example, Ruhm (2006) argues that there are a greater number of heart attacks during upturns in the business cycle.[8] Our paper suggests an alternative mechanism that operates through the supply side. In our model, labor market conditions, combined with rigid national pay setting, affect the supply of skilled nurses, which, in turn, affects health outcomes.

The paper is structured as follows. In Section I we discuss the institutional background and the nature of the research design. We discuss the economic model in Section II, the empirical strategy in Section III, and the data in Section IV. Section V presents the main econometric results, and some extensions and robustness tests are discussed in Section VI. Finally, Section VII offers concluding comments.

I. Institutional Background and the Research Design

In the United Kingdom, health care is free at the point of use and is provided through the NHS, a state monopoly provider.[9] Just over 1.2 million workers are employed in the NHS, and the wages and conditions of clinical staff are strictly regulated. Nurse pay is regulated to a precise national scale that has little differentiation over the country, despite a wide variation in regional labor markets. Since 1984 these pay scales have been set by the Review Body for Nursing Staff, Midwives and Professions Allied to Medicine.[10] Each year, the review body takes evidence from the Department of Health, the main labor unions, and other interested parties before making a recommendation on changes to the level and structure of pay. The government makes the final decision about whether or not to follow their recommendations (it generally implements them in full). For other hospital employees (such as managers, health care assistants, and porters) there is no pay review body,

[8] We explore the implications of any association between cyclical upturns and community health in Sec. VI.

[9] There is a small privately funded sector, which specializes in the provision of nonacute services for which there are long NHS waiting lists.

[10] This also covers other professionals such as radiologists and physiotherapists. Physicians are covered by the Review Body for Doctors and Dentists, which operates in the same way. We focus on nurses since the labor market for physicians is basically a national one, as the NHS operates a career track for physicians that ends with a lifetime appointment to a single NHS hospital, but is preceded by movement between hospitals and regions in a national context, which reflects the national provision of training facilities for physicians.

and employers have more discretion over setting pay in response to local conditions.

Under these national scales the same terms and conditions apply across the United Kingdom, and they allow only minor differences in pay between different areas. Additional allowances are paid to those who work in London and contiguous areas, but these are small relative to the differences in the external labor market. These allowances are up to 11 percent higher in the highest-cost area of Inner London compared to the low-cost areas, whereas the raw outside wage differential is closer to 40 percent (see App. A). Beyond these regional allowances, hospitals have little scope for aligning the pay of nurses to local labor market conditions. Pay scales are short and offer very little scope for either appointing new hires at different points on the scale or accelerating workers up to higher grades. The centralized pay-setting arrangements do not allow pay to be easily adjusted to address staff shortages in local markets.

From an econometric perspective, this institutional setting is attractive because it enables an examination of the impact of different local wages on hospital outcomes. In most labor markets, changes in equilibrium wages will be the outcome of demand and supply shocks, so identifying their impact on hospital outcomes is difficult since the labor price is endogenous to the unobserved shocks. In the U.K. case the wage inside the hospital (which we will call the "inside wage") is held broadly fixed as outside shocks change skill prices (which we will call the "outside wage") in the local labor market. There is therefore a wedge between the worker's inside wage and the outside wage. Consequently, variation in the outside wage can be used to analyze the effects of labor markets on hospital performance.

In principle, the regulated wage could be set above the competitive wage, so it acts as a minimum wage and thus employers shed staff. However, in the period studied here there were chronic shortages of nurses, and nurse unemployment has been close to zero (e.g., Finlayson et al. 2002). Therefore, it is more likely that the regulated wage is being generally set below the competitive wage.[11]

Can employers effectively avoid the pay regulation? To some extent they can substitute tasks away from qualified nurses to either unqualified workers (labor-labor substitution) or medical devices (capital-labor substitution). Yet this is limited by regulation since most nurse tasks cannot be legally performed by unqualified employees and much of health care

[11] In the absence of pay regulation, large local hospitals may have monopsony power, so the equilibrium wage will not be at the intersection of the labor demand and supply curves. But as long as the regulated wage lies below the unregulated monopsony wage, the constraint will still be binding on some employers and the mechanism we identify will operate.

requires human services. Second, employers could offer various nonpecuniary benefits such as better working conditions in the high outside wage areas. These strategies are limited by clinical unions' power in pushing for homogeneous national conditions, and governments have been reluctant to challenge this.[12] Ultimately, it is an empirical question: if the parties could fully contract around the regulation, we would not be able to identify any negative effects in the areas where the regulation would ex ante be expected to have its largest effects.

II. A Model of the Effects of Pay Regulation in the Nursing Labor Market

A. *Introduction*

The idea of this simple model is to capture the salient features of pay regulation in the nursing labor market and how this affects productivity. The basic premise is that nurses will respond to the wages they are offered. If the pay regulation causes wages to fall, nurses are likely to geographically migrate to where wages are relatively higher. The hospitals in areas they migrate to will benefit in performance through having access to higher-quality human capital. The regulated wage will make the high-price region (we will generally refer to this as the "high outside wage" region since nominal wages are higher in the absence of regulation) less attractive to nurses relative to the low-price region. Consequently, some skilled nurses move to the low-price region, and relative productivity in the high-price region deteriorates.

In addition, even if they remain in the same region, nurses can move into sectors where their pay is not regulated. We show this in the simplest context of a two-sector model and demonstrate how this will tend to cause productivity to rise in the unregulated sector as skilled workers switch toward this sector. A third result is that for some plausible values of the regulated wage, we may observe "convexity" in the sense that the deterioration in the quality of public health care in the high outside wage region is greater than the (possible) increase in quality in the low outside wage region. Thus the regulation can decrease aggregate productivity in the public health care system.

[12] The desire for nominal wage equality across workers in different geographical areas has long been a mainstay of union activity. It is not obvious why this should be the case, as real wages within the NHS are made more unequal since the cost of living varies by area. However, if unions represent the view of the median worker, as in the model of Grossman (1983), this worker may be better off with a more compressed wage policy.

B. Basic Setup

Consider an economy with two sectors, $j = \{1, 2\}$, where sector 1 is the "skill-sensitive sector," and we will assume that this is the public sector/NHS. Sector 2 is less sensitive to skill (empirically this is plausible since we will be using the nursing home sector, which is much more low-tech than the hospital sector). There are two regions, $r = \{L, H\}$, where L is the low-price region and H is the high-price region. Prices are P_r and $P_H > P_L$ (e.g., because of ground rental prices due to land scarcity). We refer to region H (L) as the "high (low) outside wage" region because nominal wages are higher in the unregulated sector 2. There are two skill types, $s = \{S, U\}$, where S is skilled and U is unskilled, and with nominal wages W_{js}. We first analyze the effects of national pay regulation on skilled workers in sector 1.[13]

We assume that consumers in region r need to be serviced by hospitals located in region r and that producer prices and consumption prices are equal within a region. Unskilled workers' wages are fixed in the world market, and there is an infinitely elastic supply of such workers.

1. Labor Demand

To keep things simple, we assume that the production function exhibits constant marginal revenue products (MRP) of nursing skill groups (we abstract away from other inputs):

$$Q_j = a_j S_j + bU_j.$$

We have assumed that sector 1 is the more skill sensitive, so $a_1 > a_2$. Since skilled workers are more productive than unskilled workers, $a_1 > a_2 > b$. Because workers are perfect substitutes, skilled labor will be allocated in a "bang-bang" solution with all skilled workers working in sector 1 or all workers in sector 2. Workers will work where the offer wage is higher, and the offer wage will depend on the MRP of labor in each sector and the regulatory regime in sector 1 (sector 2 is always unregulated). Since productivity (or service quality per worker) is increasing in the hospital's share of skilled workers, in order to analyze productivity we simply have to keep track of the equilibrium allocation of skilled workers.

[13] As noted above, less skilled nursing staff (e.g., health care assistants) do not have their pay nationally regulated.

2. Labor Supply

There are a fixed number of $\bar{S}$ skilled nurses in the economy, and all are employed.[14] Nurses have heterogeneous preferences over an amenity that is higher in the high outside wage region. We denote the intensity of this preference x_i, which has a density function $f(x_i)$ and a distribution function $F(x_i)$.[15] Utility of worker i in the high outside wage region is $x_i + (W_{1H}/P_H)$ and W_{1L}/P_L in the low outside wage region. Thus, a skilled worker who is going to work in sector 1 will supply her labor to the high outside wage region if the index $v_i > 0$, where

$$v_i = \frac{W_{1H}}{P_H} - \frac{W_{1L}}{P_L} + x_i;$$

that is, a nurse will work where the relative real wage (adjusted for idiosyncratic tastes) is higher. Aggregate labor supply to the high outside wage region is therefore

$$L_H = F(\omega)\bar{S}; \quad \omega = \frac{W_{1H}}{P_H} - \frac{W_{1L}}{P_L}.$$

For this group of workers it is obvious that an increase in the real wage in the high outside wage region (compared to the low outside wage region) will increase labor supply in the high outside wage region because $F'(\omega) > 0$. The elasticity of the regional supply curve will depend on the distribution of tastes for the regional amenity: labor supply will be less elastic if tastes are more heterogeneous.

C. *Unregulated Equilibrium*

Assume that markets are competitive so that in the absence of regulation workers are paid their MRP. Since the MRP of skilled workers is higher in sector 1 than in sector 2, all skilled nurses will work in sector 1 and sector 2 will rely solely on unskilled workers. The equilibrium wage will be $W^*_{1H} = \text{MRP}_{1H} = P_H a_1$ in the high outside wage region and $W^*_{1L} = \text{MRP}_{1L} = P_L a_1$ in the low outside wage region. Of course nominal wages are higher in the high outside wage region, $W^*_{1H} > W^*_{1L}$, since $P_H > P_L$.

[14] In the long run, the regulated wage will affect the decision to train as a nurse. This typically takes many years, however, and the empirical analysis focuses on short-term fluctuations. The long-run effect will depend on the degree to which the average nurse wage (in regulated and unregulated sectors) is below the average nurse wage prior to the regulation. Below we show that, on average, private- and public-sector wages are similar, so these occupational choice effects are not as great.

[15] More generally, one could view the taste parameter, x_i, as reflecting mobility costs due to child care, schools, housing, family networks, etc. Since the majority of nurses are women who are more likely to have child care responsibilities, these are likely to be very important (e.g., Shields 2004).

D. Regulated Equilibrium

The form of the national regulation is that firms in sector 1 are forced to pay the same nominal wage ($\overline{W}$) to skilled workers in all regions. We consider introducing this regulation to an initially unregulated economy where the regulated wage is (weakly) less than the initial unregulated wage, $\overline{W} \leq P_L a_1$.[16] We obtain three results.

RESULT 1. After regulation, in sector 1 (the regulated sector) productivity decreases in the high outside wage region.

When regulated wages are imposed, the change in the sector 1 wage (which is negative) is $\overline{W} - P_H a_1$ in the high outside wage region, which is greater in absolute terms than the wage change in the low outside wage region, $\overline{W} - P_L a_1$. Consequently, the number of skilled workers in the regulated sector falls in the high outside wage region, which reduces productivity.[17]

There are two mobility effects of regulation: migration across regions and movement across sectors. First, since sector 1 wage rates in the low outside wage region now look relatively more attractive than in the high outside wage region, some nurses will migrate (even if wages fall in region L, they fall by less than in region H). Formally, ω has fallen, and since $F'(\omega) > 0$, some margin of workers will move to another region. Second, there will also be mobility toward the unregulated sector if the regulated wage falls below the unregulated wage (detailed next). As this is more likely to happen in the high outside wage region, this will also reduce the number of skilled workers in sector 1 and therefore lower productivity in the high outside wage region.

RESULT 2. After regulation, productivity is nondecreasing in sector 2 (the unregulated sector) in the high outside wage region.

To see this, note that since $\overline{W} \leq P_L a_1$, there are three possible cases (ignoring equalities):

i. If $P_H a_2 < \overline{W}$, then the regulated wage is above the sector 2 wage in all regions, so skilled workers continue to work in the regulated sector in all regions.
ii. If $\overline{W} < P_L a_2$, then the regulated wage is below the sector 2 wage in all regions, so all skilled workers move to sector 2 in all regions.

[16] From the definition of the two regions, $P_L a_1 < P_H a_1$, so the regulated wage is strictly less than the initial unregulated wage in the high outside wage region. If the regulated wage was above the MRP, the model would be the same as the standard neoclassical analysis of the effects of a minimum wage in a two-sector model. We argued above that near zero nurse unemployment made this model less plausible than the model presented here.

[17] Productivity may rise or fall in the low outside wage region, but if it falls, it goes down by less than in the high outside wage region. Thus relative productivity always falls in the high outside wage region.

iii. If $P_L a_2 < \overline{W} < P_H a_2$, then the regulated wage is above the sector 2 wage in the low outside wage region but below the sector 2 wage in the high outside wage region, so skilled workers remaining in the high outside wage region all switch to sector 2.

In all cases, productivity in sector 2 is never decreasing and may increase either in both regions (case ii) or just in the high outside wage region (case iii).

RESULT 3. After regulation, if $P_L a_2 < \overline{W} < P_H a_2$ (case iii above), then productivity falls in the regulated sector in the high outside wage region and does not necessarily rise in the regulated sector in the low outside wage region.

The intuition behind this result is that if the regulated wage lies between the sector 2 wage in the two regions ($P_L a_2 < \overline{W} < P_H a_2$), skilled workers will continue working for the regulated sector 1 in the low outside wage region but will switch to working to the unregulated sector 2 in the high outside wage region. This generates a kind of "convexification" of the effects of pay regulation on hospital quality. In the high outside wage region, consumers of public health care lose out from a sectoral shift and a geographical migration of the skilled, so the cost of the regulation for these consumers is severe. In the extreme case in which there is no labor mobility due to strong area-based taste preferences, the quality of public hospital services will decline in the high outside wage region (as workers switch to unregulated sector 2) and not improve at all in the low outside wage region.

E. *Discussion*

We have shown that a simple model can generate three testable results. The effect of a regulated wage is

i. to reduce service quality in the regulated sector where the regional outside wage is high,
ii. to increase service quality in the unregulated sector when the regional outside wage is high, and
iii. to cause an aggregate loss of service quality for some values of the regulated wage.

We have not specified the hospital's objective function other than to assume cost minimization in order to allow for more general classes of behavior than profit maximization. There are, of course, many extensions that could be made to this model. For example, we could allow for a sector mobility cost, and the presence of the effects we focus on would depend on the relative magnitude of the costs of moving between sectors.[18] Other outcomes are also possible depending on the exact value of the regulated wage,[19] so ultimately the question of the effects of the regulation is an empirical question. In our application we will find that there is evidence for all three implications of this simple model.

III. Empirical Strategy

In terms of identification the ideal experiment is a "surprise" introduction of a nationally regulated wage. We would then observe behavior in regions characterized by different ex ante wage levels. Since the introduction of such national wages dates back to the early 1980s prior to the availability of hospital quality data, we implement a different identification strategy that exploits the fact that different geographical areas are subject to shocks to the "outside wage" for reasons that are exogenous to performance in the hospital sector. Our model showed how the effects of the regulation will be different depending on the area's outside regional wage. Empirically, we examine how changes in the outside wage (and therefore the "bite" of the regulation) affect hospital performance in different geographic areas.

A. *Basic Equation*

Our measure of quality, Y_{it}, is the death rate following emergency admission for heart attacks (AMI) by individuals 55 years and over for hospital i at time t. Consider the equation

$$\ln Y_{it} = \phi \ln W_{it}^{O} + \delta \ln W_{it} + Z_{it}\theta + \eta_i + \tau_t + \nu_{it}, \quad (1)$$

where W_{it}^{O} is the outside wage; W_{it} is the inside wage; Z_{it} is a vector of controls that includes AMI-specific controls for case mix, hospital size, and other factors; η_i is a hospital-specific effect; τ_t are a set of time dummies; and ν_{it} is a stochastic error term whose properties we discuss below. We present results treating η_i as a fixed effect (e.g., long-differenced results and generalized method of moments [GMM] estimators) and also results treating η_i as uncorrelated with the right-hand-side variables (i.e., standard ordinary least squares [OLS]).

We construct various measures of the outside wage ($\ln W_{it}^{O}$) based on private-sector wages in the local labor market around the hospital (see

[18] Sectoral mobility costs may depend on the preference a worker has for the "mission" of the public hospital (Besley and Ghatak 2005).

[19] For example, if the regulated wage is set at a very high level above the competitive nominal wage in the high-price region, it would act as a neoclassical minimum wage. In this case employers in sector 1 would use only unskilled workers in the low outside wage region as well as in the high outside wage region, and quality would decline in both regions.

the data section for an extensive discussion). Since hospitals are a small part of the local labor market, we treat the outside wage as exogenous, although we lag the variable by a year to avoid any immediate feedback effects from transient area-level shocks (permanent shocks are picked up by hospital fixed effects). Identification of the coefficient on the inside wage is more challenging. We observe the hospital inside wage, but higher wages may simply reflect more nurses in higher pay grades. Thus, finding a positive coefficient on the average inside wage could reflect the better performance of hospitals with higher average human capital. Consequently, our main results do not condition on the inside wage information under the assumption that the inside wage is truly national and so absorbed by the time dummies, τ_t. We include the inside wage as a robustness check in Section VI.

B. *Heterogeneity in the Effect of Outside Wages by Region*

Equation (1) assumes that the coefficient on the outside wage is the same across all regions. But the theoretical analysis suggested that in some cases the effects on the high-price regions may be greater than the effects on the low-price regions (as many skilled nurses may simply switch into another sector such as nursing homes, which do not have these pay regulations). Consequently, we will estimate models in which we allow ϕ to be different between regions to test whether there is "convexity." If the negative effects on quality are much larger in the high outside wage regions as suggested by result 3 in the previous section, we will find that the regulation increases aggregate deaths.

C. *Performance in Nursing Homes*

The theory also suggested that there should be equilibrium effects of regulation on the performance of other sectors. We use nursing homes for senior citizens as our alternative sector because it employs many nurses and their wages are not regulated by the pay review bodies, so it seems a good comparator. Unfortunately, clinical outcomes are not publicly available for nursing homes, so we have to rely on alternative performance measures (essentially measures of the volume of activity per employee). If our model is correct, we would expect outside wages to have a positive effect on these performance measures for nursing homes (result 2 in the previous section).

D. *Selection Issues*

A concern with equation (1) is that there may be unobservable patient characteristics correlated with the outside wage that make it more likely for AMI death rates to be greater in high outside wage areas. Although this could in principle be a problem, we argue that our research design and robustness tests indicate that selection is not contaminating our results.

1. Research Design

Section I argued that our research design makes selection much less than it would in a study on U.S. data. Our outcome is in-hospital death rates from emergency AMI admissions. Incentives for patients to select hospitals were very limited in the time period we study. Treatment in the NHS is free at the point of use, so quality is the only dimension patients would make a choice on, but there was almost no public information on hospital quality until after 2001.[20] So for much of the period we study, patients (and their doctors and buyers of health care) had almost no quality information on which to choose hospitals. In addition, patient and buyer choice of hospital was not encouraged: competition between hospitals for patients was a policy that operated in the mid-1990s but was abolished in 1997 and not reinstated again until 2006 (Appleby and Dixon 2004). There is no private-sector hospital care for emergency AMI in the United Kingdom. Finally, patients having heart attacks are not in a position to make a choice of hospital (Volpp et al. 2003), which is why emergency AMI is a good marker of hospital quality.

Any incentives to select will therefore come from the hospital side. But during this period incentives for hospitals to choose patients were also weak since hospitals were not monitored on their clinical performance. So on performance grounds they had little incentive to refuse potentially sicker patients. Hospitals were not paid per patient, but received a block budget for emergency care. So in theory they had greater financial incentives to turn away high-cost patients than in a system in which reimbursement rates are (at least to some extent) adjusted for severity (e.g., the U.S. Diagnostic Related Group [DRG] arrangements). However, the fact that hospitals were not paid per patient and all treatment was (and remains) free meant that the financial systems in NHS hospitals were not set up to identify high-cost patients.[21] We conclude that, in practice, incentives for hospitals to select or to engage in dif-

[20] There were no published indicators of quality available until 1999, when six were published. More were made available from 2001.

[21] There are large fixed costs with introducing such a patient monitoring system, and in the absence of strong incentives, hospitals did not use them. As evidence of this, when a cost per patient reimbursement system was developed in England in 2004, 2 years had to be allowed between its introduction and full rollout.

ferential treatment of sicker patients were very weak in the period we study.

2. Controls for Patient Quality

Although we believe that our research design makes selection issues less of a concern, to be cautious we introduce a large number of controls for patient comorbidity. First, we include in all regressions controls for case mix: the age and gender composition of emergency AMI admissions in a particular hospital in a particular year. Second, we condition on mortality rates in the local area, which should pick up other unobservable influences on death rates from ill health. Third, we present estimates controlling for unobservable hospital fixed effects. Finally, in robustness checks we also condition on further information on the severity of AMI admissions.[22]

3. Business Cycle Effects on Demand for Health

Recently it has been suggested that population heart attack fatalities are positively correlated with the business cycle. For example, Ruhm (2006) argues that there is a greater number of heart attacks during "good times" using U.S. state-level unemployment rates. It is therefore possible that the impact of high outside wages that we find is not due to the result of poor hospital quality, but to increases in fatalities due to the business cycle. There are at least two reasons why our results seem unlikely to be driven by this effect. First, the group that appears to suffer most from falls in unemployment during cyclical upturns is those aged 20–44 (Ruhm 2006). We examine fatalities of persons aged 55 and over. Second, any relationship between business cycles and population health is likely to be affected by institutions (Gerdtham and Ruhm 2002; Ruhm 2006). The United Kingdom has a more generous welfare state than the United States, which may limit the effect of upswings on heart attack fatalities. One mechanism is the impact of increased hours and reduced leisure, which it is argued leads to less investment in health by younger workers and less time to care for older persons. Hours are significantly less associated with the cycle in the United Kingdom than in the United States (Gali 2005), so there may be less cyclicality of health investment.[23]

We therefore consider it unlikely that a positive association between high outside wages and heart attack fatalities in the population outside

[22] We do not use this in our main analyses to avoid possible endogeneity of severity because poor hospital quality could itself be a reason for the additional severity.

[23] The indirect caring effect is also likely to be mitigated by the U.K. social care system that provides subsidized care for older persons and acts as a substitute for care from relatives.

the hospital drives our results. However, as noted above, we use time-varying controls for local population health to control for any impact of the cycle on the health of those admitted to hospitals. We also undertake an extensive series of robustness checks to attempt to establish that a negative relationship between wages and population health is not the source of our results.

4. Selection prior to Hospital Arrival or after Hospital Exit

Data constraints mean that we do not observe deaths prior to being admitted to the hospital nor after 30 days in the hospital or after leaving the hospital. Lack of data after hospital discharge is unlikely to be a problem, as we discuss in detail in Appendix B. In short, 98 percent of AMI deaths occur within 30 days of being admitted to the hospital, and over half of deaths occur within the first 48 hours. The time between heart attack and hospital arrival ("floor to door") is in principle important, and we examine whether there is a relationship between this and outside wages in robustness tests.

IV. Data

We have built an original, very rich, database with "plant-level" panel information on hospital quality and inputs such as patient case mix. This was compiled from a large number of mainly administrative data sources that we discuss briefly here and in more detail in Appendix A and tables A1–A6.

A. Basic Information and Sample Selection

We construct a panel data set of NHS hospitals (called "trusts" in the United Kingdom[24]) covering the financial years 1997/98–2005/6.[25] We examine only short-term general hospitals, which we refer to as acute hospitals, following the U.K. convention. Nonacute hospitals are very heterogeneous (e.g., mental health and community hospitals) and generally do not provide emergency AMI treatment.

Our sample selection criteria are laid out in table A2 (see also App. A). Column 2 shows that the population of acute hospitals falls from

[24] An NHS "trust" is a financial, managerial, and administrative unit and may contain more than one hospital site. It is appropriate to think of a hospital as a firm that may be single plant or multiplant. We use the term "hospital" rather than "hospital trust" for expositional convenience.

[25] We refer to a financial year by its first calendar year of coverage.

219 in 1997 to 166 in 2005 (primarily as a result of mergers).[26] The main selection rule is to select hospitals with at least 150 AMI admissions to avoid the well-known problem (e.g., Kessler and McClellan 2000) of variability of rates from small denominators (we examine this restriction in robustness tests). Second, we omit hospitals that were designated as "foundation trusts." Foundation trust status was an experimental policy, rolled out gradually from 2004 onward, that allowed greater freedom from central directives. To avoid potential contamination from this new policy introduced at the end of our sample period, we omit observations for these hospitals once they were given foundation trust status.[27] We are left with a sample of 209 hospitals for the pooled OLS-level sample and 149 for the 3-year long-differenced sample covering a 9-year period.

To test whether exit is correlated with outside wages, we investigate the association between the probability of a hospital exiting our sample and outside wages. Using all hospitals with over 150 AMI admissions as the initial sample, table A3 shows that exit is not associated with the contemporaneous or the lagged level of outside wages (cols. 1 and 2). Nor is it associated with the 3-year growth rates in outside wages, either lagged 1 year (col. 3), contemporaneously dated (col. 4), or in future growth rates (cols. 5, 6, and 7). We take from this that the selection made necessary by reconfigurations and missing data does not result in a biased sample.[28]

B. *Measures of Hospital Quality and Case Mix*

Hospital quality data are derived from Hospital Episode Statistics (HES) data, which are administrative data on every NHS hospital episode such as an operation or physician consultation. Our measure of quality is in-hospital deaths within 30 days of admission for emergency AMI for patients aged 55 or over. AMI was chosen for several reasons. First, it is a common condition, and the infrastructure used to treat AMI is common to other hospital services, making it a good general marker

[26] The Department of Health undertook reconfiguration of hospitals in the late 1990s and early 2000s, with the aim of gaining scale economies by bringing together closely located existing hospitals under single management. When this occurred, hospitals were treated as new entities following the reconfiguration and the old hospitals disappeared from the data. We experimented with creating pseudo-entities by joining up past observations on hospitals that were subsequently merged, which gave results qualitatively similar to the ones reported here.

[27] In the robustness section we show that our results are robust to inclusion of these observations and to the exclusion of hospitals that subsequently became foundation trusts.

[28] We also investigated the relationship between outside wages and attrition within our OLS sample (col. 7, table A2) and within the sample with any positive AMI admissions (col. 3, table A2) and found no significant associations.

of hospital quality (Gaynor 2004).[29] Second, all patients with a recognized AMI are admitted, so there is little scope for selection bias to affect the decision of who gets admitted (see Sec. III.D). Third, the quality of hospital care has been established to have an important effect on survival rates, so there is ample scope for hospitals to affect outcomes (e.g., Volpp et al. 2003). As an indication, deaths following emergency admission for AMI have been published by both the U.S. and U.K. governments as indicators of hospital quality. McClellan and Staiger (2000) argue that measures of AMI death rates correlate well with other measures of quality. Fourth, variants of this measure have been used widely in studies of hospital quality (starting with Kessler and McClellan [2000]).

To allow for differences in case mix, we include three sets of controls. First, we generally control for unobserved hospital fixed effects. Second, we control for all-cause time-varying mortality of the catchment area of the hospital, which will pick up the degree of ill health of the population from which the hospital draws its cases.[30] Third, we control for the age-gender distribution of admissions for emergency AMI: the proportion of emergency AMI admissions in 5-year age bands separately for men and women (14 variables). Fourth, in robustness tests we control for more detailed AMI case mix measures based on the severity of the heart attack. Of course, there may remain some time-varying, within-area, unobservable that increases AMI death rates that is not captured by area mortality rates or the other observables. However, changes in this unobservable would have to be systematically positively correlated with changes in outside wages in order to bias our results.

In our analysis of nursing homes, similar measures of quality are not publicly available, so we examine a measure of productivity: total "output" (measured as either occupied beds or deflated revenues) per worker.

C. *Wages*

We examine several measures of outside wages. Our basic measures are derived from the Annual Survey of Hours and Earnings (ASHE), which is a 1 percent sample of all employees in Great Britain covering about

[29] Many of the actions to reduce deaths from emergency admissions for AMI need to be taken soon after an attack, and so the performance of a hospital in terms of AMI reflects the performance of its accident and emergency department. Around half the patients admitted to an acute English hospital are admitted through the accident and emergency department.

[30] Constructed from data on 354 local authorities and standardized for age and gender.

300,000 workers a year.[31] It is a mandatory administrative panel data set provided by employers to the Department of Work and Pensions and contains information on earnings and hours. We use the area code (these are for unitary authorities, metropolitan councils, or London boroughs, referred to as "authorities" below) to construct travel to work areas around each hospital in our sample. Using the zip codes (postcodes) of the headquarters of the authorities, we matched each NHS hospital to all authorities that fell within a 20-kilometer radius from the hospital. The local area wage is constructed as the average of the wage for all authorities that fell into this radius. Where no authorities fell within the 20-kilometer radius, the wage applicable to the nearest authority was used. Because ASHE has only limited personal characteristics for workers, we supplement the information in ASHE with the Labor Force Survey (LFS), which is a self-reported individual-level survey similar in structure to the U.S. Current Population Survey. The LFS has details on the characteristics of workers such as age, schooling, gender, and occupation but has wage information for only a subsample of these workers (in the first quarter they enter the LFS and five quarters later when they leave). Thus, we tend to use the ASHE administrative data for wages and the LFS for worker characteristics. Full details are in Appendix A.

We use several measures of the outside wage. One relatively sophisticated method that we used is to create area- and time-specific outside wages for nurses using the observed characteristics (age, gender, years of schooling, etc.) of nurses in a particular area-year cell (we do not observe these characteristics at the hospital level). Consider the private-sector wage equation for women who are not nurses:

$$w_{jrt} \equiv \ln W_{jrt} = \beta_{rt} x_{jrt} + \mu_{rt} + \varepsilon_{jrt}, \qquad (2)$$

where W_{jrt} is the wage of worker j in area r at time t. The vector x_{jrt} is the personal worker characteristics, μ_{rt} are the area time shocks, and ε_{jrt} is an error term. We run these individual wage regressions and then plug in the nurse characteristics in each area in each year to generate a counterfactual ln(wage), $\hat{w}_{jrt}$, for each nurse. We take the average of the $\hat{w}_{jrt}$ in each local area to construct the outside wage. Examination of this series shows that the difference in characteristics between female nurses and women working in other sectors does not vary greatly over time in an area. Thus, the main cause of area-specific time-series changes (which is what we use in our preferred specifications) in outside wages is simply the growth in the nonmanual female wage in an area rather

[31] Before 2001 this was called the New Earnings Survey (NES). All workers whose Social Security numbers end in the same two digits (which are randomly assigned to all workers) are included.

than changes in observables (or the price of these observables) over time. Consequently, our baseline results simply use the growth of the nonmanual female wage in the area, but we are careful to show the robustness of these results to the characteristic-adjusted outside wages.

As a third alternative outside wage we consider the wages of nurses working in nursing homes (who are not covered by the pay regulation). This group is more finely matched by occupation, but nursing home wages are more likely to be endogenous since the equilibrium wage in this sector may be affected by the (much larger) public nursing sector.

Finally, we also run placebo tests replacing female wages with male wages in the local labor market. If our model is correct, we would not expect the male series to be as closely linked to hospital performance as female wages since few men work as nurses.

Each hospital has its own outside wage, but given their construction, wages at the hospital level may not be independent across areas. We allow for this by clustering standard errors at the area (local authority) level. In a robustness test we also cluster at the higher level of aggregation (regional).

Other controls are derived from a variety of sources and are detailed in Appendix A. Table 1 presents the means, standard deviations, minimum and maximum, and the within and between variation for the variables used in the regression analysis for our sample of hospitals. The average hospital has just over 2,000 workers and has 447 emergency AMI admissions a year. About 18.3 percent of those admitted die, but there is wide variance—from 3.7 percent in the best hospital to 37.1 percent in the worst. A little under half of the variation in AMI death rates is between hospitals, indicating long-term differences between hospitals in death rates. However, as our primary identification strategy relies on exploitation of time-series variation within hospitals over time, it is also reassuring that over half of the variation is within a hospital. If we take out common macro shocks, just under 45 percent of the variation in AMI death rates is still within a hospital.

D. *Test of Difference-in-Difference Estimation Strategy*

Our long-difference approach is akin to a difference-in-difference (DD) strategy. Formally, equation (1) would be the same as the DD if we had only two periods (first difference) and two areas (second difference). In our implementation we have multiple periods and multiple areas, which increases our ability to identify the causal effect and allows us to explore heterogeneity of response across different outside wage regions. To test whether our DD assumptions are satisfied, we examine the bivariate relationship between the observed baseline conditions and subsequent 3-year growth in outside wages. Differences in outside wage

TABLE 1
Descriptive Statistics

Variable	Mean	Standard Deviation	Minimum	Maximum
AMI mortality rate (55 plus):				
Overall	18.3	5.4	3.7	37.1
Between		3.9	5.1	31.3
Within		4.1	5.7	33.4
Total AMI admissions (55 plus):				
Overall	446.9	199.4	151.0	1,550.0
Between		204.3	163.0	1,334.6
Within		66.0	231.3	860.3
ln(female full-time nonmanual wage) in area:				
Overall	9.69	.17	9.27	10.16
Between		.15	9.32	10.13
Within		.11	9.39	9.99
ln(male full-time nonmanual wage) in area:				
Overall	10.11	.16	9.63	10.64
Between		.15	9.63	10.54
Within		.10	9.84	10.40
Total clinical staff in hospital:				
Overall	2,019	1,045	343	8,333
Between		1,087	343	7,848
Within		186	1,262	3,198
Physicians as a percentage of total clinical staff:				
Overall	15.5	2.9	5.8	24.0
Between		2.7	7.4	22.5
Within		1.5	7.5	21.4
Qualified nurses as a percentage of total clinical staff:				
Overall	58.7	3.6	47.3	71.7
Between		3.3	50.3	70.3
Within		1.8	52.6	69.3
Retained surplus (£1,000):				
Overall	−.9	3.7	−44.6	13.6
Between		2.3	−17.7	1.3
Within		3.2	−37.7	18.1
Area directly standardized mortality rate area (per 100,000):				
Overall	688.2	78.3	510.5	944.4
Between		72.5	520.1	864.1
Within		34.2	599.1	783.9
Teaching trusts as a percentage of all observations:				
Overall	13.6	34.3	.0	100.0
Percentage of AMI patients admitted with complications (HRG E11):				
Overall	19.0	8.4	2.4	57.1
Between		7.2	4.7	42.4
Within		4.8	4.1	42.4

TABLE 1
(*Continued*)

Variable	Mean	Standard Deviation	Minimum	Maximum
Percentage of urgent ambulance call-outs not more than 15 minutes late:				
Overall	80.7	13.4	44.8	98.3
Between		11.3	50.7	98.3
Within		7.4	45.6	102.9

NOTE.—Unless otherwise stated, data are for 1,164 observations in 209 acute hospitals between 1997 and 2005. We also use, as measures of case mix, admissions within 5-year age-gender bands for emergency AMI (age 55+). Staffing variables refer to whole time equivalent clinical staff.

growth associated with these baseline conditions may indicate that areas that differ in terms of wage growth also differ in terms of unobserved factors. The bivariate associations between these baseline conditions—the controls used in the main analyses and in the robustness checks—and the subsequent 3-year growth rates in outside wages are presented in columns 1–8 of table 2. In no cases were any of the baseline conditions significantly associated with subsequent wage growth.[32] In columns 9–11 we also check for any association between the past level of outside wages, the past level of AMI, and the lagged growth in AMI and subsequent growth rates in wages. The results show that none of these were significantly associated with subsequent wage growth. We conclude that our DD assumptions are likely to be satisfied.

E. Preliminary Data Description

Figure 1 presents the distribution of AMI deaths between 1997 and 2005. The most striking feature is the large variation of death rates at any point in time between different hospitals. For example, in 1997 the death rate is 16 percent in the bottom quintile and 28 percent in the top quintile. Some of this variation can be accounted for by case mix, but there remains much residual variation that is potentially related to the outside wage. Looking at the evolution of the distribution,[33] figure

[32] Column 2 examines the health of the local population, which depends on the local demographics. This suggests that our results are also robust to any possible immigration and migration.

[33] The sharper falls between 2001 and 2002 follow a major government initiative to reduce the incidence of coronary heart disease through the National Service Framework. This framework set new standards and protocols, backed by increased resources and incentives.

TABLE 2

Tests of Association between Outside Wage Growth and Covariates

Dependent Variable: 3-Year Growth Rate of Outside Wage

	Covariate										
	ln(Clinical Employment) (1)	ln(Area Standardized Mortality Rate) (2)	AMI Case Mix (3)	Proportion Physicians in Clinical Staff (4)	Proportion Qualified Nurses in Clinical Staff (5)	Retained Surplus in Hospital (£1,000s) (6)	Proportion Ambulance Callouts Delayed by 15 Minutes or More (7)	Proportion AMI Admissions with Complications (8)	Level ln(Outside Wage) (9)	Lagged Level ln(AMI Mortality) (10)	Lagged 3-Year Growth Rate ln(AMI Mortality) (11)
Coefficient on covariate	.0020	−.0007		−.0115	.0097	−.0002	.0028	−.0001	−.0046	−.0002	−.004
	(.0014)	(.0074)		(.0190)	(.0165)	(.0004)	(.0053)	(.0001)	(.0057)	(.0027)	(.0101)
p-value for joint Wald test of 14 age/sex variables			.715								
Year dummies (6)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of hospitals	149	149	149	149	149	137	149	149	149	149	103
Observations	598	598	598	598	598	486	598	598	598	598	325

Note.—The time period is 1997–2005. Each coefficient is from a separate regression of the 3-year (annualized) long difference in outside wages on covariates lagged 1 year. Column 3 reports the *p*-value of the test for the joint significance of 14 age-gender proportions of AMI admissions. Standard errors in parentheses under the coefficients are robust to arbitrary heteroskedasticity and autocorrelation (clustered at the local area level).

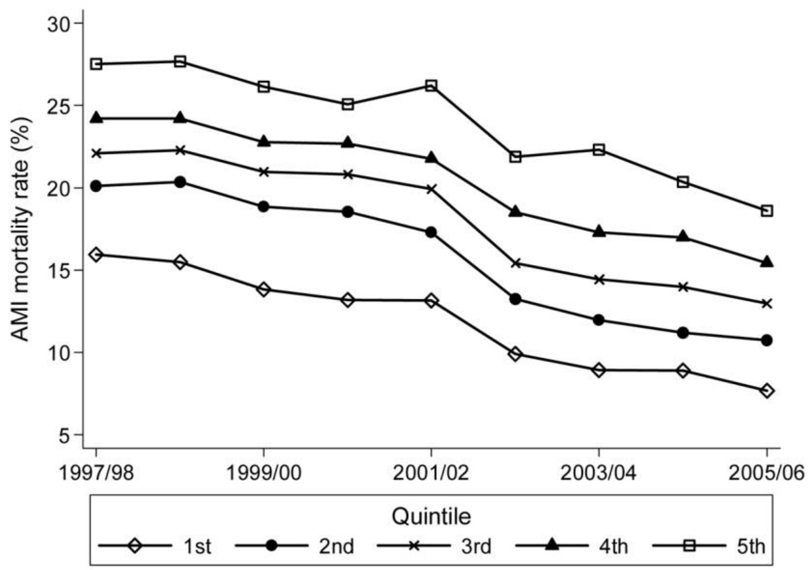

FIG. 1.—AMI mortality rate by quintile. For each cross section of hospitals in each year, the figure notes the quintiles of the distribution of in-hospital deaths within 30 days of emergency admission for AMI for those over age 55. The time period is financial years 1997/98–2005/6.

1 shows a gradual decrease in death rates over time, indicating the long-run trend of a decline in the emergency AMI death rate.[34]

Figure 2 shows the relationship between growth rates in AMI death rates and outside wages at the hospital level. While there is clearly noise in the relationship, the nonparametric estimation of the relationship is positive, indicating that hospitals that experienced highest growth rates in outside wages had lower falls in AMI death rates. High outside wages are characterized by higher death rates from AMI in the cross section and the time-series dimension. There are, of course, many reasons why these figures may be misleading because of omitted variables, so we now turn to more rigorous econometric estimation of the relationship controlling for confounding variables such as case mix and local mortality rates.

[34] In Hall, Propper, and Van Reenen (2008) we show considerable overlap in the spatial distribution of AMI deaths and outside wages. The same paper also shows a positive association between nurse vacancy rates, use of agency staff, and outside wages at the regional level. Elliott et al. (2007) find a positive cross-sectional relationship at the sub-regional level between vacancies and high outside wages. Gosling and Van Reenen (2006) use a long panel of regions between 1984 and 2001, when there were some significant changes in mandated regional differences in NHS wages, to find that a 10 percent fall in nurse relative wages reduces nurse labor supply up to 15 percent.

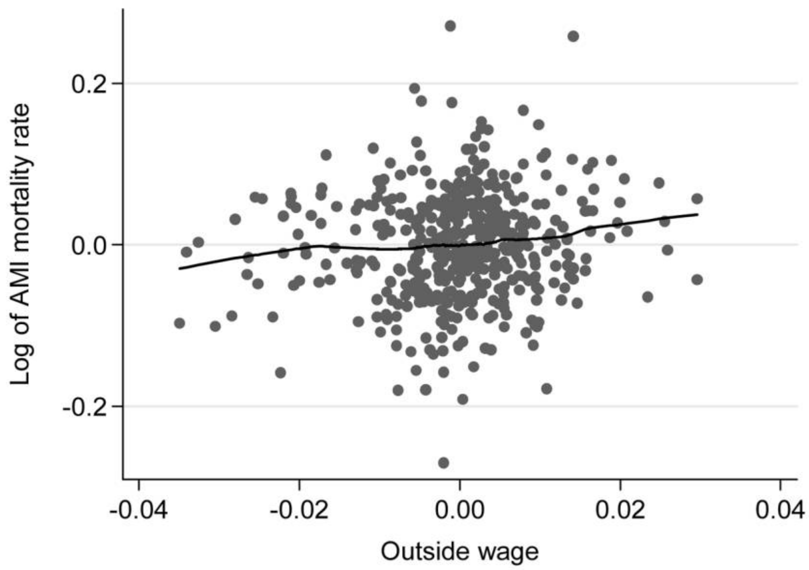

FIG. 2.—Growth rates in outside wages and growth rates of AMI death rates at the hospital level. Each point in the figure represents a hospital. Outside pay is the average ln(wage) of female nonmanual workers in the area. AMI rates are within-hospital deaths within 30 days of emergency admission for AMI for those over age 55 admitted to the hospital with AMI. Variables plotted are the detrended 5-year lagged long differences between financial years 1997/98 and 2005/6. The line is the prediction from a locally weighted regression of AMI mortality on the outside wage.

V. Main Results

We begin by examining the average relationship between hospital quality and outside wages. We then test for "convexity" by examining outcomes across hospitals located in outside labor markets of differential degrees of "tightness." After presenting a simple cost-benefit analysis, we test the same relationship between outside wages and productivity in the unregulated nursing home sector and present a falsification test using male rather than female outside wages.

A. The Impact of Outside Wages on Hospital Quality

Table 3 presents the basic estimates for hospital quality as a function of the outside wage. Column 1 presents the pooled OLS estimates controlling for AMI case mix (admissions in 14 age-gender bands), hospital type (i.e., whether the hospital was a normal acute hospital or a teaching hospital), the local area mortality rates, hospital size (the log of total

TABLE 3
Hospital Quality and the Outside Wage
Dependent Variable: ln(AMI)

Method	OLS Levels (1)	OLS Levels (2)	OLS 3-Year Long Difference (3)	GMM (4)	GMM (5)
ln(outside wage)	.523***	.682**	.743**	.437**	.563**
	(.202)	(.287)	(.352)	(.180)	(.252)
Case mix controls (14)	Yes	Yes	Yes	Yes	Yes
Year dummies (9)	Yes	Yes	Yes	Yes	Yes
Region dummies (10)	Yes	Yes	No	Yes	Yes
SC(1) *p*-value				.000	.000
SC(2) *p*-value				.822	.649
Hansen-Sargan *p*-value				.821	.266
Number of hospitals	209	149	149	209	149
Observations	1,164	598	598	1,164	598

Note.—Quality is measured as ln(AMI), the in-hospital mortality rate within 30 days of emergency admission for AMI in patients aged 55 or over. The outside wage is the (lagged) area outside wage. We control for case mix with the proportion of the admitted population in each 5-year age-gender band from 55 upward. We also control for area mortality rates, ln(lagged number of clinical employees) and hospital type (i.e., whether the acute hospital was a specialist or teaching hospital). The time period is 1997–2005. Standard errors in parentheses under the coefficients are robust to arbitrary heteroskedasticity and autocorrelation (clustered at the local area level). In the system-GMM estimates (one-step robust estimates), we use current employment, which is treated as endogenous. Instruments for the differenced equation are ln(employment) and ln(nurses' inside wage) $t-2$ through $t-5$ and instruments for the levels equations are the lagged changes of these variables. SC(k) is the Arellano-Bond (1991) test of serial correlation of order k (the outside wage is lagged and treated as exogenous). Sargan-Hansen is a test of the validity of the overidentifying restrictions.
* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

clinical employees),[35] and year and regional dummies. We find that higher outside wages are associated with a statistically significant increase in AMI death rates: a 10 percent increase in outside pay is associated with a 5.2 percent increase in mortality. Column 2 conditions on the subsample of observations for which we can estimate in 3-year differences and shows that the relationship is slightly stronger for this subsample. Column 3 presents our preferred specifications (annualized 3-year long differences), so that we identify the effect of outside wages from only the hospital-level time-series variation. The coefficient on the outside wage increases slightly from column 2 to 0.743, remaining significant at the 5 percent level.[36] Columns 4 and 5 contain results from a GMM specification that treats total employment as endogenous (Blundell and Bond 1998). This estimator exploits the "within" information used in column 3 and the "between" information used in columns 1

[35] The results were robust to including other measures of hospital size as extra controls (number of finished consultant episodes, total number of beds, or total admissions). These terms were never significantly different from zero.

[36] The results are very similar for 4- and 5-year long differences, but the coefficients were less precise because of smaller sample sizes. For the 4-year long-differenced estimates, the coefficient (standard error) on the outside was 0.501 (0.335). For the 5-year long-differenced estimates, the coefficient (standard error) was 0.760 (0.428).

TABLE 4
HOSPITAL QUALITY AND THE OUTSIDE WAGE (3-Year Long Differences): ALLOWING THE COEFFICIENT ON OUTSIDE WAGES TO BE HETEROGENEOUS ACROSS REGIONS
Dependent Variable: ln(AMI)

	All Regions (1)	High-Wage Regions Interaction (2)	High-Wage Regions (3)	Middle-Wage Regions (4)	Low-Wage Regions (5)
ln(outside wage)	.743**	.572	1.472**	.755	.066
	(.352)	(.360)	(.711)	(.992)	(.295)
ln(outside wage) × high-wage region		.452**			
		(.207)			
Case mix controls (14)	Yes	Yes	Yes	Yes	Yes
Year dummies (6)	Yes	Yes	Yes	Yes	Yes
Number of hospitals	149	149	38	44	67
Observations	598	598	153	175	270

NOTE.—Quality is measured as ln(AMI), the in-hospital mortality rate within 30 days of emergency admission for AMI in patients aged 55 or over. The outside wage is the (lagged) area outside wage. We control for case mix with the proportion of the admitted population in each 5-year age-gender band from 55 upward. We also control for area mortality rates and ln(lagged number of clinical employees). The time period is 1997–2005. The estimation method is 3-year (annualized) long differences. Standard errors in parentheses under coefficients are robust to arbitrary heteroskedasticity and autocorrelation (clustered at the local area level).
* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

and 2. The marginal effect of the outside wage is statistically significant and similar in magnitude to the results in the previous columns.[37]

The results in table 3 indicate that higher outside wages tend to depress the quality of hospitals. However, the average effect may mask heterogeneity across areas with different levels of outside wages as suggested by the theoretical model. To examine this we divide the sample into three groups based on the distribution of (regional) outside wages. The "high-wage regions" are London and the South East, where economic activity has been strongest for several decades; the "low-wage regions" are the South West (a sparsely populated fringe) and North East (traditionally the home of heavy industry). The other regions lie in the middle of the wage distribution (see App. A for details).[38]

Column 1 of table 4 re-presents the baseline long-difference result (col. 3 of table 3). Column 2 includes an interaction of outside wages with a dummy variable for whether the hospital was located in a high-wage region. The coefficient on the interaction is positive and signifi-

[37] Diagnostics for the GMM specifications are given at the base of cols. 4 and 5 and are consistent with the validity of the instrument set.

[38] The mean log outside wage in the high-wage regions was 9.83, in the middle, 9.66, and in the low, 9.63. The regions in the three groups and the number of observations in each are given in table A6.

cant, indicating that a 10 percent increase in the outside wage is associated with a 10.2 percent increase in AMI death rates in the high-wage region and only a 5.7 percent increase in other regions (with this difference being significant at the 5 percent level). Columns 3–5 generalize the model by estimating separate regressions for all three regions. Consistent with column 2, these show a considerably larger effect in hospitals in the high-wage regions: a statistically significant effect of 1.472. The coefficient on the outside wage is essentially zero for those in the low-wage regions, with those in the middle-wage regions in between. Although the standard errors increase as expected with the smaller sample size, the effects decline monotonically across the three regional groupings. A 10 percent increase in outside wages is associated with a 15 percent increase in death rates in the high-wage regions, an 8 percent increase in the middle-wage regions, and a 1 percent increase in the low-wage regions.[39]

The results indicate a "convexity" in which the costs of regulation (in terms of death rates) in the high-wage regions are less than the benefits of regulation to the low-wage regions. The results therefore indicate a loss of quality in the high-wage region with little corresponding offset of an increase in quality in low-wage regions. This is consistent with result 3 in the theoretical model.

B. *Magnitudes and a Simple Cost-Benefit Analysis*

In order to get a sense of the quantitative implications of our results, we present a simple cost-benefit exercise using the magnitudes from the estimates in columns 3–5 of table 4. We focus on quality in the public regulated sector and do not factor in any potential benefits to the unregulated sector nor the costs of the sectoral and geographical allocative inefficiency caused by the regulatory distortion.

Our thought experiment is to begin with a situation in which nurses are paid the competitive wage, so there is wage variation across the three regional groupings in the NHS. We then consider introducing the pay regulation in its current form, where all nurses must essentially be paid the same regardless of the outside labor market (we factor in the relatively small regional allowances in the regulated wage). Regulated pay is below the competitive wage in the high outside wage regions and above the competitive wage in the low outside wage regions. The costs of the regulation are that there will be more lives lost because of lower hospital quality in the high outside wage regions. Although there are

[39] The results are robust to classifying the marginal region (East of England) in the middle outside wage regions as in the high outside wage regions: the coefficient (standard error) on the high-wage regions becomes 1.823 (0.702).

benefits in the low-wage regions as lives are saved (average nurse wages have risen), the econometric analysis showed that these are minimal. The main benefit comes from the fact that wage costs are saved since less needs to be spent on paying nurses' wages. Despite these wages being a transfer, because U.K. health care is almost completely publicly funded, there will be a lower tax burden and therefore a lower social deadweight loss from taxation.[40]

In table 5 we examine the social cost of regulation for a wide range of assumptions. We do not directly observe the unregulated market wage, so we estimate the wage a nurse with the same observable characteristics would have earned in the relevant region (this is based on eq. [2] above and discussed in detail in App. A). Our most robust estimates suggest that regulated pay is set to be about 5.5 percent above the market wage in the low-wage regions, 7.6 percent below the market wage in the high-wage regions, and about the same as the market wage in the middle-wage regions. We show below that the qualitative results are very robust to alternative assumptions over these calculations.

Row 1 of table 5 shows that, compared to the unregulated position, regulation brings a small saving to the government of £14 million (about $22 million) in column 1, which is a social benefit of £4 million (using a 30 percent deadweight cost of taxation) as shown in column 2. The cost of the regulation shown in column 3 is that 366 more lives are lost, which (over an extra 10 years of life) implies a loss of £220 million (col. 4). Putting this together in column 5, there is a net social loss under this estimate of £215 million.

The other rows of table 5 show that these conclusions are robust to changing many of the assumptions. Allowing the deadweight cost of taxation to be as low as 10 percent (row 2) or as high as 60 percent (row 3) does not change the social cost much: the loss is between £211 million and £218 million. Our baseline calculations use a value of a year of life of £60,000 following the $100,000 benchmark of Cutler and McClellan (2001). If we double the value of a year of life from the baseline of £60,000 to £120,000 (row 4), we more or less double the social loss to £435 million. Row 5 halves the value of life to £30,000 and shows that at this value regulation still costs around £106 million. Moving the coefficient of the outside wage down or up by a standard error produces net social losses of between £265 million (row 6) and £117 million (row 7). The last three rows shift the assumptions on the level of the competitive wage. In row 8 we set it to be 11 percent below the regulated wage in the high-wage regions and 1.5 percent above and 4.3 percent

[40] The tax savings from the regulation may be one reason why these forms of flat pay regulation are attractive to governments in publicly funded health care systems. There is a taxpayer benefit from "fair" wages, and the true social costs in terms of lives lost are not so visible.

TABLE 5
The Costs and Benefits of Wage Regulation

Experiment and Assumptions	Benefits of Regulation (Reduction in Wage Costs): Government Saving (£ Millions) (1)	Benefits of Regulation (Reduction in Wage Costs): Social (£ Millions) (Col. 1 × Deadweight Loss) (2)	Costs of Regulation (Increased Deaths): AMI-Related Death Because of Regulation (3)	Costs of Regulation (Increased Deaths): Value of Lives Lost (over 10 Years; £ Millions) (Col. 3 × 10 × Value of Life) (4)	Net Benefit-Cost: Social Benefits Minus Social Costs (£ Millions) (Col. 2 − Col. 4) (5)
1. Baseline	14	4	366	220	−215
2. Deadweight loss is 10%	14	1	366	220	−218
3. Deadweight loss is 60%	14	8	366	220	−211
4. Value of life £120,000	14	4	366	439	−435
5. Value of life £30,000	14	4	366	110	−106
6. + SE of coefficient on outside wage (all outside wage regions)	14	4	449	269	−265
7. − SE of coefficient on outside wage (all outside wage regions)	14	4	202	121	−117
8. Alternative outside wage assumption A	118	36	513	308	−272
9. Alternative outside wage assumption B	162	49	561	336	−288
10. Alternative outside wage assumption C	−86	−26	226	135	−161

Note.—The baseline estimates take the coefficients from cols. 3–5 of table 4 and use a value of a healthy life year of £60,000 per year and a deadweight cost of taxation of 30 percent. For the valuation of deaths, we assume the sample average age of admission for emergency AMI (73) and assume life expectancies from government estimates (males aged 73 in 2004 = 13 years, females = 15 years). In the baseline we use the values of the estimated outside wage over the sample period as a whole, so regulation is estimated to lead to paying 7.6 percent less than the market wage in the high-wage regions, 5.5 percent more than the market wage in the low-wage regions, and the same level in the middle-wage wage regions (see App. A). Under "Alternative outside wage assumption A," we assume that the regulated wage is 11percent below the market wage in the high-wage regions, 1.5 percent above the market wage in the middle-wage regions, and 4.3 percent above the market wage in the low-wage regions. Under "Alternative outside wage assumption B," we assume that the regulated wage is 7.6 percent below the market wage in both the high- and middle-wage regions and 5.5 percent above the market wage in the low-wage regions. Under "Alternative outside wage assumption C," we assume that the regulated wage is 7.6 percent below the market wage in the high-wage and 5.5 percent above the market wage in the middle- and low-wage regions.

above in the middle- and low-wage regions, respectively:[41] this gives a social loss of £272 million.[42] In rows 9 and 10 we explore different assumptions about the level of the regulated wage in the middle-wage regions. In row 9 we set it to be the same as that of the high-wage regions in the baseline assumption, and in row 10 we set it to be the same as that of the low-wage regions in the baseline assumption. In both cases a social loss remains, driven by the excess deaths under regulation in row 9 and by both excess deaths and the excess wages paid under regulation in row 10. In conclusion, our results are robust because although the government saves money as the counterfactual market wage rises in the high-wage regions, more lives are also lost counterbalancing this.

The excess deaths we calculate could arise from two sources. First, and our focus here, is the lack of geographical variation in regulated wages with market conditions. Second, regulated wages could be different from the market outside wage in aggregate. For example, the regulated national average wage may be too low compared to the unregulated national average wage. To show that our results are primarily driven by geographical distortions rather than simply average differences, we calculate the fraction of the excess deaths that are due to the wage rate being wrong on average (for details, see App. B). In the baseline case, the aggregate regulated wage is only very slightly below the market wage, so that the government is setting a wage below the market rate in aggregate.[43] But this aggregate level of regulation accounts for less than a percentage point (0.3 percent) of the excess deaths due to regulation. So the source of the cost of the pay regulation is not the government getting the average wage wrong, but the lack of geographical variation of regulated wages with market conditions.

In summary, under a wide range of assumptions, the pay regulation policy has a net social cost. Although the absolute value of the loss may seem relatively small (around $350 million), we believe that this is likely to be a large underestimate of the true loss because we are considering only one marker of hospital quality, the AMI death rate. In addition, to put the excess deaths into context, our baseline estimates imply that 366 extra deaths occur each year. The AMI death rate on average has been falling over time, the fall in the number of deaths between 1997 and 2005 being on the order of 6,700.[44] Regulation results in 366 excess deaths per year, which summed over the same 9 years is 3,294. So the

[41] These are the coefficients from the wage regressions when we do not trim the upper and lower percentiles of the hourly wage distribution. See App. A.

[42] Further details are in App. B.

[43] The difference between the average regulated wage and the average competitive wage is just over 0.02 percent. Even in the extreme scenarios A–C, the difference is not more than 1.9 percent.

[44] Including out-of-sample deaths, the number of AMI deaths was in the order of 15,600 in 1997 and 8,900 in 2005.

number of excess deaths due to regulation is around half the number of deaths that have been averted as a result of technological change and government attempts to increase standards of care. This is a large fraction. Finally, if we were able to calculate the fall in quality across a much wider range of illnesses (deaths and more minor loss of quality of life), we would scale up the social loss by a very large amount.

VI. Extensions and Robustness

A. *The Impact of the Outside Wage on Nursing Home Performance*

The theory section (result 2) argued that we would expect to see a positive effect of higher outside wages in an unregulated sector that competed for skilled nurses. The closest sector is probably the nursing home sector for senior citizens. Health care workers are employed intensively in this sector, which provides both medical and hospitality services for elderly people on a long-term basis. Unlike hospitals, however, firms are free to set wages without being constrained by a centralized regulated nursing wage. We would therefore expect a positive relationship between productivity in the nursing home sector and high outside wages as higher-quality nurses leave the hospital sector and move to the nursing home sector.

We test this idea using firm-level panel data for 1998 and 1999 for several hundred randomly selected nursing homes that included extensive characteristics.[45] One measure of labor productivity specific to this sector is the number of occupied beds per hour worked. This does not take into account the differential quality of nursing homes, so we also examine revenues per hour; this weights the raw productivity figure by the price charged to stay in the home (under the assumption that the higher-quality nursing homes are more expensive).

Table 6 contains the results. Column 1 indicates that the outside wages are associated with a significant increase in the revenues per worker hour in nursing homes. A 10 percent rise in outside wages is associated with a 4.7 percent increase in this measure of nursing home performance. In column 2 we use the alternative measure of productivity (occupied beds per worker hour): higher outside wages appear to increase output per hour by 2.5 percent, and this is significant at the 10 percent level.

These results support the predictions from the model outlined in Section II: the effect of wage regulation is to increase performance in the nonregulated sector as high-quality nurses leave the regulated sector, whose performance deteriorates.

[45] See App. A and Machin and Wilson (2004) for a more detailed data description.

TABLE 6
Effect of Regulated Pay on Productivity in the Unregulated Sector
(Nursing Homes for Senior Citizens)

	Dependent Variable	
	ln(Revenues/Hour) (1)	ln(Output/Hour) (2)
Estimation technique	OLS	OLS
ln(outside wage)	.465***	.246*
	(.128)	(.134)
Year dummies	Yes	Yes
Area fixed effects	Yes	Yes
Number of hospitals/nursing homes	366	370
Observations	510	517

Note.—The estimation method is OLS with standard errors robust to arbitrary heteroskedasticity and autocorrelation (clustered at the local area level). Productivity measured in col. 1 is revenues (number of occupied beds multiplied by average price per bed) divided by total hours, and in col. 2, productivity is occupied beds ("output") per hour. All columns control for the proportion of qualified nurses, proportion female, average age of worker, a quintic in size of the nursing home (measured by employees), the proportion of residents who are paid for by the government, area (county) dummies, and year dummies. Data in cols. 1 and 2 are taken from U.K. nursing homes in 1998 and 1999 with at least one qualified nurse employed (see Machin and Wilson 2004).
* Significant at 10 percent
** Significant at 5 percent.
*** Significant at 1 percent.

A related concern is that the significance of the outside wage for public hospital quality could simply reflect some other unobserved trend. The fact that the outside wage coefficient has opposite signs in the theoretically expected direction in the two sectors (hospitals in table 3 and nursing homes in table 6) makes this unlikely. As a further check we looked at 42 other service sectors and showed that our measure of the outside wage was essentially unrelated to productivity in these sectors (see Hall et al. [2008] for details).

B. A Falsification Test on Outside Wages: Male Wages

As nurses are predominately female, male outside wages should have little impact on hospital quality. Table 7 explores this falsification test, again allowing for convexity across outside wage regions. Panel A of the table replicates the analysis of table 4. Panel B uses nonmanual male wages only as a placebo test.[46] None of the estimates of the coefficients on the placebo outside wages are significantly greater than zero. In addition, the estimates are much smaller than those using female wages and show no impact at the average. Panel C undertakes a "horse race" using both the female and male nonmanual wages. The estimated impact of female wages is very close to those in panel A, and the estimated impact of male wages is insignificant at the 5 percent level in all col-

[46] The region is split on the basis of average nonmanual female outside wages for the period.

TABLE 7
HOSPITAL QUALITY AND THE OUTSIDE WAGE FOR MALES AND FEMALES
(3-Year Long Differences)

	All Regions (1)	High-Wage Regions (2)	Middle-Wage Regions (3)	Low-Wage Regions (4)
	A. Baseline Model			
ln(female outside nonmanual wage)	.743**	1.472**	.755	.066
	(.352)	(.711)	(.992)	(.295)
	B. Model with Male Outside Wage Instead of Female Outside Wage			
ln(male outside nonmanual wage)	−.215	.172	−.086	−.593*
	(.215)	(.587)	(.343)	(.347
	C. Model with Both Male and Female Outside Wages			
ln(female outside nonmanual wage)	.848**	1.517*	.798	.224
	(.374)	(.751)	(1.03)	(.312)
ln(male outside nonmanual wage)	−.391*	−.198	−.184	−.637*
	(.230)	(.591)	(.375)	(.362)
Case mix controls (14)	Yes	Yes	Yes	Yes
Year dummies (6)	Yes	Yes	Yes	Yes
Number of hospitals	149	38	44	67
Observations	598	153	175	270

NOTE.—Quality is measured as ln(AMI), the in-hospital mortality rate within 30 days of emergency admission for AMI in patients aged 55 or over. The outside wage is the (lagged) area outside wage. We control for case mix with the proportion of the relevant admitted population in each 5-year age-gender band from 55 upward. We also control for area mortality rates and ln(lagged number of clinical employees). The time period is 1997–2005. The estimation method is by 3-year (annualized) long differences. Standard errors in parentheses under the coefficients are robust to arbitrary heteroskedasticity and autocorrelation (clustered at the local area level).
* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

umns.[47] Thus female wages appear to matter for AMI death rates whereas male wages do not, which is consistent with our interpretation of the basic outside wage results.

C. *Robustness Tests*

Our econometric strategy controls for unobserved time-invariant effects between hospitals and examines the conditional associations between changes in local female wages and changes in AMI death rates. It is possible that there are time-varying differences between areas that drive the results, and we describe here a sample of the large number of

[47] The weakly significant negative sign in some specifications may indicate that areas where income as a whole is rising fast tend to have better health outcomes (e.g., because of an improved socioeconomic mix).

robustness checks we performed on the main results. These are summarized in table 8. All cells report the coefficient and standard error on the outside wage from separate regressions. Column 1 has results for AMI death rates for the whole sample; the other columns present results by the three outside wage regions. We begin in row 1 with the regressions from table 4. The other regressions use this as the baseline in the rest of the table.

1. The Impact of the Local Labor Market on the Severity of AMI Cases

We have sought to deal with comorbidity through an extensive set of demographic controls, area mortality rates, and hospital fixed effects. But it is possible that there are still omitted case mix variables and that a positive correlation between economic activity and severity of patients admitted is driving our results.[48] We test the robustness of our results to this idea.

We first test for any association between AMI case mix and the outside wage by running 14 separate regressions of each of the case mix variables on outside wage, using a 3-year long-difference specification. In the 14 regressions we found only one significant association between a case mix variable and the outside wage and no systematic pattern in the association of wages and case mix.[49] We then examine whether the severity of those admitted with AMI is associated with outside wages within the period. First, using the HES data, we calculated the proportion of emergency AMI cases that were admitted "with complications" compared to the total.[50] Regressing this AMI case severity measure on the outside wage and all the variables in the same specification as column 3 of table 3 shows that there is no significant association with outside wages.[51] We then include this measure of AMI case mix severity directly in the AMI death rate regressions (table 8, row 2). The results remain very similar and suggest that our included demographics and local area mortality are doing a good job of reflecting case mix.

While our finding of a positive relationship between mortality and outside wages seems unlikely to be driven by an association between

[48] Note that our outcome measure differs from the population AMI rates in Ruhm (2006). Our measure is the death rate conditional on having a heart attack. So Ruhm's effect of strong labor markets causing more heart attacks is distinct from our measure of hospital quality.

[49] Outside wages have a significantly negative association with the percentage of males admitted for AMI who are 60–64.

[50] Constructed from the proportion of emergency patients 55 years or older admitted with Healthcare Resource Group (HRG) codes E11 (AMI with complications) and E12 (AMI without complications). For a discussion of HRGs, see App. A.

[51] The coefficient on outside wages was −3.39 with a standard error of 6.90.

TABLE 8
ROBUSTNESS TESTS (Reports Coefficient on Outside Wage)

Robustness Test	All Regions	High-Wage Regions	Middle-Wage Regions	Low-Wage Regions
1. Baseline	.743**	1.472**	.755	.066
	(.352)	(.711)	(.992)	(.295)
Observations	598	153	175	270
2. Controlling for percentage of AMI admissions "with complications"	.741**	1.488**	.742	.063
	(.350)	(.717)	(.980)	(.298)
Observations	598	153	175	270
3. Controlling for ambulance speeds (percentage of urgent calls not more than 15 minutes late)	.751**	1.533**	.772	.095
	(.350)	(.744)	(1.009)	(.294)
Observations	598	153	175	270
4. Controlling for hospital financial surplus	.743**	1.477**	.758	.064
	(.353)	(.715)	(.992)	(.296)
Observations	598	153	175	270
5. Using level (instead of log) AMI mortality rate	10.943**	28.104**	8.073	−.998
	(5.470)	(12.696)	(14.150)	(4.78)
Observations	598	153	175	270
6. Weighted by average AMI admissions	.613**	1.276*	.096	.269
	(.289)	(.755)	(.958)	(.337)
Observations	598	153	175	270
7. Include lagged dependent variable	.621**	1.602**	.201	.019
	(.309)	(.635)	(.766)	(.280)
Coefficient on lagged dependent variable	.086***	.115***	.092**	.077***
	(.016)	(.020)	(.038)	(.023)
Observations	598	153	175	270
8. Using the regression-corrected outside wage	.753**	1.441**	.704	.233
	(.307)	(.692)	(.816)	(.310)
Observations	598	153	175	270
9. Using regional nursing home wage as an outside wage measure (1998–2005 data only)	.058	.257**	.256	−.142
	(.088)	(.111)	(.277)	(.108)
Observations	486	127	140	219
10. Clustering by region instead of local area	.743*	1.472*	.755	.066
	(.333)	(.463)	(.412)	(.221)
Observations	598	153	175	270
11. Weighted by ASHE cell size	.703*	1.543**	.938	−.020
	(.364)	(.722)	(1.044)	(.321)
Observations	598	153	175	270
12. Controlling for staff skill shares	.739**	1.492**	.812	.071
	(.351)	(.721)	(1.014)	(.302)
Observations	598	153	175	270
13. Controlling for inside wages	.713**	1.456*	.794	.022
	(.349)	(.720)	(1.006)	(.314)
Observations	598	153	175	270

NOTE.—Each cell reports the coefficient and robust standard error from a separate 3-year (annualized) long-difference regression. Quality is measured as the in-hospital mortality rate within 30 days of emergency admission for AMI in patients aged 55 or over. Outside wage is (lagged) area wage unless otherwise specified. The time period is 1997–2005 unless otherwise specified. Standard errors in parentheses under coefficients are robust to arbitrary heteroskedasticity and autocorrelation (clustered at the local area level). See the text for exact experiments.

* Significant at 10 percent.

** Significant at 5 percent.

*** Significant at 1 percent.

outside wages and poorer health in the area from which the hospital draws its patients, it is possible that the state of the labor market affects the severity of patients when they arrive at the hospital. The medical literature distinguishes two important periods after a heart attack: “floor to door” (from having the heart attack to admission to the hospital) and “door to needle” (from admission to initial treatment—usually injection of an anti-blood-clotting agent such as a thrombolytic drug). Since our measure of quality is death rates from AMI taken from the moment a patient is admitted to a hospital, it is possible that the outside wage is actually affecting treatment in the floor to door period. Perhaps the most obvious mechanism would be that stronger economic activity generates more road congestion, causing patients to arrive at hospitals later and decreasing their chances of survival. To check this we first estimated whether there is a relationship between ambulance speeds and outside wages and found no significant relationship.[52] We then reestimated the AMI equations including an additional control for ambulance speeds (the proportion of urgent ambulance journeys arriving on time). The results in row 3 show that our outside wage estimates are robust to this control.[53]

2. Financial Pressure

An alternative explanation for the importance of the outside wage is that hospitals in stronger local labor markets face sharper budgetary constraints. The British government’s funding formula for the health service contains a “market forces factor” that allocates more funds to reflect the higher costs in more expensive areas, but it may not fully compensate (e.g., Crilly et al. 2007). Consequently, hospitals in high-wage areas may be chronically underfunded, and this could cause lower quality. To test this idea we include a measure of the hospital’s financial surplus (or deficit) as an additional control. In row 4 we show that the coefficient on the outside wage remains essentially unchanged.

[52] When we estimate the same model as col. 3 of table 3, the coefficient on outside wage in the ambulance speed equation is 10.05 with a standard error of 12.91.

[53] More subtly, hospitals in high outside wage areas may have higher death rates because of the behavior of ambulance crews. If ambulance crews were of poorer quality in high outside wage areas (for the same reason that nurse quality is poorer), then patients might arrive at hospitals in a worse state and therefore be more likely to die in the door to needle period. Over our time period, however, there was very little treatment of heart attack patients in ambulances. For example, in 2000 only 0.6 percent of reperfusions (thrombolytic drugs) for heart attack patients were given before admission to a hospital in 2000 and 2001. We conclude that poorer treatment by ambulance crews in high outside wage areas is unlikely to drive our results.

3. Functional Form, Weighting by AMI, and Dynamics

In row 5 we estimate the model with the dependent variable in levels instead of natural logarithms. The patterns of the estimates remain unchanged, being nearly twice the size in the high-wage regions as for the whole sample in both sets of estimates and declining monotonically by level of the outside wage. In addition, the magnitude of the effects is very similar.[54]

As we do not weight the observations, our results might be influenced by the smaller hospitals within our sample. In row 6 we weight by AMI admissions. The overall estimates and those for the high-wage regions fall a little in magnitude and significance, but the pattern across regions remains.[55]

We were concerned that we may have not allowed for sufficient dynamics. The specification in row 7 includes a lagged dependent variable and presents the long-run effects of outside wages. Although the lagged dependent variable is significant, the long-run effects of the outside wage remain significant and become slightly larger in absolute magnitude both at the average and for the high-wage regions. The fact that there is a long-run effect of outside wages is important and consistent with the OLS cross-sectional results. Labor supply difficulties in the NHS are not simply due to hospitals optimally smoothing their labor force in response to positive shocks (e.g., due to higher adjustment costs for permanent compared to temporary staff) or to short-run monopsony power, which may be the case in the United States (see Houseman, Kalleberg, and Erickek 2003). They appear to be long-term problems.

4. Outside Wages: Alternatives and Sampling Issues

As discussed above, there are several alternatives that could be used in constructing the outside wage. The more sophisticated measure in equation (2) corrects the area female nonmanual wage for the differential observable nurse characteristics (e.g., age and years of schooling). We use this regression-corrected outside wage in row 8 of table 8 to show that this makes little difference. This is unsurprising: although observable nurse characteristics do differ from those of nonnurse workers, the

[54] For the baseline pooled regression the estimates imply a 7 percent increase in mortality following a 10 percent increase in outside wages. At the sample mean of 18.4 this is equal to an increase of 1.28 percentage points. The levels point estimate in row 5 of table 8 is close—a 1.09-percentage-point increase.

[55] We also reestimated including all hospitals with nonzero admissions (weighted by AMI admissions). The coefficient estimates for the full sample and high outside wage regions fall somewhat but remain significant at the 10 percent level: the coefficient (standard error) for the full sample was 0.455 (0.267) and 1.082 (0.613) for the high-wage regions.

area differences in these characteristics do not change very much over time and so are removed when long-differencing.[56]

The largest employment of nurses in a nonregulated setting is the nursing home sector. We therefore reestimated the model using nursing home wages instead of nonmanual female wages. Because there are insufficient data at the local area level, we aggregate up to the regional level. Row 9 shows that the estimated outside wage is positive and significant in the high-wage regions, but the magnitude of the estimates is much lower than in the baseline, which is likely to be because of aggregation bias.

Our outside wages are constructed from local authority–level data, so our baseline estimates allow for nonindependence across hospital outside wage areas by clustering at this local area level. In row 10, we clustered at the much larger regional level (there are only 10 regions) and show that the estimates are significant at the 10 percent level for the pooled results and for the high-wage regions. Sampling error may also arise because the outside wage is estimated from a sample of the population by the ASHE survey. To test the robustness of our results to such sampling error, we weight the results by the average ASHE cell size. Row 11 shows that the wage coefficients results remain robust to this.[57]

5. Skills and Inside Wages

Our main models do not condition on either skill shares or inside wages since these are potentially endogenous. In row 12 of table 8 we present results controlling for skill shares (the share of physicians and the share of qualified nurses), and in row 13 we control for inside wages (the average observed clinical wage in the hospital). The results are little changed after conditioning on either variable.[58]

[56] This uses a version of eq. (2), where we estimate only for women and use a Mincerian wage equation (i.e., years of schooling and a quadratic in experience for the x_{jnt}). The results are robust to a much longer list of controls such as ethnicity, marital status, union membership, etc.

[57] Using the (appropriately weighted) log of the mean outside wage (instead of the mean of the log outside wage) generates a coefficient on the outside wage of 0.593 with a standard error of 0.341. Since the standard labor supply equation is in terms of ln(wages), our preference is for the current measure, which uses the mean of the individual ln(wage) (see App. A).

[58] In Hall et al. (2008) we show that part of the effect of the outside wage works through the greater reliance on lower-quality temporary nursing staff in public hospitals. This parallels the findings in Autor and Houseman (2005) and Houseman et al. (2003).

These are just a sample of the many tests we ran that overall suggest that our results are very robust.[59]

VII. Conclusions

In many countries, especially in the public and unionized sectors, pay is regulated to be flat across heterogeneous local labor markets. We sketch a simple economic model that confirms the basic economic intuition that in regions where the outside unregulated wage is higher than the regulated wage, it will be hard to attract and retain skilled workers, and this will lead to lower-quality service. The model also makes two further predictions: first, that aggregate quality of service can fall (as the increased quality in the low outside wage regions may not be great) and, second, that quality in the unregulated sector competing for skilled workers will rise.

We examine the English hospital market, which is dominated by the government and where pay for nurses is regulated to be essentially the same across the country. The three predictions of the model are broadly consistent with the data. Using a panel of the population of acute hospitals and using death rates from emergency AMI as a measure of quality, we find support for the prediction that higher outside wages decrease hospital quality. Furthermore, the aggregate death rate rises as the effect is stronger in the high outside wage regions (presumably because skilled workers migrate to other unregulated sectors such as nursing homes) than in the lower outside wage regions. This implies that the regulation has serious costs to welfare that we attempt to quantify. Finally, we find that performance improves in the competing unregulated sector (nursing homes) when the outside wage rises.

One further direction we would like to explore is the impact of regulated prices on technology adoption (see Acemoglu and Finkelstein 2008). Our setting is useful because the external regulation of wages enables us to examine whether part of the performance effect comes from the adoption of suboptimal techniques. We are also analyzing data on the management practices of some of these hospitals in order to understand in more detail the mechanisms generating the extreme variation in hospital performance (Bloom et al. 2009).

At the time of writing, major health care bills have been passed into law by the president. The U.S. government will likely become an in-

[59] Our estimates are also robust to including hospitals with foundation trust status. When we include observations once the hospital has foundation trust status, the outside wage coefficient (standard error) was 0.634 (0.347) in the pooled sample and 1.464 (0.683) in the high-wage regions. Excluding any observation in a hospital that subsequently became a foundation trust gave an outside wage coefficient (standard error) of 0.782 (0.373) for the full sample and 1.262 (0.676) for the high-wage regions.

creasingly important player in the health care market, as is the case in the United Kingdom and most other developed countries. Our study indicates a potential risk if this leads under political or union pressure to more homogeneous wage setting across local labor markets. Publicly run health care markets would be wise to relax the regulatory systems and allow local wages to reflect local conditions as they do in the private sector. According to our analysis, such deregulation would lead to higher-quality public services and fewer deaths.

Appendix A

Data Description

Data sources for all variables are in table A1.

A. *Sample of Hospitals*

The sampling frame is all acute (short-term general) hospitals operating in financial years 1997/98–2005/6 in England. Sample selection is described in Section IV.A and presented in tables A2 and A3. As shown in column 1, 277 hospitals were active at some point in our sample period. The number active in each year is given in column 2. We drop those with no AMI admissions in column 3. Since we use lagged values of some variables, we have to drop observations on the year they enter the sample in column 4. Column 5 drops hospitals that ever had fewer than 150 AMI admissions in a year. This reduces

TABLE A1
Data Sources

	Source of Data	Years
AMI deaths and admission rates	Hospital Episode Statistics (HES)	1996–2005
AMI case mix	HES; age/sex distribution in 5-year age bands from 55–59 to 85 and above	1996–2005
Outside wage data (regional and area wages)	Annual Survey of Hours and Earnings (formerly New Earnings Survey); Labour Force Survey	1996–2005
Local authority directly standardized all-cause mortality rates and AMI rates	Office of National Statistics	1996–2005
Whole time equivalents of clinical staff	Department of Health Medical Workforce Census	1996–2005
Ambulance times	Department of Health, Health Care Statistics	1998–2005
Trust retained surplus and deficits	Trust financial returns TAC01 (from Department of Health)	1997–2005
Nursing home data	Machin and Wilson (2004) own survey	1998–99

Note.—ASHE, HES, and Department of Health years are financial years commencing in April of each calendar year. Office of National Statistics and nursing home data are for calendar years.

TABLE A2
Sample Selection

Year	Total Ever Active Acute Hospitals in Sample Period (1)	Active Acute Hospitals (2)	Hospitals with Strictly Positive AMI Admissions (3)	Hospitals That Are at Least 1 Year Old (4)	Hospitals That Always Have at Least 150 AMI Admissions (5)	Hospitals with Nonmissing Employment Data (6)	Hospitals That Are Not Foundation Trusts (OLS/GMM Sample) (7)	3-Year Long-Difference Sample (8)
1997/98	277	219	208	204	161	154	154	0
1998/99	277	207	195	182	145	138	138	0
1999/2000	277	197	185	177	143	137	137	0
2000/2001	277	190	181	173	140	135	135	112
2001/2	277	181	171	161	133	130	130	103
2002/3	277	169	158	148	124	123	123	97
2003/4	277	166	157	154	128	127	127	100
2004/5	277	166	156	155	131	130	112	91
2005/6	277	166	156	156	131	131	108	95

Note.—The table shows our sample selection criteria. It presents the numbers of hospitals in our sample in each year under different restrictions on the population of active acute hospitals. Column 1 presents the total population of ever-active acute hospitals at any time in the sample period. Column 2 presents the sample of all active acute hospitals in each year. Each column puts a further restriction on the sample compared to the column before it. So col. 3 is a strict subsample of col. 2 etc.

TABLE A3
Exit Regressions: Marginal Effect of Outside Wage on Probability of Leaving Sample

	Level of Outside Wage			3-Year Growth Rate of Outside Wage			
	t[a] (1)	$t-1$ (2)	$(t-1)-(t-4)$ (3)	$t-(t-3)$ (4)	$(t+1)-(t-2)$ (5)	$(t+2)-(t-1)$ (6)	$(t+3)-t$ (7)
ln(outside wage)	−.022	−.027	.165	.351	−.377	−.0	.215
	(.047)	(.052)	(.519)	(.480)	(.475)	(.474)	(.490)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of hospitals	262	256	210	227	250	256	259
Observations	1,701	1,504	940	1,122	1,311	1,359	1,411

Note.—The table reports the results of a probit in which the dependent variable is a dummy variable equal to unity if a hospital exits the sample and zero otherwise. Marginal effects are shown with standard errors underneath (in parentheses) clustered at the local area level. The sample is that of col. 5 of table A2. The right-hand-side variable is the ln(outside wage) plus a full set of time dummies. The outside wage is defined as follows: In col. 1, it is in levels, dated contemporaneously. In col. 2, it is in levels, lagged one period. In cols. 3–7, it is the 3-year (annualized) difference, lagged one period in col. 3, contemporaneous in col. 4, a one-period lead in col. 5, a two-period lead in col. 6, and a three-period lead in col. 7.

[a] The dates in the column headings refer to the timing of the outside wage, so t refers to contemporaneously dated outside wages, $t-1$ to outside wages lagged 1 year, and so on.

the sample significantly but is necessary to deal with the problem of small denominators (i.e., those hospitals with few AMI admissions). Column 6 drops observations for which data are missing on employment. Column 7 drops those hospitals that became “foundation trusts,” which matters only in 2004 and 2005. This is the sample for OLS levels and GMM. Column 8 shows the sample numbers for the 3-year long-difference estimation.

B. *AMI Death Rates*

AMI death rates are derived from HES data. We use the “30-day” death rate for AMI. This measures in-hospital deaths within 30 days of emergency admission with a myocardial infarction for patients aged 55 and over. There are several issues in using this measure. The first is the variability in rates: death rates may be quite variable over time hospital by hospital, reflecting, in part, small denominators (hospitals may treat relatively few patients in any one year). This noise in the measures of death rates can lead to misclassification of the quality of hospitals (McClellan and Staiger 2000). Propper, Burgess, and Gossage (2008) conclude that raw U.K. hospital-level rates exhibit considerably less variability than the raw U.S. data, but not than the U.S. rates that have been “filtered” to reduce noise. We deal with this by dropping hospitals with fewer than 150 AMI admissions in a year (and test the sensitivity of the results to this restriction). The second issue is that we use the 30-day rate. The 7-day rate was not available until 1999, but it is highly correlated with the 30-day rate, and results using this as the dependent variable show similar patterns. Third, we do not have out-of-hospital deaths, but examination of the distribution of AMI deaths in hospitals shows that about half of deaths from AMI occur within the first day of admission (see table A4) and 98 percent of the deaths occur within the first 30 days. Any

TABLE A4
AMI Admissions and In-Hospital Death Rates in 1997/98, over 55s Only

	Primary Diagnosis		Primary or Secondary Diagnosis	
Time until Death	Frequency	Percentage of Total	Frequency	Percentage of Total
0 days	3,220	26	3,647	25
1 day	2,780	22	3,213	22
2 days	1,424	11	1,683	11
3–5 days	2,111	17	2,511	17
6–10 days	1,456	12	1,811	12
11–20 days	914	7	1,197	8
21–30 days	251	2	336	2
More than 30 days	276	2	362	2
Total known	12,432	100	14,760	100
Unknown	1		1	
Total	12,433		14,761	

Source.—Authors' calculations from HES data.

bias from omission of deaths following discharge will be small. Further, incentives to discharge early were small since these death rates were not published until 1999, and hospitals were not ranked by the Department of Health in terms of outcomes until 2001, when they were ranked on a composite bundle of over 20 indicators. Finally, in richer-area hospitals, it may be possible that there are earlier discharges because patients have more care available. This would bias our results against finding an effect of the outside wage on AMI death rates.

C. HES Data

HES data are used for AMI and productivity variables and the case mix variables. HES is a data set of all activity delivered in NHS hospitals. The main unit of recording is the Finished Consultant Episode (a period of admitted patient care under a consultant or allied health care professional within an NHS trust). This is not always the same as a single stay (spell) in a hospital because patients may be transferred from one consultant to another during their stay. In these cases, there will be two or more episode records for the spell of treatment. Diagnoses are coded according to the International Classification of Diseases, tenth revision (ICD-10), and surgical procedures (operations) according to the Office of Population, Censuses and Surveys: Classification of Surgical Operations and Procedures, fourth revision (OPCS-4.2). HES records include further codes, for example, age of the patient (http://www.hesonline.nhs.uk/Ease/servlet/ContentServer?siteID = 1937&categoryID = 537).

D. Wages

1. Regulation

To get an idea of the regulated pay structure, consider nurse pay scales on April 1, 1999. Clinical grades range from A to I and correspond to spine points 3–

TABLE A5
Example of Nursing Pay Bands, April 1, 1999

Clinical Grade	A. Basic Pay Scales: Description, Example	Minimum (£ per Year)	Spine Points
A	Nursing auxiliary	£7,955 (< 18) £8,705 (18+)	3–9
B	Nursing auxiliary working without supervision	£10,310	8–12
C	Enrolled nurse	£11,735	12–17
D	Staff nurse without further qualifications	£14,400	18–21
E	Staff nurse with qualifications	£15,395	20–24
F	Ward sister	£17,075	23–28
G	Ward sister with additional ward experience, district nurse, health visitor, community midwife	£20,145	27–31
H	Senior nurse with responsibility for management of more than one ward	£22,505	30–34
I	Senior nurse with management responsibility and teaching qualifications	£24,920	33–37

Clinical Grade	B. Local Allowances: Inner London	Outer London	Fringe
A and B	£1,850 + 5% of salary up to a maximum of £750	£1,570 + 5% of salary up to a maximum of £750	£285 + 2.5% of salary up to a maximum of £375
C and above	£2,205 + 5% of salary up to a maximum of £750	£1,570 + 5% of salary up to a maximum of £750	£285 + 2.5% of salary up to a maximum of £375

Source.—Income Data Services (2000).

37 (see table A5). For example, clinical grade G, a “district nurse,” corresponds to a grade between spine point 27 (£20,145 per year) and spine point 31 (£23,300 per year). There are allowances (or “weightings”) for being in high-cost areas. For Inner London this was £2,205 plus 5 percent of salary up to a maximum of £750, for Outer London this was £1,570 plus 5 percent of salary up to a maximum of £750, and for the “fringe” (various areas in the South East) this was £285 plus 2.5 percent of salary up to a maximum of £375. For a senior nurse on £23,300 a year working in the most expensive area of the United Kingdom, Inner London, the extra regional allowance would be worth only 11 percent of salary: (£2,205 + £750)/(£2,205 + £750 + £23,300). Since this is capped, for a more senior nurse on a higher salary the proportional value is lower. By contrast, in 1999 using ASHE, the annual nonmanual wage in Inner London is just under 40 percent higher than that of the North East, the lowest-wage region.

2. Outside Wages

As discussed in the text, we consider several ways of constructing the outside wage. The simplest method is to calculate the mean of the ln(wage) for female nonmanual workers (excluding nurses) in the local area. To do this we use ASHE, which is an administrative panel data set of 1 percent of all U.K. workers. All workers whose Social Security numbers end in the same two digits are in the random sample. We calculate annual pay per worker (including any bonus), take natural logarithms, and then average the individual ln(wages) in an area. There are 119 local areas (local authorities) in ASHE and 78 in its precursor the NES, which was structured in an identical way. All wage variables are for workers aged 18–60, working for 35 or more hours per week. The average cell size for female nonmanual wages is 211. For male nonmanual wages (which we use as a placebo test) the average cell size is 265. We construct the hospital wage from the average over all the authorities falling into a 20-kilometer (13-mile) radius of the hospital headquarters. Where there is no local authority in this radius, we use the closest one. Twenty percent of trusts had no authority within a 20-kilometer radius, 41 percent of the trusts had only one authority within a 20-kilometer radius from the trust, and 21 percent had 10 or more.

The more sophisticated version of the outside wage uses regressions to adjust the raw wage series for the fact that nurses have different observable characteristics than nonnurses. We run wage regressions of the form

$$w_{jrt} \equiv \ln W_{jrt} = \beta_{rt} x_{jrt} + \mu_{rt} + \varepsilon_{jrt}, \tag{A1}$$

where W_{jrt} is the wage of worker j in area r at time t. The vector x_{jrt} contains the worker characteristics, μ_{rt} are the area time shocks, and ε_{jrt} is an error term.

To estimate equation (A1) we use individual data from the LFS. The LFS is a quarterly survey of all individuals aged 18 or over in the United Kingdom and is structured like the U.S. Current Population Survey. Unlike ASHE, the LFS has much more detailed individual information such as qualifications, ethnicity, country of origin, marital status, number of children, and so forth. One-quarter of all workers are asked their wages (workers are asked their wages in the quarter they enter the sample and five quarters later when they leave the sample). Thus the disadvantage of the LFS compared to ASHE is that the sample size for wages is smaller, and because it is self-reported, it will have greater measurement error than administrative data.

We pool all individual quarters of the LFS between 1996 and 2007 (3.7 million observations) and run equation (A1) by year and region on female workers who are not nurses or teachers (education is also subject to national pay regulation and employs many women). We take the nurse characteristics in each area (from the full LFS including those nurses whose pay we do not know) in each year to generate an outside ln(wage) series for each nurse, $\hat{w}_{jrt}$, and then average these by area and year. We refer to this as the "regression-corrected" outside wage.

Decomposition of the regression-corrected outside wage shows that the difference in characteristics between female nurses and women working in other sectors does not vary greatly over time within an area. Thus, the main cause of area-specific time-series changes (which is what we use in our preferred specifications) in outside wages is simply the growth in the female wage in an area rather than changes in observables (or the "price" of these observables, β_{rt}) over time. Consequently, our baseline results simply use the growth of the nonmanual female wage in the area, but we are careful to show the robustness of these results to the characteristic-adjusted outside wages in row 8 of table 8.

We also consider the wages for nurses employed in nursing homes as a third alternative measure of the outside wage. Small cell sizes mean that it is infeasible to do this at the local area level, so we construct these at the regional level for each year (table 8, row 9).

Note that using the log of the arithmetic mean produces results similar to those using our preferred mean of the individual ln(wages), although with a lower point estimate (see n. 57). We prefer to use the geometric mean because it arises directly from aggregation of the standard microeconomic relationships. To see this, note that the performance of hospitals is a function of labor supply, which under the standard models (e.g., Blundell, Reed, and Stoker 2003) is a function of ln(wages), for example,

$$H_{irt} = H_0 + \phi \ln W_{irt}, \tag{A2}$$

where H is the supply of labor of nurse i in area r at time t. The expectation of this in a region year is $E(H_{irt}) = H_0 + \phi E(\ln W_{irt})$. Assuming that the $\ln W_{irt}$ are distributed normally with standard deviation σ_{rt}, we can write this in terms of the log of the mean wage, $E(H_{irt}) = H_0 + \phi \ln E(W_{irt}) - (\phi\sigma_{rt})/2$. Since we estimate in long differences (Δ), this becomes

$$\Delta E(H_{irt}) = \phi\Delta \ln E(W_{irt}) - \frac{\phi\Delta\sigma_{rt}}{2}. \tag{A3}$$

So if equation (A2) is the true model and we estimate equation (A3) but omit the variance term (the "aggregation factor," σ_{rt}), then there will be a bias on the estimate of the key parameter, ϕ. Empirically, in our data the growth of mean wages in an area is positively correlated with the growth of inequality in the area, so the coefficient on ln(mean wages) will be biased toward zero. If we follow the theory model of Section II and assume that hospital quality is a function of the supply of skilled workers, then we will find that the coefficient on the log of the arithmetic mean wage is biased toward zero (which is what we find empirically).

3. Definition of High-, Medium-, and Low-Wage Regions

All hospitals are located in one of 10 English regions. We allocate these 10 regions (and all hospitals in a region) into three groups based on terciles of the sample distribution of the average outside wage. The English regions and the number of observations falling into each of the three outside wage regions are in table A6. The mean log outside wage in the high-wage regions was 9.83, in the middle-wage regions, 9.66, and in the low-wage regions, 9.63.

TABLE A6
Location of Hospitals by Outside Wage Regions

Outside Wage Regions	Observations (Hospitals × Year)
Low	532
South West	126
West Midlands	152
East Midlands	75
Yorkshire and the Humber	113
North East	66
Middle	329
East of England	130
North West	199
High	303
Inner London	50
Outer London	76
South East	177

Note.—Data are for 1,164 observations in 209 acute hospitals between 1997 and 2005. The names within each of the three outside wage regions are those of the 10 English government regions.

E. *Case Mix Adjustment*

For additional AMI case mix we constructed "AMI with complications" from the proportion of emergency patients 55 years or older admitted with HRG codes E11 (AMI with complications) and E12 (AMI without complications). An HRG is a code for a group of clinically similar treatments and care that require similar levels of health care resources. They are similar to Diagnostic Related Groups or DRGs in the United States. An example of an HRG is renal dialysis, separated into hemodialysis and peritoneal dialysis. HRG codes are derived from ICD-10 and the OPCS-4.2 codes on HES records.

F. *Nursing Home Data*

The nursing (or "care") home data are discussed in the text and in more detail in Machin and Wilson (2004). Homes were surveyed in 1998 and 1999 (there are also data in 1992 and 1993, but they do not contain the information needed to construct revenue). We observe individual worker data, so we can construct various measures of the internal wage structure of the firm. Information was also collected on average price (a quality measure), the proportion of residents who are government subsidized, and various demographic characteristics of workers (their qualifications, age, gender, etc.).

Total revenue and profits are not reported directly in the care home data. We calculated them from the underlying home-specific components. Sales (S) is calculated as occupancy proportion × number of beds × average price (all reported in the survey). Average weekly hours are reported in the survey, and our key measure is therefore revenues per (hours × workers). We also consider the physical measure of productivity as output per hour: (occupancy proportion × number of beds)/(hours × workers). We matched in outside wages using postcodes using exactly the same data and methods as for hospitals.

Appendix B

More Details of the Cost-Benefit Analysis

Table 5 summarizes the cost-benefit analysis. The experiment we investigate is to start from a position in which nurse wages are equal to our estimate of the outside competitive wage and then introduce a stylized form of the pay regulation as it currently exists.

Since we found heterogeneity on the coefficient on the outside wage across the three regional groupings, we simulate a policy of moving from nurses' pay being equal to the outside wage in these regions to one in which nurses' pay is the same nationwide. We estimate LFS wage equations of the form of equation (A1) with controls for year dummies, education, and a quadratic in age. We estimated that regulated nurse wages are a statistically significant 7.6 percent below the outside market wage in the high outside wage regions, a statistically significant 5.5 percent above the outside market wage in the low outside wage regions, and a statistically insignificant 2.4 percent above the outside market wage in the middle outside wage regions. So for our baseline estimates we set the outside market wage at 7.6 percent, 0 percent, and −5.5 percent for high, middle, and low, respectively. Note that we trim observations in the top and bottom percentiles of the wage distribution for this calculation. If we include these outliers, the differentials are 11 percent, −1.5 percent, and −4.3 percent, respectively. We also show in table 5 the robustness of our results to this (and other) alternative set of market wages.

Since nurses' wages are paid by the taxpayer through the government, there is a fiscal saving as (for most of the scenarios) the total wage bill is predicted to fall. From the social planner's perspective, though, this is just a transfer away from nurses to taxpayers, so the only social benefit of this is that there is a reduced deadweight cost of taxation. Our baseline deadweight loss uses a figure of 30 percent, assuming that $0.30 in the dollar is lost, and we examine varying this number over a wide range of values (10–60 percent).[60]

We simulate what would be the change in the number of aggregate deaths by looking at the change in deaths in each of the three regional groupings if wages moved from being unregulated to being regulated. We use the estimated coefficient of 1.472 from column 3 of table 4 for the effect of the outside wage on death rates on the high outside wage regions. We use the estimate of 0.755 of column 4 for the middle and the estimate of 0.066 of column 5 for the low-wage regions.

Given the coefficient estimates, the regulation certainly will raise aggregate death rates. We calculate the cost of these increased death rates by assuming that a saved life would lead to an extra 10 years of life.[61] We use the £60,000 value of life that is equivalent to a consensus U.S. estimate of $100,000 (Cutler and McClellan 2001), but we also consider values of half and double this value

[60] There is controversy over what is the magnitude of the deadweight loss from taxation. We follow Duflo (2001) and consider this wide band (Duflo considers rates between 20 percent and 60 percent).

[61] Life expectancy is taken from http://www.statistics.gov.uk/downloads/theme_population/Interim_Life/period_cohort_tables_index.pdf.

(i.e., £30,000 and £120,000). Obviously as the value of life declines, the costs of regulation also decline.

The baseline scenario leads to an increase in social costs of regulation associated with more deaths, but the issue is whether this will be counterbalanced by significant savings from the deadweight cost of taxation. The findings of table 5 suggest that there is a net social cost for a plausible range of calculations.

To calculate the fraction of the excess deaths due to the average level of regulation, we estimate the mean wage differential. We use our LFS estimates of the markup of the outside wage over the regulated national wage in each region. The wage differential is (an upper bar indicates a mean over all regions)

$$\bar{\gamma} = \bar{w}^0 - \bar{w}^N = \frac{\overline{W}^0 - \overline{W}^N}{\overline{W}^N} = \frac{\sum_r \gamma_r p_r W_r^N}{\sum_r p_r W_r^N},$$

where w_r^0 is the outside (LFS) ln(wage) in region r, w_r^N is the national regulated wage (allowing for regional supplements in region r), p_r is the employment share of nurses working in region r, and $\gamma_r = w_r^0 - w_r^N$. Lowercase w's are logs and capital w's are levels, so $w = \ln(W)$ and so forth.

We observe the regulated wage W_r^N and therefore $\overline{W}^N$, but not the outside wage W_r^O. However, using the markups by region derived from the LFS estimates of equation (A1), we can estimate the aggregate outside wage as

$$\overline{W}^0 = \sum_r \gamma_r p_r W_r^N = \gamma_H p_H W_H^N + \gamma_M p_M W_M^N + \gamma_L p_L W_L^N.$$

Therefore,

$$\bar{\gamma} = \frac{\overline{W}^0 - \overline{W}^N}{\overline{W}^N} = \frac{\sum_r \gamma_r p_r W_r}{\overline{W}^N}.$$

The deaths (Y^{AG}) due to the difference in aggregate wage between regulated and outside wage are

$$Y^{AG} = \sum_r \bar{\gamma}\phi_r m_r = \bar{\gamma}\sum_r \phi_r m_r,$$

where ϕ_r is the coefficient on the outside wage in the AMI death regression from equation (1) and m_r is the total number of emergency AMI admissions. We calculate this as a fraction of the total excess deaths given in table 5. This is always a very small fraction indicating that the excess deaths due to regulation arise from inadequate variation in regulated pay across geographical regions rather than a low average national regulated wage across the entire country.

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