

# **Can pay regulation kill? Panel data evidence on the effect of labor markets and skills on hospital quality and productivity**

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## Abstract

Labor market regulation can have harmful unintended consequences. In markets, especially for public sector workers, pay is regulated to be the same for workers across heterogeneous labor markets. We would predict that this will mean labor supply problems and potential falls in the quality of service provision in areas with stronger labor markets. In this paper we exploit panel data from the population of English acute hospitals where pay for medical staff is almost flat across geographies. We predict that areas with higher outside wages should suffer from problems of recruiting, retaining and motivating workers and this should harm hospital performance. We construct hospital-level panel data on both quality as measured by death rates (within hospital deaths within thirty days of emergency admission for acute myocardial infarction, AMI) and productivity. We present evidence that stronger local labor markets significantly *worsen* hospital outcomes in terms of quality and productivity. A 10% increase in the outside wage is associated with a 4% to 8% increase in AMI death rates. We find that an important part of this effect operates through hospitals in high outside wage areas having to rely more on temporary “agency staff” as they are unable to increase (regulated) wages in order to attract permanent employees. We quantify the magnitudes of these “hidden costs” of labour market regulation, which appear to be substantial.

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Economists have long warned of the unintended consequences of labor market regulation (see Botero et al, 2004, for a recent contribution). Many rules are justified by unions and the state to promote equality, but can often end up being harmful both to those that they are meant to help and to consumers. There are many studies of labor quantity restrictions (e.g. hiring and firing costs) and labor price floors (e.g. minimum wages). One common, but relatively less studied form of regulation is centralized national wage setting, where pay is mandated to be almost flat over an entire country even with very heterogeneous local labor markets. In many countries the pay of physicians, nurses, high school teachers are set centrally by government with little local variation. U.S. examples would include postal workers and Federal government employees. We would expect such regulations for nominal wage equality to have unintended effects on both labor supply and the level of public services in areas with strong local labor markets. The regulated wage can act as a *pay ceiling* when the outside wage is strong and we would expect this to cause labor supply problems (e.g. difficulties in recruitment and retention), particularly of higher quality workers. Over and above the pure effects of reduced labor supply of permanent workers large pay gaps may lead to lower motivation or to the employment of lower quality workers. This in turn should lead to a lower quality of service provision.

The main contribution of this paper is to confirm the simple economic intuition and show that centralized pay regulation has exactly this negative impact on consumers in a very stark setting. National pay setting of nurses in the English NHS (employing 1.2 million workers) leads to lower quality (higher death rates from heart attacks in our hospital panel) in areas with strong labor markets.

In general, testing the impact of wages on organizational performance is challenging, because in a competitive market wages are equalized for workers of the same skill. Where pay is set by regulation, however, there is a wedge between inside and outside wages, which in principle allows the econometrician to identify the impact of fluctuation in external labor markets on firm outcomes. We exploit this identification strategy using the regulated skill prices of medical staff in the English National Health Service (NHS). The advantages of this setting are that there is a very rigid national pay setting structure for key labor inputs and well-measured outcomes of hospital quality. Regional pay differences are considerable in England (e.g. Bulman, 2002) –

for example, female white collar wages in the North-East of England about 60% lower than in Inner London (these persist after controlling for human capital characteristics). As in the United States (e.g. Borjas, 2004) the cross sectional dispersion of UK public sector pay is much lower than in the private sector (e.g. Disney and Gosling, 1998).

Pay for staff in NHS hospitals – which provide almost all hospital care in the UK - is set by a central review body that sets pay scales in which there is limited regional variation. The variation that exists does not fully reflect the wages differentials in the external labor markets in which the staff are employed. We would expect to see these pay differences reflected in staffing difficulties that manifest themselves in the lower performance of hospitals operating in high outside wage labor markets<sup>1</sup>. This has never been rigorously examined. Our measure of hospital quality is within hospital deaths within thirty days of emergency admission for acute myocardial infarction (AMI)<sup>2</sup>. Our productivity measure is the volume of clinical activity undertaken by the hospital per medical employee. Our data are from a panel of almost all acute hospitals in England for 1995 to 2001. This period is one in which there were very weak incentives for hospitals to turn away high cost patients, there was almost no public information on the quality or productivity of English hospitals and extremely limited choice of location of care by patients<sup>3</sup>. This means that the potential econometric problems that result from patient selection (e.g. that observed hospital death rates may partially reflect unobserved patient attributes) are considerably reduced.

We find evidence that the impact of pay regulation is to generate lower hospital quality and lower hospital productivity in the areas where labor markets are strong (as measured by higher outside wages relate to inside wages). This is effect is not simply due to fewer staff as the effect of outside wages is present even after controlling for standard labor inputs. We argue that hospitals in high wage areas have difficulty in

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<sup>1</sup> There is evidence that falling UK public sector relative wages have led to a decline in the quality of the male public sector workforce (Nickell and Quintini, 2002).

<sup>2</sup> Examples of the use of AMI death rates to proxy hospital quality include Kessler and McClellan (2002), Gaynor (2004) and, for the UK, Propper et al (2004 and forthcoming)

<sup>3</sup> There were no published indicators of quality available until 1999, when six were published. More were made available from 2001 onwards (discussed in more detail below). The data we use in this study are confidential and not known to the general public.

retaining high quality/high effort permanent workers. In particular, such hospitals have to rely disproportionately on *temporary agency staff* and this intensive use of agency staff is associated with worse health outcomes. In addition, consistent with basic production theory, hospitals with a richer mix of skills (i.e. more physicians) also have better hospital outcomes.

Our paper is connected to several other literatures in addition to the economic impact of pay regulation. First, as mentioned, 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, 1980) or perceptions of inequity (e.g. Mas, 2007). As noted above, it is difficult to test these ideas. Where pay is set by regulation, however, there is a wedge between inside and outside wages which enables identification of the impact of external labor markets on firm outcomes, so we can use effectively use regulation to generate exogenous variation in factor prices<sup>4</sup>. Second, our findings on the use of temporary staff relates to an emerging line of literature on the causes and consequences of the use of temporary workers (e.g. Autor and Houseman, 2005; Ericcek et al, 2003; Houseman et al, 2003).

We also connect to a 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, 2005). We argue that one reason for this heterogeneity is the affect of regulated wages on outcomes.

Fourthly, our study relates to the literature on the impact of local economic conditions on health. These studies focus on how economic conditions affect the demand for

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<sup>4</sup> Cawley et al (2006) find that higher outside wages are associated with worse health outcomes in US 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 towards labor saving medical interventions. In our paper, by contrast, we hold inside wages fixed and still identify a negative effect of outside wages. In the US where wages are not regulated, increasing factor prices move hospitals on the labor demand curve rather than the labor supply curve. The positive marginal effects of inside wages we find here are consistent with our interpretation that NHS wages are generally set on the labor curve

health e.g. by changing people's wealth or stress levels. For example, in recent work, Ruhm (2006) argues that there is a greater number of heart attacks during "good times" using US state level unemployment rates<sup>5</sup>. We suggest an alternative mechanism operating through the supply side. In our model, labor market conditions combined with rigid national pay setting affect the supply of a key clinical input - health workers - that, in turn, negatively affects health.

Finally, our study relates to the labor supply literature looking at the effect of wages on employment of medical staff and the literature of skills and productivity. These last two literatures are briefly surveyed in Appendix A.

The structure of the paper is as follows. In Section I we discuss the institutional background to our study, the nature of the research design and related literature. In section II we sketch the econometric model that we are estimating and issues surrounding the approach. In Section III we discuss the data and in Section IV offer a preliminary data description. Section V presents the main econometric results and some robustness tests are discussed in Section VI. Section VII offers concluding comments.

## **I. Institutional background**

In the UK health care is free at the point of use for all citizens and is provided through the National Health Service (NHS), a state monopoly provider<sup>6</sup>. Just over 1.2 million workers are employed in the NHS and the wages and conditions of clinical staff are highly regulated. Our study examines clinical care in acute hospitals so we focus on three main groups of staff: physicians, qualified nurses and "health care assistants" (essentially unqualified clinical staff). Exact definitions are given in the data section, but broadly speaking these cover over three quarters of all hospital staff with the residual group made up of managers, clerical assistants, porters, janitors and other

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<sup>5</sup> In our estimation we control for negative association of labor markets with population health by including local area mortality rates in all the regressions and control for case-mix of the admissions to hospital (discussed below) and also investigate this possible association in robustness checks.

<sup>6</sup> There is a small privately funded sector, which specializes in the provision of elective services for which there are long NHS waiting lists.

support staff. In our sample 13% of clinical staff are physicians, 65% are nurses and 22% are health care assistants.

Physicians and nurses' pay is regulated to a precise national scale - a maximum and minimum 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 two "National Pay Review Bodies" (NPRBs) known as the Review Body for Nursing Staff, Midwives and Professions Allied to Medicine and the Review Body for Doctors and Dentists. Each year, the Review Bodies take 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 implement their recommendations (it generally does this in full).

Under these national scales the same terms and conditions apply across the UK 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 about 11% higher in the highest cost area of Inner London compared to the low cost areas. The outside wage differential is closer to 60% - see Appendix B). Beyond these regional allowance hospitals have little scope for aligning the pay of qualified nurses to conditions in local labour 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 for do not allow pay to be easily adjusted to address staff shortages in local markets.

For the health care assistants group there is no Pay Review Body and employers have some more discretion over setting pay in response to local conditions. This flexibility should not be exaggerated, however, as pay is generally determined by national bargaining with another set of unions.

From an econometric perspective, this institutional setting is attractive because it enables an examination of the impact of different local wages on health 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 as the labor price is endogenous to unobserved shocks. In the UK case the “inside” wage is held broadly fixed as outside shocks change skill prices in the local labor market. There is a wedge between the worker’s offer wage and the outside wage. Consequently, variation in the outside wage can be used to analyze the effects of labor markets on performance.

Figure 1 illustrates the case for two local labor markets, “North” and “South” where outside wages are much higher in the South than in the North (generating a shift to the left in the labor supply curve). Given an equal pay rate across geographic areas this will mean a lower level of employment in the South compared to the North.

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, there are chronic shortages of clinical staff in all parts of the NHS and clinical unemployment has been close to zero (e.g. Finlayson et al, 2002). Therefore, it is more likely that the wage is being set below the competitive wage generating excess demand<sup>7</sup>. It is possible, however, that wages are set above the competitive wage in some low wage areas in the North and NHS staff in these areas are enjoying rents.

[Figures 1 and 2 about here]

We consider in more detail the consequences of regulating wages for health workers in this way, first analyzing employer responses then worker responses. Turning first to the firm side, employers have incentives to overcome the regulatory constraint. First, they could use over-promoting identical workers to higher grades even if they do not have the requisite skills (so-called “grade drift”). This will help them achieve the desired quantity, but at the cost of the lower quality of the over-promoted staff. Second, employers could offer various non-pecuniary benefits such as better working conditions in the high wage areas. These strategies are limited by clinical unions’

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<sup>7</sup> 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 curve. But so long as the regulated wage lies below the monopsony wage, the constraint will still be binding on employers.

power in pushing for homogeneous national conditions and governments have been reluctant to challenge this<sup>8</sup>.

Third, assuming the regulated wage is binding, cost-minimizing employers will try to adjust by substituting towards other factors of production. Consequently, other types of staff whose pay is less constrained by regulated pay will increase and non-labor factors will be in higher demand. Because of the specific skills required for different medical interventions, however, substitution to much less skilled workers (health care assistants and non-clinical NHS staff) will be limited. One key group of workers where substitution is easier is temporary “agency” staff. There are a large number of nurses (and other staff) employed in hospitals on temporary contracts. Private sector firms supply agency nurses to the NHS and these workers do not have their pay regulated by the state. Consequently, the availability of agency staff will enable NHS employers to bring their employment closer to their desired levels.

This is illustrated in Figure 2 where we consider a high wage local labor market (“South”). The regulated wage determines the number of “permanent” staff that the hospital can employ. If the supply of agency nurses is competitive<sup>9</sup> then the wage paid to agency nurses will be bid up to the point that labor demand intersects with labor supply at the competitive wage.

The reliance on temporary agency workers in the high outside wage areas may have other costs. Agency staff may be less flexible and less well trained than permanent staff and they will have built up less job specific human capital<sup>10</sup>. They may also be less committed to the public service ‘mission’ of the NHS (e.g. Besley and Ghatak,

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<sup>8</sup> The desire for nominal equality across workers in geographical areas and other dimensions 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. If unions represent the view of the median worker as in the model of Grossman (1983), however, this worker may be better off with a more compressed wage policy.

<sup>9</sup> If the hospital has monopsony power then the wage (and also employment) will be below the competitive level.

<sup>10</sup> The Audit Commission (2001) finds that agency nurses have little notice before working their shifts, that they are often employed to provide cover at weekends and at night when direct supervision is less likely to be available, and have poorer attendance at training sessions. As a result, induction and handover may be inadequate, agency nurses may have little time to get accustomed to the workings of the hospital, may be unfamiliar with the patients under their care or with local procedures, practices and equipment, with their surroundings and their colleagues. The Commission argues all these factors increase the chances of poorer quality care.



2005) and their presence can cause resentment among the permanent staff. Therefore, the presence of agency staff may be a mechanism through which regulation in stronger external labor markets can lead to worse hospital outcomes. We find some evidence of this in the empirical work discussed below.

Turning to the employee side, in local areas where outside opportunities are better clinical staff will supply less labor as shown in Figure 1. In a static sense, this may lead to lower participation rates, as fewer qualified staff will offer themselves for work<sup>11</sup>. In a dynamic search setting this will lead to higher rates of vacancies and turnover in the high wage areas.

Our focus in this paper is whether there are effects of wage regulation on hospital outcomes over and above the pure effects of reduced labor supply of permanent workers. First, a stronger local labor market may reduce the quality of staff that is prepared to work in the hospital sector as permanent staff or from the substitution into temporary agency nurses as discussed above<sup>12</sup>. Secondly, there may be effects on the effort of workers through various “efficiency wage” channels such as lower motivation and greater shirking. We model this formally below.

If mobility were costless and there exists a single national labor market then using local wages as a signal of labor market pressure makes little sense. Mobility in Britain is far less than in the US (for example, less than one per cent of adults move between the UK regions in a given year, compared to 7% across the much larger US states). Nurses and health care assistants are predominantly female, often with childcare responsibilities and need to be geographically close to hospitals, as they are required to work shifts. It is likely, therefore, that mobility is not perfectly responsive to wage differentials. We show some evidence for this below (if mobility was costless it is

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<sup>11</sup> NHS employees face low switching costs into agency work or employment in the limited private sector.

<sup>12</sup> This raises the question of why any nurses want to work in the public sector instead of simply becoming agency nurses and earning more. There may be stigma attached to being an agency nurse rather than a full-time employee and because nurses are “motivated agents” (see Besley and Ghatak, 2005) this may be a factor. In addition, permanent staff have other non-pecuniary benefits such as greater job security, better promotion prospects and enhanced pensions. Nurses could also work in the private sector. However, the demand for nurses in the private sector is limited by the small size of this sector and nurses in this sector have less opportunities for promotion, training and contribution to ‘public service mission’.

unlikely that geographic differentials of the magnitude we observe could be sustained).

## II. Empirical strategy

### A. Modeling Approach

To motivate our empirical work, assume that we can characterize the output of a representative hospital by a Cobb–Douglas production function<sup>13</sup>

$$Y = AL^\alpha K^\beta \quad (1)$$

where  $Y$  is quality constant output,  $L$  is effective labour input allowing for quality and quantity dimensions,  $K$  is a vector of non-labour inputs (which for expositional simplicity we will treat as scalar) and  $A$  is a Hicks neutral efficiency parameter. We write  $L$  as the product of “effort” ( $E$ ) and labour quantity ( $\tilde{L}$ ), so  $L = \tilde{L}E$ . Effort is a catch all term for the other factors that transforms labour into efficiency units along dimensions we cannot directly observe.

We consider disaggregating the labor quantity into different types of heterogeneous workers so that the labour quantity index can be written as:

$$\tilde{L} = \sum_k \gamma_k N_k \quad (2)$$

where  $N_k$  is labour of skill type  $k$  with relative marginal productivity  $\gamma_k$  ( $>1$ ) and we normalize  $\gamma = 1$  for  $k = 0$ , the lowest skill type<sup>14</sup>. Taking logs of equation (1) and substituting in equation (2) and the definition of  $L$ , we obtain:

$$\ln Y = \ln E + \ln A + \alpha \ln N + \beta \ln K + \alpha \ln \left\{ 1 + \sum_{k,k \neq 0} (\gamma_k - 1) S_k \right\} \quad (3)$$

where  $N$  is the sum of employees<sup>15</sup> and the share of workers of skill class  $k$  is

$S_k = \frac{N_k}{N}$ . We model the effort function as:

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<sup>13</sup> This should be viewed as a first-order approximation to a more complicated functional form. It is straightforward to generalize this to more complex functional forms such as translog and some experiments are included in the empirical results.

<sup>14</sup> See *inter alia* Hellerstein, Neumark and Troske (1999) or Dearden, Reed and Van Reenen (2006).

<sup>15</sup> See Machin and Manning (1992) for an example of this approach in the efficiency wage literature.

$$E = e(W, W^O, Z) \quad (4)$$

Where  $W$  is the “inside” wage (i.e. the “inside” wage paid in the hospital),  $W^O$  is the “outside” wage and  $Z$  are other factors affecting effort/quality of workers. We expect effort to be rising in the inside wage and falling in the outside wage other things equal. Using a first order log linear approximation for the effort function in equation (4) and substituting this into equation (3) gives:

$$\ln Y = \ln A + \alpha \ln N + \beta \ln K + \alpha \ln \left\{ 1 + \sum_{k, k \neq 0} (\gamma_k - 1) S_k \right\} + \delta \ln W + \varphi \ln W^O + \ln Z' \theta \quad (5)$$

An alternative to estimating (5) directly by nonlinear least squares is by using the approximation  $\ln(1+x) \approx x$  which gives us:

$$\ln Y = \ln A + \alpha \ln N + \beta \ln K + \alpha \sum_{k, k \neq 0} (\gamma_k - 1) S_k + \delta \ln W + \varphi \ln W^O + \ln Z' \theta \quad (6)$$

Theoretically, the object of the left hand side of equation (6),  $Y$ , is quality-adjusted output. However, we do not observe this directly. Instead, we have various proxies for this measure of performance. We utilize a key measure of quality ( $D$ , death rates following admission for emergency AMI) and a simple measure of hospital activity - the number of “Finished Consultant Episodes” (FCEs) - essentially admissions (see next section for details).

### *B. Interpreting the wage effects*

The wage effects in equation (6) reflect any impact wages may have on (average) worker effort or worker quality. We expect  $\varphi < 0$  because conditional on a given “inside” wage in a hospital an increase in the outside wage should reduce effort,  $E$ . Similarly, an increase in the inside wage should increase output ( $\delta > 0$ ). The various mechanisms have been extensively discussed in the efficiency wage literature. For example when outside wages rise relative to inside wages this may (i) induce lower effort because the effective cost of shirking has fallen as losing one’s job is less important if the outside labor market is strong (Shapiro-Stiglitz, 1984); (ii) demotivate staff for socio-psychological reasons (Akerlof, 1982); (iii) increase turnover rates under models of search; (iv) make it harder to attract higher quality workers. In

the context of the publicly run UK health system this may be reflected in a greater reliance on agency nurses (who are contracted on a temporary basis to NHS hospitals). We examine some of these possible mechanisms in the results sections, although it is obviously difficult to observe many of these mechanisms directly.

As discussed above wages can also have an effect on hospital performance through the *quantity* of employees of different skill types. Conditioning the production function on labour inputs in equation (6) abstracts away from these effects so we can focus on whether there is an impact of the labour markets through the  $E(\cdot)$  function<sup>16</sup>. We will therefore be underestimating the importance of wages on hospital production and show specifications where we relax this.

### C. Econometric models

We estimate a “production function” for hospital  $i$  at time  $t$  as:

$$\ln(Y/N)_{it} = \ln A_{it} + \mu \ln N_{it} + \beta_1 S_{it}^{PHYS} + \beta_2 S_{it}^{NURSES} + \delta \ln W_{it} + \varphi \ln W_{it}^O + \ln Z_{it}' \theta \quad (7)$$

$S_{it}^{PHYS}$  is the share of physicians in total clinical staff and  $S_{it}^{NURSES}$  is the share of qualified nurses. In the “nurses” group we include qualified AHPs (Allied Healthcare Professionals such as radiologists and physiotherapists). Compared to equation (6) we have used three main skill groups – physicians, qualified nurses and health care assistants (the base category which also includes unqualified nurses and AHPs). Physicians receive the largest amount of training<sup>17</sup>, nurses the second highest amount and health care assistants the least. So in terms of the model we expect  $\beta_1 > \beta_2 > 0$ . Note that we have transformed the dependent variable from output into “productivity” (Finished Consultant Episodes per worker) so  $\mu = \alpha - 1$  is a scale parameter that will be equal to zero under “constant returns”. Our baseline regressions

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<sup>16</sup> Because we observe a discrete number of skill groups one concern is that we are grouping over heterogeneous skill groups within these categories. The interpretation of the inside wage becomes ambiguous because it may simply reflect unobserved labor quality. The coefficient on the outside wage, however, should be robust to this problem of interpretation as the theory implies that it should take a *negative* value. If outside wages simply proxied for the measurement error in human capital inside the hospital we would expect the coefficient on outside wages to be *positive*.

<sup>17</sup> Physicians in the UK follow a five year undergraduate B.A. program and then spend a further four to eleven years in training, depending on the specialty. Nurses follow a three year undergraduate degree to become a registered nurse. Specialist nurses then train for a further year (or more).

include employment as a size control but we also show the robustness to imposing constant returns to avoid an obvious division bias (employment being on the left hand side and right hand side of the regression). Note that we have absorbed the non-labor inputs into the  $Z$ -vector. Another important set of controls in  $Z$  are casemix terms (see below).

Since we have panel data we decompose the unobserved total factor productivity term into its variance components:  $\ln A_{it} = \eta_i + \tau_t + \nu_{it}$  where  $\eta_i$  is a hospital effect,  $\tau_t$  are a set of time dummies and  $\nu_{it}$  is a stochastic error term whose properties we discuss below.

$$\ln(Y/N)_{it} = \ln A_{it} + \mu \ln N_{it} + \beta_1 S_{it}^{PHYS} + \beta_2 S_{it}^{NURSES} + \delta \ln W_{it} + \varphi \ln W_{it}^O + \ln Z_{it}' \theta + \eta_i + \tau_t + \nu_{it} \quad (8)$$

We present results treating  $\eta_i$  as a fixed effect (e.g. long-differenced results or Blundell and Bond's (1998) system GMM method described below) and also results treating  $\eta_i$  as uncorrelated with the right hand side variables (i.e. standard OLS). We will use various proxies for the outside wage ( $\ln W_{it}^O$ ) based on average wages in the local labor market around the hospital. We experiment with measures based at a disaggregated level (we have over one hundred distinct "travel to work" areas in our data) and a relatively aggregate level (the ten regions of England). We focus on female non-manual wages as this is the most likely comparator group for nurses but we also consider other comparison groups for the outside wage (e.g. male wages). We view physicians as operating essentially in a national labor market so the time dummies will capture their outside wages, but we also examine this using alternative wage measures discussed in the next section. Since hospitals are a small part of the local labour 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 inside wage coefficient,  $\delta$  is more challenging. We observe the hospital inside wage, but the interpretation of its coefficient has two problems. First, to the extent that the hospital has any influence on the inside wage to move away from

regulated pay, the inside wage will potentially respond to shocks that affect hospital performance. Secondly, higher observed wages may reflect a better skill mix such as a superior grading structure, which we do not perfectly observe. Thus finding a positive coefficient on the average inside wage could simply reflect the better performance of hospitals with higher average human capital. Consequently, we present a first set of results that do not condition on inside wage information under the assumption that the inside wage is truly national. We then consider alternative methods of including the inside wage. Our main method is to include the inside wage and use the system GMM approach discussed below to allow for endogeneity. Secondly, we follow Gosling and Van Reenen (2005) and include the predicted nurse wage in the hospital based on the decisions of the National Pay Review Body for Nurses (see Data Section for details).

Identifying the coefficients on the factor inputs in production functions is an old problem in econometrics (see Akerberg et al, 2007, for a recent survey). In equation (8) the endogenous factor inputs are the numbers of employees of different skill types. Our preferred method draws on a recent contribution by Bond and Söderbom (2005) which examines the estimation of a model of a Cobb Douglas production function when inputs with differential adjustment costs are optimally chosen. In our context we make the plausible assumption that the hospital faces larger adjustment costs from changing the number of physicians relative to adjusting the number of nurses. Under reasonable parameterizations of the adjustment cost process lags of the endogenous variables will be correlated with current values and this can be used to justify the moment conditions underlying the Blundell and Bond (1998) estimator as applied in the production function context (e.g. Blundell and Bond, 2000). Essentially this estimator builds on the traditional moment conditions that lagged levels of the endogenous variables can be used to instrument the first differenced endogenous variables (Arellano and Bond, 1991). By (testable) assumptions on the initial conditions the “System GMM” approach also allows lagged differences to be used as instruments for the equation in levels. The approach allows the current employment of all skill groups to be affected by shocks to productivity (i.e. endogenous in the production function). We compare the GMM approach to alternative methods of estimating equation (8) by long differences and by OLS in levels.

The hospital quality equation is estimated in a symmetric way to the production function (all the coefficients are allowed to differ, of course, as indicated by the “d” superscript):

$$\ln D_{it} = \mu^D \ln N_{it} + \beta_1^D S_{it}^{PHYS} + \beta_2^D S_{it}^{NURSES} + \delta^D \ln W_{it} + \varphi^D \ln W_{it}^O + \ln Z_{it}' \theta^D + \eta_i^D + \tau_t^D + \nu_{it}^D \quad (9)$$

We again include controls for AMI-specific casemix variables in  $Z$ . A concern with interpreting the outside wage term in equation (9) is that higher economic activity can increase the likelihood of death. This might be due to greater air pollution, traffic congestion or stress at work (Ruhm, 2006). To guard against this we condition on mortality rates in the catchment area of the hospital. If general mortality rates in the area increase because of higher economic activity or for any other reason, we should be controlling for this through mortality rates. As a further test we use information on the severity of AMI admissions, both as a control and to examine whether AMI severity is affected by the strength of the external labor market.

A further concern is that patients with more severe AMI conditions may be non-randomly selected into particular hospitals (Tay, 2003), so that measured quality reflects not hospital productivity but the nature of the patients treated. In the period we study English hospitals had no incentives to reject high severity patients because payments were not based on individual patient costs. Moreover patients admitted for emergency AMI had no choice of which hospital to attend. Consequently, selection based on hospital characteristics is unlikely to be an issue (especially after controlling for fixed effects). Finally, there was almost no public information on hospital quality until after 2006.

### III. Data

#### A. Basic Information

The unit of observation in this study is the hospital, so all measures are at hospital level. We construct a panel data set of NHS hospitals (called “trusts” in the UK<sup>18</sup>)

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<sup>18</sup> An NHS “trust” is a financial, managerial and administrative unit and may cover more than one physical hospital. 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.

covering the financial years 1995/6-2002/3. The panel is unbalanced as the number of hospitals changes over the period due to administrative reorganization. In 1995 the number of acute hospitals in our data was 234, and fell to 175 in 2002. We focus only on acute hospitals<sup>19</sup>. We match data from a variety of sources to these hospitals for our analyses. Details of the data and sources are given in Appendix B and Table A1.

### *B. Measures of quality, productivity and casemix*

These are derived from hospital episode statistics (HES) data for the financial years 1995/6 through 2002/03. We measure quality of output by within hospital deaths within thirty days of admission for emergency acute myocardial infarction (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 of hospital quality<sup>20</sup>. 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. Third, the quality of hospital care has an important effect on survival rates, so there is ample scope for hospitals to affect outcomes (Volpp et al, 2003). Deaths following emergency admission for AMI have been published by both US and UK governments as indicators of hospital quality. McClellan and Staiger (2000) argue that measures of AMI death rate correlate well with other measures of quality. Variants of this measure have been used widely in studies of hospital quality (starting with Kessler and McClellan, 2000).

We use annual hospital-level averages and to avoid the problem of variability of rates from small denominators we only undertake analyses using hospitals with at least 150 emergency AMI admissions per year (results are robust to changes in this threshold). Appendix B discusses further our AMI measure as a measure of hospital quality.

To allow for differences in case-mix we include three sets of controls. First, we control for unobserved hospital fixed effects, which will control for differences in case-mix that are constant over time. Second, we control for all-cause mortality of the

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<sup>19</sup> Non-acute hospitals are a more heterogeneous set (they include mental health and community hospitals) and generally do not provide emergency AMI treatment.

<sup>20</sup> 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.



catchment area of the hospital (which is time varying) and will pick up the degree of ill health of the population that the hospital draws its cases from<sup>21</sup>. Third, we control for the age-gender distribution of admissions for emergency AMI<sup>22</sup>. Fourth, in robustness tests we control for more detailed AMI case-mix measures based on the severity of the heart attack. There may be some time varying, within area unobservable that increases AMI death rates that are not captured by area mortality rates or the other observables. However, this error would have to be systematically negatively correlated with outside wages in order to bias our results.

Productivity is measured in a way similar to labour productivity in studies of other sectors – as total output per head. Our measure of total output is finished consultant episodes (FCEs). This is a standard output measure used in the NHS and indicates the total volume of medical activity (e.g. Vita 1990). To allow for variation in case mix, we again control for hospital fixed effects and for the age-gender profile of total admissions at hospital level and type of admission<sup>23</sup>. In robustness tests we include further controls for the case-mix of total admissions using an index based on the costs of the procedures and diagnoses of all inpatient admissions (discussed below).

### *C. Wages*

We use several measures of outside wages. Our main measure is derived from the New Earnings Survey (NES) that is a 1% sample of all employees in Great Britain covering about 300,000 individuals a year. The NES is mandatory administrative panel data provided by firms to the Department of Work and Pensions and contains information on earnings and hours. Our main measure is average annual earnings, but we also consider hourly wages. We use the area code in the NES to construct over one hundred county-based travel to work areas (or boroughs in London). Using the postcodes of the headquarters of county (and borough) councils, we matched each NHS hospital to all county (borough) councils that fell within a twenty-kilometer radius from the hospital headquarters. The local area wage is constructed as the average of the county wages of all the councils that fell into this radius. Where no

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<sup>21</sup> This is constructed from data on 354 Local authorities and is standardized for age and gender.

<sup>22</sup> The proportion of emergency AMI admissions 55 and above in five year bands separately for men and women.

<sup>23</sup> The proportion of admissions in eighteen five-year age bands for males and females separately plus the proportion of admissions in three categories: emergencies, electives, transfers (omitted category is elective).

councils fell within the twenty kilometer radius the wage applicable to the nearest council was used<sup>24</sup>.

Our main measure is the average wages of non-manual female workers since the overwhelming bulk of nurses are women<sup>25</sup>. We also considered male average non-manual wages as there are more male physicians, but the nature of the NHS means physicians' labor markets are national in scope and so local outside wages may be less important<sup>26</sup>.

We construct two measures of the hospital-specific “inside” wage facing health workers. Our main measure of the inside wage is simply the average wage paid to clinical workers in the hospital, derived from staff numbers and expenditure data (we also show the robustness of this to disaggregating by different groups). A problem with a measure of average wages is that it may reflect the skill mix, such as differential grades within the hospital (as discussed above in the econometric section). We therefore also use a more exogenous measure of the price of labor based on the predicted regional wage for a nurse following the method of Gosling and Van Reenen (2006). We use mandated wage uprating by the National Pay Review Body (which has an area and grade specific component) to calculate the predicted wage increase for an average nurse in the trust using the regional characteristics based on the NES.

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<sup>24</sup> About one quarter of trusts had no council within a twenty kilometre (thirteen mile) radius. Almost half of the trusts had only one council within a twenty kilometre radius from the trust, with the remaining quarter having two or more councils.

<sup>25</sup> We also calculated the average wage for women in secretarial occupations and for all females: results using these measures are very similar to those presented here. The correlations between the wages for women in secretarial occupations, all females and female non manual workers were all above 0.95.

<sup>26</sup> As an alternative to the NES we also considered the Labour Force Survey (LFS). The LFS is a self-reported household survey containing about 320,000 individuals per year (with 80,000 observations on wages). From the LFS we can extract spatial wage differentials conditioning on more characteristics to build up the outside wage offered to a “typical” nurse. We experimented with such measures that successfully predict labor supply problems in the cross section (e.g. Elliot et al, 2007). The smaller sample size and sampling variation, however, means that such constructed variables are less useful in a panel data analysis. So for the most part, we rely on the larger sample sizes of the NES that has less measurement error as it is taken directly from employer records. We also experimented with using measures of unemployment rates and employment rates as alternative indicators of labor market “tightness”. We did not find that these added explanatory power over and above the information in the wage, which in principle should fully reflect labour market conditions.

#### *D. Skill Groups*

We use data on four different main groups of staff: physicians, nurses, allied health professionals (AHPs – e.g. radiographers) and health care assistants. In the main regressions presented here, we combine all qualified staff (nurses and qualified allied health professionals) to one group (“nurses”) and combine unqualified nurses and unqualified allied health professionals with the health care assistants group. Extensive checks show that our results are not sensitive to this summation. Our staff measure is annual whole time equivalents. We define total clinical staffing as the sum of staffing across these groups. Shares for each group of staff are defined relative to this measure of total clinical staff<sup>27</sup>.

### **IV. Preliminary Data Description**

Table 1 presents the means, standard deviations, minimum and maximum for the variables used in the regression analysis for our sample of hospitals. On average, 21.1% of emergency heart attack admissions die within thirty days, very close to the median of 21.3%. Nevertheless the variation is great from as few as 7% to as many as 36%. A similar wide variation is observed for productivity. The average acute hospital is large with around 1900 whole time equivalent clinical staff. About two thirds of clinical staff are nurses and 13% are physicians. We now turn to more detailed description of the trends in AMI death rates, productivity, staffing and wage rates across time.

#### *A. Variation of AMI rate and productivity over time*

Figure 3 presents the distribution of AMI deaths 1995-2002 for acute hospitals. This (and Figure 5) shows the 10<sup>th</sup>, 25<sup>th</sup>, 50<sup>th</sup>, 75<sup>th</sup> and 90<sup>th</sup> quantiles of the distribution. The most striking feature of Figure 3 is the remarkable variation of death rates at any point in time between different hospitals. There are twice as many deaths in the bottom decile as there are in the top decile. Some of this variation can be accounted for by case mix but there remains much residual variation that is potentially related to

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<sup>27</sup> We also have a total employment measure that includes the non-clinical staff, but this does not disaggregate between highly skilled groups such as senior managers and less skilled groups such as janitors. Consequently, our main results use total clinical staff as the main employment measure and we check the robustness of the results to conditioning on the total employment measure (see Table 6).

the quantity and quality of labour inputs. Looking at the evolution of the distribution Figure 3 shows a gradual decrease in death rate over time indicating the long run trend in a decline in the emergency AMI death rate. There is some convergence in death rates between hospitals at the top and bottom of the distribution towards the end of the period. This variation over time is useful to us in identifying changes in the panel. The falls in 2002 follow a major government initiative to reduce the incidence of coronary heart disease through the “National Service Framework”<sup>28</sup>. Although this is a genuine feature of the data we dropped 2002 to avoid any contamination of the main results because of the major policy initiative<sup>29</sup>.

[Figures 3 and 4 about here]

Figure 4 shows the distribution of productivity. There is a large dispersion in productivity: some 60% in 1995 between the top and bottom deciles. This heterogeneity mirrors the well-known findings in the productivity literature that has looked at differences between private sector firms: there is significant and persistent variation in productivity even within very disaggregated sectors that cannot be explained by observable factor inputs. The figure also shows clearly the lack of trend in our measure of productivity. This is consistent with the national pattern in total FCEs, which have not grown faster than NHS clinical employees. Similarly to the AMI death rates there is evidence of a reduction in the variance across hospitals at the end of the period.

#### *B. Outside wages and outcomes: vacancies, labor supply and AMI death rates*

In Figure 5 we plot the mean of outside wages, the intensity use of use of temporary agency nurses and AMI deaths to see if the raw data suggest the relationships we have been discussing. There is considerable spatial variation in all three measures. The similarity in patterns between the distribution of outside wages and of intensity of agency nurses is particularly striking being concentrated heavily in London and other

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<sup>28</sup> The framework, introduced in 2000, set new standards and protocols, backed by increased resources and incentives. For example, all patients with heart attacks were to be given use of a defibrillator within eight minutes of calling for professional help. Hospitals had to give ‘clot-busting’ drugs within a short time of arrival in hospital (aimed at reducing ‘call to needle time’). There were also improvements in the use of effective medicines after heart attacks. For examples of the protocols see: <http://www.rcplondon.ac.uk/pubs/books/minap/HowHospitalsManageHeartAttacks12Nov2002.pdf>

<sup>29</sup> The results are stronger if we include this year. There were many other reforms to improve healthcare after 2001, such as tougher targets in Emergency Rooms (see Friedman and Kelman, 2006, for an analysis).

urban centres with stronger labor markets. The pattern in AMI deaths is slightly different, reflecting amongst other things, the distribution of population ill-health which is higher in areas of greater poverty such as the North-East. Nevertheless there is considerable overlap in the regional distribution of AMI deaths and that of outside wages and use of agency staff.

[Figure 5 about here]

We then examine some simple correlations across regions. First we plot the mean outside wage against the nurse vacancy rate in Figure 6 across the ten English regions. A clear upward sloping pattern emerges with the highest outside wage area (London) having a vacancy rate that is fourfold higher than the vacancy rate in the lowest outside wage area (the North East).

Gosling and Van Reenen (2005) discuss the correlation between nurse labor supply as measured as the proportion of women with a nursing qualification who are employed as nurses (rather than being employed in another occupation or non-participants). They show that labor supply is much lower in the regions where outside wages are higher. In Inner London, for example nurse wages are about 5% lower than the regional average and nurse participation is 50%. In the North of England nurse wages 30% higher than the regional average and participation rates are 75%.<sup>30</sup>

[Figures 6 and 7 about here]

Figure 7 examines the intensity of using agency nurses and the outside wage. Again, we find that the regions with high outside wages rely a lot more on agency nurses than the regions with low outside wages. Figure 8 plots the AMI death rate as a function of the regional outside wage. There appears to be a positive relationship, London having the highest AMI death rates and the low- outside wage regions of the North having lower AMI death rates. In Figure 9 we consider these variables in growth rates over

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<sup>30</sup> It cannot be concluded from this simple comparison that nurses outside London are earning quasi-rents from the public sector so that the regulated wage is above the unregulated wage in Figure 1. This is because the comparison does not take into account non-pecuniary aspects of being a nurse, which may be an unattractive occupation for many people. These non-pecuniary aspects are likely to be relatively stable over time, however, so econometrically we control for them with fixed effects.

the five-year period in our sample. Again, it appears that there is a positive relationship between the change in the outside wage and the change in the growth rate of AMI. Although all areas have had some improvement in the quality of hospitals as measured by AMI death rates, the rate of improvement was fastest in those regions with the slowest increase in outside wages.

[Figures 8 and 9 about here]

Overall then, regions with high outside wages are characterized by higher vacancy rates, greater use of temporary agency staff and higher death rates from AMI<sup>31</sup>. There are of course many reasons why these figures may be misleading and there is no causal connection between high outside wages and poor hospital performance. For example, there may be many other factors positively influencing the outside wage and the AMI death that we have not controlled for. To tackle this we turn to the econometric results where we look at within region variation both in the cross section (by using area and hospital trust level data) and in the time series (by controlling for hospital fixed effects) as well as conditioning on confounding variables such as casemix and local mortality rates.

## V. Main Results

### *A. Hospital Quality as measured by Death rates from AMI*

Table 2 presents the estimates for hospital quality as measured by  $\ln(\text{AMI death rates})$ . The outside wage is measured by the  $\ln(\text{average wage})$  of non-manual women in over one hundred local “travel to work” areas. Column (1) presents the pooled OLS estimates of the association of AMI death rates with staff shares and outside wages, controlling for AMI casemix (admissions in fourteen age-gender bands), hospital type (i.e. whether the hospital was a specialist hospital, or a teaching hospital), the local

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<sup>31</sup> These results fit with Gosling and Van Reenen (2005), who use a long panel of regions between 1984 and 2001 (when there were some significant changes in mandated regional differences) to find a 10% fall in nurse relative wages reduces nurse employment by up to 15%. Elliott et al (2007) find a positive cross sectional relationship between vacancies and high outside wages.

area mortality rates, hospital size (as measured by the log of total clinical employees), year dummies and ten regional dummies<sup>32</sup>.

Column (1) of Table 2 confirms that the bivariate correlation in Figure 8, which shows higher outside wages are associated with higher AMI death rates, remains robust after adding controls. A 10% increase in outside pay is associated with a 4% increase in AMI death rates. It also reveals that hospitals with better qualified employees (i.e. a higher proportion of physicians and/or qualified nurses relative to health care assistants) have significantly lower AMI death rates. The coefficients are sensible being larger for physician share (the highest human capital group) compared to nurse share. Nevertheless, even after controlling for skill mix, the outside wage enters the regression with a significantly positive sign.

Column (2) of Table 2 considers long-differenced specifications (annualized three year differences)<sup>33</sup>. The patterns of signs on the key variables are the same as OLS in levels, but the marginal effects are larger in magnitude. The coefficient on the outside wage is significant at the 5% level, but the skill share variables are insignificant<sup>34</sup>. Finally, column (3) contains our preferred GMM specification that treats skill shares and total employment as endogenous. This estimator exploits the “within” information used in columns (2) and the levels information used in column (1). The marginal effect of the outside wage is statistically significant and similar in magnitude to column (1). The coefficients on the skill shares are significant and larger in absolute magnitude compared to the previous columns. This suggests some endogeneity bias, possibly because hospitals with high AMI rates rely on skilled staff to a greater extent, which biases the coefficients on the skill shares towards zero.

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<sup>32</sup> The results were robust to including other measures of hospital size as extra controls (e.g. number of finished consultant episodes, total number of beds or the total admissions). These terms were never significantly different from zero.

<sup>33</sup> We focus on long-differences to reduce the attenuation bias associated with transitory measurement error. Including a full set of hospital dummies (within groups) leads to similar marginal effects with larger standard errors. For example the coefficient on the outside wage in an identical specification to column (1) estimated by within groups is 0.433 with a standard error of 0.324.

<sup>34</sup> Running the regressions using five year differences also lead to strong results: the coefficient on the outside wage was 1.718 with a standard error of 0.707. Note that the casemix variables were jointly insignificant in the long-differences specifications so we do not include them (p-value of joint significance is 0.353).

The diagnostics are given at the base of Table 2. For the instrumental variables to be valid for GMM-SYS there should be no second order serial correlation in the differenced residuals and no correlation of the error term with the instruments. The high p-values on the LM(2) and Sargan test are consistent with the validity of the instrument set<sup>35</sup>.

### *B. Hospital Productivity*

Table 3 repeats the analysis on the same sample as Table 2 but uses productivity as the dependent variable as measured by the log of the number of FCEs per whole time equivalent clinical worker. The order of the specifications is identical to Table 2 and the control variables are the same except we use a different vector of case mix controls (admission types) specific to total admissions. Column (1) shows that the share of physicians is significantly and positively associated with higher productivity, while the share of nurses is also positive, but statistically insignificant. Importantly, outside area pay is associated with significantly *lower* productivity. A 10% increase in outside pay is associated with a 6.6% decrease in productivity.

The long-differenced results are much weaker with no variable significant, possibly reflecting the low degree of within-hospital variation in productivity. The final column of Table 3 presents the preferred GMM results. Here, all three key variables are correctly signed and significant. The magnitudes of outside pay and physician share are similar to OLS, but the coefficient on qualified nurse share is larger. The Hansen-Sargan test rejects the validity of the instruments in this column. In the next sub-section we show that this is due to misspecification – the omission of inside wages.

In summary, and taking both tables together, we have found two key results. First, a richer skill mix, in particular using more physicians, appears to have a positive effect on raising the quality and quantity of hospital output. This is what basic human capital theory would predict although, to our knowledge, this has not been demonstrated before in the health sector. Secondly, and more interestingly, higher outside wages

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<sup>35</sup> The p-value of the Sargan-Dif test of the invalidity of the extra moment conditions used by the Blundell and Bond estimator (compared to the standard Arellano and Bond moments) is 0.973. This implies the additional moments are not rejected, justifying the more efficient estimation technique.



tend to depress the quality and productivity of hospitals. This is a more controversial finding. In particular, we find that these outside wage effects exist even after conditioning on skill inputs, so the outside wage coefficient is not simply reflecting a lower *quantity* of key staff. We consider the other mechanisms through which external labour markets may be having an effect on hospital quality and productivity below.

### *C. Inside Wages*

According to our model effort, quality and labor supply are determined by the comparison between the inside wage in the hospital and the outside wage in the labor market. Our empirical work above focused on the outside wage because of the relatively small exogenous variation in the inside wage. In Table 4 we look at this in more detail by re-running all the specifications in Tables 2 and 3 conditioning on the inside wage (the average clinical wage in the hospital).

As discussed above there is a regional and grade-specific component of the regulated wage that does vary over time and across regions, so we can use this to construct a National Pay Review Body inside wage (we call this the “NPRB predicted inside wage”). Unfortunately, a lot of the within region time series variation took place prior to our sample period in the late 1980s and early 1990s when extra increases were given to nurse grades in different parts of the high wage South-East. Consequently, the instrument may have insufficient variation to identify performance effects.

Column (1) of Table 4 includes the inside wage in the OLS AMI death rate regressions. The marginal effect of inside wages is negative as expected - higher inside wages are associated with a reduction in AMI death rates – and it is significant at the 5% level. The coefficient on outside wages remains very similar to Table 2 and remains significant. The coefficients on the skills share terms do fall, however and they are no longer statistically significant. This suggests that the inside wage, at least partially, reflects the different skill mix within a hospital. Similar results are observed in column (2) for long differences and column (3) for GMM. In column (4) we include the NPRB predicted inside wage directly in the regression. The coefficient is negative as expected but is insignificant at conventional levels.

The final four columns of Table 4 use productivity as the dependent variable. In contrast to the AMI regressions, the skill share terms dominate the inside wage in the OLS productivity regressions of columns (5) and (6). The strongest evidence is in the GMM results of column (7) where we do find a positive and significant coefficient on the inside wage – a 10% increase in hospital pay is associated with a 2.5% increase in productivity. Outside wages remain negative and highly significant, however. The Hansen-Sargan test now fails to reject at 5% suggesting that the diagnostic problems of Table 3 were related to the omission of inside wages. The final column of Table 4 includes the NPRB predicted inside wage and although correctly signed the coefficient is again insignificant.

Overall, we do find some suggestive evidence that inside wages matter for hospital performance, but identifying their effects is much harder than the effect of the outside wage.

#### *D. Magnitudes*

These results appear to be statistically significant, but they are also economically significant. Using the estimates in column (3) of Tables 2 and 3, we can compare the effect of a change in outside wages and/or skill mix on the quality and quantity of hospital output.

A 10% increase in the outside wages (holding the inside wage and labour inputs fixed) is associated with a 4.6% increase in death rates (Table 2 column (3)) and a 5.5% fall in productivity (Table 3 column (3)). The decile ratio of outside wages between areas is about 33% (in 1996) so a move from the worst to best decile of labour markets is associated with a 15.2% increase in death rates (or just over one quarter of the 60% quality spread. The difference in productivity in between the bottom and top decile is also about 60% in our sample. Moving from the best to worst decile of labour markets is associated with an 18.2% increase in productivity (or just under a third of the between hospital productivity distribution).

The staffing variables are expressed as shares (the variable ranges between zero and unity) and the AMI variables as expressed in natural logarithms of rates. From Table 2, column (3), a one-percentage point increase the share of physicians is associated

with a reduction in AMI deaths rates of 2.6%. A one-percentage point increase in nurse share of total staffing is predicted to decrease the AMI death rate by 1.4%. There is a seven percentage point difference in physician share between the top and bottom decile of hospitals so a change of this size would be predicted to lead to a fall in AMI death rates of 18% (just under one third of this difference in the quality spread). Similar calculations for productivity suggest that a move from the lower to the upper decile of physician share is associated with a 27% increase in productivity (almost half of the productivity spread).

Our estimates of the impact of a change in the outside labor market can be compared to those from specific medical interventions in the treatment of AMI. Our estimates suggest a 10% increase in the outside wage is associated with just under a 5% increase in 30-day death rates, which is around a one percentage point increase at the mean death rate of 21% in our data. Heidenrieck and McClellan (2001) estimate in the USA that the effect of increasing the use of aspirins in treatment of AMI patients from 5 to 75 percent was a fall of 3.3 percentage points in 30 day mortality rates, the effect of increasing the use of thrombolytics from 0 to 31 percent was a 1.6 percentage point fall and the effect of increasing use of beta blockers from 21% to 50% was a 0.6 percentage point fall. Austin and Mamdami (2006) estimate that prescription of statins at discharge reduced three-year mortality by 2.1 to 4.5 percentage points in Ontario, Canada. Our estimates, therefore, of the impact of difference across local labour markets are not out of line from those produced by specific medical interventions.

Although all of these calculations are very crude, they suggest that labour markets and skills potentially could be very important in accounting for the cross sectional health differences in health sector productivity.

*E. What is the mechanism through which higher outside wages affect hospital outcomes? The role of temporary agency staff*

The estimates above show that quality and quantity of hospital output are negatively associated with the outside labour market wage. We have made the assumption that outside wage is associated with the quality of staffing: we examine here whether alternative measures of staff quality are directly associated with outside wages. One channel that has been suggested (e.g. Audit Commission, 2001) is the greater reliance

on temporary, agency staff in high wage areas. If agency staff have lower general or hospital-specific human capital they may depress hospital outcomes. In a related literature several recent papers (e.g. Autor and Houseman, 2005) have suggested that temporary jobs are not “stepping stones” to better careers which is consistent with the notion that workers build up little human capital in these positions.

We have information available to construct a measure of the intensity to which hospitals rely on nursing agency staff for a sub-sample of the data. We use the proportion of total staff costs accounted for by agency nurses as our key indicator in Table 5 (“Agency”). The first column reports regressions of the intensity of use of agency nurses on the outside pay rate (and other controls). There is a highly significant correlation, suggesting that agency nurses are more commonly used in the high outside wage areas. Column (2) then includes the agency term directly in a hospital AMI death rate regression identical to our preferred model in column (3) of Table 2<sup>36</sup>. A greater use of agency staff is associated with significantly higher death rates in hospitals: a doubling of the intensity of use of agency staff is associated with a 5.7% increase in the death rate. Column (3) simply repeats the preferred specification on the sub-sample with non-missing agency nurse information with only outside wages for comparison purposes. The results are very similar to the larger sample. Then column (4) has a “horse race” with both agency and outside wage measures entered simultaneously. The outside wage coefficient falls to almost half of its value in the previous column and is no longer significant at even the 10% level. The coefficient on the agency variable also falls but it remains significant at the 5% level. This suggests that a significant part of the way that the outside labor market is affecting quality is through greater agency staffing. In support of this finding, detailed analysis of the payroll data of a small number of trusts indicates that agency staff are disproportionately deployed in accident and emergency departments, through which emergency AMI cases are admitted.<sup>37</sup>

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<sup>36</sup> We treat the agency variable as the other hospital-level variables like physician share and instrument it with past values in the GMM approach.

<sup>37</sup> There are no published breakdowns of where specialist units agency staff are deployed in NHS hospitals. The staff census data which is collected on a routine basis for all hospitals is not detailed enough to undertake this analysis. Analysis of detailed payroll data of fourteen NHS Trusts (data used in Crilly et al, 2007) shows that, in these hospitals, agency staff are disproportionately deployed in the Emergency Room departments (called Accident and Emergency, A&E, in UK) relative to other nursing staff and relative to their share in the rest of the hospital. The share of agency staff expenditure accounted for by A&E departments averages 9.1%. This is around twice the share of total nursing staff

The next three columns of Table 5 repeat the experiment but use productivity instead of AMI death rates as an outcome measure. In column (5) we show that a greater use of agency staff is associated with significantly lower productivity. Column (6) shows that the marginal effect of the outside wage on productivity is significant in the subsample where we have agency staff information (although somewhat larger in magnitude). In column (7) where we condition on agency intensity the magnitude of the outside wage coefficient has fallen by about 10% of its value in the previous column, but it remains significant at the 1% level. The coefficient on agency staffing has fallen by about half but remains significant at the 1% level<sup>38</sup>.

To further explore the role of agency staff, we examine whether there is a link between agency staffing and a specific adverse medical outcome that is a major problem in UK hospitals. Since 2001/2 acute NHS hospitals in England have been subject to mandatory reporting of Methicillin-resistant *Staphylococcus aureus* (MRSA) bacteraemias (bloodstream infections)<sup>39</sup>. Hospital acquired infections have been argued to be associated with hospital physical cleanliness. For the UK, it has been hypothesized that greater reliance on agency nurses may be associated with higher rates of MRSA. Temporary nurses may be less conversant with local infection control policies, and move around more between wards within hospitals as well as switching between hospitals. This implies a greater number of patient contacts per agency nurse, and patients on wards with higher numbers of temporary staff may come into contact with more staff, since temporary staff often work shorter shifts than permanent staff.

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spend in A&E departments (4.7%) and is higher than the share of total nursing staff costs accounted for by agency staff (7.9%). Since all emergency AMI cases must come through A&E, this suggests a direct route why agency staff may be more important for AMI than other conditions.

<sup>38</sup> We also re-estimated Table 5 by OLS and GMM excluding the inside wage and the results are similar.

<sup>39</sup> *Staphylococcus aureus* (SA) are bacteria commonly carried on the skin and it is estimated around 30% of the population of the UK carries SA bacteria at any one time. SA bacteria can cause serious infections such as surgical wound infections and pneumonia. Treatment of such infections has become more difficult because these bacteria have become resistant to antibiotics. Bacteraemia infections occur when the MRSA enters a normally sterile bloodstream either through an intravenous catheter or a local site of infection.

MRSA data is only available for 2002 and 2001. For these years we regressed MRSA rates against agency staff intensity controlling for the same variables as in Table 2 column (1). Consistent with McCormick et al (2007) we found a positive and significant association between levels of MRSA rates and use of agency staff (a coefficient of 0.015 with a robust standard error of 0.006).

Taken as a whole, these results are suggestive that agency staffing may be part of the mechanism through which higher outside wages negatively affect hospital outcomes.

## VI. Robustness Checks

We describe here a sample of the large number of robustness checks we performed on the main results. These are summarized in Table 6. All cells report the coefficient and standard error on the outside wage from separate regressions. The first column has AMI death rates as the dependent variable and the second column has productivity as the dependent variable. Both columns include the inside wage. We begin in row 1 with a baseline regression taken from Table 4 columns (3) and column (6). The other regressions use this as the baseline in the rest of the table.

### A. Severity of AMI cases and the local labor market

As discussed above, Ruhm (2006) has suggested that a booming local labor market could increase ill health and the incidence of heart attacks and has presented US evidence to support this hypothesis<sup>40</sup>. We have sought to deal with co-morbidity through an extensive set of demographic controls, area mortality rates and hospital fixed effects. But it is possible there are still omitted casemix variables and that a positive correlation between economic activity and severity of patients admitted that is driving our results. We test the robustness of our results to this idea in a number of ways.

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<sup>40</sup> Note our outcome measure differs from the population AMI rates in Ruhm (2006). Our measure is the death rate from *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. Ruhm also finds the strongest effects of labor markets are for prime age adults, which he argues is due to stress at work. Our main outcome, by contrast, is for individuals over 55, the majority of whom are not working.

First, we examine whether the *severity* of those admitted with AMI is associated with outside wages. Using HES data we calculated the proportion of emergency AMI cases that were admitted “with complications” compared to the total<sup>41</sup>. Regressing this AMI case severity measure on the outside wage and all the variables in column (1) of table 2 shows that there is no significant association with outside wages (the coefficient on outside wages was 0.001 with a standard error of 0.041)<sup>42</sup>. We then include this measure of AMI casemix severity in the AMI regressions (row 2). In simple OLS specifications - as in Table 2 column (1) - the coefficient on this variable took the expected positive sign and was statistically significant (0.262 with a standard error of 0.116) when we omitted our demographic controls. When the demographic controls were included, the extra co-morbidity variable was positive but insignificant (in OLS or in GMM). This suggests that our included demographics are doing a good job at reflecting casemix. The marginal effects of the outside wage was, if anything, stronger when this variable was included as shown in the first two columns of row 2.

As a robustness check for the productivity regressions we use a casemix variable based on the severity of all admissions to each hospital. This index, known as the reference cost index, is derived from all admissions to the hospital as classified by Healthcare Resource Group (HRGs, which are similar to US Diagnostic Related Groups (DRGs); see Appendix B). A higher value of the index reflects greater complexity of cases in the hospital. When included in the productivity regressions the relative costs index takes the expected sign and is significant (the index has a coefficient of -0.716 and a standard error of 0.186). The effect of the outside wage is robust, however, remaining negative and significant (row (3) column (2) of Table 6).

Second, we investigate whether our results could be driven by poorer health of the population in strong local labor markets by directly testing whether economic activity at local level is associated with higher illness and higher deaths in England. We use data at local authority level for the same time period as examined here (1996-2001)<sup>43</sup>

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<sup>41</sup> We constructed this ratio from the proportion of emergency patients 55 year old and over admitted with HRG codes E11 (AMI with complications) and E12 (AMI without complications). For discussion of HRGs, see Appendix B.

<sup>42</sup> If we drop the time dummies, however, the coefficient on the outside wage becomes positive and significant (0.068 with a standard error of 0.030), suggesting that it is important to control for omitted aggregate time shocks.

<sup>43</sup> There are 354 local authorities in England.

and examine the relationship between death rates and our pay variable. If economic activity leads to more deaths then this could be driving out results. We examine two causes of death – all cause mortality and deaths from suicides – chosen on the grounds that the former is a general measure of the health of the population and both measures are relatively unaffected by the quality of care provided in hospitals, so any association with outside pay should be little affected by the association between outside pay and the quality of hospital care<sup>44</sup>. We regressed these (age and gender standardized) deaths rates on outside pay with controls for time dummies and local authority dummies. If local upturns are associated with higher death rates, the coefficient on outside pay should be positive in these regressions. In fact, we find no association. For all cause mortality, the coefficient on outside pay is 0.30 (standard error = 0.35) and for deaths from suicide, the coefficient is 0.15 (standard error = 0.50). Local upturns in England do not appear to be associated with poorer population health. Thus our finding that increases in the local wage rate are associated with more hospital-based AMI deaths seems unlikely to be driven by a negative association between economic activity and the health of the local population.

*B. Outside labor markets affecting care in ambulance prior to hospital?*

Medics distinguish two important periods after a heart attack “floor to door” (from having the heart attack to admission to 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 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 arrive at hospitals later and decreasing their chances of survival. Hence we re-estimated AMI equations including an additional control for ambulance speeds (the proportion of urgent ambulance journey arriving on time). Our outside

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<sup>44</sup> While all cause mortality does include deaths from AMI, the number of AMI deaths at population level is a small proportion of all deaths. The average number of deaths at local authority level from all cause mortality is 670; the number of AMI deaths is 59. Furthermore, around three-quarters of AMI fatalities in England for patients under age 75 occur in the community (Norris et al 1998).



wages estimates were robust to this control (coefficient of 0.446 with a standard error of 0.172)<sup>45</sup>.

More subtly, hospitals in high outside wage areas may have higher death rates because of behaviour of ambulances. If ambulance crews were of poorer quality in high outside wage areas (for the same reason as nurse quality is poorer) then patients might arrive in hospitals in a worse state and therefore be more likely to die in the first so-called “golden hour”. Over this period, however, there was hardly any treatment of heart attack patients in ambulances. For example, in 2000 and 2001 only 0.6% of reperfusion (thrombolytic drugs) for heart attack patients was given before admission to hospital in 2000 and 2001 (Birkenhead, 2005). So worse treatment by ambulance crews in high outside wage areas is unlikely to drive our results.

### *C. 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 should reflect the higher costs in more expensive areas, but it may not fully compensate (e.g. Crilly et al, 2006). Consequently, hospitals in high wage areas may be chronically under-funded and this could cause worse quality and productivity. To test this idea we included a measure of the hospital’s financial surplus (or deficit) as an additional control. In row 3 we show that the coefficient on the outside wage remains significant and very similar in magnitude in the sub-sample where we have information on hospital’s financial position.

### *C. Dynamics*

We were concerned that we may have misspecified the econometric model and not allowed for sufficient dynamics. The specification in row 4 of Table 6 includes a lagged dependent variable (treated as endogenous using the standard GMM approach) and presents the long-run effects of outside wages. Although the lagged dependent variable was significant, the long-run effects of the outside wage remain significant and become slightly larger in absolute magnitude for both equations.

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<sup>45</sup> These data are only available as a consistent series for three years in our data.

#### *D. Regional heterogeneity in effect of outside wage*

According to Figure 1, the high outside wage areas may be affected more by the regulated wage than the low cost areas (for example, the regulated wage may be close to the unregulated equilibrium wage for the North). Consequently, we would expect a larger effect in London than in the rest of the country. We experimented with dropping London from the sample in row 5. As we expect, the marginal effects are somewhat smaller in this reduced sample for both AMI and productivity, but they remain significant at the 5% level.

#### *E. Further Robustness Tests*

We also tried dropping some outliers in the change in the outside wage in row 6 and running the regressions only on the balanced panel in row 7. The results are stronger than the baseline in these sub-samples. In row 8, we use an alternative measure of the outside wage – the regional wage in each of the ten English regions (i.e. much more aggregated than the one hundred plus area outside wages used in our main analysis). The marginal effects are larger in magnitude but statistically insignificant unless we drop the regional dummies (row 9). This illustrates the importance of using spatially disaggregated measures. In row 10 we include total hospital employees, including non-clinical workers. The employment variable is insignificant and the coefficient on outside wages falls, but is still significant at the 5% level.

One simple concern is that the inclusion of linear skill shares in equation (7) may be too restrictive. We relax this by including squares and cross-product terms of the three skill groups in row 11. The higher order terms were generally insignificant and the coefficients on the outside wage terms remain significant and are larger in absolute magnitude.

Overall then, our results appear robust to a wide variety of experiments.

## **VII. Conclusions**

This paper has examined the impact of centralized pay regulation on the performance of hospitals. We find that keeping pay flat over heterogeneous local labor markets leads to lower hospital performance (as indicated by AMI death rates and productivity) in areas with stronger outside labor markets. Regulated skill prices

offers a useful identification strategy for examining the impact of labor markets on organizational performance relative to a competitive labor market were wage inside and outside the firm should be equalized. Our unusually rich data of a panel of essentially all acute hospitals in the UK allows us to control for a number of confounding influences.

Our main result is that hospitals who operate in areas with high outside wages (relative to inside wages) suffer from lower quality and lower productivity. This is not simply because they have trouble maintaining high enough staffing levels as we condition on labour inputs. It is more likely to be due to the difficulty of retaining high quality staff and/or the lower levels of effort that are supplied when outside wages are high. The reliance on temporary agency staff instead of permanent staff is an important mechanism in generating these worse outcomes. This finding is related to the emerging literature on the quality of temporary jobs for workers (e.g. Autor and Houseman, 2005). In addition, consistent with basic production theory, hospitals with a richer mix of skills have better quality health outcomes and higher productivity.

From a policy perspective, our study has important implications for regulated labor markets. The National Health Service, a quasi-monopoly provider, dominates the UK health system and wages for physicians and nurses are determined centrally. The local variation of wages does not fully reflect the higher outside wage in areas where the labour market is tight (such as London and the South East). The low relative wages in these high outside wage areas appear to have a direct impact on the death rates in hospitals and the level of their productivity. Changing the system of wage setting to allow wages to reflect market realities would be predicted to both improve productivity and save lives in the higher wage areas.

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**Table 1: Descriptive Statistics**

	Mean	Standard deviation	Min	Max
<b>AMI Variables</b>				
AMI death rate (55 plus)	21.14	4.483	7.454	36.941
Total AMI deaths (55 plus)	7,999.13	3,383.89	1300	29,400
Total AMI admissions (55 plus)	385.02	160.84	151	1,348
<b>Productivity and FCE (finished Consultant Episodes)</b>				
Productivity (total FCEs/ total clinical staffing)	31.17	7.57	12.09	65.12
Total FCEs	58,664.58	24,515.83	13,490	138,984
<b>Staffing Variables</b>				
Total clinical staffing (physicians + nurses + Allied Health Professionals + Health Care Assistants)	1675.79	692.25	398.61	4010.70
Physicians share of staffing	0.148	0.030	0.058	0.270
Qualified Nurses (plus qualified Allied Health Professionals) share	0.597	0.037	0.476	0.741
Health Care Assistants share	0.246	0.046	0.121	0.393
<b>Hospital Expenditure Variables</b>				
Share of expenditure on agency staff as a proportion of total expenditure (“Agency”)	0.034	0.028	0.001	0.163
<b>Wages</b>				
Ln(Area outside wage)	9.60	0.140	9.27	9.99
Ln(nurse inside wage)	9.99	0.152	9.52	10.50
Ln(area inside wage)	10.09	0.110	9.53	10.45
<b>Other variables</b>				
Directly Standardized Mortality rate in local area (per 100,000)	723.43	77.13	518.73	944.21
Teaching trust	0.111	0.341	0	1
Proportion of emergency admissions (to total admissions)	0.411	0.082	0.224	0.808
Proportion of transfer admissions (to total admissions)	0.160	0.066	0	0.448

**Notes:** Acute hospitals in 211 English NHS (regression sample used in Table (2)-(4). 901 observations between 1996 and 2001. Other case mix variables are admissions within 5 year age-gender bands for emergency AMI (55+) and total admissions (all ages). Staffing refers to whole time equivalent clinical staffing.

**Table 2: Hospital quality and the outside wage**

Dependent variable	Ln(AMI Death Rate)	Ln(AMI Death Rate)	Ln(AMI Death Rate)
Estimation technique	OLS	3 year annual Long Differences	GMM-SYS
	(1)	(2)	(3)
Ln (Area outside wage)	0.405*** (0.124)	0.766** (0.386)	0.455*** (0.176)
Physicians share	-0.868*** (0.317)	-0.654 (0.616)	-2.611** (1.263)
Qualified Nurses share	-0.418** (0.227)	-0.288 (0.146)	-1.387 (0.959)
(omitted base is unqualified nurses/ health care assistants)			
Hospital fixed effects	No	No	Yes
Casemix controls (14)	Yes	Yes	Yes
Year dummies (6)	Yes	Yes	Yes
Region dummies (10)	Yes	No	Yes
SC(1) p-value			0.000
SC(2) p-value			0.170
Hansen-Sargan p-value			0.926
No of Hospitals	210	133	210
Observations	901	345	901

**Notes:** \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Quality is measured by the within hospital deaths within 30 days of emergency admission for acute myocardial infarction (AMI) for over 55 year olds admitted with AMI to the hospital. Casemix controls are the proportion of total emergency admissions for AMI made up by each 5-year age-gender band from age 55 upwards. All regressions control for area mortality rates, (lagged) employment and hospital type (i.e. whether the acute hospital was a specialist hospital, teaching hospital or “normal” hospital). Long-differences are three- year annual average growth rates. In the System-GMM estimates, one-step robust estimates are presented equations are levels of own values t-2 through t-5; instruments in the levels equations are once lagged differences. SC(k) is Arellano-Bond (1991) test of serial correlation. Physician share, nurse share and total employment are treated as endogenous (the outside wage is lagged and treated as exogenous). We also use the lagged predicted inside wage as an instrument. In the GMM specification instruments in the differenced of order  $k$  of the first differenced residuals. Sargan-Hansen is a test of all the over-identifying restrictions. Time period is 1996-2001. Standard errors in parentheses under coefficients are robust to arbitrary heteroskedacity and autocorrelation.

**Table 3: Productivity in hospitals and the outside wage**

Dependent variable	Ln(Productivity)	Ln(Productivity)	Ln(Productivity)
Estimation technique	OLS	3 year annual Long Differences	GMM-SYS
	(1)	(2)	(3)
Ln (Area outside pay)	-0.662*** (0.144)	0.252 (0.279)	-0.552*** (0.181)
Physicians share	3.741*** (0.390)	0.248 (0.411)	3.908*** (0.897)
Nurses share	0.358* (0.207)	0.006 (0.216)	1.743*** (0.625)
(omitted base is unqualified nurses/health care assistants)			
Hospital fixed effects	No	No	Yes
Casemix controls (39)	Yes	Yes	Yes
Year dummies (6)	Yes	Yes	Yes
Region dummies (10)	Yes	No	Yes
SC(1) p-value			0.004
SC(2) p-value			0.459
Hansen-Sargan p-value			0.042
No of Hospitals	210	133	210
Observations	901	345	901

**Notes:** \*significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Productivity measured as the number of Finished Consultant Episodes (FCEs) per whole-time equivalent (WTE) employee; casemix controls are proportion of admissions in five year age-gender cells. Quality is measured by the within hospital deaths within 30 days of emergency admission for acute myocardial infarction (AMI) for over 55 year olds admitted with AMI to the hospital. Casemix controls are the proportion of total emergency admissions for AMI made up by each 5-year age-gender band from age 55 upwards and proportions of admissions that are (a) elective, (b) emergency, (c) transfers. All regressions control for area mortality rates, (lagged) employment size and hospital type (i.e. whether the acute hospital was a specialist hospital, teaching hospital or “normal” acute hospital). Long-differences are three- year annual average growth rates. In the System-GMM estimates, one-step robust estimates are presented. Physician share, nurse share and total employment are treated as endogenous (the outside wage is lagged and treated as exogenous). We also use the lagged predicted inside wage as an instrument. In the GMM specification instruments in the differenced equations are levels of own values t-2 through t-5; instruments in the levels equations are once lagged differences. SC(k) is Arellano-Bond (1991) test of serial correlation of order k of the first differenced residuals. Sargan-Hansen is a test of the over-identifying restrictions. Sargan-Hansen is a test of all the over-identifying restrictions; Sargan-DIF is a test of the extra initial conditions assumptions in Blundell-Bond. Time period is 1996-2001. Standard errors in parentheses under coefficients are robust to arbitrary heteroskedacity and autocorrelation.

**Table 4: Inside Wage experiments**

Dependent variable	Ln(AMI Death Rate)	Ln(AMI Death Rate)	Ln(AMI Death Rate)	Ln(AMI Death Rate)	Ln(Productivity)	Ln(Productivity)	Ln(Productivity)	Ln(Productivity)
Estimation technique	OLS	Long Differences	GMM-SYS	GMM-SYS	OLS	Long Differences	GMM-SYS	GMM-SYS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ln (Area outside pay)	0.404*** (0.126)	0.791** (0.378)	0.428** (0.174)	0.427** (0.173)	-0.662*** (0.144)	0.193 (0.265)	-0.548*** (0.172)	-0.550*** (0.180)
Average inside wage	-0.282*** (0.101)	-0.129 (0.156)	-0.338** (0.169)		0.071 (0.115)	0.071 (0.115)	0.244** (0.126)	
Predicted ln(inside wage using NPRB IV)				-0.352 (0.719)				0.226 (0.342)
Physicians share	-0.507 (0.341)	-1.091 (0.691)	-1.796 (1.242)	-2.131* (1.284)	3.741*** (0.390)	1.990*** (0.467)	4.127*** (0.930)	3.949*** (0.903)
Nurses share	-0.299 (0.224)	-0.528 (0.563)	-0.907 (0.822)	-1.000 (0.857)	0.358* (0.207)	0.591 (0.338)	1.689*** (0.607)	1.742** (0.626)
(omitted base is unqualified nurses/health care assistants)								
SC(1) p-value			0.000	0.000			0.002	0.004
SC(2) p-value			0.197	0.201			0.440	0.478
Hansen- p-value			0.809	0.729			0.083	0.033
Hospital fixed effects	No	No	Yes	Yes	No	No	Yes	Yes
Casemix controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies (6)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region dummies (10)	Yes	No	Yes	Yes	Yes	No	Yes	Yes
Hospitals	211	211	211	211	211	211	211	211
Obs	901	901	901	901	901	901	901	901

**Notes:** \*significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Quality is measured by the within hospital deaths within 30 days of emergency admission for acute myocardial infarction (AMI) for over 55 year olds admitted with AMI to the hospital. Productivity measured as the number of Finished Consultant Episodes (FCEs) per whole-time equivalent (WTE) employee; casemix controls are the same as in Table 2 (for AMI regressions) and Table 3 (for productivity). All regressions control for area mortality rates, (lagged) employment size and hospital type (i.e. whether the acute hospital was a specialist hospital, teaching hospital or “normal” acute hospital). System-GMM estimates with one-step robust estimates are presented. Physician share, nurse share, employment size and lagged inside wage (except in columns (4) and (8) where we exclude this variable) are treated as endogenous. Outside wage is lagged and treated as exogenous. Instruments in the differenced equations are levels of own values t-2 through t-5; instruments in the levels equations are once lagged differences. We also use the lagged predicted inside wage as an instrument.  $SC(k)$  is Arellano-Bond (1991) test of serial correlation of order  $k$  of the first differenced residuals. Sargan-Hansen is a test of the over-identifying restrictions. Time period is 1996-2001. Predicted inside wage using NPRB IV uses the predicted regulated wage as described in the text (coefficients and standard errors divided by 10).

**Table 5: The role of agency staff expenditure in accounting for the impact of the labour market on hospital quality and productivity**

Dependent variable	Ln(Agency)	Ln(AMI)	Ln(AMI)	Ln(AMI)	Ln (productivity)	Ln (productivity)	Ln (productivity)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Ln (Area outside pay)	2.858** (1.138)		0.314* (0.172)	0.174 (0.201)		-0.805*** (0.182)	-0.729*** (0.194)
Ln(Inside Pay)	0.077 (1.045)		-0.491*** (0.153)	-0.477*** (0.161)		0.221 (0.134)	0.295** (0.141)
Ln(Agency)		0.057** (0.026)		0.046* (0.024)	-0.107*** (0.027)		-0.057*** (0.018)
Hospital fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies (6)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region dummies (10)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SC(1) p-value	0.886	0.003	0.002	0.006	0.006	0.011	0.043
SC(2) p-value	0.132	0.982	0.490	0.922	0.356	0.485	0.941
Hansen p-value	0.390	0.325	0.637	0.312	0.384	0.356	0.476
No. of hospitals	176	176	176	176	176	176	176
Observations	523	520	520	520	520	520	520

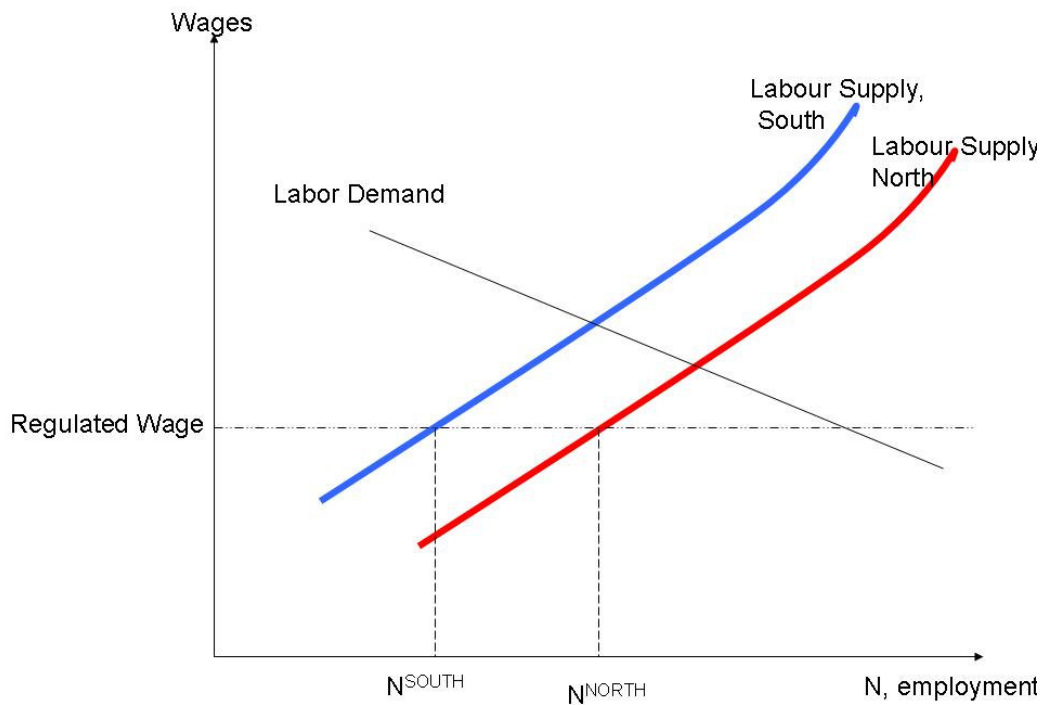
**Notes:** \*significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. “Agency” Expenditure is the share of total staff expenditure that is accounted for by expenditure on non-NHS nursing staff. Productivity measured as the number of Finished Consultant Episodes (FCEs) per whole-time equivalent (WTE) employee. Quality is measured by the within hospital deaths within 30 days of emergency admission for acute myocardial infarction (AMI) for over 55 year olds admitted with AMI to the hospital. Physician share, nurse share, employment size, agency (except in columns (3) and (5) where agency is excluded) and inside wage (except in columns (2) and (5) where we exclude inside wages) are treated as endogenous. All columns are estimated by System GMM (instruments in the differenced equations are levels of own values t-2 through t-5; instruments in the levels equations are once lagged differences). Column (3) is an identical specification to column (3) of Table 4 and column (6) is an identical specification to column (6) of Table 5 except we estimate on the sub-sample where we observe agency staff expenditure. All standard errors are robust to arbitrary heteroskedacity and autocorrelation; in the System-GMM estimates one step robust estimates are presented and all staff variables are treated as endogenous (outside wage is lagged and treated as exogenous). SC(*k*) is Arellano-Bond (1991) test of serial correlation of order *k* of the first differenced residuals. Sample size is larger in column (1) because we do not condition on lagged agency as we do in other columns.

**Table 6: Robustness Tests - coefficient (standard error) on outside wage**

Dependent variable	Ln(AMI) (1)	Ln(Productivity) (2)	Observations
1 Baseline	0.455** (0.176)	-0.552*** (0.181)	901
2 Additional casemix controls	0.424*** (0.173)	-0.556*** (0.153)	900 (for AMI) 892 (for productivity)
3 Include hospital financial surplus	0.394** (0.182)	-0.517*** (0.184)	745
4 Include lagged dependent variable: long-run [p-value]	0.504*** [0.010]	-0.572*** [0.020]	901
5 Drop Inner and Outer London	0.298** (0.157)	-0.391** (0.173)	776
6 Drop big jumps in outside wage	0.534** (0.198)	-0.623*** (0.167)	885
7 Balanced Panel	0.601*** (0.212)	-0.612*** (0.162)	582
8 Regional outside wage	0.595 (1.024)	-0.456 (0.594)	901
9 Regional outside wage (drop regional dummies)	0.521*** (0.172)	-0.315** (0.101)	901
10 Include alternative total hospital employment measure	0.399** (0.168)	-0.540** (0.170)	901
11 Include higher order and cross product terms in skill shares	0.539*** (0.201)	-0.637*** (0.181)	901

**Notes:** \*significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Each cell reports the coefficient and robust standard error from a separate GMM regression. Productivity measured as the number of Finished Consultant Episodes (FCEs) per whole-time equivalent (WTE) employee; Quality is measured by the within hospital deaths within 30 days of emergency admission for acute myocardial infarction (AMI) for over 55 year olds admitted with AMI to the hospital. All regressions have the same System GMM estimation and specifications as follows: column (1) equivalent to Table 2 column (3); column (2) equivalent to Table 5 column (3) (for the AMI regressions); column (3) equivalent to column (3) of Table 3 and column (4) equivalent to column (7) of Table 5 (for the productivity regressions). Outside wage is (lagged) area wage unless elsewhere specified. See text for exact experiments.

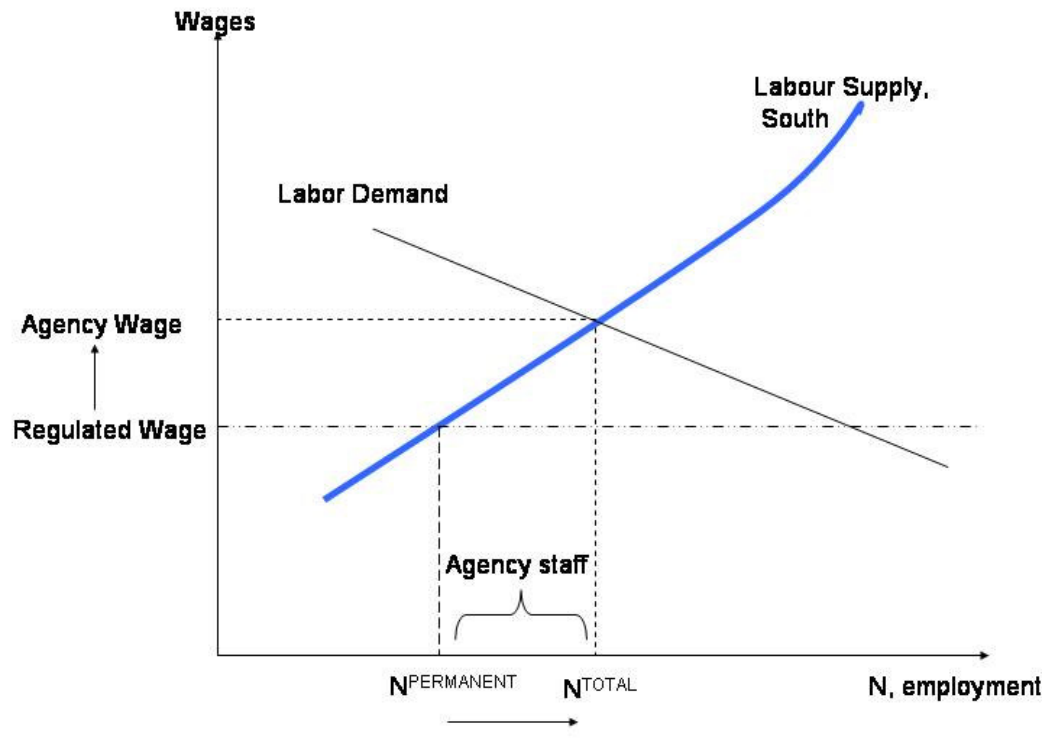
**Figure 1: Hypothetical Impact of regulated wage on the labor market**



**Notes:** The theoretical impact of a nationally regulated wage on NHS labor supply in two areas with different outside labor markets. South (e.g. London area) has a stronger outside labor market with higher alternative wages than North, so the supply curve lies to the left. A single nationally fixed wage (so long as it is below the competitive level) will result in a lower number of employees in the South than in the North.

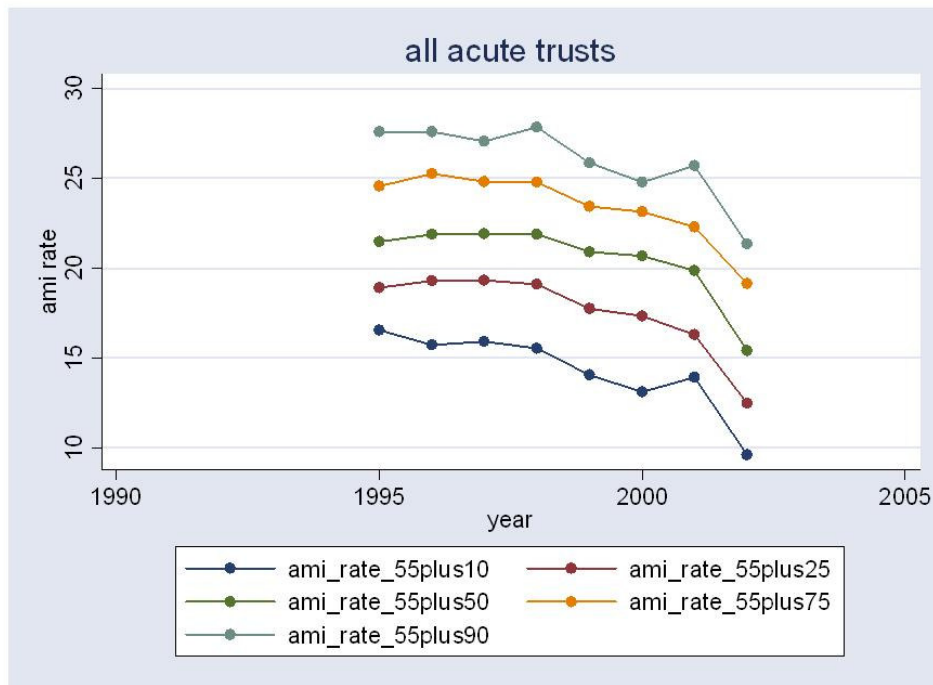


**Figure 2: The role of private sector temporary agency nurses**



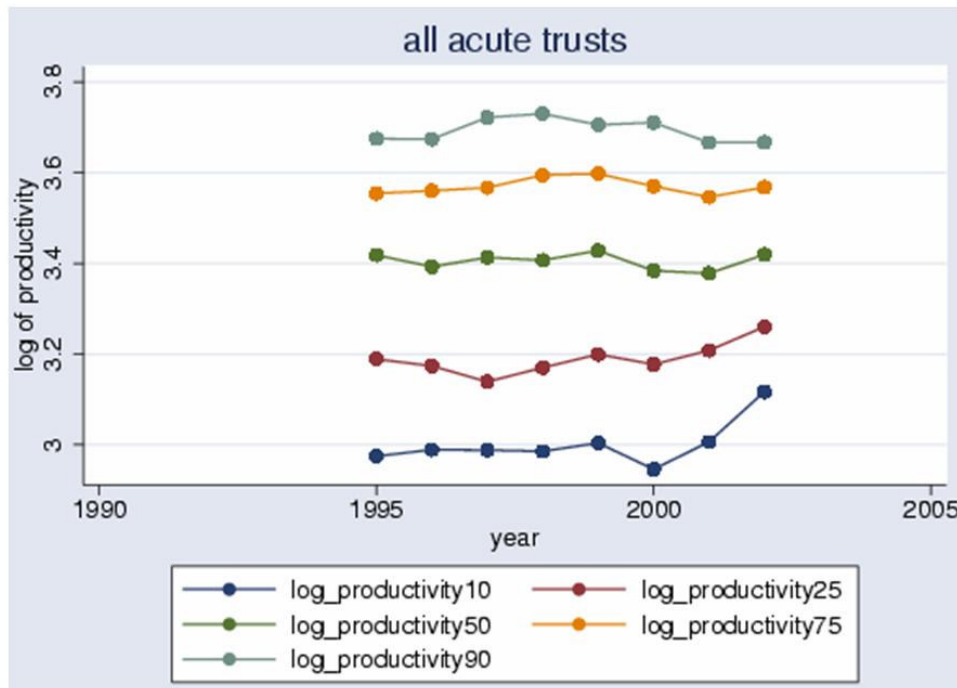
**Notes:** This describes the possible reaction of hospitals to a mandated wage when there is also the possibility of hiring temporary agency staff whose wages are not restricted by the regulation mandated to permanent staff. In the face of a regulated wage and a competitive market for agency nurses, the agency wage will rise and employment will from  $N^{\text{PERMANENT}}$  to  $N^{\text{TOTAL}}$

**Figure 3: The distribution of Death rates from AMI across hospitals, 1995-2002**



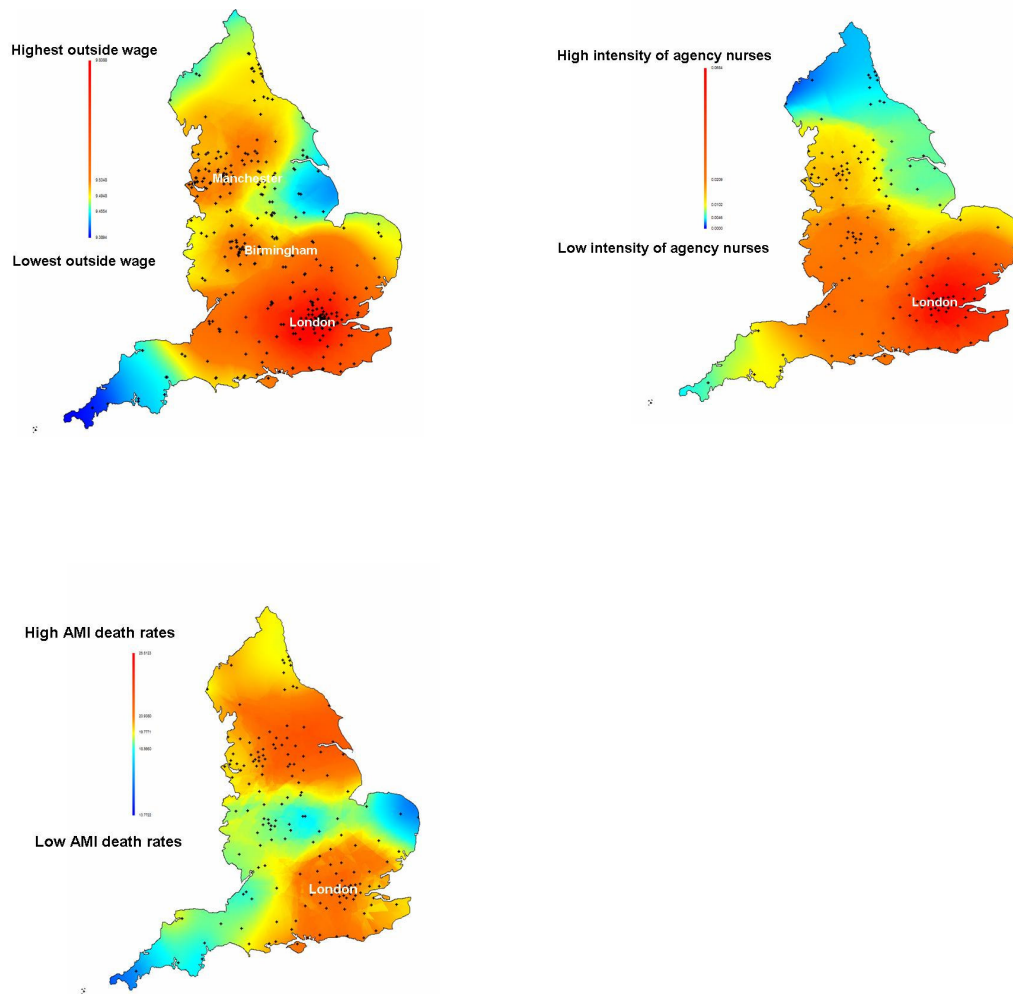
**Notes:** Data for full panel of acute hospital trusts. The top line shows the evolution of AMI death rates at the 10<sup>th</sup> percentile (highest death rates) and the bottom line the evolution of death rates at the 90<sup>th</sup> percentile (lowest death rates).

**Figure 4: The distribution of Productivity (Finished Consultant Episodes per clinical staff member) across hospitals, 1995-2002**



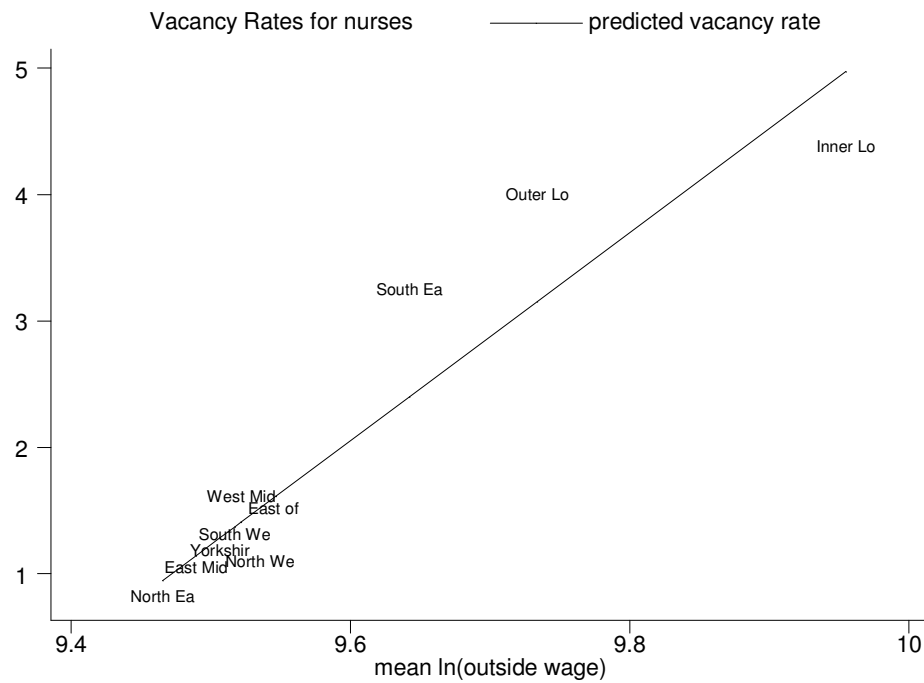
**Notes:** Data for the full sample of acute hospital trusts. The top line shows the evolution of  $\ln(\text{productivity})$  at the 90<sup>th</sup> percentile (highest productivity) and the bottom line the evolution of  $\ln(\text{productivity})$  at the 10<sup>th</sup> percentile (lowest productivity).

**Figure 5: Distribution of outside wages, intensity of use of agency nurses and AMI death rates in England**



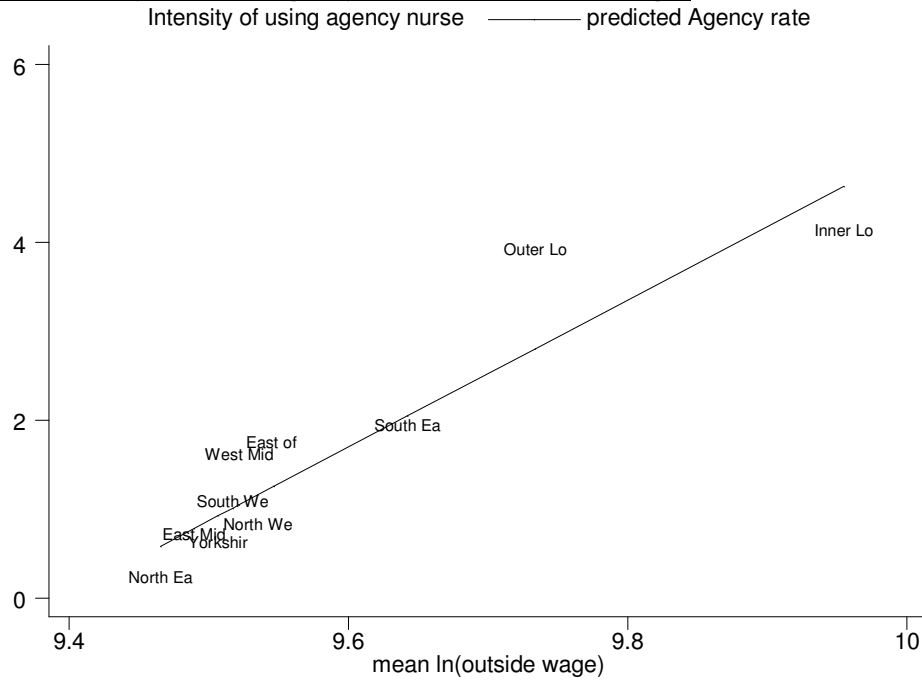
Notes: Data are 1996-2001 averages. Outside pay is the average log wages of all female non-manual workers (from New Earnings Survey). Intensity of use of agency nurses is the proportion of employees who are agency staff. AMI rates are within hospital deaths within 30 days of emergency admission for acute myocardial infarction for over 55 year olds admitted with AMI to the hospital.

**Figure 6: Nurse Vacancy Rates and outside Wages**



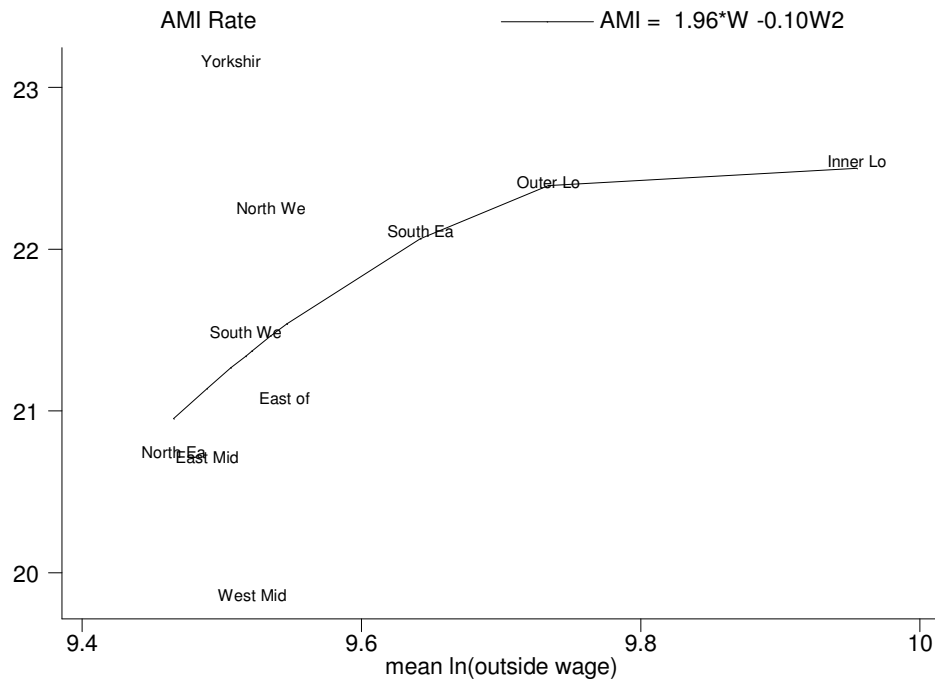
**Notes:** Each observation is one of the ten regions in England (the average region has 4.9m people and we have a 1% example of workers from the New Earnings Survey). Outside pay is the average log wages of all female non-manual workers (from New Earnings survey). Vacancy rates are the proportion of nurse posts that have been vacant for three months or more (Office of Manpower Economics, 2003). 1996-2001 averages. The straight line is the predictions from a linear regression.

**Figure 7: Intensity of use of agency nurses and outside wages**



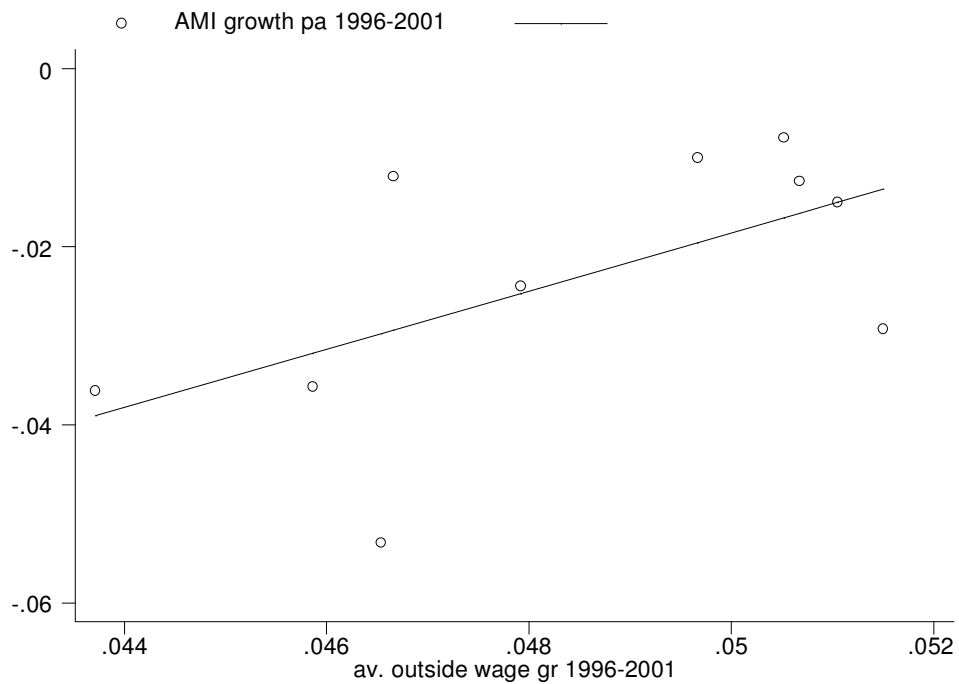
**Notes:** Each observation is one of the ten regions in England. Outside pay is the average log wage of all female non-manual workers. Intensity of use of agency nurses is the proportion of employees who are agency staff. 1996-2001 averages. The straight line is the predictions from a linear regression.

**Figure 8: AMI Death Rates and outside wages**



**Notes:** Each observation is one of the ten regions in England. Outside pay is the average log wage of all female non-manual workers. AMI rates are within hospital deaths within 30 days of emergency admission for acute myocardial infarction for over 55 year olds admitted with AMI to the hospital. 1996-2001 averages. The curved line is the predictions from a linear regression of AMI death rates on the level and square of the ln(outside wage).

**Figure 9: Changes in AMI Death Rates and Changes in outside wages**



**Notes:** Each observation is one of the ten regions in England. Outside pay is the average log wage of all female non-manual workers. AMI rates are within hospital deaths within 30 days of emergency admission for acute myocardial infarction for over 55 year olds admitted with AMI to the hospital. The variables are the growth rates between 1996 and 2001. The straight line is the predictions from a linear regression.



## **Appendix A: Short literature view of the role of staff in hospital production and the responsiveness of medical labor supply to wages**

### *The impact of medical staff on hospital output*

There is a growing literature on the importance of nurses in hospital production, though less focus on the impact of physicians. Jensen and Morrissey (1986) is one of the few papers that focus directly on the impact of physicians on hospital production. They find that increases in physicians increased hospital output but by less than increases in nurses (although by more than medical residents). This is surprising as physicians have longer in training and therefore are likely to have higher human capital. However, since the period covered by the data in this study, the amount of training received by nurses has risen and there has been considerable technological change in hospital production.

Most of the literature on the impact of nurses on patient outcomes is from the USA (see Curtin, 2003, for a recent review). Cross sectional studies tend to find a significant effect of nurse labor on outcomes. For example, Aiken et al. (2002) using a large cross section of acute care Pennsylvanian hospitals, examine the relationship between two measures of patient care (deaths within 30 days of admission and 'failure to rescue') and nurse staffing ratios (the average number of patients in a nurse's workload). They find an effect of nurse staffing on both outcomes: the odds of patient mortality increased by 7% for every additional patient in the average nurse's workload in the hospital. Aiken et al. (2003) finds a positive relationship between education level and patient mortality Needleman et al. (2004) study over 5 million patient discharges from 799 hospitals in 11 states and use several measure of patient outcomes and hours of nursing care per day (adjusted for the severity of the casemix). They found a strong and consistent relationship between hours of nurse staffing and five outcomes for medical patients. These – and most - studies on the impact of nurses on quality of care use cross sectional data, so are unable to control for unobserved heterogeneity between hospitals. Mark et al (2004) is one of the few studies using panel data. They find few statistically significant relationships between nurse staffing and hospital mortality.

### *Is the labour supply of health workers responsive to labour market conditions?*

There are relatively few studies of the impact of the labour market conditions on physicians and US based ones focus on physicians in private practice. Older examples include Sloan (1975) and Noether (1986). More recently, Rizzo and Blumenthal (1994) examine the impact of wage and non-wage income for a sample of self employed US physicians. They find uncompensated wage elasticity for male physicians of 0.23. Showalter and Thurston (1997) distinguish between those in self-employment and those who are employees. They find a wage elasticity of around 0.33 for all groups, but one that is essentially zero for the employed and is around 0.6 for self-employed solo practitioners. One recent paper has studied physicians employed in the public sector in an institutional setting not dissimilar to the UK. Baltagi et al (2003) use panel data on 1303 Norwegian male hospital physicians, covering the period 1993-1997. Using GMM techniques, they find long run wage coefficients of around 0.55. These are rather higher than the US estimates: the differences may be due both to institutional settings and to the fact the Baltagi et al (2003) study is one of the few to use panel data.

Annazzo et al (2003) review empirical evidence of nursing labour supply. The estimates (mainly from North American studies) display a large degree of variation. Most of the studies are of hours, rather than participation per se. The hours margin is relatively inflexible in the UK as there is little choice in the number of hours worked per week in the NHS. Skatun et al (2005) and Frijters et al (2003) both use longitudinal data from the Quarterly Labour Force Survey (QLFS). Frijters et al (2003) look at quitting decision and conclude that wages have a small effect relative to non-wage factors. Skatun et al (2005) look at labor market participation (but not at occupational choice between nursing and other in work options). Both these papers find that the wage elasticity of participation is below unity; the estimates in Frijters et al are less than under 0.1.

On the other hand, Gosling and Van Reenen (2005) use the structure of pay determination for public sector nurses to identify the impact of wages on participation in nursing. This approach deals with the endogeneity of wages by using regulatory decisions as an instrumental variable for the observed wage in a (selection adjusted) participation equation. They also allow for the participation choice to include working in another profession, rather than simply to be not-working. They find estimates of the

elasticity of wage on employment elasticities are biased downwards in OLS and reasonably large in their IV results, typically around unity. And research in another setting in which wages for nurses are set centrally – Norway – which uses panel data also finds nurses' labour supply responds to wages (and other factors): see Askildsen et al (2002) and Holmas (2002).

Finally, Elliot et al (2007) uses a sample of English hospitals for the years 1999-2002 and examines the association between the average (over the four years) wage gap between nurses pay and that of comparable women at regional level and the nursing vacancy rate at hospital level. They find that higher levels of the wage gap are positively associated with higher vacancy rates. They do not exploit the panel nature of their data, so do not control for heterogeneity across hospitals.

## **Appendix B: Data Description**

Data sources are in table A1.

### *Sample of hospitals*

We use data from financial year 1995/6 onwards. Trust data pre-1995/6 is only for those hospitals that had been given freestanding financial status within the NHS prior to that date. By 1995/6 almost all hospitals had freestanding status; before that date the finances (and so expenditure data) for some were still recorded at District Health Authority level. AMI episodes (from HES) were subject to recoding (a change in ICD codes) in 1994/5.

### *Use of AMI as a measure of quality*

We use the "30-day" death rate for acute myocardial infarction (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, 1999). Propper et al (forthcoming) concludes that raw UK hospital level rates exhibit considerably less variability than the raw US data, but not than the US rates which have been 'filtered' to reduce noise. To reduce misclassification based on small sample sizes, we omit all hospitals with less than 150 emergency AMI admissions in any one year

The second issue is that we use the thirty-day rate itself. The seven day rate was not available until 1999, but it is highly correlated with the thirty day rate and results using this as the dependent variable show similar patterns. We were also concerned that some patients may die after thirty days and we are missing these deaths. Examination of the distribution of AMI deaths in hospitals from other sources, however, shows that about half of deaths from AMI occur within the *first day* of admission (see Table A2 below). 98% of the deaths occur within the first thirty days. Consequently the thirty day window is more than adequate.

The third issue arises because our measure is the death rates within a hospital. Deaths occurring after transfer to another provider are credited to the provider where the patient was first admitted, whilst deaths following discharge are omitted. This may bias the results if hospitals have a motive to discharge early. Such incentives would have been small as these death rates were not published until 1999 and hospitals not ranked by the Department of Health in terms of outcomes until 2001, when they were ranked on a composite bundle of over twenty indicators. Finally, in richer areas 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 in hospital deaths.

### *Wages*

To get an idea of the regulated pay structure consider nurse pay scales at 1<sup>st</sup> April 1999. Clinical grades range from A to I and correspond to spinal points 3 to 37. For example, Clinical Grade G, a "ward sister" corresponds to a grade between spinal points 12 (£20,145 per annum) to spinal point 9 (£23,300 per annum). There are allowances (or "weightings") for being in high cost areas. For Inner London this was £2205 plus 5% of salary up to a maximum of £750, for Outer London this was 1570 plus 5% of salary up to a maximum of £750 and for the "fringe" (various areas in the South East) this was £285 plus 2.5% of salary up to a maximum of £375. For a ward sister on 23,300 a year working in the most expensive area of the UK, Inner London, and the extra regional allowance would be worth only 11%

more salary ( $2205+750 / (2205+750+23,300)$ ). Since this is capped, for a more senior nurse on a higher salary the proportional value is lower. By contrast, in 1999 in the NES the annual non-manual wage in Inner London is about 65% higher than that of the Northeast, the lowest wage region.

In calculating the instrumental variable for inside pay measure we take into account the NHS grade structure in a region in a year (using wage data from the NES). We then use the decisions of the NPRB over the changes in the wage structure taking into account all the London weightings, etc (which may differ by grade) to form the predicted wage in the next period (specific to each region).

#### *HES Data*

HES data are used for the AMI, productivity and case mix variables. HES are discharged based records of all inpatient 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 healthcare professional within an NHS trust). This is not always the same as a single stay (spell) in hospital, because a patient 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 currently coded according to the International Classification of Diseases, 10th 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 includes further codes – for example, age of the patient<sup>46</sup>.

#### *Case-mix adjustment of FCEs*

To estimate the casemix adjustment for a hospital, all inpatient spells are allocated to a HRG category. An HRG is a code for a group of clinically similar treatments and care that require similar levels of healthcare resources. (An example of an HRG is renal dialysis, separated into haemodialysis and peritoneal dialysis.) HRG codes are derived from ICD-10 and the OPCS 4.2 codes on HES records. A weight representing the expected cost (the reference cost) is attached to each HRG to derive the scalar case mix index for all spells treated over a year for each hospital. The national average case weight is set to equal 100: case mix indices above 100 represent hospitals that have treated a more complex than average mix of cases. The index used here based on reference costs from 1998/99 onwards (when reference costs were first available). Prior to that the cost weights prior are based on expected costs. Because we are concerned about the precise consistency of this variable before and after 1998, we use it only as a robustness check rather than include it in our main specifications.

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<sup>46</sup> <http://www.hesonline.nhs.uk/Ease/servlet/ContentServer?siteID=1937&categoryID=537> (accessed 4 July 2006)

**Table A1: Data sources**

	<b>Source of Data</b>	<b>Years</b>
AMI deaths and admissions rates	Hospital Episode Statistics (HES)	1995-2002
Finished Consultant Episodes (FCEs)	HES	1995-2002
AMI case mix	HES; HRG codes E11 (AMI w/ complications) and E12 (AMI w/o complications)	1996-2002
FCE case mix	HES; index based on HRGs and national reference costs	1995-2001
Whole time equivalents of clinical staff (Physicians, Qualified Nurses Unqualified Nurses; Qualified AHPs; Unqualified AHPs; Health Care Assistants)	Department of Health Medical Workforce Census	1995-2002
Expenditure on agency nurses (and all other clinical staff groups)	Trust financial returns (from Dept Health)	1995-2002
Nurse vacancy rates	Office of Manpower Economics	1996-2001
Local authority directly standardized all cause mortality rates and AMI rates	Office of National Statistics	1995-2004
Outside wage data (regional and area wages)	New Earnings Survey	1995-2001
MRSA rates	Health Protection Agency Communicable Disease Surveillance Centre	2001-2002
Ambulance times	Department of Health, Health Care Statistics	1998-2002
Trust retained surplus and deficits	Trust financial returns TAC01 (from Department of Health)	1997-2002
NPRB IV wage	Gosling and Van Reenen (2005), predicted regional wage based on National Pay Review Body recommendation.	1984-2001

**Notes:** Both NES and NHS years are financial years commencing in April of each calendar year. ONS data are for calendar years.

**Table A2: AMI admissions and in-hospital death rates in 1997/98,  
Over 55s only**

Time until death	Primary diagnosis		Primary or secondary diagnosis	
	Frequency	Percent of total	Frequency	Percent 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 to 5 days	2,111	17	2,511	17
6 to 10 days	1,456	12	1,811	12
11 to 20 days	914	7	1,197	8
21 to 30 days	251	2	336	2
More than 30 days	276	2	362	2
<b>Total known</b>	<b>12,432</b>	<b>100</b>	<b>14,760</b>	<b>100</b>
Unknown	1		1	
<b>Total</b>	<b>12,433</b>		<b>14,761</b>	