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Three Essays on Health and Labor Markets

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Contents

Chapter 1

Introduction

The thesis consists of three parts. The first part (chapter 2) examines the impact of state-level minimum wages on employment outcomes in local labor markets for youth. This chapter uses county-level Quarterly Workforce Indicators (QWI) data for 2003-2007 in a panel regression to analyze the effects of state variation in the minimum wage on employment levels, earnings, and job flows. Controlling for county fixed effects and general time fixed effects, and using a single Census Region and economic area time effects to control for spatial heterogeneity, this chapter finds a negative relationship between the minimum wage and the level of employment for those age 14-18 and 19-21. However, this chapter finds a positive employment effect for workers age 22-24 suggesting the possibility of substitution towards more experienced workers in higher minimum wage jurisdictions, and also finds that the minimum wage is correlated with reduced job turnover for all age groups and faster net job growth for youth between the ages of 22 and 24.

The second part (chapter 3) studies the effects of hospital mergers on the quality of health outcomes for acute myocardial infarction (AMI) patients, patients receiving coronary artery bypass grafting (CABG) surgery, and pregnant women in Pennsylvania during the period 1994-2010. This chapter measures the quality of outcomes as risk-adjusted mortality and readmission for both AMI and CABG patients and as the incidence of preventable complications for pregnant patients, and also uses total charges to measure the resources used for each patient. Using propensity scores to match hospitals involved in mergers with hospitals that have never been involved in a merger, this chapter finds that mergers that occurred in concentrated markets are associated with increased probability of in-hospital mortality and higher readmission rates for AMI patients and with increased probability of preventable complications and reduced resource utilization for pregnant patients. However, there is no evidence showing that mergers in more concentrated areas are associated with worse outcomes for CABG patients. Finally, this chapter finds that the effects of a merger on health outcomes may last for years.

The third part (chapter 4) is the combination of labor and health economics, which is concerned with the question how economic recessions affect the aggregate health outcomes of a population living in the same community. This chapter uses all inpatient data in Pennsylvania for the 2000-2011 period, during which people experienced two recessions, uses county unemployment rates as the primary indicator of recessions, and analyzes the effects of recessions on the percentage of total population in a community that are hospitalized for certain stress-related diseases, while controlling for the community's socio-demographic characteristics. This chapter finds that recessions significantly increase the risk of hospitalization due to alcohol-related conditions among all communities, but decrease the hospitalization rates for AMI and stroke among the high-population-density and high-poverty communities, respectively. Finally, there is no evidence showing any persistent effects of recessions on health.

Chapter 2

Impact of the Minimum Wage on Youth Labor Markets

2.1 Introduction

Efforts to measure the labor market effects of cross-state differences in the minimum wage have yielded a range of estimates. Case studies using difference-in-difference methods often find positive employment effects following changes in the minimum wage on one side of a state border. Panel data studies seem to confirm the old consensus that a 10% higher minimum wage would result in 1% to 3% fewer jobs for affected workers. But these estimated negative employment effects often disappear when controls for spatial heterogeneity are included.

This paper reexamines the effect of the minimum wage on labor market outcomes by focusing on local labor markets for youth. We concentrate on the five states in the Great Lakes region and use separate time effects for each Bureau of Economic Analysis (BEA) Economic Area (EA) in that region to control for spatial heterogeneity. We find evidence that the level of the state minimum wage is negatively correlated with teenage employment levels, with low elasticity estimates in the 0.1 to 0.3 range, but positively correlated with the employment of young workers between the ages of 22-24. We also examine dynamic aspects of youth labor markets and find that accessions, separations and turnover rates are lower in local labor markets with higher minimum wages for all youth. While we find there is no minimum wage effect on net job growth for teens in our sample, there is a positive effect for older youth in these markets.

The rest of the paper proceeds as follows. Section 2.2 briefly reviews the literature, with a focus on identifying the methodology and different conclusions about minimum wage effects on employment. Section 2.3 describes the data and how we construct our sample. Section 2.4

presents the empirical model. Section 2.5 provides the main results. Section 2.6 concludes.

2.2 Backgroud

What are the effects of higher minimum wage levels on labor market outcomes? This is a question that has attracted the attentionof many economists. Previous studies, both theoretical and empirical, have generally reached a variety of conclusions about the effects on the employment level. The large empirical literature, mainly looking at teenage workers or those employed in restaurants or other establishments likely to be affected by the minimum wage can be divided into four clusters, ordered chronologically.

First, by the early 1980s, most studies of the effects of time series variation in the national minimum wage reported results suggesting that higher minimum wages decreased employment opportunities for low wage workers. Browns influential survey (1982) identified a consensus finding that the minimum wage elasticity of teenage employment ranged from -0.1 to -0.3 in time series studies using Current Population Survey data with varying sample periods and specifications.

Second, starting in the 1990s, the results of the "new minimum wage research",[1] which tended to study cross-state differences, raised questions about the effects on low-wage employment of a higher minimum wage (Neumark and Wascher, 2007). Case studies of cross border differences following an increase in a states minimum wage level (Card and Krueger 1994 and 1995) often find a positive and statistically significant effect of the minimum wage on employment in low-wage labor markets. This effect can be explained, in part, by assuming that employers of low-wage workers have market power and act as monopsonistic buyers of labor.

Third, panel studies, employing national-level longitudinal data on individuals or time-series data for a cross-section of geographic areas, often find a negative correlation between employment and the minimum wage.[2] A good example of such panel models is that specified in Neumark and

[1] The new minimum wage research began in November 1991, when there were an innovative set of studies on the effects of the minimum wage presented and discussed in "New Minimum Wage Research Conference". And a special issue of the Industrial and Labor Relations Review (ILRR) was published in the early 1990s. (Neumark and Wascher, 2007)

[2] Generally speaking, nationwide individual-level data, such as the Current Population Survey (CPS), can provide the worker-level demographic information to estimate the employment effect by age, gender or race; time-series and cross-section data (the geographic data) can incorporate both state and time variation in minimum wages. For example, Quarterly Census of Employment and Wages (QCEW) and Quarterly Workforce Indicators (QWI) can provide a full census of quarterly count of employment and wages, available at the county, MSA, and state levels

Wascher (1992) and written in equation (2.1). Here E_{it} represents employment in state i at time t, MW_{it} is the minimum wage in state i at time t and X_{it} includes control variables. The equation also includes year dummies τ_t to control for state-invariant time effects and state dummies ϕ_i to capture state-specific, time-invariant unobserved characteristics:

$$E_{it} = \beta_0 + \beta_1 MW_{it} + X_{it}\beta_2 + \phi_i + \tau_t + \varepsilon_{it}, \tag{2.1}$$

Neumark and Wascher (2008) conclude that panel studies focusing on cross-state variation with time and state fixed effects suggest renewed support for the "consensus" employment elasticity estimate of -0.1 to -0.3.

Several recent papers of the employment effects of cross-state variation in minimum wage levels reinforce the conclusion of this third cluster of studies. Thompson (2009), using Quarterly Workforce Indicators data for 1996-2000, evaluates how state differences in minimum wages affect teenage employment at the county level. By referring to quintiles of teen average quarterly earnings, he identifies high-impact counties (where the minimum wage is most likely to affect teenage markets) and low-impact counties (where prevailing wages exceed the minimum). Using difference-in-difference estimations, he demonstrates that the employment elasticity in the high-impact counties ranged between -0.26 and -0.37. In addition, using the same DID model with an alternative dependent variable (hiring), he shows that the teen share of new hires (THS) declined markedly following a minimum wage increase.

Sabia (2009) points out that industry studies narrowly based on sub-sectors of the retail sector, such as the fast-food restaurants in many case studies, may not capture the minimum wage effects across the entire retail sector. Using monthly data from the 1979-2004 CPS, Sabia supplements the model in equation (2.1) by including a fixed effect for each month to capture unmeasured seasonal employment patterns. He finds that the employment elasticity in retail trade is -0.1, while the adverse employment effects are much larger, with estimated elasticities of -0.34 to -0.38, for teenagers in retail sectors.

Taking into consideration that a change in the minimum wage in a state affects employment not only in its own state but also in neighboring states through spatial dependence and spatial

by industry.

spillovers, Kalenkoski and Lacombe (2011) use annual average state level panel data from the BLS for 1990-2004 and employ a Spatial Autoregressive (SAR) model to estimate the teen employment elasticity relative to the real effective minimum wage at -0.21.

Sabia et. al. (2012) use 2004 and 2006 CPS data to examine the effects of a minimum wage increase in New York State on the employment rates of 16-to-19-year-old workers without high school diplomas. They employ a difference-in-difference method, which is similar in spirit to the case studies by Card and Krueger (1994 and 1995), with a control group of similar workers in nearby states that did not experience a rise in the minimum wage over the same period. They find that a median employment elasticity of the minimum wage is around -0.7, which is larger than the previous consensus of -0.1 to -0.3.

The fourth cluster of minimum wage studies again questions the existence of a consensus on negative employment effects from minimum wages once controls for spatial heterogeneity are included in panel regressions. Dube, Lester, and Reich (2010) generalize the case study method by estimating the employment effects of state variation in the minimum wage in a panel of contiguous border county-pairs in the United States over the years from 1990 to 2006. They find that the fixed-effects specification in the canonical model presented in equation (2.1) cannot account for trends in employment prior to the increase in the minimum wage and that spatial heterogeneity may have a time-varying component. To control for this they add a separate pair-specific time effect τ_{pt} for each cross border county pair in their sample to their regressions. Unlike much of the literature from panel studies after the 1990s, they find that an estimated negative employment effect associated with a higher state minimum wage in estimates of models like equation (2.1) disappears when the pair-specific time effect is included in their regressions.

Taking a different approach to controlling for spatial heterogeneity, Allegretto, Dube and Reich (2011) estimate the effects on teen earnings and employment with CPS panel data for the period 1990-2009. Each observation in their sample is at individual level i, in state s and time t. They add a Census division-specific time effect τ_{dt} to sweep out the variation across the nine Census divisions and a state-specific linear trend $\varphi_s t$ to capture long-run growth differences across states and conclude that the estimated employment elasticity with these controls in the model is indistinguishable from zero.

Finally, Addison, Blackburn, and Cotti (2009) examine minimum wage effects on employment in low-wage subsectors of the retail trade industry., Their approach to controlling for spatial heterogeneity is to include a county-specific time trend in the error term to sweep out a county-specific linear trend, and then use de-trended data to estimate models similar to equation (2.1). They find evidence of modest (but robust) positive employment effects in many sectors, and explain this by monopsony and efficiency wages.

Neumark et al. (2013) provide a detailed critique of these approaches to controlling for spatial heterogeneity. They argue that the strategy of limiting identification of the minimum wage effect to within-area or relative-to-area-trend variation leads to neglect of valid information. Still this cluster of minimum wage studies raises an important question about the evidence for minimum wage effects in local labor markets. We compare results using the panel data approach with estimates that incorporate controls for spatial heterogeneity to add additional information on this issue.

The literature has focused considerable attention on the impact of a minimum wage on the level of employment. Far less attention has been paid to the impact on labor market flows. Thompson (2009), as noted above, provides evidence that the teen share of new hires is significantly lower in counties where the minimum wage affects the local wage structure. Dube, Lester and Reich (2011) estimate that the number of hires, the number of separations and the turnover rates for teens and restaurant workers are significantly lower in counties with higher minimum wage levels, even controlling for spatial heterogeneity. This finding is similar to that of Portugal and Cardoso (2006) in their analysis of the mid-1980s change in the minimum wage in Portugal. They conclude that a higher minimum wage reduces the teen share of accessions in continuing and new firms, and sharply reduces the share of teenagers in job separations from continuing firms. We also examine the effect of the minimum wage on such labor market flows as well as net job growth, again comparing results from a typical panel model with those from a model incorporating controls for spatial heterogeneity.

2.3 Data

We use data from the Quarterly Workforce Indicators (QWI) from the Longitudinal Employer-Household Dynamics (LEHD) program at the U.S. Census Bureau.[3] The QWI are built on wage records in the Unemployment Insurance (UI) system and information from the Bureau of Labor Statistics Quarterly Census of Employment and Wages (QCEW, formerly known as ES-202[4]). The QWI data provide employment levels, employment flows (accessions, separations and turnover rates), job creation and destruction, and average earnings for demographic subgroups (age and gender) by different levels of geography: state, county, metro, and workforce investment area, as well as by detailed industry. We measure the minimum wage at the state level as the higher of the federal or state minimum and include the total population each year at the county level[5] in the QWI panel dataset for all counties in the five states of the Great Lakes or East North Central region, from 2003 through 2007.

The QWI system can apply the job concepts to both full-quarter and point-in-time (not lasting the full quarter) levels. Take employment, for example. Employment for a full quarter assumes that the individual has been continuously employed throughout the quarter with the same employer, i.e., he has valid UI wage records in the current quarter, the preceding quarter, and the subsequent quarter. Employment at a point in time can be divided into two types in the QWI at the beginning of the quarter and at the end of the quarter. When the individual has valid UI wage records for the current and the preceding (subsequent) quarter she is defined as employed at the beginning (end) of the quarter (Abowd et al., 2005). In our paper, we use the end-of-quarter employment count as the measurement of point-in-time employment.

Following a similar definition, the QWI also provides information about earnings, accessions, separations, job creation, job destruction, and net job flows, at the full-quarter and point-in-time levels, respectively. To accurately estimate the minimum wage effects on teenage workers, we

[3]The QWI variables, including employment, job creation, job loss, net job flows, accessions, separations, and average monthly earnings for both full-quarter and point-in-time jobs can be accessed at the county level in each state separately, which are published by the LEHD Program at the U.S. Census Bureau. http://lehd.did.census.gov/led/ For our study, the full public-use QWI data were accessed through the Cornell Institute for Social and Economic Research, using the VirtualRDC @ Cornell. http://www2.vrdc.cornell.edu/news/data/qwi-public-use-data/

[4]The ES-202 program, also known as the Covered Employment and Wages (CEW) program, includes the employer reports based on information from each states Department of Employment Security.

[5]Intercensal Estimates of the Resident Population for Counties, United States Census Bureau. http://www.census.gov/popest/data/intercensal/county/CO-EST00INT-01.html

prefer to employ the concept of point-in-time jobs and job changes. Thompson (2009) finds that transitory jobs account for nearly half of all teen employment, compared to just over one-fourth of non-teen employment; and he defines transitory employment as the difference between total employment and stable employment (full-quarter employment). The point in time data are also more comparable to other federal statistics that measure labor market situations as of a given day or week each month.

In the QWI system, accessions are divided into two subcategories new hires and recalls. If there are no valid wage records for this job within the last four quarters, then an accession into a job during the current quarter is called a new hire; otherwise, it is a recall. Separations are the number of workers who left the employer during the current quarter. We define the point-in-time turnover rate as the ratio of the average of point-in-time accessions and separations over the end-of-quarter employment: $Turnover = \frac{(EA_{kt}+ES_{kt})/2}{E_{kt}}$.[6]

In the QWI system, dynamic job flows – job creation and destruction – are defined at the employer (establishment) level rather than at an individual level. Jobs are created (destroyed) at the establishment if end-of-quarter employment is greater (less) than beginning-of-quarter employment. We calculate the net job flow rate for point-in-time jobs at the county level as the ratio of net point-in-time job changes across the establishments within the county divided by the end-of-quarter employment.

The QWI data allow us to examine the separate effects of the minimum wage on local labor market outcomes at the county level for workers in three age categories: 14-18, 19-21 and 22-24. Workers in these age groups are most likely to be affected by minimum wage legislation. In 2007 about a fifth of hourly wage earners earning the minimum wage were 16 to 19 years old and nearly half were under 25.[7] And younger workers are also more likely to be constrained in their choice of residence and so limited to employment opportunities within a geographic area (Ihlanfeldt 1990 and Stoll 1999).

The Bureau of Economic Analysis (BEA) defines eight multi-state regions in the U.S.[8] We limit our analysis to local labor markets in a single BEA region as a first level control for spatial

[6]An arbitrary aggregate k = county × age group.

[7]U.S Department of Labor, Bureau of Labor Statistics. http://www.bls.gov/cps/minwage2007.pdf

[8]The eight regions are New England region, Mideast region, Great Lakes region, Plains region, Southeast region, Southwest region, Rocky Mountain region, and Far West region. http://www.bea.gov/regional/docs/regions.cfm

heterogeneity, focusing on the five state Great Lakes region, also known as the East North Central Region in Census data. QWI data for all five states[9] in the Great Lakes region are available for the sample period. Some other regions have incomplete region-wide data, due to the lack of state participation in the QWI program. The states in the Great Lakes region share similar geographic characteristics, common business cycles and comparable levels of economic development (Crone 1998/1999). And there is sufficient time series and cross state variation in the minimum wage within the Great Lakes region (see Table 2.1). But this is not true for some other regions. For example, the minimum wages in most states of the Southeast region and in all states of the southwest region are exact the same as the federal minimum wage, \$5.15, from 2001 to 2006. And even the states neighboring to the Great Lakes region, except for Minnesota, have virtually no changes in the minimum wage from 2001 to 2006.[10]

We control for spatial heterogeneity within the Great Lakes region by allowing for separate time effects for BEA Economic Areas (EAs). BEA Economic Areas consist of one or more economic "nodes"—metropolitan or micropolitan statistical areas—and the surrounding counties that are economically related to these nodes.[11] There are 32 Economic Areas centered in the Great Lakes region. Nine of these Economic Areas are defined to include counties in two or more neighboring states; 39.4 % of the 437 counties in the Great Lakes region are in Economic Areas that overlap state boundaries within the region. So we can take advantage of potential differences in the minimum wage within these Economic Areas as well as time variation within each Economic Area to help identify minimum wage effects in local labor markets.

State minimum wages from 2003 to 2007 in the Great Lakes region are reported in Table 2.1. This is a time period during which the national labor market approached full employment after the 2001 recession and stayed there until the onset of the recession of 2008-2009. The federal minimum of \$5.15 per hour prevailed in all five states in 2003 but legislation adopting higher state minimum wages was enacted in four of the five states at several points during the sample period. The minimum wage in Indiana increased only in the last two quarters of 2007 when the

[9]According to the definition by BEA, the Great Lakes region includes five states: Illinois, Indiana, Michigan, Ohio, and Wisconsin.

[10]The minimum wage for states—North Dakota, South Dakota, Nebraska, Kansas, Iowa, Missouri, Kentucky, and West Virginia—is constant at \$5.15 from 2001 to 2006. The minimum wage in Minnesota rose from \$5.15 to \$6.15 in 2006.

[11]Bureau of Economic Analysis, U.S. Department of Commerce. www.bea.gov/regional/docs/econlist.cfm

federal minimum wage was increased to \$5.85 per hour. Thus differences in federal and state legislation define different minimum wage regimes over time within each state and across states within the region at various intervals in the sample period.

2.4 Empirical Model

Using county-level quarterly data, we examine the effect of the level of the minimum wage prevailing in each state from 2003 to 2007 (the minimum wage is the higher of the federal or state minimum wage level) on labor outcomes for youth in three age groups—teenagers between the ages of 14 and 18, youth between the ages of 19 and 21, and older youth between the ages of 22 and 24 in the Great Lakes region.

We start with what Dube et al. (2010) refer to as the "canonical model" for panel studies of spatial differences in the minimum wage, which is written as equation (2.2) below:

$$ln(Y_{ist}) = \beta_0 + \beta_1 ln(MW_{st}) + \beta_2 ln(EMP_{ist}^{TOT})) + \beta_3 ln(POP_{ist}^{TOT})) + \phi_i + \tau_t + \varepsilon_{ist}, \quad (2.2)$$

where i, s, and t respectively indicate county, state, and quarterly time for all observations. The dependent variables in our paper can be divided into three sets, all of which are measured for three age groups at the point-in-time level: first, the static level of employment and earnings—the natural log of total employment and average monthly earnings; second, employment flows—the natural log of accessions and separations as well as turnover rates, and third, the dynamic job changes—the net job flows rate.

All independent variables have been transformed into natural log form. $ln(MW_{st})$ refers to the natural log of the minimum wage, which is the same for all counties within each state in each quarter. To control for aggregate labor market conditions and relative size of the local labor market, two control variables are added—the natural log of total employment of persons between the ages of 14 and 99 years old at the county level [$ln(EMP_{ist}^{TOT})$] and the natural log of total population at the county level [$ln(POP_{ist}^{TOT})$]. The model also includes county fixed effects ϕ_i and time effects τ_t common to all of the counties in the sample.

To further address unmeasured spatial heterogeneity in the traditional panel data model, we modify this model by adding the specific time effects, $EA \times \tau_t$, for each multi-county Economic Area within the region. Our main focus then is comparing the estimates of β_1 in regressions with equation (2.2) with those obtained from the expanded model in equation (2.3).

$$ln(Y_{ist}) = \beta_0 + \beta_1 ln(MW_{st}) + \beta_2 ln(EMP^{TOT}_{ist})) + \beta_3 ln(POP^{TOT}_{ist})) + \phi_i + \tau_t + EA \times \tau_t + \varepsilon_{ist}. \quad (2.3)$$

2.5 Results

Our main findings about the effects of the minimum wage on the level of employment and earnings for youth in the three age groups are presented in Table 2.2. Panel A shows the results from the traditional panel data model (Equation (2.2)) and panel B reports estimates of the model with EA-specific time effects (Equation (2.3)). Because state minimum wages are the same for all counties within one state, the error term ε_{ist} in equation (2.2) and (2.3) often does not satisfy the basic assumption $\varepsilon_{ist} \sim (0, \sigma^2_\varepsilon)$ in the panel dataset, but instead the idiosyncratic error terms are probably correlated within each state. Therefore, we use clustered standard errors for the estimated coefficients, which allow for an arbitrary pattern of correlation in the error terms across different counties within the same state. All the robust standard errors in brackets in are clustered at the state level for all regressions.

In Column 1 of Table 2.2, we find that the minimum wage is negatively correlated with the teenage (14-18 years old) employment level. By adding EA-specific time effects, our model shows a bigger negative teenage employment effect with the elasticity of -0.21, compared with that of -0.10 based on the traditional panel model. Thus, our estimation of the teenage employment elasticity falls into the range between -0.1 and -0.3 in the consensus of national CPS studies but in contrast to Dube et al. (2010) and Allegretto et al. (2011) who find that the employment elasticity is indistinguishable from zero, after controlling for spatial heterogeneity.

Column (3) in Table 2.2 presents the effects of the minimum wage on employment for 19-to-21-year-old young workers. Based on the model with EA-specific time effect, we find that a higher minimum wage is associated with a significantly lower level of employment for 19-21 year old workers. Again the estimated minimum wage elasticity for this group is strengthened considerably in the regression controlling for Economic Area specific time effects. In contrast with the results for those 14-18 and 19-21, the results in column (5) reveal a positive correlation between the minimum wage and the employment of 22-24 year old workers. This result is statistically significant with a positive elasticity of .104 in Panel B. The results in that panel suggest that the minimum wage has the biggest effect on the youngest workers, slightly smaller but still negative effects on employment for 19-21 year olds, and a positive relationship with employment for the 22-24 year olds. The minimum wage is positively related to the average monthly earnings of workers in all three age categories, with statistically significant results in Panel B, where we are controlling for spatial heterogeneity.

Table 2.3 reports the estimates of minimum wage effects on dynamic job changes for all youth in the Great Lakes region, using the panel data model with Economic Area specific time effects. We find that both accessions and separations for all youth are substantially lower in counties with a higher minimum wage level. The negative effect of the minimum wage on accessions and separations is also seen in lower turnover rates in jurisdictions with higher minimum wage levels. This is consistent with the evidence found by Dube et al. (2011) and Portugal and Cardoso (2006) and suggests that, by raising the wage and making jobs more valuable to their holders, the minimum wage lowers quit rates and spurs greater worker effort leading to fewer dismissals, both of which are consistent with our findings of lower separations with a higher minimum wage. With a given level of employment and a lower separation rate, we would also expect a lower hiring rate since there would be fewer vacancies to fill. Another possible reason for a negative turnover rate elasticity is that a higher minimum wage shifts the employment distribution away from high-turnover, low-wage firms to low-turnover, high-wage ones (Dube, 2011).

Finally, Column 4 presents the estimated coefficients for the minimum wage in regressions with the net job growth rate as the dependent variable. County job growth rates aggregated across establishments for workers 14-18 and 19-21 are not significantly affected by the level of the minimum wage. On the other hand, counties with a higher minimum wage exhibit faster job growth for workers age 22-24 in our sample. This reinforces our conclusion that higher minimum wages improve labor market outcomes for this group of workers in our sample perhaps because of substitution toward older, more experienced workers in areas with higher minimum wages.

2.6 Conclusions

As revealed by the wide ranging results reported in the myriad empirical studies of minimum wage results, the impacts of this policy initiative are complex and vary considerably by time, place and worker group. We find that state level differences in the minimum wage are associated with higher wages, lower employment levels and lower turnover rates for workers age 14-18 and 19-21 in the five-state Great Lakes region during the period from 2003 to 2007 when the national labor market could well be described as at full employment. In contrast with several recent studies, our estimates of these effects are strengthened when we control for unmeasured spatial heterogeneity by including separate time effects for multi-county Economic Areas in the regressions. The elasticity of employment for these two age groups with respect to the minimum wage falls into the range of -.1 to -.3—a range that is often found in studies that do find negative employment effects.

However, when we focus on workers in the age group 22-24 we find that higher minimum wage levels are associated with higher employment levels and faster net job growth as well as higher wages and reduced turnover. Again, the results are stronger in both the size of the estimated coefficients and the level of statistical significance in the regressions using our control for unmeasured spatial heterogeneity. While positive employment effects are often attributed to monopsony in labor markets, our differing results by age category suggest the possibility of labor-labor substitution, with higher minimum wage levels shifting employment away from the youngest workers toward older, more experienced and, perhaps, reliable workers.

The normative analysis of minimum wage legislation has often pointed to offsetting wage and employment effects from this policy intervention. Like a few studies in the literature, our results also suggest an additional positive impact of minimum wage policy through a statistically significant decrease in labor turnover and, presumably, the associated costs of recruiting, hiring and training workers to fill vacancies.

2.7 Tables and Figures

Table 2.1: State Minimum Wages in the Great Lakes Region

State	2003	2004	2005	2006	2007Q1Q2	2007Q3Q4
Illinois[a]	$5.15	$5.50	$6.50	$6.50	$6.50	$6.50
Indiana[b]	$5.15	$5.15	$5.15	$5.15	$5.15	$5.85
Michigan[b]	$5.15	$5.15	$5.15	$5.15	$6.95	$6.95
Ohio[c]	$5.15	$5.15	$5.15	$5.15	$6.85	$6.85
Wisconsin	$5.15	$5.15	$5.15	$5.70	$6.50	$6.50

Note: [a]-Rates applicable to employers of four or more

[b]-Rates applicable to employers of two or more

[c]-Ohio sets a lower rate for employers with gross annual sales under $150,000 to $500,000 ($3.35 January 1,1991- January1, 2005) and for employers with gross annual sales under$150,000($2.50 January 1,1991- January1, 2005), so we assign $5.15 to minimum wage of Ohio from 2001 to 2006.

Resource: Changes in Basic Minimum Wages in Non-farm Employment under State Law: Selected Years 1968 to 2010, U.S. Department of Labor, Office of State Standards Programs Wage and Hour Division web site Minimum Wage and Overtime Pay Standards Applicable to Nonsupervisory NONFARM Private Sector Employment under State and Federal Laws.

Table 2.2: Minimum Wage Effects on Employment and Monthly Earning in the Great Lakes Region, All Youth, 2003-2007

Age Group	**14-18**		**19-21**		**22-24**	
	(1) ln(Employment)	(2) ln(Earnings)	(3) ln(Employment)	(4) ln(Earnings)	(5) ln(Employment)	(6) ln(Earnings)
Panel A	**Panel Model**					
ln(Minimum Wage)	-0.102*	0.176**	-0.044**	0.086**	0.001	0.012
	(0.040)	(0.038)	(0.012)	(0.022)	(0.016)	(0.016)
ln(Total Employment)	1.008***	0.128*	1.246***	0.107	1.266***	0.111
	(0.123)	(0.046)	(0.117)	(0.070)	(0.071)	(0.069)
ln(Population)	0.328***	-0.271*	0.083	-0.343***	0.027	-0.326*
	(0.053)	(0.109)	(0.207)	(0.053)	(0.066)	(0.131)
Observations	8,740	8,740	8,740	8,740	8,740	8,740
R-squared	0.681	0.626	0.717	0.479	0.684	0.411
Panel B	**Panel Model with EA Time FE**					
ln(Minimum Wage)	-0.210***	0.199**	-0.126**	0.218***	0.095***	0.104***
	(0.043)	(0.066)	(0.041)	(0.015)	(0.015)	(0.016)
ln(Total Employment)	0.949***	0.105*	1.219***	0.085	1.233***	0.090
	(0.088)	(0.049)	(0.116)	(0.073)	(0.065)	(0.068)
ln(Population)	0.110	-0.256	0.119	-0.268**	0.021	-0.199*
	(0.061)	(0.152)	(0.194)	(0.074)	(0.071)	(0.090)
Observations	8,740	8,740	8,740	8,740	8,740	8,740
R-squared	0.724	0.722	0.752	0.567	0.717	0.506

Note: All the dependent variables and independent variables are taken in the natural log. Panel A presents results from the traditional panel data model including county fixed effects and time fixed effects. Panel B presents results from the panel data model with BEA-Economic Areas (EA)-specific time effects. Column (1) and (2) present the minimum wage effects on static employment and monthly earnings for teenage worker between the ages of 14 and 18; column (3) and (4) are for the youth aged between 19 and 21; and Column (5) and (6) are for the youth aged between 22 and 24. Average monthly earnings are in nominal dollars. Robust standard errors in parentheses are clustered at the state level for all regressions. *Statistically Significant at the .10 level; ** at the .05 level; *** for the .01 level.

Table 2.3: Minimum Wage Effects on Job Flows in the Great Lakes Region, All Youth, 2003-2007, Using Panel Model with EA Specific-Time Effects

	(1) ln(Accessions)	(2) ln(Separations)	(3) ln(Turnover rates)	(4) Net Job Flow Rates
Panel A Age 14-18				
ln(Minimum Wage)	-0.507***	-0.558***	-0.301***	-0.018
	(0.090)	(0.093)	(0.038)	(0.055)
Observations	8,736	8,734	8,730	8,740
R-squared	0.791	0.786	0.754	0.694
Panel B Age 19-21				
ln(Minimum Wage)	-0.316***	-0.391**	-0.222***	0.034
	(0.066)	(0.120)	(0.043)	(0.045)
Observations	8,738	8,740	8,738	8,740
R-squared	0.770	0.752	0.695	0.714
Panel C Age 22-24				
ln(Minimum Wage)	-0.261**	-0.262***	-0.363***	0.084***
	(0.059)	(0.047)	(0.039)	(0.015)
Observations	8,737	8,735	8,732	8,740
R-squared	0.640	0.559	0.533	0.422

Notes: All the dependent variables and independent variables are taken in the natural log, except for the dependent variablenet job flow rates. All the regression results are based on the panel data model with BEA-Economic Areas (EA)-specific time effects. Panel A presents the minimum wage effects on job flows for teenage worker between the ages of 14 and 18; Panel B are for the youth aged between 19 and 21; and Panel C are for the youth aged between 22 and 24. There are some missing values for accessions, separations and turnover rates for each group of the youth. Robust standard errors in parentheses are clustered at the state level for all regressions. *Statistically Significant at the .10 level; ** at the .05 level; *** for the .01 level.

Chapter 3

Hospital Mergers and Quality of Care

Evidence from Heart Attack and Pregnancy Outcomes in Pennsylvania

3.1 Introduction

The health care industry has experienced significant increases in hospital consolidations during the past two decades. Nationwide, between 1989 and 1996, there were 190 hospital mergers (Dafny, 2009), and by the mid-1990s, hospital merger and acquisition activity was nine times its level at the start of the decade (Vogt and Town, 2006). During the 2000s, 597 hospital mergers and acquisitions were announced. While most research on the effects of hospital mergers has focused on price and efficiency, a key concern for both patients and policymakers is whether the quality of care is affected by merger activity. In this paper we investigate the impact of hospital mergers, particularly mergers in already concentrated markets, on the quality of care for three types of patients in Pennsylvania.

Mergers may result in both greater market power and greater efficiency (Williamson, 1968), and there is evidence of both effects arising from hospital mergers. A number of papers show that mergers are associated with greater market power and higher prices for merging hospitals (Vita et al., 2001; Krishnan, 2001; Vogt et al., 2006; Dafny, 2009), and some show that mergers are associated with significant cost savings and efficiency gains (Sinay, 1998; Connor et al., 1998). However, few focus on the consequences of hospital consolidation on the quality of care, even though economists at the Federal Trade Commission (FTC), when evaluating merger effects, state that well-supported claims regarding clinical quality should be given more weight than other claims of pro-competitive merger effects (Farrell et al., 2011).

There are reasons to believe that mergers may improve the quality of care. As market power increases, merged hospitals, facing less competitive pressure, may attract greater volumes of patients, which could improve surgical outcomes in particular due to "learning by doing" or "practice makes perfect" (Gaynor et al., 2005). However, mergers may also reduce the quality of care. In the short run, reorganization may cause confusion and lack of coordination. In the longer run, merged hospitals may exercise greater market power to reap higher profits by cutting costs at the sacrifice of quality.

In this paper, we use outcomes for acute myocardial infarction (AMI) patients and for pregnant women to compare the quality of care in hospitals that have undergone mergers with the care provided by hospitals that did not undergo mergers. A number of researchers have used the health outcomes for AMI patients to measure the quality of care when they study the effects of hospital market concentration (Kessler and McClellan, 2000; Kessler and Geppert, 2005; Gowrisankaran and Town, 2003). However, because health outcomes for a single type of patient may not capture all aspects of health care quality, we also include health outcomes for two other types of patients: patients receiving coronary artery bypass grafting (CABG) surgery and pregnancy cases (Ho and Hamilton, 2000). Examining CABG patient outcomes is interesting because of the well-known benefits associated with higher volume surgical programs, and also because independent information about the quality of CABG surgery was available in Pennsylvania during this period. In addition, we examine obstetric services because delivering a baby is one of the most common medical procedures and quality of care is very important: low quality can result in poor health outcomes for mothers and infants, and even impair the future health and mental development of children. Finally, the health outcomes for these three groups of patients can be easily observed and measured using the data we have.

The general term "merger" may be used to describe different types of consolidations. One type, which we do not examine, occurs when two formerly independent hospitals have common ownership but continue to operate with separate hospital licenses, providing roughly the same services as they did before the merger. Instead, we examine mergers in which separate hospitals in the same local market come together under a single license: usually one of the hospitals closes and they combine most or all of their assets.

Of these mergers, we focus on mergers between hospitals where both hospitals provided the relevant service before the merger took place to insure that the mergers we examine created a change, at the service level, in the number of competitors facing the newly merged hospital. Thus, for each of our three types of patients, a horizontal merger has occurred if both merging hospitals provided the same particular type of service prior to merger: for example, they both delivered babies. A "non-horizontal" merger, on the other hand, is a merger between a hospital that does provide the particular type of service with another that does not. This type of merger joins complementary, rather than substitute, services, and we expect it to have a different impact on market power and efficiencies and thus on the quality of care (Huckman, 2006). While our paper focuses principally on the effects of horizontal mergers on the quality of care, we are able to explore the effects of "non-horizontal" mergers in the case of obstetric care for more evidence on the relationship among mergers, market power, and the quality of care.

The degree of existing market concentration may alter the relative size of effects on quality. For example, merged hospitals in more concentrated markets may gain greater market power and be less concerned about losing customers, giving them a greater ability to reduce costs at the sacrifice of quality of care as well as raise prices. That is, because the quality of care is imperfectly observed by patients and it is a non-contractible input, such hospitals may increase their profit after mergers by reducing resource use, for example, by decreasing the number of personnel per admission. We expect market power effects are more likely to exceed efficiency gains where mergers are occurring in already concentrated markets, resulting in worse health outcomes in those markets. Thus, in this paper, we focus on the impact of existing market concentration on how mergers affect quality.

As in other studies of mergers, the mergers in our sample are not random across hospital markets, and the hospital characteristics between merging and non-merging hospitals are not perfectly comparable. To address this selection problem, we use Propensity Score Matching to identify a control group of non-merging hospitals with similar characteristics to the merging hospitals, that is, with predicted probabilities of merging that are closest to those of the merging hospitals.

We use this statistical matching technique to explore the impact of horizontal mergers on the quality of care in markets with different pre-existing levels of concentration, and over different lengths of time. The full effects of mergers may take time to develop (Focarelli et al., 2003). Coordination of activities after a merger may take some time to complete, and merger effects may therefore take an even longer time to materialize. Using Pennsylvania inpatient data over two decades, and information on the occurrence and timing of mergers of Pennsylvania hospitals, we are able to distinguish transitory from longer-lived effects of mergers in markets with different levels of concentration on the quality of care.

Our results show that the effects of hospital mergers on health outcomes are affected by market concentration. We find that in Pennsylvania mergers that occurred in concentrated markets are associated with worse health outcomes for AMI patients and pregnant patients but not for CABG patients. We also find that the negative impacts persist for several years following the merger.

The rest of the paper proceeds as follows. Section 3.2 briefly reviews the literature on the effects of hospital mergers on quality of care. Section 3.3 presents the empirical strategy and specification. Section 3.4 describes the data and summary statistics. Section 3.5 provides the main results and robustness checks. Section 3.6 is the conclusion.

3.2 Mergers and Quality

Previous empirical studies provide mostly indirect evidence of merger effects by examining the relationship between market concentration and quality. For fixed-price Medicare patients,[1] most papers find that increases in hospital competition lead to improved mortality, and the evidence therefore suggests that hospital mergers, which increase concentration, will increase adverse health outcomes (Kessler and McClellan, 2000; Kessler and Geppert, 2005). Patients whose care is not reimbursed at a fixed price also appear to have better outcomes in less concentrated hospital markets (Sari, 2002; Gowrisankaran and Town, 2003).

Ho and Hamilton (2000) examine the impact of both hospital mergers and acquisitions on quality directly. Using California data for the period 1992-1995, they measure hospital quality

[1] Gaynor (2006) reviews recent findings on the relationship between concentration in health care markets and its impact on quality from both the theoretical and empirical literature. He divides studies of the effect of mergers on quality into two—studies with fixed prices and studies with prices set by firms. When prices are fixed (i.e., Medicare or Medicaid patients are reimbursed a fixed price based on patient's DRG code, regardless of where or how much care they obtain) and above marginal cost, theory shows that competition increases quality and improves consumer welfare, and empirical studies are consistent with this prediction. However, when prices are set by firms, the impact of competition on quality from both theoretical and empirical evidence is ambiguous.

as inpatient mortality for heart attack and stroke patients, 90-day readmission for heart attack patients, and discharge within 48 hours for normal newborn babies. They compare outcomes at hospitals before and after merging, and find no detectable impact of mergers on inpatient mortality; but they find that mergers raise readmission rates for heart attack patients and the likelihood of early discharge of newborns, if they control for hospital market concentration by adding the interaction of the hospital merger dummy with the level of local market concentration. Gaynor et al. (2012) use hospital-level panel data in England over the period 1997-2004 to examine the impact of mergers on outcomes that include clinical quality as well as financial performance, productivity, and waiting times. They find that none of these outcomes are improved by a merger.

In sum, both indirect and direct evidence suggests that hospital mergers do not improve the quality of care.

3.3 Empirical Strategy and Specification

Our empirical strategy must deal with several issues. First, hospital mergers are unlikely to have occurred randomly. That is, merging hospitals may differ from non-merging hospitals in both observed and unobserved ways so that their "selection" into the merging group might be correlated with hospital characteristics. For example, as their share of patients with HMO insurance increased, hospitals may have chosen to merge to enhance their bargaining power vis-a-vis managed care organizations (MCOs).[2] In such a situation, those different hospital characteristics could either correlate with the post-merger dummy variable, or independently influence hospital outcomes even in the absence of mergers. Thus, a control group, a group of "pseudo-merging" hospitals, needs to be constructed to estimate the counterfactual "what would a patient's health outcome have been if the merger had not taken place?" While it is true that there rarely exists absolute "pseudo-merging" hospitals, that is, hospitals that have never been exposed to merger activities by themselves or others in their market, we can use *Propensity Score Matching* to select hospitals that closely resemble merging hospitals and that were not themselves involved in

[2] The bargaining power of the MCO depends on its ability to exclude the hospital from its network, and then patients would not visit the hospital outside of the network. The bargaining power of the hospital depends on its ability to leave the MCO network, which can make the network less valuable to attract patients (Farrell et al., 2011).

a merger during the sample period as the control group. Propensity score matching helps us to select hospitals for the control group that have a similar likelihood of being exposed to mergers based on measured characteristics.

Second, we need to identify hospital markets to measure market concentration. There is general agreement that administrative boundaries, such as county level, zip code or Metropolitan Statistical Areas (MSAs), are poor definitions of hospital markets (Cooper et al., 2011). In order to create stronger market definitions, some studies use a "fixed radius" around a hospital, but fixed radius measures are highly correlated with urban density and ignore the preferences and travel patterns of patients. Other studies use a "variable radius", creating a radius according to travel distances (Makuc et al., 1991), actual patient flows (Gresenz et al., 2004), or predicted patient flows (Kessler et al., 2000)[3]. In this paper, we use the "variable radius" approach, defining a hospital market as the area from which the hospital draws 75% of all of its patients, an approach that represents a "reasonable balance between convenience and accuracy" (Gresenz et al., 2004). We then use the resulting market shares based on hospital admissions to calculate Herfindahl-Hirschmann Indices (HHI),[4] our measure of the degree of concentration in a hospital's market.

We use the HHIs in two ways. First, we include the value of the contemporaneous HHI for each hospital market in all estimations, to control for effects of market concentration in general on hospital outcomes. Second, because we are particularly interested to see whether mergers in already-concentrated markets have a greater impact on the quality of care, we use interaction terms to identify mergers that take place in markets with different degrees of pre-existing concentration, using the HHI of a market in the year prior to a merger to measure pre-existing concentration.

Finally, the severity of a patient's condition might affect outcomes independently even in the absence of hospital mergers. For example, a person who has a heart attack may be relatively unhealthy or have other diseases; these types of patients, if admitted to a merging hospital, might

[3]Kessler and McClellan (2000) instrument for HHI with hospital market shares based on the average expected probabilities of hospital choice, determined by the distance between the patients zip of residence to the hospital (predicted patient flows). But even this measure of concentration is still controversial, since patient demographics are not definitely exogenous to health outcomes (Cooper et al., 2011).

[4]$HHI = \sum_{i=1}^{N} S_i^2$, where S_i is hospital i's market share, which is calculated as the ratio of the number of all patients attracted by hospital i over the total number of patients in the entire hospital market. HHI is equal to the sum of squared market shares of all hospitals in a local market.

make the merging hospital's outcomes look worse, although the poorer outcomes have nothing to do with the merger. There may also be a matching process. Before a merger, some hospitals may refer more complex cases to other hospitals that are better able to provide more intensive care; while after merging, those hospitals may have more comprehensive facilities and treat more severe cases, which may result in worse outcomes. As a result, we must control for patients' severity to avoid overestimating the adverse effects of mergers.

The first specification we estimate is:

$$y_{iht} = c_0 + \alpha_1 M_{ht}^{1-2} + \alpha_2 M_{ht}^{3-5} + \alpha_3 M_{ht}^{6+} + \gamma_1 HHI_{ht} + \gamma_2 X_{iht} + \gamma_3 Z_{ht} + \phi_h + \tau_t + \epsilon_{iht}, \quad (3.1)$$

where y_{iht} represents the quality of outcomes for patient i admitted to hospital h in year t. A continuous variable, HHI_{ht}, represents hospital h's local market concentration in time t. The vectors X_{iht} and Z_{ht} represent time-varying characteristics of the patient and the hospital where the patient was admitted, respectively. In addition, we include hospital fixed effects, ϕ_h, to control for time-invariant properties of hospitals that might be related to mergers or the quality of care; year fixed effects, τ_t, to control for common trends affecting all hospital outcomes, and ϵ_{iht} to represent the unobserved random error.

We analyze the effect of mergers over the short term, the medium term, and the long term, where the short term (the transition period) is defined as less than two years following the merger; the medium term is defined as three to five years after the merger; and the long term is defined as six or more years after the merger (Focarelli et. al, 2003). That is, the dummy variable M_{ht}^{1-2} equals one if a patient was treated at a hospital that merged with another hospital less than two years ago, the dummy variable M_{ht}^{3-5} equals one if the patient's hospital was involved in a merger between three and five years ago, and the dummy variable M_{ht}^{6+} equals one if the hospital was involved in a merger six or more years ago.

We then examine the effects of hospital mergers that occurred in markets with different levels of concentration by adding an interaction term between the merger dummy and a dummy indicating the degree of local market concentration one year prior to the merger:

$$y_{iht} = c_0+\beta_1(M_{ht}^{1-2}\times HHI)+\beta_2(M_{ht}^{3-5}\times HHI)+\beta_3(M_{ht}^{6+}\times HHI)+\gamma_1 HHI_{ht}+\gamma_2 X_{iht}+\gamma_3 Z_{ht}+\phi_h+\tau_t+\epsilon_{iht}. \tag{3.2}$$

In this equation, HHI is a vector of three dummy variables that indicate whether local market concentration was in the top quartile (75%), the median (50%), or the bottom quartile (25%) of the HHI distribution among the sample of HHIs faced by each of the three types of patients. Thus, the interaction terms capture how mergers of different ages affect the quality of care when occurring in markets with different degrees of concentration. For example, the interaction $M_{ht}^{1-2} \times HHI$ (top quartile) equal to one indicates that the patient is being treated at a hospital that merged within the previous two years, and that the merger occurred in a market with an HHI that was already in the top quartile of the HHI distribution. Thus, the estimated β parameters show how mergers of different ages affect health outcomes for AMI, CABG, or pregnant patients in a hospital market in a given category of concentration.

Patient Outcomes

We measure health outcomes for AMI and CABG patients using two binary variables. The first indicates whether an AMI or CABG patient died during her stay in hospital, and the second indicates whether she was readmitted during the subsequent quarter after the initial event with a primary diagnosis of either subsequent AMI or heart failure (HF).[5] (Since we only have quarterly inpatient data, we can only examine whether a patient was admitted in subsequent quarters.) For pregnant patients, we measure health outcomes using the incidence of preventable complications for either vaginal or C-section delivery.[6] The outcome variable equals one if a preventable complication occurred during the patient's delivery; otherwise, the outcome variable equals zero. Thus, in all cases a dependent dummy variable equal to one represents poorer quality of care. We use

[5]We define the readmission with the primary diagnosis of AMI, old AMI, angina pectoris, other forms of (chronic) ischemic heart disease, or heart failure. We obtained similar results for the readmission rate within 2 quarters and within 4 quarters, so we report results for the readmission rate in subsequent quarter.

[6]Currie and MacLeod (2008) point out that while certain types of complications, such as breech delivery and cephalopelvic disproportion (baby's head too big for mother's pelvis), are unlikely to be caused by the doctor's behavior at the time of delivery; other types, such as excessive bleeding, fetal distress, or anesthetic complications, may be potentially preventable in many cases. In our paper, we measure the preventable complications as maternal fever, excessive bleeding, maternal seizure, precipitous labor, prolonged labor, dysfunction labor, anesthetic complications fetal distress, uterine rupture during labor, meconium staining, and chorioamnionitis.

a linear probability model to estimate these outcome equations.

Besides these direct measures of health outcomes, we also measure the total resources used for each patient, using the log of total charges incurred per admission. Although total charges are the list prices, not the actual prices hospitals charge, they can be used to reflect the hospital resources or amount of care a patient consumes (Gaynor and Vogt, 2003). Therefore, our total charges variable includes charges for room and board, ancillary, drug, equipment, specialty, and other charges. We use total charges, deflated to 1994 dollars with the annual average Consumer Price Index for all urban consumers (CPI-U), as an approximation of resource utilization.

Hospital Characteristics

We employ hospital fixed effects, ϕ_h, to control for time-invariant properties of hospitals that might be related to mergers or affect the quality of care. However, some hospital characteristics may vary over the sample period. For example, hospitals, particularly merging hospitals, may change in size. Thus, we include a vector of hospital characteristics, Z_{ht}, consisting of bedsize (100-199, 200-299, 300-399, 400-499, and greater than 500, with the omitted group being less than 100 beds) and the volume of service, measured by the number of inpatient admissions with the same primary DRG code in hospital h in year t. We include the latter variable to control for spillovers effects: for example, more patients with the same DRG code treated within one hospital may increase the hospital fixed-facilities investment and the doctors' proficiency, resulting in better outcomes for all patients with the same DRG code.

Patient Characteristics

X_{iht} is a vector of patient characteristics including patient age,[7] race,[8] gender, and urban residence.[9] We control for severity differences among patients using dummy variables to indicate whether the patient is transferred from another acute care hospital and whether the admission is urgent or from the emergency room. For AMI and CABG patients, we also include the Charlson

[7]We divide AMI and CABG patients into five age groups: 50-59 years old, 60-69 years old, 70-79 years old, 80 years old or above, and the omitted group is 49 years old or younger. Divide pregnant patients into five age groups: 19-29, 30-39, 40-49, 50 or older, and the omitted group is 18 or younger.

[8]Race is divided into black, Asian, other race, and the omitted group is white.

[9]The Center for Rural Pennsylvania defines a county as urban area, if its population density is 284 persons or more per square mile. There are 19 counties in Pennsylvania considered urban.

Co-morbidity Index,[10] which stratifies patients into groups with a similar risk of comorbidity and predicts their ten-year mortality and burden of disease (equal to 2, 3, or 4 or more; the omitted group is 1); and an indicator for whether the AMI or CABG patient has had AMI treatment or CABG surgery within the previous three years. To control for the severity among pregnant patients, we use two variables: one indicates whether a patient has at least one non-preventable complication, and the other indicates whether she has at least one pre-existing complication.[11] We also create dummy variables that indicate whether the delivery is vaginal with instrument, C-section, or a multiple birth; and whether the patient had a C-section before.

Finally, because various types of insurance coverage affect the payment received by hospitals differently, which may also affect the quality of care provided by hospitals, we control for insurance type by including variables to indicate whether the patient the patient was covered by Medicare, Medicaid, other government insurance, or HMO, with the omitted group being private insurance.

3.4 Data and Summary Statistics

The patient level data used in this paper are from the inpatient data collected by the Pennsylvania Health Care Cost Containment Council (PHC4). The data provide a rich set of information for patients who are admitted to Pennsylvania hospitals, including age, race, gender, admission type, insurance coverage type, diagnosis and procedure codes, zip code of residence, total in-hospital charges, and whether the patient died in the hospital. Our sample begins from 1994, because before 1994, there was incomplete information for patients in the PHC4, such as missing information on patients' race, ethnicity, primary insurance payer, etc.

The hospital level data are from the American Hospital Association (AHA) *Annual Survey of Hospitals*, which provides comprehensive information about hospital characteristics, including

[10]The Charlson Co-morbidity Index includes 19 medical conditions, and each condition is assigned a score of 1, 2, 3, or 6, depending on the risk of dying associated with each one. A patient's Charlson index is the sum of these weights to predict the ten-year mortality (Charlson et al. 1987). In this paper, Charlson Co-morbidity Index is calculated using STATA command "charlson".

[11]We define the non-preventable complications as one or more of the following conditions exist: breech, cephalopelvic disproportion, cord prolapse placental abruption, placenta previa, and premature rupture of membranes. We define the pre-existing complications as one or more of the following conditions exist: malpresentation, herpes, diabetes mellitus/abnormal glucose, hypertensive disorder/eclampsia, oligohydramnios, incompetent cervix and other congenital or acquired abnormality of cervix, congenital or acquired abnormality of vagina, rhesus isoimmunization, anemia, habitual aborter, uterine bleeding, and renal failure. (Currie and MacLeod, 2008)

location, number of beds, teaching status, ownership status, and system member status. We used the hospital name to link each patient admission record in the PHC4 with Pennsylvania hospital characteristics obtained from the AHA *Annual Survey of Hospitals*.

Information on the occurrence and timing of mergers of Pennsylvania hospitals among our sample period, 1994-2010, comes from the PHC4 and the AHA *Annual Survey of Hospitals*. Each year of the AHA survey records hospital mergers since the previous survey, information we verified from mergers listed in the "Facility Profile Changes" from quarterly Pennsylvania Inpatient Data Notes released by the PHC4.[12]

This paper employs three samples of patients. The first sample contains cardiac patients with a primary diagnosis of acute myocardial infarction (AMI) based on ICD-9 diagnosis codes.[13] The second sample contains patients who experienced either vaginal or Caesarean-section delivery based on ICD-9 procedure codes.[14] The third sample contains patients undergoing CABG surgery.[15]

We identified mergers between hospitals that both provided the relevant service prior to the merger. While most hospitals provide medical treatment for AMI patients and many provide obstetric services, many fewer provide intensive cardiac surgeries such as CABG. There were 30 hospital mergers in Pennsylvania from 1994 to 2010, shown in Figure 1.[16] Of these, there were 24, 16 and 6 mergers that involved hospitals that both provided medical treatment for AMI patients, routine obstetrical care, and CABG surgeries, respectively.[17]

[12]The PHC4 provide the accurate dates of mergers after 1996. For 1994 and 1995, we rely on the AHA only.

[13]The ICD-9 diagnosis code for AMI is 410.

[14]The ICD-9 procedure codes for pregnancy are 72, 73, 740, 741, 742, 744, and 7499.

[15]The ICD-9 procedure codes for CABG are 3610-3619.

[16]Because our study focuses on the effects of hospital mergers on outcomes through increasing in the market concentration, we limit the cases of mergers to the hospitals that are geographically close to each other. If the two merging hospitals are too far away from each other, then it is less likely that this case of merger would affect the local market share of the merging hospital. We found only one merger that involved hospitals that were more than 15 miles apart. We do not include this merger in the analysis so that in our sample, each pair of acquiring and acquired hospitals is within 15 miles of each other.

[17]We focus on general medical and surgical hospitals, so two pairs of merging hospitals are excluded from our sample. One is the merger between two rehabilitation hospitals in 1997, and the other is the merger between two children's hospitals in 2007. This restricts the sample to 28 mergers in total.

Propensity Score Matching

In each of the hospital samples that admit AMI, CABG, or pregnant patients, there are systematic differences in characteristics between the merging and non-merging hospitals. Thus, we use propensity score matching to identify a control group of non-merging hospitals with characteristics similar to the acquiring hospitals in each merger.[18] We identify non-merging hospitals as hospitals that do not undergo any form of mergers during the sample period.

The propensity score for a hospital is the probability of its merging conditional on its characteristics. The score is estimated with a multivariate logistic regression model[19] with time fixed effects, where the merger dummy is the outcome and hospital characteristics are the predictor variables.[20] These predictor variables are: whether a hospital belongs to health care system; whether it is a member of Council of Teaching Hospital of the Association of American Medical Colleges (COTH); the number of beds, the number of cardiac intensive care beds, and the number of medical/surgical intensive care beds; whether a hospital offers medical/surgical intensive care, obstetric care, cardiac intensive care, neonatal intensive care, neonatal intermediate care, or skilled nursing; whether it is located in a metropolitan area with population under 2,500,000, a metropolitan area with population over 2,500,000, or a non-metropolitan area (the omitted group); the share of a hospital's patients covered by Medicare, Medicaid or HMOs each year; and the average severity of patients treated in the hospital each year.[21] Table 3.8 shows the coefficients of all variables in the logit model for horizontal merging hospitals and their matched hospitals that treat AMI, CABG, and pregnant patients, respectively.

[18] We define acquired hospital as the hospital whose records are submitted under the acquiring hospital's identification number after the merge.

[19] $p_i = Pr[D_i = 1|X_i] = \{1 + exp(-\alpha - \beta X_i\}^{-1}$, where $D_i = 1$ if a hosptial has ever merged between 1994 and 2010; $D_i = 0$ otherwise. X_i is a vector of covariates.

[20] The merger dummy equals one after the hospital had merged in any time between 1994 and 2010. In Stata, we also tried "xtlogit" with hospital fixed effect and time fixed effect, but STATA shows "not concave". This occurs because that hospital fixed effects only use the information of changes within a hospital across the sample time period, but most of hospital characteristics we include in the regression model do not change much over time. That is, there are not enough within-hospital variations, so we use the "logit" model.

[21] For AMI and CABG patients, we use the average Charlson index in each hospital per year. For pregnant patients, we calculate the overall severity by adding up these binary indicators—non-preventable complication, preexisting complication, vaginal delivery with instrument, C-section, a multiple birth, and whether the patient had a C-section before. We have tried to include the operating margins as one of the hospital characteristics to predict the probability of merging, but the information on operating margins in the PHC4 website is not available for public report until 1997, and the coefficient of operating margins is not statistically significant when we estimated dropping the years before 1997. Thus, we drop this variable and choose the hospital sample of 1994-2010 to predict the propensity.

We use 1:5 variable ratio caliper matching with replacement to select control hospitals, because this method is proven to outperform fixed ratio matching, due to its higher precision, with little cost in bias (Rassen, 2012). We manually match each merged hospital to the non-merging hospitals which have the closest propensity score one year prior to merger, within a caliper width equal to 0.2 of the standard deviation of the propensity score.[22] This method can eliminate most bias in the crude estimator, and lead to confidence intervals with approximately the correct coverage rates (Austin 2011). With 1:5 variable ratio matching, one merged hospital can be matched to up to 5 non-merging hospitals as long as the difference in propensity score between these two hospitals is within a predefined distance (caliper).[23]

We then identify the patients treated at the merged and the control hospital in each of the three samples. We exclude the records missing a patient's identification number, and records for patients who are not Pennsylvania residents. Next, we eliminate records that are missing hospital or patient characteristics, and records with invalid zip code information for patients or hospitals. Because we want to know whether a hospital was involved in a merger within at least the previous five years (the short term and the medium term), we limit the outcomes analysis to the years, 1999-2010. As a result, the total number of observations is 160,637, 497,553, and 8,644 in the sample of AMI, pregnant, and CABG patients, respectively. (There are 233,363 observations in the sample for the analysis of "non-horizontal" mergers involving obstetric care) Table 3.1 presents a comparison of patient profiles in the propensity score-matched hospitals one year before the merger occurs. Overall, the numbers suggest that the matching process has created a group of control hospitals with patients that are quite similar to those of the acquiring hospitals.

[22] The caliper of width is 0.0282 (0.2*0.1401=0.0282), 0.0505 (0.2*0.2524=0.0505), and 0.0273 (0.2*0.1363=0.273) for the samples of hospitals that attract AMI, CABG, and pregnant patients, respectively.

[23] Among the sample of horizontal acquiring and non-merging hospitals that service AMI patients, after 1:5 propensity score matching, 12 unique merging hospitals are matched with 41 unique non-merging hospitals, from 1994 to 2010. For the CABG analysis, 4 unique merging hospitals are matched with 12 unique non-merging hospitals. For the pregnancy analysis, 8 merging hospitals are matched with 35 unique non-merging hospitals. For the analysis of the effect of non-horizontal mergers on pregnancy outcomes, 3 merging hospitals are matched with 11 unique non-merging hospitals. In all cases, we dropped from the sample acquiring hospitals for which there was no acceptable match.

3.5 Empirical Results

3.5.1 Results

AMI Patients

Columns (1) to (3) in Panel A of Table 3.2 show the results from estimating Equation (1) for AMI patients. We find that mergers do not affect the probability of in-hospital mortality for AMI patients, which is consistent with Ho and Hamilton (2000). Furthermore, we find that mergers do not affect the probability of readmission for AMI patients, but significantly decrease resource utilization, i.e., total charges, in the short run.

The results in Column (1) to (3) of Table 3.2 Panel B show the effects of mergers in markets with different degrees of concentration on outcomes for AMI patients. Compared with all the non-merging hospitals in markets with any level of concentration, mergers that occurred in concentrated (HHI at median 50% of its distribution) or highly concentrated (HHI at top quartile of its distribution) markets significantly increase the probability of adverse health outcomes.[24] That is, compared with patients treated in non-merging hospitals, patients treated in merging hospitals in highly concentrated markets experience significantly higher readmission rates in the short, medium, and long term, and experience significantly higher probability of death in the medium and long term. The estimates suggest that treatment in a merged hospital located in a concentrated area increases the probability of in-hospital mortality by about 2.4 percentage points or 34.3% (0.024/0.07=34.3%) either in the short or medium term, and by 3.6 percentage points or 51.4% (0.036/0.07=51.4%) in the long run;[25] and raises readmission rates by 1.4 (0.014/0.22=6.4%) and 2.0 percentage points (or 0.02/0.22=9.1%) in the short and medium term, respectively, compared to the mortality rate in non-merging hospitals.[26]

In contrast, patients treated in merging hospitals located in competitive markets (HHI at bottom 25% of its distribution) do not experience worse health outcomes in all subsequent years

[24] In the sample of AMI patients from 1999 to 2010, after 1:5 propensity score matching, the cut-off for the top 25% of HHI distribution is above 0.605 (highly concentrated markets) and the cut-off for the bottom 25% is below 0.120 (competitive market). Markets with HHI between 0.120 and 0.605 are defined as concentrated markets. The median of HHI distribution is 0.363.

[25] The inpatient mortality rate averages 0.07 among AMI patients treated in hospitals located in concentrated markets.

[26] The readmission rate averages 0.22 among AMI patients treated in hospitals located in concentrated markets.

following the merger, but instead experience lower mortality rates in the short run and lower readmission rates in the long run, compared to patients of non-merging hospitals. These hospitals continue to provide the same or better quality of care while at the same time resource utilization (total charges) declines, suggesting that these mergers have created efficiencies.

Pregnant Patients

Columns (4) and (5) of Table 3.2 present the effects of hospital mergers on pregnancy outcomes. From Panel A, we can see that without controlling for market concentration where a merger is occurring, mergers only affect total charges, which are significantly reduced by over 10% for a long time period.

However, the results in Panel B show that after controlling for market concentration where a merger takes place, differences in preventable complications appear and grow substantially over time. For example, compared with pregnant patients treated in non-merging hospitals, patients treated in merging hospitals in highly concentrated markets[27] experience 2.2, 1.4, and 3.0 percentage point or 27.5%, 17.5%, and 37.5% increases in the probability of having preventable complications during delivery in the short, medium and long run, respectively, at the significance level of 0.01.[28] In addition, pregnant women treated in merging hospitals in concentrated markets experience 2.0 (or 25.0%) and 2.3 percentage points (28.8%) increases in the probability of preventable complications in the short and medium term. All of these adverse health outcome results are accompanied by reductions in resource utilization (lower total charges).

Again in contrast, mergers in competitive markets are instead followed by substantial decreases in the incidence of preventable complications in the short term, as well as lower resource utilization, and no change in the probability of preventable complications in the medium and long term. Again, the results suggest that these hospitals achieve efficiency gains following a merger.

[27] In the sample of all pregnant patients from 1999 to 2010, after 1:5 propensity score matching, the cut-off for the top 25% of HHI distribution is above 0.605 and the cut-off for the bottom 25% is below 0.153. The median of HHI distribution is 0.403.

[28] The probability of preventable complications averages 0.08 among pregnant women treated in hospitals located in any of the highly concentrated, concentrated, or competitive markets.

CABG Patients

The analysis in Panel A of Table 3.3 provides some weak evidence that mergers are associated with greater in-hospital mortality for CABG patients in the long run and with decreased resource utilization in the short run. However, after controlling for market concentration where the merger takes place, the results for CABG patients are different from those for AMI and pregnant patients: we find little evidence that mergers in more concentrated areas are systematically associated with worse outcomes. Instead, CABG patients treated in merging hospitals located in highly concentrated markets[29] have significantly lower probability of in-hospital death and readmission rates in the transition period.[30] Mergers in concentrated markets do not have any detrimental effects on CABG patients in the short or medium run, but they show higher readmission rates and lower resource utilization in the long term. Mergers in the least concentrated market show improvements in mortality and great resource use, and there is the same evidence of decreased readmission rates particularly in the medium term.

3.5.2 Results on Pregnant Patients in "Non-Horizontal" Merging Hospitals

On Table 3.4 we report the effects of mergers between hospitals offering obstetrical services and hospitals that do not.[31] The results show that, compared with non-merging hospitals, mergers in either concentrated or least concentrated markets[32] significantly decrease the likelihood of preventable complications in all subsequent years following the merger. In concentrated markets these improvements are associated with increases in resource utilization, and in least concentrated markets with decreases in resource utilization. Our results show that mergers between hospitals that are not competing with each other to provide pregnancy services prior to the merger are not associated with worse outcomes. Instead, outcomes may improve because the new hospital is

[29] In the sample of CABG patients from 1999 to 2010, after 1:5 propensity score matching, the cut-off for the top 25% of HHI distribution is above 0.621 and the cut-off for the bottom 25% is below 0.266. The median of HHI distribution is 0.381.

[30] Due to the small sample size of horizontal merging hospitals that provide CABG surgery, hospitals that are located in highly concentrated markets and merged more than six years ago are omitted.

[31] There are only few cases of "non-horizontal" mergers involved acquiring hospitals that offer AMI services while acquired hospitals that do not. Therefore, we use the pregnancy cases to examine the effects of "non-horizontal" mergers. There were 11 cases of "non-horizontal" mergers involved acquiring hospitals offering obstetrics while acquired hospitals not serving any pregnant patient at all.

[32] The lack of results for all categories is due to the small sample size of "non-horizontal" merging hospitals. There are not enough observations of patients treated in merging hospitals that are located in highly concentrated market.

stronger financially and able to offer a wider range of healthcare services through related marketing channels or service processes.

3.5.3 Robustness Check

Most hospital mergers in Pennsylvania involve a small hospital being acquired by a large hospital, and after they merge the combined data are reported under the large hospital's original ID in the PHC4.[33] Our main estimation compares patient outcomes at the acquiring (large) hospitals with those at our matched set of control hospitals. That is, we exclude patients who were treated in acquired hospitals prior to the merger, and estimate outcomes for patients who would have been treated at the acquired hospital if no merger had taken place, but instead went to the acquiring hospital after the merger. Therefore, it is possible that the poorer health outcomes in merged hospitals are not due to the merger activities, but due to selection in the patient sample. To test for this selection bias, we re-estimated our specification (3.2) using larger samples that included patients treated at the acquired hospitals before the merger. Both the magnitude and the precision of the estimates of the effects of mergers on the health outcomes for AMI, CABG, and pregnant patients, shown in Table 3.5, are virtually identical to our original results (Panel B of Table 3.2 and 3.3).

Another concern is that the interaction between the merger dummy and the categorical HHI dummy in Equation (3.2) may not capture the full information of the HHI. Therefore, we re-estimate using actual HHI, measured in the year before the merger. This specification allows for different slopes of hospital market concentration at specific values (zero or one) of the merger dummy. Equation (3.2) becomes:

$$\begin{aligned} y_{iht} = c_0 + \alpha_1 M_{ht}^{1-2} + \alpha_2 M_{ht}^{3-5} + \alpha_3 M_{ht}^{6+} + \beta_1(M_{ht}^{1-2} \times HHI) + \beta_2(M_{ht}^{3-5} \times HHI) \\ + \beta_3(M_{ht}^{6+} \times HHI) + \gamma_1 HHI_{ht} + \gamma_2 X_{iht} + \gamma_3 Z_{ht} + \phi_h + \tau_t + \epsilon_{iht}, \end{aligned} \tag{3.3}$$

[33]For example, Episcopal Hospital merged with Temple University Hospital on September 7, 2000, and services for Episcopal Hospital appear under ID of Temple University Hospital. In 1999, the bed size in Temple University hospital is 400-499, which is twice as large as that in Episcopal Hospital; and the number of AMI patients in Temple University hospital is 320, which is eight times as many as that in the Episcopal Hospital. After merge, the merged facility—Temple University Episcopal campus—provide emergency services, radiology, cardiology testing, pulmonary diagnostic services, laboratory services, inpatient medical unit, behavioral health, primary care and specialty physicians.

where the HHI is now a continuous variable rather than a vector of three dummy variables.

The results for AMI patients are presented in Columns (1) to (3) of Table 3.6. As before, increased market concentration increases the likelihood of inpatient mortality and readmission rate for AMI patients who were admitted to a merging hospital. For any given AMI case, we can calculate the overall effect of hospital mergers on the health outcomes as the sum of the coefficient on the merger dummy in each of the three periods and the coefficient on its interaction term multiplied by the HHI for the admitting hospital, i.e., $\alpha + \beta \times HHI$. For example, if the merging hospitals are located in an already highly-concentrated market, then a merger significantly raises the likelihood of inpatient mortality by 3.07 percentage points in the short run and 5.08 percentage points in the long run.[34] Therefore, the outcomes for AMI patients in merging hospitals are getting worse as market concentration increases, a conclusion that is consistent with the analysis from Table 3.2 Panel B. This consistency also exists for pregnant patients (Column (4) and (5) in Table 3.6) and CABG patients (Column (6) to (8) in Table 3.6). Therefore, the main merger effects estimated from Equation (3.2) are robust to this alternative specification.

Finally, our main regression results are based on propensity score-matched cohorts, but it is possible that there is still some bias in the estimated results due to the particular match method we used. For the results reported so far, there are different numbers of non-merging hospitals in the control group for each merging hospital. That is, while some merging hospitals can be matched with five non-merging hospitals within the caliper, some may be matched with only one. Therefore, it is possible that our significant results on merger effects may be due to the different numbers of non-merging hospitals we selected into the control group for each merging hospital. To address this potential problem, we use the nearest neighbor 1:1 "best match" with replacement to ensure that every merging hospital is matched with the one non-merging hospital with the closest propensity score one year prior to merger. While the number of observations in the sample of 1:1 best matching is much less than that of 1:5 variable ratio matching, the estimates of merger

[34] In the sample of AMI patients from 1999 to 2010, after 1:5 propensity score matching, the mean of HHI is 0.392, the standard deviation is 0.284, the minimum value is 0.044 and the maximum value is 1. The cut-off for the top 25% of HHI distribution is above 0.605 (highly concentrated markets) and the cut-off for the bottom 25% is below 0.120 (competitive market). Therefore, the overall effects of hospital mergers in the transition period on the probability of inpatient mortality are -0.031+0.102*0.605 =0.0307 in highly concentrated markets. The overall effects of hospital mergers in the long term on the probability of inpatient mortality are -0.026+0.127*0.605=0.0508 in highly concentrated markets.

effects, shown in Table 3.7, are similar to those reported in Table 3.2 and 3.3.

To sum up, our significant results in Section 5.1 are not affected by the alternative estimation samples, alternative variable ratio matching rule or alternative specifications we use.

3.6 Conclusions

This paper provides important insight into the impact of hospital mergers on the quality of care by examining their impact in markets with different degrees of concentration, and by examining whether and how that impact changes over time. We examine this issue for three different types of patients: AMI patients, pregnant patients, and patients undergoing CABG surgery.

We find that the effects of horizontal mergers occurring in more concentrated markets are generally detrimental to the quality of care for AMI and pregnant patients. The results demonstrate that mergers that occurred in more concentrated markets led to significant increases in the probability of mortality and readmission rate for AMI patients, and that these adverse merger effects last for a relatively long time period. Further, mergers that occurred in already concentrated markets are associated with increases in permanent adverse effects on pregnancy outcomes, and lower resource utilization. We also find that these negative results for pregnancy patients do not appear when the merger is between a hospital that offers obstetrical care and one that does not. Instead, the latter type of merger is associated with better health outcomes for pregnant patients in all the subsequent years following the merger.

In contrast, we find no evidence that pre-existing market concentration is associated with reductions in the quality of care after a merger for patients undergoing CABG surgery. However, it is also the case that information on the quality of CABG outcomes for hospitals and for surgeons was available during this period, making it more difficult for hospitals to exercise market power by reducing the quality of this procedure. Further, intensive surgeries require a large fixed investment in equipment and staff and a high level of technical proficiency. In a highly concentrated market, two merging hospitals might benefit disproportionately from the greater volume a merged program might achieve, resulting in better outcomes.

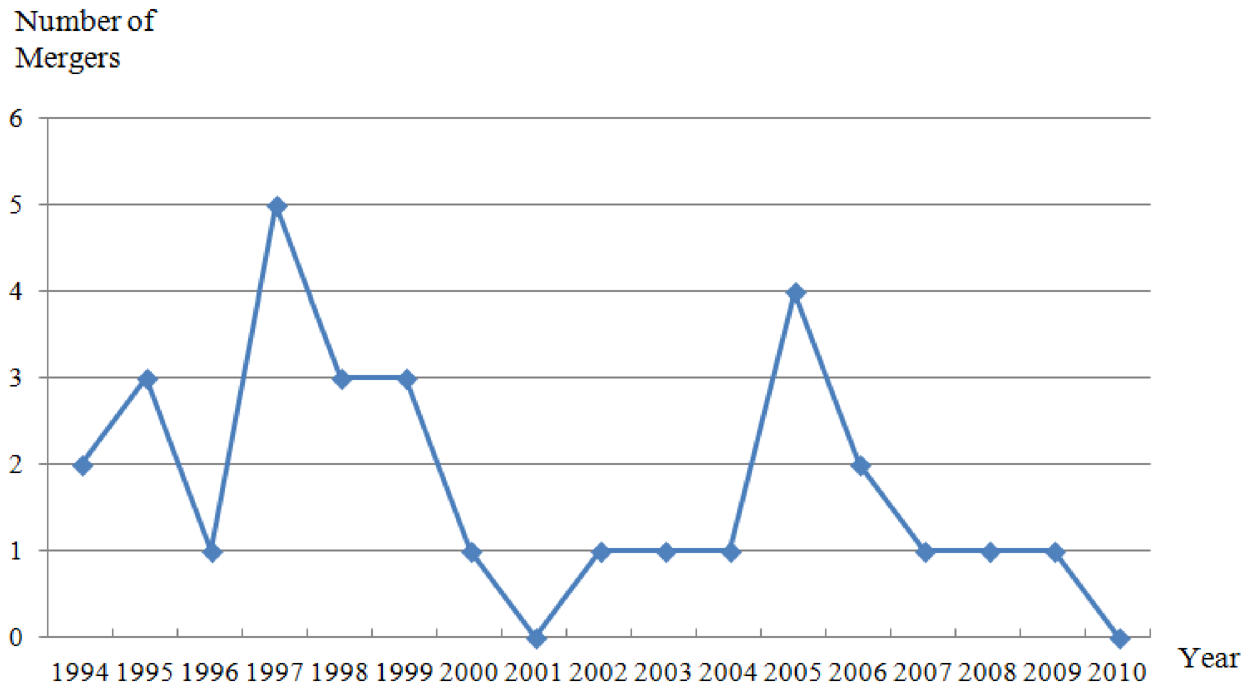
In sum, estimates of the effects of mergers for different samples of patients indicate that hospital mergers in Pennsylvania do not have a unilateral impact on the quality of care. First, our evidence suggests that it matters whether the merging hospitals both offer the same particular service. Second, our results for CABG patients suggest that the quality impact of a merger on surgical services may be different.

A possible limitation of this study is that there rarely exists a control group of non-merging hospitals that have never been exposed to merger activities at all, and that could affect our estimates. On the one hand, if merging hospitals exploit cost saving opportunities by reducing quality of care, ceteris paribus the non-merging hospitals located in the same local market may compete by reducing quality of care as well. On the other hand, even if the matched non-merging hospital is not in the same local market as the merging hospital, we cannot guarantee that this non-merging hospital we selected for the control group has not been affected by merger activities in other hospital markets. Therefore, our specification may underestimate merger effects. In future work we plan to explore the possibility of controlling for possible spillover effects on the unmerged hospitals.

We would also like to examine other measures of patient outcomes to more comprehensively evaluate the effects of mergers on various aspects of health care quality. In addition, we plan to estimate welfare changes for patients, hospitals, and insurers so that we can gain a better idea of whether mergers improve overall social welfare. Additional research examining changes in out-of-pocket prices for patients, the actual charges paid by insurance companies, as well as the cost savings and price increases made by hospitals will allow us to form a more complete assessment of the welfare impact of mergers.

3.7 Tables and Figures

Figure 3.1: Hospital Mergers in Pennsylvania, 1994-2010



Notes: There are 30 hospital mergers in Pennsylvania from 1994 to 2010. And there are 28 cases of mergers involved general medical and surgical hospitals. For the remaining two cases, one is the merger between two rehabilitation hospitals in 1997, and the other is the merger between two children's hospitals in 2007.

Table 3.1: Patient Profile in Propensity Score Matched Hospitals, One Year Before Merge

	AMI Patients		CABG Patients			Pregnant Patients	
	Merge	Nonmerge	Merge	Nonmerge		Merge	Nonmerge
Gender (male=1)	0.58	0.59	0.63	0.72			
Age Group					**Age Group**		
Age 49 or younger	0.12	0.12	0.08	0.10	Age 18 or younger	0.09	0.06
Age 50-59	0.17	0.17	0.17	0.20	Age 19-29	0.56	0.53
Age 60-69	0.22	0.22	0.28	0.29	Age 30-39	0.33	0.38
Age 70-79	0.28	0.27	0.33	0.30	Age 40-49	0.02	0.02
Age 80 or older	0.21	0.22	0.13	0.11	Age 50 or older	0.00	0.00
Race Group					**Race Group**		
White	0.87	0.90	0.87	0.92	White	0.70	0.77
Black	0.07	0.05	0.06	0.05	Black	0.24	0.10
Asian	0.00	0.00	0.00	0.00	Asian	0.01	0.01
Other Races	0.05	0.05	0.07	0.02	Other Races	0.05	0.12
Urban Residence	0.79	0.66	0.69	0.64	**Urban Residence**	0.79	0.72
Admission Source					**Admission Source**		
Emergency Room					Emergency Room		
or Urgent Admission	0.92	0.95	0.81	0.93	or Urgent Admission	0.43	0.40
Transferred Admission	0.20	0.29	0.40	0.38	Transferred Admission	0.00	0.00
Patient Severity					**Patients Severity**		
Charlson Index							
CCI=1	0.37	0.37	0.30	0.36	Pre-existing Complications	0.21	0.23
CCI=2	0.34	0.33	0.41	0.37	Non-preventable Complications	0.14	0.11
CCI=3	0.18	0.18	0.19	0.19	Previous C-section	0.12	0.12
CCI=4 or above	0.11	0.11	0.10	0.08	Multiple Birth	0.02	0.02
Previous 3-year AMI	0.10	0.10	0.09	0.08	C-section	0.23	0.23
Previous 3-year CABG	0.02	0.01	0.00	0.00	Vaginal Delivery with Instrument	0.82	0.80
Insurance Type					**Insurance Type**		
Medicare	0.58	0.58	0.52	0.52	Medicare	0.02	0.01
Medicaid	0.07	0.05	0.08	0.06	Medicaid	0.24	0.29
Commercial	0.13	0.14	0.12	0.10	Commercial	0.37	0.34
HMO	0.30	0.20	0.40	0.22	HMO	0.49	0.43
No Insurance	0.04	0.04	0.06	0.04	No Insurance	0.08	0.03
Number of Hospitals	12	41	4	12	**Number of Hospitals**	8	35
Number of Patients	6,727	16,180	212	569	**Number of Patients**	11,248	36,779

Notes: After 1:5 variable ratio caliper propensity score matching, we selected the non-merging hospitals as the control group for each merging hospital. This table compares the patient profile in the matched hospitals one year prior to merger, 1994-2010. For AMI patients, their severity is measured as Charlson co-morbidity index, the indicator for whether the patient has AMI (or CABG surgery) within previous three years, as well as patient's admission source—emergency room or urgent care visits. For pregnant patients, their severity is measured by the indicators: whether a patient has at least one non-preventable complication; whether she has at least one pre-existing complication; whether this delivery is vaginal delivery with instrument, C-section or a multiple birth; and whether the patient had a C-section before.

Table 3.2: Effects of Mergers on Outcomes of AMI and Pregnant Patients, 1999-2010

	AMI Patients			Pregnant Patients	
	(1) Mortality	(2) Readmission	(3) Ln(Total Charges)	(4) Preventable Complication	(5) Ln(Total Charges)
Panel A: Without Market Concentration Measures					
Short-term	-0.002	-0.000	-0.110**	0.002	-0.106***
	[0.014]	[0.006]	[0.045]	[0.006]	[0.032]
Medium-term	-0.003	0.007	-0.115	0.007	-0.113**
	[0.011]	[0.008]	[0.093]	[0.007]	[0.049]
Long-term	0.005	-0.004	-0.127	0.013	-0.184*
	[0.013]	[0.008]	[0.123]	[0.009]	[0.101]
HHIt	0.038	0.028	-0.333	-0.010	0.132
	[0.031]	[0.023]	[0.452]	[0.018]	[0.254]
Observations	160,637	149,606	160,637	497,553	497,553
Number of Hospitals	52	52	52	43	43
Adj. R-squared	0.098	0.036	0.127	0.013	0.473
Panel B: Add Interactions between Merger Dummies and Categorical Market Concentration					
Short-term×Highly Concentrated	0.008	0.038***	-0.000	0.022***	-0.165**
	[0.005]	[0.005]	[0.069]	[0.006]	[0.077]
Medium-term×Highly Concentrated	0.017***	0.007**	0.048	0.014***	-0.166***
	[0.005]	[0.004]	[0.052]	[0.004]	[0.043]
Long-term×Highly Concentrated	0.021***	0.033***	-0.017	0.030***	-0.075*
	[0.004]	[0.006]	[0.059]	[0.006]	[0.044]
Short-term×Concentrated	0.024*	0.014**	-0.207*	0.020**	-0.200*
	[0.013]	[0.006]	[0.119]	[0.009]	[0.105]
Medium-term×Concentrated	0.024*	0.020**	-0.230	0.023*	-0.270**
	[0.013]	[0.008]	[0.219]	[0.013]	[0.128]
Long-term×Concentrated	0.036***	0.014	-0.236	0.028	-0.177
	[0.012]	[0.009]	[0.214]	[0.019]	[0.142]
Short-term×Competitive	-0.026*	-0.006	-0.184***	-0.011***	-0.057*
	[0.014]	[0.008]	[0.058]	[0.003]	[0.030]
Medium-term×Competitive	-0.008	-0.007	-0.262***	-0.004	0.016
	[0.018]	[0.006]	[0.069]	[0.005]	[0.042]
Long-term×Competitive	-0.010	-0.043***	-0.119*	0.001	-0.241**
	[0.010]	[0.007]	[0.060]	[0.006]	[0.089]
HHIt	0.001	0.007	-0.259	-0.024	0.207
	[0.028]	[0.024]	[0.479]	[0.017]	[0.311]
Observations	160,637	149,606	160,637	497,553	497,553
Number of Hospitals	52	52	52	43	43
Adj. R-squared	0.099	0.036	0.129	0.013	0.476

Notes: The observations of AMI and pregnant patients are from the 1:5 propensity score matched hospitals. Column (1)-(3) are health outcomes for AMI patients, and Column (4)-(5) are for pregnant patients. The results in Panel A are estimated from Equation (3.1). The results in Panel B are estimated from Equation (3.2). If hospital market concentration one year prior to merger is in the top 25% of HHI distribution, then this market is defined as highly concentrated market; if in the median 50%, then it is defined as concentrated market; if in the bottom 25%, then it is defined as competitive market. Robust standard errors in brackets are clustered at the hospital level for all regressions. Significance levels are indicated by: *** for the 1% level, ** for the 5% level, * for the 10% level (two-tail test).

Table 3.3: Effects of Mergers on Outcomes of CABG Patients, 1999-2010

	(1) Mortality	(2) Readmission	(3) Ln(Total Charges)
Panel A: Without Market Concentration Measures			
Short-term	0.020	-0.004	-0.119**
	[0.020]	[0.039]	[0.056]
Medium-term	0.001	-0.018	0.048
	[0.014]	[0.013]	[0.124]
Long-term	0.025*	0.003	-0.216
	[0.014]	[0.022]	[0.125]
HHIt	-0.034	-0.103	-0.525
	[0.067]	[0.117]	[0.799]
Observations	8,644	8,412	8,644
Number of Hospitals	16	16	16
Adj. R-squared	0.018	0.04	0.374
Panel B: Add Interactions between Merger and Categorical Market Concentration			
Short-term×Highly Concentrated	-0.048***	-0.072***	-0.068
	[0.009]	[0.009]	[0.051]
Medium-term×Highly Concentrated	-0.007	-0.016	-0.014
	[0.005]	[0.010]	[0.081]
Short-term×Concentrated	0.026	0.015	-0.320**
	[0.062]	[0.043]	[0.147]
Medium-term×Concentrated	0.044	0.059	-0.459**
	[0.063]	[0.051]	[0.158]
Long-term×Concentrated	0.042	0.066*	-0.670***
	[0.034]	[0.034]	[0.190]
Short-term×Competitive	0.000	0.118***	-0.147
	[0.011]	[0.011]	[0.102]
Medium-term×Competitive	-0.035***	-0.022*	0.516***
	[0.009]	[0.011]	[0.070]
Long-term×Competitive	-0.009	0.030*	0.017
	[0.008]	[0.015]	[0.070]
HHIt	-0.090	-0.242**	0.598
	[0.079]	[0.103]	[0.478]
Observations	8,644	8,412	8,644
Number of Hospitals	16	16	16
Adj. R-squared	0.019	0.041	0.403

Notes: The observations of CABG patients are from the 1:5 propensity score matched hospitals. The results in Panel A are estimated from Equation (3.1). The results in Panel B are estimated from Equation (3.2). Due to the small sample size of horizontal merging hospitals that provide CABG surgery, hospitals that are located in highly concentrated markets and merged more than six years ago are omitted. Robust standard errors in brackets are clustered at the hospital level for all regressions. Significance levels are indicated by: *** for the 1% level, ** for the 5% level, * for the 10% level (two-tail test).

Table 3.4: Effects of "Non-Horizontal" Mergers on Outcomes of Pregnant Patients, 1999-2010

	(1) Preventable Complication	(2) Ln(Total Charges)
Panel A: Without Market Concentration Measures		
Short-term	-0.010	-0.137**
	[0.008]	[0.062]
Medium-term	-0.025**	0.234
	[0.011]	[0.218]
Long-term	-0.050***	0.078
	[0.012]	[0.220]
HHIt	0.015	-0.890**
	[0.017]	[0.387]
Observations	233,363	233,363
Number of Hospitals	14	14
Adj. R-squared	0.011	0.501
Panel B: Add Interactions between Merger Dummies and Categorical Market Concentration Measures		
Medium-term×Concentrated	-0.011*	0.620***
	[0.005]	[0.091]
Long-term×Concentrated	-0.038***	0.351***
	[0.009]	[0.106]
Short-term×Competitive	-0.018***	-0.034
	[0.005]	[0.038]
Medium-term×Competitive	-0.039***	-0.059
	[0.009]	[0.075]
Long-term×Competitive	-0.059***	-0.228**
	[0.012]	[0.095]
HHIt	0.015	-0.965**
	[0.018]	[0.435]
Observations	233,363	233,363
Number of Hospitals	14	14
Adj. R-squared	0.011	0.51

Notes: The "non-horizontal" merger occurs when a hospital offering obstetrical care and one that does not. The observations of pregnant patients are from the 1:5 propensity score matched hospitals. The results in Panel A are estimated from Equation (3.1). The results in Panel B are estimated from Equation (3.2). Robust standard errors in brackets are clustered at the hospital level for all regressions. Significance levels are indicated by: *** for the 1% level, ** for the 5% level, * for the 10% level (two-tail test).

Table 3.5: Robustness Checks: Add Acquired Hospitals

	AMI Patients			**Pregnant Patients**		**CABG Patients**		
	(1) Mortality	(2) Readmission	(3) Ln(Total Charges)	(4) Preventable Complication	(5) Ln(Total Charges)	(6) Mortality	(7) Readmission	(8) Ln(Total Charges)
Short-term×Highly	0.007	0.038***	-0.01	0.024***	-0.158**	-0.050***	-0.070***	-0.073
	[0.005]	[0.005]	[0.066]	[0.006]	[0.073]	[0.009]	[0.010]	[0.050]
Medium-term×Highly	0.018***	0.007*	0.04	0.013***	-0.160***	-0.007	-0.017*	-0.016
	[0.005]	[0.004]	[0.049]	[0.004]	[0.041]	[0.005]	[0.010]	[0.078]
Long-term×Highly	0.021***	0.034***	-0.023	0.031***	-0.065	-	-	-
	[0.004]	[0.006]	[0.056]	[0.007]	[0.041]			
Short-term×Concentrated	0.025**	0.011**	-0.192*	0.019**	-0.187*	0.025	0.012	-0.326**
	[0.012]	[0.006]	[0.107]	[0.009]	[0.102]	[0.061]	[0.042]	[0.144]
Medium-term×Concentrated	0.027**	0.017**	-0.214	0.021	-0.254**	0.046	0.055	-0.466***
	[0.013]	[0.008]	[0.208]	[0.013]	[0.125]	[0.062]	[0.049]	[0.156]
Long-term×Concentrated	0.037***	0.011	-0.215	0.027	-0.167	0.045	0.063*	-0.667***
	[0.011]	[0.009]	[0.207]	[0.019]	[0.140]	[0.034]	[0.033]	[0.188]
Short-term×Competitive	-0.026*	-0.006	-0.173***	-0.011***	-0.056*	0.000	0.120***	-0.154
	[0.014]	[0.008]	[0.056]	[0.004]	[0.032]	[0.011]	[0.011]	[0.098]
Medium-term×Competitive	-0.008	-0.006	-0.249***	-0.002	-0.02	-0.035***	-0.025**	0.517***
	[0.019]	[0.006]	[0.069]	[0.005]	[0.050]	[0.008]	[0.012]	[0.069]
Long-term×Competitive	-0.011	-0.042***	-0.124**	-0.001	-0.204**	-0.01	0.032**	0.013
	[0.010]	[0.007]	[0.059]	[0.006]	[0.095]	[0.008]	[0.015]	[0.068]
HHIt	-0.001	0.014	-0.272	-0.021	0.176	-0.099	-0.227**	0.599
	[0.027]	[0.024]	[0.472]	[0.017]	[0.305]	[0.077]	[0.097]	[0.469]
Observations	168,421	156,769	168,421	531,103	531,103	9,019	8,772	9,019
Number of Hospitals	64	64	64	48	48	20	20	20
Adj. R-squared	0.098	0.036	0.125	0.013	0.482	0.018	0.042	0.398

Notes: Column (1)-(3) are health outcomes for AMI patients, Column (4)-(5) are for pregnant patients, and Column (6)-(8) are for CABG patients. The observations of patients are from the 1:5 propensity score matched hospitals, and add the patients treated in acquired hospitals prior to the merger. Robust standard errors in brackets are clustered at the hospital level for all regressions. Significance levels are indicated by: *** for the 1% level, ** for the 5% level, * for the 10% level (two-tail test).

Table 3.6: Robustness Check: with Continuous Market Concentration Measures

	AMI Patients			**Pregnant Patients**		**CABG Patients**		
	(1) Mortality	(2) Readmission	(3) Ln(Total Charges)	(4) Preventable Complication	(5) Ln(Total Charges)	(6) Mortality	(7) Readmission	(8) Ln(Total Charges)
Short-term	-0.031**	-0.015***	-0.178***	-0.011***	-0.060*	0.030*	0.213***	-0.304**
	[0.013]	[0.005]	[0.052]	[0.003]	[0.032]	[0.015]	[0.016]	[0.131]
Medium-term	-0.014	-0.008	-0.15	-0.004	0.073	-0.053***	0.003	0.396***
	[0.017]	[0.008]	[0.140]	[0.008]	[0.060]	[0.011]	[0.037]	[0.130]
Long-term	-0.026*	-0.027***	-0.025	-0.005	-0.247**	-0.038***	0.05	0.056
	[0.015]	[0.009]	[0.165]	[0.007]	[0.110]	[0.009]	[0.032]	[0.132]
Short-term×HHI	0.102***	0.047**	0.223	0.050***	-0.021	-0.120**	-0.493***	0.809**
	[0.030]	[0.020]	[0.268]	[0.014]	[0.134]	[0.043]	[0.046]	[0.280]
Medium-term×HHI	0.057	0.040*	0.025	0.028	-0.391***	0.071***	0.005	-0.694*
	[0.035]	[0.020]	[0.338]	[0.018]	[0.123]	[0.022]	[0.075]	[0.337]
Long-term×HHI	0.127***	0.088***	-0.501	0.049***	0.358*	0.131**	-0.012	-0.962*
	[0.034]	[0.018]	[0.447]	[0.017]	[0.212]	[0.056]	[0.075]	[0.516]
HHIt	0.014	0.02	-0.292	-0.009	0.149	-0.052	-0.162	-0.108
	[0.024]	[0.026]	[0.421]	[0.015]	[0.236]	[0.082]	[0.119]	[0.567]
Observations	145,623	135,898	145,623	455,819	455,819	7,469	7,281	7,469
Number of Hospitals	52	52	52	43	43	16	16	16
Adj. R-squared	0.094	0.037	0.118	0.012	0.472	0.018	0.041	0.368

Notes: Column (1)-(3) are health outcomes for AMI patients, Column (4)-(5) are for pregnant patients, and Column (6)-(8) are for CABG patients. The observations of patients are from the 1:5 propensity score matched hospitals and the results are estimated from Equation (3.3) with the interactions between the merger dummy and continuous HHI. Robust standard errors in brackets are clustered at the hospital level for all regressions. Significance levels are indicated by: *** for the 1% level, ** for the 5% level, * for the 10% level (two-tail test).

Table 3.7: Robustness Check: 1:1 Matching

	AMI Patients			**Pregnant Patients**		**CABG Patients**		
	(1) Mortality	(2) Readmission	(3) Ln(Total Charges)	(4) Preventable Complication	(5) Ln(Total Charges)	(6) Mortality	(7) Readmission	(8) Ln(Total Charges)
Short-term×Highly	0.005	0.043***	-0.099	0.035***	-0.300***	-0.048**	-0.085***	-0.029
	[0.006]	[0.006]	[0.086]	[0.004]	[0.085]	[0.016]	[0.013]	[0.119]
Medium-term×Highly	0.011**	0.011**	-0.013	0.018***	-0.264***	-0.01	-0.031	-0.057
	[0.005]	[0.005]	[0.095]	[0.004]	[0.057]	[0.010]	[0.017]	[0.136]
Long-term×Highly	0.016***	0.036***	-0.062	0.032***	-0.153**	-	-	-
	[0.005]	[0.006]	[0.063]	[0.007]	[0.054]			
Short-term×Concentrated	0.024*	0.026***	-0.065	0.015	-0.078	-0.061*	-0.052	-0.157
	[0.014]	[0.007]	[0.161]	[0.010]	[0.081]	[0.029]	[0.043]	[0.095]
Medium-term×Concentrated	0.024	0.033***	-0.064	0.011	-0.108	-0.05	0.003	-0.356**
	[0.017]	[0.010]	[0.235]	[0.013]	[0.074]	[0.034]	[0.061]	[0.120]
Long-term×Concentrated	0.036**	0.027**	-0.005	0.014	0.071	-0.017	0.014	-0.459**
	[0.015]	[0.012]	[0.240]	[0.015]	[0.058]	[0.039]	[0.062]	[0.141]
Short-term×Competitive	-0.025*	-0.006	-0.116	-0.010***	-0.04	0.004	0.109***	-0.244**
	[0.014]	[0.009]	[0.075]	[0.002]	[0.030]	[0.014]	[0.021]	[0.073]
Medium-term×Competitive	-0.003	-0.008	-0.209*	-0.013***	0.105*	-0.035***	-0.017	0.514***
	[0.018]	[0.006]	[0.101]	[0.004]	[0.056]	[0.008]	[0.017]	[0.052]
Long-term×Competitive	-0.001	-0.050***	-0.058	-0.009	-0.08	-0.013	0.006	-0.061
	[0.011]	[0.008]	[0.106]	[0.005]	[0.076]	[0.011]	[0.021]	[0.102]
HHIt	-0.008	-0.048	-0.944	-0.017	0.084	0.107	-0.032	-0.41
	[0.045]	[0.032]	[0.965]	[0.019]	[0.316]	[0.141]	[0.203]	[0.670]
Observations	110,424	102,717	110,424	208,872	208,872	4,044	3,922	4,044
Number of Hospitals	25	25	25	17	17	8	8	8
Adj. R-squared	0.101	0.039	0.118	0.017	0.456	0.026	0.048	0.459

Notes: Column (1)-(3) are health outcomes for AMI patients, Column (4)-(5) are for pregnant patients, and Column (6)-(8) are for CABG patients. The observations of patients are from the 1:1 propensity score "best" matched hospitals and the results are estimated from Equation (3.2). Robust standard errors in brackets are clustered at the hospital level for all regressions. Significance levels are indicated by: *** for the 1% level, ** for the 5% level, * for the 10% level (two-tail test).

Table 3.8: Logistics Regression of Horizontal Mergers on Hospital Baseline Characteristics, 1994-2010

Hospital Characteristics	Hospitals Treat AMI Patient	Hospitals Treat CABG Patient	Hospitals Treat Pregnant Patient
System membership	0.709**	0.758**	0.571*
	[0.286]	[0.363]	[0.313]
Council of teaching hospital	-0.414	-0.45	-0.199
	[0.358]	[0.383]	[0.396]
Cardiac intensive care beds	0.045***	0.040**	-0.01
	[0.014]	[0.018]	[0.021]
Medical/surgical intensive beds	0.020**	0.035***	-0.005
	[0.009]	[0.009]	[0.013]
Total beds	0.006***	0.005***	0.008***
	[0.001]	[0.001]	[0.002]
Medical/surgical intensive care	-0.172	-0.584	0.789
	[0.548]	[0.703]	[0.537]
Obstetric care	-0.084	-0.603	0.945
	[0.348]	[0.566]	[0.853]
Cardiac intensive care	-0.649**	0.38	-0.179
	[0.294]	[0.404]	[0.329]
Neonatal intensive care	1.306***	2.022***	1.532***
	[0.291]	[0.486]	[0.335]
Neonatal intermediate care	-2.174***	-1.858***	-2.239***
	[0.442]	[0.405]	[0.483]
Skilled nursing hospital	0.143	0.026	0.274
	[0.238]	[0.278]	[0.247]
Metropolitan area	-0.276	14.352***	-0.511
(population$\leq$2,500,000)	[0.346]	[0.470]	[0.373]
Metropolitan area	-2.559***	10.838***	-1.787***
(population>2,500,000)	[0.525]	[0.666]	[0.566]
Share of admission-Medicare	4.396***	3.679	7.582***
	[1.577]	[2.298]	[2.511]
Share of admission-Medicaid	8.005***	5.543**	9.754***
	[1.940]	[2.483]	[2.284]
Share of admission-HMO	2.015*	6.587***	0.842
	[1.101]	[1.606]	[1.891]
Severity of AMI patients	-0.702		
	[0.578]		
Severity of CABG patients		0.244	
		[0.548]	
Severity of pregnant patients			2.339***
			[0.822]
Observations	2,474	775	1,975

Notes: The number of observations is hospital-year based. All the coefficients are estimated from a multivariate logistic regression model with time fixed effects. Robust standard errors are in brackets. Significance levels are indicated by: *** for the 1% level, ** for the 5% level, * for the 10% level (two-tail test).

Chapter 4

Recessions and Stress-related Health Outcomes

Evidence from Inpatient Data at the Community Level in Pennsylvania

4.1 Introduction

The effects of economic recessions on people's health are paradoxical, and previous literature has not reached a consensus. It is understandable that losing a job during a recession may hurt people both physically and mentally. A number of papers (Jin et al., 1997; Eliason et al., 2009; Black et al., 2012) find that unemployment caused by involuntary job loss is associated with adverse health outcomes. Conversely, other evidence (Ruhm, 2000; 2003; 2005) indicates that during recessions, people improve their health by increasing leisure-time physical activities, reducing such harmful behaviors as smoking, heavy drinking or overeating, and decreasing the chance of work-related stress and inflammation.

Although there is a large literature on the effect of economic conditions on health outcomes at either individual level or state level, fewer address whether economic recessions affect the average health outcomes of a population living in the same community. Several recent studies (Warhover, 2014; Williams, 2013; Marks, 2009) show a strong connection between health and communities, and conclude that peoples' zip codes may be even more important to their health than their genetic code. Therefore, this paper provides a more comprehensive evaluation of how communities' different characteristics change the effects of recessions on average health outcomes at the community level, defining a community as the zip code of residence.[1]

[1]The information at the zip code level is the best we can find to represent a community. We do not use

This paper uses the inpatient data from the Pennsylvania Health Care Cost Containment Council (PHC4) for the 2000-2011 period, which includes two recessions. We analyze the effects of recessions on the percentage of total population in a community who experience stress-related diseases requiring inpatient care, i.e., myocardial infarction, stroke, alcohol-related diagnoses, diabetes, and obesity (Eliason and Storrie, 2009; Ruhm 2003), while controlling for the community's socio-demographic characteristics. On the one hand, people who lose their jobs during recession could experience changes in their health status; on the other hand, individuals who are still in the labor force could also suffer from adverse macroeconomic conditions in terms of increased job insecurity, decreased promotion opportunities, reduced payment, or other negative financial changes. We use hospitalization rates to reflect actual changes in people's health status because hospitalizations have a lower discretionary component and lower demand elasticities than outpatient care (Ruhm, 2003).

Aside from the effects of recession, however, the incidence of these diseases might be also associated with community-based measures of socioeconomic status. For example, the health outcomes for people living in the poor community may be more sensitive to recessions. They are more likely to work under hazardous conditions and have work-related physical exertion or medical problems. If they are forced to work fewer hours during recessions or lose their jobs entirely, they are less exposed to health risks and tend to be healthier. In addition, people living in the low-income communities with limited choices of restaurants, most of which are fast food restaurants (Zenk et al., 2009), are more likely to have time to cook at home during recessions, which may result in better health. Therefore, to see how a community's socio-demographic factors affect the impact of recession on health, we add interactions between the categories of community characteristics (i.e., poverty or population density) and the unemployment rate at the county level.

This paper presents evidence that recessions are associated with higher risk of alcohol-related hospitalization and lower risk of hospitalization for diabetes for all communities, and associated with reduced risk of hospitalization for severe stress-related diseases among high-poverty or high-density communities.

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county to represent community, because there are too many variations in poverty, income, crime rates, or education achievement within one county in the same period of time.

The rest of the paper is organized as follows. Section 4.2 reviews the literature. Section 4.3 describes the data and summary statistics. Section 4.4 presents the empirical model. Section 4.5 provides the main results and robustness check. Section 4.6 concludes.

4.2 Literature Review

There are a number of papers on the relationship between macroeconomic conditions and health status. Some of them focus on the effects of recessions or expansions on peoples' health; others focus on the effects of specific labor market conditions, such as job displacement, on health outcomes.

Ruhm (2000) uses state-level aggregate data for the 1972-1991 period and employs state unemployment rates as the primary proxy for macroeconomic conditions. He finds that state unemployment rates are negatively related with total mortality and with eight of ten sources of fatalities, with suicides as one important exception. Ruhm (2003) examines relationships between individual-level data from the 1972-1981 National Health Interview Survey (NHIS) and state level unemployment rates, and finds that physical health deteriorates as the economy expands. Ruhm (2005) also uses individual-level data for the 1987-2000 period, and state-level measures of economic conditions (employment rate, weekly working hours, and personal income). He concludes that smoking and excess weight decline during temporary economic downturns because of increases in physical activity. To sum up, Ruhm finds that recessions are good for health. However, when mental health is also taken into account, the opposite results are found by Davalos and French (2011) — increases in the average state unemployment rate cause people's health-related quality of life (HRQL), which is measured by physical and mental health summary scores, to decline.

While some studies use unemployment rates or personal income to measure the economic downturn, some directly examine the impact of temporary unemployment on individual's health, without the control for business cycle. Jin et al. (1997) review the evidence from previous studies and reach a conclusion that unemployment causes adverse health outcomes, both at the population and individual levels. Because of the ambiguous causal link between ill health and unemployment (i.e., unemployment may cause the poor health, but it is also possible that less healthy people are more likely to lose their jobs), some papers use job loss due only to firm or plant closures as a quasi-experiment in order to rule out the selection effect. Eliason and Storrie (2009) create the linked employee-employer data in Sweden from 1983 to 1999, and find that job loss significantly increases the risk of hospitalization due to alcohol-related conditions, traffic accidents and self-harm, but does not affect the risk of cardiovascular diseases. Black et al. (2012) employ a difference-in-difference approach to compare changes in the conditional mean of health for the displaced workers (treatment group) and non-displaced workers (control group). They find that job displacement negatively affects the health of both men and women in their early forties, a result driven mainly by increases in smoking.

There is also epidemiological research examining the relationship between the incidence of diseases and neighborhood environment. Bindman et. al (1995) find that higher hospital admission rates for chronic medical conditions are more likely to happen in low-income communities due to the limited access to care. Acevedo-Garcia (2001) finds that zip-code level risk factors (poverty, crowded housing, dilapidated housing, and segregation indices) are associated with very high tuberculosis rates for Hispanics and African Americans. However, fewer studies comprehensively examine the relationship among recessions, health outcomes and neighborhood environment.

Since the increased unemployment rates during recessions may not only affect health outcomes for jobless persons, but also affect the health of a person who still has a job by increasing the stress-related (working harder and longer due to the concern about losing job) behaviors, such as smoking or heavy drinking, this paper examines whether the average physical health outcomes, measured by stress-related conditions, are affected by recessions. In addition, the question remains unclear whether the effects of recessions on average health outcomes are different for people who live in high-poverty (or high population density) communities and those in the high-income (or low population density) communities.[2] Therefore, this paper further examines the relationship between recessions and the incidence of certain stress-related disease at the community level to test for any effects of neighborhood environment. Finally, this paper provides a more comprehensive evaluation of how recessions affect health outcomes at the community level, examining not only the contemporaneous recession effects, but also the persistent effects of recessions.

[2]The communities with high population density are usually located within urban area. They can be either wealthy communities with complete modern infrastructure, or poor communities lacking of a fully developed infrastructure. Therefore, in the empirical model part, we also use the interaction between poverty and population density to characterize communities.

4.3 Data

This paper analyzes the physical health outcomes at community level in Pennsylvania from 2000 to 2011, during which people experienced two recessions (March 2001 to November 2001, and December 2007 to June 2009). The data come from three primary sources: the Pennsylvania Health Care Cost Containment Council (PHC4), United States Census Bureau, and Bureau of Labor Statistics.

The health outcomes are derived from the inpatient data collected by the PHC4, which include patient characteristics such as age, race, gender, admission type, insurance coverage type, diagnosis and procedure codes, zip code of residence, total in-hospital charges, and length of stay. We define a community as the zip code where people live. For each community, we count the number of patients hospitalized for each of the stress-related diseases, based on ICD-9 codes: acute myocardial infarction (AMI) (ICD-9 codes: 410), stroke (ICD-9 codes: 430-438), alcohol-related diagnoses (ICD-9 codes: 291, 303, 305.0, 357.5, 425.5, 535.3, 571.0-571.3, 577.0, 577.1, 980), diabetes (ICD-9 codes: 250), and obesity (ICD-9 codes: 278). This choice of diagnoses is based on the possible health outcomes from stress, depression, or anxiety, and the conditions have been used in previous work (Eliason and Storrie, 2009; Ruhm, 2003). We exclude records for patients who are not Pennsylvania residents and records with invalid zip code information.[3] Hospitalization rates are calculated as the ratio of hospitalized patients over the total population in each community.

We use the information on the ZIP Code Tabulation Areas (ZCTA) to approximately estimate community characteristics. The ZIP Code Tabulation Areas (ZCTA) Gazetteer File 2010 and 2000, from United States Census Bureau, contain census population and land area at the ZCTA level. The population for non-census years is interpolated by assuming a constant rate of growth between census periods, and the population density of a community is calculated as the ratio of population over land area at the ZCTA level. Other community characteristics come from the 2008-2012 American Community Survey (ACS) 5-year average data, including income, poverty, educational attainment, insurance coverage, etc. [4] Furthermore, because in most instances the

[3]We exclude homeless patients from our sample, and also eliminate patient records with less than 5-digital zip code or with missing zip codes.

[4]http://www2.census.gov/acs2012_5yr/summaryfile/ACS_2008-2012_SF_Tech_Doc.pdf

ZCTA code is the same as the ZIP code for an area, we can link patient data at the zip code level with community characteristics measured at the ZCTA level.[5]

Finally, we use the annual average county unemployment rate to be the primary indicator for recessions, which is released by Bureau of Labor Statistics.[6] We use county unemployment rates, rather than zip code unemployment rates, because the zip code where people live is frequently different from the zip code where they work.[7]

During study period, Pennsylvania has 67 counties and roughly 2,185 zip codes. The summary statistics for community hospitalization rates, community characteristics, and yearly county unemployment rates are presented in Table 4.1. The mean hospital admission rates due to acute myocardial infarction and stroke, are 0.42% and 0.57%, while those for two chronic diseases, diabetes and obesity, are much lower, 0.24% and 0.06%, respectively.

4.4 Empirical Model

We study a regression of the form

$$H_{ijt} = c_0 + \alpha_1 UNEMPLOYMENT_{jt} + \phi_i + \tau_t + \epsilon_{ijt}, \tag{4.1}$$

where H_{ijt} is the rate of hospitalization for a stress-related disease in zip code i county j year t. UNEMPLOYMENT is the unemployment rate in county j in year t. Throughout the paper, hospitalization and unemployment rates are measured in percentages.[8] In addition, we include zip code fixed effects, ϕ_i, to control for time-invariant community characteristics that may be correlated with economic conditions, and year fixed effects, τ_t, to hold constant determinants of

[5]In creating ZCTAs, the Census Bureau took the most frequently occurring ZIP Code in an area for the ZCTA code. In most instances the ZCTA code is the same as the ZIP Code for an area, but ZCTAs are not developed for ZIP codes that comprise only a small number of addresses nor do they include US PO Box ZIP codes or unique ZIP codes. In our sample, we drop these communities and communities with a ZCTA code that is different from their ZIP code. https://www.census.gov/geo/maps-data/data/gazetteer.html

[6]http://www.bls.gov/lau/

[7]In Pennsylvania, some zip codes fall into more than one county. Then the question becomes which county's unemployment rate should be used to measure recessions. To solve this problem, we use the average unemployment rates of the neighboring counties that one zip code overlaps, as the primary indicator for recession. Alternatively, we delete these zip codes that cross county borders. However, the empirical results based on these two methods are still similar to the main results.

[8]For example, a five percent rate of hospitalization is entered in our data set as 5, and a seven percent unemployment rate is represented as 7.

health that vary uniformly across communities over time. Zip code and year fixed effects control for a wide variety of difficult-to-observe factors that might affect health outcomes. An error term is represented by ϵ_{ijt}.

Finally, since the community people live in could impact their own health, we test whether community poverty levels or population density alters the relative size of recession effects on health, using the following equation:

$$H_{ijt} = c_0 + \alpha_1 UNEMPLOYMENT_{jt} + \alpha_2 UNEMPLOYMENT_{jt} \times Characteristics_i + \phi_i + \tau_t + \epsilon_{ijt} \tag{4.2}$$

Characteristics is a binary variable indicating whether the poverty (or population density) of community i is in the top 50% of its distribution. That is, High Poverty equals one if the percentage of community population below the poverty level is at or above the median level, and zero otherwise. We also interact the poverty level with population density to characterize community, where the control group is communities of low poverty and low density, i.e., wealthy and suburban areas, or rich and less densely populated cities.[9]

It is also possible that health status may not respond immediately to economic fluctuations, or the recession may influence health outcomes on a lagged basis. Therefore, we use unemployment rates one or two years ago to account for the persistent effects of recessions on stress-related outcomes; two years has been found to be the optimal lag time in prior research (Fenwick and Tausig, 1994).[10]

$$\begin{aligned} H_{ijt} = c_0 + \alpha_1 UNEMPLOYMENT_{jt} + \alpha_2 UNEMPLOYMENT_{jt-1} \\ + \alpha_3 UNEMPLOYMENT_{jt-2} + \phi_i + \tau_t + \epsilon_{ijt} \end{aligned} \tag{4.3}$$

[9]This type of community includes Bangor City (zip code 18013) in Northampton county, Danielsville City (zip code 18038) in Northampton county, New Tripoli (zip code 18066) in Lehigh county, Zionsville (zip code 18092) in Lehigh county and so on. Take Bangor for example: the population growth rate is lower than the state average rate of 3.43%, and the median household income is 56,924 in 2008-2012 with a faster growth rate than the state average rate.

[10]Fenwick and Tausig study how macroeconomic changes affect stress level using a two-year lagged occupation-specific unemployment rate as an indicator of macroeconomic conditions.

4.5 Results

The effects of recessions on health outcomes at community level are shown in Table 4.2. Without controlling for community characteristics in Panel A, we find that recessions significantly increase the risk of hospitalization due to alcohol-related conditions, and significantly decrease the rate of hospitalization due to diabetes. Specifically, a one percentage point increase in the county unemployment rate increases the alcohol-related hospitalization rates by 0.019 percentage point, corresponding to a 9 percent rise at the sample average (0.019/0.21=9.05%), but decreases hospitalization rates for diabetes by 0.014 percentage point or 5.83% (0.014/0.24=5.83%). The first finding is consistent with Eliason and Storrie (2009) that job loss significantly increases the risk of alcohol-related hospitalization, based on Swedish evidence. Even Ruhm (2000) finds a positive relationship between state unemployment rates and alcohol use.[11] The second result is consistent with Ruhm's (2003) finding that chronic ailments, such as diabetes mellitus, increase during economic expansions.

We do not find any significant evidence that recessions affect the risk of hospitalization due to severe stress-related diseases, i.e., AMI and stroke; Eliason and Storrie (2009) also find no evidence that job loss increases the risk of severe cardiovascular diseases such as myocardial infarction or stroke in Sweden; and Browning et al. (2006) find that job displacement in Denmark does not cause hospitalization for heart disease.

However, the remaining concern is related to the possibility that changes in hospitalization rates during recessions might be due to losses in insurance coverage. Therefore, we re-estimate Equation (1) with additional control for a community's insurance penetration rate. The 2008-2012 American Community Survey (ACS) 5-year data release is the first 5-year data release to include health insurance coverage at the community level. However, we want to see whether the inpatient admission rates are affected by time-varying insurance coverage rates at community level. Thus, we use yearly insurance coverage rates among inpatients at zip code level to predict the rates at community level. [12]

[11] Since this finding is contrary to his previous research, which documents a procyclical variation in state alcohol sales and motor vehicle fatalities, Ruhm explains that recreational drinking could rise during downturns, whereas problem alcohol use declines.

[12] First, we examine the relationship between 4-year average inpatient insurance rates at zip code level, 2008-2011(2011 is the latest year we have in the PHC4), represented by X, and 5-year average health insurance coverage

The results, shown in Panel B of Table 4.2, indicate that the estimates from Panel A are insensitive to the inclusion of health insurance coverage rates at community level.

When communities are categorized by poverty in Panel A of Table 4.3, we find that increases in the unemployment rate cause the risk of hospitalization for stroke to decline among high-poverty communities compared to relatively rich communities. When communities are categorized by population density in Panel B, we find that in more densely populated communities, recessions significantly decrease the risk of hospitalization for AMI relative to those that are less densely populated. If communities are categorized by both poverty and population density, then increases in unemployment rate cause the hospitalization rates for AMI to decrease among high-poverty and high-density communities, and cause hospitalization rates for stroke to decrease among high-poverty and low-density communities, compared to communities that are more affluent and less densely populated.

However, the finding for poor or high-density communities must be interpreted carefully. For example, it is possible that there are fewer inpatient records for poor people who live in the poor community, because they have less access to health care due to the lack of health insurance and so do not use hospitals, not because they are healthier than people who live in richer communities. Thus, we test whether the uninsured rates decrease during recessions for: all communities, high-poverty, and high-population-density communities. As shown in Table 4.4, the percentage of the population that is uninsured does not fluctuate significantly during recessions for all types of communities. These results provide indirect evidence that decreases in hospitalization for poorer people are not because they lose insurance coverage, but possibly because of their improved health status during recessions.

Finally, the results in Table 4.5 suggest that there are no lagged or persistent effects of recessions on health. Even a deteriorating economy lasting for several years may not increase the likelihood of people's harmful behavior, such as heavy drinking. These patterns suggest that the incidence of alcohol-related diagnoses at community level may be more affected by contemporaneous recession conditions, which immediately increase job demands and insecurity and thus

rates at community level, 2008-2012, from ACS, represented by Y. The linear regression is estimated as: Y=-66.915+1.6*X. Second, we use this equation to predict the expected yearly insurance coverage rates at community level, based on inpatient insurance rates each year from the PHC4.

exposure to stressful work conditions, than accumulated exposure to medium-term economic slump.

Robustness Check

It is possible that people may not work in the county which the zip code of their residences falls into, but work in other neighboring counties. We therefore replace the county unemployment rate with unemployment rate in the labor market area, as an alternative indicator of recession, where labor market areas in Pennsylvania are defined as areas which "individuals can reside and find employment within a reasonable distance or can readily change employment without changing their place of residence."[13] In 2011, there were 49 labor market areas in Pennsylvania, which are divided into three types — metropolitan, micropolitan, and small labor market areas. The number of counties included in any individual labor market area ranges between one and seven in Pennsylvania.[14] The unemployment rate in each labor market area is calculated by averaging unemployment rates of all counties included in this labor market area. The empirical results in Table 4.6 indicate stronger effects of recession than our main results, except for the effects on the risk of diabetes-related hospitalization which become insignificant for all communities.

4.6 Conclusions

We explore the relationship between recessions and hospitalization for stress-related diseases at the community level in Pennsylvania. The most important finding of this paper is that alcohol-related conditions exhibit a countercyclical variation for all communities, even after controlling for insurance penetration at the community level. That is, economic stress, anxiety, and depression during recessions can lead to an increase in negative coping behaviors such as alcohol consumption or abuse for both individuals who lose their jobs and those who are still in the labor force.

Furthermore, recessions significantly decrease the hospitalization rate for chronic medical conditions, such as diabetes. The indication for this finding should be addressed carefully. First, the

[13]Labor Market Areas, 2013, U.S. Department of Labor Bureau of Labor Statistics, March 2013. http://www.bls.gov/lau/lmadir.pdf

[14]Every 10 years, the Nation's system of labor market areas is reevaluated and redefined, using the latest decennial census information on the population and on commuting patterns. Resource: Labor Market Areas, 2013.

incidence of diabetes may decline because people have more time to do exercise, prepare healthy meals, and improve diet. Second, the incidence of diabetes might not change, but it is possible that such chronic conditions can be managed with timely and effectively treatment in an outpatient setting, and thereby preventing hospitalization (Bindman et al., 1995). However, economic downturns may decrease patients' income, and therefore reduce or postpone their utilization of inpatient care. Thus, even though we find significant decreases in diabetes related hospitalization, it may not necessarily mean that people can better control their chronic disease or become healthier during recessions.

However, we do not find any of these health effects of economic downturns persist or accumulate over time, indicating a larger short-run than medium-term recession impacts.

Finally, the community's socio-demographic factors have important impacts on the effects of recession on overall health status. To be specific, unemployment rates are negatively correlated with hospitalization rates for AMI and stroke patients living in the high-density and high-poverty communities, respectively. And we preliminarily exclude the possibility that the lower rates are due to the potential access barriers to hospital care. Thus, our results do not provide evidence that "the worst came to the worst".

Instead, people living in poor communities might have better health than those living in more affluent communities, because wealthier people may feel more stress during recessions, i.e., they may experience substantial negative financial changes, but still have to pay high-cost mortgages. People who live in poor communities may have more experience with unemployment even during economic expansions, so they are more familiar with losing a job and searching for a new one. In addition, some fast food restaurants increase their revenues during recessions, which probably result in an increase in employment for poor people. Therefore, the benefits from recessions may overcome the small marginal bad effects of recession on their health.

In the future, we wish to study whether there is a stronger relationship between the incidence of stress-related diseases in a community and economic conditions for the prime working-age population compared to the entire population. In addition, we want to further explore the mechanism through which the risks of severe stress-related diseases decrease among poor or high-density communities during the recession.

4.7 Tables and Figures

Table 4.1: Variables Used in Analysis of Community Level Data

Variable	Mean	Standard Deviation
Percentage of Hospitalization (%)		
Myocardial Infarction	0.42	2.85
Stroke	0.57	4.09
Alcohol-related Diagnoses	0.21	0.89
Diabetes	0.24	2.25
Obesity	0.06	0.31
Community Characteristics		
Median Household Income ($)	51463.71	19571.54
% of Population Below Poverty Level	12.50	10.19
% of Population High School Graduate or Higher (25 years and over)	87.02	8.28
% of Population Who Have Health Insurance	89.29	7.80
Population Density (=Population/Land Area (square miles))	1309.79	3058.70
Unemployment Rate at County Level (%)	6.04	1.74

Notes: Data on hospitalization rates for stress-related diseases, at the zip code level, come from the PHC4, data on community characteristics, measured at the ZCTA level, come from U.S. Census Bureau, and data on average annual unemployment rates at county level are from the Bureau of Labor Statistics. Since in most instances, the ZCTA code is the same as the ZIP code for an area, we can link patient level data at zip code level with community characteristics at the ZCTA level.

Table 4.2: Effects of Recessions on Health Outcomes at Community Level, 2000-2011

	(1) AMI	(2) Stroke	(3) Alcohol	(4) Diabetes	(5) Obesity
Panel A	**Basic Model**				
UNEMPLOYMENT	-0.011	-0.001	0.019***	-0.014***	-0.000
	[0.008]	[0.010]	[0.007]	[0.005]	[0.003]
Observations	19,847	19,847	19,847	19,847	19,847
Adj. R-squared	0.619	0.892	0.404	0.726	0.165
Panel B	**Control for Health Insurance Coverage at Community Level**				
UNEMPLOYMENT	-0.010	-0.001	0.019***	-0.014**	-0.000
	[0.008]	[0.010]	[0.007]	[0.005]	[0.003]
Insurance Coverage	-0.004	0.003**	-0.001	-0.003	0.000
	[0.004]	[0.002]	[0.002]	[0.003]	[0.001]
Observations	19,847	19,847	19,847	19,847	19,847
Adj. R-squared	0.619	0.892	0.404	0.726	0.164

Notes: Panel A is estimated based on Equation (4.1). The dependent variables are hospitalization rates for five stress-related diseases at community level, and independent variables are county unemployment rates and hospital characteristics. Zip code and year fixed effects are also included in the regression models. Panel B is estimated based on Equation (4.1), additionally controlling for insurance penetration rates at community level. Robust standard errors in brackets are clustered at the county level for all regressions. Significance levels are indicated by: *** for the 1% level, ** for the 5% level, * for the 10% level (two-tail test).

Table 4.3: Effects of Recessions on Health Outcomes, Controlling for Community Characteristics

	(1) AMI	(2) Stroke	(3) Alcohol	(4) Diabetes	(5) Obesity
Panel A	**Categorize Community by Poverty**				
UNEMPLOYMENT	-0.012	0.005	0.019***	-0.012	0.001
	[0.009]	[0.009]	[0.007]	[0.008]	[0.003]
UNEMP*High Poverty	0.002	-0.011*	-0.001	-0.004	-0.002
	[0.003]	[0.006]	[0.003]	[0.008]	[0.002]
Observations	19,847	19,847	19,847	19,847	19,847
Adj. R-squared	0.619	0.892	0.404	0.726	0.164
Panel B	**Categorize Community by Population Density**				
UNEMPLOYMENT	-0.006	0.002	0.018**	-0.012**	-0.000
	[0.008]	[0.009]	[0.007]	[0.006]	[0.003]
UNEMP*High Density	-0.014**	-0.011	0.003	-0.007	-0.000
	[0.005]	[0.009]	[0.004]	[0.006]	[0.001]
Observations	19,847	19,847	19,847	19,847	19,847
Adj. R-squared	0.619	0.892	0.404	0.726	0.164
Panel C	**Categorize Community by Poverty and Population Density**				
UNEMPLOYMENT	-0.010	0.017*	0.020**	-0.007	0.002
	[0.009]	[0.009]	[0.008]	[0.012]	[0.003]
UNEMP*HigPov*HigDen	-0.013*	-0.022	0.003	-0.011	-0.002
	[0.007]	[0.014]	[0.005]	[0.015]	[0.003]
UNEMP*HigPov*LowDen	0.007	-0.028**	-0.004	-0.009	-0.004
	[0.006]	[0.014]	[0.005]	[0.015]	[0.003]
UNEMP*LowPov*HigDen	-0.008	-0.028	-0.000	-0.012	-0.003
	[0.006]	[0.017]	[0.004]	[0.013]	[0.002]
Observations	19,847	19,847	19,847	19,847	19,847
Adj. R-squared	0.619	0.892	0.404	0.726	0.164

Notes:Panel A, B, and C are estimated based on Equation (4.2). The dependent variables are hospitalization rates for five stress-related diseases at community level, and independent variables are county unemployment rates and hospital characteristics. Zip code and year fixed effects are also included in the regression models. Robust standard errors in brackets are clustered at the county level for all regressions. Significance levels are indicated by: *** for the 1% level, ** for the 5% level, * for the 10% level (two-tail test).

Table 4.4: Effects of Recessions on Uninsured Rates at Community Level

	(1) All Communities	(2) High-poverty Communities	(3) High-population-density Communities
UNEMPLOYMENT	-0.141	-0.272	0.030
	[0.142]	[0.179]	[0.200]
Observations	19,847	9,988	9,923
Adj. R-squared	0.426	0.463	0.292

Notes: The dependent variables are uninsured rates at community level, and independent variables are county unemployment rates. Column (1), (2), and (3) represent three samples: all communities, high-poverty communities, and high-population-density communities, respectively. Zip code and year fixed effects are also included in the regression models. Robust standard errors in brackets are clustered at the county level for all regressions. Significance levels are indicated by: *** for the 1% level, ** for the 5% level, * for the 10% level (two-tail test).

Table 4.5: The Persistence of Recession Effects on Health Outcomes

	(1) AMI	(2) Stroke	(3) Alcohol	(4) Diabetes	(5) Obesity
UNEMPLOYMENT	-0.018*	0.008	0.024**	-0.016**	-0.000
	[0.009]	[0.013]	[0.011]	[0.007]	[0.002]
UNEMP_Lag 1 Year	0.004	-0.019	-0.001	-0.007	0.001
	[0.009]	[0.027]	[0.010]	[0.007]	[0.004]
UNEMP_Lag 2 Years	0.019	0.011	-0.017	0.022	-0.002
	[0.020]	[0.012]	[0.011]	[0.018]	[0.004]
Observations	19,846	19,846	19,846	19,846	19,846
Adj. R-squared	0.619	0.892	0.404	0.726	0.164

The results are estimated based on Equation (4.3). The dependent variables are hospitalization rates for five stress-related diseases at community level, and independent variables are county unemployment rates in current year, last year, and the year before last year. Zip code and year fixed effects are also included in the regression models. Robust standard errors in brackets are clustered at the county level for all regressions. Significance levels are indicated by: *** for the 1% level, ** for the 5% level, * for the 10% level (two-tail test).

Table 4.6: Robustness Check: Unemployment Rate at Labor Market Area Level

	(1) AMI	(2) Stroke	(3) Alcohol	(4) Diabetes	(5) Obesity
Panel A	**Basic Model**				
UNEMPLOYMENT	-0.009	0.007	0.021***	-0.007	-0.000
	[0.008]	[0.012]	[0.007]	[0.011]	[0.004]
Observations	19,847	19,847	19,847	19,847	19,847
Adj. R-squared	0.619	0.892	0.404	0.726	0.165
Panel B	**Categorize Community by Poverty**				
UNEMPLOYMENT	-0.009	0.013	0.021***	-0.005	0.001
	[0.008]	[0.011]	[0.007]	[0.015]	[0.004]
UNEMP*High Poverty	0.001	-0.013*	-0.000	-0.005	-0.002
	[0.003]	[0.007]	[0.003]	[0.009]	[0.002]
Observations	19,847	19,847	19,847	19,847	19,847
Adj. R-squared	0.619	0.892	0.404	0.726	0.164
Panel C	**Categorize Community by Population Density**				
UNEMPLOYMENT	-0.005	0.011	0.020***	-0.005	0.000
	[0.008]	[0.011]	[0.007]	[0.013]	[0.004]
UNEMP*High Density	-0.014***	-0.012	0.004	-0.007	-0.000
	[0.005]	[0.010]	[0.004]	[0.007]	[0.001]
Observations	19,847	19,847	19,847	19,847	19,847
Adj. R-squared	0.619	0.892	0.404	0.726	0.164
Panel D	**Categorize Community by Poverty and Population Density**				
UNEMPLOYMENT	-0.007	0.027***	0.022***	0.001	0.003
	[0.008]	[0.010]	[0.007]	[0.021]	[0.005]
UNEMP*HigPov*HigDen	-0.015**	-0.026	0.004	-0.013	-0.003
	[0.006]	[0.016]	[0.005]	[0.016]	[0.003]
UNEMP*HigPov*LowDen	0.004	-0.032**	-0.004	-0.012	-0.005*
	[0.006]	[0.015]	[0.005]	[0.017]	[0.003]
UNEMP*LowPov*HigDen	-0.009	-0.031*	-0.000	-0.013	-0.004
	[0.006]	[0.017]	[0.004]	[0.014]	[0.002]
Observations	19,847	19,847	19,847	19,847	19,847
Adj. R-squared	0.619	0.892	0.404	0.726	0.164

Notes: The unemployment rates are measured at Labor Market Area level. Robust standard errors in brackets are clustered at the county level for all regressions. Significance levels are indicated by: *** for the 1% level, ** for the 5% level, * for the 10% level (two-tail test).

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Curriculum Vitae

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Education

September 2014	Ph.D. in Business and Economics, Lehigh University Dessertation: *Three Essays on Health and Labor Markets* Advisor: Mary E. Deily Dissertation Committee: Thomas J. Hyclak (Interim Dean), Shin-Yi Chou, Douglas Mahony
May 2010	M.S., Economics, Lehigh University
May 2008	B.S., Economics, Northwest University, China

Research Fields

Primary Fields: Health Economics, Labor Economics

Secondary Fields: Industrial Organization, Applied Microeconometrics

Research Interests: Public Policy and Social Welfare in Healthcare and Labor Markets

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Research Projects

Research Papers:

1. **Shanshan Liu**, Mary E. Deily and Shin-Yi Chou, "Hospital Mergers and Quality of Care: Evidence from Heart Attack Patients and Pregnancy Outcomes in Pennsylvania", submitted to *Review of Industrial Organization*, March 2014.
2. **Shanshan Liu**, and Thomas J. Hyclak, "Impact of the Minimum Wage on Youth Labor Markets", resubmit to *Labour: Review of Labour Economics and Industrial Relations*, August 2014.
3. **Shanshan Liu**, "Recessions and Stress-related Health Outcomes", May 2014.

Working Papers:

1. **Shanshan Liu**, "Concentration and Persistence of Health Care Expenditures, 2004-2006", December 2010.
2. **Shanshan Liu**, "The Game Among the Government, the Bank and the Borrower, under Obama's Anti-Foreclosure Policy", May 2009.

Scholarships & Assistantships

Spring 2014, Fall 2013, Summer 2011 & 2009	Research Assistantship, Lehigh University
Fall 2010 - Spring 2013	Teaching Assistantship, Lehigh University
Spring 2010	Graduate Assistantship, Lehigh University
2004 - 2006	Excellent Student Scholarship, Northwest University

Professional Presentations

1. American Society of Health Economists Fifth Biennial Conference, "Hospital Mergers and Quality of Care: Evidence from Heart Attack Patients and Pregnancy Outcomes in Pennsylvania", Los Angeles, CA, June 2014.
2. Eastern Economic Association Annual Meeting, "Recessions and Stress-related Health Outcomes", Boston, MA, March 2014.
3. Department Seminar, "Hospital Mergers and Quality of Care: Evidence from Heart Attack Patients and Pregnancy Outcomes in Pennsylvania", Department of Economics, Lehigh University, Bethlehem, PA, October 2013.
4. Chinese Economists Society Annual Conference, "Hospital Mergers and Quality of Care: Evidence from Heart Attack Patients and Pregnancy Outcomes in Pennsylvania", Chengdu, Sichuan, China, June 2013.
5. Eastern Economic Association Annual Meeting, "Impact of the Minimum Wage on Youth Labor Markets", New York, NY, May 2013.
6. Eastern Economic Association Annual Meeting, "Hospital Mergers and Quality of Care: Evidence from Heart Attack Patients and Pregnancy Outcomes in Pennsylvania", New York, NY, May 2013.
7. The Exhibition of Student Research and Scholarship, Academic Symposium, "Impact of the Minimum Wage on Youth Labor Markets", Lehigh University, Bethlehem, PA, April 2013.
8. Department Seminar, "Impact of the Minimum Wage on Youth Labor Markets", Department of Economics, Lehigh University, Bethlehem, PA, October 2012.

Teaching Experience

Instructor, Lehigh University

Principles of Economics: Summer 2013, Evaluations: 4.7/5.0 with Department Average 4.1

Teaching Assistant, Lehigh University

Principles of Economics: Fall 2010, Spring 2011, Fall 2011

Money, Banking, and Financial Markets: Spring 2012, Fall 2012, Spring 2013

Nominated for the Teaching Assistant of the Year Award, Lehigh University, Spring 2013

Graduate Assistant, Lehigh University

Intermediate Microeconomics, Spring 2010

Professional Affiliations

American Economic Association, American Society of Health Economists,

Eastern Economic Association, Chinese Economists Society

Skills and Other Information

Computer Skills:

Stata, SAS (earned Certification of Base Programmer for SAS 9)

LaTeX, Maple, SPSS, and Eviews

Passed CFA Level I Exam, June 2010

Languages:

English (fluent) and Chinese (native)

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