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Abstract

We exploit plausibly exogenous regulatory changes in the mortgage lending market to estimate causal effects of the financial boom and bust on personal income in the health sector. We find that counties that were exogenously more exposed to the crisis because of the regulatory reforms experienced a greater rise in the size of the health sector over the course of the boom and the bust relative to control counties, with the differential persisting through the recovery. We provide suggestive evidence that increased mortality during the bust and greater capital investment during the boom contributed to this persistence of health spending.

Key words: health spending, Great Recession, anti-predatory lending

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1 Introduction

It is a truism that U.S. health care spending is growing much faster than the rest of the economy. Between 1960 and 2009, national health care spending rose from 5% to 17.3% of U.S. GDP. Rising health spending as a share of the economy creates concerns that fewer resources remain for other types of consumption, and, since a large fraction of health spending is done by the government, that the share of distortionary taxation in the economy will need to rise. However, since 2009, the secular growth in health spending paused, with the health care share of GDP growing by only 0.2 percentage points between 2009 and 2014, only one-sixth the pace of the previous fifty years. Had the U.S. health care spending share grown at its historical average, the U.S. would have spent $175 billion more on health care in 2014 than it actually did.

The coincidence of the slowdown in health care spending growth with the financial crisis of 2008 has suggested the hypothesis that the slowdown was due to the crisis. In particular, if the spending slowdown can be explained by the decline in economic activity during the Great Recession having a particularly large impact on health spending, then we should expect health care spending to resume its pre-crisis growth path once the recovery is complete, all else the same. If, on the other hand, the spending slowdown can be explained by some structural transformation within the healthcare sector (possibly sparked by the Great Recession), we may be more sanguine about the future rate of health care spending growth (Kaiser Family Foundation, 2013). Roehrig (2012, 2013) argues that health spending depends predictably on some macroeconomic aggregates, with their interrelationship implying a steady-state health care share of the economy of about one-third. Garthwaite, Dranove and Ody (2014) argue that health spending growth fell the most in areas that have experienced the largest contractions in employment during the period 2007-2011, and show that this pattern cannot be explained by preexisting relationships between employment and health spending. Cutler and Sahni (2013) claim that the Great Recession explains 37 percent of the decline in health care spending growth in the U.S., with most of the remainder explained by structural changes in health care. On the other hand Chandra, Holmes and Skinner (2013) argue that health care spending growth changed little during the Great Recession relative to the period just before or just after. The challenge in distinguishing these hypotheses lies in the fact that the financial crisis was an endogenous event, whose differential intensity across the U.S. may be related to potential paths of health spending, which makes it difficult to use cross-sectional variation in the intensity of the crisis to identify its impact on health spending.

In this paper, we use a set of plausibly exogenous credit reforms from the financial economics literature (DiMaggio and Kermani 2015) to causally identify the impact of the credit boom and bust cycle that culminated with the Great Recession upon health spending. The reform that we consider is the preemption of state anti-predatory lending laws (APLs) by the federal government agency regulating national banks.

Between 1999 and 2004, many states issued anti-predatory lending legislation, which intended to lower the risk of foreclosure on mortgage loans by requiring verification of borrower income, as well as by limiting fees, rates and penalties associated with the loan. A number of papers document that anti-predatory legislation was effective in limiting high-risk loans (Ho and Pennington-Cross 2008, Ding et al. 2012, Agarwal et al. 2014). However, in January 2004, the OCC adopted sweeping regulations that preempted the application of the anti-predatory lending laws to the banks that it regulated. DiMaggio and Kermani (2015) show that as a result of this reform, counties in states that had APL legislation passed and that had a high fraction of lending activity from OCC-regulated banks (hereafter, treated counties) experienced a decline in lending restrictions relative to other counties, and thus had a relatively more intense boom and bust cycle over the mid- and late 2000s than other counties (hereafter, control counties).

Armed with this regulatory variation, we can study the effects of the financial crisis on health spending. We find that aggregate personal income in the health sector (hereafter health personal income for short, which we use as a proxy for county-level health spending) in treated counties increased during the boom period relative to health personal income in control counties. Moreover, we find that health personal income in treated counties continued to increase differentially through the Great Recession up to 2010, after which the differential stagnated and may have fallen. This finding stands in stark contrast with the cyclical behavior of personal income in non-tradable industries, and suggests that the immediate effects of the financial crisis on health spending cannot explain its decline. Health personal income slowed down in spite of the crisis, not because of it.

One channel through which health spending may have remained elevated during the Great Recession may have been a deterioration of health in the treated counties. While studies of previous business cycles have found health indicators to be countercyclical (Ruhm 2000), recent work (Currie and Tekin 2015) have shown that areas experiencing higher unemployment and greater foreclosures or house price declines in the Great Recession also experienced increased ill health. Using our identification strategy, we find that mortality rose substantially (though not statistically significantly) in treated counties during the boom period, and rose further during and after the bust. While we do not claim that worsened health explains the rise in health spending in counties exogenously affected by the crisis, worse health during the Great Recession may be one of the contributing factors.

Looking in more detail at the hospital industry, we also find suggestive evidence that different types of spending within the health sector reacted differently to the credit boom and bust. Non-payroll spending, net assets and the presence of diagnostic technologies rose shortly after APL preemption in the mid-2000s above their 2003 levels and stayed elevated through the recession and recovery in treated relative to control counties. On the other hand, measures of operating spending – payroll spending, admissions, employment

and numbers of beds – did not increase during the boom, but only rose during the bust. This pattern further corroborates the notion that the health care industry had to expand to react to worsening health in areas most affected by the crisis, and also suggests that capital investments made during the boom may have made the increase in health spending more persistent by increasing future utilization.

This paper is most closely related to DiMaggio and Kermani (2015), who use the variation in APL preemption and national bank prevalence to identify the effects of marginal expansions of credit activity during a financial bubble. It is also related to the voluminous literature on understanding the role of credit expansion in the financial crisis of 2008. Its contribution is most directly in the literature on understanding the evolution of U.S. health spending growth, for example Acemoglu, Finkelstein and Notowidigdo (2013), who compute a low (typically less than unit) elasticity between health spending and income by exploiting the effects of the changing price of oil on regional economies in the U.S. South. While we do not estimate an elasticity of health spending with respect to income, our finding that health personal income grew faster (or shrank slower) in counties that experienced a stronger bust during the Great Recession than in counties that experienced a weaker bust is consistent with their finding of a relatively low income elasticity for health spending.

The rest of the paper is organized as follows: Section 2 describes the data. Section 3 provides a short summary of the empirical strategy from DiMaggio and Kermani (2015). Section 4 provides the baseline results for health personal income. Section 5 explores the effects of the financial boom and bust on health. Section 6 investigates possible mechanisms of the effect on health personal income by looking at hospital data from the AHA Annual Survey. Section 7 concludes.

2 Data

2.1 Independent Variables

We conduct our analysis at the county-year level. Our main independent variables are indicators for the presence of an APL in a state in 2004, as well as the fraction of loans made in any county in 2003 that came from national (OCC-regulated) banks. These variables come directly from DiMaggio and Kermani (2015): we use the dataset of Ding et al. (2012) for data on anti-predatory legislation and HMDA data on the fraction of loans made by OCC-regulated entities.

Along with the main independent variables, we make use of a number of controls. We use data on the fraction of borrowers with a credit score that is lower than 620 from Equifax, and data on the Saiz elasticity of housing supply from Saiz (2010). We obtain detailed county demographic information (breakdowns by

race, gender Hispanic origin and 10-year age bins) from the CDC.

2.2 Dependent Variables

National health spending is reported at the national and the state level, but unfortunately, not at the county level. Instead, we measure health spending using the regional accounts of the Bureau of Economic Analysis, which are county-level. Our main dependent variable is the log of personal income accruing to health care and social assistance (hereafter, health personal income). We can also look individually at personal income accruing to ambulatory health care, hospitals, long-term care facilities and social assistance.[1]. We additionally use BEA data on the log of personal income coming from the retail sector, the accommodation and restaurant sector and the construction sector. Following DiMaggio and Kermani (2015), we also use new mortgage loan amount data at the county-year level from the HMDA dataset, as well as house price data from CoreLogic. We obtain mortality data at the county-year level from the CDC.

For more detailed analysis of which components of spending were affected by the financial boom and bust we turn to the American Hospital Association (AHA) Annual Survey. We focus on hospital-level data on payroll and non-payroll spending along with data on net assets over time, although we discuss other data series less formally and present results for them on request.

Figure II documents the dynamics of health personal income across U.S. counties by their tercile of lending growth between 2002 and 2006. We see that in all three lending terciles of counties, health personal income has grown rapidly over the period 2001-2013, increasing by over 0.6 log points in total. Until 2004, the growth of health personal income is nearly identical for the three terciles. However, starting in 2005 (as the lending boom is beginning), health personal income begins to grow more rapidly in high-lending counties than in medium-lending or low-lending ones. In 2009 (towards the end of the financial crisis), the health personal income growth rate decreases by a similar amount for all three terciles, although it falls the most for the low-lending tercile. These trends do not appear to be consistent with the idea that it was the financial crisis that decreased health care personal income growth through an income effect, because if that was the case, the greatest decrease would have taken place for the high-lending counties, not for the low-lending counties. Instead, high-lending counties appear to experience excess health personal income growth during the boom period and not compensate with lower growth during the bust period.

[1] Our choice of main dependent variable would be superior if we could purge it of the personal income accruing to social assistance. However, the BEA regional accounts are missing for many counties because of small sample sizes triggering confidentiality restrictions, which means that the variables for personal income accruing to various subdivisions of health care and social assistance are often missing. Therefore, we consider that using the headline total for the entire category is the least bad solution, especially given that social assistance is only 10% of the total. We discuss this issue further in our results section.

3 Identification Strategy

Our identification strategy is the same as in DiMaggio and Kermani (2015): we exploit county-level variation in the fraction of loans coming from national (OCC-regulated) banks in 2003, as well as whether these banks were or were not subject to the 2004 preemption of state anti-predatory lending laws by the OCC. The advantage of this strategy is that the prevalence of OCC-regulated banks within a county varies very slowly over time, appears to have been set long before the OCC decided to preempt anti-predatory lending laws, and did not change substantially following this preemption. Moreover, the passage of anti-predatory lending laws in the early 2000s seems to have been independent of states' and counties' prevalence of OCC-regulated banks (which is not surprising if the preemption of these APLs by the OCC was relatively unexpected). It also is intuitive that the OCC did not decide to preempt anti-predatory lending laws in order to affect the lending market of any particular county. DiMaggio and Kermani (2015) document that while the fraction of loans generated by OCC-regulated banks is correlated with various relevant county characteristics (such as elasticity of housing supply, fraction of borrowers who are subprime and securitization activity), these correlations do not differ significantly in states that passed anti-predatory lending laws versus states than did not. Hence, using a triple-difference analysis should sweep out any possible endogeneities of the prevalence of OCC-regulated banks by comparing counties with similar OCC prevalence such that one county had its anti-predatory lending law preempted and the other one never passed such a law.

Figure I presents a map of the U.S., with counties whose states had an anti-predatory lending law in 2004 colored in blue, other counties colored in green, and darker shades representing greater OCC penetration. We see that no region seems to have a predominance of states with an APL in 2004, or of counties with a high fraction of OCC loans in 2003. There are APL states with high OCC penetration (like Minnesota), APL states with low OCC penetration (like Connecticuit), non-APL states with high OCC penetratin (like Maine) and non-APL states with low OCC penetration (like Massachusetts). The wide geographical dispersion of anti-predatory lending law presence and of OCC loan share provides further confidence in our identification strategy.

The regressions that we run are essentially the same as in DiMaggio and Kermani (2015), but with different dependent variables. First, we regress our dependent variables of interest on county and year fixed effects, as well as the triple difference of an indicator for the presence of an APL in county i and year t ($APL_{i,t}$), a variable measuring the fraction of loans coming from OCC-regulated banks in 2003 (OCC_i) and an indicator for the year being 2004 or greater ($Post_t$).

$$y_{i,t} = \alpha_i + \lambda_t + \beta_1 OCC_i \times Post_t + \beta_2 APL_{i,t} \times Post_t + \beta_3 APL_{i,t} \times OCC_i + \gamma APL_{i,t} \times OCC_i \times Post_t \quad (1)$$

We are interested in the coefficient on the triple interaction (γ) as a measure of the effect of APL preemption on the dependent variable by the prevalence of OCC-regulated banks.

We are also interested in a year-by-year version of specification (1), in which the APL and OCC variables can have heterogenous effects in different years. This specification lets us observe the precise timing of changes in the correlations between the dependent variable and the financial reforms that we hypothesize affected the intensity of the crisis.

$$y_{i,t} = \alpha_i + \lambda_t + \mu_t OCC_i + \nu_t APL_i^{2004} + \gamma_t APL_i^{2004} \times OCC_i \quad (2)$$

Here, we replace the indicator for the presence of an APL in county i and year t with an indicator for the presence of an APL in county i in 2004, the year of the APL preemption. The coefficients γ_t in this specification present the partial difference in the outcome variable in year t between treated counties (counties that experience APL preemption and have a high share of loans coming from OCC-regulated lenders) and control counties (all other counties). The graph of γ_t over time will show the evolution of this difference. If APL preemption was indeed exogenous and unexpected, we should expect the plot of the γ_t's to be flat before 2004, and then begin deviating in response to the change in financial regulations.

Finally, we investigate cross-sectional specifications in which we regress the growth of a dependent variable on the difference-in-difference of $APL_{i,t}$ and OCC_i.

$$\Delta y_i = \alpha + \beta_1 OCC_i + \beta_2 APL_i^{2004} + \delta APL_i^{2004} \times OCC_i \quad (3)$$

If we assume that any direct effects of APL preemption were only on lending, then we can obtain instrumental variable estimates of the impact of an increase in loan growth during the boom on other outcome variables by running the instrumental variable regression:

$$\begin{aligned} \Delta y_i &= \alpha + \gamma \Delta l_i^{01-06} + \varepsilon_i \quad (4) \\ \Delta l_i^{01-06} &= \chi + \beta_1 OCC_i + \beta_2 APL_i^{2004} + \delta APL_i^{2004} \times OCC_i + \eta_i \end{aligned}$$

Following DiMaggio and Kermani (2015), we weigh all regressions by county population in 2000.

We briefly review the findings of DiMaggio and Kermani (2015), which suggest that the APL pre-

emption increased lending, house prices and the local economy in the boom period and decreased all of them in the bust period. Panel 1 of Figure III shows the plot of the coefficients γ_t for mortgage lending, with the coefficient for 2003 is normalized to zero. It shows a clear cyclical pattern, with treated counties experiencing higher values of mortgage lending during the boom, and lower values during the recession, than control counties (similar graphs for other measures of crisis intensity are available in DiMaggio and Kermani 2015). Hence, it is plausible to believe that our independent variables (the APL indicator, the fraction of OCC-regulated lending and their interactions) are capturing regulatory shocks that differentially affected the intensity of the financial boom and bust in different areas of the U.S.

4 Basic Results on Health Personal Income and Financial Crisis Intensity

The dynamics of Figure **??** cannot be taken as causal effects of lending growth on health spending because lending growth over the 2000s is endogenous. To obtain associations between health spending and the financial crisis that might be interpreted as causal, we estimate regressions (1) and (3) in Table I in order to see the intent-to-treat effect of APL preemption on log health personal income. The first column reports the estimates from the regression of log loans on the triple difference of the presence of anti-predatory lending laws in the county's state, the county's fraction of loans coming from OCC-regulated banks in 2003 and a post-2004 dummy, all over a balanced sample of 1028 counties, which have data on log health personal income in each year between 2001 and 2013. The regression itself is run for the period 2001-2006. We see that the estimate on the triple interaction is equal to 0.634, and is statistically significant at 10%.[2] This is slightly higher than DiMaggio and Kermani's (2015) estimate of 0.449 for the same coefficient in a sample of all counties with loan data. Hence, in the sample that we are considering, we have a "first stage" – an association between APL preemption and lending activity – that is, if anything, larger than the one identified by DiMaggio and Kermani (2015). This is reassuring for the later interpretation of the association between APL preemption and log health personal income as being mediated by the impact of APL preemption on the amplitude of the financial crisis.

The second column of Table I presents our baseline estimates for the effect of the financial boom and bust on health personal income. The regression specification (1) is run over the balanced sample of 1028 counties for 2001 through 2010. We see that the interaction coefficient is a statistically significant 0.212. This means that a county in a state with a preempted APL, and with a third of its loans coming from

[2]Here, we are clustering standard errors on state, while DiMaggio and Kermani (2015) cluster on county. If we cluster on county, the significance level becomes 1%.

OCC-regulated banks, experienced approximately 7% higher health personal income after 2004 relative to before, relative to a county with the same fraction of OCC-regulated loans but no APL prior to 2004. Hence, health personal income rose overall during the boom and bust cycle for treated counties relative to control counties.

Column 2 of Table I does not provide us with information on the time pattern of the post-2004 increase in health personal income in treated counties. We can trace out the time path of the effect of the financial boom and bust on health personal income by estimating equation (2) and plotting the time-varying interaction coefficients γ_t. Panel 2 of Figure III shows the graph of these coefficients over time from 2001 to 2013, with γ_{2003}, the effect in the year preceding APL preemption, normalized to zero without loss of generality. We see that in the pre-period – for the years 2001 and 2002 – the differences between treated and control counties are small and statistically insignificantly different from zero. Starting in 2004, the year of APL preemption, the coefficients turn positive and begin increasing. By 2006, the coefficients become statistically significantly different from zero and begin to approach the 0.212 value on the pooled post-2004 interaction coefficient in column 2 of Table I. They continue to increase up to 2010 (hence, they increase both through the financial boom and through the bust), and then gradually decrease slightly to somewhat less than 0.2 by 2013. All the estimated coefficients between 2006 and 2011 are individually statistically significantly different from zero with 95% confidence. From this graph, we can conclude that 1) health personal income rose in treated relative to control counties both in the boom and during the recession, and 2) if the differential in health personal income declined at any point, it was after 2010, and hence, well after the official end of the Great Recession.

Columns 3, 4 and 5 of Table I check the robustness of the results in column 2. In column 3, we do not restrict ourselves to a balanced sample, and instead include all available counties. This means that each county may not have observations of the dependent variable in all years under study. The marginal counties that enter this unbalanced sample are generally small, and likely have substantial measurement error in their dependent variable (which is counteracted by their population also being small, and thus their regression weight being small). The estimate of the interaction coefficient falls slightly to 0.172, but remains significant at 5%. In column 4, we consider using log health personal income per capita instead of log health personal income as our dependent variable. We do this in order to account for the possibility that health spending in treated counties could have risen if their population increased during the boom, but did not decline during the bust. We find that the magnitude of our effect declines somewhat (to 0.159) but that it remains statistically significant. In column 5, we explore a range of additional counterfactuals by adding a bevy of controls, which include log county population, log county median income, the fraction of borrowers who are subprime interacted with a post-2004 dummy, the Saiz elasticity interacted with a

post-2004 dummy and the fractions of the county population in each year who fall into a detailed race by age by gender classification of demographics. We now switch back to having log health personal income as our dependent variable because we controlling for log population on the right hand-side. The sample size falls to 527 counties (largely because of the lack of availability of the Saiz elasticity for a large number of counties). The interaction coefficient slightly increases from the baseline to 0.245, and is statistically significant at 1%.

4.1 Results by Health Sector

It is important to examine whether the association between health personal income growth and the financial crisis is uniform across different types of health personal income or is primarily driven by one of these types. Columns 6, 7 and 8 of Table I replicate column 2 for each of the health-related subcategories of health and social assistance personal income. These categories are personal income from ambulatory services (physician offices as well as other outpatient settings), personal income from hospitals and personal income from long-term care (LTC) facilities. In 2010, they made up 49%, 32% and 11% of personal income from health care and social assistance, respectively (social assistance making up the remaining 8%). We see that for all the categories of health personal income, the interaction coefficients are similar in magnitude to the one in column 2, though often are not statistically significant.

Panels 4, 5 and 6 of Figure III show the time paths of the year-by-year coefficients γ_t on the interaction term in the specification (2) for the different types of health personal income. We see that while the individual year-by-year coefficients often fail to be statistically significantly different from zero, their time path is very similar to the path for the coefficients in the specification with total health personal income in Panel 2. The coefficients are close to zero in magnitude before 2004, then begin to rise, and continue rising up to 2010, well after the end of the Great Recession.

4.2 Instrumental Variable Estimates of the Effect of the Financial Boom and Bust on Health Spending

Columns 9, 10 and 11 of Table I attempt to estimate a causal effect of a greater bubble amplitude on the growth of health personal income using only cross-sectional variation. Column 9 establishes a correlation between the difference in the growth in lending between 2003 and 2005 (the boom) and 2008 and 2010 (the bust) and the interaction of having an APL preempted in 2004 and a high OCC-regulated bank loan volume in 2003. Column 10 establishes the same correlation between the growth in health personal income between 2003 and 2010 and the APL-OCC interaction. Finally, in column 11, we run an IV regression of the growth in health personal income on the difference in lending growth during the boom and the bust, the latter

instrumented by the APL-OCC interaction, as well as by the APL and OCC variables individually. (Column 9 is the first stage of this analysis, while Column 10 is its reduced form). The IV analysis suggests that a 1 log point increase in the difference in lending growth (near the 95th percentile of this variable) is associated with 3.4% higher health personal income average annual growth over the period 2003-2010. This result, however, is only statistically significant at 10%.

5 Mortality

While we observe that health personal income rose in the Great Recession in treated counties relative to control counties, our exercise so far does not explain the mechanisms through which this happened. One plausible channel that may explain part of this phenomenon is changing health. Currie and Tekin (2015) document that counties that experienced higher foreclosure rates as well as greater housing price drops also saw a greater incidence of hospital admissions for severe illnesses, suggesting that the financial crisis may have hurt health.

We estimate the effect of APL preemption by fraction of OCC regulated bank prevalence on mortality in Table II. This table replicates Table I, but using the log of the age-adjusted death rate per 100,000 as the dependent variable. We estimate specifications (1) and (3) over a balanced sample of counties with mortality data. We find that the coefficient on the triple interaction is around 0.051, which implies that a county in a state with a preempted APL, and with a third of its loans coming from OCC-regulated banks, experienced an increase in its age-adjusted number of deaths per 100,000 people of around 1.7%. If we look at the age-adjusted mortality rate only for people over 65 (which tends to be a less noisy measure of mortality, since most deaths take place after that age), the coefficient on the triple interaction rises to 0.061 (or an increase of around 2.0% in our example) and becomes statistically significant at 10%. Adding controls (column 3) slightly decreases the coefficient on the triple interaction (to 0.57) and it loses significance. Column 5 of Table II shows that repeal of an anti-predatory law in 2004 increased mortality growth by a statistically significant 0.011 log points in a county with all loans coming from national banks relative to a county with no loans coming from national banks. As in our analysis of Table I we can compute an instrumental variables estimate of the effect of the amplitude of the lending boom and bust cycle on mortality. We find (column 8) that a lending amplitude of 1 log point during the boom and bust cycle increased mortality by 1.7%, which is statistically significant at 10%.

Panel 3 of Figure III plots the year-by-year coefficients γ_t that show the differences in over-65 mortality between treated and control counties over time. Once again, the coefficient in 2003 is normalized to zero. The mortality differential is flat up to 2003, but then rises sharply (though not statistically significantly)

in 2004, and rises again (this time to a value that is marginally significantly different from zero) in 2009. Interestingly enough, mortality is neither procyclical nor countercyclical here, but rises both during the boom and during the bust. A possible explanation could be that mortality rose during the boom because it is generally countercyclical (Ruhm 2000) and may increase with liquidity shocks (Gross and Tobacman 2011), but that the Great Recession, having been borne out of a financial crisis, was so severe, that it also worsened health instead of improving it, as several previous recessions did.

6 Patterns of Changing Hospital Spending during the Boom and Bust

It is interesting to break down health spending into more refined categories in order to see exactly which components of spending differentially rose in areas of the country that were particularly exposed to the financial boom and bust. To this end, we use hospital-level data from the AHA Annual Survey. We first look at Panel 7 of Figure III, which plots year-by-year coefficients γ_t that show the differences in log payroll hospital expenditures between treated and control counties over time. We see that while the coefficients before 2003 are close to zero, so are the coefficients between 2003 and 2006, although log hospital payroll expenditures clearly (though not statistically significantly) increase in 2007 and remain elevated until the end of the sample period. Therefore, it seems that only the bust, and not the boom, affected payroll spending in hospitals. In the Appendix, I present similar graphs for other variables capturing hospital day-to-day demand, operating costs and variable expenses, such as hospital admissions, employment, number of beds and average compensation. This is consistent with worsened health during the more intense bust that these counties experienced during the Great Recession, which would also result in higher variable expenditures for hospitals in order to meet this additional demand. This is in contrast with Panel 5 in Figure III, where it is clear that the rise in log total hospital expenditures begins right after APL preemption in 2004.

In contrast, Panel 8 of Figure III presents the year-by-year coefficients for log non-payroll and benefits spending (the differential between total spending and payroll spending in the AHA data). We see that while the pre-2003 coefficients are close to zero, the coefficient for 2004 is large and statistically significantly different from zero, as is the coefficient for 2007. The post-2004 coefficients are all considerably larger than the pre-2003 coefficients, and suggest that non-payroll and benefits spending rose in the treated counties right after APL preemption and stayed elevated (with the differential possibly increasing over time). Similarly, Panel 9 of Figure III plots the coefficients for log hospital net assets. The net assets data in the AHA, unfortunately, begins only in 2003 and is not available for 2004, so no pre-period coefficients can be

estimated, making it impossible to judge concerning possible pre-trends. However, the coefficients spike in 2005 and then again in 2007. The patterns in both Panels 8 and 9 of Figure III are consistent with the treated counties engaging in the acquisition of additional hospital assets in or before those years relative to the control counties. We also confirm this pattern with AHA data on the adoption of diagnostic technologies described in Cutler and Sheiner (1997), as described in the Appendix.

While almost all of our estimates are statistically insignificantly different from zero (mirroring the lack of significance in the coefficients for log hospital personal income), the pattern indicated by the coefficients is consistent with hospitals increasing their investment spending during the boom and obtaining new technologies, while increasing their operating costs during the bust as demand for hospital services increased (with the decline in health during the recession).

7 Conclusion

In this paper, we provide evidence that, contrary to conventional wisdom, the financial crisis and the Great Recession increased health care spending rather than decreased it. To identify causal effects of the financial crisis, we exploit the 2004 preemption of state anti-predatory lending laws by the OCC, which generated heterogeneous positive shocks to risky lending in counties with differential presences of OCC-regulated banks in the mortgage market. We find that health spending (proxied by personal income in the health sector) in counties that are treated by this regulatory variation diverges from health spending in control counties soon after the 2004 preemption, and their differential rises through both the boom and the bust phase of the financial cycle. We also find suggestive evidence that mortality also increases in treated relative to control counties over the course of the financial boom and bust, which is consistent with some of this increase in health spending coming from deteriorating health. Finally, we find that the increase in spending by hospitals was heterogeneous over time, coming from investment spending during the boom and operating costs (to meet the increased demand for health services) during the recession.

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8 Tables

Table I

Effect of Boom and Bust on Health Spending											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Log Loans New Sample	Log Health Care Soc Spending (HCS)	Log HCS All Counties	Log HCS per Capita	Log HCS With Controls	Log Ambulatory Spending	Log Hospital Spending	Log Long Term Care Spending	Growth Loans Boom-Bust	Growth HCS 03-10	Growth HCS 03-10
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	IV
DiD Log Loans											.034*
											(.018)
APL*OCC*Post	.649*	.212***	.172**	.159**	.245***	.173*	.259	.256***			
	(.339)	(.081)	(.080)	(.069)	(.081)	(.091)	(.192)	(.097)			
APL*Post	-.227*	-.070***	-.061**	-.058***	-.090***	-.057	-.090	-.098***			
	(.126)	(.025)	(.026)	(.018)	(.022)	(.035)	(.069)	(.035)			
APL*OCC	-.292	-.094	-.076	-.036	-.032	-.082	.060	-.043			
	(.269)	(.079)	(.070)	(.056)	(.071)	(.070)	(.138)	(.067)			
OCC*Post	-.802***	-.063	-.074*	-.030	-.033	-.128**	-.160	-.139**			
	(.265)	(.047)	(.041)	(.042)	(.066)	(.063)	(.174)	(.061)			
APL 2004 * OCC 2003									.360**	.035**	
									(.166)	(.014)	
APL 2004									-.167***	-.013***	
									(.058)	(.004)	
OCC 2003									-.467***	-.012	
									(.134)	(.009)	
R2	.028	.007	.005	.009	.421	.006	.004	.010	.071	.015	-.021
Time Period	00-06	01-10	01-10	01-10	01-10	01-10	01-10	01-13	CX	CX	CX
County FE	YES	YES	YES	YES	YES	YES	YES	YES	NO	NO	NO
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	NO	NO	NO
Std. Errors	State	State	State	State	State	State	State	State	Robust	Robust	Robust
Number of Obs.	7196	10280	18976	10280	5270	18140	2900	13770	1028	1028	1028
Number of Clusters	51	51	51	51	45	51	47	51			

(I)

All regressions weighted by county population in 2000. Columns 1-8 present estimates from specification (1) and Columns 9-10 present estimates from specification (3), while Column 11 presents an IV specification based on specification (4). "Growth Loans Boom-Bust" is the difference in difference of loan growth between 2007 and 2009, and between 2003 and 2005. The controls in Column 4 are log county population, log county median income, the fraction of borrowers who are subprime interacted with a post-2004 dummy, the Saiz elasticity interacted with a post-2004 dummy and the fractions of the county population in each year who fall into a detailed race by age by gender classification of demographics.

Table II

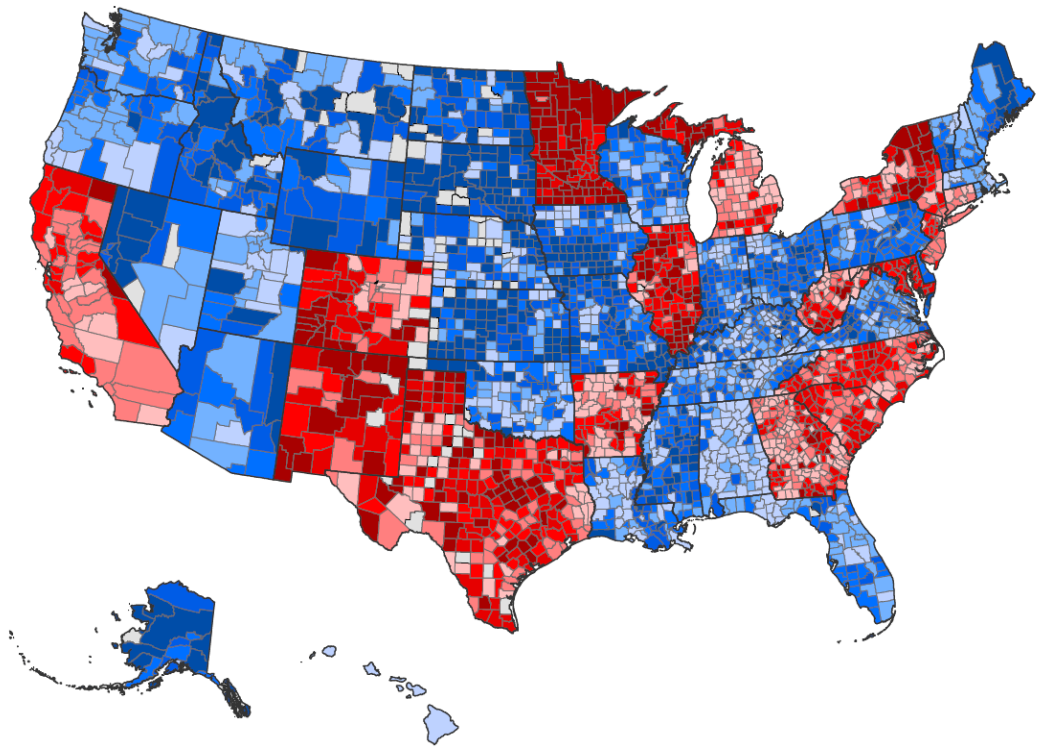
Mortality Regressions						
	(1)	(2)	(3)	(4)	(5)	(6)
	Log Mortality New Sample OLS	Log Mortality Over 65 OLS	Log Mortality Over 65 Controls OLS	Growth Loans Boom-Bust OLS	Growth Mortality Over 65 03-10 OLS	Growth Mortality Over 65 03-10 IV
DiD Log Loans						.017*
						(.010)
APL*OCC*Post	.051	.061*	.059			
	(.039)	(.034)	(.048)			
APL*Post	-.029*	-.028**	-.023			
	(.015)	(.013)	(.016)			
APL*OCC	.001	-.026	-.076*			
	(.034)	(.030)	(.045)			
OCC*Post	.013	.004	-.033			
	(.022)	(.020)	(.028)			
APL 2004 * OCC 2003				.199*	.011**	
				(.106)	(.004)	
APL 2004				-.116***	-.005***	
				(.041)	(.001)	
OCC 2003				-.292***	.004	
				(.080)	(.003)	
R2	.019	.008	.063	.048	.023	-.083
Time Period	01-10	01-10	01-10	CX	CX	CX
County FE	YES	YES	YES	NO	NO	NO
Year FE	YES	YES	YES	NO	NO	NO
Std. Errors	State	State	State	Robust	Robust	Robust
Number of Obs.	33494	33440	7677	3043	3037	3036
Number of Clusters	51	51	45			

All regressions weighted by county population in 2000. Columns 1-3 present estimates from specification (1) and Columns 4-5 present estimates from specification (3), while Column 6 presents an IV specification based on specification (4). "Growth Loans Boom-Bust" is the difference in difference of loan growth between 2007 and 2009, and between 2003 and 2005. The controls in Column 3 are the same as in Column 4 of Table **I.**

(II)

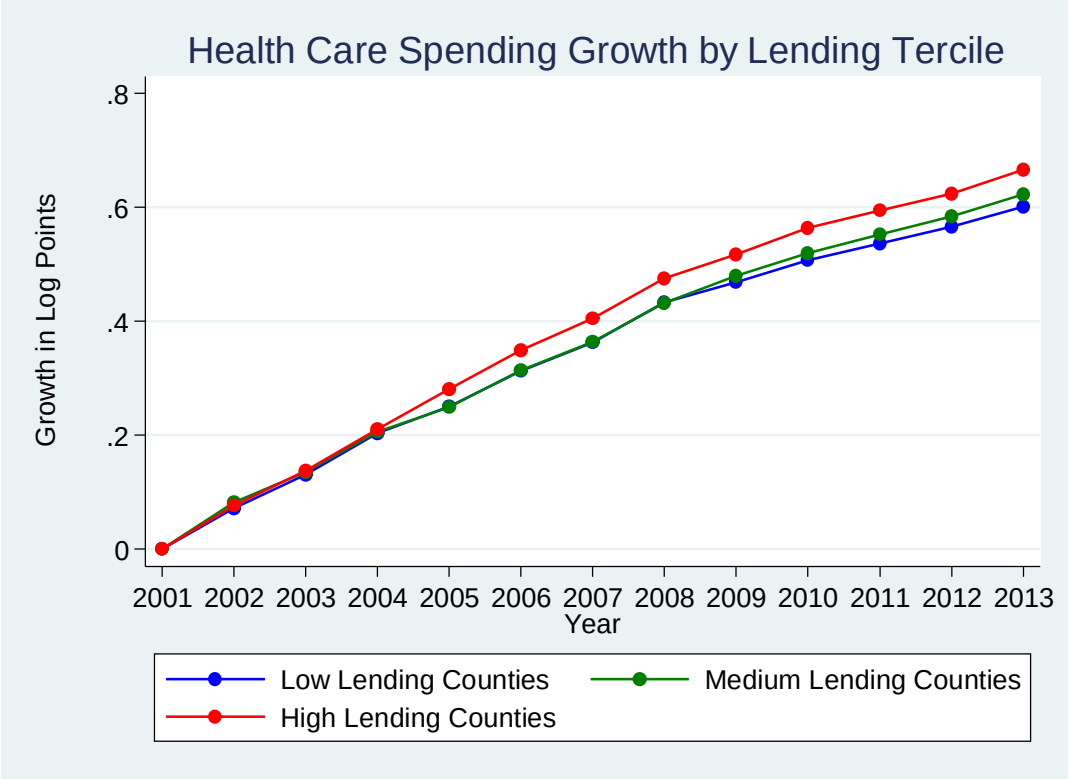
9 Figures

Figure I: U.S. Counties by APL in 2004 and OCC Loans in 2003 (I)



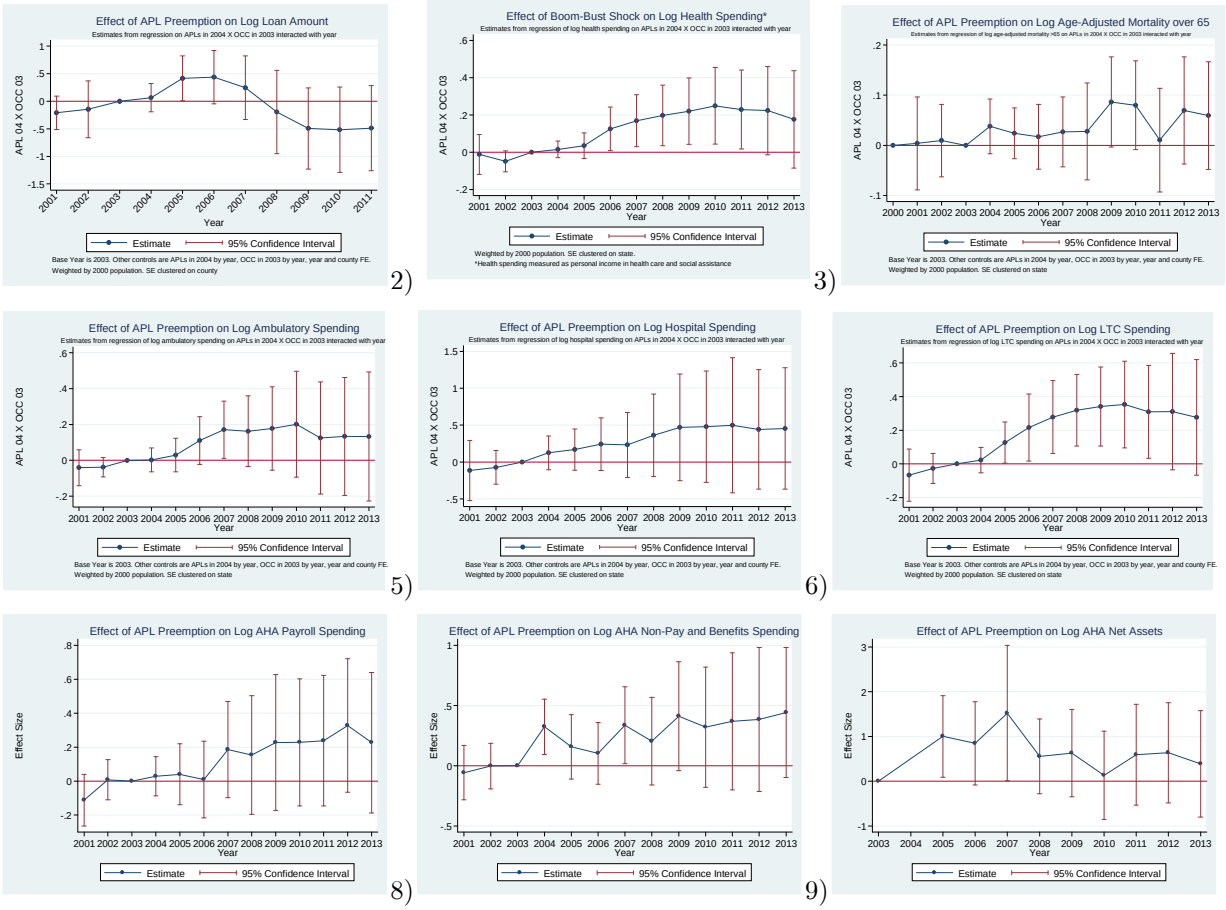
States colored red had outstanding anti-predatory lending laws (APLs) in 2004, while states colored blue did not. Counties with a darker shading (dark red or dark blue) had a higher fraction of loan volume made by OCC-regulated banks in 2003 than did counties with a lighter shading (light red or light blue).

Figure II (II)



Each group of counties corresponds to the appropriate tercile of the unweighted county distribution of lending growth between 2002 and 2006

Figure III (III)



Each above graph plots the coefficients γ_t from specification (2) with the dependent variable given in the graph title. Error bars denote 95% confidence intervals, computed by clustering the regression standard errors on state. All regressions are weighted by county population in 2000.