# Credit Markets and Racial Mortality Inequality

**Abstract**

This paper tests whether and how local banking market conditions affect mortality rates in the United States. Exploiting the staggered relaxation of branching restrictions in the 1990s across states, we find that banking deregulation decreases local mortality rates. This effect is driven by a decrease in the mortality rate of black residents, implying a decrease in the black-white mortality gap. We provide evidence for expanding the healthcare sector in black communities, access to private health insurance, and home ownership as channels through which local banking markets affect individuals' longevity and decrease mortality inequality.

***JEL Codes:*** G21; I14; I15
***Keywords:*** Banking Deregulation; Mortality; Racial Inequality

# 1 Introduction

Mortality rates in the United States vary substantially across regions (Murray et al., 2006; Finkelstein et al., 2021) and within them (Koskenvuo et al., 1986; Case and Deaton, 2015, 2022). Particularly relevant is the difference between black and white individuals (Deaton and Lubotsky, 2003; Schwandt et al., 2021). Despite a declining racial mortality gap during the last decades, black mortality rates consistently surpass white mortality rates throughout the country's history. Furthermore, the gap expanded again during the recent COVID-19 crisis (Wrigley-Field, 2020; Alsan et al., 2021).

Previous literature has identified several socio-economic factors that can explain spatial mortality disparities and inequality, including pollution, crime, behavioral risk factors, and healthcare quality (e.g., Dwyer-Lindgren et al., 2017; Deryugina and Molitor, 2020, 2021). This line of research is particularly relevant considering the substantial estimated economic gains from decreasing mortality rates.[1] Our paper aims to contribute to this literature by analyzing the importance of local banking market conditions for mortality dynamics.

There are several reasons to believe that well-developed banking markets could influence mortality rates and decrease mortality inequality. Indeed, banking development might increase access to finance for financially constrained individuals, enabling them to access goods that were inaccessible but crucial for their health, such as health insurance or housing. Additionally, it could allow previously constrained firms or new firms to access credit, leading to higher local employment and income, and expanding

[1] For example, Murphy and Topel (2006) estimate that from 1970 to 2000, gains in life expectancy in the United States added approximately $3.2 trillion per year to national wealth.

sectors particularly relevant to individuals' well-being, such as the healthcare sector.

To assess the influence of local banking market conditions on health outcomes, we consider the 1994 Interstate Banking and Branching Efficiency Act (IBBEA) and exploit in our empirical analysis the succeeding wave of banking deregulation events as positive shocks to banking market development. This law allowed states to gradually remove interstate bank branching restrictions, thereby fostering competition and efficiency in the banking markets and expanding the supply of credit (e.g., Favara and Imbs, 2015; Krishnan et al., 2015; Berger et al., 2021). Subsequently, we investigate the effects on mortality rates using detailed administrative information on the universe of death certificates.

Combining the staggered timing with the heterogeneous intensity of interstate branching deregulation across states, we find that lifting banking restrictions significantly reduces local mortality rates. We observe that the decrease in the mortality rates of black individuals drives this effect. As mortality rates of white individuals are unaffected, we document a decline in the black-white mortality gap due to banking sector development. Our findings are consistent with previous empirical evidence arguing that less regulated banking markets disproportionately benefit financially constrained individuals and minorities (Levine et al., 2008; Beck et al., 2010; Tewari, 2014).

Our analysis highlights financial development's significant role in reducing mortality rates and inequality. Indeed, the effects that we estimate are economically meaningful. Our results imply a decrease in the black mortality rates and the black-white mortality gap equal to almost 15% in fully deregulated counties concerning the average values of the pre-deregulation period. Since only some of the states deregulated fully, coun-

terfactual estimates show that a fully deregulated banking sector in the states that deregulated would have further decreased the racial mortality gap by 14% at the end of the analyzed period.

To probe the robustness of our identification strategy, we combine our difference-in-differences strategy with a border discontinuity approach that increases comparability between treatment and control groups by focusing only on counties that scatter around a state border (Huang, 2008). In addition, our findings are unchanged after considering the recent advances in the econometrics literature, arguing that difference-in-differences estimators can be severely biased in the presence of time-varying treatment effect heterogeneity (De Chaisemartin and d'Haultfoeuille, 2020; Baker et al., 2022).

After documenting the crucial role of local banking markets in decreasing racial mortality inequality in the United States, we turn our attention to identifying the underlying mechanisms behind our main results. From a conceptual standpoint, the deregulation of the banking system could impact both supply- and demand-side factors. As discussed by Cramer (2021), these factors have the potential to interact with each other, indicating that our partial equilibrium analysis may not fully capture the extent of these interactions or quantify the degree to which each mechanism contributes to reducing inequality in mortality. Nevertheless, our analysis enables us to highlight and discuss several interesting, previously unexplored mechanisms through which banking deregulation can directly affect health outcomes and mortality dynamics.

Beginning with an examination of the supply-side factors, we consider that disparities in healthcare access significantly contributed to racial health inequality in the United States (Weinick et al., 2000; Williams and Jackson, 2005). Therefore, we first

investigate whether banking deregulation affects firm dynamics in the healthcare sector. As a matter of fact, during the 1990s, US healthcare organizations were facing financial constraints in accessing external debt markets, and insurers were conducting several cost containment measures. Lower operating margins and liquidity within the hospital industry followed, leading to further deteriorating credit access (Calem and Rizzo, 1995). As a result, banking deregulation could have played a crucial role in expanding the healthcare sector.

Using information on the universe of establishments and their location, we find that banking deregulation does not affect the healthcare sector size. However, we show that this average effect masks a sizable county-level heterogeneity. When we interact deregulation events with the share of the black population in the county, we document a differential increase in new establishments that operate in the healthcare sector in black communities. Simultaneously, we document in these counties an increase in healthcare expenditures.

A possible concern is that banking deregulation could have also led to an increase in the consumption of goods that can be detrimental to the health of individuals. However, we do not find any evidence that banking deregulation increases sales for *temptation goods* (tobacco and alcohol), both on average and in black communities. The rationale for this finding is that as individuals' financial situations improve, they may be less likely to consume tempting goods, as they may have financial resources to spend on healthier options or save for the future (Banerjee and Duflo, 2007; Banerjee and Mullainathan, 2010).

Turning to the household perspective, an expansion of the healthcare sector might

also improve health insurance access for black individuals. Indeed, when healthcare services are more widely available, people may be more likely to seek out medical care, which can increase the demand for health insurance. In addition, elevated healthcare service supply can lead to increased competition among healthcare providers and, ultimately, lower medical service prices. Given the racial differences in the use of health insurance in the United States (Lillie-Blanton and Hoffman, 2005; Griffith et al., 2017), making healthcare more affordable and accessible might especially benefit black people.

Using household-level information from the Current Population Survey (CPS), we demonstrate that banking deregulation positively impacts access to private health insurance among black individuals. Simultaneously, we observe a reduction in the likelihood of dependence on government-run health insurance. The transition to private health insurance, as evidenced by our findings, may yield numerous health benefits, facilitating improved access to superior treatment and more comprehensive medical care (Sorlie et al., 1994; Bhattacharya et al., 2003).

Given the importance of local labor markets for health outcomes (e.g., Aghion et al., 2016; Pierce and Schott, 2020; Adda and Fawaz, 2020), we also look at the labor market effects of deregulation on black households. As a matter of fact, the development of the banking sector has been shown to lift corporations' credit constraints and increase corporate investments and entrepreneurship activities (Black and Strahan, 2002; Chava et al., 2013), potentially allowing better employment opportunities in the communities. At the same time, the emergence of new jobs can lead to improved economic stability, job assurance, and decreased financial pressure, all of which can favor both physical and mental well-being (Hu et al., 2019).

We find that banking deregulation increases the wages and job opportunities for black individuals, aligning with previous empirical research indicating that banking deregulation activities boost the income of the people in the lower part of the income distribution (Beck et al., 2010). However, it is important to note that simply having a higher income is insufficient to enhance health expenditures and health outcomes (Acemoglu et al., 2013; Banerjee et al., 2015; Dupas and Miguel, 2017), particularly in regions where such services are limited (Cramer, 2021). Furthermore, in contrast to the notion that economic prosperity positively impacts health, numerous papers in the literature have documented that mortality in the United States decreases during economic recessions (Ruhm, 2000, 2003, 2005; Finkelstein et al., 2024). Nevertheless, in our setting, since the wage increase meets more supply of healthcare services, we are confident that it will turn into better health outcomes for individuals, too.

Finally, we investigate the role of households' mortgage access for being a possible transmitter between banking deregulation and mortality. In fact, access to the mortgage market and home ownership positively affects the living standards of individuals and communities (Green and White, 1997; DiPasquale and Glaeser, 1999) and even more so for low-income and black households (Tewari, 2014). Moreover, mortgage access might expose households to beneficial neighborhood effects such as increasing education (Bayer et al., 2008; Chetty et al., 2016), less crime (Kling et al., 2005), or better health outcomes via pollution reduction (Currie et al., 2015, 2023).

To shed light on this mechanism, we gather comprehensive data on mortgage applications and demonstrate that banking deregulation enhances households' access to the mortgage market. More importantly, we illustrate that this effect is particularly

pronounced for black individuals.

**Previous literature and contribution.** Our research contributes to three strands of literature.

The first strand is on the link between credit availability and health outcomes. Hu et al. (2019) show that banking deregulation in the United States reduces mental depression among low-income households. Cramer (2021) finds that bank branch openings in under-banked areas in India improve the health of individuals via the demand and supply of healthcare. Kopytov et al. (2021) show that relaxing financing frictions through an increase in house prices positively affects the longevity of cancer patients. Adams et al. (2022) provide evidence that financial assistance programs improve health care access for low-income patients. Aghamolla et al. (2021) find that bank stress tests push affected hospitals to shift their operations to enhance their profit margins in response to a negative credit shock but reduce the quality of their care to patients across a variety of measures.

Our paper advances this recent growing literature by providing compelling evidence that competitive banking markets affect mortality dynamics. In doing so, we show that banking market development played a crucial role in reducing mortality rates and inequality in the history of the United States. A better understanding of these dynamics is particularly relevant considering the substantial economic value of improvements in life expectancy (Murphy and Topel, 2006; Hall and Jones, 2007)

The second strand of literature analyzes the impact of local banking market conditions on economic and social outcomes. More specifically, well-developed financial

markets reduce information, enforcement, and transaction costs, positively affecting economic growth, socio-economic outcomes, and the overall allocative efficiency of resources in the economy (e.g., Rajan and Zingales, 1998; Peek and Rosengren, 2000; Cole et al., 2008; Berger et al., 2023). In this way, financial development has been shown to favor low-income individuals and decrease overall income inequality (e.g., Black and Strahan, 2001; Beck et al., 2010; Levine et al., 2008; Delis et al., 2014).

Our paper complements this part of the literature by providing evidence of the impact of financial development on communities' well-being and health disparities. This finding holds particular importance in elucidating the influence of banking markets on the welfare of society. Moreover, prior research indicating a positive impact of economic recessions on mortality (Ruhm, 2000, 2003, 2005; Finkelstein et al., 2024) suggests that it would be wrong to merely extrapolate our conclusions from previous findings regarding the effects of deregulation on economic outcomes. Therefore, an independent evaluation of this relationship is necessary and provided in this paper.

Finally, our findings add to a third strand of literature on the relationship between geography and health outcomes in the United States (Couillard et al., 2021; Badinski et al., 2023). Previous literature identifies several factors that can explain regional differences in mortality rates and mortality inequality, including for example unhealthy behaviors (smoking and drinking alcohol), the environment, and crime (e.g., Deryugina and Molitor, 2020, 2021), among others.

Our paper contributes to this literature since we identify a novel factor that can explain regional differences in mortality rates; in our setting, local banking markets and households' access to credit significantly predict local mortality patterns. Importantly,

our estimated effects are economically meaningful and even greater in terms of economic magnitude than other factors identified in the previous literature.

# 2 Data

## 2.1 County-level analysis

Our primary source of information is administrative data on individual death certificates in the United States. We use it to build age-adjusted mortality rates and a measure of mortality inequality between black and white residents at the county level. Next, we merge this database with information on the timing and the intensity of the banking deregulation process across states, mortgage market lending, and economic characteristics of counties for the period 1990-2005. Additionally, we use in our empirical analysis the Current Population Survey (CPS), a survey database that provides us with information at the household level.

**Mortality rates.** We use proprietary information on death certificates for the universe of United States residents from the U.S. Centers for Disease Control (CDC). Each death certificate provides detailed information on the underlying cause and deceased demographic information, such as age, place of residence, and race.

We count the number of individuals who died by county, year, individuals' race, and age category. We use this information and population estimates from the National Cancer Institute's Surveillance, Epidemiology End Results (SEER) Program to compute yearly age-adjusted mortality rates, a weighted average of the crude death rates

across age categories within a county, where the shares of the overall population in each age category are used as weights.[2] Also, the database provides detailed information on the underlying cause of death, allowing us to compute mortality rate patterns for alternative causes of death separately.

Following previous literature (e.g., Pierce and Schott, 2020; Currie and Schwandt, 2016), we consider the county of residence of the deceased person for constructing the age-adjusted mortality rates. Furthermore, a county is included in the analysis if at least one person died in this area during the period 1990-2005 and if the size of the population of interest differs from zero.

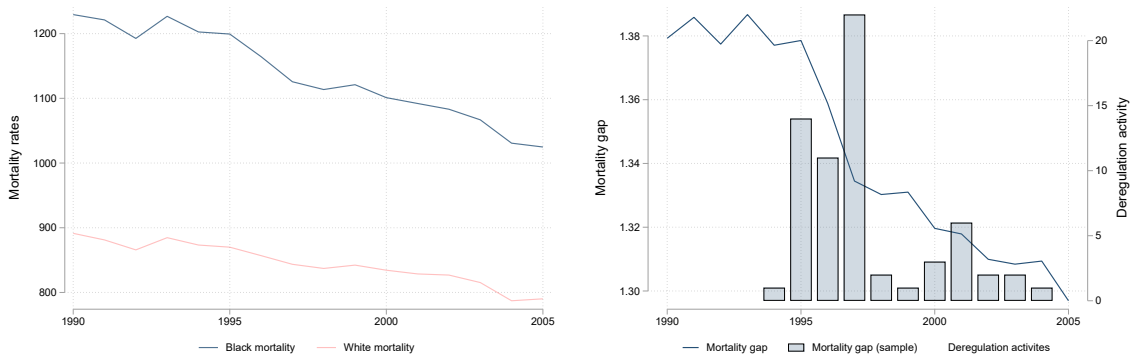
Figure 1(a) shows the evolution of age-adjusted mortality rate (per 100,000 inhabitants) for black and white residents separately. In line, for example, with Satcher et al. (2005), the mortality rate for black residents is much higher than for white residents for the whole spanning period. During 1990-2005, there has been a decrease in mortality rates for both groups. However, the decrease in mortality rates has been greater for black individuals.

Figure 1(b) shows the black-white mortality gap, defined as the ratio between black and white age-adjusted mortality rates. By construction, a value higher than one implies a higher age-adjusted mortality rate for black individuals. The ratio decreased by ten percentage points from the early 1990s until 2005, coinciding with the majority of banking deregulation activities across all states, as depicted by the grey bars.

Figure 2 shows the spatial distribution of the black-white (age-adjusted) mortality

[2]The age categories we consider in our analysis are less than one year, 1-4 years, 5-14 years, 15-24 years, 25-34 years,... and older than 85 years (Klein, 2001). The weights are computed considering U.S. population shares in the year 2000.

Figure 1: Mortality rates and deregulation activities




(a) Black and White Mortality Trends

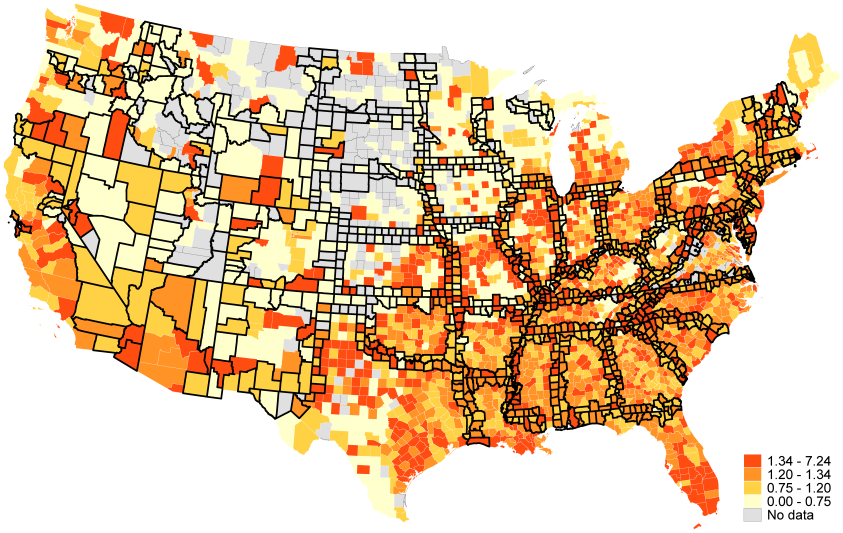
(b) Black-White Mortality Gap

**Notes**: Figure 1(a) displays the trends in black and white age-adjusted mortality rates (per 100,000 population) in the United States between 1990 and 2005. Figure 1(b) shows the trend in the black-white age-adjusted mortality gap in the United States between 1990 and 2005. The mortality gap is defined as the ratio of black-to-white age-adjusted mortality rates. The grey bars represent the total sum of yearly deregulation activities across all states.

gap between 1990 and 2005, confirming a sizeable cross-sectional variation found in the previous literature (e.g., Deryugina and Molitor, 2021; Finkelstein et al., 2021; Murray et al., 2006). In 68% of the counties, the mortality rate for black individuals is higher than that of white individuals. Additionally, state-border counties in the figure are delineated by black lines, as we conduct our analysis focusing on this subgroup in the border-discontinuity robustness check.

**Bank deregulation index.** Cross-state branching was fully legal after the passage by the Congress of the Interstate Banking and Branching Efficiency Act (IBBEA) of 1994. However, each state reserved the right to oppose entry by out-of-state banks and set regulations to erect barriers on interstate branching through four critical aspects. First, states could impose a minimum age for the target institution, with a maximum restriction of five years. Second, states could not allow *de novo* interstate branching.

Figure 2: Black-White Mortality Gap




**Notes**: The figure displays the spatial distribution of the black-white age-adjusted mortality gap across counties in the United States from 1990-2005. The mortality gap is defined as the ratio of black-to-white age-adjusted mortality rates. We have marked state-border counties with black lines.

Third, states could disallow the acquisition of individual branches and require an out-of-state bidder bank to acquire all branches of an in-state target bank. Moreover, fourth, states could impose a statewide deposit cap and restrict the fraction of deposits an out-of-state bank could acquire in that state.

Most states exercised their authority under the new law. Rice and Strahan (2010) consider these four elements and build an index of interstate branching restrictions which is heavily used in the finance literature to investigate the effects of banking deregulation (e.g., Favara and Imbs, 2015; Cornaggia et al., 2015). The index ranges between zero and four, whereas four refers to the most deregulated states.

Table OA1 reports the deregulation timing, the set of adopted regulations, and the related index score for each state. Along the same line, the right y-axis of Figures 1(b) graphically shows the total number of deregulation activities by year after the IBBEA.

Many deregulation activities started immediately after 1994, but we also observe significant changes occurring between 1996 and 2002. By the end of the sample period, eight states had still not experienced any form of deregulation.[3]

To further validate this strategy, Johnson and Rice (2008) constructed a dataset to represent the percentage of interstate branches out of the total branches for each state and year. Their findings revealed that before 1994, interstate branching was rarely utilized. In fact, in 1994, there were only 62 out-of-state branches across the entire country. However, by 2005, this number had surged to 24,728, accounting for 37.28% of all domestic branches. This increase in interstate branching led to heightened competition and enhanced efficiency within the banking sector.

**Establishment level information.** We collect information on the universe of firm establishments in the United States and their location from the National Establishment Time Series (NETS) database provided by Dun and Bradstreet (e.g., Currie et al., 2010; Addoum et al., 2020). Using the NAICS code 62, we can trace establishments operating in the healthcare sector. More specifically, we use the number of establishments to proxy for the industry's size in a county. We also use this database to collect information on the revenues in the healthcare industry and in tobacco, beer, wine, and liquor stores.[4]

**Mortgage market access.** To measure mortgage market access, we rely on the Home Mortgage Disclosure Act (HMDA) dataset providing annual mortgage application information on newly issued mortgage loans from individual mortgage lenders to

[3]Based on Favara and Imbs (2015) and Rice and Strahan (2010), we assume that all states had full restrictions before deregulation events.

[4]We identify tobacco and beer, wine, and liquor stores using the NAICS codes 453991 and 445310, respectively.

individual households.

The Home Mortgage Disclosure Act from 1975 requires approximately 80% of all mortgage lending institutions nationwide to disclose information about the property's location, the year of application, the dollar amount of the loan, the application decision, and, notably, the income or race/ethnicity of the applicant. Most depository institutions (commercial banks, savings associations, and credit unions) with home or branch offices in a county must report. The only exemptions are small institutions with assets below an annually updated threshold[5], lenders not in the home-lending business, or those that have offices exclusively in rural areas (non-MSAs). Non-depository consumer- and mortgage-finance companies must report if they originate one hundred or more home purchases or home refinancing loans per year.

We aggregate all accepted mortgage loan flows for each lending institution according to the purchased property's location, namely by county. We keep all loan purpose types[6], all lien types, and all owner-occupancy types. Also, since most loan type indicators are available as of 2004 only, removing certain loan types would make the data incomparable with past sample years.

**County controls.** Most of the previous literature argues that mortality differences across areas could be explained by differences in individuals' income and unequal geography of opportunity (e.g., Snyder and Evans, 2006; Kaplan et al., 1996). Therefore, we use several county characteristics as control variables in our analyses, sourced from the Bureau of Labor Statistics. More specifically, in our main specification, we con-

[5]The threshold increased from $10 million in 2010 to $47 million in 2020. See http://www.ffiec.gov/hmda/pdf/2010guide.pdf or http://www.ffiec.gov/hmda/pdf/2020guide.pdf.

[6]Loan types can be categorized as home purchase, home improvement, and refinancing loans.

trol for income per capita, the natural logarithm of the county population, and the employment rate.

**Final county-level sample and descriptives.** Mortality raw data contains information for the 52,094,491 individuals who died between 1990 and 2011 in 3,159 counties in the United States. We use this information to compute age-adjusted mortality rates separately for the black and white populations. Specifically, the white population that died during this time frame is 44,769,425 (in 3,154 counties), and the number of black individuals who died is 6,251,770 (in 2,888 counties). We categorize the number of people who died by age category, county of residence, and year, and then merge population information for each group. Next, we collapse the data to create the age-adjusted mortality rates and exclude observations in which the population of interest equals zero.

After generating information at the county and year levels from the administrative database on death certificates, we merge information regarding deregulation, mortgage market acceptance rates, and county control variables. Additionally, we remove singletons (Correia, 2016) and limit our sample period to 1990-2005. Consequently, our final sample comprises 2,756 counties with 36,322 observations. Summary statistics of the county characteristics are presented in the top panel of Table 1, while detailed variable descriptions are provided in Table OA2.[7]

[7]We compare the summary statistics of the main variables of interest included in our final sample of counties (2,756) and in the sample of counties that we excluded (481), separately, in Table OA3. We also show the normalized differences in the last column; according to Imbens and Wooldridge (2009), two variables are considered similar if this value falls within the range of +/- 0.25. As expected, the excluded counties exhibit smaller white and black populations. Moreover, these counties display reduced employment rates while maintaining comparability in terms of income per capita. Importantly for our analysis, however, there are no statistical differences in the intensity of the deregulation activities.

## 2.2 Household-level analysis

**Cross-sectional household data.** We use the Current Population Survey (CPS). This survey database provides yearly cross-sectional information for our analysis period on a large representative sample of households in the United States. Importantly for our paper, the database contains detailed information on the demographic characteristics of household members (i.e., race, age, education, and marital status), their labor outcomes (wages), and their health insurance (whether the respondent holds private or public health insurance). However, one limitation of this database is its inability to track families over time. Furthermore, it provides information on their state of residence but lacks data on their county of residence.

We restrict our sample to working-age individuals (between 16 and 65 years old), for which information is not missing. Also, we follow Hacamo (2021) and remove households that move within the last year from another state or abroad. Our final database comprises 1,626,625 observations for the spanning period 1990-2005. Summary statistics for this sample are presented in the bottom panel of Table 1, and detailed variable descriptions can be found in Table OA2.

Table 1: Descriptive statistics

| | Mean | SD | 25th | 75th |
|---|---|---|---|---|
| **County-level data (N=36,322; i=2,756)** | | | | |
| MortalityRate | 914.50 | 134.15 | 824.62 | 995.75 |
| WhiteMortalityRate | 894.48 | 129.20 | 812.03 | 968.27 |
| BlackMortalityRate | 1021.59 | 991.04 | 550.36 | 1276.72 |
| MortalityGap | 1.14 | 1.14 | 0.63 | 1.41 |
| Deregulation | 1.03 | 1.41 | 0.00 | 2.00 |
| Population | 10.66 | 1.24 | 9.77 | 11.37 |
| Income | 22400.47 | 6717.27 | 17880.00 | 25559.00 |
| Employment | 0.49 | 0.14 | 0.40 | 0.57 |
| Acceptance(all) | 86.68 | 6.55 | 83.12 | 90.65 |
| Acceptance(white) | 88.32 | 6.11 | 85.22 | 91.92 |
| Acceptance(black) | 80.98 | 18.22 | 74.78 | 94.94 |
| New Establishments (Healthcare) | 17.31 | 64.23 | 1 | 11 |
| Healthcare Sales | 2712.03 | 2239.15 | 1314.15 | 3454.08 |
| Alcohol Sales | 65.14 | 83.06 | 14.83 | 87.25 |
| Tobacco Sales | 7.77 | 21.20 | 0 | 7.90 |
| Total Inflows | 4.84 | 10.80 | 0.69 | 4.02 |
| Total Outflows | 4.79 | 12.13 | 0.67 | 3.64 |
| **Household-level data (CPS) (N=1,626,625)** | | | | |
| Female | 0.52 | 0.50 | 0.00 | 1.00 |
| Age | 38.44 | 13.31 | 28.00 | 49.00 |
| Married | 0.58 | 0.49 | 0.00 | 1.00 |
| Wage (log) | 7.23 | 4.38 | 0.00 | 10.31 |
| College | 0.21 | 0.40 | 0.00 | 0.00 |
| Private Insurance | 0.75 | 0.43 | 1.00 | 1.00 |
| Public Insurance | 0.10 | 0.20 | 0.00 | 0.00 |
| Occupation | 0.21 | 0.41 | 0.00 | 0.00 |

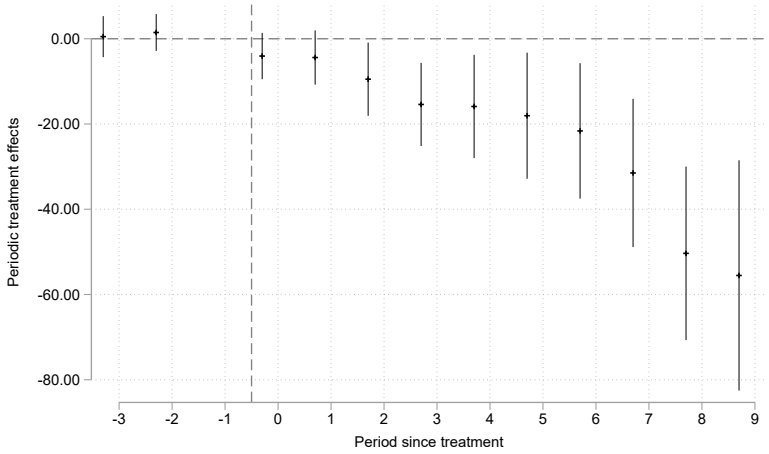
**Notes**: The table presents the descriptive statistics for the samples that we have analyzed. See Table OA2 for a detailed description of every variable.

# 3 Banking Deregulation and Mortality Rates

**Mortality dynamics after deregulation.** In this section, we analyze whether the deregulation of the banking sector affects mortality rate patterns. To do so, we take the staggered banking deregulation wave starting in the 1990s across states as a laboratory. As reported in Table OA1, this period is characterized by many banking deregulation activities across United States regions.

We start by showing local mortality dynamics considering yearly point estimates before and after the first deregulation event in Figure 3. The plot reveals two insights. First, pre-event point estimates are insignificant, suggesting that the parallel-trend assumption is likely to hold. Second, we find negative and statistically significant coefficients after the deregulation event. In terms of magnitude, the effects that we estimate are also economically meaningful, with yearly mortality rates decreasing by even 50 deaths per 100,000 inhabitants in the subsequent years.[8]

Figure 3: Deregulation event and mortality




**Notes**: The figure illustrates the dynamic impact of the initial banking deregulation on mortality rates. The plot exhibits annual point estimates and 95% confidence intervals based on standard errors clustered by state. We derived the estimates by regressing county mortality rates on the leads and lags of the first deregulation event, while controlling for county and year-fixed effects.

**Empirical specification.** After providing the first evidence on mortality dynamics after the 1990s banking deregulation wave, we further investigate the relationship between banking deregulation and mortality using the regression methodology used in

[8]Recent research has highlighted issues with pre-event trend analysis, expressing doubts about its ability to effectively detect significant deviations from parallel trends (Roth, 2022; Bilinski and Hatfield, 2018). To further test whether this assumption holds, we conduct a sensitivity test following the approach proposed by Rambachan and Roth (2023), and show the results in Figure OA1. We find that the fixed length confidence intervals of the first post-deregulation event are still negative and different from zero when allowing for violations of parallel trends that are approximately linear and for larger degrees of possible non-linearity in the violation of parallel trends.

Favara and Imbs (2015).

$$Y_{i,t} = \beta \text{Deregulation}_{s,t-1} + \Gamma X_{i,t-1} + \eta_i + \theta_t + \epsilon_{i,t} \quad (1)$$

$Y_{i,t}$ stands for different outcome variables used. Mortality Rate, White Mortality Rate, and Black Mortality Rate are the age-adjusted mortality rates in county $i$ of state $s$ at year $t$ for the whole population and white and black residents separately. Mortality Gap is the ratio of the Black Mortality Rate to the White Mortality Rate. Deregulation is the one-year lagged deregulation index of county $i$ located in state $s$ at time $t$. $X_{i,t-1}$ is the set of time-varying county characteristics. More specifically, we control for the natural logarithm of the population size (Population), local income over population (Income), and the employment share (Employment).[9] $\eta_i$ and $\theta_t$ are county and year-fixed effects, respectively. We cluster standard errors at the state level, and further consider the possible heterogeneity in mortality responses across counties estimating Equation (1) using weighted least squares (WLS). Following Favara and Imbs (2015), the weights used are the (inverse of) the number of counties per state.

**Results.** We report estimation results for Equation (1) in Table 2. We start by investigating the effect of deregulation on overall mortality rates in Column (1). In line with our previous results, we find that with the deregulation of banking markets,

[9]Figure OA2 displays the coefficients of a regression using deregulation intensity, measured as the maximum value of deregulation activity, as the dependent variable of interest, and as independent variables alternative county characteristics measured as in 1990. Prior to conducting the regression, we standardized all the variables to facilitate easier coefficient comparison. We observe a positive correlation between deregulation intensity and both income per capita and population initial conditions. Deregulation intensity is also positively correlated with the employment ratio, although the coefficient is not statistically significant.

yearly mortality rates decrease significantly. Considering the period until 2005, we find that an increase of one unit induces mortality rates to decrease by roughly 3 individuals per 100,000 inhabitants. Regarding mortality rates of white individuals in Column (2), we estimate a negative and statistically insignificant coefficient equal to 1.7. Column (3) shows an opposite, economically meaningful, and statistically significant effect of banking deregulation on the mortality rates of black residents. More specifically, a one-unit increase in the deregulation index decreases black mortality by 40 black individuals per 100,000 black residents.

As also indicated by the decline in the racial mortality gap concurrent with banking deregulation activity in Figure 1(b), the difference in the economic and statistical significance between the black and white mortality estimates in Table 2 suggests that bank deregulation decreases black-white mortality inequality. In order to further investigate this hypothesis, we estimate Equation (1) using the black-white mortality gap ratio as the outcome variable and present the estimates in Column (4). Again, we find a negative and statistically significant effect. More specifically, a one-unit increase in the deregulation index decreases the gap by 0.044, which is considerable given that the initial gap is close to 1.3 in our sample in 1994. Put differently, if a state made its banking market fully deregulated (from 0 to 4 in terms of the index), the effect on the gap would have been a reduction of $(0.047/1.3) * 4 = 14.4\%$.

Table 2: Baseline

| **Dependent variable:** | Mortality Rate | White Mortality Rate | Black Mortality Rate | Mortality Gap |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Deregulation | -3.1135* | -1.4673 | -40.0940*** | -0.0439*** |
| | (1.6244) | (2.1438) | (11.4113) | (0.0142) |
| County FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| County controls | Yes | Yes | Yes | Yes |
| Observations | 36,322 | 36,322 | 36,322 | 36,322 |
| R-squared | 0.7443 | 0.6152 | 0.2185 | 0.2058 |

**Notes**: The table shows estimation results from Equation (1). In every column, we employ various dependent variables, including overall, white, and black mortality rates and the black-white mortality gap. All the regressions include the county and year-fixed effects and controls for county characteristics (population, local income per capita, and employment share). We use the (inverse of) the number of counties per state to weigh our regressions. We cluster standard errors at the state level. ***, **, and * indicate significant coefficients at the 1%, 5%, and 10% levels, respectively. See Table OA2 for a detailed explanation of every variable.

**Alternative causes of death.** We distinguish the causes of death to understand whether a specific cause drives our main findings. In this way, we want to test whether the impact of banking deregulation is asymmetric across alternative causes of death. On this point, Ruhm (2000) finds that economic recessions have an adverse effect across several types of mortality rates but positively affect suicide rates. Therefore, we measure the black-white mortality gap for the main causes of death leading to higher levels of racial mortality inequality in the United States and estimate Equation (1), again. We report the results in Table 3.

In Column (1) of Panel A, it is evident that banking deregulation reduces the black-white mortality gap for deaths from circulatory diseases (Circulatory). The coefficient is negative, statistically significant, and equals 0.07.

Similar findings are evident for other causes of death contributing to racial mortality inequality in the United States. Specifically, as shown in Column (2), deregulation

decreases the black-white mortality gap for deaths attributed to cancer (Cancer). The coefficient of interest is negative, statistically significant, and equals 0.06. This result is consistent with the findings of Kopytov et al. (2021), who emphasize the positive impact of an increase in borrowing limits on the longevity of cancer patients.

We also find a negative effect when examining a broader category and analyzing mortality patterns for internal causes of death (Internals), with the estimated coefficient equal to 0.04. Furthermore, we investigate the impact of deregulation on the black-white mortality gap for deaths resulting from alcohol-related diseases (Alcoholic) and external causes of death (External). The coefficient for deaths from alcohol-related diseases remains negative and statistically significant. However, the coefficient related to the impact of deregulation on external causes of death is negative but lacks statistical significance. Nevertheless, the absence of statistical significance for this coefficient aligns with the idea that banking deregulation may influence mortality rates through an expansion of the healthcare sector and improved access to private health insurance, as we will demonstrate in our mechanism section.

Overall, these findings support the hypothesis that a specific cause of death does not drive our main results. Additionally, they suggest that no specific cause of mortality is positively affected by banking deregulation activities.

**Counterfactual simulation.** To better understand the implication of the development of the banking sector for mortality inequality, we consider a counterfactual simulation approach inspired by Chodorow-Reich (2014). More specifically, we aim to measure the potentially foregone reduction in the mortality gap since not all states

Table 3: Alternative causes of deaths

| **Dependent variable:** | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Mortality gap | | | | |
| | Circulatory | Cancer | Internal | Alcoholic | External |
| Deregulation | -0.0710** | -0.0519** | -0.0392*** | -1.2871* | -0.0666 |
| | (0.0271) | (0.0217) | (0.0129) | (0.6566) | (0.0835) |
| Average Outcome | 0.92 | 1.21 | 1.14 | 1.93 | 1.11 |
| County FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| County controls | Yes | Yes | Yes | Yes | Yes |
| Observations | 31,071 | 33,907 | 36,174 | 15,244 | 31,359 |
| R-squared | 0.0806 | 0.1372 | 0.1959 | 0.1515 | 0.1170 |

**Notes**: The table shows estimation results from Equation (1). In each column, we use alternative outcome variables, which include the black-white mortality gap for the following causes of death: circulatory diseases, cancer, internal causes of death, alcohol-related diseases, and external causes of death. All the regressions include county and year-fixed effects and control for county characteristics (population, local income per capita, and employment share). We use the (inverse of) the number of counties per state to weigh our regression. We cluster standard errors at the state level. ***, **, and * indicate significant coefficients at the 1%, 5%, and 10% levels, respectively. See Table OA2 for a detailed explanation of every variable.

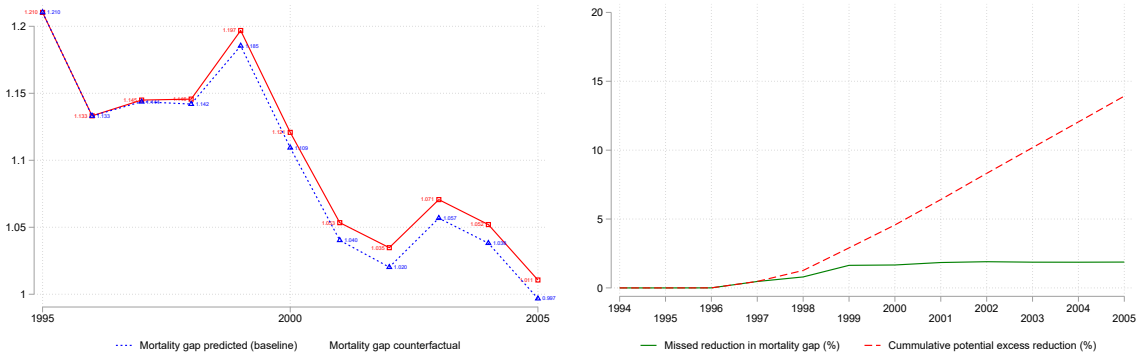
have fully deregulated.

The counterfactual regression assumes that all states deregulate fully, providing a predicted mortality gap under complete deregulation. Using this measure, we calculate the counterfactual gap as the sum of the predicted gap from the baseline regression reported in Equation (1) and the difference between the baseline and the counterfactual regressions' gap multiplied by the $\beta$ of the counterfactual regression.

We report our findings in Figure 4. The left graph shows the differences between the predicted mortality gap from the baseline regression and the counterfactual gap for every year. On the right, we show the missed reduction in the mortality gap and the cumulative potential excess reduction in %. These results suggest great losses in terms of missed reduction in the racial mortality gap due to the absence of complete deregulation of the banking sector (a cumulative effect of 14% at the end of the deregu-

lation period) and highlight the importance of the banking sector in shaping mortality inequality.

Figure 4: Simulation




(a) Counterfactual

(b) Potential Reduction

**Notes**: The figure shows estimation results of a simulation exercise where we analyze what would have happened if all the states fully deregulate during our period of analysis. On the left, we show the differences between the predicted black-white mortality gap from the baseline regression reported in Equation (1) and the counterfactual gap. On the right, we show the missed reduction in the black-white mortality gap and the cumulative potential excess reduction every year in %.

# 4 Robustness checks

We undertake several analyses to check the robustness of our results. To save space, we report all the results from this section in the Online Appendix.

**Treatment effect heterogeneity.** A recent strand of the literature in econometrics argues that difference-in-differences estimators can be biased when the treatment varies across groups and over time (De Chaisemartin and d'Haultfoeuille, 2020; Baker et al., 2022). In fact, estimating staggered treatment effects with a standard two-way fixed effects model (TWFE) can lead to "forbidden comparisons". While the standard two periods difference-in-differences only compares treatment and control groups, the stag-

gered treatment setting might falsely compare later-treated groups with earlier-treated groups. This leads to a bias with an arbitrary sign and magnitude.

In order to deal with this problem, we first compute the average treatment effect in the treated (ATT) weights and find that the sum of the negative weights in our main specification reported in Equation (1) is less than 10 %. Also, we test the impact of deregulation on mortality by considering alternative estimation approaches that aim to deal with this problem by carefully selecting treatment and control groups. More specifically, we consider as deregulation event the first deregulation period and plot the yearly point estimates for our outcomes of interest in Figure OA3 using the two-way fixed effects (TWFE) estimator and comparing it with the estimators proposed by Cengiz et al. (2019) and Sun and Abraham (2021).

Figure OA3 shows that the effects are consistent across the alternative estimators, suggesting that negative weights do not invalidate our results. In line with our baseline estimates, the effect is more substantial for black mortality rates and translates into a decrease in the racial mortality gap. More specifically, the first deregulation event leads to an average decrease in the mortality rates equal to 30 units for white and 70 units for black individuals in the post-deregulation period; the average decrease in the black-white mortality gap ratio is equal to 0.05. It is also important to note that the effect of the deregulation on black mortality rates and on the mortality gap is not immediate; as we will better explore in the mechanism section, a likely channel through which deregulation can affect the mortality gap is through its effect on the expansion of the healthcare sector in black communities. Therefore, it is possible that differences in the initial facility conditions explain these dynamics.

**Deregulation intensity and initial mortality conditions.** The absence of statistically significant coefficients before the deregulation event, as presented in Figure 3, and the formal test on the validity of the parallel trend assumption reported in Figure OA1, support the hypothesis that the timing of deregulation is orthogonal with respect to mortality patterns.

An additional assumption behind our identification strategy is that the *intensity* of deregulation activities is also not explained by mortality patterns before deregulation events. In order to provide empirical evidence for this hypothesis, we show in Figure OA4 that the *intensity* of deregulation (the maximum value of the deregulation index in a county during our period of analysis) is not correlated with the mortality rates and the mortality gap at the beginning of the deregulation period.

**Border discontinuity approach.** To ensure that omitted variable characteristics do not affect our results, we follow Huang (2008) and augment our differences-in-differences approach with a border discontinuity design. Intuitively, we want to increase the comparability between counties that are differentially exposed to banking deregulation due to their location in different states but are adjacent to each other around state borders and, thereby, likely to be similar in both observable and unobservable characteristics.

In doing so, we also consider Dube et al. (2010); they argue that while most counties in the border pair sample are geographically proximate, counties in the western U.S. are much larger and irregular in shape. For this reason, in a further check, we focus on county pairs that share a state border and whose centroids are within 75 km of each

other.[10]

Using these alternative approaches, we report the estimated effect of complete deregulation on the alternative mortality variables used in Equation (1) in % of pre-1995 y-mean in Figure OA5. The estimated results support the hypothesis that our findings are insensitive to the sample of analysis we use and suggest that omitted variables are not affecting our findings.

**State-level confounders.** We further test the robustness of our results by explicitly considering additional control variables to include in our preferred specification reported in Equation (1). On this point, Kroszner and Strahan (1999) show that political institutions and banks' private interests affect the banking deregulation process. For this reason, we control for political institutions in our regressions using a dummy variable equal to one if the state governor is a Democrat. Following Dow et al. (2020), we further control for differences in local economic characteristics and include current transfers per capita in the model.[11] Finally, we control for the racial composition using the share of black people in the county.

We report the estimated magnitudes of the effects of full deregulation on the outcome variables of interest after controlling for these additional control variables in Figure OA6. We find that the coefficients of interest are still statistically significant; in terms of magnitude, the estimated effects are within one standard deviation of the baseline results.

[10] A data-driven randomization inference procedure has been used to show that this cutoff minimizes the MSE of the estimator (Dube et al., 2010).

[11] Current transfers are defined as the sum of government social benefits and net current transfer receipts from business.

**Outliers.** We test whether outliers affect our results by reporting added variable plots in Figure OA7 for each outcome variable of interest. These plots allow us to graphically show the influence of the deregulation on each outcome while accounting for the influence of all the other control variables. Importantly, we find no evidence that a subset of observations drives the adverse effect of deregulation on mortality. Furthermore, Figure OA8 shows that the effects of full deregulation on the mortality outcomes used in Equation (1) are within one standard deviation of the effects we estimate using our baseline specification when we winsorize all the variables at the first and last percentiles.

**Measurement errors.** Another potential concern of our findings is that our measures of age-adjusted mortality rates could be affected by measurement errors. In fact, the information we use in the numerator and the denominator to compute age-adjusted mortality rates comes from different sources. Therefore, the population count is likely not consistent between the two databases. Also, measurement errors could also arise in counties with fewer deceased individuals.

In order to deal with this problem, we consider a three-year average of our outcome variables of interest and estimate again Equation (1). This approach allows us to minimize noise due to measurement errors (Currie and Schwandt, 2016).

Results showing the effect of full deregulation are reported in Figure OA9. The findings are consistent and the magnitudes of the new coefficients are within one standard deviation of our baseline coefficients.

**Weighting.** In our main specification, we follow Favara and Imbs (2015) and weigh the observations using the (inverse of) the number of all existing counties per state. The purpose of using WLS is to make our regression estimates close to the population target and deal with the heteroscedasticity of the standard errors (Solon et al., 2015).

We further test whether our results depend on the weighting we adopt to estimate Equation (1). Crucially, Figure OA10 shows that our results hold when we use weights coming from (the inverse of) county population and when we do not use any weight.

**Excluding the control variables.** Including additional control variables in our model can be beneficial if deregulation events and intensity are not entirely orthogonal concerning other local economic characteristics. However, a potential issue that arises with this approach is that any variable directly affected by banking deregulation events should not be included as a time-varying control in our regressions. This is known as the problem of "bad controls" (Angrist and Pischke, 2009). For this reason, we consider in our main specification the one-period lag of all the control variables. However, to further test the robustness of our results, Figure OA11 demonstrates that our findings remain consistent even when these controls are excluded from our main specification.

# 5 Mechanisms

There are several simultaneous mechanisms through which well-developed banking markets could affect mortality rates and decrease mortality inequality. As we explained in the introduction, banking development could increase access to finance for previously financially constrained individuals, allowing them to access inaccessible goods essential

for their health, such as health insurance or housing. At the same time, it could allow previously constrained firms to access credit, leading to higher local employment and income, and increasing the size of specific sectors relevant to individuals' well-being, such as the healthcare sector. These factors can potentially interplay with one another and our reduced form analysis does not allow us to capture the scope of these interactions and quantify the specific contributions of each mechanism to reducing mortality inequality. Nevertheless, within this section, we aim to highlight and deliberate on several intriguing, previously unexplored, mechanisms by which banking deregulation may influence health inequality.

**Healthcare sector.** Cramer (2021) emphasizes the healthcare sector as an essential channel through which financial development in India affects the health of individuals. More specifically, she finds that an exogenous increase in bank presence in the local community allows local hospitals to access finance and expand.

We investigate this hypothesis in our setting as differences in health service access are an important cause of racial mortality inequality in the history of the United States (Weinick et al., 2000; Williams and Jackson, 2005). Also, the healthcare sector during the 1990s was severely financially constrained due to concerns about the stability and profitability of the sector (Calem and Rizzo, 1995).

In order to analyze this mechanism, we collect information on the universe of establishments that operate in the healthcare sector from the NETS database (NAICS code equal to 62).[12] We next estimate the following interaction model:

[12]Detailed information on the frequency of establishment-year observations by 4-digit NAICS code in the United States for our period of analysis is available in Table OA4.

$$New\ Establishments_{i,t} = \beta_0 \text{Deregulation}_{s,t-1} + \beta_1 Share\ Black_{i,t-1} + \tag{2}$$

$$\beta_2 \text{Deregulation}_{s,t-1} \times Share\ Black_{i,t-1} + \Gamma X_{i,t-1} + \eta_i + \theta_t + \epsilon_{i,t}$$

In this setting, *New Establishments* is the number of new establishments that operate in the healthcare industry in county $i$ at time $t$. *Share Black* is the share of black population in the community. We standardize this variable to make our results easier to interpret; the coefficient $\beta_0$ represents the effect of the deregulation when the share of the black population is equal to its average value of zero, and $\beta_2$ is the effect of one unit increase in the deregulation index on the outcome variable when the share of the black population increase by one standard deviation (one unit). We estimate this equation for three alternative samples (considering all the counties in the sample, only the counties on the state borders, and only the counties on the state borders whose distance is within 75 km of each other). Next, we report the results in Table 4.

We show in Panel A that banking deregulation does not affect the healthcare sector. However, this average effect masks sizable effect heterogeneity. Indeed, when we interact deregulation events with the share of black individuals in Panel B, we document positive and statistically significant effects of deregulation on the number of new establishments. Furthermore, consistent with the banking competition hypothesis, we observe a stronger effect on border counties. More specifically, according to the results reported in Column (3), a one standard deviation increase in the share of black individuals (11% of the share of the black population) increases the total number of new

establishments that operate in healthcare services by 1 unit.

Table 4: Healthcare sector expansion

| | (1) | (2) | (3) |
|---|---|---|---|
| Sample | All counties | Counties on border | Counties on border ($\leq$ 75 km) |
| **Dependent variable:** | New Establishments (healthcare) | New Establishments (healthcare) | New Establishments (healthcare) |
| ***Panel A: average effect*** | | | |
| Deregulation | -0.2051 | 0.3132 | 0.3807 |
| | (0.4687) | (0.5191) | (0.5877) |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| County controls | Yes | Yes | Yes |
| Observations | 36,322 | 12,881 | 11,537 |
| R-squared | 0.7438 | 0.7715 | 0.7927 |
| ***Panel B: heterogeneous effect black neighbour*** | | | |
| Deregulation | 0.0213 | 0.5113 | 0.6086 |
| | (0.4707) | (0.4383) | (0.5031) |
| Share Black (STD) | 21.8531*** | 31.3442** | 32.6640** |
| | (7.1436) | (12.6864) | (13.1319) |
| Deregulation $\times$ Share Black (STD) | 0.8380*** | 1.1878*** | 1.0656*** |
| | (0.2580) | (0.3241) | (0.3070) |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| County controls | Yes | Yes | Yes |
| Observations | 36,322 | 12,881 | 11,537 |
| R-squared | 0.7442 | 0.7732 | 0.7946 |

**Notes**: The table shows estimation results from Equation (2). We use the number of new establishments that operate in the healthcare sector as a dependent variable. We also standardize the share of the black population to make the results easier to interpret. All the regressions include county and year-fixed effects and controls for county characteristics (population, local income per capita, and employment share). We use the (inverse of) the number of counties per state to weigh our regressions. We cluster standard errors at the state level. ***, **, and * indicate significant coefficients at the 1%, 5%, and 10% levels, respectively. See Table OA2 for a detailed explanation of every variable.

**Healthcare sector consumption.** An expansion of the healthcare sector is likely to lead to an increase in healthcare expenditures by individuals. To test this hypothesis, we estimate Equation (2) using as an outcome variable the total sales per person in the healthcare industry in county $i$ at time $t$. We report the results in Table 5.

As reported in Panel A, we still do not find any average effect of deregulation on the outcome variable. Again, when we interact deregulation events with the (standard-

ized) share of black individuals in Panel B, we document a positive and statistically significant effect. More specifically, according to the results reported in Column (3), a one standard deviation increase in the share of black individuals (11%) increases sales per capita in the healthcare industry by almost 118 $ after a one unit increase in the deregulation index.

Table 5: Healthcare expenditures

| | (1) | (2) | (3) |
|---|---|---|---|
| Sample | All counties | Counties on border | Counties on border ($\leq$ 75 km) |
| **Dependent variable:** | Healthcare Sales | Healthcare Sales | Healthcare Sales |
| ***Panel A: average effect*** | | | |
| Deregulation | 11.4361 | 0.0095 | 24.6257 |
| | (32.6182) | (43.4748) | (38.3689) |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| County controls | Yes | Yes | Yes |
| Observations | 36,322 | 12,881 | 11,537 |
| R-squared | 0.7860 | 0.8342 | 0.8665 |
| ***Panel B: effect in black neighborhoods*** | | | |
| Deregulation | 30.1325 | 6.5181 | 32.5971 |
| | (31.8122) | (30.8413) | (27.2672) |
| Share Black (STD) | 591.3339 | -70.9844 | 49.9084 |
| | (412.2195) | (619.2683) | (603.1073) |
| Deregulation $\times$ Share Black (STD) | 90.9964*** | 117.9552*** | 118.1405*** |
| | (31.0642) | (31.1746) | (28.2619) |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| County controls | Yes | Yes | Yes |
| Observations | 36,322 | 12,881 | 11,537 |
| R-squared | 0.7876 | 0.8378 | 0.8709 |

**Notes**: The table shows estimation results from Equation (2). We use the total sales per capita in the healthcare sector as a dependent variable. We also standardize the share of the black population to make the results easier to interpret. All the regressions include county and year-fixed effects and controls for county characteristics (population, local income per capita, and employment share). We use the (inverse of) the number of counties per state to weigh our regressions. We cluster standard errors at the state level. ***, **, and * indicate significant coefficients at the 1%, 5%, and 10% levels, respectively. See Table OA2 for a detailed explanation of every variable.

**Consumption of temptation goods.** Our results show that healthcare consumption increases after deregulation events in black communities. A possible concern is

that deregulation could also lead to an increase in consumption of other goods that can be detrimental to the health of the individuals. We investigate this hypothesis by running the regression model from Equation (2) considering total sales per capita in tobacco and alcohol stores as outcome variables.

As reported in Table 6, we do not find evidence that banking deregulation increases sales in *temptation goods*, both on average and in black communities. The rationale for this finding is that as individuals' financial situations improve, they may be less likely to consume tempting goods, as they may have financial resources to spend on healthier options or save for the future (Banerjee and Mullainathan, 2010; Banerjee and Duflo, 2007).[13]

**Health insurance.** An expansion in the healthcare sector and a simultaneous increase in the income of individuals could also lead to access to better health insurance for black individuals. Also, it is well documented that there are substantial racial differences in the use of health insurance in the United States. These have essential consequences for black individuals' health and mortality rates (Lillie-Blanton and Hoffman, 2005; Griffith et al., 2017).

We investigate this hypothesis using information from the Current Population Survey (CPS). More specifically, we estimate the following Equation (3):

[13] As reported in Table 3, the coefficient of the effect of deregulation on the mortality gap related to alcohol-related deaths is negative and statistically significant. Conversely, the triple interaction coefficient for alcohol sales is negative, although it lacks statistical significance. This limitation may arise from our lack of access to individual-level alcohol consumption data, which restricts our analysis to comparing alcohol consumption within black and white communities.

Table 6: Temptation goods

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Sample | All counties | Counties on border | Counties on border (≤ 75 km) | All counties | Counties on border | Counties on border (≤ 75 km) |
| **Dependent variable:** | Alcohol Sales | Alcohol Sales | Alcohol Sales | Tobacco Sales | Tobacco Sales | Tobacco Sales |
| ***Panel A: average effect*** | | | | | | |
| Deregulation | -1.2682 | -0.3062 | 0.3362 | -0.9548* | -0.5811 | -0.8166 |
| | (1.3066) | (1.0452) | (0.9944) | (0.4913) | (0.5805) | (0.6108) |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 36,322 | 12,881 | 11,537 | 36,322 | 12,881 | 11,537 |
| R-squared | 0.8155 | 0.8877 | 0.8837 | 0.6680 | 0.6852 | 0.7128 |
| ***Panel B: effect in black neighborhoods*** | | | | | | |
| Deregulation | -1.4335 | -0.3294 | 0.3630 | -1.0075** | -0.5791 | -0.8368 |
| | (1.4152) | (1.0647) | (0.9823) | (0.4864) | (0.5680) | (0.5939) |
| Share Black (STD) | -0.8678 | -0.2844 | 6.7186 | 0.1855 | 3.2024 | 0.6495 |
| | (10.8106) | (13.7426) | (14.4425) | (2.9917) | (3.7061) | (3.3318) |
| Deregulation × Share Black (STD) | -0.8949 | -0.3835 | -0.1323 | -0.2947 | -0.2213 | -0.3663 |
| | (0.7531) | (0.5672) | (0.5690) | (0.1847) | (0.2343) | (0.2399) |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 36,322 | 12,881 | 11,537 | 36,322 | 12,881 | 11,537 |
| R-squared | 0.8156 | 0.8877 | 0.8837 | 0.6682 | 0.6853 | 0.7131 |

**Notes**: The table shows estimation results from Equation (2). We use total expenditures per capita in tobacco and alcohol stores as distinct dependent variables. We also standardize the share of the black population to make the results easier to interpret. All the regressions include county and year-fixed effects and controls for county characteristics (population, local income per capita, and employment share). We use the (inverse of) the number of counties per state to weigh our regressions. We cluster standard errors at the state level. ***, **, and * indicate significant coefficients at the 1%, 5%, and 10% levels, respectively. See Table OA2 for a detailed explanation of every variable.

$$Health\ Insurance_{j,t} = \beta_0 \text{Dereg.}_{s,t-1} + \beta_1 Black_j + \beta_2 \text{Dereg.}_{s,t-1} Black_j \qquad (3)$$
$$+ \Gamma X_{j,t} + \eta_s + \theta_t + \epsilon_{i,t}$$

In this Equation, $Health\ Insurance_{j,t}$ is a dummy variable equal to one if the individual $j$ has private health insurance and zero otherwise. In an alternative exercise,

we also use a dummy variable equal to one if the individual has government-run health insurance (Medicare or Medicaid) and zero otherwise. *Black* is a dummy variable equal to 1 if the individual is black and zero otherwise. $\eta_s$ and $\theta_t$ are state and year fixed effects, respectively. Since this database does not provide information on the county of residence of the individuals, we can not run our regressions for alternative sub-samples. We report the results in Table 7.

As reported in Column (1) or Column (3), we do not find evidence that deregulation affects the probability that an individual has private or government-run health insurance. In Column (2) and Column (4), we find that black individuals are, on average, less likely to use private health insurance and more likely to use public health insurance by respectively 13% and 8%. However, deregulation events positively affect the probability that black individuals get access to private health insurance and negatively affect the probability of relying on government-run health insurance. More specifically, one standard deviation increase in the deregulation variable increases the probability that a black individual has private insurance by 1% and decreases the use of government-run health insurance by 0.5%.[14]

Overall, these results suggest that banking deregulation helps black individuals to access private health insurance while decreasing their use of public insurance. The shift to private health insurance could have brought several benefits to the health of black individuals, considering that it allows for enhanced access to superior treatment and more comprehensive medical care (Sorlie et al., 1994; Bhattacharya et al., 2003)

[14]We further investigate whether black individuals got access to private health insurance through their employer or whether they individually purchased the insurance. This information is available in the CPS starting from the year 1996. We, therefore, create two distinct outcome variables and estimate again Equation (3). We report the results in Table OA5 and document an increase in individually purchased private health insurance.

Table 7: Health Insurance

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Dependent variable:** | Private Insurance | Private Insurance | Public Insurance | Public Insurance |
| Deregulation | -0.0012 | -0.0022 | 0.0015 | 0.0021 |
| | (0.0017) | (0.0017) | (0.0013) | (0.0015) |
| Black | | -0.1209*** | | 0.0952*** |
| | | (0.0129) | | (0.0059) |
| Deregulation × Black | | 0.0093*** | | -0.0063** |
| | | (0.0034) | | (0.0025) |
| State FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Individual controls | Yes | Yes | Yes | Yes |
| Observations | 1,632,165 | 1,632,165 | 1,632,165 | 1,632,165 |
| R-squared | 0.0903 | 0.0957 | 0.0640 | 0.0711 |

**Notes**: The table shows estimation results from Equation (3). We use a dummy variable equal to one if the individual has private insurance and another dummy variable equal to one if the individual has government-run insurance as distinct dependent variables. All the regressions include state and year-fixed effects and controls for individuals' characteristics (a dummy variable equal to one if the individual is a female, a dummy variable equal to one if the individual is married, a dummy variable equal to one if the individual has a college degree, and controls for the age and the age squared of the individual). We cluster standard errors at the state level. ***, **, and * indicate significant coefficients at the 1%, 5%, and 10% levels, respectively. See Table OA2 for a detailed explanation of every variable.

**Labour markets.** Financial development could allow previously constrained firms to access credit, creating better job opportunities in the communities. Meanwhile, better jobs can create greater economic stability, job security, and reduced financial stress. Overall, it can positively impact both physical and mental health (Hu et al., 2019).

In this paragraph, we will investigate whether banking deregulation improves labor outcomes for black individuals. To do so, we first estimate Equation (3) using as outcome variable the natural logarithm of the wage of individual $j$ at time $t$. We report the results in Table 8. As reported in Column (2), we find that deregulation improves the wages of black individuals. Specifically, one unit increase in the deregulation index increases wages by 4 basis points. This result is in line with previous empirical research showing that banking deregulation activities boost the income of the people in the lower part of the income distribution (Beck et al., 2010).

We next investigate whether individuals in deregulated areas obtain better jobs by

examining their occupations. More specifically, we consider a dummy variable equal to one if the individual's occupation falls within managerial and professional specialty occupations, and 0 otherwise. [15] We find that individuals who work in occupations that we classify as 'better' not only have more prestigious jobs but also report substantially higher wages (an average of $43,000 versus $15,000 in our period of analysis), enjoy more stable employment (87% versus 77% full-time jobs), have similar weekly working hours (42 versus 38 hours), and are more likely to report that their employers provide them with private health insurance (86% versus 63%).

We use this outcome variable and estimate Equation (3). We report the results in Column (3) and Column (4). Still, we find that black individuals are more likely to obtain better jobs by a probability of 0.2 basis points after a one-point increase in the deregulation index.

**Housing.** Access to the mortgage market and home ownership has been shown to positively affect the living standards of the family and the communities (DiPasquale and Glaeser, 1999; Green and White, 1997). Mortgage access might also expose households to beneficial neighborhood effects such as increasing education, less crime, or better health outcomes via pollution reduction. These factors are particularly relevant to the health of black individuals. Indeed, unequal geography of opportunity and residential segregation is considered the main factors explaining the racial mortality gap and racial differences in health outcomes in the United States (e.g., Acevedo-Garcia et al., 2008;

[15] This classification includes executive, administrative, and managerial occupations, as well as professional specialty occupations, such as engineers, architects, scientists, lawyers, and judges. You can find information on the variable that we used at this link: https://cps.ipums.org/cps-action/variables/OCC1990#codes_section.

Table 8: Wages

| **Dependent variable:** | (1) Wage(log) | (2) Wage(log) | (3) Better Occupation | (4) Better Occupation |
|---|---|---|---|---|
| Deregulation | -0.0032 | -0.0082 | 0.0006 | 0.0003 |
| | (0.0103) | (0.0119) | (0.0008) | (0.0009) |
| Black | | -0.4808*** | | -0.0437*** |
| | | (0.0776) | | (0.0039) |
| Black × Deregulation | | 0.0443* | | 0.0024* |
| | | (0.0223) | | (0.0014) |
| State FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Individual controls | Yes | Yes | Yes | Yes |
| Observations | 1,632,165 | 1,632,165 | 1,632,165 | 1,632,165 |
| R-squared | 0.1367 | 0.1375 | 0.2453 | 0.2461 |

**Notes**: The table shows estimation results from Equation (3). We use the natural logarithm of wages as well as a dummy variable set to one when an individual's occupation falls within the category of managerial and professional specialty occupations as our dependent variables. All the regressions include state and year-fixed effects and controls for individual characteristics (a dummy variable equal to one if the individual is a female, a dummy variable equal to one if the individual is married, a dummy variable equal to one if the individual has a college degree, and controls for the age and the age squared of the individual). We cluster standard errors at the state level. ***, **, and * indicate significant coefficients at the 1%, 5%, and 10% levels, respectively. See Table OA2 for a detailed explanation of every variable.

Woolf and Braveman, 2011; Christensen and Timmins, 2022).

In this paragraph, we investigate whether bank deregulation improves individuals' access to the mortgage market using loan-level application information on mortgage origination. We do this by estimating:

$$Mortgage_{i,t} = \lambda_i + \gamma_t + \beta_1 \text{Deregulation}_{s,t-1} + \Gamma X_{i,t-1} + \epsilon_{i,t} \tag{4}$$

$Mortgage_{i,t}$ denotes mortgage access defined as the share of the accepted number of applications to the sum of accepted and declined applications in county $i$ located in state $s$ at year $t$. We compute this variable separately for all individuals and for black and white.

The results from Equation (4) in Table 9 indicate that the deregulation index pos-

itively and significantly affects mortgage acceptance rates for overall and both black and white applicants. More specifically, a one-unit increase in the deregulation index increases the share of overall accepted loans by 0.44%. When we consider white and black mortgage acceptance rates separately as outcome variables, we find an effect equal to respectively 0.5% and 0.77%, respectively. Considering that the average acceptance rate is 80% for black and 88% for white applicants, we conclude that the impact of deregulation on black acceptance rates is significantly higher. More specifically, black acceptance rates increase in fully deregulated areas by 3.8% concerning the average value, while only 2.2% for white applicants. These results align with previous research arguing that deregulation and financial development help minorities access finance (Tewari, 2014; Howell et al., 2021). The estimated effects are more remarkable when we consider only the counties on the state borders, as reported in Panel B and Panel C. Overall, these results show that banking deregulation is highly predictive of mortgage access.

Table 9: Housing

| **Dependent variable:** | Acceptance Rates | | |
|---|---|---|---|
| | All | White | Black |
| | (1) | (2) | (3) |
| ***Panel A: all counties*** | | | |
| Deregulation | 0.4673*** | 0.5334*** | 0.8389*** |
| | (0.1482) | (0.1434) | (0.2725) |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| County controls | Yes | Yes | Yes |
| Observations | 36,322 | 36,322 | 36,322 |
| R-squared | 0.5515 | 0.5034 | 0.1987 |
| ***Panel B: counties on the border*** | | | |
| Deregulation | 0.5889*** | 0.6929*** | 0.9668*** |
| | (0.1586) | (0.1630) | (0.2632) |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| County controls | Yes | Yes | Yes |
| Observations | 12,881 | 12,881 | 12,881 |
| R-squared | 0.5622 | 0.5190 | 0.1965 |
| ***Panel C: border counties and distance less than 75km*** | | | |
| Deregulation | 0.5565*** | 0.6442*** | 0.8976*** |
| | (0.1821) | (0.1699) | (0.2496) |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| County controls | Yes | Yes | Yes |
| Observations | 11,537 | 11,537 | 11,537 |
| R-squared | 0.5403 | 0.5048 | 0.1940 |

**Notes**: The table shows estimation results from Equation (4). We use the mortgage acceptance rates for the overall white and black populations as distinct dependent variables. All the regressions include county and year-fixed effects and controls for county characteristics (population, local income per capita, and employment share). We use the (inverse of) the number of counties per state to weigh our regressions. We cluster standard errors at the state level. ***, **, and * indicate significant coefficients at the 1%, 5%, and 10% levels, respectively. See Table OA2 for a detailed explanation of every variable.

**Migration patterns.** Finally, we investigate whether banking deregulation affects migration dynamics. Banking deregulation could indeed attract more educated (and healthier) individuals in affected areas, affecting overall mortality rates.

To investigate this hypothesis, we start by estimating the following Equation:

$$Migration_{i,t} = \lambda_i + \gamma_t + \beta_1 \text{Deregulation}_{s,t-1} + \Gamma X_{i,t-1} + \epsilon_{i,t} \quad (5)$$

In Equation (5), *Migration* are, alternatively, the total number of individuals that migrate in the county (*Total Inflow*), and the total number of individuals that migrate outside the county (*Total Outflow*).

We report the results for the alternative samples in Table 10. Importantly, we do not find any effect of deregulation activities on these alternative outcome variables.

Table 10: Migration

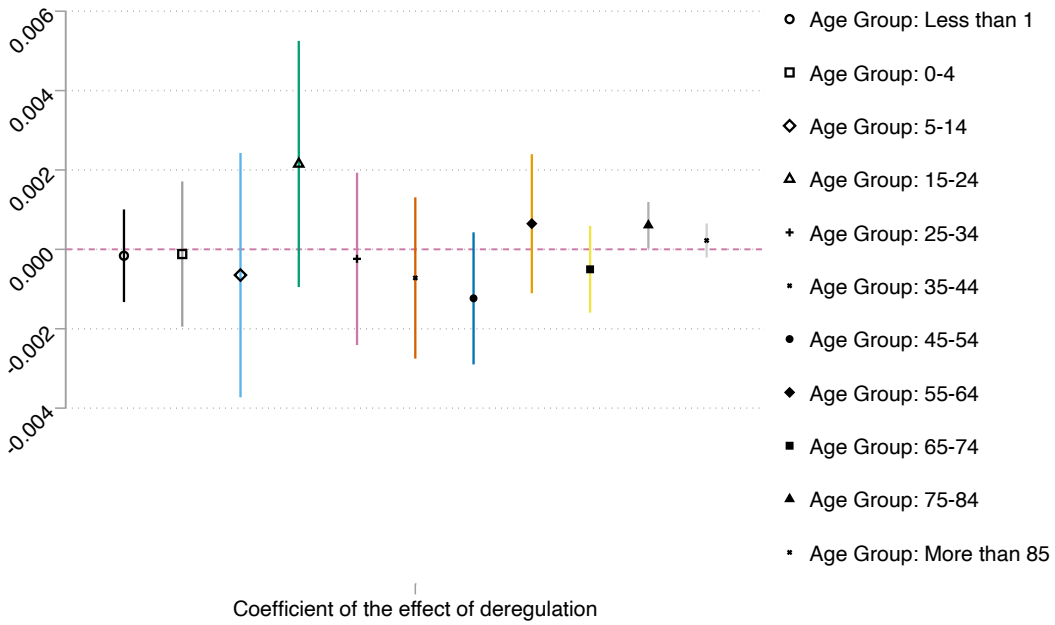
| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Sample | All counties | All counties | Counties on border on | Counties on border | Counties on border ($\leq$ 75 km) | Counties on border ($\leq$ 75 km) |
| **Dependent variable:** | Total Inflow | Total Outflow | Total Inflow | Total Outflow | Total Inflow | Total Outflow |
| Deregulation | 0.1190 | 0.0274 | 0.1190 | -0.0228 | 0.0788 | -0.0651 |
| | (0.0785) | (0.0762) | (0.1066) | (0.1113) | (0.0976) | (0.1226) |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 36,322 | 36,322 | 12,881 | 12,881 | 11,537 | 11,537 |
| R-squared | 0.9035 | 0.8937 | 0.8996 | 0.9001 | 0.8967 | 0.9028 |

**Notes**: The table shows estimation results from Equation (5). We use the total inflow and total outflow of individuals as distinct dependent variables. All the regressions include county and year-fixed effects and controls for county characteristics (population, local income per capita, and employment share). We use the (inverse of) the number of counties per state to weigh our regressions. We cluster standard errors at the state level. ***, **, and * indicate significant coefficients at the 1%, 5%, and 10% levels, respectively. See Table OA2 for a detailed explanation of every variable.

Since separate detailed information for the migration of black individuals is unavailable, we explore black population changes in deregulated counties. Specifically, to gain insight into population dynamics, we examine the share of the black population within different age groups. Next, we estimate Equation (5).

We present the coefficients representing the effect of deregulation on the share of the black population within each age bin in Figure 5. As depicted in the figure, we do not observe significant changes in population composition across the various age groups following deregulation events. The only coefficient that is marginally statistically significant is the one related to the impact of deregulation on the share of black people within the 75-84 age category. Overall, the results in this paragraph suggest that our main findings are not driven by migration patterns or changes in population composition.

Figure 5: Deregulation and changes in the share of black population within age groups




**Notes**: The figure displays the coefficients representing the effect of deregulation on the the share of black population within each age bin. These estimates are derived from Equation (5). Additionally, our analysis concentrates on counties along the borders of the states.

# 6 Conclusions

Understanding the determinants of mortality rate patterns is crucial since mortality rates and inequality vary widely, even within countries like the United States (Finkelstein et al., 2021). We shed light on this issue by focusing on the unexplored role of local banking market conditions for mortality dynamics and inequality in the United States.

We find that banking deregulation significantly decreases mortality. This effect is driven by a decrease in the mortality rate of black residents, implying a decrease in the black-white mortality gap. We posit that a synergistic interplay between wage augmentation, access to private health insurance, and heightened healthcare supply could hold the key to explaining our findings. Additionally, we show that deregulation activities improve access to the mortgage market, foremost benefiting the health of black individuals.

Our robust results suggest that lifting financial constraints through less banking regulation can increase health conditions for underprivileged groups. In light of the recent COVID-19 pandemic and the debate about minority ethnicities disadvantages through economic factors (Abuelgasim et al., 2020), our results seem to highlight that banking development can be a crucial factor in raising public health and making societies more resilience towards future pandemics.

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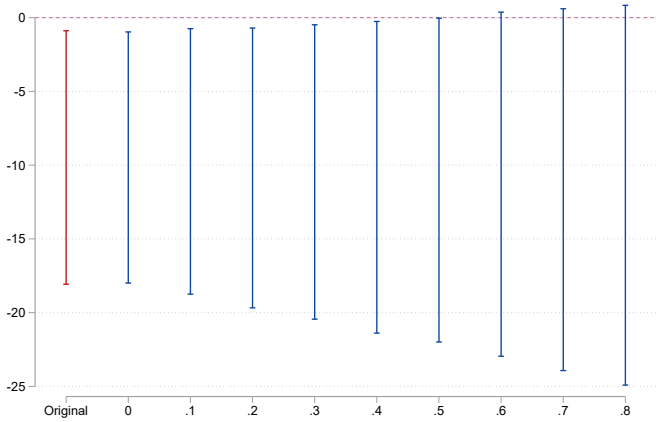
# Online Appendix

This Appendix is for Online Publication and provides further details on the data and the results of the paper "Credit Markets and Racial Mortality Inequality".

## OA1 Figures and Tables

**Parallel trend assumption.** Figure OA1 shows the results of a sensitivity test proposed by Rambachan and Roth (2023). It estimates the extent to which our main results remain robust in the face of potential nonlinearities with varying magnitudes in the counterfactual trend. We calculate 95 % confidence intervals of the effect of the first deregulation event under varying assumptions of the value M, the upper limit for the change between two consecutive time periods in the slope of the underlying linear trend. A value of M equal to 0 on the x-axis corresponds with allowing for linear violations of parallel trends. Larger values of M allow for larger deviations from linearity. We show that the fixed length confidence intervals are similar to those from our baseline estimators when allowing for violations of parallel trends that are approximately linear. Our results are also robust to allow for larger degrees of possible non-linearity in the violation of parallel trends. The breakdown value for a significant effect is indeed equal to a value of M of around 0.6.

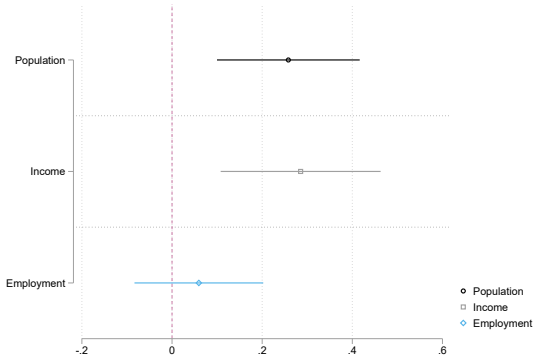
Figure OA1: Sensitivity test




**Notes**: Figure OA1 provides the results of a formal sensitivity analysis that relates the magnitude of violations of parallel trends to the robustness of treatment estimates in post-treatment periods (Rambachan and Roth, 2023). It shows 95 % confidence intervals for our main estimators under varying assumptions of the value M.

**Deregulation intensity and county characteristics.** Figure OA2 shows the coefficients representing the correlation between the maximum deregulation value in each county and the pre-deregulation county characteristics.

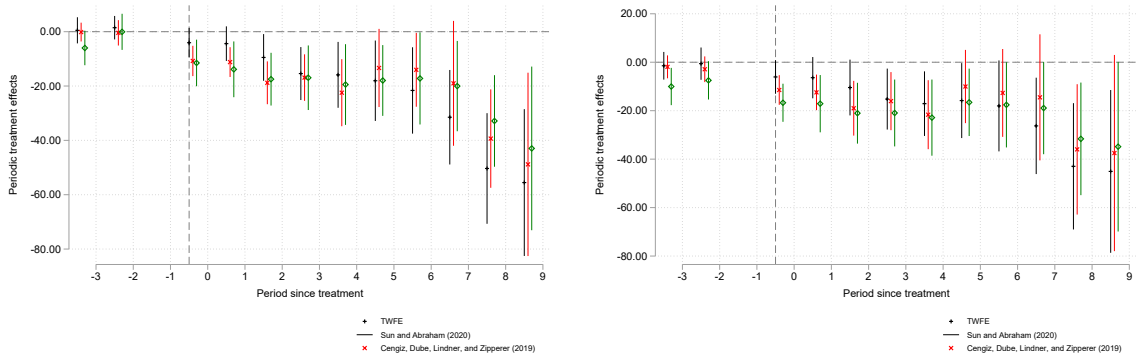
Figure OA2: Deregulation intensity and county characteristics




**Notes**: Figure OA2 displays the coefficients of a regression using deregulation intensity, measured as the maximum value of deregulation activity, as the dependent variable of interest, and as independent variables alternative county characteristics measured as in 1990. Prior to conducting the regression, we standardized all the variables to facilitate easier coefficient comparison.

**Differences-in-differences bias.** We consider as the event the first deregulation period and plot the yearly point estimates for our outcome of interest in Figure OA3 using the two-way fixed effects (TWFE) estimator and compare it with the estimators proposed by Cengiz et al. (2019) and Sun and Abraham (2021).

Figure OA3: Banking Deregulation and Mortality Rates: Dynamic Effects




(a) Overall Mortality Rates

(b) White Mortality Rates

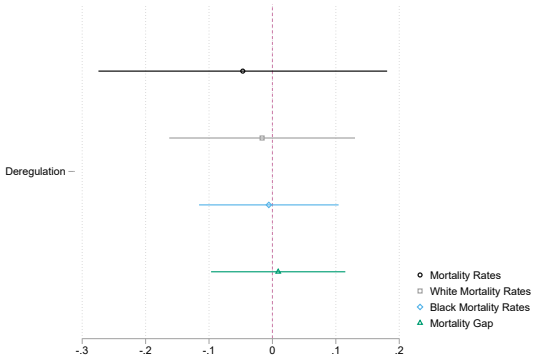
(c) Black Mortality Rates

(d) Mortality Gap

**Notes**: The figure displays the dynamic impact of the first banking deregulation event on overall, white, and black mortality rates, as well as the black-white mortality gap. We use alternative estimation approaches (Cengiz et al., 2019; Sun and Abraham, 2021). The graphs show point estimates and 95%-confidence intervals based on standard errors clustered by state.

**Deregulation intensity and initial mortality conditions.** Figure OA4 shows the coefficients representing the correlation between the maximum deregulation value in each county and initial mortality conditions.

Figure OA4: Deregulation intensity and initial mortality conditions



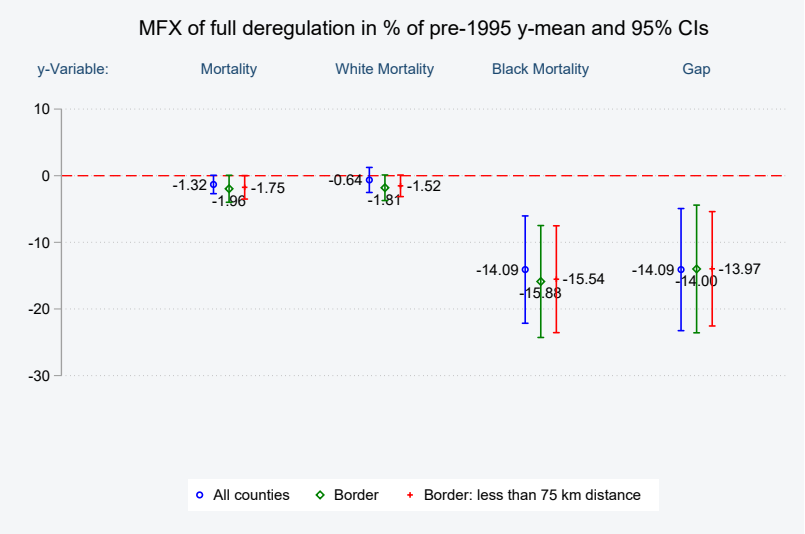

**Notes**: FigureOA4 displays the coefficients of a regression using deregulation intensity, measured as the maximum value of deregulation activity, as the independent variable of interest, along with the alternative outcome variables measured as in 1990. To run these regressions, we standardized all the variables to facilitate easier coefficient comparison.

**Alternative samples.** We follow Huang (2008) and combine our difference-in-differences approach with a border discontinuity design. The purpose of this exercise is to further test the robustness of our results by comparing communities that are differentially exposed to changes in the banking deregulation process because they are located in different states but are adjacent and therefore expected to be similar in both observable and unobservable conditions.

In doing so, we also consider Dube et al. (2010). They argue that while most counties in the border pair sample are geographically proximate, counties in the west of the country are much larger and irregular in shape; therefore, they suggested considering only county pairs whose centroids are within 75 km of each other.

Using these alternative approaches, we report the effects of complete deregulation on the alternative mortality variables used in Equation (1) in % of pre-1995 y-mean in Figure OA5.

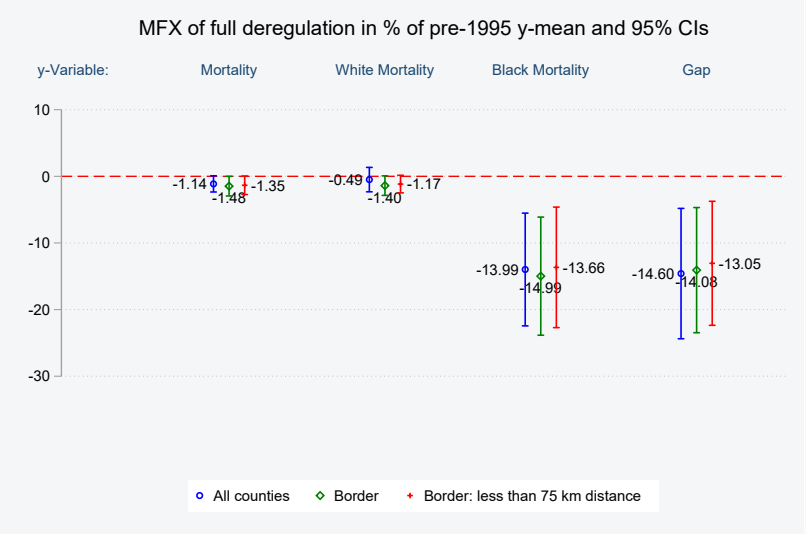
Figure OA5: Samples




**Notes**: The figure shows the effect of full deregulation in % of pre-1995 y-mean for alternative samples. We employ various dependent variables, including overall, white, and black mortality rates and the black-white mortality gap ratio. All the regressions include county and year-fixed effects and controls for county characteristics (population, local income per capita, and employment share). We use the (inverse of) the number of counties per state to weigh our regressions and cluster standard errors at the state level.

**Additional control variables.** We show that our results hold when we control local political institutions using a dummy variable equal to one if the state governor is a Democrat. Following Dow et al. (2020), we further control for local economic characteristics and include current transfers per person in the model. Finally, to consider neighboring differences between white and black communities, we consider the share of black people in the county in our regressions. We report the estimated effects in Figure OA6.

Figure OA6: Additional controls

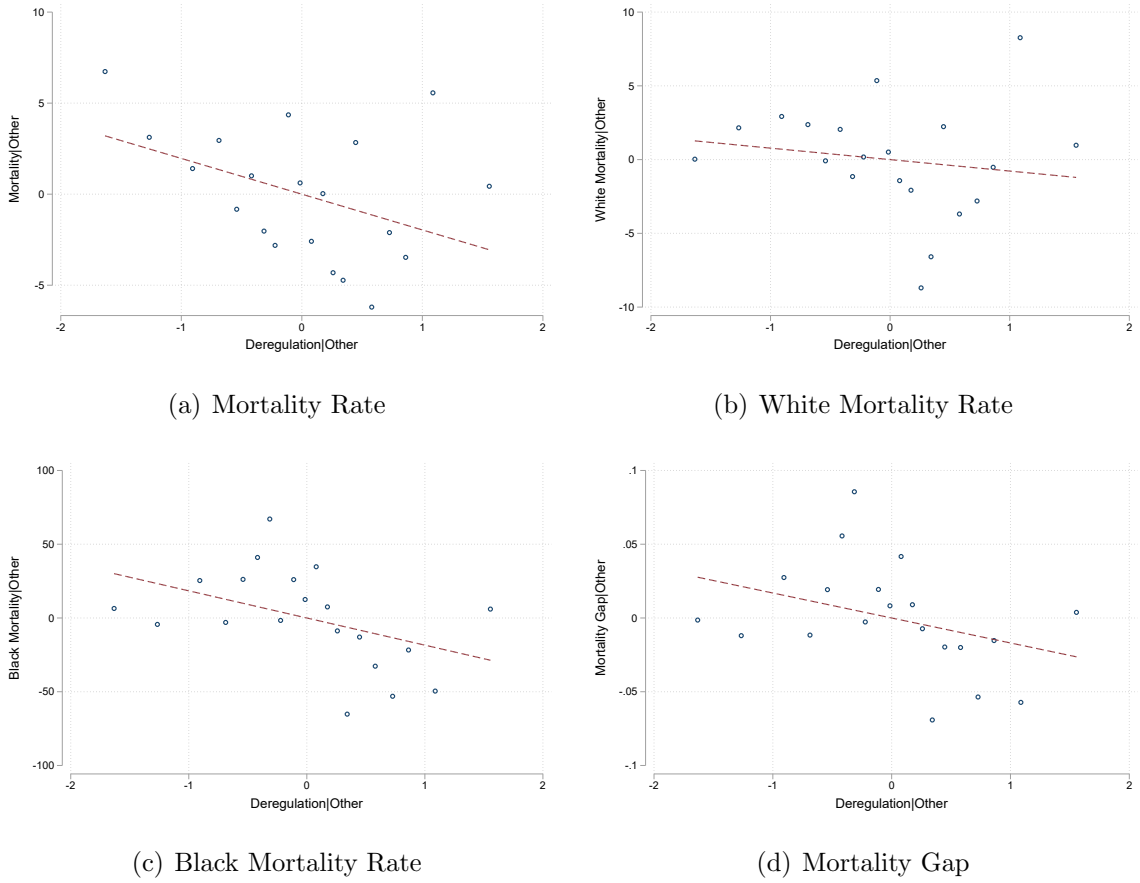



**Notes**: The figure shows the effect of full deregulation in % of pre-1995 y-mean for alternative samples when we include additional control variables (a dummy variable equal to one if the state governor is a Democrat, the HHI based on the number of mortgages by each branch, current transfers per person, and the share of black people). We employ various dependent variables, including overall, white, and black mortality rates and the black-white mortality gap. All the regressions include county and year-fixed effects and controls for county characteristics (population, local income per capita, and employment share). We use the (inverse of) the number of counties per state to weigh our regressions and cluster standard errors at the state level.

**Added variable plot.** We want to test whether outliers drive our results. We analyze this issue graphically and report our main outcome variables' added variable plots in Figure OA7.

The figure shows the relationship between deregulation and mortality rates while accounting for the influence of all the other control variables and fixed effects. We do not find evidence that a subset of observations drives our main findings.

Figure OA7: Added variable plots




(a) Mortality Rate

(b) White Mortality Rate

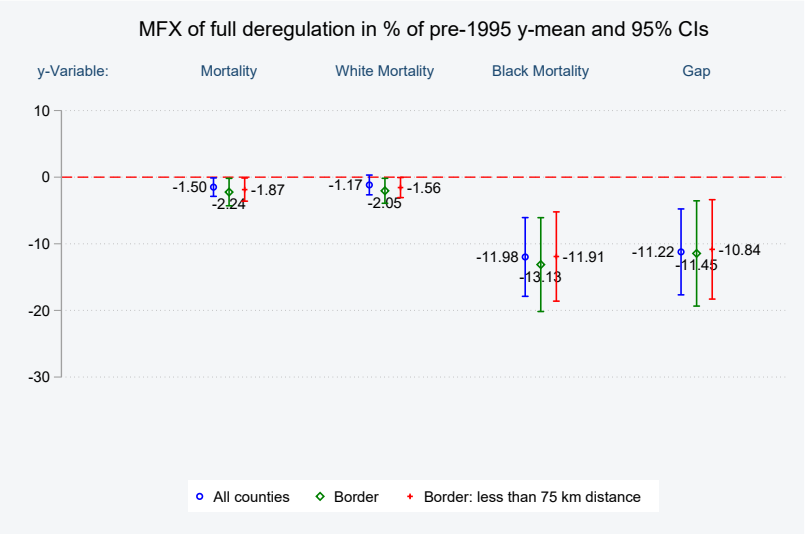
(c) Black Mortality Rate

(d) Mortality Gap

**Notes**: The figure shows binned added variable plots from Equation (1). We employ various dependent variables, including overall, white, and black mortality rates and the black-white mortality gap ratio. All the regressions include county and year-fixed effects and controls for county characteristics (population, local income per capita, and employment share). We use the (inverse of) the number of counties per state to weigh our regressions and cluster standard errors at the state level.

**Outliers.** We test whether outliers affect our results by winsorizing all the variables used in our empirical analysis at the first and last percentiles. Using this approach, Figure OA8 shows the effect of full deregulation on the alternative outcome variables proposed in Equation (1).

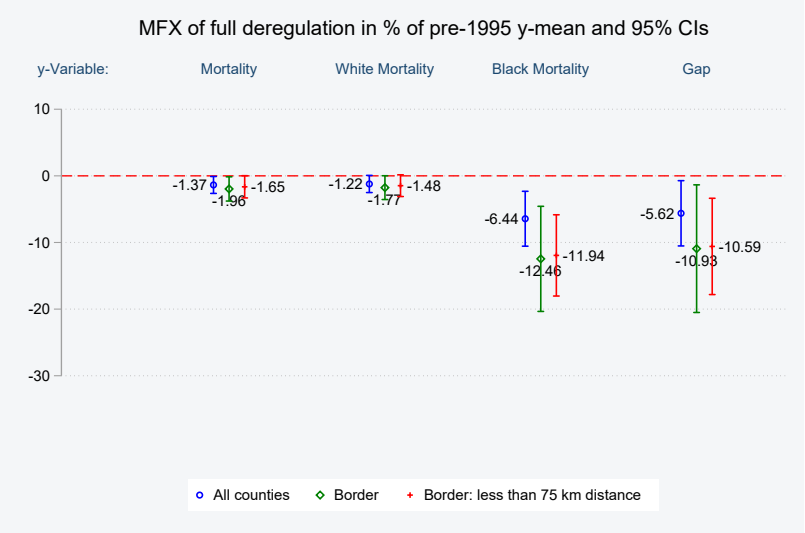
Figure OA8: Outliers




**Notes**: The figure shows the effect of full deregulation in % of pre-1995 y-mean for alternative samples when we winsorize all the variables at the first and last percentiles. We employ various dependent variables, including overall, white, and black mortality rates and the mortality gap. All the regressions include county and year-fixed effects and controls for county characteristics (population, local income per capita, and employment share). We use the (inverse of) the number of counties per state to weigh our regressions and cluster standard errors at the state level.

**Measurement errors.** We test if our main results are affected by measurement errors in our dependent variables. To do so, we estimate Equation (1) using as outcome variables the average outcomes over three years (Currie and Schwandt, 2016). Figure OA9 shows the effect of complete deregulation on the alternative outcome variables used in Equation (1) when we propose this approach.

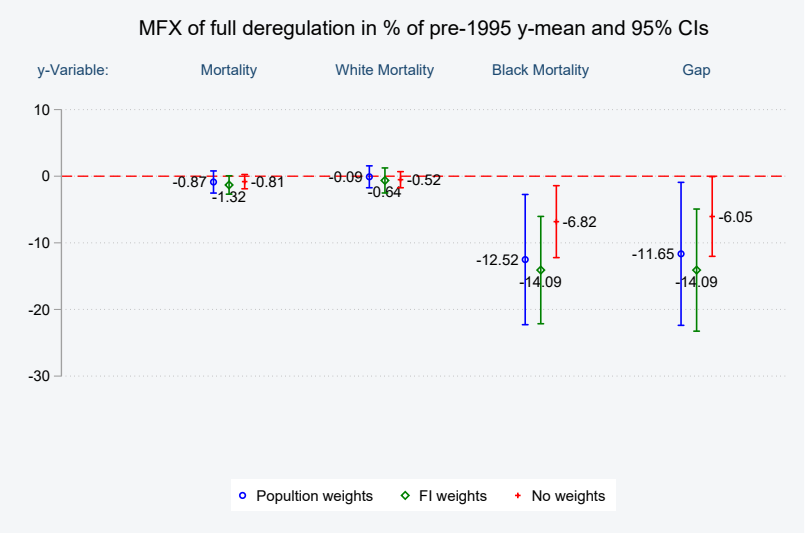
Figure OA9: Measurement Errors




**Notes**: The figure shows the effect of full deregulation in % of pre-1995 y-mean for alternative samples when we consider a three-year average of the dependent variables to deal with measurement errors. We employ various dependent variables, including overall, white, and black mortality rates and the black-white mortality gap. All the regressions include county and year-fixed effects and controls for county characteristics (population, local income per capita, and employment share). We use the (inverse of) the number of counties per state to weigh our regressions and cluster standard errors at the state level.

**Weights.** We test whether our results are not affected by the weighting we adopted to estimate Equation (1). In our main specification, we follow Favara and Imbs (2015) and weight using the (inverse of) the number of counties in each state. Figure OA10 shows that our results do not change when we weigh the observations using the (inverse of) county population or when we do not use any weight.

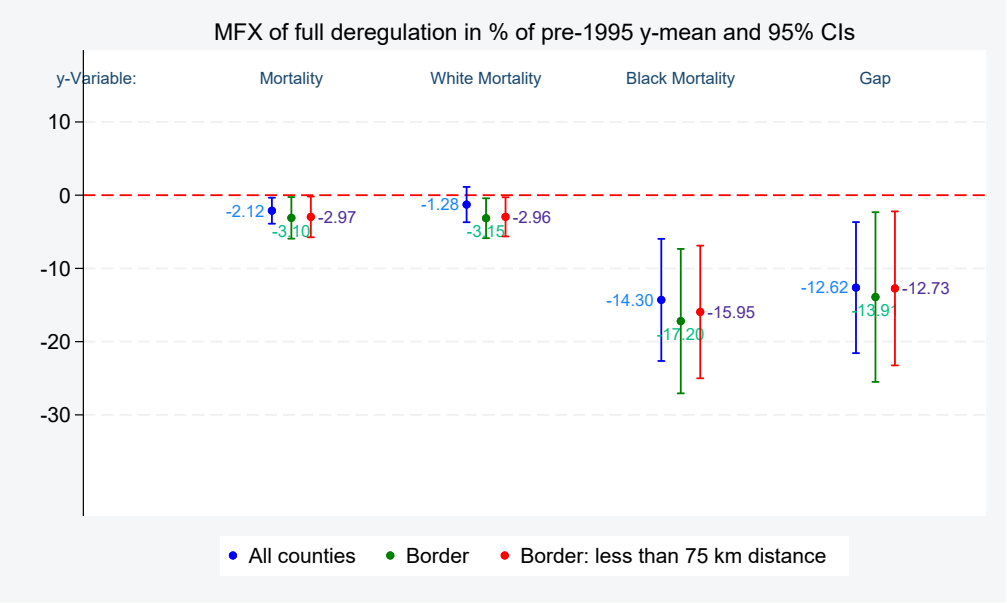
Figure OA10: Weights




**Notes**: The figure shows the effect of full deregulation in % of pre-1995 y-mean when we consider alternative ways to weight the regressions. We employ various dependent variables, including overall, white, and black mortality rates and the black-white mortality gap. All the regressions include county and year-fixed effects and controls for county characteristics (population, local income per capita, and employment share). We use the (inverse of) county population or alternatively, no weights, and cluster standard errors at the state level.

**Excluding the control variables.** A potential issue in our main specification is that any outcome directly affected by banking deregulation should not be included as a time-varying control in our main regression. This is known as the problem of "bad controls" (Angrist and Pischke, 2009). To further test the robustness of our results, Figure OA11 demonstrates that our findings remain consistent even when these controls are excluded.

Figure OA11: Excluding the control variables




**Notes**: The figure shows the effect of full deregulation in % of pre-1995 y-mean when we consider alternative ways to weight the regressions. We employ various dependent variables, including overall, white, and black mortality rates and the black-white mortality gap. All the regressions include county and year-fixed effects. We use the (inverse of) the number of counties per state to weigh our regressions and cluster standard errors at the state level.

**Deregulation timing.** After 1994, states reserve the right to set regulations on interstate branching about four important aspects:

(*i*) States could impose a minimum age for the target institution, with a maximum restriction of five years;

(*ii*) States could not allow *de novo* interstate branching;

(*iii*) States could disallow the acquisition of individual branches and require that an out-of-state bidder bank acquire all branches of an in-state target bank;

(*iv*) States could impose a statewide deposit cap and restrict the fraction of deposits an out-of-state bank could acquire in that state.

Rice and Strahan (2010) consider these four elements and build an index of interstate branching restrictions ranging from zero to four. Specifically, we add one to the index: if a state imposes a minimum age of less than 3 years on target institutions of interstate acquirers; if a state permits de novo interstate branching; if a state permits the acquisition of individual branches by an out-of-state bank; and if a state imposes a deposit cap of more than 30%.

Table OA1 reports for each state the timing of the deregulation, the set of regulations adopted, and the related index score we use in our analysis. Following Favara and Imbs (2015), it is assumed that every state was under full restrictions before deregulation events.

Table OA1: Deregulation Intensity (Rice and Strahan, 2010)

| State | Branching Index | Effective Year | Minimum Age | De-novo Interstate Branching | Acquisition Single Branch | Deposit Cap |
|---|---|---|---|---|---|---|
| Alabama | 1 | 1997 | 5 years | No | No | 0,3 |
| Alaska | 2 | 1994 | 3 years | No | Yes | 0,5 |
| Arizona | 2 | 2001 | 5 years | No | Yes | 0,3 |
| Arizona | 1 | 1996 | 5 years | No | No | 0,3 |
| Arkansas | 0 | 1997 | 5 years | No | No | 0,25 |
| California | 1 | 1995 | 5 years | No | No | 0,3 |
| Colorado | 0 | 1997 | 5 years | No | No | 0,25 |
| Connecticut | 3 | 1995 | 5 years | Yes | Yes | 0,3 |
| Delaware | 1 | 1995 | 5 years | No | No | 0,3 |
| DC | 4 | 1996 | No | Yes | Yes | 0,3 |
| Florida | 1 | 1997 | 3 years | No | No | 0,3 |
| Georgia | 1 | 2002 | 3 years | No | No | 0,3 |
| Georgia | 1 | 1997 | 5 years | No | No | 0,3 |
| Hawaii | 4 | 2001 | No | Yes | Yes | 0,3 |
| Hawaii | 1 | 1997 | 5 years | No | No | 0,3 |
| Idaho | 1 | 1995 | 5 years | No | No | None |
| Illinois | 1 | 1997 | 5 years | No | No | 0,3 |
| Indiana | 3 | 1998 | 5 years | Yes | Yes | 0,3 |
| Indiana | 4 | 1997 | No | Yes | Yes | 0,3 |
| Iowa | 0 | 1996 | 5 years | No | No | 0,15 |
| Kansas | 0 | 1995 | 5 years | No | No | 0,15 |
| Kentucky | 1 | 2004 | No | No | No | 0,15 |
| Kentucky | 1 | 2000 | No | No | No | 0,15 |
| Kentucky | 0 | 1997 | 5 years | No | No | 0,15 |
| Louisiana | 1 | 1997 | 5 years | No | No | 0,3 |
| Maine | 4 | 1997 | No | Yes | Yes | 0,3 |
| Maryland | 4 | 1995 | No | Yes | Yes | 0,3 |
| Massachusetts | 3 | 1996 | 3 years | Yes | Yes | 0,3 |
| Michigan | 4 | 1995 | No | Yes | Yes | None |
| Minnesota | 1 | 1997 | 5 years | No | No | 0,3 |
| Mississippi | 0 | 1997 | 5 years | No | No | 0,25 |
| Missouri | 0 | 1995 | 5 years | No | No | 0,13 |
| Montana | 0 | 2001 | 5 years | No | No | 0,22 |
| Nevada | 1 | 1995 | 5 years | Limited | Limited | 0,3 |
| New Hampshire | 4 | 2002 | No | Yes | Yes | 0,3 |
| New Hampshire | 3 | 2000 | 5 years | Yes | Yes | 0,3 |
| New Hampshire | 0 | 1997 | 5 years | No | No | 0,2 |
| New Jersey | 3 | 1996 | No | No | Yes | 0,3 |
| New Mexico | 1 | 1996 | 5 years | No | No | 0,4 |
| New York | 2 | 1997 | 5 years | No | Yes | 0,3 |
| North Carolina | 4 | 1995 | No | Yes | Yes | 0,3 |
| North Dakota | 3 | 2003 | No | Yes | Yes | 0,25 |
| North Dakota | 1 | 1997 | No | No | No | 0,25 |
| Ohio | 4 | 1997 | No | Yes | Yes | 0,3 |
| Oklahoma | 3 | 2000 | No | Yes | Yes | 0,2 |
| Oklahoma | 0 | 1997 | 5 years | No | No | 0,15 |
| Oregon | 1 | 1997 | 3 years | No | No | 0,3 |
| Pennsylvania | 4 | 1995 | No | Yes | Yes | 0,3 |
| Rhode Island | 4 | 1995 | No | Yes | Yes | 0,3 |
| South Carolina | 1 | 1996 | 5 years | No | No | 0,3 |
| South Dakota | 1 | 1996 | 5 years | No | No | 0,3 |
| Tennessee | 3 | 2003 | 3 years | Yes | Yes | 0,3 |
| Tennessee | 3 | 2001 | 5 years | Yes | Yes | 0,3 |
| Tennessee | 2 | 1998 | 5 years | No | Yes | 0,3 |
| Tennessee | 1 | 1997 | 5 years | No | No | 0,3 |
| Texas | 2 | 1999 | No | Yes | Yes | 0,2 |
| Utah | 3 | 2001 | 5 years | Yes | Yes | 0,3 |
| Utah | 2 | 1995 | 5 years | No | Yes | 0,3 |
| Vermont | 4 | 2001 | No | Yes | Yes | 0,3 |
| Vermont | 2 | 1996 | 5 years | No | Yes | 0,3 |
| Virginia | 4 | 1995 | No | Yes | Yes | 0,3 |
| Washington | 1 | 1996 | 5 years | No | No | 0,3 |
| West Virginia | 3 | 1997 | No | Yes | Yes | 0,25 |
| Wisconsin | 1 | 1996 | 5 years | No | No | 0,3 |
| Wyoming | 1 | 1997 | 3 years | No | No | 0,3 |

**Notes**: This table shows for each state the timing of the deregulation, the set of regulations adopted, and the related index score we use in our analysis.

**Description of the variables.** Table OA2 contains detailed information on the variables we use in the empirical analysis, their definitions, and their sources.

Table OA2: Variable description

| Variable name | Description | Source |
|---|---|---|
| **County-Level Database** | | |
| Mortality Rate | Age-adjusted mortality rate per 100,000 county population. Rates are computed as a weighted average of the crude death rates across age categories within a county, where the shares of the overall U.S. population in each age category are used as weights | CDC |
| White Mortality Rate | Black age-adjusted mortality rate per 100,000 black county population. Rates are computed as a weighted average of the crude death rates across age categories within a county, where the shares of the overall U.S. population in each age category are used as weights | CDC |
| Black Mortality Rate | White age-adjusted mortality rate per 100,000 black county population. Rates are computed as a weighted average of the crude death rates across age categories within a county, where the shares of the overall U.S. population in each age category are used as weights | CDC |
| Mortality Gap | The ratio between black and white age-adjusted mortality rates. Rates are computed as a weighted average of the crude death rates across age categories within a county, where the shares of the overall U.S. population in each age category are used as weights | CDC |
| Deregulation | Index of interstate branching deregulation ranging from 0 (most restricted) to 4 (least restricted). | Rice and Strahan (2010) |
| Employment | The share of employed people in a county | BLS |
| Income | County income divided by 1'000 total population | BLS |
| Population | The natural logarithm of the county population | BLS |
| Current transfers | The sum of government social benefits and net current transfer receipts from business | BLS |
| Acceptance (all) | The share of accepted number of mortgage applications to the sum of accepted and declined applications | HMDA |
| Acceptance (white) | The share of accepted number of mortgage applications to the sum of accepted and declined applications for white individuals | HMDA |
| Acceptance (black) | The share of accepted number of mortgage applications to the sum of accepted and declined applications for black individuals | HMDA |
| Democrat governor | Dummy variable equal to 1 if the state governor is a democrat | National Governors Association |
| Share Black | The share of black people in the county | SEER |

**Notes**: The table describes each variable and its source.

Table OA2: Variable description cont'd

| Variable name | Description | Source |
|---|---|---|
| New Establishments (Healthcare) | The total number of new establishments that operate in the healthcare sector (NAICS code equal to 62) divided by the total population. We identify the creation of a new establishment considering the year reported by the establishment. | NETS |
| Healthcare Sales | The total amount of sales of establishments that operate in the healthcare sector (NAICS code equal to 62) divided by the total population | NETS |
| Alcohol Sales | The total amount of sales of beer, wine, and liquor stores (NAICS code equal to 445310) divided by the total population | NETS |
| Tobacco Sales | The total amount of sales for beer, wine, and liquor stores (NAICS code equal to 453991) divided by the total population | NETS |
| Total Inflows | The total number of individuals moving in the county (divided by 1,000) | IRS |
| Total Outflows | The total number of individuals moving outside the county (divided by 1,000) | IRS |
| **Household-Level Data** | | |
| Income | The natural logarithm of respondent's total pre-tax personal income | CPS |
| Wage | The natural logarithm of respondent's total pre-tax wage and salary income | CPS |
| Married | Dummy variable equal to one if the respondent is married | CPS |
| Black | Dummy variable equal to 1 if respondent's race is black | CPS |
| Female | Dummy variable equal to 1 if the respondent is a female | CPS |
| Private Insurance | Dummy variable equal to 1 if any private health insurance covers the individual (employer-sponsored or individually purchased) | CPS |
| Public Insurance | Dummy variable equal to 1 if the individual makes use of Medicare or Medicare health insurance | CPS |

**Notes**: The table describes each variable and its source.

**Excluded counties.** We show in Table OA3 the summary statistics of the main variables of interest for our final sample and for all the counties that we excluded from the analysis. We also show in the last column the normalized differences; according to Imbens and Wooldridge (2009), two variables are similar if this value is in the range of +/- 0.25. All the variables are measured as in 1994 with the exception of Deregulation (Maximum Value), which is the maximum value of the deregulation index during our period of analysis.

Table OA3: Descriptive statistics

| | Mean | SD | Mean | SD | ND |
|---|---|---|---|---|---|
| | Sample | | Out of Sample | | |
| Deregulation (Maximum Value) | 1.86 | 1.49 | 1.75 | 1.56 | 0.05 |
| Population | 10.33 | 1.27 | 9.57 | 2.77 | 0.25 |
| Population White | 10.14 | 1.45 | 8.04 | 2.26 | 0.78 |
| Population Black | 6.17 | 3.02 | 1.68 | 2.86 | 1.08 |
| Employment | 0.47 | 0.13 | 0.55 | 0.16 | -0.35 |
| Income | 15,544.71 | 3,549.79 | 16,545.69 | 3,804.26 | -0.19 |

**Notes**: The table shows the summary statistics of the main variables of interest for our final sample and for all the counties that we excluded from the analysis. It also shows in the last column the normalized differences. Deregulation (Maximum Value) is the maximum value the index assumes during the analyzed period. All the other variables are measured as in 1994.

**Establishments that operate in the healthcare industry.** We show in Table OA4 the number of establishment-year observations and the relative share by 4-digit NAICS code information for the establishments that operate in the healthcare sector in the United States during the period 1990-2005.

Table OA4: Health care sector

| | Freq. | Percent |
|---|---|---|
| Offices of Physicians (6211) | 4,780,575 | 30.26 |
| Offices of Dentists (6212) | 2,069,707 | 13.10 |
| Offices of Other Health Practitioners (6213) | 2,669,577 | 16.90 |
| Medical and Diagnostic Laboratories Outpatient Care Centers (6214) | 302,570 | 1.915 |
| Medical and Diagnostic Laboratories (6215) | 191,051 | 1.209 |
| Home Health Care Services (6216) | 238,937 | 1.512 |
| Other Ambulatory Health Care Services (6219) | 155,554 | 0.984 |
| General Medical and Surgical Hospitals(6221) | 155,982 | 0.987 |
| Psychiatric and Substance Abuse Hospitals (6222) | 79,858 | 0.505 |
| Specialty Hospitals (6223) | 100,351 | 0.635 |
| Nursing Care Facilities (6231) | 450,036 | 2.848 |
| Residential Disability, Mental Health, and Substance Abuse Facilities (6232) | 73,889 | 0.468 |
| Continuing Care Retirement Communities (6233) | 60,470 | 0.383 |
| Other Residential Care Facilities (6239) | 221,882 | 1.404 |
| Individual and Family Services (6241) | 1,860,451 | 11.77 |
| Community Food and Housing (6242) | 40,197 | 0.254 |
| Vocational Rehabilitation Services (6243) | 165,816 | 1.049 |
| Child Day Care Services (6244) | 2,183,752 | 13.82 |

**Notes**: The table shows the number of establishment-year observations and the percentage of the total by 4-digit NAICS code information for the establishments that operate in the healthcare sector in the United States during the period 1990 - 2005.

**Alternative types of private health insurance.** The Current Population Survey (CPS) provides information after 1996 on whether private health insurance has been individually purchased or whether the insurance has been employer-sponsored. We use these two variables as distinct dependent variables of interest in Equation (3) and report the results in Table OA5.

Table OA5: Health Insurance Type

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Variables | Employer-sponsored | Employer-sponsored | Individually Purchased | Individually Purchased |
| Deregulation | -0.0008 | -0.0007 | 0.0009 | 0.0006 |
| | (0.0015) | (0.0014) | (0.0011) | (0.0011) |
| Black | | -0.0665*** | | -0.0303*** |
| | | (0.0150) | | (0.0033) |
| Deregulation × Black | | -0.0005 | | 0.0028** |
| | | (0.0048) | | (0.0011) |
| State FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Individuals controls | Yes | Yes | Yes | Yes |
| Observations | 1,070,745 | 1,070,745 | 1,070,745 | 1,070,745 |
| R-squared | 0.0919 | 0.0936 | 0.0159 | 0.0166 |

**Notes**: The table shows estimation results from Equation (3). We use a dummy variable equal to one whether the insurance has been employer-sponsored or has been purchased privately by the individuals. All the regressions include state and year-fixed effects and controls for individuals' characteristics (a dummy variable equal to one if the individual is a female, a dummy variable equal to one if the individual is married, a dummy variable equal to one if the individual has a college degree, and controls for the age and the age squared of the individual). We cluster standard errors at the state level. ***, **, and * indicate significant coefficients at the 1%, 5%, and 10% levels, respectively. See Table OA2 for a detailed explanation of every variable.