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Banking Market Deregulation and Mortality Inequality *

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Abstract

This paper shows that 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 further analyze the role of mortgage markets as a transmitter between banking deregulation and mortality and show that households' easier access to finance explains mortality dynamics. We do not find any evidence that our results can be explained by improved labor outcomes.

Keywords: Banking Deregulation; Mortality; Racial Inequality

JEL codes: G21, I14, I15

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1 Introduction

Mortality rates in the United States vary substantially across regions (Finkelstein et al., 2021). Huge differences exist even considering the life expectancy of individuals located in the same area but with different socio-economic and demographic characteristics (Deaton and Lubotsky, 2003; Koskenvuo et al., 1986; Case and Deaton, 2021, 2015). Particularly relevant is the difference between black and white individuals. Despite a declining racial mortality gap during the last decades, black mortality rates have been higher than white mortality rates for every year in the United States history. The gap expanded again during the recent Covid-19 crisis (Wrigley-Field, 2020; Alsan et al., 2021).

Previous literature identifies several socio-economic factors that can explain spatial mortality differences and mortality inequality, such as income, income inequality, the supply and demand of healthcare, and crime (e.g., Deryugina and Molitor, 2018; Deaton, 2003; Snyder and Evans, 2006). Our paper contributes to this literature by analyzing the relationship between local banking market conditions and mortality patterns.

There are several reasons to think that well-developed banking markets could affect mortality rates and decrease mortality inequality. Banking development could increase access to finance for previously financially constrained individuals, allowing them to access goods that were inaccessible and incredibly important for their health, such as health insurance or housing. Also, it could allow previously constrained firms to access credit, positively affecting corporate investments and potentially leading to higher local employment and income, which could positively affect communities' well-being.

For identification, we exploit the Interstate Banking and Branching Efficiency Act (IBBEA) of 1994 as a positive shock to banking market development and analyze the effects on mortality rates based on detailed information on the universe of death certificates in the United States. The deregulation allowed states to gradually remove interstate bank branching restrictions, increasing competition and efficiency in the banking sector (e.g., Favara and Imbs, 2015; Krishnan et al., 2015; Berger et al., 2021).

Combining the staggered timing with the intensity of interstate branching deregulation across states in a differences-in-differences approach, we find that lifting banking restrictions through the IBBEA significantly reduced local mortality rates. Further analysis shows that this effect is driven by a decrease in the mortality rates of black individuals. 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, therefore, consistent with previous empirical evidence arguing that less regulated banking markets disproportionately benefit financially constrained individuals and minorities (Tewari, 2014; Beck et al., 2010; Levine et al., 2012).

Our analysis highlights financial development's crucial role in reducing mortality and mortality inequality in the United States. 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, counterfactual 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 augment our differences-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. In addition, our findings are unchanged after considering the recent advances in the econometrics literature arguing that differences-in-differences estimators can be severely biased when the treatment variable is heterogeneous between groups or over time (De Chaisemartin and d'Haultfoeuille, 2020; Baker et al., 2022).

Once we establish the relationship between banking deregulation and mortality patterns, we analyze the mechanism underlying our results. First, we consider whether households' access to the mortgage markets is a transmitter between banking deregulation and mortality. Access to the mortgage market and homeownership positively affects the living standards of individuals and communities (DiPasquale and Glaeser, 1999; Green and White, 1997). Moreover, mortgage access might expose households to beneficial neighborhood effects such as increasing education (Chetty et al., 2016; Bayer et al., 2008), less crime (Kling et al., 2005), or better health outcomes via pollution reduction (Currie et al., 2015, 2020). Unequal geography of opportunity, residential segregation, and socio-economic status are also 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; Woolf and Braveman, 2011; Christensen and Timmins, 2022).

To shed light on this mechanism, we instrument mortgage access with the IBBEA banking deregulation shock by following Favara and Imbs (2015). Using this approach, we first document that banking deregulation positively affects households' access to the mortgage market and that this effect is stronger for black individuals. Next, we show that easier access to mortgage finance due to the relaxation of branching restrictions affects local mortality patterns and narrows the racial mortality gap between 1990 and 2005.

A further channel on how banking market development can affect mortality is through labor markets effects on employment and income (e.g., Pierce and Schott, 2020; Aghion et al., 2016; Adda and Fawaz, 2020). 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.

In order to investigate this channel, we consider a large survey database with detailed information on households' demographic characteristics and labor outcomes. In our setting, we do not find any evidence that banking deregulation positively affects total wages or income. Also, we do not find differential effects between white and black individuals for these results. However, we do find a positive effect on the probability that black individuals get private health insurance after deregulation activities, further supporting the hypothesis that households' access to finance is key to our findings.

Our research contributes to the first strand of literature concerned with 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. (2021) 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 contributes to this strand by shifting the focus toward financial development in the U.S. and providing compelling evidence that competitive banking markets and households' access to finance affect mortality dynamics.

Our paper is also related to the strand of literature analyzing the impact of local credit conditions on economic and social outcomes. It has been shown that a well-developed financial market reduces 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). In this way, financial development decreases overall income inequality and the racial and gender wage gap (e.g., Beck et al., 2010; Levine et al., 2008; Delis et al., 2014; Black and Strahan, 2001). Our paper complements this strand of literature by providing evidence of the impact of financial development on communities' well-being and health disparities.

Finally, our findings shed new light on the relationship between geography and mortality in the United States (Couillard et al., 2021; Deryugina and Molitor, 2021). Previous literature identifies several factors that can explain regional differences in mortality rates and mortality inequality, such as unhealthy behaviors (smoking and drinking alcohol), income, income inequality, crime, and the supply and demand of health services (e.g., Deryugina and Molitor, 2018; Deaton, 2003; Snyder and Evans, 2006). Our paper contributes to the 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 and mortality inequality.

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 at the county level and a measure of mortality inequality between black and white residents. We merge the data 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. We also use 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.[1] Also, the database provides detailed information on the underlying cause of death, which allows us to compute mortality rate patterns for

[1]The age categories we consider in our analysis are less than one year, 1-4 years, 4-14 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.

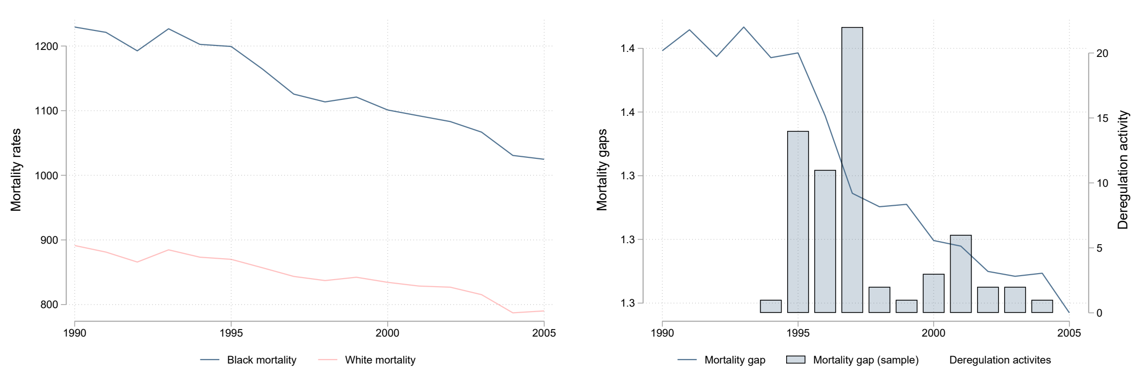
alternative causes of death, separately.

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. 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 age-adjusted mortality rate (per 100'000) trends 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 ratio, 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 decreases by ten percentage points from the early 1990s until 2005. Figure 1(b) also shows the sum of banking deregulation activities across all states (see also Table OA1), which took place between 1994 and 2004.

Figure 1: Mortality rates and deregulation activities



(a) Black and White Mortality Trends

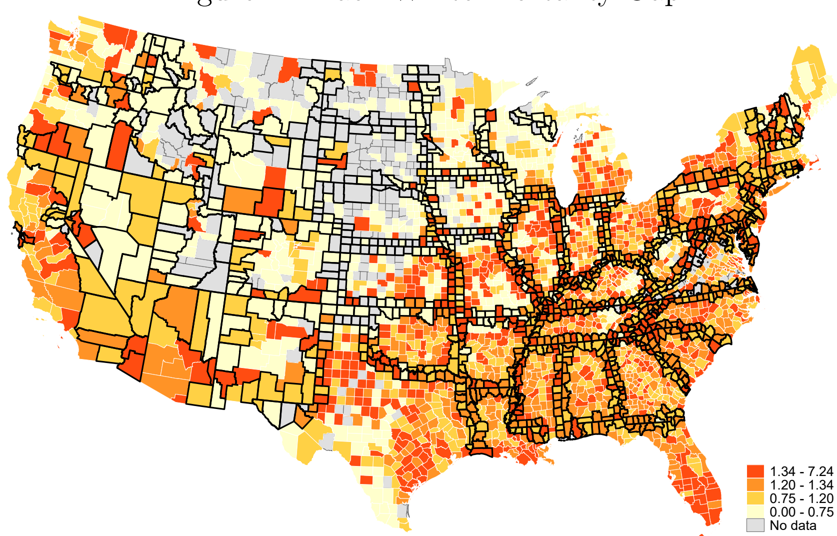
(b) Black-White Mortality Gap

Notes: Figure 1(a) shows the black and white age-adjusted mortality rates (per 100'000 population) trends in the United States for the period 1990-2005. Figure 1(b) shows the black and white age-adjusted mortality gap ratio trend in the United States from 1990-2005. We also plot the sum of deregulation activities across all states.

Our data allows us also to replicate findings from previous literature showing that mortality rates vary extensively across all states of the United States (e.g., Deryugina and Molitor, 2021; Finkelstein et al., 2021). A regressing of county mortality rates on interacted state-year fixed effects can explain around 34% of the variation in mortality rates.

Figure 2 shows the spatial distribution of the black-white (age-adjusted) mortality gap, defined as the ratio between black and white age-adjusted mortality rates in the county between 1990 and 2005. In 68% of the counties, the mortality rate for black individuals is higher than that of white individuals. We also highlight state-border counties with black lines around them since we will focus on this group of counties in an additional exercise.

Figure 2: Black-White Mortality Gap



Notes: This figure shows the black-white (age-adjusted) mortality gap spatial distribution across counties in the United States for the period 1990-2005. We highlight state-border counties with black lines around them.

Deregulation Index. Even though cross-state branching was fully legal after the passage of the Interstate Banking and Branching Efficiency Act (IBBEA) of 1994, states reserve the right to set regulations on interstate branching concerning 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. Third, 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. 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.

Rice and Strahan (2010) consider these four elements and build an index of interstate branching restrictions ranging from zero to four which is heavily used in the literature to investigate effects from banking deregulation (e.g., Favara and Imbs, 2015; Cornaggia et al., 2015). In our setting, four refers to the most deregulated states.

We report in Table OA1 for each state the timing of the deregulation, the set of regulations adopted, and the related index score. This setting allows us to exploit the heterogeneity in the timing and the extent of the deregulation across states. The right y-axis of Figures 1(b) shows the total number of deregulation activities by year as a consequence of the IBBEA.

Mortgage Market. To measure mortgage market access, we rely on the Home Mortgage Disclosure Act (HMDA) dataset. This database has already been used in other papers (D'Acunto and Rossi, 2022; Adelino et al., 2016; Favara and Imbs, 2015), and it provides annual mortgage application information on every newly issued mortgage loan from individual mortgage lenders to 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[2], 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

[2]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.

purchases or home refinancing loans per year.

We aggregate all accepted mortgage loans for each lending institution according to the purchased property's location, namely by county. We keep all loan purpose types[3], 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 which we draw from the Bureau of Labour Statistics. In our main specification, we control 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 that 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 overall, black, and white populations. The white population that died during this framing period is 44,769,425 (in 3,154 counties). The number of black individuals who died during this period is 6,251,770 (in 2,888 counties). We count the number of people who died by age category, county of residence, and year and merge population information for each group. We next collapse the data to create the age-adjusted mortality rates and drop the observations in which the population of interest equals 0.

We then merge information about deregulation, mortgage market acceptance rates, and the county control variables, remove singletons from our sample, and restrict our sample period to 1990-2005. Our final sample, thereby, has 2,757 counties with 36,351 observations. We report the summary statistics of the county characteristics in the top panel of Table 1 and provide detailed variable descriptions in Table OA2.

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

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 demographic characteristics (i.e., race, age, education, and marital status), labor outcomes (wages and income), and health insurance (whether the respondent holds private or public health insurance). A shortcoming of this database is that it does not allow us to follow the same families over the years and does not provide us with information on their mortgage situation.

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 from the sample households that move within the last year from another state or abroad. Our final database comprises 1,626,624 observations for the spanning period 1990-2005. We report the summary statistics for this sample in the bottom panel of Table 1 and provide detailed variable descriptions in Table OA2.

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 staggering banking deregulation wave of 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.

Figure 3 shows local mortality dynamics considering yearly point estimates before and after the first deregulation event. From the plot, we obtain several vital pieces of information. First, the absence of statistically significant pre-event point estimates suggests that the pre-trend assumption for our differences-in-differences approach holds. Second, we find a negative and statistically significant effect after the deregulation

Table 1: Descriptive statistics

	Mean	SD	25th	75th
County-level data (N=36,351; i=2,757)				
Mortality Rate	914.40	134.20	824.52	995.73
White Mortality Rate	894.39	129.25	811.97	968.24
Black Mortality Rate	1021.44	990.72	550.36	1276.58
Mortality Gap	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	22402.99	6717.48	17884.00	25561.00
Employment	0.49	0.14	0.40	0.57
Acceptance (all)	86.69	6.55	83.12	90.65
Acceptance (white)	88.32	6.11	85.22	91.92
Acceptance (black)	80.98	18.21	74.78	94.94
Household-level data (CPS) (N=1,626,624)				
Female	0.52	0.50	0.00	1.00
Age	38.42	13.31	28.00	49.00
Married	0.58	0.49	0.00	1.00
Income (log)	8.78	3.03	8.68	10.46
Wage (log)	7.25	4.38	0.00	10.31
Health Insurance	0.75	0.43	1.00	1.00

Notes: This table shows descriptive statistics for the samples we analyze. See Table OA2 for a detailed description of every variable.

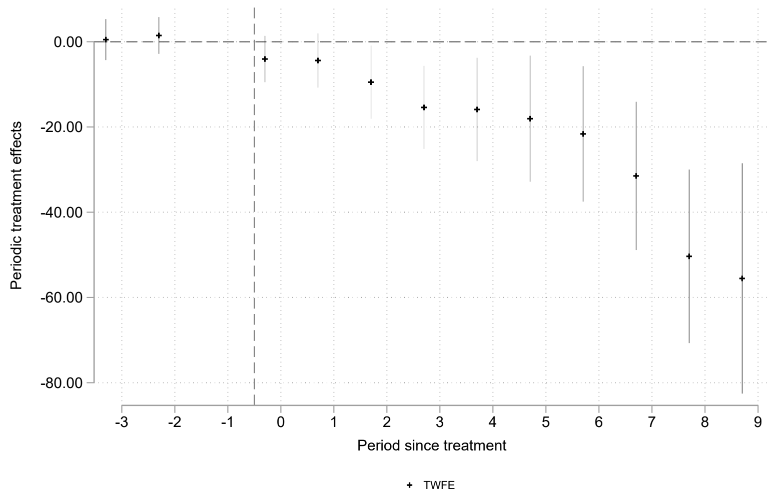
event on the mortality rates. The effect is also economically meaningful; more specifically, mortality rates decrease on average by 40 deaths per 100,000 inhabitants in the subsequent years.

Empirical Specification. In order to further analyze the relationship between banking deregulation and mortality, we estimate the following Equation (1):

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

Y 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.

Figure 3: Deregulation event and mortality



Notes: Figure 3 shows the dynamic effect of the first banking deregulation event on local mortality patterns. The graph shows yearly point estimates and 95 %-confidence intervals based on standard errors clustered by state. We obtain the estimates regressing county mortality rates on the leads and lags of the first deregulation event and controlling for county and year-fixed effects.

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. $X_{i,t}$ is a set of 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). η_i and θ_t are county and time-fixed effects, respectively.

We cluster standard errors on the state level. We further consider the possible heterogeneity in mortality responses across counties, estimating Equation (1) using weighted least squares. Following Favara and Imbs (2015), the weights used are the (inverse of) the number of counties per state. In this way, states with many counties are given more influence.

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). If deregulation increases by one unit, mortality rates decrease by 3.2 (3 individuals over 100,000 population). When we consider the mortality rates of white individuals as the outcome variable in Column (2), we estimate a negative coefficient equal to 1.7. However, the standard errors for this point estimate are higher than the coefficient; we thereby refrain from stating a significant effect of deregulation on white mortality. However, when we turn at the mortality rates of black residents in Column (3), we find an adverse, economically meaningful, and statistically significant effect of banking deregulation. More specifically, a one-unit increase in the deregulation index decreases black mortality by 41 black residents over 100,000 black population.

Our results of the first three columns of Table 2 suggest that the deregulation activities of the 1990s decrease black-white mortality inequality. As shown in Figures 1(b), banking deregulation activities coincided when the black-white mortality gap decreased from 1994-2005. In order to further investigate this hypothesis, we estimate Equation (1) using the black-white mortality gap ratio as the outcome variable 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 0.047, 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	-3.1835*	-1.7283	-41.6667***	-0.0472***
	(1.6702)	(2.2746)	(11.5026)	(0.0150)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes
Observations	36,351	36,351	36,351	36,351
R-squared	0.7433	0.6172	0.2188	0.2063

Notes: This table shows estimation results from Equation (1). In each column, we use a different dependent variable: Mortality Rate, White Mortality Rate, Black Mortality Rate, and the Mortality Gap. We use the inverse rate of all counties per state to weigh our regressions. See Table OA2 for a detailed explanation of every variable we use and Table 1 for more summary statistics. We cluster standard errors at the state level. ***, **, and * indicate significant coefficients at the 1%, 5%, and 10% levels, respectively.

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 missed reduction in the mortality gap since not all states 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 and the difference between the baseline and the counterfactual regressions' gap multiplied by the β of the counterfactual regression.

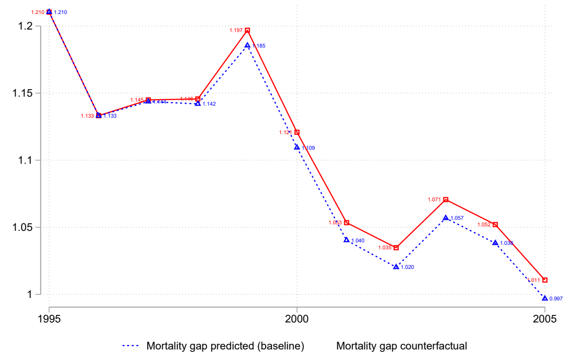
We report our findings in Figure 4. The left graph shows the differences between the predicted mortality gap from the baseline regression and the counterfactual gap for every year. On the right, we show the missed reduction in the mortality gap and the cumulative potential excess reduction in %.

Overall, our 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 deregulation period) and highlight the importance of the banking sector in shaping mortality patterns.

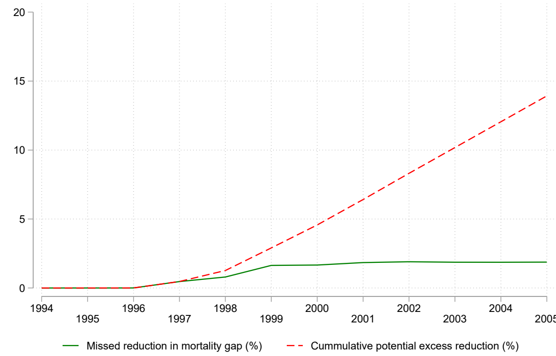
4 Robustness checks

We undertake several analyses to check the robustness of our results. First, we consider a recent strand of the literature arguing that differences-in-differences estimators can be severely biased when the treatment variable is heterogeneous between groups and over time (De Chaisemartin and d'Haultfoeuille, 2020; Baker et al., 2022). Next, we also consider that state-level factors that exhibit differently across states could have affected the timing of deregulation (Kroszner and Strahan, 1999). In order to ensure that omitted variables are not affecting our results, we combine our differences-in-differences strategy with alternative border discontinuity approaches. Also, we show

Figure 4: Simulation



(a) Counterfactual



(b) Potential Reduction

Notes: Figure 4 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 mortality gap from the baseline regression and the counterfactual gap. On the right, we show the missed reduction in the mortality gap and the cumulative potential excess reduction for every year in %.

that other sources of endogeneity are unlikely to affect our estimation results.

Heterogeneous treatments. A recent strand of the literature in econometrics argues that differences-in-differences estimators can be severely biased when the treatment variable is heterogeneous between groups and over time (De Chaisemartin and d'Haultfoeuille, 2020; Baker et al., 2022). The bias arises mainly because some groups of observations could have "negative weights", leading to misleading estimates of the actual average treatment effect (ATE).

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 OA1 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 OA1 shows that the effects are consistent across the alternative estimators, suggesting that negative weights are not invalidating our results. In line with our baseline estimates, the effect is much 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.

Border discontinuity approach. To ensure that omitted variable characteristics do not affect our results, we follow Huang (2008) and combine our differences-in-differences approach with a border discontinuity design. The purpose of this exercise is to further test the robustness of our results through a comparison of 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 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.[4]

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 OA2. Estimated results support the hypothesis that our findings are not sensitive to the sample of analysis we use and suggest that omitted variables are not affecting our findings.

Outliers. We test whether outliers affect our results. To do so, we report added variable plots in Figure OA3 for each outcome variable of interest. The plots allow

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

us to show the influence of the deregulation on each outcome while accounting for the influence of all the other control variables. We find no evidence that a subset of observations drives the adverse effect of deregulation on mortality. Furthermore, Figure OA4 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. First, 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. Second, measurement errors could also arise in counties with fewer deceased individuals.

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

Results showing the effect of full deregulation are reported in Figure OA5. 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. We further test whether our results depend on the weighting we adopted to estimate Equation (1). Figure OA6 shows that our results hold when we use weights coming from (the inverse of) county population and when we do not use any weight at all.

Additional control variables. We test the robustness of our results by considering additional control variables to include in our preferred specification reported in Equation (1).

Kroszner and Strahan (1999) show that the banking deregulation process is affected by political institutions and banks' private interests. For this reason, we consider political institutions in our regressions using a dummy variable equal to one if the state governor is a Democrat. We also control for the competition in the mortgage market using a Herfindahl–Hirschman Index (HHI) based on the number of mortgages issued by each branch. Following Dow et al. (2020), we further consider differences in local economic characteristics and include current transfers per person in the model.[5] Finally, to control for neighboring differences between white and black communities, we control for the share of black people.

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 OA7. 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.

Alternative causes of death. We analyze separately alternative causes of death to understand whether a specific cause drives our main findings. Furthermore, we want to test whether the impact of banking deregulation was asymmetric across alternative causes of death. On this point, Ruhm (2000) finds that recessions have an adverse effect across several types of mortality rates but have a positive effect on suicide rates.

To shed light on this critical issue, 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 for alternative samples in Table OA3.

Column (1) of Panel A shows that banking deregulation decreased the black-white mortality gap for deaths from circulatory diseases (Circulatory). The coefficient is negative, statistically significant, and equal to 0.08. The estimated coefficient is even greater if we consider only the counties on the state borders, as reported in Panel B

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

and Panel C of the table.

We find similar results for other causes of death driving racial mortality inequality in the United States. More specifically, we show in Column (2) that deregulation decreased the black-white mortality gap for deaths caused by cancer (Cancer). The coefficient of interest is negative, statistically significant, and equal to 0.06. Again this effect is more remarkable if we consider only the counties on the state borders. This result is also in line with Kopytov et al. (2021). They show that an increase in borrowing limit positively affects the longevity of cancer patients.

Also, we find a similar effect when considering a broader category and analyzing mortality patterns for internal causes of death (Internals).[6] In this specification, the estimated coefficient is equal to 0.04.

Finally, we analyze the impact of deregulation on the black-white mortality gap for deaths caused by alcohol-related diseases (Alcoholic) or external causes of death (External).[7] We report the coefficients for these outcome variables in Column (3) and Column (4). The coefficients of interest are still negative, even if the coefficient related to the effect of deregulation on external causes of death is not statistically significant. Lu and Stabile (2020) find opposite results in a similar setting; they show that payday lending bans lead to an overall decrease in suicides and fatal drug overdose deaths.

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

[6] An internal cause of death is defined as one that occurs due to an internal factor that causes the body to shut down.

[7] An external cause of death is one due to accidents and violence, including environmental events, circumstances, and conditions, such as injury, poisoning, and other adverse effects.

5 Mechanisms

5.1 Households' access to mortgage finance

This subsection analyzes a specific mechanism through which banking deregulation might affect mortality dynamics: households' access to finance. Indeed, banking competition could allow previously financially constrained individuals to have access to finance and access goods that were inaccessible and critical for their health.

To test this hypothesis, we focus on the mortgage market, the most important source of debt for households. More specifically, 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 are 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; Woolf and Braveman, 2011; Christensen and Timmins, 2022).

We investigate this hypothesis using an instrumental variable approach. In the vein of Favara and Imbs (2015), we use banking deregulation as an instrument for a positive shock to credit supply and mortgage access. The idea behind this instrument is that it affects mortgage access via increased entry into the mortgage market but, at the same time, is orthogonal to mortality rates.[8]

[8] The imposed homogeneity of the instrument should reduce concerns of omitted variable bias and reverse causality. One objection might be that bank lobbyists or consumer protectionists anticipate increasing mortgage demand and motivate politicians to deregulate (Kroszner and Strahan, 1999; Rice and Strahan, 2010). The causal chain would run the other way: mortgage demand would trigger deregulation. However, these hypotheses seem unlikely since Favara and Imbs (2015) show that unaffected banks, due to their legal status, such as independent mortgage lenders, thrifts, or credit unions, should have taken advantage of such a hypothetical boom as well as depository institutions. In fact, they did not.

First stage. The first stage of the Two-Stage-Least-Squares IV-estimation model is reported in the following Equation (2).

$$M_{i,t} = \lambda_i + \gamma_t + \beta_1 \text{Deregulation}_{s,t-1} + \Gamma X_{i,t} + \epsilon_{i,t} \tag{2}$$

M 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 the time (year) t. We compute this variable alternatively for black, white, and all the individuals. Deregulation is the reverse deregulation index from Rice and Strahan (2010). It ranges between zero (most restrictive states) and four (least restrictive states). X are time-varying county controls like population (log), income per capita, and employment rate.

The first stage results from Equation (2) in Table 3 indicate that the deregulation index positively 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. Also, the first stage F-statistic on the excluded instruments yields values above the recommended F-statistic of 10 in most specifications and diminishes worries about weak instruments. These results show that banking deregulation is highly predictive of mortgage access.

Table 3: First Stage

Dependent variable first stage:	Acceptance Rates		
	All	White	Black
	(1)	(2)	(3)
Panel A: all counties			
Deregulation	0.4415***	0.5147***	0.7676***
	(0.1436)	(0.1377)	(0.2565)
Observations	36,351	36,351	36,351
R-squared	0.5520	0.5055	0.2008
Panel B: counties on the border			
Deregulation	0.5975***	0.6891***	0.9078***
	(0.1509)	(0.1568)	(0.2414)
Observations	12,883	12,883	12,883
R-squared	0.5636	0.5213	0.1981
Panel C: border counties and distance less than 75km			
Deregulation	0.5584***	0.6313***	0.8593***
	(0.1766)	(0.1619)	(0.2449)
Observations	11,537	11,537	11,537
R-squared	0.5422	0.5085	0.1949

Notes: This table shows estimation results from Equation (2). We use a different dependent variable in each column: Acceptance Rate, White Acceptance Rate, and Black Acceptance Rate. 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 rate of all counties per state to weigh our regressions. See Table OA2 for a detailed explanation of every variable we use and Table 1 for more summary statistics. We cluster standard errors at the state level. ***, **, and * indicate significant coefficients at the 1%, 5%, and 10% levels, respectively.

Second stage. The second stage regression in our analysis is reported in Equation (3).

$$Y_{i,t} = \lambda_i + \gamma_t + \delta_1 \hat{M}_{i,t} + \Gamma X_{i,t} + \varepsilon_{i,t} \tag{3}$$

Y is again the short-hand for the various age-adjusted mortality rates and the gaps in county i at time t. As in Table 2, we alternate between overall black and white mortality rates and the black-white mortality gap. $\hat{M}$ is the predicted values of mortgage acceptance rates from the first stage. Further, we include the same time-varying control variables as the first stage and year plus county fixed effects. We cluster standard errors at the state level.

Consistent with our previous findings, the first column of Table 4 shows that an exogenous increase in access to the mortgage market negatively affects overall mortality rates. More specifically, the coefficient estimated in our main specification shows that an exogenous increase of one unit in the overall acceptance rate decreases overall mortality rates by 7 units.

In line with our previous findings, Column (2) and Column (3) reveal that the deregulation-induced variation of mortgage access has substantial explanatory power for black mortality dynamics. At the same time, it does not affect white mortality rates. Moreover, Column (4) documents a decrease in the black-white mortality gap when there is an exogenous variation in access to the mortgage market for black applicants. More specifically, we find that for each increase in mortgage acceptance rates for black individuals of 1 percentage point, black mortality rates significantly drop by 54, and the gap reduces by 0.06.

Supposing black acceptance rates increased by 3.8, as estimated in fully deregulated areas in our previous table, led to a reduction of black mortality rates by $54*3.8 = 205.2$. The same calculation made the mortality gap shrink by $0.06 * 3.8 = 0.23$, which is a sizeable effect in the pre-deregulation gap of 1.3.

Our results are still consistent when we focus on counties located on the state borders, as reported in Panel B and C. In terms of magnitude, the estimated coefficients are within one standard deviation of our baseline coefficients. In this set of results, the coefficients related to the white mortality rates also turn out to be negative and statistically significant; however, these are still much smaller concerning the estimated coefficients for the black mortality rates.

Table 4: IV

Dependent variable second stage:	Mortality Rate	White Mortality Rate	Black Mortality Rate	Mortality Gap
	(1)	(2)	(3)	(4)
Panel A: all counties				
Acceptance (all)	-7.210**			
	(3.429)			
Acceptance (white)		-3.358		
		(4.085)		
Acceptance (black)			-54.280**	-0.062**
			(21.789)	(0.027)
Observations	36,351	36,351	36,351	36,351
Kleibergen-Paap (p)	0.01	0.00	0.01	0.01
Kleibergen-Paap F	9.46	13.97	8.96	8.96
Panel B: border counties				
Acceptance (all)	-8.595**			
	(3.534)			
Acceptance (white)		-6.935**		
		(2.818)		
Acceptance (black)			-51.079***	-0.049**
			(16.650)	(0.020)
Observations	12,883	12,883	12,883	12,883
Kleibergen-Paap (p)	0.00	0.00	0.00	0.00
Kleibergen-Paap F	15.68	19.31	14.14	14.14
Panel C: border counties and distance less than 75km				
Acceptance (all)	-8.241**			
	(3.256)			
Acceptance (white)		-6.604**		
		(2.730)		
Acceptance (black)			-52.358***	-0.051**
			(17.912)	(0.019)
Observations	11,537	11,537	11,537	11,537
Kleibergen-Paap (p)	0.01	0.00	0.01	0.01
Kleibergen-Paap F	9.99	15.20	12.31	12.31

Notes: This table shows estimation results from Equation (3. In each column, we use a different dependent variable: Mortality Rate, White Mortality Rate, Black Mortality Rate, 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 rate of all counties per state to weigh our regressions. See Table OA2 for a detailed explanation of every variable we use and Table 1 for more summary statistics. We cluster standard errors at the state level. ***, **, and * indicate significant coefficients at the 1%, 5%, and 10% levels, respectively.

5.2 Alternative explanations.

Since local labor markets can influence individuals' well-being and mortality patterns (e.g., Pierce and Schott, 2020; Aghion et al., 2016; Adda and Fawaz, 2020), a mutually non-exclusive channel through which competitive credit markets can affect mortality patterns is through their effect on employment and income. Development of the banking sector has been shown to lift corporations' credit constraints, increasing corporate investments and, therefore, potentially allowing better employment opportunities in the community.

In order to investigate this channel, we consider a large cross-sectional survey database with detailed information on households' demographic characteristics and labor outcomes. We then estimate the following Equation (4):

$$Outcome_{j,t} = \beta Deregulation_{s,t-1} + \Gamma X_{j,t} + \eta_s + \theta_t + \epsilon_{i,t} \tag{4}$$

In this setting, $Outcome$ is a set of outcome variables for individual j at time t. We use (the natural logarithm of total) $Income$, (the natural logarithm of total) $Wages$, and $Health\ Insurance$ (a dummy variable equal to one if the respondent holds private health insurance). X is a set of control for household characteristics (a dummy variable equal to one if the respondent is a female, a dummy variable equal to one if the respondent is married, a dummy variable equal to one if the respondent has a college degree, and birth cohort fixed effects). η_s and θ_t are state and year fixed effects. Also, we cluster the standard errors at the state level.

We report the results in Table 5. We do not find any evidence that banking deregulation improved the total income or the wages of individuals. Also, as reported in Panel B, we do not find for these outcome variables a differential effect for black individuals ($Black$ is a dummy variable equal to 1 if the respondent's race is black). However, estimation results support findings from previous literature that black individuals have lower incomes and earn lower wages (Johnson et al., 2000).

In the last column, we consider as outcome variable of interest access to private health insurance. Insurance expansion is indeed a crucial factor able to reduce disparities in access to health care (Lillie-Blanton and Hoffman, 2005). In this sense, estimation results provide interesting insights. Black individuals are 12% less likely to have private health insurance; however, this probability increases after deregulation by 1% for each unit increase in the deregulation index. This result further supports the hypothesis that households' access to finance is key to our findings.

Table 5: Alternative Explanations

	(1)	(2)	(3)
Panel A: Deregulation and households' outcomes			
Variables	Income	Wage	Health Insurance
Deregulation	0.0076	-0.0013	-0.0014
	(0.0074)	(0.0104)	(0.0017)
Observations	1,626,624	1,626,624	1,626,624
R-squared	0.2557	0.1583	0.1006
Panel B: Deregulation, households' outcomes, and race			
Deregulation	0.0071	-0.0052	-0.0025
	(0.0082)	(0.0120)	(0.0017)
Black	-0.3739***	-0.4884***	-0.1199***
	(0.0504)	(0.0777)	(0.0128)
Deregulation × Black	0.0089	0.0364	0.0100***
	(0.0126)	(0.0220)	(0.0033)
Observations	1,626,624	1,626,624	1,626,624
R-squared	0.2569	0.1591	0.1058

Notes: This table shows estimation results from Equation (4). In each column, we use a different dependent variable: Income, Wage, and Health Insurance. All the regressions include state and year fixed effects and controls for individuals' characteristics. See Table OA2 for a detailed explanation of every variable we use and Table 1 for more summary statistics. We cluster standard errors at the state level. ***, **, and * indicate significant coefficients at the 1%, 5%, and 10% levels, respectively.

6 Conclusions

The value of increased longevity in the last hundred years is as immense as the value of growth in non-health goods and services (Nordhaus, 2010). For this reason, understanding the determinants of mortality rate patterns is a crucial issue. This paper analyses the role of local banking market conditions in mortality dynamics in the United States.

Using alternative identification strategies and exploiting the timing and the extent of banking deregulation across states in the United States, we find that banking deregulation significantly decreases mortality. We show that our results are driven by a decrease in the black mortality rates and that deregulation played a crucial role in decreasing the racial mortality gap during our analysis period.

Further results suggest that the effect of banking deregulation on mortality works through households' access to finance. We find that deregulation significantly improves mortgage acceptance rates for all individuals, but this effect is more substantial for black individuals. We show that more accessible mortgage loans significantly reduce mortality and close the gap between black and white mortality rates. We also find that deregulation activities increase the probability that black individuals get private health insurance, further supporting the hypothesis that households' access to finance is key. On the other side, we do not find any evidence that our results can be explained by improved labor outcomes.

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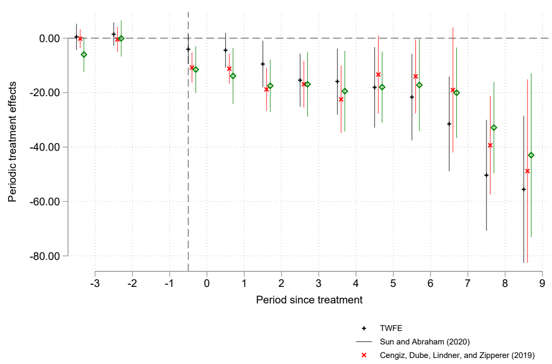
Online Appendix

This Appendix is for Online Publication and provides further details on the data and the results of the paper "Banking Market Deregulation and Mortality Inequality".

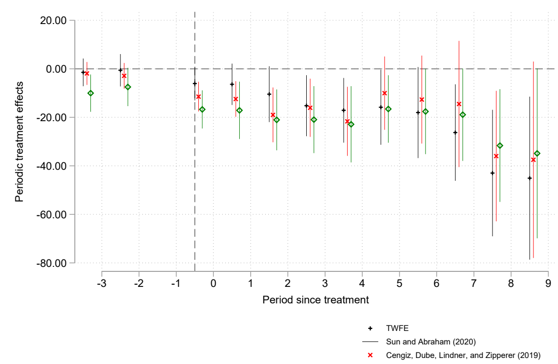
OA1 Figures and Tables

Differences-in-differences bias. We consider as the event the first deregulation period and plot the yearly point estimates for our outcomes of interest in Figure OA1 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).

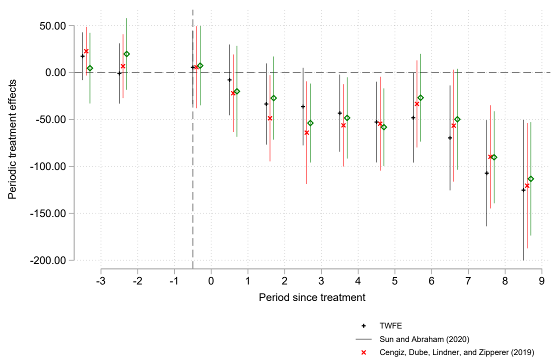
Figure OA1: Banking Deregulation and Mortality Rates: Dynamic Effects



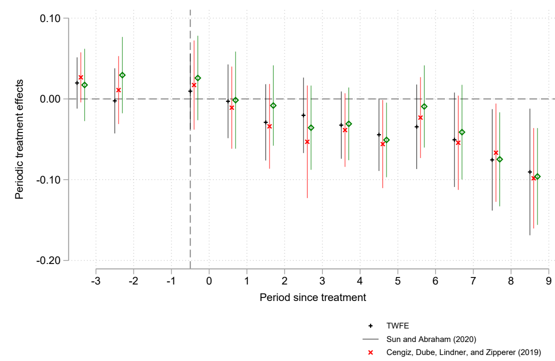
(a) Overall Mortality Rates



(b) White Mortality Rates



(c) Black Mortality Rates



(d) Mortality Gap

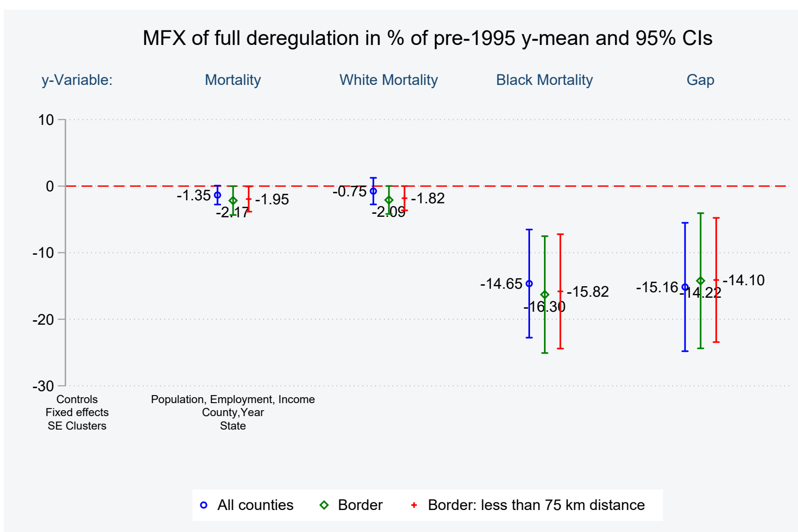
Notes: The figure shows the dynamic effect of banking deregulation on the white and black mortality rates and the black-white mortality gap ratio using alternative estimation approaches (Cengiz et al., 2019; Sun and Abraham, 2021). The graphs show point estimates and 95 %-confidence intervals based on standard errors clustered by state.

Alternative Samples. We follow Huang (2008) and combine our differences-in-differences approach with a border discontinuity design. The purpose of this exercise is to further test the robustness of our results through a comparison of 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 OA2.

Figure OA2: Samples

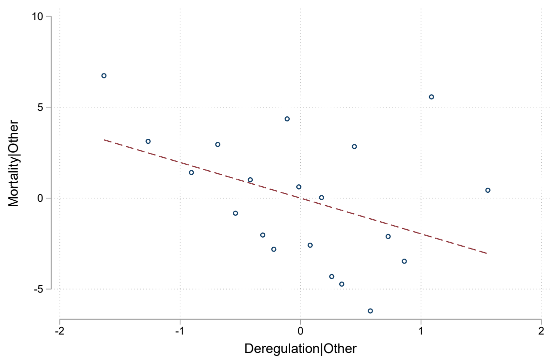


Notes: Figure OA2 shows the effect of fully deregulation in % of pre-1995 y-mean on the alternative outcome variables used in Equation (1) for alternative samples.

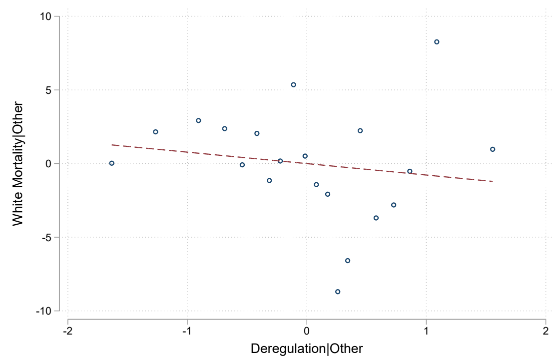
Added Variable Plot. We want to test whether outliers drive our results. We analyze this issue graphically and report the added variable plots in Figure OA3 for our main outcome variables.

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 OA3: Added variable plots

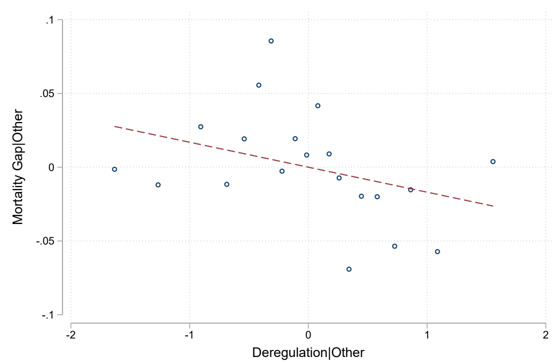


(a) Mortality Rate



(b) White Mortality Rate

(c) Black Mortality Rate

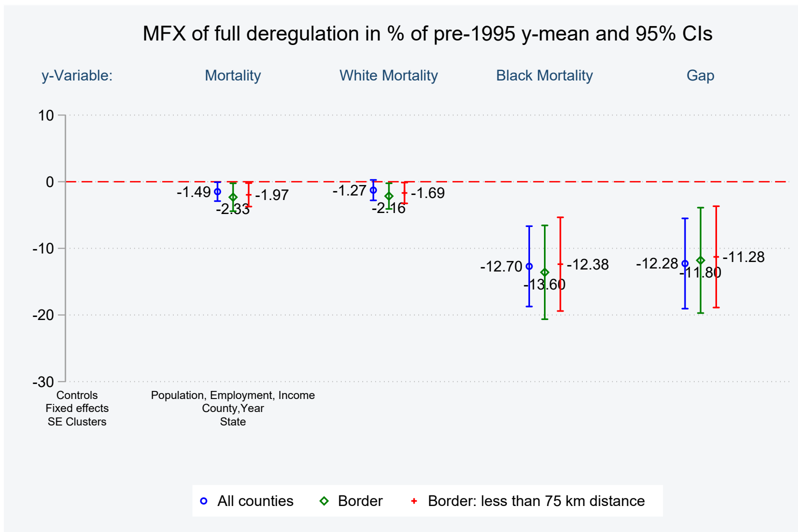


(d) Mortality Gap

Notes: Figure OA3 shows binned added variable plots for each of our four dependent variables. We control for year and county fixed effects and county characteristics (population, local income per capita, and employment share).

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 OA4 shows the effect of full deregulation on the alternative outcome variables proposed in Equation (1).

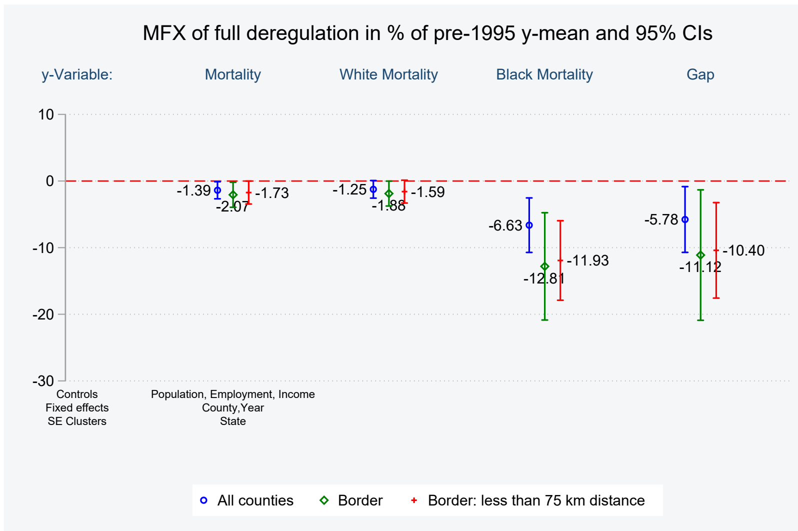
Figure OA4: Outliers



Notes: Figure OA4 shows the effect of full deregulation in % of pre-1995 y-mean on alternative outcome variables used in Equation (1) when we winsorize all the variables at the first and last percentiles.

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 OA5 shows the effect of complete deregulation on the alternative outcome variables used in Equation (1) when we propose this approach.

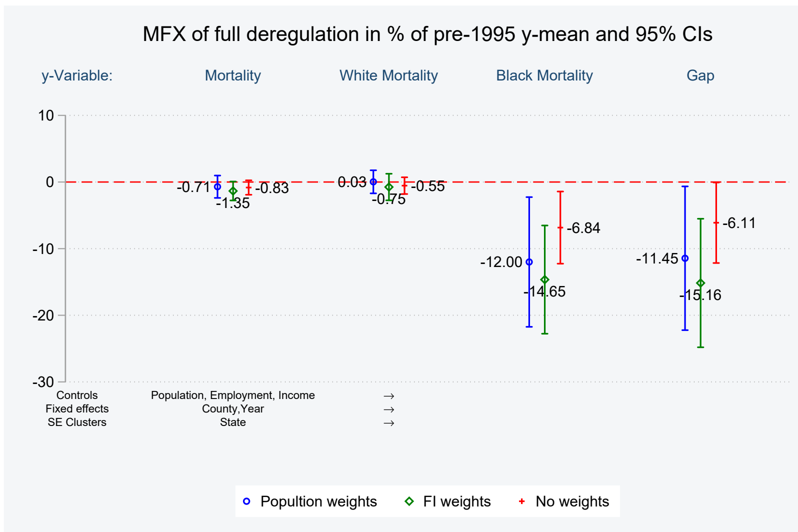
Figure OA5: Outliers



Notes: Figure OA4 shows the effect of full deregulation in % of pre-1995 y-mean on alternative outcome variables used in Equation (1) when we consider a three-year average of the outcome variables to deal with measurement errors.

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 OA6 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.

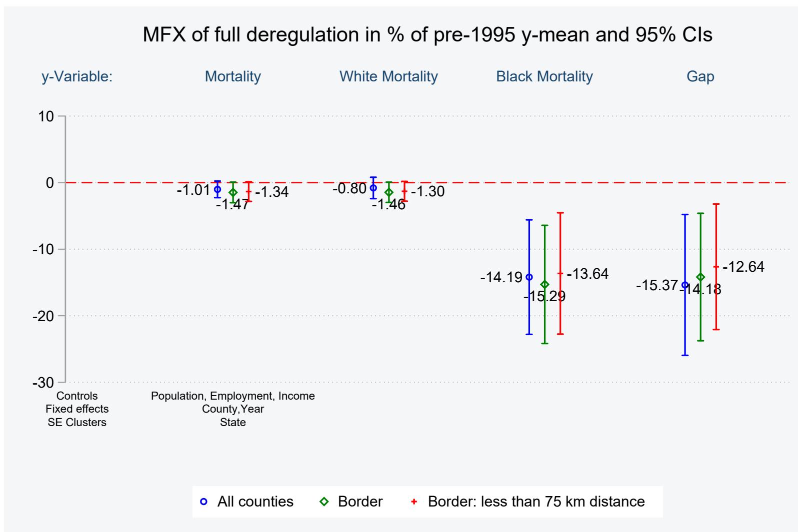
Figure OA6: Weights



Notes: Figure OA6 shows the effect of full deregulation in % of pre-1995 y-mean on alternative outcome variables used in Equation (1) when we consider alternative ways to weight the regressions reported in Equation (1).

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. We also consider the competition in the mortgage market using an HHI based on the number of mortgages issued by each branch. 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 OA7.

Figure OA7: Additional controls



Notes: Figure OA7 shows the effect of full deregulation in % of pre-1995 y-mean on alternative outcome variables used in Equation (1) 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).

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. In our setting, four refers to the most deregulated states. 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.

Table OA1: Deregulation Timing (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
HHI	The Herfindahl-Hirschman Index (HHI) based on the number of mortgages issued by each branch	Summary of Deposits (SOD)
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: This table describes each variable and its source.

Table OA2: Variable description cont'd

Variable name	Description	Source
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
Marital Status	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
Health Insurance	Dummy variable equal to 1 if any private health insurance covers the individual (employer-sponsored or individually purchased)	CPS

Notes: This table describes each variable and its source.

Alternative causes of death. We analyze separately alternative causes of death to understand whether a specific cause drives our main finding. Furthermore, we want to test whether the impact of banking deregulation was asymmetric across alternative causes of death. On this point, Ruhm (2000) finds that recessions have an adverse effect across several types of mortality rates but have a positive effect on suicide rates. To do so, we estimate Equation (1) using as an outcome variable the mortality gap from circulatory diseases, cancer, internal causes of death, alcohol-related diseases, or other external causes of death. We report estimation results in Table OA3.

Table OA3: Alternative causes of deaths

Variables	(1)	(2)	(3)	(4)	(5)
		Mortality gap			
	Circulatory	Cancer	Internal	Alcoholic	External
Panel A: all sample					
Deregulation	-0.0806***	-0.0575**	-0.0422***	-1.1418*	-0.0768
	(0.0280)	(0.0235)	(0.0135)	(0.6469)	(0.0840)
Average Outcome	0.92	1.21	1.14	1.93	1.11
Observations	31,099	33,935	36,202	15,264	31,386
R-squared	0.0807	0.1372	0.1960	0.1508	0.1173
Panel B: border					
Deregulation	-0.1538**	-0.0913**	-0.0428**	-1.1348	-0.0964
	(0.0622)	(0.0341)	(0.0200)	(0.8796)	(0.1210)
Average Outcome	0.94	1.23	1.14	2.00	1.14
Observations	10,903	11,853	12,810	5,748	11,005
R-squared	0.0771	0.1533	0.1832	0.1355	0.1195
Panel C: border and distance less than 75					
Deregulation	-0.1404**	-0.0868**	-0.0456**	-0.3858***	-0.0834
	(0.0602)	(0.0344)	(0.0186)	(0.0921)	(0.1072)
Average Outcome	0.94	1.25	1.19	1.80	1.11
Observations	9,923	10,782	11,489	5,290	9,979
R-squared	0.0748	0.1742	0.1980	0.0957	0.0871

Notes: This table shows estimation results from Equation (1). In each column, we use a different dependent variable: Circulatory, Cancer, Internal, Alcoholic, and External. 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 rate of all counties per state to weigh our regressions. See Table OA2 for a detailed explanation of every variable we use and Table 1 for more summary statistics. We cluster standard errors at the state level. ***, **, and * indicate significant coefficients at the 1%, 5%, and 10% levels, respectively.

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