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Abstract

In July 2005, a set of cuts to Medicaid eligibility and coverage went into effect in the state of Missouri. These cuts resulted in the elimination of the Medical Assistance for Workers with Disabilities program, more stringent eligibility requirements, and less generous Medicaid coverage for those who retained their eligibility. Overall, these cuts removed about 100,000 Missourians from the program and reduced the value of the insurance for the remaining enrollees. Using data from the Medical Expenditure Panel Survey, we show how these cuts increased out-of-pocket medical spending for individuals living in Missouri. Using data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP) and employing a border discontinuity differences-in-differences empirical strategy, we show that the Medicaid reform led to increases in both credit card borrowing and debt in third-party collections. When comparing our results with the broader literature on Medicaid and consumer finance, which has generally measured the effects of Medicaid expansions rather than cuts, our results suggest there are important asymmetries in the financial effects of shrinking a public health insurance program when compared with a public health insurance expansion.

Keywords: Medicaid, health insurance, consumer credit
JEL Codes: I13, I18, G51, G52

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1 Introduction

What is the value of Medicaid, and how do Medicaid recipients react to changes in the program? These questions have increased in importance as many states have expanded their programs in response to the Affordable Care Act (ACA), while others consider new forms of Medicaid reforms and restrictions such as work requirements. Since Medicaid's origin in 1965, the general trend has been to expand eligibility. The program originally tied Medicaid eligibility to eligibility for cash assistance (Aid for Families with Dependent Children), but it has since expanded to cover some disabilities, long-term care, and low-income individuals without children. Because of these expansions, potential Medicaid recipients have generally opted to take advantage of their eligibility, which has led to enrollment increasing from 4 million individuals in 1966 to 73.8 million in 2017.[1]

The body of research on the effects of Medicaid expansions is extensive and has covered a wide variety of outcomes, including health, employment, provider behavior, and consumer financial health. This research on financial outcomes has generally found that individuals who received coverage (or were eligible for coverage) received substantial financial benefits, including reduced out-of-pocket medical spending (Finkelstein et al., 2012; Baicker et al., 2013), reduced medical debt in collections (Finkelstein et al., 2012; Hu, Kaestner, Mazumder, Miller, and Wong, 2018; Miller, Hu, Kaestner, Mazumder, and Wong, 2020; Brevoort, Grodzicki, and Hackmann, 2020), higher credit scores (Miller et al., 2018; Brevoort et al., 2020) and a lower likelihood of bankruptcy (Gross and Notowidigdo, 2011). These financial benefits to recipients are unsurprising, given that Medicaid coverage is relatively generous as health insurance, covering most medical services with low to zero premiums and cost sharing.

While this previous research is well identified and provides estimates of the financial effects of recent Medicaid *expansions*, it does not necessarily indicate what the effects of future Medicaid program *contractions* would be. Expansions and contractions to a public program could have asymmetric effects along a number of dimensions, and research on

[1] In terms of overall population, Medicaid went from covering 2 percent of all Americans in 1968 to 22.6 percent by 2017. For more information, see https://www.macpac.gov/publication/medicaid-enrollment-and-total-spending-levels-and-annual-growth/.

Tennessee's 2005 Medicaid reform suggests this to be the case for Medicaid for general hospitalizations (Ghosh and Simon, 2015), behavioral health-care hospitalizations (Maclean, Tello-Trillo, and Webber, 2020), and financial well-being (Argys, Friedson, Pitts, and Tello-Trillo, 2020). Understanding the impacts of program contractions are of particular interest, given the current policy landscape as recent proposals for Medicaid program reforms most commonly discussed by states are not just simple reversals of the recent expansions of eligibility to low-income adults. Instead, states have been proposing either new forms of eligibility requirements, such as work requirements or more frequent income verification, or making their programs less generous to recipients by introducing or increasing premiums, deductibles, and copays or removing coverage for certain types of services.[2]

To examine the effects of a public insurance program contraction on consumer financial outcomes, we study the effect of a major reform to Missouri's Medicaid program in 2005. This reform resulted in approximately 100,000 Missourians losing their Medicaid eligibility and lower benefit generosity for the remaining enrollees. Unlike Tennessee's Medicaid reform, in which the majority of the changes were centered on the disenrollment of a specific subpopulation from the program, Missouri's reform was much broader in scope.

In the first half of the paper, we show that the contraction of Missouri's Medicaid program led to lower Medicaid enrollment and health-care spending. Using data from the restricted version of the Medical Expenditure Panel Survey (MEPS), we first estimate that the probability of an individual in Missouri being on Medicaid declined by 4 percentage points, and the uninsured rate increased by 2 percentage points in the years following the reform. These results are similar to those found by Zuckerman, Miller, and Pape (2009), who estimated that the uninsured rate in Missouri increased by 1.7 percentage points following the reform. We also find that the Medicaid cut led to a 30 percent increase in out-of-pocket (OOP) medical spending and an 18 percent decrease in Medicaid spending for individuals living in Missouri.

[2] Reforms to a state's Medicaid program are done via Section 1115 demonstrations (also known as Section 1115 waivers), which are approved by the U.S. Department of Health and Human Services (HHS). The waivers are intended to give states additional flexibility in designing their Medicaid programs. For more information, see https://www.medicaid.gov/medicaid/section-1115-demonstrations/about-section-1115-demonstrations/index.html.

These estimates are consistent with previous studies that have shown that Medicare *expansions* generally *decrease* out-of-pocket expenses by 28 percent to 33 percent (Blavin, Karpman, Kenney, and Sommers, 2018; Gotanda, Jha, Kominski, and Tsugawa, 2020).

In the second half of the paper, we provide evidence that Missouri's Medicaid contraction led to increased financial strain for Missouri residents by using individual-level credit report data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP). To estimate the causal effect of the Medicaid contraction on financial health, we implement a straightforward border discontinuity differences-in-differences empirical design to compare individuals living in census blocks within a 10-mile radius on either side of the Missouri state border. Similar to other studies that have used anonymized credit bureau data, we estimate intent-to-treat (ITT) effects as we are unable to observe insurance status for the individuals in our CCP data.

Consistent with our earlier results that the Medicaid contraction increased overall OOP health-care spending, we find that individuals in Missouri had higher amounts of debt in third-party collections, owned more bankcards, and held higher bankcard balances. We estimate that the Medicaid contraction led to increases of 0.04 accounts in third-party collections and $64 in debt owed to a third-party debt collector, both of which represent 13 percent increases relative to their prereform means. We also find that the number of bankcards held increased by 0.02 accounts (1 percent), bankcard balances increased by $133 (3 percent). Back-of-the-envelope calculations for average treatment on the treated (ATT) effects suggest that credit card borrowing increased by $558 to $915, bankcard accounts increased by 0.07–0.12, and debt in collections increased by $271 to $444.

Our results for debt in collections, an often-used measure of financial distress when using credit report data, are on the lower end of those reported in the prior literature. Estimates from studies on recent Medicaid *expansions* have found that debt in collections can be reduced by $390 to $1,231 (Finkelstein et al., 2012; Hu et al., 2018; Brevoort et al., 2020; Miller et al., 2020). Given that the upper bound of our estimates falls in the lower end of those from the previous research may indicate that financial effects from a *contraction* in Medicaid are asymmetric to those from an expansion. Given that losing health insurance is both

qualitatively and quantitatively different than gaining health insurance, one may expect these effects to be asymmetric (Garthwaite, Gross, and Notowidigdo, 2014; Ghosh and Simon, 2015; DeLeire, 2019; Tello Trillo, 2020). More broadly, our results are consistent with those of Argys et al. (2020), who found that individuals in Tennessee experienced worse financial outcomes after the state contracted its Medicaid program.

We identify two major reasons why Missouri's Medicaid contraction could be particularly informative about the potential effects of future Medicaid program cuts. First, because Medicaid has predominately expanded over time, there are relatively few studies about Medicaid contractions, and the few studies that have been done have largely focused on Tennessee's 2005 cut.[3] By adding a second case with Missouri, we can establish which results are "stylized facts" about Medicaid cuts and which are idiosyncratic effects in a single state. Second, the Missouri contraction affected populations and parts of the program that are very different from the recent ACA expansions or the 2005 Tennessee cut, which primarily affected childless adults. Instead, the Missouri cut, which contracted or eliminated specific programs and made coverage generally less generous, has more in common with potential future Medicaid reforms currently being discussed by various states, such as the introduction of work requirements.

2 Background and Literature

2.1 Missouri's Medicaid Program Contraction

After the 2001 recession, the state of Missouri faced a series of severe budget shortfalls that totaled over $2 billion (Kruckemeyer and Blouin, 2004). To address these budget deficits, Missouri Governor Matt Blunt enacted a series of cuts to social service programs, including the state's Medicaid program. On April 26, 2005, Blunt signed bill SB539 into law, which enacted a number of reforms to shrink the size of the state's Medicaid program. Taking effect on July 1, 2005, the law led to immediate changes in the Medicaid program. In particular, the state

[3] This is in part due to the fact that the population affected by the TennCare cut in 2005 (childless adults) was very similar to the population that gained access to Medicaid via the Affordable Care Act in 2014.

experienced a 20 percent drop in elderly enrollment in Medicaid and an increase in uninsured emergency room visits by the end of 2006 (Zuckerman et al., 2009).

The bill achieved its goal of shrinking Missouri's Medicaid program by enacting a few key reforms. First, the bill required copays from the Children's Health Insurance Program (CHIP) participants whose incomes exceeded 150 percent of the Federal Poverty Level (FPL) (Procter, 2005).[4] This new cost sharing rose proportionately with the income of the household. Income eligibility for working parents was lowered from 75 percent of FPL to about 20 percent (Ferber, 2007). The bill also eliminated the Medical Assistance for Workers with Disabilities (MAWD) program (Procter, 2005), a Medicaid buy-in program for low-income disabled workers. Along with the increased cost sharing, thousands of elderly beneficiaries were shifted to spenddown-based eligibility, which required that they first spend a proportion of their own income or their own assets to receive benefits (Hargraves, 2008). Finally, a number of services, including dental and optometrist services, were no longer covered under the bill, affecting approximately 370,000 of the remaining Medicaid enrollees (Procter, 2005).[5]

On September 18, 2007, Blunt announced a new three-phase health-care initiative called Insure Missouri, which was intended to reform the state's public insurance program and restore Medicaid coverage. Phase I of the Insure Missouri initiative was enacted on February 1, 2008, and implemented a new Ticket to Work program, which would replace the former MAWD program and aid workers with disabilities (Shield, 2007).[6] While Insure Missouri proposed a reversal of the earlier eligibility cuts and an expansion of Medicaid coverage, the new bill did not attempt to reverse the copayments clause or service cuts of the original SB539 bill, leaving the majority of the overall 2005 reform in place (Shield, 2007).

[4] Prior to the passage of SB539, only families with income above 225 percent of the FPL required to pay premiums on CHIP plans.

[5] Other benefits and services that were no longer covered included hearing aids, prosthetics, wheelchairs, durable medical equipment, hospice, and rehabilitative therapy (Zuckerman et al., 2009).

[6] Phases II and III of Insure Missouri, which would have restored coverage for working parents and childless adults with incomes between 100 percent and 185 percent of the FPL and encouraged small businesses to provide employer-based insurance, were never implemented (Ferber, 2007).

Table 1: Table of Dates Regarding the Missouri Medicaid Reforms of 2005–2007

Event	Date
Governor Matt Blunt signs SB539	April 26, 2005
Changes from SB539 go into effect	July 1, 2005
Insure Missouri gets announced	September 18, 2007
Phase I of Insure Missouri is enacted	February 2008
Phase I rollout is completed	End of FY2009

2.2 Medicaid Studies: Many Expansions, Few Cuts

Since its inception in 1965, the Medicaid program has grown tremendously over the past five decades, with total state and federal spending of almost $600 billion by 2017. Despite the size and scope of the program, major cuts to Medicaid and associated research have been relatively rare. The majority of the studies on Medicaid cuts focus on Tennessee's 2005 reform, in which approximately 170,000 individuals were disenrolled from its Medicaid program. Comparing the results from these previous studies with the body of literature on Medicaid expansions suggests that the effects from gaining and losing access to Medicaid may not be symmetric. Given that there are relatively few studies examining Medicaid cuts, studying Missouri's reform presents an opportunity to further the literature.

To the best of our knowledge, the only economic study to take advantage of the Missouri policy experiment is Garthwaite, Gross, and Notowidigdo (2018). Garthwaite et al. conduct a commuting zone-level analysis to determine the effect of such a fiscal tightening on the costs of uncompensated care in hospitals. They find that uninsured visits at surrounding hospitals rise after the closure of a hospital, but insured visits do not seem to rise as dramatically, suggesting insured visits are more discretionary than uninsured visits. This result is tested using Missouri data from the Current Population Survey (CPS). The paper's use of the Missouri reform is rather limited, however, as the authors use it more as a case study. Overall, the authors argue that the policy changes described previously do have an impact on the uninsured population.

While there is only one previous economic study that has looked at the Missouri's Medicaid contraction, a number of papers in the public health literature have examined the consequences of the reform. Most prominently, Zuckerman et al. (2009) examined what

happened to both enrollees and providers in Missouri after the 2005 reform using both the CPS and administrative data. They find that, after the reform, the uninsured rate increased, Medicaid enrollment declined, a larger uncompensated care burden on providers emerged, and community health clinics shifted their revenue source to more government grants.

For the Tennessee case, studies have shown that individuals affected by that Medicaid reform also experienced worse outcomes afterward. Ghosh and Simon (2015) use data from the Agency of Healthcare Research and Quality's (AHRQ) State Inpatient Database (SID) and find that the share of hospitalizations covered by Medicaid decreased, while the share of uninsured hospitalizations rose by nearly 61 percent, largely because of emergency department visits, and not linked to a lack of preventive care. Similar to Ghosh and Simon (2015), Tello-Trillo (2016) finds that the TennCare disenrollment led to worse self-reported health and decreased the number of visits to a primary care physician. He also finds evidence of decreased and delayed access to medical care, with approximately 30 percent of individuals affected by the reform changing their primary place of care. DeLeire (2019), using data from the Survey of Income and Program Participation, confirms the previous findings that the Medicaid cut led to worse self-reported health and worsened access to care. Similar to the other studies, he also finds that doctor visits and dental visits declined after the TennCare reform.

While the literature documents that both contractions and expansions of Medicaid coverage have significant effects on the health and finances of affected individuals, there is little evidence that these effects are symmetric. For example, Garthwaite et al. (2014) find that the Medicaid cut in Tennessee led to a large increase in labor supply: A 6.9 percentage point increase in the proportion of childless adults without public insurance led to an increase in employment of up to 5.7 percentage points. The authors find that 97 percent of the change in employment is explained by workers working for private employers who provide employer-sponsored health insurance (ESI). In contrast, Baicker, Song, Finkelstein, and Taubman (2014) find that individuals who received Medicaid coverage through the Oregon health insurance experiment did not experience a change to their labor force participation or to their earnings. Despite these opposing results, it is unclear if this difference in labor outcomes is because of an asymmetric effect of Medicaid expansions and contractions, the difference in the affected populations, since Oregon's

expansion was below the FPL, while Tennessee's cut included incomes between 100 percent and 175 percent FPL, or to other state-level idiosyncrasies. Earlier expansions that targeted pregnant women led to sizable decreases in employment (Dave et al., 2015), which may imply that differences are primarily driven by certain subpopulations.

2.3 Medicaid and Financial Distress

The recent literature on the effect of Medicaid expansions on individual financial outcomes is most relevant to this study. Several studies have estimated how the ACA's Medicaid expansions affect individuals' financial health. Medicaid eligibility has been shown to reduce bankruptcies, mostly among low-income and high-childhood neighborhoods (Gross and Notowidigdo, 2011). Catastrophic medical expenditures drastically decrease after individuals gain access to Medicaid (Baicker et al., 2013), and individuals may be able to take fewer payday loans (Allen, Swanson, Wang, and Gross, 2017). Hu et al. (2018) use a synthetic control method and data on individual credit outcomes and find that the ACA's Medicaid expansions led the average recipient to owe $1,140 less in debt in collections. Brevoort et al. (2020), using similar data and a differences-in-differences empirical strategy, find a similar reduction of medical debt in collections of $1,231 per new Medicaid recipient. They further emphasize that Medicaid brings as much value to households in the form of improved credit and the ability to borrow at lower interest rates as it does in direct reduction of out-of-pocket health expenditures. Miller et al. (2020) focus on Michigan's ACA Medicaid expansion and find a smaller $515 reduction in debt in collections per new Medicaid recipient. The smaller estimate for the decline in debt in collections may be attributable to the fact that they are able to link credit report data to administrative data on Medicaid enrollment.

While the previous literature makes it clear that receiving free or subsidized health insurance improves the finances of recipients, the more difficult question is whether recipients value this insurance above its cost. Finkelstein, Hendren, and Luttmer (2019) find that the average Medicaid recipients value Medicaid substantially less than they would value the cash spent to provide them Medicaid, and that up to 60 percent of the value of Medicaid functions as a transfer to providers for the uncompensated care they would otherwise provide. Like most

other recent empirical work on Medicaid, Finkelstein, Hendren, and Luttmer (2019) focus on an expansion of Medicaid to low-income non-disabled adults, and their results do not necessarily apply to other populations.

Argys et al. (2020) is the paper most similar to ours, which is the only study before ours to estimate the effect of a Medicaid contraction on *individual* financial outcomes. Using individual-level data on credit and debt outcomes, they find that credit scores declined in counties that had more Medicaid recipients relative to counties with fewer recipients following Tennessee's 2005 Medicaid cut. In some specifications, they find increases in delinquent debt and personal bankruptcy. Given that theirs is the first paper to analyze the individual financial effects of a Medicaid cut, we view our contribution as continuing the process of determining the extent to which the financial distress of such contractions is generalized (rather than a function of a particular place and time). Argys et al. (2020) focus solely on measures of distress, such as risk scores and delinquent accounts, while we consider more broad measures of an individual's finances, such as number of credit cards or amount of card debt.

3 Medicaid Coverage and Medical Expenditures Analysis

To analyze the effect of the Missouri Medicaid contraction on consumer financial outcomes, we first establish that medical expenditures changed as a result of the reform. To do this, we use data from the Household Component of the Medical Expenditure Panel Survey (MEPS), which surveys about 40,000 individuals per year with extensive questions on their health spending, health status, health insurance, and demographics. We use the restricted version of the MEPS, which has information on the state of residence for each respondent, allowing us to identify individuals in the data who were subject to the Missouri Medicaid contraction.[7] We use the MEPS data to study the effect of the Missouri cuts on Medicaid receipt and on a number of different measures of health spending.

[7] The publicly available version of the MEPS does not include information on the state of residence for respondents. For information on the difference between the restricted and publicly available version of the MEPS, see https://meps.ahrq.gov/data_stats/onsite_datacenter.jsp.

Table 2 shows the summary statistics for our variables of interest and our control variables. Our summary statistics show that Missouri is generally similar to the rest of the country and to its border states, with its largest observed difference being a smaller Hispanic population. Medicaid recipients, by contrast, are quite different from nonrecipients along almost every measure. In particular, the Medicaid population includes a larger fraction of children, which skews many measures down, for example, age, educational attainment, and proportion married.

Table 2: Summary Statistics for Medical Expenditure Panel Survey Data

	Total	No Medicaid	Medicaid	Border States	Missouri
Medicaid	12.0%	0.0%	100.0%	11.5%	12.9%
Privately Insured	66.4%	74.4%	7.5%	67.5%	68.0%
Uninsured	12.2%	13.9%	0.0%	12.4%	9.7%
Total Medicaid Spending	$374	$55	$2,484	$307	$348
Total Out-of-Pocket Spending	$678	$736	$260	$751	$699
Total Private Insurance Spending	$1,477	$1,640	$286	$1,618	$1,481
Total Health-care Spending	$3,526	$3,429	$4,233	$3,813	$3,535
Percent with Any Out-of-Pocket Spending	78.0%	81.0%	56.0%	79.6%	81.5%
Family Size	3.2	3.1	3.9	3.1	3.1
Male	48.9%	49.7%	43.0%	49.1%	49.6%
Black	9.4%	7.9%	20.3%	7.6%	10.8%
Asian	6.3%	5.9%	8.7%	3.6%	5.5%
Hispanic	14.1%	12.4%	26.7%	7.8%	2.8%
Married	41.4%	45.5%	11.9%	43.0%	43.7%
High School Graduate	59.4%	64.7%	21.0%	59.9%	60.0%
College Graduate	18.9%	21.2%	2.0%	16.3%	18.4%
Age	36.3	38.1	23.1	36.6	36.4

Notes: Data are based on authors' calculations using individual level data from the weighted 2000–2007 MEPS. Means reported in all columns. All dollar amounts are in constant 2012 dollars. Border states include all states that border Missouri, except Tennessee.

Because we lack the detailed individual-level geographic information of the consumer credit data set we use in the following section (though benefiting from more extensive

demographic controls), our MEPS analysis employs a simple state-level differences-in-differences (DID) empirical strategy that compares Missouri with other neighboring states before and after the Missouri Medicaid cut. We exclude Tennessee from the control group because it also reformed its Medicaid program in 2005. Our DID specification takes the following form:

$$y_{ist} = \beta_0 + \beta_1(Missouri_{ist} \times Post2005_{ist}) + \gamma Post2005_{ist} + S_s + T_t + \phi X_{ist} + \epsilon_{ist}. \quad (1)$$

S_s is a vector of state fixed effects (FEs), T_t is a vector of year FEs, and X_{ist} represents individual-level demographics (gender, race, ethnicity, education, marital status, family size, and age dummies). β_1 is our coefficient of interest, representing the treatment effect of living in Missouri in the years after the 2005 Medicaid cuts. We consider several dependent variables y_{ist}: dummies indicating insurance status (Medicaid, Uninsured), the natural log of several health-spending measures (total health spending, Medicaid spending, and out-of-pocket (self/family) health spending), and a dummy indicating whether the individual had any out-of-pocket health-care spending. Standard errors are clustered at the state level, and all regressions use MEPS-provided person weights.

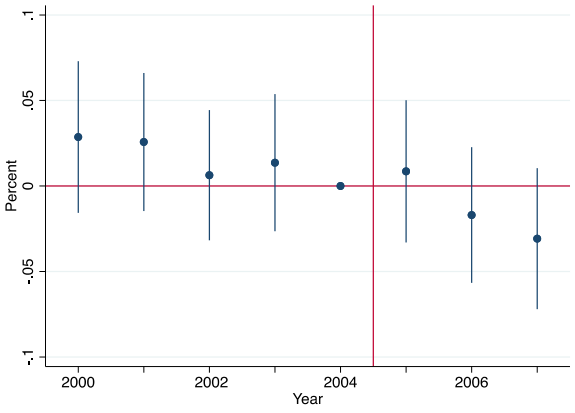
We also conduct event-study analyses that interact the Missouri dummy with a vector of year dummy variables from 2000–2007, rather than only interacting it with a post-2005 dummy variable as in the standard differences-in-differences framework. This allows us to consider whether any trends in insurance coverage or health spending in Missouri relative to control states existed prior to the 2005 cut, though we should note that the MEPS is not well powered for single years in Missouri, especially compared with the credit bureau data set we use in the next section. Our event study results are shown in Figure 1.

For our five dependent variables of interest, we see little evidence of statistically significant pretrends. In the case of the probability of being uninsured, we see that one year in the preperiod is statistically significant, which may be an indication of a pretrend, though the dummy variable estimates for the other remaining years do not trace out a linear relationship

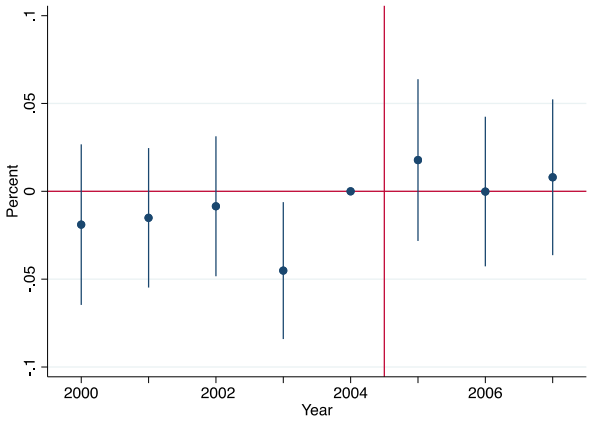
during the preperiod. The event study estimates for the postcontraction years indicate that Medicaid enrollment and spending decreased following the reform, while out-of-pocket spending seemed to increase, especially in the second full year after the contraction. We also note that our event study estimates are not statistically significant, which is not surprising given the potential power issues we mentioned previously. To overcome this power issue, we estimate Equation (1) and replace the vector of year dummies used in the event study analysis with a simple postreform dummy variable equal to one, if the year occurs after the Medicaid contraction and estimate the standard DID model.

Figure 1: Event Study Results from MEPS

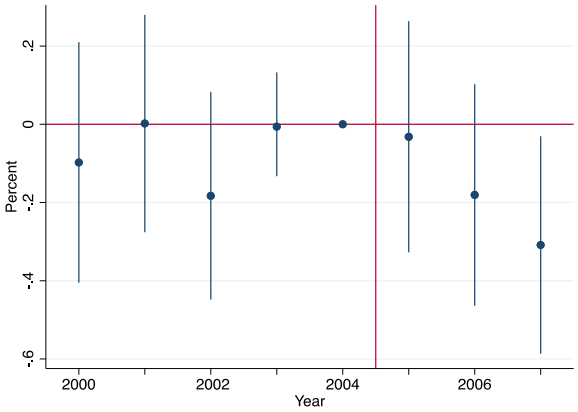
Panel A: Probability of having Medicaid



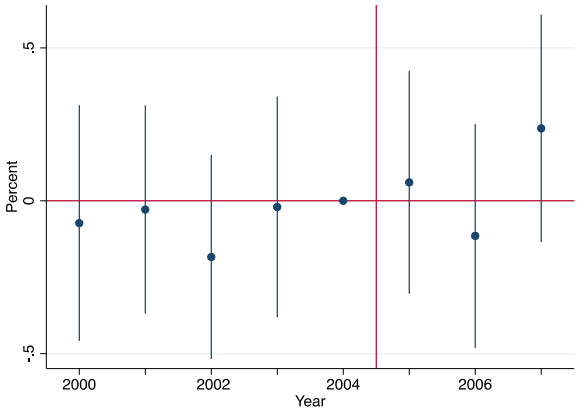
Panel B: Probability of being uninsured



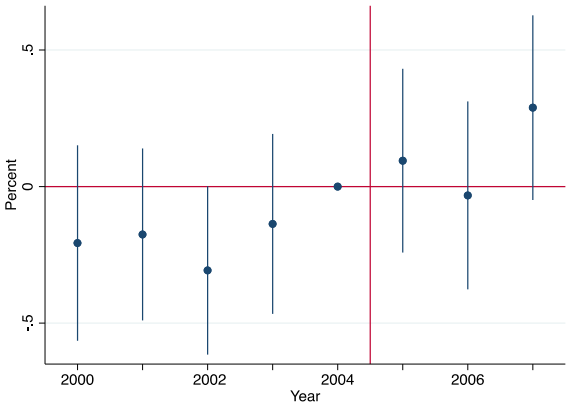
Panel C: Medicaid spending



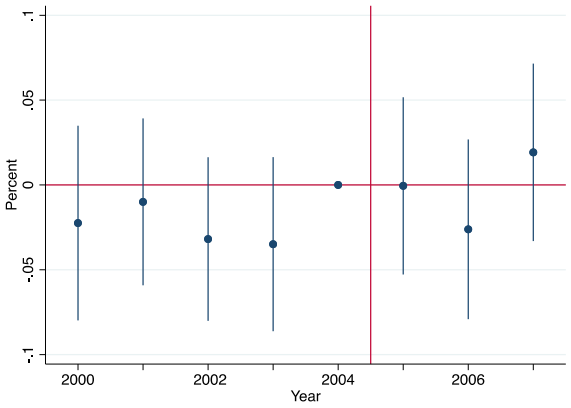
Panel D: Total medical spending



Panel E: Total OOP medical spending



Panel F: Likelihood of having any OOP spending



Notes: Based on authors' calculations using the restricted version of the Medical Expenditure Panel Survey (MEPS) data. Dots represent coefficient estimates, while bands show 95 percent confidence intervals. Spending variables are natural logs. The control group includes all states that border Missouri, except Tennessee.

Table 3 summarizes our DID estimates based on the specification in Equation (1). This table shows the direct effect of the Medicaid contraction: a 3.9 percentage point decline in Medicaid receipt. Based on our results, it appears that most of those who lost Medicaid coverage simply became uninsured rather than being able to find alternate coverage, given the 2.2pp increase in the uninsured rate. Table 3 also shows how these changes to the Medicaid program flow through to change health-spending patterns. As expected, we observe a large, 18 percent drop in Medicaid spending. However, this drop in Medicaid spending seems to be entirely offset by an increase in other sources of health spending. OOP spending increased by 30.3 percent, while the change in total health spending is positive, though not statistically significant. This increase in OOP spending is primarily along the intensive margin, as the proportion reporting any OOP spending increased by a relatively modest 1.7 percentage points. Overall, these results indicate that Missouri residents both incurred higher health-care expenditures and were at greater risk of high health-care expenditures in the years following the Medicaid program contraction. Given both of these results, we may expect overall financial outcomes to be worse for Missouri residents.

Table 3: Effect of Missouri Medicaid Cut on Insurance Status and Spending

	Medicaid Receipt	Uninsured	Medicaid Spending	OOP Spending	Any OOP spending	Total Spending
$Missouri_i$						
$\times Post2005_t$	-0.039***	0.022**	-0.180***	0.303***	0.017***	0.129*
	(0.004)	(0.007)	(0.037)	(0.009)	(0.003)	(0.063)
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.15	0.11	0.09	0.24	0.12	0.19
N	26,714	26,714	26,714	26,714	26,714	26,714

Notes: Based on authors' calculations using the restricted Medical Expenditure Panel Survey (MEPS) data. Spending dependent variables are natural logs of spending plus one. Robust standard errors clustered by household are reported in parentheses. *** indicates p<0.01, ** indicates p<0.05, * indicates p<0.10. Control variables include state and year fixed effects, sex, race (Black, Asian), ethnicity (Hispanic), education (high school grad, college grad), marital status, family size, and age (dummies for each year). Data are from 2000–2007 with 2005 omitted as partially treated since the contraction happened halfway through the year. Spending is measured in constant 2012 dollars. Control states are Oklahoma, Kansas, Nebraska, Iowa, Illinois, and Arkansas; Tennessee is omitted because of its own Medicaid reform in 2005.

4 Credit Panel Analysis

4.1 Consumer Credit Data and Empirical Approach

To measure the effect of the Missouri Medicaid cuts on consumer financial outcomes, we use the Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP), which is a 5 percent random and anonymized sample of all U.S. consumers with a credit bureau file from 1999 to the present. Individuals in the CCP are followed at a quarterly frequency and remain in the data set until they die, change their Social Security number, or no longer have a credit bureau file, which typically happens after an extended period of inactivity with the credit market.[8] We take a subsample of all individuals living in Missouri and its neighboring states from Q1:2003–Q4:2007 in the CCP. We exclude earlier years of data from our subsample to avoid any effects from the 2001 recession. Our final sample is an unbalanced panel of 155,028 individuals with 2.9 million total observations.

Our main credit report variables of interest are the total number of bankcard accounts an individuals has, the total balance on those bankcard accounts, the amount of bankcard debt that is delinquent, number of accounts that have been send to a third-party debt collector, the amount of debt that is owed to a third-party debt collector, and the individual's credit score.[9] Table 4 provides summary statistics for these variables and for our control variables, and Appendix Table 1A provides similar summary statistics for the prereform period, Q1:2003–Q2:2005.

[8] See Lee and van der Klaauw (2010) for a more detailed overview of the CCP.

[9] Our credit score measure is the Equifax Risk Score, which is a proprietary credit score produced by Equifax that is similar to other risk scores used in the industry.

Table 4: Summary Statistics for the CCP Data and Control Variables

	Treatment Group			
	N	Mean	Median	St. Dev.
Card balance	1,139,934	4,272	1,186	8,170.01
Number of cards	1,832,879	1.99	1	2.20
Amount card delinquent	1,327,424	262.59	0	1,341.36
Number of collections	1,882,022	0.32	0	1.04
Amount in collections	687,715	478.14	0	3,230.27
Risk Score	1,792,002	679.78	701	110.50
Unemployment rate	2,017,900	5.65	5.4	1.40
Percent in poverty	2,017,900	12.65	9.6	6.17
Proportion disabled	2,017,900	0.03	0.03	0.01
Proportion Black	2,017,900	0.18	0.21	0.16
Age	1,941,504	49.17	47.00	18.52

	Control Group			
	N	Mean	Median	St. Dev.
Card balance	652,931	4,476	1,238	8,833.73
Number of cards	1,001,975	2.07	1	2.23
Amount card delinquent	742,766	237.27	0	1,482.00
Number of collections	1,027,150	0.26	0	0.93
Amount in collections	324,673	436.22	0	2,052.95
Risk Score	992,433	689.30	715	106.31
Unemployment rate	1,096,816	5.62	5.3	1.72
Percent in poverty	1,096,816	11.47	11.3	5.43
Proportion disabled	1,096,816	0.03	0.03	0.01
Proportion Black	1,096,816	0.09	0.04	0.11
Age	1,056,899	49.23	48	18.19

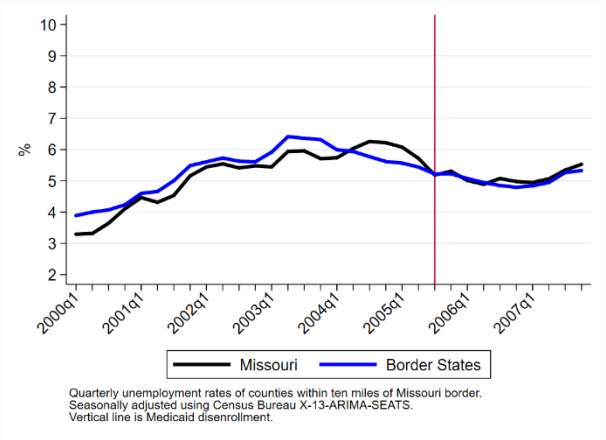
Notes: Authors' calculations using the CCP data. Age is individual level, based on year of birth in the CCP. The Risk Score is the Equifax Risk Score. The county-level demographic data are from the BLS's Local Area Unemployment Statistics, the Census Bureau, the Small Area Income and Poverty Estimates, and the Old Age, Survivors and Disability Insurance data.

For the most of control variables, there are few differences between individuals living in Missouri border census blocks and non-Missouri census blocks. The notable exception is the percent of the population that is Black, where Missouri border census blocks have double the Black population that non-Missouri census blocks (18 percent versus 9 percent). As a robustness check, in Section 4.5, we create a new subsample of our data by restricting the treatment and control areas to the two largest metropolitan areas in Missouri, which are split by the state border: Kansas City and St. Louis.

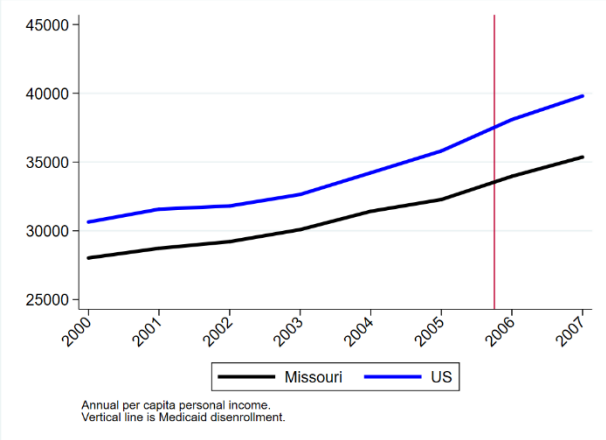
Figure 2 shows the trends in broad economic conditions between our treatment and control areas. Similar to statistics reported in Table 4, there are similar trends for the unemployment rate for border counties in Missouri and the surrounding states. Although county-level statistics for income or GDP are not available for this period, panel B of Figure 2 shows that state-level per-capita personal income had a similar trend in Missouri and the U.S. prior to the reform.

Figure 2: Unemployment Rate and GDP Growth in Missouri versus U.S. Overall

Panel A: Unemployment



Panel B: Income

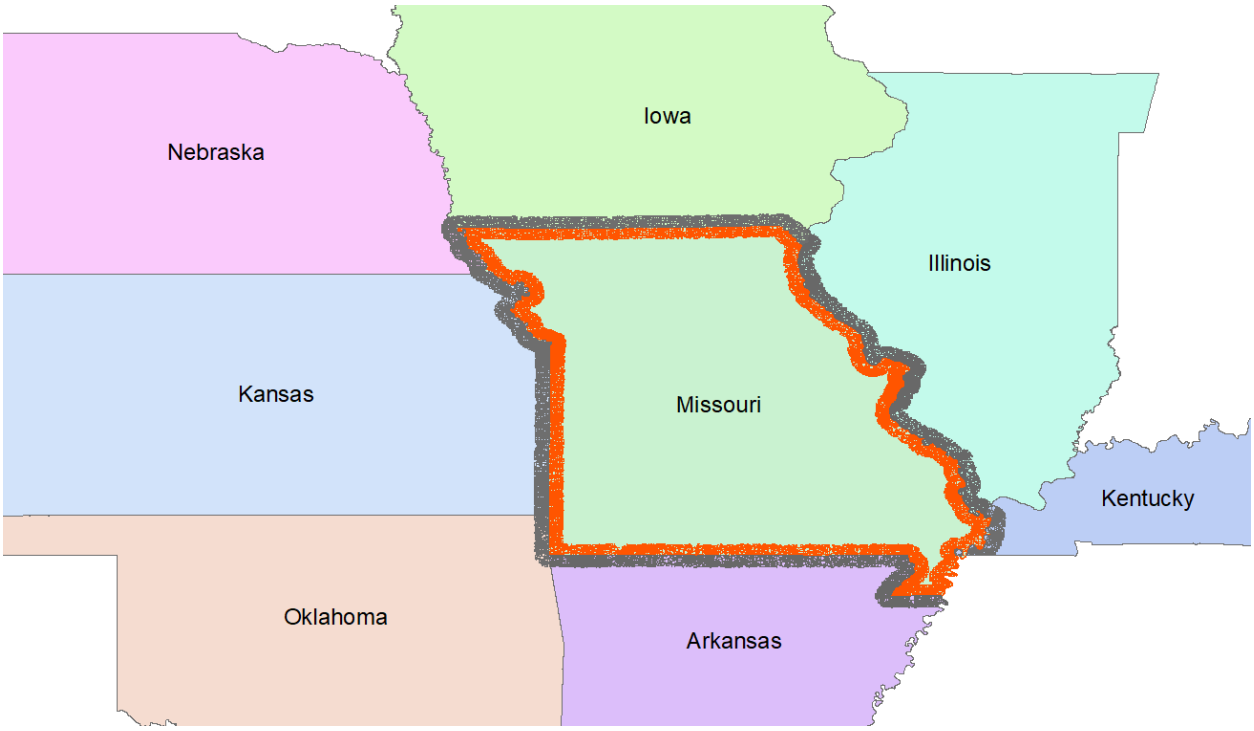


Notes: Figures based on authors' calculations using data from the U.S. Bureau of Labor Statistics and the U.S. Bureau of Economic Analysis.

To estimate the effects of the Medicaid cut on individuals' financial outcomes, we employ a differences-in-differences framework that uses the policy discontinuity at the Missouri state border to compare individuals in Missouri to individuals living in states that border Missouri. To account for geographic heterogeneity, we use census blocks located within a 10-mile bandwidth around the Missouri border to define treatment and control regions. We assign individuals who reside in Missouri census blocks within the 10-mile bandwidth to the treatment group, while any individuals living in non-Missouri census blocks within the 10-mile bandwidth of the Missouri border are placed in the control group. As mentioned previously, we exclude individuals who reside in Tennessee from the control group as Tennessee enacted its own Medicaid reform in 2005. Including individuals from Tennessee in the control group could

potentially bias our results. Figure 3 illustrates this design, with the treatment group census blocks shaded in orange and the control group census blocks shaded in black. Our identifying assumption is that, in the absence of the Medicaid contraction, changes in our credit variables would have evolved similarly on either side of the Missouri border.

Figure 3: Border Discontinuity



To conduct an event study analysis, our estimating equation takes the following form:

$$y_{it} = \alpha_0 + \boldsymbol{\beta}(Treated_i \times T_t) + \alpha_1 Treated_i + \gamma T_t + \phi X_{it} + \mu_i + \epsilon_{it}, \quad (2)$$

where $Treated_i$ is equal to one if an individual lives in a Missouri census block within the 10-mile bandwidth and T_t is a vector of quarter dummy variables from Q1:2004 to Q4:2007. To avoid inducing seasonal patterns in our estimates, we use all four quarters of 2003 as the base period. The vector X_{it} includes a second order polynomial in age, county fixed effects, county-level unemployment rate, the percent of the population at or below the FPL, the percent of the population that is disabled, and the percent of the population that is Black. Since the CCP is an

individual-level longitudinal data set, we are also able to include individual fixed effects μ_i. The coefficients of interest are captured in the vector β, which contains the estimates for each quarter before and after the policy change. Standard errors are clustered at the census tract level.

In addition to the event study model in Equation (2), we estimate a traditional DID specification in which we interact the Missouri dummy with a single dummy variable for the postreform period (Q3:2005 to Q4:2007). Our estimating equation is now:

$$y_{it} = \alpha_0 + \alpha_1(Treated_i \times Post_t) + \alpha_2 Treated_i + \gamma_1 Post_t + \phi X_{it} + \mu_i + \epsilon_{it}. \quad (3)$$

All control variables in this specification are the same as in Equation (2), and we cluster the standard errors at the census tract level.

4.2 Results for Credit Outcomes

Figure 4 shows the event study results from Equation (2) for our credit variables of interest. We do not observe strong evidence of pretrends in five out of six of our credit outcomes; the exception is the number of accounts in third-party collections, which shows a statistically significant pretrend, though the difference between the treatment group and the control group is stable across the quarters prior to the policy change. In the four quarters immediately after the Missouri Medicaid contraction, we see a statistically significant increase in the number of accounts in third-party collections by 0.033 accounts, which represents an approximately 10 percent increase relative to the prereform treatment group mean. In the same period, the amount of debt in third-party collections increases by $30–$80 (7 percent–18 percent increase relative to the prereform mean). This increase is not statistically significant until fourth quarter 2006. In the five to 10 quarters after the reform, the number of accounts in third-party collections and the amount of debt in third-party collections are both significantly higher, with the number of collections increasing by 0.09 and the amount of debt in collections increasing by $200. Relative to the prereform average, this change in accounts in third-party collections represents a 28 percent increase and the increase in debt in collections is a 42 percent change.

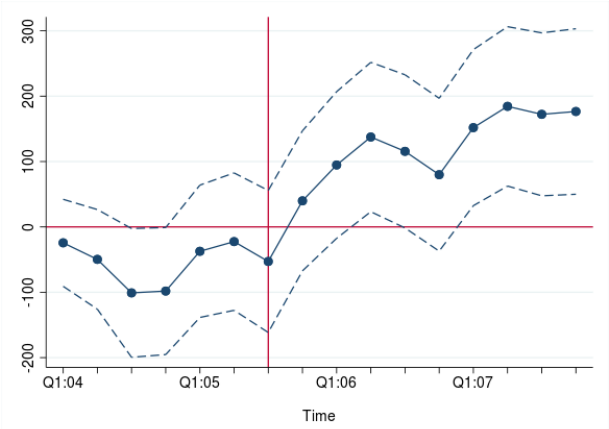
We also observe that credit card balances increase by $100–$140 (2.4 percent–3.3 percent) in the first year after the reform, and continue to grow to $200 (4.8 percent) by the second year while the number of credit cards increases by 0.005–0.02 (0.2 percent–1 percent). Unlike credit card balances, the number of credit cards owned declines in the second year after the reform. Point estimates for the amount of credit card debt delinquent are also positive, but not statistically significant until 2007. We also observe a decrease in Risk Scores, which becomes statistically significant only in 2007.

It is worth noting that, during this time period, some unpaid medical bills would go straight into third-party collections, which could appear on an individual's credit report in a short period of time. In contrast, if consumers with access to credit cards pay medical bills with their cards, we may expect delinquent debt on credit cards to increase with a lag. This would occur if consumers first increase their credit card balances by paying medical bills, and then gradually default on these credit cards over time. Our empirical results in Figure 4 are consistent with this story, as we find that debt in third-party collections increases more quickly than delinquent credit card debt after the reform.

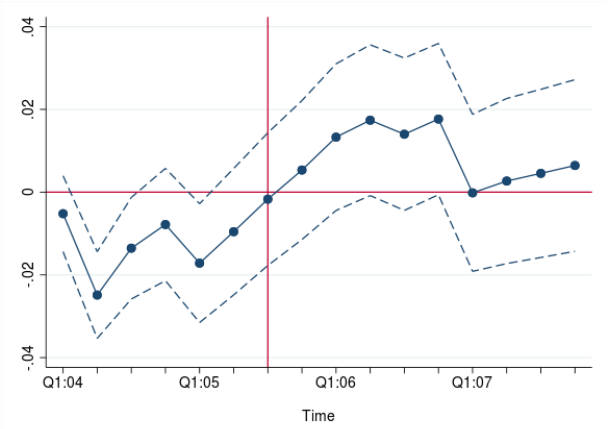
Table 5 shows our standard differences-in-differences results. We find that in the 10 quarters after the Medicaid reform was implemented in June 2005 (from Q3:2005 to Q4:2007), individuals living in Missouri border census blocks had 0.017 more bankcard accounts and had $134 higher bankcard balances. Consistent with our event study results, the fourth and fifth columns of Table 5 show that the number of third-party collections increased by 0.043 accounts, and the amount of collection grew by $65 in the postreform period.

Figure 4: Event Study Results

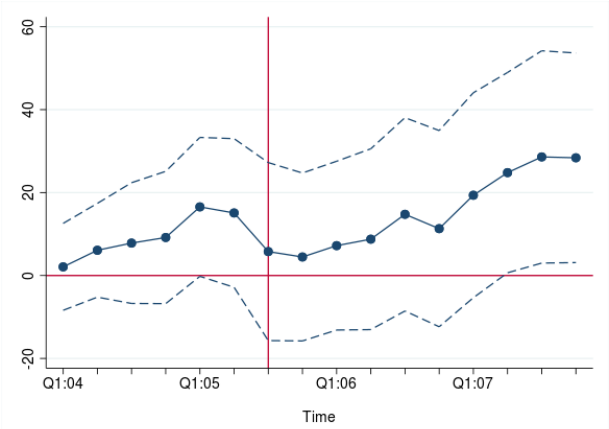
Panel A: Total Bankcard Balance



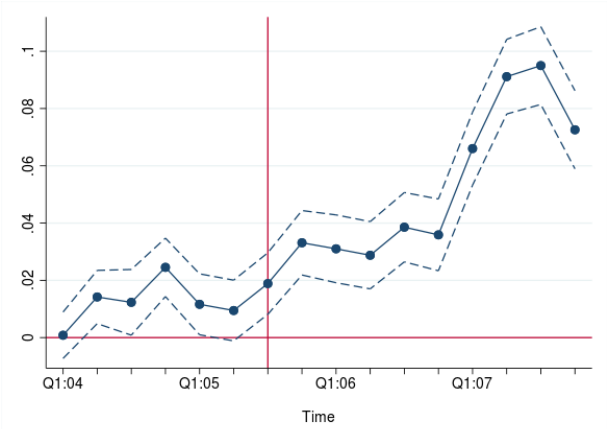
Panel B: Number of Bankcards



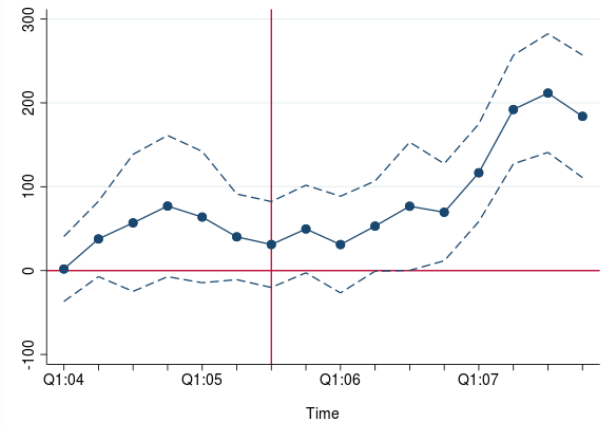
Panel C: Amount Delinquent on Bankcards



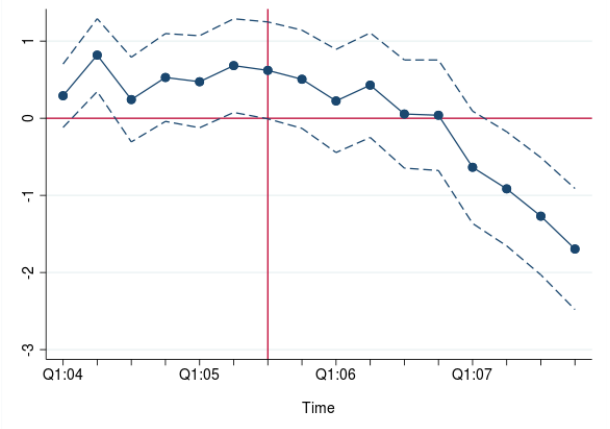
Panel D: Number of Accounts in Third-Party Collections



Panel E: Amount in Third-Party Collections



Panel F: Risk Score Points



Notes: Authors' calculations using data from the FRBNY/Equifax Consumer Credit Panel. Risk Score is the Equifax Risk Score. The omitted time period is all four quarters of 2003. Estimates in panels A, C, and E are measured in dollars.

Table 5: Differences-in-Differences Results from the Consumer Credit Panel (CCP)

	Total Bankcard Balance ($)	Number of Bankcards	Amount Delinquent on Bankcards ($)	Number of Accounts in Third-Party Collections	Amount in Third-Party Collections ($)	Risk Score (points)
$Missouri_i \times Post2005_t$	133.6***	0.0169**	7.949	0.0431***	64.61***	-0.593*
	(0.001)	(0.009)	(0.318)	(0.000)	(0.001)	(0.011)
N	1,783,257	2,773,887	2,059,688	2,850,431	986,623	2,739,476
Adj. R^2	0.694	0.841	0.512	0.432	0.143	0.884

Notes: Authors' calculations using data from the FRBNY/Equifax Consumer Credit Panel. $Missouri_i$ is an indicator variable for living within 10 miles of the Missouri border on the Missouri side and $Post2005_t$ is a dummy for quarters between Q3:2005 and Q4:2007. Robust standard errors are clustered at the census tract level and reported in parentheses. ***, **, * - denote significance at the 0.001, 0.01, and 0.05 levels. Risk Score is the Equifax Risk Score.

4.3. Average Treatment Effects and Potential Asymmetry of Medicaid Expansions and Contractions

Since we do not observe Medicaid status for individuals in the CCP, the estimates in Figure 4 and Table 5 are intent-to-treat (ITT) effects. Because our ITT estimates average over individuals both affected and unaffected by the Medicaid program cut, we divide our DID coefficients by the percent of the population that was actually eligible for Medicaid in Missouri to generate a back-of-the-envelope estimate of the treatment effect. We use poverty rates from the 2000 decennial census to proxy for the percent of the population eligible for Medicaid in Missouri. These poverty rates were approximately 14.64 percent–24 percent, depending on the measure.[10] Using these poverty rates, credit card debt increased by $\frac{\$134}{0.24} = \$558$ for the higher poverty measure, and an average treatment on the treated (ATT) estimate of $\frac{\$134}{0.1464} = \$915$ for the lower poverty measure. Our ATT estimate for the number of credit cards is between 0.07 and 0.116 accounts.

[10] Since there is no poverty data at the census tract level for the years 2004 or 2005, we use data from the 2000 decennial census. The two poverty measures represent the percent of the population at or below 100 percent of the FPL and the percent of the population at or below 150 percent of the FPL, respectively.

The $65 estimate for debt in collections implies an ATT estimate for individuals with Medicaid of $\frac{\$65}{0.24} = \$271$ for the higher-poverty measure, and an ATT estimate of $\frac{\$65}{0.1464} = \$444$ for the lower-poverty measure. The estimates for the accounts in third-party collections imply ATT effects of between 0.167 and 0.27 additional accounts. Since we are considering all Medicaid enrollees, this increase in collections would be for both Medicaid recipients whose insurance became less valuable and for those who fully lost Medicaid.

While sizable, this increase in debt in third-party collections is relatively smaller in magnitude than the *declines* in third-party collections resulting from Medicaid expansions as reported by Hu et al. (2018), who found a decrease of $1,145 after the ACA Medicaid expansion in 2014, Brevoort et al. (2020), who found a decrease of $1,231 in *medical debt* in collections after the ACA's Medicaid expansion, and Miller et al. (2020), who found a decline of $515 for individuals who enrolled in Medicaid in Michigan. Our estimates are similar to those reported by Finkelstein et al. (2012), who found a decrease of $390 in debt in collections for individuals who received Medicaid in the Oregon health insurance experiment. Since the upper bound of our ATT estimate falls within the lower end of the range of previous results for debt in collections, we cannot completely rule out the possibility of symmetric financial effects of Medicaid contractions and expansions. However, given that the majority of the prior literature finds larger effects, our results may suggest that the effect of a Medicaid program cut is smaller than the effect of an expansion of Medicaid for debt owed to third-party debt collectors.

There may also be financial spillover effects of the reform on non-Medicaid recipients. Only about half of the value of Medicaid accrues to its recipients, with the rest covering care that would otherwise be provided without full compensation, or would be covered by family, friends, or charities (Finkelstein et al., 2019). If friends and family members help cover medical bills of former Medicaid recipients in postreform Missouri, this could potentially lead to lower relative incomes for those individuals attempting to help out. In turn, this could lead these other groups to borrow more and have more trouble paying back debt, just as Medicaid recipients would. While the lower Medicaid spending could have lowered taxes in a way that benefited nonrecipients, much of this benefit would accrue to those outside of Missouri. At

least half of Medicaid funding comes from the federal rather than state government, and this share in Missouri was 62 percent as of 2006 (Ferber, Bednarek, and Islam, 2005).

We chose to end our analysis in 2007 because of the partial Medicaid restoration in 2008 (Shield, 2007) and because of the difficulty of separating the effects of the Medicaid cut from state-specific effects of the Great Recession. However, even if the recession did not hit Missouri especially hard in conventional terms, an economic shock could still affect Missouri disproportionally if it lacked the safety net that other states have. In other words, the effect of a Medicaid cut becomes more severe when an economic crisis pushes incomes down in a way that would have made more people eligible for Medicaid, but for the cut.

While we cannot credibly measure the longer-run effects of the cuts, we expect the effect of such a cut on some outcomes to persist or possibly grow over time. Although the reform first took effect in 2005, its effect is a continuous flow as people no longer receive benefits that were worth thousands of dollars per year, each year after the reform. Our credit measures, by contrast, are stock measures. For example, debt could gradually increase each year that Medicaid is not there to cover medical bills.

4.4 Border City Analysis

In our main analysis, we compare census blocks near the Missouri state border with neighboring state census blocks on the other side of the border and find evidence of worse financial outcomes for Missouri residents because of Medicaid cuts. However, since Medicaid coverage differs in rural areas and urban areas in Missouri during this time period (Zuckerman and Cook, 2006), including both rural and urban census blocks together in the treatment and control groups may lead us to less reliably estimate the causal effects of the Medicaid cut (Dube, Lester, and Reich, 2010).

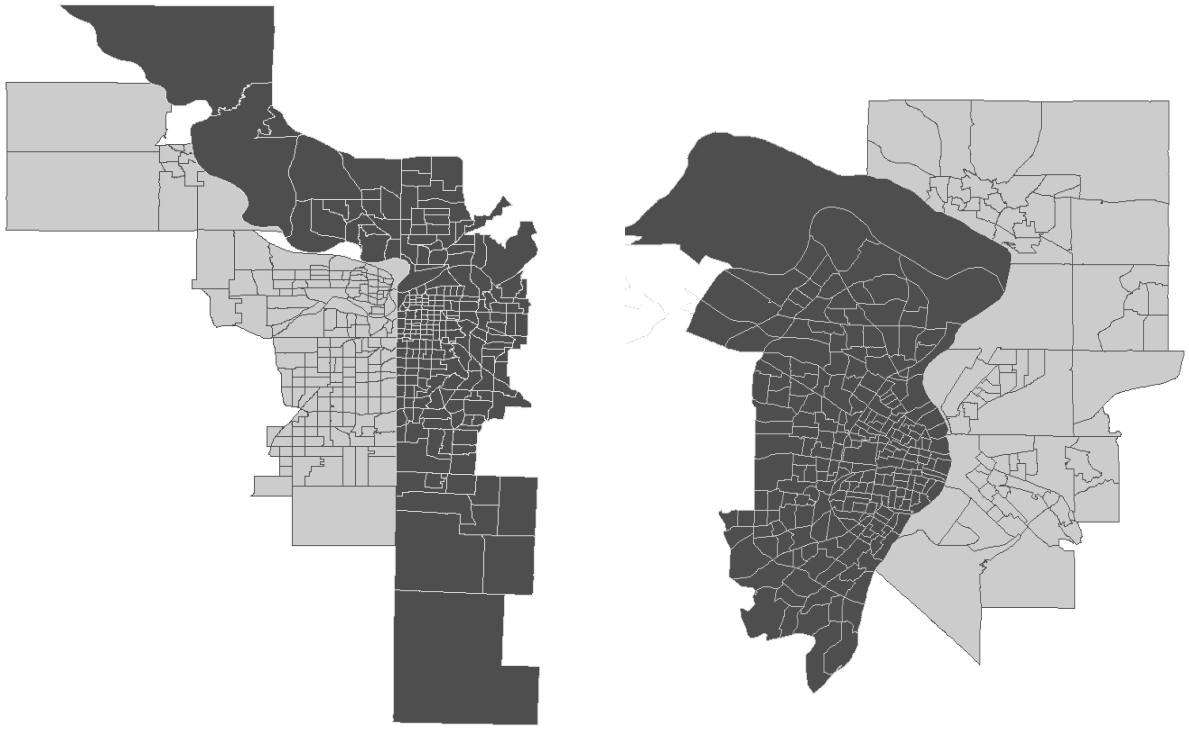
To address this, we focus on the two largest urban areas in Missouri, Kansas City, and St. Louis, which straddle the Missouri state border.[11] Similar to the full border analysis, we select census blocks within a 10-mile bandwidth of the state border. Focusing on Kansas City and St.

[11] Part of the Kansas City MSA is in Kansas, and part of the St. Louis MSA is in Illinois.

Louis (KC/STL) ensures that we have enough observations to credibly estimate our DID event study coefficients.[12] Figure 5 shows the geographic breakdown of the two cities.

Figure 5: City Border Discontinuity

Kansas City **St. Louis**



Note: The Missouri part of each city is more darkly shaded.

Similar to our main analysis of the full Missouri border, we estimate both an event study model and a traditional DID model with a single postreform dummy variable using Equations (2)

[12] Although Zuckerman and Cook (2006) show that Medicaid coverage was less prevalent in Kansas City and St. Louis than in the rest of the state (11.6 percent versus 19.5 percent), 58 percent of the state's population resided in these cities. This disparity is even more evident in our CCP border census tract sample, where approximately 75 percent of all observations reside in the Kansas City and St. Louis MSAs. Despite the fact that a higher percentage of the noncity population received Medicaid coverage, because the CCP is a 5 percent random sample of individuals with a credit report, the likelihood we would observe a noncity individual with Medicaid is actually lower than the likelihood of observing an individual with Medicaid living in either of the two cities.

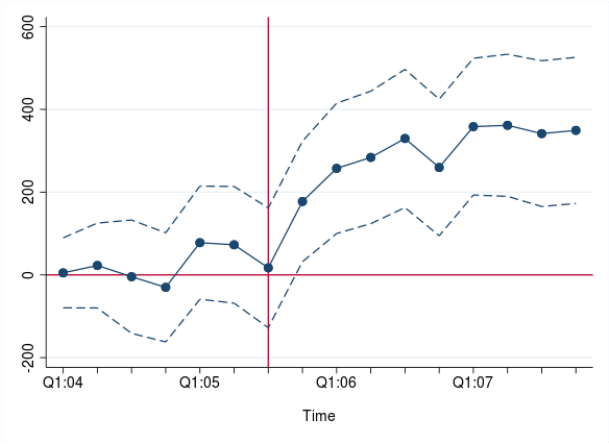
and (3), respectively. Our border city event study results are presented in Figure 6, and our traditional DID results are shown in Table 6.

For our event study results, the trends are generally similar to those in the main analysis, though the immediate response to the Medicaid contraction are more visible in the border cities analysis. As in our main analysis, there appears to be a difference between our treatment and control areas in the number of accounts in third-party collections, though the trend is relatively stable across the preperiod. We also observe some statistically significant differences in the number of credit cards, though this difference is also relatively stable across time.

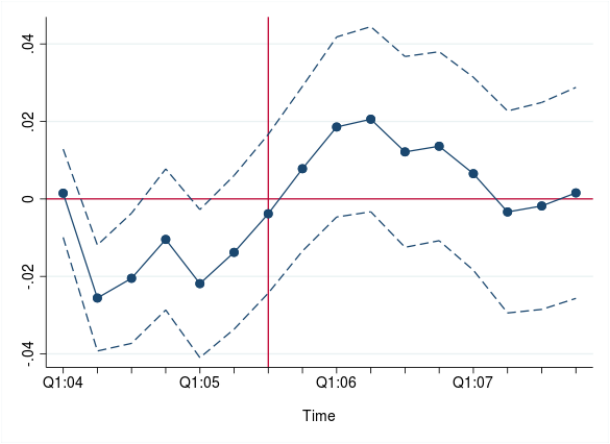
Immediately after the Medicaid contraction, we see that the number of accounts in third-party collections increased by 0.04 accounts, and the amount of debt owed in third-party collections increased by $100. By the end of the second year after the reform, both of the debt in collections measures have increased in magnitude, which is similar to the trend we observed in our main results. Both of the estimates are larger than those found in our main analysis, which is consistent with our hypothesis that mixing the two geographies together yields conservative estimates. We also observe that the number of bankcards and bankcard balances both increased immediately after the Medicaid contraction. The increase in the number of bankcards is statistically significant but similar in magnitude to our main analysis estimates, while the $190 increase in bankcard balances is over twice the size of the effect we found in the main analysis. Similar to the main analysis, the number of bankcards held actually decreases by the second year after the reform, while total bankcard balances grow to over $350 by the end of 2007. Our estimates for delinquent credit card debt are not statistically significant immediately after the Medicaid cut, but they become significant in the final two quarters of 2007, which is similar to the main analysis. Finally, our estimates for Risk Score show no immediate effect after the Medicaid contraction but decline starting in the middle of 2006.

Figure 6: Border City (KC/STL) Event Study Results

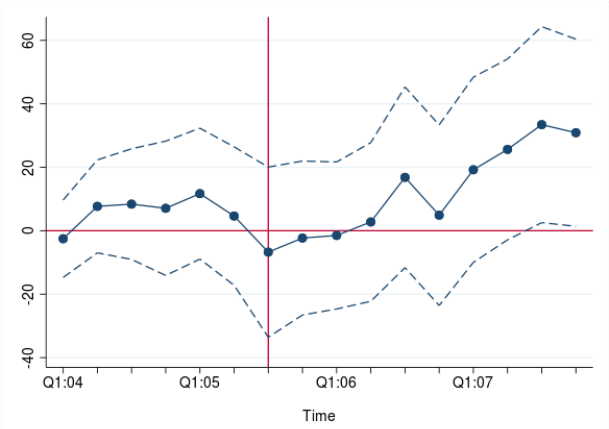
Panel A: Total Bankcard Balance



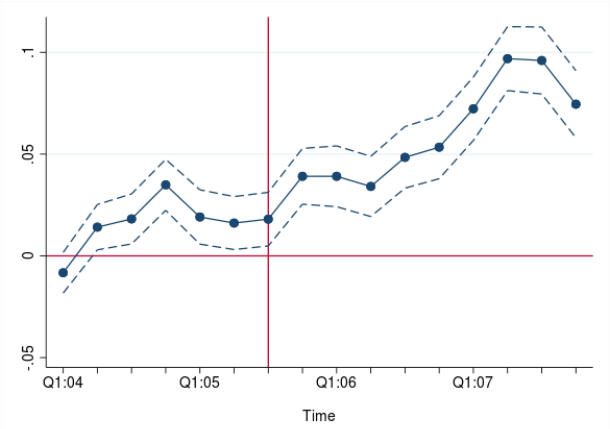
Panel B: Number of Bankcards



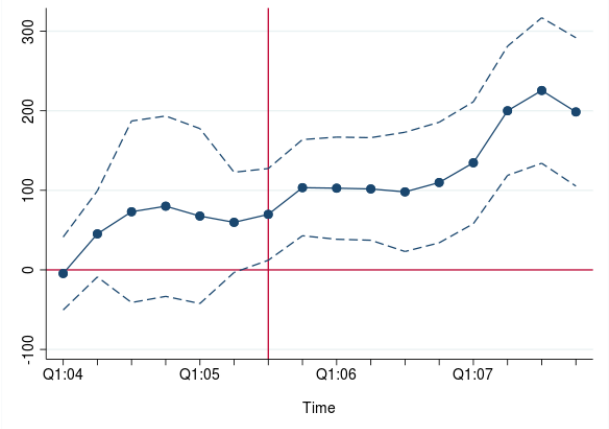
Panel C: Amount Delinquent on Bankcards



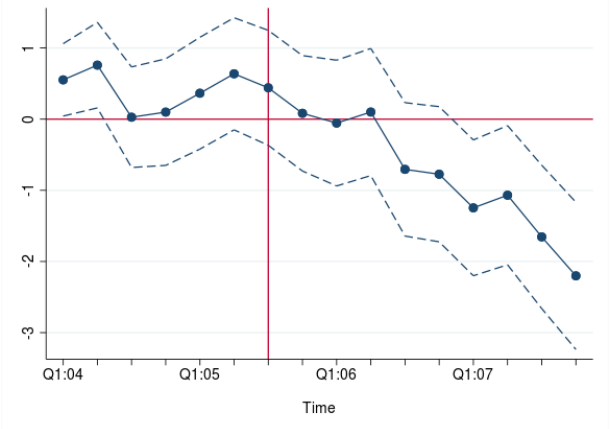
Panel D: Number of Accounts in Third-Party Collections



Panel E: Amount in Third-Party Collections



Panel F: Risk Score Points



Notes: Authors' calculations using data from the FRBNY/Equifax Consumer Credit Panel. Risk Score is the Equifax Risk Score. Omitted time period is all four quarters of 2003. Estimates in panels A, C, and E are measured in dollars.

Results from our standard DID analysis on the border cities subsample in Table 6 are also similar to our main results. We find evidence of an increase in the number of bankcards held and in bankcard borrowing and increases in the number of accounts and debt owed in third-party collections. Our DID estimate for bankcard balance of $234 is 1.8 times larger than the $133 increase we estimated in the main analysis. This disparity in credit card borrowing results between the two analyses is not surprising since there are more low-income households living in the non-KC/STL areas than in KC/STL (Zuckerman and Cook, 2006) and low-income households are less likely to have credit cards and borrow less, conditional on having credit card accounts (Bricker et al., 2017). The implied ATT effect of this increase ranges from $\frac{\$234}{0.23} = \$1{,}017$ for the higher-poverty measure and $\frac{\$234}{0.14} = \$1{,}671$ for the lower-poverty measure.[13] While this range appears very high, the increase in total health-care spending that we found using the MEPS data in Table 3 is approximately 27 percent to 45 percent of this increase in credit card spending, which suggests that there are additional negative financial spillovers from the Medicaid contraction.

Table 6: Differences-in-Differences Results from Consumer Credit Panel (CCP), KC/STL Sample

	Total Bankcard Balance ($)	Number of Bankcards	Amount Delinquent on Bankcards ($)	Number of Accounts in Third-Party Collections	Amount in Third-Party Collections ($)	Risk Score (points)
$Missouri_i \times Post2005_t$	234.3***	0.0178*	5.960	0.0435***	89.58***	-0.922**
	(0.000)	(0.024)	(0.495)	(0.000)	(0.000)	(0.001)
N	1,355,001	2,089,601	1,564,622	2,145,049	739,432	2,058,996
Adj. R^2	0.694	0.844	0.516	0.439	0.145	0.886

Notes: Authors' calculations using data from the FRBNY/Equifax Consumer Credit Panel. Risk Score is the Equifax Risk Score. $Missouri_i$ is an indicator variable for living within 10 miles of the Missouri border in Kansas City or St. Louis. $Post2005_t$ is a dummy for quarters between Q3:2005–Q4:2007. Robust standard errors are clustered at the census tract level and reported in parentheses. ***, **, and * denote significance at the 0.001, 0.01, and 0.05 levels, respectively.

[13] The two poverty measures for the two cities are very similar to the measures used in our main analysis. They are computed in the same way, based on the 2000 decennial census data.

For debt in third-party collections, our estimate of an $89 increase is $25 higher than in our main analysis, which translates to an ATT effect of $\frac{\$89}{0.23} = \$387$ for the higher-poverty measure and $\frac{\$89}{0.14} = \$636$ for the lower-poverty measure. This range of values from the border city analysis is smaller than the estimates from a number of previous studies but falls well within the range established by the literature. ATT effects for number of accounts in third-party collections range from 0.181–0.297 accounts, which is very similar to our main results.

5 Conclusion

Our study provides new evidence on the effect of a Medicaid contraction on the financial health of those affected. Analyzing Missouri's 2005 Medicaid cut, we find substantial declines in Medicaid eligibility, coverage, and spending. This in turn led to higher out-of-pocket health-care spending, increased borrowing, and increased financial strain for Missouri residents. These results are qualitatively similar to previous studies of recent Medicaid expansions and of the 2005 Medicaid cut in Tennessee (Argys et al., 2020) and reinforce the existing evidence that health insurance provides significant financial protections.

We estimate that the Medicaid cut led to a $64 increase in debt in third-party collections in Missouri, which implies an average treatment-on-the-treated effect of approximately $546 per Medicaid recipient. This result is broadly consistent with previous work that found Medicaid *expansions* reduced debt in collections by $390 (Finkelstein et al., 2012) to $1,231 (Brevoort et al., 2020) per new recipient. Comparing these estimates to ours, we argue that Medicaid expansions and contractions may have asymmetric effects on financial distress, with contractions having smaller effects than expansions. We also find evidence that the Medicaid contraction led to an increase credit card borrowing of $134, which translates to an ATT effect of $558 to $915 per person. These results, taken together, imply that a decrease in the generosity of health-insurance benefits have important spillovers into the credit market behavior of lower socioeconomic status households.

Our results for financial distress seemingly contrast with those of Argys et al. (2020), as our estimates from Missouri imply relatively *smaller* financial effects because of the

contraction, while Argys et al. (2020) find *larger* financial effects than the previous literature because of the TennCare disenrollment in Tennessee. While we find a different type of asymmetry, we do not believe our results necessarily contradict those of Argys et al. (2020) because of the qualitative differences in the Medicaid reforms between the two states. In particular, the Missouri Medicaid reform was primarily a reduction in the generosity of health insurance for the majority of Medicaid enrollees, while the Tennessee reform disenrolled a large number of participants from their program. It may also be the case that newly enrolled Medicaid beneficiaries examined in the previous studies have higher medical expenses, and thus experience larger financial benefits, than the Medicaid-eligible individuals in our study, who were already covered by Medicaid for some time.

Given the current policy discussions that states are having regarding Medicaid reform, our study provides important information regarding the potential financial spillover effects that may result from decreasing benefit generosity or restricting eligibility. In particular, acknowledging the presence of asymmetries in these effects is important to properly assess the costs and benefits of any policy change, especially for populations that may be either credit constrained or less able to take on and manage additional debt.

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Appendix

Table 1A: Summary Statistics for the CCP Data and Control Variables

	Treatment Group			
	N	Mean	Median	St. Dev.
Card balance	572,478	4203.46	1153	8187.66
Number of cards	916,703	2	1	2.21
Amount card delinquent	671,327	232.06	0	1151.93
Number of collections	942,853	0.27	0	0.89
Amount in collections	329,936	407.42	0	3466.55
Risk Score	899,517	676.8	697	109.71
Unemployment rate	1,005,973	6.09	5.6	1.51
Percent in poverty	1,005,973	12.34	9.6	5.82
Proportion disabled	1,005,973	0.03	0.03	0.01
Proportion Black	1,005,973	0.18	0.21	0.16
Age	964,600	48.41	46	18.44

	Control Group			
	N	Mean	Median	St. Dev.
Card balance	652,931	4,476	1,238	8,833.73
Number of cards	1,001,975	2.07	1	2.23
Amount card delinquent	742,766	237.27	0	1,482.00
Number of collections	1,027,150	0.26	0	0.93
Amount in collections	324,673	436.22	0	2,052.95
Risk Score	992,433	689.30	715	106.31
Unemployment rate	1,096,816	5.62	5.3	1.72
Percent in poverty	1,096,816	11.47	11.3	5.43
Proportion disabled	1,096,816	0.03	0.03	0.01
Proportion Black	1,096,816	0.09	0.04	0.11
Age	1,056,899	49.23	48	18.19

Notes: Authors' calculations using the CCP data from Q1:2003 to Q2:2005. Age is individual level based on year of birth in the CCP. The Risk Score is the Equifax Risk Score. The county-level demographic data are from the BLS's Local Area Unemployment Statistics, the Census Bureau, the Small Area Income and Poverty Estimates, and the Old Age, Survivors and Disability Insurance data.