What is the impact of a Medicaid expansion on the bankruptcy rate? Evidence from the ACA

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Abstract

The ACA Medicaid expansion increased the number of individuals enrolled in Medicaid, reducing the risk that many will have to pay for medical procedures out of pocket. While this decreased the medical bankruptcy rate, the exact magnitude of this impact is unknown. Using a difference-in-differences model with controls and a fixed effects model with an instrumental variable, I estimate that 75,000-125,000 bankruptcies were prevented by the ACA Medicaid expansion between 2014-2016 among low-income individuals, an approximate 6-8% decrease in the bankruptcy rate. This effect is stronger among those earning below the median income; there is no evidence of a similar effect among those earning above the median income. These results are strengthened through a series of falsification tests and robustness checks.

1 Introduction

According to the Himmelstein (2009), 60% of bankruptcies are attributable to medical costs. The U.S. medical system is one of the most expensive health systems in the world (Papanicolas et al, 2018). The median cost of an emergency appendectomy in the United States is $33,000 (Hsia, 2012), and the 20.6% of non-elderly Americans who were uninsured in 2013 – 36 million individuals (KFF, 2017) – must pay this entire expense out of pocket, or risk bankruptcy. A similar procedure would cost only $4,000 in Australia (Kliff & Oh, 2018), with none of this cost charged to the patient. This difference in costs - and increase in debt - has real impacts on individuals, with over half (53%) of the uninsured reporting that they struggled with medical debt, and more than 125,000[1] bankruptcies in the past year were directly attributed to medical debts among the uninsured (Kaiser Family Foundation, 2016).

The Affordable Care Act (ACA) Medicaid expansion increased the number of individuals enrolled in Medicaid by 11.9 million (KFF, 2018), reducing the risk that many will have to pay out-of-pocket for an emergency medical procedure. However, the significance of this impact in terms of actual bankruptcies prevented is unknown. In light of the passage of Medicaid expansion ballot initiatives in Idaho, Nebraska and Utah, and the election of pro-Medicaid expansion governors in Kansas, Maine, and Wisconsin in 2018, this question assumes a particularly relevant role in the literature.

Prior research finds an 8% decrease in bankruptcies as a result of a 10% increase in Medicaid eligibility as a result of the CHIP and Medicaid expansions of the 1990s (Gross et al 2011). Gross's (2011) study is somewhat limited by the state-level nature of Gross's data, which neglects within state trends. In addition, they do not test for heterogeneous impacts based on the pre-bankruptcy income of the filer.

More recently, Brevoort et al (2018) estimate using a difference-in-differences approach that 25,000 bankruptcies a year were prevented by the ACA Medicaid expansion in expansion-implementing states. However, Brevoort et al (2018) do not test the robustness of this estimate, nor do they en-

[1] 1% of the 53% who reported that they struggled with medical debt. This does not include bankruptcies declared more than 12 months before the survey date, nor bankruptcies among the insured, who may also struggle with medical bills.

sure that this decrease in bankruptcies is localized specifically to those who become eligible for Medicaid under the ACA, as their focus is on the impacts of the Medicaid expansion on credit availability. To my knowledge, there have been no studies focused specifically on estimating the number of bankruptcies prevented by the ACA Medicaid expansion.

In this paper, I contribute by estimating the number of bankruptcies prevented by the ACA Medicaid expansion. I utilize a novel dataset which allows a county-level analysis; in addition, I perform a series of robustness checks, including a previously unutilized check which ensures that my results are localized among those who became eligible for Medicaid. I utilize two econometric models: first, a county-level difference-in-differences approach, which exploits the fact that some states expanded their Medicaid programs while others held their criteria constant, given the optionality of this expansion as a result of a 2012 Supreme Court decision; and second, I use a county level fixed-effects model. In both cases I find evidence that the ACA Medicaid expansion prevented approximately 75,000-125,000 bankruptcies in Medicaid expansion implementing states between 2014-2016. This comprises between one-third and two-thirds of the decrease in bankruptcies over this time period, and a reduction by 6-8% in the overall bankruptcy rate. This effect is stronger when examining only those who earn below the median income, and non-existent among those earning above the median income.

2 Bankruptcy Background and Definitions

[2] The decision to file for bankruptcy is generally thought to be driven by a strategic cost/benefit analysis on the part of the filer (Fay, 2002). Among the costs is a social stigma (Fay et al, 1998; Hackney, 2015), a decrease in credit score, the potential loss of property (Domowitz, 1999), and the shame of needing to appeal to charity in order to afford the nearly $1,500 expense in order to actual file bankruptcy (Mann, 2010; Kiel, 2018). The benefits of filing for bankruptcy include forgiveness of debt and the end of harassment by debt collectors (Domowitz 1999). The costs of filing for bankruptcy have a discernible impact on the choices individuals make with respect to their

[2] A brief literature review can be found in Appendix A

healthcare; one in twelve individuals in a discrete choice experiment were found to value preventing bankruptcy over having an illness cured (Shrime, 2018).

Research suggests that the pressure from debt collectors plays an outsized role in an individuals decision to file for bankruptcy. Individuals might decide against filing for bankruptcy in some cases if they have not faced aggressive debt collection efforts (White, 1998). Mann (2010) supports this through a study of judicial filings and interviews, finding that regardless of the amount of debt, individuals who do not face aggressive debt collection efforts tend not to file bankruptcy when compared with their peers who faced pugnacious debt collectors. This implies that the social costs of bankruptcy are heavily weighted when an individual decides whether or not to file for bankruptcy.

Once an individual decides to file for bankruptcy, they must choose which chapter of the bankruptcy code to file under. Within the United States, bankruptcies for individuals generally fall into two categories: Chapter 7 and Chapter 13. Under Chapter 7, individuals must surrender all assets above an exemption limit, but have their debts fully forgiven. Meanwhile, under Chapter 13, individuals are allowed to keep all of their assets, but must work with the courts and their lenders to develop a plan to repay at least part of their debts over a period of time. Both chapters of the bankruptcy code leave the same mark on an individual's credit report. However, under Chapter 13 that mark only exists for 7 years, while under Chapter 7 the mark remains for 10 years.

In most cases, the individual has no say as to which Chapter they file under. As a result of the 2005 bankruptcy code reform, an individual is forced to file under Chapter 13 if their income is above the median income in their state of residence, to prevent abuse of the system (Li, 2007). However, if an individual has an income which qualifies them for Chapter 7 bankruptcy, they may face the opposite problem of being too poor to afford to file for bankruptcy, due to the costs associated with filing for bankruptcy.

Lawyers are almost always a requirement when filing for bankruptcy, given the complex nature of the bankruptcy forms (Kiel, 2018). Most bankruptcy attorneys require an upfront payment before assisting with a Chapter 7 bankruptcy filing, because all debts are eliminated under Chapter 7, including those owed to a lawyer (Kiel, 2018). Since a low-income individual seeking bankruptcy is unlikely to have access to enough cash to cover the $1,000 or greater attorney fee upfront, their lawyer may instead offer a payment plan, and induce them to file under Chapter 13. The lawyer then becomes one of the individual's creditors, rolling their fee into a payment plan under Chapter 13 (Fresques, 2017). This barrier results in many individuals that would benefit the most under Chapter 7 instead filing under Chapter 13.

To address this distortion, I categorize consumer bankruptcy filings into low and high-income filers based on the whether the filer's average monthly income for the past 12 months, as declared at filing on Line 16, Schedule I of their bankruptcy filing, is above or below their state's median income. Because individuals that became eligible for Medicaid under the ACA Medicaid expansion will almost certainly have income below the median income, any change in the bankruptcy rate as a result of the ACA Medicaid expansion should be entirely localized to this group. I exploit this fact in a series of robustness checks, to substantiate my claim of identification.

3 Empirical Approach

I utilize two econometric models to identify the impact of the ACA Medicaid Expansion on the bankruptcy rate. I first utilize a difference-in-differences approach defined below:

$$Y_{it} = \beta_0 + \beta_1 Expanded_i + \beta_2 PostExpansion_t + \beta_3 PostExpansion_t * Expanded_i + \beta_4 X_{it} + \varepsilon_{it} \quad (1)$$

where Y_{it} is the bankruptcy rate per 1,000 residents in county i during year t, β_1 is the treatment group term, which accounts for the average constant differences between expansion and non-expansion counties, β_2 is the average time trend post-2014 in expansion and non-expansion states, β_3 is the average treatment effect, β_4 is a vector of control variables for each county i in year t, and ε_{it} is an error term.

Under this approach, the treatment and control groups must remain the same over the entire time period. Unfortunately, several states chose to expand their Medicaid programs post-2014, as shown in Figure 1a. To ensure that my treatment and control groups remain the same, I recategorize states as presented in Figure 1b. I lose all observations from Pennsylvania, Indiana, Alaska, Montana, and Louisiana, however, I do not exclude Michigan or New Hampshire, as both states expanded their Medicaid programs in 2014, and I furthermore do not exclude any state that expanded after 2016.

The vector of controls, X, is necessary because there are clear differences in the overall bankruptcy patterns between states that expanded and those that did not, as evidenced by Figure 4's non-parallel trends in bankruptcy pre-expansion. Non-parallel trends are similarly found when splitting bankruptcy filers by income, as shown in Figure 3. In all of these instances, I assume that parallel trends are created through the use of appropriate controls,[3] and use a series of robustness checks to corroborate my results.

Because of the strength of assumptions needed for an accurate estimate of average treatment effect under difference-in-differences, I attempt to confirm these results by estimating the change in bankruptcy rate as a result of a change in Medicaid enrollment. I modify a log-linear fixed effects specification employed by previous researchers (Gross et al, 2011):

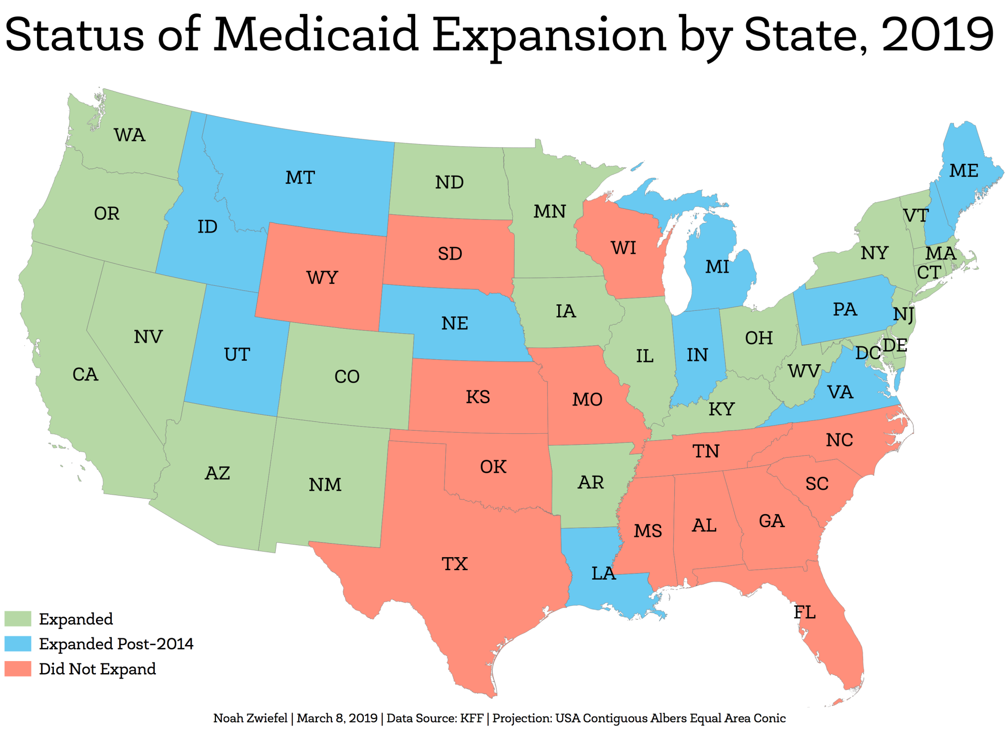
$$\text{Inverse Hyperbolic Sine}^4(Y_{it}) = \beta_0 + \beta_1 \text{MedicaidEnrolled}_{it} + \alpha_i + \alpha_t + \varepsilon_{it} \tag{2}$$

where Y_{it} is the number of consumer bankruptcies per 1,000 filed in county i in year t, $\text{MedicaidEnrolled}_{it}$ is the percent of the 18-64 population in county i in year t that is enrolled in Medicaid, α_i and α_t are fixed effects for year and county respectively, and ε_{it} is an error term. With this specification, I am able to include all states, even those that expanded after 2014.

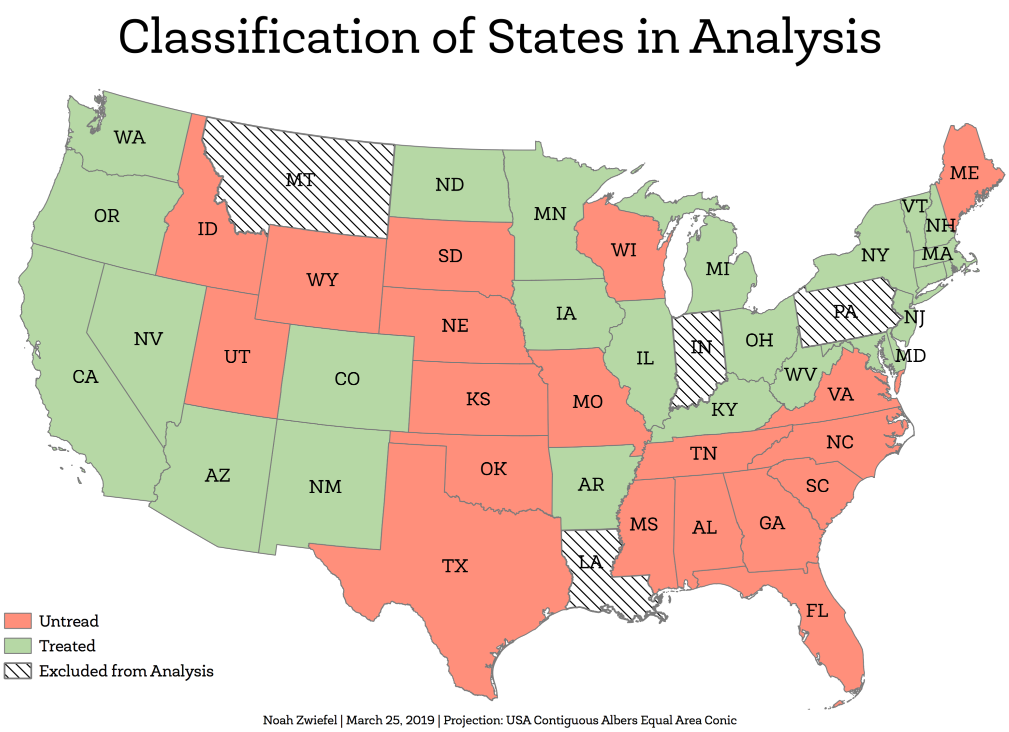
Unfortunately, this specification introduces the possibility of omitted variable bias. Medicaid enrollment is influenced by factors outside of the change in eligibility under the ACA. A change in enrollment within a county could occur because of an unobservable shock – not associated with the Medicaid expansion – which impacts the health, beliefs or behaviors of residents, inducing them to enroll in Medicaid, even if they were previously eligible.

[3]See Section 5 for a description of the controls that I utilize.

[4]The inverse hyperbolic sine is a form of a log transformation, defined as $log(y + \sqrt{y^2+1})$. This transformation is not undefined where $y = 0$, and thus ensures that all counties are included in my regression.



(a) Status of states' decisions on Medicaid as of 2019.



(b) For the purposes of my analysis, I treat New Hampshire and Michigan as if they expanded at the same time as all other states in the treatment group, given that I have annual data. I additionally treat Virginia, Maine, Idaho, Nebraska, and Utah as non-expansion states, since their Medicaid expansions had not occurred by 2016, so the possibility of contamination is non-existent.

Figure 1

Causality cannot be inferred with certainty, and there is a high likelihood that the estimate of average treatment effect is inconsistent and biased. To address this, I use Medicaid eligibility as an instrumental variable. Given that a change in Medicaid eligibility criteria is what produced increased Medicaid enrollment under the ACA, I will be able to make a direct causal inference of the ACA Medicaid expansion's impact on the bankruptcy rate.

I use a fixed effects two-stage least squares estimator to instrument Medicaid enrollment. During the first stage, an instrumented version of Medicaid enrollment is calculated, using Medicaid eligibility as the instrumental variable.

$$\text{Medicaid}\hat{\text{E}}\text{nrollment}_{it} = \gamma_0 + \gamma_1 \text{MedicaidEligibility}_{it} + \alpha_i + \alpha_t + u_{it} \tag{3}$$

Then, in the second stage, that instrumented version of Medicaid enrollment is utilized in my originally specified regression.

$$\text{Inverse Hyperbolic Sine}(Y_{it}) = \beta_0 + \beta_1 \text{Medicaid}\hat{\text{E}}\text{nrollment}_{it} + \alpha_i + \alpha_t + \varepsilon_{it} \tag{4}$$

I assume that bankruptcy rates would have had a similar pattern post-Medicaid expansion had the eligibility criteria for Medicaid not changed. I further assume that the county fixed effects absorb all local characteristics which impact the bankruptcy rate, and that these characteristics remain constant during my sample period; I also assume that the year fixed effects will absorb all time-specific U.S. macroeconomic shocks that may influence the bankruptcy rate. I finally assume that there are no systematic drivers of a change in the percent of individuals eligible for Medicaid within a county, besides the change in Medicaid rules under the ACA. Under these assumptions, instrumented Medicaid enrollment will be the only variable influencing the bankruptcy rate in this regression.

To test the validity of these assumptions, I examine several specifications with controls for confounding variables[5]. When including these controls, my system of equations becomes:

[5] I discuss this in Section 5; I use the same controls as in my difference-in-differences models.

$$\text{Medicaid}\hat{\text{E}}\text{nrollment}_{it} = \gamma_0 + \gamma_1 \text{MedicaidEligibility}_{it} + \gamma_2 X + \alpha_i + \alpha_t + u_{it} \tag{5}$$

$$\text{Inverse Hyperbolic Sine}(Y_{it}) = \beta_0 + \beta_1 \text{Medicaid}\hat{\text{E}}\text{nrollment}_{it} + \beta_2 X_{it} + \alpha_i + \alpha_t + \varepsilon_{it} \tag{6}$$

where X is a vector of control variables.

To validate this model, as with my difference-in-differences, I use a series of falsification tests designed to ensure that my results are specifically localized to bankruptcy filings among low income individuals.

4 Data

Ideally, I would have an annual count of bankruptcies specifically caused by medical costs, and a measure of Medicaid enrollment, absent any unmeasured or unobserved shocks, aside from the change in policy. Direct causality could be readily established were this data available. Unfortunately, these ideal data do not exist, since measuring and collecting such data would be both prohibitively expensive, and likely impossible in the case of medical bankruptcy filings. As my measure of bankruptcy, I utilize a county-level derived annual count of bankruptcies by chapter and type of filer between 2011-2016 (Federal Judicial Center, 2018). This is a novel, previously unutilized dataset that enables a more localized analysis when compared with previous literature, which has tended to utilize a state level count of bankruptcies from the U.S. Court system's F-2 annual report (2018).

For Medicaid enrollment, I use the one-year ACS PUMS to derive a weighted estimate of the percent of the 18-64 year old population in a Public Use Microdata Area (PUMA) that is enrolled in Medicaid. I then use a weighted geocorrelation average to convert the PUMA-level estimate into a county-level estimate. As my instrumental variable, I utilize a simulation detailed in Appendix B to derive an estimate of the 18-64 year old population that is Medicaid eligible through the income-qualifying pathway at a county level. This estimate of eligibility may be lack precision[6]. I address

[6]See Appendix B. My simulation does not exclude adults already eligible for health insurance (for example under their parents' plan), or who might have insurance through their workplace, but still have an income which qualifies

this through several robustness checks, in addition to my difference-in-differences approach.

I utilize a number of control variables which prior studies have used as potential predictors of bankruptcies (Domowitz, 1999; Gross 2011). As economic predictors of bankruptcy, I utilize county level data on median income, the bottom and second quintile of income distribution, the business bankruptcy rate, per-capita personal income, and the unemployment rate. As demographic control variables, I utilize the percent of population aged 25-44, one of the populations most likely to file for bankruptcy (Domowitz, 1999), the percent of the population at or below the poverty level, the percent of the population with subprime credit scores, the percent of the population that owns their own home, and the percent of the population that is black or Hispanic. These controls were all retrieved from GeoFRED (St Louis Federal Reserve, 2018). I additionally use control variables for the relative liberalness of a state's government, which may impact how the ACA Medicaid expansion was implemented, or what other potentially confounding safety net and consumer protections programs exist. These controls include whether or not the governor of the state was a Democrat in 2014, and the percent of the vote margin for the Democratic presidential candidate in 2008, 2012, and 2016 (Leip, 2018). I test the robustness of these controls by estimating a model with fixed effects, in an attempt to control for unobserved or unmeasured heterogeneities.

My summary statistics are presented in Table 1. For my summary statistics and figures, I omit Pennsylvania, Indiana, Alaska, Montana, and Louisiana unless otherwise specified – all states that chose to expand their Medicaid program after 2014 but before 2016.

Figure 2 compares the estimated percentage of the 18-64 population in expansion and non-expansion states that are eligible or enrolled in Medicaid. The Medicaid eligibility rate greatly increased post-2014 in Medicaid expansion-implementing states, while the eligibility rate among non-Medicaid expansion-implementing states remained relatively flat. The enrollment rate increased in expansion states, however it did not increase by as much as eligibility, which is to be expected, given that take up for nearly all government programs tends to lag behind eligibility (Currie, 2004).

Controlling for no other economic characteristics of a community, this large increase in eligibility seems to have had a limited impact on the number of bankruptcies. Figure 3 stratifies bankruptcy

them for Medicaid; in addition, the derivation process may introduce some slight bias. As a result, my estimate may be somewhat impercise.

	Expanded						Did Not Expand					
	2011-2013			2014-2016			2011-2013			2014-2016		
VARIABLES	N	mean	sd	N	mean	sd	N	mean	sd	N	mean	sd
Panel A - Dependent Variables												
Total Number of Consumer Bankruptices per 1,000	4,011	3.043	1.544	4,011	2.068	1.192	4,035	3.021	2.136	4,034	2.323	1.894
Total Number of Business Bankruptices per 1,000	4,011	0.0983	0.112	4,011	0.0642	0.0917	4,035	0.105	0.130	4,034	0.0658	0.0991
IHS(Consumer Bankruptcies per 1,000)	4,011	1.719	0.513	4,011	1.366	0.502	4,035	1.619	0.693	4,034	1.365	0.697
IHS(Business Bankruptcies per 1,000)	4,011	0.0970	0.103	4,011	0.0634	0.0853	4,035	0.103	0.120	4,034	0.0648	0.0928
Number of Bankruptcies per 1,000 (Below Median Income)	4,011	1.865	0.918	4,011	1.398	0.821	4,035	1.671	1.260	4,034	1.445	1.270
Number of Bankruptcies per 1,000 (Above Median Income)	4,011	1.111	0.750	4,011	0.615	0.455	4,035	1.263	1.003	4,034	0.800	0.681
IHS(Bankruptcies per 1,000 - Below Median Income)	4,011	1.302	0.444	4,011	1.058	0.444	4,035	1.135	0.605	4,034	1.005	0.612
IHS(Bankruptcies per 1,000 - Above Median Income)	4,011	0.880	0.455	4,011	0.547	0.344	4,035	0.935	0.550	4,034	0.659	0.463
# of Evictions per 100	3,047	1.596	1.784	3,028	1.319	1.416	3,396	1.667	1.985	3,325	1.668	1.936
# of Evictions Filed per 100	3,443	2.401	4.669	3,421	2.256	4.457	3,564	3.570	5.565	3,493	3.477	5.114
IHS(# of Evictions per 100)	3,047	1.001	0.739	3,028	0.893	0.676	3,396	1.004	0.788	3,325	1.004	0.796
IHS(# with Subprime Credit per 1,000)	4,029	10.88	0.266	4,026	10.78	0.276	4,032	11.09	0.343	4,029	11.00	0.383
IHS(# of Evictions Filed per 100)	3,443	1.138	0.932	3,421	1.119	0.888	3,564	1.370	1.092	3,493	1.382	1.075
Panel B - Predictor Variables												
Percent of 18-64 Population Eligible for Medicaid	4,029	0.147	0.103	4,026	0.478	0.160	4,035	0.142	0.0532	4,035	0.157	0.0694
Percent of 18-64 Population Enrolled in Medicaid	4,029	0.125	0.0506	4,026	0.183	0.0773	4,035	0.111	0.0404	4,035	0.120	0.0462
Panel C - Control Variables												
% that Own Home	4,029	74.41	8.254	4,026	73.11	8.412	4,035	72.89	7.527	4,033	71.59	7.783
% with Subprime Credit	4,029	27.54	6.886	4,026	24.88	6.365	4,032	34.58	9.847	4,029	31.56	9.099
% with Bachelor's Degree or Higher	4,261	20.63	9.131	4,260	21.64	9.393	4,032	17.95	7.561	4,032	18.81	7.788
Unemployment Rate	4,258	8.566	3.742	4,259	6.170	3.153	4,035	8.025	2.995	4,035	5.669	2.047
Median Income	4,029	47,061	11,824	4,026	50,698	13,018	4,035	42,001	9,677	4,033	45,415	10,720
Log(Median Income)	4,029	10.73	0.235	4,026	10.80	0.242	4,035	10.62	0.219	4,033	10.70	0.225
Bottom Quintile of Income ($100s)	4,029	208.9	59.52	4,026	214.5	59.91	4,035	185.8	52.16	4,032	191.2	53.69
Second Quintile of Income ($100s)	4,029	379.5	102.8	4,027	391.9	105.5	4,035	338.7	84.23	4,031	349.5	87.24
Democratic Margin for President in 2008	4,029	0.0508	0.171	4,026	0.0510	0.171	4,035	-0.0958	0.105	4,035	-0.0958	0.105
Democratic Margin for President in 2012	4,029	0.00260	0.189	4,026	0.00270	0.189	4,035	-0.141	0.112	4,035	-0.141	0.112
Democratic Margin for President in 2016	4,029	-0.0470	0.209	4,026	-0.0470	0.209	4,035	-0.156	0.112	4,035	-0.156	0.112
Legislature Controlled by Dems.	4,026	0.352	0.478	4,023	0.352	0.478	3,690	0	0	3,690	0	0
Governor is Dem.	4,026	0.577	0.494	4,023	0.578	0.494	3,690	0.0813	0.273	3,690	0.0813	0.273
State is Controlled by Dems.	4,026	0.295	0.456	4,023	0.295	0.456	3,690	0	0	3,690	0	0
% Pop. 18-24	4,029	0.0890	0.0338	4,027	0.0887	0.0324	4,032	0.0905	0.0331	4,032	0.0897	0.0319
% Pop. 25-44	4,029	0.231	0.0317	4,027	0.230	0.0318	4,032	0.236	0.0315	4,032	0.235	0.0318
% Pop. 45-64	4,029	0.284	0.0308	4,027	0.277	0.0279	4,032	0.273	0.0290	4,032	0.267	0.0274
% Living Below Pov. Line	2,686	15.25	5.853	4,026	15.46	5.944	2,690	18.20	6.771	4,033	18.22	6.823
Per-Capita Personal Income	4,026	39,381	11,083	4,023	42,146	11,672	4,035	35,814	10,395	4,032	38,684	11,575

Table 1: Summary statistics.

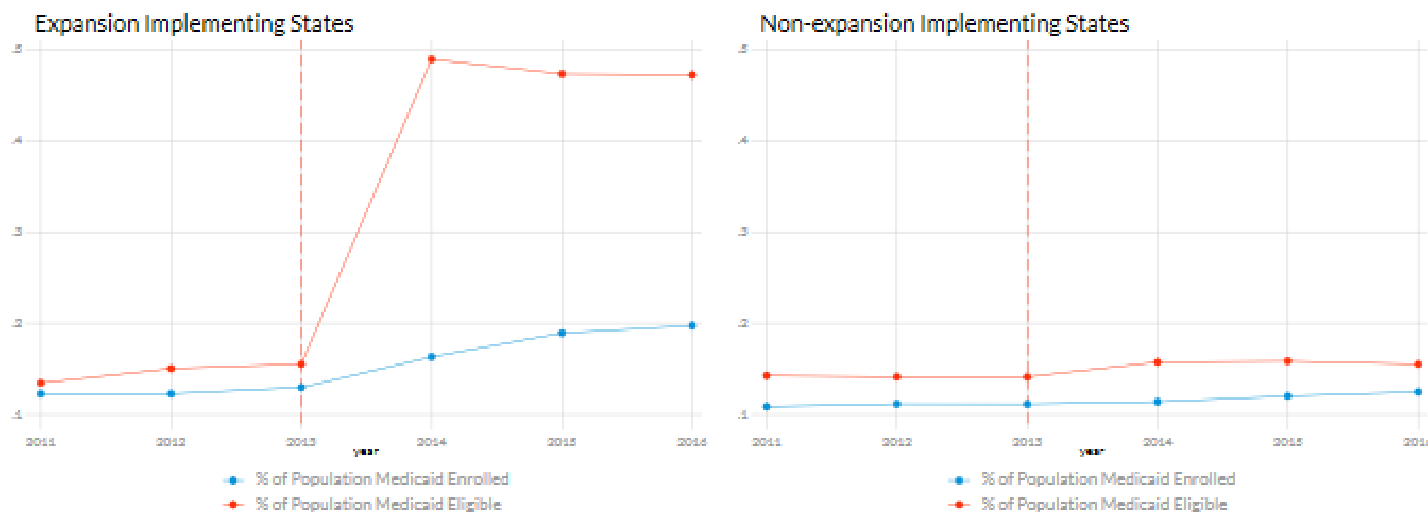


Figure 2: Trends in Medicaid eligibility and enrollment between 2011-2016.

by pre-bankruptcy income of the filer. Post-Medicaid expansion, it appears as if Medicaid expansion states had a larger decrease in low-income bankruptcy filings when compared with non-Medicaid expansion states. Meanwhile, the post-Medicaid expansion time period is associated with a relatively parallel trend in high-income bankruptcy filings in expansion and non-expansion states. This is consistent with the ACA Medicaid expansion impacting the bankruptcy rate, as we would expect to see a change in bankruptcy filing patterns to be localized to low-income bankruptcy filers.

Theoretically, these trends could be attributable to the recovery from the 2008 recession. The 2008 financial crisis resulted in a massive spike in bankruptcies, which led to a steep decrease in bankruptcies after the recession ended. Thus, examining only the raw bankruptcy rate does not enable us to extrapolate pre-treatment trends as a "but-for", or counterfactual, bankruptcy rate. Given this, I attempt to estimate a more accurate average treatment effect by attempting to control for economic characteristics that would be driving a change in bankruptcy as a result of a recovering economy, such as the median income of an area, the income distribution, and the unemployment rate.

Figure 4 explores the trend in aggregate consumer bankruptcies filings when compared with aggregate business bankruptcies. Under the hypothesis that the ACA Medicaid expansion impacted bankruptcy filings, we should observe consumer bankruptcy filings fall relative to business bankruptcies and this is, indeed, what we note. Consumer bankruptcies appear to have fallen by a greater amount than business bankruptcies in expansion states, when compared with non-expansion states.

More quantitatively, I present summary statistics on the bankruptcy rate in Table 2. Bankruptcies fell more post-expansion in Medicaid expansion states, when compared with non-Medicaid expansion states. Overall, a visual inspection of these figures and tables implies that the bankruptcy rate fell by slightly more in expansion states than in non-expansion states. This is, of course, a naive approach, given that we are controlling for no other macroeconomic or community level features that may impact the bankruptcy rate. Thus, a more thorough econometric approach is needed to analyze this question.

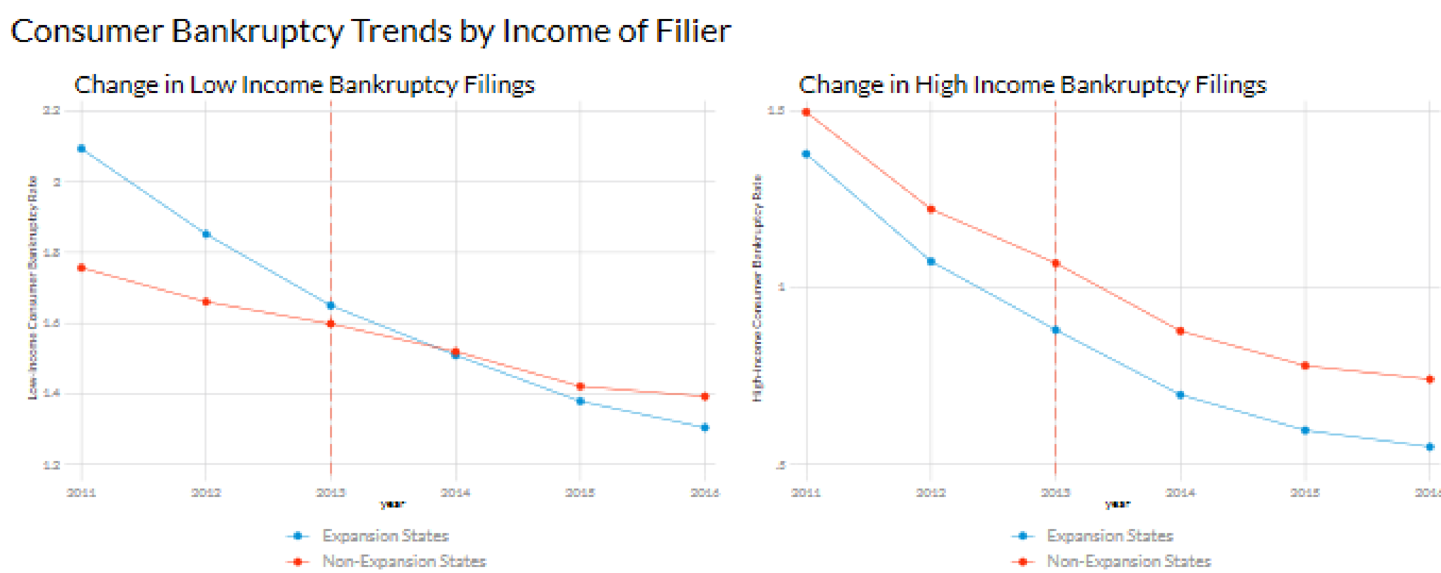


Figure 3: Trends in bankruptcy filings in expansion and non-expansion states between 2011-2016.

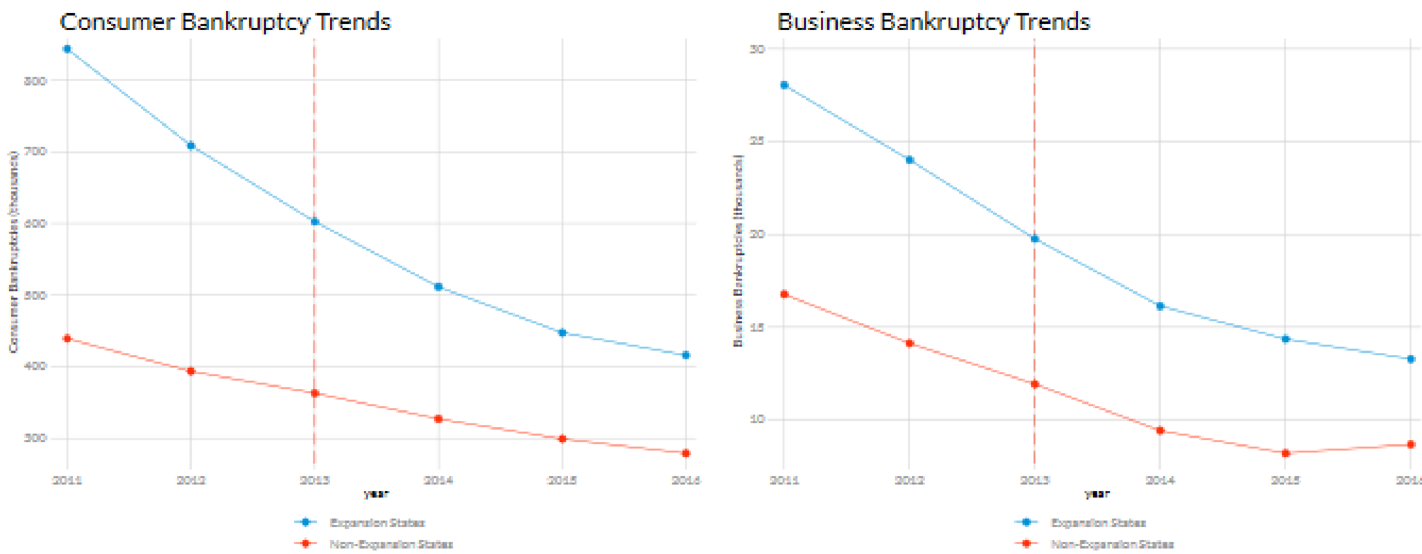


Figure 4: Comparing trends in consumer and business bankruptcy filings in expansion and non-expansion states between 2011-2016.

Mean percent of 18-64 population eligible for Medicaid			
	2013	2014	Change
Expanded	15.50%	48.90%	33.40%***
Did Not Expand	14.10%	15.7%	1.60%
Mean consumer bankruptcies per 1,000 residents earning less than the median income in expansion and non-expansion states			
	2011-2013	2014-2016	Percent Change
Expanded	1.89	1.45	-23.28%***
Did Not Expand	1.67	1.44	-13.77%***
Mean Chapter 13 consumer bankruptcies per 1,000 residents earning less than the median income in expansion and non-expansion states			
	2011-2013	2014-2016	Percent Change
Expanded	1.11	0.641	-42.3%***
Did Not Expand	1.26	0.779	-36.6%***
Mean consumer bankruptcies per 1,000 residents earning less than the median income in expansion and non-expansion states			
	2011-2013	2014-2016	Percent Change
Expanded	3.078	2.159	-29.9%***
Did Not Expand	3.021	2.323	-23.1%***
t-test for significance between means reported above: *** p<0.01, ** p<0.05, * p<0.1			

Table 2: Patterns in bankruptcy rates pre- and post- Medicaid expansion.

5 Results

5.1 Difference-in-Differences

I first utilize a difference-in-differences approach to estimate the effect of the ACA Medicaid expansion on the bankruptcy rate. I initially estimate a model with no control variables, the results of which are presented in Table 3. This model implies that the ACA Medicaid expansion is associated with a decrease of approximately 27 bankruptcies per 100,000 residents in a county, or a 10% decrease from the 2011-2013 mean. To ensure that this statistical significance is not the product of statistical abnormalities or heteroskedasticity, I utilize robust standard errors in Model 2, and cluster standard errors by state in Model 3. The coefficient on the interaction terms remains statistically significant in all cases. Given the selection bias, non-parallel trends, and contamination from the 2008 recession recovery, a degree of skepticism ought to be applied to these results.

To address these shortcomings, I next estimate this model using a set of control variables. These estimates are presented in Table 4. The coefficient of interest attributes an approximate decrease in the bankruptcy rate of 18 per 100,000 to the ACA Medicaid expansion, or a 5.9% decrease over pre-treatment bankruptcy rates. This coefficient remains statistically significant, regardless of whether I utilize heteroskedastic robust or clustered standard errors.

I explore several different specifications with controls for how liberal a state's government may be, given that this may have influenced the implementation of the ACA Medicaid expansion. States with more liberal political environments may have invested increased resources in advertising the ACA Medicaid expansion, or may have had additional navigators available to help individuals sign up. Furthermore, liberal states may have had more consumer protections, or better social safety net programs available, confounding the impacts of the ACA Medicaid expansion. If not controlled for, this political heterogeneity may lead to a corrupted estimate of average treatment effect. Thus, I test several different controls, including the percent Democratic margin for president in 2008, 2012, 2016, and whether or not the state's governor in 2014 was a Democrat. As we see in Models 3, 4, and 5 in Table 4, these each have little impact on the average treatment effect.

To further test this model, I utilize several robustness checks, presented in Table 5. As a

VARIABLES	(1) Regular SE	(2) Robust SE	(3) Clustered SE
State Expanded = 1	0.0216	0.0216	0.0216
	(0.0386)	(0.0415)	(0.614)
Post-Expansion = 1	-0.698***	-0.698***	-0.698***
	(0.0385)	(0.0449)	(0.113)
State Expanded*Post-Expansion	-0.277***	-0.277***	-0.277*
	(0.0546)	(0.0545)	(0.147)
Constant	3.021***	3.021***	3.021***
	(0.0272)	(0.0336)	(0.570)
Observations	16,091	16,091	16,091
R-squared	0.058	0.058	0.058
r2_a	0.0574	0.0574	0.0574
F	327.5	431.8	54.16
rss	48142	48142	48142

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Difference-in-difference estimate of impact of ACA Medicaid expansion on the bankruptcy rate per 1,000 residents.

first falsification test, I estimate my model with business bankruptcies per 1,000 as my dependent variable. As we see in Model 1, the interaction term loses all significance – or, in other words, the ACA Medicaid expansion is not associated with a change in the business bankruptcy rate. I next re-estimated my model twice: once with the bankruptcy filers which I classified as low-income and once with high-income bankruptcy filers. We expect the impacts of the ACA Medicaid expansion on the bankruptcy rate to be localized to low-income filers, and indeed that is what we observe. We see a coefficient that lacks any form of statistical significance large Model 2, while in Model 3 we see a statistically significant coefficient.

I next examined the degree to which states that exercised an option to expand their Medicaid programs early may be contaminating my results. To test this, I excluded any states that took advantage of this waiver from my regression.[7] The results of this are presented in Table 5, Models 4 and 5. While the standard error rises[8], the estimate of average treatment effect remains similar

[7]Specifically California, Connecticut, D.C, Minnesota, New Jersey, and Washington (Kaiser Family Foundation, 2012).

[8]This likely occurs because of the loss of observations leading to a loss of predictive power

VARIABLES	(1)	(2) Robust SE	(3) Clustered SE	(4) Clustered SE	(5) Clustered SE	(6) Clustered SE
State Expanded = 1	0.389***	0.389***	0.389	0.387	0.343	0.441
	(0.0404)	(0.0411)	(0.317)	(0.319)	(0.324)	(0.359)
Post-Expansion = 1	-0.0524	-0.0524	-0.0524	-0.0515	-0.0569	-0.0134
	(0.0361)	(0.0415)	(0.0931)	(0.0940)	(0.0993)	(0.0836)
State Expanded*Post-Expansion	-0.180***	-0.180***	-0.180**	-0.180**	-0.181**	-0.221***
	(0.0475)	(0.0485)	(0.0881)	(0.0881)	(0.0876)	(0.0816)
Democratic Margin for President in 2008	-0.872***	-0.872***	-0.872			
	(0.0928)	(0.0842)	(0.720)			
% Pop. 25-44	4.310***	4.310***	4.310*	4.190*	4.228*	4.669**
	(0.510)	(0.547)	(2.288)	(2.250)	(2.158)	(2.255)
Bottom Quintile of Income ($100s)	0.00405***	0.00405***	0.00405	0.00396	0.00429	0.00462*
	(0.000828)	(0.000808)	(0.00264)	(0.00266)	(0.00261)	(0.00249)
Second Quintile of Income ($100s)	-0.00282***	-0.00282***	-0.00282**	-0.00278**	-0.00304**	-0.00342***
	(0.000650)	(0.000619)	(0.00115)	(0.00118)	(0.00117)	(0.00126)
Median Income	2.58e-05***	2.58e-05***	2.58e-05***	2.60e-05***	2.63e-05***	2.51e-05***
	(3.97e-06)	(3.81e-06)	(9.03e-06)	(9.08e-06)	(9.10e-06)	(9.06e-06)
Percent with Subprime Credit	0.0473***	0.0473***	0.0473**	0.0486**	0.0496**	0.0506**
	(0.00255)	(0.00288)	(0.0210)	(0.0211)	(0.0220)	(0.0219)
Percent that own home	-0.0222***	-0.0222***	-0.0222***	-0.0225***	-0.0224***	-0.0233***
	(0.00193)	(0.00220)	(0.00739)	(0.00728)	(0.00718)	(0.00788)
Total Number of Business Bankruptices per 1,000	1.602***	1.602***	1.602***	1.608***	1.627***	1.607***
	(0.117)	(0.146)	(0.483)	(0.483)	(0.484)	(0.488)
Unemployment Rate	0.183***	0.183***	0.183***	0.182***	0.178***	0.175***
	(0.00687)	(0.00774)	(0.0371)	(0.0378)	(0.0435)	(0.0344)
Per-Capita Personal Income	-1.86e-05***	-1.86e-05***	-1.86e-05***	-1.85e-05***	-1.87e-05***	-1.86e-05***
	(1.57e-06)	(1.35e-06)	(4.48e-06)	(4.47e-06)	(4.22e-06)	(5.17e-06)
Percent Living Below Poverty Line	-0.0410***	-0.0410***	-0.0410*	-0.0414*	-0.0411*	-0.0390
	(0.00392)	(0.00433)	(0.0235)	(0.0235)	(0.0231)	(0.0244)
% Pop. Black	2.006***	2.006***	2.006*	2.066**	2.038*	1.795
	(0.116)	(0.178)	(1.023)	(1.002)	(1.018)	(1.092)
% of Pop. Hispanic	-3.150***	-3.150***	-3.150***	-3.132***	-3.046***	-3.341***
	(0.0943)	(0.0879)	(0.718)	(0.719)	(0.673)	(0.749)
Democratic Margin for President in 2012				-0.795		
				(0.679)		
Democratic Margin for President in 2016					-0.553	
					(0.826)	
Governor of State is a Dem.						-0.279
						(0.327)
Constant	0.822***	0.822***	0.822	0.785	0.813	0.985
	(0.250)	(0.261)	(1.025)	(1.036)	(1.040)	(1.104)
Observations	13,382	13,382	13,382	13,382	13,382	12,807
R-squared	0.366	0.366	0.366	0.366	0.363	0.365
r2_a	0.365	0.365	0.365	0.365	0.363	0.364
F	481.3	358.2	37.53	35.60	36.14	42.78
rss	24143	24143	24143	24143	24222	23557

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Testing impact of controls on difference-in-differences estimate of the decrease in bankruptcies per 1,000 attributable to the ACA Medicaid expansion.

to prior estimates. I thus assume that any contamination of results from including these states is minimal. I confirm this assumption in Section 6.2, using a regression specification that should be unimpacted by these early expanders.

Finally, I investigated if there were additional unmeasured or uncontrolled for heterogenities between the treated and control states by adding county and year fixed effects. As we see in Table 5, Model 6, adding fixed effects had little impact on the average treatment effect. My control variables thus seem to be adequately accounting for the idiosyncrasies between the two groups, lending credence to this being an accurate estimate of average treatment effect.

5.2 Fixed Effects with Instrumental Variable

To further establish causality and strengthen this conclusion, I estimate a fixed effects specification. My results from this specification are presented in Table 6. In this model, a 1% increase in instrumented Medicaid enrollment is associated with a 1.483% decrease in the bankruptcy rate. When we consider that instrumented Medicaid eligibility increased by 5% on average in expansion states, we can attribute a decrease by approximately 22.56 bankruptcies per 100,000 residents to the ACA Medicaid expansion. This estimate falls to 15.3 bankruptcies per 100,000 residents when adding controls. These estimates are almost identical as the estimates from my difference-in-differences model, and remain significant when clustering standard errors by state.

To analyze the robustness of this model, I tested a series of confounding predictors of bankruptcy, to ensure that fixed effects were properly accounting for community demographics and macroeconomic trends. These results are presented in Table 7. The coefficient of interest – the coefficient on instrumented Medicaid enrollment – changes only slightly, regardless of which controls I utilize. While it is impossible to test every possible confounding factor, given this representative selection of variables, it seems likely that fixed effects roughly proxy for any observed or unobserved heterogeneity which could be influencing my results.

Finally, as with my difference-in-differences model, I utilize a series of robustness checks to ensure that these results are localized to low income bankruptcy filers. The results of these falsification tests are presented in Table 8. Once again, I find no evidence that an increase in enrollment under

VARIABLES	(1) Business	(2) Above Median Income	(3) Below Median Income	(4) Below Median Income	(5) Below Median Income	(6) Below Median Income
State Expanded = 1	-0.00696	-0.151	0.194	0.185	0.393*	0.393*
	(0.0118)	(0.273)	(0.345)	(0.355)	(0.227)	(0.227)
Post-Expansion = 1	-0.0395***	-0.463***	-0.227***	-0.227***	0.00509	0.00509
	(0.00670)	(0.0892)	(0.0502)	(0.0503)	(0.0469)	(0.0469)
State Expanded*Post-Expansion	0.00538	-0.0333	-0.240***	-0.221***	-0.166***	-0.166***
	(0.00797)	(0.105)	(0.0646)	(0.0667)	(0.0526)	(0.0526)
Constant	0.105***	1.263***	1.671***	1.671***	0.309	0.309
	(0.0101)	(0.252)	(0.319)	(0.319)	(0.536)	(0.536)
Early Expansion States Excluded				✓	✓	✓
Controls					✓	✓
Year Fixed Effects						✓
County Fixed Effects						✓
Observations	16,091	16,091	16,091	14,813	13,382	13,382
R-squared	0.028	0.103	0.029	0.025		
r2_a	0.0279	0.103	0.0286	0.0244	.	
F	32.58	40.52	51.17	41.78	.	.
rss	192.4	9018	18982	18301	.	
Number of county					2,677	2,677
r2_o						0.261
r2_b						0.276
r2_w						0.157
sigma_u						0.827
sigma_e						0.396
rho						0.813
F_f						.

Standard errors clustered by state
*p<.05; **p<.01; ***p<.001

Table 5: Series of falsification tests on difference-in-differences model. I first estimate this model with three unique dependent variables: business bankruptcies per 1,000 residents, low-income individual that filed for bankruptcy per 1,000 residents, and high-income individuals that filed for bankruptcies per 1,000. I then exclude states that began implementing the ACA Medicaid expansion early, to determine the degree to which this contamination of my quasi-experiment could be influencing my results. Next, I test controls and fixed effects. In all cases, I find that the change in bankruptcies is localized to those earning below the median income, and this change is robust to potential contaminates.

VARIABLES	(1)	(2) Robust SE	(3) Clustered SE
Percent of 18-64 Population Enrolled in Medicaid	-1.483***	-1.483***	-1.483**
	(0.117)	(0.152)	(0.682)
Constant	1.966***	1.966***	
	(0.0139)	(0.0177)	
County Fixed Effects	✓	✓	✓
Year Fixed Effects	✓	✓	✓
Observations	18,802	18,802	18,802
R-squared			0.396
Number of counties	3,135	3,135	3,135
F	.	.	70.79
r2_o	0.0240	0.0240	.
r2_b	0.0486	0.0486	.
r2_w	0.396	0.396	.
sigma_u	0.597	0.597	.
sigma_e	0.223	0.223	0.223
rho	0.878	0.878	.
F_f	38.40	38.40	.

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Predicting Inverse Hyperbolic Sine of overall consumer bankruptcy rate per 1,000 residents using an instrumented version of Medicaid enrollment.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES							
Percent of 18-64 Population Enrolled in Medicaid	-1.483***	-1.441***	-1.477***	-1.469***	-1.507***	-1.491***	-1.483***
	(0.152)	(0.117)	(0.117)	(0.117)	(0.118)	(0.117)	(0.117)
% Pop. 25-44		2.854***					
		(0.429)					
Bottom Quintile of Income ($100s)			-0.000437***				
			(0.000153)				
Second Quintile of Income ($100s)				-0.000620***			
				(0.000110)			
Log(Median Income)					-0.369***		
					(0.0462)		
Percent with Subprime Credit						0.00228	
						(0.00145)	
Percent that own home							0.00127
							(0.000997)
Constant	1.966***	1.291***	2.052***	2.188***	5.907***	1.895***	1.872***
	(0.0177)	(0.103)	(0.0347)	(0.0437)	(0.495)	(0.0483)	(0.0756)
County Fixed Effects	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓
Observations	18,802	18,790	18,799	18,799	18,801	18,794	18,801
Number of counties	3,135	3,132	3,135	3,135	3,135	3,134	3,135
F	.	.	.	.	.	.	.
r2_o	0.0240	0.0527	0.0272	0.0301	0.0319	0.0355	0.0229
r2_b	0.0486	0.000813	0.0507	0.0337	0.0188	0.0190	0.0504
r2_w	0.396	0.399	0.396	0.398	0.398	0.396	0.396
sigma_u	0.597	0.584	0.595	0.594	0.593	0.590	0.598
sigma_e	0.223	0.222	0.223	0.222	0.222	0.223	0.223
rho	0.878	0.873	0.877	0.877	0.877	0.875	0.878
F_f	38.40	36.30	38.35	38.42	38.48	34.64	38.32

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 7: Predicting the change in the bankruptcy rate as a function of fixed effects and various controls.

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6) Robust SE	(7) Clustered SE
Percent of 18-64 Population Enrolled in Medicaid	-1.488***	-1.420***	-1.425***	-1.135***	-1.478***	-1.008***	-1.008*
	(0.117)	(0.118)	(0.118)	(0.127)	(0.117)	(0.150)	(0.557)
% Pop. 25-44						2.091***	2.091*
						(0.788)	(1.096)
Bottom Quintile of Income ($100s)						0.000383	0.000383
						(0.000390)	(0.000436)
Second Quintile of Income ($100s)						-0.000967***	-0.000967***
						(0.000272)	(0.000239)
Log(Median Income)						-0.179***	-0.179**
						(0.0646)	(0.0782)
Percent with Subprime Credit						0.00376	0.00376
						(0.00289)	(0.00399)
Percent that own home						0.00381**	0.00381*
						(0.00182)	(0.00198)
IHS(Business Bankruptcies per 1,000)	0.164***					0.133***	0.133**
	(0.0199)					(0.0464)	(0.0589)
Unemployment Rate		0.0148***				0.0119***	0.0119
		(0.00207)				(0.00348)	(0.00750)
Per-Capita Personal Income			-7.61e-06***			-6.74e-06***	-6.74e-06***
			(6.67e-07)			(1.66e-06)	(1.80e-06)
Percent Living Below Poverty Line				0.00451***		0.00194	0.00194
				(0.00136)		(0.00241)	(0.00321)
% Pop. Black					-0.823***	-0.510	-0.510
					(0.299)	(0.570)	(0.549)
% of Pop. Hispanic					-0.0301	0.305	0.305
					(0.209)	(0.719)	(0.629)
Constant	1.947***	1.829***	2.236***	1.432***	2.042***	3.030***	
	(0.0140)	(0.0248)	(0.0277)	(0.0286)	(0.0351)	(0.751)	
County Fixed Effects	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓
Observations	18,802	18,799	18,485	15,668	18,754	15,362	15,362
R-squared							0.319
Number of counties	3,135	3,134	3,082	3,135	3,126	3,073	3,073
F	.	.	.	.	.	.	49.91
r2_o	0.0265	0.0510	0.0655	0.0151	0.00128	0.0323	.
r2_b	0.0383	0.00506	0.00263	0.0284	0.152	0.00609	.
r2_w	0.399	0.400	0.403	0.301	0.396	0.319	.
sigma_u	0.596	0.583	0.580	0.591	0.647	0.594	.
sigma_e	0.222	0.222	0.222	0.214	0.223	0.212	0.212
rho	0.878	0.873	0.872	0.884	0.894	0.887	.
F_f	37.91	35.62	37.17	34.79	31.70	22.75	.

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

	Business Bankruptcies		Above Median Income Filers		Below Median Income Filers	
	No Controls	Controls	No Controls	Controls	No Controls	Controls
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Percent of 18-64 Population Enrolled in Medicaid	0.0340	0.0743	-1.083	-0.618	-1.642***	-1.599***
	(0.104)	(0.0623)	(0.665)	(0.470)	(0.114)	(0.599)
County Fixed Effects	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
Observations	18,802	15,362	18,802	15,362	18,802	18,433
R-squared	0.059	0.045	0.475	0.388	0.206	0.214
Number of counties	3,135	3,073	3,135	3,073	3,135	3,073
F	36.44	18.92	75.42	47.10	785.1	27.60
sigma_e	0.0891	0.0821	0.194	0.178	0.216	0.216

Standard errors clustered by state
*p<.05; **p<.01; ***p<.001

Table 8: I examine the impacts of an increase in instrumented Medicaid enrollment on the business bankruptcy rate, bankruptcy filings among those earning below the median income, and bankruptcy filings among those earning above the median income. My results are consistent with what we would expect if the ACA Medicaid expansion were driving this change, strengthening the causality I propose.

the ACA is associated with a change in the business or high-income consumer bankruptcy rates; however, it is strongly associated with bankruptcy filings among those that are earning below the median income, even when clustered by state. This corroborates my identifying assumptions, given that any change in the bankruptcy rate as a result of the ACA Medicaid expansion should be entirely localized to low income bankruptcy filers.

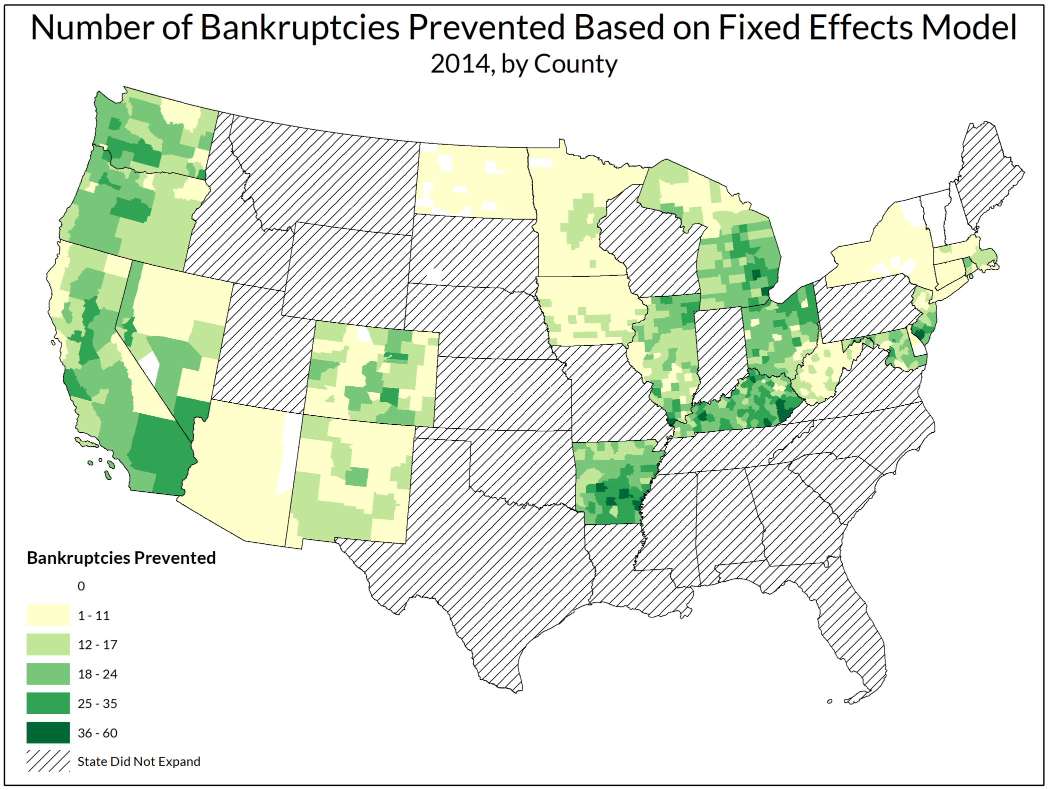
6 Discussion

6.1 Overall Bankruptcy Rate

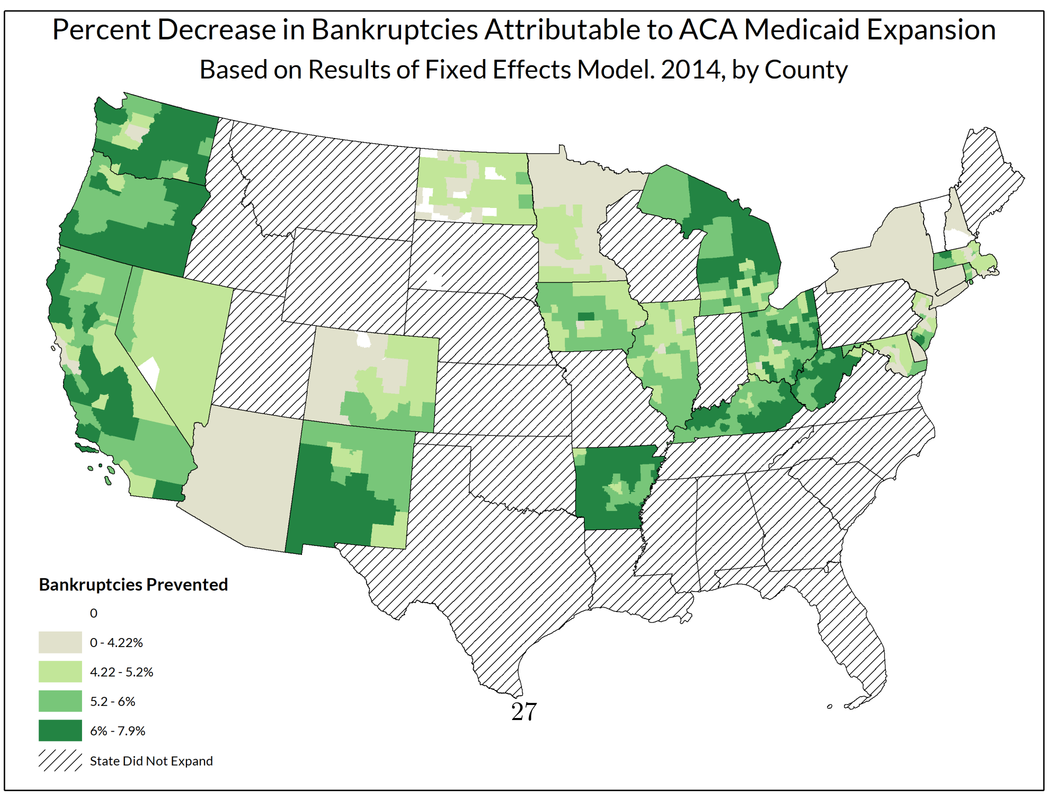
In Figures 5a and 5b, I present the number and percent of bankruptcies in 2014 that were prevented by the ACA Medicaid expansion among all Americans in Medicaid expansion states, based on the results of my Fixed Effects model. Among all bankruptcy filers, the ACA Medicaid Expansion decreased the bankruptcy rate by between 1-8% in each county. A comparison of how many bankruptcies each of my approaches predicts were prevented between 2014-2016 by the ACA Medicaid expansion is presented in Figure 6. Each of my approaches produces fairly consistent point estimates of between 75,000-125,000, as well as roughly equivalent 95% confidence intervals, with the exception of my fixed-effects model, which is afflicted with large standard errors. As we see in Figure 7, this implies that the ACA Medicaid expansion is responsible for between one-third to two-thirds of the decrease in bankruptcies between 2014-2016. This is a fairly significant impact, and one that is in similar to estimates from Brevoort et al (2018), although given the large confidence intervals, a degree of uncertainty that remains.

6.2 Low Income Bankruptcy Rate

One reason for the large confidence interval is that we are measuring the impact on the overall bankruptcy rate, for both rich and poor Americans. When we examine only the change in bankruptcy rate for Americans earning below their states median income in Figure 8, we see a much starker impact.



(a) Number of bankruptcies that were prevented by the ACA Medicaid expansion, based on the results of my fixed effects model when using data from the overall bankruptcy rate. Breaks selected using Jenks natural breaks optimization.



(b) Percent decrease in bankruptcies that can be attributed to ACA Medicaid expansion. Breaks selected using Jenks natural breaks optimization.

Figure 5

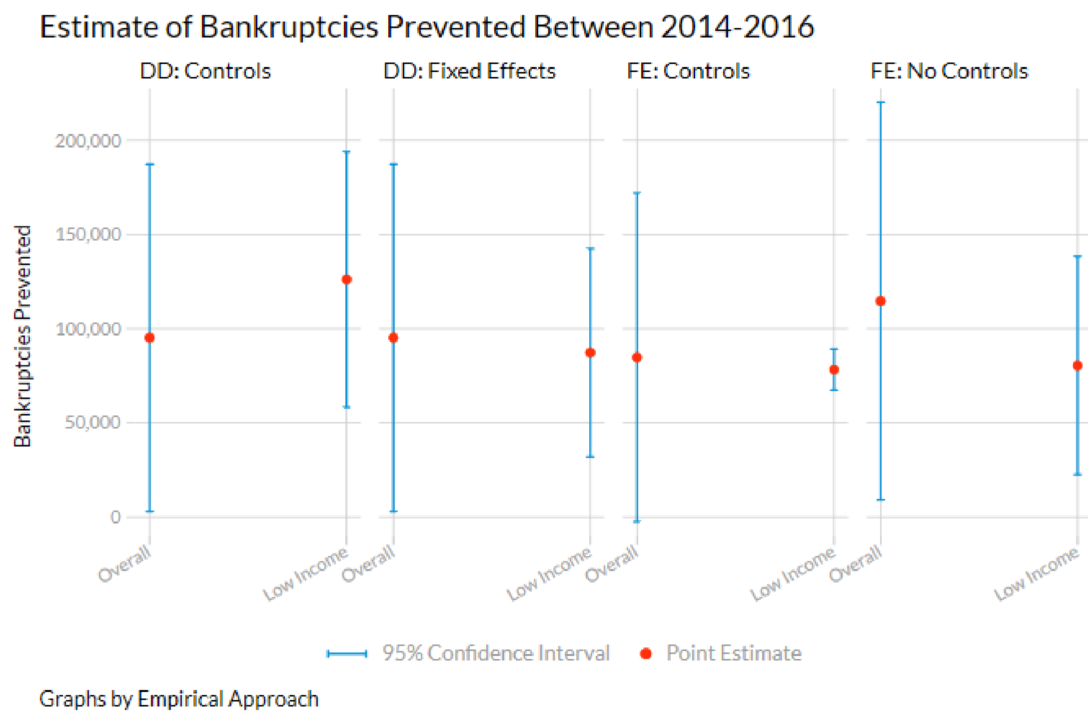


Figure 6: Comparing each model's point estimate and 95% confidence interval of the number of bankruptcies in Medicaid expansion states that the ACA Medicaid expansion prevented between 2014-2016. 95% confidence interval is derived from standard errors clustered by state.

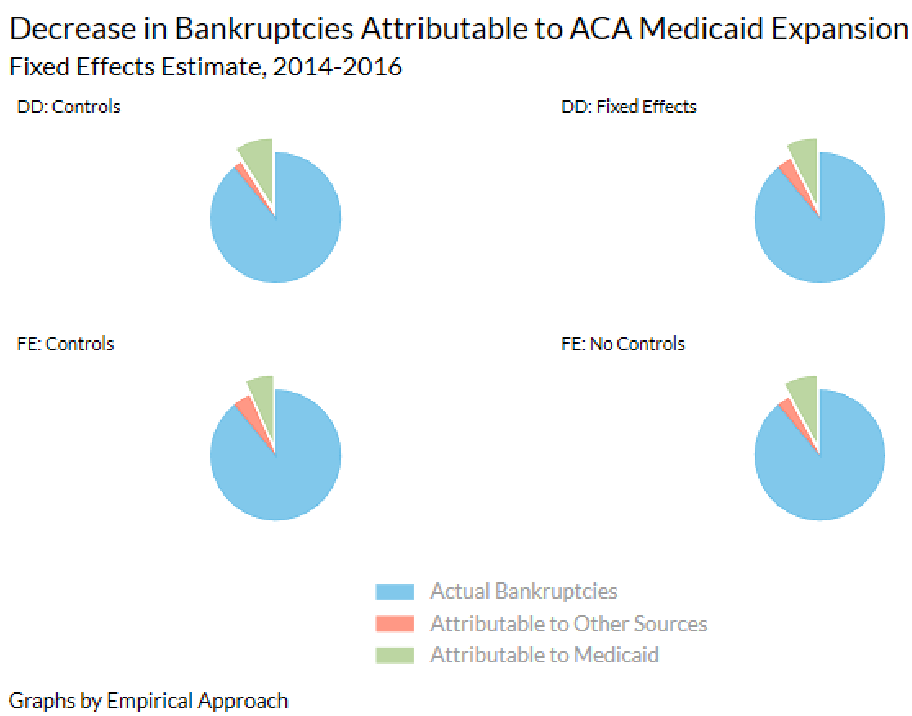


Figure 7: Comparing decrease in overall bankruptcy rate that can be attributed to the Medicaid expansion under the ACA.

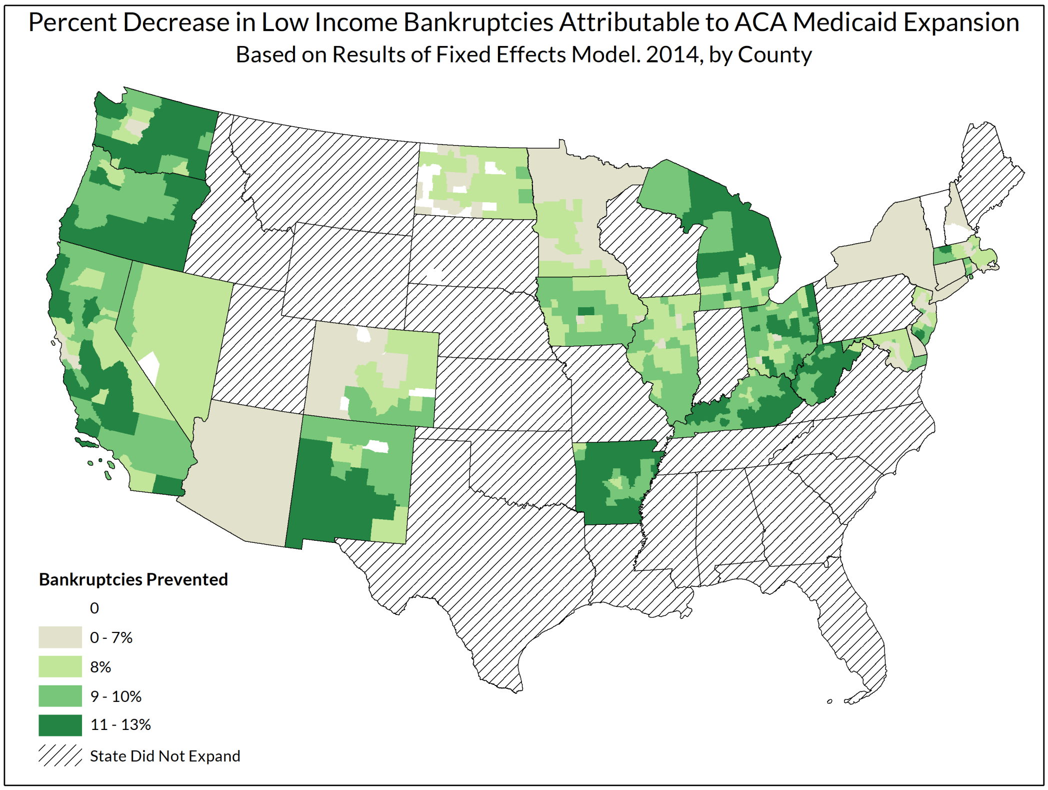


Figure 8: Percentage decrease in bankruptcies among those earning below the median income that can be attributed to the ACA Medicaid expansion, given the results of my fixed effects model. Breaks selected using Jenks natural breaks optimization.

This model implies that the ACA Medicaid expansion resulted in a 1-13% decrease in each county's bankruptcy rate year-over-year in 2014 among those earning less than the median income. Figure 6 reports the number of bankruptcy filings among those earning below the median income that each approach predicts were prevented between 2014-2016 due to the ACA Medicaid expansion. As we see, these models consistently provide a point estimate of between 75,000 and 125,000 bankruptcies, with very similar 95% confidence intervals. Given the similarity between the estimates from the overall bankruptcy model and this model, my case for causality is further validated. This estimate is, again, in line with estimates from Brevoort et al (2018)

These estimates imply that the ACA Medicaid expansion is responsible for the majority of the decrease in bankruptcy rates among the low income between 2014-2016, as we observe in Figure 7. This is, of course, assuming that there were no factors influencing bankruptcy upwards; it is possible that there could be macroeconomic trends forcing bankruptcies among the low income upwards, and but-for this trend the impact of the ACA on the bankruptcy rate would have been relatively lower. Regardless of this potential, however, if my estimates are accurate, the bankruptcy rate would have been at least 6-8% higher in expansion implementing states in a counterfactual scenario.

7 Limitations

There are several factors that could threaten the validity of my study, which I briefly summarize in Table 9, and address in detail in this section.

I am first limited by my data. The overall bankruptcy rate, and even the low-income bankruptcy rate I calculate may be problematic. I cannot be certain whether these bankruptcy filers filed for bankruptcy for medical reasons, what their credit scores were, or if they were even eligible for Medicaid. Individuals may misreport their average monthly income, or there could be underlying clerical errors. It is further not uncommon for the wealthy to hide their income prior to bankruptcies (Goldstein, 2013). This could induce downwards bias in my results, given that the ultra-wealthy may end up included in the "low income" bankruptcy group. While there is nothing I can do to address

Source	Reason	Potential Impacts	Mitigation Attempts	Risk After Mitigation
Panel A: Overall				
Bankruptcy Data	We cannot be certain that any specific bankruptcy actually occurred among an individual that was eligible for Medicaid.	Slight	• Falsification Tests • Recategorizing bankruptcy filers based on their stated income.	Slight
Nature of ACA Expansion	There were several components of the ACA that launched at the same time which may contaminate my results.	Moderate	• Quasi-Experimental Design • Control Variables • Fixed Effects	Slight
Recovery from 2008 Recession	Both models may accidentally pick up some signal from recovery from the 2008 recession	Moderate	• Control Variables	Slight
States that Implemented the ACA Medicaid Expansion Early	Several states expanded their Medicaid programs early. While not widely advertised, this introduces the potential for significant bias.	Significant	• Falsification Tests • Fixed Effects Approach	Slight
Panel B: Difference-in-Differences Approach				
Non-Random Treatment Assignment	Treatment was not randomly assigned. There are significant economic, demographic, and social differences between treated and non-treated states.	Significant	• Control Variables • County and Year Fixed Effects • Fixed-Effects Specification	Moderate
Non-Parallel Trends Pre-Treatment	The trends in bankruptcy filings between the treatment and control group were not parallel pre-treatment, which could significantly bias my results.	Significant	• Control Variables • County and Year Fixed Effects • Fixed-Effects Specification	Moderate
Spillover Effects	Non-expansion states also experienced a slight increase in Medicaid enrollment post-2014, due to increased public awareness, and a new, easier enrollment process introduced nationwide.	Moderate	• Fixed-Effects Specification	Slight
Panel C: Fixed Effects Approach				
Unobserved or Unmeasured Shocks	Unobserved and unmeasured shocks may be impacting the bankruptcy rate (or Medicaid enrollment or eligibility).	Significant	• Instrumental Variable • County and Year Fixed Effects • Control Variables	Slight
Medicaid Data	Medicaid enrollment and eligibility estimates may lack precision.	Moderate	• Using Census Bureau weights	Slight

Table 9: Major threats to the validity of my estimates.

this given my resources, I assume that any bias that is introduced due to my bankruptcy data is a very limited downwards bias. These are official court house records. Significant discrepancies or inaccuracies would be concerning for the stability of our legal system.

I am equally limited by my estimates of Medicaid eligibility and enrollment, which may lack precision. Because I am relying on the annual American Community Survey PUMS, sampling bias in either direction is a likely possibility; however, I utilize the ACS provided person weights, which greatly mitigates this potential inaccuracy[9].

My difference-in-differences model is limited by the fact that treatment was not randomly assigned. As we observe in Table 10, states in the expansion and non-expansion groups were demographically and economically heterogeneous prior to the Medicaid expansion. On average, people in non-expansion states tended to be poorer, and were more likely to have subprime credit scores. This implies that all else constant, were non-expansion states to implement the Medicaid expansion, the impact would likely be much greater. Complicating this conclusion, however, is the fact that counties in non-expansion states simultaneously tended to have fewer low-income bankruptcy filers, due perhaps to the poverty trap described earlier, where low income individuals are unable to file for bankruptcy because they are too poor (Kiel, 2018). It is thus possible that an extrapolation of these results may result in either an understatement or an overstatement of the number of bankruptcies prevented, depending on which of these two is a greater factor.

More significantly, I am limited by non-parallel pre-treatment trends in bankruptcy filings between the two groups pre-treatment. As with the selection bias, if left unmitigated, this has the potential to significantly bias my estimates in either direction.

To address both the selection bias and non-parallel trends, I explore Difference-in-Differences specifications with control variables, county and year fixed effects, and falsification tests. I further use an alternative fixed effects regression approach. Given the consistency among all of these estimates, I believe that the risk that selection bias and non-parallel trends poses to the validity of my results is significantly lessened, as long as I utilize control variables or fixed effects.

This model is additionally limited by the fact that Medicaid enrollment increased in both ex-

[9]See Appendix B for more details

pansion and non-expansion states, because of increased publicity and a new enrollment process. This results in a contamination of my untreated group, which may bias the average treatment effect downward. I believe that the impacts of this are limited, as the difference in mean Medicaid enrollment between 2013 and 2014 in non-expansion counties was a statistically insignificant 1-3% (see Table 2), compared to 10% or greater in expansion states (Wehby, 2018; Wachino, 2014).

Some of the aforementioned limitations in my difference-in-differences models are addressed through a fixed effects approach. However, this model is limited as well, mainly by the fact that there are many unobservable shocks that may impact enrollment beyond the change in eligibility under the ACA Medicaid expansion. I utilize several strategies in an attempt to mitigate any bias introduced. I use county-level fixed effects, which theoretically captures all within-county heterogeneity, thus resulting in a more accurate estimation of the treatment effect when compared with prior studies (Gross, 2011). I further use Medicaid eligibility as an instrumental variable, in an attempt to isolate the change in Medicaid enrollment to just that which is due to the change in Medicaid eligibility, the driver under the causality I posit.

More generally, the Medicaid expansion causality I propose, and the specific estimate for bankruptcies prevented I calculate may be biased upwards by other components of the Affordable Care Act. These components include a ban on health insurance policies that cap the amount of benefits given out in a year or over a lifetime, a requirement that insurance companies offer coverage to those with pre-existing conditions, and a mandate that all individuals must purchase health insurance. Given that these were national policies, in theory the impact of these changes should be homogeneous across states within a year. However, it is entirely possible that some localities may have been more impacted. In some regions pre-treatment, benefit caps may have been common, while in other regions, benefit caps may have been uncommon. I believe that the impact of this is limited, given my quasi-experimental design[10], the relative consistency between the estimated treatment effect from both my fixed effects model and difference-in-differences model, the set of control variables I use[11], and that the change in bankruptcies is localized to those earning below

[10]It is possible that this too may be compromised by the other components which came into force in 2014 (e.g. the health insurance exchanges and the requirement to buy health insurance). I assume any heterogeneity in the impact of these programs is captured by either my fixed effects or my control variables.

[11]Specifically the controls for how Democratic a state was, given that heterogeneity between how these components

Variables	Non-Expansion States	Expansion States
Panel A - Economic Variables		
Business Bankruptcies per 100,000 Residents	9.861	9.321***
Consumer Bankruptcies per 100,000 Residents	295.8	300.6
Median Income	42,623	47,275***
Per-Capita Personal Income	36,654	39,279***
Percent with Subprime Credit Score	33.68	27.60***
Bottom Quintile of Income Distribution ($100s)	189.2	210.3***
Second Quintile of Income ($100s)	343.4	382.2***
Bankruptcy Rate (Earning Below Median Income)	169.8	183.9***
Bankruptcy Rate (Earning Above Median Income)	117.7	109.6***
Panel B - Demographic Variables		
Population (1,000s)	66.74	138.0***
Unemployment Rate	7.67	8.158***
Percent Below the Poverty Line	17.48	15.45***
Percent of Population 18-24	0.0905	0.0891***
Percent of Population 25-44	0.233	0.232***
Percent of Population 45-64	0.274	0.286***
Percent of Population that is Non-white	0.247	0.151***
Percent of Population that is Black	0.124	0.0427***
Percent of Population that is Hispanic	0.0987	0.0784***
Percent of Population that Owns Home	72.97	74.48***

t-test for significance between means reported above: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 10: Comparison of the mean of key variables between 2011-2013 by treatment group.

the median income. However, despite these mitigation attempts, the risk of a slight upwards bias remains.

I do not evaluate whether this decrease in bankruptcies was driven by a decrease in medical debt, or whether it was driven by an increase in earnings due to less sick time. It is plausible that preventing the shock to income from a prolonged or serious illness may be a major driver of medical bankruptcy, especially given the fact that many low income individuals work jobs where they are not eligible for sick-time pay. For the 78% of Americans who reported living paycheck to paycheck in 2017 (CareerBuilder), missing even a week of pay for medical reasons is enough to place them in precarious financial position.

Finally, I am limited in what conclusions can be drawn regarding the actual medical bankruptcy rate. While a 3% decrease in the overall bankruptcy rate may seem more in line with Dobkin's (2018)

were implemented or supported on a state-level would likely be driven by how conservative or liberal a state is.

estimate of a 6% medical bankruptcy rate, rather than Himmelstein and Warren's (2005) estimate of 50% of bankruptcies being medically caused, it is essential to realize that the vast majority of individuals in the United States receive their insurance through the private sector. I evaluate only one portion of the Affordable Care Act, and fully ignore the impact of its private health insurance reforms. A far greater driver of the medical bankruptcy rate could be the privately insured with a high deductible plan who face unexpected medical costs, or who discover that their insurance company refuses to pay claims because a medical provider was out of network, or an emergency procedure was not pre-approved (Rosenthal, 2015). Moreover, while an individuals medical bills from an extended hospital stay may be covered, they may still have lost income during their illness, and thus been unable to meet other financial obligations. These other expenses may result in an individual still having to declare bankruptcy, even if covered by health insurance.

8 Conclusion

I have found evidence that the ACA Medicaid expansion prevented between 75,000-125,000 bankruptcies between 2014-2016 in Medicaid expansion-implementing states. Without this program, the bankruptcy rate would have likely been at least 8% higher between 2014-2016 among those earning below the median income in Medicaid-expansion states, and at least 6% higher overall. This is a significant impact for just one portion of the ACA, and yet another way that the ACA Medicaid expansion has left poor individuals in an improved state of being.

While this impact may seem numerically small, it should be noted that I do not examine how medical debt burden was impacted by this program. Bankruptcies in general are relatively rare in the population: a recent New York Times survey found that for every 100 people who reported struggling with medical debt, only 2 had filed for bankruptcy that year (Sanger-Katz, 2016). Extrapolating this to my findings, it is conceivable that 2.5 million or more individuals may have avoided a crippling level of medical debt between 2014-2016 as a result of the ACA Medicaid expansion. This program is impacting real people, such as Alex Andrews, who was shot by a home intruder. Medical coverage under the ACA Medicaid expansion protected him from a shock of $500,000 in medical bills (Campbell, 2017), which would have likely resulted in an eventual bankruptcy filing.

From a policy perspective, this provides yet another reason to support the ACA Medicaid expansion. Ignoring the improved health and wellness outcomes among those who become eligible for Medicaid, the financial benefits are clear. This incentives all states to fully implement the ACA Medicaid expansion, given the expenses on the court systems, businesses and individuals within these states that could be avoided through this program. These effects would likely be even more pronounced were Southern states to implement this program, given that these states are somewhat poorer on average than those that implemented the ACA Medicaid expansion. Future research should focus on how the recently approved Medicaid expansions in Virginia, Idaho, Maine, Nebraska, and Utah impact the bankruptcy rate, to examine if consistent results are noted. Further research should also focus on the impacts of private insurance reform, as well as the implications of recent Medicaid-for-all proposals. Finally, future research should not lose sight of what should be the fundamental reason behind a Medicaid expansion: improving the health and well-being of Americans.

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A Literature Review

A.1 Uniqueness of Medical Debt

There are many reasons behind bankruptcies that range from overconsumption to excessive usage of credit cards (Zhu, 2011). However, an overarching cause of bankruptcy is the inability to cover emergency expenses. A Federal Reserve survey found that only 59% of Americans in 2017 would be able to produce $400 of cash in an emergency (Federal Reserve, 2018). This, combined with a lack of health insurance, are the major drivers of medical bankruptcies. In 2015, one in five adults between 18-64 years old had some form of medical debt (Karpman, 2017).

Medical emergencies are unique when compared with other types of emergencies that might incur large costs. Broadly speaking, a broken down car can be repaired later; a damaged washer can be replaced when you can afford it. However, when an individual is facing a life threatening medical condition, they must go to a hospital or risk death. Further complicating the problem, while you can often estimate the price of repairing your car or buying a new washer in advance, you can never be certain what you will be billed when you are admitted to the hospital. Moreover, even if an individual avoids a doctor visit for a symptom, medical issues may become worse (and more expensive) over time.

Once an uninsured individual receives the bill, there is no "returning" the health care received. They can choose to pay the bill in full, appeal to charity for a reduction in cost, or utilize a payment plan. Because of these factors, the presence of medical debt is the largest signifier of a potential future bankruptcy filing (Domowitz, 1999).

This issue of medical debt only becomes compounded when one considers that uninsured and low-income individuals often avoid preventative care because they cannot afford it (Kaiser Family Foundation, 2017). Low-income individuals with chronic but manageable health conditions are more likely to be admitted to the hospital for something that could have been prevented given primary care intervention (Kruzikas 2004). In this way, medical problems that could have been addressed by a primary care provider for $100-$200 (Saloner, 2015) may instead result in thousands of dollars of costs if a hospital stay is required (Moore et al, 2014). Such an expense is enough to push a financially vulnerable individual into bankruptcy. Furthermore, this fails to consider any lost earnings due to missing work during illness, which could be even more detrimental to the poor (Himmelstein, 2005).

A.2 Medical Bankruptcy

There is a debate regarding what percentage of bankruptcies are actually "medical bankruptcies". Using survey data, Himmelstein and Warren (2005) suggest that 46.5% of bankruptcies in 2001 were caused by medical costs. This figure is controversial in the literature, with other scholars such as Dranove (2006)[12] and Hackney (2015)[13] using slightly different approaches to measuring medical bankruptcies, and finding lower rates[14] as a result. Despite this, Himmelstein et al. continue to find consistent percentages of around 50%-60% in survey data, finding 62.1% of bankruptcies in 2007 were caused by medical expenses (2009) and that 52.9% of bankruptcies in Massachusetts are caused by medical costs (2011).

Jacoby (2010) proposes that the disparity between survey-based and bankruptcy filing analysis approaches to estimating the medical bankruptcy rate occurs because medical debt owed to care providers is often shifted to more generic payment methods such as credit cards. A 2012 survey found that 47% of low-income households with medical debt had transferred part of it to a credit card (McElwee, 2016). However, this argument has been challenged in recent research from Dobkin et al (2018). Using a novel dataset that combines survey data and credit reports of patients aged 18-64 at California hospitals, Dobkin finds that approximately 6% of uninsured adults admitted to a hospital proceed to file for bankruptcy as a result. It is important to note that this study only looked at medical events that resulted in a hospital admission, ignoring all other types of visits. Given that many may not be admitted to a hospital after visiting an emergency room, but still incur costly medical bills or have to miss work (and thus lose income), it seems likely that this figure is a lower-bound to the true number of medical bankruptcies.

[12] Dranove recategorized Himmelstein's data so that only those that explicitly stated that they filed for bankruptcy because of medical costs were listed as medical bankruptcies.

[13] Hackney used courthouse record analysis (i.e. he looked at the kinds and amounts of debts individuals owed and categorized them as "medically bankrupt" if they owed a significant amount to medical providers).

[14] 17% and 23.1%, respectively

While much of the literature surrounding medical bankruptcies is observational, there are a few quasi-experimental studies which examine how specific medical events change the likelihood of filing for bankruptcy. Both cancer (Ramsey, 2013) and spinal cord injury (Hollingsworth, 2007) increase the likelihood that one will file for bankruptcy; however, Medicaid, Medicare, and Social Security mediate this impact (Ramsey, 2013; Hollingsworth, 2007). Using data on cancer patients from 1995-2009, Ramsey (2013) finds that being over the age of 65 (and thus eligible for Medicare and Social Security) is associated with a fourfold decrease in the bankruptcy rate, when compared with younger cancer patients[15]. Similarly, among individuals hospitalized for a brain or spinal cord injury, being on Medicaid is associated with an approximately 50% lower bankruptcy rate, as compared with those who have private or no insurance (Hollingworth, 2007), a drop of approximately 3% in the overall bankruptcy rate. Given these facts, it follows that the ACA Medicaid expansion would decrease the bankruptcy rate, however there has been little direct research to confirm this.

A.3 Impacts of Public Health Expansions

Public health expansions are socially beneficial. Using an estimate of Medicaid eligibility, Currie (1995) finds that the Medicaid expansions of the 1990s are associated with a decrease in child mortality. Similar effects have been noted as a result of the ACA Medicaid expansion, including a decrease in death, an increase in self-reported health and an increase in subjective well-being among poor and non-white individuals in Medicaid-expanding states (Sommers, 2012; Flavin et al 2018). The ACA Medicaid expansion also increases the likelihood that people will receive preventative care (Wherry, 2016), have better health (Brown, 2018), get needed supplies to manage their diabetes (Myerson et al, 2018), and have access to care, particularly among young adults (Chavez 2018).

The impacts of the ACA Medicaid expansion with respect to the uninsurance rate is similarly clear in the literature. The uninsurance rate is lower in Medicaid expansion states than in non-expansion states. (Sommers, 2015; Courtemanche, 2017). More quantitatively, Frean et al. (2017) finds that the ACA Medicaid expansion decreased the uninsurance rate by between 9-19%. This

[15] One must remember that there could be other reasons for this decrease, such as the fact that individuals over 65 years of age likely lost no earnings as a result of illness, as one might expect that they are both retired and receiving Medicare and social security benefits.

increase in the insurance rate happened regardless of whether or not the Medicaid expansion was driven by an expansion of true public health insurance, as in Kentucky, or whether it was through a private insurance company supported with public dollars, as in Arkansas (Sommers, 2016). Furthermore, the Medicaid take up rate increased by between 1-3% post-ACA expansion in non-expansion states (Wehby, 2018), which is potentially due to a new simplified enrollment process implemented nationwide that makes it easier for individuals to apply for Medicaid, regardless of their state of residence (Wachino, 2014).

Public health insurance expansions improve the financial well-being of individuals. Sommers (2013) finds that Medicaid decreases the poverty rate by 1%, thus keeping between 2.6-3.4 million individuals out of poverty, with the greatest impact localized among disabled adults, elderly, children, and non-white minorities. Similar effects are seen in government programs at large, with payments directly to individuals through TANF and unemployment benefit being associated with a decrease in bankruptcies (Fisher, 2005). In this regard, public health expansions are no different. The CHIP and Medicaid expansions of the 1990s are associated with an 8% decrease in bankruptcies as a result of a 10% increase in Medicaid eligibility (Gross et al 2011). Unfortunately Gross's (2011) study is somewhat limited by the state-level nature of Gross's data, the fact that they do not examine how the impacts vary based on the pre-bankruptcy income of the filer, and the fact that it was published before the ACA Medicaid expansion went into effect.

The Massachusetts reform of 2006 lowered out of pocket costs for individuals (Miller, 2012), which aligns with Finkelstein's (2016) findings that the Oregon Health Insurance Medicaid lottery resulted in a decrease in the probability of having unpaid bills sent to collectors. However, Finkelstein (2016) did not find a statistically significant reduction in bankruptcies among participants in this experiment; however this study was performed only a year after the expansion. It is possible that changes in bankruptcies lag behind new health insurance legislation, since health insurance expansions do not retroactively cover individuals' expenses. Individuals in Michigan that were eligible and enrolled in Medicaid were found to have better credit scores (Miller et al, 2018). This generalizes to the U.S. at large, with Brevoort (2017), finding that the ACA Medicaid Expansion decreased the amount of unpaid medical bills by $3.4 billion over two years, increased credit availability for individuals by $520 million a year, and prevented approximately 25,000 bankruptcies a year (Brevoort, 2018).

While Brevoort et al (2018) estimate using a difference-in-differences approach that 25,000 bankruptcies a year were prevented by the ACA Medicaid expansion in expansion states, they do not examine the robustness of this estimate, nor do they ensure that this decrease in bankruptcies is localized specifically to those who become eligible for Medicaid under the ACA, as their focus is on the impacts of the Medicaid expansion on credit availability. To my knowledge, there have been no studies to specifically determine the number of bankruptcies prevented by the ACA Medicaid expansion. Furthermore, the majority of prior analyses of the ACA utilize state level data. While state level data enables an evaluation of overall trends across states, it misses trends that may be present within states. While a state on aggregate may have low bankruptcy and poverty rates, areas within a state may have high bankruptcy and poverty rates. It is these areas that may provide the strongest evidence as to the ramifications of the ACA Medicaid expansion on the bankruptcy rate.

In this study, I provide an estimate of the ACA Medicaid expansion's impact on the bankruptcy rate using county level data. I additionally stratify bankruptcies based on the pre-bankruptcy income of the filer, to ensure that my results are localized to those who actually became eligible for Medicaid. Finally, I perform a series of robustness checks to ensure the accuracy of my results.

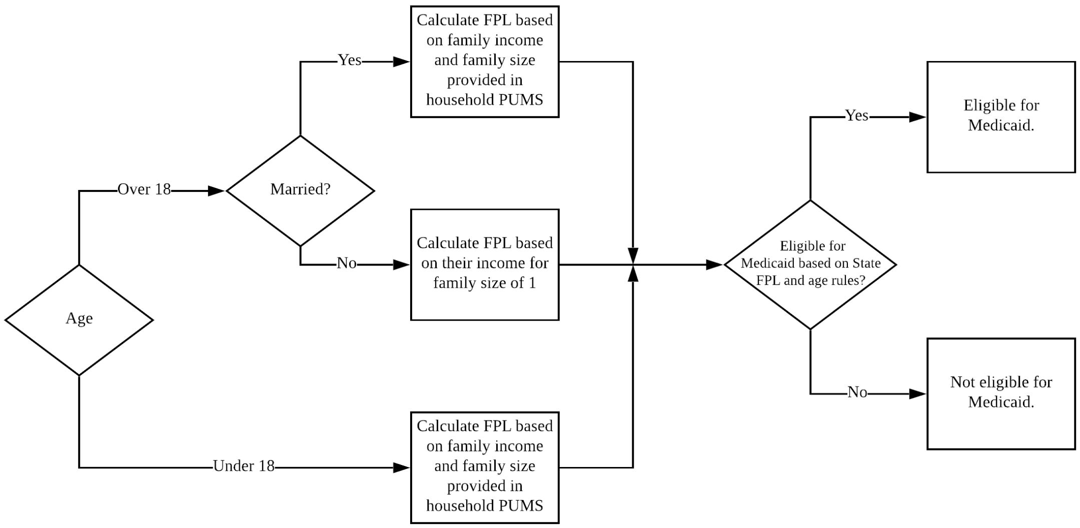
B Medicaid Simulation

I utilize a protocol developed and enhanced by Currie et al (1995), Selden (1998), Banthin (2003), and most recently the Urban Institute (Haley, 2014). I use the American Community Survey (ACS) Public Use Microdata Sample (PUMS) and a Python script to simulate the number of individuals eligible for Medicaid in a public use microdata area (PUMA). This differs slightly from prior approaches which tend to use the Current Population Survey (CPS); however, this approach is supported in the literature (Boudreaux 2011) and is utilized by the Urban Institute (Haley 2014).

My script models the income pathway of Medicaid eligibility at a per-person level in my sample. I first determine an individual's age. If they are under the age of 18, I use their family's income as listed on the household level ACS data to estimate whether or not they are eligible for CHIP and Medicaid under their state's rules, based on what percent poverty that income falls in for their family size, as defined by the U.S. Dept. of Health and Human Services for that year (2018).

If the individual is over the age of 18 and listed as unmarried in the PUMS, I use their own income as listed on the individual level ACS PUMS to calculate whether or not they are eligible for Medicaid. If the individual is married, I use their family income as listed in the ACS PUMS to determine whether or not they are eligible for Medicaid.

My output is separated into three categories: children eligible for CHIP, children eligible for Medicaid, and the percentage of adults 18-64 eligible for Medicaid. I ignore adults 65 years of age or older, since these individuals are typically eligible for Medicare, and the impact of the Medicaid expansion should be limited among this group. Each individual in the ACS PUMS is assigned a weight by the Census Bureau to ensure that aggregate data is correctly calculated. I utilize this weight to produce a weighted estimate of the percent of individuals in an PUMA eligible for Medicaid. This process is summarized in the diagram below.



Finally, I convert the data from a PUMA level to a county level using a geo-spatial crosswalk. I use geocorr 2014 from the Missouri Census Data Center (2010) to build a weighted county-level average of Medicaid eligibility. This is necessary because PUMAs, unlike counties, are a population-based geospatial area. Each PUMA contains between 100,000-200,000 individuals, whereas a county can have anywhere between thousands to millions of people. A PUMA may be composed of multiple counties in rural areas, or a county may be split into multiple PUMAs in urban areas. Furthermore, PUMAs rarely nest nicely within counties, instead slicing seemingly arbitrarily through counties.

There are several potential sources of error in this process. The ACS yearly estimates rely on a minuscule sample of approximately 1% of US households (Gomez, 2017) and thus can have a quite large margin of error (Fuller, 2018). I attempt to correct for this using the ACS provided weights (U.S. Census Bureau, 2010), however a slight upwards or downwards bias may remain. Further bias may be introduced during the PUMA to county crosswalk, given that multiple counties may exist in a single PUMA. Despite my attempts to correct this via a geocorrelation weighted average, my estimates may overstate or understate the true percentage eligible for Medicaid depending on the county.

Given these weaknesses, I present several alternative tests and specifications which do not rely on my estimate of Medicaid eligibility to boost the robustness of my conclusions.

C Impacts of the ACA Medicaid Expansion on Other Financial Measures

To further validate these results, I briefly explore the impact of the ACA Medicaid expansion on several precursors to bankruptcies.[16] I focus only on the fixed effects model, given that this model does not rely on the assumption of parallel trends in each of my dependent variables pre-treatment.

C.1 Eviction Filings

Evictions filings are often a precursor for bankruptcy. Under U.S. law, individuals are generally granted an automatic eviction stay if they file for bankruptcy, assuming that their only reason for eviction is non-payment of rent; individuals may thus be induced to file for bankruptcy to stay an eviction. Moreover, medical debt drives housing insecurity, including the eviction rate (Seifert, 2006; Pollitz, 2014). Given these two factors, evictions should decrease in areas where there is an increase in individuals that become eligible for (and enroll in) Medicaid.

I used my fixed effects model with instrumented Medicaid enrollment to estimate the impact of an increase in Medicaid enrollment on the Inverse Hyperbolic Sine of the eviction rate per 100 renters filed within a county (Priceton, 2018). The results of this regression are presented in Table 11. There does, indeed, appear to be a connection between an increase in Medicaid enrollment as a result of the ACA Medicaid expansion and a decrease in eviction filings. This relationship becomes insignificant when clustering standard errors by state, which I attribute to within-state variations in the impacts of an increase in Medicaid on the eviction rate. It is conceivable that some areas within a state might have experienced a large increase in Medicaid eligibility, but there exists a local culture of owning ones' home, and thus there would be little change in the eviction rate. A more thorough analysis is needed to test this hypothesis.

Thus, we have preliminary evidence that the ACA Medicaid expansion decreased eviction filings, substantiating my previous findings; however, further research is needed to confirm the specific

[16]For a more complete analysis of additional financial impacts of the ACA Medicaid expansion, see Antonisse's (2018) literature review.

magnitude of this impact.

C.2 Mortgage Delinquency

I next examine the impacts of the ACA Medicaid expansion on the rate of mortgages that are 30-89 days delinquent. Individuals who have missed this many mortgage payments may still avert a bankruptcy, but are clearly in financial trouble, making this an clear precursor to bankruptcy. If the ACA Medicaid expansion was preventing bankruptcies, we would expect to see a decrease in the number of homes with delinquent mortgages.

To test this hypothesis, I retrieved data from the Consumer Financial Protection Bureau (2018) on the number of homes that were 30-89 days delinquent [17], and then performed my fixed effects analysis.

My results are presented in Table 12. We again see a statistically significant relationship between an increase in instrumented Medicaid enrollment and a decrease in mortgage delinquency. This is consistent with what we would expect to find given our prior results; however, further research is needed to confirm the specific treatment effect on mortgage delinquency.

C.3 Subprime Credit

Poor credit is an obvious predictor of impending or future bankruptcy (Domowitz, 1999). Individuals with poor credit may have fallen behind on bill payments, or may have had prior bankruptcies, which increases the likelihood of a future bankruptcy. Furthermore, those struggling with medical debt hypothetically have a higher likelihood of missing payments, and prior research has found that the ACA Medicaid expansion resulted in a decrease in subprime credit scores (Antonisse, 2018).

In Table 13, I examine the impact of an increase in Medicaid enrollment on the inverse hyperbolic sine of individuals per 1,000 with subprime credit (GeoFRED, 2018). Unfortunately, there is

[17] Unfortunately, this data is only available at a state-level, so I am no longer performing a county level analysis. I additionally had to normalize this data by retrieving the total housing units in a state from the U.S. Census Bureau, and then multiplying by the average annual rate of homes 30-89 days delinquent in each state. I then divide this number by the total housing units in the state to find the percent of homes in an area that were hypothetically 30-89 days delinquent, and then multiplied by 100,000 to estimate the rate. Unfortunately, this process introduces significant upwards bias in this rate, as it assume that each housing unit is a house with a mortgage. This is not the case, as many people own their home outright, while others may be vacant and are owned by a bank.

	(1)	(2)	(3)	(4)
VARIABLES		Robust SE	Clustered SE	Clustered SE
Percent of 18-64 Population Enrolled in Medicaid	-1.085***	-1.085***	-1.085	-1.370
	(0.176)	(0.183)	(0.852)	(0.893)
County Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓
Controls				✓
Observations	15,000	15,000	15,000	11,676
R-squared	0.011	0.011	0.011	0.015
Number of counties	2,600	2,600	2,600	2,411
F	31.55	27.03	1.418	3.492
sigma_e	0.292	0.292	0.292	0.290

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11: Predicting change in eviction filings as a result of an increase in Medicaid enrollment after the ACA.

	(1)	(2)	(3)	(4)
VARIABLES		Robust SE	Robust SE	+ Clustered SE
Percent of 18-64 Population Enrolled in Medicaid	-0.820***	-0.820	-0.604*	-0.604*
	(0.305)	(0.527)	(0.339)	(0.324)
Constant	8.834***	8.834***	9.146***	
	(0.0378)	(0.0660)	(1.378)	
State Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓
Controls			✓	✓
Observations	306	306	245	245
R-squared				0.893
Number of states	51	51	49	49
F	.	.	.	76.17
r2_o	0.157	0.157	0.428	.
r2_b	0.0157	0.0157	0.384	.
r2_w	0.865	0.865	0.893	.
sigma_u	0.310	0.310	0.311	.
sigma_e	0.0647	0.0647	0.0483	0.0463
rho	0.958	0.958	0.976	.
F_f	130.7	130.7	51.47	.

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 12: Predicting inverse hyperbolic sine of number of mortgages 30-89 days delinquent per 1,000 as a function of a change in instrumented Medicaid enrollment.

little to be concluded, given the large standard errors. This could be due to unobserved and unmeasurable heterogeneities between counties that may not be captured in year or county fixed effects, or due to an incorrect model specification for analyzing this relationship; I believe that could we control for these factors or structure the model appropriately, we would see a statistically significant relationship, given prior (Antonisse, 2018) research.

VARIABLES	(1) Controls
Percent of 18-64 Population Enrolled in Medicaid	-0.0514
	(0.142)
County Fixed Effects	✓
Year Fixed Effects	✓
Controls	✓
Observations	14,787
Number of counties	2,958
R-squared	0.261
F	102.5
sigma_e	0.0939

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 13: Predicting the inverse hyperbolic sine of the number of individuals with subprime credit per 1,000 residents as a function of instrumented Medicaid enrollment.