a new bankruptcy filing (2.8%). Given that those affected by the Medicaid expansions comprise a much smaller group than those ages 18 to 64, these estimates suggest much larger effects for those who newly enrolled in Medicaid as a result of the expansions.

Previous Research

The existing literature on the effect of health insurance on personal finance is much less developed than the corresponding literature on access to care and health outcomes. Nonetheless, as the burden of health care costs has grown, more attention has focused on the burden that those costs place on families' income (e.g., Blumberg, Waidmann, Blavin, & Roth, 2014; Caswell, Waidmann, & Blumberg, 2012) and whether that burden may change with the ACA's Medicaid expansion (e.g., Caswell, Waidmann, & Blumberg, 2014; Hill, 2015). The number of empirical papers that specifically study the causal effect of health insurance expansions on financial outcomes related to personal credit, debt, and bankruptcy, however, is much more limited.

Gross and Notowidigdo (2011) estimate the effect of previous Medicaid expansions (1992-2004), mostly covering children and parents, on personal bankruptcy filings. The authors use aggregated state-level data on personal bankruptcy filings provided by the Administrative Office of the U.S. Courts, combined with other sources, and estimated a simulated-instrumental-variables model commonly used to study previous Medicaid expansions (Currie & Gruber, 1996). In essence, this approach exploits within-state variation across eligible groups over time to identify the effect of expansions on bankruptcy filings. The authors find that a 10-percentage-point increase in Medicaid eligibility resulted in an 8% reduction in personal bankruptcies.

Finkelstein et al. (2012) use the Oregon Health Insurance Experiment to study the effect of access to Medicaid on medical debt and medical out-of-pocket expenditures, in addition to health care utilization and health. This was a random experiment where, through a lottery, uninsured adults in Oregon with family income up to 100% of the federal poverty level (FPL)—slightly below the ACA's Medicaid income-eligibility threshold—randomly acquired the ability to enroll in Medicaid. About *1 year* after enrollment, using linked administrative data, the authors estimate that Medicaid enrollment reduced the probability of unpaid medical bills sent to collection by 6.4 percentage points, or an average reduction in the amount owed of $390 (see Table VII in Finkelstein et al., 2012). From survey data on lottery participants, they estimate that insurance reduced the probability of (see Table VIII in Finkelstein et al., 2012): out-of-pocket expenses (20.0 percentage points), owing money for medical expenses (18.0 percentage points), borrowing money or skipping bills to pay medical bills (15.4 percentage points), and being refused treatment because of medical debt (3.6 percentage points).

More recent work by Mazumder and Miller (2016) studied the effect of the Massachusetts health insurance expansion that began in April 2006, which was the template for the ACA, on multiple financial outcomes related to personal credit and debt. In addition to bankruptcy filings, this work investigated the effect on the total balance among all credit accounts, debt past due on all accounts, debt past due as a percentage of total debt, and the amount of third-party collections. The authors used the Federal Reserve Bank of New York Consumer Credit Panel covering years 1999 to 2012. This is a unique and nonpublicly available data source, produced by the credit agency Equifax, of consumer-level data available to researchers employed with the U.S. Federal Reserve Bank system. Their identification strategy—used previously by Miller (2012) as well as the present article—uses variation in exposure to the reform immediately prior to implementation in order to identify the effect of the reform. Specifically, they use the prereform rate of uninsured among nonelderly adults across counties in Massachusetts as their measure of exposure. The authors estimate that, across all individuals age 18 to 64, the reform decreased the total amount of debt past due ($182; 22%) and the fraction of past-due debt to total debt (0.6 percentage points; 10%), decreased total collections balances ($12; 20%), improved creditworthiness as measured by risk scores (2.4 points; 0.5%), and reduced the likelihood of personal bankruptcy (0.2 percentage points; 19%).

Finally, a recent working paper by Hu et al. (2016) studied the effect of the ACA Medicaid expansions on financial well-being. These researchers use quarterly data from the Federal Reserve Bank of New York Consumer Credit Panel, covering calendar years 2010 through 2015, and implement a differences-in-differences analysis using a synthetic control group of states that did not expand Medicaid. Specifically, these authors study total debt, debt past due, credit card debt, number of nonmedical bills in collections, and balance on nonmedical collections. They estimate that the balance on nonmedical collections decreased by approximately $600 to $1,000 per newly enrolled Medicaid beneficiary as a result of the expansions.

New Contribution

The present article contributes the growing literature in several ways. First, it extends the work of Gross and Notowidigdo (2011) by studying a much broader expansion of Medicaid. That is, their study covered previous Medicaid expansions focused on low-income children and parents, whereas the ACA Medicaid expansions also cover low-income childless adults. It builds on the work by Finkelstein et al. (2012) and Mazumder and Miller (2016) as the ACA Medicaid expansions cover a much broader geographic area (28 states and DC), compared with two states (Oregon or Massachusetts). This article also focuses on the low-income Medicaid population, like Finkelstein et al. (2012), but unlike Mazumder and Miller (2016), which includes all nonelderly adults in Massachusetts.

Importantly, this work goes beyond the recent paper by Hu et al. (2016) insofar as it studies both nonmedical and medical collection balances, in turn, compared with only nonmedical collections, as well as the flow of new medical collections and derogatory debt. This is a significant contribution for several reasons. Most important, medical collections are directly related with medical out-of-pocket spending risk, which is the direct mechanism through which the expansions might influence consumers' personal finances. While nonmedical collections may also be influenced by the expansions, the mechanism is seemingly less direct. Furthermore, studying the incidence of new medical collections more closely addresses whether medical spending risk changed as a results of the expansions, compared with total balances on medical collections that may take time to adjust. Finally, the addition of new derogatory balances, which include new medical collections in addition to other unpaid debt, sheds some light on the magnitude of any decreased flow of unpaid bills. In short, this work contributes to a growing body of literature that is important for policy makers to consider when debating the costs and benefits of expanding their Medicaid programs.

The Affordable Care Act Medicaid Expansions

Medicaid expansions were the intended mechanism through which most uninsured low-income Americans in all states were to obtain health insurance coverage via the ACA. Those with income up to 138% of the FPL would be income eligible, unlike "categorical" eligibility requirements such as being disabled or a single parent, in large part expanding eligibility of existing Medicaid programs to low-income childless adults. States also had the option to expand their programs as early as 2010, prior to the intended country-wide expansion on January 1, 2014 (summarized below).[1] The 2012 Supreme Court ruling *National Federation of Independent Business v. Sebelius*, however, made the decision for states to expand their Medicaid programs optional. And as of March 2016, 30 states and the District of Columbia had implemented Medicaid expansions (The Henry J. Kaiser Family Foundation, 2016).[2]

Table 1 summarizes the timing of the ACA Medicaid expansions as they relate with the timing of the data used in this analysis, discussed in more detail below, covering years 2010 through 2015. Connecticut, the District of Columbia, Minnesota, and 48 California counties expanded prior to 2014.[3] Twenty-one states expanded January 1, 2014; Michigan and New Hampshire expanded mid-2014; and Pennsylvania and Indiana expanded early 2015. Finally, Alaska and Montana both expanded after August 2015.[4]

Table 1. Timing of the Affordable Care Act Medicaid Expansions, 2010 to 2015.

	Pre-expansion period					Expansion year	Post-expansion period			
Time = *t* equals time with respect to expansion (calendar year − expansion year)	−5	−4	−3	−2	−1	0	1	2	3	4

	Pre-expansion period					Expansion year	Post-expansion period			
Calendar year = *y* of expansion\states (month\day)										
2010: CT (4/1), DC (7/1)						2010	2011	2012	2013	2014
2011: MN (3/1), CA (10 counties; 7/1)[a]					2010	2011	2012	2013	2014	2015
2012: CA (38 counties; 1/1)[b]				2010	2011	2012	2013	2014	2015	
2014: 23 states[c], CA (10 counties; 1/1)[d]		2010	2011	2012	2013	2014	2015			
2015: PA (1/1), IN (2/1)	2010	2011	2012	2013	2014	2015				
Nonexpansion states										
2014: 22 states[e]		2010	2011	2012	2013	2014	2015			

Note. States identified in italics expanded Medicaid using an 1115 waiver.

Source. The Henry J. Kaiser Family Foundation (2016); Harbage and King (2012).

[a] CA counties (10): Alameda, Contra Costa, Kern, Los Angeles, Orange, San Diego, San Francisco, San Mateo, Santa Clara, Ventura. [b]CA counties (38): Riverside, San Bernardino, Santa Cruz, Alpine, Amador, Butte, Calaveras, Colusa, Del Norte, El Dorado, Glenn, Humboldt, Imperial, Inyo, Kings, Lake, Lassen, Madera, Marin, Mariposa, Mendocino, Modoc, Mono, Napa, Nevada, Plumas, San Benito, Shasta, Sierra, Siskiyou, Solano, Sonoma, Sutter, Tehama, Trinity, Tuolumne, Yolo, Yuba. [c]Expanded January 1, 2014: AZ, *AR*, CO, DE, HI, IL, *IA*, KY, MD, MA, NV, NJ, NM, NY, ND, OH, OR, RI, VT, WA, WV; Expanded mid-year: *MI* (April 1, 2014); *NH* (August 15, 2014). [d]CA counties (10): Fresno, Merced, Monterey, Placer, Sacramento, San Joaquin, San Luis Obispo, Santa Barbara, Stanislaus, Tulare. [e]AL, FL, GA, ID, KS, ME, MS, MO, NE, NC, OK, SC, SD, TN, TX, UT, VA, WI, WY; Expansion states treated as no expansion states (expansion after last year of credit bureau data): AK (September 1, 2015), *MT* (January 1, 2016), LA (to be determined).

The fraction of individuals who were uninsured, among those with incomes up to 138% of the FPL, decreased more rapidly in states that expanded their Medicaid programs. Figure 1 reports statistics from the American Community Survey on the population targeted for Medicaid eligibility. It excludes states that expanded Medicaid before and after January 1, 2014, in order to make clear comparisons. The left panel of Figure 1 reports the percentage point change in the fraction who was uninsured among the population age 18 to 64 with incomes up to 138% of the FPL in expansion and nonexpansion states. Between 2013 and 2015, this fraction decreased by 15.5 percentage points in expansion states compared with 9.6 percentage points in nonexpansion states. The right panel reports the percentage point change in the key measure of exposure to expansion we use in this analysis: the fraction of the population that was *both* uninsured and had income up to 138% of the FPL among all individuals aged 18 to 64. This fraction decreased by 3.4 percentage points in expansion states between 2013 and 2015, compared with 2.4 percentage points in nonexpansion states. The reported changes between 2013 and 2015 are also larger compared with the changes between 2013 and 2014, highlighting that the first expansion year was indeed a year of transition.

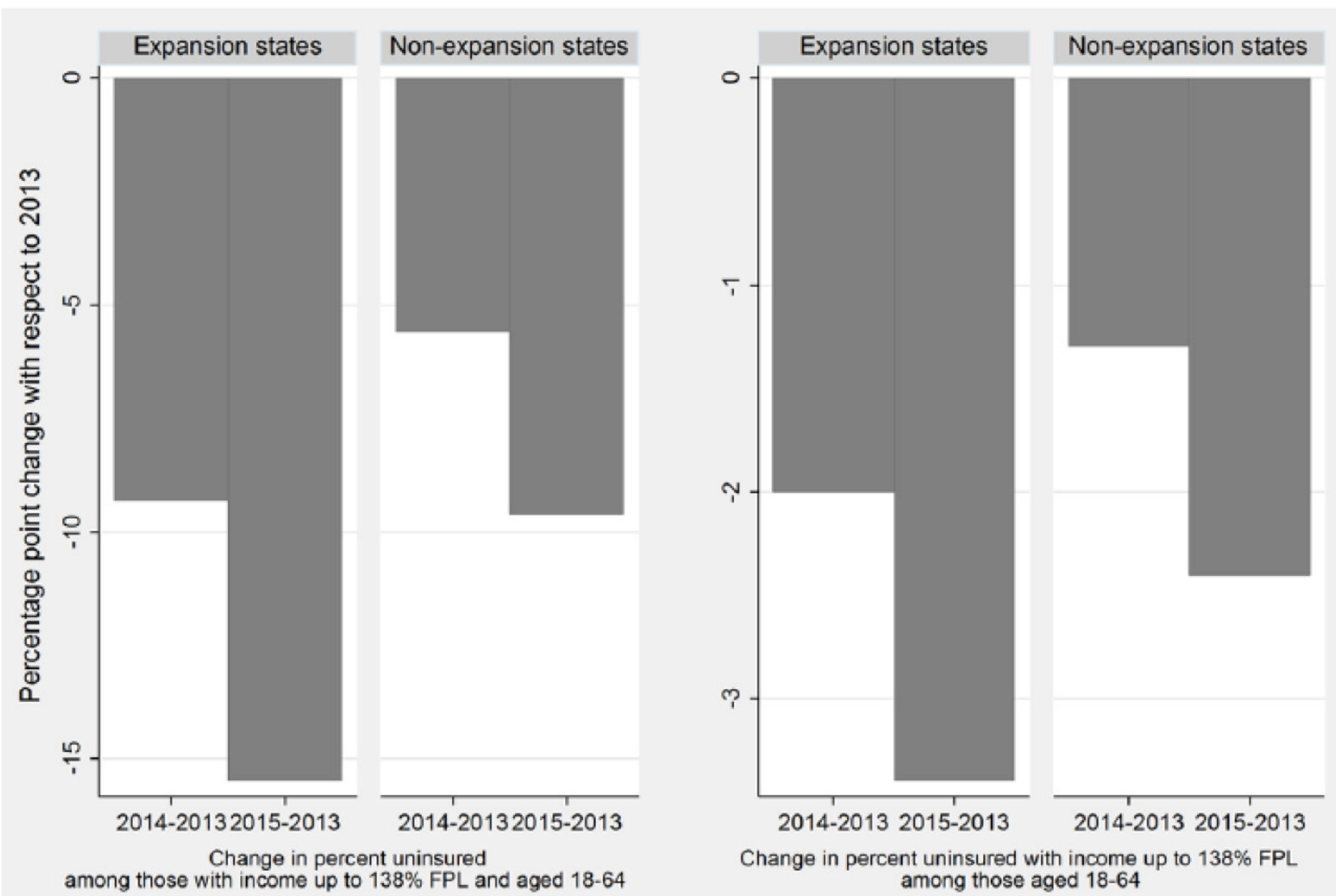

Figure 1. Percentage point change in the rate of uninsured among the targeted Medicaid eligible population, 2015 to 2013 and 2014 to 2013.

Note. Estimates exclude states that expanded Medicaid before or after January 1, 2014.

Source. Authors' calculations using the American Community Survey.

Data

Credit Bureau Data

The unique and primary data of interest on financial outcomes is from one of the three major credit bureaus.[5] It is a nationally representative 2% sample of consumers from a universe of more than 250 million consumer records. This work uses six annual data archives covering years 2010 through 2015. Each archive represents the characteristics of consumers at the end of August for a given year. It is designed such that the same consumers appear in each year for which they have a record in the master file, while consumers newly entering the credit market enter in proportion to their representation relative to the consumer population for a given year. As a result, the sample is appropriate to use as a single-year cross-section, repeated cross-sections, as well as a longitudinal panel. The final subsample of consumers aged 18 to 64 in a given year consist of 23.5 million consumer-year observations, covering years 2010 through 2015, or approximately 3.9 million consumers per year.

Note that the population represented in data from the three nationwide credit reporting agencies differs from the civilian noninstitutionalized population typically analyzed using federal household surveys. In particular, to be included in these data, at a minimum it is necessary for an individual to interact with the formal credit market and/or have some public record information, for example, the former could include an application for credit (approved or disapproved), having an account with a utility company, or a visit to the hospital and subsequent nonpayment for medical services received, and the latter may include a civil judgement, tax lien, or bankruptcy. Recent research by the Consumer Financial Protection Bureau carefully documents how the population in credit bureau data differ with respect to the general population (Brevoort, Grimm, & Kambara, 2015; Consumer Financial Protection Bureau, 2014). In short, these authors report that approximately 11% of U.S. adults are not represented in the credit bureau data and that such individuals are more likely to reside in lower income areas, which is a limitation of this study.[6]

Outcomes

Using the credit bureau data we study several outcomes that reflect various degrees of financial stress, and a direct relationship with medical spending risk, that may be influenced by the Medicaid expansions, which we categorize into "stocks" and "flows." This distinction is important insofar as any effect of the Medicaid expansions may be more apparent on recent events (flows) compared with the cumulative summary of past events both recent and distant (stocks), especially during the early phase of the expansions.

In terms of stocks, we study Vantage credit score, which is a credit risk score with a range of 350 to 850 that has become an increasingly popular metric used to summarize consumers' overall creditworthiness. A higher score represents a lower predicted risk of delinquency. Credit scores are categorized here as a stock as they incorporate past and current information from consumers' credit history. We also study total balance on all credit accounts, which includes all accounts in good standing, as well as those that are not and could be on a consumer's record for many years. In addition, we study balances past due (90 to 180 days), and past due balance as a percentage of total balances. Overall, these are very general financial outcomes insofar as they reflect many types of debt combined (e.g., mortgages, auto loans, third-party collections, etc.), which may be influenced by the Medicaid expansions.

In addition to the general outcomes above we study medical and nonmedical collections balances, in turn. This addition is important as medical collections are directly related to medical out-of-pocket spending risk—the direct mechanism through which we hypothesize Medicaid coverage may improve personal finances. Nonmedical collections may be influenced by the expansions insofar as there is an income effect of Medicaid coverage, whereby the previously uninsured have more disposable income as their out-of-pocket spending for medical care decreases with Medicaid coverage. Furthermore, note that medical collections are defined here as only those that originated with a medical provider. They do not include balances initially paid via credit obtained from a source other than the provider, such as a credit card. Such debt will be included in "nonmedical" collections.

We also study a number of flow outcomes that occurred within the previous 6 months with respect to the date a given data archive was culled. Importantly, we study incidence of new medical collections that occurred in the last 6 months, a flow outcome directly relevant to medical spending risk. Relatedly, we study new derogatory debt balances, excluding mortgages, which occurred in the last 6 months. Derogatory is a term used by credit agencies for debt that is not in good standing where the creditor took significant action to retrieve any unpaid balance and includes categories such as collections, repossessions, and bankruptcy. New medical collection balances are included in new derogatory balances; however, we are not able to identify them separately in our data. We are only able to identify new derogatory mortgage balances, which we exclude as we consider them much less directly relevant to the Medicaid expansions. Finally, we study bankruptcy filings that occurred within the past 6 months, which are severe and low-probability events.[7]

Control Variables

In terms of more general information related to individuals, the credit bureau data include information on the age of each consumer as well as their zip code and county for each year.[8] It does not include other demographic information such as race and ethnicity or sex, nor does it include data on income, wealth, or health insurance status. Therefore, we rely on external information related to each consumer's county of residence.

Key to the estimation strategy, discussed in the following section, are data on the relative size of the potentially affected Medicaid expansion population in the calendar year immediately prior to expansion. Specifically, we use estimates on the percent of each county's population, aged 18 to 39 and 40 to 64, that was uninsured with family income up to 138% of the FPL—the income eligibility threshold in expansion states. These age categories were chosen because they are the most refined categories available. These data are produced by the Small Area Health Insurance Estimates (SAHIE) group at the U.S. Census Bureau. They are model-based estimates based on information from the American Community Survey, IRS federal tax returns, the 2010 decennial Census, population estimates from the Census Bureau's Population Estimates Program, County Business Patterns data from the Business Register, and administrative data on participation in Medicaid, CHIP, and the Supplemental Nutrition Assistance Program (Bauder, Luery, & Szelepka, 2015; U.S. Census Bureau, 2016).

For each Medicaid expansion state we merge the SAHIE statistics with the consumer data by age-groups (18-39 and 40-64) and county for each year of the consumer data. The SAHIE estimates correspond to the calendar year prior to a given state's Medicaid expansion, or county in the case of California. For nonexpansion states we merge the SAHIE statistics to consumers in the same way but use data corresponding to 2013, the year for most Medicaid expansion states.

We also incorporate data on the rate of unemployment from the Bureau of Labor Statistics, Local Area Unemployment Statistics program (Bureau of Labor Statistics, 2016b). County-level unemployment rates, corresponding to August of a given year, are merged with the consumer data by county and year.

Empirical Method

The empirical approach is similar to that used by Miller (2012) and Mazumder and Miller (2016), who studied the effects of the Massachusetts health insurance expansion. Like these authors' work, we exploit two sources of variation to estimate the effect of the ACA Medicaid expansions on outcomes observed in the credit-bureau data. The first source of variation is that across individuals, similarly exposed to the Medicaid expansions, who resided in states that expanded their Medicaid program compared with those in states that did not. The second source of variation is, within states that expanded Medicaid and those that did not, variation in the pre-expansion rate of exposure across county age-category groups. Exposure is measured as the percent of the county population that is both uninsured and with income up to 138% FPL for each age category, 18 to 39 and 40 to 64.[9]

Unlike the Massachusetts expansion, however, not all states or counties within states (i.e., California) expanded Medicaid via the ACA simultaneously. The timing of the expansions with respect to the timing of the six credit bureau data files (2010 to 2015) is summarized in Table 1. Each row includes states that expanded Medicaid during the same calendar year (e.g., the first row includes both CT and DC, which expanded in 2010). Effectively, three states and 48 California counties (of 58) expanded prior to January 1, 2014; 23 states and 10 California counties expanded on January 1, 2014; two states expanded mid-2014; and two states expanded in 2015.[10] Our preferred specification incorporates information from all 50 states and the District of Columbia from 2010 through 2015, where "event time" (indexed by subscript *t*) is defined as the difference between the reference year of data (indexed by *y*) and the calendar year in which a given state or county expanded Medicaid. Table 1 shows that the number of observed pre- and post-expansion time periods across geographies range between zero and five.

This empirical approach assumes that, in the absence of Medicaid expansion, trends in outcomes among individuals in similarly exposed county-age categories would have evolved similarly across expansion and nonexpansion geographies. As these assumptions are not directly testable, we examine differences in outcomes in Medicaid geographies relative to nonexpansion geographies before and after the reform, taking into account higher or lower rates of exposure to the expansions. Should the outcomes studied not exhibit a trend before the reform, yet exhibit a different trend after implementation, we have more confidence that the expansions caused any changes in the outcomes.

To test for differences in the pre- and post-expansion period trends, we estimate models that take the following form, which we refer to the "event-study approach":

$$Y_{icgy} = \sum\nolimits_t \Big\{ \delta_{1t} 1\left(Time = t\right) \cdot E_c \cdot ULE138_{cg} + \delta_{2t} 1\left(Time = t\right) \cdot E_c + \delta_{3t} 1\left(Time = t\right) \cdot U$$
$$\beta_1 ULE138_{cg} \cdot E_c + \beta_2 ULE138_{cg} + \rho \,\mathrm{Age}_{iy} + \phi U_{cy} + \gamma_c + \eta_y + e_{icgy},$$

where *i* represents a given individual, *c* is a given U.S. county, *g* indexes one of two age categories (18-39; 40-64), *y* represents calendar year (2010 to 2015, as available), and *t* equals calendar year, *y*, minus the Medicaid expansion calendar year for county *c*. Specifically, $t \in$ (−4 or more, −3, −2, −1, 0, 1 or more). The first Medicaid expansion year is indicated by *t* = 0, and *t* = −1 is the reference time period. The dependent variable Y_{icgy} equals a financial outcome of interest for individual *i*, in county *c*, in age-group *g*, during calendar year *y*. Counties within states that expanded Medicaid are identified by E_c, and $ULE138_{cg}$ equals the percentage of individuals in county *c* and age-group *g* that are uninsured and have income up to 138% of the FPL in the calendar year prior to Medicaid expansion. Finally, Age_i is a dummy variable, indicating whether consumer *i* is age 40 to 64, and U_{cy} is the unemployment rate in county *c* during August of calendar year *y*, γ_c are time invariant county effects, η_y are calendar year time effects (2013 reference year), and e_{icgy} is the error term.

Coefficient estimates from the three-way interaction terms, $\widehat{\delta}_{1t}$, represent the change in a given outcome *Y* in expansion states compared with nonexpansion states, per percentage point change in exposure, with respect to the year prior to expansion (*t* = −1). Coefficient estimates from the two-way interactions of the time period dummies with expansion counties, $\widehat{\delta}_{2t}$, capture trends in the outcomes over time that are specific to the expansion counties. Likewise, coefficients from the two-way interactions between the time period dummies and the exposure proxy, $\widehat{\delta}_{3t}$, account for possible trends in the exposure rate over time common to county-age group categories. Finally, estimates $\widehat{\delta}_{4t}$ capture trends in event time common to both expansion and nonexpansion geographies.

Should trends in outcomes be similar prior to the expansions the corresponding three-way interaction coefficient estimates should equal zero (*t* = −4 or more, −3, −2). We formally estimate *F* tests where the null hypothesis is that all corresponding pre-expansion period coefficient estimates for a given outcome are jointly equal to zero $\left(\widehat{\delta}_{1,-4\ or\ more} = \widehat{\delta}_{1,-3} = \widehat{\delta}_{1,-2} = 0\right)$, which we use as the basis for evaluating whether an outcome exhibits differential pre-period trends, or not. Should the expansions cause a change in a given outcome, a break in trend should be apparent and result in nonzero coefficient estimates during initial expansion year and the post-period (*t* = 0, 1 or more). We group estimates together for four or more pre-expansion periods, and more than one post-expansion period, as not all geographies have the same number of pre- and post-expansions periods (see Table 1).[11,12] Finally, all standard errors are clustered at the state level to address serial correlation in the outcomes studied. This is important insofar as many of the same consumers are included in the data for multiple time periods, and Medicaid expansion occurred at the state level (Bertrand, Duflo, & Mullainathan, 2004).

To estimate the effects of the Medicaid expansions on a given outcome we estimate models that take the following form, which we refer to as the “triple-difference design”:

$$
\begin{aligned}
Y_{icgy} = {} & \delta_{11}\,\mathrm{Post}_t \cdot E_c \cdot ULE138_{cg} + \delta_{21}\,\mathrm{Post}_t \cdot E_c + \delta_{31}\,\mathrm{Post}_t \cdot ULE138_{cg} + \delta_{12}\,\mathrm{Expansion\ y} \\
& \cdot E_c \cdot ULE138_{cg} + \delta_{22}\,\mathrm{Expansion\ year}_t \cdot E_c + \delta_{32}\,\mathrm{Expansion\ year}_t \cdot ULE138_{cg} + \beta_1 ULE138 \\
& \cdot E_c + \beta_2 ULE138_{cg} + \theta_1\,\mathrm{Post}_t + \theta_2\,\mathrm{Expansion\ year}_t + \rho\,\mathrm{Age}_{it} + \phi U_{cy} + \gamma_c + \eta_y + e_{icgy},
\end{aligned}
$$

where Post_t is an indicator for one or more periods after the initial Medicaid expansion calendar year, and $\mathrm{Expansion\ year}_t$ is an indicator for the calendar year in which county *c* expanded Medicaid, the “transition” year.

This model is similar in structure to that of Equation (1), where the three- and two-way interactions for all pre-expansion years are omitted. The estimate of interest is $\widehat{\delta}_{11}$, which is the reduced-form effect of the Medicaid expansions per unit of exposure on a given outcome *Y*. This model accounts for any effects that occurred during the initial expansion year ($t = 0$) separately, which may be considered a transition period and are captured by the coefficient estimates $\widehat{\delta}_{12}$ $\widehat{\delta}_{22}$ $\widehat{\delta}_{32}$, and $\widehat{\theta}_2$.

Limitations

A limitation of this study is that the postimplementation period observed in the data is most likely too short to reflect full implementation of the Medicaid expansions. The channel through which we postulate the Medicaid expansions affect financial outcomes is via decreased risk of out-of-pocket medical expenditures and debt for those who are newly eligible and take up Medicaid. This chain of events and the full-implementation effects will not be immediate. And given the credit bureau data reflects a maximum of 1.5 years after expansion for most states, results presented here are best interpreted as early impacts of the Medicaid expansions.

A second limitation to this study regarding the proxy used for pre-expansion exposure is that we are unable to distinguish rates above the poverty threshold and up to 138% of the FPL. This may be important insofar as individuals in nonexpansion states with income in this range have access to marketplace health insurance and tax subsidies to purchase insurance.

A third potential limitation is that the estimates will be reduced form and will consequently incorporate additional dimensions of the reform related with Medicaid expansion and take-up of coverage. For example, the reduced-form estimate may include any potential effects resulting from the additional provisions of the law such as Medicaid take-up as a result of the individual mandate, or substitution from less comprehensive private insurance to Medicaid (i.e., crowd out). While it would be desirable to obtain structural estimates, it is beyond what our data and methods can produce. Nonetheless, we believe that the reduced-form estimates are informative to policy makers considering whether to expand their Medicaid programs as the expansion decision is within the context of the additional ACA provisions.

Results

Summary Statistics

Figure 2 demonstrates variation in estimates of the county-level rate of potential exposure to the Medicaid expansions by age category. All county-age categories are weighted equally. For each age-group exposure is defined as the percentage of the county population that was both uninsured and had family income up to 138% of the FPL in the calendar year prior to the expansions.[13] For nonexpansion states we report the rate corresponding to 2013. It is apparent that there is more variation in the rate of exposure among the 18 to 39 age-group compared with the 40 to 64 group, where the older population has less potential exposure to the expansions reflecting the fact that they are more likely to have higher income and less likely to be uninsured. The overall average pre-expansion rate of exposure for those 18 to 64 was 7.2% in expansion states and 10.2% in nonexpansion states.

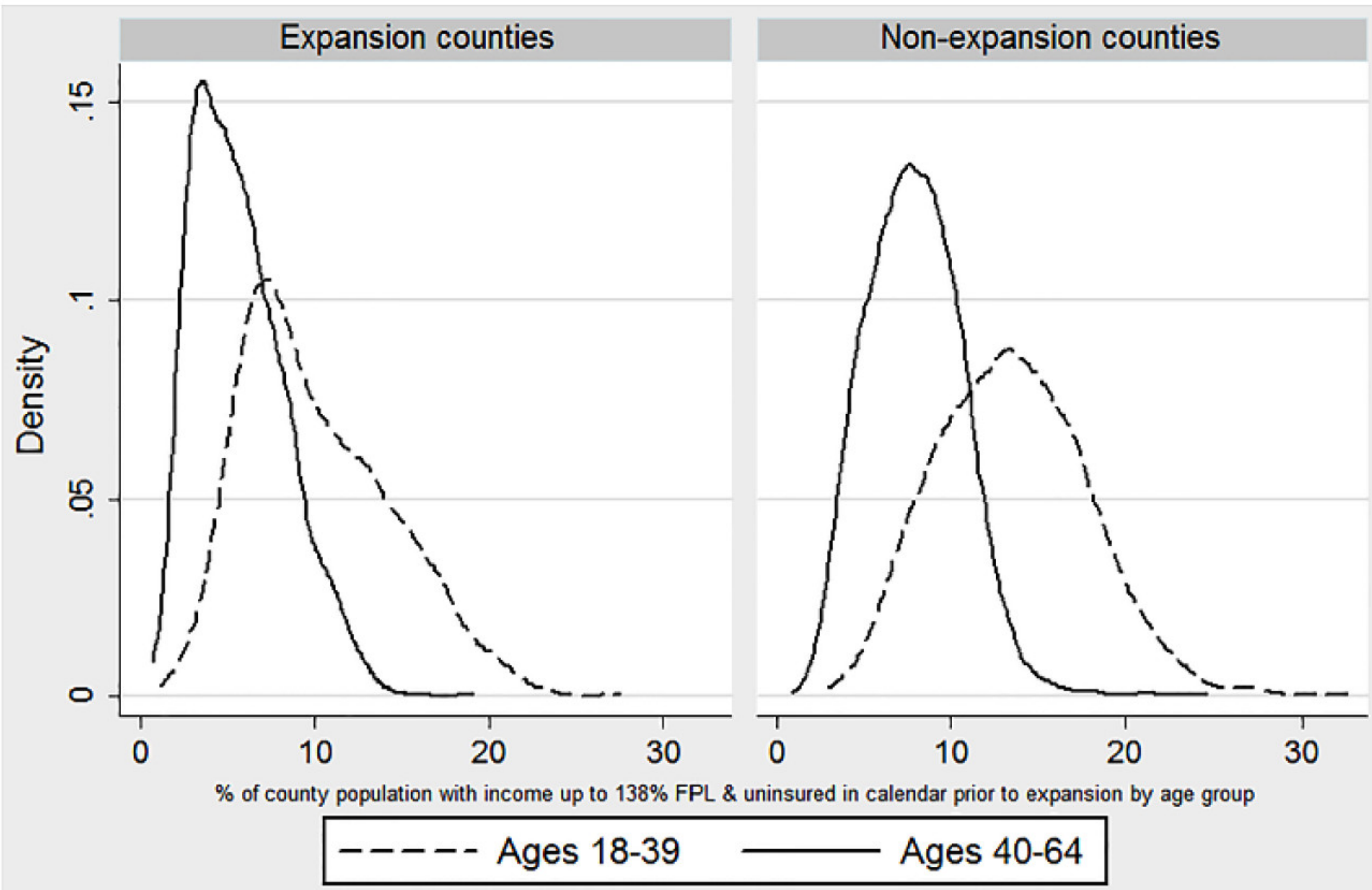

Figure 2. Distribution of county-level rate of exposure to Medicaid expansions by age-group and expansion status.

Note. Early, late, and 1115 waiver expansion states are included. AK and MT are defined as non expansion states. County-age groups are weighted equally. Kernel = epanechnikov, bandwidth (from left to right) = 0.5600, 0.9102, 0.5678, 0.8771.

Source. U.S. Census Bureau, Small Area Heath Insurance Estimates (SAHIE).

Table 2 reports summary statistics from the credit-bureau data for the period prior to the Medicaid expansions by age-group (18-64, 18-39, 40-64). Note that all outcomes measured in dollars are top coded at the 99.9th percentile throughout this analysis, by year, due to extreme and influential outliers (see the appendix for more details). Among the Medicaid expansion states, the pre-expansion period varies by county (see Table 1), whereas the pre-expansion period for nonexpansion states span 2010 through 2013.[14] For those aged 18 to 64 there are approximately 8.2 million individual-year observations in the pre-expansion period within expansion states, and 6.2 million individual-year observations for nonexpansion states. All monetary values are expressed in constant 2015 dollars (Bureau of Labor Statistics, 2016a).

Table 2. Summary Statistics on Financial Outcomes by Age and Medicaid Expansion Status Prior to the Medicaid Expansions[a].

	Ages 18-64		Ages 18-39		Ages 40-64	
	Expansion states	Nonexpansion states	Expansion states	Nonexpansion states	Expansion states	Nonexpansion states
Stocks						
Credit risk score (Vantage score 3.0; range 300-850)	665	651	636	622	688	674
Total balance	$82,843	$67,678	$54,009	$47,186	$106,264	$84,754
Balance past due (90-180 days)	$305	$273	$224	$229	$371	$309
Balance past due as a % of total	0.4%	0.5%	0.4%	0.6%	0.3%	0.4%
Balance on medical collections[b]	$414	$641	$479	$708	$362	$585

	Ages 18-64		Ages 18-39		Ages 40-64	
	Expansion states	Nonexpansion states	Expansion states	Nonexpansion states	Expansion states	Nonexpansion states
Medical collection balance >$0[b]	18.4%	25.9%	21.0%	28.5%	16.4%	23.8%
Medical collection balance ≥$1,000[b]	7.4%	11.7%	8.8%	13.3%	6.3%	10.3%
Balance on nonmedical collections[b]	$724	$895	$743	$889	$710	$900
Nonmedical collection balance >$0[b]	24.6%	28.9%	29.1%	33.0%	21.1%	25.6%
Nonmedical collection balance ≥$1,000[b]	13.0%	15.0%	15.4%	17.3%	11.1%	13.1%
Flows						
Medical collection last 6 months[b]	4.6%	7.7%	5.4%	8.9%	4.0%	6.6%
New derogatory balance last 6 months	13.8%	18.2%	16.7%	21.6%	11.4%	15.4%

	Ages 18-64		Ages 18-39		Ages 40-64	
	Expansion states	Nonexpansion states	Expansion states	Nonexpansion states	Expansion states	Nonexpansion states
New derogatory balance last 6 months ≥$1,000	6.0%	7.9%	7.2%	9.6%	5.0%	6.6%
Bankruptcy filed last 6 months	0.5%	0.4%	0.4%	0.3%	0.5%	0.5%

Note. Credit bureau data cover years 2010 to 2015 and reflect consumers' status at the end of August in each year. All monetary values are expressed in constant 2015 U.S. dollars and are top coded at the 99.9th percentile by year. New derogatory balances exclude those related with mortgages.

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[a] The pre-expansion period varies by expansion state (or county for California) and equals 2010-2013 for nonexpansion states. See Table 1 for details on the timing of the expansions. [b]2010 data are unavailable for outcomes related to medical collections.

On average, compared with nonexpansion states, Table 2 shows that those age 18 to 64 in expansion states had slightly higher credit scores (665 and 651), held significantly higher total credit balances ($83,000 and $68,000) yet only slightly higher past due balances ($305 and $273). Table 2 also reports statistics on collection balances disaggregated by medical and nonmedical. Medical collections in this context are limited to unpaid balances providers (e.g., hospitals and individual medical practices) send to collections. Medical collections do not include balances initially paid via credit from a source other than the provider (e.g., credit card) ultimately sent to collections. This is an important distinction as some providers require (at least partial) payment at the time of service. Therefore, medical collection balances as defined here are a lower bound for all medical-related collection balances. Average medical and nonmedical collection balances are lower for those in Medicaid expansion states. For those 18 to 64 years old in expansion states the average medical collection balance was $414 per person, compared with $641 per person in nonexpansion states.

Given the importance of collections balances we also study whether consumers had any collections balance (greater than zero), or a "high" balance that we define as $1,000 or more. While the latter is somewhat arbitrary—in a given year, $1,000 is approximately the 91st percentile of the nonelderly adult medical collections distribution, and the 87th percentile of the nonmedical collections distribution—our main results are not sensitive to this definition. It is not uncommon that individuals had a collections balance at a given point in time. And adults age 18 to 64 in nonexpansion states were more likely to have a medical collection balance (25.9% compared with 18.4%), or a nonmedical collection balance (28.9% and 24.6%, respectively). Likewise, adults in nonexpansion states were more likely to have a medical collections balance of $1,000 or more (11.7% compared with 7.4%), or a high nonmedical collection balance (15.0% compared with 13.0%).[15]

The bottom of Table 2 reports statistics on the flow of new financial events that may be the most likely outcomes influenced by the early phase of the Medicaid expansions. In expansion states 4.6% of consumers aged 18 to 64 had one or more medical collections trades within the previous 6 months, compared with 7.7% in nonexpansion states. Similarly, consumers in nonexpansion states were more likely to experience a new derogatory balance, which is a broader metric including medical collections as one component (18.2% compared with 13.8%). And those in nonexpansion states were more likely to experience a new "high" derogatory balance equal to $1,000 or more (7.9% compared with 6.0%). Finally, consumers in expansion states were slightly more likely to have filed for bankruptcy in the past 6 months compared with nonexpansion states (0.5% and 0.4%, respectively).

There are a few notable contrasts in these outcomes by age-group. Older individuals aged 40 to 64 had higher credit scores, higher total credit balances, and balances past due, yet lower past due balances as a fraction of total balances. Nonmedical collections balances were higher for younger individuals in expansion states, yet very similar across age-groups in nonexpansion states. However, average medical collection balances, the flow of medical collections and new derogatory balances, were higher for the younger age-group in both expansion and nonexpansion states, which may reflect higher rates of uninsured among younger individuals.

Event-Study Approach

Figure 3 presents results from the event-study approach for "stock" outcomes. It plots coefficient estimates, and 90% confidence intervals, corresponding to the triple-interaction terms from Equation (1) for a given outcome. Coefficient estimates measure the average change in a given outcome in expansion states relative to nonexpansion states, per percentage point in exposure relative to the year immediately prior to the expansions (marked with a gray dot at −1).

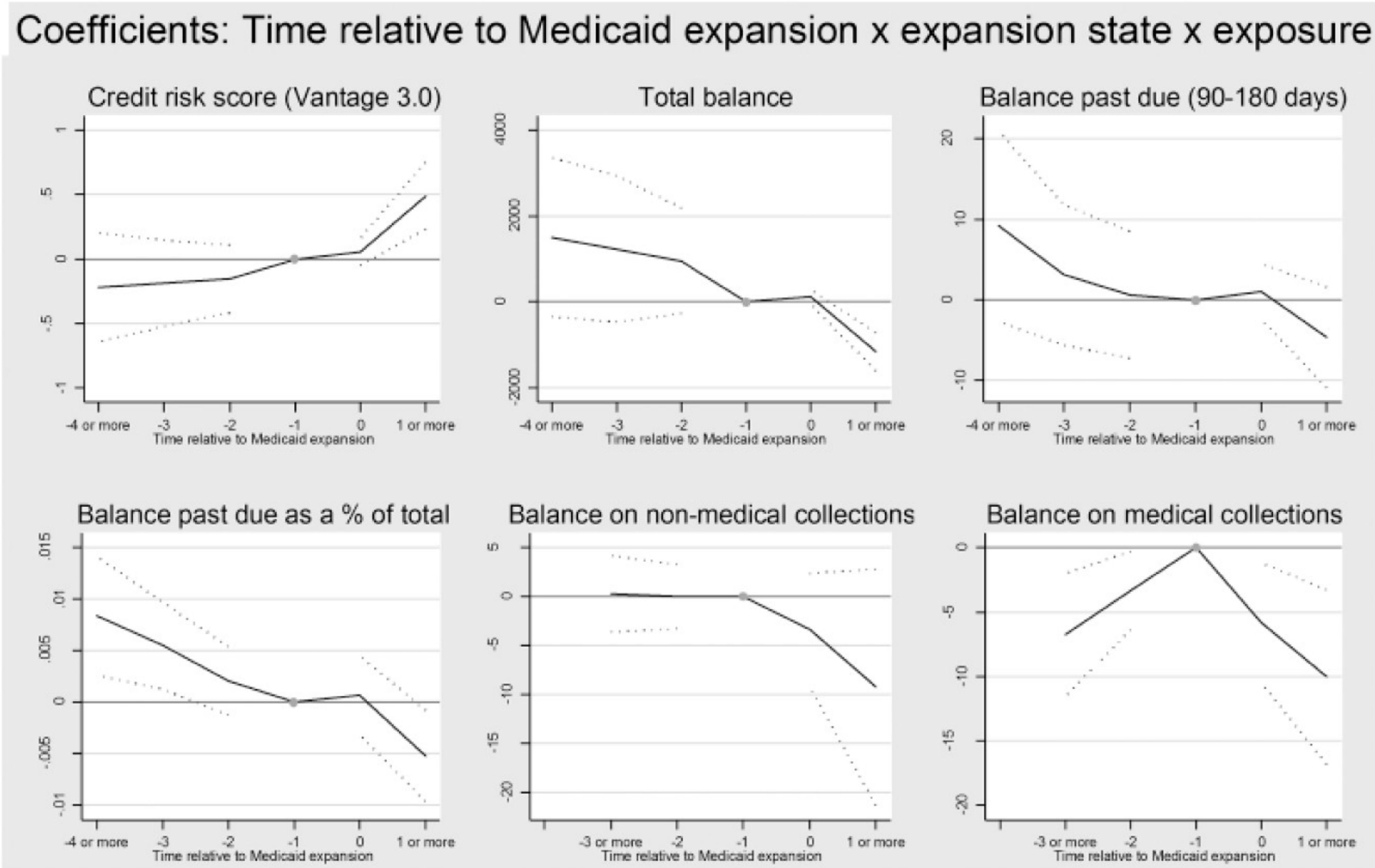

Figure 3. Event-study figures of financial outcomes and time until Medicaid expansion.

Note. Coefficient estimates from three-way interaction terms (Equation 1) and corresponding 90% confidence intervals that account for clustering at the state level are reported. Estimates incorporate early, late, and 1115 waiver expansion states. AK and MT are defined as non expansion states. Additional independent variables include: county fixed effects, time period fixed effects, calendar year fixed effects, county unemployment rate, rate of exposure, expansion state x years until expansion, expansion state x exposure, years until expansion x exposure. Omitted time period = −1 (calendar year prior to expansion) is marked with the black dot. Exposure is measured as the percent of the county population that is both uninsured and with income up to 138% FPL by age category, 18-39 and 40-64.

Using this methodology, outcomes consistent with a causal interpretation are those that do not exhibit a differential pre-expansion period trend and a break in the relative trend during the post-expansion period. Immediately clear from Figure 3 is that results for several outcomes are seemingly inconsistent with a causal interpretation. Indeed, *F* tests for the joint significance of the pre-expansion period coefficient estimates reject the null hypothesis (10% level) that the estimates jointly equal zero for total balance, balance past due, and balance on medical collections. That is, the direction of the relative trend for these outcomes during the post-expansion period is not inconsistent with our hypothesis. Rather, it is the apparent difference in the pre-expansion period trend that makes a causal interpretation for these outcomes less convincing. However, results for credit score appear generally consistent with a causal interpretation. And those for nonmedical collections are compelling, yet the coefficient estimates are not significantly different from zero in the post period. Finally, results for balance past due as a percent of total show that although the interaction terms for two of the three preperiod interactions are significant, the joint *F* test for the preperiod coefficients is insignificant (p = .103).

Figure 4 takes a closer look at medical and nonmedical collection balances. Specifically, it reports event study results for any balance greater than zero, and a balance of $1,000 or more for each type of collection balance. Results from *F* tests for the joint significance of the pre-expansion period coefficient estimates fail to reject the null hypothesis for all outcomes (10% level), suggesting no differential pre-expansion period trends. There is evidence that the expansions decreased medical collection balances of $1,000 or more, possibly nonmedical collection balances greater than $1,000, and medical collections balances greater than zero.

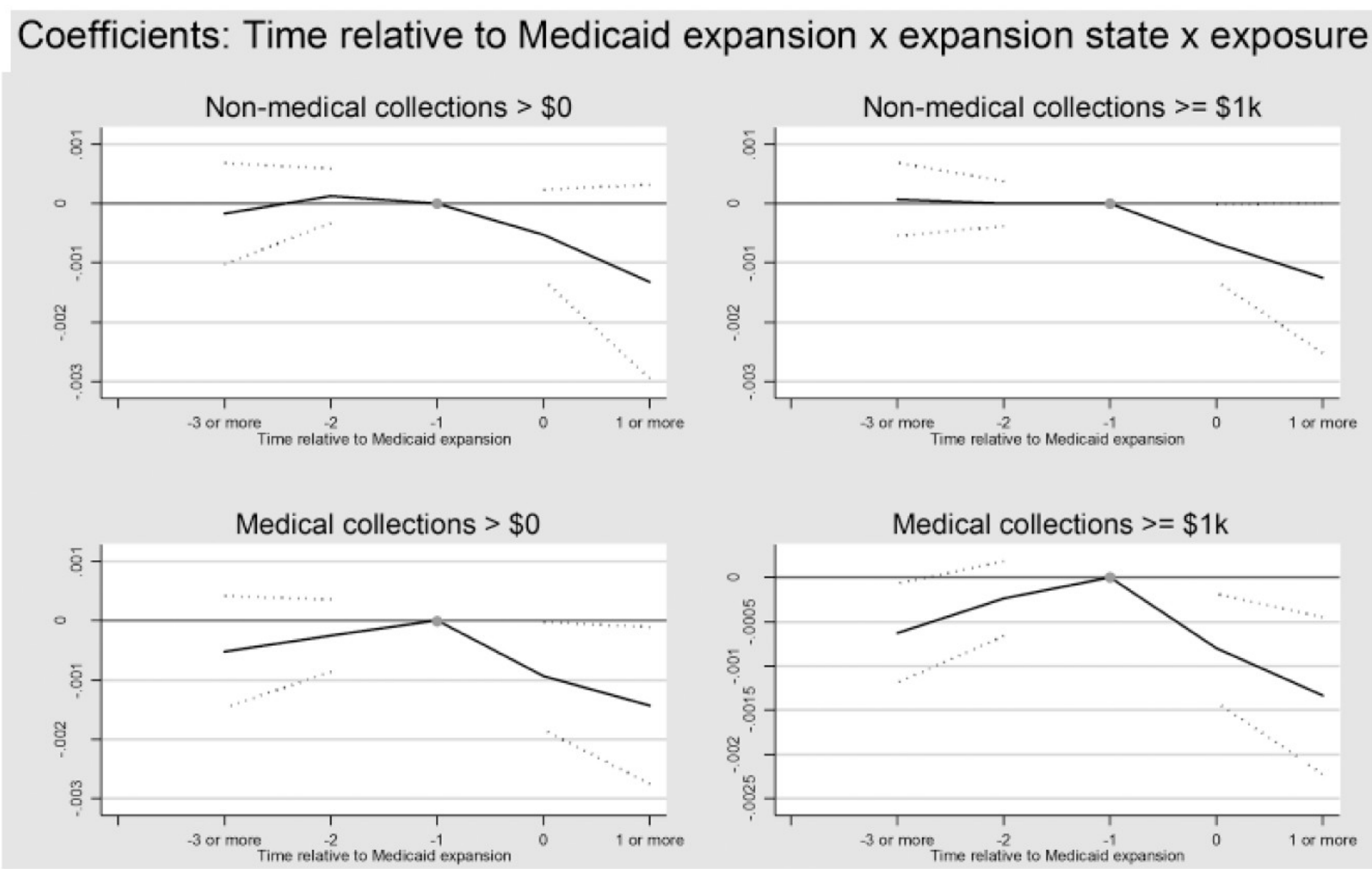

Figure 4. Event-study figures of medical and nonmedical collections and time until Medicaid expansion.

Note. Coefficient estimates from three-way interaction terms (Equation 1) and corresponding 90% confidence intervals that account for clustering at the state level are reported. Estimates incorporate early, late, and 1115 waiver expansion states. AK and MT are defined as non expansion states. Additional independent variables include: county fixed effects, time period fixed effects, calendar year fixed effects, county unemployment rate, rate of exposure, expansion state x years until expansion, expansion state x exposure, years until expansion x exposure. Omitted time period = −1 (calendar year prior to expansion) is marked with the black dot. Exposure is measured as the percent of the county population that is both uninsured and with income up to 138% FPL by age category, 18-39 and 40-64.

Figure 5 reports results for the flow outcomes. We cannot reject the null hypothesis from *F* tests of the joint significance of the pre-expansion period coefficient efficient estimates corresponding to any outcome, lending confidence to the hypothesis that the post-expansion period change is due to the expansions. Results for one or more new medical collections and derogatory balances (greater than $0 and $1,000 or more) that occurred during the previous 6 months are very compelling. Recall that new derogatory balances as defined here include medical collection balances, yet exclude those related with mortgages. That is, while we are not able to directly measure new medical collection balances separately, such balances are included in new derogatory balances, and the results are consistent across both outcomes. Finally, there is some evidence that the expansions may have decreased recent bankruptcy filings.

Figure 5. Event-study figures of recent financial outcomes and time until Medicaid expansion.

Note. Coefficient estimates from three-way interaction terms (Equation 1) and corresponding 90% confidence intervals that account for clustering at the state level are reported. Estimates incorporate early, late, and 1115 waiver expansion states. AK and MT are defined as non expansion states. Additional independent variables include: county fixed effects, time period fixed effects, calendar year fixed effects, county unemployment rate, rate of exposure, expansion state x years until expansion, expansion state x exposure, years until expansion x exposure. Omitted time period = −1 (calendar year prior to expansion) is marked with the black dot. Exposure is measured as the percent of the county population that is both uninsured and with income up to 138% FPL by age category, 18-39 and 40-64. Derogatory balances that occured in the last 6 months excludes mortgage balances.

While not all outcomes presented in Figures 3 through 5 are consistent with a causal interpretation due to differential preperiod trends, it is reassuring that some results relevant to collections, especially the flow of new medical collections, are generally consistent. Should the Medicaid expansions affect the financial outcomes of individuals, it is anticipated that the most direct and immediate means through which that process occurs is via decreased probability of unpaid medical bills and, as observed here, decreased flow of new medical collections. It is also known that the most common type of collections are medical collections (Consumer Financial Protection Bureau, 2014), thus lending credibility to the focus on collections. Also, while credit score incorporates historical information from consumers' credit history, it should be, to some degree, responsive to recent changes in consumers' creditworthiness.

That the remaining outcomes exhibit different trends in the pre-expansion period may reflect different experiences across expansion and nonexpansion states in the recovery to the great recession, unrelated to the ACA. For example, total balances include balances on mortgages or even derogatory unpaid balances related with foreclosures and bankruptcies that are maintained on consumers' records for up to 7 to 10 years. In short, while the Medicaid expansions may have influenced these outcomes, and the post-expansion period trends are consistent with our hypothesis, the differences in the pre-expansion period trends suggest that any changes in these outcomes due to the Medicaid expansions are overshadowed by factors unrelated with the expansions. This suggests that changes in measures that exhibit differential preperiod trends, including total balance, balance past due, and balance on medical collections, are best not interpreted as a result of the expansions.

Main Results: Triple-Difference Design

Table 3 reports results from the triple-difference design. It includes results for all 14 outcomes; however, we focus the discussion on results identified in the event study figures as consistent with a causal interpretation (i.e., those with no differential preperiod trends). Results presented in bold are the main results and are coefficient estimates corresponding to the triple-interaction term in Equation (2). These estimates represent the average change in a given outcome per percentage point in the pre-expansion rate of exposure among all individuals age 18 to 64.

Table 3. Regression Results of the Impact of the Medicaid Expansions on Financial Outcomes per Percentage Point Change in Exposure.

Panel A	(1) Credit risk score			(2) Total balance			(3) Balance past due 180 days)	
	Coeff.	***SE***	***p***	**Coeff.**	***SE***	***p***	**Coeff.**	***SE***
Post-expansion * Expansion geography * Exposure	**0.6131**	**0.2782**	**.032**	**−2062.2**	**714.4**	**.006**	**−10.2**	**6.0**
Post-expansion * Expansion state	−1.3596	1.8795	.473	12195.4	4841.2	.015	23.3	32.3
Post-expansion * Exposure	0.4009	0.0926	.000	297.8	86.6	.001	2.5	3.4

Panel A	(1) Credit risk score			(2) Total balance			(3) Balance past due 180 days	
Expansion * Expansion geography * Exposure	0.1917	0.1785	.288	−799.3	541.1	.146	−3.7	5.7
Expansion * Expansion geography	0.2032	1.3192	.878	4838.2	3606.5	.186	18.3	32.6
Expansion * Exposure	0.2718	0.0652	.000	251.5	65.5	.000	1.1	2.6
Exposure * Expansion geography	−0.4162	0.4336	.342	−3161.2	2090.5	.137	−10.6	7.9
Post-expansion period	−4.5819	1.5176	.004	−3910.5	1893.3	.044	−69.7	29.7
Expansion period	−3.7730	0.9965	.000	−1723.3	1394.0	.222	−8.8	21.4
2015	2.9908	1.2672	.022	5635.1	1477.9	.000	50.3	21.0
2014	1.7754	0.7701	.025	1862.5	924.3	.049	1.3	16.4
2012	0.1376	0.2424	.573	2402.6	431.2	.000	19.7	12.5
2011	1.3728	0.4688	.005	4442.9	837.7	.000	−20.9	20.2
2010	0.4490	0.6028	.460	9140.0	1170.4	.000	−25.3	19.4
Exposure	−1.5182	0.4632	.002	4025.0	893.6	.000	18.8	4.0
County unemployment rate	−0.3266	0.2709	.234	1668.2	494.6	.001	41.5	14.5
Age 40 to 64	42.0146	2.2470	.000	58465.7	4973.6	.000	170.5	26.0
Constant	653.2318	5.0508	.000	9913.6	12485.7	.431	−236.8	105.8

Panel A	(1) Credit risk score	(2) Total balance	(3) Balance past due 180 days)
F	1.048.993	321.627	
Probability > *F*	0.000	0.000	
Adjusted R^2	0.094	0.068	
N	23,079,017	23,521,668	23,52
DV mean: Expansion states, pre-expansion period	665.2	$82,842.8	

Panel B	(8) Nonmedical collections ≥$1,000			(9) Medical collections >$0			(10) Medical collec ≥$1,000	
	Coeff.	*SE*	*p*	Coeff.	*SE*	*p*	Coeff.	*SE*
Post-expansion * Expansion geography * Exposure	**−0.0012**	**0.0007**	**.104**	**−0.0011**	**0.0008**	**.189**	**−0.0010**	**0.0005**
Post-expansion * Expansion state	0.0061	0.0056	.278	−0.0022	0.0060	.718	0.0004	0.0039
Post-expansion * Exposure	−0.0013	0.0004	.001	−0.0013	0.0004	.001	0.0000	0.0003
Expansion * Expansion geography * Exposure	−0.0007	0.0004	.115	−0.0007	0.0006	.279	−0.0005	0.0004

Panel A	(1) Credit risk score			(2) Total balance			(3) Balance past due 180 days)	
Expansion * Expansion geography	0.0044	0.0028	.126	−0.0009	0.0044	.848	−0.0004	0.0030
Expansion * Exposure	−0.0004	0.0003	.190	−0.0006	0.0002	.003	0.0001	0.0002
Exposure * Expansion geography	0.0007	0.0008	.397	0.0004	0.0014	.765	−0.0002	0.0009
Post-expansion period	0.0147	0.0044	.002	0.0135	0.0038	.001	0.0044	0.0024
Expansion period	0.0043	0.0028	.128	0.0056	0.0028	.047	0.0016	0.0022
2015	−0.0206	0.0044	.000	−0.0069	0.0036	.059	−0.0036	0.0021
2014	−0.0067	0.0025	.011	−0.0005	0.0020	.819	−0.0002	0.0015
2012	0.0062	0.0007	.000	−0.0063	0.0016	.000	−0.0042	0.0012
2011	0.0085	0.0012	.000	−0.0099	0.0026	.000	−0.0055	0.0015
2010	—	—	—	—	—	—	—	—
Exposure	0.0034	0.0010	.001	0.0053	0.0015	.001	0.0038	0.0008
County unemployment rate	0.0016	0.0006	.015	−0.0005	0.0011	.657	−0.0002	0.0008
Age 40 to 64	−0.0235	0.0046	.000	−0.0208	0.0050	.000	−0.0100	0.0029
Constant	0.1053	0.0104	.000	0.1801	0.0144	.000	0.0662	0.0092
F			195.353			39.566		

Panel A	(1) Credit risk score	(2) Total balance	(3) Balance past due 180 days)
Probability > *F*	0.000	0.000	
Adjusted R^2	0.023	0.056	
N	19,585,807	19,585,807	19,585
DV mean: expansion states, pre-expansion period	0.1301	0.1843	

Note. Coefficient estimates from the triple interaction terms (in bold) measure the change in a given outcome with respect to a percentage point change in exposure to the Medicaid expansions (Equation 2). All models include county fixed effects. Results incorporate early, late, and 1115 waiver expansion states. AK and MT are defined as nonexpansion states as these expansions occurred after the most recent credit bureau data file reference period. Exposure is measured as the percent of the county population that is both uninsured and with income up to 138% federal poverty level by age category, 18 to 39 and 40 to 64. *SE* = standard error; *SE* are clustered at the state level. "—" indicates "not available"; medical collections are not available for the 2010 data file. Monetary values are expressed in constant 2015 dollars.

Table 3 shows that credit scores increased by 0.61 points per percentage point in the pre-expansion rate of exposure (column 1). And balance past due as a percent of total decreased by 0.01 percentage points per percentage point in the exposure rate (column 4). Subsequent results reported in columns 5 and 7 through 9 take the expected sign yet are statistically insignificant: namely, balance on nonmedical collections (−$9.40; p = .203), probability of nonmedical collections balance greater than zero (−0.12 percentage points; p = .233), probability of nonmedical collections balance greater $1,000 (−0.12 percentage points; p = .104), and probability of nonmedical collections balance greater than zero (−0.11 percentage points; p = .189).

The remaining results presented in columns 10 through 14 are statistically significant at conventional levels and take the hypothesized sign. The probability of having a medical collections balance of $1,000 or more decreased by 0.10 percentage points per percentage point in the exposure rate (column 10); the probability of experiencing one or more new medical collections decreased by 0.15 percentage points (column 11); the probability of having any new derogatory balance decreased by 0.19 percentage points (column 12); the likelihood of experiencing a new derogatory balance greater than $1,000 increased by 0.16 percentage points (column 13); and the probability of a new bankruptcy filing decreased by 0.01 percentage points (column 14).

Finally, the remaining outcomes are those where the event-study results exhibit differential preperiod trends, where we have less confidence that the reported changes are (solely) a result of the expansions: total balance (column 2), balance past due (column 3), and balance on medical collections (column 6).

To interpret results from Table 3 in terms of the average effect of the Medicaid expansions per person age 18 to 64, we assume that a percentage point change in the pre-expansion period exposure rate corresponds to a commensurate change in the share of the low-income, uninsured population as a result of the expansions. The estimates based on ACS data presented in Figure 1 suggest that the decrease in the share of uninsured, low-income adults between 2013 and 2015 equals −1.0 percentage points (or 13.9%) in expansion states relative to nonexpansion states; that is, −3.4 percentage points in expansion states compared with −2.4 percentage points nonexpansion states. In Table 4, we interpret our coefficient estimates as corresponding to this one-percentage point change in the fraction of uninsured, low-income adults to arrive at the average effect of the Medicaid expansions per person age 18 to 64. Results presented here are limited to those that did not exhibit differential preperiod trends and are statistically significant as reported in Table 3.

Table 4. Estimated Effects of the Medicaid Expansions on Financial Outcomes.

	(1) Change in credit score	(2) Change in balance past due as a % of total balance	(3) Change in probability of medical collections balance $1,000 or more	(4) Change in probability of medical collection during last 6 months	(5) Change in probability of new derogatory balance during last 6 months	(6) Change in probability of new derogatory balances $1,000 or more during last 6 months	(7) Change in probability of bankruptcy filing during last 6 months
1.0 percentage point (13.9%) decrease in low-income, uninsured							
Level change per person age 18-64	0.61	−0.0001	−0.0010	−0.0015	−0.0019	−0.0016	−0.0001

	(1) Change in credit score	(2) Change in balance past due as a % of total balance	(3) Change in probability of medical collections balance $1,000 or more	(4) Change in probability of medical collection during last 6 months	(5) Change in probability of new derogatory balance during last 6 months	(6) Change in probability of new derogatory balances $1,000 or more during last 6 months	(7) Change in probability of bankruptcy filing during last 6 months
Percent change per person age 18-64	0.1%	−2.9%	−1.3%	−3.3%	−1.4%	−2.6%	−2.8%

Note. Estimates of the level change per person are based on coefficient estimates from Table 3 (in bold) multiplied by the stated percentage point change in the proportion of individuals who are low income and uninsured; estimates of percent change incorporate the pre-expansion period average of a given outcome in expansion states.

Results reported in Table 4 imply that, per person age 18 to 64: credit scores increased by 0.61 points (0.1%); debt past due as a percent of total decreased by 0.01 percentage points (2.9%); the probability of having a medical collections balance of $1,000 or more decreased by 0.10 percentage points (1.3%); the probability of having one or more medical bills sent to collections over a 6-month period decreased by 0.15 percentage points (3.3%); the probability of any new derogatory balance decreased by 0.19 percentage points (1.4%); the probability of a new derogatory balance greater than $1,000 decreased by 0.16 percentage points (2.6%); and the probability of a new bankruptcy filing decreased by 0.01 percentage points (2.8%).

Given that the reduced-form estimates above correspond to *all* individuals age 18 to 64, and those who gained Medicaid coverage due to the expansions represent a relatively small share of this group, these estimates imply *much* larger changes for those directly affected by the expansions. In our view these results do, however, demonstrate that the ACA Medicaid expansions significantly increased financial security of new beneficiaries. And given that our data reflect consumers' experiences through August 2015, these effects are best interpreted as the initial effects of the expansions, where it will most likely take several years to reach a new equilibrium.

It is important to keep in mind that the price Medicaid pays providers for services is likely much lower than the prices the uninsured are charged for the same services. Consequently, any decrease in the amount of medical collections or new derogatory debt balances due to the expansions is likely larger than what Medicaid would have paid and would not translate into a dollar-for-dollar shift from collections to Medicaid spending. That said, some portion of the related dollar amount contributes to the large estimated transfer of $0.6 per dollar of public spending on Medicaid to providers for implicit insurance for the low-income uninsured (Finkelstein, Hendren, & Lutttmer, 2015). These effects also reflect inefficiencies relative to providing insurance to the low-income uninsured when taking into consideration resources employed to (partially) recover unpaid bills.

Robustness of Results

In the appendix, we present and discuss results from multiple alternative model specifications to assess the robustness and validity of the main results. These models generally support the main findings discussed above and presented in Table 4, with a few caveats. To summarize, we find that results regarding new medical collections and derogatory debt (any balance and balance $1,000 or more) that occurred in the previous 6 months are the most unaffected by choice of model specification in terms of statistical significance and magnitude of results. This is an important finding as the flow of new medical collections, and derogatory balances more generally, should arguably be the first and most likely outcome studied here, if any, influenced by the expansions.

Results for recent bankruptcy filings and balance past due as a percent of total were less sensitive to different model specifications, although these were the only outcomes that that failed placebo tests estimated among adults age 65 and older. The latter finding suggests that factors other than the expansions may be responsible for the observed changes in these outcomes. Results for credit score and medical collection balances $1,000 or more were more sensitive to alternative specifications, which may reflect the fact that they change more slowly over time and the relatively short post-expansion period observed in the data. However, results that include state- or county-level time trends are generally consistent with those reported in Table 4.

Summary and Discussion

Using data from one of the major credit bureaus, combined with information on the likelihood of exposure to the ACA Medicaid expansions, we estimate triple-difference models to evaluate the early effects of the expansions on multiple dimensions of personal finance. Overall, results demonstrate financial improvements in states that expanded their Medicaid programs.

In summary, our estimates of the effect of the Medicaid expansions per individual age 18 to 64 include improved credit scores (0.1%), reduced balances past due as a percent of total debt (2.9%), reduced probability of a medical collection balance of $1,000 or more (1.3%), a 3.3% reduction in the probability of having one or more medical bills go to collections in the previous 6 months, a 1.4% reduction in the probability of experiencing a new derogatory balance of any type, a 2.6% reduction in the probability of incurring a new derogatory balance equal to $1,000 or more, and a 2.8% reduction in the probability of a new bankruptcy filing. Given that the proportion of individuals affected by the Medicaid expansions is much smaller than the population adults age 18 to 64, these estimates reflect much larger effects per newly enrolled Medicaid beneficiary.

These results are broadly consistent with recent work by Hu et al. (2016), using data on *nonmedical* collection balances, that suggests that ACA Medicaid expansions reduced average balances by −$600 to −$1,000 per new beneficiary. We extend those findings to other measures of beneficiaries' financial well-being and more clearly illustrate the mechanism through which any improvements occurred. Indeed, this work demonstrates that the Medicaid expansions significantly reduced the likelihood of new medical collections and, more generally, the flow of new and large derogatory debt balances. This finding is consistent with the hypothesis that Medicaid coverage directly decreased the risk of medical out-of-pocket expenditures and ultimately unpaid medical bills.

These results are important for policy decisions. This work demonstrates how the ACA Medicaid expansions have improved economic well-being of low-income Americans, which at the same time has implications for providers and payers of medical services. From the consumer perspective our results show that increased access to Medicaid substantively decreases the risk of bills that go unpaid, which are at times nontrivial in magnitude especially for low-income families. Overall this suggests that the ACA Medicaid expansions provide meaningful financial protection to the low-income uninsured. From the provider perspective our results indirectly suggest that the Medicaid expansions have decreased reliance on third-party bill collectors, likely a very inefficient means of obtaining payment for services. Finally, from the payer prospective the results may suggest decreased need for funding of uncompensated care, such as disproportionate share hospital payments and upper payment limit supplemental payments, much of which is funded by Medicaid.

Acknowledgments

The authors thank Tal Gross, Sharon Long, John Goddeeris, Brendan Saloner (discussant) and participants at the 6th Biennial Conference of the American Society of Health Economists, session on Medicaid Expansion and the ACA, and two anonymous reviewers who provided helpful comments. We also thank Adam Weiss who assisted in assembling the analytical data set.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) disclosed receipt of the following financial support for the research, authorship, and/or publication of this article: Support for this research was provided by the Robert Wood Johnson Foundation, Policy-Relevant Insurance Studies Grant Number 72671.

Footnotes

1. See Sommers, Arntson, Kenney, and Epstein (2013) and Sommers, Kenney, and Epstein (2014) for more details on the Medicaid expansions prior to 2014, as well as Harbage and King (2012) for details on the California expansions.

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2. As of March 2016, Louisiana had yet to implement their expansion.

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3. New Jersey and Washington were technically early expansion states. However, in these states existing enrollees were transferred to new programs, and no new beneficiaries were enrolled prior to 2014 (Sommers et al., 2013).

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4. As discussed in more detail in the following section, August 2015 corresponds to the reference period of the most recent data used in this analysis. Consequently, Alaska and Montana are considered nonexpansion states throughout this work.

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5. The legal agreement with the credit bureau states that we cannot use the bureau's name unless given permission. Consequently, we use the generic language "credit bureau" throughout this article. The data obtained from the credit bureau are confidential and proprietary to the credit bureau. These data may be used for research but they cannot be transferred to third parties.

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6. The work by Brevoort et al. (2015) studies consumers with limited credit histories in two groups. The first are "unscoreable" consumers who have a credit record that is sufficiently limited such that it is not possible to estimate a credit score for the consumer. "Credit invisibles" are consumers that do not have any credit record. The data in this study include the "unscorable" but not "credit invisibles."

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7. New medical collections and bankruptcy filings were derived from information on the number of months since a given consumer's most recent medical collection or bankruptcy filing (if any). Results are very similar when we used the definition: one or more medical collections or bankruptcy filing in the previous 12 months. Note that we do not have similar information on the number of months since the most recent nonmedical collection in our data, and consequently are not able to similarly study the flow of nonmedical collections. Finally, we do not have information on new derogatory debt balances other than those which occurred in the previous 6 months.

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8. It is possible that related individuals are included in these data. However, we are not able to identify relationships between consumers in the data.

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9. This approach is similar to a traditional difference-in-differences model, with the modification of an additional interaction term with the difference-in-differences estimator that is continuous.

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10. Two states, Alaska and Montana, expanded after August 2015, the reference period of our last year of credit bureau data. These states are included throughout the analysis and are classified as nonexpansion states.

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11. IN and PA have five periods of pre-expansion data; DC, CT, MN, and 48 counties in CA have three or more post-expansions periods.

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12. Medical collections data are not available for 2010. Consequently, we modify these models slightly for these outcomes accordingly; that is, t = (−3 or more, −2, −1, 0, 1, or more).

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13. For Pennsylvania and Indiana, who expanded in 2015, we use 2013 data which is the most recent SAHIE data available.

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14. Medical collections data are not available for 2010.

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15. A recent report, using a similar sample of data from a credit bureau, reported that 19.4% of all consumer credit reports (all ages and all states) include one or more medical collection trade lines (Consumer Financial Protection Bureau, 2014). The estimated prevalence of medical collections using our data is comparable.

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Appendix

Distribution of Financial Outcomes and Outliers

Table A1 reports statistics on the distribution of the monetary financial outcomes studied in this work by year among all adults age 18 to 64. These statistics reveal that these data contain extreme values. For example, in 2011, the 99th percentile of nonmedical collections was $15,362, the 99.9th percentile was $50,909, and the maximum value was $11.8 million. We also found that some regression results were sensitive to these values, mostly for nonmedical collection balances. While it is not clear that these extreme cases are misreported values, it is reasonable to hypothesize that the Medicaid expansions did not reduce (or cause) balances in nonmedical collections, or changes thereof, in the millions of dollars. The fact that the maximum values for medical collections do not exceed $1.4 million in a given year supports this proposition.

Table A1. Distribution of Financial Outcomes Among Adults Age 18 to 64 by Year.

	Year	% >$0	p25	p50	p75	p90	p95	p99	p9
Total balance	2010	87.0%	$1,211	$13,891	$105,155	$263,065	$388,813	$779,163	$1,83
	2011	87.0%	1,110	12,684	97,928	249,442	368,000	730,187	1,69
	2012	87.8%	1,169	12,540	91,090	238,248	351,624	694,164	1,58
	2013	88.1%	1,205	12,287	82,843	228,861	338,245	659,839	1,49
	2014	88.3%	1,272	12,882	83,840	228,866	336,835	653,532	1,47
	2015	88.4%	1,283	13,312	82,260	229,090	337,248	652,366	1,48
Balance past due (90-180 days)	2010	4.6%	0	0	0	0	0	9,421	62,
	2011	4.2%	0	0	0	0	0	7,596	67,
	2012	4.3%	0	0	0	0	0	6,622	70,
	2013	3.8%	0	0	0	0	0	4,027	67,
	2014	3.8%	0	0	0	0	0	2,979	61,
	2015	3.6%	0	0	0	0	0	2,252	48,
Nonmedical collections balance	2011	26.8%	0	0	105	1,927	4,332	15,362	50,
	2012	26.6%	0	0	96	1,831	4,187	15,776	56,
	2013	25.8%	0	0	63	1,651	3,764	13,940	52,
	2014	25.2%	0	0	32	1,485	3,336	11,849	47,
	2015	23.5%	0	0	0	1,214	2,751	9,315	33,

	Year	% >$0	p25	p50	p75	p90	p95	p99	p9
Medical collections balance	2011	20.0%	0	0	0	736	2,108	9,829	43,
	2012	20.3%	0	0	0	753	2,145	9,986	44,
	2013	21.0%	0	0	0	824	2,295	10,425	46,
	2014	20.8%	0	0	0	828	2,308	10,578	47,
	2015	20.2%	0	0	0	780	2,193	9,913	43,
New derogatory balances excluding mortgage	2010	15.2%	0	0	0	435	1,721	15,630	63,
	2011	15.3%	0	0	0	417	1,504	13,093	59,
	2012	15.5%	0	0	0	448	1,631	14,231	64,
	2013	14.9%	0	0	0	393	1,420	12,065	59,
	2014	14.8%	0	0	0	421	1,482	12,248	60,
	2015	14.3%	0	0	0	387	1,418	11,956	59,

Note. Monetary values are expressed in constant 2015 dollars. Top coded mean estimates are based on data that were top coded at the 99.9th percentile by year. Data on medical collections are not available for 2010.

To address this issue throughout this analysis we top-coded the data at the 99.9th percentile by year. We prefer this strategy for two reasons. First, this method addresses the issue in such a way that does impose judgment on whether particular values are misreported, which we cannot discern with confidence from the data. Second, by top coding only 0.1% of the data by year we affect a very small proportion of the data while gaining confidence that our main results are not influenced by extreme values. Note that due to computing constrains using this very large data set we are not able to implement more formal diagnostics such as “robust regression” (e.g., Stata’s command “rreg”).

Alternative Specifications and Placebo Tests

To test the robustness and validity of our main results we estimate several alternative model specifications reported in Table A2, some of which are also used in the work by Mazumder and Miller (2016) who studied the Massachusetts health insurance expansion. Results from Specification 1 include county fixed effects, and correspond to those reported in Table 3. Table A2 reports only the main coefficient estimate of interest for each model, the corresponding standard error in parenthesis and *p* value in brackets. Specification 2 allows outcomes in each state to follow state-specific trends in the most flexible way possible by including state-year fixed effects. This could be important, for example, if states recovered uniquely from the great recession, which could threaten the assumptions of our identification strategy. Outcomes not robust to the inclusion of state-specific time trends include medical collections balance $1,000 or more and new bankruptcy filings, which are no longer significant, and balance past due as a percentage of total, which is significant but changes sign. Results for credit score and new derogatory balances increase in magnitude (absolute value).

Table A2. Alternative Model Specifications.

Specification (Description identifies difference with respect to Specification 1)	Credit risk score	Balance past due as a % of total	Medical collections ≥$1,000	Medical collection last 6 months	Derogatory balance last 6 months >$0	Derogatory balance last 6 months ≥$1,000	Bankruptcy filed last 6 months
(1) Base specification: All states and county fixed effects	0.6 (0.3) [.032]	−0.0001 (0.0000) [.029]	−0.0010 (0.0005) [.077]	−0.0015 (0.0004) [.000]	−0.0019 (0.0006) [.004]	−0.0016 (0.0004) [.001]	−0.0001 (0.0000) [.000]
(2) State-year fixed effects	1.4 (0.3) [.000]	0.0001 (0.0000) [.027]	−0.0011 (0.0008) [.158]	−0.0013 (0.0005) [.017]	−0.0035 (0.0007) [.000]	−0.0022 (0.0004) [.000]	0.0000 (0.0000) [.132]
(3) County-year fixed effects	1.2 (0.3) [.001]	0.0000 (0.0001) [.887]	−0.0026 (0.0005) [.000]	−0.0020 (0.0004) [.000]	−0.0036 (0.0007) [.000]	−0.0025 (0.0004) [.000]	0.0000 (0.0000) [.958]
(4) County-age group fixed effects	0.2 (0.2) [.175]	−0.0001 (0.0000) [.005]	0.0000 (0.0004) [.914]	−0.0009 (0.0004) [.016]	−0.0007 (0.0004) [.101]	−0.0008 (0.0003) [.019]	−0.0001 (0.0000) [.017]

Specification (Description identifies difference with respect to Specification 1)	Credit risk score	Balance past due as a % of total	Medical collections ≥$1,000	Medical collection last 6 months	Derogatory balance last 6 months >$0	Derogatory balance last 6 months ≥$1,000	Bankruptcy filed last 6 months
(5) Excluding early, late, and 1115 waiver states	0.1 (0.2) [.481]	−0.0001 (0.0001) [.126]	−0.0003 (0.0006) [.663]	−0.0011 (0.0004) [.016]	−0.0007 (0.0006) [.249]	−0.0008 (0.0004) [.054]	−0.0001 (0.0000) [.053]
(6) High exposure subsample (DD)	2.3 (1.6) [.163]	−0.0011 (0.0005) [.036]	−0.0025 (0.0033) [.448]	−0.0134 (0.0027) [.000]	−0.0110 (0.0038) [.006]	−0.0112 (0.0040) [.008]	−0.0015 (0.0004) [.001]
(7) Low exposure subsample (DD)	0.9 (0.7) [.196]	0.0001 (0.0002) [.772]	−0.0055 (0.0024) [.027]	−0.0115 (0.0024) [.000]	−0.0060 (0.0022) [.010]	−0.0056 (0.0016) [.001]	0.0002 (0.0002) [.301]
(8) Low credit score subsample	0.2 (0.1) [.149]	−0.0002 (0.0001) [.002]	−0.0001 (0.0007) [.909]	−0.0011 (0.0005) [.034]	−0.0018 (0.0008) [.019]	−0.0017 (0.0005) [.001]	−0.0001 (0.0001) [.054]
(9) High credit score subsample	0.3 (0.1) [.006]	0.0000 (0.0000) [.010]	0.0000 (0.0001) [.719]	0.0000 (0.0001) [.728]	0.0000 (0.0001) [.904]	0.0000 (0.0000) [.676]	0.0000 (0.0000) [.000]
(10) Medical debt in collections at some point prior to expansions	0.4 (0.3) [.173]	−0.0002 (0.0001) [.009]	−0.0007 (0.0012) [.598]	−0.0034 (0.0010) [.001]	−0.0038 (0.0014) [.011]	−0.0031 (0.0009) [.002]	−0.0001 (0.0001) [.246]
(11) No medical debt in collections at some point prior to expansions	0.6 (0.2) [.014]	−0.0001 (0.0000) [.116]	0.0005 (0.0002) [.006]	0.0004 (0.0001) [.000]	0.0001 (0.0003) [.723]	0.0000 (0.0002) [.877]	−0.0001 (0.0000) [.000]

Specification (Description identifies difference with respect to Specification 1)	Credit risk score	Balance past due as a % of total	Medical collections ≥$1,000	Medical collection last 6 months	Derogatory balance last 6 months >$0	Derogatory balance last 6 months ≥$1,000	Bankruptcy filed last 6 months
(12) Ages 65+	0.1 (0.1) [.398]	0.0000 (0.0000) [.052]	0.0001 (0.0002) [.530]	−0.0001 (0.0002) [.539]	−0.0002 (0.0002) [.363]	−0.0002 (0.0002) [.177]	−0.0002 (0.0000) [.000]

Note. Coefficient estimates from the triple interaction terms are reported—unless indicated by "DD," which indicates differences-in-differences, which measure the change in a given outcome with respect to a percentage point change in exposure to the Medicaid expansions. See Table 3 for additional covariates included but not reported. Standard errors are reported in parentheses and are clustered at the state level. *P* values are reported in brackets. Data on medical collections are not available for the 2010 data.

Similarly, Specification 3 accounts for county-specific trends in outcomes with the inclusion of county-year fixed effects. All results are robust to county-specific time trends except balance past due as a percent of total and recent bankruptcy filings, and coefficient estimates for the remaining outcomes are greater in magnitude with respect to Specification 1. These results are reassuring as these models also effectively control for unobserved state- or county-level factors, which change over time that we have not explicitly accounted for.

To account for unobservable time-invariant characteristics specific to age categories (18 to 39, 40 to 64) within each county, Specification 4 includes county age category fixed effects. Therefore, this model relies on variation within each county age category over time. Results for medical collections in the previous 6 months, total balance as a percent of total, new derogatory balance $1,000 or more, and bankruptcy filing in the previous 6 months are robust to this specification; results for any new derogatory balance is marginally insignificant, whereas results for credit score and medical collection balance $1,000 or more is insignificant. The latter result could indicate that there were divergent trends by age category for credit score. Alternatively it could be that the number of post-expansion time periods we observe is too few to measure the effect of the expansions given the significant loss of variation. While the coefficients are closer to zero with respect to Specification 1, the standard errors are comparable with Specification 1.

Specification 5 excludes early expansion states, late expansion states, and 1115 waiver states. Consequently, there is no variation in the length of the pre- or post-expansion time periods among expansion states in this specification, and event time equals calendar time. By August 2015, the last reference period of the data, 18 months passed since the Medicaid expansion implementation date (January 1, 2014). Results for the probability of a medical collection during the previous 6 months, new derogatory balance $1,000 or more, and bankruptcy filings in the last 6 months are robust to this exclusion, while the remaining results are insignificant. Should 18 months be an insufficient amount of time for the full effects of the expansions to materialize it could be expected that the coefficient estimates in this model be smaller in magnitude, or insignificant, compared with Specification 1 that includes early expansion states.

The following two Specifications (6 and 7) are estimated on high and low pre-expansion exposure subsamples, where differences-in-differences coefficient estimates (expansion state times expansion time period) are reported. Here we may expect that results for the high exposure group to be more pronounced. High and low exposure is defined, for each county age-group weighted equally, as a pre-expansion exposure rate above or below the median. The median was 11.9% for ages 18 to 39, and 6.8% for ages 40 to 64. Results for medical collections in the last 6 months are significant for both models, and slightly in absolute value for the high exposure group. Estimates from either model suggest that the Medicaid expansions decreased the probability of a medical collection by approximately one percentage point (or approximately 20%) among all individuals age 18 to 64. Results for medical collections balance $1,000 or more is only significant for the low-exposure sample, which is unexpected, and both credit score results are insignificant for both specifications. However, results for balance past due as a percent of total, new derogatory balance (any and $1,000 or higher) and recent bankruptcy filings are more consistent are either larger or only significant for the high exposure group, which is generally consistent with our hypothesis.

Should individuals with lower credit scores also be more likely uninsured and have lower incomes, the measured effects of the Medicaid expansions should be stronger among the low credit score group. Specifications 8 and 9 stratify the sample into low and high credit score groups respectively based on the median vantage credit score in 2011 across all consumers age 18 to 64, which was 666. Results for the low credit score group are generally greater (in absolute value) or significant compared with the high credit score group. Two exceptions are results for medical collection balance $1,000 or more, which is insignificant for both Specifications, 8 and 9, and credit score that is significant only for the high score sample.

The following specifications, 10 and 11, split the sample by whether individuals had any medical collections up to three years prior to the Medicaid expansion. Should those with medical collections at one point in time be more likely to have future medical collections, and the Medicaid expansions reduce the probability financial distress, we may expect a larger impact among those who had medical collections prior to the expansions. Twenty-nine percent of overall person-year observations correspond to the group with prior medical collections. Most results are consistent with the hypothesis in that they are either larger in magnitude (absolute value) or significant for Specification 10 compared with Specification 11. There are three exceptions. Results for credit score, recent bankruptcy filings, and medical collections balance $1,000 or more are only significant among those with no prior medical collections balance. Also the significant result for large medical collection balance is positive, albeit small in magnitude.

Specification 12 includes only individuals age 65 and older, where we use the county-level exposure rate for those aged 18 to 64. These models serve as a placebo test as this age-group is not directly affected by the Medicaid expansions. Results are insignificant for all outcomes except balance past due as a percent of total and recent bankruptcy filings.

Finally, results from a regression model corresponding to Equation (2), where the county-level unemployment rate equals the dependent variable (instead of an explanatory variable), reveal statistically insignificant results for the triple interaction term of interest (−0.0348; p = .288). This is a falsification test used in previous studies and is valid insofar the ACA Medicaid expansions did not cause a change in the unemployment rate. That said there may be concern about the validity using the unemployment rate as a placebo test given recent work on the "job lock" hypothesis (Dague, DeLeire, & Leininger, 2014; Garthwaite, Gross, & Notowidigdo, 2014). Should individuals no longer work with increased access to health insurance outside the workplace, unemployment may change insofar as the Medicaid expansions influence the labor market overall.

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