NBER WORKING PAPER SERIES

THE ACA MEDICAID EXPANSION IN MICHIGAN AND FINANCIAL HEALTH

Sarah Miller
Luojia Hu
Robert Kaestner
Bhashkar Mazumder
Ashley Wong

Working Paper 25053
http://www.nber.org/papers/w25053

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
September 2018, Revised March 2020

Thank you to Leah Plachinski and Christopher Hollrah for their excellent research assistance. We are grateful to the Russell Sage Foundation (grant # 94-16-04) for support of this project. Drs Kaestner, Mazumder, and Miller also benefited from support from the National Institute of Child Health and Human Development (NICHD) grant #1R01HD081129. We thank Tara Watson, Lara Shore-Sheppard, seminar participants at the Federal Reserve Banks of Philadelphia and Chicago and conference participants at ASHEcon and the Federal Reserve Bank of Atlanta's Credit Bureau User conference for their comments. We also gratefully acknowledge the Michigan Department of Health and Human Services for making this data available, Sarah Clark and Lisa Cohn for helping assemble the MDHHS data set, and Dave Fogata for facilitating the data purchase from TransUnion. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

The ACA Medicaid Expansion in Michigan and Financial Health
Sarah Miller, Luojia Hu, Robert Kaestner, Bhashkar Mazumder, and Ashley Wong
NBER Working Paper No. 25053
September 2018, Revised March 2020
JEL No. I1,I13,I18

ABSTRACT

This article examines how the financial health changes following an individual's enrollment in Michigan's Medicaid program (Healthy Michigan Program, HMP). We use unique data that links credit reports of HMP enrollees to Medicaid administrative data on enrollment and use of health care services. We find that Medicaid enrollment is associated with large improvements in several measures of financial health, including reductions in unpaid bills, medical bills, over limit credit card spending, and public records (such as evictions, judgments, and bankruptcies). These improvements are apparent across several subgroups, although individuals with greater medical need, such as those with chronic illnesses, experience the largest benefits.

Sarah Miller
Ross School of Business
University of Michigan
701 Tappan Street
Ann Arbor, MI 48109
and NBER
mille@umich.edu

Luojia Hu
Federal Reserve Bank of Chicago
230 S. LaSalle Street
Chicago, IL 60604
lhu@frbchi.org

Robert Kaestner
Harris School of Public Policy
University of Chicago
1307 East 60th Street (Room 3057)
Chicago, IL 60637
and NBER
kaestner@uchicago.edu

Bhashkar Mazumder
3625 N Springfield Ave
230 S. LaSalle Street
Chicago, IL 60604
bhash.mazumder@gmail.com

Ashley Wong
Federal Reserve Bank of Chicago
230 S. LaSalle Street
Chicago, IL 60604
ashley.wong816@gmail.com

1 Introduction

As part of the Affordable Care Act (ACA), the state of Michigan expanded Medicaid eligibility to those earning up to 138 percent of the Federal Poverty Level (FPL). Expanded eligibility became effective April of 2014 with the creation of the Healthy Michigan Plan (HMP). In the same year, similar expansions occurred in 29 states and the District of Columbia. To date, many states have still not adopted the expansion, and some who have adopted are considering new policies, such as work requirements, that may reduce the number of program participants (Ayanian et al., 2018; Sommers et al., 2019).

A number of studies have shown that these expansions significantly increased enrollment in Medicaid and decreased the number of people without insurance,[1] increased access to health care and the use of health services,[2] and improved the health of those gaining coverage.[3] However, fewer papers have explored the impact of Medicaid on financial well-being, despite the fact that one of the most important intended consequences of the expansion of health insurance is to provide financial protection from losses associated with illness or injury.[4] More importantly, previous studies that have examined the impact of the ACA Medicaid expansions (Hu et al., 2018; Brevoort et al., 2017; Caswell and Waidmann, 2017) have not been able to observe the financial outcomes of individuals who actually enroll in Medicaid. Instead, these studies rely on changes in financial outcomes among samples that include only a fraction of people actually affected by the ACA Medicaid expansions; for example, a sample of people living in low-income zip codes or people living in counties with high rates of uninsurance prior to the ACA Medicaid expansions. Therefore, to obtain the effect of gaining insurance through Medi-

[1]For example, Courtemanche et al. (2017), Kaestner et al. (2017), Miller and Wherry (2017) and Frean et al. (2017).

[2]Gruber and Sommers (2019) provide an overview. Other examples include Wherry and Miller (2016), Miller and Wherry (2017), Sommers et al. (2015), Ghosh et al. (2017), Simon et al. (2017), Gruber and Sommers (2019).

[3]For example, Cawley et al. (2018), Miller et al. (2019)

[4]See, e.g., Dobkin et al. (2018), Hu et al. (2018), Brevoort et al. (2017), Caswell and Waidmann (2017), Barcellos and Jacobson (2015), Gross and Notowidigdo (2011), Finkelstein et al. (2012), Argys et al. (2019).

caid expansion, these studies "back out" the effect of insurance coverage on financial outcomes by comparing the observed changes in financial outcomes to aggregate estimates of insurance coverage changes.

This indirect approach is not ideal because Medicaid beneficiaries represent only a small fraction of the total sample used. Therefore, small differences in estimates of the share of the sample that likely enrolled result in large changes in the estimated impact of Medicaid enrollment on financial outcomes. This problem is significant because estimates of the change in insurance coverage used in these back-of-the-envelope calculations are often based on survey data, which under-reports Medicaid coverage (Boudreaux et al., 2015). Therefore, there is still some uncertainty about the magnitude of the effect of gaining insurance coverage through the Medicaid expansions on financial well-being.

Also, because previous studies used samples containing only a small share of Medicaid enrollees, they likely lacked statistical power to reliably detect effects of Medicaid enrollment on rare, but policy-relevant outcomes such as evictions, bankruptcies, or wage garnishments. Finally, because prior studies have not had any information about Medicaid enrollees, they were unable to examine how gaining insurance affects those with poor health who are most likely to benefit from obtaining Medicaid. Nor could these studies examine whether the effects differed by the types of health conditions experienced by Medicaid enrollees. In sum, previous studies have provided an incomplete picture of the effects of the ACA Medicaid expansions on the financial well-being of those affected.

The only prior study to have access to financial information for those who obtained Medicaid was the Oregon Health Insurance Experiment (OHIE, Finkelstein et al., 2012) which examined an expansion that occurred several years prior to the ACA. Results from this study showed that gaining Medicaid significantly improved financial health. However, the sample sizes in the OHIE were relatively small with about 10,000 individuals gaining coverage. This may have limited the statistical power of the study to detect effects on relatively rare events such as bankruptcies or court judgments. In addition, the OHIE did not examine sub-groups within the Medicaid population, such as the chronically ill, for whom the benefits of health insurance are likely to differ.

This article reports novel evidence of the changes in financial health that occur upon enrollment in Medicaid. We analyze a new and unique dataset that links administrative records from Michigan on Medicaid participants' enrollment, demographic characteristics, and use of health services to their credit report data. For our main analysis, we leverage differences in the timing of Medicaid enrollment and examine changes in financial outcomes around the time of enrollment and up to 21 months after enrollment as compared to a counterfactual trend. The linked data allows us to: measure the changes in financial outcomes that occur upon enrollment in Medicaid for those actually affected; identify how such trends around the time of enrollment vary for subgroups defined by illness burden; and study the effect of Medicaid on rare, but particularly salient, financial outcomes such as bankruptcies.

Our results show that beneficiaries experience substantial improvements in financial well-being in the first 21 months after enrollment in Medicaid. First, the amount of medical bills in collections falls by about \$511 (about 51% relative to the pre-ACA mean) upon enrollment. This is between the per person reduction of \$390 found in Finkelstein et al. (2012) and the \$1,231 estimated reduction in Brevoort et al. (2017). We also find that, upon enrollment, the amount of debt past due that has not yet been sent to a third party collection agency falls by about \$234 (about 27%) and the number of public records (such as evictions, bankruptcies, or wage garnishments) falls by 0.05 (about 11%). Finally, the number of bankruptcies falls by 0.01 (about 11%). Our estimated change in bankruptcies is within the confidence intervals reported by Hu et al. (2018) and Finkelstein et al. (2012) (although neither of these two studies detects a statistically significant effect on bankruptcy), and smaller than the treatment effect implied by estimates in Gross and Notowidigdo (2011) and Argys et al. (2019).[5]

We also see evidence that enrollment in Medicaid is associated with improved access to credit markets. We find that the probability that an enrollee has a credit score in the "subprime" (≤ 600) range falls by about 2.6 percentage points, or about 4%, and in the "deep subprime" (< 500) range by about 3.7 percentage points, or about 21%, upon enrollment in Medicaid. This improvement in credit score is consistent with other research that finds that interest rates offered to low income individuals fall when Medicaid coverage expands (Brevoort et al., 2017) and that Medicaid expansion reduces use of payday loans (Allen et al., 2017). In addition, we see that individuals overdraw their credit cards 0.37 fewer months (an approximately 13% reduction) following enrollment, suggesting they may be less credit constrained.

We also examine whether these observed changes differed by enrollee characteristics. We find larger improvements in bills sent to third party collections and credit scores for enrollees with chronic illnesses (relative to those without) and among enrollees with a hospitalization or emergency department visit within the first 12 months of enrollment (relative to those with no such utilization). Among those who are chronically ill, effects of enrollment did not appear to vary substantially across those with different diseases. Even among groups without apparent high healthcare need, we see statistically significant improvements in financial outcomes around the time of enrollment. These results suggest that the financial benefits of Medicaid coverage are apparent across almost all subgroups of beneficiaries.

While our study has several advantage over other studies in the literature, it also has several disadvantages. Hu et al. (2018), Brevoort et al. (2017), and Caswell and Waidmann (2017) use data on credit reports for residents of non-expansion states as a comparison group in their analysis, which allows them to account for time effects. A limitation of our analysis is that we do not have a natural comparison group, such as individuals who were not eligible to obtain Medicaid

[5]Note, however, that Argys et al. (2019) examines the impact of losing, rather than gaining, coverage.

coverage, but for whom we also observe credit report outcomes. As a result, we cannot fully rule out that a contemporaneous but unrelated change in the economic or credit environment may be responsible for some of the improvements in credit outcomes we observe around the time of Medicaid enrollment. Second, we examine only one state, Michigan. This may naturally limit the generalizability of our study to other states and contexts. We also only have data on those who enrolled in the first 12 months that coverage was available; it's possible that these beneficiaries had the most to gain, and may experience greater financial improvements upon enrollment, relative to those who signed up for coverage in later years. Finally, while other studies can examine the average effect of health insurance coverage expansions over many years (Mazumder and Miller, 2016; Gross and Notowidigdo, 2011), we are only able to observe outcomes for, at a maximum, 21 months after enrollment. Our results therefore measure changes that occur relatively soon after gaining Medicaid coverage and may under-estimate the long-term impact if the effects grow with time, which is plausible.

2 Background

The ACA resulted in one of the largest expansions of health insurance coverage since the 1960s, with over 20 million individuals gaining insurance coverage since 2010 (Cohen et al., 2017). As part of the ACA, eligibility for Medicaid was expanded to include all individuals in households with incomes under 138 percent of the Federal Poverty Level. These eligibility expansions were made optional by a 2012 Supreme Court decision, and, to date, 33 states and DC have adopted these expansions. While estimates differ, it is widely acknowledged that most of the increase in health insurance coverage associated with the ACA came from the Medicaid expansion; Frean et al. (2017) estimate this fraction to be about 60 percent.

The Healthy Michigan Plan (HMP) was passed by the Michigan legislature and signed by the governor in September 2013, and was implemented in April of 2014. HMP was approved through a Section 1115 waiver that allowed Michigan to make modifications to the traditional Medicaid program. Although it is similar to other Medicaid programs in terms of services covered, HMP has additional cost sharing requirements for enrollees with higher incomes. After 6 months of enrollment, enrollees from households with incomes between 100 and 138 percent of the FPL are required to pay premiums equal to 2 percent of monthly income, but this can be reduced by completing a "health risk assessment" with a primary care provider. Fewer than 30 percent of enrollees are required to pay such a contribution, and, on average, the contribution amount is less than $5 per month (Cliff et al., 2017). Ayanian (2013) provides more details on the plan's characteristics and cost sharing provisions.

Upon implementation, there was rapid enrollment in HMP. The plan was advertised broadly through community organizations, hospital associations, and public health departments. Within the first 3 months, over 300,000 adults had enrolled in HMP, representing about 3.3 percent of Michigan's total population and substantially exceeding enrollment expectations (Ayanian et al., 2014).

As part of the state-sponsored evaluation of the expansion, researchers conducted a survey of HMP enrollees called Healthy Michigan Voices. The survey found that, in the 12 months prior to enrollment in HMP, approximately 60 percent had no insurance at any time in the year prior to enrollment; approximately 45 percent of enrollees reported problems paying medical bills; and about 26 percent said that they had foregone necessary care due to concerns about costs. After HMP enrollment, access to and use of care significantly increased according to these survey results (Goold and Kullgren, 2018). This evidence is in line with similar survey evidence examining the ACA Medicaid expansions nationally, e.g., Miller and Wherry (2017), and indicates that an important effect of HMP could be the financial protection it offers individuals who are faced with illness or injury.

3 Data

Data for this analysis comes from two sources. First, we used Medicaid administrative data that includes the month and year of HMP enrollment, income relative to FPL as determined at the time of enrollment, the number of emergency department visits and hospitalizations over the first 12 months enrollment, and a dichotomous flag indicating the presence of a diagnosis code for a chronic illness on any encounter in the first 12 months of HMP enrollment.[6] The administrative data includes all individuals who enrolled in HMP between April of 2014 and March of 2015. We excluded individuals who were enrolled in a different state program (e.g., the Adult Benefits Waiver program, which covered some services for adults in households with incomes under 35 percent of the FPL) in the year before their enrollment in HMP in order to focus on individuals transitioning from no insurance to HMP coverage. In our main analysis, we exclude those who had other sources of insurance when they enrolled in Medicaid (i.e., those for whom Medicaid is a secondary payer), but we use these individuals as an additional control group in a supplementary differences-in-differences analysis. Since HMP re-determines eligibility for the program after 12 months, most (82%) of the enrollees in our sample are enrolled for at least that long.[7] Our data do not include information on insurance status prior to enrollment in Medicaid, although previously noted survey evidence and prior research has found relatively little evidence that beneficiaries dropped private coverage in order to enroll in Medicaid (Kaestner et al., 2017; Frean et al., 2017). If some beneficiaries dropped private plans in order to enroll in Medicaid, we would expect that the change in financial outcomes upon enrolling would be muted for this group.

[6] Chronic illnesses were defined using HCUP Chronic Condition Indicator software, see: Healthcare Cost and Utilization Project Chronic Condition Indicator for ICD-9-CM. Accessed 9/5/2017. Available at: http://www.hcup-us.ahrq.gov/toolssoftware/chronic/chronic.jsp.

[7] We only observe the number of months enrolled during the first year in our data, so we do not know how many beneficiaries disenrolled after the 12-month period. Beneficiaries are not dropped from our sample if they disenroll over the sample period.

Second, data from TransUnion on consumer credit histories was matched with the Healthy Michigan administrative data using name, address, and social security number. TransUnion credit reports were semi-annual and reported in January and July, starting with July 2011 and ending with January 2016 for a total of ten observation periods. In order to prevent TransUnion from identifying HMP enrollees, the research team included 550,000 randomly-selected individuals from another state health database, the Michigan Care Improvement Registry (MCIR), to "mask" the HMP sample. The MCIR database from which the masking sample was drawn includes any Michigan residents who have had a vaccine (such as a flu shot) in the last 20 years. The masking sample was further limited to those in the same age range as the HMP enrollees. Prior to providing the matched data to the researchers, TransUnion removed all personally identifying information. See the Appendix for additional details on the match process.

An enrollee may appear in the credit reporting agency data if he or she had applied for or opened an account (such as a credit card or personal loan), generated a "public record" with the court system (such as an eviction or bankruptcy), or failed to pay a bill that was reported to collection agencies (such as an unpaid hospital or utility bill). Table 1 compares characteristics across HMP enrollees who were and were not successfully matched to a credit record. Approximately 98 percent of HMP enrollees in our sample were matched; those who were not matched were disproportionately poorer, younger, and less likely to have a hospital or emergency department visit or a chronic illness within the first 12 months of enrollment. Our match rate is much higher than the 68.5 percent match rate reported in the OHIE, likely due to our inclusion of social security number as a match variable.[8]

From the credit report information, we use the total amount of debt that has been sent by the original creditor to a third party collection agency in the last 12 months.[9] This debt includes

[8]Note that this table includes enrollees for whom Medicaid is a secondary payer, which we exclude for our main analysis.

[9]To be clear, we include those with zeroes in their collection balances. In addition, since the amount recently

unpaid bills (such as a utility bill), or severely delinquent credit accounts (such as a credit card bill that is over 180 days late). Within third party collections, we also examine collections specifically for medical bills. We also look at debt on credit accounts that is 30 days or more past due but not yet sent to a collection agency. The total amount of debt on which a consumer is delinquent is the sum of the amount in collections and the amount past due but not yet in collections. It is worth noting that in 2015 there was a settlement between the three main credit bureau agencies and the New York Attorney General that created a 180 day waiting period before medical debt can be reported. However, this phase of the settlement only began in 2017 and did not actually take place until 2018 which is well past our period of analysis.

In addition, we evaluated the number of public records recorded on an individual's credit report. Public records include evictions, wage garnishments, and bankruptcies, as well as any law suits or other court judgments that could negatively affect an individual's credit worthiness. A subset of public records are bankruptcies, which we examine separately. Public records stay on an individual's credit report for many years, and may be removed over time if they expire (typically after 7 to 10 years). We examine changes in public records (relative to the pre-HMP trend); these changes represent the net flow into (or out of) public records.

If health insurance coverage affects delinquencies, then it may also affect access to credit. Medicaid coverage may lead individuals to experience improved access to credit markets, for example in the form of lower interest rates or higher credit card or loan approval rates.[10] Indeed, Brevoort et al. (2017) find that when states expand Medicaid, individuals receive more favorable interest rate offers from credit card companies. Accordingly, we examine an individual's Vantage 3.0 score, a commonly-used version of the credit score that is similar to a FICO score.

sent to collections can change over time, our estimates will provide effects on the "flow" into collections rather than the "stock" of collections.

[10]A previous draft of this analysis also included direct measures of borrowing, auto and credit card debt, as outcome variables. However, in later analysis, we found that the results associated with these outcomes were not robust to alternative specifications and sample definitions. We have therefore omitted these results.

Lenders use this score when evaluating whether to extend credit, and at what price, making it a convenient summary of access to credit markets and general creditworthiness. We examine the probability that an individual has a credit score in the "subprime" (≤ 600) range, as well as in the "deep subprime" (< 500) range, indicating that this individual would have a high expected default rate and therefore experience poor access to credit.[11] Another outcome we analyze is the number of months a consumer is overdrawn on his or her credit card out of the last 12 months. While being overdrawn is not a measure of credit access or delinquency per se, it is a sign that the consumer is having difficulty spending less than his or her card limit (and incurring fees as a result).

Table 2 presents descriptive statistics from our matched sample. The top panel shows descriptive statistics related to the credit report outcomes prior to HMP enrollment. The bottom panel shows characteristics that are either measured at enrollment (age at enrollment, gender, income relative to the FPL) or during the first 12 months of enrollment (chronic illness status, hospitalizations, and ED visits). Column 1 shows statistics for our main sample and columns 2 and 3 show statistics for those in two subgroups of the main sample: those with a hospitalization in the first 12 months of enrollment and those with a chronic illness.

In the full sample, average household income upon enrollment in HMP is about 36 percent of the FPL, which would be about $7200 for a family of 3 or $4200 for an individual in 2014. HMP enrollees also tend to be in poor health, with about 73 percent of enrollees having a chronic illness. The average number of hospitalizations in a year is 0.15 and the number of ED visits in a year is about 1. HMP enrollees have high rates of delinquencies relative to their income. In our main sample, enrollees owe about $1985 to third party collectors (with $1002 related to medical bills) and an additional $874 on average past due on open credit accounts. Note that the amount in collections is nearly 6 times higher in this group than in a random sample of

[11] Our data use agreement with TransUnion prohibits us from using the credit score itself as an outcome.

credit reports from low-income, high uninsurance rate zipcodes, although the average amount past due on open credit accounts is lower (Hu et al., 2018). About 69 percent of HMP enrollees have credit scores in the subprime range, with about 18 percent in the deep subprime range. In general, delinquencies appear to be worse among those with higher apparent health need. Among enrollees with a hospitalization or ED visit in the first year, collections were $2686 prior to HMP enrollment, with $1615 related to medical bills. Similarly, among those with a chronic illness, collections were $2221, with $1194 related to medical bills. These descriptive statistics show that HMP enrollees tend to be poor, in poor health, and in dire financial straits.

As noted, our analysis is a case study of Michigan's Medicaid expansion. While a limitation in terms of external validity, examining Michigan may generate an interesting case because Michigan is relatively conservative for an expansion state according to survey data from Gallup[12] and is at the forefront of policy innovations in the Medicaid program such as work requirements (Ayanian et al., 2018). At the same time, analysis of only Michigan could be of limited use when predicting behavior in other states if Michigan Medicaid enrollees are substantially different than enrollees in other states. To examine how characteristics of Michigan Medicaid enrollees compare to those in other expansion states, we use data from the American Community Survey. We focus on Medicaid enrollees who are childless adults, who were most likely to have gained coverage through the ACA expansion. Appendix Table A1 compares Michigan Medicaid enrollees to those in all other expansion states, while Appendix Table A2 compares Michigan enrollees to only those in Midwestern expansion states. As Appendix Table A1 shows, Medicaid enrollees in Michigan are similar to those in the other expansion states on several dimensions (focusing on standardized differences), although those on Medicaid in Michigan are less likely to be Hispanic, work somewhat less, are more likely to be male and have lower incomes than Medicaid enrollees in other expansion states. These differences are

[12]https://news.gallup.com/poll/226730/conservative-leaning-states-drop.aspx

smaller, however, when we compare Michigan enrollees to similar enrollees in other Midwestern expansion states; the largest difference with this comparison group is only one tenth of a standard deviation. This descriptive evidence suggests that the results from our case study are unlikely to differ significantly from broader analyses of other states in the same region because of large differences in sample characteristics, although the political, demographic, and economic setting in Michigan may cause results to be less applicable when extrapolating to states outside the region.

4 Empirical Approach

We conduct an event study analysis that examines changes in financial outcomes that occur around the time an individual enrolls in HMP. In our data, we observe the month in which an individual enrolls in the Healthy Michigan program and their credit report outcomes semi-annually beginning in July of 2011. We combine these two pieces of information to trace out changes around the time of enrollment, relying on the fact that individuals enroll at different times relative to the calendar months in which we observe the credit data. For example, in order to identify the effect after one month of enrollment in the program, we must use individuals who enrolled exactly one month before we observe their credit reports; since we observe credit reports in January and July, the coefficient on the event study indicator for one month after enrollment is identified by individuals who enrolled in December or June.

We illustrate this data structure with a brief example. First, consider the cohort who enrolled in May of 2014. The first credit report we observe for this cohort is July of 2011, which is 34 months prior to their enrollment. The last credit report we observe for this cohort is January of 2016, which is 20 months after their enrollment. The average medical collections for this cohort is plotted by calendar time in the first panel of Figure 1, with the months relative to enrollment ("event time") displayed above each mean. We also include a linear trend to show how medical collections change around the time of enrollment.

The cohort who enrolled in August of 2014 is also observed for the same 10 calendar time periods; however, for this cohort, the July 2011 credit report corresponds to 37 months prior to enrollment, while the January 2016 credit report corresponds to 17 months post enrollment. Average medical bills in collection for this cohort are plotted in the second panel. Finally, we also plot the outcome for the cohort who enrolled in January of 2015 in panel 3 of Figure 1. For this cohort, we observe 42 months prior to enrollment but only 12 months after enrollment.

The fourth panel of Figure 1 subtracts the mean level of medical collections from each cohort and plots the residual against event time, rather than calendar time. The fact that each cohort began the program at a different time allows us to trace out changes relative to time of enrollment. Using only these three cohorts, we see that later event periods correspond with larger decreases in collections even if they refer to the same calendar month; for example, event months 12, 17, and 20 are all estimated using the credit reports observed in January of 2016. As we increase the number of cohorts, we are able to gain precision in our estimate of each event study coefficient, as multiple cohorts will contribute to the same event study coefficient; for example, the event study coefficient for event time 1 month after enrollment will be estimated using cohorts that enrolled in either June and December of 2014. The final panel of Figure 1 groups the months into quarters. This reduces noise and also makes the figures easier to read, given the large number of month effects potentially identified. In our main analysis, we estimate quarter, rather than month, event time effects, although the results using month-level event times are very similar.[13]

Our analysis takes advantage of this variation in beneficiaries' enrollment date and the timing at which we observe credit reports using an event study design. Specifically, we examine whether there were significant deviations in the trend of financial outcomes around the time

[13]Note we still estimate monthly cohort effects and include indicators for whether the report is observed in July or January.

an individual enrolls in HMP, similar to models used in, e.g., Dobkin et al. (2018), Blascak et al. (2016), and Gross et al. (2018). We estimate this model using the following regression specification:

$$Y_{ic\tau} = \alpha_c + \delta_\tau + \beta_m + \epsilon_{ic\tau} \tag{1}$$

where i refers to individual enrollees, α_c refers to enrollment month fixed effects, and τ refers to event quarter indicators. We also include indicators for the calendar month (January or July) to account for seasonality (β_m). Our primary variables of interest are the fixed effects associated with each event period, denoted δ_τ, ranging from 13 quarters prior to enrollment to 7 quarters after enrollment, with $\tau = 0$ denoting the quarter of enrollment. We use the quarter prior to enrollment ($\tau = -1$) as our reference category and set this equal to zero.

We estimate two versions of this model that account for linear trends in event time.[14] For these specifications, the event study coefficients can be interpreted as the change in outcomes experienced by beneficiaries relative to a counterfactual linear trend. The first version estimates a linear trend in event time on the pre-enrollment data. We then remove this trend from the outcome variable to create de-trended predicted values of the outcome $\tilde{Y}_{ic\tau}$. This de-trended outcome is then used in place of $Y_{ic\tau}$ in equation (1).[15]

The second version estimates a variation on model (1) that imposes the linear pre-enrollment trend rather than estimate pre-enrollment fixed effects in the following way (similar to Gross et al. (2018)):

$$Y_{ic\tau} = \alpha_c + \beta_1\tau + \delta_\tau(\tau > 0) + \beta_m + \epsilon_{it}. \tag{2}$$

[14]We select a linear trend, rather than quadratic or higher order polynomial, because pre-trends in our data appeared to be approximately linear.

[15]A degrees of freedom correction is required due to this first stage; however, given our large sample sizes, this correction is not discernible for the number of significant digits we report.

In this model, only post-enrollment fixed effects are included, as is a linear trend in event time (τ). We estimate all models using ordinary least squares and report heteroskedasticity-robust standard errors that are clustered at the individual level.

4.1 Endogenous enrollment timing

Our empirical approach assumes that there is no factor that affects financial outcomes among our sample that is correlated with the timing that an individual enrolls in Medicaid. This assumption could be violated if, for example, an individual enrolls in Medicaid as the result of a health shock; in that case, the timing of enrollment is correlated with the outcome variable (with lag) and may generate a spurious relationship between Medicaid enrollment and the outcome. We believe the concern about endogenous enrollment is mitigated in our setting relative to other contexts. First, if individuals enroll because they experience a negative health or income shock, then this would result in a worsening of financial health and a spurious *positive* relationship between Medicaid and financial distress. Instead, we find that Medicaid reduces financial distress. This suggests that, if anything, our results are too conservative. Second, over 30 percent of the individuals in our sample enrolled in the first month that HMP became available; see Figure 2, which presents a histogram of enrollment times. For these individuals, it is likely the timing of enrollment was driven by the policy change, rather than an individual-specific shock. We analyze this group separately and find similar effects as when we use the full sample. It does appear that earlier enrollees had somewhat worse financial outcomes (see Appendix Figure A1), but inclusion of enrollment cohort fixed effects (β_c) should account for any non-time varying differences such as these. In addition, enrollee characteristics are fairly similar across early and late enrollers, as reported in Figure 2. These considerations provide some evidence to suggest endogenous enrollment may not be severe.

However, we cannot rule out endogenous enrollment, particularly for the 70 percent of the sample that enrolled after the first month. If we think that individuals first experience a health decline, and then opt to enroll in Medicaid as a consequence, we would see pre-existing trends in our outcome variables, especially those closely tied to medical bills. We find little evidence of pre-existing trends, with practically no evidence of any pre-trends for medical collections, which is the variable that would be most sensitive to this problem. This suggests that enrollment for this particular group made eligible by the ACA is unlikely to be caused by a slowly changing but unmeasured factor (e.g., underlying health) correlated with the outcome variable. At the same time, if there is a health shock that onsets rapidly (for example, due to a car accident), it may not be detected in pre-enrollment trends. If this health shock results in Medicaid enrollment (for example, because of processes to enroll patients when hospitalized), then this would bias estimates upwards assuming such health shocks harm financial outcomes (for example, by reducing labor income, as documented in Dobkin et al. (2016)). However, this type of problem would not apply to medical debt because the hospitalization would be covered by Medicaid. Thus, to the extent that this type of selection is a problem, we might expect results to differ with respect to medical debt versus other types of debt. However, our findings appear to be fairly consistent across outcomes. Although we cannot fully rule out that endogenous enrollment timing plays some role in the changes in outcomes we observe upon Medicaid enrollment, these patterns suggest such endogeneity may be limited in our setting.

5 Results

We present the coefficients on the event study variables described in equation (1) in Figures 4 and 5. In these figures, the horizontal axis displays the quarter relative to enrollment, with the vertical dotted line indicating the enrollment quarter. The quarter prior to enrollment is our reference month and this coefficient is set equal to zero. For most outcomes related to delinquency, we see relatively little trends prior to enrollment in HMP, but observe divergence around the time of enrollment. One exception is total collections, for which there appears to be a slight negative trend in the six quarters immediately prior to enrollment. In all cases, we see reductions in measures of delinquency upon enrollment in HMP, and these reductions appear to grow larger over time. This pattern suggests that the changes in outcomes may be even larger in later time periods for which we do not have data.

The estimates for selected event time coefficients related to delinquency and creditworthiness are reported in Table 3. We report the effects observed 2, 4, and 7 quarters after enrollment. The first column shows the effects for the model described in equation (1). The second column removes the pre-enrollment trend; these estimates match the event study figures. The third column reports the estimates from the model in equation (2). Across all three specifications, we see evidence of substantial reductions in the total amount of bills sent to third party collection agencies that occurs around the time of enrollment in HMP. By the end of the analysis period (1.75 years after enrollment), we see that third party collections have fallen by between \$609 and \$763, or 31 to 38 percent relative to the average amount in collections before enrollment. This is somewhat smaller than the \$1,140 reduction in collections estimated by Hu et al. (2018), although larger than the \$469 reduction in total collections reported in Finkelstein et al. (2012).

Much of the reduction in third party collections appears to be driven by a reduction in medical bills being sent to collection; we observe reductions in medical collections of between \$328 and \$563, or 33 to 56 percent. This is also smaller than the \$1,231 reduction in medical bills found in Brevoort et al. (2017), but similar in magnitude to the \$390 reduction in medical bills reported in Finkelstein et al. (2012). We also see a reduction in the amount of debt past due on credit accounts (i.e., debt past due that has not yet been sent to a third party collection). Our estimates indicate that nearly two years after HMP enrollment, the amount past due is reduced by between \$233 and \$257, or 27 to 29 percent. In contrast, Hu et al. (2018) find no significant effect on this outcome.

For some outcomes, the improvements in delinquencies (panels a through c in Figure 4) tend to emerge between 1 and 3 quarters after enrollment and grow larger over time. The lag in these effects may reflect the period between the time when care is used and when improvements in financial outcomes become apparent. It may also be due, in part, to new requirements for non-profit hospitals in the Affordable Care Act. In particular, Section 501(r) of the Internal Revenue Code placed new obligations on non-profit hospitals to determine patient eligibility for charity care policies and to provide several rounds of notifications to patients before pursuing debt collection measures (Nikpay and Ayanian, 2015).[16] As noted earlier, our results are entirely unrelated to the settlement with the New York Attorney General's office which did not go into effect until 2018.

In addition to these reductions in delinquencies, we also see reductions in other measures of financial distress. The number of public records on an individual's credit report falls by between 0.05 and 0.07 by the end of our sample period (11 to 16 percent), and the number of bankruptcies on the credit report falls by about 0.01 (about 11 percent). This change in bankruptcies is smaller than the treatment effect implied by estimates in Gross and Notowidigdo (2011) but within the confidence intervals reported by Hu et al. (2018) and Finkelstein et al. (2012).[17]

Table 4 displays the results on the effect of HMP on credit access outcomes. The fraction of individuals who are classified as "subprime" falls by about 2.6 percentage points, or about 4 percent relative to the baseline mean. The fraction of those classified as "deep subprime" also falls about 3.7 percentage points, or 21 percent relative to the baseline mean. We also observe a reduction in the number of months that an individual has overdrawn his or her credit card in

[16] The IRS required hospitals to file information on their compliance with these measures on their tax returns beginning in 2012, although this rule was not fully enforced (i.e., no hospitals actually lost non-profit status for failure to comply) until 2016.

[17] We also examine how the probability that the consumer has any bankruptcy, rather than the number of bankruptcies, changes around the time of Medicaid enrollment. These results are reported in Appendix Figure A.4. Because most beneficiaries have no more than 1 bankruptcy on their credit report, the results are very similar to those presented here using the number of bankruptcies as the dependent variable.

the last 12 months of about 0.37 months, about 13 percent relative to the baseline mean of 2.75 months. Note that sample sizes are smaller for this final outcome because not all individuals in the sample have a credit card.

5.1 Subgroup Analyses

As noted, an advantage of our data is information on HMP enrollees, which we used to conduct analyses by subgroup based on health status (chronic illness), use of health services, and income at the time of enrollment. We present these results graphically in Figures 6 - 9.

Figure 6 compares the effect of HMP enrollment across individuals who did and did not have a hospitalization or ED visit in the first 12 months of enrollment in the HMP program. We find significantly stronger effects of HMP enrollment on collections and medical collections during the first year for the group with an ED visit or hospitalization. The magnitudes of the effects are large (e.g., \$500 to \$1000 in quarters 3 to 7) and almost all due to a decrease in medical collections, which is exactly what is expected from the financial protection provided by Medicaid. However, even among the group with no hospitalization or ED visit in the first year, we still detect statistically significant reductions in collections, albeit smaller in magnitude. There also is greater improvement in the fraction deep subprime and larger reductions in the months overdrawn on a credit card among the group that was hospitalized or had an ED visit. Both groups experienced similar reductions in amount past due, public records, and bankruptcies.

A similar pattern is apparent in Figure 7, which compares outcomes across individuals with and without a chronic illness recorded in their first 12 months of enrollment. The changes in collections observed upon HMP enrollment is much larger (\$400 to \$800 between quarter 3 and quarter 7) for the group with the chronic illness than those without. Again, almost all of this effect on collections is because of a reduction in medical debt. The reduction in public records, bankruptcies, and fraction deep subprime are also larger. Other outcomes exhibit similar patterns across the two groups.

It could be the case that, among those with chronic conditions, some illnesses result in greater financial burden than others because of the cost of treatment. To examine this, we evaluated the changes that occurred around the time of Medicaid enrollment for chronically ill beneficiaries who have one (or more) of the ten most prevalent diagnoses based on the broad diagnosis groups defined by the International Classification of Diseases. These ten most common categories are: Endocrine, nutritional and metabolic diseases and immunity disorder; diseases of blood and blood-forming organs; mental disorders; diseases of the nervous system and sense organs; diseases of the circulatory system; diseases of the respiratory system; diseases of the digestive system; diseases of the genitourinary system; diseases of the musculoskeletal system; and ill-defined conditions and symptoms. In Figure 8, we plot the Quarter 7 estimate on all collections for each of these subgroups. Surprisingly, the effects are relatively similar across these different groups, with reductions in collections ranging from -$874 (for endocrine and metabolic disorders) to -$1600 (for diseases of the blood and blood-forming organs). Other outcomes are reported in the Appendix, and exhibit a similar pattern, with large improvements upon enrollment documented across all disease classes. Note that many beneficiaries have more than one condition, so the sample has some overlap across these disease categories.[18]

We also separately examine the group of beneficiaries who enrolled the first month of the program. In this analysis, we do not exploit the timing of enrollment. These beneficiaries likely enrolled due to the policy change rather than, for example, a health shock. The results are presented in Table 5 and Figure 9. By the end of the sample period, the results for most outcomes are similar in this sample relative to what we found when we use the entire sample. We also find similar, though somewhat larger, effects in earlier months for collections, medical

[18]In the Appendix, we also present results for beneficiaries with a cancer diagnosis. These beneficiaries experience somewhat larger reductions in bankruptcy upon enrollment in HMP, although changes in other outcomes are similar to those with any chronic illness.

collections, public records, bankruptcies, subprime and deep subprime outcomes. However, we note that there is a longer lag before we observe an effect on the amount of debt past due; we do not observe statistically significant changes in this outcome after a year and a half of enrollment.

Finally, we investigate how the trends in financial outcomes vary across enrollees with different baseline income. Higher incomes upon enrollment may help beneficiaries weather financial shocks, and beneficiaries with incomes at or above 100% of the FPL faced a small (maximum 2% of income) premium. The financial benefit of Medicaid might therefore be lower for this group. These results are presented in Figure 10, which shows the changes in financial outcomes for those with incomes under 100 percent of the FPL (in black) and those with incomes between 100 and 138 percent of the FPL (in red). In general, lower income beneficiaries experienced greater improvement in financial outcomes than higher income beneficiaries. However, the change in bankruptcies and public records is slightly larger among higher income enrollees. This may be because higher income enrollees have more assets that could be protected by bankruptcy, making it a more attractive option.

5.2 Alternative Empirical Approaches

Our main analysis compares changes in outcomes among enrollees before and after they enroll in Medicaid relative to the trend in outcomes observed prior to enrollment. Put differently, we assume that, had the enrollee not enrolled in HMP, they would have continued on their pre-existing trend, and that we can use this trend to form a reasonable counterfactual when estimating the treatment effect of enrollment. This assumption may not be valid if there were other changes besides HMP that would have caused outcomes to deviate from their trend around the time of HMP enrollment that are unrelated to the program itself. Furthermore, while enrollment time does not correspond perfectly to calendar time (since beneficiaries choose to enroll in different months), it is correlated with calendar time, so changes in outcomes around the time of enrollment could be affected by unrelated secular changes in the economic environment or local credit markets. To address this issue, we conduct two difference-in-differences (DiD) analyses that use two different "control" groups that were either not affected or less affected by Medicaid expansion in Michigan.

The first comparison group we use is individuals who enroll in HMP, but for whom Medicaid is a secondary payer. These enrollees have another form of insurance, which we refer to as "third party liability" (or TPL)–that is, a third party is responsible for paying for some portion of the enrollees' medical care (e.g., worker's compensation or employer-sponsored health insurance). These enrollees may be viewed as a reasonable comparison group because they were likely to be less affected by Medicaid coverage than enrollees for whom Medicaid is their only coverage. The second comparison group is the "masking" sample that was drawn at random from an administrative health database for the state of Michigan, the Michigan Care Improvement Registry (MCIR). Recall that this database includes any Michigan residents who have had a vaccine (such as a flu shot) in the last 20 years. The sample is further limited to those in the same age range as the HMP enrollees who are living in low-income zip codes with relatively high rates of uninsured individuals prior to the ACA.[19] This "masking" sample excludes HMP enrollees. The original purpose of this sample was to prevent TransUnion from identifying HMP enrollees (see Appendix), but we make use of it here as a second comparison group to the HMP enrollees. Because the TPL and non-enrollee Michigan comparison group exhibit somewhat different pre-trends from the HMP enrollees (see Appendix Figures A4-A5), we re-weight each group to better resemble the HMP population. See the Appendix for further information on this re-weighting and details about the empirical approach. Estimates in Appendix Figure A6 for the DiD analysis that uses the TPL comparison group are largely similar to the event-

[19]Specifically, we limit individuals to those living in zip codes that fall in the top quartile of the distribution of the fraction of residents who were uninsured and under 138 percent of the FPL prior to the ACA, using the 5-year 2012 ACS estimates.

study estimates reported above, although the point estimates tend to be smaller and in some cases not significant. For example, the DiD estimates for medical collections suggest a $200 to $300 reduction in medical debt. This difference is expected since the TPL group also likely experienced some financial protection from HMP because of the comprehensive coverage of Medicaid. Estimates pertaining to the analysis that uses the "masking" comparison group (Appendix Figure A7) differ somewhat from previous results. Of note is the estimate showing an increase in collections and subprime rates immediately following the HMP coverage expansion that only become negative at the end of the post-expansion period. We also do not find significant improvements in bankruptcies in this analysis, although we continue to find significant reductions public records, the number of months overdrawn on credit cards, and (at least for the last time period), the amount of debt 30 days or more past due. While there is not much evidence of differential pre-trends in Figure A7, the lack of an improvement in medical collections is concerning because of the near mechanical link between the financial protection against medical debt associated with Medicaid. Accordingly, we caution that the masked comparison group may not be a valid control.

Despite the consistency between the event-study estimates and DiD estimates using the TPL comparison, the lack of agreement between the DiD estimates using the masked sample and other estimates suggests that we cannot make strong claims about the causal nature of our estimates. We are unable to definitively rule out the possibility that there were concurrent, but unrelated changes in local economic or financial factors that may be affecting these patterns.

6 Discussion

Our study provides new evidence on how financial outcomes change around the time of enrollment for beneficiaries of the ACA Medicaid expansion. Our analysis uses new data from Michigan's Medicaid expansion plan (HMP) linked to longitudinal credit report information at the individual level. These unique data allow us to identify the effect of the ACA Medicaid expansion in Michigan on the financial health of those who actually enrolled in Medicaid. The unique data also allow us to conduct analyses using samples stratified by health, by use of health care services and income, which are all interesting from both an economic and policy perspectives. Results from our analysis suggests that upon enrollment in HMP, beneficiaries experience improvements across a broad swath of financial measures. Effects are largest among those with chronic illnesses and who used the ED or hospital during their first year of enrollment.

The broad improvement in financial health is an important finding because while it is near mechanical that Medicaid would reduce medical debt, the improvements in other dimensions of financial well-being may have salutary effects on a broad range of socioeconomic outcomes. Improved credit scores and less debt mean greater access to credit and the ability to allocate family resources to other uses that may have lasting effects. This possibility is something future research should investigate, particularly because effect sizes are large when compared to the sample mean and appear to grow larger over time. The amount of money "saved" is non-trivial when compared to family incomes of Medicaid enrollees and the growing impacts suggest even larger amounts over a longer time horizon.

Our analysis is complementary to other studies that have examined how financial outcomes change when individuals gain or lose Medicaid coverage. Other work examining the ACA Medicaid expansions have been national in scope, but did not have access to data on actual enrollees. Instead these studies relied on data aggregated to geographic areas (such as states, counties, or zip codes) that includes many non-enrollees (Hu et al., 2018; Brevoort et al., 2017; Caswell and Waidmann, 2017). As noted, the linked data allows us to conduct several analyses that is not possible in the aggregated data, such as examining policy-relevant subgroups. At the same time, the broader analyses and national scope facilitate the use of quasi-experimental methods using individuals in non-expansion states as controls. Our ability to conduct similar analyses was more limited, although we included two additional exercises to complement the primary, event-study analysis. Nevertheless, we cannot make strong claims about the causal nature of our estimates in the absence of quasi-experimental design using a clearly valid comparison group. Future work taking advantage of new data sets that generate new linkages but are national in scope (such as the Census Bureau's Mortality Disparities in American Communities project or the National Health Interview Survey linked Medicaid files) may be able to combine the strengths of both approaches.

Our analysis is also limited to only one state-–Michigan-–and may not generalize, although in terms of enrollee characteristics, Michigan Medicaid participants are quite similar to those in other expansion states in the Midwest region. Finally, we observe outcomes for less than two years following enrollment and there was evidence of growing impacts. Therefore, our estimates may understate the full effect of Medicaid on financial health that accumulates over time.

Despite these limitations, the use of linked data highlights several interesting avenues for future research. We observe that changes in financial outcomes vary considerably on the basis of beneficiary health. It would be similarly useful to identify whether beneficiary health is an important dimension of heterogeneity in determining the impact of Medicaid coverage on other policy-relevant outcomes such as labor market participation (Kaestner et al., 2017). We also see some evidence that financial effects vary across groups that face different cost-sharing requirements. Future work using linked data to identify the consequences of other Medicaid program features, such as work requirements or healthy behavior incentive programs, could also provide important, policy-relevant evidence on the desirability of such features.

References

Allen, H., A. Swanson, J. Wang, and T. Gross (2017). Early medicaid expansion associated with reduced payday borrowing in California. *Health Affairs 36*(10).

Argys, L., A. Friedson, M. M. Pitts, and D. S. Tello-Trillo (2019). Losing public health insurance: Tenncare disenrollment and personal financial distress. FRB Atlanta Working Paper.

Ayanian, J. (2013). Michigan's approach to medicaid expansion and reform. *New England Journal of Medicine 369*, 1773–1775.

Ayanian, J., S. Clark, and R. Tipirneni (2014). Launching the healthy michigan plan — the first 100 days. *New England Journal of Medicine 371*, 1573–1575.

Ayanian, J., R. Tipirneni, and S. Goold (2018). Mitigating the risks of medicaid work requirements. *New England Journal of Medicine 379*, 803–805.

Barcellos, S. and M. Jacobson (2015). The effects of medicare on medical expenditure risk and financial strain. *American Economic Journal: Economic Policy 7*(4), 41–70.

Blascak, N., J. S. Cheney, R. M. Hunt, V. Mikhed, D. Ritter, and M. Vogan (2016). Identity theft as a teachable moment. Federal Reserve Bank of Philadelphia Working Paper.

Boudreaux, M. H., K. T. Call, J. Turner, B. Fried, and B. O'Hara (2015). Measurement Error in Public Health Insurance Reporting in the American Community Survey: Evidence from Record Linkage. *Health Services Research 50*(6).

Brevoort, K., D. Grodzicki, and M. B. Hackmann (2017). Medicaid and financial health. NBER Working Paper 24002.

Caswell, K. and T. Waidmann (2017). The affordable care act medicaid expansions and personal finance. *Medical Care Research and Review*.

Cawley, J., A. Soni, and K. Simon (2018). Third year of survey data shows continuing benefits of medicaid expansions for low-income childless adults in the u.s. *Journal of General Internal Medicine 33*(9), 1495–1497.

Cliff, E. Q., S. Miller, R. A. Hirth, J. T. Kullgren, and J. Z. Ayanian (2017). Price sensitivity to premiums in medicaid: Evidence from discontinuous program rules.

Cohen, R., E. Zammitti, and M. Martinez (2017). Health insurance coverage: Early release of estimates from the national health interview survey, 2016. NCHS Report.

Courtemanche, C., J. Marton, B. Ukert, A. Yelowitz, and D. Zapata (2017). Early impacts of the Affordable Care Act on health insurance coverage in medicaid expansion and non-expansion states. *Journal of Policy Analysis and Management 36*.

Dobkin, C., A. Finkelstein, R. Kluender, and M. Notowidigdo (2016). The economic consequences of hospital admissions. *American Economic Review 108*(2), 308–352.

Dobkin, C., A. Finkelstein, R. Kluender, and M. J. Notowidigdo (2018, February). The economic consequences of hospital admissions. *American Economic Review 108*(2), 308–52.

Finkelstein, A., S. Taubman, B. Wright, M. Bernstein, J. Gruber, J. Newhouse, H. Allen, and K. Baicker (2012). The oregon health insurance experiment: Evidence from the first year. *Quarterly Journal of Economics 127*(3), 1057–1106.

Finkelstein, A., S. Taubman, B. Wright, M. Bernstein, J. Gruber, J. P. Newhouse, H. Allen, K. Baicker, and the Oregon Health Study Group (2012). The Oregon health insurance experiment: Evidence from the first year. *Quarterly Journal of Economics 127*(3), 1057–1106.

Frean, M., J. Gruber, and B. Sommers (2017). Premium subsidies, the mandate, and medicaid expansion: Coverage effects of the affordable care act. *Journal of Health Economics 53*, 72–86.

Ghosh, A., K. Simon, and B. Sommers (2017). The effect of state medicaid expansions on prescription drug use: Evidence from the affordable care act. NBER Working Paper.

Goold, S. D. and J. Kullgren (2018). Report on the 2016 healthy michigan voices enrollee survey. University of Michigan Institute for Healthcare Policy and Innovation Report.

Gross, T. and M. Notowidigdo (2011). Health insurance and the consumer bankruptcy decision: Evidence from expansions of Medicaid. *Journal of Public Economics 97*(7–8), 767–778.

Gross, T., M. Notowidigdo, and J. Wang (2018). The marginal propensity to consumer over the business cycle. Working Paper.

Gruber, J. and B. Sommers (2019). The affordable care act's effects on patients, providers and the economy: What we've learned so far. *Journal of Policy Analysis and Management*.

Hu, L., R. Kaestner, B. Mazumder, S. Miller, and A. Wong (2018). The effect of the affordable care act medicaid expansions on financial well-being. Journal of Public Economics.

Kaestner, R., B. Garrett, J. Chen, A. Gangopadhyaya, and C. Fleming (2017). Effects of aca medicaid expansions on health insurance coverage and labor supply. *Journal of Policy Analysis and Management 36*(3), 608–642.

Mazumder, B. and S. Miller (2016). The effects of the massachusetts health reform on household financial distress. *American Economic Journal: Economic Policy*.

Miller, S., S. Altekruse, N. Johnson, and L. Wherry (2019). Medicaid and mortality: New evidence from linked survey and administrative data. NBER Working Paper # 26081.

Miller, S. and L. Wherry (2017). Health and access to care during the first 2 years of the aca medicaid expansions. *New England Journal of Medicine 376*, 947–956.

Nikpay, S. and J. Ayanian (2015). Hospital charity care — effects of new community-benefit requirements. *New England Journal of Medicine 373*, 1687–1690.

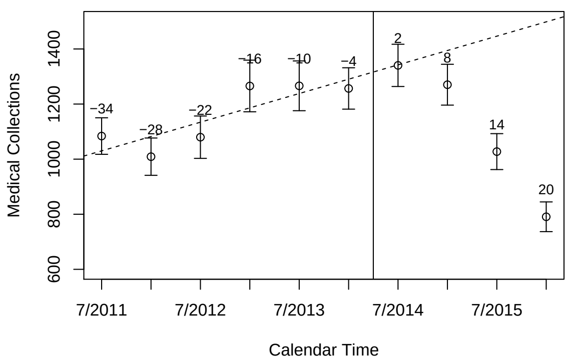
Simon, K., A. Soni, and J. Cawley (2017). The impact of health insurance on preventive care and health behaviors: evidence from the first two years of the aca medicaid expansions. *Journal of Policy Analysis and Management*.

Sommers, B., A. Goldman, R. Blendon, E. J. Orav, and A. Epstein (2019). Medicaid work requirements — results from the first year in arkansas. *New England Journal of Medicine 381*, 1073–1082.

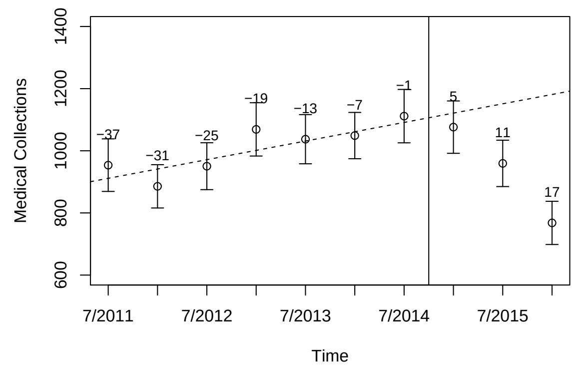
Sommers, B., M. Gunja, K. Finegold, and T. Musco (2015). Changes in self-reported insurance coverage, access to care, and health under the affordable care act. *JAMA 341*(4), 366–374.

Wherry, L. and S. Miller (2016). Early coverage, access, utilization, and health effects of the affordable care act medicaid expansions: A quasi-experimental study. *Annals of Internal Medicine 164*(12), 795–803.

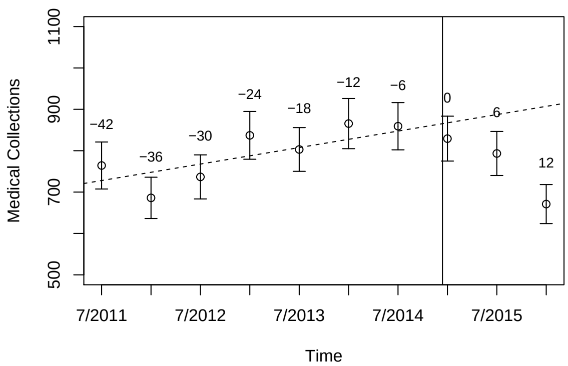
Figure 1: Event Study Construction Example



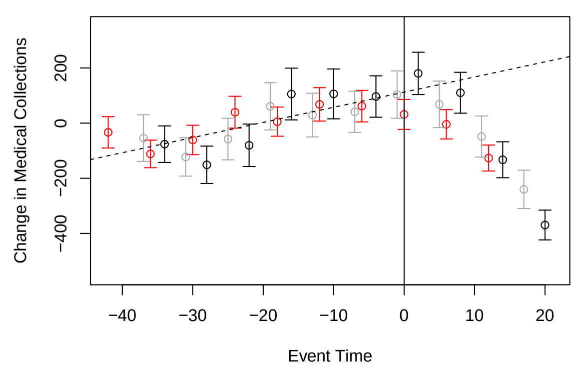
(a) May Cohort



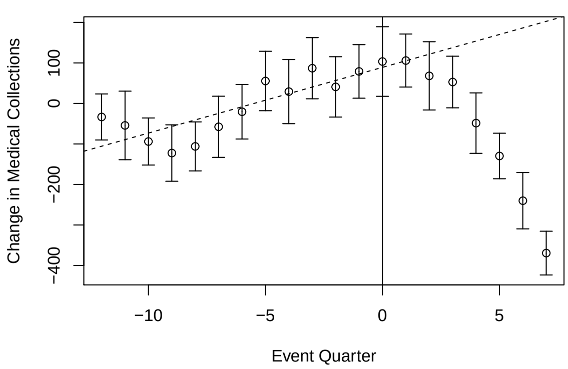
(b) August Cohort



(c) January Cohort



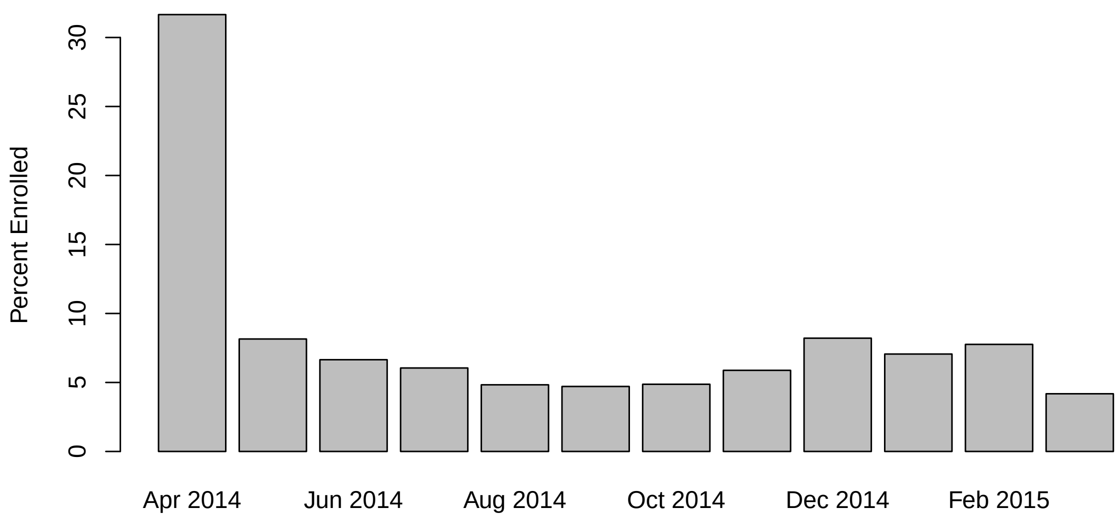
(d) Three Cohorts Combined



(e) Months Grouped in to Quarters

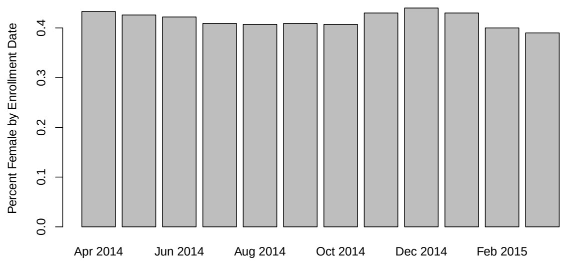
Figure depicts means and 95 percent confidence intervals for medical collections for three enrollment cohorts, with the vertical line indicating the date enrolled. The first three panels show these values relative to calendar time on the x-axis. The third panel shows the May (black), August (grey) and January (red) enrollment cohorts plotted against event time on the x-axis. The fourth panel groups the monthly means reported in panel (d) into quarters.

Figure 2: Histogram of Enrollment Times

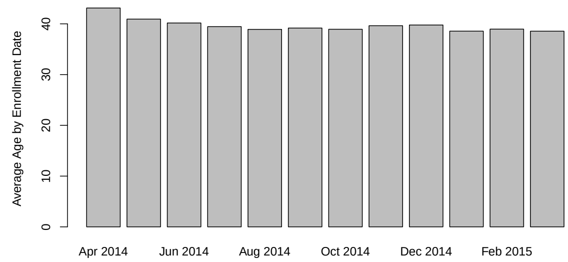


This figure displays the distribution of enrollment times for individuals included in the analysis. Source: authors' calculations from Healthy Michigan administrative data.

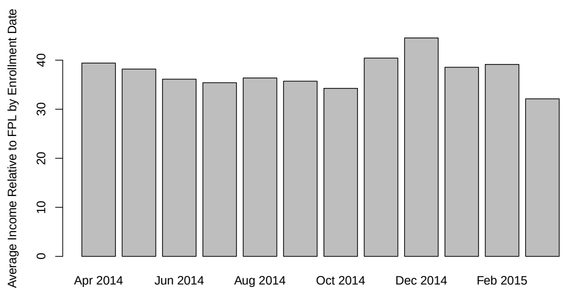
Figure 3: Characteristics by Enrollment Times



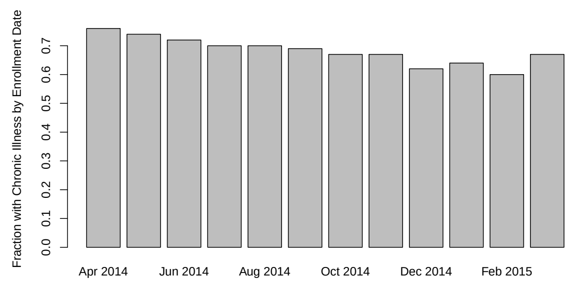
(a) Fraction Female



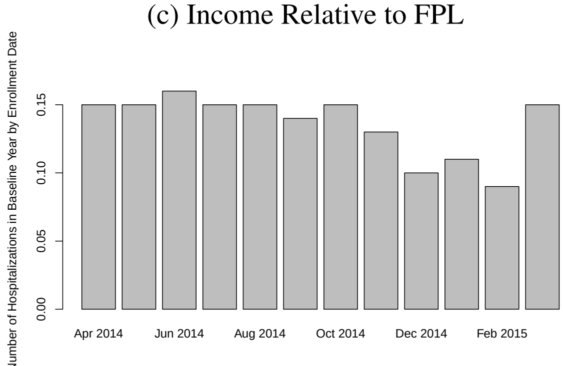
(b) Age



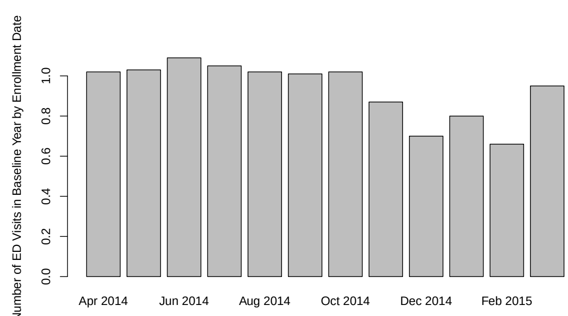
(c) Income Relative to FPL



(d) Chronic Illness



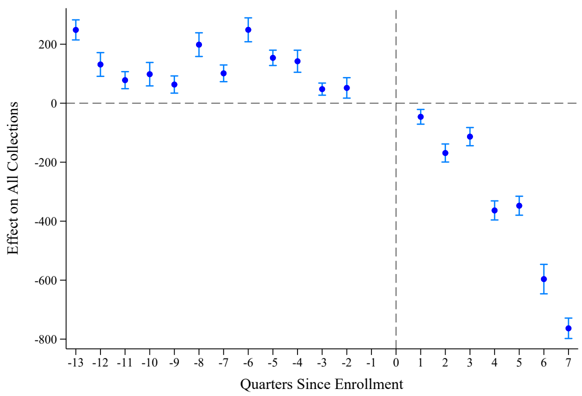
(e) Hospitalizations



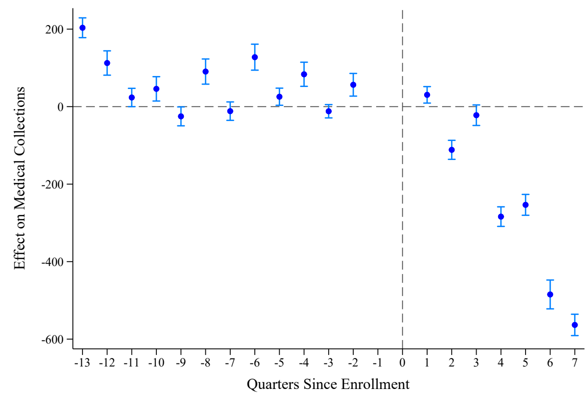
(f) ED Visits

This figure displays the characteristics of enrollees based on enrollment times for individuals included in the analysis. Source: authors' calculations from Healthy Michigan administrative data.

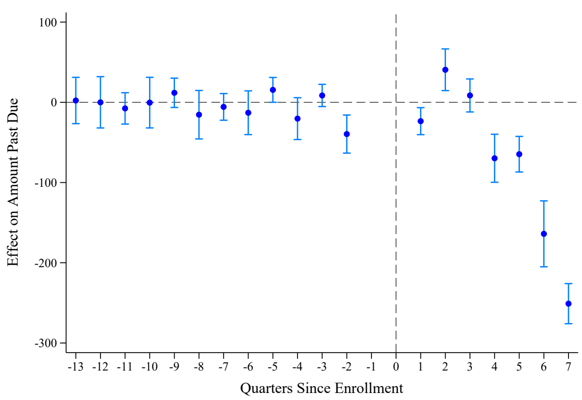
Figure 4: Event Study Coefficients: Delinquency Outcomes



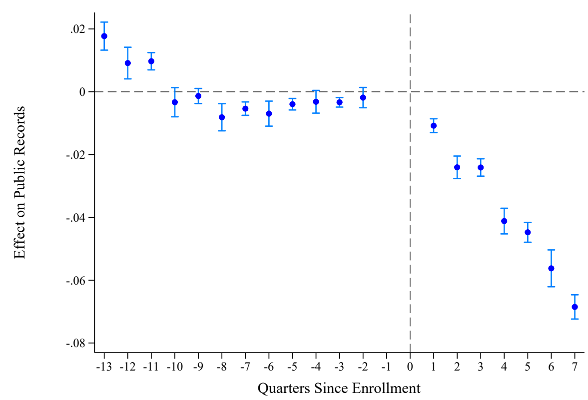
(a) All Collections



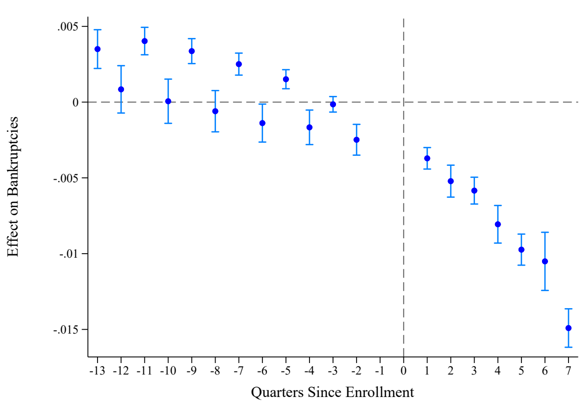
(b) Medical Collections



(c) Credit Market Amount Past Due



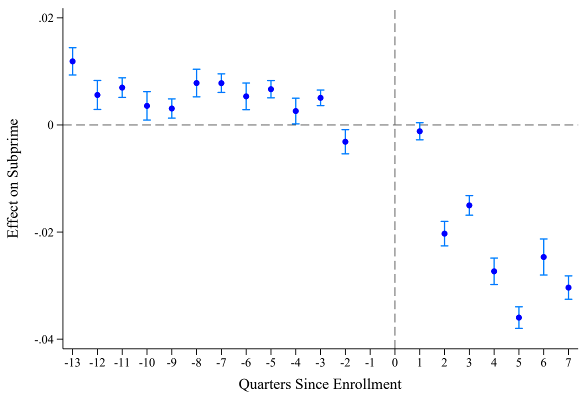
(d) Public Records



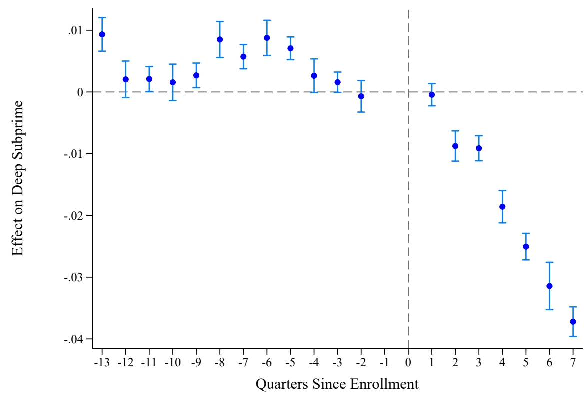
(e) Bankruptcies

Vertical line indicates quarter of enrollment in Medicaid. Event study conducted at the quarterly level. These figures present coefficients and 95 percent confidence intervals estimated from a model that includes event time indicator variables, month x year enrollment cohort fixed effects, calendar month fixed effects (to account for seasonality), and a linear time trend estimated in the pre-HMP period.

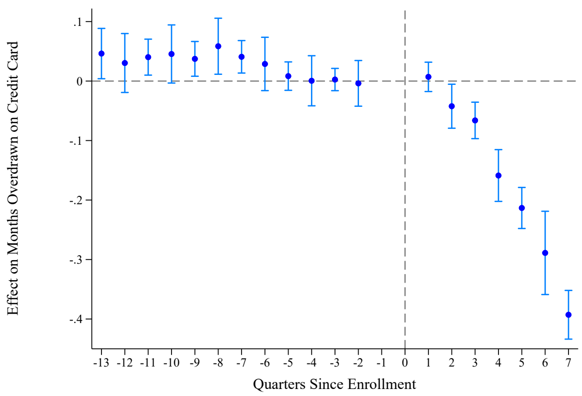
Figure 5: Event Study Coefficients: Access to Credit



(a) Subprime



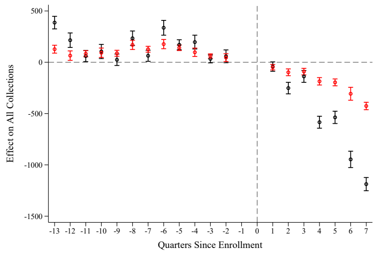
(b) Deep Subprime



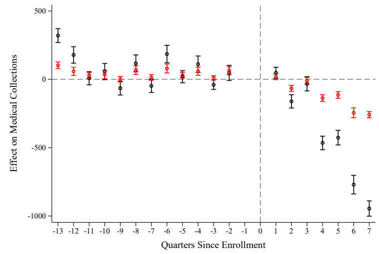
(c) Months Overbalance on Credit Card

Vertical line indicates quarter of enrollment in Medicaid. Event study conducted at the quarterly level. These figures present coefficients and 95 percent confidence intervals estimated from a model that includes event time indicator variables, month x year enrollment cohort fixed effects, calendar month fixed effects (to account for seasonality), and a linear time trend estimated in the pre-HMP period.

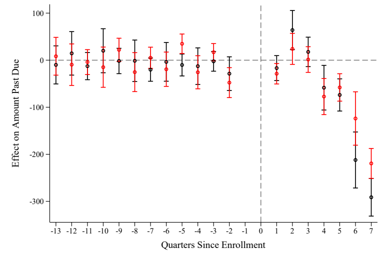
Figure 6: Hospitalized/ED (Black) vs no hospitalization/ED (Red)



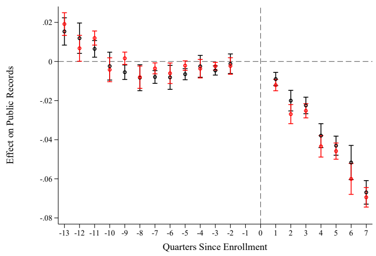
(a) All Collections



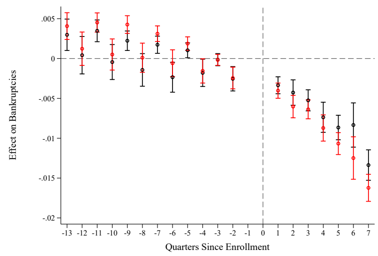
(b) Medical Collections



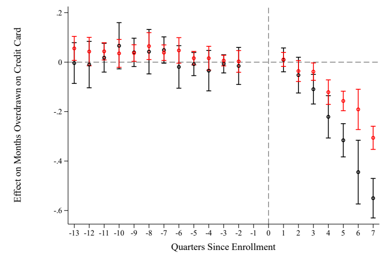
(c) Credit Market Amount Past Due



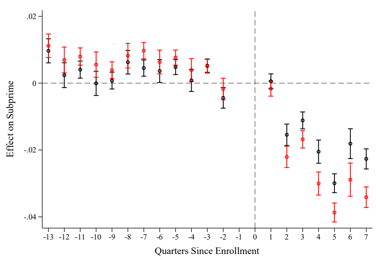
(d) Public Records



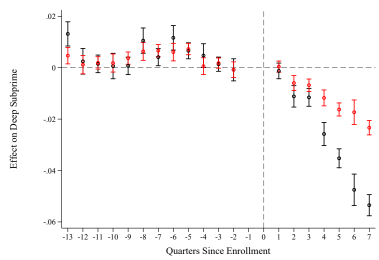
(e) Bankruptcies



(f) Overdrawn on CC



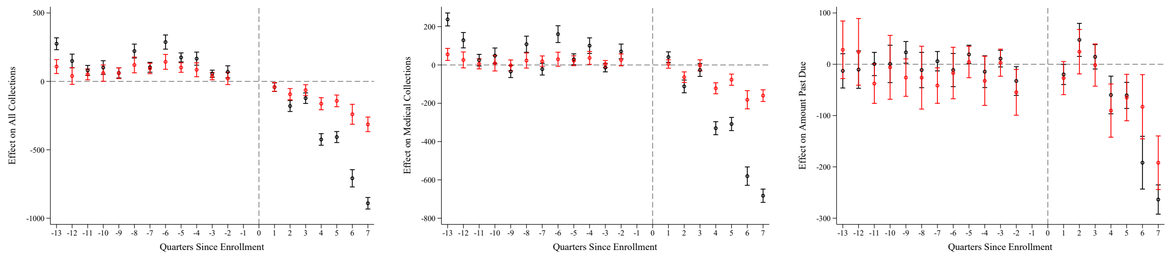
(g) Subprime



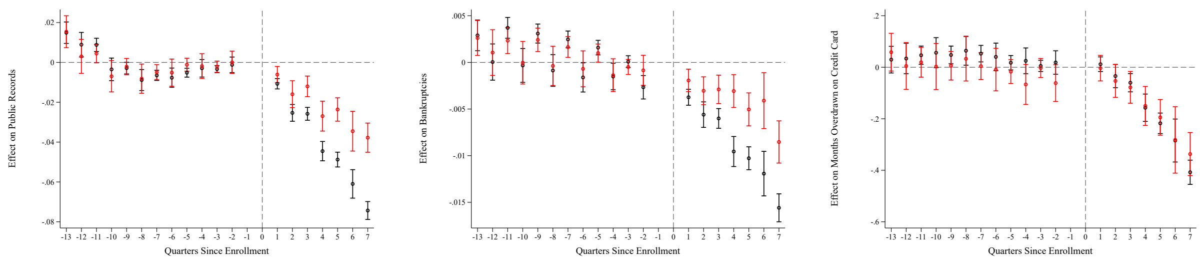
(h) Deep Subprime

Figure depicts event study coefficients and 95 percent confidence intervals for post-enrollment time periods across a sample that had a hospitalization or ED visit in the baseline year (black) relative to the sample without a hospitalization or ED visit in baseline year (red). Model includes event time indicator variables, month x year enrollment cohort fixed effects, calendar month fixed effects (to account for seasonality), and a linear time trend.

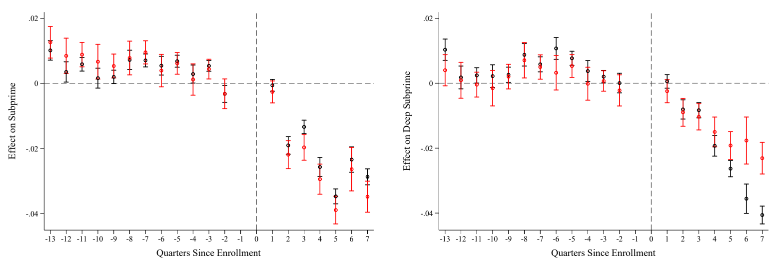
Figure 7: Chronic Illness (Black) vs no Chronic Illness (Red)



(a) All Collections (b) Medical Collections (c) Credit Market Amount Past Due



(d) Public Records (e) Bankruptcies (f) Overdrawn on CC



(g) Subprime (h) Deep Subprime

Figure depicts event study coefficients and 95 percent confidence intervals for post-enrollment time periods across a sample that had a chronic illness recorded in the baseline year (black) relative to the sample without chronic illness in baseline year (red). Model includes event time indicator variables, month x year enrollment cohort fixed effects, calendar month fixed effects (to account for seasonality), and a linear time trend.

Figure 8: Heterogeneity by Disease Category – Quarter 7 Estimate of Change in Collections

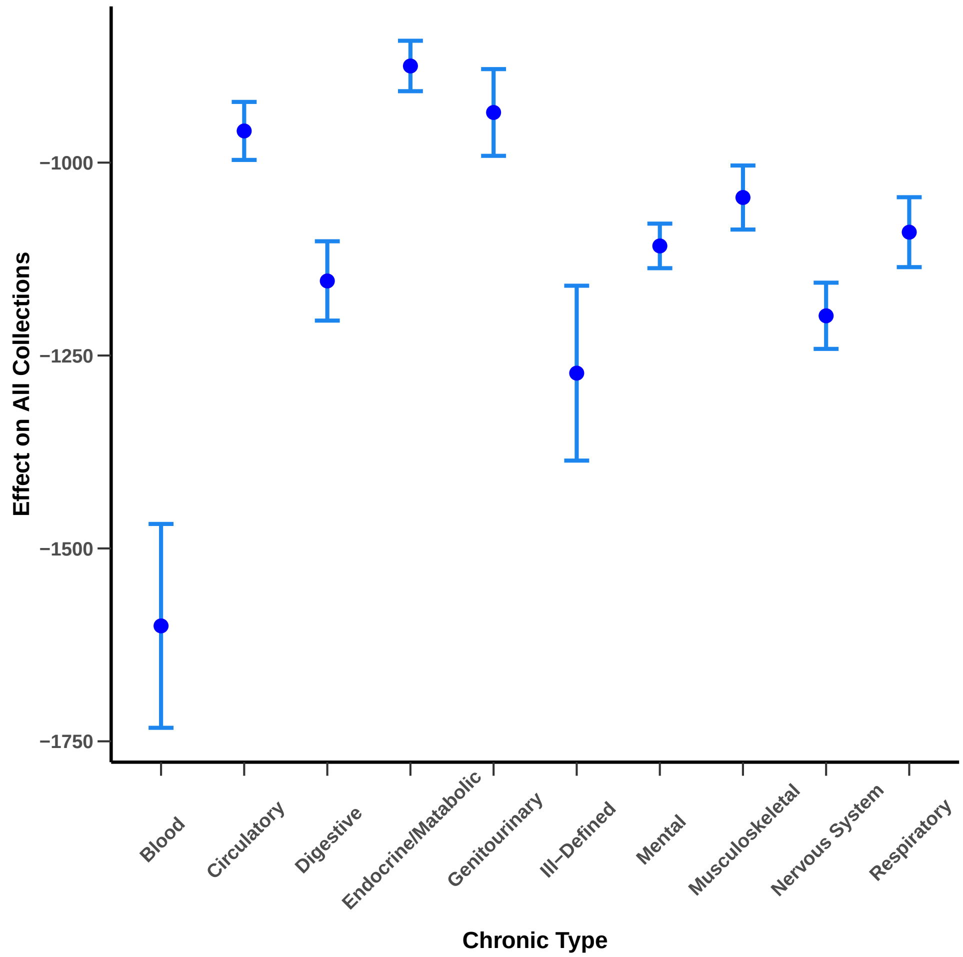
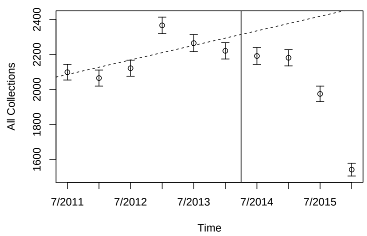
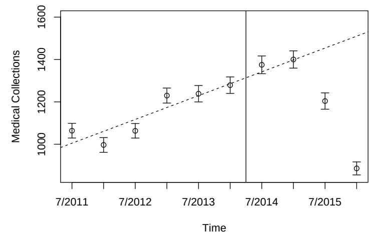


Figure depicts event study coefficients for Quarter 7 with 95 percent confidence intervals across different disease categories (listed on horizontal axis). Model includes event time indicator variables, month x year enrollment cohort fixed effects, calendar month fixed effects (to account for seasonality), and a linear time trend.

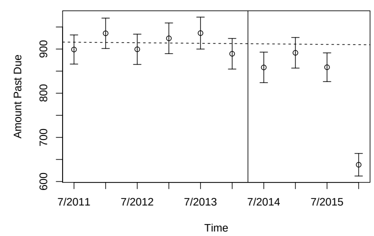
Figure 9: First Enrollment Cohort



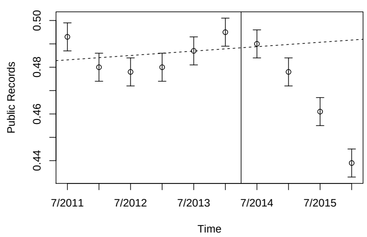
(a) All Collections



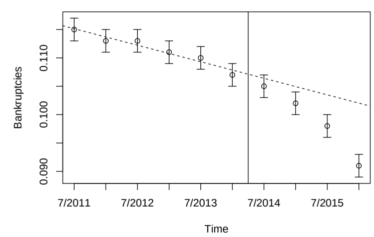
(b) Medical Collections



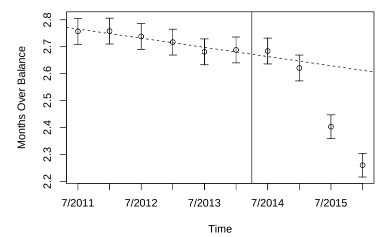
(c) Credit Market Amount Past Due



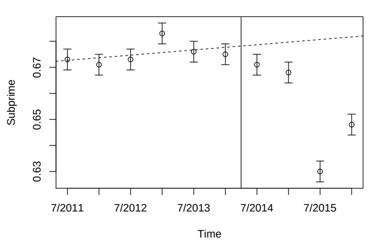
(d) Public Records



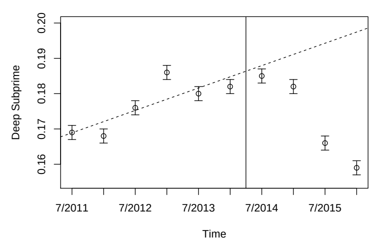
(e) Bankruptcies



(f) Overdrawn on CC



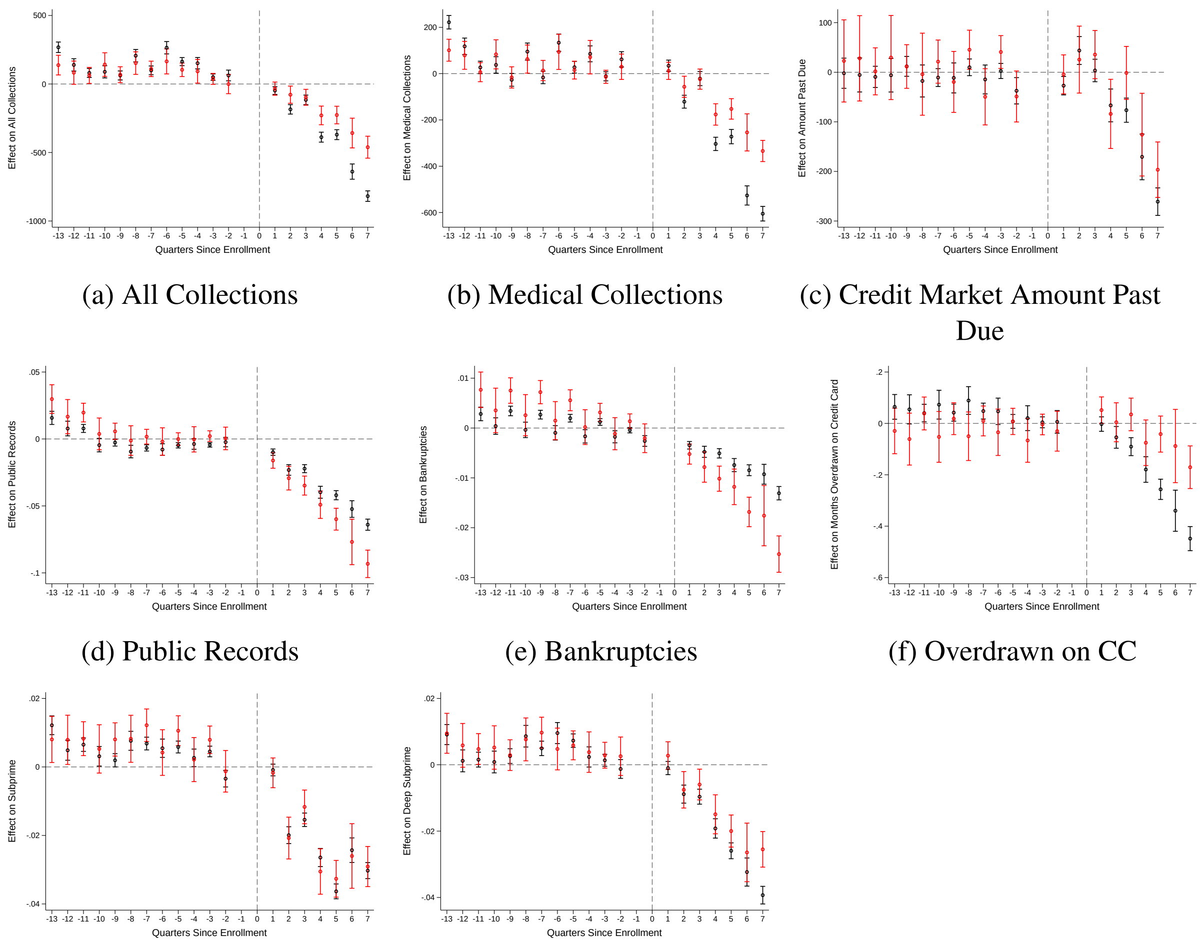
(g) Subprime



(h) Deep Subprime

Figure depicts event study coefficients and 95 percent confidence intervals for post-enrollment time periods for the cohort who enrolled in HMP during the first month, April of 2014. Model includes event time indicator variables, calendar month fixed effects (to account for seasonality), and a linear time trend.

Figure 10: Heterogeneity for under 100 FPL vs 100-138 FPL



(a) All Collections

(b) Medical Collections

(c) Credit Market Amount Past Due

(d) Public Records

(e) Bankruptcies

(f) Overdrawn on CC

(g) Subprime

(h) Deep Subprime

Figure depicts event study coefficients and 95 percent confidence intervals for across a sample earning under 100% of the FPL (black line) and between 100 and 138% (inclusive) of the FPL (red line). Model includes event time indicator variables, month x year enrollment cohort fixed effects, calendar month fixed effects (to account for seasonality), and a linear time trend.

Table 1: Comparison of matched and unmatched samples

	Un-Matched	Matched
Age	36.99 (14.72)	38.78 (12.37)
Gender=Female	42%	57%
Inpatient Discharges	0.09 (0.42)	0.14 (0.66)
ED Visits	0.59 (1.49)	0.96 (2.66)
Chronic Illness Flag=1	62%	70%
Income as Percent of FPL	22.43 (40.07)	38.92 (47.09)
N	3,717	322,305

This table presents descriptive statistics for HMP enrollees that were not matched (column 1) and matched (column 2) to a TransUnion credit report. Standard deviations are in parentheses.

Table 2: Descriptive Statistics: Main Sample and Subsamples, Credit Report Characteristics (Top Panel) and HMP Characteristics (Bottom Panel)

	Main Sample	Hospitalization or ED Visit in First Year	Enrollees with Chronic Illness in First Year
Pre-Enrollment Characteristics			
All Collections	1984.73	2685.67	2220.67
Medical Collections	1002.74	1614.57	1194.45
Amount Past Due	873.79	894.03	902.60
Fraction Subprime	0.69	0.80	0.70
Fraction Deep Subprime	0.18	0.24	0.19
# Public Records	0.44	0.46	0.48
# Bankruptices	0.09	0.09	0.11
Months Over Limit on Credit Card	2.75	3.41	2.80
Characteristics from HMP			
Observed at Enrollment:			
Age at Enrollment	38.26	37.43	40.16
Female	0.59	0.61	0.56
Income Relative to FPL (%)	36.23	29.83	35.81
Observed During First Year:			
Chronic Illness=1	0.73	0.90	1.00
Hospitalization	0.15	0.34	0.20
ED Visits	1.05	2.41	1.35
# Individuals	268,601	117,013	197,016

Note: Table displays summary statistics. Financial outcomes are presented for pre-HMP enrollment periods, defined relative to the individual's enrollment month. Note that the variables "months over limit on credit cards," "subprime" and "deep subprime" are not defined for all individuals. Baseline sample characteristics for HMP enrollees are presented in the bottom panel.

Table 3: Results: Delinquency Outcomes

	All Collections			Medical Collections			Amount Past Due		
Effect at Quarter 2	-79.599***	-191.442***	-168.835***	24.659**	-99.312***	-111.459***	36.850***	54.282***	40.579***
	(15.627)	(15.849)	(15.626)	(12.513)	(12.800)	(12.512)	(13.252)	(12.607)	(13.252)
Effect at Quarter 4	-244.287***	-367.415***	-363.505***	-101.941***	-258.589***	-283.791***	-74.743***	-53.872***	-69.761***
	(16.512)	(18.079)	(16.511)	(12.895)	(14.299)	(12.895)	(15.262)	(16.304)	(15.262)
Effect at Quarter 7	-609.090***	-742.461***	-763.179***	-328.207***	-511.143***	-563.247***	-257.402***	-233.831***	-250.963***
	(17.676)	(20.982)	(17.675)	(14.043)	(16.398)	(14.042)	(12.759)	(16.245)	(12.759)
Remove pre-enrollment trend		X	X		X	X		X	X
Include Pre-Enrollment Event Time Indicators	X	X		X	X		X	X	
N	2,648,014	2,648,014	2,648,014	2,648,008	2,648,008	2,648,008	2,648,021	2,648,021	2,648,021
	Public Records			Bankruptcies					
Effect at Quarter 2	-0.013***	-0.013***	-0.024***	-0.006***	-0.002***	-0.005***			
	(0.002)	(0.002)	(0.002)	(0.001)	(0.000)	(0.001)			
Effect at Quarter 4	-0.026***	-0.028***	-0.041***	-0.009***	-0.004***	-0.008***			
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)			
Effect at Quarter 7	-0.049***	-0.051***	-0.069***	-0.017***	-0.010***	-0.015***			
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)			
Remove pre-enrollment trend		X	X		X	X			
Include Pre-Enrollment Event Time Indicators	X	X		X	X				
N	2,648,021	2,648,021	2,648,021	2,648,021	2,648,021	2,648,021			

Note: Table displays estimates of equations (1) and (2). The first model listed under each outcome corresponds to (1). The second model listed, which removes a pre-enrollment trend, corresponds to equation (1) estimated with dependent variable $\tilde{Y_i}c\tau$, while the third model corresponds to equation (2). Robust standard errors clustered at the individual level are reported in parentheses. Significance levels: *=10 percent, **= 5 percent, ***=1 percent.

Table 4: Results: Access and Use of Credit Outcomes

	Subprime			Deep Subprime		
Effect at Quarter 2	-0.018***	-0.019***	-0.020***	0.001	-0.010***	-0.009***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Effect at Quarter 4	-0.024***	-0.025***	-0.027***	-0.006***	-0.019***	-0.019***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Effect at Quarter 7	-0.026***	-0.026***	-0.030***	-0.020***	-0.037***	-0.037***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Remove pre-enrollment trend		X	X		X	X
Include Pre-Enrollment Event	X	X		X	X	
Time Indicators						
N	2,483,274	2,483,274	2,483,274	2,483,274	2,483,274	2,483,274

	Months Overdrawn on Credit Card		
Effect at Quarter 2	-0.071***	-0.031	-0.042**
	(0.019)	(0.019)	(0.019)
Effect at Quarter 4	-0.196***	-0.140***	-0.159***
	(0.022)	(0.024)	(0.022)
Effect at Quarter 7	-0.441***	-0.366***	-0.393***
	(0.021)	(0.026)	(0.021)
Remove pre-enrollment trend		X	X
Include Pre-Enrollment Event	X	X	
Time Indicators			
N	919,899	919,899	919,899

Note: Table displays estimates of equations (1) and (2). The first model listed under each outcome corresponds to equation (1). The second model listed, which removes a pre-enrollment trend, corresponds to equation (1) estimated with dependent variable $\tilde{Y}_{ic\tau}$, while the third model corresponds to (2). Robust standard errors clustered at the individual level are reported in parentheses. Significance levels: *=10 percent, **= 5 percent, ***=1 percent.

Table 5: Results: First Enrollment Cohort Only

	All Collections	Medical Collections	Amount Past Due	Public Records	Bankruptcies	Months Overdrawn On Credit Card
Effect at Quarter 2	-123.481***	-63.647***	8.256	-0.026***	-0.003***	0.016
	(21.465)	(18.197)	(12.660)	(0.002)	(0.001)	(0.016)
Effect at Quarter 4	-100.183***	29.059	4.716	-0.025***	-0.005***	-0.048***
	(21.404)	(19.289)	(11.971)	(0.001)	(0.000)	(0.018)
Effect at Quarter 7	-400.634***	-326.784***	11.201	-0.063***	-0.010***	-0.246***
	(26.799)	(23.055)	(15.241)	(0.002)	(0.001)	(0.023)
Remove pre-enrollment trend	X	X	X	X	X	X
Include Pre-Enrollment Event Time Indicators	X	X	X	X	X	X
N	899,707	899,706	899,709	899,709	899,709	322,387
	Subprime	Deep Subprime				
Effect at Quarter 2	-0.008***	-0.005***				
	(0.001)	(0.002)				
Effect at Quarter 4	-0.008***	-0.007***				
	(0.001)	(0.001)				
Effect at Quarter 7	-0.050***	-0.031***				
	(0.002)	(0.002)				
Remove pre-enrollment trend	X	X				
Include Pre-Enrollment Event Time Indicators	X	X				
N	850,559	850,559				

Note: Table displays estimates of equation (1). Robust standard errors clustered at the individual level are reported in parentheses. Significance levels: *=10 percent, **= 5 percent, ***=1 percent.