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ABSTRACT

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Abstract

This article examines the impact of the Affordable Care Act Medicaid expansion in Michigan, the Healthy Michigan Program (HMP), on the financial well-being of new Medicaid enrollees. Our analysis uses a dataset on credit reports matched to administrative data on HMP enrollment and use of health care services. We find that enrollment is associated with large improvements in several measures of financial health, including reductions in unpaid bills, medical bills, over limit credit card spending, delinquencies, and public records (such as evictions, judgments, and bankruptcies). These benefits are apparent across several subgroups, although individuals with greater medical need (such as those with chronic illnesses) experience the largest improvements.

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, although to date, many states have still not adopted the expansion (Dorn et al. (2014)).

A number of studies have shown that these expansions significantly increased enrollment in Medicaid and decreased the number of people without insurance[1] and affected the ability to access care, health care utilization, and the health of those gaining coverage.[2] However, fewer papers have explored the impact of this policy 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.[3] Previous studies that have explored this topic (Hu et al. (2018), Brevoort et al. (2017) and Caswell and Waidmann (2017)) have not been able to observe the financial outcomes of individuals who actually enroll

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

[2]For example, Wherry and Miller (2016), Miller and Wherry (2017), Sommers et al. (2015), Ghosh et al. (2017), Simon et al. (2017).

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

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 Medicaid 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, and may be particularly under-represented in data about financial outcomes due to reporting issues (see, e.g., Brevoort et al. (2015)). This data limitation may be especially problematic when considering rare but policy-relevant outcomes such as evictions, bankruptcies, or wage garnishments, where aggregate analysis may lack adequate power. Second, estimates of the change in insurance coverage used in these back-of-the-envelope calculations are mostly based on survey data, which under-reports Medicaid coverage (Boudreaux et al., 2015). This measurement problem adds further error to these calculations. Therefore, there is still some uncertainty about the magnitude of the effect of gaining insurance coverage through the Medicaid expansions on financial well-being. Finally, the lack of linked data has prevented researchers from examining how gaining insurance affects those with poor health who are most likely to benefit from obtaining Medicaid. 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)). Results from this study showed that gaining Medicaid significantly improved financial health. However, the sample sizes in the OHIE were relatively small, about 10,000 individuals gaining coverage, which limited the statistical power of the study, particularly with respect to 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 or near-poor, for whom benefits of health insurance are likely to differ.

This article reports novel evidence of the impact of Medicaid on financial outcomes. We analyze a new dataset that links administrative records from Michigan on Medicaid enrollment, demographic characteristics, and use of health services to 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 as compared to counterfactual trend. The linked data allows us to: measure the effect of Medicaid on financial well-being for those actually affected; identify the effect of Medicaid 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 gaining Medicaid substantially improves financial well-being. In particular, we estimate that enrolling in Medicaid reduces the amount of medical bills in collections by \$511 (about 52% relative to the pre-ACA mean) and reduces the amount of debt past due that has not yet been sent to a third party collection agency of about \$234 (about 27%). We find significant reductions in the number of public records (such as evictions, bankruptcies, or wage garnishments), which falls by 0.05, or about 12%, and the number of bankruptcies, which falls by 0.01, or about 11%.

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%. 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 fewer months (an approximately 13% reduction) following enrollment, suggesting they may be less credit constrained.

We also have sufficiently large samples to determine whether the effects differed by enrollee characteristics. We find larger effects on 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). However, even among groups without apparent high healthcare need, we see statistically significant benefits for many of the outcomes we examine. These results suggest that the financial benefits of Medicaid coverage are apparent across almost all subgroups of beneficiaries.

A limitation of our main empirical approach that focuses on the timing of when individuals enrolled in Medicaid is that we may not be able to fully account for other time period effects. To address this, we also conduct two separate analyses that use the "difference-in-differences" method by comparing the effects on Medicaid enrollees to two different "control" groups. First, we use Medicaid beneficiaries who have an additional source of insurance and for whom Medicaid is a secondary payer and compare them to those who enroll in Medicaid without any additional insurance. Second, we compare Medicaid enrollees to other Michigan residents who reside in low income zip codes but who did not enroll in Medicaid. Results from both analyses largely confirm our main findings.

2 Background

The ACA resulted in one of the largest expansions of health insurance coverage since the 1960s, with some estimates indicating that over 20 million individuals gained insurance coverage since 2010 (Cohen et al. (2017)). 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. 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.

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 a 2% contribution that is not to exceed of 5% of their 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 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, but that these measures fell after HMP enrollment (Goold and Kullgren (2018)). This 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.[4] 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.

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 observed twice per year, in January and July, starting with July 2011 and

[4]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.

ending with January 2016, resulting in 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 who are living in low-income, high uninsurance rate zip codes. 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.

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.

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.[5] This debt could represent unpaid bills (such as a utility bill), or severely derogatory 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

[5]To be clear, we include those with zeroes in their collection balances. In addition, since the amount recently sent to collections can change over time, our estimates will provide effects on the "flow" into collections rather than the "stock" of collections.

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 effect an individual's credit worthiness. A subset of public records are bankruptcies, which we examine separately.

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.[6] 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. 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.[7] We also include as an outcome variable 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 their card limit (and incurring fees as a result) and may benefit from a higher limit.

[6] 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 in the current draft.

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

Table 2 presents descriptive statistics from our matched sample. The top panel shows descriptive statistics related to the credit report outcomes we consider in this paper, and shows the averages for enrollees in our main sample, as well as for subgroups, for the period before and after the individual enrolls in HMP. The bottom panel shows baseline 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).

The first column show statistics for our main sample (pre and post enrollment) while the next sets of columns show statistics for those for two relevant 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 39 percent of the FPL, which would be about \$7500 for a family of 3 or \$4400 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 number of ED visits in a year is about 1. Financially, HMP enrollees have high rates of delinquencies relative to their income; in our main sample, enrollees owe about \$1985 to third party collectors (with \$981 related to medical bills) and an additional \$874 on average past due on open credit accounts. 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 \$2591 prior to HMP enrollment, with \$1536 related to medical bills. Similarly, among those with a chronic illness, collections were \$2099, with \$1112 related to medical bills. These descriptive statistics show that HMP enrollees tend to be poor, in poor health, and in dire financial straits.

In Table 3 we show similar descriptive statistics for our two comparison groups: enrollees with another source of insurance and Michigan residents living in low income, high uninsurance rate zip codes who did not enroll in HMP. Those with Medicaid as a secondary paper are less poor, in better health and have better baseline financial health than those for whom Medicaid is the primary payer. Because it is reasonable to expect those with an additional source of insurance to be less "treated" by enrollment into Medicaid, we use them as a control group in a supplementary analysis. Similarly, we see that residents of low income zip codes who did not enroll in HMP also tend to have better financial outcomes than HMP enrollees.

Notably, we see that financial well-being improves after enrolling in HMP. For example, in the main sample, the total amount in collection declined by $397 (20 percent) pre-to-post the HMP expansion. Similar pre-to-post changes are observed among other subgroups of HMP enrollees in the main sample. For the the sample for whom Medicaid is a secondary panel, and for the non-HMP sample, there are smaller or no improvements in financial indicators.

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 twice 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 3, 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 3. For this cohort, we observe 42 months prior to enrollment but only 12 months after enrollment.

The fourth panel of Figure 3 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 generally 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 21 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 3 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.[8]

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 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.[9] 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).[10]

The second version estimates a variation on model (1) that imposes the linear pre-enrollment

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

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

[10]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.

trend rather than estimate pre-enrollment fixed effects in the following way (similar to Gross et al. (2018)):

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

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 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 1, 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. Although any fixed difference in characteristics across cohorts is accounted for by our cohort-specific fixed effect (β_c), it is reassuring that characteristics are fairly similar across early and late enrollers, as reported in Figure 2. Finally, 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. However, we find little evidence of pre-existing trends, with practically no evidence of any pre-trends for medical collections, as we describe in the subsequent section. This suggests that enrollment for this particular group made eligible by the ACA is unlikely to be caused by an unmeasured factor correlated with the outcome variable.

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. In all cases, we see reductions in measures of delinquency, and these effects appear to grow over time.

The estimates for selected event coefficients related to delinquency and creditworthiness are reported in Table 4. We report the effects observed 2 quarters after enrollment, 4 quarters after enrollment, and in the last post-enrollment quarter that we observe, 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 that HMP enrollment substantially reduces the total amount of bills sent to third party collection agencies. By the end of the period we observe, 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. 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 57 percent. 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 by the end of the period, HMP enrollment reduces the amount past due by between $233 and $257, or 27 to 29 percent.

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).[11] As noted earlier, our results are entirely unrelated to the settlement with the New York Attorney Generals 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 10 percent).

Because all individuals in our sample enrolled in HMP, these estimates can be directly interpreted as the treatment effect of Medicaid coverage. Our estimated effects on medical collections are smaller than those documented in Hu et al. (2018) and Brevoort et al. (2017), who es-

[11]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.

timate that Medicaid enrollment reduces medical collections by $1140 and $1236 respectively. However, the treatment effect estimates reported by these papers are sensitive to assumptions about Medicaid enrollment, which are not directly measured in the credit report samples that they use. Also, our estimates pertain only to enrollees in Michigan. Our estimates are similar to those reported in Finkelstein et al. (2012), who find that Medicaid enrollment reduces medical debt in collections by $390 (relative to our estimate of $515).

Table 5 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 the last 12 months of about 0.40 months, about 16 percent relative to the baseline mean of 2.5 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

We conducted our analyses by subgroup based on observable characteristics of HMP recipients. We present these results graphically in Figures 6 and 7.

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 (e.g. $500 to $1000 in quarters 3 to 7) and medical collections for the group with utilization during the first year. 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 effects of HMP enrollment on collections is much stronger for the group with the chronic illness than those without, similar to the hospitalization analysis. The reduction in public records, bankruptcies, and fraction deep subprime are also larger. Other outcomes exhibit similar patterns across the two groups.

Finally, we 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 6 and Figure 8. By the end of the sample period, the results for most outcomes look similar in this sample relative to what we uncover when we use the entire sample. We also find similar, though somewhat larger, effects in earlier months for collections, medical collections, public records, bankruptcies, subprime and deep subprime outcomes. However, we do 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.

5.2 Alternative Empirical Approaches

Our main analysis compares changes in outcomes among enrollees before and after they enroll relative to the trend in the outcomes we 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 be wrong if there were other changes besides HMP that would have caused them 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. In this section, we incorporate various "control" groups that were not affected, or less affected, by HMP enrollment. These control groups give us another counterfactual to use when estimating the impact of HMP on credit outcomes.

In this section, we explore two alternative "difference-in-differences" analyses that incorporate these "control" samples. For the first comparison group, we use 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. However, to the extent that they are still treated, we may be differencing away some of the true effect and so these results might be viewed as an underestimate. 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.[12] This "masking" sample ex-

[12]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.

cludes 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.

Both the TPL and the non-enrollee Michigan comparison groups exhibit somewhat different pre-trends than the HMP enrollees prior to enrollment. To address this, we first estimated propensity score weights at the individual level using changes in the outcome variables during the pre-enrollment period as well as age bins to estimate the probability an individual was in the "treated" group (i.e., did not have additional insurance in the case of the TPL sample control group, or enrolled in HMP in the case of the masked sample control group). We used the estimated probability of being in the treated group, $\hat{p}$, to re-weight the control group and applied these weights throughout the analysis.[13]

We incorporate these different comparison groups in slightly different ways, given that different information is available for each group. For the TPL sample, we re-estimate equation (1) but interact all variables with an indicator that the individual has Medicaid as their primary insurance (i.e. is not in the TPL group). The event quarter coefficients therefore capture the difference in the effect across the "treated" (Medicaid only) group and the TPL comparison group. Any concurrent, unrelated changes in financial conditions affecting both the TPL and Medicaid group are absorbed by the TPL group, and the remaining difference can be attributed to exclusively Medicaid coverage. To reiterate, because Medicaid may have a beneficial effect on financial outcomes even for the TPL comparison group, this approach is likely to yield a conservative estimate of the effect of Medicaid relative to what we would observe if the TPL group were completely untreated.

The results of this analysis are reported in Figure 9. Event quarter coefficients prior to enrollment are, for the most part, close to zero, and exhibit divergence after enrollment, with

[13] Specifically, we weighted individual i the control group with $\frac{\hat{p}_i}{1-\hat{p}_i}$.

the Medicaid only group experiencing improvements in financial outcomes relative to the TPL comparison group. The point estimates are smaller than what we observe in our main analysis, which we would expect if the TPL comparison group experiences improved financial outcomes by gaining Medicaid as an additional payer, which is likely the case. Overall, this analysis suggests that Medicaid enrollment itself, rather than a secular improvement in financial outcomes occurring around the same time as (but unrelated to) Medicaid enrollment, is driving the improvements in financial outcomes that we observe.

We are unable to estimate a similar model for the Michigan "masking" comparison sample since there is no equivalent to enrollment time for this comparison group. Instead, we limit our analysis to only those who enrolled in the first month that HMP became available, April 2014, and compare trends in calendar time across the enrollees and the non-enrollee comparison group in a standard difference-in-differences setup.

The results for this analysis are presented in Figure 10.Here too, we observe common pre-trends between the HMP sample and the re-weighted masked comparison group. However, while we find similar results for public records and months overdrawn on credit card, there is a somewhat different pattern for our collections and credit score variables when compared to our main analysis. Using the re-weighted non-enrollee Michigan sample as a comparison group, we observe initially higher collections in the enrollee group–and in particular, higher medical collections–in the first year after enrollment, but these eventually fall and the effect of enrolling is significantly negative by the beginning of 2016. The improvements in credit scores that we document in the main analysis are not replicated in this approach, and while we continue to observe negative point estimates for bankruptcy, they are not statistically significant.

6 Discussion

Our study provides the first evidence on the impact of the ACA Medicaid expansion using data that actually links participants to financial outcomes at the individual level. We see that enrollment in Michigan's Medicaid expansion plan, HMP, is associated with improvements across a broad swath of financial measures. We found large reductions in the amount of debt sent to third party collectors ($763), particularly medical debt ($563), in the amount of debt that is past due ($257), in the number of public records and bankruptcies, and in the propensity of enrollees to go over their credit card limits. Our effects are large when compared to the sample mean and appear to grow larger over time.

Our large sample also allowed us to examine subgroups within the HMP population. We find that groups with higher apparent medical need experienced larger effects, particularly for measures related to third party collections. However, we also detected meaningful improvements on financial outcomes across all subgroups examined. Medicaid enrollment appears to have salutary effects on financial outcomes even among Medicaid recipients who are apparently healthier and have greater financial resources.

This study has several limitations. First, we are limited to only one state: Michigan. The impact of the ACA Medicaid expansion may be different in other states depending on demographic composition, existing social programs, or other factors. Second, our study period only allowed us to observe financial outcomes through 7 quarters following enrollment. Given that the improvement in financial outcomes appears to become larger over time, we may underestimate the true impact due to our limited sample period. However, despite these limitations, our study provides important new information on the role of Medicaid in providing financial protections to low-income individuals.

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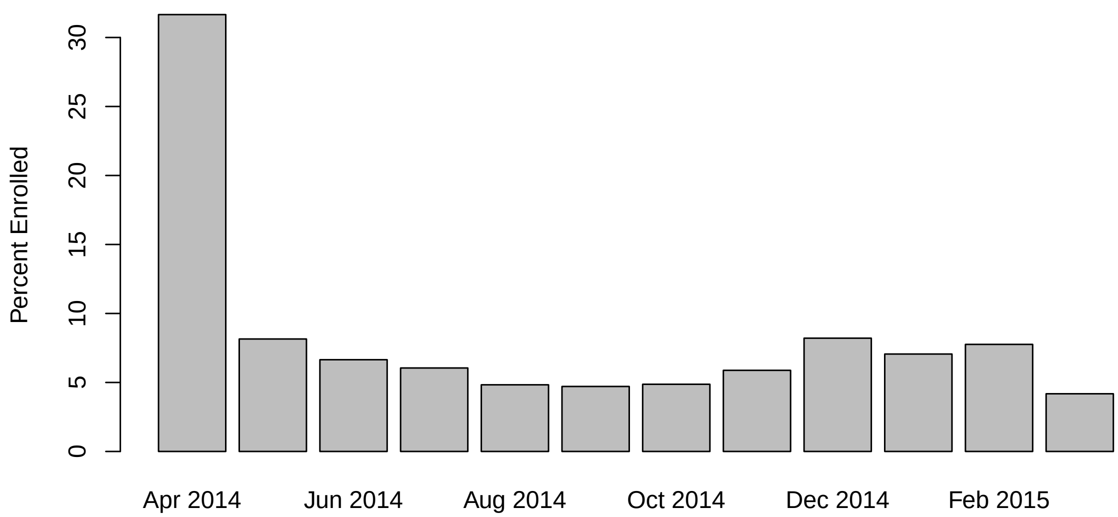
A Appendix. Further Details on the TransUnion Match and Matching Procedure

To conduct this analysis, a dataset was created to match individual-level data for Healthy Michigan Plan enrollees to their credit reports at TransUnion. The match was conducted using social security number, name, and address. Over 98 percent of all Healthy Michigan Plan enrollees were matched to a credit report. Those who were not matched were excluded from the study.

In order to preserve the confidentiality of HMP enrollees identities, the matching process utilized a double blind matching procedure as follows: an administrative associate of the Michigan Department of Health and Human Services extracted identifying information on HMP enrollees (name, address, and social security number). They appended to this dataset a randomly selected sample of approximately 550,000 Michigan residents drawn from an unrelated state health database, the Michigan Care Improvement Registry (MCIR), to serve as "masking" observations. Because social security number is not included in this database, these masking observations were assigned randomly-selected but valid social security numbers with group codes (first three digits) selected to match the distribution of group codes among the HMP population. These were inserted into the dataset in random order. To this, they appended an anonymized study ID code. This file (Dataset 1) was then provided to TransUnion. As a result of the masking observations included in the input file, TransUnion was unable to distinguish which observations were associated with HMP enrollees and which observations were generated for the purposes of masking. Simultaneously, the administrative associate of the Michigan Department of Health and Human Services extracted and de-identified information on HMP enrollees (enrollment date, FPL on record at the time of initial HMP enrollment, number of emergency department visits and hospitalizations in the first 12 months of enrollment, and the presence of a diagnosis code for a chronic illness on any encounter in the first 12 months of enrollment, , the presence of any third-party liability during the first 12 months of HMP enrollment, and any pre-HMP Medicaid enrollment). The anonymized study ID was appended to this file (Dataset 2). Dataset 2 was provided to the researchers. Finally, TransUnion extracted credit report information and merged this information with Dataset 1. Then, TransUnion removed all personally identifying information, resulting in a de-identified file, Dataset 3, and provided this file to the researchers. Using the anonymized study ID code, the researchers merged Dataset 3 and Dataset 2, resulting in the final analytic dataset.

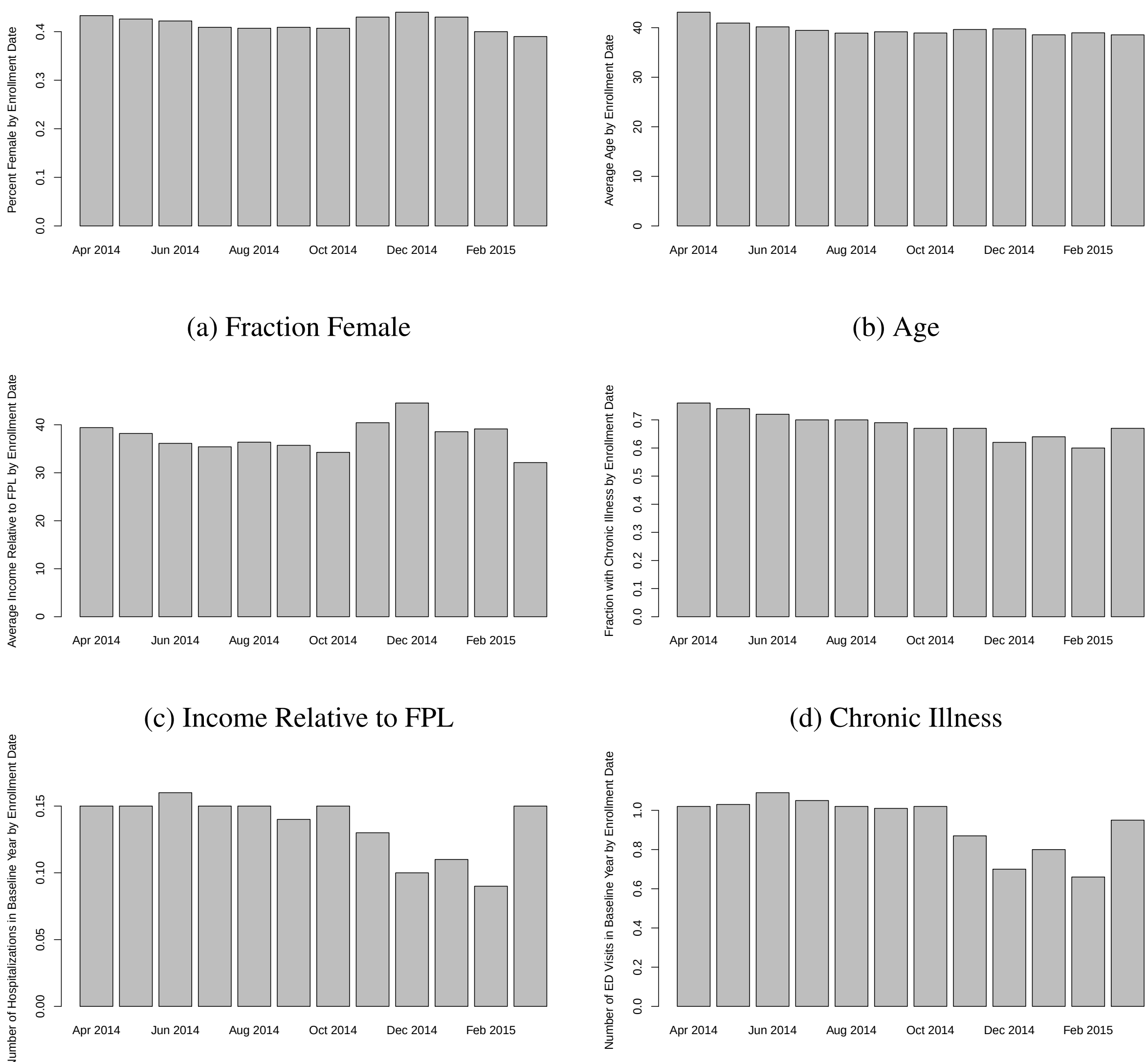
Table A3 compares the characteristics of those who were and were not matched to a credit report. All differences in characteristics across the matched and unmatched samples are statistically significant at the 1% level. In our sample, 322,305 were matched to a credit report and 3,717 were not, resulting in a match rate of 98.8%. Those unmatched tended to be younger, with an average age of 36.99, relative to the average age in the matched sample, 38.78 . This discrepancy in age is consistent with analysis of those without credit reports conducted by the CFPB (Brevoort et al. (2015)), who find that age is strongly predictive of having a credit report. Unmatched individuals were also from much lower income households, with average income relative to the FPL at 22.4% for unmatched individuals and 38.92% for matched individuals. The unmatched sample has fewer inpatient discharges (0.09 vs. 0.14 in the matched sample) and ED visits (0.59 vs 0.96 in the matched sample) during the baseline year and is less likely to be diagnosed with a chronic illness (62% vs. 70% in the matched sample). Those in the unmatched sample are less likely to be female (42% female in unmatched sample vs 52% female in matched sample).

Figure 1: 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 2: Characteristics by Enrollment Times



(a) Fraction Female

(b) Age

(c) Income Relative to FPL

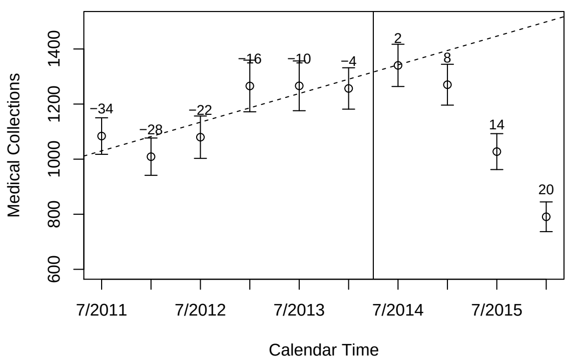
(d) Chronic Illness

(e) Hospitalizations

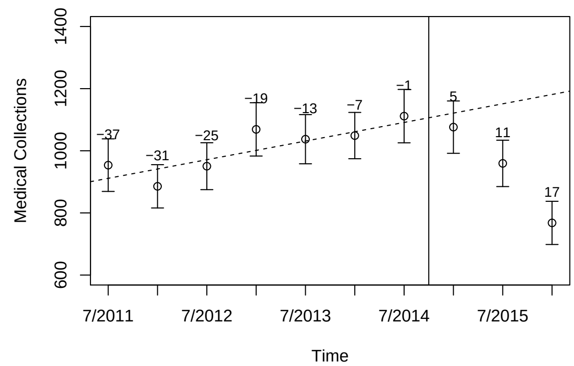
(f) ED Visits

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.

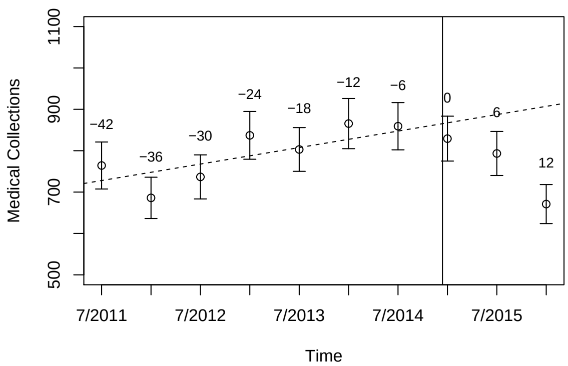
Figure 3: Event Study Construction Example



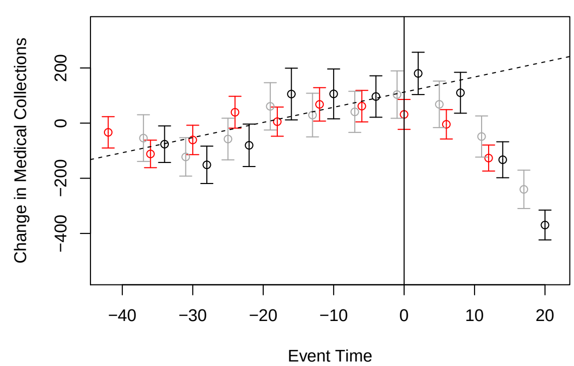
(a) May Cohort



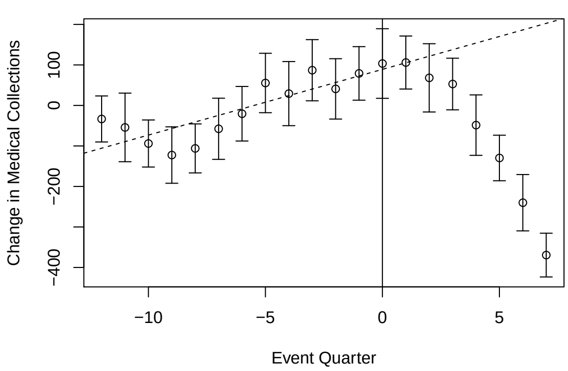
(b) August Cohort



(c) January Cohort



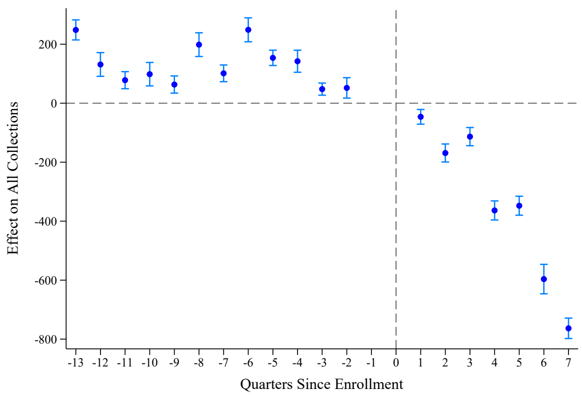
(d) Three Cohorts Combined



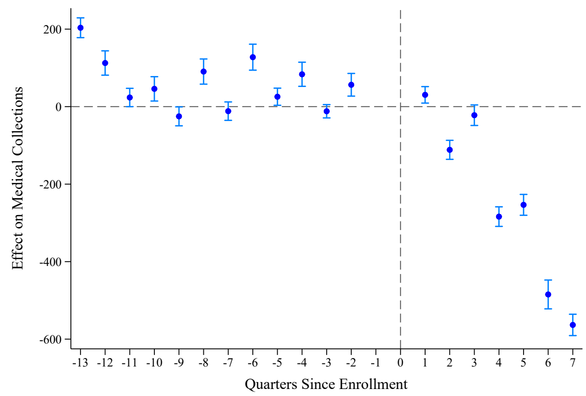
(e) Months Grouped in to Quarters

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.

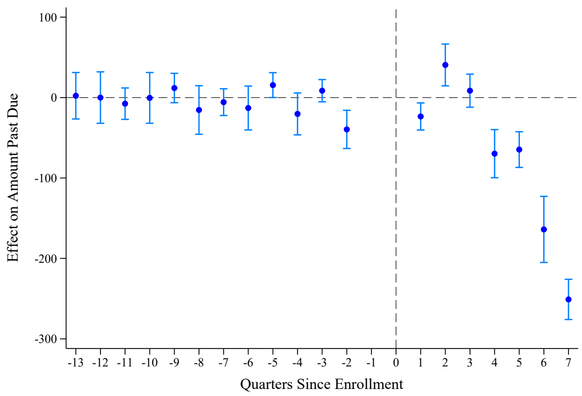
Figure 4: Event Study Coefficients: Delinquency Outcomes



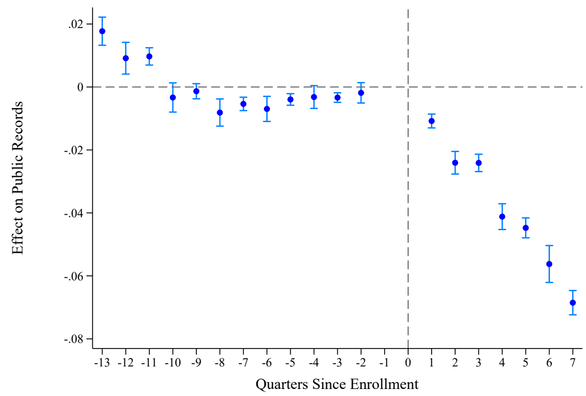
(a) All Collections



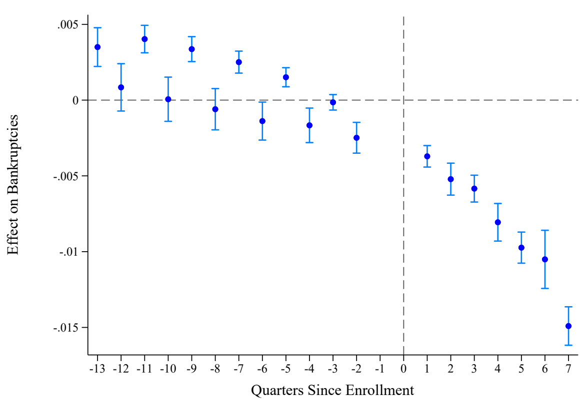
(b) Medical Collections



(c) Credit Market Amount Past Due



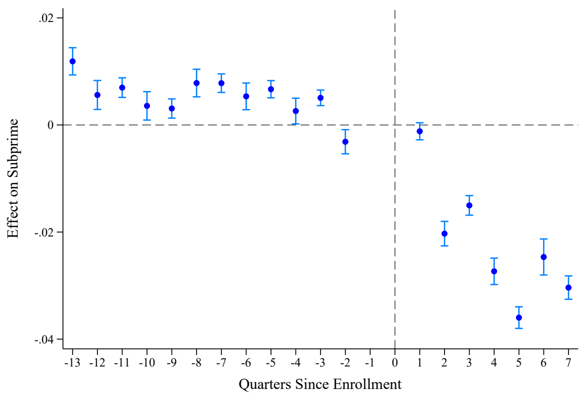
(d) Public Records



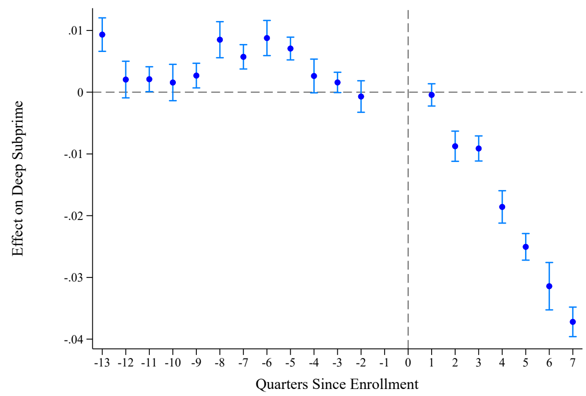
(e) Bankruptcies

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.

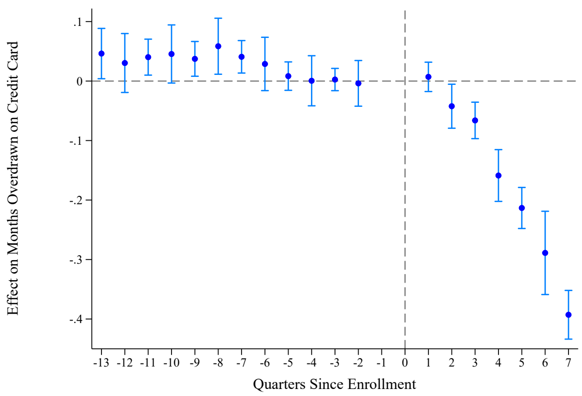
Figure 5: Event Study Coefficients: Access to Credit



(a) Subprime



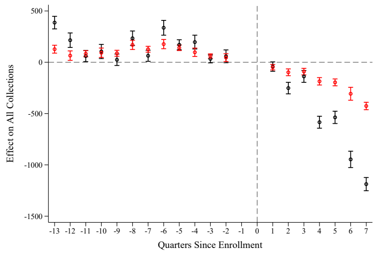
(b) Deep Subprime



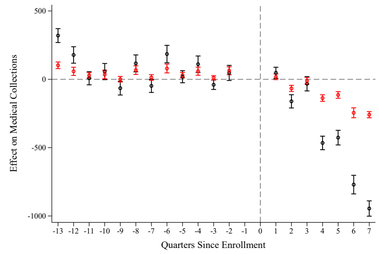
(c) Months Overbalance on Credit Card

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.

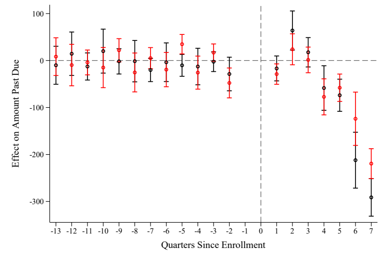
Figure 6: Hospitalized/ED (Black) vs no hospitalization/ED (Red)



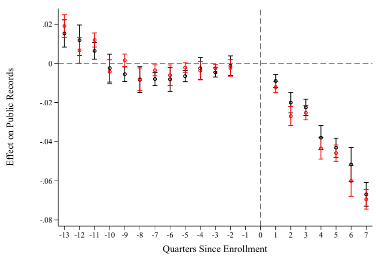
(a) All Collections



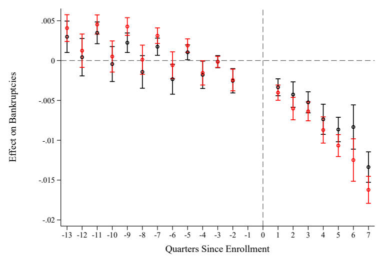
(b) Medical Collections



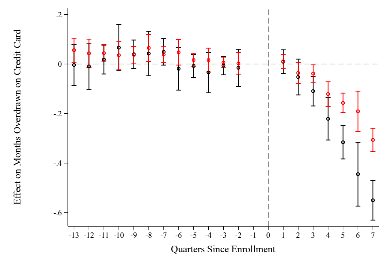
(c) Credit Market Amount Past Due



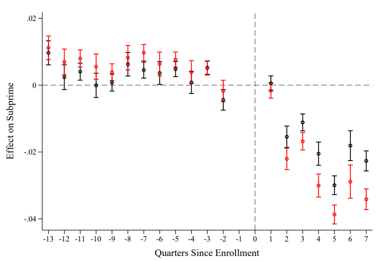
(d) Public Records



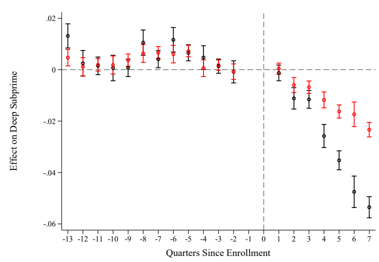
(e) Bankruptcies



(f) Overdrawn on CC



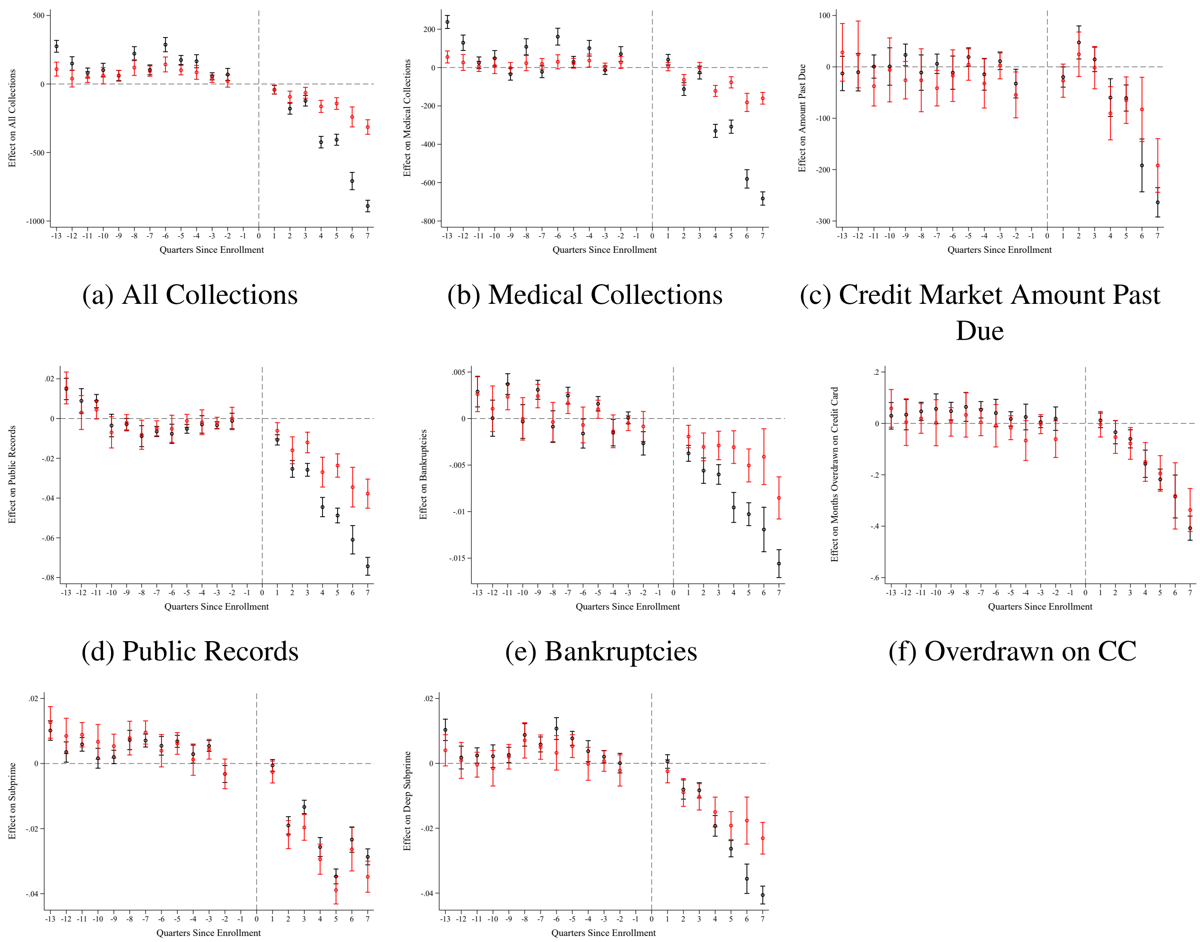
(g) Subprime



(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

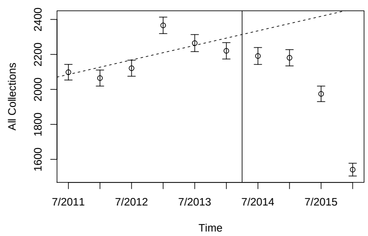
(f) Overdrawn on CC

(g) Subprime

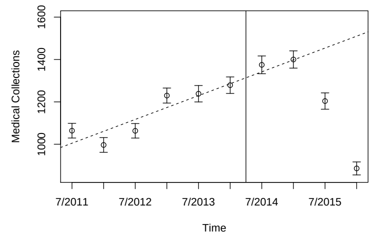
(h) Deep Subprime

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.

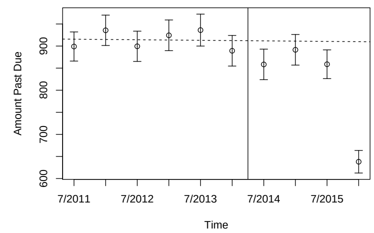
Figure 8: First Enrollment Cohort



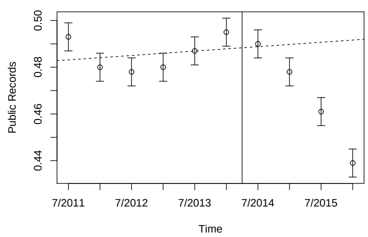
(a) All Collections



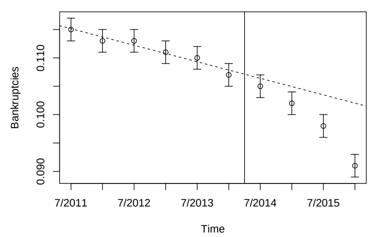
(b) Medical Collections



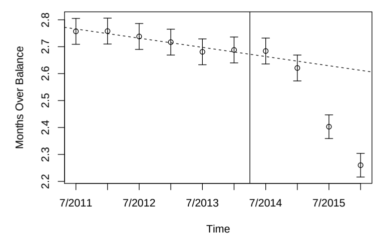
(c) Credit Market Amount Past Due



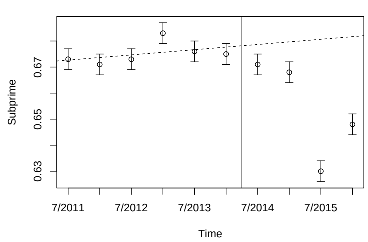
(d) Public Records



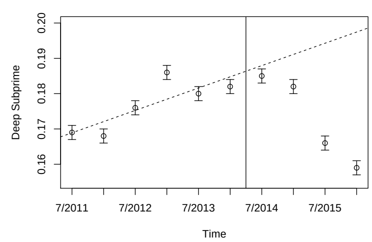
(e) Bankruptcies



(f) Overdrawn on CC



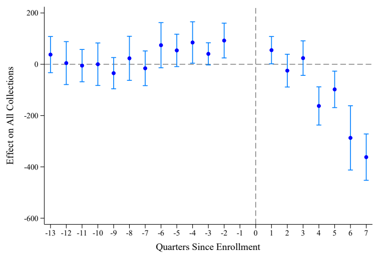
(g) Subprime



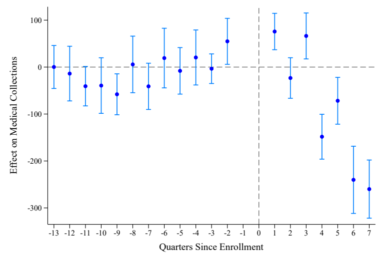
(h) Deep Subprime

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.

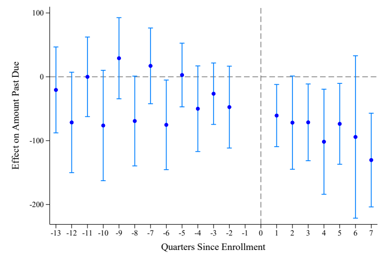
Figure 9: Medicaid Only versus Medicaid as Secondary Payor Difference-in-Differences



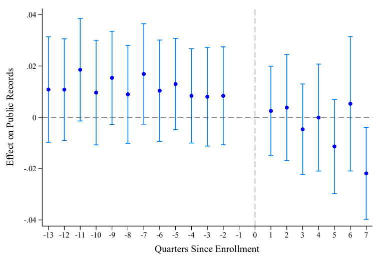
(a) All Collections



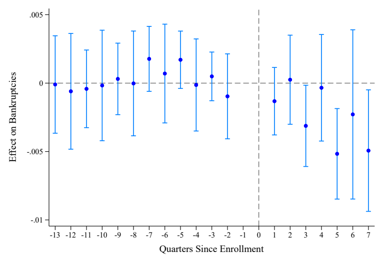
(b) Medical Collections



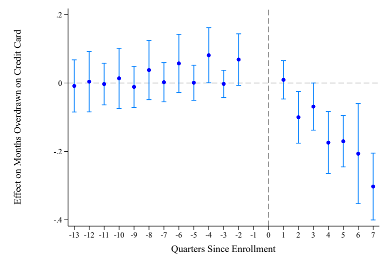
(c) Credit Market Amount Past Due



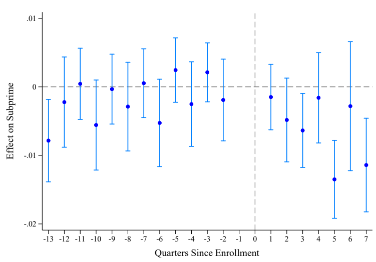
(d) Public Records



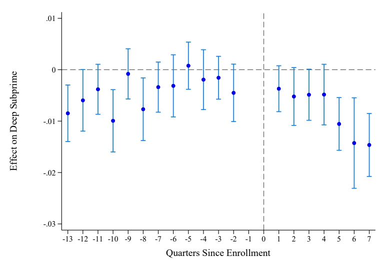
(e) Bankruptcies



(f) Overdrawn on CC



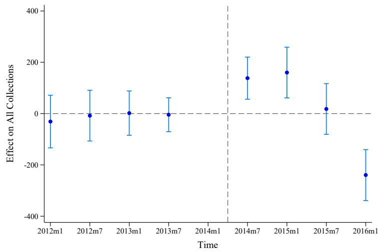
(g) Subprime



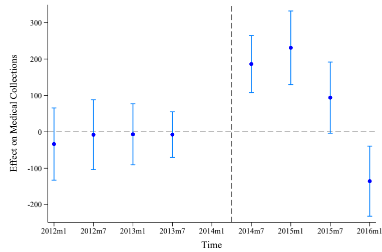
(h) Deep Subprime

Figure depicts event study coefficients and 95 percent confidence intervals for a difference-in-differences event study model that compares HMP enrollees with only Medicaid coverage ("treated") to HMP enrollees with coverage additional to Medicaid ("untreated"). The untreated group is weighted using propensity scores derived from pre-HMP values of the outcome variable (see text). 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, all interacted with treatment status.

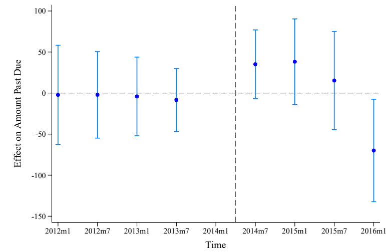
Figure 10: Medicaid Enrollees versus Low Income Zip Code Michigan Sample Differences-in-Differences



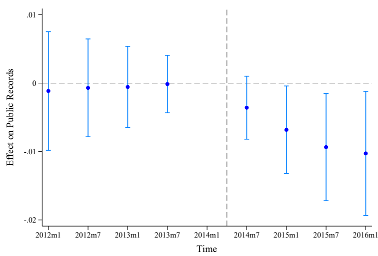
(a) All Collections



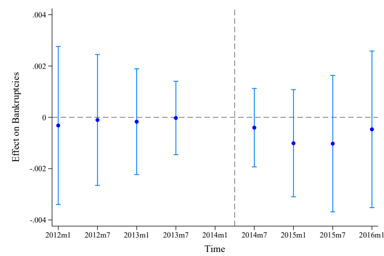
(b) Medical Collections



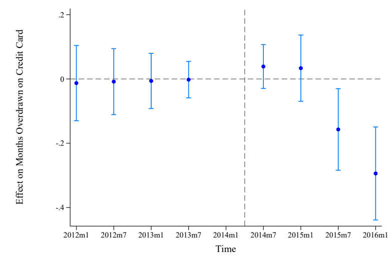
(c) Credit Market Amount Past Due



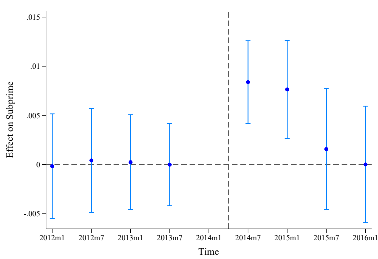
(d) Public Records



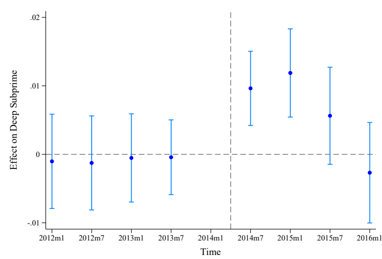
(e) Bankruptcies



(f) Overdrawn on CC



(g) Subprime



(h) Deep Subprime

Figure depicts event study coefficients and 95 percent confidence intervals for a difference-in-differences event study model that compares HMP enrollees who enrolled in April of 2014 and reside in low income zip codes ("treated") to a random sample of non-enrollee low income zip code residents drawn from the MCIR database ("untreated"). The untreated group is weighted using propensity scores derived from pre-HMP values of the outcome variable (see text). Model includes event time indicator variables and calendar month fixed effects (to account for seasonality), all interacted with treatment status.

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

	Main Sample		Enrollees with Hospitalization or ED Visit in First Year		Enrollees with Chronic Illness in First Year	
	Pre	Post	Pre	Post	Pre	Post
All Collections	1984.73	1587.66	2591.26	2284.78	2099.21	1755.46
Medical Collections	981.98	923.26	1536.24	1511.37	1112.56	1074.46
Amount Past Due	873.79	718.21	892.06	795.44	885.22	750.92
Fraction Subprime	0.69	0.67	0.78	0.78	0.66	0.65
Fraction Deep Subprime	0.18	0.18	0.23	0.24	0.18	0.18
# Public Records	0.44	0.38	0.47	0.42	0.48	0.43
# Bankruptcies	0.09	0.08	0.09	0.08	0.11	0.09
Months Over Limit on Credit Card	2.75	2.56	3.26	3.12	2.62	2.48
Baseline characteristics:						
Age at Enrollment	38.78		38.02		40.82	
Female	0.59		0.59		0.54	
Income Relative to FPL (%)	36.26		31.39		37.67	
Chronic Illness=1	0.73		0.90		1	
Hospitalization	0.15		0.34		0.19	
ED Visits	1.05		2.35		1.28	
# Individuals	267,704		130,814		227,008	

Note: Table displays summary statistics. Financial outcomes are presented for both pre- and post-HMP enrollment. These time periods are 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: Descriptive Statistics: "Comparison" Samples

	Medicaid Secondary Payer		Non-Enrollees in Low Income Zip Codes	
	Pre	Post	Pre	Post
All Collections	1141.75	968.9	1346.97	1501.086
Medical Collections	447.64	465.83	581.96	577.11
Amount Past Due	707.85	669.29	745.61	761.19
Fraction Subprime	0.44	0.43	0.62	0.63
Fraction Deep Subprime	0.10	0.11	0.17	0.17
# Public Records	0.41	0.40	0.42	0.42
# Bankruptcies	0.13	0.11	0.09	0.09
Months Over Limit on Credit Card	1.86	1.88	2.34	2.52
Baseline characteristics:				
Age at Enrollment	38.77		38.59	
Female	0.48		N/A	
Income Relative to FPL (%)	51.61		N/A	
Chronic Illness=1	0.59		N/A	
Hospitalization	0.07		N/A	
ED Visits	0.48		N/A	
# Individuals	54,601		116,028	

Note: Table displays summary statistics. Financial outcomes are presented for both pre- and post-HMP enrollment for thsoe who enroll with Medicaid as a secondary payer. For non-enrollee, "pre" and "post" are defined relative to April of 2014. These time periods are 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 4: Results: Delinquency Outcomes

	All Collections			Medical Collections			Amount Past Due		
Effect at Quarter 2	-79.60***	-191.44***	-168.84***	24.66**	-99.31***	-111.46***	36.850***	54.282***	40.579***
	(15.63)	(15.85)	(15.63)	(12.51)	(12.80)	(12.51)	(13.252)	(12.607)	(13.252)
Effect at Quarter 4	-244.29***	-367.42***	-363.51***	-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	2,648,021	2,648,021	2,648,021	2,648,021	2,648,021	2,648,021	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 (1) estimated with dependent variable $\tilde{Y_i c \tau}$, 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: 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 (1). The second model listed, which removes a pre-enrollment trend, corresponds to (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 6: Results: First Enrollment Cohort Only

	All Collections	Medical Collections	Amount Past Due	Public Records	Bankruptcies	Months Overdrawn On Credit Card
Effect at Quarter 2	-118.62***	-64.83***	8.71	-0.026***	-0.003***	-0.012
	(19.68)	(16.44)	(11.68)	(0.001)	(0.001)	(0.014)
Effect at Quarter 4	-99.18***	25.08	18.35	-0.025***	-0.004***	-0.056***
	(19.31)	(17.30)	(11.32)	(0.001)	(0.0005)	(0.016)
Effect at Quarter 7	-372.96***	-304.81***	14.81	-0.063***	-0.009***	-0.27***
	(24.58)	(20.82)	(14.38)	(0.002)	(0.001)	(0.021)
Remove pre-enrollment trend	X	X	X	X	X	X
Include Pre-Enrollment Event Time Indicators	X	X	X	X	X	X
N	1,020,431	1,020,431	1,020,431	1,020,431	1,020,431	391,101
	Subprime	Deep Subprime				
Effect at Quarter 2	-0.01***	-0.005***				
	(0.001)	(0.001)				
Effect at Quarter 4	-0.01***	-0.007***				
	(0.001)	(0.001)				
Effect at Quarter 7	-0.05***	-0.031***				
	(0.002)	(0.002)				
Remove pre-enrollment trend	X	X				
Include Pre-Enrollment Event Time Indicators	X	X				
N	967,248	967,248				

Note: Table displays estimates of equations (1) and (2). Robust standard errors clustered at the individual level are reported in parentheses. Significance levels: *=10 percent, **= 5 percent, ***=1 percent.