THE EFFECT OF THE PATIENT PROTECTION AND AFFORDABLE CARE ACT
MEDICAID EXPANSIONS ON FINANCIAL WELLBEING

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ABSTRACT

We examine the effect of the Medicaid expansions under the 2010 Patient Protection and Affordable
Care Act (ACA) on consumer financial outcomes using data from a major credit reporting agency
for a large, national sample of adults. We employ the synthetic control method to compare individuals
living in states that expanded Medicaid to those that did not. We find that the Medicaid expansions
significantly reduced the number of unpaid bills and the amount of debt sent to third-party collection
agencies among those residing in zip codes with the highest share of low-income, uninsured individuals.
Our estimates imply a reduction in collection balances of approximately $1,140 among those who
gain Medicaid coverage due to the ACA. Our findings suggest that the ACA Medicaid expansions
had important financial impacts beyond health care use.

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1. Introduction

In 2010, President Barack Obama signed the Patient Protection and Affordable Care Act (ACA) into law, which included a provision to expand Medicaid eligibility to low-income adults, many of whom were previously ineligible. A major motivation for this expansion was to provide financial security to individuals if they experience a sudden deterioration in their health and cannot afford to pay for their medical expenses.

Indeed, the financial consequences of not having health insurance can be severe for individuals who become seriously ill or injured. According to data from the 2012 Medical Expenditure Panel Survey (MEPS), the annual cost of inpatient care for a person aged 18 to 64 who was hospitalized was approximately $15,000, and the annual cost of all types of care for that person was $25,000. Studies using survey data suggest that the uninsured often have difficulty paying medical expenses, become delinquent on their medical and non-medical bills, and are more likely to be contacted by collection agencies.[1] Dobkin et al. (2017) find that uninsured individuals who become hospitalized experience a host of financial setbacks over the next four years including reduced access to credit, a 170% increase in unpaid medical bills, and a more than doubling in the likelihood of filing for bankruptcy.

These statistics highlight how the Medicaid expansions under the ACA could play an important role in providing low-income individuals with financial protection by improving their ability to pay their medical expenses. Additionally, expanded health care coverage may also have indirect effects on financial wellbeing. Access to health insurance and a reduction in medical expenses has the potential to improve access to credit markets, increase savings, and facilitate consumption of other goods and

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[1] Cunningham (2008) reported that 34% of those without medical insurance had trouble paying their medical bills, and among this group, 62% had been contacted by a collection agency. Doty et al. (2008) found that 62% of persons that had trouble paying medical bills reported having more than $2,000 of outstanding medical bills, while 20% reported having more than $8,000 in outstanding medical bills. Finkelstein et al. (2012) reported that approximately 60% of participants in the control group of the Oregon Health Insurance Experiment currently owe money for a medical expense, and 36% indicated that they borrowed money or skipped other bills to pay for medical.

services. These other channels can potentially have salutary effects on the wellbeing of low-income individuals.[2]

Despite the potentially important role that publicly provided health insurance plays in the financial wellbeing of low-income individuals, only two studies have evaluated the role of Medicaid on consumer financial wellbeing. Gross and Notowidigdo (2011) examined the effect of Medicaid eligibility expansions in the 1990s, which were mostly for children, on bankruptcy. They found that increasing Medicaid eligibility by 10 percentage points reduced personal bankruptcy by about 8%. The Oregon Health Insurance Experiment (Baicker et al., 2013; Finkelstein et al., 2012) found that Medicaid coverage of low-income adults in Oregon reduced the likelihood of borrowing money or skipping bills to pay for medical care by 44% and reduced the probability of having a medical collection by 23%. Other studies have evaluated the effects of other types of health insurance coverage on financial outcomes and have also documented substantial improvements in financial wellbeing (Barcellos and Jacobson, 2015; Mazumder and Miller 2016; Dobkin et al., 2017).

We extend this literature by evaluating the effect of the expansion of Medicaid under the ACA to low-income adults on consumer, financial wellbeing. Although originally intended to apply to all states, in 2012 the U.S. Supreme Court decision in the *National Federation of Independent Business v. Sebelius* case made the Medicaid expansions optional for states. As of the end of 2015, 29 states and the District of Columbia had chosen to expand Medicaid coverage (at least in some form) and 21 states had opted not to expand Medicaid coverage.[3] Rates of health insurance coverage have improved substantially more in the states that offer expanded Medicaid coverage than in those that do not (Black and Cohen 2015; Kaestner et al., 2017; Sommers 2014; Wherry and Miller 2016; Miller and Wherry 2017), and total Medicaid enrollment in these states increased by 12.3 million between 2013 and 2015 (Centers for Medicare &

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[2] Doty et al. (2008) found that among the uninsured who were paying off medical bills, 47% stated that they had exhausted their savings and 40% reported that they had foregone other necessities such as food, heat, or rent in order to pay medical bills. Leininger et al. (2010) reported that SCHIP expansions were associated with increased consumption and savings. In contrast, Gruber and Yelowitz (1996) found that savings and asset accumulation were reduced as Medicaid eligibility expanded in late 1980s and early 1990s.

[3] -As we discuss below, for our analysis, however, the classification of treatment and control states differs from this simple distinction, and we consider various groupings of states based on their implementation dates.

Medicaid Services, 2015). We exploit the variation in Medicaid eligibility and coverage induced by these state-level policy choices to estimate the effect of the Medicaid expansions on individual financial outcomes. We utilize the synthetic control approach (Abadie et al., 2010) to address concerns about the potential non-randomness of states' decisions to expand Medicaid.

As far as we are aware, ours is the first national study that evaluates how public health insurance coverage for non-elderly adults affects financial wellbeing. We use data from a large, nationally-representative sample of credit reports, the Federal Reserve Bank of New York Consumer Credit Panel/Equifax (CCP) dataset to conduct our analysis. The CCP data contain timely information on a random sample of the credit reports of approximately 38 million adults in the United States each quarter (covering about 17% of the adult population) and provide many indicators of financial wellbeing. We focus on a few, broad measures of financial wellbeing where the effects of the 2014 Medicaid expansion could potentially be detected during our sample period. Specifically, we examine credit score, total debt, total debt past due, credit card debt, credit card debt past due, the number of non-medical bills sent to collections, and the total non-medical balance outstanding in collections.

Our main finding is that Medicaid expansions that began in 2014 significantly reduced the number of unpaid, non-medical bills and the amount of non-medical debt sent to third-party collection agencies among people living in zip codes that are most likely affected by the expansions. Our baseline intention-to-treat (ITT) estimates indicate that the Medicaid expansions are associated with a decrease in the amount of unpaid balances in collections of between $65 and $88. This effect is an average over the entire sample and includes many individuals who did not obtain Medicaid insurance coverage through the expansion. Rescaling this estimate based on the fraction of the target population who were likely to have obtained insurance coverage yields estimates of the effect of obtaining Medicaid (i.e., treatment on the treated) on non-medical collection balances of approximately $1,140. These estimates indicate a substantial improvement in financial well-being for individuals who gained coverage and show that gaining health insurance provides financial benefits beyond the direct benefit of eliminating medical debt.

These results also imply that the benefits of expanding Medicaid likely include hospitals and creditors that serve the low-income population.

2. Framework for the Analysis

Conceptual Framework

Medicaid provides health insurance coverage at no, or very low, cost to the enrollee. Given the low income of individuals who became eligible for Medicaid through the ACA (<138% of federal poverty), even relatively minor, unexpected medical expenses can represent a substantial fraction of their total income, and more serious illness may be catastrophic financially for them. Consequently, we hypothesize that the financial protection provided by Medicaid for low-income individuals should largely eliminate most of their significant medical expenses, as well as reduce delinquencies and other indicators of financial distress that are the focus of our study.

While the Medicaid expansion should decrease the amount of unpaid medical bills and delinquencies, the effects of gaining Medicaid eligibility on debt and borrowing are theoretically ambiguous. The financial protection afforded by Medicaid coverage should reduce the need for low-income individuals to borrow to smooth consumption when medical issues arise. Thus, Medicaid has the potential to decrease a person's borrowing and total debt. Alternatively, Medicaid may reduce the need for individuals to save for precautionary reasons, which may increase consumption and borrowing. In this case, the Medicaid expansions would be associated with increases in total debt for low-income individuals. In sum, the effect of the Medicaid expansions on measures of debt are ambiguous and an empirical question.

Although our analysis focuses on measuring the effect of the Medicaid expansions on individuals, the potential benefits of the Medicaid expansions extend to hospitals (healthcare providers) and consumer financial services companies. Research has shown that improving the capacity of low-income, uninsured individuals to pay for medical care through Medicaid expansions can improve hospital profitability (Garthwaite, Gross, and Notowidigdo, forthcoming; Nikpay et al., 2017). Presumably, improving

repayment rates would also lower the costs of providing credit, which would benefit consumer-finance firms and the customers they serve. Finally, we note that even if the financial benefits accrue entirely to hospitals or other creditors (i.e., medical bills would never have been otherwise paid), Medicaid recipients likely gain a psychological benefit as a result of not having to interact with debt collectors or worry about medical bills.

Research Design

To study the effect of Medicaid on consumer, financial wellbeing, we use variation in Medicaid eligibility and coverage stemming from the expansion of Medicaid under the ACA, which targeted non-elderly adults with incomes below 138% of the Federal Poverty Level (FPL). The fact that not all states expanded Medicaid, as originally intended by the ACA, provides plausibly exogenous variation in health insurance coverage among low-income adults that can be used to identify estimates of the effect of Medicaid eligibility on consumer financial wellbeing.

There is a large literature that examined the effect of prior Medicaid expansions on a variety of outcomes and most of these studies used a difference-in-differences (DiD) research design. The implementation of the DiD method is straightforward and consists of a comparison of changes in outcomes before and after the expansion of Medicaid for individuals in states that did and did not expand Medicaid. Individuals living in states that expanded Medicaid are the treatment group and those in states that did not expand Medicaid are the comparison group. The key assumption underlying the validity of the DiD approach is that, in the absence of the ACA Medicaid expansions, changes in the financial indicators of wellbeing would be the same for persons in states that did and did not expand Medicaid. This assumption is often referred to as the "parallel trends" assumption.

The parallel trends assumption is often difficult to maintain in practice and preliminary analyses of our data indicated some violations of the assumption. Specifically, we find evidence that year-to-year changes in some of our outcomes differed between the expansion and non-expansion states prior to the

ACA Medicaid expansions (see results reported in Appendix Table 10).[4] The failure of the parallel trends assumption is perhaps unsurprising given that the DiD approach assumes that all non-expanding states (e.g., Texas and Florida), provide a good comparison for states that did expand Medicaid (e.g., Illinois and California). Therefore, instead of the usual DiD approach, we implement the synthetic control method of Abadie et al. (2010), which uses a matching procedure to create a synthetic comparison (control) group composed of a weighted average of observations from states that did not expand Medicaid. The Abadie et al. (2010) approach is in the same spirit of DiD because the estimate of the effect of Medicaid on consumer financial outcomes is obtained by taking the difference in means between treated states and a weighted average of non-treated states (i.e., synthetic control), but only in the post-intervention period of 2014 and 2015. The Abadie et al. (2010) approach assumes that pre-intervention differences between treatment and synthetic control groups are zero. Indeed, the approach's objective is to select a comparison group in such a way as to minimize the pre-intervention differences in means between the treatment group and the control group.

The key to the Abadie et al. (2010) approach is the selection of the weights that are used to construct the synthetic control group, or counterfactual outcome. Following Abadie et al. (2010), we choose weights that minimize the differences between the pre-Medicaid expansion mean values of the dependent variable and covariates of the treatment and control groups. The argument underlying this approach is that, if the pre-expansion means are equal between treated and untreated states, then the post-Medicaid expansion difference between the groups is likely to represent a valid estimate of the effect of the Medicaid expansion. An advantage of the Abadie et al. (2010) approach is that the closeness of the match can be assessed easily (e.g., graphically), and the weight for each potential comparison state is provided.[5]

There are a variety of ways to select weights that are used to construct the synthetic comparison group and it is not obvious that there is one correct method. Therefore, we use two approaches. Our first

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[4] For purposes of comparison, we also report the difference-in-differences estimates corresponding to our main analysis in Appendix Table 9.
[5] Only states with positive weights are used to construct the synthetic comparison group.

approach minimizes the difference between the pre-expansion values of the dependent variable and covariates of treated and untreated states for each pre-expansion year. As an alternative, we also minimize the difference between the average value of the dependent variable during the pre-expansion period, the 2013 value of the dependent variable, and each pre-expansion value of the covariates.[6]

Once the weights are selected and the synthetic comparison group is constructed, the estimates of the effect of Medicaid on financial wellbeing are derived by taking the post-2014 (Medicaid expansion) mean difference between the outcome in the treatment group (combined into one unit) and in the synthetic comparison group. Inferences for these estimates are derived from permutation tests (randomization inference). These tests consist of performing the analysis 1,000 times, but each time using randomly-selected states to form the treatment group. For each of these 1,000 "random" estimates, we estimate the post-2014 difference in outcomes as if the treatment indicator had been correctly assigned to the expansion states. We then calculate the share of "randomized" estimates that are larger in absolute value than the estimate obtained using the actual treatment group assignments. This share is the *p*-value corresponding to a two-sided test. This method captures the probability of obtaining estimates as large as the actual treatment group's estimate even when treatment is randomly assigned. The approach is the same as that used by Abadie et al. (2010), and the same or similar techniques have been implemented in other applications of the synthetic control method, such as Barone and Mocetti (2014), Billmeier and Nannicini (2013), and Eren and Ozbeklik (2016).[7]

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[6] See Kaul et al. (2015) for an analysis of the potential consequences of different approaches; matching on each pre-period value of the dependent variable reduces the influence of covariates. In our analysis, the method of choosing weights does not materially affect estimates.

[7] An earlier version of this paper used the distribution of the mean squared prediction error (MSPE) ratio from the placebo analyses to evaluate the statistical significance of estimates (Abadie et al. 2010). The MSPE ratio is the mean squared prediction error in the post period divided by mean squared prediction error in the pre-intervention period. However, we found a relatively low correlation between the magnitude of the post-period difference (our key estimate of interest) and the MSPE ratio. This finding reflects the fact that the MSPE ratio considers only the ratio of the difference of the post-to-pre prediction error. The MSPE is a relative (proportional) difference that can differ dramatically from absolute differences. So, many small and economically unimportant (post) estimates may seem "unusual" (significant) based on the proportional differences of the MSPE ratio. The main advantage of the MSPE is that it incorporates the "quality" of the experiment (matching), but we show results for alternate matching approaches to address this issue. However, we report p-values using the MSPE approach in Appendix Table 8 for the baseline estimates.

As we describe below, we use a sample selected on the basis of location to identify individuals who are more likely to have been treated due to the Medicaid expansion. However, this sample still includes many people not affected by the Medicaid expansions. Therefore, our estimates are intention-to-treat (ITT) effects. ITT estimates are useful and provide policy- and theory-relevant evidence of the effect of a state's expansion of Medicaid on the financial wellbeing of the low-income inhabitants. We also provide estimates of the effect of Medicaid coverage on individual financial wellbeing (i.e., treatment on treated) by rescaling the ITT estimates using "first stage" estimates of the proportion of individuals in our sample likely to have gained Medicaid coverage.

3. Data

American Community Survey

We use data from the 2010-2015 American Community Survey (ACS) to estimate the impact of the Medicaid expansions on health insurance coverage. This analysis constitutes the "first stage" of our analysis. Although other studies have used these data to show that the Medicaid expansions increased insurance coverage (e.g., Buchmueller et. al. 2016; Kaestner et al. 2017), we replicate their results using the synthetic control method so that the first stage is estimated in a consistent fashion as our reduced form equation that measures the effect of Medicaid expansions on financial well-being. In order to focus on the most-affected geographic regions, we restrict our analysis to include only individuals living in areas with the largest fraction of residents who are uninsured and in households earning under 138 percent of the FPL. In the ACS, the smallest geographic area available is the Public Use Microdata Area (PUMA). We therefore order all PUMAs based on the fraction of residents who are uninsured and low income, and include only the top quartile of PUMAs in our analysis. The sample for this analysis is adults ages 19 to 64.

Consumer Credit Panel/Equifax

We use information from the Federal Reserve Bank of New York Consumer Credit Panel/Equifax (CCP) data to measure the financial outcomes of the population between the ages of 19-64. The CCP is a quarterly database containing data from one (Equifax) of the three major credit bureaus. Credit bureaus maintain records for all individuals who apply for credit. The data we use cover all adults with a social security number who have ever applied for any type of credit. We use the nationally representative 5% sample of the CCP data. These data have been used to study the credit market effects of other social programs, for example, unemployment insurance (Hsu, Matsa, and Melzer 2014). Our age restriction is designed to ensure that our sample is representative of the adult population below age 65.[8] The resulting sample consists of about 8 million records per quarter.

We use all quarters of data from 2010 through 2015, giving us four years of data prior to the Medicaid expansion and two years of data post-expansion. The CCP contains no socioeconomic information and the only demographic information is birth year. However, there is detailed geographic information including zip code of residence. We utilize the information on age and geography in order to focus on individuals in the income ranges targeted by the Medicaid expansions, namely 138% or less of the FPL. Specifically, we used estimates from the 2008-2012 American Community Survey (ACS) of the share of a zip code adult population under age 65 who are both uninsured and have an income less than 138% of the FPL to select the sample.[9] We selected individuals living in the quartile of zip codes with the highest shares of people that were both uninsured and had income less than 138% of the FPL. We refer to this sample as the "most treated." This includes about 8,100 zip codes covering all states. On average, 17% of persons in these zip codes were uninsured and had incomes less than 138% of the FPL. In our

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[8] About 8% of individuals between the ages of 20 and 64 have no credit report; this fraction is higher (29%) for those living in low-income Census tracts (Brevoort et al., 2015). Although low-income adults often use informal credit such as payday loans (Agrawal et al., 2009), most individuals have had some interaction with credit markets. For example, the Oregon Medicaid Experiment matched 68.5% of adults earning under the FPL to a credit report. In ongoing work with administrative data from Michigan we have matched 90.1% percent of Medicaid enrollees to credit bureau data.

[9] See http://www.census.gov/acs/www/data/data-tables-and-tools/data-profiles/. The American Community Survey provides small area (i.e., zip codes) estimates of uninsured in the five-year data file. We used the 2008-2012 file. The ACS includes an indicator of whether a person is below 138% of FPL and provides health insurance information. For this analysis we are forced to use the 18-64 age range in order to obtain ACS estimates at the zip code level.

CCP sample, there are approximately 1.8 million individual records in the top quartile of zip code per quarter. The unit of observation for our analysis is state-by-quarter.

In some analyses, we stratify the sample by age in order to evaluate heterogeneous effects of the Medicaid expansions by age. Young individuals are less likely to experience a serious illness, so they may be less likely to be affected by the Medicaid expansions. However, young individuals are also more likely to be uninsured, and experienced larger coverage gains as a result of the Medicaid expansion (see Appendix Table 4). Analyzing the data separately by age allows us to document any differential effects due to age. Thus, we divided the sample into three age groups: 19 to 32, 33 to 44, and 45 to 64.

Although the CCP database consists of over 600 potential indicators of financial wellbeing based on the various forms of debt and account line information (e.g., credit cards, mortgages, auto loans, etc.), we purposefully restricted our attention to only a limited number of measures that *a priori* we thought most suitable. Our choices were based on the following general considerations: we preferred broad, aggregate measures to narrow ones; we selected outcomes likely to have been quickly affected by a health shock (and therefore, ameliorated by access to insurance); and where possible, considered outcomes that are unambiguous in terms of their welfare consequences. Our variables include: the total amount of debt (excluding mortgage debt); the total amount of debt at least 30 days past due; credit card debt; credit card debt past due; the number of new non-medical third party collections in the last 12 months; the total balance of non-medical collections; and the credit score.[10] We note that some of the financial outcomes we selected represent accumulated balances, or "stocks" (e.g., total amount of debt), and some are time-specific and represent "flows" (e.g., collections in the last 12 months). Since we are evaluating changes in

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[10] Although credit cards are a specific category of debt, we thought to include it separately since it is a common source of credit for our population of interest. We also included the credit score, even though it may be slow to adjust to financial shocks because it is a useful summary measure of credit access. For total amount of debt (past due), we excluded amounts from first mortgage trades, home equity installment trades, and home equity revolving trades. Total (credit card) debt at least 30 days past due excludes trades currently in bankruptcy and includes trades currently 30 days past due, 60 days past due, 90 days past due, 120 days past due or collections, and severe derogatory. We considered evaluating bankruptcy rates as an additional outcome, but we decided against including it in the analysis due to its relatively low incidence and concerns that we were not sufficiently well-powered to detect a meaningfully sized effect. We also believed that bankruptcies would be less quick to respond in the Medicaid population relative to a higher income population (such as the one affected by the Massachusetts health reform) due to the role that liquidity plays in the bankruptcy decision (Gross, Notowidigdo, and Wang 2014).

these variables over time, and because we selected variables that respond within relatively short time periods to external events, such as health insurance coverage, we expect to have sufficient statistical power to detect effects of Medicaid expansions. One possible exception is credit score, which is based only partially on delinquency and debt behavior and may therefore be slower to respond.

Previous studies, notably the Oregon Health Insurance Experiment (Baicker et al., 2013; Finkelstein et al., 2012) highlighted how access to insurance had a relatively quick effect in reducing unpaid medical balances reported to third-party collection agencies within a year of the reform. Dobkin et al. (2017) also showed that non-medical collections increased significantly in the first year after a hospitalization, and Barcellos and Jacobson (2015) also found relatively rapid responses of collection balances to insurance coverage. There is less evidence that the other selected financial indicators will respond within two years, although Dobkin et al. (2017) reported that credit card balances and credit limits decreased significantly within one year of hospitalization for both insured and uninsured persons while Mazumder and Miller (2016) found reductions in credit market delinquencies and bankruptcies one year following the expansion of coverage through the Massachusetts health care reform. Although the CCP is supposed to exclude information on medical collections, given the difficulty in classifying collections, there is a possibility that our collections variable includes some medical collections.

In addition to information on financial outcomes from the CCP, we use data on state demographic and socioeconomic characteristics, which are used in the synthetic control method to match treatment and control states. To capture changing economic conditions at the state level during the pre-reform period from 2010 to 2013, we use annual state poverty rate from the Small Area Income and Poverty Estimates (SAIPE) produced by the U.S. Census Bureau, annual state unemployment rate from the U.S. Bureau of Labor Statistics, and annual state 25th and 75th percentile of the log wage distribution for adults 19-64, calculated using the March Current Population Survey (CPS). We also construct a measure of Medicaid eligibility using the 2010 March CPS sample to capture the share of adults 19-64 that would be eligible

for Medicaid in each state and year.[11] Additionally, we aggregate zip code level demographics data to the state level to capture the population characteristics in the top quartile of uninsured-low-income quartile zip codes. Specifically, we use the 2012, five-year ACS estimates of the following: share of zip code non-elderly adult population Hispanic; share of zip code non-elderly adult population Black; and share of zip code non-elderly adult population with a high school diploma or less.[12]

Finally, because the CCP data allow us to follow the same adult over time, we can examine the potential for endogenous migration patterns by fixing a person's state and zip code of residence at the 2013 location, which is immediately prior to the Medicaid expansion. Results are largely unchanged when we restrict the sample in this way- and we verify that our estimates are not sensitive to the year in which we assign state and zip code of residence (see Appendix Table 5). Moreover, recent evidence specific to Medicaid (Schwartz and Sommers, 2014) suggests that there is no evidence that low-income individuals moved in response to past Medicaid expansions. Similarly, evidence on whether low-income persons moved for AFDC/TANF benefits also suggest little migration (Kaestner et al., 2003).

Assigning States to Treatment and Control Groups

As a result of the U.S. Supreme Court ruling on Medicaid (*National Federation of Independent Business v. Sebelius*), states were given the option of expanding Medicaid to cover all adults with incomes less than or equal to 138% of the FPL beginning in 2014. As of the end of 2015, 29 states and the District of Columbia had expanded Medicaid (in some form) while 21 states had not. For our analysis, however, the classification of treatment and control states differs from this simple distinction.

First, Delaware, Massachusetts, New York, Vermont, and Washington D.C. fully expanded Medicaid to parents and childless adults prior to 2014; we place them in the control group since they were

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[11] We obtain the Medicaid eligibility thresholds from the 2010-2013 Kaiser Family Foundation's annual reports on Medicaid eligibility rules. (See http://kff.org/medicaid/report/annual-updates-on-eligibility-rules-enrollment-and/.) We use the March 2010 CPS to calculate whether each individual aged 19-64 is eligible for Medicaid for each year from 2010 to 2013, given the state of residence, total household income, work status in the past year, and number of children. For simulated Medicaid eligibility, we match on the average pre-2014 values because there is virtually no change in eligibility over this period.
[12] These calculations use persons 18-64 years old because these estimates are available from the ACS at the zip code level.

effectively untreated in 2014 and 2015 and did not change status.[13] Second, there were seven states that expanded Medicaid under the ACA (Arizona, California, Connecticut, Hawaii, Iowa, Minnesota, and Washington) and two states (Maine and Wisconsin) that did not opt to expand under the ACA that had partially expanded Medicaid to the low-income adult population in some significant way prior to 2014.[14] These nine states that were "prior expanders" pose the largest challenge for classification. To address this issue, we consider three samples: (1) a *broad* sample where we include these nine states (seven in the treatment group and 2 in the control group), (2) a *narrow* sample that drops these nine states from the analysis, and (3) a *partially treated* sample that includes only the seven states with pre-ACA expansions in the treated group. Finally, since we define the treatment period as the eight quarters spanning from 2014:Q1 through 2015:Q4, we cannot include four "late expander" states (Alaska, New Hampshire, Pennsylvania, and Indiana) directly in our main analysis because they expanded after the beginning of the treatment period.[15] Recall that the synthetic control approach requires constructing a comparison group by matching on common, pre-policy outcomes, which necessitates a sharp temporal distinction between the treatment and control groups.

Thus, for our *broad* sample we have 21 states in the treatment group, 26 states in the control group, and 4 late expander states excluded. In our *narrow* sample, we have 14 states in the treatment group, 24 states in the control group, and 13 states excluded (9 partial expanders and 4 late expanders excluded). In our *partially treated* sample we have 7 states in the treatment group, 26 states in the control group, and 4 late expander states excluded. Appendix Table 1 shows how we classify the states.

In addition to the main analysis, we estimate the effects on the four late expanders as a separate group, using the first quarter of 2015 as the implementation date. We present two versions of this analysis

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[13] Results are similar if we drop these states; see Appendix Table 11.
[14] We base this classification on Garrett and Kaestner (2015). It is worth noting that an additional six states that expanded Medicaid under the ACA (Colorado, Illinois, Maryland, New Jersey, Oregon, and Rhode Island) and one state (Tennessee) that did not expand under the ACA also had partial Medicaid expansions but their pre-2014 expansions were sufficiently minor in importance to be reasonably ignored. There is considerable variation in these earlier state expansions in terms of coverage (e.g., parents and/or childless adults), benefits (e.g., outpatient only) and generosity (income eligibility). More detailed information can be found in Heberlein et al., 2011 and Heberlein et. al. 2012.
[15] Although Michigan did not expand in the beginning of 2014, we do not drop them since their expansion started by 2014:Q2. The classification error in the case of Michigan is likely to be small.

in the appendix (Appendix Table 6): all four late expanders grouped together, and only Indiana and

Pennsylvania included. We do not find evidence of significant improvement among these states, possibly

due to the short post-expansion period.

4. Results

Selecting Weights

The synthetic control approach first requires the selection of weights to construct the comparison

group. The weights are chosen to minimize differences in pre-2014 outcomes and covariates between

states that did and did not expand Medicaid. We use two approaches for matching on pre-2014 values: we

match on each pre-2014 value of the dependent variable and covariates, and we match on the pre-2014

average and 2013 values of the dependent variable, and each pre-2014 value of covariates.[16]

Table 1 presents the results of the matching procedure for one dependent variable: total collection

balance in past 12 months.[17] We show the means for treated states, means for control states (unweighted),

and means for the synthetic control states selected using the two matching procedures. The most notable

result is the close match between the pre-period (2014) means of the total collection balance between the

treated states and the synthetic control states. In contrast, the pre-2014 means for the treated and control

states (unweighted) are considerably different. The close tracking of the pre-2014 means between the

treated states and the synthetic control states bolsters the case for the credibility of the research design and

the interpretation of estimates from it as causal.

Estimates of the Effect of Medicaid Expansions on Insurance Coverage

We first document that the Medicaid expansions had a significant impact on insurance coverage.

Although this has been shown in other studies (e.g, Black and Cohen 2015; Kaestner et al., 2017;

Sommers 2014; Wherry and Miller 2016; Miller and Wherry 2017), we replicate the results here using the

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[16] We also estimated models using (1) only the average pre-treatment dependent variable along with covariates and (2) only using the 2013 lagged value of the dependent variable along with covariates and obtained similar results.
[17] In Appendix Table 2, we provide the weights for each potential control state that were used to construct the synthetic control states for all seven dependent variables used in the analysis for the broad sample. Many states get zero weights.

synthetic control method and a similar sample to obtain a first-stage estimate in a manner consistent with our reduced form analysis. The results are reported in Appendix Table 3. We find that individuals living in the most-treated areas (i.e., highest share uninsured and low income PUMAs) in expansion states experienced an increase in insurance coverage of approximately 5 percentage points and an increase in Medicaid coverage of between 5 and 6 percentage points. We do not find statistically significant changes in the fraction of individuals reporting they have private insurance, indicating that crowd-out was likely minimal in these locations. These estimates are similar to those in other studies.

Estimates of the Effect of Medicaid Expansions on Consumer Financial Wellbeing

Figure 1 shows the time series for all seven indicators of financial wellbeing for our broad sample of treated states where we match on all pre-2014 values of the dependent variable and covariates. As can be seen in Figure 1, the pre-2014 time trends in the financial indicators are virtually identical for the treated and synthetic control groups except for one outcome: number of collections. Even with respect to the number of collections, the trend in collections is similar for the treated and control groups. This graphical evidence strongly supports the validity of the synthetic control research design. Similarly, Figure 2 shows trends in financial indicators for the narrow sample of treated states. Here too, pre-2014 trends in financial indicators between the treatment and control groups are very similar. Finally, Figure 3 presents results for the group of seven states that had some form of prior Medicaid expansion. Again, the pre-trends appear to be similar between the treated and synthetic control groups.

In terms of impact of Medicaid expansions, Figures 1 through 3, show that, at least through the end of 2015, there appears to be little or no effect of the Medicaid expansions on most indicators of financial wellbeing. The exceptions are the two outcomes related to bills reported to third-party collection agencies: there was a clear, substantial decline in the amount of balances in collections in the treated states relative to the synthetic control unit, and a relative, but less marked decline in the number of bills sent to collections in treated states.

Table 2 presents synthetic control estimates and *p*-values of the average post-2014 differences in outcomes between treatment and control groups corresponding to Figures 1 through 3 (columns 2, 5 and 8). For each outcome, we also show the pre-reform means for the treated states in each of our three samples (columns 1, 4 and 7), along with estimates based on our alternative matching procedure for constructing the synthetic control group (columns 3, 6 and 9). The last point to mention about the presentation of Table 2 is that we use bold typeface to indicate estimates that remain statistically significant at the 5 percent level after adjusting our inference using the Holm-Bonferroni correction (Holm 1979). This correction controls the probability of a false positive within a domain of outcomes to be less than the significance level (in our case, 0.05). However, this correction is overly conservative (i.e., it fails to reject a false null hypothesis too frequently) unless outcomes are independent—which is not the case in our context.[18]

We begin the discussion of results with credit score (row 1), which is a summary measure of credit-worthiness that largely governs an individual's access to credit. The results suggest that the Medicaid expansion improved credit scores of those living in the most treated zip codes within the expansion states, but estimates are all small relative to the sample mean. In addition, estimates are not consistently statistically significant and only one of six estimates is significant at the 5 percent level before applying the Holm-Bonferroni correction. With that correction, the estimate is not significant. The lack of consistent effect on this outcome is not necessarily surprising, as credit scores use several years of credit history and may therefore be slow to change.[19]

Row 2 presents the results for total debt excluding mortgage liabilities. Recall, that the effect of the Medicaid expansion on debt is theoretically ambiguous. Estimates in row 2 indicate that the Medicaid expansions reduced total debt, although estimates are not consistently statistically significant across samples and models. Only one of the six estimates remains statistically significant after applying the

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[18] Because of the computationally intensive way our p-values are calculated, it is not feasible for us to use a more powerful family-wise error rate correction that rely on bootstrapping such as those described in Anderson (2007).
[19] Difference-in-difference estimate in Appendix Table 9 is of the same magnitude as estimates in Table 2 and not statistically significant.

Holm-Bonferroni correction. It is important to note that the mean value of total debt masks considerable variation between those individuals who are likely to be affected by the Medicaid expansions and those who are not. Those who are likely affected by the Medicaid expansion have debt levels that are likely to be considerably lower than average. For example, for those living in the most treated zip codes (top quartile of zip codes ranked by un-insurance and income less than 138% of the FPL), the pre-2014 mean of total debt is $10,341 (column 1 of Table 2). The corresponding figure for those living in the least treated zip codes is significantly higher at $16,898 (column 1 of Table 4). However, even this approximately $7,000 difference likely underestimates the difference between those likely and unlikely to be affected by the Medicaid expansions because there are both high- and low-income individuals in both samples. These differences imply that simply comparing the coefficient to the pre-Medicaid expansion mean may not be an accurate approach to assessing the magnitude of the effects. In addition, we may have less power to detect significant changes for some debt measures because the group affected by the Medicaid expansion has a relatively smaller impact on the average total debt.

Table 2 also presents the results for total debt past due (row 3). Estimates of the post-2014 difference between the treated and synthetic control states are all small relative to the mean, and not statistically significant. While some estimates are sizable (25%) relative to the standard deviation, it is important to note that for the group likely affected by Medicaid, the mean and standard deviation are expected to be considerably above the mean because income and past due debt are negatively correlated. Again, it is instructive to compare the pre-2014 mean of individuals living in different zip codes. The pre-2014 mean of total debt past due for individuals in the most treated zip code in the broad sample is $1,537. This is about 25% more than the corresponding figure for individuals in the least treated zip codes of $1,153 (see Tables 2 and 4). Moreover, the $1,537 value constitutes a much higher fraction of total debt (about 15%) than is the $1,153 (about 6%). As discussed earlier, the heterogeneity in the amount of debt and past due debt by income level implies that for measures of delinquent debt, we will have relatively more statistical power. Nevertheless, estimates of the effect of the Medicaid expansions on amount past due are not close to being statistically significant.

In row 4 of Table 2, we present estimates of the effect of the Medicaid expansions on total credit card debt. We find a statistically significant reduction in credit card debt in one of the six specifications, indicating a reduction in credit card debt of $93. This effect remains statistically significant after applying the Holm-Bonferroni correction. Estimates of the effects of the Medicaid expansions on credit card debt past due (row 5) are also negative, but not statistically significant, and range from -$3 to -$70. For this outcome, individuals in the most treated zip codes have a lower pre-2014 mean amount of credit card debt past due than individuals in the least treated zip codes (see Table 4). This suggests that both the total amount of credit card debt and amount of credit card debt past due rise with average income.[20]

The last two outcomes in Table 2 relate to non-medical bills that are past due and sent to third-party collection agencies: the number of collections and the total amount of collections. The estimated effects on the amount of non-medical collections (row 6) are all negative, have similar magnitudes and four out of six estimates are statistically significant even after accounting for multiple testing. The effect sizes range from -$47 to -$88. Similarly, estimates of the effect of the Medicaid expansions on the number of accounts in collection (row 7) are also all negative and of similar magnitudes, although there is less consistency in terms of statistical significance. Estimates range from -0.021 to -0.045. It is worth noting that difference-in-differences estimates pertaining to these outcomes in Appendix Table 9 are very similar to those in Table 2 and statistically significant. In the partially treated group of states (final two columns), we find somewhat smaller point estimates for both collection variables. The smaller and not significant estimates in this group are consistent with the idea that these states were less affected by the Medicaid expansions due to the substantial partial expansions that existed prior to the ACA.

The significant decreases in the collection balance post-2014 is consistent with the expectation that expanded Medicaid coverage would reduce the debt burden of those who obtain coverage. It is larger than the estimated reduction in average collection amounts associated with the Massachusetts health care reform of -$12 (Mazumder and Miller 2016), which may reflect that the population affected by the

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[20] Difference-in-differences estimates for credit card debt and credit card debt past due are also small and statistically insignificant.

Medicaid expansion is more disadvantaged than the one affected by the Massachusetts reform. Dobkin et al. (2017) found that hospitalization among the uninsured led to increased collection balances for both medical and non-medical bills, although the effects were larger for medical collections. As previously noted, while our measure of collections is ostensibly for non-medical debt, there is a non-trivial likelihood that it includes at least some medical debt because of the difficulty of classifying the type of debt. Our estimates describe the average effect of Medicaid in the entire post-2014 period. However, it is clear from Figures 1 through 3 that the effect becomes larger over time. In Appendix Table 7, we report the year-by-year estimates. Consistent with the visual evidence in Figures 1 through 3, we find larger effects in 2015 (Panel B) than in 2014 (Panel A), indicating the effect of Medicaid is increasing over time.

The estimates described above capture the overall change in financial outcomes among the entire adult population living in our target zip codes. However, only a fraction of these individuals actually obtained health insurance coverage through the ACA Medicaid expansions. Using ACS data, we estimate that in the most treated areas, Medicaid enrollment increased by about 5.7 percentage points (see Appendix Table 3). Focusing on the broad treatment group and the match using each value of the dependent variable, we find a treatment-on-treated (TOT) estimate of the effect of expansions on non-medical debt in collection of -$1,140 (-$65/0.057). In the Oregon study (Finkelstein et. al. 2012), the mean amount of total non-medical collections was $2,740. Thus, our TOT estimate indicates a reduction in non-medical collections of about 42 percent. We note, however, that Medicaid enrollment is often measured with considerable error in surveys; discrepancies between survey and administrative records typically range from 10 to 35 percent (Call et al. 2013). Measurement error in reported Medicaid coverage results in attenuation and, as a result, can bias our treatment effect estimate upward (Hausman, Abrevaya and Scott-Morton 1998, Meyer and Mittag 2014). This may explain in part why our estimated treatment effect is considerably larger than that reported in Finkelstein et al.(2012), who found a decrease in total collections of $469 and in non-medical collections specifically of only $79. Another possible explanation of the difference between our estimate and Finkelstein et al. (2012) is due to heterogeneous responses for those in our sample vis-à-vis the Oregon sample.

We also consider a second TOT calculation for uninsured individuals who are likely to face

serious illness requiring hospitalization or emergency room admission. Here we used the National Health

Interview Survey (NHIS) from 2010 through 2013 and found that 22% of adults under the age of 65 who

were uninsured with household incomes under 138% of the FPL had experienced either a hospitalization

or an ER visit. If we further rescale our earlier TOT estimates to apply to just individuals who had such an

experience (1.25%), the effects of gaining Medicaid among those with a serious health event is about

$5,183. This TOT estimate is the effect of acquiring health care insurance for those with a serious medical

incident and assumes that individuals without a serious medical incident experienced no beneficial

financial effects due to acquiring coverage. Dobkin et al. document that the impact of a hospitalization on

total unpaid collections for uninsured individuals is approximately $6000, quite close to our estimated

effect. Overall, our estimates in Table 2 suggest that the Medicaid expansions substantially improved the

financial wellbeing of those who gained coverage by reducing the number of collections and amount of

debt in third-party collections. Notably, the financial benefits of gaining insurance coverage extend

beyond the direct reduction in medical debt to reductions in other types of debt, which is consistent with

survey evidence (Doty et al. 2008).

We also conducted analyses on samples stratified by age to examine whether the effects of the

Medicaid expansions differed by age group. There may be differences in income, health, and/or

preferences that may affect both the probability of obtaining Medicaid and household finances. Panel A of

Table 3 presents estimates of the effect of Medicaid on the six financial indicators for each of three age

groups: a) 19-32 year olds, b) 33-44 year olds, and c) 45-64 year olds using our broad sample; Panel B

presents estimates for our narrow sample. Figures 3-5 present graphical evidence corresponding to

estimates in Panel A.[21] The pre-2014 trends (and levels) for the outcomes are virtually the same for the

treated and synthetic control states in nearly every case.

In Table 3, across all panels, we find fairly consistent evidence that the Medicaid expansions

decreased the number of collections and the total amount of debt in third-party collection across all age

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[21] We present the corresponding figures for Panel B in the Appendix Figures 4-6.

groups. This finding is consistent with the increases in Medicaid coverage for all groups that are similar in

size and only slightly larger for the youngest age group (see Appendix Table 4). For the 21 states in the

broad sample of treated states, we find statistically significant reductions in collection balance for all age

groups even when we apply the Holm-Bonferroni correction. Magnitudes for this sample range from -$71

to -$104. We also find that the expansions were associated with similar sized reductions in the number of

collections for all age groups, but estimates are less consistently significant. For other groupings of

treated states, which are shown in panels B and C of Table 3, we find similar qualitative results as in

panel A, but with less consistency in terms of statistical significance. However, overall, estimates in Table

3 suggest that the financial benefits of the Medicaid expansions were experienced by all age groups.

Tests of Validity of Research Design

Although the consistent similarity of the pre-2014 trends for treated and synthetic control states in

all of our figures provide substantial evidence of a valid research design, we conducted two additional

analyses to further bolster the credibility of our approach.[22] First, we conducted analyses using a sample

of individuals living in what we consider the least treated zip codes, those in the lowest quartile of zip

codes ranked according to the proportion of individuals who are both uninsured and have incomes below

138% of FPL. According to the ACS, only about 2% of individuals in the least treated zip codes would be

expected to have been eligible for the Medicaid expansions. Therefore, we expect this group to be much

less affected by the Medicaid expansions overall.

Figure 7 shows the time series patterns of outcomes for treated and synthetic control states

(selected for this sample) for the broad sample of treated states. During the treatment period, the Medicaid

expansions had little effect on the four non-collections outcomes. We also find evidence of reductions in

the number of collections and in collection balances for the least treated zip codes, but the differences

between the treatment and synthetic control groups are not nearly as large as those in Figure 1.[23] We

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[22] The close match of pre-trends between the treatment and synthetic control groups is found for alternative methods of selecting weights, which provides additional support for the validity of the research design (see, for example, Appendix Figures 1-3).
[23] It is also important to take into account the different scales for the y-axis between Figure 1 and Figure 7.

expect the effect to be small because relatively few individuals are affected, although those who are affected are likely to have a relatively large influence on the mean of total collections because of the strong association between income and third-party collections.

Estimates in Table 4, which have the same format as in Table 2, are consistent with the graphical evidence. The estimates on the number of collections are much lower, ranging from -0.004 to 0.005. Similarly, the effects on collection balances are also much lower than they were when estimated with the highest treatment group, ranging from -$19 to $1. The fact that estimates are significantly lower in the areas less likely to be treated provides further validation of the synthetic control research design.

Second, we incorporate these least-affected zip codes directly into our analysis by estimating a triple difference model that uses these zip codes as an additional source of variation in the effect of the expansion. These results are reported in Table 5. As expected from the models that estimate the effects on the most and least treated zip codes separately, we find similar results in the triple difference model as in our main specification for the broad and narrow sample of states, with statistically significant reductions in collections of between -$50 and -$89 and in the number of collections of between -0.038 and -0.046. We also find similarly smaller findings for the sample of states with some form of previous Medicaid expansion.

The third assessment of our research design consisted of analyses using a sample of individuals over age 65 living in the most treated zip codes. Almost all of these individuals are covered by Medicare and should not be affected by the Medicaid expansions, which explicitly target those under age 65. The Medicaid eligibility rules for those over 65 (dual eligible) were not altered by the ACA, although they may have been affected by the expansion indirectly, for example, if a family member gained coverage. Table 6 provides estimates for those over age 65 and Figure 8 presents the graphical evidence corresponding to Table 6 estimates in column 2. In two of six cases, estimates of the effect of Medicaid expansions on collections (balances and number) are statistically significant, but these estimates are much smaller (-$5 to -$22) than those we observe in the under age 65 samples. After applying the Holm-

Bonferroni correction, only one estimate remains statistically significant at the 5% level or better. Overall, these results provide additional validation of the synthetic control research design.

Estimates of the Effect of Medicaid on the Distribution of Outcomes

In addition to evaluating the impact of Medicaid eligibility on the mean of financial outcomes, we also examine how the ACA Medicaid expansions affected the distribution of these outcomes. This analysis can be suggestive of underlying mechanisms. For example, if we find that the ACA expansions prevented collections of small amounts among a large group of individuals, this would suggest that Medicaid coverage improves financial outcomes through widespread and diffuse income effects. Alternatively, if we find an impact on large collections concentrated among a small number of individuals, this would suggest that Medicaid mostly improves financial outcomes by protecting individuals who experience very large medical expenses from significant health shocks.

To examine how Medicaid affects delinquencies of different sizes, we created four categories for each of our delinquency outcomes (total amount past due, total credit card debt past due, and total collections).[24] For total amount past due, we created categories indicating whether the individual had $0 past due, $1-5,000 past due, $5,001-10,000 past due and more than $10,000 past due. For credit card debt past due and total amount in collections, we used lower thresholds as the amounts are generally much smaller than total amount past due (see Table 2). We created variables indicating $0 in collections or credit card delinquency respectively, $1-1,000 in collections or credit card delinquency, $1,001-$2,000 in collections or credit card delinquency, and more than $2,000 in collections or credit card delinquency.

In addition to delinquency measures, we also examine the effects on the distribution of credit scores. Since credit scores summarize an individual's creditworthiness, changes in the credit score distribution may capture effects not quantified by the delinquency measures described above. For credit score, we created variables indicating a credit score less than or equal to 600, between 601 and 660,

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[24] We selected thresholds based on the distribution of the outcomes using data from 2010-2013. For example, for total debt past due, $4000 is the median for people with balance past due and $10,000 is the 75th For credit card debt past due, $2,000 is the median for people with credit card balance and $6,000 is the 75th percentile.

between 661 and 780, and above 780. These bins roughly correspond to ratings of bad, poor, fair/good, and excellent credit used by creditors. We applied the Holm-Bonferroni correction within each set of outcomes (i.e., we adjusted for the fact we were conducting 4 tests in each category).

Table 7 presents estimates of the effect of Medicaid on these categories of debt and credit score. While there are several statistically significant findings among the many estimates, the most consistent estimates are those pertaining to a large collection balance. In this case, estimates indicate a reduction in the probability of having a collection balance of $2000 or more. Estimates indicate that individuals in the most treated zip codes in the 21 Medicaid expansions states experienced a reduction in the probability of having a large amount in collection by between 0.4 and 0.8 percentage points. Estimates pertaining to the stratified sample of treated states are similar in magnitude. Before applying the Holm-Bonferroni correction, all estimates are statistically significant at .10 level or less. These estimates suggest that the overall reduction in collections observed in previous tables is largely being driven by large reductions in collections consistent with the elimination of large medical bills.

Conclusion

The financial protection provided by health insurance is arguably its most important function. This is particularly true in the case of Medicaid because of the relatively high prevalence of disease among low-income individuals and the substantial financial burden that illness imposes on those who become seriously ill or injured. Indeed, a major justification for the Patient Protection and Affordable Care Act (ACA) of 2010 was to provide such financial protection. In this study, we examined whether the recent expansion of Medicaid to individuals aged 19-64 as part of the ACA affected the financial wellbeing of persons living in low-income zip codes. Ours is the first national study of the effect of expanding Medicaid to these individuals on several measures of financial wellbeing.

We used high-quality data from a large panel of credit reports from the Federal Reserve Bank of New York Consumer Credit Panel/Equifax. To obtain estimates of the effect of the Medicaid expansions on financial wellbeing, we employed the synthetic control approach of Abadie et al. (2010). We provide

evidenced that the approach was likely valid, so estimates of the effect of the Medicaid expansion are plausibly interpreted as causal.

Results indicated that the Medicaid expansions significantly reduced the amount of debt in third-party collection among individuals living in the top quartile of zip codes ranked by the proportion of poor and uninsured persons. Intention-to-treat (ITT) estimates indicated that the 2014 Medicaid expansions were associated with a reduction in the amount of collections of between $61 and $88, with a mean (simple average) estimate of $70. These reduced form estimates imply a treatment-on-treated (TOT) effect of approximately $1,140. For other measures of debt and debt past due, we did not find any consistent evidence that the ACA Medicaid expansions had any effect, although it would be useful to revisit these estimates as more years of post-expansion data become available.

While these results show that the ACA Medicaid expansions had important financial impacts outside of health care use, they are also consistent with recent work documenting that much of the incidence of these financial effects falls on third parties, as much as the uninsured themselves (Finkelstein et al., 2015; Garthwaite, Gross, and Notowidigdo, forthcoming; Nikpay et al., 2017). Given that the ACA Medicaid expansions decreased unpaid bills, the financial benefits of the ACA expansions extend, at least partially, to organizations that extend credit to low-income uninsured individuals, as these creditors are less likely to be adversely impacted by bad debt. As a result, it may be easier to obtain credit or increase borrowing among those individuals who gained coverage through the ACA Medicaid expansions, which may improve their material wellbeing in the future.

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Table 1
Comparison of Pre-2014 Means for Treated States and Synthetic Control States
Dependent Variable is Total Collection Balance; 21 Treatment States, 26 Potential Control States

	(1)	(2)	(3)	(4)
	Treated States	Control States	Synthetic Control States	
			Match on All Lagged Y and X's	Match on Average Y, Y in 2013, and All Lagged X's
State Simulated Medicaid Eligibility	0.080	0.035	0.102	0.077
State Unemployment Rate				
2010	0.108	0.094	0.097	0.100
2011	0.101	0.087	0.092	0.095
2012	0.090	0.076	0.085	0.086
2013	0.081	0.068	0.075	0.076
State Poverty Rate				
2010	0.158	0.173	0.162	0.163
2011	0.166	0.180	0.170	0.167
2013	0.167	0.178	0.170	0.168
2013	0.165	0.176	0.169	0.167
State 25th Percentile of Log Wage				
2010	9.66	9.71	9.75	9.77
2011	9.70	9.74	9.77	9.77
2012	9.70	9.74	9.75	9.76
2013	9.76	9.81	9.84	9.82
State 75th Percentile of Log Wage				
2010	10.93	10.84	10.93	10.94
2011	10.95	10.87	10.93	10.94
2012	10.96	10.87	10.94	10.95
2013	10.99	10.90	10.95	10.93
% Hispanic	37.0%	24.8%	21.6%	8.9%
% Black	15.6%	24.4%	23.0%	20.5%
% HS Degree or Less	29.4%	28.3%	28.3%	25.6%
% Uninsured & <138% FPL	15.9%	16.6%	15.3%	17.1%
Average Total Collection Balance	333.08	479.69		340.16
Total collection balance				
2010Q1	320.65	426.74	294.39	
2010Q2	335.59	443.37	302.04	
2010Q3	328.47	452.59	307.58	
2010Q4	313.62	448.69	311.91	
2011Q1	312.05	442.19	307.31	
2011Q2	320.05	459.68	314.34	
2011Q3	326.15	465.59	320.93	
2011Q3	331.11	479.38	334.29	
2012Q1	345.71	479.17	339.22	
2012Q2	354.62	496.47	346.49	
2012Q3	347.88	488.42	324.19	
2012Q4	355.32	519.92	351.91	
2013Q1	341.96	519.59	355.71	355.52
2013Q2	340.37	512.96	347.79	346.48
2013Q3	322.44	509.20	355.15	385.60
2013Q4	333.26	535.89	372.28	391.33

Table 2

Synthetic Control Estimates of the Effect of Medicaid on Indicators of Financial Wellbeing for Most Treated Zip Codes, Ages 19-64

	Post-2014 Difference in Means Between Treatment States minus Synthetic Control								
	21 Treatment States, 26 Potential Control States			14 Treatment States, 24 Potential Control States			7 Treatment States, 26 Potential Control States		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable
Credit Score	648	1	2**	643	-1	0	654	3*	3
	(13)	(0.554)	(0.038)	(15)	(0.245)	(0.989)	(8)	(0.088)	(0.138)
Total Balance	10341	-221	**-831***	10636	-441*	-540**	10026	-514	-781**
	(1278)	(0.241)	**(0.000)**	(1613)	(0.054)	(0.022)	(639)	(0.128)	(0.042)
Total Balance Past Due	1537	29	56	1451	39	39	1629	-66	-41
	(271)	(0.488)	(0.255)	(240)	(0.455)	(0.431)	(273)	(0.282)	(0.601)
Total Credit Card Balance	2580	-26	**-93***	2455	-61	6	2714	-37	-77
	(397)	(0.405)	**(0.007)**	(434)	(0.103)	(0.854)	(303)	(0.468)	(0.175)
Total Credit Card Balance Past Due	855	-24	-39	676	-3	-19	1047	-66	-70
	(315)	(0.347)	(0.168)	(238)	(0.891)	(0.395)	(271)	(0.141)	(0.165)
Total Collections Balance	333	**-65***	**-66***	394	**-88***	**-61***	268	-47	-57
	(105)	**(0.000)**	**(0.005)**	(107)	**(0.000)**	**(0.006)**	(46)	(0.165)	(0.126)
Number of Collections	0.461	**-0.045***	-0.033*	0.591	-0.038*	-0.037	0.322	-0.021	-0.021
	(0.177)	**(0.008)**	(0.100)	(0.141)	(0.063)	(0.109)	(0.076)	(0.513)	(0.584)

Table 2 reports the estimates of the post-2014 differences in financial indicators between treated and synthetic control states for non-elderly adults in the most treated zip codes. Columns (1) - (3) present the results for the broad sample with 21 treatment states and 26 potential control states. Columns (4) - (6) present the results for the narrow sample with 14 treatment states and 24 potential control states. Columns (7) - (9) present the results for 7 partially treated states and 26 potential control states, with the 7 treated states being those included in the broad treatment group but not the narrow. For each expansionary definition, we present the 2010-2013 pre-reform mean outcome for the treated states, and the average post-reform quarterly difference between the treated states and their synthetic counterpart using the two different weighting methods used to construct the synthetic control group. In all results, AK, IN, NH, and PA are dropped. Bolded results are also significant at the 5% level when the Holm-Bonferroni correction using 7 outcomes is applied to assess absolute gap p-values. Significance levels: * = 10%, ** = 5%, *** = 1%.

Table 3

Synthetic Control Estimates of the Effect of Medicaid on Indicators of Financial Wellbeing for Most Treated Zip Codes by Age Group

A. 21 Treatment States, 26 Potential Control States

	Post-2014 Difference in Means Between Treatment States minus Synthetic Control					
	Ages 19-32		Ages 33-44		Ages 45-64	
Outcome	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable
Credit Score	617	1	637	2*	676	2*
	(12)	(0.363)	(14)	(0.062)	(14)	(0.061)
Total Balance	9566	-203	11143	8	10351	-135
	(1231)	(0.364)	(1604)	(0.976)	(1491)	(0.481)
Total Balance Past Due	1404	54	1870	-52	1415	-128**
	(235)	(0.297)	(331)	(0.374)	(315)	(0.014)
Total Credit Card Balance	1209	-17	2577	-84*	3508	**-167***
	(251)	(0.580)	(429)	(0.078)	(535)	**(0.004)**
Total Credit Card Balance Past Due	400	-11	882	-92**	1226	-89**
	(139)	(0.580)	(350)	(0.022)	(461)	(0.037)
Total Collections Balance	377	**-71***	372	**-104***	278	**-74***
	(137)	**(0.004)**	(118)	**(0.000)**	(88)	**(0.000)**
Number of Collections	0.535	-0.044**	0.524	-0.036	0.369	-0.028*
	(0.221)	(0.042)	(0.204)	(0.186)	(0.141)	(0.098)

Table 3 reports the estimates of the post-2014 differences in financial indicators between treated and synthetic control states by age group for the most treated zip code. Panel A reports the results for the broad sample with 21 treatment states and 26 potential control states. Panel B reports the results for the narrow sample with 14 treatment states and 24 potential control states. Panel C reports the results for 7 treatment states and 26 potential control states, with the 7 treated states being those included in the broad treatment group but not the narrow. For each age group (ages 19-32: columns (1) - (2); ages 33-44: columns (3) - (4); ages 45-64: columns (5) - (6)), we present the 2010-2013 pre-reform mean outcome for the treated states and the average post-reform quarterly difference between the treated states and their synthetic counterpart. In addition to AK, IN, NH, and PA, DC and MA are dropped from all age results due to not having enough observations for many credit categories. HI is additionally dropped from ages 19-32 results. Bolded results are also significant at the 5% level when the Holm-Bonferroni correction using 7 outcomes is applied to assess absolute gap p-values. Significance levels: * = 10%, ** = 5%, *** = 1%.

Synthetic Control Estimates of the Effect of Medicaid on Indicators of Financial Wellbeing for Most Treated Zip Codes by Age Group

B. 14 Treatment States, 24 Potential Control States

	Post-2014 Difference in Means Between Treatment States minus Synthetic Control					
	Ages 19-32		Ages 33-44		Ages 45-64	
Outcome	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable, clean	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable, clean	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable, clean
Credit Score	610	-1	629	1	672	1
	(13)	(0.492)	(15)	(0.679)	(16)	(0.325)
Total Balance	9823	-250	11565	222	10588	-151
	(1491)	(0.213)	(2011)	(0.449)	(1882)	(0.544)
Total Balance Past Due	1396	52	1811	17	1270	41
	(263)	(0.449)	(314)	(0.832)	(247)	(0.454)
Total Credit Card Balance	1060	-51*	2406	-97*	3369	-104*
	(247)	(0.091)	(430)	(0.071)	(589)	(0.052)
Total Credit Card Balance Past Due	321	-9	686	-8	973	-13
	(101)	(0.611)	(248)	(0.817)	(373)	(0.692)
Total Collections Balance	470	**-94***	444	-93***	314	-50**
	(127)	**(0.004)**	(116)	(0.010)	(104)	(0.035)
Number of Collections	0.707	-0.062**	0.677	-0.059**	0.463	-0.037*
	(0.170)	(0.023)	(0.164)	(0.050)	(0.124)	(0.068)

Table 3 reports the estimates of the post-2014 differences in financial indicators between treated and synthetic control states by age group for the most treated zip code. Panel A reports the results for the broad sample with 21 treatment states and 26 potential control states. Panel B reports the results for the narrow sample with 14 treatment states and 24 potential control states. Panel C reports the results for 7 treatment states and 26 potential control states, with the 7 treated states being those included in the broad treatment group but not the narrow. For each age group (ages 19-32: columns (1) - (2); ages 33-44: columns (3) - (4); ages 45-64: columns (5) - (6)), we present the 2010-2013 pre-reform mean outcome for the treated states and the average post-reform quarterly difference between the treated states and their synthetic counterpart. In addition to AK, IN, NH, and PA, DC and MA are dropped from all age results due to not having enough observations for many credit categories. HI is additionally dropped from ages 19-32 results. Bolded results are also significant at the 5% level when the Holm-Bonferroni correction using 7 outcomes is applied to assess absolute gap p-values. Significance levels: * = 10%, ** = 5%, *** = 1%.

Synthetic Control Estimates of the Effect of Medicaid on Indicators of Financial Wellbeing for Most Treated Zip Codes by Age Group

C. 7 Treatment States, 26 Potential Control States

	Post-2014 Difference in Means Between Treatment States minus Synthetic Control					
	Ages 19-32		Ages 33-44		Ages 45-64	
Outcome	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable
Credit Score	624	6***	645	7**	681	6**
	(6)	(0.009)	(9)	(0.031)	(8)	(0.040)
Total Balance	9311	-588**	10705	-394	10081	-588*
	(829)	(0.044)	(823)	(0.228)	(765)	(0.086)
Total Balance Past Due	1411	-42	1932	-167*	1581	**-275***
	(204)	(0.564)	(338)	(0.068)	(303)	**(0.001)**
Total Credit Card Balance	1357	7	2753	-81	3667	-124
	(144)	(0.870)	(351)	(0.372)	(413)	(0.204)
Total Credit Card Balance Past Due	478	-18	1086	-134*	1515	-137*
	(127)	(0.446)	(325)	(0.071)	(374)	(0.063)
Total Collections Balance	284	-26	298	-77*	236	**-106***
	(65)	(0.496)	(59)	(0.060)	(32)	**(0.001)**
Number of Collections	0.364	-0.054	0.366	-0.057	0.261	-0.023
	(0.097)	(0.154)	(0.090)	(0.113)	(0.056)	(0.352)

Table 3 reports the estimates of the post-2014 differences in financial indicators between treated and synthetic control states by age group for the most treated zip code. Panel A reports the results for the broad sample with 21 treatment states and 26 potential control states. Panel B reports the results for the narrow sample with 14 treatment states and 24 potential control states. Panel C reports the results for 7 treatment states and 26 potential control states, with the 7 treated states being those included in the broad treatment group but not the narrow. For each age group (ages 19-32: columns (1) - (2); ages 33-44: columns (3) - (4); ages 45-64: columns (5) - (6)), we present the 2010-2013 pre-reform mean outcome for the treated states and the average post-reform quarterly difference between the treated states and their synthetic counterpart. In addition to AK, IN, NH, and PA, DC and MA are dropped from all age results due to not having enough observations for many credit categories. HI is additionally dropped from ages 19-32 results. Bolded results are also significant at the 5% level when the Holm-Bonferroni correction using 7 outcomes is applied to assess absolute gap p-values. Significance levels: * = 10%, ** = 5%, *** = 1%.

Table 4

Synthetic Control Estimates of the Effect of Medicaid on Indicators of Financial Wellbeing for Least Treated Zip Codes, Ages 19-64

	Post-2014 Difference in Means Between Treatment States minus Synthetic Control								
	21 Treatment States, 26 Potential Control States			14 Treatment States, 24 Potential Control States			7 Treatment States, 26 Potential Control States		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable
Credit Score	719	1	-0	718	-1	-0	721	1	2*
	(8)	(0.542)	(0.908)	(8)	(0.306)	(0.805)	(7)	(0.203)	(0.082)
Total Balance	16898	178	-10	17149	539**	379*	16599	-11	-83
	(1336)	(0.237)	(0.944)	(1336)	(0.018)	(0.071)	(1279)	(0.949)	(0.718)
Total Balance Past Due	1153	-48	-29	1113	-57	-48	1201	-70	-72
	(235)	(0.122)	(0.343)	(159)	(0.113)	(0.217)	(297)	(0.165)	(0.172)
Total Credit Card Balance	5326	-61	-28	5274	-246***	11	5388	-24	-31
	(583)	(0.190)	(0.565)	(492)	(0.010)	(0.844)	(673)	(0.692)	(0.678)
Total Credit Card Balance Past Due	1992	-31	-11	1744	19	17	2288	-98*	-51
	(746)	(0.468)	(0.758)	(407)	(0.694)	(0.692)	(931)	(0.100)	(0.336)
Total Collections Balance	114	-7	-17**	118	1	-11	110	-12	-19*
	(28)	(0.263)	(0.017)	(30)	(0.906)	(0.197)	(25)	(0.182)	(0.080)
Number of Collections	0.155	0.000	-0.002	0.177	0.005	0.001	0.127	-0.004	-0.003
	(0.057)	(0.948)	(0.790)	(0.059)	(0.592)	(0.920)	(0.041)	(0.604)	(0.777)

Table 4 reports the estimates of the post-2014 differences in financial indicators between treated and synthetic control states for non-elderly adults in the least treated zip codes. Columns (1) - (3) present the results for the broad sample with 21 treatment states and 26 potential control states. Columns (4) - (6) present the results for the narrow sample with 14 treatment states and 24 potential control states. Columns (7) - (9) present the results for 7 treatment states and 26 potential control states, with the 7 treated states being those included in the broad treatment group but not the narrow. For each expansionary definition, we present the 2010-2013 pre-reform mean outcome for the treated states and average quarterly difference between the treated states and their synthetic counterpart using the two different weighting methods used to construct the synthetic control group. In all results, AK, IN, NH, and PA are dropped. Bolded results are also significant at the 5% level when the Holm-Bonferroni correction using 7 outcomes is applied to assess absolute gap p-values. Significance levels: * = 10%, ** = 5%, *** = 1%.

Table 5

Synthetic Control Triple Difference Estimates of the Effect of Medication on Indicators of Financial Wellbeing, Ages 19-64, Most – Least Treated Zip Codes

	Post-2014 Difference in Means Between Most and Least Treated Zip Codes in Treated States minus					
	Difference in Means Between Most and Least Treated Zip Codes in Synthetic Control					
	(1)	(2)	(3)	(4)	(5)	(6)
	21 Treatment States, 26 Potential Control States		14 Treatment States, 24 Potential Control States		7 Treatment States, 26 Potential Control States	
Outcome	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable
Credit Score	-0	2*	-0	0	2	1
	(0.999)	(0.086)	(0.795)	(0.891)	(0.277)	(0.561)
Total Balance	-399	**-820***	**-980***	**-918***	-503	-698*
	(0.105)	**(0.000)**	**(0.000)**	**(0.003)**	(0.135)	(0.060)
Total Balance Past Due	77	85	96	87	4	31
	(0.166)	(0.150)	(0.139)	(0.170)	(0.946)	(0.717)
Total Credit Card Balance	35	-65	185**	-5	-13	-46
	(0.460)	(0.198)	(0.026)	(0.923)	(0.838)	(0.598)
Total Credit Card Balance Past Due	7	-28	-21	-36	32	-19
	(0.877)	(0.502)	(0.663)	(0.456)	(0.547)	(0.746)
Total Collections Balance	**-57***	-50**	**-89***	-50**	-35	-38
	(0.004)	(0.011)	**(0.000)**	(0.022)	(0.303)	(0.239)
Number of Collections	-0.046**	-0.031	-0.043**	-0.038*	-0.017	-0.018
	(0.012)	(0.126)	(0.040)	(0.097)	(0.587)	(0.599)

Table 5 reports the estimates of the post-2014 differences in financial indicators between most and least treated zip codes for the treated and synthetic control states for non-elderly adults. Columns (1) - (2) present the results for the broad sample with 21 treatment states and 26 potential control states. Columns (3) - (4) present the results for the narrow sample with 14 treatment states and 24 potential control states. Columns (5) - (6) present the results for 7 treatment states and 26 potential control states, with the 7 treated states being those included in the broad treatment group but not the narrow one. For each expansionary definition, we present the average post-reform difference between the treated states and their synthetic counterpart using the two different weighting methods. In all results, AK, IN, NH, and PA are dropped. Bolded results are also significant at the 5% level when the Holm-Bonferroni correction using 7 outcomes is applied to assess absolute gap p-values. Significance levels: * = 10%, ** = 5%, *** = 1%.

Table 6

Synthetic Control Estimates of the Effect of Medicaid on Indicators of Financial Wellbeing for Elderly in Most Treated Zip Codes, Ages 65 and Over

	Post-2014 Difference in Means Between Treatment States minus Synthetic Control								
	21 Treatment States, 26 Potential Control States			14 Treatment States, 24 Potential Control States			7 Treatment States, 26 Potential Control States		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable
Credit Score	736	0	0	735	-0	-1	738	2*	2*
	(12)	(0.700)	(0.677)	(14)	(0.751)	(0.490)	(8)	(0.063)	(0.098)
Total Balance	5349	-337**	-48	5349	-449*	-30	5349	-107	-39
	(459)	(0.040)	(0.627)	(536)	(0.053)	(0.808)	(324)	(0.577)	(0.818)
Total Balance Past Due	620	-14	2	566	19	-2	696	-61*	-23
	(153)	(0.700)	(0.934)	(135)	(0.568)	(0.943)	(145)	(0.070)	(0.537)
Total Credit Card Balance	2510	-42	4	2370	-128**	-55	2708	37	138
	(330)	(0.308)	(0.920)	(351)	(0.017)	(0.232)	(150)	(0.550)	(0.160)
Total Credit Card Balance Past Due	872	-22	-41	740	-3	-1	1057	-40	-89
	(288)	(0.534)	(0.396)	(262)	(0.919)	(0.973)	(211)	(0.475)	(0.267)
Total Collections Balance	91	**-19*****	-5	95	-9	-7	86	-22**	-11
	(27)	**(0.000)**	(0.507)	(33)	(0.234)	(0.468)	(15)	(0.016)	(0.372)
Number of Collections	0.145	-0.003	-0.008	0.172	-0.002	-0.008	0.106	-0.006	-0.034**
	(0.056)	(0.631)	(0.244)	(0.058)	(0.776)	(0.257)	(0.012)	(0.519)	(0.011)

Table 6 reports the estimates for the post-2014 differences in financial indicators between treated and synthetic control states for elderly adults in the most treated zip codes. Columns (1) - (3) present the results for the broad sample with 21 treatment states and 26 potential control states. Columns (4) - (6) present the results for the narrow sample with 14 treatment states and 24 potential control states. Columns (7) - (9) present the results for 7 treatment states and 26 potential control states, with the 7 treated states being those included in the broad treatment group but not the narrow. For each expansionary definition, we present the 2010-2013 pre-reform mean outcome for the treated states and average quarterly difference between the treated states and their synthetic counterpart using the two different weighting methods used to construct the synthetic control group. In addition to AK, IN, NH, and PA, DC and MA are dropped due to not having enough observations. Bolded results are also significant at the 5% level when the Holm-Bonferroni correction using 7 outcomes is applied to assess absolute gap p-values. Significance levels: * = 10%, ** = 5%, *** = 1%.

Table 7
Synthetic Control Estimates of the Effect of Medicaid on the Distribution of Delinquencies and Credit Score

	Post-2014 Difference in Means Between Treatment States minus Synthetic Control								
	21 Treatment States, 26 Potential Control States			14 Treatment States, 24 Potential Control States			7 Treatment States, 26 Potential States		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable
$0 Total Debt Past Due	0.807	-0.000	-0.001	0.808	-0.005	-0.001	0.805	0.004	0.002
	(0.023)	(0.903)	(0.687)	(0.025)	(0.140)	(0.769)	(0.022)	(0.309)	(0.716)
$1-$5000 Total Debt Past Due	0.114	-0.001	-0.002	0.116	-0.002	-0.001	0.111	0.002	0.000
	(0.013)	(0.528)	(0.250)	(0.016)	(0.107)	(0.420)	(0.008)	(0.409)	(0.805)
$5001-$10000 Total Debt Past Due	0.034	-0.000	-0.001*	0.032	0.002*	0.001	0.036	-0.002*	-0.004**
	(0.006)	(0.992)	(0.099)	(0.005)	(0.082)	(0.552)	(0.006)	(0.099)	(0.027)
$10000+ Total Debt Past Due	0.046	-0.000	-0.001	0.043	0.001	0.001	0.049	-0.003	-0.006**
	(0.009)	(0.904)	(0.376)	(0.008)	(0.361)	(0.749)	(0.010)	(0.188)	(0.032)
$0 Credit Card Balance Past Due	0.822	0.014*	0.008	0.838	0.010**	0.005	0.805	0.015	0.009
	(0.034)	(0.051)	(0.168)	(0.033)	-0.023	(0.245)	(0.025)	(0.101)	(0.271)
$1-$1000 Credit Card Balance Past Due	0.033	-0.001	-0.001	0.034	-0.001	-0.001	0.032	-0.003	-0.000
	(0.007)	(0.269)	(0.640)	(0.009)	(0.360)	(0.497)	(0.005)	(0.153)	(0.978)
$1001-$2000 Credit Card Balance Past Due	0.037	0.000	0.000	0.035	-0.002	-0.001	0.039	0.001	0.002
	(0.007)	(0.987)	(0.902)	(0.008)	(0.282)	(0.453)	(0.004)	(0.567)	(0.302)
$2000+ Credit Card Balance Past Due	0.080	**-0.008***	-0.004	0.066	-0.003	-0.002	0.094	-0.014**	0.001
	(0.022)	**(0.009)**	(0.169)	(0.018)	(0.133)	(0.160)	(0.016)	(0.022)	(0.720)
$0 Collections	0.765	0.003	-0.000	0.723	-0.000	0.002	0.810	0.002	0.004
	(0.058)	(0.561)	(0.996)	(0.048)	(0.990)	(0.786)	(0.025)	(0.851)	(0.730)
$1-$1000 Collections	0.160	0.005	0.005	0.185	0.006	0.003	0.132	0.000	0.000
	(0.036)	(0.152)	(0.219)	(0.031)	(0.130)	(0.456)	(0.015)	(0.946)	(0.939)
$1001-$2000 Collections	0.039	-0.004**	-0.001	0.048	-0.001	-0.001	0.030	-0.003	-0.010**
	(0.012)	(0.021)	(0.433)	(0.011)	(0.420)	(0.667)	(0.005)	(0.264)	(0.031)
$2000+ Collections	0.036	-0.004*	**-0.008***	0.044	-0.007**	-0.005**	0.028	-0.008**	-0.012**
	(0.012)	(0.052)	**(0.002)**	(0.011)	(0.013)	(0.041)	(0.005)	(0.045)	(0.014)
Credit Score <=600	0.310	-0.001	-0.004	0.328	-0.003	0.000	0.290	**-0.025***	-0.007

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	(0.049)	(0.823)	(0.258)	(0.053)	(0.539)	(0.971)	(0.035)	**(0.007)**	(0.186)
Credit Score 601-660	0.185	-0.007**	**-0.010***	0.177	-0.002	-0.001	0.194	-0.006	-0.011*
	(0.014)	(0.013)	**(0.003)**	(0.009)	(0.615)	(0.778)	(0.013)	(0.206)	(0.055)
Credit Score 661-780	0.283	0.005	**-0.037***	0.262	0.003	0.000	0.307	0.011	0.010
	(0.037)	(0.140)	**(0.000)**	(0.035)	(0.443)	(0.942)	(0.021)	(0.162)	(0.183)
Credit Score 780+	0.221	0.005	0.004	0.233	0.002	0.002	0.209	0.004	-0.002
	(0.030)	(0.210)	(0.243)	(0.032)	(0.705)	(0.593)	(0.021)	(0.288)	(0.690)

Table 7 reports the estimates of the post-2014 differences in financial indicators between treated and synthetic control states for non-elderly adults in the most treated zip codes. Columns (1) - (3) present the results for the broad sample with 21 treatment states and 26 potential control states. Columns (4) - (6) present the results for the narrow sample with 14 treatment states and 24 potential control states. Columns (7) - (9) present the results for 7 treatment states and 26 potential control states, with the 7 treated states being those included in the broad treatment group but not the narrow. For each expansionary definition, we present the 2010-2013 pre-reform mean outcome for the treated states and the average post-reform quarterly difference between the treated states and their synthetic counterpart using the two different weighting methods used to construct the synthetic control group. In all results, AK, IN, NH, and PA are dropped. Bolded results are also significant at the 5% level when the Holm-Bonferroni correction using 4 outcomes (for each categorical variable group) is applied to assess absolute gap p-values. Significance levels: * = 10%, ** = 5%, *** = 1%.
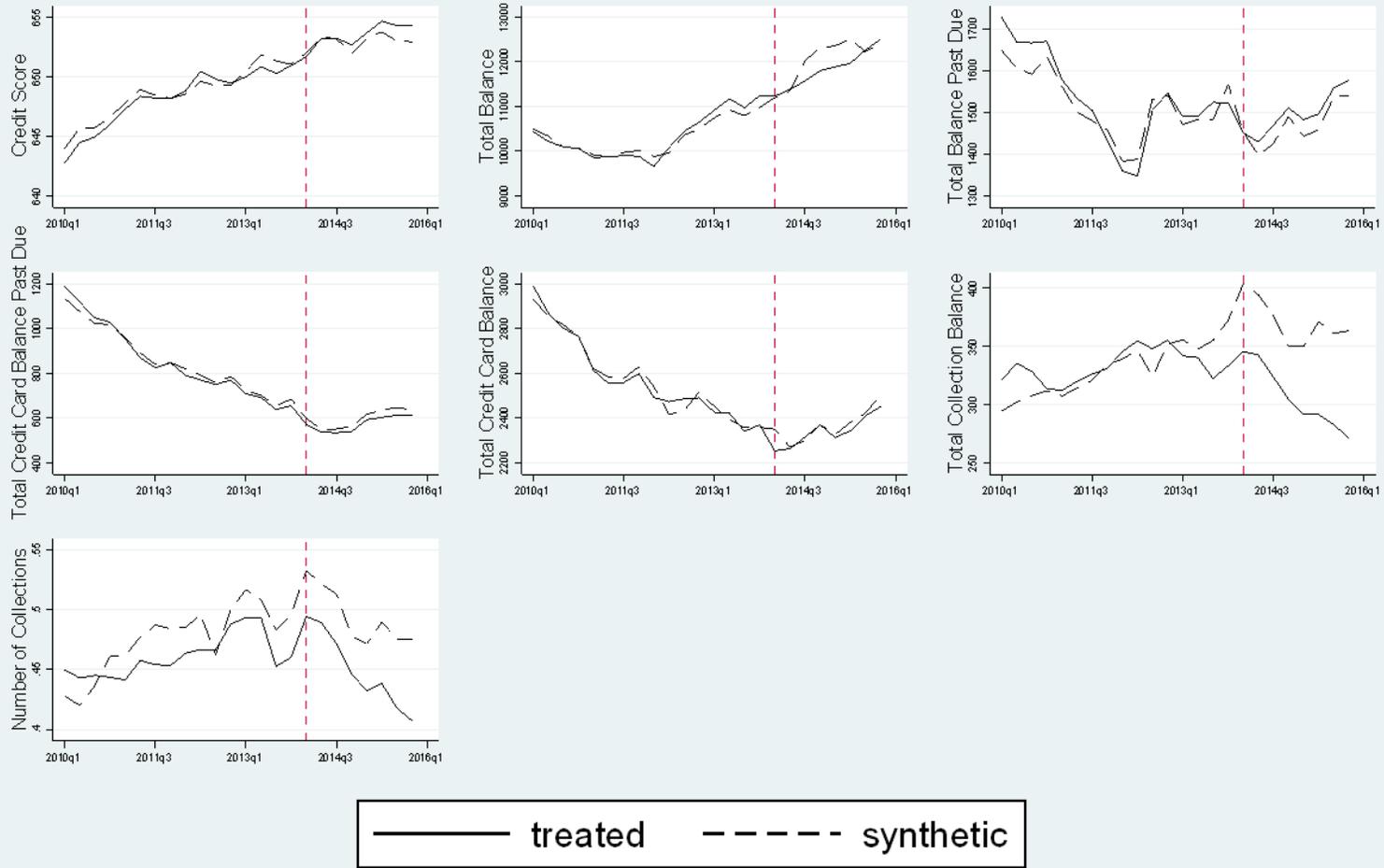
Figure 1. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Most Treated Zip Codes Using 21 Treated States, 26 Potential Control States
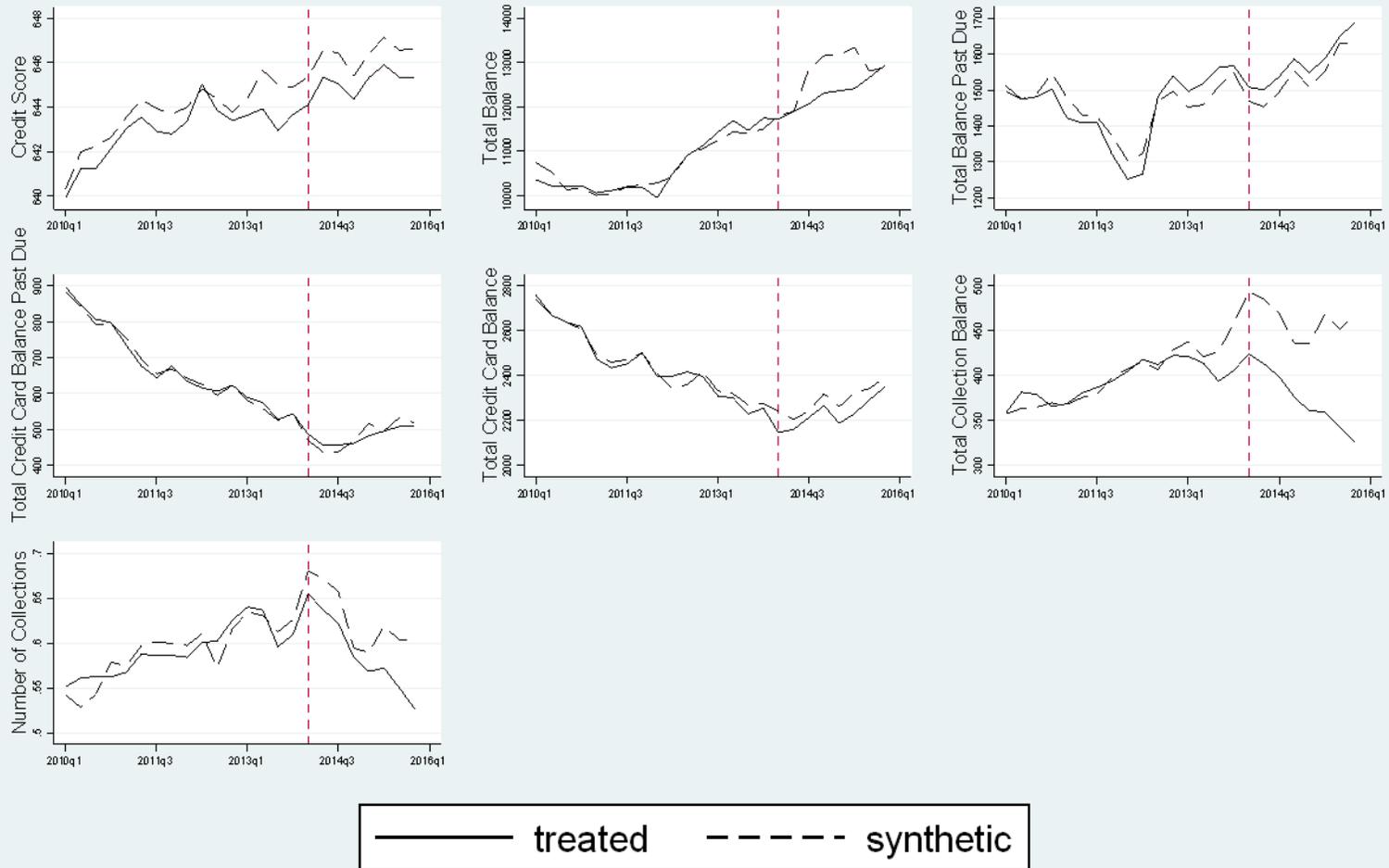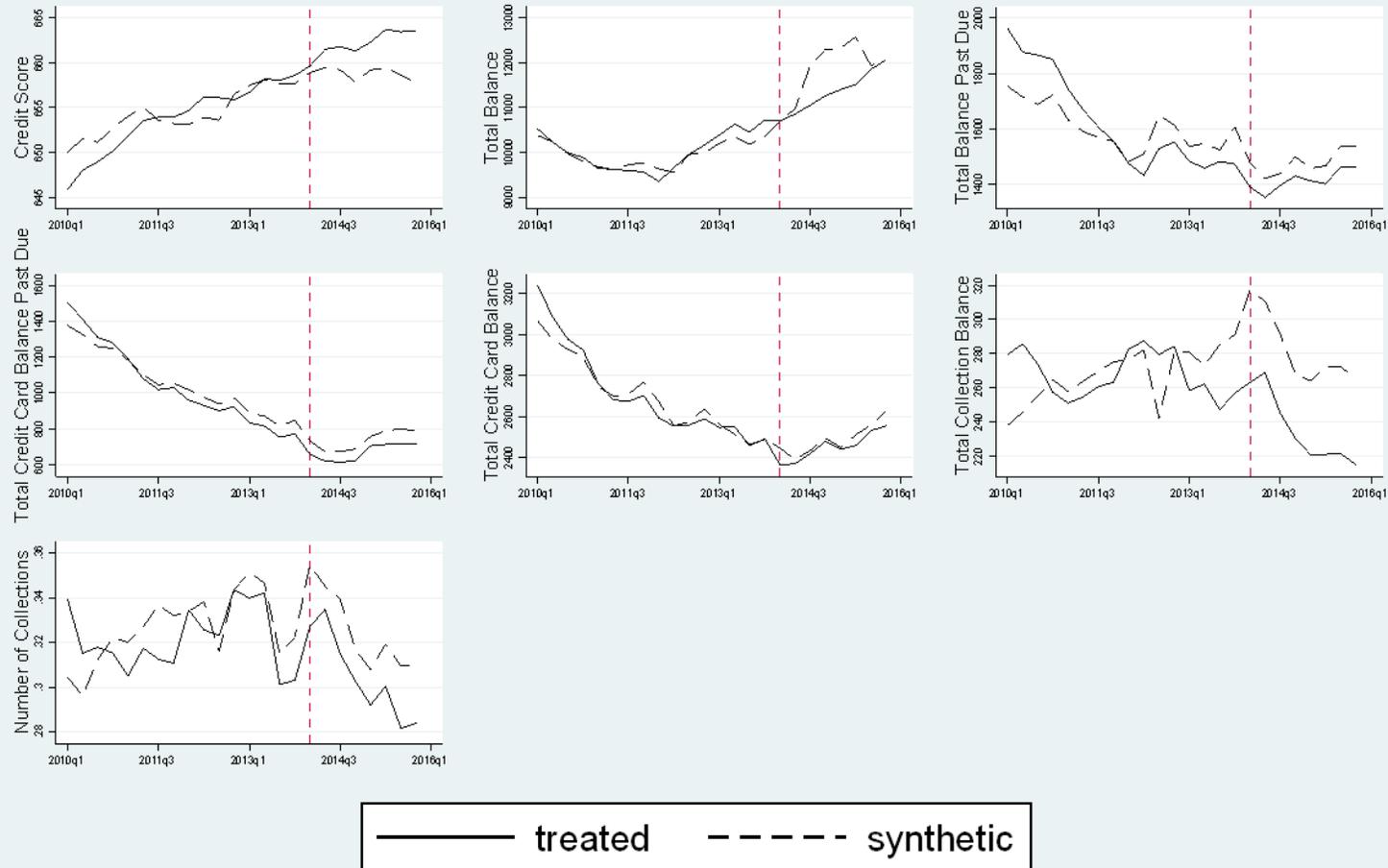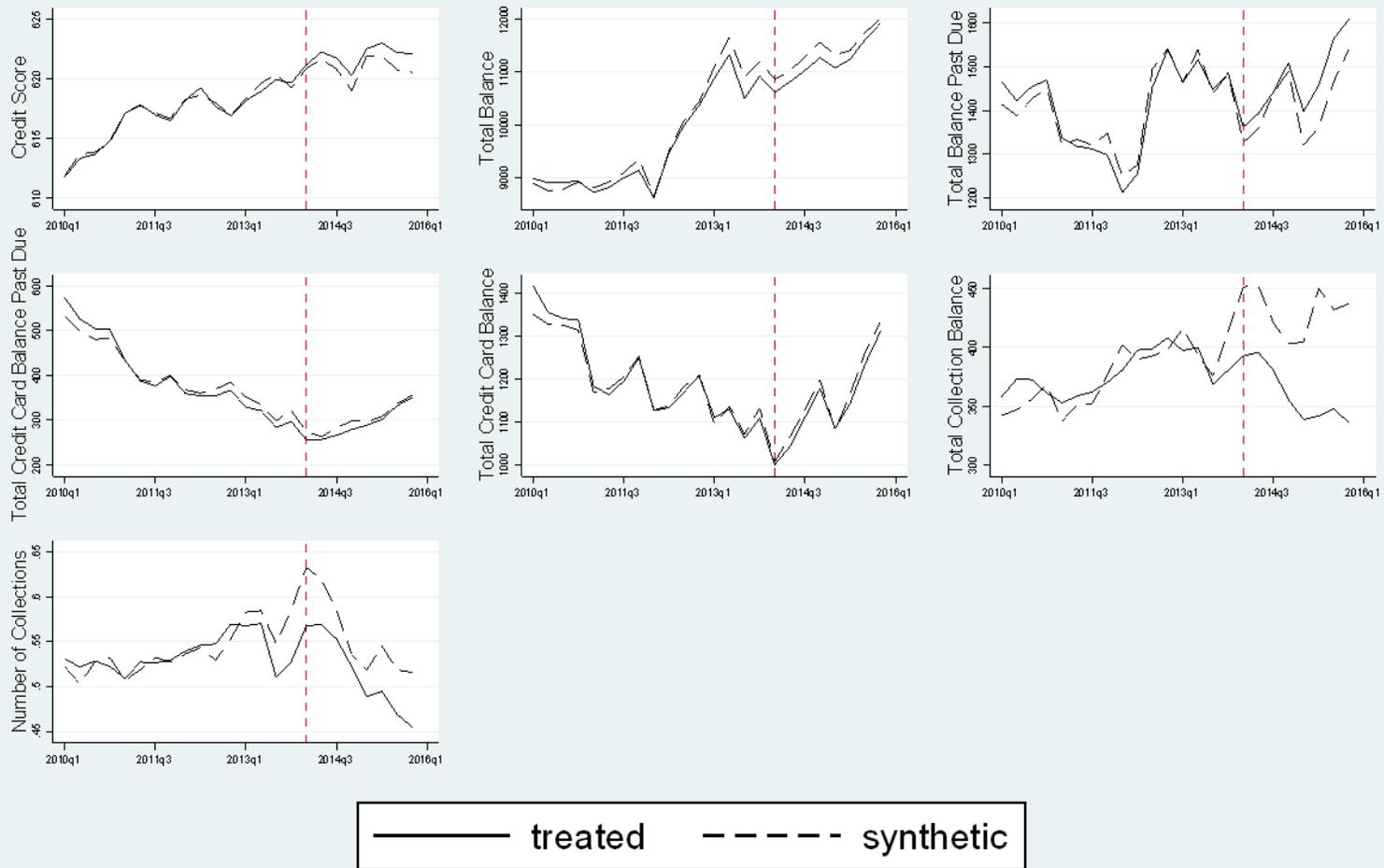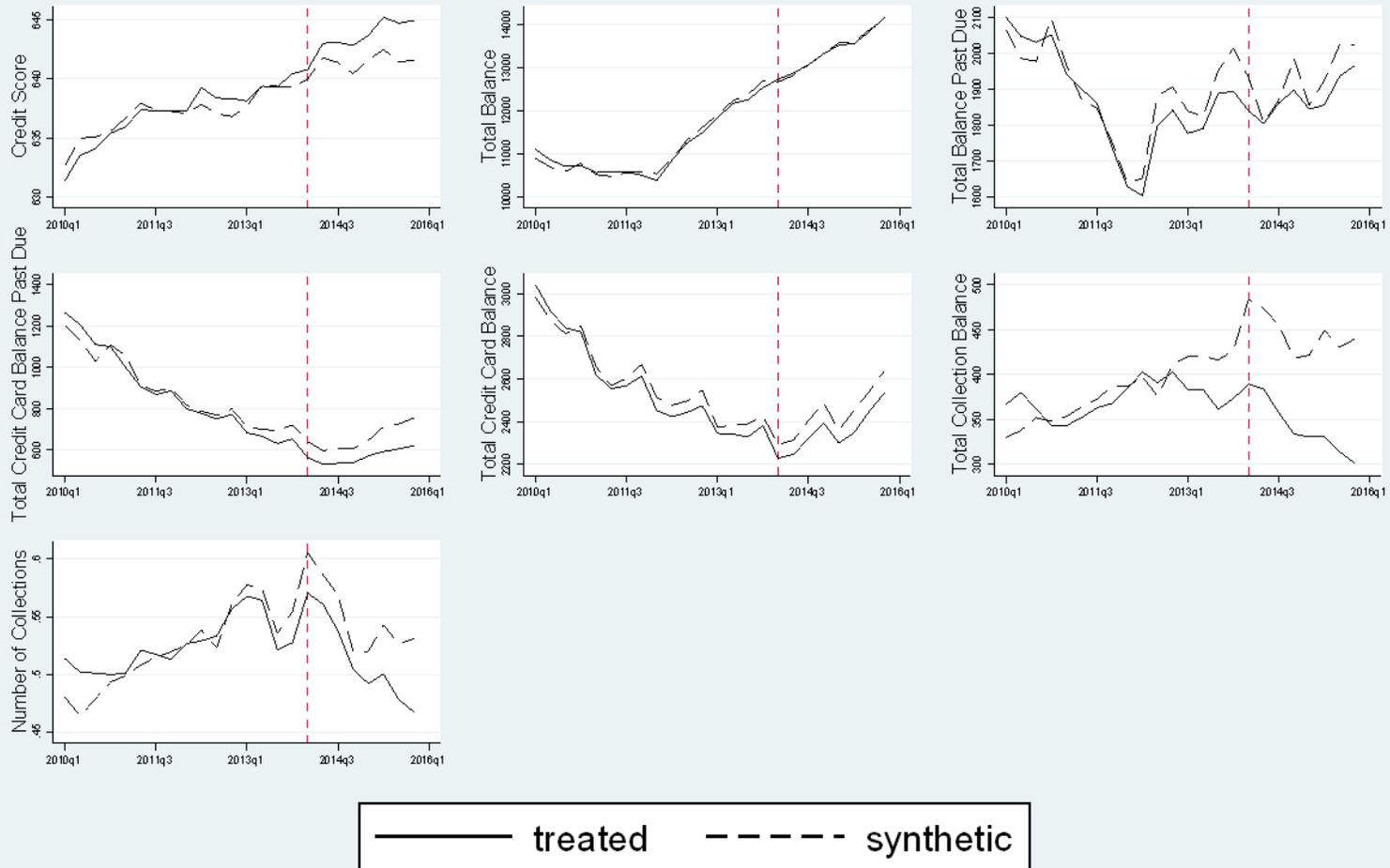
Figure 2. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Most Treated Zip Codes, Using 14 Treated States, 24 Potential Control States

Figure 3. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Most Treated Zip Codes, Using 7 Treated States, 26 Potential Control States

Figure 4. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Ages 19 to 32 in Most Treated Zip Codes Using 21 Treated States, 26 Potential Control States

DC, MA and HI are dropped (not enough observations for many credit categories)

Figure 5. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Ages 33 to 44 in Most Treated Zip Codes Using 21 Treated States, 26 Potential Control States

DC and MA are dropped (not enough observations for many credit categories)
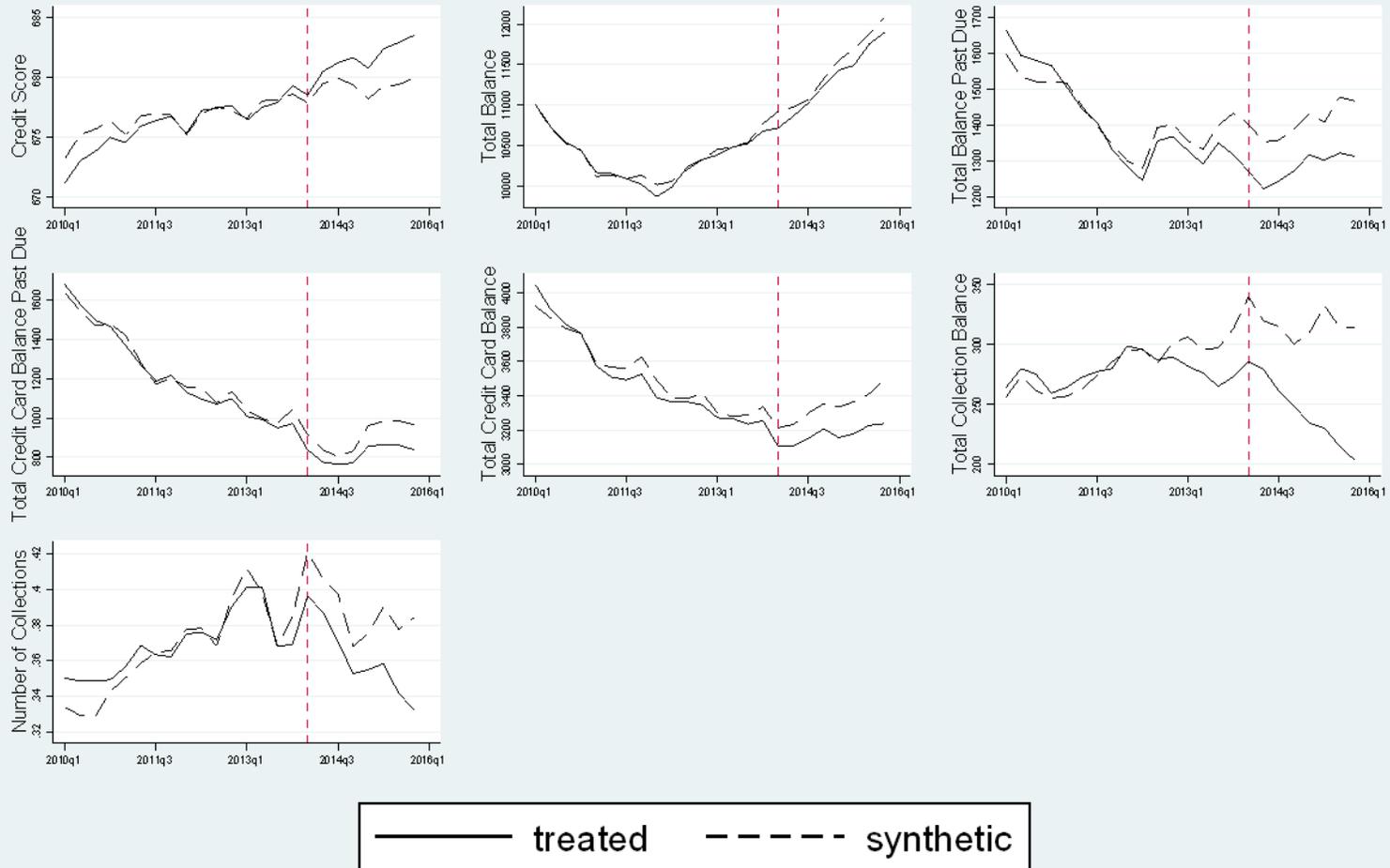
Figure 6. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Ages 45-64 in Most Treated Zip Codes Using 21 Treated States, 26 Potential Control States
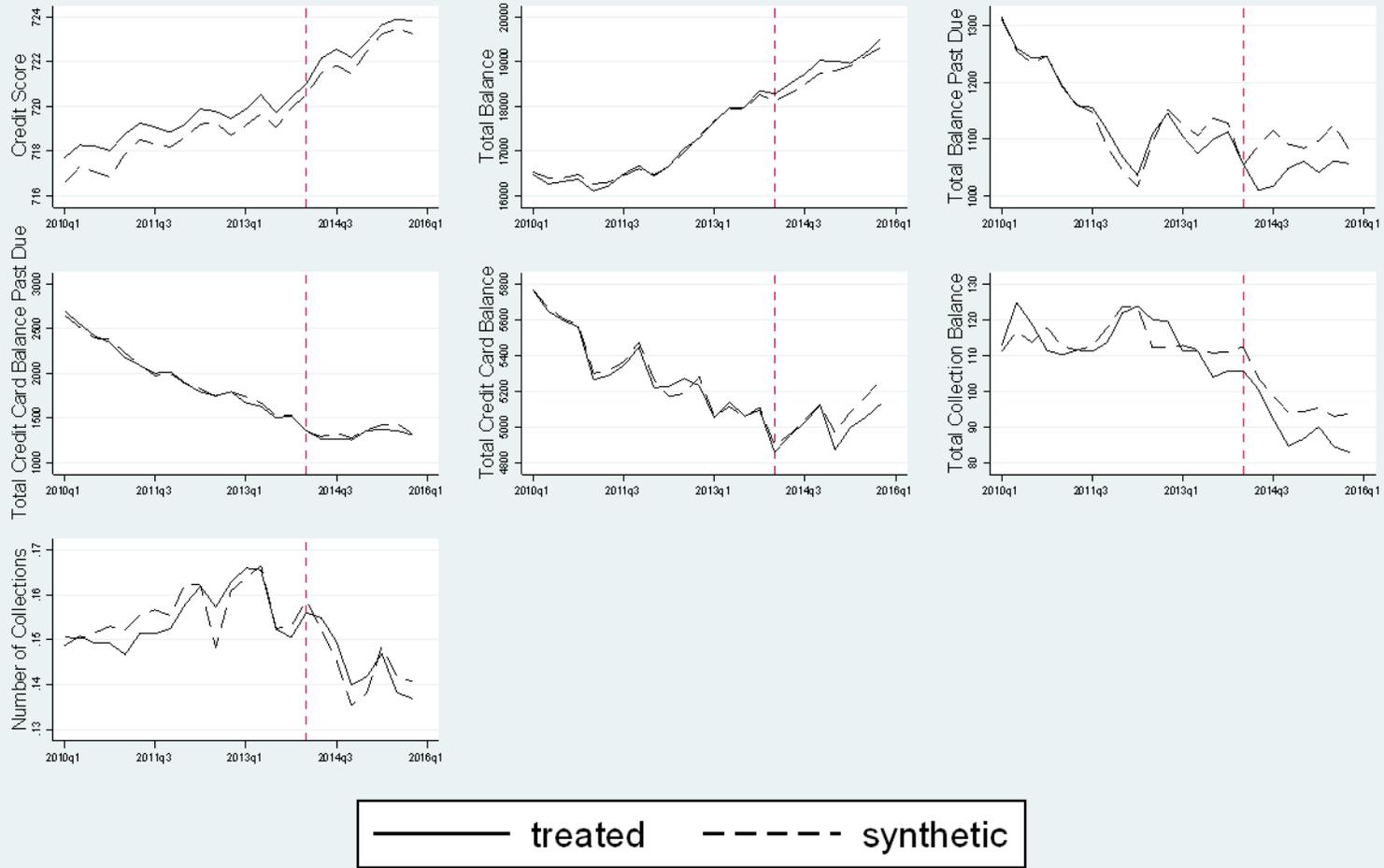DC and MA are dropped (not enough observations for many credit categories)

Figure 7. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Non-elderly in Least Treated Zip Codes Using 21 Treated States, 26 Potential Control States
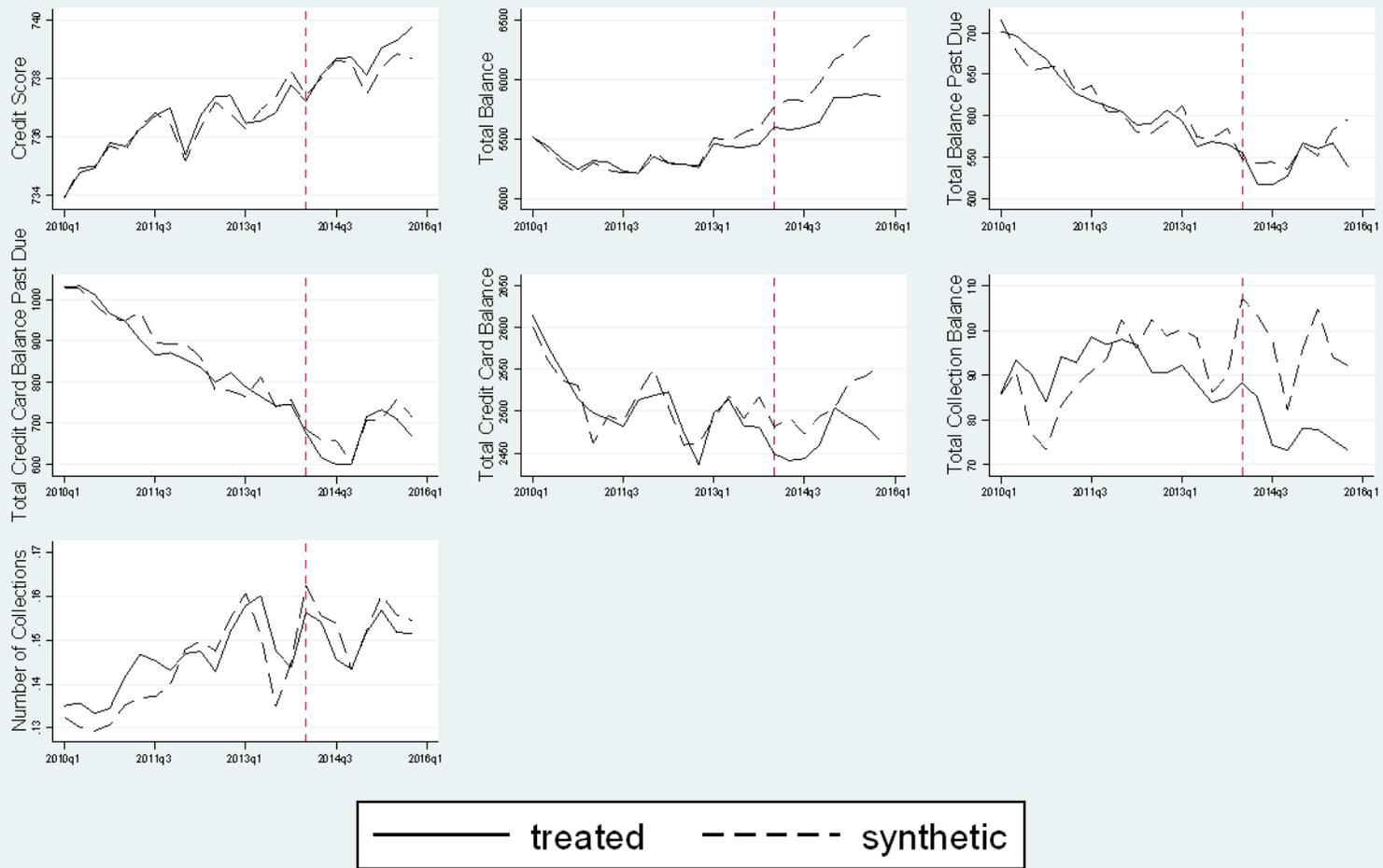
Figure 8. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Elderly in Most Treated Zip Codes Using 21 Treated States, 26 Potential Control States
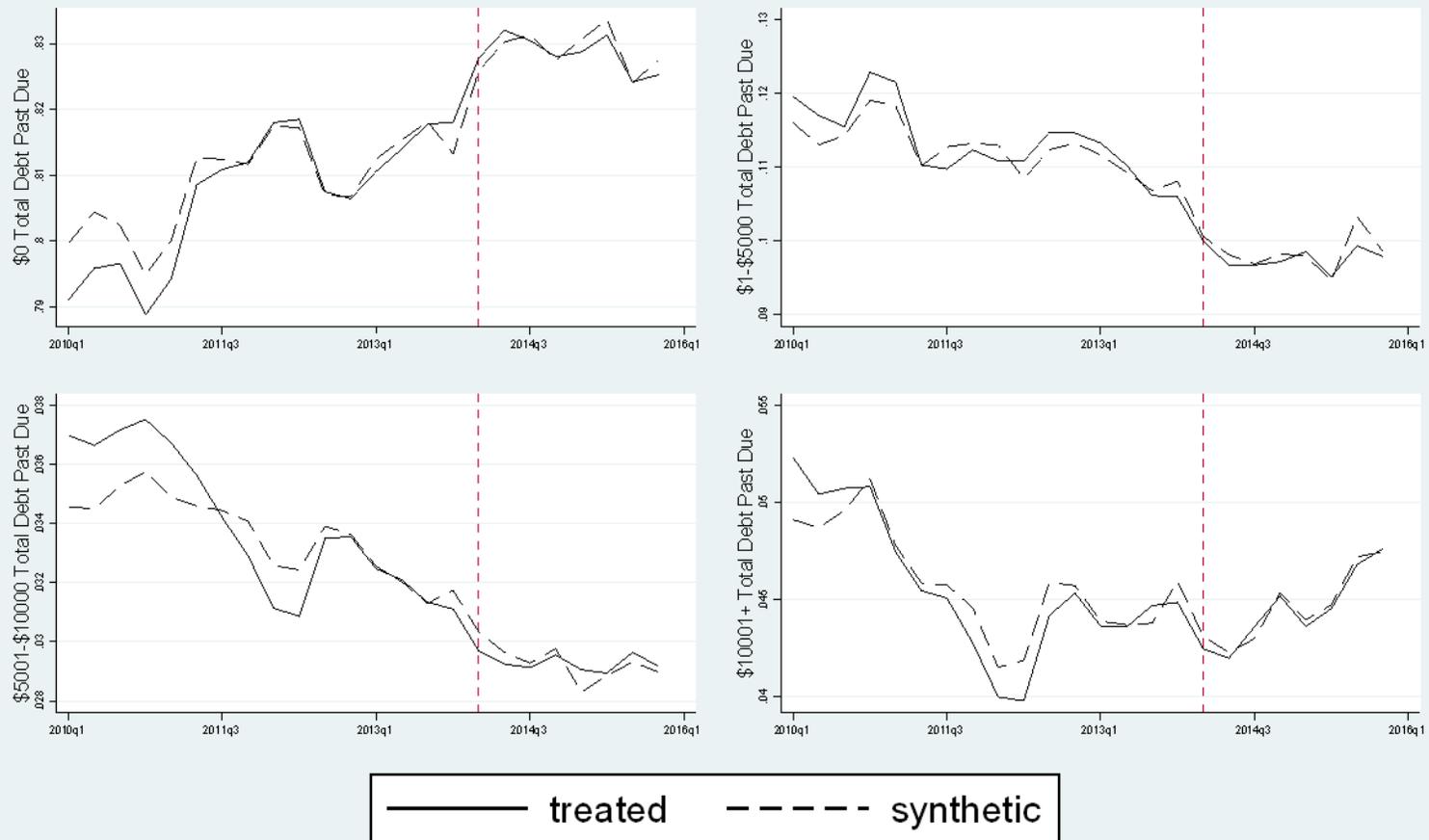DC and MA are dropped (not enough observations for many credit categories)

Figure 9a. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Total Balance Past Due Distribution Using 21 Treated States, 26 Potential Control States

Figure 9b. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Credit Card Balance Past Due Distribution Using 21 Treated States, 26 Potential Control States

Figure 9c. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Total Collection Balance Distribution Using 21 Treated States, 26 Potential Control States
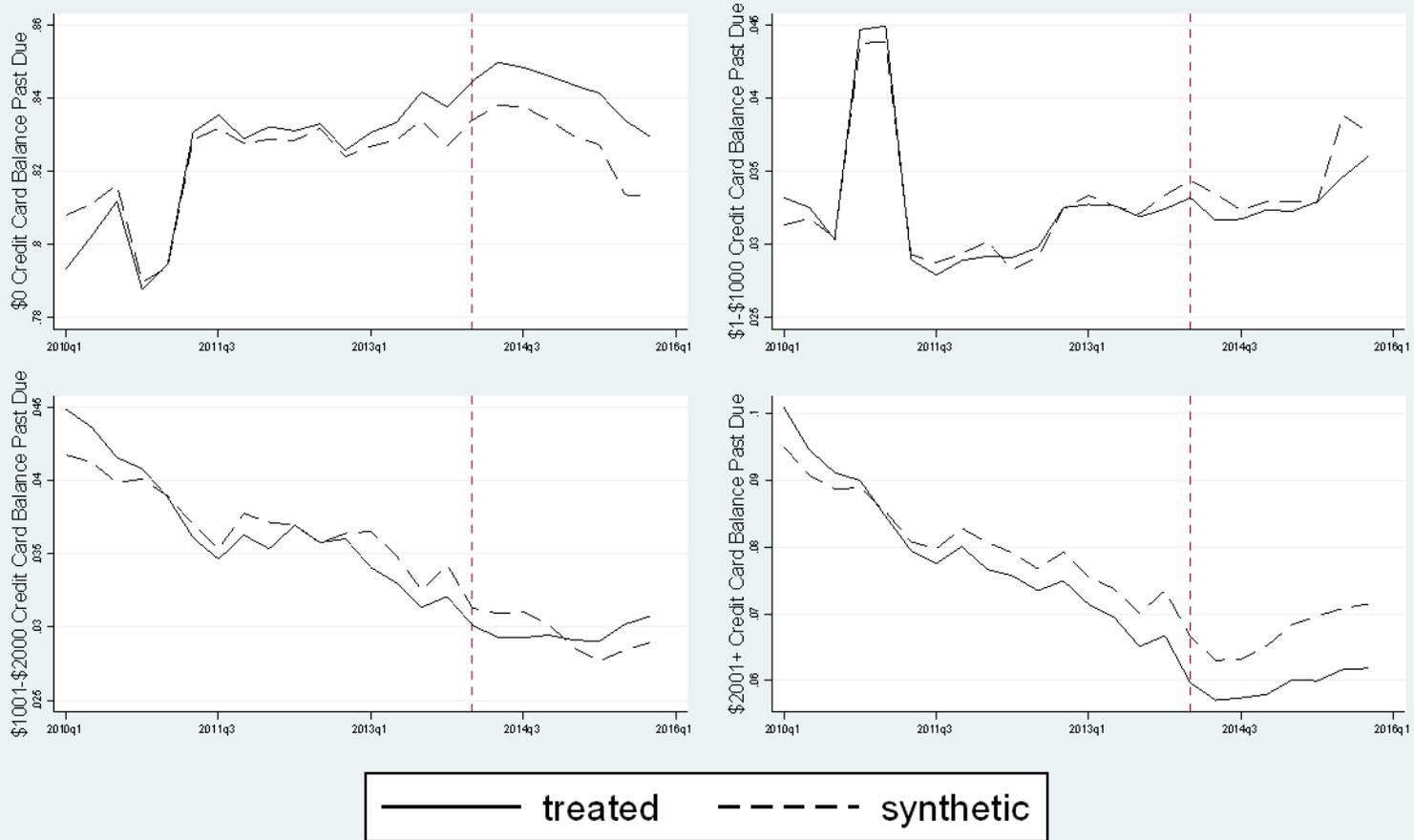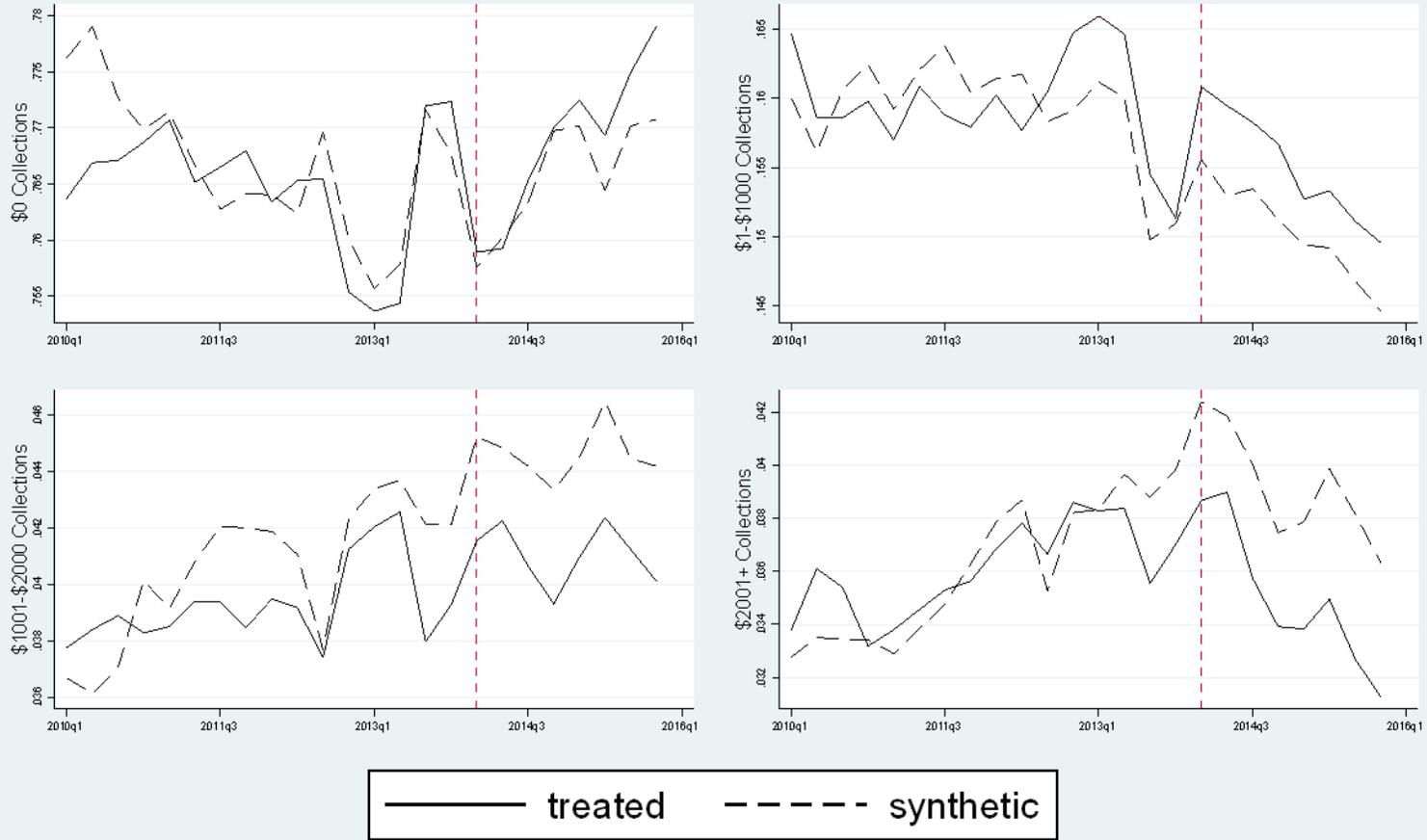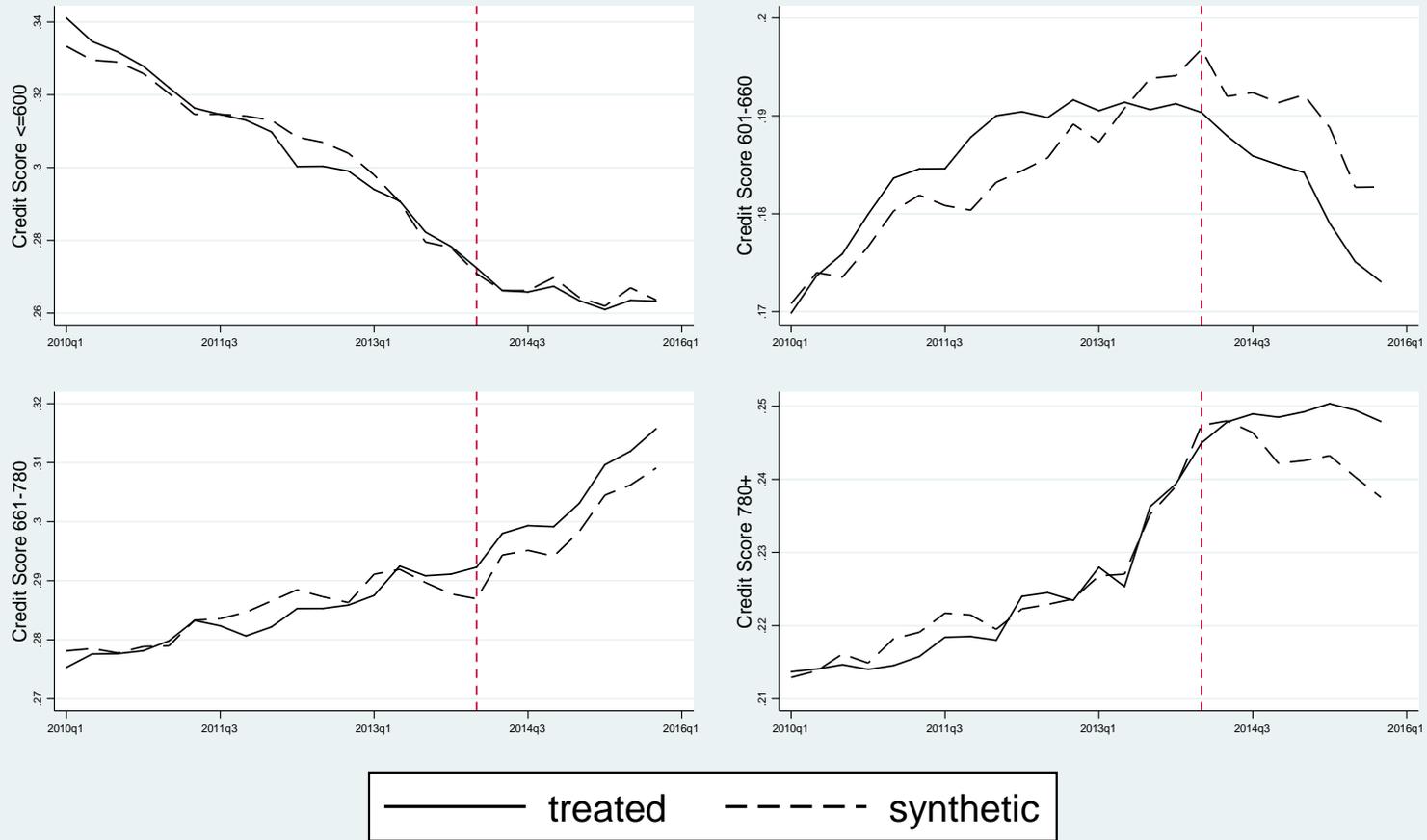
Figure 9d. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Credit Score Distribution Using 21 Treated States, 26 Potential Control States

Appendix: Additional Tables and Figures

Appendix Table 1
Classification of Treatment and Control States

A. Did not expand under ACA as of 12/31/15 (21 states)

Limited or no expansion pre-2014 (19 states)

AL, FL, GA, ID, KS, LA, MS, MO, MT, NE, NC, OK, SC, SD, TN, TX, UT, VA, WY

Broad sample:	Control
Narrow sample:	Control
Partially treated sample:	Control

Partial expansion pre-2014 (2 states)

ME, WI

Broad sample:	Control
Narrow sample:	Excluded
Partially treated sample:	Control

B. Did Expand under ACA as of 12/31/15 (30 states)

Limited or no expansion pre-2014 (14 states)

AR, CO, IL, KY, MD, MI, NJ, NV, NM, ND,OH, OR, RI, WV

Broad sample:	Treatment
Narrow sample:	Treatment
Partially treated sample:	Excluded

Partial expansion pre-2014 (7 states)

AZ, CA, CT, HI, IA, MN, WA

Broad sample:	Treatment
Narrow sample:	Excluded
Partially treated sample:	Treatment

Fully expanded pre-2014 (5 states)

DE, DC, MA, NY, VT

Broad sample:	Control
Narrow sample:	Control
Partially treated sample:	Control

Expanded between 2014:Q2 and 2015:Q4 (4 states)

AK, IN, NH, PA

Broad sample:	Excluded
Narrow sample:	Excluded
Partially treated sample:	Excluded

Appendix Table 2
State Weights for Synthetic Control for Each Dependent Variable, Most Treated Zip Codes, Ages 19-64
A. Weights Selected by Matching on Each Pre-2014 Value of Dependent Variable and Covariates

State	Credit Score	Total Debt Balance	Total Balance Past Due	Total Credit Card Balance	Credit Card Balance Past Due	Total Collections Balance	Number of Collections
Alabama	0	0	0	0	0	0	0
District Of Columbia	0.016	0.030	0	0	0.023	0.021	0.039
Florida	0.284	0	0.211	0.193	0.290	0.037	0.009
Georgia	0.085	0.007	0.226	0.199	0.380	0.267	0.211
Idaho	0	0	0.063	0	0	0.032	0
Kansas	0	0	0	0	0	0	0
Louisiana	0	0	0	0	0	0	0
Maine	0	0	0.005	0.028	0	0	0.054
Massachusetts	0	0.051	0.025	0.129	0	0.083	0.051
Mississippi	0	0	0	0	0.207	0	0
Missouri	0	0	0	0	0	0	0
Montana	0	0	0	0	0	0	0
Nebraska	0	0	0	0	0	0	0
New York	0.371	0.275	0.463	0.214	0.1	0.273	0.371
North Carolina	0.244	0.598	0.006	0.237	0	0.261	0.266
Oklahoma	0	0	0	0	0	0	0
South Carolina	0	0	0	0	0	0	0
South Dakota	0	0	0	0	0	0	0
Tennessee	0	0	0	0	0	0	0
Texas	0	0	0	0	0	0	0
Utah	0	0	0	0	0	0	0
Vermont	0	0	0	0	0	0	0
Virginia	0	0.039	0	0	0	0	0
Wisconsin	0	0	0	0	0	0.001	0
Wyoming	0	0	0	0	0	0.025	0

B. Weights Selected by Matching on Average Pre-2014 Value of Dependent Variable, 2013 Value of Dependent Variable and Covariates

State	Credit Score	Total Debt Balance	Total Balance Past Due	Total Credit Card Balance	Credit Card Balance Past Due	Total Collections Balance	Number of Collections
Alabama	0	0	0	0	0	0	0
District Of Columbia	0	0.028	0.041	0.061	0.008	0.11	0.068
Florida	0	0.777	0.381	0.563	0	0	0.203
Georgia	0.339	0	0	0	0	0	0.035
Idaho	0	0	0	0	0	0	0
Kansas	0	0	0	0	0	0	0
Louisiana	0	0	0	0	0	0	0
Maine	0	0	0	0	0	0	0.034
Massachusetts	0	0.093	0.152	0.094	0.119	0.191	0.087
Mississippi	0	0	0	0	0	0	0
Missouri	0	0	0	0	0	0	0
Montana	0	0	0	0	0	0	0
Nebraska	0	0	0	0	0	0	0
New York	0.451	0	0	0	0.22	0.037	0.306
North Carolina	0.14	0.099	0.426	0.212	0.654	0.662	0
Oklahoma	0	0	0	0	0	0	0
South Carolina	0	0	0	0.069	0	0	0.266
South Dakota	0	0	0	0	0	0	0
Tennessee	0	0	0	0	0	0	0
Texas	0	0	0	0	0	0	0
Utah	0	0	0	0	0	0	0
Vermont	0	0.003	0	0	0	0	0
Virginia	0	0	0	0	0	0	0
Wisconsin	0.07	0	0	0	0	0	0
Wyoming	0	0	0	0	0	0	0

Synthetic Control Estimates of the Effect of Medicaid Expansions on Health Insurance Coverage for Most Treated PUMAs, Ages 19-64

	Post-2014 Difference in Means Between Treatment States minus Synthetic Control					
	21 Treatment States, 26 Potential Control States			14 Treatment States, 24 Potential Control States		
Outcome	(1) Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	(2) Weights: Match on All Values of Dep. Variable	(3) Weights: Match on Average Value of Dep. Variable	(4) Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	(5) Weights: Match on All Values of Dep. Variable	(6) Weights: Match on Average Value of Dep. Variable
Medicaid	0.152	0.057***	0.053**	0.152	0.055**	0.054**
	(0.008)	(<0.001)	(0.001)	(0.005)	(0.005)	(0.001)
Uninsured	0.305	-0.051***	-0.047***	0.281	-0.051***	-0.047**
	(0.028)	(<0.001)	(<0.001)	(0.024)	(<0.001)	(0.001)
Private	0.527	-0.007	0.001	0.546	-0.011	-0.015
	(0.035)	(0.429)	(0.899)	(0.026)	(0.230)	(0.146)

Appendix Table 3 reports the synthetic control estimates of the effect of Medicaid expansions on health insurance coverage. Data from 2010-2015 American Community Survey. Column (2)-(3) and (5)-(6) reports the estimates of the post-2014 differences in indicators for health insurance coverage between treated and synthetic control states for all residents age 19-64 in the most treated PUMAs. Column (2)-(3) present the results for broad sample with 21 treatment states and 26 potential control states. Column (5)-(6) present the results for the narrow sample with 14 treatment states and 24 potential control states. For each expansionary definition, results from two weighting methods are presented. State-level covariates for both methods include the average pre-reform values of simulated Medicaid Eligibility, percent Hispanic, percent black, percent high school degree or less, and percent uninsured and < 138% of FPL, and all pre-reform values of unemployment rate, poverty rate, 25% and 75% percentile of log wage. In all results, AK, IN, NH, and PA are dropped. Significance levels: * = 10%, ** = 5%, *** = 1%.

Appendix Table 4

Synthetic Control Estimate of the Effect of Medicaid Expansions on Health Insurance Coverage for Most Treated PUMAs, by Age Group

Post-2014 Difference in Means Between Treatment States minus Synthetic Control

Outcome	Ages 19-32			Ages 33-44			Ages 45-64		
	(1) Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	(2) Weights: Match on All Values of Dep. Variable	(3) Weights: Match on Average Value of Dep. Variable	(4) Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	(5) Weights: Match on All Values of Dep. Variable	(6) Weights: Match on Average Value of Dep. Variable	(7) Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	(8) Weights: Match on All Values of Dep. Variable	(9) Weights: Match on Average Value of Dep. Variable
Panel A. 21 Treatment States, 26 Potential Control States									
Medicaid	0.169	0.071***	0.057**	0.148	0.050*	0.046**	0.139	0.057**	0.052***
	(0.009)	(<0.001)	(0.001)	(0.007)	(0.016)	(0.003)	(0.008)	(0.004)	(<0.001)
Uninsured	0.363	-0.058***	-0.051***	0.326	-0.048**	-0.052***	0.238	-0.040**	-0.030*
	(0.038)	(<0.001)	(<0.001)	(0.030)	(0.003)	(<0.001)	(0.017)	(0.001)	(0.016)
Private	0.462	-0.013	0.006	0.523	0.005	0.014	0.589	-0.009	-0.020
	(0.045)	(0.155)	(0.521)	(0.034)	(0.573)	(0.161)	(0.024)	(0.441)	(0.082)
Panel B. 14 Treatment States, 24 Potential Control States									
Medicaid	0.171	0.067**	0.059**	0.149	0.057*	0.058**	0.137	0.056**	0.052**
	(0.004)	(0.001)	(0.003)	(0.008)	(0.015)	(0.002)	(0.007)	(0.004)	(0.001)
Uninsured	0.346	-0.051**	-0.050**	0.304	-0.048**	-0.047**	0.213	-0.036*	-0.034**
	(0.036)	(0.001)	(0.001)	(0.025)	(0.002)	(0.002)	(0.014)	(0.017)	(0.009)
Private	0.481	-0.012	-0.015	0.540	-0.009	-0.018	0.604	-0.013	-0.015
	(0.040)	(0.246)	(0.197)	(0.025)	(0.405)	(0.114)	(0.016)	(0.388)	(0.276)

Appendix Table 4 reports the synthetic control estimates of the effect of Medicaid expansions on health insurance coverage by age group. Data from 2010-2015 American Community Survey. Column (2)-(3), (5)-(6), and (8) and (9) reports the estimates of the post-2014 differences in indicators for health insurance coverage between treated and synthetic control states for residents of the age range in the most treated PUMAs. Panel A presents the results for broad sample with 21 treatment states and 26 potential control states. Panel B presents the results for the narrow sample with 14 treatment states and 24 potential control states. For each expansionary definition, results from two weighting methods are presented. State-level covariates for both methods include the average pre-reform values of simulated Medicaid Eligibility, percent Hispanic, percent black, percent high school degree or less, and percent uninsured and < 138% of FPL, and all pre-reform values of unemployment rate, poverty rate, 25% and 75% percentile of log wage. In all results, AK, IN, NH, and PA are dropped. Significance levels: * = 10%, ** = 5%, *** = 1%.

	Post-2014 Difference in Means Between Treatment States minus Synthetic Control								
	21 Treatment States, 26 Potential Control States			14 Treatment States, 24 Potential Control States			7 Treatment States, 26 Potential Control States		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable
Credit Score	648	2*	2*	643	-0	0	654	5**	2
	(13)	(0.087)	(0.065)	(15)	(0.673)	(0.791)	(8)	(0.020)	(0.186)
Total Balance	10614	407***	**373***	10910	241	129	10298	469*	397
	(1336)	(0.010)	**(0.007)**	(1664)	(0.285)	(0.542)	(740)	(0.070)	(0.111)
Total Balance Past Due	1584	-21	42	1499	39	26	1674	-115*	-117
	(286)	(0.606)	(0.352)	(257)	(0.435)	(0.681)	(289)	(0.081)	(0.138)
Total Credit Card Balance	2629	27	**113***	2501	-13	-7	2765	11	56
	(404)	(0.465)	**(0.005)**	(435)	(0.769)	(0.869)	(316)	(0.849)	(0.352)
Total Credit Card Balance Past Due	883	-26	-57*	702	-26	-34	1076	-35	-62
	(329)	(0.360)	(0.091)	(256)	(0.364)	(0.281)	(287)	(0.488)	(0.191)
Total Collections Balance	336	**-68***	**-58***	398	-54**	-53**	271	-43	-59
	(105)	**(0.000)**	**(0.003)**	(108)	(0.034)	(0.024)	(46)	(0.268)	(0.112)
Number of Collections	0.466	-0.030	-0.039	0.596	-0.037	-0.031	0.326	-0.034	-0.014
	(0.178)	(0.168)	(0.110)	(0.142)	(0.112)	(0.217)	(0.078)	(0.294)	(0.744)

Appendix Table 5 reports the estimates of the post-2014 differences in financial indicators between treated and synthetic control states for non-elderly adults in the most treated zip codes. In this analysis, zip code is fixed for an individual at its 2013Q1 value. Columns (1) - (3) present the results for the broad sample with 21 treatment states and 26 potential control states. Columns (4) - (6) present the results for the narrow sample with 14 treatment states and 24 potential control states. Columns (7) - (9) present the results for 7 treatment states and 26 potential control states, with the 7 treated states being those included in the broad treatment group but not the narrow one. For each expansionary definition, we present the 2010-2013 pre-reform mean outcome for the treated states, and the average post-reform quarterly difference between the treated states and their synthetic counterpart using the two different weighting methods used to construct the synthetic control group. In all results, AK, IN, NH, and PA are dropped. Bolded results are also significant at the 5% level when the Holm-Bonferroni correction using 7 outcomes is applied to assess absolute gap p-values. Significance levels: * = 10%, ** = 5%, *** = 1%.

Synthetic Control Estimates of the Effect of Medicaid on Indicators of Financial Wellbeing for Most Treated Zip Codes, Ages 19-64, Late Expanders

	Post 2015 Difference in Means Between Treatment States minus Synthetic Control					
	4 Treated States			2 Treated States		
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Weighted Means & SDs	Weights: Match on All Values of Dep. Variable	Weights: Match on All Values of Dep. Variable	Weighted Means & SDs	Weights: Match on All Values of Dep. Variable	Weights: Match on All Values of Dep. Variable
Credit Score	639	-2	-2	637	-1	-3
	(13)	(0.176)	(0.136)	(10)	(0.407)	(0.146)
Total Balance	10390	-46	-206	10350	-41	-225
	(1262)	(0.775)	(0.374)	(1208)	(0.886)	(0.396)
Total Balance Past Due	1479	125**	126**	1502	133*	132*
	(194)	(0.018)	(0.032)	(154)	(0.078)	(0.062)
Total Credit Card Balance	2248	-6	-19	2174	-5	-20
	(476)	(0.873)	(0.649)	(286)	(0.946)	(0.781)
Total Credit Card Balance Past Due	526	9	-2	510	-2	-3
	(143)	(0.704)	(0.935)	(108)	(0.929)	(0.854)
Total Collections Balance	395	-4	-5	404	-5	-6
	(85)	(0.893)	(0.876)	(75)	(0.941)	(0.851)
Number of Collections	0.590	-0.014	-0.013	0.603	-0.015	-0.013
	(0.138)	(0.626)	(0.752)	(0.123)	(0.645)	(0.794)

Appendix Table 6 reports the estimates of post-2015 differences in financial indicators between treated and synthetic control states for non-elderly adults in the most treated zip codes for late expanders. Columns (1) - (3) mark AK, IN, NH, and PA as our treated states and treat 2015Q1 as the quarter of expansion. Column (2) presents the results for 4 treatment states and 26 potential control states, to compare late expanders to the same control states as in our broad treatment group. Column (3) presents the results for 4 treatment states and 24 potential control states, to compare late expanders to the same control states as in our narrow treatment group. Columns (4) - (6) mark IN and PA as our treated states and treat 2015Q1 as the quarter of expansion. Column (4) presents the results for 2 treatment states and 26 potential control states, to compare late expanders to the same control states as in our broad treatment group. Column (6) presents the results for 2 treatment states and 24 potential control states, to compare late expanders to the same control states as in our narrow treatment group. For each expansionary definition, we present the 2010-2014 pre-reform mean outcome for the treated states, and the average post-reform quarterly difference between the treated states and their synthetic counterpart. Bolded results are also significant at the 5% level when the Holm-Bonferroni correction using 7 outcomes is applied to assess absolute gap p-values. Significance levels: * = 10%, ** = 5%, *** = 1%.

Synthetic Control Estimates of the Effect of Medicaid on Indicators of Financial Wellbeing by Year, Ages 19-64

A. 2014 Effects of Medicaid

	2014 Difference in Means Between Treatment States minus Synthetic Control					
	21 Treatment States, 26 Potential Control States		14 Treatment States, 24 Potential Control States		7 Treatment States, 26 Potential Control States	
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable
Credit Score	0	2*	-1	0	2	2
	(0.925)	(0.053)	(0.193)	(0.959)	(0.179)	(0.217)
Total Balance	-224	**-786***	-420**	-530**	-509	-726**
	(0.143)	**(0.000)**	(0.030)	(0.018)	(0.102)	(0.048)
Total Balance Past Due	24	39	40	44	-67	-50
	(0.487)	(0.359)	(0.354)	(0.331)	(0.267)	(0.538)
Total Credit Card Balance	-23	**-69***	-56*	-1	-34	-51
	(0.339)	**(0.007)**	(0.056)	(0.958)	(0.409)	(0.234)
Total Credit Card Balance Past Due	-20	-19	12	-0	-63	-46
	(0.398)	(0.493)	(0.485)	(0.986)	(0.155)	(0.282)
Total Collections Balance	**-52***	**-65***	**-68***	**-54***	-45	-57
	(0.003)	**(0.006)**	**(0.000)**	**(0.005)**	(0.147)	(0.118)
Number of Collections	-0.036**	-0.019	-0.027	-0.024	-0.019	-0.016
	(0.033)	(0.313)	(0.189)	(0.285)	(0.508)	(0.661)

Appendix Table 7 reports the estimates of the post-2014 differences in financial indicators between treated and synthetic control states for the most treated zip codes by year. Panel A reports the results for differences in 2014, and Panel B reports the results for differences in 2015. Columns (1) - (2) present the results for the broad sample with 21 treatment states and 26 potential control states. Columns (3) - (4) present the results for the narrow sample with 14 treatment states and 24 potential control states. Columns (5) - (6) present the results for 7 treatment states and 26 potential control states, with the 7 treated states being those included in the broad treatment group but not the narrow one. For each expansionary definition, we present the average post-reform quarterly difference between the treated states and their synthetic counterpart for 2014 and 2015, using the two different weighting methods used to construct the synthetic control group. In all results, AK, IN, NH, and PA are dropped. Bolded results are also significant at the 5% level when the Holm-Bonferroni correction using 7 outcomes is applied to assess absolute gap p-values. Significance levels: * = 10%, ** = 5%, *** = 1%.

Synthetic Control Estimates of the Effect of Medicaid on Indicators of Financial Wellbeing by Year, Ages 19-64

B. 2015 Effects of Medicaid

	2015 Difference in Means Between Treatment States minus Synthetic Control					
	21 Treatment States, 26 Potential Control States		14 Treatment States, 24 Potential Control States		7 Treatment States, 26 Potential Control States	
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable
Credit Score	1	2**	-1	-0	5*	4
	(0.339)	(0.029)	(0.320)	(0.989)	(0.070)	(0.110)
Total Balance	-218	**-876***	-462*	-550**	-519	-837**
	(0.366)	**(0.000)**	(0.090)	(0.034)	(0.214)	(0.040)
Total Balance Past Due	34	74	38	35	-65	-32
	(0.513)	(0.205)	(0.547)	(0.570)	(0.325)	(0.714)
Total Credit Card Balance	-28	-117***	-66	12	-40	-103
	(0.474)	(0.010)	(0.178)	(0.747)	(0.541)	(0.163)
Total Credit Card Balance Past Due	-29	-59*	-17	-38	-70	-94**
	(0.360)	(0.063)	(0.560)	(0.257)	(0.188)	(0.036)
Total Collections Balance	**-77***	-68**	**-109***	-68**	-49	-56
	(0.004)	(0.014)	**(0.001)**	(0.048)	(0.294)	(0.220)
Number of Collections	-0.054**	-0.046	-0.049	-0.050	-0.022	-0.026
	(0.046)	(0.127)	(0.112)	(0.150)	(0.557)	(0.566)

Appendix Table 7 reports the estimates of the post-2014 differences in financial indicators between treated and synthetic control states for the most treated zip codes by year. Panel A reports the results for differences in 2014, and Panel B reports the results for differences in 2015. Columns (1) - (2) present the results for the broad sample with 21 treatment states and 26 potential control states. Columns (3) - (4) present the results for the narrow sample with 14 treatment states and 24 potential control states. Columns (5) - (6) present the results for 7 treatment states and 26 potential control states, with the 7 treated states being those included in the broad treatment group but not the narrow one. For each expansionary definition, we present the average post-reform quarterly difference between the treated states and their synthetic counterpart for 2014 and 2015, using the two different weighting methods used to construct the synthetic control group. In all results, AK, IN, NH, and PA are dropped. Bolded results are also significant at the 5% level when the Holm-Bonferroni correction using 7 outcomes is applied to assess absolute gap p-values. Significance levels: * = 10%, ** = 5%, *** = 1%.

Synthetic Control Estimates of the Effect of Medicaid on Indicators of Financial Wellbeing for Most Treated Zip Codes, Ages 19-64

	Post-2014 Difference in Means Between Treatment States minus Synthetic Control								
	21 Treatment States, 26 Potential Control States			14 Treatment States, 24 Potential Control States			7 Treatment States, 26 Potential Control States		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable
Credit Score	648	1	2	643	-1	0	654	3	3
	(13)	(0.548)	(0.143)	(15)	(0.554)	(0.996)	(8)	(0.413)	(0.305)
Total Balance	10341	-221	-831	10636	-441	-540	10026	-514	-781
	(1278)	(0.590)	(0.278)	(1613)	(0.382)	(0.149)	(639)	(0.398)	(0.154)
Total Balance Past Due	1537	29	56	1451	39	39	1629	-66	-41
	(271)	(0.927)	(0.698)	(240)	(0.792)	(0.716)	(273)	(0.910)	(0.939)
Total Credit Card Balance	2580	-26	-93	2455	-61	6	2714	-37	-77
	(397)	(0.518)	(0.118)	(434)	(0.174)	(0.709)	(303)	(0.743)	(0.166)
Total Credit Card Balance Past Due	855	-24	-39	676	-3	-19	1047	-66	-70
	(315)	(0.703)	(0.817)	(238)	(0.404)	(0.498)	(271)	(0.578)	(0.695)
Total Collections Balance	333	-65	-66	394	-88*	-61	268	-47	-57
	(105)	(0.391)	(0.304)	(107)	(0.096)	(0.256)	(46)	(0.686)	(0.286)
Number of Collections	0.461	-0.045	-0.033	0.591	-0.038	-0.037	0.322	-0.021	-0.021
	(0.177)	(0.617)	(0.476)	(0.141)	(0.419)	(0.616)	(0.076)	(0.695)	(0.964)

Appendix Table 8 reports the estimates of the post-2014 differences in financial indicators between treated and synthetic control states for non-elderly adults in the most treated zip codes. Columns (1) - (3) present the results for the broad sample with 21 treatment states and 26 potential control states. Columns (4) - (6) present the results for the narrow sample with 14 treatment states and 24 potential control states. Columns (7) - (9) present the results for 7 treatment states and 26 potential control states, with the 7 treated states being those included in the broad treatment group but not the narrow. For each expansionary definition, we present the 2010-2013 pre-reform mean outcome for the treated states, and the average post-reform quarterly difference between the treated states and their synthetic counterpart using the two different weighting methods used to construct the synthetic control group. In all results, AK, IN, NH, and PA are dropped. Bolded results are also significant at the 5% level when the Holm-Bonferroni correction using 7 outcomes is applied to assess post/pre-reform MSPE ratio p-values. Significance levels: * = 10%, ** = 5%, *** = 1%

Appendix Table 9

Difference-in-Differences Estimates for Effect on Medicaid on Indicators of Financial Wellbeing for Most Treated
Zip Codes, Ages 19-64

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Credit Score	Total Balance	Total Balance Past Due	Total Credit Card Balance	Total Credit Card Balance Past Due	Total Collection Balance	Number of Collections
A. 21 Treatment States, 26 Potential Control States							
Treated # Post	2.108	-167.6	9.123	16.20	30.07	-63.19***	-0.0498***
	(1.419)	(127.8)	(75.48)	(36.75)	(54.04)	(17.69)	(0.0168)
Observations	3,273	3,273	3,273	3,273	3,224	3,273	3,273
Y Mean	642.2	11894	1646	2510	750.2	428.3	0.612
Y SD	29.58	2058	411.5	1064	469.3	152.5	0.232
B. 14 Treatment States, 24 Potential Controls							
Treated # Post	0.354	-129.7	93.37	4.279	61.28	-67.98***	-0.0515**
	(1.396)	(144.7)	(76.09)	(42.52)	(56.79)	(19.66)	(0.0196)
Observations	2,625	2,625	2,625	2,625	2,581	2,625	2,625
Y Mean	638.7	12242	1670	2483	704.2	468.3	0.681
Y SD	29.32	2066	422.2	1080	450.5	138.3	0.197
C. 7 Treatment States, 26 Potential Controls							
Treated # Post	5.315***	-259.6	-122.4	48.69	-11.94	-60.61**	-0.0538**
	(1.757)	(168.7)	(97.37)	(42.62)	(68.39)	(23.06)	(0.0207)
Observations	2,265	2,265	2,265	2,265	2,216	2,265	2,265
Y Mean	641.8	12070	1685	2541	776.9	438.9	0.618
Y SD	29.25	1997	412.1	1068	488.5	155	0.241

Appendix Table 9 reports the difference-in-differences estimates for the non-elderly adults in the most treated zip codes for the broad, narrow and partially treated samples. Each column of each panel corresponds to a different regression with the corresponding dependent variable in the first row. The DiD estimates are reported. Treated is an indicator variable that takes on the value of 1 if the zip code is considered part of an expansionary state and 0 otherwise. Post is an indicator that takes on the value of 1 if the observation is post 2014 and 0 otherwise. Observations refers to the number of zip code level observations included in the regression. Y Mean and Y SD refers to the mean and standard deviation, respectively, of the dependent variable in the regression. All specifications include year fixed effects, age group fixed effects and the following controls: yearly state unemployment rate, yearly state unemployment rate squared, yearly state poverty rate (where 2014 and 2015 data are imputed using 2013 levels), yearly state 25th and 75th percentile of log wage (where 2014 and 2015 data are imputed using 2013 levels). Robust standard errors clustered at the state level are in parentheses. Significance levels: * = 10%, ** = 5%, *** = 1%.

Appendix Table 10

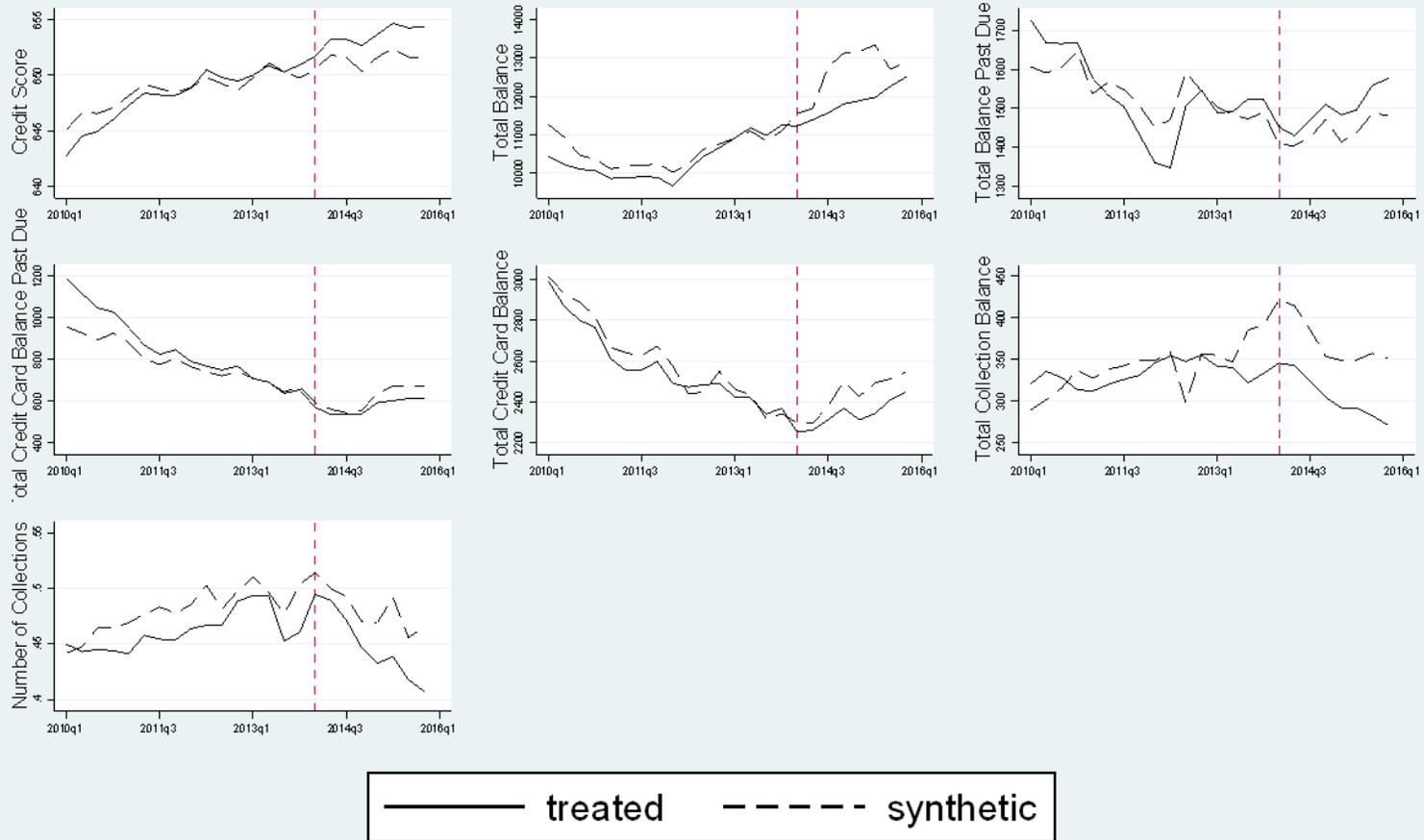Event Study Results for Effect of Medicaid on Indicators of Financial Wellbeing for Most Treated Zip Codes, Ages 19-64

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Credit Score	Total Balance	Total Balance Past Due	Total Credit Card Balance	Total Credit Card Balance Past Due	Total Collection Balance	Number of Collections
A. 21 Treatment States, 26 Potential Control States							
Treated # 2011	1.263	-65.93	-56.44	-45.35	-51.11	-22.96*	-0.0404**
	(0.949)	(116.8)	(45.37)	(48.82)	(52.43)	(12.55)	(0.0181)
Treated # 2012	2.218	-67.44	-58.24	-39.75	-36.63	-26.59	-0.0438**
	(1.398)	(157.5)	(66.30)	(64.14)	(65.07)	(16.05)	(0.0207)
Treated # 2013	2.827	-22.29	-58.38	-15.96	-49.76	-67.60***	-0.0747***
	(1.710)	(223.9)	(83.04)	(58.59)	(68.53)	(19.25)	(0.0247)
Treated # 2014	3.670	-126.9	-41.88	-13.10	-23.30	-85.31***	-0.0892***
	(2.287)	(239.2)	(109.1)	(68.54)	(101.3)	(27.55)	(0.0321)
Treated # 2015	4.142	-289.8	-34.23	-6.452	8.915	-109.7***	-0.100***
	(2.688)	(222.3)	(127.2)	(66.86)	(95.13)	(26.22)	(0.0300)
B. 14 Treatment States, 24 Potential Controls							
Treated # 2011	-0.116	123.2	12.07	28.48	25.45	-4.967	-0.0215
	(0.546)	(87.33)	(24.58)	(37.52)	(37.64)	(11.61)	(0.0162)
Treated # 2012	0.516	181.1	37.20	44.12	50.58	-7.232	-0.0264
	(1.183)	(125.6)	(47.73)	(49.45)	(48.29)	(14.28)	(0.0178)
Treated # 2013	0.804	251.0	57.71	-0.292	19.15	-51.45***	-0.0560**
	(1.396)	(202.3)	(63.48)	(52.43)	(49.39)	(16.62)	(0.0243)
Treated # 2014	0.783	137.6	108.8	26.65	84.64	-66.24***	-0.0631**
	(1.860)	(226.8)	(87.35)	(66.11)	(84.28)	(24.33)	(0.0302)
Treated # 2015	0.535	-115.3	132.2	17.97	85.67	-102.4***	-0.0928***
	(2.265)	(224.1)	(115.4)	(68.02)	(89.01)	(28.23)	(0.0335)
C. 7 Treatment States, 26 Potential Controls							
Treated # 2011	3.671***	-320.1***	-162.8***	-114.4***	-144.4***	-42.43***	-0.0592***
	(0.728)	(76.37)	(34.14)	(31.13)	(36.73)	(9.711)	(0.0181)
Treated # 2012	6.305***	-462.3***	-248.7***	-109.2**	-159.7**	-49.66**	-0.0654**
	(1.640)	(153.8)	(61.76)	(49.97)	(62.54)	(21.11)	(0.0286)
Treated # 2013	8.119***	-495.2*	-303.3***	-9.841	-165.6*	-92.01***	-0.105***
	(2.260)	(248.8)	(92.41)	(68.22)	(91.64)	(30.14)	(0.0342)
Treated # 2014	10.69***	-624.2**	-351.7***	-19.89	-187.3	-116.4***	-0.132***
	(2.824)	(288.3)	(121.3)	(77.18)	(119.8)	(41.02)	(0.0432)
Treated # 2015	12.04***	-709.1**	-359.2**	9.293	-119.8	-129.0***	-0.125***
	(3.274)	(283.4)	(149.4)	(82.84)	(125.8)	(37.76)	(0.0396)

Appendix Table 10 reports the event study estimates for the non-elderly adults in the most treated zip codes for the broad, narrow and partially treated samples. Each column of each panel corresponds to a different regression with the corresponding dependent variable in the first row. The coefficients on the interaction term of treatment status and year are reported. Treated is an indicator variable that takes on the value of 1 if the zip code is considered part of an expansionary state and 0 otherwise and the year refers to the observation date. All specifications include year fixed effects, age group fixed effects and the following controls: yearly state unemployment rate, yearly state unemployment rate squared, yearly state poverty rate (where 2014 and 2015 data are imputed using 2013 levels), yearly state 25th and 75th percentile of log wage (where 2014 and 2015 data are imputed using 2013 levels). Robust standard errors clustered at the state level are in parentheses. Significance levels: * = 10%, ** = 5%, *** = 1%

Synthetic Control Estimates of the Effect of Medicaid on Indicators of Financial Wellbeing for Most Treated Zip Codes, Ages 19-64

	Post-2014 Difference in Means Between Treatment States minus Synthetic Control								
	21 Treatment States, 21 Potential Control States			14 Treatment States, 19 Potential Control States			7 Treatment States, 21 Potential Control States		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable	Weights: Match on Average Value of Dep. Variable
Credit Score	648	1	3**	643	1	0	654	6**	7**
	(13)	(0.631)	(0.025)	(15)	(0.433)	(0.928)	(8)	(0.040)	(0.018)
Total Balance	10341	-328**	**-521***	10636	-326	-339*	10026	-270	**-922***
	(1278)	(0.045)	**(0.006)**	(1613)	(0.111)	(0.080)	(639)	(0.313)	**(0.002)**
Total Balance Past Due	1537	-100*	-69	1451	-37	-14	1629	-123*	**-242***
	(271)	(0.076)	(0.154)	(240)	(0.521)	(0.793)	(273)	(0.075)	**(0.004)**
Total Credit Card Balance	2580	**-93***	-38	2455	-85**	-88**	2714	-69	-63
	(397)	**(0.007)**	(0.212)	(434)	(0.011)	(0.014)	(303)	(0.202)	(0.210)
Total Credit Card Balance Past Due	855	-0	-4	676	7	-11	1047	-29	-13
	(315)	(0.997)	(0.895)	(238)	(0.750)	(0.593)	(271)	(0.523)	(0.713)
Total Collections Balance	333	**-73***	-114***	394	**-89***	**-117***	268	**-108***	-89**
	(105)	**(0.002)**	(0.000)	(107)	**(0.004)**	**(0.000)**	(46)	**(0.001)**	(0.017)
Number of Collections	0.461	**-0.067***	-0.054**	0.591	-0.071**	**-0.079***	0.322	**-0.091***	-0.053
	(0.177)	**(0.004)**	(0.013)	(0.141)	(0.018)	**(0.006)**	(0.076)	**(0.006)**	(0.118)

Appendix Table 11 reports estimates of the post-2014 differences in financial indicators between treated and synthetic control states for non-elderly adults in the most treated zip codes. States that fully expanded prior to 2014 (DE, DC, MA, NY, and VT) are excluded from all control groups. Columns (1) – (3) present the results for the broad sample with 21 treatment and 21 potential control states. Columns (4) – (6) present the results for the narrow sample with 14 treatment states and 19 potential control states. Columns (7) – (9) present the results for 7 treatment states and 21 potential control states, with the 7 treated states being those included in the broad treatment group but not the narrow. For each expansionary definition, we present the 2010-2013 pre-reform mean outcome for the treated states, and the average post-reform quarterly difference between the treated states and their synthetic counterpart using the two different weighting methods used to construct the synthetic control group. In all results, AK, IN, NH and PA are dropped. Bolded results are also significant at the 5% level when the Holm-Bonferroni correction using 7 outcomes is applied to assess absolute gap p-values. Significance levels: * = 10%, ** = 5%, *** = 1%.
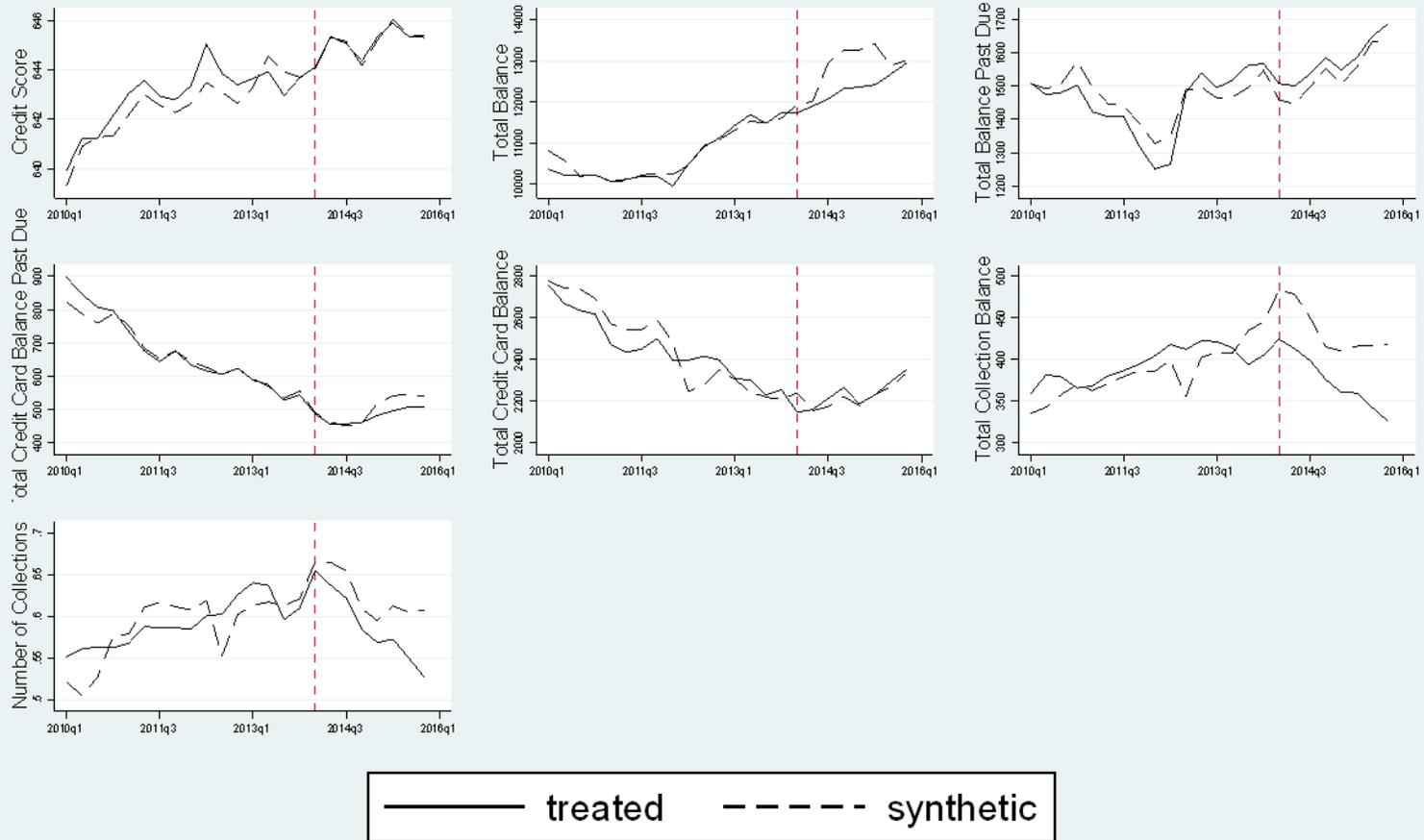
Appendix Figure 1. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing Using Alternative Weights (Match on Pre-Reform Average Lagged Outcome and 2013 Lagged Outcome) for Nonelderly in Most Treated Zip Codes Using 21 Treated States, 26 Potential Control States
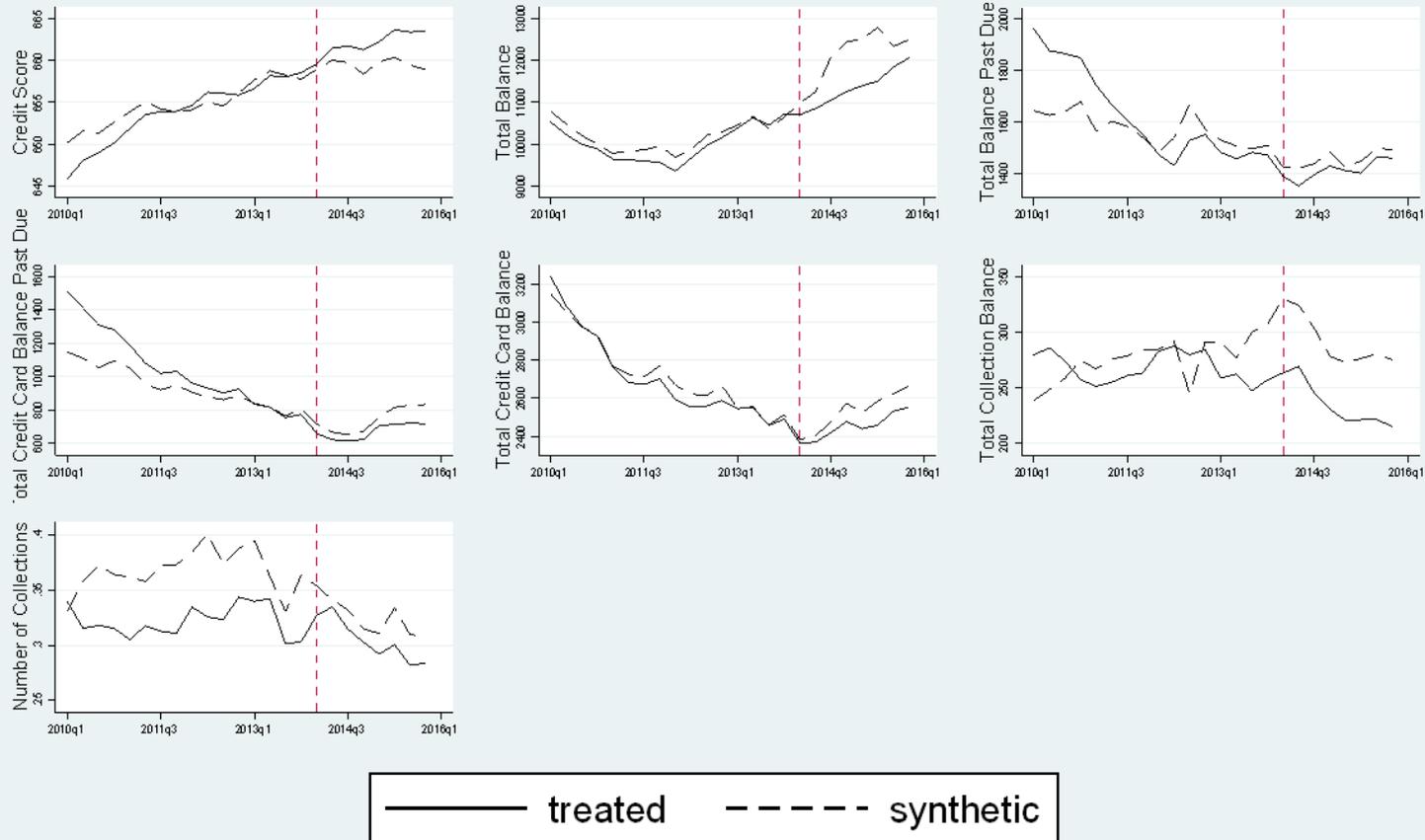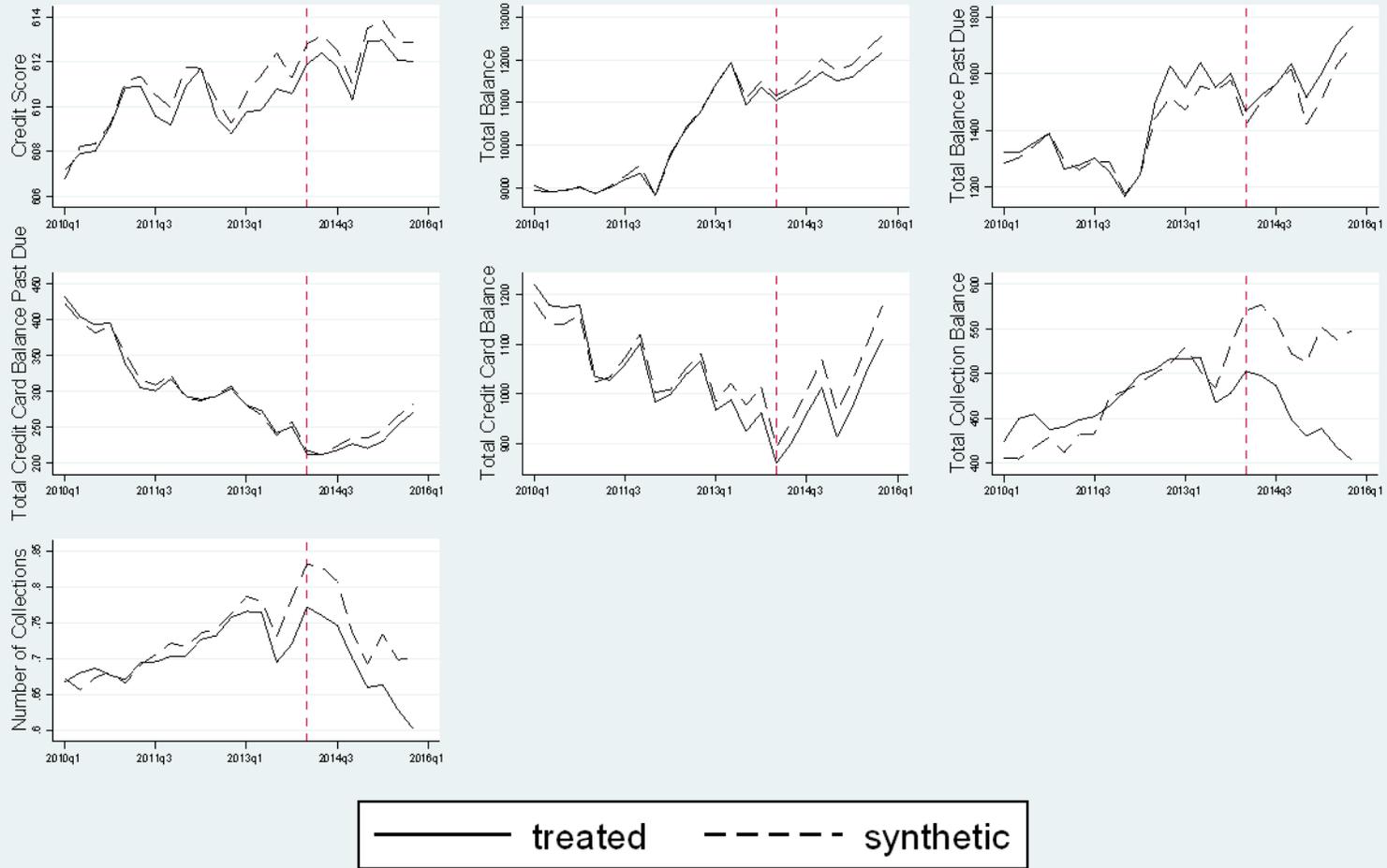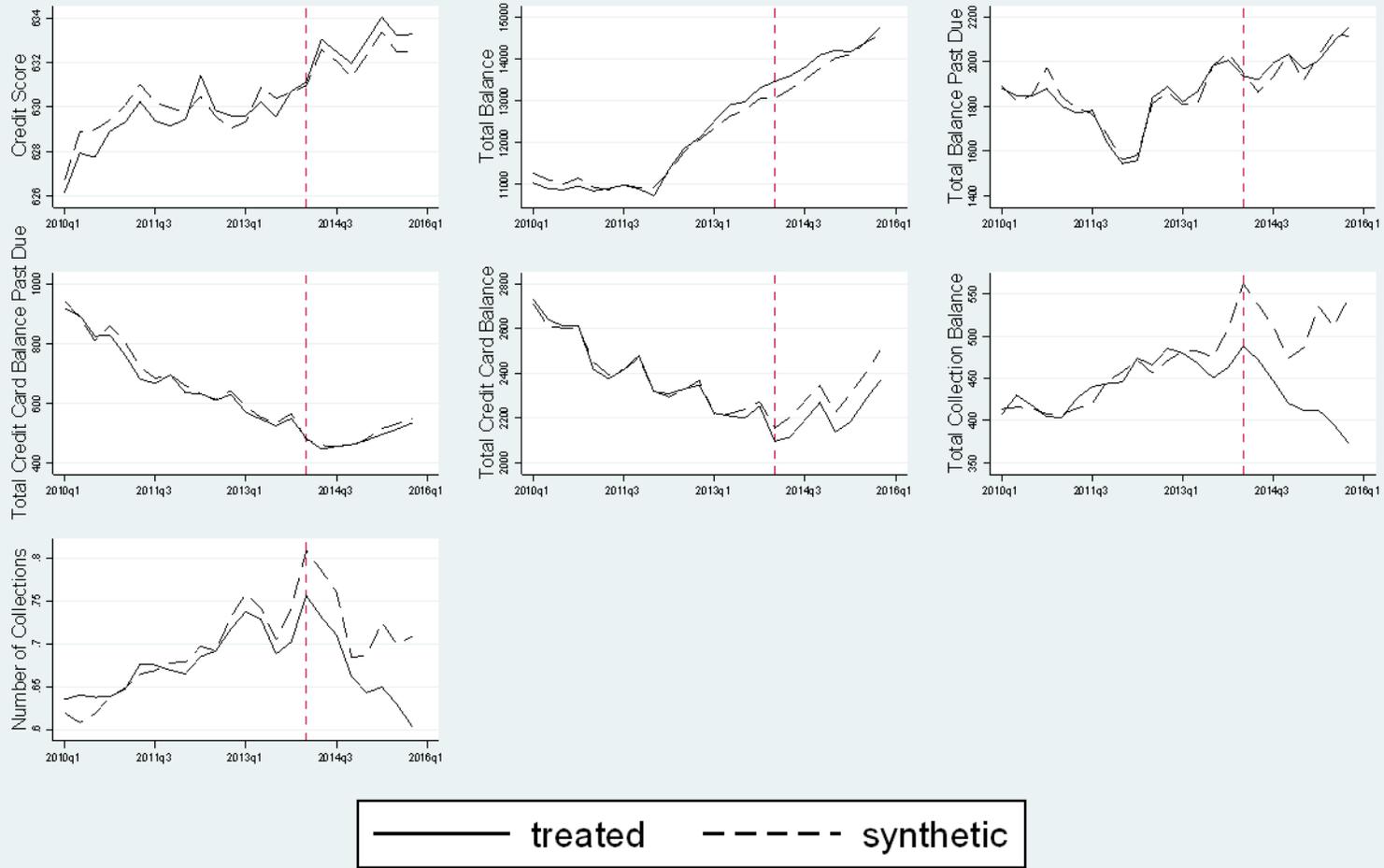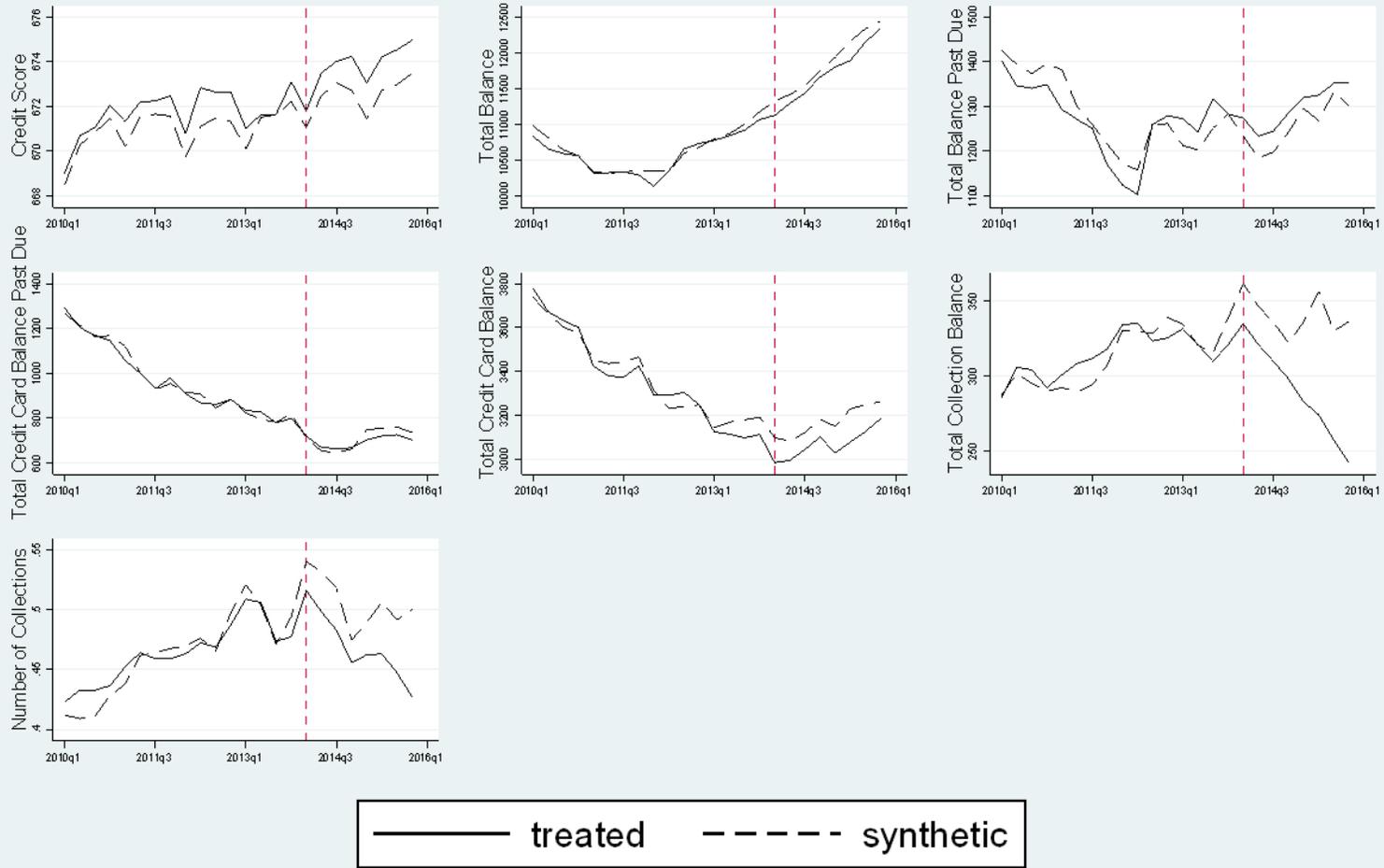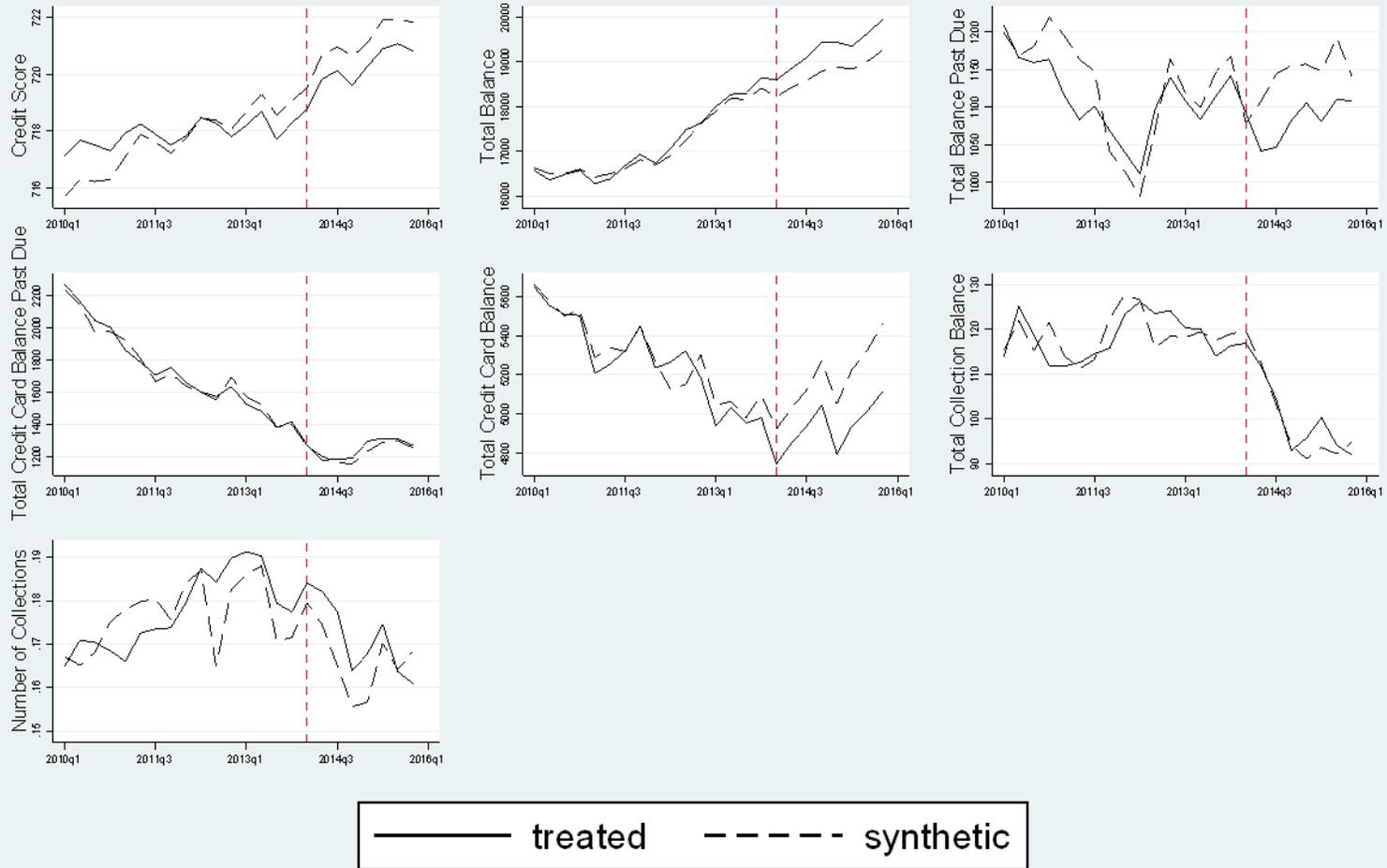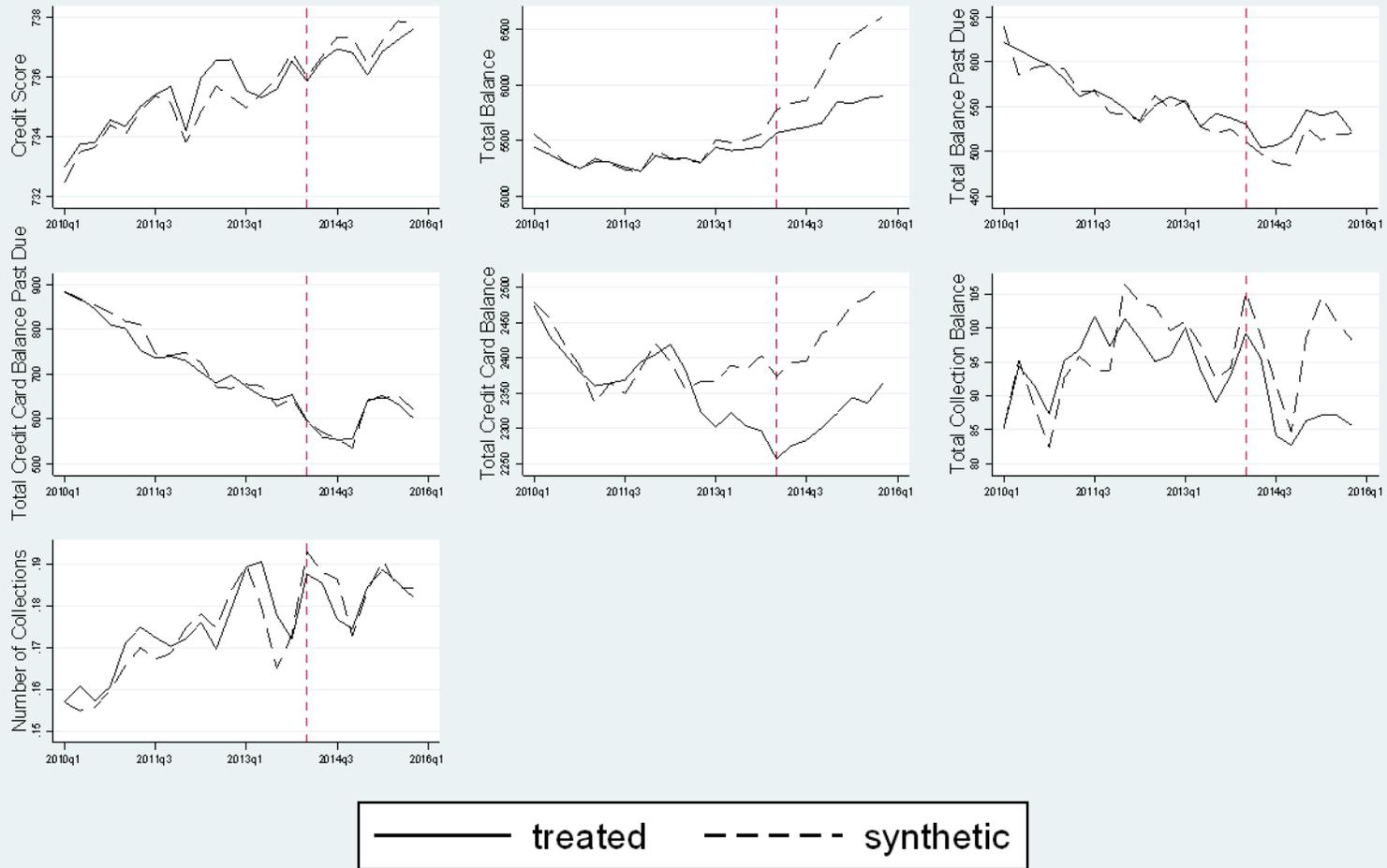
Appendix Figure 2. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing Using Alternative Weights (Match on Pre-Reform Average Lagged Outcome and 2013 Lagged Outcome) for Nonelderly in Most Treated Zip Codes Using 14 Treated States, 24 Potential Control States
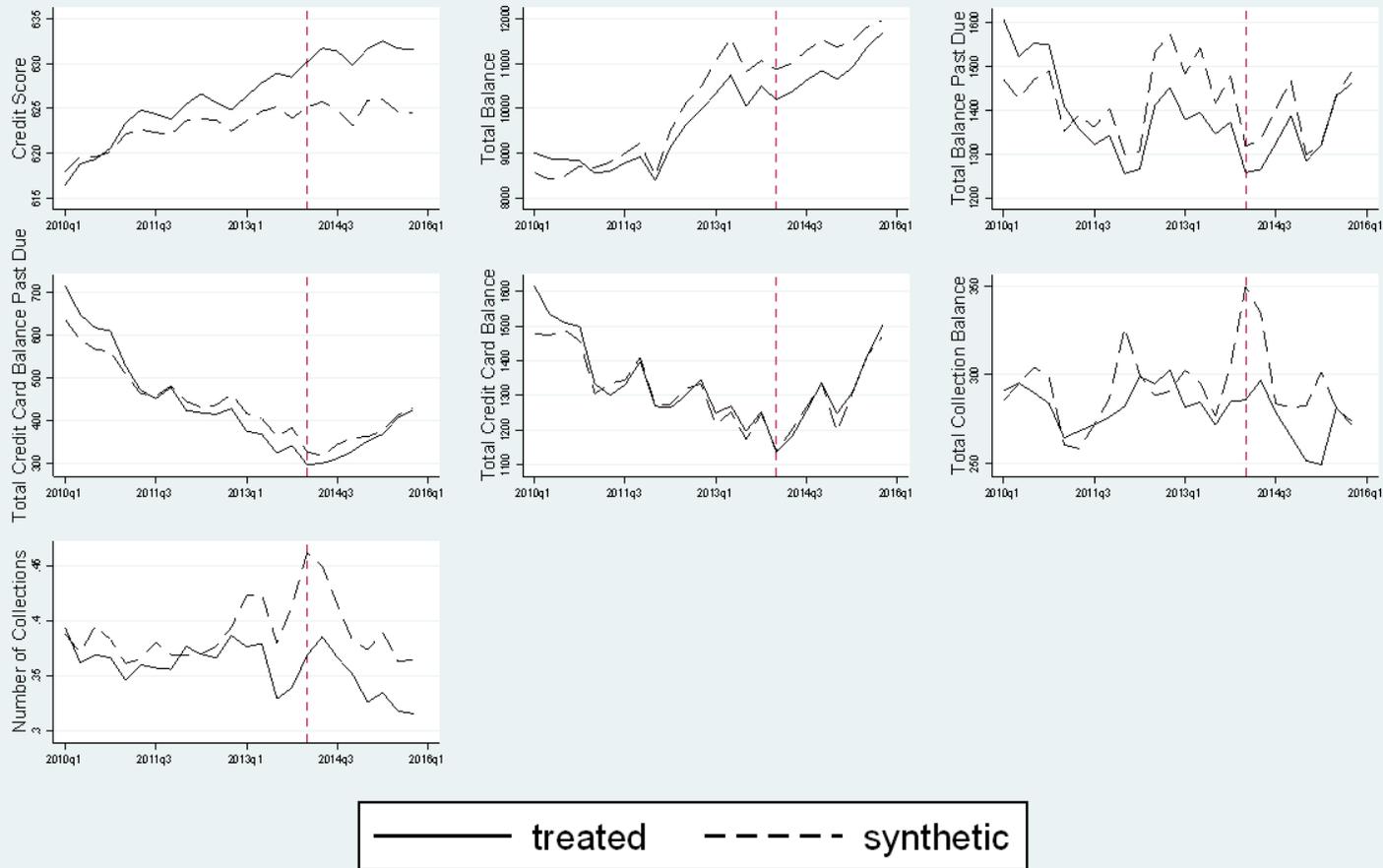
Appendix Figure 3. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing Using Alternative Weights (Match on Pre-Reform Average Lagged Outcome and 2013 Lagged Outcome) for Nonelderly in Most Treated Zip Codes Using 7 Treated States, 26 Potential Control States

Appendix Figure 4. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Ages 19 to 32 in Most Treated Zip Codes Using 14 Treated States, 24 Potential Control States
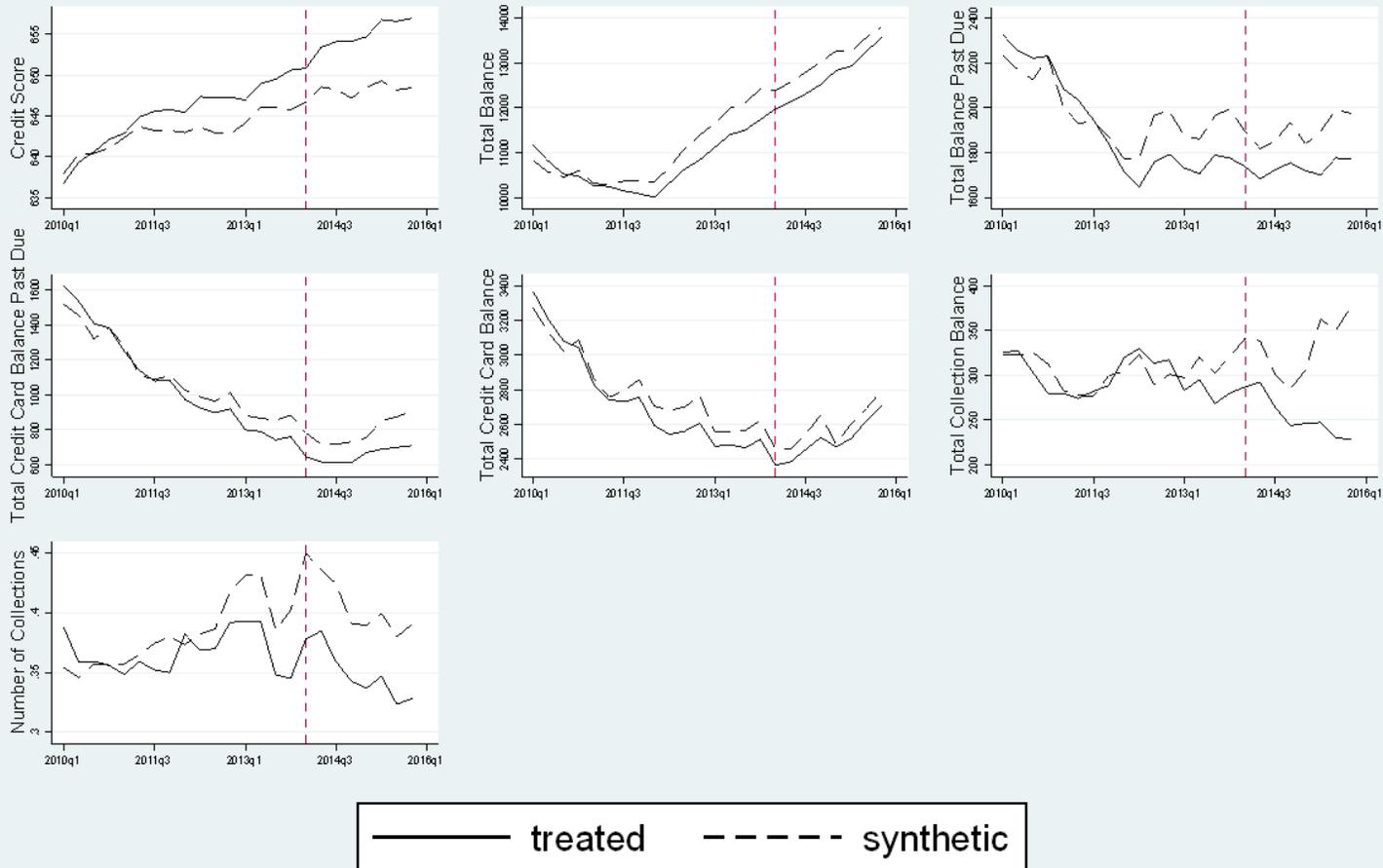
Appendix Figure 5. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Ages 33 to 44 in Most Treated Zip Codes Using 14 Treated States, 24 Potential Control States

Appendix Figure 6. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Ages 45 to 64 in Most Treated Zip Codes Using 14 Treated States, 24 Potential Control States

Least Treated Zipcodes, Ages 19-64

14 Treated States, 24 Potential Controls

treated   - - - - - synthetic

Appendix Figure 7. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Non-elderly in Least Treated Zip Codes Using 14 Treated States, 24 Potential Control States
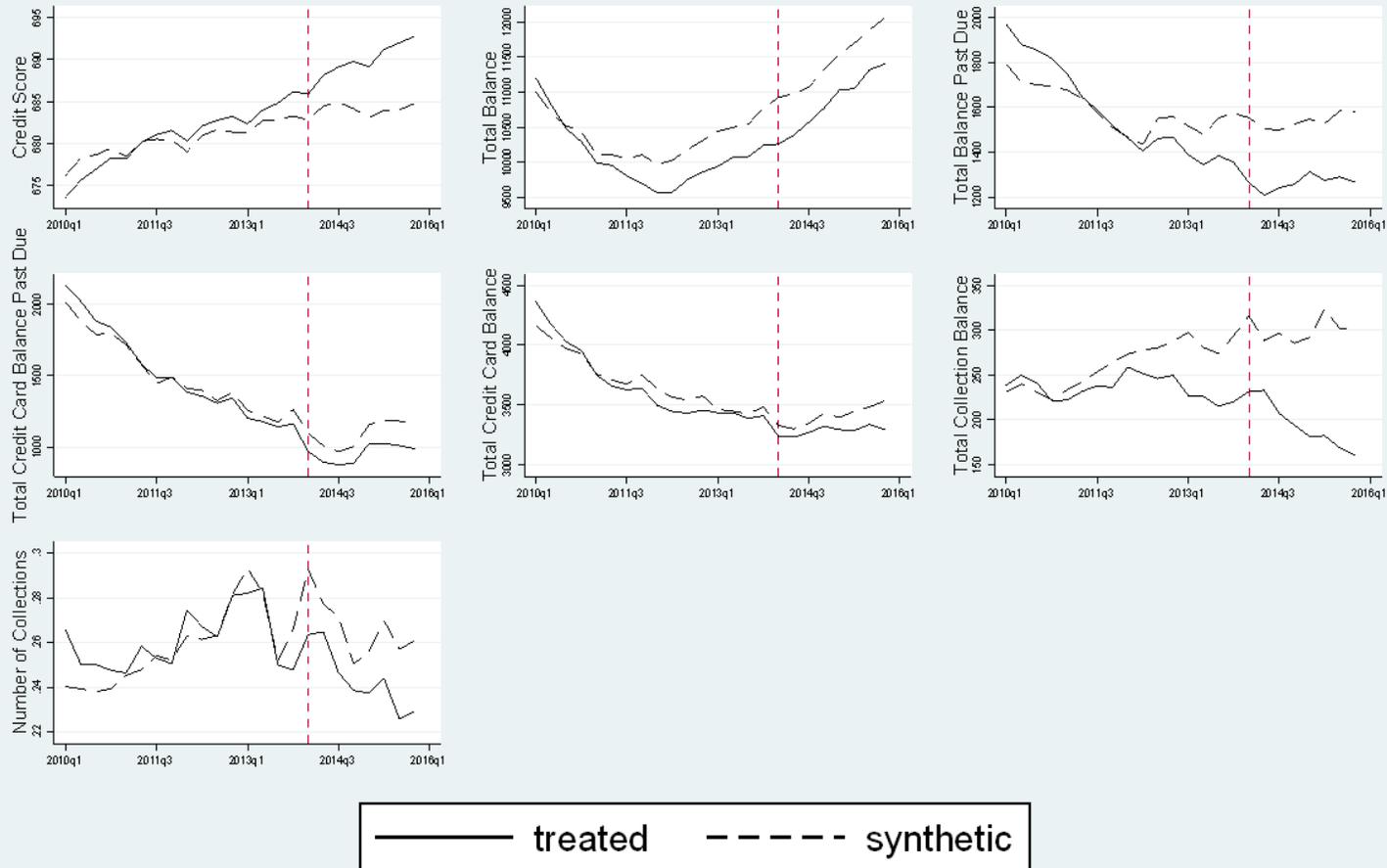
Appendix Figure 8. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Elderly in Most Treated Zip Codes Using 14 Treated States, 24 Potential Control States
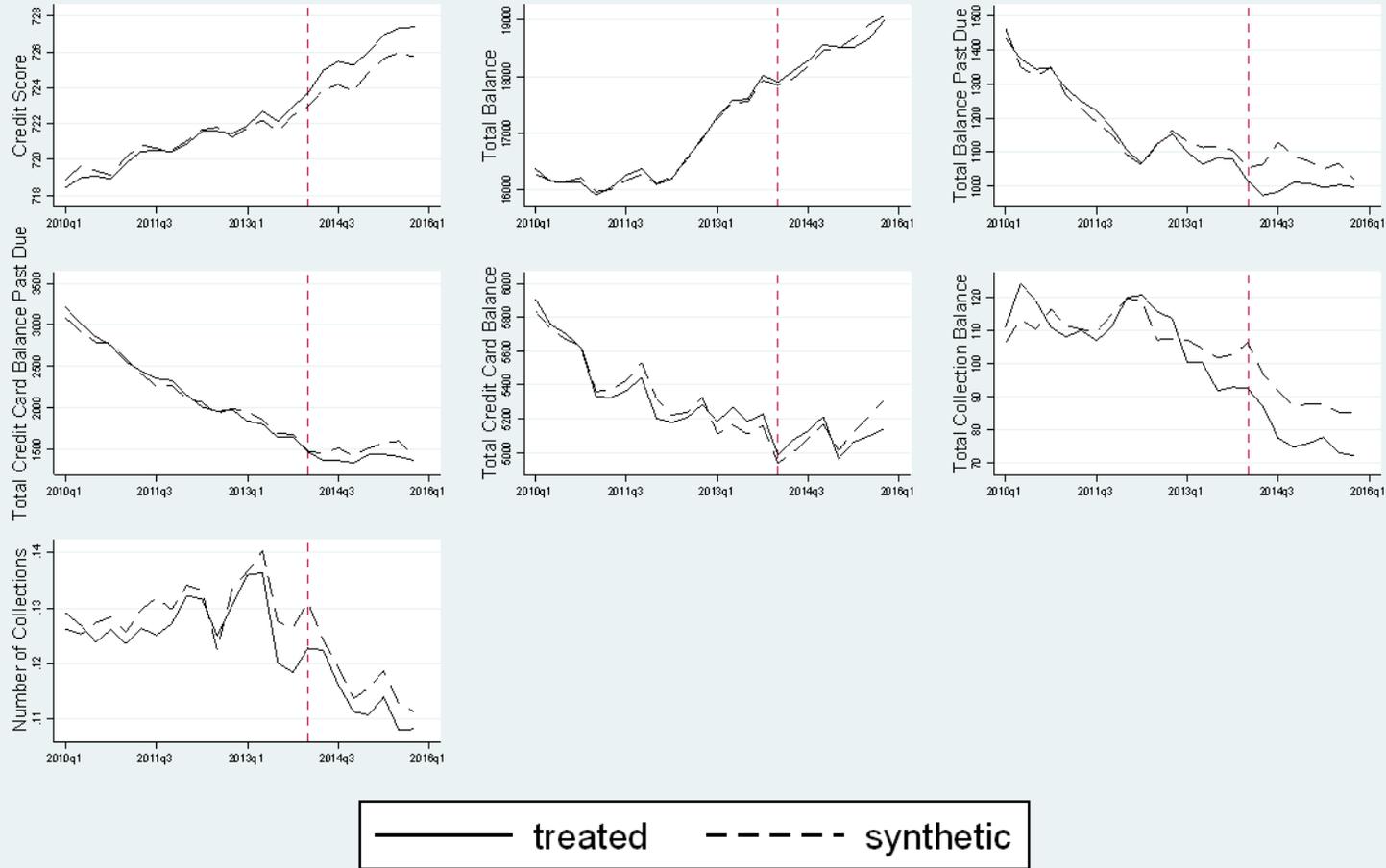
Appendix Figure 9. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Ages 19 to 32 in Most Treated Zip Codes Using 7 Treated States, 26 Potential Control States
DC, MA and HI are dropped (not enough observations for many credit categories).

Appendix Figure 10. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Ages 33 to 44 in Most Treated Zip Codes Using 7 Treated States, 26 Potential Control States
DC and MA are dropped (not enough observations for many credit categories)

Appendix Figure 11. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Ages 45-64 in Most Treated Zip Codes Using 7 Treated States, 26 Potential Control States
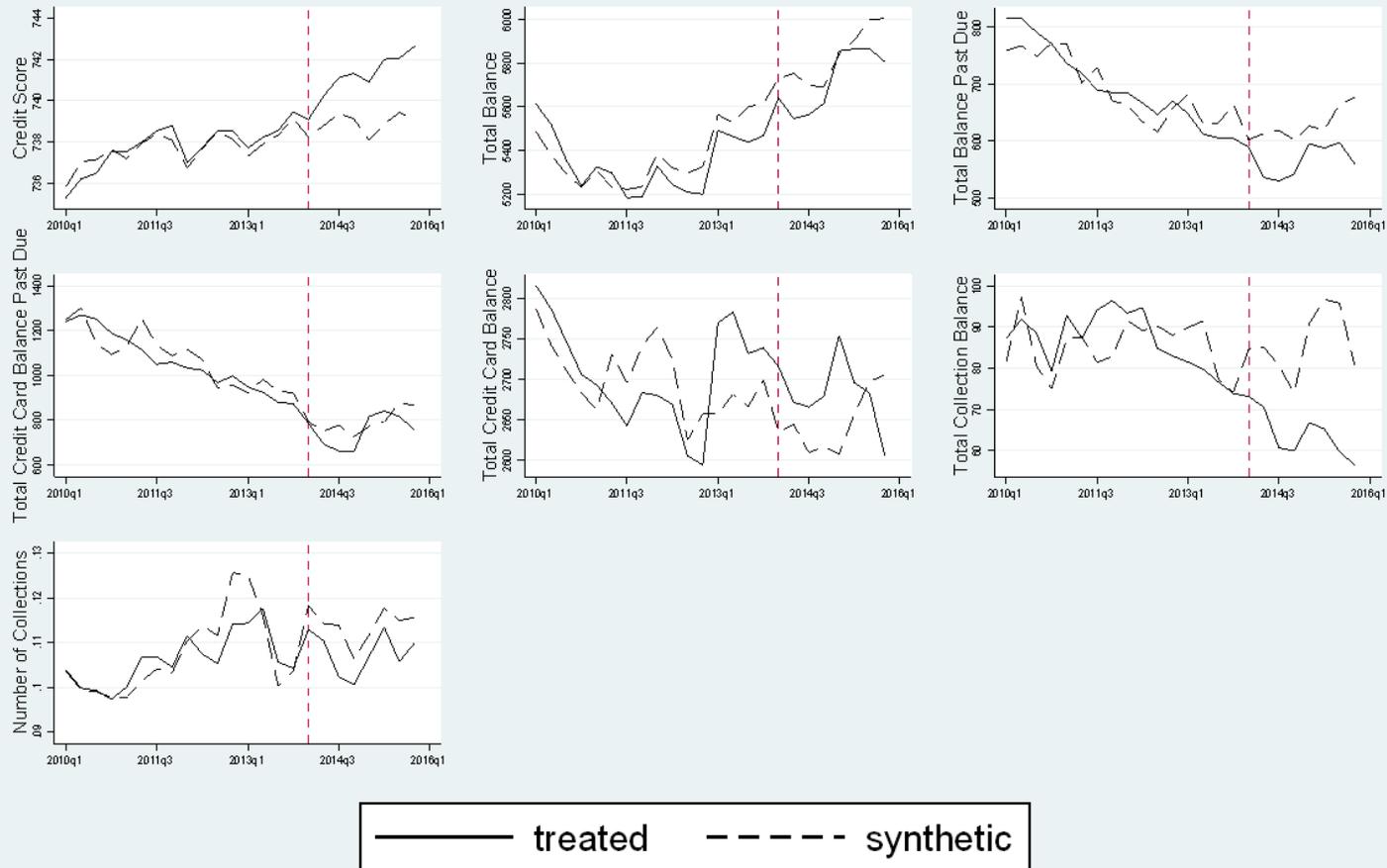DC and MA are dropped (not enough observations for many credit categories)

Appendix Figure 12. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Non-elderly in Least Treated Zip Codes Using 7 Treated States, 26 Potential Control States

Appendix Figure 13. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Elderly in Most Treated Zip Codes
Using 7 Treated States, 26 Potential Control States
DC and MA are dropped (not enough observations for many credit categories).