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ABSTRACT

We examine the effect of the Medicaid expansions under the 2010 Patient Protection and Affordable Care Act (ACA) on financial outcomes using credit report data for a large sample of individuals. We employ the synthetic control method (Abadie et al., 2010) 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 around $600 to $1,000 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 Medical Expenditure Panel Survey (MEPS), the annual cost of inpatient care for a person aged 18 to 64 who was hospitalized in 2012 was approximately $15,000 and the annual cost of all types of care for that person for the year 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. (2015) 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 well-being. Access to health insurance has the potential to improve access to credit markets, increase savings, and facilitate consumption of other goods and services. These other channels can potentially have salutary effects on the well-being of low-income individuals.[2]

[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) report 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.

[2] Doty et al. (2008) find 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

Despite the potentially important role that publicly provided health insurance plays in the financial well-being of low-income individuals, only two studies have evaluated the role of Medicaid on consumer financial well-being. Gross and Notowidigdo (2011) examined the Medicaid eligibility expansions in the 1990s, which were mostly for children. 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 insurance coverage on financial outcomes in other settings and have also documented substantial improvements in financial well-being (Barcellos and Jacobson, 2015; Mazumder and Miller forthcoming; Dobkin et al., 2015).

We extend this literature by evaluating the expansion of Medicaid under the ACA to low-income adults. 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., 2016; Sommers 2014; Wherry and Miller, forthcoming), and total Medicaid enrollment in these states increased by 12.3 million between 2013 and 2015 (Centers for Medicare & 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

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] Louisiana and Montana will begin Medicaid expansion in 2016. As we discuss below, in our analysis we drop some states that expanded during 2014 or 2015. In some specifications we also focus on a narrower sample of states who had not experienced a significant expansion of Medicaid prior to 2014.

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 well-being. 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 well-being. We focus on several measures of debt and delinquency that potentially could be affected within the two-year period since the expansion of Medicaid in 2014. Specifically, we examine 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.[4]

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 \$51 and \$85. 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 treatment on the treated (ToT) of between \$600 and \$1,000.

[4] As we discuss below, our current data are not supposed to include medical bills sent to collections. However, we suspect that due to classification error, our collections variables include some amount of medical bills.

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, 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, 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.

Research Design

To study how Medicaid affects financial well-being, 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 put forth in the ACA provides plausibly exogenous variation in health care insurance coverage among low-income adults that can be used to identify estimates of the effect of Medicaid eligibility on consumer financial well-being.

Most prior studies of Medicaid expansions use 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 well-being 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 as preliminary analyses of our data indicate.[5] 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 those 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 control groups are zero. Indeed, we 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 the 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-

[5] We find evidence that year effects for some outcomes differed between treated and untreated states prior to the ACA Medicaid expansions.

Medicaid expansion difference between the groups is likely to represent a valid estimate of the effect of the Medicaid expansion. An advantage of our 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.[6]

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 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.[7]

Once the weights are selected and the synthetic comparison group is constructed, the estimates of the effect of Medicaid on financial well-being is derived by taking the post-2014 mean difference between the outcome in the treatment group (combined into one unit) and in the synthetic comparison group. Inferences for this estimate are derived from permutation tests (randomization inference). These tests consist of performing this analysis 1,000 times, but each time using randomly-selected states to form the treatment group. For the actual treatment group and each of these 1,000 "random" estimates, we calculate the pre- and post-reform mean squared prediction error (MSPE), or the average of the squared differences between the financial outcomes in the treatment group and its synthetic counterpart. We then assess this gap for the states in the treatment group relative to the placebo estimates by obtaining a *p*-value from the entire distribution of the ratios of post/pre-reform MSPE, an approach that is similar to the one used by Abadie et al. (2010). This method captures the probability of obtaining estimates as large as the actual treatment group's while minimizing the influence of placebo estimates from analyses that have poorly matched treatment and synthetic control units.

[6] Only states with positive weights are used to construct the synthetic comparison group.
[7] 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.

We identify individuals who are more likely to have been treated due to the Medicaid expansion based on their age and location. However, this targeting is still imperfect. Therefore, our estimates are 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 well-being of the low-income inhabitants. However, we also provide estimates of the effect of Medicaid coverage on individual financial well-being (i.e., treatment on treated) by rescaling the ITT estimates by individuals likely to have gained Medicaid coverage.

3. Data

Consumer Credit Panel/Equifax

We use 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 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% primary sample of the CCP data. 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.

For our analysis, 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 use estimates from the 2008-2012 American Community Survey (ACS) of the share of a zip code adult population under age 65 who are both

[8] Only 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.

uninsured and have an income less than 138% of the FPL.[9] We then selected the quartile of zip codes with the highest shares of such persons. We refer to this sample as the "most treated." This includes about 8,100 zip codes covering all of the states. On average, 17% of persons in these zip codes were uninsured and had incomes less than 138% of the FPL. In our 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 therefore might experience larger effects of the Medicaid expansion. 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.

The CCP database consists of over 600 potential indicators of financial well-being based on the various forms of debt and account line information (e.g., credit cards, mortgages, auto loans, etc.). We examine several broad measures of debt. These include: the total amount of debt (excluding mortgage debt), total amount of debt (excluding mortgage debt) at least 30 days past due, total amount of credit card debt, credit card debt at least 30 days past due, the number of new non-medical collections in the last 12 months, and the total balance of non-medical collections.[10] 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. (2015) also showed that non-medical collections

[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.

[10] 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.

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 other financial indicators will respond within two years, although Dobkin et al. (2015) reported that credit card balances and credit limits decrease significantly within one year of hospitalization for both insured and uninsured persons while Mazumder and Miller (forthcoming) find 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 the financial outcomes data, we use data on state demographic and socioeconomic characteristics in our matching exercise. 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 uninsured-low-income quartile zip codes. Specifically, we use the ACS 2012 five-year estimates for share of total zip code non-elderly adult population Hispanic; share of total zip code non-elderly adult

[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.

population Black; and share of total 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, immediately prior to the Medicaid expansion. We verify (in results not reported here) that our estimates are not sensitive to the year in which we assign state and zip code of residence. 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, since we define the treatment period as the eight quarters spanning from 2014:Q1 through 2015:Q4, we dropped four states (Alaska, New Hampshire, Pennsylvania, and Indiana) from the analysis because they expanded after the beginning of the treatment period.[13] 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.

Second, 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

[12] These calculations use 18-64 year olds since these estimates are available from the ACS at the zip code level.
[13] 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.

effectively untreated in 2014 and 2015 and did not change status. Third, 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, we consider two samples: (1) a broad sample where we include these nine states (seven in the treatment group and 2 in the control group) and (2) a narrow sample that drops these nine states from the analysis.

Thus, for our broad sample we have 21 states in the treatment group, 26 states in the control group, and 4 states excluded. In our narrow sample, we have 14 states in the treatment group, 24 states in the control group, and 9 states excluded. Appendix Table A1 shows how we classify the states.

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.[15]

Table 1 presents the results of the matching procedure for one dependent variable: total collection balance in past 12 months.[16] We show the means for treated states, means for control states (unweighted),

[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] 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.

[16] In Appendix Table 1, we provide the weights for each potential control state that were used to construct the synthetic control states for all six dependent variables used in the analysis. Many states get zero weights.

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 Consumer Financial Well-being

Figure 1 shows the time series for all six indicators of financial well-being for our broad sample where we match on all pre-2014 values of the dependent variable and covariates. Figure 2 presents similar time series for the narrow sample. 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. This graphical evidence strongly supports the validity of the synthetic control research design. Similarly, Figure 2 shows very similar pre-2014 trends in financial indicators between the treatment and control groups. Both figures, however, 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 well-being. The exceptions are the two outcomes related to bills reported to third-party collection agencies: there was a clear decline in the number of bills sent to collections, as well as a decrease in the amount of balances in collections in the treated states.

Table 2 presents point estimates and *p*-values of the post-2014 differences in outcomes between treatment and control groups corresponding to Figures 1 and 2 (columns 2 and 5). For each outcome, we also show the pre-reform means for the treated states in each of our two samples (columns 1 and 4), along with estimates based on our alternative matching procedure for constructing the synthetic control group (columns 5 and 6). We begin by examining total debt excluding mortgage liabilities. Recall, that the effect of the Medicaid expansion on debt is theoretically ambiguous. None of the estimates in row 1 are statistically significant and all are small relative to the mean or standard deviation.

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 about $11,000 (column 1 of Table 2). The corresponding figure for those living in the least treated zip codes is significantly higher at approximately $17,000 (column 1 of Table 4). However, even this $6,000 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. This implies that we have less power to detect significant changes since 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. Estimates of the post-2014 difference between the treated and synthetic control states are all positive, 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. 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 is $1,459, whereas the corresponding figure for individuals in the least treated zip codes is $1,144, or 22% more (see Tables 2 and 4). Moreover, the $1,144 value is a much lower (50%) fraction of total debt than is the $1,449. 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.

We also examine the effect of the Medicaid expansions on total credit card debt in Table 2. The estimates of the post-2014 difference with respect to credit card debt are mostly negative, and approximately the same magnitude ($0 to -$23), but statistically insignificant. Relative to the 2013 mean of approximately $2,800, the estimates are small. The estimates of the effects of the Medicaid expansions on credit card debt past due are also not statistically significant and range from -$7 to -$58. 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.

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 number of non-medical collections (row 5) are all negative and range from -0.038 to -0.057. In three of four cases, they are statistically significant at the 1% level; in all cases they are significant at the 10% level. Similarly, estimates of the effect of the Medicaid expansions on the amount of collections (row 6) are also all negative and statistically significant at the 10% level; in two cases they are also significant at the 1% level. The point estimates range from -$51 to -$85. The estimates are also slightly larger (more negative) for the narrow sample, where states had virtually no prior Medicaid expansion than the estimates obtained when including the additional 10 states that had some partial expansion. This finding is what would be expected if inclusion of the 10 states with prior expansions in the treatment group resulted in attenuated estimates because of the smaller treatment effects in those states.

The apparent significant decrease in collection balance post-2014 is consistent with the expectation that expanded Medicaid coverage would reduce the debt burden of those who obtain coverage. Dobkin et al. (2015) 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 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.

The estimates described above capture the overall change in financial outcomes among the entire 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 our most treated zip codes, approximately 17% of individuals were uninsured and had incomes below 138% of the FLP. If we assume that half of this group gained coverage as a result of the Medicaid expansions and scaled our estimates by this figure, the implied treatment-on-treated (TOT) estimates range between -$600 and -$1,000. This is a rough approximation of TOT estimates and relatively small changes in the share of the sample that is assumed to be affected by the Medicaid expansions will alter implied TOT estimates significantly. In addition, while the 2013 mean of the total collection balance is approximately $350, it obscures the fact that for the group of low-income individuals likely affected by the Medicaid expansion, the mean could be much higher as the number and amount of collections is strongly related to income. For example, the total collection balance is more than twice as large for individuals in the low-income zip codes ($346) as for those in the high income zip codes ($146).[17] In the Oregon study (Finkelstein et. al. 2012), the mean amount of total non-medical collections was $2,740.

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, the effects rise to between $1,364 and $2,272. 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. Overall, our estimates in Table 2 suggest that the Medicaid expansions substantially improved the financial well-being of those who gained coverage by reducing the amount of debt in third-party collections.

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

[17] For the two measures of collection, the pre-2014 means of individuals in the most treated zip codes is 2.4 times greater than the analogous mean for individuals living in the least treated zip codes.

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.[18] 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 both 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 groups. For the two older age groups, the effects are unambiguous. For those aged 33 to 44, estimates of the effect of the Medicaid expansions on the number of collections range from -0.061 to -0.074 and are significant at the 1% level. The effect on balances in collections for this age group ranges from -$84 to -$112. For the oldest age group, those between the ages of 45 and 64, the estimates are highly consistent across the two samples. The effect on the number of collections is between -0.039 and -0.041 and the effect on collection balances is between -$49 and -$61. For those ages 19 to 32, the effects are more varied and are only statistically significant for the narrow sample. Estimates of the effect on the number of collections range from -0.004 to -0.041. The effect on collection balances is between -$49 and -$91 for the youngest age group. As with the full sample, none of the other four outcomes is statistically significant.

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.[19] First, we conduct 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%

[18] We present the corresponding figures for Panel B in the Appendix (Figures 3-5).

[19] 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 and 2).

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 6 shows the time series patterns of outcomes for treated and synthetic control states (selected for this sample). 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.[20] We 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 -0.014 to -0.021, and are only statistically significant at the 10% level for one of the four estimates. Similarly, the effects on collection balances are also much lower than they were when estimated with the highest treatment group, ranging from -\$14 to -\$25. The fact that the estimates are now significantly lower provides further validation of the synthetic control research design.

Second, we conduct 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. Table 5 provides estimates for those over age 65. Figure 7 presents the graphical evidence corresponding to Table 5 estimates. These charts show that the synthetic control approach is plausibly valid. None of the estimates in Table 5 are statistically significant, economically meaningful or consistent across samples. In particular, our point estimates on the number of collections and on collection balances are very close to zero. We conclude that these results also provide validation of our synthetic control research design.

[20] It is also important to take into account the different scales for the y-axis between Figure 1 and Figure 6.

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 well-being 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 well-being.

We use 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 well-being, we employ the synthetic control approach of Abadie et al. (2010). We provide evidence that the approach was likely valid, so estimates of the effect of the Medicaid expansion are plausibly interpreted as causal.

Results indicate 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 indicate that the 2014 Medicaid expansions were associated with a reduction in the amount of collections of between $51 and $85, with a mean estimate of $69. These reduced form estimates imply a treatment-on-treated (TOT) effect of between -$600 and $1,000 under the assumption that half of the low-income (<138% FPL) and uninsured individuals in our sample acquired Medicaid coverage. For other measures of debt and debt past due, we did not find any 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 et al., 2015). Given that the ACA Medicaid expansions decreased unpaid bills, the financial benefits of the ACA expansions appear to fall at least partially on third-party creditors. As a result, those individuals who gained coverage through the ACA Medicaid expansions may have better access to credit markets 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 Controls

	(1)	(2)	(3)	(4)
	Treated States	Control States	Synthetic Control States	
		Unweighted	Match on All Lagged Y and X's	Match on Average Y, Y in 2013, and All Lagged X's
State Simulated Medicaid Eligibility	0.084	0.065	0.085	0.089
State Unemployment Rate				
2010	0.093	0.084	0.090	0.093
2011	0.086	0.078	0.083	0.085
2012	0.078	0.069	0.076	0.078
2013	0.071	0.063	0.069	0.070
State Poverty Rate				
2010	0.145	0.156	0.146	0.147
2011	0.151	0.160	0.151	0.150
2012	0.150	0.160	0.152	0.151
2013	0.149	0.159	0.151	0.151
State 25th Percentile of Log Wage				
2010	9.705	9.709	9.755	9.774
2011	9.746	9.738	9.751	9.755
2012	9.759	9.759	9.750	9.759
2013	9.809	9.808	9.825	9.833
State 75th Percentile of Log Wage				
2010	10.916	10.862	10.922	10.932
2011	10.940	10.876	10.932	10.935
2012	10.960	10.899	10.951	10.953
2013	10.974	10.923	10.963	10.957
% Hispanic	0.257	0.128	0.187	0.153
% Black	0.153	0.177	0.189	0.168
% HS Degree or Less	0.287	0.271	0.293	0.282
% Uninsured and < 138% FPL	0.153	0.164	0.152	0.163
Average Total Collection Balance	346.39	370.07		337.14
Total collection balance				
2010Q1	318.63	338.85	316.97	
2010Q2	337.71	350.66	335.46	
2010Q3	334.05	345.11	326.56	
2010Q4	325.67	354.95	331.14	
2011Q1	315.32	346.18	320.66	
2011Q2	330.97	354.64	318.91	
2011Q3	330.84	352.82	318.85	
2011Q4	339.08	363.75	338.04	
2012Q1	361.66	369.76	357.28	
2012Q2	374.67	382.07	374.37	
2012Q3	383.33	375.49	378.06	
2012Q4	385.23	402.86	385.90	
2013Q1	374.52	410.26	384.71	372.83
2013Q2	366.99	390.93	361.14	350.51
2013Q3	325.86	385.23	334.00	337.92
2013Q4	337.66	397.50	366.29	358.95

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		
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable (p-value in parentheses)	Weights: Match on Average Value of Dep. Variable (p-value in parentheses)	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable (p-value in parentheses)	Weights: Match on Average Value of Dep. Variable (p-value in parentheses)
Total Balance	10,997	-231	-89	11,067	4	-34
	(2,032)	(0.59)	(0.88)	(2,367)	(0.95)	(0.90)
Total Balance Past Due	1,459	52	59	1,448	47	82
	(304)	(0.69)	(0.60)	(246)	(0.43)	(0.45)
Total Credit Card Balance	2,825	-23	-16	2,559	0	-21
	(1,200)	(0.96)	(0.84)	(460)	(1.00)	(0.95)
Total Credit Card Balance Past Due	904	-58	-17	703	-10	-7
	(870)	(0.33)	(0.75)	(267)	(0.72)	(0.89)
Number of Collections	0.505	-0.038***	-0.043***	0.569	-0.043*	-0.057***
	(0.180)	(0.04)	(0.03)	(0.145)	(0.08)	(0.01)
Total Collection Balance	346	-77***	-51*	381	-85***	-61*
	(128)	(0.00)	(0.09)	(124)	(0.03)	(0.08)

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 broad sample with 21 treatment states and 26 potential control states. Columns (4)-(6) presents the results for the narrow sample with 14 treatment states and 24 potential control states. For each expansionary definition, we present the 2010-2013 pre-reform mean outcome for the treated states and the 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.

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	
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable (p-value in parentheses)	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable (p-value in parentheses)	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable (p-value in parentheses)
Total Balance	9,186	-53	11,654	-242	11,143	320
	(2,718)	(1.00)	(1,993)	(0.51)	(3,151)	(0.44)
Total Balance Past Due	1,137	17	1,706	-24	1,378	-14
	(378)	(0.85)	(449)	(0.46)	(432)	(0.71)
Total Credit Card Balance	951	-66	2,594	-191	3,774	-16
	(468)	(0.50)	(1,325)	(0.76)	(1,183)	(0.95)
Total Credit Card Balance Past Due	291	-3	657	-32	1,321	-70
	(145)	(0.92)	(338)	(0.79)	(1,254)	(0.67)
Number of Collections	0.588	-0.004	0.597	-0.061***	0.395	-0.041***
	(0.224)	(0.36)	(0.229)	(0.03)	(0.145)	(0.04)
Total Collection Balance	388	-49	400	-84*	281	-61***
	(167)	(0.26)	(159)	(0.06)	(122)	(0.00)

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	
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable (p-value in parentheses)	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable (p-value in parentheses)	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable (p-value in parentheses)
Total Balance	9,191	-164	11,689	49	11,400	-3
	(1,550)	(0.15)	(2,174)	(0.60)	(3,711)	(0.75)
Total Balance Past Due	1,203	14	1,747	84	1,339	35
	(301)	(0.85)	(333)	(0.33)	(306)	(0.70)
Total Credit Card Balance	890	12	2,324	-42	3,518	-81
	(205)	(0.80)	(463)	(0.88)	(664)	(0.77)
Total Credit Card Balance Past Due	282	2	639	11	1,035	-21
	(107)	(0.85)	(263)	(0.86)	(407)	(0.91)
Number of Collections	0.671	-0.041***	0.675	-0.074***	0.442	-0.039***
	(0.181)	(0.04)	(0.181)	(0.00)	(0.128)	(0.04)
Total Collection Balance	440	-91*	449	-112***	303	-49***
	(153)	(0.06)	(139)	(0.04)	(123)	(0.04)

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 codes. Panel A reports the results for the broad sample and panel B reports the results for the narrow sample. 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 quarterly difference between the treated states and their synthetic counterpart. In additional 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.

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		
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable (p-value in parentheses)	Weights: Match on Average Value of Dep. Variable (p-value in parentheses)	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable (p-value in parentheses)	Weights: Match on Average Value of Dep. Variable (p-value in parentheses)
Total Balance	16,871	82	316	16,969	160	193
	(1,894)	(0.68)	(0.35)	(1,992)	(0.64)	(0.70)
Total Balance Past Due	1,144	3	4	1,136	-25	5
	(318)	(0.50)	(0.64)	(303)	(0.68)	(0.81)
Total Credit Card Balance	4,926	-97	-51	4,799	-153	-51
	(881)	(0.14)	(0.28)	(908)	(0.22)	(0.71)
Total Credit Card Balance Past Due	1,674	-27	-13	1,516	-12	1
	(766)	(0.55)	(0.77)	(637)	(0.89)	(0.96)
Number of Collections	0.207	-0.015*	-0.014	0.237	-0.021	-0.019
	(0.105)	(0.05)	(0.17)	(0.113)	(0.13)	(0.15)
Total Collection Balance	146	-19*	-14	161	-25	-19
	(71)	(0.07)	(0.11)	(81)	(0.16)	(0.11)

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) presents the results for broad sample with 21 treatment states and 26 potential control states. Columns (4)-(6) presents the results for the narrow sample with 14 treatment states and 24 potential control states. For each expansionary definition, we present the 2010-2013 pre-reform mean outcome for the treated states and the 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.

Table 5
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		
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable (p-value in parentheses)	Weights: Match on Average Value of Dep. Variable (p-value in parentheses)	Pre-Reform Mean Outcome of Treated States (s.d. in parentheses)	Weights: Match on All Values of Dep. Variable (p-value in parentheses)	Weights: Match on Average Value of Dep. Variable (p-value in parentheses)
Total Balance	5,381	-4	90	5,340	-150	131
	(1,054)	(0.95)	(0.69)	(703)	(0.78)	(0.74)
Total Balance Past Due	675	-48	-31	594	44	61
	(477)	(0.17)	(0.24)	(157)	(0.37)	(0.16)
Total Credit Card Balance	2,484	-12	-41	2,350	6	9
	(600)	(0.55)	(0.66)	(454)	(0.96)	(1.00)
Total Credit Card Balance Past Due	1,209	-164	-111	782	3	-6
	(1,940)	(0.64)	(0.50)	(384)	(0.78)	(0.88)
Number of Collections	0.140	0.003	0.008	0.163	-0.000	-0.002
	(0.061)	(0.67)	(0.65)	(0.056)	(0.94)	(0.99)
Total Collection Balance	85	-13	-1	95	-11	-4
	(43)	(0.27)	(0.88)	(40)	(0.16)	(0.89)

Table 5 reports the estimates of 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) presents the results for broad sample with 21 treatment states and 26 potential control states. Columns (4)-(6) presents the results for the narrow sample with 14 treatment states and 24 potential control states. For each expansionary definition, we present the 2010-2013 pre-reform mean outcome for the treated states and the 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 additional to AK, IN, NH, and PA, DC and MA are dropped due to not having enough observations.

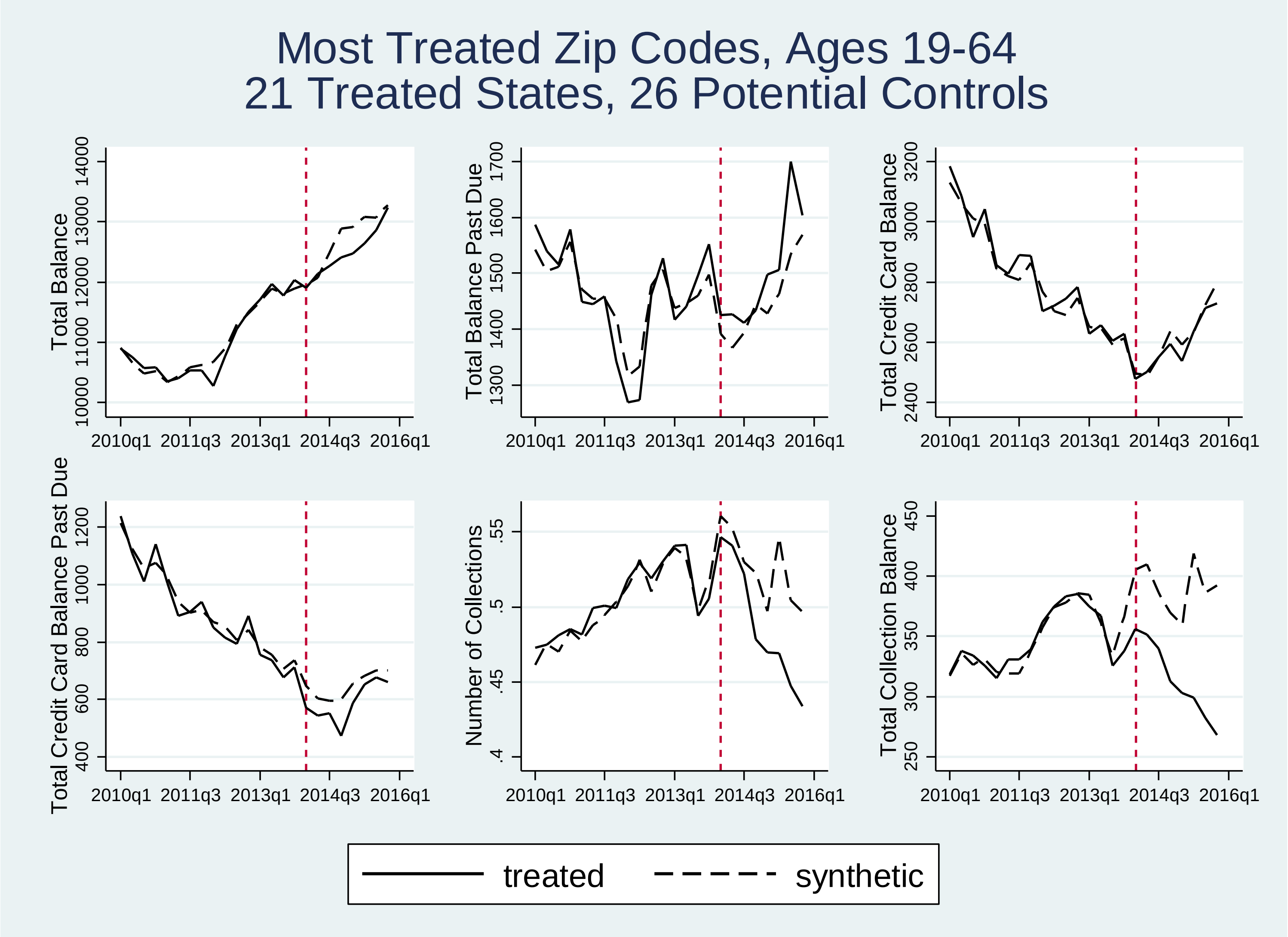


Figure 1. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Most Treated Zip Codes Using 21 Treated States, 26 Potential Control States

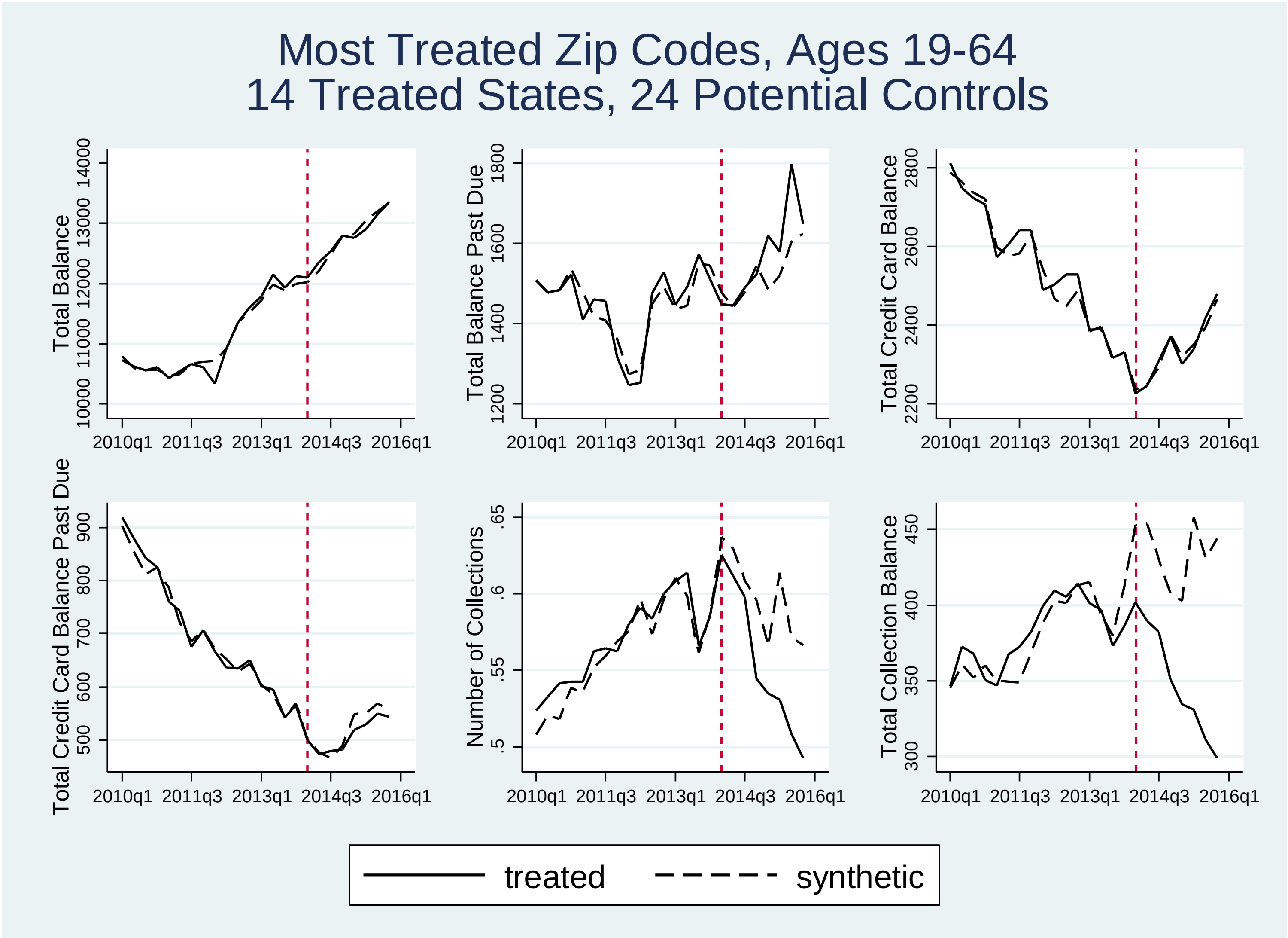


Figure 2. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Most Treated Zip Codes, Using 14 Treated States, 24 Potential Control States

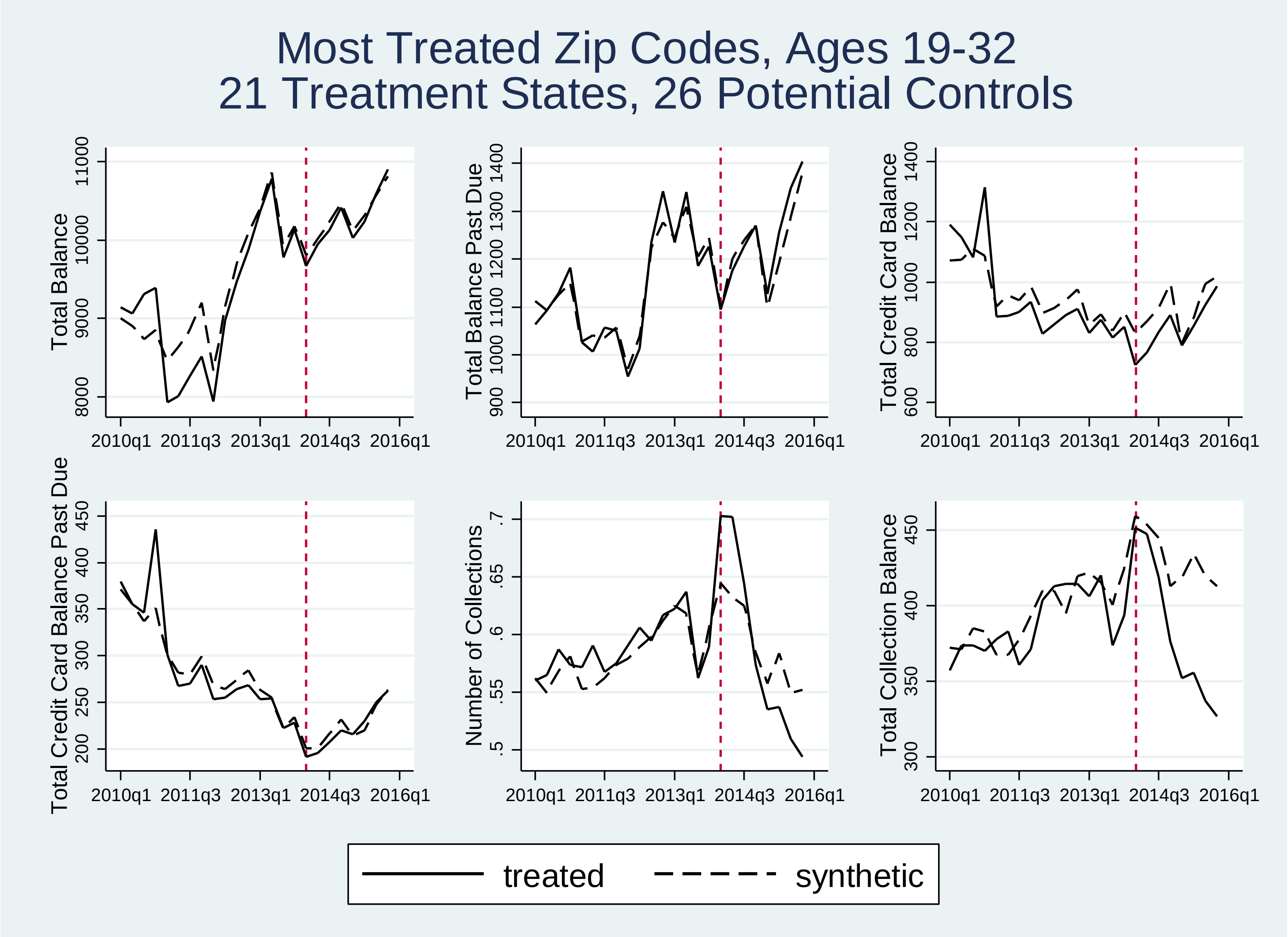


Figure 3. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Ages 19 to 32 in Most Treated Zip Codes Using 21 Treated States, 26 Potential Control States
DC, MA and HI are dropped (not enough observations for many credit categories).

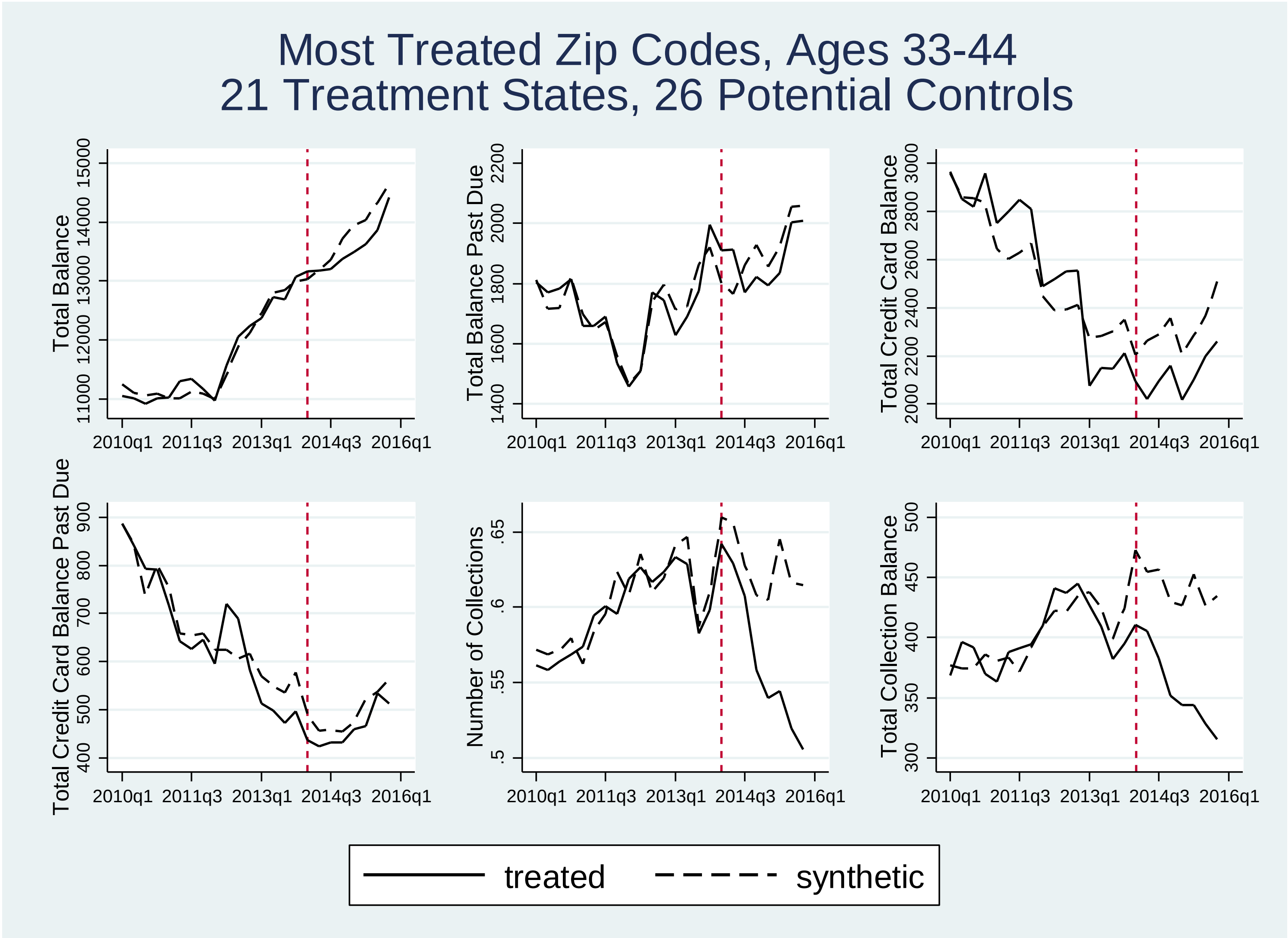


Figure 4. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Ages 33 to 44 in Most Treated Zip Codes Using 21 Treated States, 26 Potential Control States
DC and MA are dropped (not enough observations for many credit categories).

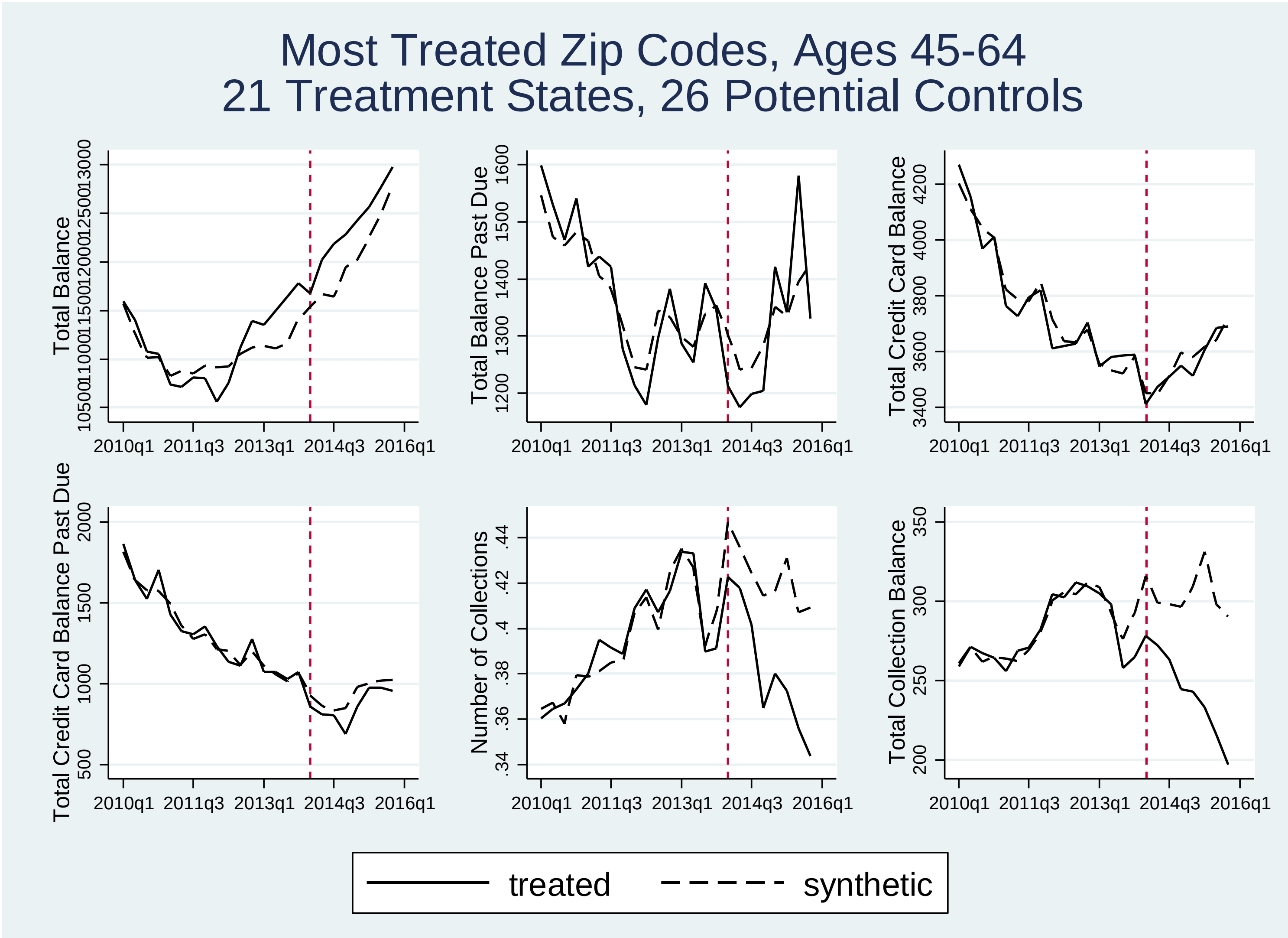


Figure 5. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Ages 45-64 in Most Treated Zip Codes Using 21 Treated States, 26 Potential Control States

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

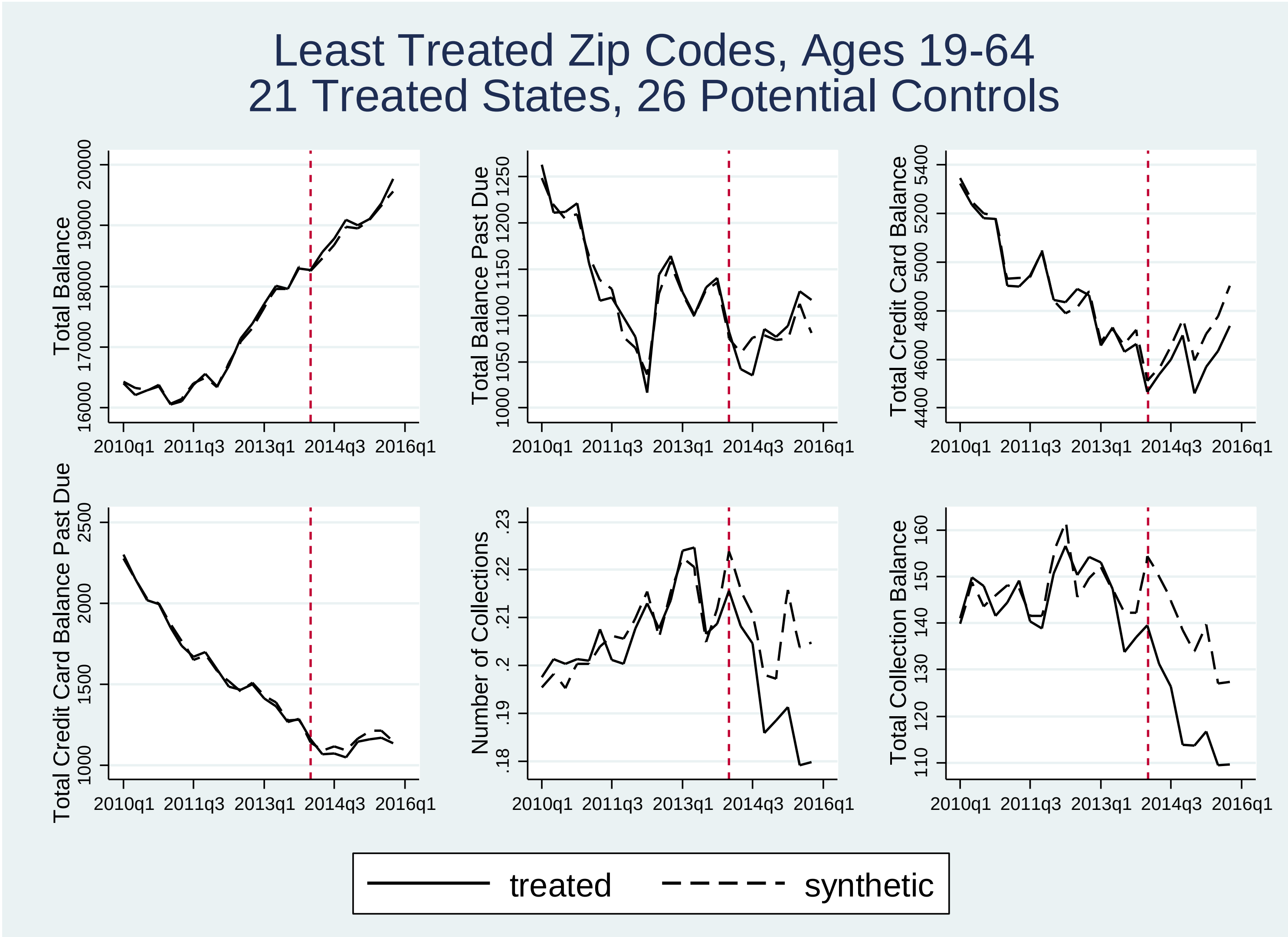


Figure 6. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Non-elderly in Least Treated Zip Codes Using 21 Treated States, 26 Potential Control States

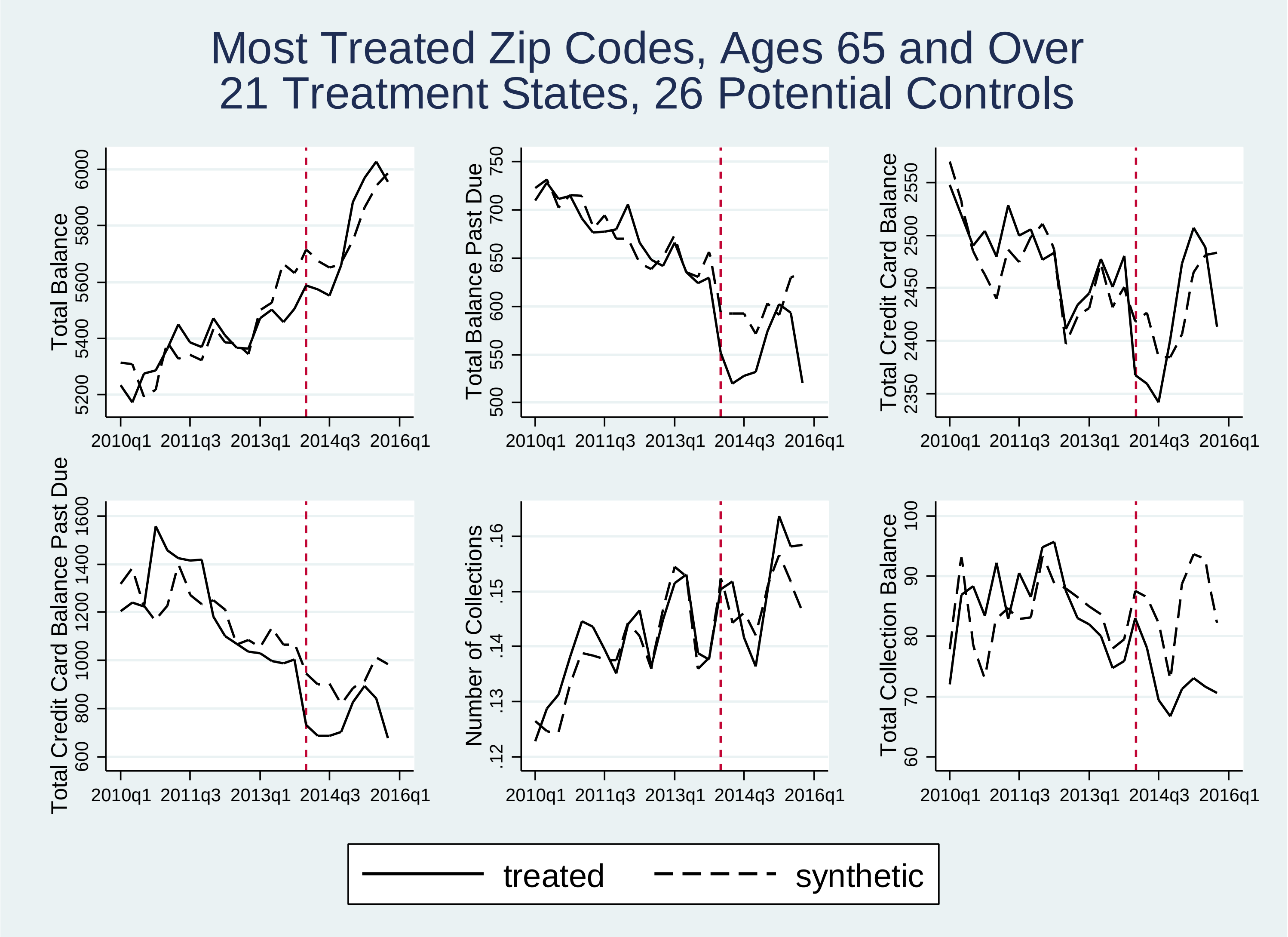


Figure 7. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Elderly in Most Treated Zip Codes Using 21 Treated States, 26 Potential Control States
DC and MA are dropped (not enough observations for many credit categories).

Appendix Table 1
Classification of Treatment and Control States

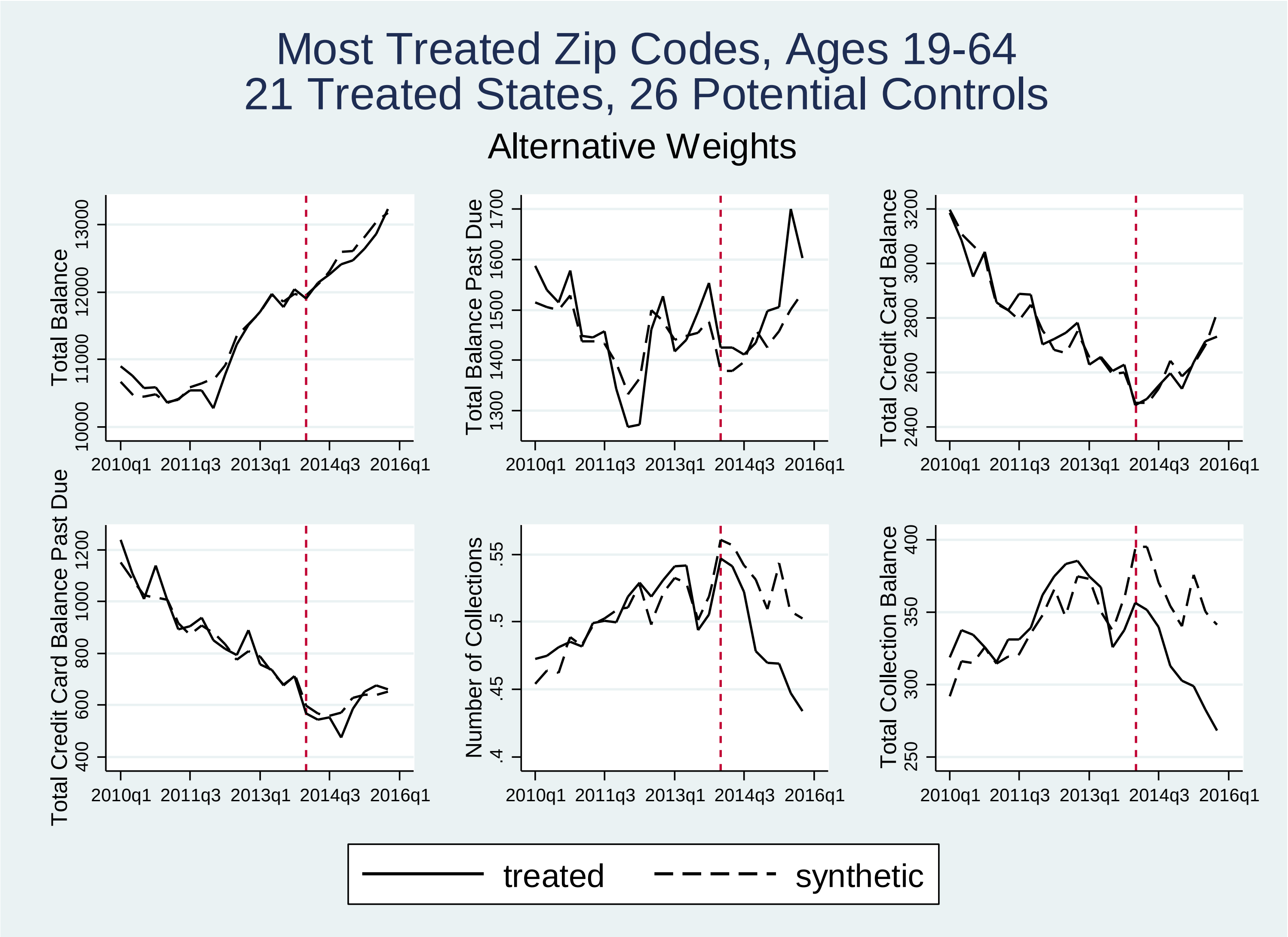
A. Did not expand under ACA as of 12/31/15 (21 states)	
Limited or no expansion pre-2014 (19 states)	
AL, FL, GA, ID, KS, LA, MS, MO, MT, NE, NC, OK, SC, SD, TN, TX, UT, VA, WY	
Broad sample:	Control
Narrow sample:	Control
Partial expansion pre-2014 (2 states)	
ME, WI	
Broad sample:	Control
Narrow sample:	Excluded
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
Partial expansion pre-2014 (7 states)	
AZ, CA, CT, HI, IA, MN, WA	
Broad sample:	Treatment
Narrow sample:	Excluded
Fully expanded pre-2014 (5 states)	
DE, DC, MA, NY, VT	
Broad sample:	Control
Narrow sample:	Control
Expanded between 2014:Q2 and 2015:Q4 (4 states)	
AK, IN, NH, PA	
Broad sample:	Excluded
Narrow sample:	Excluded

Appendix Table 2
State Weights for Synthetic Control for Each Dependent Variable
A. Weights Selected by Matching on Each Pre-2014 Value of Dependent Variable and Covariates

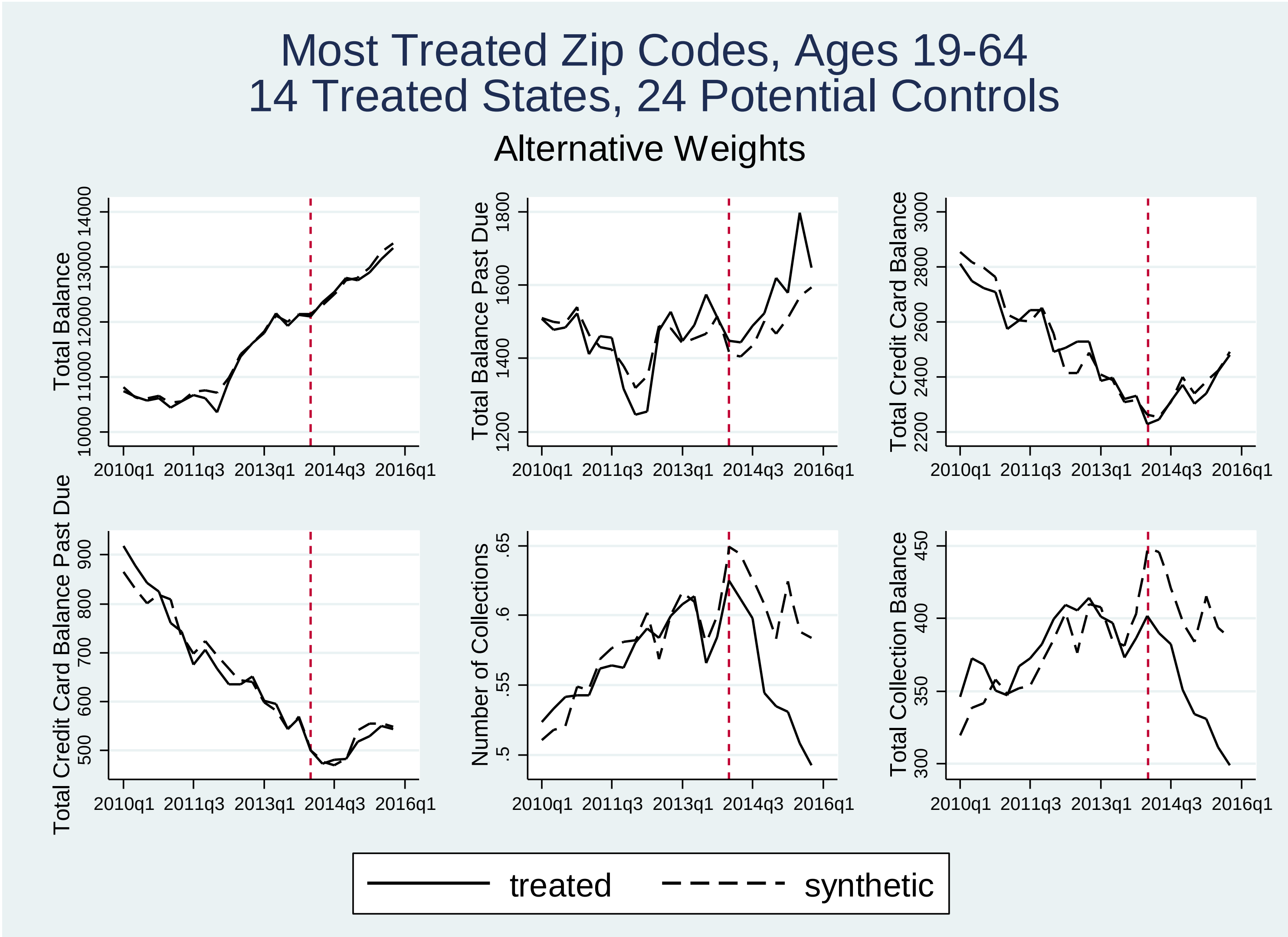
State	(1) Total Debt Balance	(2) Total Balance Past Due	(3) Total Credit Card Balance	(4) Credit Card Balance Past Due	(5) Number of Collections	(6) Total Collection Balance
Alabama	0	0	0	0	0	0
District Of Columbia	0	0	0	0.009	0.027	0.015
Florida	0.154	0.157	0.175	0.156	0.062	0.05
Georgia	0	0.174	0.036	0.273	0.107	0.212
Idaho	0	0.035	0.061	0.028	0.059	0.058
Kansas	0	0	0	0	0	0
Louisiana	0	0	0	0	0	0
Maine	0.064	0	0.123	0.02	0.058	0
Massachusetts	0.03	0	0.033	0.088	0.209	0.157
Mississippi	0	0	0	0	0	0
Missouri	0	0	0	0	0	0
Montana	0	0	0	0.146	0	0
Nebraska	0	0	0	0	0	0
New York	0.134	0.295	0.257	0.223	0.117	0.125
North Carolina	0.293	0	0.073	0	0.129	0.052
Oklahoma	0	0	0	0	0	0
South Carolina	0	0	0	0	0.006	0
South Dakota	0	0	0	0	0	0
Tennessee	0	0	0	0	0	0
Texas	0	0	0	0	0	0
Utah	0	0	0	0	0	0
Vermont	0.029	0	0.016	0	0	0
Virginia	0.231	0.095	0.09	0	0	0
Wisconsin	0.066	0.111	0.116	0	0.227	0.25
Wyoming	0	0.132	0.021	0.057	0	0.081

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

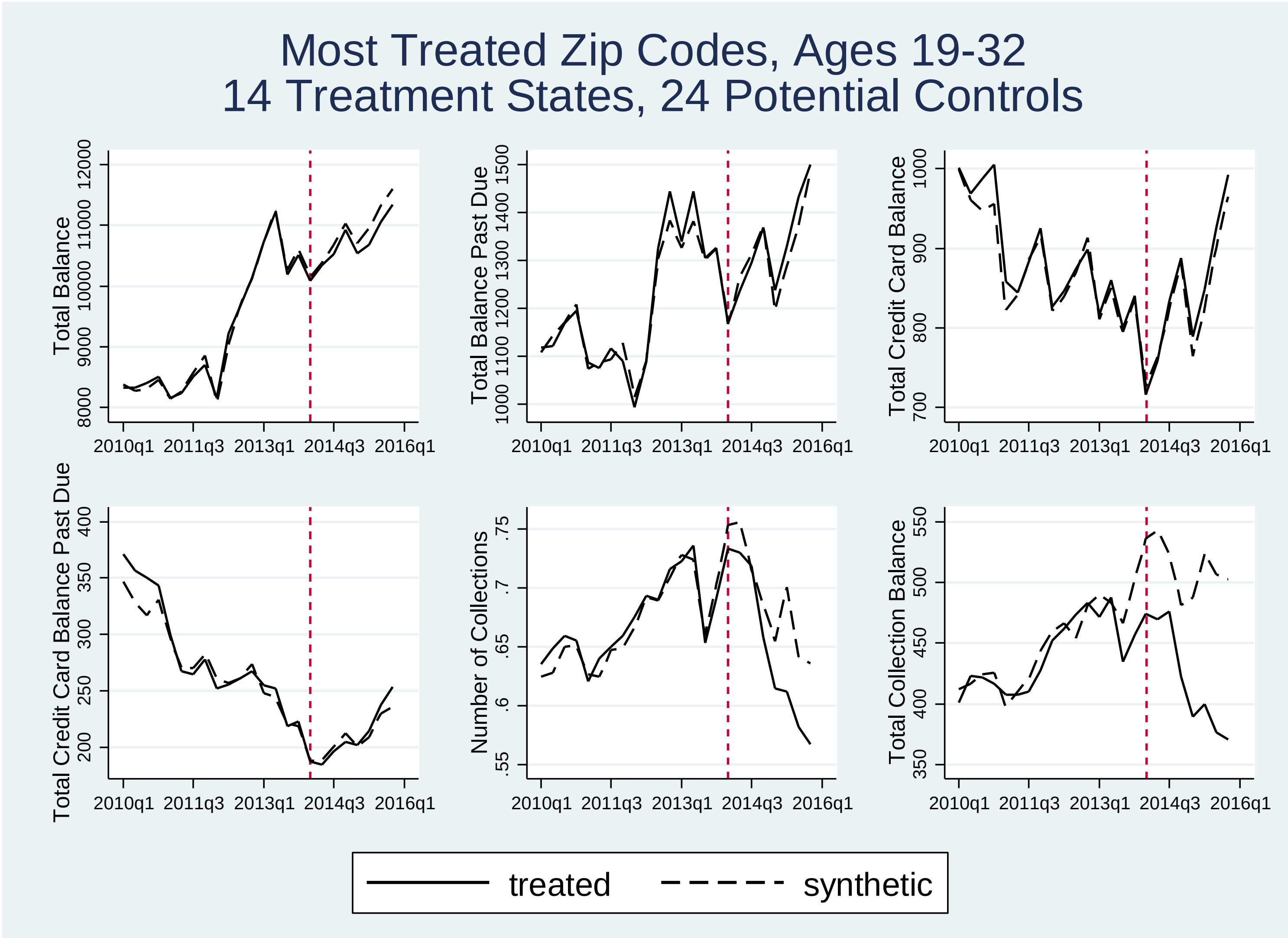
State	(1) Total Debt Balance	(2) Total Balance Past Due	(3) Total Credit Card Balance	(4) Credit Card Balance Past Due	(5) Number of Collections	(6) Total Collection Balance
Alabama	0	0	0	0	0	0
District Of Columbia	0.012	0.015	0	0.004	0.036	0.062
Florida	0.029	0.281	0.376	0.286	0	0.062
Georgia	0	0	0	0	0.088	0
Idaho	0	0	0	0.001	0.007	0.031
Kansas	0	0	0	0	0	0
Louisiana	0	0	0	0	0	0
Maine	0	0	0.139	0.091	0.077	0.029
Massachusetts	0.006	0.068	0.052	0.147	0.272	0.213
Mississippi	0	0	0	0	0	0
Missouri	0	0	0	0	0	0
Montana	0	0	0	0	0	0
Nebraska	0	0	0	0	0	0
New York	0.149	0.112	0.131	0.172	0	0.06
North Carolina	0.357	0.107	0.076	0.057	0.184	0.269
Oklahoma	0	0	0	0	0	0
South Carolina	0.014	0.039	0	0	0	0
South Dakota	0	0	0	0	0	0
Tennessee	0	0	0	0	0.109	0
Texas	0	0	0	0	0	0
Utah	0	0	0	0	0.034	0.022
Vermont	0.033	0	0	0	0	0
Virginia	0.275	0.162	0.182	0	0	0
Wisconsin	0.108	0.216	0.044	0.242	0.181	0.252
Wyoming	0.017	0	0	0	0.012	0



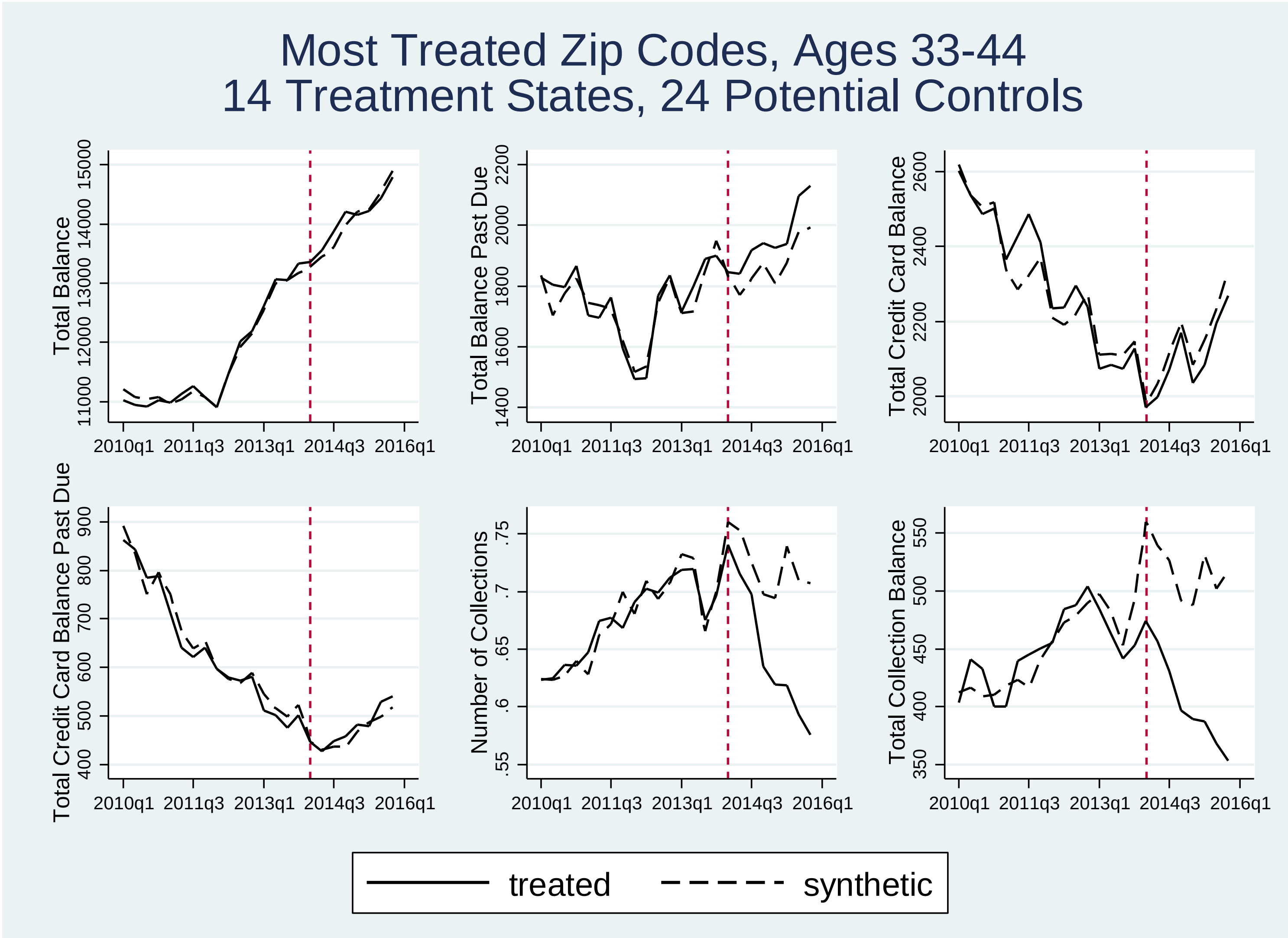
Appendix Figure 1. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing Using Alternative Weights (Match on Pre-Reform Average Lagged Outcome and 2013 Lagged Outcome) for Nonelderly in Most Treated Zip Codes Using 21 Treated States, 26 Potential Control States



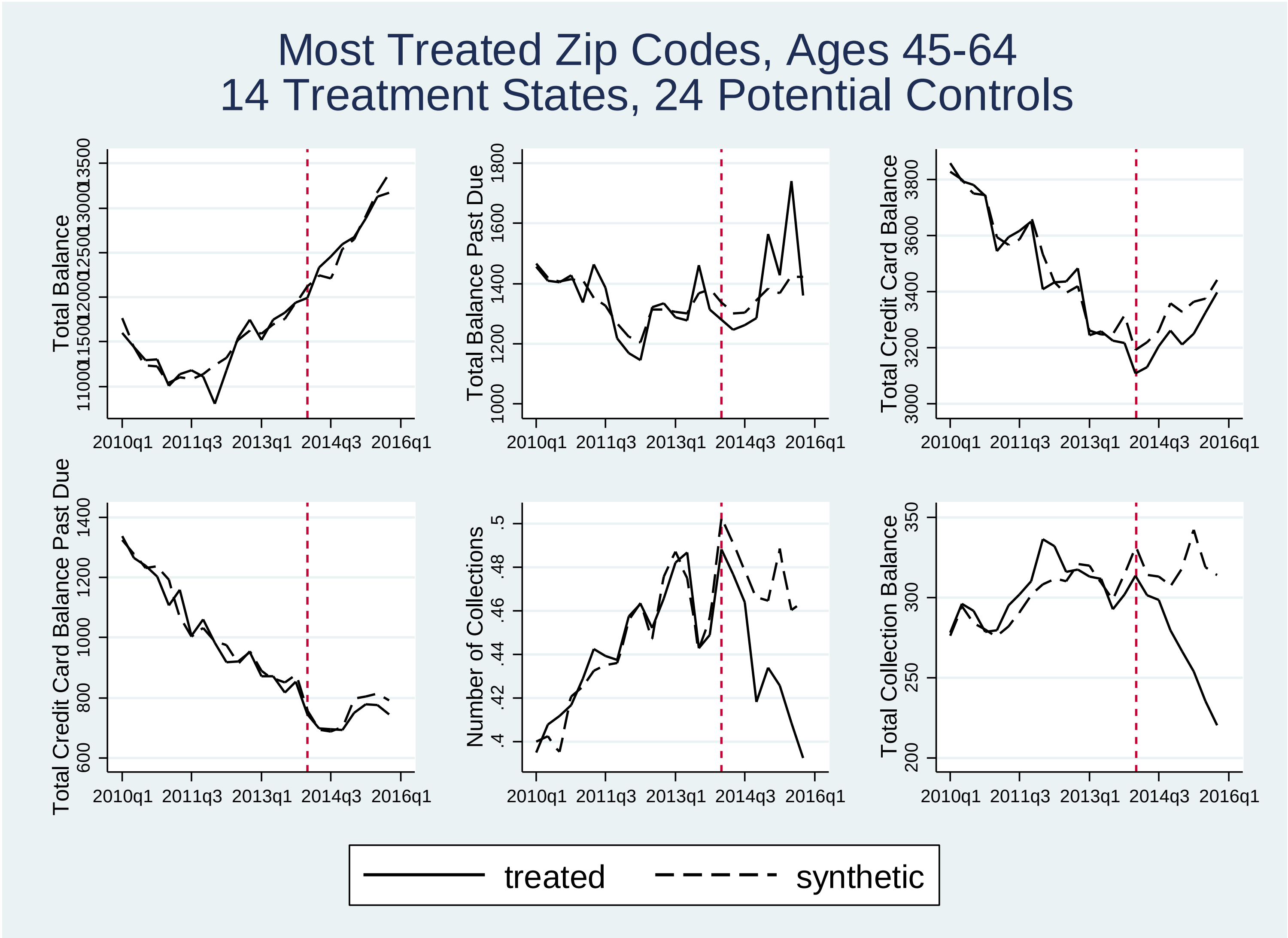
Appendix Figure 2. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing Using Alternative Weights (Match on Pre-Reform Average Lagged Outcome and 2013 Lagged Outcome) for Nonelderly in Most Treated Zip Codes Using 14 Treated States, 24 Potential Control States



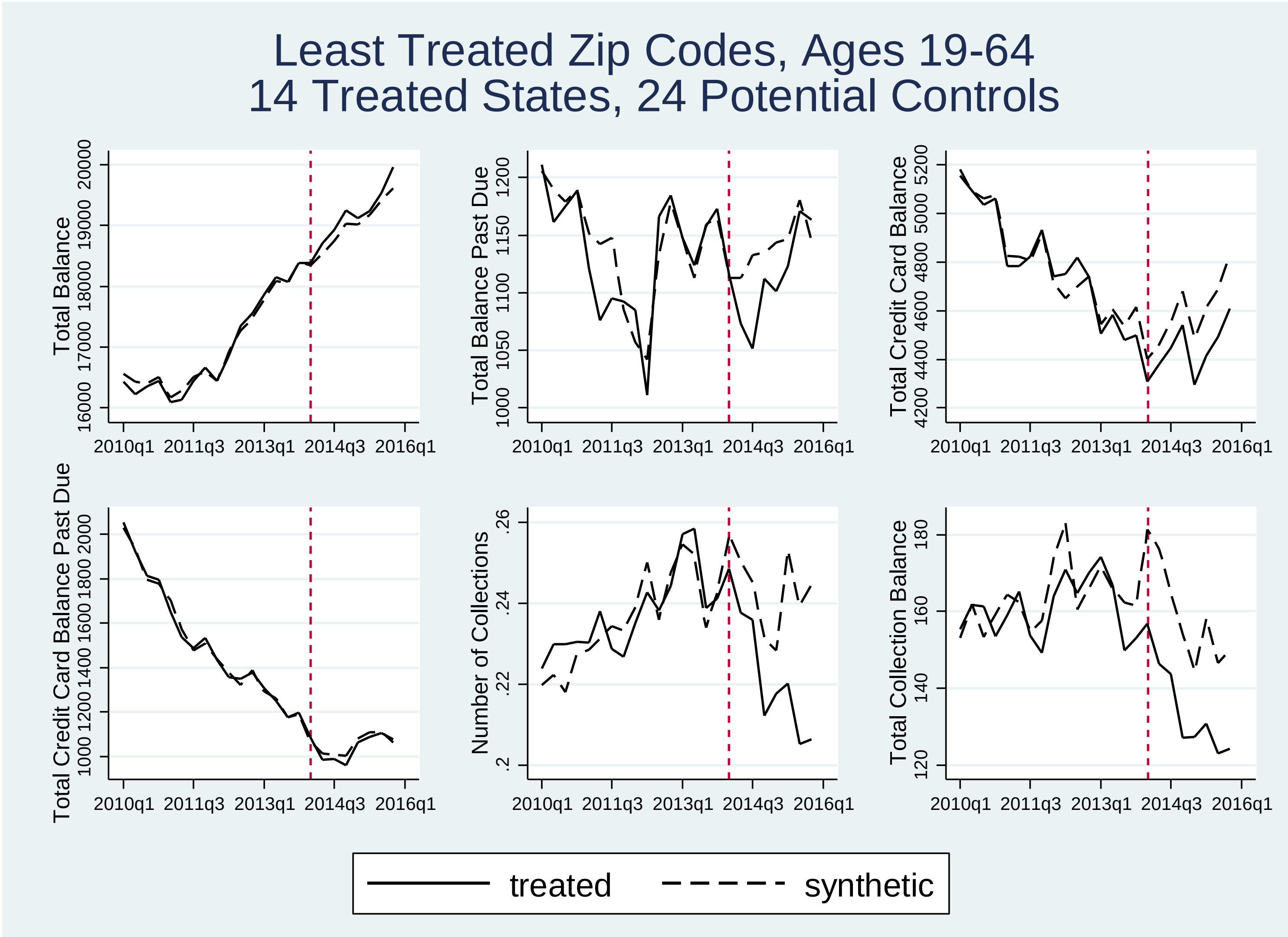
Appendix Figure 3. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Ages 19 to 32 in Most Treated Zip Codes Using 14 Treated States, 24 Potential Control States



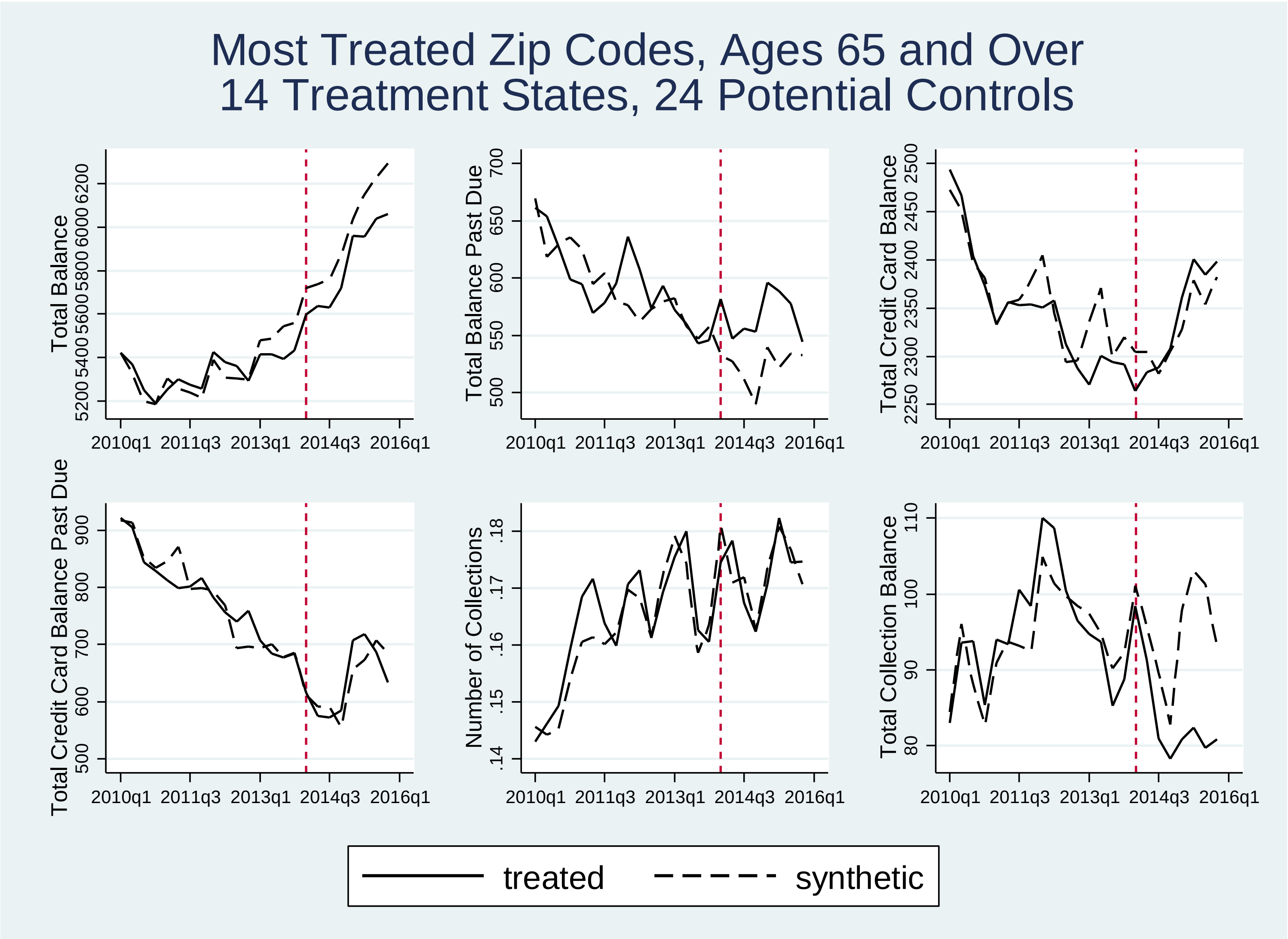
Appendix Figure 4. Synthetic Control Estimates of Effect of Medicaid Expansions on Indicators of Financial Wellbeing for Ages 33 to 44 in Most Treated Zip Codes Using 14 Treated States, 24 Potential Control States



Appendix Figure 5. 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



Appendix Figure 6. 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 7. 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