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**The Effects of the Massachusetts
Health Reform on Household
Financial Distress**

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The Effects of the Massachusetts Health Reform on Household Financial Distress*

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Abstract

A major benefit of health insurance coverage is that it protects the insured from unexpected medical costs that may devastate their personal finances. In this paper, we use detailed credit report information on a large panel of individuals to examine the effect of a major health care reform in Massachusetts in 2006 on a broad set of financial outcomes. We exploit plausibly exogenous variation in the impact of the reform across counties and age groups using levels of pre-reform insurance coverage as a measure of the potential effect of the reform. We find that the reform reduced the total amount of debt that was past due, the fraction of all debt that was past due, improved credit scores and reduced personal bankruptcies. We also find suggestive evidence that the reform decreased third party collections. The effects are most pronounced for individuals who had limited access to credit markets before the reform. These results show that health care reform has implications that extend well beyond the health and health care utilization of those who gain insurance coverage.

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1 Introduction

The primary purpose of health insurance is to protect the insured against the risk of incurring medical expenses. Individuals who lack health insurance are exposed to potentially catastrophic medical expenses should they become ill or injured. Therefore, the effectiveness of public policies that expand health insurance coverage depends fundamentally on whether such policies actually improve the financial security of those who gain coverage. Indeed, advocates of such policies often cite the financial risk faced by the uninsured as justification for government action. However, despite the widespread concern about the effect of foregoing insurance on the financial well-being of the uninsured, evidence on the causal relationship between insurance coverage and financial outcomes remains limited. Relatively few studies have attempted to use micro data and credible research designs to assess the effects of health insurance provision on financial outcomes. In this paper, we provide evidence on this relationship by evaluating how the provision of health insurance through a major state-level health policy reform affected a variety of financial measures such as credit score, total debt, delinquency, and personal bankruptcy.

In 2006, Massachusetts enacted an ambitious health care reform with the goal of achieving universal health insurance coverage within the state. In many ways, this reform served as the basis of the Affordable Care Act (ACA) that followed at the national level, combining a mandate for individual insurance with insurance market reforms and a broad expansion of subsidized coverage for low- and middle-income households. Because the Massachusetts law required all residents to obtain health insurance, counties and age groups with lower insurance rates prior to the reform experienced larger increases in coverage as a result of the reform. Following a strategy similar to Miller (2012a), we exploit this variation in the “stock” of uninsured residents at the time of the reform across counties and age groups to measure the effect of insurance coverage on financial outcomes.

We estimate the effect of the reform on financial outcomes using data on a large panel of individuals from a national credit reporting agency. These data include credit report information on a 5 percent primary sample of all adults in the United States with a credit report and every individual with the same mailing address as the sampled adult. In Massachusetts

and states in the New England census division alone, this dataset contains about 1.3 million individuals, and provides information on financial outcomes ranging from credit scores to personal bankruptcy.

We find that the Massachusetts reform improved financial outcomes across many dimensions: it improved credit scores, reduced delinquencies, lowered the fraction of debt past due and reduced the incidence of personal bankruptcy. We find a particularly pronounced reduction in large delinquencies of over \$5,000, but observe almost no effect on delinquencies of smaller amounts. We also find suggestive evidence that the reform reduced total debt and decreased third-party collections, with most of the reduction in collections resulting from a decrease in the probability of having a large (greater than \$1000) amount in collections. Additionally, we conduct this analysis separately for individuals who had low and high credit scores prior to the reform. The effects of the reform on credit score, personal bankruptcy, and delinquency are most pronounced for those whose credit scores were lower before the reform, but those with higher credit scores (and therefore, better access to credit), experienced a larger relative decline in total debt. Placebo tests find no significant effect of the reform on the financial outcomes of the elderly (whose insurance status would not have been affected by the law), business bankruptcies, the poverty rate, median income, or the unemployment rate, indicating that these results are not driven by concurrent but unrelated improvements in the economic environment in Massachusetts.

Previous analysis has documented the correlation between insurance status and financial outcomes (e.g., Gross and Souleles (2002)) or shown that individuals with high medical expenses are over-represented among bankruptcy filers (e.g., Dranove and Millenson (2006), Himmelstein et al. (2005)). However, these studies are unable to address the common empirical problem that financial outcomes and health insurance status or medical bills may be correlated because of unobserved factors such as risk preference, or that financial shocks may themselves cause poor health. In fact, Mahoney (2011) shows that the presence of generous bankruptcy protection itself serves as a substitute for health insurance coverage and affects the decision of individuals to obtain insurance.

Three recent studies use experimental or quasi-experimental methods to overcome the

endogeneity of insurance status to financial well-being. These studies have focused on three particular groups: those living and poverty, pregnant women and children, and the elderly. The landmark Oregon Health Insurance Experiment (Finkelstein et al. (2012)) surveyed adults with incomes under the Federal Poverty Level (FPL, \$10,400 for an individual in 2008 at the time of the experiment) who gained health insurance coverage through a lottery and found that they reported less financial strain and fewer medical bills than those who did not receive coverage through the lottery. Using administrative data from a credit bureau, the study also found that lottery winners had significantly fewer bills sent to third-party collectors and owed less in medical debt. The study focused only on the poor and did not find conclusive evidence linking health insurance coverage to personal bankruptcy, delinquency, credit scores, or overall debt levels.

Gross and Notowidigdo (2011) also use state-level variation in the timing of Medicaid eligibility expansions as a natural experiment to investigate the link between personal bankruptcy and health insurance coverage. The authors find that increasing Medicaid eligibility by 10 percentage points reduces personal bankruptcy by about 8 percent. However, Gross and Notowidigdo (2011) did not have data on other relevant measures of financial stability, such as debt and delinquency, that are significantly more common than personal bankruptcy.

Barcellos and Jacobson (2015) use the discontinuity in insurance coverage that occurs at age 65, when individuals enroll in Medicare, to examine how health insurance coverage affects financial outcomes. The authors find that when individuals turn 65, their average out-of-pocket medical expenditures drop by over 30 percent and the fraction of the population with out-of-pocket medical expenses exceeding their income falls by more than half. The authors also find that the amount owed in medical bills and the fraction reporting they have been contacted by a collection agency also drops substantially.

A major advantage of analyzing the reform in Massachusetts is that we are able to examine the effects of a health insurance policy designed to expand insurance coverage to the entire population of uninsured residents, rather than only those uninsured with incomes below the FPL (as in the Oregon Health Insurance Experiment), low-income pregnant women

and children (as in Gross and Notowidigdo (2011)), or the elderly (as in Barcellos and Jacobson (2015)). Table 1 compares the incomes of those who gained coverage through the Massachusetts reform with early evidence on the coverage gains in the first year of the Affordable Care Act using information from Sommers et al. (2014) and the treatment group in the Oregon Health Insurance Experiment. Over 70 percent of those who gained coverage through the Affordable Care Act earned above the FPL, about the same fraction as those who gained coverage through the Massachusetts reform. In contrast, the Oregon Health Insurance Experiment only included those earning incomes below the poverty level. The distribution of incomes of those who gained health insurance coverage is potentially important for evaluating the effect of coverage on financial outcomes. For example, personal bankruptcy may be relatively more attractive for the non-poor than for the poor because the non-poor may have more assets that are protected by bankruptcy. Similarly, the poor may receive more charity care from hospitals than the non-poor. Although other features of Massachusetts (such as its demographic, economic, or health care supply characteristics) may imply that its experience is unique, the fact that the income mix the Massachusetts policy experiment closely resembles the ACA makes it of particular interest to the ongoing debate surrounding health care reform.

Another advantage of our study is that we use broad measures of financial risk that capture changes in financial well-being on many margins. Although a considerable amount of attention has been paid to measures of severe financial distress such as bankruptcy, much of the financial risk of foregoing health insurance may manifest in less dramatic events such as paying bills late or increasing credit card debt. Because we consider outcomes ranging from credit score to severe delinquency, our analysis provides a broad view of the effect of health care reform on household financial stability.

Our approach is also able to contribute to the analysis of fairly rare events such as bankruptcy. One reason the Oregon Health Insurance Experiment may have not found significant effects of gaining coverage on personal bankruptcy is that bankruptcy is a rare event and relatively few (about 10,000) individuals gained coverage as part of the experiment. In contrast, the Massachusetts reform expanded coverage to over 400,000 individuals. Fur-

thermore, we employ a large administrative dataset that allows us to observe over 350,000 individuals in Massachusetts each year. The size of the expansion and dataset allow us to credibly investigate the effect of health insurance coverage even on relatively infrequent events and on sub-state populations and age groups that would otherwise be difficult to measure.

While a great deal of research has focused on the effects of health insurance on health and health care utilization, far less attention has been placed on the pre-eminent purpose of health insurance which is to protect individuals from financial distress. Our results indicate that public policies that expand health insurance coverage do have pronounced effects on financial stability and well-being. We find that the reform in Massachusetts had an impact across a broad set of financial measures, even affecting households' future access to credit markets through improved credit scores. These results suggest that the financial implications of health care reform extend well beyond patients and health care providers and into many areas of the economy.

2 Health Insurance and Financial Security

Spending on medical care is a large and uncertain expense for individuals and families without health insurance, and total spending on health has increased over time (Gruber and Levy (2009)). In surveys, the uninsured consistently report that medical expenses represent a substantial financial burden. For example, a study by the Commonwealth Fund (Doty et al. (2008)) reports that 36 percent of uninsured individuals surveyed were paying off medical bills over time, and of these individuals, 62 percent reported having over \$2000 of outstanding medical debt and 20 percent reported having over \$8000 of outstanding medical debt. Among respondents who reported paying off medical bills over time, 47 percent of the uninsured and 26 percent of the insured stated that they had exhausted their savings paying for medical bills. Among the same group, 40 percent of the uninsured and 16 percent of the insured reported that they had foregone other necessities such as food, heat, or rent in order to pay medical bills. About 30 percent of uninsured individuals who reported difficulties paying

medical bills took on credit card debt to pay for medical care, and about 10 percent took out another type of loan to cover medical expenses.

Despite the important financial component to health insurance coverage, most studies evaluating public policies to expand coverage have limited their inquiry to the relationship between coverage and the use of health care or measures of health. In general, these studies have found that insurance coverage increases the consumption of health care services and has mixed effects on direct measures of health.¹ A smaller literature uses variation in public health insurance coverage to evaluate how such programs affect household consumption of non-health goods and financial outcomes. Evidence from the expansion of the State Children’s Health Insurance Program (SCHIP) suggests that low-income households with children who gained public insurance coverage increased their consumption and also saved more for retirement (Leininger et al. (2010)). Gross and Notowidigdo (2011) use the expansion of Medicaid eligibility in the 1990s as a natural experiment and find that increasing Medicaid eligibility by 10 percentage points reduced personal bankruptcy by about 8 percent. In the Medicare context, Finkelstein and McKnight (2008) analyze the introduction of Medicare in 1965 and found that the program led to a dramatic reduction in the out-of-pocket costs of medical care for the elderly and Engelhardt and Gruber (2011) find that Medicare Part D reduced out-of-pocket spending for Medicare beneficiaries. Barcellos and Jacobson (2015) use the discontinuity in Medicare eligibility at age 65 to show that gaining Medicare coverage lowers out-of-pocket expenditures and financial strain.

The Oregon Health Insurance Experiment (Finkelstein et al. (2012), Baicker et al. (2013)) provides the most credible evidence to date that the absence of health insurance coverage harms financial well-being. This experiment surveyed participants who gained Medicaid coverage through a lottery and compared them to a control group that did not receive Medicaid coverage. In the control group, 36 percent of those surveyed reported borrowing money or skipping other bills in order to pay for medical care. The provision of insurance reduced this probability by 15.8 percentage points, or 44 percent, in the first year. Twenty-eight percent of the control group had severely delinquent medical bills that were under the supervision

¹See Levy and Meltzer (2008) and Buchmueller et al. (2005) for an overview.

of a third party collection agency; the average amount of these medical bills in collection was \$2000. Medicaid coverage reduced medical collections by an average of \$390 in the first year, reduced the probability of having a medical collection by 6.4 percentage points (23 percent), and reduced the probability of having any medical debt by 18 percentage points (30 percent). Medicaid reduced out-of-pocket spending by approximately \$215, despite substantially increasing the use of health care services, and reduced the probability of having “catastrophic” health costs exceeding 30 percent of household income by 4.5 percentage points, an 82 percent reduction relative to the control group average of 5.5 percent. The study did not, however, find conclusive evidence linking health insurance coverage to other policy-relevant measures of financial well-being including personal bankruptcy, delinquency, or total debt. In general, confidence intervals for these outcomes were large, suggesting the need for studies using larger samples.

Our paper contributes to our understanding of the role of health insurance coverage in several ways. First, we evaluate an expansion of health insurance to almost *all* uninsured residents in Massachusetts. This provides an opportunity to study the effect of health insurance coverage among the general uninsured population, rather than on special sub-groups such as pregnant women and children (as in Leininger et al. (2010) and Gross and Notowidigdo (2011)), those in poverty (as in the Oregon Health Insurance Experiment), or the elderly (as in Barcellos and Jacobson (2015) and Finkelstein and McKnight (2008)).² Most of those who gained coverage through the ACA in the first year did not fall into these categories (see Table 1 and Congressional Budget Office (2012)), making these studies of limited use in predicting the effects of such broad-based expansions. Second, we analyze a broad set of financial outcomes from a large administrative database, in contrast to many of the existing studies that look exclusively at out-of-pocket medical costs using survey data. This allows us to evaluate the effect of the Massachusetts reform on both severe negative outcomes, such as bankruptcy, but also on less dramatic events, such as paying a bill late. Because our dataset is large, we are able to detect even small changes in these outcomes. Finally, the context of our study is a major state-level reform that closely resembles the ACA, making this analysis

²In a non-United States context, this topic has been discussed by Wagstaff and Lindelow (2008) and Shigeoka (2014).

immediately relevant for the ongoing debate surrounding the current federal program.

3 The Massachusetts Reform

In April of 2006, Massachusetts enacted a major health reform act with the goal of achieving universal health insurance coverage within the state. The law mandates that all Massachusetts residents must purchase health insurance that meets a minimum standard of coverage if such coverage is affordable, or pay a non-compliance fee. Standards of affordability and coverage are set forth by a newly-formed organization that also serves as a clearinghouse for insurance plans, the Commonwealth Health Insurance Connector Authority. Failure to purchase health insurance results in the loss of the personal exemption to the income tax, which was valued at \$219 for an individual in 2007. In 2008, monthly penalties for not having insurance coverage were added. These penalties vary with income and can reach up to half of the monthly cost of the least-expensive available plan. For example, in 2012 the annual penalty for not having health insurance for an individual older than 26 who made above 300 percent of the FPL was \$1,260.

The reform combines the individual mandate with an expansion of the Massachusetts Medicaid program, called “MassHealth,” and new subsidies for individuals earning up to 300 percent of the FPL to purchase insurance.³ The MassHealth expansion raises the family income limit for children, expands coverage to some low-income workers, and removes caseload caps on people living with HIV, the long-term unemployed, and the disabled. The law also restores vision and dental benefits that had been cut from MassHealth in 2002. In addition to the expansion of MassHealth, a new program, “Commonwealth Care,” provides free insurance to families earning up to 150 percent of the FPL, and tiered subsidies for insurance for families earning up to 300 percent of the FPL. In addition to offering low-income plans, the Connector Authority offers special low-cost plans for young adults between the ages of 19 and 26 who do not have access to employer-based coverage and requires that private health insurance providers allow young adults to remain on their parents’ plan for

³This is in contrast to the Affordable Care Act, whose subsidies extend to 400 percent of the FPL.

up to two years after they cease to be dependents.

The new law also requires employers to participate in providing health care. All employers with over 10 employees are required to contribute to their employees' health insurance either by providing an insurance plan of their own, or by paying at least 33 percent of their employees' health insurance premium costs. Employers who fail to do so must pay a "fair share" assessment of up to \$296 per uninsured employee. For residents not enrolled in a group health plan, a new small-group market was created by merging the non-group and small-group insurance markets. This reform permits such residents to purchase insurance coverage from less expensive small-group plans. For more details on the Massachusetts reform and its implementation, see Raymond (2007) and Gruber (2008).

These combined policies led to a large increase in insurance coverage in Massachusetts. The first panel of Figure 1 plots the Current Population Survey (CPS) estimates of the uninsurance rate among non-elderly adults in Massachusetts and in other states in New England from 1999 to 2012. From 1999 to 2006, the uninsurance rate in Massachusetts was about 13 percent in both Massachusetts and New England. Then, in 2007, the percent uninsured in Massachusetts dropped dramatically, to about half its level or 6.5 percent. By 2012, the uninsurance rate in Massachusetts had fallen to 5 percent, but had remained at 13 percent in New England. Massachusetts currently has the highest rate of insurance coverage in the United States.

In many ways, the Massachusetts reform served as the basis of the ACA that followed it in 2010: it employed a similar combination of policies and extended coverage to a similar mix of low income and middle income uninsured individuals. As such, there is a natural interest in studying the Massachusetts reform as a way to gain insight into the potential effects of the Affordable Care. However, there are several ways in which the experiences of the Massachusetts reform may not correspond well to the expansions occurring through the Affordable Care Act. First, Massachusetts differs demographically from other states across several dimensions. For example, Massachusetts ranks within the top ten states for educational attainment and income and even prior to the reform, Massachusetts had the sixth highest health insurance coverage rate of all states. Second, at the time of the reform,

Massachusetts had the largest number of physicians per capita of any state,⁴ which may have made it easier to absorb the newly-insured population into the health care system. Third, Massachusetts had in place a relatively generous uncompensated care program prior to the reform that covered emergency department bills for many low income uninsured residents. As a result, the uninsured may not have been as exposed to as much financial risk and so we might expect larger effects in other states. Although we believe analysis of the Massachusetts reform can provide useful insight into the effects of the ACA in states with similar demographic and health care supply features (e.g., other states in New England), such analysis may be of limited use in predicting the effects of the ACA in states that bear little resemblance to Massachusetts on these dimensions.

4 Empirical Approach and Preliminary Evidence

Our empirical strategy relies on leveraging the differential effect of the Massachusetts reform not only across states, but across different groups of people within Massachusetts. This approach is similar to Miller (2012a) and Finkelstein (2007). In the year before the reform, there was significant variation in insurance coverage across counties and age groups. The second panel of Figure 1 plots the percent uninsured in Massachusetts across time for two age groups: individuals age 18 to 39 (indicated by the black line) and individuals age 40 to 64 (indicated by the grey line). While both groups experienced a reduction in uninsurance following the reform, the 18 to 39 age group experienced a much larger reduction of about 13 percentage points, while the 40 to 64 age group experienced a reduction of 4 percentage points. By 2011, the uninsurance rate in these two groups had converged, with both groups exhibiting an uninsurance rate of about 5 percent. However, because the younger group began at much lower coverage rates, the gain in coverage for this group was substantially larger.

To measure variation in initial coverage levels across both age groups and counties, we use data from the Small Area Health Insurance Estimates (SAHIE). These model based estimates

⁴See <http://www.census.gov/statab/ranks/rank18.html>.

produced by the Census Bureau provide information on the uninsurance rates by county and for two age groups (18 to 39 and 40 to 64).⁵ The Census produces these estimates using a Bayesian model that combines direct estimates of the uninsurance rate from 3 years of data pooled from the CPS with predicted values based on characteristics of the county (number of IRS tax exemptions, food stamp participation, number of Medicaid/SCHIP participants).⁶ There are 14 counties in Massachusetts, resulting in a total of 28 levels of variation in the pre-reform uninsurance rate. The histogram in Figure 2 shows this variation of the 2005 uninsurance rate among county-age groups in Massachusetts. The uninsurance rate varied from below 10 percent to over 25 percent. Because the reform requires all residents to purchase insurance, county-age groups where a large fraction of Massachusetts residents were uninsured before the reform had the potential to experience larger increases in coverage than county-age groups where coverage was already quite high. For example, over 92 percent of Bristol county residents age 40 to 64 had insurance coverage even before the reform was enacted; at most, the reform could increase coverage among this group by just under 8 percentage points. In contrast, almost a quarter of Suffolk residents age 18 to 39 were uninsured in 2005, resulting in a relatively large population who could have gained insurance coverage as a result of the reform. It is this variation in the potential effect of the reform that we use to measure the impact of the reform on financial outcomes.

There are several mechanisms, both direct and indirect, through which the reform may have affected financial outcomes. By expanding insurance coverage, the reform may have improved financial outcomes by reducing the risk that individuals faced an unexpected out-of-pocket medical expenses. The risk reduction aspect of insurance suggests that there may be large effects of insurance on the small subset of individuals who experienced a health shock such as a car accident or a cancer diagnosis. Financial outcomes may have also been improved through income effects, as much of the new coverage was heavily subsidized. For example,

⁵Because the elderly were unaffected by the reform, we drop individuals from the sample once they turn 65, but use the elderly as a placebo test in later robustness checks. In the appendix, we present results that exclude individuals that turn 65 at *any* point in the sample. Results are similar to those presented in the main text.

⁶More information on this methodology is available at <http://www.census.gov/did/www/sahie/methods/20052007/index.html>.

uninsured residents who were paying for health care out-of-pocket may have experienced substantial income effects as they became covered by subsidized health insurance plans with low or no co-payments. In contrast to the pure risk reducing aspect of insurance, these income effects may be more widespread, affecting even those individuals who did not experience an illness or injury. The reform may have also improved financial well-being through indirect means; for example, by improving the health of Massachusetts residents, resulting in higher productivity and higher wages, or by changing employment patterns in ways that affect household finances. For example, Kolstad and Kowalski (2012a) find that the Massachusetts reform led employers to increase the frequency with which they offered health insurance and lower wages by an offsetting amount; Garthwaite et al. (2013) and Pashchenko and Porapakkarm (2013) show that public health insurance coverage leads individuals to reduce their labor supply. These changes in employment may affect income and, ultimately, financial outcomes. Finally, the reform may have “crowded out” less generous private coverage with more generous public coverage, lowering the out-of-pocket costs of medical care even to those who were insured before the reform.

Survey data from Massachusetts provides some evidence that the reform improved the financial situation of Massachusetts residents who were affected. Long et al. (2012) use data from the Massachusetts Health Reform Survey, a survey funded by the Blue Cross Blue Shield of Massachusetts Foundation conducted annually beginning in 2006. The authors find that after the reform, Massachusetts residents report fewer problems paying medical bills and spent less on out-of-pocket medical expenses than those surveyed in 2006 as the reform was being implemented. The same survey finds a reduction in the fraction of respondents reporting delaying or foregoing health care because of costs. In contrast, analysis of a survey of bankruptcy filers Himmelstein et al. (2011) finds no conclusive effect of the reform on medical bankruptcy; however, this study was limited by a small size (only 44 survey respondents in 2007 and 199 in 2009) and severe non-response (only approximately 40 percent of the surveyed bankruptcy filers responded).

Administrative data on hospitalizations from the Massachusetts Acute Hospital Case Mix Database show that prior to the reform, the uninsured faced potentially large out-of-pocket

hospital charges. In 2005, about 8.3 percent of emergency room (ER) and inpatient hospitalizations were “self-paid,” i.e., were paid for out-of-pocket by the uninsured. Although the charges for self-paid hospitalizations are often negotiated for low-income uninsured patients, this category excludes uninsured patients with incomes under 200 percent of the FPL who would have been covered by the uncompensated care pool; that is, it excludes uninsured patients from whom the hospital has decided *a priori* not to collect charges. In 2005, estimates from the CPS show that there were about 545,000 total uninsured people living in Massachusetts. In the same year, there were 13,365 self-paid hospital visits and 218,900 self-paid ER visits, resulting in total charges of over \$435 million. These charges represent about \$800 per uninsured person in 2005 alone, suggesting that the uninsured had significant exposure to out-of-pocket hospital costs. The uninsured who actually used such services were charged about \$16,000 on average per hospital admission and \$1,000 per outpatient emergency room visit. These measures exclude the approximately two-thirds of medical spending accounted for by non-hospital charges (e.g., doctor’s visits, physical therapy and other outpatient care, pharmaceuticals, and other medical expenses, see Carper and Machlin (2013)); more comprehensive measures of total medical charges levied on the uninsured are likely to be much larger.

These data also provide some evidence that the reform reduced the medical expenses of the uninsured as they gained coverage and that it did so differentially across counties and age groups. As patients gained coverage through the reform, there was a substantial reduction in the fraction of hospitalizations that were self-paid. The first panel of Figure 3 plots the fraction of hospitalizations and ER visits that are self-paid over time. In 2003, about 9 percent of hospital and ER visits were self-paid. This fell to a little over 4 percent by 2008. The change over this period was particularly large among groups that had high rates of uninsurance before the reform. The second panel of Figure 3 displays the change in the fraction of hospitalizations that were self-paid against the pre-reform uninsurance rates of the county-age groups. Groups for which the reform had a larger potential effect—that is, groups whose insurance coverage was relatively low prior to the reform—experienced the sharpest reduction in self-paid hospital visits. The number of hospitalizations itself may be directly affected by insurance coverage. For example, hospitalizations or emergency department visits

may fall if the uninsured receive more preventive care or receive care in a different setting such as a physician’s office; see Kolstad and Kowalski (2012b) and Miller (2012a) for evidence of this effect. However, these results provide suggestive evidence that the reform affected out-of-pocket expenses for the uninsured and that these effects were larger among groups where the potential effect of the reform was stronger.

Rather than using both sources of variation (within Massachusetts or across state), most other studies on the Massachusetts reform have used only one level of variation or the other. In this paper, we focus on the triple difference specification exclusively for several reasons. First, it allows us to use *a priori* knowledge on who was affected by the reform to focus in on the relevant groups. Second, a simple difference-in-difference approach will likely be less credible as it is susceptible to shocks that affect all groups in Massachusetts (in the case of a “cross state” difference-in-difference estimate) or shocks that affect all high uninsurance rate groups (in the case of the “within Massachusetts” difference-in-difference estimate). As a recession occurred during our post-reform period, both issues represent serious threats to our identification strategy and lead us to use the more robust triple difference framework.

5 Financial Outcomes Data

To analyze the effect of insurance coverage on financial outcomes, we use the Federal Reserve Bank of New York Consumer Credit Panel data set. In this section, we describe the data set; more information on these data are available from Lee and van der Klauw (2010). The data contain information on credit reports for a panel of individuals and are observed from 1999 through 2012. The data are observed quarterly; we take the average over the four quarters to arrive at yearly observations. The primary sample is composed of 5 percent of adults over the age of 18 who have a social security number and a record at a national credit reporting company. Additionally, the data include all adults with the same mailing address as the primary sampled individual. We drop individuals who are over age 65 from our main analysis as they would have already been covered by Medicare and thus would not have experienced a change in their insurance status as a result of the reform; later, we use these

individuals as a placebo test. In Massachusetts, we use the entire sample. For other states in the New England census division (Connecticut, Maine, New Hampshire, Rhode Island, and Vermont), we use only a 1 percent sample of the adult population and all household members of this 1 percent sample. This results in approximately 1.2 million individual-year observations in the New England census division excluding Massachusetts, and about 5 million individual-year observations in Massachusetts.

The main variables we analyze are total balance on all active credit accounts, total amount past due (30 days or more) on credit balances, the fraction of debt that is past due, the amount of third-party collections associated with an account, and the presence of a bankruptcy in the last 24 months. We also analyze the effect of the reform on an individual’s “risk score,” a credit score that ranges from 280 to 850, with higher values indicating a lower probability of future delinquencies. Credit scores are large determinants in access to consumer credit and interest rates. In our data, these scores range between 280 and 850. A credit score above 780 is considered “excellent” and results in the best access to credit at the lowest rates; scores between 660 and 780 are considered “good,” between 601 and 660 are considered “fair,” and below 600 are considered to be “poor.”

One limitation of these data is that they only include individuals who have had some formal connection with credit markets (e.g., via a cell phone contract, credit card application, car loan, etc). About 8 percent of adults between the age of 20 and 64 do not have a credit report, and those with low income (who may have been disproportionately affected by the Massachusetts health care reform) are most likely to be included in this group of “credit invisibles” (Brevoort et al. (2015)). Furthermore, a large percentage of those 18 to 19 do not have credit reports and would therefore not be included in our data. However, compared to survey data, credit report data offer many advantages: there are large number of observations, the data do not require individuals to recall and accurately report financial information, and coverage is good relative to surveys which also suffer from non-response and non-random attrition.⁷

⁷For example, the Oregon Health Insurance Experiment was able to match 68.5 percent of participants to a credit report, but only had a 36 percent response rate using a basic mailing survey and was able to achieve only a 50 percent response rate when using more intensive survey methods on a subset of non-responders

In addition to credit report outcomes, we also observe zipcode of residence and year of birth. We use these variables to merge in the SAHIE data on the pre-reform uninsurance rate of each individual's county and age-group. To account for the possibility that the reform may have induced some individuals to move, we define county of residence as the county where the individual lived in the 4th quarter of 2005. For zipcodes that span counties, we assign county of residence based on the county in which the majority (or plurality) of residential mailing addresses are located. In the appendix, we explore several alternative ways of defining the sample, including assigning county by the county of residence in each year, limiting the sample to only the primary sampled adult, and using the entire Northeast census region as the comparison group. In general, analysis performed on these alternative samples yields similar results.

Table 2 presents descriptive statistics from the data set. We observe about 380,000 individuals in Massachusetts and 100,000 in other New England states each year. The top panel displays the descriptive statistics for the entire sample for different age groups, while the bottom panel reports statistics for the years prior to the reform. In general, the pre-reform credit report outcomes are better for both Massachusetts and other New England states due to the timing of the 2008 recession. The first column shows the mean and standard deviation for the risk score, total credit debt, total amount past due on credit accounts, fraction of debt past due, total third party collections and the presence of a personal bankruptcy in the last 24 months for all ages in Massachusetts. On average, Massachusetts residents had \$22,407 in debt (including mortgage debt) on active accounts and \$829 in debt that was at least 30 days past due. Other New England residents had average total debt of \$23,172 and a total amount past due of \$842. On average, individuals in Massachusetts had about 6 percent of debt past due. Similarly, about 5 percent of debt was past due for other New England residents. In both Massachusetts and other New England states, about 1 percent of individuals had experienced a bankruptcy in the last 24 months. On average, Massachusetts residents had about \$60 in third party collections; residents of other New England states had slightly more, \$83, in third party collections. In Massachusetts, the average risk score was approximately 700, indicating that Massachusetts residents overall

(Finkelstein et al. (2012)).

have good credit. Massachusetts residents are slightly better credit risks than residents of other New England states, where the average credit score is 693.

The next two sets of columns show credit report outcomes for the two age groups we evaluate, the 18 to 39 year old group and the 40 to 64 year old group. The younger group tends to have worse credit than the older group, both in Massachusetts and the rest of New England. This group has lower debt, lower credit scores, higher amounts past due, and a greater amount in collections. Bankruptcy rates are approximately the same for the two age groups.

Although we do not have information on the insurance status of the individuals in the data, evidence from the Oregon Medicaid Experiment (Finkelstein et al. (2012)) and from household surveys indicates that the uninsured poor have much worse financial profiles than the average individual observed in the panel. For example, the control group for the Oregon Medicaid Experiment had an average of approximately \$4700 of debt in collections, substantially higher than the average of \$60 observed in Massachusetts in the data. Survey data similarly indicate that the uninsured tend to have worse financial outcomes than the insured. For example, in the 2007 Survey of Consumer Finances, respondents with at least one uninsured household member were 70 percent more likely to report making payments late, 60 percent more likely to have declared bankruptcy in the last year, and more than twice as likely to report being more than two months late on payments than respondents in households where all members were covered by health insurance. Although we cannot directly verify the difference in the Consumer Credit Panel, it is likely that uninsured individuals in our data set have significantly worse financial outcomes than the insured.

6 The Effect of the Massachusetts Reform on Financial Outcomes

In this section, we estimate the effect of the Massachusetts health reform on financial outcomes. Our strategy uses the pre-reform uninsurance rates by age and county as a measure of

ex-ante exposure to the reform. We compare people in the same age group living in similar counties in 2005 across Massachusetts and other states in the New England Census division (Maine, New Hampshire, Vermont, Connecticut, and Rhode Island), and those living within Massachusetts in more- and less-affected groups to each other, employing a “triple difference” strategy. This technique allows us to produce estimates that are robust both to Massachusetts-specific time trends and trends correlated with the 2005 uninsurance rate.

This approach assumes that any change in financial outcomes among the more-affected individuals in Massachusetts relative to other New England states over the period of the reform is caused by the reform. If the reform had not occurred, this assumption implies that financial outcomes in county-age groups in Massachusetts would have changed at the same rate as similar county-age groups in other states. This assumption is more credible if, prior to the reform, financial outcomes were evolving similarly across these groups. To evaluate whether trends in financial outcomes differed across groups in Massachusetts and New England before the reform, we estimate

$$\begin{aligned}
 Y_{cat} = & \beta_{ca} + \sum_{y=1999}^{2012} (\beta_{y1} \times I(\textit{Year} = y) + \beta_{y2} \textit{Uninsured2005}_{ca} \times I(\textit{Year} = y) \\
 & + \beta_{y3} \textit{MA}_c \times I(\textit{Year} = y) + \beta_{y4} \textit{MA}_c \times \textit{Uninsured2005}_{ca} \times I(\textit{Year} = y)) + \epsilon_{cat},
 \end{aligned} \tag{1}$$

where the financial outcome dependent variables Y vary by county (c), age group (a) and year (t). In this model, the interaction between MA and the year binary variables measures a trend specific to all county-age groups within Massachusetts. Similarly, the interaction between $Uninsured2005$ and the year binary variables captures trends associated with the 2005 uninsurance rate. The 3-way interaction between MA , $Uninsured2005$, and the year variables estimates the change in outcomes for each year in Massachusetts relative to other states in New England associated with county-age group uninsurance rates, measured from the excluded base year, 2005. We also include county-age group fixed effects, β_{ca} and control for the county level unemployment rate. If the reform improved financial outcomes, and did so differentially across county-age groups based on their uninsurance rate at the time the law was adopted, we would expect to observe a relative improvement in Massachusetts associated with the potential effect of the reform only for years after the reform took place.

That is, we would expect the coefficients on $MA_c \times Uninsured2005_{ca} \times I(Year = y)$ to be significant only for $y > 2007$, after the reform was implemented, and small or statistically insignificant for prior years.

Figure 4 plots the coefficients on the three-way interaction term by year from equation (1) for the outcome variables risk score, total balance on all accounts, total amount past due, fraction of debt past due, amount of third-party collections, and bankruptcy in the last 24 months. In this figure, the solid line plots the coefficients in each year while the dotted line plots the upper and lower bounds of a 95 percent confidence interval. For all outcome variables we consider, we only observe small or statistically insignificant effects from 1999 to 2005. This indicates that these financial outcomes in high-uninsurance groups in Massachusetts followed the same trends as similar groups in New England states prior to the reform. Beginning in 2008, the first year after the reform was fully implemented, several financial outcomes visibly diverge for the Massachusetts groups that were most affected by the reform: total debt, total amount past due, fraction of debt past due, amount of third party collections, and bankruptcy rates relatively decrease and risk score relatively increases. For risk score and total amount of third party collections, individual year estimates are not statistically significant. In contrast, we find significant reductions by year for total debt, total amount past due, the fraction of debt past due, and the probability of a bankruptcy. These results indicate that following the reform, there was a relative improvement in financial outcomes among groups in Massachusetts that were most affected by the reform relative to similar groups in other states.

In addition to examining the average amount of debt that is past due, we also analyze how the Massachusetts reform affected the distribution of bad debt. To that end, we construct binary variables describing the probability that an individual has a past due amount in a certain range: \$0 past due, \$1–\$5,000 past due, \$5,001–\$10,000 past due, or more than \$10,000 past due. To examine how the trends of these variables changed over time in Massachusetts relative to other states, we plot the coefficients from model (1) in Figure 5. For all delinquency categories, we find no evidence of differential trends prior to the 2006 reform. Following the reform, we find that high uninsurance groups in Massachusetts were signifi-

cantly less likely to be in the \$5,001–\$10,000 or over \$10,000 past due categories. The point estimates indicate that the reform may have increased the probability that residents were in the \$0 past due and \$1–\$5,000 past due categories, although the confidence intervals on these estimates are large. These effects suggest that the expansion of insurance coverage protected individuals from having very large delinquencies but not from having small delinquencies.

Similarly, we construct a series of binary variables for the amount owed in third-party collections in order to examine the effect of the Massachusetts reform on the distribution of collections. We create four indicator variables, each equal to one if the individual owes \$0 in collections, \$1 to \$1000 in collections, \$1001 to \$2000 in collections, and over \$2000 in collections. Figure 6 plots the year-by-year effects. Beginning in 2008, we observe a relative decrease in the probability of collections between \$1001 to \$2000 and over \$2000, although the confidence intervals are large.⁸

In our main specification, we replace the year indicator variables with indicators for the “implementation” period of the reform (2006 and 2007) and the “post-reform” period (2008–2012) to produce estimates of the average effect of the reform over all of the post-reform years. We estimate the three-way interaction model between these indicator variables, the pre-reform uninsurance rate of the county-age group, and an indicator that the individual lives in Massachusetts. Specifically, we estimate

$$\begin{aligned}
 Y_{cat} = & \beta_{ca} + \beta_t + \beta_1 MA_c \times Implementation_t + \beta_2 MA_c \times Post_t & (2) \\
 & + \beta_3 Implementation_t \times Uninsured2005_{ca} + \beta_4 Post_t \times Uninsured2005_{ca} \\
 & + \beta_5 MA_c \times Implementation_t \times Uninsured2005_{ca} \\
 & + \beta_6 MA_c \times Post_t \times Uninsured2005_{ca} + \epsilon_{cat}.
 \end{aligned}$$

This model also includes year fixed effects (denoted here as β_t), county-age group fixed effects (β_{ca}), and controls for the county unemployment rate. The term $Post_t \times Uninsured2005_{ca}$ captures any shocks or trends associated with the 2005 uninsurance rate of county-age group

⁸We also conduct analysis using a larger number of binary variables indicating each of the deciles of Risk Score, Total Debt, Amount Past Due, and Amount in Collections as dependent variables. Although the point estimates are imprecise, they indicate that the largest changes are coming from the highest deciles. These results are available upon request.

ca. The term $Post_t \times MA_c$ captures any shocks or trends that occur only in Massachusetts and are common to all Massachusetts county-age groups. For example, if Massachusetts experienced a more severe a recession than other states in New England, to the extent that this was a common experience for all groups in Massachusetts, the $Post_t \times MA_c$ term would capture such an effect. The coefficient on the three-way interaction of MA , $Uninsured2005$ and $Post$ is our parameter of interest. This coefficient measures the effect of a one percentage point increase in “exposure” to the reform on the financial outcome variable.

The dependent variables we consider are the risk score, the total amount past due, total balance on all accounts, fraction of debt past due, total collections, and the presence of a bankruptcy in the last 24 months per 1000 residents.⁹ Standard errors are clustered by county to account for correlation of the error terms within counties contemporaneously and over time. In addition to accounting for within-county correlation of the error terms, we also provide several alternative methods of conducting inference that account for the correlation of the error term within states and across geographic space. In the appendix, we report results using inference based on state-level clustering, state-level clustered percentile-t bootstrap, state-level clustered wild bootstrap, and spatially correlated standard errors (see Appendix Tables 2 and 3). While we observe individual-level data, we compute our regression coefficients using aggregated data that is weighted by the cell size. We have 67 counties, 2 age groups, and 14 years, resulting in $67 \times 2 \times 14 = 1876$ county by age group by year observations for each regression.

Table 3 reports the results of specification (2). Results for risk score are presented in the first column. The risk score is a summary measure of an individual’s overall credit-worthiness and largely governs an individual’s access to credit markets. We find a small but statistically significant positive effect of the reform on credit score. The point estimate indicates that every one percentage point increase in exposure to the reform increased average credit scores by 0.34 points. Because the risk score is based on many years of historical credit data, and because it is only partially based on payment history, it may be slower to adjust to changes in behavior than other direct measures of an individual’s interactions with credit markets.

⁹We find similar results when we model bankruptcy using a beta regression model rather than a linear model.

Indeed, it appears the effect of the reform on credit scores is increasing over time (see the first panel of Figure 4) and may therefore be larger in the long run.

Columns two and three report the effects of the reform on direct measures of credit market activity. The second column displays the estimated effect of the reform on the total balance for all active accounts. The estimate indicates that the reform significantly reduced the total balance by approximately \$180 for every 1 percentage point increase in the potential effect of the reform. It is not necessarily true that the reduction in debt reflects an improvement in financial well-being, although this may be the case if, e.g., the debt was caused by unexpected medical bills. For example, if the decrease in debt is a result of reduced access to credit markets, a reduction in debt may actually be associated with worsening conditions for the household. We therefore turn to analyzing outcomes that are unambiguously signs of household financial distress: past due bills, third party collections, and bankruptcies.

In the third column, we report the effect of the reform on the total amount past due. We find that the reform reduced the total amount past due by about \$26 for every one percentage point increase in the potential effect of the reform. This estimate is statistically significant at the 0.01 level. In the fourth column, we examine the effect of the reform on the fraction of debt that is past due by at least 30 days. The total dollar amount past due might fall mechanically as total debt falls if there is a constant hazard of missing a payment. By looking at the fraction of total debt past due, we account for the fact that the total amount owed is declining. We find that the fraction of debt past due fell significantly after the reform for the most-affected groups by about 0.1 percentage points for every percentage point increase in the potential effect of the reform.

Column five presents the effect of the reform on third party collections. These collections may be from credit accounts, such as severely derogatory credit card or auto loan bills, or they may be from public records of other collection efforts, for example hospital or utility bills. The point estimate indicates that the reform reduced such collections by about \$1.72 for every one percentage point increase in the potential effect of the reform, but the effect is only marginally statistically significant at the 0.10 level.

Finally, in the sixth column, we report our estimates for the effect of the reform on the presence of a bankruptcy in the last 24 months. We find that a 1 percentage point increase in the potential effect of the reform is associated with a significant reduction in the probability of having a bankruptcy of about 0.03 percentage points.

Assuming that the non-compliance rate is fixed across counties, i.e., that county-age groups insurance rates converged in Massachusetts following the reform, each marginal percentage point increase in the pre-reform uninsurance rate translates directly to a percentage point increase in coverage following the reform.¹⁰ As the reform increased coverage by about 7 percentage points, these estimates imply that the reform increased average credit scores by about 2.4 points (7×0.34), or about 0.5 percent relative to the Massachusetts average. Similarly, these results imply that the reform reduced the average amount of debt by \$1260 (a 6 percent reduction), reduced the average amount past due by \$182 (22 percent), reduced the fraction of debt past due by 0.6 percentage points (10 percent), reduced collections by \$12 (20 percent), and reduced bankruptcies by 0.20 percentage points (19 percent).

While it may be tempting to use our results to “back out” the treatment effect of having insurance on financial outcomes, our findings are probably best interpreted as simply capturing the “reduced form” effect of the Massachusetts reform in all its dimensions. This is because the reform appeared to have other effects beyond providing insurance to those who were previously uninsured. For example, evidence from both hospitalizations and surveys suggests that there may have been changes in insurance coverage along the intensive margin, particularly among children for whom the expansions of public health insurance were quite generous (Kolstad and Kowalski (2012b), Miller (2012b)). Further, the quality of insurance was also affected: the Medicaid program expanded to cover dental and vision services and, beginning in 2009, employer sponsored health insurance were required to meet certain cost-sharing and coverage requirements to satisfy the mandate. It also appears that employers expanded access to coverage in the wake of the reform, perhaps in response to demands from employees. If employers also improved the quality of their health insurance plans then this could have led to improved financial outcomes. Another important consideration is that

¹⁰Assuming that the insurance rate fell proportionally across groups by 75 percent would result in the same estimates scaled by $1/0.75$.

the reform may have produced significant “spillover effects.” For example, the improvement in a young adult’s insurance status could conceivably affect the financial well-being of his or her parents. Therefore, simple calculations that seek to identify the treatment effect of having insurance would require strong assumptions and would likely over-estimate the effect of insurance coverage on financial outcomes. In any event, since the Massachusetts reform served as a model for the ACA, from a policy perspective, it is the reduced form effect of the policy that is of primary interest.

If the reform is preventing a small number of individuals from experiencing very large negative shocks, the estimated improvements would be most pronounced for large collections and delinquencies. In contrast, if the improvement in outcomes is mostly dominated by broad but diffuse income effects, the reductions might occur evenly across the distribution of collections and delinquencies. To explore these mechanisms further, we analyze how the reform affected different parts of the delinquency and collections distribution. We do this by constructing delinquency indicator variables equal to 1 if the individual has \$0 past due, \$1 to \$5,000 past due, \$5,001 to \$10,000 past due, or over \$10,000 past due. Similarly, we construct collections indicator variables equal to 1 if the individual has \$0 in collections, \$1 to \$1000 in collections, \$1001 to \$2000 in collections, and greater than \$2000 in collections.¹¹

The results are presented in Table 4. Consistent with the year-by-year analysis presented in Figures 5 and 6, we find that the reform significantly reduced the probability that an individual had a large delinquency of between \$5,001 and \$10,000 or over \$10,000. Point estimates indicate that the reform modestly increased the probability that individuals had no delinquencies and similarly increased the probability that an individual had delinquencies of \$5,000 or less. Because the reform increased insurance coverage by about 7 percentage points, these estimates suggest that the reform reduced the probability of an over \$10,000 credit delinquency by about 0.6 percentage points (7×0.0009), or about 21 percent, and reduced the probability of a \$5,001 to \$10,000 delinquency by about 0.2 percentage points, or about 10 percent. In the second panel, we present the results for collections. We find the

¹¹We use different bin sizes for collections and delinquencies as delinquencies are more common and, on average, larger in size. For example, in Massachusetts, the average amount in collections is \$60 and the average amount of delinquencies is \$829.

largest reductions for the probability of collections between \$1001 and \$2000, and collections over \$2000, although these effects are only significant at the 0.10 level. These results indicate that the reform reduced the probability of having between \$1001 and \$2000 in collections by 0.2 percentage points, or 20 percent, and reduced the probability of having over \$2000 in collections by 0.15 percentage points, or 15 percent.

The results described in this section are robust to several alternative specifications and means of conducting inference, such as using different sample definitions and comparison groups, including state by year fixed effects, restricting the analysis to only use county-level variation in the 2005 uninsurance rate, using bootstrap procedures to construct confidence intervals, clustering at the state level, and correcting the errors to account for spatial correlation. See the appendix, where these robustness checks are presented and described in detail.

6.1 Heterogeneous Effects by Credit Score

In this section, we compare the effect of the reform among people who had relatively high credit scores at the time of the reform to those whose credit scores were lower. Specifically, we separate the sample based on whether an individual's credit score was above or below the median credit score in Massachusetts in 2005, the year before the reform, and estimate our models on these two groups separately. Credit score is a summary measure of financial well-being, and a low credit score may indicate that an individual is struggling financially. Because the uninsured tend to have worse financial outcomes, those with lower credit scores likely had higher rates of uninsurance prior to the reform and therefore may be more likely to gain coverage as a result of the reform. Furthermore, credit score is the primary metric that determines access to credit. Those who can easily borrow may be better able to smooth their consumption in the event of a medical emergency without resorting to filing for bankruptcy.

The results are presented in Table 5. In this table, the symbol * indicates that the coefficient is statistically different from zero, whereas the symbol † indicates that the estimates are statistically different across the two subsamples. The top panel displays the results for

those individuals whose credit scores were below the median in 2005. We find that the reform had a stronger effect on the credit score, amount past due, fraction of debt past due, and bankruptcy for this group than for the general population. The difference in the estimates of the effect of the reform on amount past due, fraction of debt past due, and bankruptcy are statistically different for the low credit score sample than in the high credit score sample; for credit score and total debt, they are marginally significantly different at the 10 percent level. In the low credit score sample, a one percentage point increase in the pre-reform uninsurance rate (i.e., a one percentage point increase in the potential effect of the reform) is associated with an improvement of credit scores of about 0.5 points, a reduction in the average amount past due of about \$60 and a reduction in the fraction of outstanding debt past due of 0.2 percentage points. Similarly, we find that a one percentage point increase in the potential effect of the reform is associated with a reduction in the 2 year bankruptcy rate of about 0.08 percentage points. These effects are approximately twice as large in this low credit score sample as they are in the general population.

The results for the high credit score sample are presented in the lower panel. In the high credit score sample, we do not find a significant effect of the reform on total amount past due. We continue to find that the fraction of debt past due and the personal bankruptcy rate falls significantly after the reform, although the size of this effect is smaller than what we observe in the general population. Similarly, the effect of the reform on credit score is smaller for the high credit score sample than for the low credit score sample and only marginally significant. In contrast, we find that the reform had a large effect on the total debt among those who had high credit scores before the reform.

6.2 Placebo Tests

The main results estimated from the model (2) are robust to Massachusetts-specific shocks to financial outcomes as well as shocks to groups with high 2005 uninsurance rates, but they would not be robust to shocks that only occur in high uninsurance rate county-age groups within Massachusetts (for example, an increase in local demand for employment that only affects counties and age groups in Massachusetts with high uninsurance rates). To

investigate whether the improvement in financial outcomes we observe reflects a concurrent improvement in the economic environment unrelated to the health care reform, we estimate equation (2), but replace the dependent variable with several measures of economic activity that were plausibly unaffected (or only weakly affected) by the health care reform. Specifically, we consider the poverty rate, business bankruptcies, the unemployment rate and the median income at the county level. This placebo test expands upon the one used in Gross and Notowidigdo (2011). If our model uncovers strong effects on these variables, it would indicate that our measure of the potential effect of the reform is correlated with an overall improvement in the economic climate.

We use annual county-level data on the poverty rate and the median income from the Small Area Income and Poverty Estimates produced by the US Census. The number of business bankruptcies are from the U.S. Department of Justice Public Access to Court Electronic Records system, and data on the county level unemployment rate are from the Bureau of Labor Statistics Local Area Unemployment Statistics. We estimate each model twice, with a log transformation of the dependent variable as well as the level of that variable. These measures are only available at the county-level, and not by age group, so we are only able to evaluate whether high-uninsurance rate counties in Massachusetts experienced relative improvements. However, because our results are largely robust to using only county-level variation (see Appendix Table 1), and because within-county uninsurance rates are correlated, establishing that there were no unrelated economic improvements in Massachusetts counties is an important check on the credibility of our empirical design.

The results are reported in Appendix Table 5. In all eight models, we find no statistically significant improvements in Massachusetts counties relative to similar counties in other states. This suggests that it is unlikely that our findings are driven by a coinciding but unrelated improvement in economic conditions that also improved financial outcomes, and supports the hypothesis that the health reform itself is responsible for the observed improvement in financial outcomes, and is not merely correlated with this improvement.

As a second check on our empirical analysis, we perform a placebo test on a group of individuals who should not have been affected by the reform: individuals who were over

age 65 at the time of the reform. These individuals would not have experienced an increase in coverage because almost all of them would have already had health insurance coverage through the Medicare program. As a placebo test, we estimate the specification in equation (2) but only include those who were age 65 or older in 2005. We match individuals over age 65 at the time of the reform to our measure of the potential effect of the reform for their entire county and for the older (age 39–64) age group. If our analysis is capturing the effect of the expansion of health insurance, rather than a concurrent improvement of financial outcomes that is specific to the most-affected areas in Massachusetts, we should not find any effect among the elderly. The results are presented in Appendix Table 6. We do not find a significant effect of the reform among the elderly associated with either the overall 2005 uninsurance rate of their county of residence (panel 1) or the age 39–64 2005 county-level uninsurance rate (panel 2). This is consistent with our hypothesis that the observed changes in financial outcomes are a result of the health care reform, rather than a reflection of a trend among the most-affected counties in Massachusetts.

Finally, we use all 50 states in placebo tests in order to determine how likely it would be to observe effects of the size we uncover even if no health care reform had taken place. To conduct these placebo tests, we estimate model (2) for each state, using other states in its census region as the comparison group, and replacing the indicator variable for Massachusetts with an indicator for that state. This results in 50 “placebo” tests against which we can compare the true effect measured in Massachusetts.¹²

Figure 7 displays a scatter plot of the test statistics resulting from these placebo estimates. The black circles indicate the t-statistics associated with the placebo estimates, while the red stars indicate the effects we find in Massachusetts. The reduction in bankruptcies and in the probability of high delinquencies we find in Massachusetts is larger than any reduction we observe among the placebo coefficients. The reduction in the fraction of debt past due is the second largest we observe among all of the estimated effects. The effects observed for the other outcome variables are less pronounced: the change in the amount of credit debt past due is the fourth largest reduction, the change in the total amount of debt is

¹²We have also conducted these placebo tests using the coefficient, rather than the t-statistic. These results are available in the appendix.

the sixth largest reduction, the increase in credit score is the sixth largest increase, and the decrease in the probability of having a delinquency of between \$5001 and \$10000 is the tenth largest decrease we observe. These results strongly confirm our conclusions with regards to bankruptcy, large delinquencies, and the fraction of debt past due, although they indicate that the effects on the other outcomes should be interpreted with caution.

7 Conclusion

Public policy that expands health insurance coverage has broad effects on the well-being of those affected. While a large and growing body of research has established the effects of health insurance on health care utilization and health outcomes of the insured, the role of health insurance in the financial stability of a household remains under-explored. In this paper, we analyze the effect of landmark state health care legislation, the Massachusetts health care reform, on financial outcomes using credit report data.

We find that the reform significantly improved credit scores, reduced the total amount past due, reduced the fraction of debt past due, and reduced the probability of personal bankruptcy. We find particularly pronounced reductions in the probability of having a large delinquency of over \$5,000. We also find suggestive evidence that the reform reduced total debt and third-party collections, driven by a reduction in the probability of having a large amount (over \$1000) in collections. These effects tend to be larger among individuals whose credit scores were low at the time of the reform, suggesting that the greatest gains in financial security occurred among those who were already struggling financially.

Our analysis shows that health care legislation has implications that reach beyond health care providers and the uninsured and extend into credit markets, benefiting not only uninsured households who gained coverage, but also creditors who served these households. Our finding that credit scores improved as a result of the reform indicate that the reform increased future access to credit for those individuals who gained coverage. These results show that health care reform legislation has pervasive effects not just on health and the use of health services, but across many measures of household well-being.

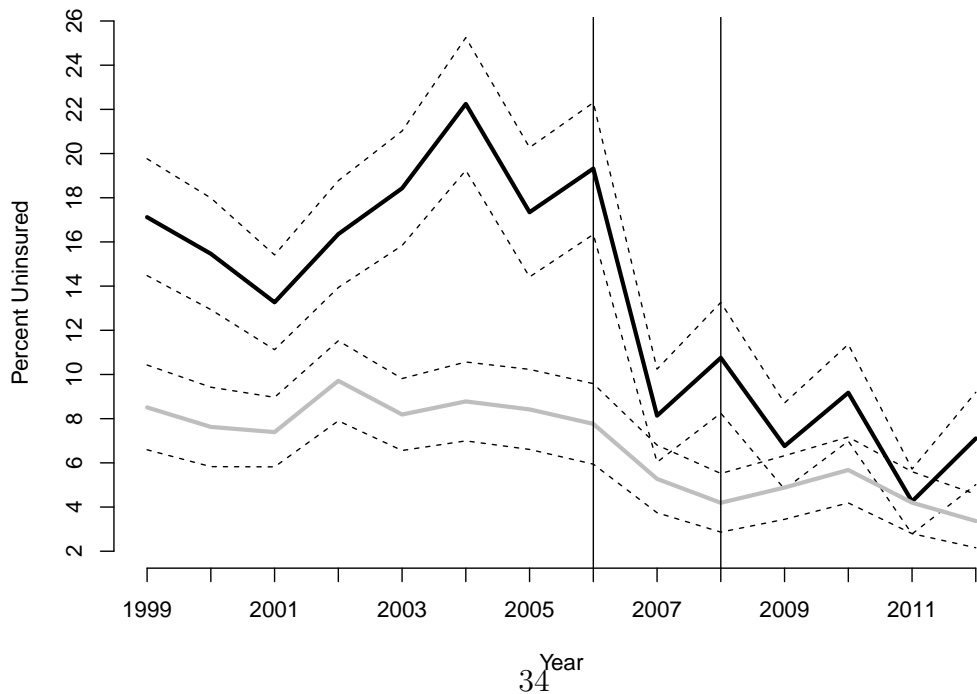
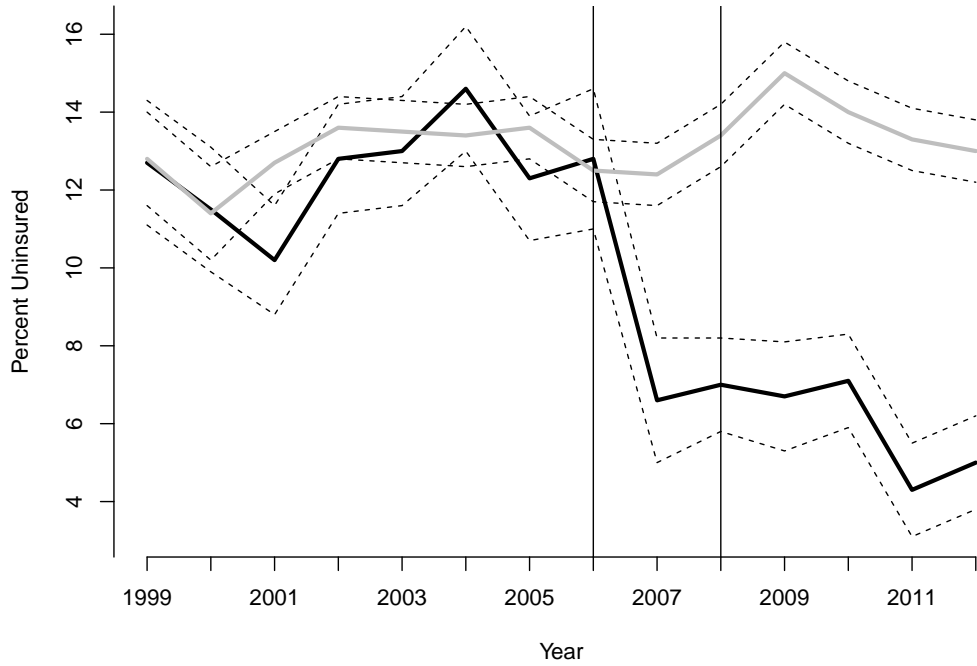
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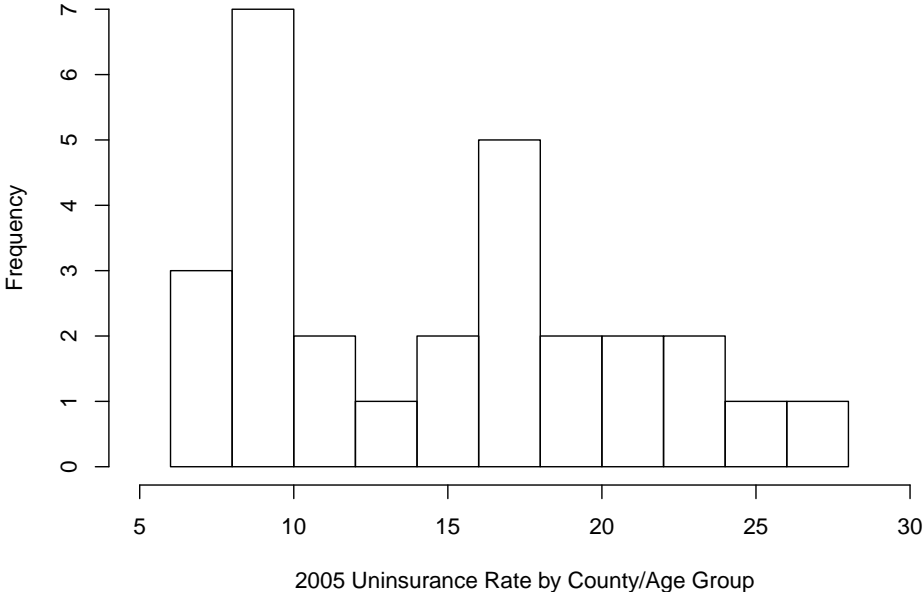
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Figure 1: Percentage Non-elderly Uninsured in Massachusetts and the rest of New England (First Panel) and Percent Uninsured in Massachusetts by Age Group (Second Panel), 1999-2012



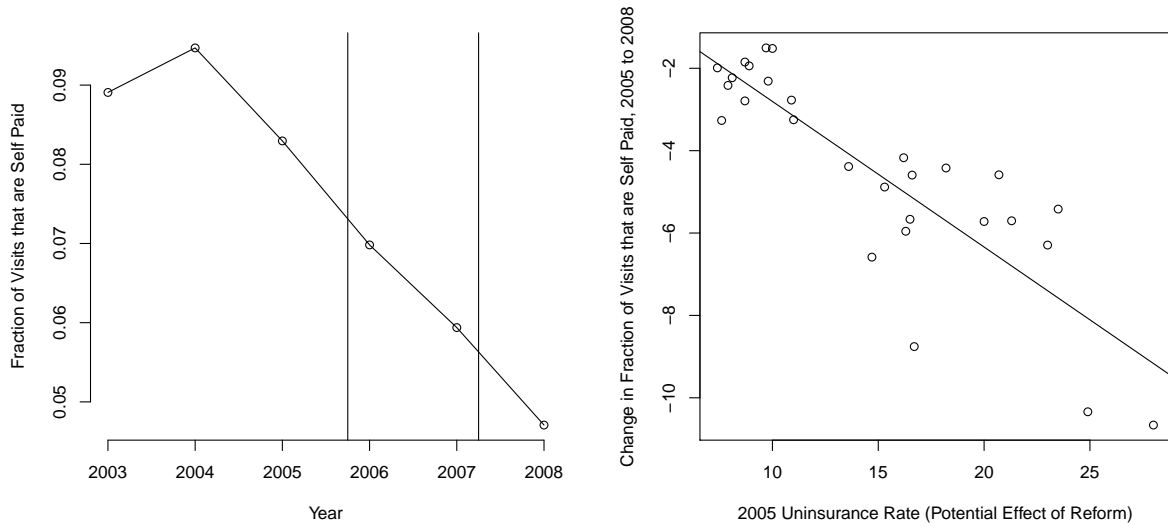
First panel presents estimates from the Current Population Survey of the percent of the population uninsured in Massachusetts (black) and New England (grey) from 1999-2012. The second panel presents estimates from the Current Population Survey of the percent of the population uninsured in Massachusetts for individuals age 18 to 39 (black) and 40 to 64 (grey). Vertical lines indicate the implementation period of the reform.

Figure 2: Histogram of Percent Uninsured by County/Age Group in Massachusetts, 2005



Data from 2005 Small Area Health Insurance Estimates, downloaded on 3/13/2013 from <http://www.census.gov/did/www/sahie/>

Figure 3: The effect of the Massachusetts reform on the fraction of hospital and emergency department visits that are self-paid.

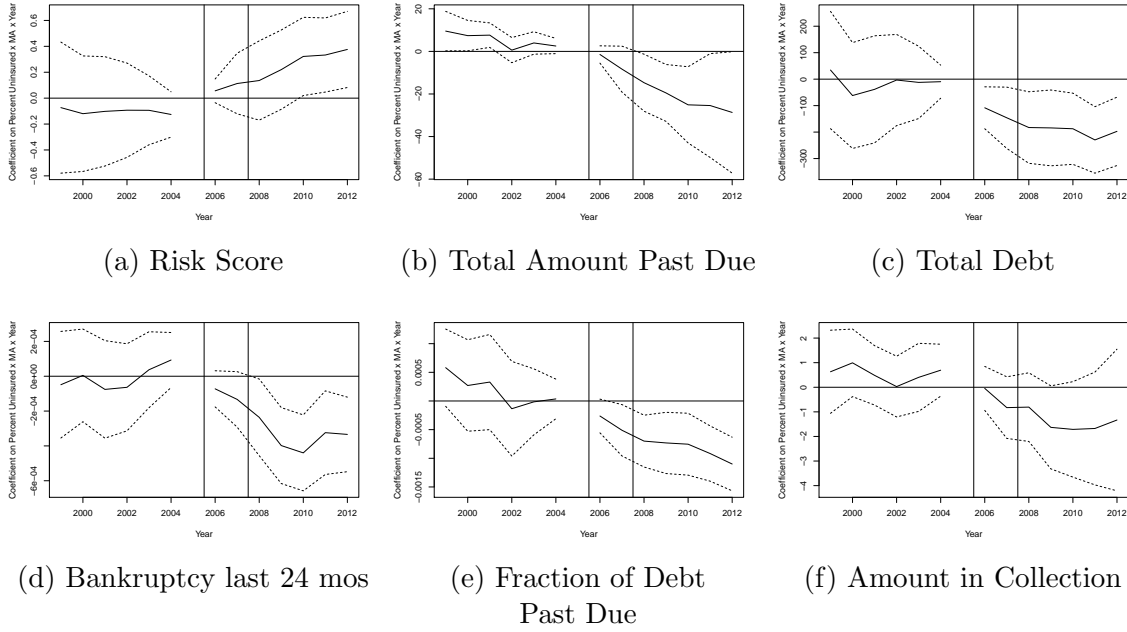


(a) Fraction Self-Paid, 2003-2008

(b) Change in Fraction Self Paid, 2005-2008

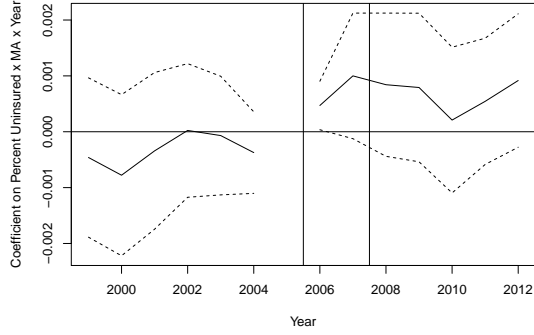
Authors' estimates from the Acute Hospital Case Mix Database. Vertical lines indicate implementation period of the reform.

Figure 4: Coefficient on $PercentUninsured \times MA \times Year$ by year. Vertical lines indicate the implementation period of the reform.

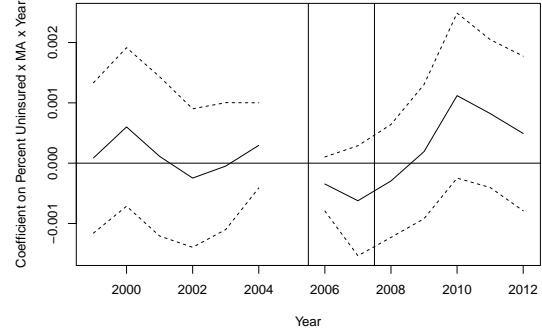


Authors' estimates from the credit bureau data. Excluded year is 2005. Vertical lines indicate implementation period of the reform.

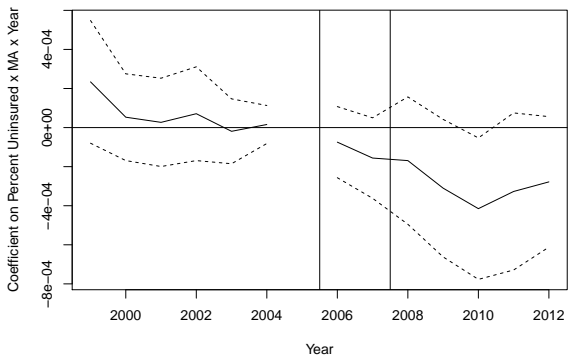
Figure 5: Coefficient on $PercentUninsured \times MA \times Year$ by year. Vertical lines indicate the implementation period of the reform.



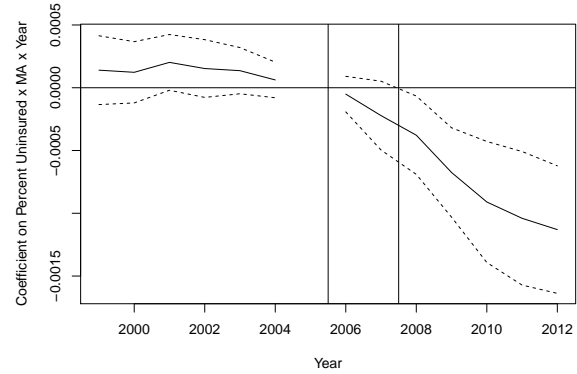
(a) \$0 Past Due



(b) \$1-\$5000 Past Due



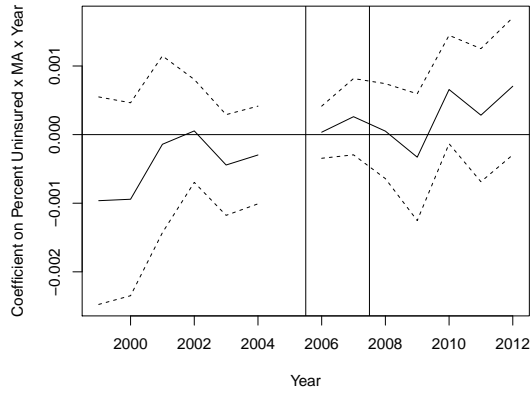
(c) \$5001-\$10000 Past Due



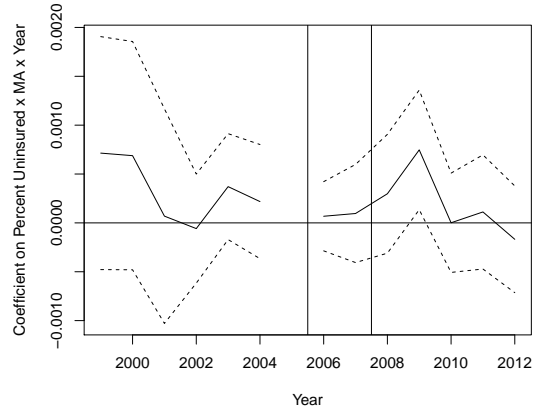
(c) \$10001 + Past Due

Authors' estimates from the credit bureau data. Excluded year is 2005. Vertical lines indicate implementation period of the reform.

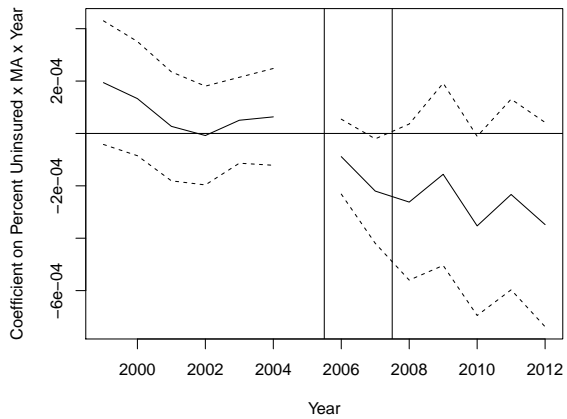
Figure 6: Coefficient on $PercentUninsured \times MA \times Year$ by year. Vertical lines indicate the implementation period of the reform.



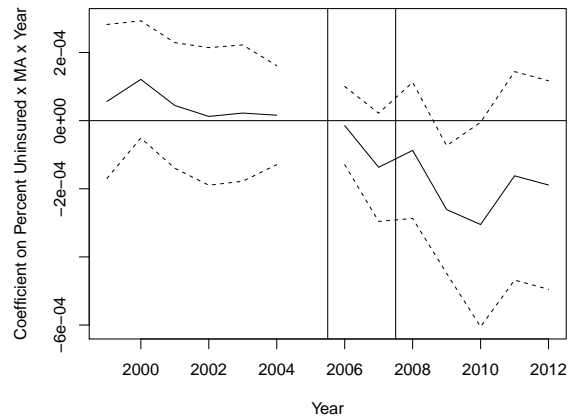
(a) \$0 In Collections



(b) \$1-\$1000 In Collections



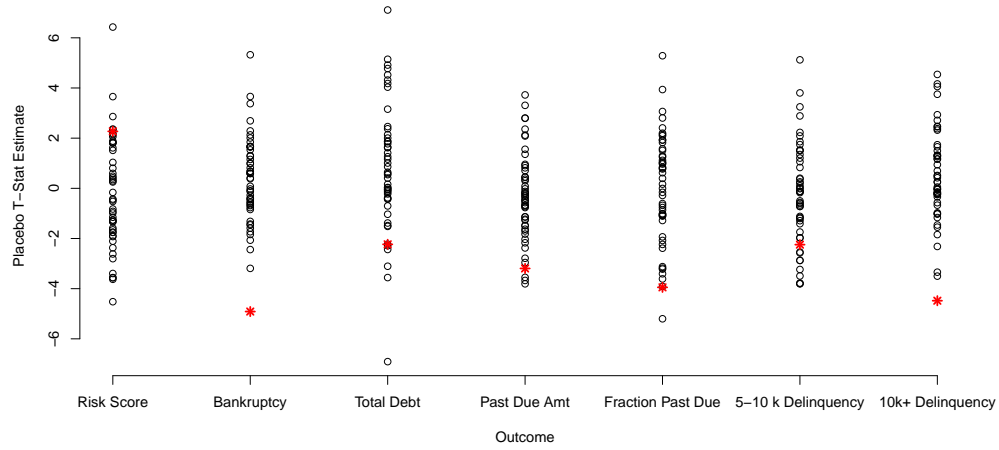
(c) \$1001-\$2000 In Collections



(c) \$2001 + In Collections

Authors' estimates from the credit bureau data. Excluded year is 2005. Vertical lines indicate implementation period of the reform.

Figure 7: Distribution of T-Statistics from Placebo Estimates Using States Other Than Massachusetts.



The red star indicates the test statistic from the “true effect” estimated using Massachusetts. The black circles are the placebo test statistics. The rank (from 1 to 50) of the Massachusetts test statistics are: risk score, 44; bankruptcy, 1; total debt, 6; amount past due, 4; fraction past due, 2; \$10000+ past due, 1.

Table 1: Distribution of Income Relative to the Poverty Level Among Individuals Who Gained Coverage Through Massachusetts Reform, Affordable Care Act, and Oregon Health Insurance Experiment

	Massachusetts Reform	ACA: States Not Expanding Medicaid	ACA: States Expanding Medicaid	Oregon Health Insurance Experiment
Under 138 FPL	26.2%	21.9%	27.2%	100%
139 to 400 FPL	51.2%	68.5%	69.9%	0%
Over 400 FPL	22.6%	9.7%	2.9%	0%

Column 1 authors' calculations from Current Population Survey. Columns 2 and 3 derived from Sommers et al. (2014). Column 2 displays statistics for states that elected to expand Medicaid. Column 3 displays statistics for states that did not elect to expand Medicaid. Column 4 derived from Finkelstein et al. (2012).

Table 2: Descriptive Statistics of Financial Outcomes

	Full Sample					
	All Ages		Ages 18-39		Ages 40-64	
	Massachusetts	Other States in New England	Massachusetts	Other States in New England	Massachusetts	Other States in New England
Risk Score	701 (31)	693 (31)	670 (19)	658 (16)	721 (16)	714 (14)
Total Balance on Open Accounts	\$22407 (\$6940)	\$23172 (\$7479)	\$18944 (\$4972)	\$19399 (\$5224)	\$24695 (\$7116)	\$25467 (\$7711)
Amount Past Due	\$829 (\$502)	\$842 (\$522)	\$864 (\$528)	\$919 (\$571)	\$805 (\$484)	\$795 (\$484)
Bankruptcy in last 24 mos	0.01 (0.01)	0.01 (0.02)	0.01 (0.004)	0.01 (0.01)	0.01 (0.004)	0.01 (0.01)
Fraction of Debt Not Current	0.06 (0.04)	0.05 (0.04)	0.07 (0.02)	0.07 (0.05)	0.05 (0.01)	0.05 (0.01)
Total collections	\$60 (\$34)	\$83 (\$53)	\$79 (\$39)	\$114 (\$61)	\$48 (\$23)	\$63 (\$35)
# of individual-year observations:	4967658	1245139	1976467	470806	2991191	774333
	Pre-Reform Years 1999-2005					
	All Ages		Ages 18-39		Ages 40-64	
	Massachusetts	Other States in New England	Massachusetts	Other States in New England	Massachusetts	Other States in New England
	694 (28)	685 (30)	660 (15)	648 (12)	714 (15)	706 (11)
Risk Score	\$17786 (\$5631)	\$18877 (\$6028)	\$14453 (\$3101)	\$14868 (\$3199)	\$19757 (\$5872)	\$21050 (\$6090)
Total Balance on Open Accounts	\$497 (\$120)	\$559 (\$137)	\$489 (\$111)	\$540 (\$133)	\$502 (\$126)	\$570 (\$138)
Amount Past Due	0.01 (0.005)	0.01 (0.01)	0.01 (0.004)	0.01 (0.01)	0.01 (0.01)	0.02 (0.01)
Bankruptcy in last 24 mos	0.05 (0.02)	0.05 (0.01)	0.06 (0.01)	0.06 (0.01)	0.04 (0.01)	0.04 (0.01)
Fraction of Debt Not Current	\$39 (\$21)	\$53 (\$29)	\$49 (\$23)	\$71 (\$34)	\$34 (\$18)	\$45 (\$22)
Total collections	2425552	607674	901529	213657	1524023	394017
# of individual-year observations:	Descriptive statistics of financial outcomes in the Consumer Credit Panel. Standard deviation in parentheses. Top panel presents descriptive statistics for 1999-2012; bottom panel presents descriptive statistics from 1999-2005.					

Table 3: The Effect of the MA Reform on Financial Outcomes

Dependent Variable:	Risk Score	Total Debt	Amount Past Due	Fraction of Debt Past Due	Total Collections	Bankruptcy last 24 mos (per 1000)
MA × Post × Uninsured2005	0.336 (0.148)**	-179.6 (80.44)**	-26.15 (8.186)***	-0.0009 (0.0002)***	-1.716 (0.893)*	-0.338 (0.067)***
<i>MA × Implement × Uninsured2005</i>	0.150 (0.133)	-109.5 (71.97)	-8.658 (3.487)**	-0.0005 (0.0002)**	-0.703 (0.508)	-0.099 (0.079)
<i>Post × Uninsured2005</i>	0.153 (0.108)	159.0 (47.18)***	39.19 (5.348)***	0.0009 (0.0001)***	5.225 (0.641)***	0.463 (0.068)***
<i>Implement × Uninsured2005</i>	0.005 (0.0676)	18.08 (44.49)	12.24 (3.103)***	0.0003 (0.0001)**	2.374 (0.350)***	0.158 (0.073)**
<i>MA × Post</i>	-7.439 (1.951)***	2,561 (1,648)	548.2 (139.1)***	0.0143 (0.0036)***	11.93 (11.92)	8.085 (1.773)***
<i>MA × Implement</i>	-1.92 (1.227)**	2,232 (1,418)	180.1 (50.30)***	0.0077 (0.0026)***	2.466 (6.482)	2.571 (1.767)
R ² :	0.995	0.951	0.911	0.921	0.891	0.780
MA mean:	700.8	\$22,406.80	\$828.54	0.06	\$60.02	0.011
County-age group-year observations	1876	1876	1876	1876	1876	1876

Each column displays the results from a separate regression. The dependent variable is listed in the first row. All models include county-age group fixed effects, year fixed effects, and the county-level unemployment rate as a control variable. Sixty-seven counties, 2 age groups and 14 years result in 1876 county x age group x year observations. Significance Levels: * = 10%, ** = 5%, *** = 1%. Robust standard errors are clustered by county.

Table 4: The Effect of the MA Reform on the Distribution of Delinquencies and Collections

Dependent Variable:	\$0 Past Due	\$1-\$5000 Past Due	\$5001-\$10000 Past Due	Over \$10000 Past Due
MA × Post × Uninsured2005	0.0008 (0.0005)	0.0004 (0.0005)	-0.0003 (0.0001)**	-0.0009 (0.0002)***
<i>MA × Implement × Uninsured2005</i>	0.0009 (0.0005)*	-0.0005 (0.0005)	-0.0001 (0.0001)*	-0.0002 (0.0001)**
<i>Post × Uninsured2005</i>	-0.0022 (0.0005)***	-0.0001 (0.0004)	0.0009 (0.0001)***	0.0013 (0.0001)***
<i>Implement × Uninsured2005</i>	-0.0012 (0.0002)***	0.0006 (0.0003)**	0.0003 (0.0001)***	0.0003 (0.0001)***
<i>MA × Post</i>	-0.0407 (0.0087)***	0.017 (0.0061)***	0.0067 (0.002)***	0.017 (0.003)***
<i>MA × Implement</i>	-0.017 (0.005)***	0.0093 (0.0048)*	0.003 (0.0011)***	0.0052 (0.0016)***
R ² :	0.964	0.967	0.848	0.933
MA mean:	0.80	0.16	0.02	0.02
Dependent Variable:	\$0 Collections	\$1-\$1000 Collections	\$1001-\$2000 Collections	Over \$2000 Collections
MA × Post × Uninsured2005	0.0004 (0.0005)	0.0001 (0.0003)	-0.0003 (0.0002)*	-0.0002 (0.0001)*
<i>MA × Implement × Uninsured2005</i>	0.0003 (0.0004)	-0.0001 (0.0002)	-0.0002 (0.0001)**	-0.0001 (0.0001)
<i>Post × Uninsured2005</i>	-0.0028 (0.0004)***	0.0013 (0.0002)	0.0009 (0.0001)***	0.0006 (0.0001)***
<i>Implement × Uninsured2005</i>	-0.0015 (0.0003)***	0.0008 (0.0002)**	0.0004 (0.0001)***	0.0003 (0.00005)***
<i>MA × Post</i>	-0.0107 (0.0066)	0.0068 (0.0039)*	0.0030 (0.0018)	0.0009 (0.0014)
<i>MA × Implement</i>	-0.0016 (0.0039)	-0.0001 (0.0028)	0.0014 (0.0008)*	0.0002 (0.0008)
R ² :	0.949	0.946	0.903	0.858
MA mean:	0.94	0.04	0.01	0.01
County-age group-year observations	1876	1876	1876	1876

Each column displays the results from a separate regression. The dependent variable is listed in the first row. All models include county-age group fixed effects, year fixed effects, and the county-level unemployment rate as a control variable. Sixty-seven counties, 2 age groups and 14 years result in 1876 county x age group x year observations. Significance Levels: * = 10%, ** = 5%, *** = 1%. Robust standard errors are clustered by county.

Table 5: The Effect of the MA Reform on Financial Outcomes: Heterogeneous Effects by Credit Score

Dependent Variable:	Risk Score	Total Debt	Amount Past Due	Fraction of Debt Past Due	Total Collections	Bankruptcy last 24 mos (per 1000)
Low Credit Score Sample						
MA × Post × Uninsured 2005	0.536 (0.163) ***†	-72.09 (72.67) †	-60.19 (13.30) ***†††	-0.0018 (0.0004) ***†††	-0.58 (1.057)	-0.820 (0.165) ***†††
<i>MA × Implement × Uninsured</i> 2005	0.231 (0.11)**	-68.00 (70.88)	-19.64 (6.37)***††	-0.0009 (0.0003)***††	-0.137 (0.762)	-0.0003 (0.0002)*†
<i>Post × Uninsured</i> 2005	-0.119 (0.12)††	320.8 (53.06)***	16.13 (7.596)**	0.0006 (0.0002)**	3.561 (0.839)***††	0.0013 (0.0002)***†††
<i>Implement × Uninsured</i> 2005	0.272 (0.065)***†††	135.8 (49.40)***	6.74 (5.75)	-0.0003 (0.0003)††	1.22 (0.67)	0.0005 (0.0002)***††
<i>MA × Post</i>	-10.92 (2.57)***†††	1508 (1238)†	1182 (293.3)***†††	0.027 (0.008)***†††	-12.48 (18.03)	0.018 (0.004)***†††
<i>MA × Implement</i>	-4.31 (1.52)***†††	1746 (1,260)	398.2 (108)***†††	0.0168 (0.0057)***†††	-9.93 (13.58)	0.0071 (0.004)
R ² :	0.845	0.786	0.800	0.601	0.510	0.739
Below-Median Risk Score MA mean:	623.67	\$20,407.91	\$1,613.07	0.12	117.15	0.022
County-age group-year observations	1876	1876	1876	1876	1876	1876
High Credit Score Sample						
MA × Post × Uninsured 2005	0.269 (0.137) *†	-328.4 (141.7) ***†	-3.259 (8.865) †††	-0.0004 (0.0002) *†††	-0.964 (0.576) *	-0.244 (0.113) ***†††
<i>MA × Implement × Uninsured</i> 2005	0.031 (0.0671)	-191.2 (88.88)**	-2.834 (1.765)††	-0.0002 (7.70e-05)*†††	-0.0433 (0.158)	0.00001 (2.96e-05)†
<i>Post × Uninsured</i> 2005	0.858 (0.088)***†††	154.9 (122.0)	14.92 (7.346)**	0.0007 (0.0001)**	1.116 (0.560)*††	0.0004 (0.0001)***†††
<i>Implement × Uninsured</i> 2005	0.883 (0.057)***†††	62.31 (62.97)	5.018 (1.741)***	0.0002 (5.88e-05)***††	0.0862 (0.156)	0.00004 (0.00002)*††
<i>MA × Post</i>	-3.471 (1.652)***†††	3912 (1681)***†	108.8 (112.6)†††	0.0064 (0.002)***†††	7.381 (7.527)	0.0038 (0.0014)***†††
<i>MA × Implement</i>	0.564 (0.895)††	2980 (1459)**	55.84 (26.87)***†††	0.0024 (0.0008)***†††	0.615 (1.916)	0.0002 (0.0004)
R ² :	0.755	0.729	0.596	0.693	0.348	0.561
Above-Median Risk Score MA mean:	768.21	\$24,134.69	\$170.85	0.01	\$7.17	0.002
County-age group-year observations	1876	1876	1876	1876	1876	1876

Each column displays the results from a separate regression. The dependent variable is listed in the first row. All models include county-age group fixed effects, year fixed effects, and the county-level unemployment rate as a control variable. Sixty-seven counties, 14 years, and 2 age groups result in 1876 county x age group x year observations. Significance Levels: * = 10%, ** = 5%, *** = 1% indicate effects are significantly different from zero. † = 10%, †† = 5%, ††† = 1% indicate effects differ significantly across the two panels. Robust standard errors are clustered by county.

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