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The Effect of Public Insurance Coverage for Childless Adults on Labor Supply*

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Abstract

This study provides the first plausibly causal estimates of the effect of public insurance coverage on the employment of non-elderly, non-disabled adults without dependent children ("childless adults"). We use both a regression discontinuity design and propensity score matching differences in differences to take advantage of the sudden imposition of an enrollment cap to compare the labor supply of enrollees with eligible applicants placed on a waitlist. We find enrollment into public insurance leads to sizable and statistically meaningful reductions in the probability of employment up to at least 9 quarters later, though the estimated size of this reduction varies from 0.9 to 10.6 percentage points depending upon the model used. In light of these results, policymakers should be prepared for a reduction in labor supply among those affected by the Medicaid expansion to childless adults under the Affordable Care Act.

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I. Introduction

Medicaid is currently the third largest federal domestic spending item after Medicare and Social Security and the second largest state spending item after education. Nearly 60 million low-income adults and children benefit from the program and up to 21.3 million additional low-income adults are could eventually gain coverage under Medicaid expansions associated with the 2010 Affordable Care Act (ACA; Stephens 2013).[1] Given the large and increasing number of people served by the program, knowing how Medicaid and other public health insurance programs affect the labor supply of recipients and their family members has become increasingly important for understanding the total costs of the program.

Economic theory predicts that cash and in-kind transfer programs should generally reduce labor supply, and extensive empirical research has consistently shown that most such programs do reduce labor supply. However, the literature on Medicaid's effect on the labor supply of low-income parents is mixed. While initial work finds strong work disincentives (Ellwood and Adams (1990), Moffitt and Wolfe (1992)), later papers find weaker or even positive effects (Yelowitz 1995, Montgomery and Navin 2000, Ham and Shore-Sheppard 2001 and 2005, Hamersma and Kim 2009, Hamersma 2010, Strumpf 2011). The inconclusive nature of the existing literature is suggestive of heterogeneous effects across populations and time periods studied.

We contribute to this literature by providing the first plausibly causal estimates of the effect of means-tested public insurance coverage on the employment of non-elderly, non-disabled adults without dependent children ("childless adults").[2] To date, researchers have been unable to explore the effects of Medicaid eligibility on the labor supply of childless adults, as states have only recently begun extending coverage to this population. Learning about the likely labor market effects of the ACA on low-income childless adults is of critical policy importance. Initial Congressional Budget Office projections suggested that the version of the legislation signed into law would have increased coverage by 33 million people by 2019, with Medicaid accounting for about half of these gains and low-income childless adults comprising the majority of the Medicaid expansion population (Congressional Budget Office 2012a). While the subsequent Supreme Court decision making the ACA-related Medicaid expansion a state option will certainly reduce the magnitude of the coverage increases, it remains the case that childless adults are projected to gain large-scale eligibility for Medicaid as of 2014 (Congressional Budget Office 2012b).

In this study, we exploit a recent policy reversal in Wisconsin, during which a major public insurance expansion for adults without dependent children ("childless adults") was implemented and, several months later, abruptly frozen. Individuals who applied after the program was frozen were placed on a waitlist. Those on the waitlist would only be allowed to enroll in the program once enrollment dropped below the capped level, which did not occur at any time during our study period. We obtain estimates of the causal effect of Medicaid on the labor supply of childless adults by comparing the labor market

[1] Approximately half of these projected new adults live in states where, as of March 5, 2013, governors either had not decided on or oppose the Medicaid expansion (Kaiser Family Foundation, 2013).

[2] A recent paper examines TennCare, which was available to any uninsured adult without income restrictions (Garthwaite et al. 2013).

outcomes of those who applied prior to the program freeze and received benefits to those who applied after the program freeze and did not receive benefits.

We use two complementary empirical strategies to identify the effect of public insurance coverage for childless adults on labor market outcomes. First, we use a regression discontinuity design that employs the timing of the enrollment suspension and waitlist introduction. Second, we use a propensity score matching difference in differences approach that matches plan enrollees with waitlisted applicants on their observable characteristics. While the regression discontinuity design likely has stronger internal validity, the propensity score matching difference in differences approach allows us to take advantage of a greater amount of our data.

A particular strength of our study is that we rely on the state's own administrative records rather than on self-reported enrollment, employment, and earnings data. The data for our study are Medicaid enrollment files merged with quarterly unemployment insurance earnings reports from Wisconsin. The Medicaid records allow us to observe all enrolled and waitlisted applicants, including their exact date of application. The unemployment insurance earnings records are from employer reporting to the state and allow us to observe quarterly wages from all employers, changes in employer, and any spells of non-employment lasting more than one quarter. We merge the two administrative datasets using Social Security numbers.

We find public insurance enrollment reduces the likelihood an adult in our sample will be employed by 0.9 to 7.2 percentage points in the difference-in-difference models and from 6.1 to 10.6 percentage points in the regression discontinuity models. These effect sizes are similar to magnitudes found in the current literature on the labor supply effects of other types of health insurance programs and the sign is consistent with both the theoretical and empirical literatures on the effects of cash and transfer programs on labor supply. We find public insurance also reduces average quarterly earnings by between $200 and $400, but we find no effect on earnings for a subset of enrollees who were continuously employed from 2009-2010.

II. Program Background

Launched in January 2009, Wisconsin's BadgerCare Plus Core Plan provides health insurance to adults with no dependent children who have incomes below 200% of the Federal Poverty Line (FPL). The state of Wisconsin applied for and received a federal 1115 waiver to extend some health benefits to this population. Once enrolled, members receive a managed care benefit package and face little cost-sharing. With few exceptions, coverage is not available to persons who already have any form of private health insurance, quit their job, or voluntarily dropped any health insurance in the 12 months prior to application. The program initially required an application fee of $60 and sliding scale premiums for those with incomes from 150-200% FPL. Upon enrollment, members were eligible to receive for a period of 12 months, after which eligibility would be reevaluated.

Enrollment started January 1, 2009 for limited groups and opened to the public on July 1, 2009. Application levels immediately exceeded projections and program budget, with enrollment reaching a high of 65,057. On October 5, 2009, then-Governor Jim Doyle announced at a news conference that Core Plan applications would be suspended effective October 9, 2009 at noon. The suspension was stated by the Governor to be the result of unanticipated demand for the program and was reported in newspapers statewide.

Subsequent eligible applications were placed on a waitlist. Waitlisted applicants were not required to pay the application fee, and were told that once openings in Core were available they would be notified. The number of waitlisted applicants had reached 89,412 by December 2010. The state has sought to decrease overall Core Plan enrollment to a sustainable level, and has thus not been enrolling waitlisted applicants as current Core Plan members leave the program. The only waitlisted applicants ever enrolled were a small number who were eligible for a medical waitlist bypass because of cancer or heart disease patients. The presence of a waitlist, imposed quickly based only on state budget criteria and not on participant characteristics, provides a natural and ready comparison group for those enrolled in the Core Plan. Those on the waitlist wanted to and were eligible to enroll, but were not able to do so before the enrollment suspension went into effect.

A stop-gap program with more limited benefits, called the BadgerCare Plus Basic Plan, was promised at the time of the announcement. The Basic Plan was formally announced in January 2010 and coverage was eventually offered to those enrolled on the waitlist effective in July 2010. The state legislature required the Basic Plan to be self-supporting through premiums. Participants in Basic were required to remain eligible for the Core Plan; this meant, among other requirements, their incomes had to remain below the 200% FPL threshold. Adverse selection has been a problem for the Basic Plan: enrollment in the program was closed on March 19, 2011 and enrollees saw multiple increases in required premiums over time.[3] Enrollment in Basic reached a high of 6,013 in April 2011 (reflecting March applicants) and has steadily declined since.

Core Plan enrollment to date has not been opened up to waitlist applicants, and attrition has reduced enrollment levels to approximately 24,000 as of July 2012. Attrition can occur because of a change in eligibility (such as an out of state move, a change in insurance status such as eligibility for insurance through a new job, or a change in categorical eligibility criteria), or a lack of re-enrollment on the part of the beneficiary. In addition, effective July 1, 2012 non-payment of newly required monthly premiums for enrollees with incomes above 133% FPL and a change in income eligibility prior to the end of the 12 month enrollment period became possible reasons for a change in eligibility. The future of the program is currently unclear; Wisconsin's governor and legislature rejected participation in federally incentivized Medicaid expansions under the Affordable Care Act in June 2013.

A potential complication is whether the particular implementation of this distribution mechanism itself influences the labor supply decisions of affected participants. If the waitlist participants we use as a control group for Medicaid recipients are themselves constrained by the waitlist because, for example, they believe they need to remain eligible for the program in order to eventually receive it, this would bias against us finding any effects of the program. If this were true, a better allocation mechanism would perhaps be a lottery since non-recipients would immediately know they would not receive the program and would make their labor supply decisions accordingly. We are unable to answer this question directly in this study. Most of the literature on waiting lists is related to allocation of medical care. Propper (1990,

[3] According to a state press release, (http://www.dhs.wisconsin.gov/News/PressReleases/2011/031811.htm) these changes were made because program expenditures had outpaced revenues.

1995) points out that there are costs of using waiting lists as mechanisms for medical care allocation in the U.K. and estimates these costs using contingent valuation. M. Johannesson et al. (1998) estimate the demand for private insurance that would reduce waiting times in Sweden. Globerman (1991) discusses the potential for decreases in productivity due to waiting times. None of these studies examine a random allocation mechanism as an alternative choice. Cullis et al. (2000) provide a general treatment of the theoretical and empirical literature on waiting lists for health care services.

III. Theory and Related Literature

A standard static labor supply model would predict income eligibility thresholds for public health insurance likely reduce the incentive to remain in or return to the workforce and, among workers, likely reduce the incentive to increase work hours. This occurs through the creation of a nonlinearity in the budget constraint known as the "Medicaid notch". The negative effect on labor supply results from both the reduced need for private, employer-sponsored coverage among recipients as well as the possibility that increased earnings would disqualify them for public coverage.

The existing economics literature portrays a mixed picture of the impact of Medicaid eligibility on the labor supply of low-income parents, the most comparable population available that has been studied. Initial work found strong work disincentives of Medicaid: Ellwood and Adams (1990) and Moffitt and Wolfe (1992) find single mothers on AFDC were less likely to exit coverage (and become employed) if the value of Medicaid to them was high. Subsequent work finds effect sizes of smaller magnitude (Yelowitz 1995, Ham and Shore-Sheppard 2001) and of the opposite sign (Ham and Shore-Sheppard 2005). Recent papers either find mixed effects (Hamersma and Kim 2009) or no effect (Hamersma 2010, Strumpf 2011). The inconclusive nature of the existing literature is suggestive of heterogeneous effects across populations and time periods studied, further motivating the need to study childless adults in isolation during recent years.

The literature on other important publicly provided health insurance programs is more conclusive. French and Jones (2011) show Medicare eligibility is an important determinant of retirement decisions. Boyle and Lahey (2010) find decreased labor supply on both the extensive and intensive margins for older veterans eligible for Department of Veteran's Affairs health programs.

Other types of cash and in-kind transfer programs in the United States have been found to negatively affect labor supply. Moffitt (2002) reviews the extensive empirical literature. More recently, Jacob and Ludwig (2012) find a 6% decline in labor force participation and a 10% decrease in earnings resulting from housing vouchers. Hoynes and Schanzenbach (2012) find reductions in employment and hours worked among single-headed households resulting from the food stamp program. Meyer (2002) and finds evidence of work disincentives of the Earned Income Tax Credit on the extensive but not on the intensive margin; Eissa and Hoynes (2004) confirm the finding of extensive margin work disincentives at the family level. Social Security Disability Insurance has generally been found to reduce employment among older men (Bound 1989, Parsons 1990, Gruber and Kubit 1997, Chen and Van der Klaauw 2008, Maestas et al. 2011, French and Song 2012).

The effect of public insurance on earnings is ambiguous in our context. If availability of public insurance leads to increased mobility and increased mobility results in better job matches, we could, all else equal, observe higher wages (and therefore earnings) among the public insurance enrollees. A second possibility is that workers could match with jobs that pay higher wages since the job would no longer need to pay health benefits. Baicker and Chandra (2006) find health insurance premium increases result in both a decreased probability of employment and lower wages, supporting the theory of a partial wage offset for health insurance. Since we do not observe hours worked, only quarterly earnings, in practice earnings could either increase (because of better matches and/or wage offsets) or decrease (because of fewer hours worked). Again, since workers must remain below the income eligibility threshold the positive effects are probably limited.

A final potential consequence of increased availability of public insurance related to the mobility argument is that such coverage may increase the likelihood a worker would leave the labor force to become self-employed. This is consistent with a compensating differential framework; the self-employment wage is effectively increased by the value of public insurance coverage. Results from the empirical literature are mixed (Lombard 2001, Holtz-Eakin et al. 1996, Zissimopoulos and Karoly 2007, Fairlie, Kapur, and Gates 2011); however, we acknowledge the possibility and discuss it further below.

IV. Data

The data sources for this project are state administrative records on enrolled and waitlisted Core Plan applicants and earnings records from Wisconsin's unemployment insurance (UI) system. In the state's records on Core plan enrollees and waitlisted applicants, we observe exact application date, age in months, monthly income at the time of application, county of residence and sex. The UI data include quarterly earnings for each individual from each covered firm at which he or she worked during that quarter; only employers not subject to unemployment insurance laws (for example, the self-employed) are exempt from reporting requirement. We have quarterly earnings for each person from Q1 2005 through Q4 2011. We merge the data on Core Plan applicants and enrollees to the UI data using Social Security numbers.

A particular strength of our analysis is that UI data exhibit superior accuracy over the survey-based data used in the existing literature. Virtually all employers are required to file quarterly wage reports for each employee on their payrolls. The wage reports include the employee's Social Security number and quarterly wages and the employer's federal tax identification number and industry classification code. Using these data, we can track quarterly earnings and employment at all covered firms, job changes, and any periods of non-employment lasting for at least one quarter.

Waitlist members were subject to basic screening, but to ensure comparability we employ several sample filters to ensure those on the waitlist would have actually been eligible for Core had they been invited to enroll (on the basis of all characteristics other than earnings, which may have changed in response to being on the waitlist). First, we drop anyone out of the eligibility age range (ages 19-64) according to date of birth. Second, we observe termination codes (reasons) for waitlist members that are removed from the waitlist, and we drop all members of the waitlist with codes indicating they either do not meet program requirements or are eligible for other Medicaid. We do not observe Core plan applicants who applied before the program cutoff and were found ineligible by the state.

Table 1 reports the demographic characteristics of our samples. Individuals who enrolled in the Core Plan are aged 43 on average and 49.6 percent were female, while the average age on those on the waitlist was lower – 38 years – and 43.7 percent were female. If we examine only those who applied within about a month of the October 9 cut-off date (i.e., those who enrolled into Core between September 1, 2009 and October 2, 2009 and those who were waitlisted and applied between October 9, 2009 and October 31, 2009), these differences are slightly smaller.

We consider several outcomes to measure labor supply using the quarterly employment records available in the Unemployment Insurance administrative data records. For employment, we consider average quarterly employment over the Q4 2009 to Q4 2011 period, with employment defined as having any earnings in a quarter. Earnings are defined as average earnings over Q4 2009 to Q4 2011. For the difference in differences models, these outcomes are defined analogously for the pre-program period. To consider intensive margin decisions, we select a subsample of applicants who were continuously employed (had positive earnings) throughout 2009 and 2010, and look at their average earnings for Q4 2009 to Q4 2011.

Finally, in order to assess the potential for our results to be explained by transitions to self-employment, which would not be recorded in our administrative data, we use the American Community Survey (ACS) from 2009 to 2011.We chose the ACS for its relatively large state sample sizes. The ACS includes a question that asks participants whether they were employed by a government, private company, nonprofit organization, or were self-employed. We classify all respondents who indicated that they were self-employed in this question (whether at an incorporated or unincorporated business) as self-employed.

V. Empirical Method

We identify the effect of the Core plan on the labor supply of childless adults using two complementary sets of analyses, each with its own relative strengths. The first is regression discontinuity (Lee and Lemieux, 2010) and the second is propensity score difference in differences (Heckman et al., 1997). Each empirical strategy relies on a slightly different assumption about the comparability of the waitlist applicants versus the enrolled applicants. If there were no differences between waitlist applicants and enrolled applicants, both approaches would be equally valid. While the regression discontinuity design likely exhibits superior internal validity relative to matching methods, the latter design is relatively better powered. We think the ability to assess the robustness of the results across these two methods provides more convincing evidence than implementing either approach on its own.

We first use a regression discontinuity (RD) design. Lee and Lemieux (2010) provide an overview of this type of research design as well as recent applications. In essence, this approach involves comparing the labor supply of those who applied just prior to October 9, 2009 (immediately before the enrollment cap was implemented) with the labor supply of those who applied just after October 9, 2009 (immediately after the enrollment cap was set). As discussed above, eligible applicants who applied prior to October 9 were enrolled into the program while those who applied after October 9 were placed on a waiting list. Because all eligible people who applied before October 9 were allowed to enroll in the Core plan and none who applied after were, we use a `sharp' regression discontinuity design.

Importantly, this date was announced precipitously (the cut-off was announced on October 5) and would have been unexpected by all potential applicants. However, the data show that the announcement resulted in an increase in applications between October 5 and October 9. Our preferred specifications use only the data on enrollees up to the announcement date, but we estimate and report specifications that include applications occurring between October 5 and 9 data as well.[4]

The RD approach enjoys a distinct advantage over simple comparisons of those enrolled in the Core Plan with those on the waiting list. Since the cutoff date was imposed arbitrarily by the state (and was not an original feature of the program), it is reasonable to assume the individuals applying just before the announced cutoff date were very similar to those applying just after the cutoff date. The standard RD identification assumption applies, and in this context is interpreted as: there is no self-selection into application based on the knowledge that the applicant will be on the waitlist rather than gain immediate insurance. We implement our estimates using a local linear regression approach. We include robustness checks to various bandwidths as part of our analysis. The standard validity checks are included in the Appendix.

The exact specification of our RD estimator is:

$$(1) \qquad Y_i = \alpha + \beta(X_i - x_0) + \tau W_i + \gamma(X_i - x_0)W_i + \epsilon_i$$

with kernel weights defined as $h - |X_i - x_0|$ where h is the bandwidth and all observations outside the bandwidth (more than h away from x_0) are discarded. Here, Y_i is the outcome under consideration, X_i is the date of application, x_0 is the cutoff date, W_i is an indicator for whether or not the individual was enrolled in Medicaid (equals one if on the waitlist, zero if in Core), and ϵ_i is a random error term. The treatment effect of interest is τ. The coefficients β and γ allow the slope of the regression to differ on either side of the cutoff x_0.

A disadvantage of RD is that it does not use the entire samples of those on the Core Plan and on the waitlist, so lack of sample size could lead to power issues (though this concern does not appear to be an issue in our case) and limit our ability to conduct sub-analyses that further stratify by age or sex of the applicant. A second issue is that the announcement prior to the actual application cutoff date makes the identification less straightforward than might be desired. Specifically, we might be concerned the announcement is a form of manipulation and affects waitlisted applicants in the post period in addition to those who enrolled during the few days between the announcement and the suspension of enrollment.

For these reasons we complement our regression discontinuity design by including a second approach, the use of difference-in-differences and propensity score weighted difference-in-differences methods. This design involves making the Core group and waiting list groups as comparable as possible based on

[4] This is similar to although not the same as the "donut-RD" estimate studied in Barreca et al. (2011) as a solution to heaping bias.

observable characteristics, as well as taking advantage of the panel nature of the earnings data. In contrast to the regression discontinuity analysis, propensity score weighting uses the entire samples of waitlisted and enrolled applicants. The most important difference with propensity score weighting relative to the discontinuity approach is the assumption required for identification: we must assume that conditional on observables included in the propensity score and an individual fixed effect, there was no selection on time-varying characteristics in the date of application (Smith and Todd 2005).

A rich methodological literature establishes the conditions under which the use of propensity scores is appropriate in examining labor market outcomes (examples include Card and Sullivan 1988; Dehejia and Wahba 1999; Deheija and Wahba 2002; Heckman, Ichimura, Smith and Todd 1996; Heckman, Ichimura and Todd 1997; Heckman and Smith 1999; and Smith and Todd 2005). A key finding from this body of work is that the underlying assumptions of propensity score methods are best met by including data on lagged labor market outcomes; indeed, lagged labor market measures have been found to be the single most important set of matching variables. We have access to historical UI data, which we use to construct such measures for the study sample. Moreover, our data meet the other key conditions established in the aforementioned methodological literature: that matched treatments and controls are drawn from the same geographical labor market and that their respective labor market outcomes are measured in the same way (Heckman, Ichimura and Todd 1997; Heckman, Ichimura, Smith and Todd 1996).[5]

We implement both standard difference indifferences with a variety of specifications as well as propensity score matched versions of these models. In particular, we estimate the following models:

$$Y_{it} = \beta_0 + \beta_1 Post_{it} + \beta_2 Core_i + \beta_3 \left(Post \times Core\right)_{it} + \delta X_i + \varepsilon_{it}, \tag{2}$$

and

$$Y_{it} = \alpha + \sum_{j=2005Q1}^{2011Q4} \gamma_j \left(Quarter_{it} = j\right) + \beta Core_{it} + \sum_{j=2005Q1}^{2011Q4} \varphi_j \left(Quarter_{it} = j\right) \times Core_{it} + \delta X_i + \varepsilon_{it}\ . \tag{3}$$

where

Y_{it} is an indicator for positive employment for individual i in quarter t,

$Post_{it}$ is an indicator for the earnings occurred in a quarter between Q3 2009 to Q4 2011,

$Core_{it}$ is an indicator that the individual enrolled into the Core plan,

$Quarter_{it}$ is an indicator for the quarter in which the earnings were observed, and

X_i is a set of indicator variables for sex, age in months, and county of residence.

[5] Also of note is a recent German study that finds that propensity score models including lagged labor market measures and a set of demographic covariates similar to our own perform just as well as models augmented with additional person-level measures such as personality traits and motivation (Biewen, Fitzenberger, Osikominu, and Paul 2010).

To implement our propensity score adjustments, we estimate the propensity score using a probit with controls for quarterly employment for each quarter from Q1 2005 to Q2 2009, quarterly earnings in each quarter from Q1 2005 to Q2 2009, age in years, sex, and county of residence. We then construct a propensity score weight for each control observation (waitlisted applicants) using an Epanechnikov kernel weight (Leuven and Sianesi 2003). The results of the propensity score models and the balancing tests are reported in the Appendix.

Finally, we also embed our regression discontinuity framework within the propensity score approach and estimate these models restricting the sample to applications that took place within thirty days of the cut-off date.

VI. Results

In this section, we present the results from the regression discontinuity analysis and those from the propensity score differences in differences analysis. Overall, both sets of analysis yield similar estimates despite being identified from different sets of assumptions.

Probability of Employment

Figure 1 illustrates the results of our local linear RD specifications for the employment outcome. All figures in the left column have the assignment variable, the exact date of application, on the x-axis and the outcome variable, average quarterly employment from Q4 2009 to Q4 2011, on the y-axis. The figure in the first row includes all application days thirty days before and after October 9, 2009. Each observation is the average of the outcome for all applicants on that day. The lines are estimated local linear regression functions.

The figure in the first column of the second row excludes the week prior to and after the cutoff day, starting from the left application date begins on September 4, 2009 and goes through October 4, 2009 and from the right application date begins October 15, 2009 and ends on November 15, 2009. The figure in the first column of the third row excludes just those days between the announcement and the cutoff, with applications from September 4, 2009 through October 4, 2009 and October 10, 2009 through November 10, 2009.

Results of the estimation are summarized in Table 2. Each specification shows a statistically significant and relatively large drop in employment among Core plan enrollees relative to waitlisted applicants, from 4.7 percentage points in the specification including all applications to 9.4 percentage points in the specification that excludes one week around the cutoff and 11 percentage points in the specification that excludes just the surge of applications between the announcement and the cutoff date. The results in Table 1 are all reported at a bandwidth of 15 days. Table 1 also includes specification checks that add all available covariates to the analysis (age, sex, employment in prior quarter, earnings in prior quarter). Results are not statistically different from the specifications without covariates.

Figure 1 also includes bandwidth robustness illustrations for each set of results in the right column. In these, the x-axis is the bandwidth at which the specification was estimated, while the y-axis is the size of the estimate. The solid dark line represents the estimate itself, and the lighter dashed lines represent the 95% confidence interval for the estimate. After some variability at the smallest bandwidths (as is to be expected), estimates do not vary with the bandwidth used for estimation.

We include standard validity checks in the Appendix. These include placebo tests (Table A3 and A4), covariates tests (Table A5), and density tests (Figure A1 and A2). All placebo and covariate tests are consistent with the regression discontinuity assumption with one possible exception: there is a small but statistically significant drop in age of applicants at the time of the cut-off of slightly over 3 years; however, including age as a covariate makes no difference in the results. Figure A1 makes clear the increase in applications during the last week. In addition, we can see in Figure A2 that applications were allowed on weekends during the post period and not during the pre period, resulting in a Monday bump. To eliminate this, we also estimate models defined by application week (Saturday-Friday) rather than day.

Figure 2 plots quarterly employment rates for those enrolled in the Core Plan and those waitlisted from Q1 2005 to Q4 2011 for our different estimation samples. In the first plot, we include all observations. In the second, we include only those observations who applied in either September or October 2009. In the fourth, we use our propensity score reweighted samples for those who applied in September and October.

Three things can be seen in Figure 2. First, Core plan enrollees and waitlisted enrollees both suffered large declines in employment rates around Q3 2009 that seemed to bottom out in about Q1 2010, suggesting that employment losses (and perhaps loss of ESI coverage) were a driver for many to apply for the Core Plan. Second, Core Plan enrollees tended to have higher employment rates in the quarters leading up to when enrollment into the plan opened in July 1, 2009. This suggests that some sort of adjustment based on observables and (at least) fixed unobservables needs to be conducted. Third, waitlisted applicants had higher employment rates in the quarters following the cut-off date, suggesting a substantial employment disincentive effect of public insurance.

The second two plots also show that the Core Plan enrollees and the waitlisted applicants who applied within one month of October 9 look relatively more similar in terms of their employment rates in the "pre" period but in the "post" period, the waitlisted applicants still show a substantially higher rate of employment.

Table 3 reports the results from our difference-in-differences models. The models based on equation (2) can be interpreted as the change in average employment rates over the "post" period (Q4 2009 to Q4 2011) from the average employment rate in the "pre" period (Q1 2005 to Q2 2009) for those enrolled in the Core Plan relative to those waitlisted.

The results indicate a relative decline in average employment rates of 7.2 percentage points for those with public insurance; these results are statistically significant and are robust to including controls for sex, age, and county of residence. When we restrict the sample to those who applied in September and October 2009, the estimated relative reduction in employment rates remains economically large – 5.8 percentage points – and statistically significant.

When we estimate the same models using our propensity score weighted sample, we find smaller estimates when the comparison is relative average employment rates between the “pre” and “post” periods (of between 0.9 and 3.3 percentage points), that are statistically significant.

Earnings

A negative earnings effect across the sample would be expected if wage rates remained the same and Medicaid enrollees were less likely to work. Figure 3 shows local linear regression discontinuity estimates of the effect of public insurance participation on quarterly earnings. The dependent variable is the average total quarterly wage and salary earnings from Q4 2009 to Q4 2011. A summary of these results is included in the second row of Table 2. In these specifications, waitlist participants earn more than Medicaid enrollees; the results suggest a negative earnings effect of Medicaid of between $200 and $400 per quarter, but are not always statistically significant. The results are more conclusive once we exclude last-minute applicants and in the specification that leaves out one week prior to and after the cutoff date, and are consistently around $400 per quarter.

Table 4 reports the results from our difference in differences models. The results indicate a relative decline in quarterly earnings of $60-80 for those with public insurance; these results are statistically significant and are robust to including controls for sex, age, and county of residence. When we restrict the sample to those who applied in September and October 2009, the estimated relative reduction in earnings becomes even larger – nearly $300 – and remains statistically significant.

When we estimate the same models using our propensity score weighted sample, we find a slightly different pattern. In the full sample, the results suggest a positive earnings effect of almost $100, while in the restricted sample, a negative earnings effect of $130. These effects are statistically significant in both samples.

Self-Employment

If some Core Plan participants are leaving wage and salary work for self-employment as a result of receiving public insurance, we would classify them as unemployed in our data. This would bias our results toward finding negative labor supply effects when none exist. As discussed above, results from the literature on the empirical relationship between health insurance portability and self-employment are mixed; however, given that it is a concern for us we wanted to test for the possibility.

We choose a sample of families with no children from the 2009-2011 American Community Survey (ACS) and compare those with incomes up to 200% of the federal poverty level to those with incomes from 200-400% of the federal poverty level in Wisconsin and nationally, before and after the Wisconsin program implementation. While we found that the share of low-income self-employed Wisconsin residents eligible for public insurance was higher than in the national sample, we found no evidence of a difference in the shares relative to the national difference over time. We interpret these results as supportive of the hypothesis that changes in self-employment are not an important determinant of changes in labor supply in our context. Full results from the triple difference estimation are available from the authors on request.

VII. Discussion

In this study, we examine the labor supply effects of publicly provided health insurance for low-income adults without dependent children. Our findings suggest that public insurance has a disincentive effect on the labor supply of low-income childless adults. The sizes of our estimated effects are large, ranging from 0.9 to 7.2 percentage points in the difference-in-difference models and from 6.1 to 10.6 percentage points in the regression discontinuity models. Among a population in which only approximately half of enrollees had any positive earnings in the quarter prior to application, these are large effects. Our evidence suggests that the net effect on earnings (including those who lost or changed jobs) was a reduction of $100-300 per quarter.

There are several caveats to our results. First, while we do find negative employment effects using two different and complementary methods that rely upon different identifying assumptions and across a variety of specifications, our identification strategies are imperfect. For example, even adjusting for observable differences between the Core Plan enrollees and the waitlisted applicants using the rich earnings and employment histories available in the UI data and employing difference in differences (which nets out any fixed unobserved differences), does not preclude the existence of time-varying unobserved differences between the two samples. Moreover, we do find differences at the cut-off discontinuity in the age of the applicants between those waitlisted and those enrolled, which may indicate a violation of the strict RD identifying assumptions. While these age differences are small and the estimated effects change little when we control for age in the RD models, this is still a concern.

Second, extrapolating from the Wisconsin Core Plan for childless adults to an expansion of Medicaid to childless adults may not be possible. The two programs differ one important way: Medicaid is an entitlement while the Core Plan is not. Since new enrollment into the Core Plan was ended on October 9, 2009, any Core Plan member who left the plan (perhaps as a result of gaining health insurance through a new employer), would not be able to go back on the plan should he or she subsequently lose that private insurance. This would not be the case with Medicaid; individuals would be free to exit and reenter the program as their eligibility changes. The fact that the Core Plan is not an entitlement could have had a "lock-in" effect on enrollees, which would exacerbate any employment disincentive relative to Medicaid. On the other hand, the waitlisted applicants had access to the Basic plan. Although only a small percentage of them took up this plan, its existence would provide a work disincentive as well, and minimize the estimated employment disincentive of public insurance.

Third, even if the Core plan is comparable to Medicaid, the implementation of the ACA will change some of the incentive effects of Medicaid. Some states will participate in the federally incentivized expansion of Medicaid, while some states will not. In states that participate, the "notch" in the budget constraint will change and perhaps be eliminated because of subsidies for coverage purchased on the exchanges, but states that do not participate will continue to have the notch.

Finally, as with other studies utilizing unemployment insurance records, we do not observe transitions into and out of self-employment. As we cannot differentiate between self-employment and being out of the labor force, we could be overstating the association between public insurance eligibility and labor market attachment. Using auxiliary data from the ACS, we explore trends in self-employment among the target population of interest over the study period in order to deduce the potential magnitude and direction of any resulting bias from this mislabeling. We find no evidence that there is important bias from our inability to identify self-employed members of our sample.

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Figure 1: Employment Rate by Day of Application

Figure 1. Employment Rate by Day of Application

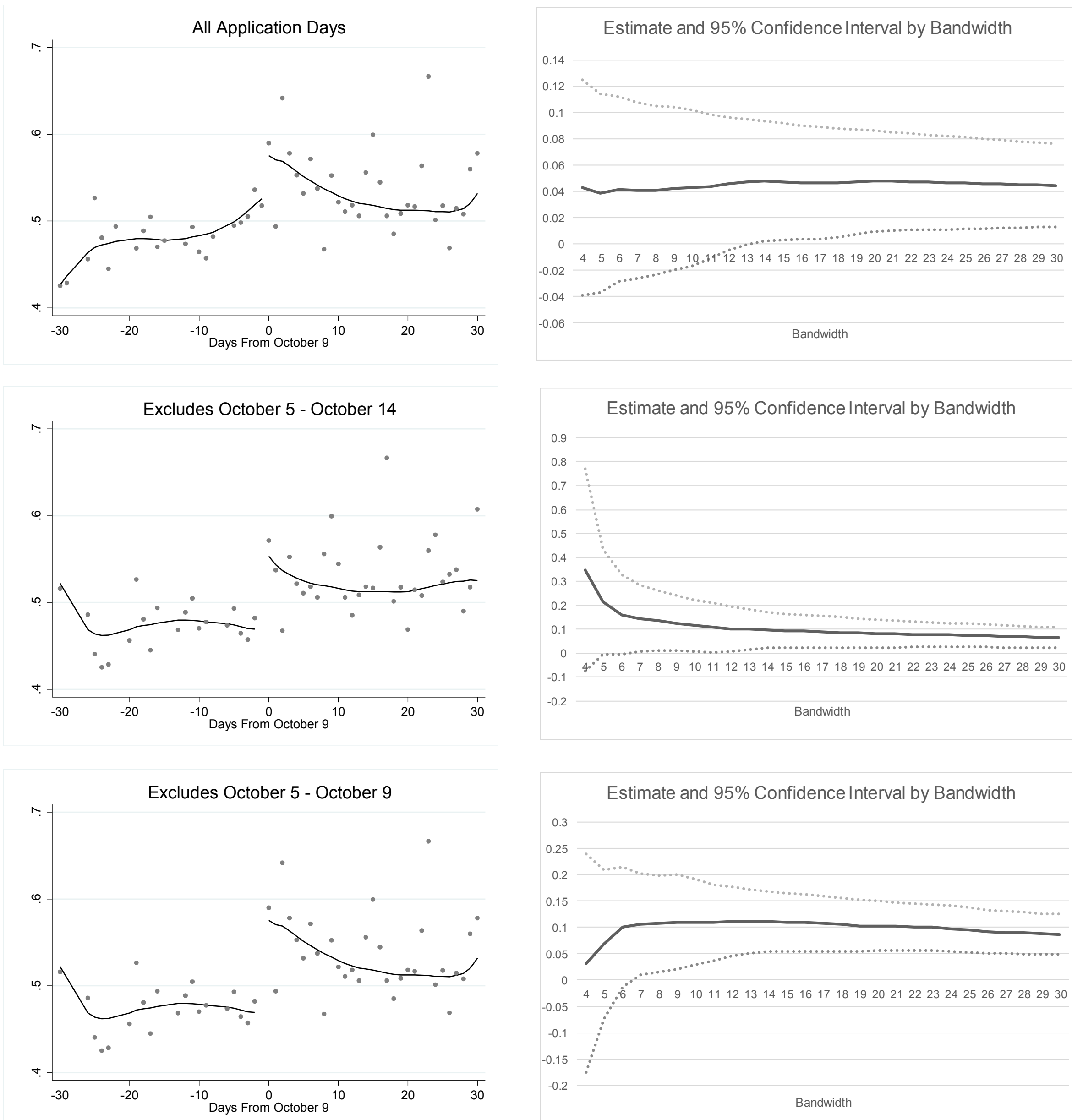
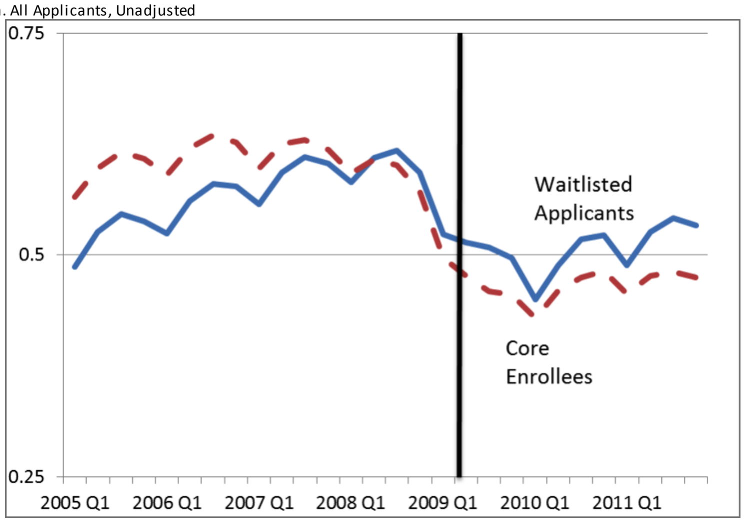


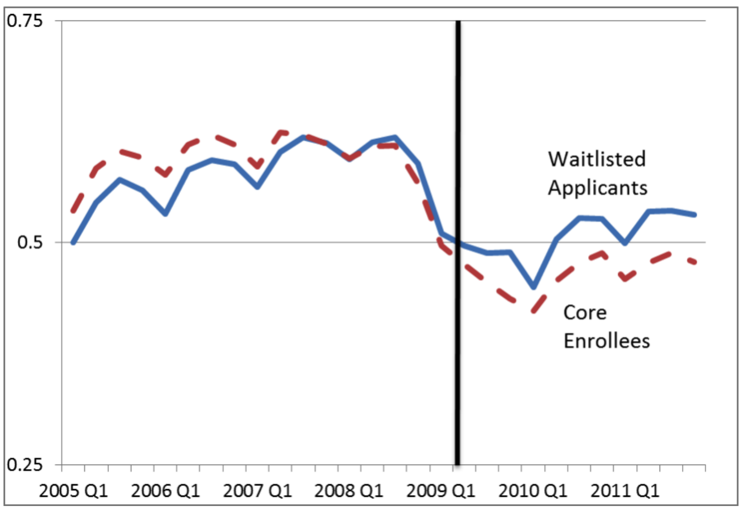
Figure 2: Employment Rates by Quarter

Figure 2: Employment Rates by Quarter

a. All Applicants, Unadjusted



b. Applicants from September and October 2009, Unadjusted



c. Applicants from September and October 2009, Propensity-Score Adjusted

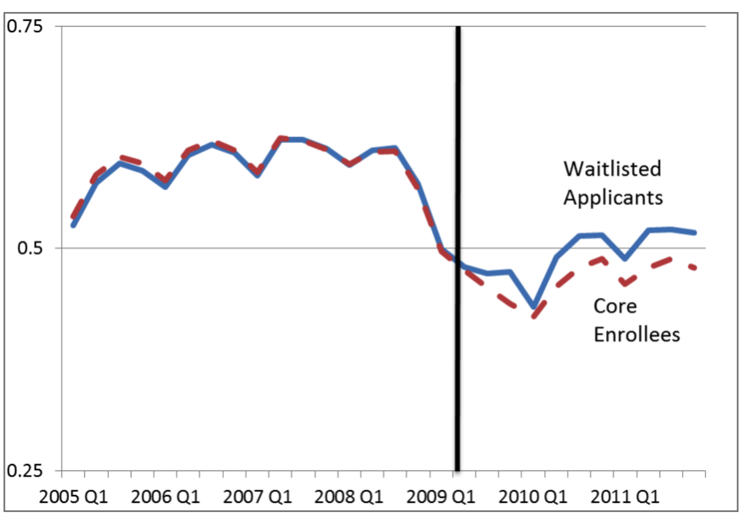


Figure 3: Earnings by Day of Application

Figure 3. Earnings by Day of Application

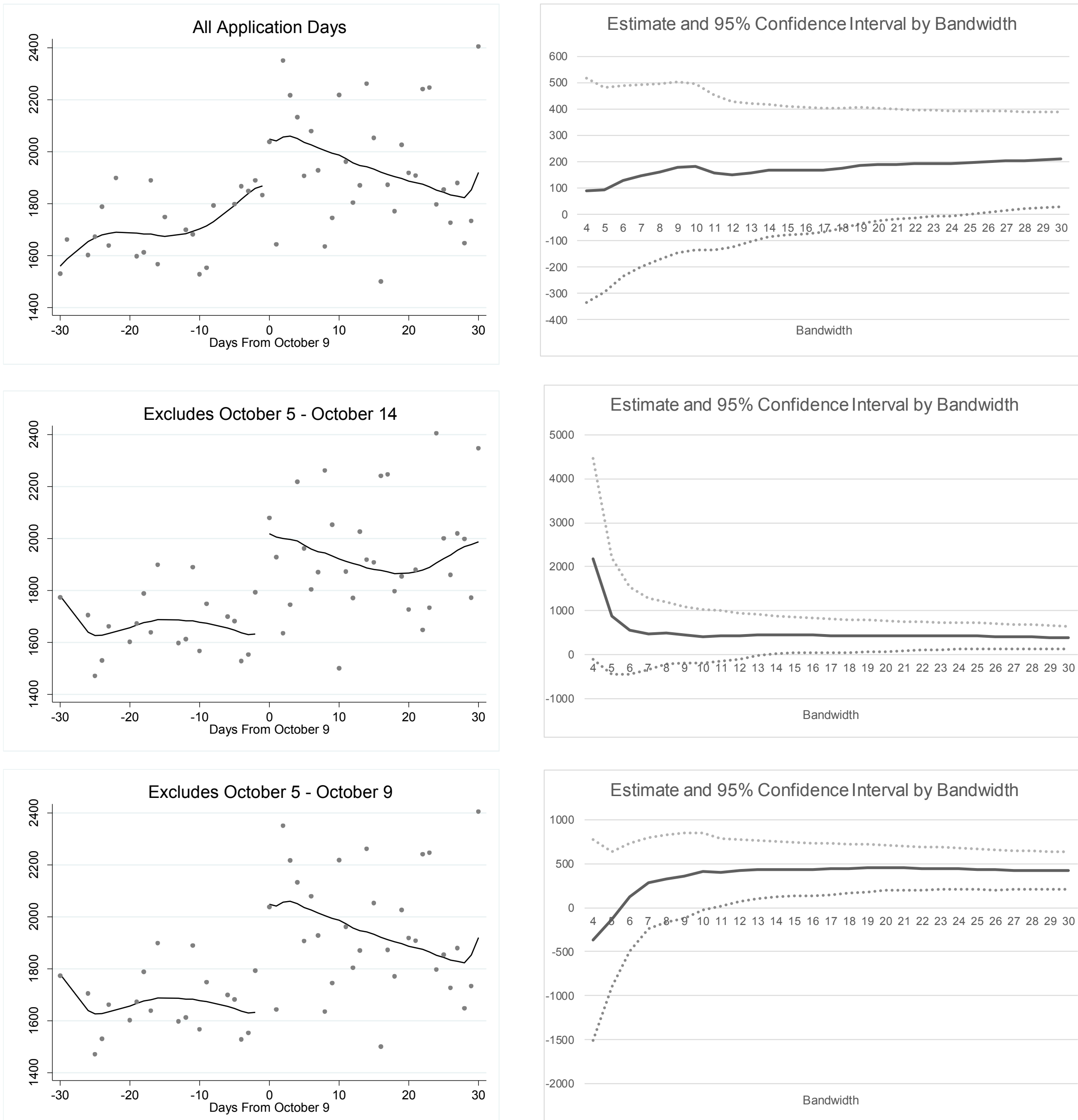


Table 1. Demographic Characteristics,Core Plan Enrollees vs. Waitlisted Applicants

	Core Plan Enrollees	Waitlisted Applicants
Ever applied		
Age	42.48	38.48
Female	0.50	0.44
Employed in quarter prior to appli	0.49	0.52
Earnings in quarter prior to applic	1430.66	1940.60
Average employment, Q409-Q41	0.49	0.52
Average earnings, Q409-Q411	1731.19	1854.87
Observations	36971	57189
Applied within 30 days of October 9, 2009		
Age	40.98	38.94
Female	0.47	0.46
Employed in quarter prior to appli	0.51	0.51
Earnings in quarter prior to applic	1650.43	1620.56
Average employment, Q409-Q41	0.50	0.52
Average earnings, Q409-Q411	1778.13	1931.20
Observations	9246	3050

Source: Authors' calculations from WI Administrative Data
Notes: Quarter prior to application defined as Q209 for Core Enrollees, Q309 for Waitlisted App

Table 2. Summary of Regression Discontinuity Results

	Specification					
	All Dates		Excludes Oct 5 - Oct 14		Excludes Oct 5 - Oct 9	
Outcome	No Covariates	Covariates	No Covariates	Covariates	No Covariates	Covariates
Average Employment Rate, Q42009-Q42011	0.0474**	0.039**	0.0942***	0.0964***	0.11***	0.0953***
	0.0226	*0.0189*	*0.0363*	*0.0326*	*0.0282*	*0.0239*
Average Earnings, Q42009-Q42011	166.1	169.7	445.9**	392.9**	434.8***	413.9***
	124.5	*109.9*	*208.8*	*193.7*	*154.7*	*138.9*
Average Earnings Q42009-Q42011, Continuously Employed	70.39	136.9	202.1	-146.8	12.44	-14.65
	239.7	*203.3*	*461.6*	*321.4*	*314.7*	*269.2*

Notes: Table displays regression discontinuity estimates of effect of Core enrollment on outcome, with robust standard error in italics.
All results calculated at bandwidth of 15 days. * indicates significance at 10% level, ** 5%, ***1%

Table 3

Employment Rates of Core Plan Enrollees and Waitlisted Applicants: Differences in Differences

	(1)	(2)	(3)	(4)
Post	-0.056 (0.001)***	-0.056 (0.001)***	-0.063 (0.006)***	-0.063 (0.007)***
Core	0.030 (0.002)***	0.012 (0.002)***	0.014 (0.007)*	0.016 (0.007)*
Post*Core	**-0.072 (0.002)***	**-0.072 (0.002)***	**-0.058 (0.008)***	**-0.058 (0.008)***
Constant	0.563 (0.001)***	0.110 (0.013)***	0.572 (0.006)***	0.124 (0.006)***
Demographic Variables	No	Yes	No	Yes
Time Applied	Ever	Ever	Sept-Oct	Sept-Oct
PS Weighted	No	No	No	No
Observations	127,972	127,972	9,378	9,378
Observation Quarters	3,583,216	3,583,216	262,584	262,584

	(5)	(6)	(7)	(8)
Post	-0.119 (0.002)***	-0.119 (0.002)***	-0.088 (0.007)***	-0.088 (0.007)***
Core	-0.006 (0.002)**	-0.011 (0.002)***	0.002 (0.007)	0.007 (0.007)
Post*Core	**-0.009 (0.002)***	**-0.009 (0.002)***	**-0.033 (0.009)***	**-0.033 (0.009)***
Constant	0.599 (0.001)***	0.123 (0.015)***	0.583 (0.006)***	0.696 (0.044)***
Demographic Variables	No	Yes	No	Yes
Time Applied	Ever	Ever	Sept-Oct	Sept-Oct
PS Weighted	Yes	Yes	Yes	Yes
Observations	127,972	127,972	9,373	9,373
Observation Quarters	3,583,216	3,583,216	262,444	262,444

Note: The "pre" period includes Q1 2005 to Q2 2009 and the "post" period includes Q4 2009 to Q4 2011. Standard errors are clustered at the individual level. Demographic variables include dummy variables for sex, age in months, and county of residence.

Table 4
Net Earnings of Core Plan Enrollees and Waitlisted Applicants: Differences in Differences

	(1)	(2)	(3)	(4)
Post	-607.98 (9.34)***	-599.83(9.43)***	-461.95(41.89)***	450.55(42.56)***
Core	96.41 (14.48)***	-339.52(14.65)***	124.45(55.65)**	31.94(52.84)*
Post*Core	**-61.10 (14.56)*** **	**-81.31(14.85)*** **	**-280.43(55.90)*** **	**-290.10(56.49)*** **
Constant	2335.45 (10.01)***	296.85(84.06)***	2201.31(43.39)***	2657.20(308.80)***
Demographic Variables	No	Yes	No	Yes
Time Applied	Ever	Ever	Sept-Oct	Sept-Oct
PS Weighted	No	No	No	No
Observations	132,331	127,972	9,465	9,378
Observation Quarters	3,705,268	3,583,216	265,020	262,584
	(5)	(6)	(7)	(8)
Post	-780.36(9.28)***	-780.36(9.28)***	598.35(44.22)***	598.35(44.27)***
Core	-73.66(13.81)***	-88.26(13.55)***	33.99(55.90)	42.56(52.89)
Post*Core	**98.97(14.75)*** **	**98.97(14.75)*** **	**-131.28(57.42)** **	**-131.28(57.48)** **
Constant	2301.36(8.68)***	339.03(82.14)***	2280.56(43.98)***	2625.96(316.12)***
Demographic Variables	No	Yes	No	Yes
Time Applied	Ever	Ever	Sept-Oct	Sept-Oct
PS Weighted	Yes	Yes	Yes	Yes
Observations	127,972	127,972	9,373	9,373
Observation Quarters	3,583,216	3,583,216	262,444	262,444

Note: The "pre" period includes Q1 2005 to Q2 2009 and the "post" period includes Q4 2009 to Q4 2011. Standard errors are clustered at the individual level. Demographic variables include dummy variables for sex, age in months, and county of residence.

Figure A1: Density Graph

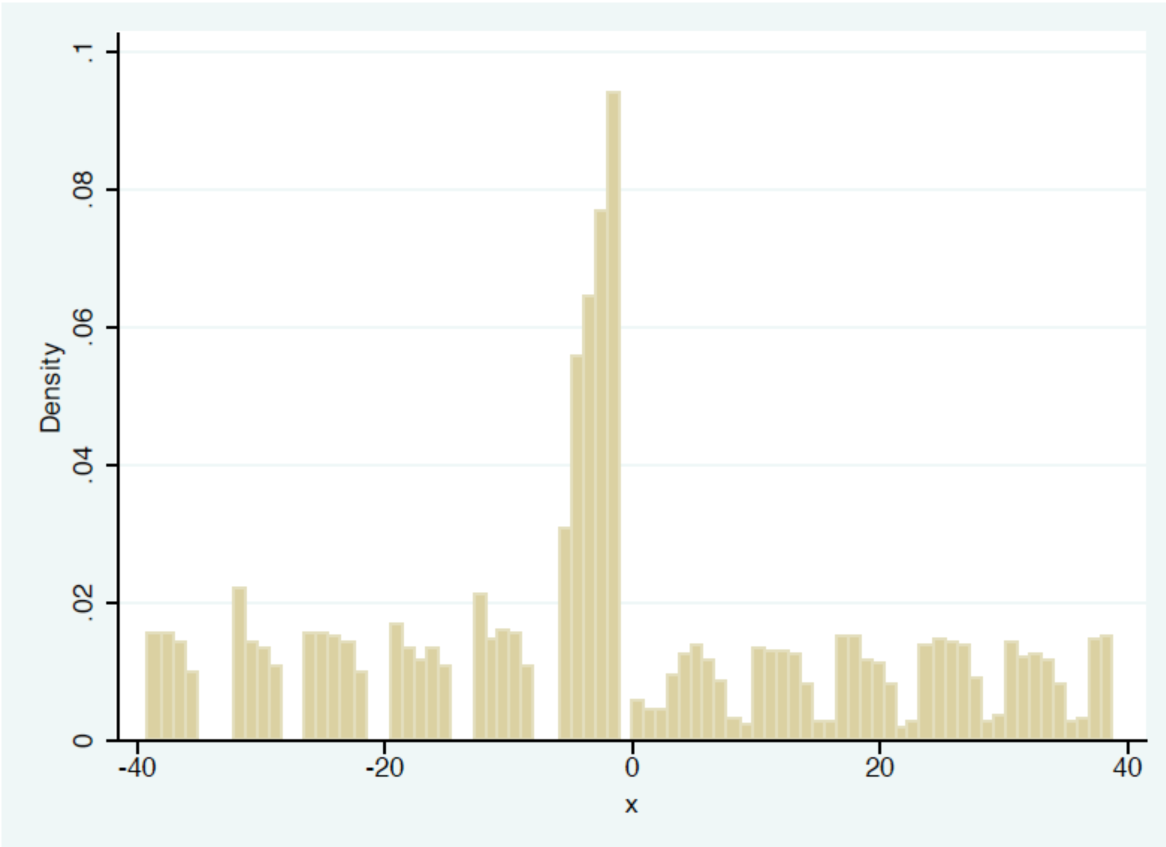


Figure A2: Density Graph Donut Model

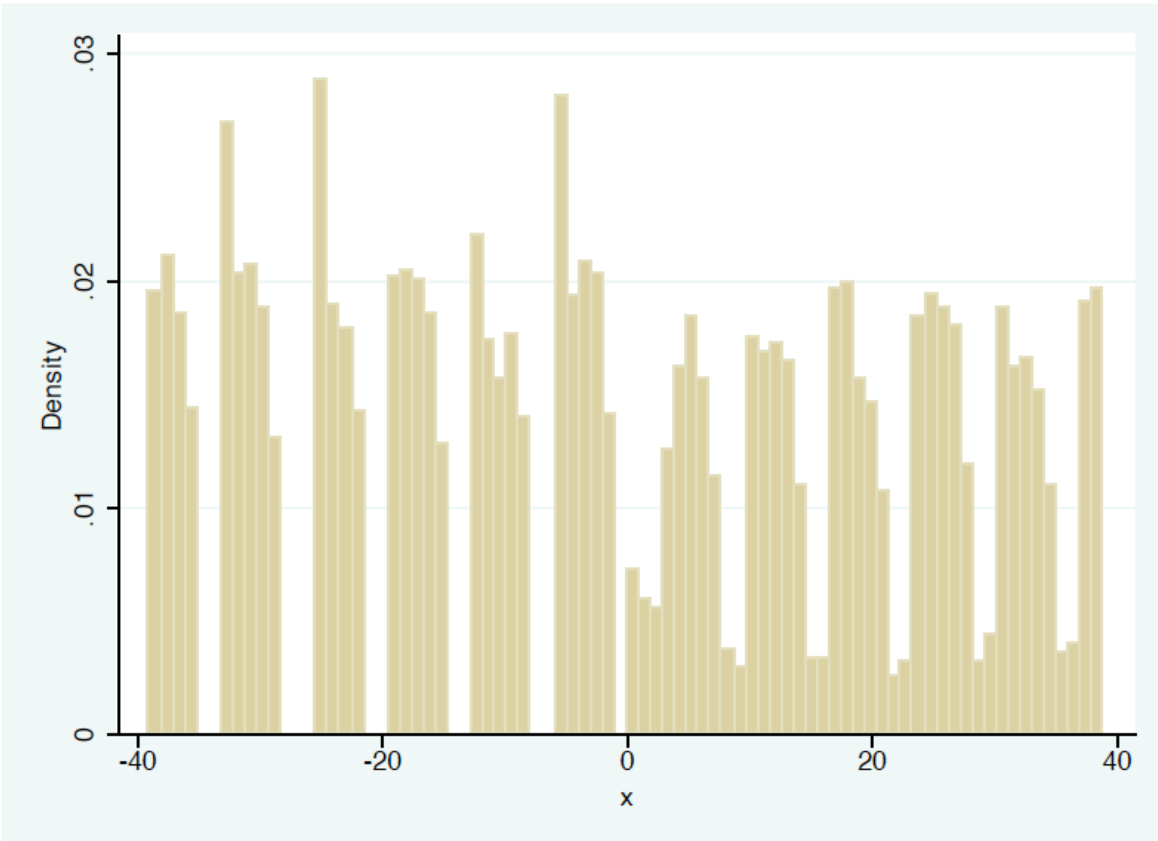


Table A1: Propensity Score (Probit) Models of Core Plan Enrollment

	Ever Applied		Applied Sept - Oct	
	Coef.	Std. Err.	Coef.	Std. Err.
Employed 2005 Q1	0.057	0.013	-0.058	0.046
Employed 2005 Q2	0.013	0.014	0.024	0.051
Employed 2005 Q3	0.049	0.014	0.002	0.050
Employed 2005 Q4	0.042	0.014	-0.017	0.050
Employed 2006 Q1	0.023	0.014	0.064	0.050
Employed 2006 Q2	0.028	0.014	-0.041	0.051
Employed 2006 Q3	0.074	0.013	0.075	0.050
Employed 2006 Q4	0.019	0.013	0.022	0.049
Employed 2007 Q1	0.007	0.013	0.035	0.049
Employed 2007 Q2	0.038	0.013	0.086	0.049
Employed 2007 Q3	0.014	0.013	-0.041	0.048
Employed 2007 Q4	0.004	0.013	-0.031	0.047
Employed 2008 Q1	0.018	0.013	0.007	0.047
Employed 2008 Q2	0.028	0.013	-0.039	0.048
Employed 2008 Q3	0.027	0.013	0.053	0.047
Employed 2008 Q4	0.008	0.013	-0.047	0.046

Employed 2009 Q1	0.066	0.013	0.014	0.046
Employed 2009 Q2	0.049	0.012	-0.005	0.042
Earnings 2005 Q1	4.69E-06	2.65E-06	0.0000178	0.0000105
Earnings 2005 Q2	-5.63E-06	3.00E-06	1.34E-06	0.0000113
Earnings 2005 Q3	6.49E-07	2.78E-06	-3.87E-06	0.0000105
Earnings 2005 Q4	-8.10E-06	2.74E-06	1.54E-07	9.62E-06
Earnings 2006 Q1	5.05E-06	2.88E-06	0.0000155	0.000011
Earnings 2006 Q2	-4.38E-06	2.92E-06	1.13E-06	0.0000106
Earnings 2006 Q3	-3.35E-06	2.73E-06	-9.85E-06	0.0000101
Earnings 2006 Q4	-3.66E-06	2.61E-06	-0.0000158	9.32E-06
Earnings 2007 Q1	1.35E-06	2.73E-06	-5.37E-06	0.00001
Earnings 2007 Q2	-6.08E-06	2.74E-06	-4.58E-06	9.00E-06
Earnings 2007 Q3	-7.11E-07	2.68E-06	4.95E-06	8.96E-06
Earnings 2007 Q4	-3.90E-06	2.57E-06	1.04E-06	9.10E-06
Earnings 2008 Q1	-2.42E-06	2.59E-06	-0.000011	0.0000101
Earnings 2008 Q2	-1.88E-06	2.57E-06	0.0000189	9.28E-06
Earnings 2008 Q3	-0.0000126	2.47E-06	-0.0000112	9.11E-06
Earnings 2008 Q4	-0.0000118	2.52E-06	-9.61E-06	8.93E-06
Earnings 2009 Q1	-0.0000163	2.85E-06	5.44E-06	0.0000101
Earnings 2009 Q2	-0.0000508	2.55E-06	-0.0000121	8.35E-06
Age in Years	0.019	0.000	0.010	0.001

Female	0.091	0.008	0.053	0.028
Constant	-1.174	0.016	-0.151	0.057

Table A2: Balancing Test for Propensity Score Matched Samples

	All Applicants			Applied Sept - Oct		
	Core Enrollees	Waitlisted Applicants	P-value	Core Enrollees	Waitlisted Applicants	P-value
Employed 2005 Q1	0.565	0.567	0.51	0.536	0.526	0.29
Employed 2005 Q2	0.598	0.602	0.19	0.583	0.574	0.32
Employed 2005 Q3	0.616	0.620	0.12	0.603	0.595	0.40
Employed 2005 Q4	0.609	0.613	0.14	0.596	0.587	0.34
Employed 2006 Q1	0.590	0.595	0.12	0.577	0.569	0.39
Employed 2006 Q2	0.621	0.627	0.07	0.610	0.605	0.52
Employed 2006 Q3	0.635	0.642	0.04	0.621	0.617	0.64
Employed 2006 Q4	0.627	0.633	0.03	0.610	0.608	0.82
Employed 2007 Q1	0.598	0.603	0.09	0.586	0.582	0.70
Employed 2007 Q2	0.625	0.631	0.05	0.624	0.622	0.78
Employed 2007 Q3	0.629	0.636	0.03	0.622	0.622	0.96
Employed 2007 Q4	0.619	0.624	0.06	0.612	0.612	0.93
Employed 2008 Q1	0.592	0.598	0.06	0.595	0.594	0.98
Employed 2008 Q2	0.608	0.615	0.02	0.609	0.611	0.84
Employed 2008 Q3	0.602	0.609	0.02	0.610	0.613	0.71
Employed 2008 Q4	0.572	0.578	0.04	0.568	0.572	0.60
Employed 2009 Q1	0.497	0.503	0.09	0.497	0.500	0.76
Employed 2009 Q2	0.476	0.482	0.05	0.475	0.480	0.63

Earnings 2005 Q1	2150	2197	0.02	2132	2042	0.12
Earnings 2005 Q2	2383	2433	0.02	2385	2295	0.15
Earnings 2005 Q3	2548	2606	0.01	2571	2494	0.23
Earnings 2005 Q4	2471	2530	0.01	2504	2411	0.15
Earnings 2006 Q1	2301	2367	0.00	2340	2258	0.18
Earnings 2006 Q2	2444	2511	0.00	2513	2449	0.31
Earnings 2006 Q3	2497	2568	0.00	2530	2498	0.61
Earnings 2006 Q4	2488	2557	0.00	2513	2492	0.74
Earnings 2007 Q1	2277	2350	0.00	2343	2310	0.58
Earnings 2007 Q2	2404	2481	0.00	2519	2481	0.55
Earnings 2007 Q3	2424	2507	0.00	2539	2529	0.88
Earnings 2007 Q4	2403	2487	0.00	2511	2508	0.97
Earnings 2008 Q1	2149	2237	0.00	2274	2271	0.96
Earnings 2008 Q2	2211	2303	0.00	2419	2406	0.83
Earnings 2008 Q3	2155	2246	0.00	2353	2370	0.76
Earnings 2008 Q4	1972	2068	0.00	2127	2137	0.86
Earnings 2009 Q1	1475	1548	0.00	1614	1618	0.94
Earnings 2009 Q2	1348	1429	0.00	1475	1480	0.91
Age in Years	43.4	43.3	0.36	41.2	40.7	0.04
Female	0.496	0.496	0.98	0.469	0.464	0.65

Table A3: Placebo tests for Regression Discontinuity Models

	Full Data		Doughnuthole	
	(1)	(2)	(3)	(4)
	Average Employment, Q3 2009 to Q4 2011	Employment Q4 2011	Average Employment, Q3 2009 to Q4 2011	Employment Q4 2011
Cut-off at day t-4				
Coef.	-0.0124	-0.0247	-0.0284	-0.0586
Star				
Std. Err.	0.0307	0.0399	0.0356	0.0460
Observations	6,882	6,882	2,761	2,761
R-squared	0.043	0.022	0.042	0.024
Cut-off at day t-6				
Coef.	-0.000433	-0.0344	-0.0164	-0.0508
Star				
Std. Err.	0.0338	0.0446	0.0345	0.0457
Observations	5,538	5,538	2,268	2,268
R-squared	0.034	0.016	0.028	0.014
Cut-off at day t-8				
Coef.	-0.0510	-0.0947	-0.0459	-0.0709
Star		(*)		
Std. Err.	0.0379	0.0495	0.0405	0.0533
Observations	3,335	3,335	1,923	1,923
R-squared	0.025	0.013	0.021	0.013

Cut-off at day t-10				
Coef.	0.0179	0.0734	0.0179	0.0734
Star				
Std. Err.	0.0460	0.0615	0.0460	0.0615
Observations	1,983	1,983	1,983	1,983
R-squared	0.023	0.015	0.023	0.015
Cut-off at day t-12				
Coef.	0.0106	0.0210	0.0106	0.0210
Star				
Std. Err.	0.0345	0.0454	0.0345	0.0454
Observations	2,084	2,084	2,084	2,084
R-squared	0.026	0.016	0.026	0.016
Cut-off at day t-14				
Coef.	0.0354	0.0998	0.0354	0.0998
Star		(**)		(**)
Std. Err.	0.0376	0.0501	0.0376	0.0501
Observations	1,878	1,878	1,878	1,878
R-squared	0.030	0.022	0.030	0.022

Cut-off at day t-16				
Coef.	-0.0556	-0.0259	-0.0556	-0.0259
Star				
Std. Err.	0.0421	0.0548	0.0421	0.0548
Observations	1,915	1,915	1,915	1,915
R-squared	0.034	0.025	0.034	0.025
Cut-off at day t-18				
Coef.	-0.0170	-0.109	-0.0170	-0.109
Star		(**)		(**)
Std. Err.	0.0369	0.0465	0.0369	0.0465
Observations	2,102	2,102	2,102	2,102
R-squared	0.033	0.028	0.033	0.028
Cut-off at day t-20				
Coef.	0.0387	0.0257	0.0387	0.0257
Star				
Std. Err.	0.0348	0.0459	0.0348	0.0459
Observations	2,036	2,036	2,036	2,036
R-squared	0.033	0.024	0.033	0.024

Note: Bandwidth is 10 days; Robust standard errors reported.

*** p<0.01, ** p<0.05, * p<0.1

Table A4: Placebo Regression Discontinuity with October 5th, 2009 as the Cut-off

Variable	Bandwidth	5	10	15	20	25	30	35
Employment Q4 2011								
	Coef.	-0.0313	-0.0349	-0.0363	-0.0355	-0.0255	-0.0127	-0.000871
	Star							
	Std. Err.	0.0428	0.032	0.0276	0.0244	0.0213	0.0189	0.0174
	Observations	2,021	7,942	9,390	10,490	11,648	13,553	14,703
	R-squared	0.001	0.002	0.002	0.002	0.002	0.002	0.002
Average Employment, Q3 2009 to Q4 2011								
	Coef.	-0.0321	-0.0244	-0.0248	-0.0221	-0.0127	-0.000873	0.00797
	Star							
	Std. Err.	0.0331	0.0248	0.0215	0.019	0.0166	0.0147	0.0135
	Observations	2,021	7,942	9,390	10,490	11,648	13,553	14,703
	R-squared	0.001	0.003	0.003	0.003	0.003	0.003	0.003

Robust standard errors reported.

*** p<0.01, ** p<0.05, * p<0.1

Table A5: Regression Discontinuity Covariate Tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bandwidth	5	10	15	20	25	30	35	40
Income at time of application								
Coef.	39.45	-75.36	-113.0	-129.6	-134.9	-129.7	-122.9	-122.0
			(**)	(***)	(***)	(***)	(***)	(***)
St. Err.	88.66	61.44	45.75	38.90	35.15	31.99	29.71	27.93
Observations	1,394	2,584	3,958	5,668	6,737	7,869	9,568	10,973
R-squared	0.011	0.009	0.009	0.009	0.008	0.008	0.008	0.008
Age in months								
Coef.	-43.96	-50.77	-43.23	-39.95	-37.19	-34.64	-31.59	-29.81
	(**)	(***)	(***)	(***)	(***)	(***)	(***)	(***)
St. Err.	20.26	14.16	10.59	9.120	8.290	7.565	7.002	6.582
Observations	1,394	2,584	3,958	5,668	6,737	7,869	9,568	10,973
R-squared	0.021	0.013	0.011	0.008	0.007	0.006	0.006	0.005
Female								
Coef.	-0.0397	-0.00656	0.0148	0.0226	0.0294	0.0299	0.0240	0.0189

St. Err.	0.0669	0.0475	0.0353	0.0302	0.0273	0.0248	0.0229	0.0214
Observations	1,394	2,584	3,958	5,668	6,737	7,869	9,568	10,973
R-squared	0.001	0.001	0.001	0.000	0.000	0.000	0.000	0.000
Employed at time of application								
Coef.	0.0971	0.0407	0.0118	0.000548	-0.00443	-0.00332	-0.00642	-0.00907
St. Err.	0.0641	0.0456	0.0337	0.0287	0.0259	0.0235	0.0216	0.0203
Observations	1,394	2,584	3,958	5,668	6,737	7,869	9,568	10,973
R-squared	0.003	0.001	0.001	0.001	0.001	0.001	0.001	0.001

Robust standard errors reported.

*** p<0.01, ** p<0.05, * p<0.1