2014

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

Laura Dague
Texas A & M University - College Station

Thomas C. DeLeire
Georgetown University

Lindsey Leininger
University of Illinois at Chicago

Upjohn Institute working paper ; 14-213

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

Upjohn Institute Working Paper 14-213

Laura Dague
Texas A&M University
E-mail: dague@tamu.edu

Thomas DeLeire
Georgetown University, NBER, and IZA
E-mail: td495@georgetown.edu

Lindsey Leininger
University of Illinois at Chicago
E-mail: lindsey1@uic.edu

April 2014

ABSTRACT

This study provides plausibly causal estimates of the effect of public insurance coverage on the employment of nonelderly, nondisabled adults without dependent children ("childless adults"). We use regression discontinuity and propensity score matching difference-in-differences methods to take advantage of the sudden imposition of an enrollment cap, comparing the labor supply of enrollees to eligible applicants on a waitlist. We find that enrollment into public insurance leads to sizable and statistically meaningful reductions in employment up to at least nine quarters later, with an estimated size of 2–10 percentage points, depending on the model used.

JEL Classification Codes: I18, I38, J22

Key Words: health insurance, labor supply, employment, Medicaid

ACKNOWLEDGMENTS

This work is supported by grants from the UC Davis Poverty Center and the W.E. Upjohn Institute for Employment Research. We thank Gaston Palmucci, Chris Reynolds, Kristen Voskuil, and Kara Mandell for excellent research assistance. Alan Barreca, Kitt Carpenter, Donna Friedsam, Kosali Simon, Tim Moore, and participants of the 2014 Affordable Care Act and the Labor Market conference at the Chicago Fed provided helpful comments.

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 could eventually gain coverage under Medicaid expansions associated with the 2010 Affordable Care Act (ACA; Stephens et al. 2013).[1] Given the large and growing population 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 generally should reduce labor supply, and extensive empirical research typically has shown that most such programs do indeed have the hypothesized effect. 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, 2005; Hamersma and Kim 2009; Hamersma 2010; Strumpf 2011). The inconclusive nature of the existing literature suggests that effects are heterogeneous across populations and time periods studied.

We contribute to this literature by providing plausibly causal estimates of the effect of means-tested public insurance coverage on the employment of nonelderly, nondisabled adults without dependent children (childless adults). Researchers have largely 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. A recent paper by Garthwaite et al. (2013)

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

examines eligibility contractions in Tennesse's program (TennCare), which had been available to childless adults until July 2005, and found both large reductions in Medicaid coverage and large increases in employment rates among childless adults in Tennessee following this contraction. These results are consistent with a 60 percentage point reduction in labor supply stemming from the availability of public insurance. Baicker et al. (2013) examined the impacts of Medicaid on the employment of recipients through the Oregon Health Insurance Experiment and found modest labor supply effects, of 1.6 percentage points, that are not statistically different from zero.

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 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 in 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 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. 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 data sets using Social Security numbers.

We find that public insurance enrollment reduces the likelihood that an adult in our sample will be employed by 2.4 to 5.9 percentage points in the difference-in-difference models and by 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.

PROGRAM BACKGROUND

Launched in January 2009, Wisconsin's BadgerCare Plus Core Plan provides health insurance to childless adults with household incomes below 200 percent 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 a $60 application fee. Upon enrollment, members were eligible to receive benefits for a period of 12 months, when eligibility would be reevaluated.

Enrollment began January 1, 2009, for a limited group 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 result from 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 they would be notified once openings in Core were available. The waitlist had reached 89,412 individuals 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. 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 enroll and were eligible, but they were not able to do so before the enrollment suspension went into effect.[2]

Core Plan enrollment to date has not been opened up to waitlist applicants, and attrition had reduced enrollment levels to approximately 24,000 as of July 2012. Attrition can occur through 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 failure to reenroll on the part of the beneficiary. In addition, effective July 1, 2012, nonpayment of newly required monthly premiums for enrollees with incomes above 133 percent 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. Wisconsin's governor and legislature chose not to participate in federally incentivized Medicaid expansions under the Affordable Care Act; however, effective April 1, 2014, all childless adults with incomes under 100 percent of the FPL are allowed to enroll in the Medicaid program, and all adults with incomes over 100 percent of the FPL are required to transition out of the program.

A potential complication is whether the 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

[2]A stop-gap program with more limited benefits, called the BadgerCare Plus Basic Plan, was promised for waitlisted applicants 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 percent 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. According to a state press release), these changes were made because program expenditures had outpaced revenues (Brueck, 2009). Enrollment in Basic reached a high of 6,013 in April 2011 (reflecting March applicants) and has steadily declined since.

they need to remain eligible for the program in order to eventually receive it, this would bias against us finding any effects. If true, a better allocation mechanism would perhaps be a lottery since nonrecipients would immediately know that they would not receive the program and would make their labor supply decisions accordingly. We are unable to answer this question directly. Most of the literature on waiting lists relates to allocation of medical care. Propper (1990, 1995) points out that there are costs to using waiting lists as mechanisms for medical care allocation in the United Kingdom and estimates these costs using contingent valuation. Johannesson, Johansson, and Soderqvist (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, Jones, and Propper (2000) provide a general treatment of the theoretical and empirical literature on waiting lists for health care services.

THEORY AND RELATED LITERATURE

A standard static labor supply model would predict that 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. The negative effect on labor supply results from the reduced need for private coverage among recipients as well as the possibility that increased earnings would disqualify them from public coverage (the “Medicaid notch”).

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 Medicaid's value 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 suggests 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 that 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 Veterans Affairs health programs. Dave et al. (2013) find declines in labor supply among pregnant women eligible for Medicaid coverage during their pregnancy.

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 percent decline in labor force participation and a 10 percent 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) finds that the Earned Income Tax Credit discourages work 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 Kubik 1997; Chen and Van der Klaauw 2008; Maestas, Mullen, and Strand 2013; French and Song 2012).

The effect of public insurance on earnings is ambiguous in our context. If availability of public insurance leads to increased job 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 that increases in health insurance premiums result in both a decreased probability of employment and lower wages, supporting 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 likely limited.

Finally, increased availability of public insurance may increase the likelihood that a worker would leave the labor force to become self-employed. 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, Penrod, and Rosen 1996; Zissimopoulos and Karoly 2007; Fairlie, Kapur, and Gates 2011); however, we acknowledge the possibility and discuss it further below.

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 where he or she worked during that quarter; only employers not subject to unemployment insurance laws (for example, the self-employed) are exempt from the reporting requirement. We observe these data for each person from the first quarter of 2005 (Q1 2005) through the final quarter of 2011 (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 not in 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 waitlist members with codes indicating that they either do not meet program requirements or they are eligible for other Medicaid programs. We do not observe Core Plan applicants who applied before the program cutoff and were found ineligible by the state.

Table 1 reports demographic characteristics. Individuals who enrolled in the Core Plan are aged 43 on average and 49.6 percent female, while the average age of those on the waitlist is lower—38 years—and 43.7 percent are female. If we examine only those who applied within about a month of the October 9 cutoff 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 UI data. 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.

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 asking 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 (whether at an incorporated or unincorporated business) as self-employed.

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, Ichimura, and Todd 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 and summary of 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 in 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, the date was announced precipitously (on October 5) and would have been unexpected by all potential applicants. However, the data show 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 including applications between October 5 and 9 as well.[3]

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 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:

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

with triangular kernel weights, where all observations outside the bandwidth h (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 θ 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

[3] This is similar to the "donut-RD" estimate studied in Barreca, Lindo, and Waddell (2011) as a solution to heaping bias.

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 that 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 observable characteristics, and it takes 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]; Deheija and Wahba [1999]; Deheija and Wahba [2002]; Heckman et al. [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: matched treatments and controls are drawn from the same geographical labor market and their respective labor market outcomes are measured in the same way (Heckman, Ichimura, and Todd 1997; Heckmanet al. 1996).[4]

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

$$(2) \qquad Y_{it} = \beta_0 + \beta_1 Post_{it} + \beta_2 Core_{it} + \beta_3 Post \times Core_{it} + \omega Z_i + \varepsilon_{it}$$

where Y_{it} is an indicator for positive employment or total earnings 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 the individual enrolled into the Core Plan, and Z_i is a set of indicator variables for sex, age in months, and county of residence.

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, 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 within 30 days of the cutoff date.

[4] 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 et al. 2010).

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 analyses 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 30 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. Most specifications show a statistically significant and relatively large drop in employment among Core Plan enrollees relative to waitlisted applicants, from 6.9 percentage points in the specification including all applications to 11.8 percentage points in the specification excluding one week around the cutoff and 5.9 percentage points in the specification excluding just the surge of applications between the announcement and the cutoff date. The results in Table 2 are all reported at a bandwidth of 20 days. While the 5.9 percentage point result is not statistically significant at the bandwidth in Table 2, it remains stable and becomes statistically significant at slightly higher bandwidths. Table 2 also includes specification checks adding 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 percent 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.

In addition to quarterly measures of employment, Table 2 also includes specifications that aggregate the results to an indicator for ever being employed in 2010. Results and conclusions are very similar.

We include standard validity checks in Appendix A. These include a density test (Figure A.1) placebo tests (Table A.3 and A.4), and covariate tests (Table A.1). All placebo and covariate tests are consistent with the regression discontinuity assumption with one exception: a small but statistically significant drop in age of applicants at the time of the cutoff of slightly over three years. However, including age as a covariate makes no difference in the results.

Figure A.1 makes clear the increase in applications during the last week. In addition, Figure A.2 shows applications were allowed on weekends during the post period and not during the pre period, resulting in a Monday bump. Therefore, we also estimate models defined by application week (Saturday–Friday) rather than day. We find no difference in the size or significance of results using these specifications. We also estimated all specifications controlling for the day of week of the application and found no differences in the results. These results are available upon request.

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 set of plots, we include all observations, with the plot on the left unweighted and on the right propensity-score reweighted. In the second set of plots, we include only those observations who applied in either September or October 2009.

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, bottoming out in about Q1 2010, suggesting that employment losses (and perhaps loss of employer-sponsored insurance coverage) led 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 than did waitlisted applicants, suggesting an adjustment based on observables and fixed unobservables needs to be conducted (as in Equation [3]). Third, waitlisted applicants had higher employment rates in the quarters following the cutoff 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 5.9 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—5.0 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 (between 2.4 and 3.3 percentage points) that also 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. Table 2 also includes an annual measure, total annual earnings in 2010. The results for annual earnings are very similar, suggesting an annual difference of $950–$1,460, depending on the specification.

Table 4 reports the results from our difference-in-differences models. The results from those who applied within 30 days indicate a relative decline in quarterly earnings of $200–$210 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 include the full sample, the estimated relative difference in earnings is essentially zero (a statistically insignificant $16–$20).

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 around $70, while in the restricted sample they suggest a negative earnings effect of $120. These effects are statistically significant in both samples.

Subgroups

Table 5 reports the results of the regression discontinuity estimation for each of the date specifications and four splits of the sample: by sex, by age, by employment status prior to application, and by local unemployment rate. The table includes only the specification that excludes October 5–October 14 applications, and all results are reported at a bandwidth of 20 days.

Results are not particularly different for men and women, although effects are slightly larger for women. For age, however, there are clear and important differences in the employment effects. The effect is approximately twice as large as average for those between 35 and 55 years old, and more than three times as large as average for those over the age of 55. For those under 35, effects are weakly negative (meaning that employment among Core Plan enrollees increased relative to those on the waitlist). This is consistent with an early retirement story for older workers who may have found it more difficult to obtain a new job.

Point estimates for those who were not employed in the second quarter of 2009 are 2.4 percentage points larger than for those who were employed in the second quarter of 2009. The effect also is substantially larger for people living in counties with low rather than high unemployment rates (split at 10 percent, the 75th percentile unemployment rate weighted by individual). In counties with unemployment rates of 10 percent or less, we find a reduction of employment of 6.8 percentage points of Core enrollees relative to waitlisted applicants. By contrast, the estimate in counties with unemployment rates greater than 10 percent is very small (0.7 percentage points) and statistically insignificant.

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 earlier, 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 ACS and compare those with incomes up to 200 percent of the federal poverty level to those with incomes from 200–400 percent of the federal poverty level in Wisconsin and nationally, before and after the Wisconsin program implementation. While we find that the share of low-income self-employed Wisconsin residents eligible for public insurance was higher than in the national sample, we find 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.

CONCLUSION

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 2.4 to 5.9 percentage points in the difference-in-differences 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 meaningful 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 find negative employment effects using two different and complementary methods relying on different identifying assumptions and across a variety of specifications, our identification strategies are imperfect. For example, even when we adjust 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), it does not preclude the existence of time-varying unobserved differences between the two samples. Moreover, we do find differences at the cutoff 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, the concern remains.

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 in an 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 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, exacerbating any employment disincentive relative to Medicaid. On the other hand, the waitlist itself may have provided a disincentive and waitlisted applicants had access to the Basic Plan. Although only a small percentage of them took up Basic, its existence would provide a work disincentive as well, and minimize the estimated employment disincentive of public insurance.

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 mislabeling. We find no evidence of important bias from our inability to identify self-employed members of our sample.

Our estimates of the labor supply disincentive of public insurance are slightly larger than those found by Baicker et al. (2013) in Oregon and substantially smaller than those found by Garthwaite et al. (2013) in Tennessee. One possible explanation for the variation in estimates is an interaction between the programs and the business cycle. Our findings suggest that almost all of the labor supply response came from individuals living in counties with relatively low unemployment rates and that the labor supply response was greater among individuals who had been working in Q2 of 2009 (a little more than a year prior to their application to the Core plan). These findings suggest that part of the reason for the larger estimates in Garthwaite et al. (2013) and for the smaller estimates in Baicker et al. (2013) may be differing levels of economic activity across years and states. For example, the statewide unemployment rate was 5.6 percent in Tennessee in 2005, was 11.1 percent in Oregon in 2009, and was 8.5 percent in Wisconsin in 2010.

In light of these results, policymakers should be prepared for a reduction in labor supply among childless adults affected by the Medicaid expansion under the Affordable Care Act. These labor supply effects may be sufficiently large to be noticeable economy wide. For example, if 21.3 million additional adults gain Medicaid coverage following the ACA expansions, then approximately between 511,000 and 2.2 million fewer individuals will be employed as a result of the labor supply response (corresponding to our labor supply estimates ranging from 2.4 and 10.5 percentage points). These aggregate numbers would be equivalent to roughly a 0.2 to 0.9 percentage point drop in the labor force participation rate in 2014.

REFERENCES

Baicker, Katherine, and Amitabh Chandra. 2006. "The Labor Market Effects of Rising Health Insurance Premiums." *Journal of Labor Economics* 24(3): 609–634.

Baicker, Katherine, Amy Finkelstein, Jae Song, and Sarah Taubman. 2013. "The Impact of Medicaid on Labor Force Activity and Program Participation: Evidence from the Oregon Health Insurance Experiment." NBER Working Paper No. 19547. Cambridge, MA: National Bureau of Economic Research.

Barreca, Alan, Jason Lindo, and Glen Waddell. 2011. "Heaping-Induced Bias in Regression Discontinuity Designs." NBER Working Paper No. 17408. Cambridge, MA: National Bureau of Economic Research.

Biewen Martin, Bernd Fitzenberger, Aderonke Osikominu, Marie Paul. 2010 . "The Comparative Effectiveness of Public Sponsored Training Revisited: The Merits of Using Rich Administrative Data." Unpublished working paper. http://www.empedu.uni-freiburg.de/udocs/aopub/fbwmeval (accessed December 13, 2011).

Bound, John. 1989. "The Health and Earnings of Rejected Disability Insurance Applicants." *American Economic Review* 79(3): 482–503.

Boyle, Melissa A., and Joanna Lahey. 2010. "Health Insurance and the Labor Supply Decisions of Older Workers: Evidence from a U.S. Department of Veterans Affairs Expansion." *Journal of Public Economics* 94(7-8): 467–478.

Brueck, Dana. 2009. "Enrollment in BadgerCare Plus Core Plan to be Suspended." *NBC15.com*, WMTV Madison, Wisconsin, October 5, 2009, Web. May 28, 2014.

Card, David, and Daniel Sullivan. 1988. “Measuring the Effect of Subsidized Training Programs on Movements into and out of Employment.” *Econometrica* 56: 497–530.

Chen, Susan, and Wilbert van der Klaauw. 2008. “The Effect of Disability Insurance on Labor Supply of Older Individuals in the 1990s.” *Journal of Econometrics* 142(2): 757–784.

Congressional Budget Office. 2012a. *Updated Estimates for the Insurance Coverage Provisions of the Affordable Care Act.* Washington, DC: Congressional Budget Office. http://www.cbo.gov/sites/default/files/cbofiles/attachments/03-13-Coverage%20Estimates.pdf (accessed October 22, 2012).

———. 2012b. *Estimates for the Insurance Coverage Provisions of the Affordable Care Act Updated for the Recent Supreme Court Decision*. Washington, DC: Congressional Budget Office. http://www.cbo.gov/sites/default/files/cbofiles/attachments/43472-07-24-2012-CoverageEstimates.pdf (accessed October 22, 2012).

Cullis, John G, Philip R. Jones, and Carol Propper. 2000. “Waiting Lists and Medical Treatment: Analysis and Policies.” In *Handbook of Health Economics* 1B, A. J. Culyer and J. P. Newhouse, eds. Elsevier: North-Holland.

Deheija Rajeev H., and Sadek Wahba. 1999. “Causal Effects in Nonexperimental Studies: Reevaluating the Evaluation of Training Programs.” *Journal of the American Statistical Association* 94(448): 1053–1062.

———. 2002. “Propensity Score Matching Methods for Nonexperimental Causal Studies.” *Review of Economic Studies* 84(1): 151–161.

Dave, Dhaval M., Sandra L. Decker, Robert Kaestner, and Kosali Ilayperuma Simon, 2013. “The Effect of Medicaid Expansions in the Late 1980s and Early 1990s on the Labor Supply of

Pregnant Women." NBER Working Paper No. 19161. Cambridge, MA: National Bureau of Economic Research.

Ellwood, David T., and Kathleen Adams. 1990. "Medicaid Mysteries: Transitional Benefits, Medicaid Coverage, and Welfare Exits." *Health Care Financing Review* 11: 119–131.

Eissa, Nada, and Hilary Williamson Hoynes. 2004. "Taxes and the Labor Market Participation of Married Couples: The Earned Income Tax Credit." *Journal of Public Economics* 88(9–10): 1931–1958.

Fairlie, Robert, Kanika Kapur, and Susan Gates. 2011. "Is Employer-Based Health Insurance a Barrier to Entrepreneurship?" *Journal of Health Economics* 30(1): 146–162

French, Eric, and John Bailey Jones. 2011. "The Effects of Health Insurance and Self-Insurance on Retirement Behavior." *Econometrica* 79(3): 693–732.

French, Eric, and Jae Song. 2012. "The Effect of Disability Insurance Receipt on Labor Supply: A Dynamic Analysis." Working Paper No. 2012-12. Chicago: Federal Reserve Bank of Chicago.

Garthwaite, Craig, Tal Gross, and Matthew J. Notowidigdo. 2013. "Public Health Insurance, Labor Supply, and Employment Lock." NBER Working Paper No. 19220. Cambridge, MA: National Bureau of Economic Research. http://www.nber.org/papers/w19220 (accessed May 6, 2014).

Globerman, Steven. 1991. "A Policy Analysis of Hospital Waiting Lists." *Journal of Policy Analysis and Management* 10(2): 247–262.

Gruber, Jonathan, and Jeffrey D. Kubik. 1997. "Disability Insurance Rejection Rates and the Labor Supply of Older Workers." *Journal of Public Economics* 64(1): 1–23.

Ham, John C., and Lara D. Shore-Sheppard. 2001. “The Impact of Public Health Insurance on Labor Market Transitions .” Working paper. Williamstown, MA: Williams College.

———. 2005. “Did Expanding Medicaid Affect Welfare Participation?” *Industrial and Labor Relations Review* 58(3): 452–470.

Hamersma Sarah. 2010. “The Effects of Medicaid Earnings Limits on Earnings Growth among Poor Workers.” *B. E. Journal of Economic Analysis and Policy* 13(2): 887–919.

Hamersma Sarah, and Matthew Kim. “The Effect of Parental Medicaid Expansions on Job Mobility.” *Journal of Health Economics* 28(4): 761–770.

Heckman James J., Hidehiko Ichimura, Jeffery Smith, and Petra Todd. 1996. “Sources of Selection Bias in Evaluating Programs: An Interpretation of Conventional Measures and Evidence on the Effectiveness of Matching as a Program Evaluation Method.” *Proceedings of the National Academy of Sciences* 93: 13416–13420.

Heckman James J., Hidehiko Ichimura, and Petra Todd. 1997. “Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme.” *Review of Economic Studies* 64(4): 605–654.

Heckman James J., and Jeffery Smith. 1999. “The Pre-Program Earnings Dip and the Determinants of Participation in a Social Program: Implications for Simple Program Evaluation Strategies.” *Economic Journal* 109(457): 315–348.

Holtz-Eakin, Douglas, John R. Penrod, and Harvey Rosen. 1996. “Health Insurance and the Supply of Entrepreneurs.” *Journal of Public Economics* 62(1–2): 209–235.

Hoynes, Hilary Williamson, and Diane Whitmore Schanzenbach. 2012. “Work Incentives and the Food Stamp Program.” *Journal of Public Economics* 96(1–2): 151–162

Jacob, Brian A., and Jens Ludwig. 2012. "The Effects of Housing Assistance on Labor Supply: Evidence from a Voucher Lottery." *American Economic Review* 102(1): 272–304.

Kaiser Family Foundation. 2013. "Medicaid Expansion under the Affordable Care Act." *Journal of American Medical Association* 309(12): 1219.

Lee, David S., and Thomas Lemieux. 2010. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature* 48(2): 281–355.

Leuven, Edwin, and Barbara Sianesi. 2003. "PSMATCH2: Stata Module to Perform Full Mahalanobis and Propensity Score Matching, Common Support Graphing, and Covariate Imbalance Testing." http://ideas.repec.org/c/boc/bocode/s432001.html (accessed May 6, 2014).

Lombard, K. 2001. "Female Self-Employment and Demand for Flexible, Nonstandard Work Schedules." *Economic Inquiry* 29(2): 214–317.

Johannesson, Magnus, Per-Olov Johansson, and Tore Soderqvist. 1998. "Time Spent on Waiting List for Medical Care: An Insurance Approach." *Journal of Health Economics* 20(1): 68–86.

Maestas, Nicole, Kathleen J. Mullen, and Alexander Strand. 2013. "Does Disability Insurance Receipt Discourage Work? Using Examiner Assignment to Estimate Causal Effects of SSDI Receipt." *American Economic Review* 103(5): 1797–1829.

Meyer, Bruce D. 2002. "Labor Supply at the Extensive and Intensive Margins: The EITC, Welfare, and Hours Worked." *American Economic Review* 92(2): 373–379.

Moffitt, Rober A. 2002. "Chapter 34 Welfare Programs and Labor Supply." In *Handbook of Public Economics*. Vol. 4. Alan J. Auerbach and Martin Feldstein, eds., pp. 2393–2430.

Moffitt Robert A., and Barbara Wolfe. 1992. “The Effect of the Medicaid Program on Welfare Participation and Labor Supply.” *Review of Economics and Statistics* 74(4): 615–626.

Montgomery, Edward, and John Navin. 2000. “Cross State Variation in Medicaid Programs and Female Labor Supply.” *Economic Inquiry* 38(3): 402–418.

Parsons, Donald. 1990. “The Health and Earnings of Rejected Disability Insurance Applicants: Comment.” *American Economic Review* 81(5): 1419–1426.

Propper, Carol. 1990. “Contingent Valuation of Time Spent on NHS Waiting Lists.” *Economic Journal* 100(400): 193–200.

———. 1995. “The Disutility of Time Spent on United Kingdom’s National Health Service Waiting Lists.” *Journal of Human Resources* 30(4): 677–700.

Smith, Jeffery and Petra Todd. 2005. “Does Matching Overcome LaLonde’s critique of Nonexperimental Estimators?” *Journal of Econometrics* 125(1-2): 305–353.

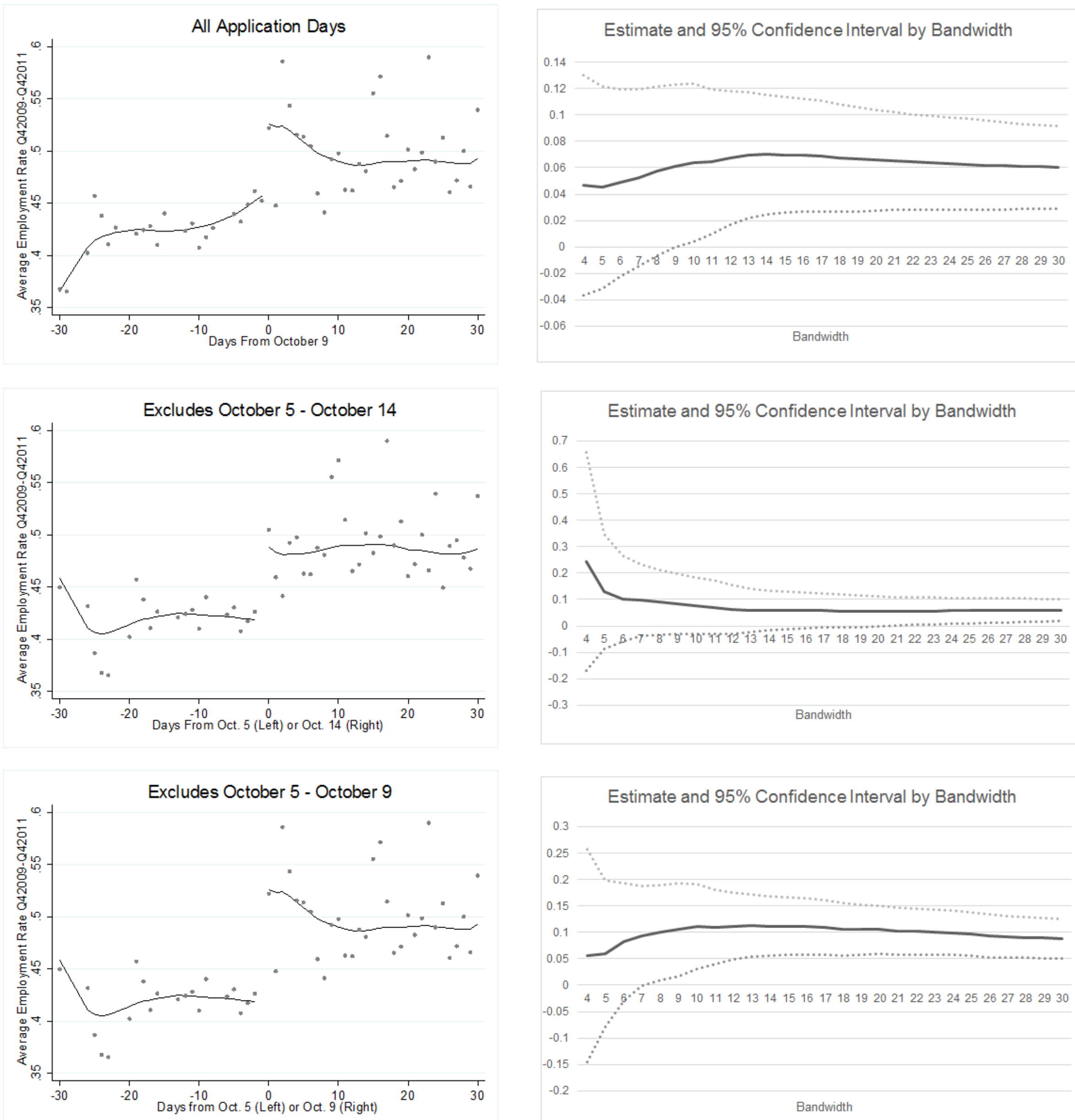
Stephens, Jessica, Samantha Artiga, Robin Rudowitz, Anne Jankiewicz, and David Rousseau. Medicaid Expansion under the Affordable Care Act. 2013. *Journal of American Medical Association* 309(12): 1219. doi:10.1001/jama.2013.2481 (accessed May 6, 2014).

Strumpf, Erin. 2011. “Medicaid’s Effect on Single Women’s Labor Supply: Evidence from the Introduction of Medicaid.” *Journal of Health Economics* 30(3): 531–548.

Yelowitz, Aaron S. 1995. “The Medicaid Notch, Labor Supply and Welfare Participation: Evidence from Eligibility Expansions.” *Quarterly Journal of Economics* 110(4): 909–940.

Zissimopoulos, Julie, and Lynn A. Karoly. 2007. “Transitions to Self-Employment at Older Ages: The Role of Wealth, Health, Health Insurance and Other Factors.” *Labour Economics* 14(2): 269–295.

Figure 1. Employment Rate by Day of Application



Notes: Figures on left show average quarterly employment over the period from the fourth quarter of 2009 to the fourth quarter of 2011 by application day. Each observation is the average for all applicants who applied on that day. The figures also show local linear regression functions estimated at a bandwidth of 15 days. The figures on the right show how the estimate and 95% confidence intervals vary with the estimated bandwidth.

Figure 2. Average Employment by Quarter

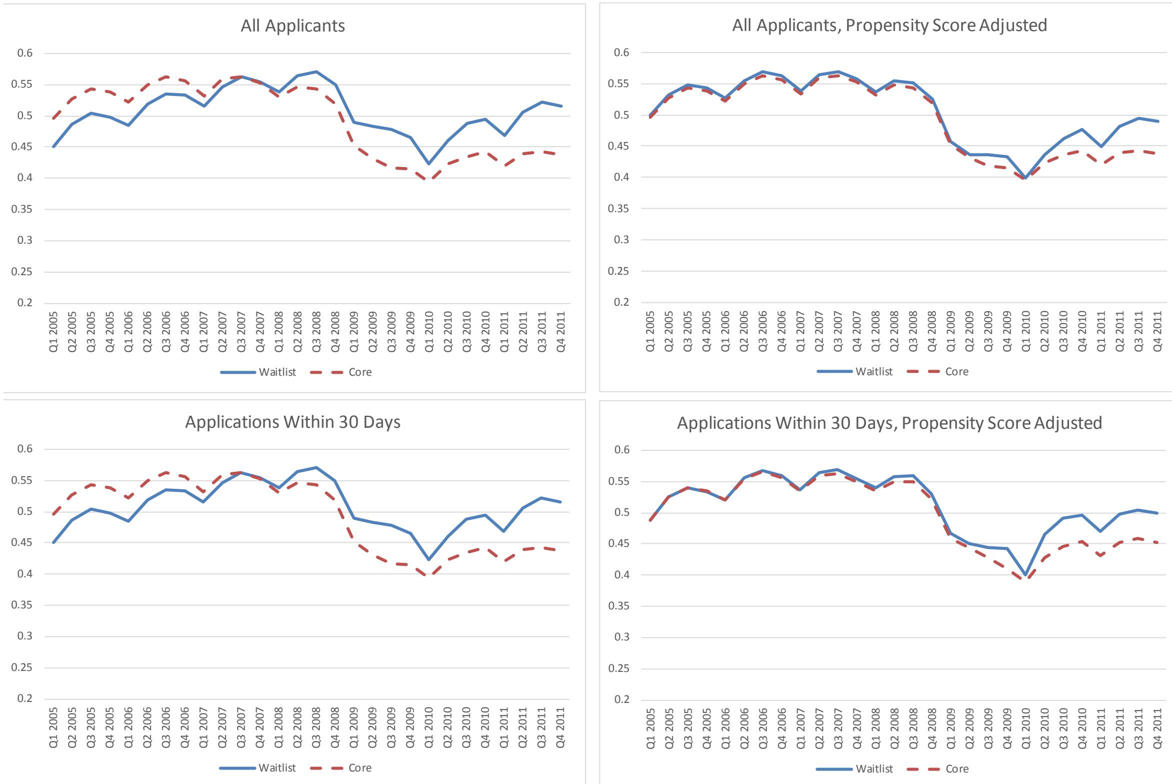
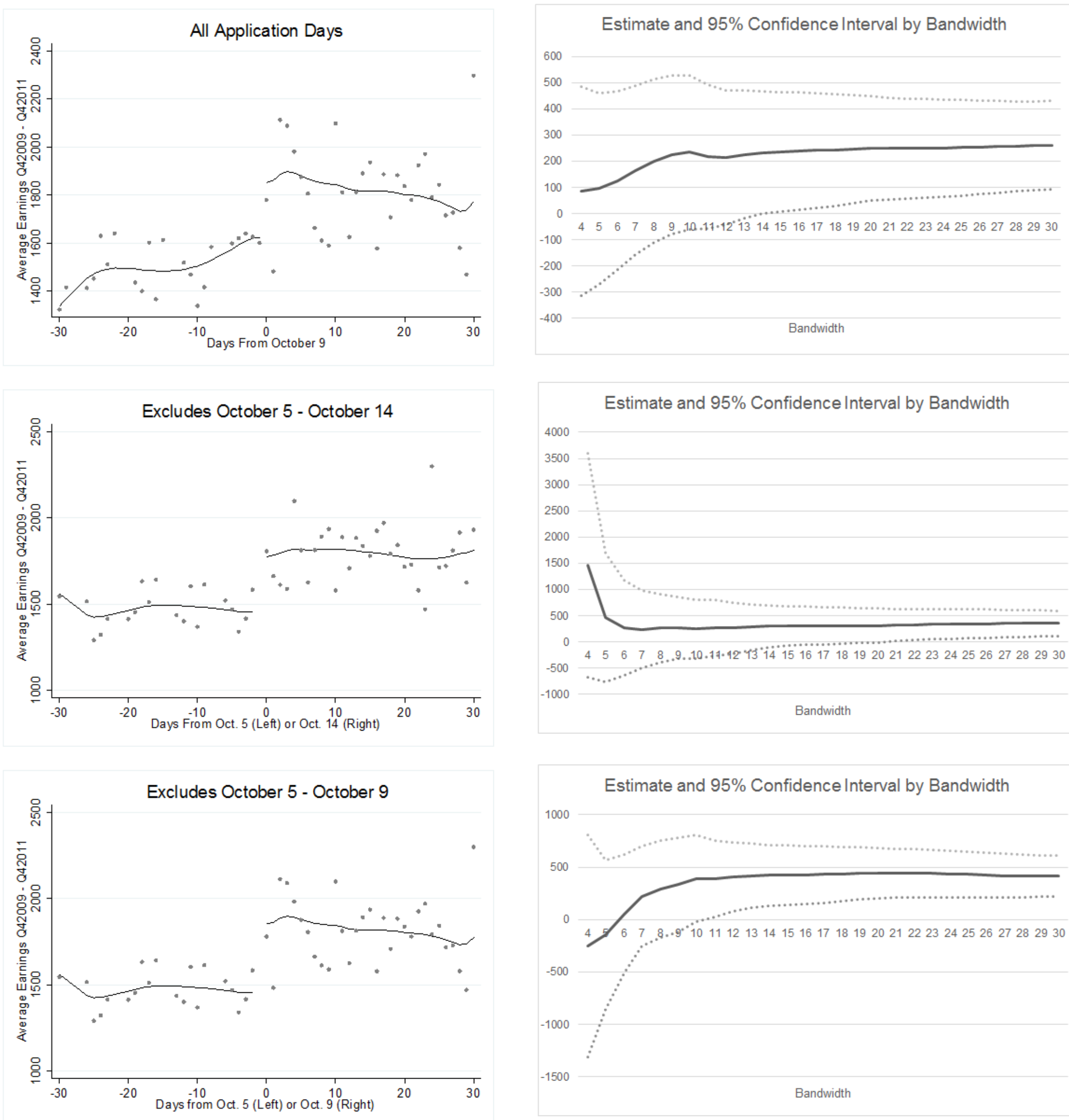


Figure 3. Earnings by Day of Application



Notes: Figures on left show average quarterly total earnings over the period from the fourth quarter of 2009 to the fourth quarter of 2011 by application day. Each observation is the average for all applicants who applied on that day. The figures also show local linear regression functions estimated at a bandwidth of 15 days. The figures on the right show how the estimate and 95% confidence intervals vary with the estimated bandwidth.

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

	Core Plan enrollees	Waitlisted applicants
Ever applied		
Age	43.63	38.87
Female	0.50	0.43
Percent employed, Q209	0.43	0.48
Average quarterly earnings, Q209	1,247.45	1,827.60
Average employment, Q409-Q411	0.43	0.48
Average earnings, Q409-Q411	1,509.49	1,723.90
Observations	42,401	60,507
Applied within 30 days of October 9, 2009		
Age	42.18	39.10
Female	0.47	0.45
Percent employed, Q209	0.45	0.48
Average quarterly earnings, Q209	1,449.45	1,624.53
Average employment, Q409-Q411	0.44	0.49
Average earnings, Q409-Q411	1,561.60	1,815.92
Observations	10,528	3,396

SOURCE: Authors' calculations from WI Administrative Data
NOTE: Age and sex are from application data. Average employment and earnings are from unemployment data.

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	0.0659***	0.037**	0.055*	0.0524**	0.105***	0.0763***
	0.0194	*0.0171*	*0.0292*	*0.0256*	*0.0235*	*0.0201*
Average earnings, Q42009-Q42011	248.4**	148	314.2*	319.3**	445.8***	367.3***
	101.8	*97.47*	*164*	*151.6*	*123*	*114.5*
Ever employed, 2010	0.0752***	0.0397*	0.0588	0.058*	0.0939***	0.06**
	0.0221	*0.0224*	*0.0358*	*0.0332*	*0.0291*	*0.0265*
Total annual earnings, 2010	977**	584	1170*	1214**	1604***	1338***
	397.9	*375.2*	*629.8*	*588.6*	*486*	*447.2*
Number of observations	11,278	11,278	6,064	6,064	6,084	6,084

NOTE: Table displays regression discontinuity estimates of effect of not getting the Core Plan, with robust standard error in italics. All results calculated at a bandwidth of 20 days. Bandwidth robustness is included in Figure 1. Bandwidths defined as distance from October 9 in “All Dates” or distance from excluded interval in other specifications. *significant at the 0.10 level; **significant at the 0.05 level; ***significant at the 0.01 level. Covariates include age, sex, day of week of application, and earnings and employment in the second quarter of 2009

Table 3 Summary of Difference-in-Differences Results, Employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All application dates								
Post*Core Plan	−0.059***	−0.059***	−0.059***	−0.059***	−0.024***	−0.024***	−0.024***	−0.024***
	0.0023	*0.0023*	*0.0023*	*0.0023*	*0.0025*	*0.0025*	*0.0025*	*0.0025*
Demographics		X	X			X	X	
County fixed effects			X				X	
Individual fixed effects				X				X
PS weighted					X	X	X	X
Number of observations	2,932,804				2,908,556			
Number of individuals	104,743				103,877			
Within 30 days of Oct 9								
Post*Core Plan	−0.050***	−0.050***	−0.051***	−0.050***	−0.033***	−0.032***	−0.033***	−0.033***
	0.0073	*0.0073*	*0.0073*	*0.0073*	*0.0076*	*0.0076*	*0.0076*	*0.0076*
Demographics		X	X			X	X	
County fixed effects			X				X	
Individual fixed effects				X				X
PS weighted					X	X	X	X
Number of observations	406,364				406,028			
Number of individuals	14,513				14,501			

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 sex, age, and county of residence. *significant at the 0.10 level; **significant at the 0.05 level; ***significant at the 0.01 level.

Table 4 Summary of Difference-in-Differences Results, Earnings

All Application Dates	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*Core Plan	19.80	16.43	17.47	19.80	71.62***	70.92***	71.62***	71.62***
	15.75	*15.85*	*15.83*	*15.75*	*15.54*	*15.56*	*15.54*	*15.54*
Demographics		X	X			X	X	
County fixed effects			X				X	
Individual fixed effects				X				X
PS weighted					X	X	X	X
Number of observations	2,932,804				2,908,556			
Number of individuals	104,743				103,877			
Within 30 days of Oct 9								
Post*Core Plan	−209.94***	−210.21***	−211.95***	−209.94***	−125.91**	−122.95**	−125.91**	−125.91**
	50.44	*50.84*	*50.72*	*50.44*	*50.85*	*51.07*	*50.86*	*−125.91*
Demographics		X	X			X	X	
County fixed effects			X				X	
Individual fixed effects				X				X
PS weighted					X	X	X	X
Number of observations	406,364				406,028			
Number of individuals	14,513				14,501			

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 sex, age, and county of residence. *significant at the 0.10 level; **significant at the 0.05 level; ***significant at the 0.01 level.

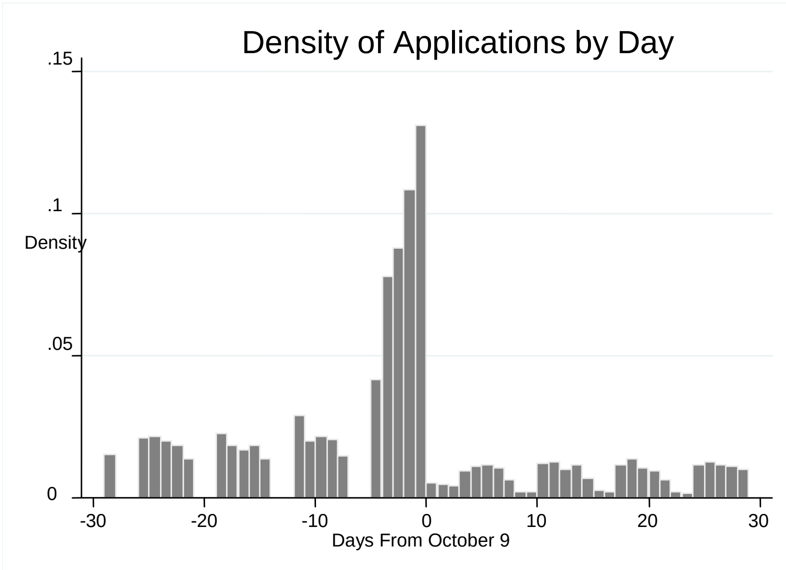
Table 5 Regression Discontinuity Results, by Subsample

	Outcome		
	Average employment, Q42009-Q42011	Average earnings, Q42009-Q42011	Observations
All (benchmark)	0.055*	314.2*	6,064
	0.0292	*164*	
Women	0.0689	419.7*	2,806
	0.0449	*254.8*	
Men	0.0519	248.8	3,258
	0.0374	*213.2*	
Age < 35	−0.0732*	−268.3	2,541
	0.042	*226.6*	
Age 35-55	0.119**	666.3**	2,367
	0.048	*265.6*	
Age > 55	0.173**	815.7*	1,156
	0.0733	*471.2*	
Employed in Q2 2009	0.0468	433.3	2,725
	0.0387	*269.5*	
Unemployed in Q2 2009	0.0717**	262.7	3,339
	0.0345	*172.6*	
High-unemployment county (>10%)	0.00675	11.37	1,445
	0.0623	*343.4*	
Low-unemployment county (<=10%)	0.0684**	399**	4,567
	0.0333	*189.4*	

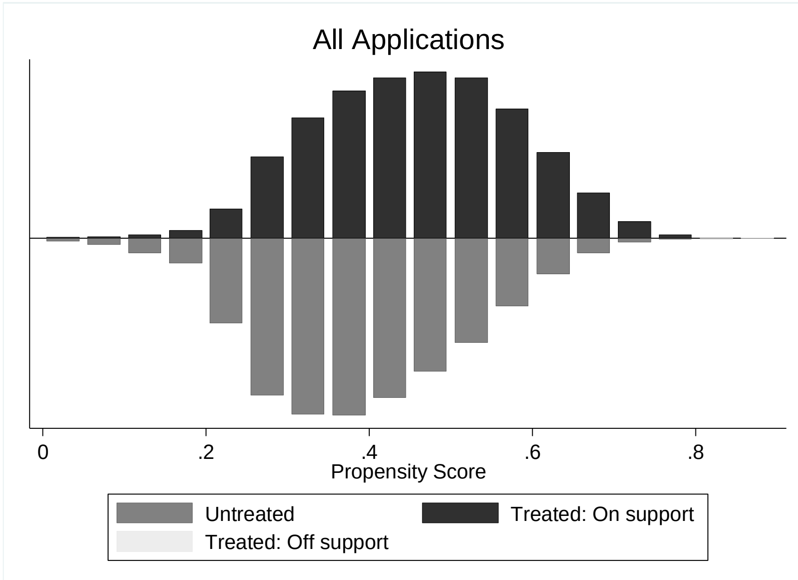
NOTE: Table displays regression discontinuity estimates of effect of not getting the Core Plan, with robust standard error in italics. All results calculated at bandwidth of 20 days, for specification excluding Oct. 5–Oct. 14. *significant at the 0.10 level; **significant at the 0.05 level; ***significant at the 0.01 level.

For Online Publication

Appendix Figure 1. Density of Applications by Day



Appendix Figure 2. Common Support Graphs for Propensity Score Analysis



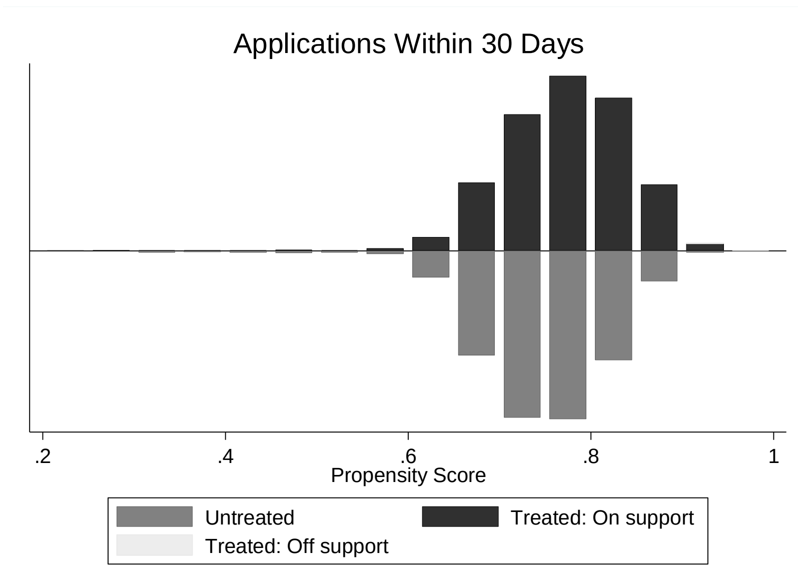


Table A.1 Covariate Tests

	Bandwidth (days)					
	5	10	15	20	25	30
All dates						
Age (Months)	−41.57***	−54.84***	−59.33***	−54.48***	−51.83***	−49.55***
	15.59	*11.98*	*8.777*	*7.65*	*6.942*	*6.309*
Female	−0.00883	−0.026	−0.0443	−0.0465*	−0.0492**	−0.0469**
	0.049	*0.0383*	*0.0282*	*0.0245*	*0.0221*	*0.02*
Employment Q2 2009	0.0606	0.0514	0.0376	0.0287	0.0229	0.0211
	0.0493	*0.0386*	*0.0284*	*0.0247*	*0.0223*	*0.0202*
Earnings Q2 2009	161.5	107	42.9	30.28	0.447	−2.446
	220.7	*175.7*	*135.8*	*120.5*	*111.1*	*101.7*
Excludes Oct. 5 – Oct. 14						
Age (Months)	−23.85	−34.32	−24.46*	−23.05**	−22.82**	−24.11***
	43.16	*21.54*	*14.22*	*11.64*	*9.78*	*8.55*
Female	−0.0574	−0.0365	−0.0355	−0.0378	−0.0286	−0.0209
	0.137	*0.0679*	*0.0448*	*0.0366*	*0.0306*	*0.0267*
Employment Q2 2009	−0.0611	−0.0448	−0.0278	−0.0154	−0.0021	0.00845
	0.136	*0.0678*	*0.0448*	*0.0366*	*0.0306*	*0.0268*
Earnings Q2 2009	561.5	185.3	82.67	49.87	75.51	106
	749.3	*361.5*	*241.4*	*201.1*	*168*	*145.9*
Excludes Oct. 5 – Oct. 9						
Age (Months)	−40.34	−55.01***	−49.48***	−48.18***	−46.33***	−43.06***
	28.3	*16.31*	*10.93*	*9.336*	*8.366*	*7.56*
Female	0.017	−0.0183	−0.0353	−0.0383	−0.044*	−0.043*
	0.0884	*0.0517*	*0.0348*	*0.0297*	*0.0266*	*0.0239*
Employment Q2 2009	0.0667	0.0591	0.0457	0.0412	0.039	0.0364
	0.0883	*0.0518*	*0.035*	*0.0298*	*0.0267*	*0.024*
Earnings Q2 2009	−173.1	17.32	74.76	43.3	19.82	26.31
	457.3	*266.5*	*176.1*	*148.2*	*134.2*	*122.8*

NOTE: Table displays regression discontinuity estimates of effect of not getting the Core Plan on pretreatment covariates, with robust standard error in italics. *significant at the 0.10 level; **significant at the 0.05 level; ***significant at the 0.01 level.

Table A.2 Placebo Tests for Alternate Cutoff Days

	Cutoff											
All dates	t−14	t−12	t−10	t−8	t−6	t−4	t+4	t+6	t+8	t+10	t+12	t+14
Average employment, Q42009−Q42011	−0.0189	−0.0248	−0.0267*	−0.017	−0.0144	−0.00974	0.0535***	−0.0157	−0.037*	−0.0382*	−0.0328	−0.0142
	0.0161	*0.0173*	*0.0151*	*0.0138*	*0.0136*	*0.0123*	*0.017*	*0.0199*	*0.0217*	*0.0227*	*0.0214*	*0.0208*
Average earnings, Q42009−Q42011	−72.88	−79.71	−102.9	17.28	−18.95	−49.18	298.9***	−0.414	−43.4	−27.95	−265.5*	−119
	93.09	*100.7*	*86.08*	*80.51*	*79.97*	*71.9*	*99.44*	*119*	*140.8*	*141*	*143.5*	*134.3*
Observations	12,146	11,968	11,493	11,621	11,635	11,187	10,501	10,649	10,566	10,118	9,832	10,129
Excludes Oct. 5 – Oct. 14												
Average employment, Q42009–Q42011	−0.00377	0.00617	−0.0106	−0.0306*	−0.0221	−0.00978	0.0254	−0.00629	−0.00358	−0.0148	−0.0344	0.0127
	0.0172	*0.0172*	*0.0165*	*0.0165*	*0.0175*	*0.016*	*0.0227*	*0.0218*	*0.0216*	*0.0216*	*0.0209*	*0.0214*
Average earnings, Q42009–Q42011	−157.4	−60.61	−117.5	−163.9*	−83.88	−41.94	272.7*	−152.1	−71.55	−159.9	−162.3	−86.99
	109.4	*101.8*	*96.5*	*96.17*	*101.3*	*90.84*	*140.9*	*147.7*	*139.5*	*136.2*	*128.9*	*130.4*
Observations	6,971	6,698	6,362	6,564	6,484	6,170	5,492	5,698	5,422	5,199	5,078	5,007
Excludes Oct. 5 – Oct. 9												
Average employment, Q42009–Q42011	−0.00606	0.00221	−0.0163	−0.0373**	−0.0235	−0.00827	0.0157	−0.0467**	−0.0524**	−0.047**	−0.0293	−0.00349
	0.0172	*0.0172*	*0.0166*	*0.0165*	*0.0175*	*0.0161*	*0.0216*	*0.0218*	*0.0227*	*0.0232*	*0.0216*	*0.0213*
Average earnings, Q42009–Q42011	−158.4	−66.05	−125.5	−188.5**	−96.56	−57.83	95.18	−173.7	−147.5	−111	−271.3*	−74.27
	109.1	*101.5*	*96.45*	*95.68*	*101.5*	*91.24*	*128.7*	*130.1*	*144.4*	*142.5*	*145.5*	*137.5*
Observations	6,972	6,631	6,226	6,570	6,460	6,054	5,340	5,683	5,431	5,078	5,089	4,982

NOTE: Table displays regression discontinuity estimates of effect of not getting the Core Plan on pre−treatment covariates, with robust standard error in italics. *significant at the 0.10 level; **significant at the 0.05 level; ***significant at the 0.01 level. All estimates at bandwidth of 20 days.

Table A.3 Propensity Score Estimation Results

Variable	All applicants Estimate	All applicants Standard error	Applied within 30 Days of Oct 9 Estimate	Applied within 30 Days of Oct 9 Standard error
Employed Q12005	0.045	0.015	−0.054	0.042
Employed Q22005	0.013	0.017	0.080	0.048
Employed Q32005	0.046	0.016	−0.084	0.047
Employed Q42005	0.031	0.016	0.023	0.047
Employed Q12006	0.032	0.016	0.089	0.047
Employed Q22006	0.019	0.016	−0.014	0.048
Employed Q32006	0.071	0.016	0.086	0.047
Employed Q42006	0.025	0.016	−0.011	0.046
Employed Q12007	0.000	0.016	0.045	0.046
Employed Q22007	0.046	0.016	0.024	0.046
Employed Q32007	0.013	0.016	0.046	0.046
Employed Q42007	0.000	0.016	−0.053	0.045
Employed Q12008	0.024	0.016	−0.030	0.045
Employed Q22008	0.012	0.016	−0.018	0.045
Employed Q32008	0.031	0.015	−0.001	0.045
Employed Q42008	0.010	0.015	0.044	0.044
Employed Q12009	0.066	0.015	−0.010	0.044
Employed Q22009	0.024	0.014	−0.042	0.039
Earnings Q12005	0.005	0.003	0.011	0.009
Earnings Q22005	−0.006	0.003	0.000	0.010
Earnings Q32005	0.000	0.003	0.000	0.009
Earnings Q42005	−0.007	0.003	−0.010	0.008
Earnings Q12006	0.004	0.003	0.008	0.010
Earnings Q22006	−0.004	0.003	0.001	0.010
Earnings Q32006	−0.004	0.003	−0.010	0.009
Earnings Q42006	−0.005	0.003	−0.001	0.008
Earnings Q12007	0.001	0.003	−0.004	0.008
Earnings Q22007	−0.005	0.003	−0.013	0.008
Earnings Q32007	−0.001	0.003	0.007	0.008
Earnings Q42007	−0.003	0.003	0.000	0.008
Earnings Q12008	−0.004	0.003	−0.002	0.008
Earnings Q22008	0.002	0.003	0.010	0.008
Earnings Q32008	−0.013	0.003	−0.014	0.008
Earnings Q42008	−0.008	0.003	−0.006	0.007
Earnings Q12009	−0.015	0.003	−0.006	0.009
Earnings Q22009	−0.059	0.003	0.000	0.007
Age	0.001	0.000	0.001	0.000
Female	0.090	0.008	0.025	0.024
Constant	−1.159	0.052	−0.121	0.142

NOTE: Regression also includes dummy variables for county of residence. Earnings in thousands of dollars.

Table A.4 Propensity Score Balancing Test

Variable	All applicants				Applied within 30 Days of Oct 9			
	Waitlist	Core	%bias	*t*	Waitlist	Core	%bias	*t*
Employed Q12005	0.50	0.50	−0.60	−0.88	0.49	0.49	0.00	0.02
Employed Q22005	0.53	0.53	−0.90	−1.28	0.53	0.53	0.10	0.06
Employed Q32005	0.54	0.55	−0.90	−1.32	0.54	0.54	−0.20	−0.18
Employed Q42005	0.54	0.54	−0.90	−1.31	0.54	0.53	0.30	0.21
Employed Q12006	0.52	0.53	−1.00	−1.44	0.52	0.52	0.10	0.08
Employed Q22006	0.55	0.56	−1.10	−1.62	0.55	0.56	−0.30	−0.22
Employed Q32006	0.56	0.57	−1.20	−1.83	0.57	0.57	−0.50	−0.35
Employed Q42006	0.56	0.56	−1.20	−1.82	0.56	0.56	−0.50	−0.40
Employed Q12007	0.53	0.54	−1.00	−1.42	0.53	0.54	−0.50	−0.38
Employed Q22007	0.56	0.56	−1.10	−1.57	0.56	0.56	−0.90	−0.67
Employed Q32007	0.56	0.57	−1.20	−1.73	0.56	0.57	−1.00	−0.77
Employed Q42007	0.55	0.56	−1.00	−1.40	0.55	0.56	−1.10	−0.80
Employed Q12008	0.53	0.54	−1.10	−1.67	0.53	0.54	−1.20	−0.93
Employed Q22008	0.55	0.55	−1.40	−2.01	0.55	0.56	−1.50	−1.13
Employed Q32008	0.54	0.55	−1.40	−2.05	0.55	0.56	−1.70	−1.29
Employed Q42008	0.52	0.52	−1.20	−1.77	0.52	0.53	−1.60	−1.18
Employed Q12009	0.45	0.46	−1.00	−1.54	0.46	0.47	−1.50	−1.10
Employed Q22009	0.43	0.44	−1.10	−1.62	0.44	0.45	−1.30	−0.95
Earnings Q12005	1.92	1.97	−1.60	−2.43	1.97	1.96	0.30	0.24
Earnings Q22005	2.13	2.18	−1.60	−2.44	2.18	2.18	0.00	0.03
Earnings Q32005	2.29	2.35	−1.60	−2.46	2.36	2.36	0.00	0.00
Earnings Q42005	2.22	2.28	−1.70	−2.57	2.30	2.28	0.40	0.30
Earnings Q12006	2.07	2.14	−1.90	−2.98	2.16	2.16	0.00	0.01
Earnings Q22006	2.21	2.27	−2.00	−3.11	2.32	2.33	−0.20	−0.15
Earnings Q32006	2.27	2.34	−2.10	−3.26	2.38	2.40	−0.60	−0.46
Earnings Q42006	2.26	2.33	−2.00	−3.16	2.37	2.39	−0.50	−0.41
Earnings Q12007	2.08	2.15	−2.10	−3.32	2.18	2.21	−0.80	−0.61
Earnings Q22007	2.20	2.28	−2.20	−3.47	2.32	2.36	−1.20	−0.89
Earnings Q32007	2.23	2.31	−2.20	−3.51	2.37	2.41	−1.10	−0.84
Earnings Q42007	2.21	2.29	−2.40	−3.72	2.33	2.37	−1.30	−0.98
Earnings Q12008	1.99	2.07	−2.50	−4.07	2.12	2.17	−1.30	−0.99
Earnings Q22008	2.06	2.15	−2.70	−4.28	2.23	2.27	−1.10	−0.87
Earnings Q32008	2.02	2.11	−2.60	−4.32	2.19	2.25	−1.90	−1.44
Earnings Q42008	1.85	1.94	−2.80	−4.57	1.99	2.05	−1.80	−1.45
Earnings Q12009	1.37	1.44	−2.50	−4.23	1.52	1.55	−1.30	−1.04
Earnings Q22009	1.24	1.32	−2.80	−4.95	1.44	1.48	−1.50	−1.12
Age	521.72	521.31	0.30	0.38	505.17	499.11	3.80	2.80
Female	0.49	0.49	−0.20	−0.25	0.47	0.46	0.80	0.61

NOTE: Regressions also include 70 county of residence indicators; none has a statistically significant difference between waitlist and core or standardized bias greater than 10. Earnings in thousands of dollars.