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The Affordable Care Act and the Market for Higher Education

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Staff Report No. 873
October 2018

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The Affordable Care Act and the Market for Higher Education

Rajashri Chakrabarti and Maxim Pinkovskiy

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Abstract

Through changing the connection between insurance and employment, the Affordable Care Act (ACA) has affected people's incentives to obtain education. We employ a triple-difference strategy comparing counties with different levels of uninsurance pre-ACA and in states with different Medicaid expansion decisions across time to investigate changes in enrollment in different types of higher education institutions. We find that enrollment in less-than-two-year for-profit colleges increased more between high- and low-uninsurance counties in states that expanded Medicaid relative to states that didn't, with nearly all the increase taking place after the 2012 Supreme Court decision that gave states the right to choose not to expand Medicaid. Differential enrollment is flat for all other comparable college types. We find this differential increase in less-than-two-year for-profit college enrollment to be remarkably general across demographic characteristics, and robust to controlling for confounders such as the differential impact of the Great Recession.

Key words: affordable care act, postsecondary education, for-profit schools, health insurance, Medicaid

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To view the authors' disclosure statements, visit https://www.newyorkfed.org/research/staff_reports/sr873.html.

1 Introduction

Getting health insurance in America is intimately connected to choosing whether and where to work. Therefore, it should not be surprising that the U.S. health insurance market may influence, and be influenced by the market for higher education. In this paper, we investigate the effects of the largest overhaul of health insurance in the U.S. in recent decades – the Patient Protection and Affordable Care Act of 2010 (ACA) – on college enrollment choices.

The Affordable Care Act revolutionized the health insurance landscape faced by individuals under 65 and made the ability to purchase meaningful health insurance much less contingent on holding a job that offered one. Previously, the only well-functioning insurance markets were those for employers providing insurance to their workers, since the workforce of a large firm offered a homogeneous pool with respect to health. Small group or nongroup health insurance markets were plagued by adverse selection, and were effectively unable to insure individuals against long-term shocks to their health expenditures. The ACA attempted to solve the adverse selection problem in the nongroup market by 1) forbidding insurers to price on health status, 2) mandating that everyone buy health insurance or pay a penalty, thus limiting the adverse selection against the nongroup market as a whole, and 3) subsidizing health insurance for those too poor to purchase it both explicitly and through an expansion of the Medicaid program to all adults earning under 138% of poverty, regardless of family status. Consequently, individuals not working in a large firm could now purchase insurance at prices closer to actuarial fairness on the newly regulated individual market, or could become eligible for Medicaid.

By extending meaningful health insurance to self-employed and small-employer settings, the Affordable Care Act increased the marginal benefits of working in these settings. Therefore, the ACA should have increased the demands for investment in the skills required for these jobs, which is typically done through higher education. In theory, there are multiple, often countervailing effects that the passage of the ACA could have on the demand for higher education. First, if it leads individuals to shift from jobs in large firms that offer health insurance to jobs in smaller firms that don't, the ACA could incentivize people to switch towards the types of higher education that provide the relevant skills for jobs in smaller firms. In particular, the passage and implementation of the ACA largely followed the rise of for-profit colleges – many offering very quick (less-than-2 year) degrees geared towards specific jobs, such as hairdressing, massage therapy and construction, many of which are performed in small establishments or by the self-employed. Second, by weakening the link between employment and insurance, the ACA could encourage people to exit the labor force – for example, by becoming homemakers – which would decrease their demand for higher education. Similarly, as colleges typically offered insurance to their students before the enactment of the ACA (since they, like large businesses, were homogeneous pools with respect to health), the ACA created incentives for individuals to leave college to enter the labor force even in a position that would not have offered insurance before the ACA. The ACA could have also discouraged work more generally through high marginal tax rates (Mulligan and Gallen 2013a and b), and thus discouraged investment in higher

education. On the other hand, the ACA may have increased the demand for education through a wealth effect of individuals receiving subsidized insurance. Finally, since the ACA increased the Medicaid eligibility threshold substantially (to a value that is in excess of the earnings of many jobs that typically do not offer insurance), it may have increased the incentive for poor people to take these jobs without losing their Medicaid coverage, and to get training for them.

We exploit the 2012 Supreme Court decision that left the choice of Medicaid expansion up to the states in order to obtain variation across states in the intensity of the implementation of the Affordable Care Act. This variation has traditionally been used to assess the effects of the ACA on various outcomes. Pinkovskiy (2015) finds positive effects of the Affordable Care Act and of the Medicaid expansion on employment, while Kaestner et al. (2015) do not find any effects of Medicaid expansion on employment. Duggan et al. (2017) finds that labor force participation increased in areas with higher potential Medicaid enrollment but these increases were offset by reductions from other parts of the ACA. Pinkovskiy (2015) looks at industry-specific effects and finds that the positive effects of the Affordable Care Act on employment were concentrated in "low-insurance industries," such as leisure and hospitality. As in these previous papers, we confront the concern that Medicaid expansion decisions by the states are endogenous. Therefore, we investigate enrollment in different types of higher education over time by comparing counties with low and high pre-ACA uninsurance rates in states that did and did not expand Medicaid. While low-uninsurance counties and high-uninsurance counties are, of course, different ex ante, and the Medicaid expansion decision is likely endogenous, it is unlikely that Medicaid expansion decisions are endogenous to economic outcomes in any given type of county that vary across states. In our county-level setup, we can formally investigate this hypothesis by including state by year fixed effects in our regression, which should flexibly control for any determinants of the state decision of whether to expand Medicaid or not.

We find that areas that were substantially affected by the Medicaid expansion (high uninsurance areas in Medicaid expanding states) saw an increase in enrollment in less-than-two-year for-profit colleges, starting in fall 2012, the academic year immediately following the 2012 Supreme court ruling. This increase is very general across demographic groups and age categories, and cannot be explained by unobserved differential county linear trends, differential county recovery paths from the Great Recession or potential endogeneity of the Medicaid expansion decision at the state by year level. We further find that the enrollment increase is particularly pronounced for Hispanic students, which is consistent with the fact that Hispanics were most likely to gain insurance in some form under the ACA. These findings are consistent with the framework described above because less-than-two-year for-profit colleges typically cater to skills demanded by low-income occupations that are carried out in low-insurance industries with small firm. Hence, a relative increase in the attractiveness of jobs in such industries should spur a relative increase in demand for the skills associated with them.

2 The Affordable Care Act and the Medicaid Expansion

Adverse selection has historically been a major problem in health insurance markets because patients tend to have much more information about their propensity to utilize medical resources than they can credibly reveal to insurers. Therefore, health insurance markets in the U.S. have functioned well only in the context of large businesses buying insurance for their employees, who would then constitute a large pool that is randomly selected from the point of view of health. The individual and small group insurance market – used by people who are not employed, self-employed, or employees of small firms – sees relatively low premiums for healthy individuals and groups, but outright denial of coverage for individuals and groups who have a high risk of substantial medical expenditures. The fact that people with high risk cannot purchase insurance at a price that is close to being actuarially fair, and the fact that insurance firms compete on attracting the best risks instead of on being the best at managing risks, indicates an information failure in the nongroup market and represents substantial welfare loss (Hendren 2013).

The approach of the ACA in solving this problem is threefold. First, insurance firms are now forbidden from denying insurance or increasing premiums based on health status – premiums can now vary only based on features of the plan, and the age and tobacco use of the policyholder. Second, everyone is obligated to buy and hold a health insurance policy in order to prevent people from buying insurance only when they are sick and thus bankrupting the insurers. Finally, to make sure that poor households and firms have affordable insurance options, the government offers a subsidy to buying insurance, which phases out as the income of the policyholder grows.

A critical component of the subsidy component is the expansion of Medicaid eligibility to all individuals legally residing in the U.S. whose income is below 138% of the federal poverty line. This expansion would have extended insurance to people earning relatively higher incomes, as well as to childless adults, who typically face much lower earning limits to qualify for Medicaid. As Medicaid is a program that is administered by the states, the ACA could not directly compel the states to implement the envisioned expansion, but instead incentivized states to expand the program by threatening to take away the matching funds that the federal government provides to state Medicaid programs. Given that nonexpanding states would have to cut their Medicaid spending considerably following the loss of the matching funds, it was anticipated that all states would expand Medicaid. However, on June 28, 2012, the U.S. Supreme Court ruled in *National Federation of Independent Businesses vs. Sebelius* that it was unconstitutional for the federal government to take away the matching funds for existing Medicaid spending from the nonexpanding states, thus making it considerably more feasible for individual states not to proceed with the expansion.[1]

[1]The 2012 Supreme Court challenge to the constitutionality of the ACA was centered around the question of the constitutionality of the individual mandate at the heart of the law, with the Medicaid expansion question of secondary concern. On the eve of the decision, most observers believed that the Supreme Court was likely to strike down the ACA in its entirety, with very few expecting the law to remain on the whole but the Medicaid expansion to be weakened. (https://fivethirtyeight.blogs.nytimes.com/2012/06/27/overconfidence-suggested-in-supreme-court-predictions/).

Shortly after the Supreme Court ruling, some states declared that they would be adopting the expansion while other states declared that they would not be. By January 2014, when the expansion was set to begin nationwide, 24 states and the District of Columbia decided to expand Medicaid to the thresholds specified in the ACA, while the remaining states did not proceed with the expansion. The number of expanding states has increased to 31 (plus D.C.) by 2016, with several additional states adopting a modified version of the expansion after negotiations with the federal government. While expanding states tended to be located in the Northeast, Midwest and on the Pacific Coast, there were many instances of similar states, such as Kentucky and Tennessee, differing in their Medicaid expansion decisions, and thus creating useful variation for observing the effect of the expansion. Figures 1 through 3 show, respectively, the states that made statements in support of adopting the expansion by July 2012 (as measured by the Advisory Board), the states implementing the expansion by January 2014, and the states that have implemented the expansion as of this date. Figure 4 shows the chronological order of these events.

It is by now well known that the Affordable Care Act substantially decreased uninsurance (Kaestner et al. 2015; Duggan et al. 2017). Figure 5 shows county-level uninsurance rates in 2009, while Figure 7 shows county-level uninsurance rates in 2014 and Figure 8 shows county-level differences between the two (all data from the U.S. Census, Small Area Health Insurance Indicators). We see that uninsurance rates decline across the country, with particularly large declines in high-uninsurance areas that adopted the Medicaid expansion, such as West Virginia, Kentucky and Arkansas. Moreover, the uninsurance rate declines are closely correlated with uninsurance rates in 2009, before the ACA was voted into law. Figure **??** plots uninsurance rate declines between 2009 and 2014 against 2009 uninsurance rates at the county level, differentiating between counties in states that expanded Medicaid by January 2014 and counties in states that did not. We see that while the correlation between uninsurance rates in 2009 and their subsequent declines is much stronger for counties in Medicaid expansion states, it is present and substantial for both sets of counties. This variation in the strength of the impact of the ACA on counties that differ in uninsurance rate and Medicaid expansion status will be the identifying variation for this paper.

3 Data

3.1 County-Level Uninsurance Data and Medicaid Data

We obtain data on uninsurance rates at the county level from the U.S. Census's Small Area Health Insurance Estimates. These estimates integrate data from the American Community Survey, the U.S. Census, participation data from Medicaid and CHIP as well as additional data from the IRS and the food stamp program (SNAP) using Bayesian hierarchical modeling (Bauder et al. 2012). While the resulting estimates of uninsurance rates contain measurement error, as long as this measurement error is uncorrelated with the errors in our dependent

variables, it should attenuate the coefficients on terms in our specification that include county-level uninsurance rates.

We use state-level Medicaid expansion data from the Advisory Board at three points in time: (i) 2012, just after the Supreme Court ruling[2] (ii) 2014, just after ACA implementation and (iii) 2016 (the latest available data). We will also use the simulated Medicaid eligibility variable from Frean, Gruber and Sommers (2017), which is the percentage eligible for Medicaid under each state's law out of a demographically and economically standardized population. We are grateful to the authors for sharing this data with us.

3.2 Higher Education Data

The higher education data we use in our analysis comes from two sources: Integrated Post-secondary Education Data System (IPEDS) and College Scorecard data (CSD). We discuss each of these datasets in turn below.

Enrollment data come from the IPEDS and are at the institution-year level. We focus on two measures of total enrollment: fall undergraduate enrollment and 12-month undergraduate enrollment[3]. Fall enrollment count measures the enrollment of an institution in the beginning of the academic year (typically on October 31st). 12-month undergraduate enrollment, in addition, counts students who enroll during the academic year, after October 31st. In addition to measures of overall enrollment, we also leverage enrollment measures by racial groups (American Indian, Asian, Black, Hispanic and White), gender groups (Female and Male), age groups (under 25, 25-29, 30-34, 35-39, 40-49, 50-64) and enrollment statuses (full-time and part-time).

We differentiate between various types of post-secondary institutions (less than two-year, two-year and four-year; public, private for-profit and private not-for-profit) to understand whether differences in exposure to the ACA at the county level led to changes in enrollment in the different types of institutions. Since our identifying variation is at the county level, we aggregate the institution-year level education data to county-year level. We balance our data at the county-year-institution type level to ensure that all regressions are run on the same set of counties. This balancing is done by imputing a zero enrollment in a county-year-institution type cell when that cell does not have any institution. In all our regressions, we include a dummy that takes a value of 1 for the imputed zero values of our dependent variables.

3.3 Other Data

We employ a range of covariates from the US Census, which contains county-by-year demographic and education information. We use total population and age distribution of the county (percent 20-29, 30-39, and 40 or over)

[2]These data reflect announced state intentions to expand Medicaid as of July 2012. We obtain these data from `http://reformmedicaid.org/wp-content/uploads/2012/07/DB_medicaid_map_lg7_6.jpg`, originally the Advisory Board.

[3]We focus on undergraduate enrollment because impacts are more likely to be perceived in undergraduate enrollment. Students who have already decided on higher studies are less likely to be discouraged by the ACA to discontinue. Alternatively, graduate studies involve considerable investments and hence it is less likely that students would opt in favor of higher studies just because of the ACA. Moreover, undergraduate enrollment measures are better populated largely because not all institutions offer graduate level courses. Undergraduates constitute bulk of higher education students (86%). Nevertheless, we do investigate effects on graduate enrollment and do not find any impacts.

at county-year level during the period under our consideration (2005-2014). In addition, we obtain education distribution in the baseline (2000) from the Census which we interact with year dummies to get relatively exogenous variation in county level education over time, which we later employ as controls. The specific education measure we use in our analysis is the share of population with at least some college, but our results are robust to other education measures such as share of population with high school or above, share of population with bachelors or above. In some of our robustness checks, we use the county-level unemployment rate in the baseline (2009) as provided by the Bureau of Labor Statistics and interact it with year dummies to get county-year level variation.

To measure poverty at the county level, we use the U.S. Census's Small Area Income and Poverty Estimates (SAIPE) which gives us each county's 2005 estimate of the fraction of people in poverty. Like the SAHIE estimates of health insuranc overagee, these are constructed using data from the ACS as well as other sources, integrated together with Bayesian hierarchical modeling. We do not have reason to believe that the measurement error problem in these model-based estimates should be any different from the measurement error problem in the health insurance estimates.

To study whether the ACA affected mobility across states and counties, we use individual-level data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP). We observe individuals' state and county of residence each quarter, allowing us to track whether mobility was affected by the Medicaid expansion status of the state in which an individual resides and whether any such effects differed by the level of uninsurance in individuals' original counties.

3.4 Descriptive Statistics

Table 1 contains descriptive statistics of our main analysis variables at the county level. Our data include 1582 counties and 10 years of data, 2005 through 2014. Since our variables of interest are different measures of enrollment, most of which are measured in the fall of an academic year, we index academic years by the fall semester of the year throughout our analysis.

There was a substantive variation in Medicaid expansion across counties and time: 27% of the counties were in states that announced after the Supreme Court ruling in 2012 their intention to expand Medicaid by 2014. We refer to these states as "2012 Medicaid states" in rest of the paper. On the other hand, 49% of counties experienced expansion by 2014 ("2014 Medicaid states") and 63% experienced expansion by 2016 ("2016 Medicaid states"). Using the Frean-Gruber-Sommers measure of simulated Medicaid elibility, we find that average Medicaid eligibility was 10% during our period of analysis.

During our sample period, uninsurance rate at the county level was on average 20% with a standard deviation of 6. Figure 6 shows there was a lot of variation in uninsurance rates across counties, with uninsurance rates ranging from around 5% in the highest insurance rate county to around 50% in the lowest insurance rate county.

While the median county had an uninsurance rate of 20%, the interquartile range was 9%. The Medicaid states had a slightly lower uninsurance rates. For example, the 2012 Medicaid states had an average uninsurance rate of 18% during our sample period with a standard deviation of 6 and an interquartile range of 10.

During our period of analysis, fall enrollment at the county level was on average 4,965; 12-month enrollment (which includes enrollments after October 31st and during the academic year) was on average 6,807. At the county level, enrollees were more likely to be white and slightly more likely to be female, more likely to have students below 25 and more likely to be full-time. At the baseline, an average county had a population of 133 million. The average unemployment rate during our period of analysis was 9.2%.

Tables 2 and 3 contain descriptive statistics at the county level by institution type. The top panel includes summary statistics from the unbalanced panel, while the bottom panel report the same for the balanced panel. In the less than two-year sector, for-profit schools are the largest in size. In both the four-year and two-year sectors, for-profit schools are much smaller than their public counterparts, but larger than their not-for-profit counterparts. While all institution types have more white students, for-profits have a higer share of minority students (blacks and Hispanics). They also have a higher share of females. Except commmunity colleges (2-year public colleges) all institution types have a higher share of full-time students. For-profit students are more likely to be older except in <2 year schools where they are more likely to be young (Figure A1) and they are less likely to be poor as evidenced in higher pell elibiligy (Figure A2)

4 Empirical Strategy

We seek to obtain plausibly exogenous variation in the exposure of an area to the Affordable Care Act. It is clear from Figure 5 that different counties experienced different changes in uninsurance because of the ACA, with uninsurance rates in 2009, a variable that is predetermined with respect to the law, being a key predictor of its effects. However, counties with different uninsurance rates in 2009 also differ on many other dimensions – such as income and unemployment – which may place them on differential trends with respect to enrollment in different types of higher education for reasons unrelated to the implementation of the ACA. We argue that we can improve the quality of the variation in county uninsurance rates in 2009 by interacting them with indicators for whether the state in which the county was located expanded Medicaid. While Medicaid expansion decisions may be endogenous, as they are taken through a political process, it is unlikely that they are made because of the circumstances of a single county or group of counties within the state in question. Hence, we may take counties that are located in Medicaid-expanding states as counterfactuals for counties with the same rates of uninsurance in 2009 in states that did not choose to expand Medicaid.

A concrete example can be found by thinking of Bolivar County in Mississippi and Phillips County in Arkansas. Both are small, rural counties in the valley of the Mississippi River with around 20% uninsurance rates and over 60% of the population being African-American. However, Arkansas decided to adopt the Medicaid

expansion, while Mississippi decided not to move forward with it. Therefore, because of the political dynamics of the rest of their states (which otherwise need not affect the counties in question), Phillips County receives the Medicaid expansion while Bolivar County does not.

Our specification is as follows:

$$\ln(y_{c,t}) = \eta_c + \mu_t + \alpha_t \times MCE_{s(c)} + \beta_t \times U_c^{2009} + \gamma_t \times MCE_{s(c)} \times U_c^{2009} + \delta z_{c,t} + \Phi X_{c,t} + \varepsilon_{c,t}$$

(1)

where $y_{c,t}$ is a dependent variable of interest (usually enrollment in some type of higher education institution in the given county c and year t), η_c are county fixed effects, μ_t are year fixed effects, $MCE_{s(c)}$ is a time-constant indicator for state s in which county c is located adopting the Medicaid expansion, U_c^{2009} is the uninsurance rate of county c in 2009, and $MCE_{s(c)} \times U_c^{2009}$ is an interaction of the two. The year fixed effects α_t multiplying the Medicaid expansion indicator flexibly control for differential nonlinear time trends between counties located in states that expanded Medicaid and counties located in states that didn't. The year fixed effects β_t flexibly control for differential time trends between counties with different uninsurance rates in 2009. The parameters of interest in this specification are the fourth set of year fixed effects γ_t, which measure the differences-in-differences in the dependent variable in question at time t between high-uninsurance and low-uninsurance counties in states that expand Medicaid compared with states that don't.[4] Our identification assumption is that the only reason why differentials in the dependent variables (e.g. enrollment rates in various types of colleges) between high- and low-uninsurance counties and across expanding and non-expanding states vary over time is because of the changes to health insurance markets brought about by the Affordable Care Act, and specifically because of the differential Medicaid expansion decisions resulting from the 2012 Supreme Court decision in *NFIB vs. Sebelius*.

Since our dependent variable $y_{c,t}$ is typically a count variable that may equal to zero, and the logarithm of zero is undefined, we follow the procedure of Pakes and Griliches (1980) (also employed by Acemoglu and Linn 2004) and include a variable $z_{c,t}$ that is an indicator for whether $y_{c,t}$ is equal to zero. This correction tends to be unimportant in practice. In some versions of our specification we also seek to control for potential omitted variables $X_{c,t}$ that may be third factors explaining the observed correlations (or lack thereof) between enrollment in various types of higher education and the variables measuring ACA exposure intensity. These will be discussed in greater detail in the Results section.

We will experiment with multiple definitions of Medicaid expansion, but will focus on defining expanding states as the states that announced their intent to expand Medicaid in July 2012, shortly after the decision. (All of these states eventually expanded Medicaid). We will also consider looking at the states that expanded by January 1, 2014, and by the current date (the last state to expand so far was Louisiana in 2016). Additionally, we

[4]It is important to note that the Medicaid expansion indicator may be measuring not just the direct effect of the Medicaid expansion on higher education enrollments but also the effects of a state's more favorable policy towards the ACA more generally. For example, states could influence the extent to which the uninsured participated in the health insurance exchanges through advertising, exchange design, outreach, and other channels.

will investigate our specification replacing the Medicaid expansion variable with simulated Medicaid eligibility from Frean, Gruber and Sommers (2017), which is the percentage eligible for Medicaid under each state's law out of a demographically and economically standardized population.

We will focus on graphical presentation of the time series γ_t and on the analysis of its evolution before and after the 2012 Supreme Court decision. However, as the patterns of γ_t typically are fairly stark, and to summarize the entire series in one number, we will also focus on a less flexible specification that captures the change in γ_t after 2012 relative to before. Specifically, we will estimate

$$\ln(y_{c,t}) = \eta_c + \mu_t + \alpha_t \times MCE_{s(c)} + \beta_t \times U_c^{2009} + \gamma \times Post_t^{2012} \times MCE_{s(c)} \times U_c^{2009} + \delta z_{c,t} + \Phi X_{c,t} + \varepsilon_{c,t}$$

(2)

where $Post_t^{2012}$ is an indicator variable for $t \geq 2012$, and all the other variables are defined as before.

5 Results

5.1 Main Results: Effects on Overall Enrollment

The objective of this section is to understand the effects of county level differences in exposure to the ACA (as captured by differences in medicaid expansion and baseline county uninsurance measures) on postsecondary education choices of individuals as captured by postsecondary enrollment. Our main results presented in this section estimate specification (1) in section 4. It is noteworthy that our main results will not include any state-year level covariates (X_{ct}) or county trends. We will later see that our results hardly change with the inclusion of controls, county trends, state by year fixed effects or more sophisticated control strategies (section 5.7). Moreover, estimates from this specification suggest that the pre-trends in differential enrollment between high- and low-uninsurance counties in expanding and non-expanding states are flat at zero, and therefore adequately accounted for. Therefore, we do not include controls in our baseline specification but rather consider them as robustness checks, since they do not appear to be necessary for identification.

We begin by studying the effects on overall enrollment. Figure 9 Panels A and B present results from estimation of the event study specification (1) on overall enrollment. Here we focus on the 2012 Medicaid expansion definition(see section 4). Throughout our analysis, we only present results for <2 year and 2-year for-profit and public institutions. We do not find any effect on enrollment in 4-year institutions or on not-for-profit instituions, so these results are not reported to save space, but are available on request. The top right panel of Figure 9 Panel A shows that counties with 1% higher uninsurance rates in Medicaid expanding states experienced 1.25% higher enrollment in <2 year for-profit institutions after three years (2014) of the 2012

Medicaid expansion announcement following the Supreme Court ruling as compared to their counterparts in non-expanding states. This effect is highly statistically significant. In the sample of 2012 Medicaid states, the median county had an uninsurance rate of 20%, a bottom quartile county had an uninsurance rate of 13% and a top quartile county a rate of 23% (Table 1). So our result implies that in comparison to a bottom quartile county, the top quartile county in 2012 Medicaid states experienced an approximately 9% increase in enrollment in <2 year for-profit institutions after three years. We do not find any evidence of effects on enrollment in any other institution-type as evidenced in the other event-study charts in the figure.

Figure 9 Panel B shows that a higher exposure to ACA leads not only to an increase in enrollment among traditional enrollees starting in early fall but also among enrollees who enroll later during the academic year. A 1% increase in uninsurance rate in a Medicaid expanding state leads 12-month enrollment (that captures both fall enrollees and enrollees who enroll later in the academic year) in <2-year for-profit institutions to increase by 1.5% after three years. This suggests that some of the new entry due to the ACA took place during the academic year, after early fall. To investigate the extent of entry among these late enrollees, Figure 9 Panel C presents results from the event study specification (1) where the dependent variable is difference in enrollment between 12-month and fall enrollment. We find a 2% increase in entry among these late enrollees driven by the ACA.

The timing of the increases are noteworthy. While we do not find any evidence of any pre-trend in <2 year for-profit enrollment, the increases commence immediately after the June 2012 Supreme court decision when the 2012 Medicaid states announced their intention to expand. Also noteworthy is that the increase continues for all the three post-years in our sample which may reflect better information percolation over time as well as more time for prospective students to make their postsecondary choices.

We study the same question in Figure 10 Panels A-B except that we use alternative definitions of Medicaid expansion. Figure 10 Panel A presents results where Medicaid expanding states are states that expanded Medicaid by 2014. In Figure 10 Panel B, Medicaid expanding states are states that expanded Medicaid by 2016. The idea here is to explore whether anticipation effects in states that later expanded ACA (by 2014 or 2016) led individuals in the more exposed counties (that had higher uninsurance rates) to switch to college, or more specicially to <2 year for-profit institutions. We find strong evidence in favor of this hypothesis. As may be expected, these effects are somewhat muted (approximately 0.75%) relative to the Medicaid 2012 effects (between 1.25-1.5%) as these effects are driven by anticipation while the Medicaid 2012 results are driven by actual announcement in the 2012 Medicaid states that they will expand Medicaid by 2014.

In Figure 11 we use yet another measure of Medicaid expansion where we use a simulated Medicaid eligibility (section 4), based on Frean, Gruber and Sommers (2017). While the results remain qualitatively similar, the <2 year for-profit enrollment effects get considerably stronger, varying between 2.5% and 2.7% for a 1% increase in uninsurance rate, given Medicaid eligibility. This is encouraging as this simulated measure likely exploits a

more exogenous variation in Medicaid expansion.

5.2 Heterogeneities in Effects

In this section, we investigate whether there were heterogenities in <2 year for-profit enrollment effects due to ACA exposure by race, gender, enrollment-status, age-group and interactions of enrollment status and age. Since we do not find any enrollment effects in the other types of institutions, we will limit our discussion to only this type of postsedondary institutions. Figure 12 explores heterogenities by race. We find that increases in ACA exposure led to increases in Hispanic enrollment in the <2 year for-profit enrollment that grows over time; these effects vary between 1-1.5%. We also find evidence in favor of increases in enrollment of Black students in <2 year for-profit institutions (approximately 1% in the first year), but the effect is no longer statistically significant in the latter years. It is worth noting that when we control for county trends and other covariates (population, age distribution of population, flexible functional forms for educational distribution of population as well as poverty at baseline) the effect on Black enrollment becomes stronger (although still smaller than the effect on Hispanic enrollment) and persists over time. These stronger results are driven by inclusion of county trends; addition of covariates do not affect our results. We do not find any statistically significant evidence in favor of increases in White enrollment.

Figures 13 explores heterogeneities by gender. We find that the enrollment increases in the <2 year institutions are contributed by increases in both male and female enrollments. Once again we find that inclusion of county trends makes the female enrollment effects stronger while the male enrollment effects become relatively imprecise and loses significance. As in above, these differences are caused by inclusion of county trends; addition of covariates do not make any dent on these results.

Figure 14 differentiates between the effects by enrollment status (part-time, full-time). The results for part-time enrollment are relatively imprecise and we do not find any statisitcally significant evidence of effects on part-time enrollment, although the effects vary between 0.5% and 0.85% in magnitude. On the other hand, we find positive statistically significant effects on full-time enrollment that grows over time and varies between 0.5-1.25% during our post-period of analysis (2012-2014).

Figure 15 looks at the effect on students by age. The estimates come from a specification that introduce interactions with three age brackets to identify differential impacts in these ranges: < 25, $25 - 40$ and >40. While the effect seems to be the largest in magnitude in the youngest age group, the effects are not statistically distinguishable between the groups.

Figures 16 explores heterogeneities in effects by age and interaction between age and enrollment status. We find that the <2 year enrollment effects have been contributed both by young enrollees (0.8% enrollment increase in under 25 age group) and relatively older enrollees (1.25% increase in 25-49 age agroup). Interestingly, once again we find that these enrollment effects in both age groups are driven more consistently by increases in

enrollment among full-time students. The effects for part-time enrollment are not statistically significant and for the younger age group seem to precede Medicaid expansion.

5.3 Effects on Employment and Degree Completion

One might expect that the ACA would increase employment in industries where it was relatively hard to obtain health insurance before, but the ACA led to a relaxation of this constraint making these industries more attractive for job-seekers. In the first chart of Figure 17, we report triple interaction coefficients from estimation of specification (1), where we replace our dependent variable above by the natural log of total employment. The left hand side panel in the figure presents population weighted estimates, while the right hand side presents unweighted estimates. We find no evidence that the ACA led to a change in overall employment, the effects are both economically and statistically zero.

In the next two rows of charts of Figure 17, we dig deeper to understand whether the composition of employment in industries changed. The population weighted estimates show that employment in leisure industries (such as restaurants and hotels) increased in counties in Medicaid states that had a marginally higher uninsured population. Interestingly, the increase is more prominent both statistically and economically after 2014 when ACA was implemented. Since health insurance changes would have taken place after the ACA went into effect, the relative increases post-2014 are telling. The right panel presents unweighted estimates. These are much noisier during the entire span of our data period, but we still notice a pick-up in employment in leisure industries following 2014, although it is not statistically different from zero.

Figure 18 looks at the effect of the ACA on degree attainment in different majors. Here, we once again estimate specification (1) where we replace the dependent variable by the natural log of degrees in different major categories. While there is no evidence of changes in overall number of degrees produced due to the ACA or in other majors, there seems to have been a composition change in degrees produced with a move towards Vocational degrees. Since Vocational degrees prepare individuals to quickly take up jobs that have fairly low compensation (and, in particular, tend not to offer insurance), this finding is consistent with the idea that the Affordable Care Act made such jobs more attractive at the margin by offering an external source of health insurance.

5.4 Distinguishing between uninsured populations at different income levels

In Figure 19a-c, we deviate from the above overall measure of uninsurance rates by considering uninsurance rates at different levels of poverty. We consider three different uninsurance rates: percentage of uninsured individuals between ages 18 and 64 in 2009 in each of the following Federal Poverty Level categories: <138% FPL, 138-400% FPL and >400%FPL. We separately run specification (1) three times, replacing our above uninsurance measure respectively by uninsurance rates in each of these poverty categories. The dependent variable is the

same: enrollment at less than 2-year private for-profit institutions. The objective is to understand whether the enrollment response (in less than 2-year private for-profit schools) with Medicaid expansion was uniform across uninsured population at different poverty levels or the effect was more prominent among uninsured population at a certain poverty level.

In Figure 19a, we find that the effects are the largest for the most wealthy category >400%FPL) and is statistically different from the effect for the poorest category. We find that in a Medicaid state, a one percentage point increase in uninsurance in the >400% FPL group increases enrollment by 4 percent. In contrast, the enrollment effect is 1 percent or lower when there is a one percentage point increase in uninsurance in the middle or lowest poverty level. One potential issue here is that these three uninsurance rates may have come from different distributions and hence these effects are not necessarily comparable. We use two methods to get these three uninsurance rates in comparable scales: (i) we normalize each uninsurance rate to a mean zero standard deviation one variable and (ii) we normalize each uninsurance rate to the mean and standard deviation of our overall uninsurance rate variable. The corresponding event study charts are presented in figures 19b and 19c respectively. While we see a similar pattern in the effects with the enrollment response in Medicaid states being the highest for uninsured people with >400% FPL, the effects are not statistically different from each other. Another possibility is that there is differential measurement error in the uninsurance rates, with the >400% FPL uninsurance rate more precisely measured than that of the other categories, perhaps because people earning more than 400% of poverty may be more aware of the institutional structures governing their eligibility for insurance. Still the patterns suggest an interesting picture. In the lowest poverty group, the uninsured people are less likely to enroll in college than in the highest poverty group. A potential explanation is that the low income individuals are eligible for Medicaid in the Medicaid states regardless of employment and hence have a lower incentive to enter college. On the other hand, the high income individuals are not eligible for Medicaid, and hence are more likely to enter college for a quick training/certificate to enhance their chances of getting a preferred job in a firm that will pay for their insurance or buy insurance at the exchanges at a cheaper rate than before if the job still doesn't offer insurance. States that expanded Medicaid were also more generous in terms of insurance purchases on the exchanges, which may have encouraged this move.

To further explore whether exposure to ACA encouraged lower income students to differentially go to for-profit (or other colleges), we estimate specification (1) where we replace the dependent variable by share of undergraduates in a college who were Pell recipients. Pell recipients are relatively poorer than their peers who did not qualify for Pell grants. So Pell recipiency is regarded as a measure of income. As figure 20 shows, we find no evidence that the ACA led to an increase in Pell enrollment. This finding is consistent with the pattern we find above.

5.5 Anticipation Effects

Another check on our identification strategy is to examine differential effects among states that confirmed that they were expanding Medicaid immediately after the 2012 Supreme Court decision relative to the states that did not commit immediately but nevertheless expanded in 2014, and relative to states that expanded after 2014. To do so, we enrich our baseline specification by interacting the 2009 uninsurance rate with three sets of Medicaid expansion dummies instead of one, with each dummy capturing one of the above Medicaid expansion timings. The specification we estimate is reported below:

The resulting three sets of interaction coefficients are plotted against time in Figure 21. We see that the effects of the 2009 uninsurance rate in states announcing their decision to expand in 2012 follow the by now expected pattern of being flat at zero before 2012 and rising and statistically significant thereafter with a similarly sized effect seen earlier. However, the effects for states expanding in 2014 but not announcing their decision to do so by 2012 are flat at zero both before and after the Supreme Court decision. This is likely because their expansion decisions were made sufficiently late that individuals and colleges did not have time to anticipate them before 2014. Lastly, the effects of the 2009 uninsurance rates in states expanding Medicaid after 2014 show a very similar pattern to the effects for states announcing their decision to expand in 2012. They are generally close to zero before 2012 (although with a statistically insignificant pretrend in 2005-2007), and then statistically significantly rise after 2012.

Since it should have been particularly difficult to anticipate the Medicaid expansions of states that expanded after 2014, the fact that their effect pattern follows that of the states announcing their decision to expand in 2012 is puzzling. However, this pattern is actually driven by only two of the late-expanding states: Louisiana and Pennsylvania. Both of these states are special because they had considerable internal political battles over the question of whether to expand, and reasonable observers could have concluded that these states would expand Medicaid eventually even while the issue was being debated. In particular, Pennsylvania, a swing state, had an unpopular Republican governor who first opposed expansion, but then supported a qualified version of the expansion under constituent pressure, so it may have been reasonable for observers to assume from the beginning that this governor would either expand Medicaid or be replaced by a challenger favorable to Medicaid expansion. Louisiana, a deeply conservative state, nevertheless had substantial support for the expansion in both parties because of the associated transfers from the federal government. While excluding any one of these two states makes the Medicaid expansion effects insignificant after 2012 for the states announcing their expansion decision after 2014 (second row of Figure 21), excluding both states makes the effect pattern for the late expanders completely flat in our entire post- period (last chart in Figure 21). This is in line with our prior that Medicaid expansion should have larger effects on outcomes in states that announce their decision to expand early rather than in states that announce this decision late.

Since we find that ACA exposure led to increases in enrollment in less than 2-year for-profit colleges, we next

investigate whether the ACA led to entry of for-profit institutions in the less than 2-year sector in Figure 22. While the Figure seems to suggest a little uptick in counties with marginally higher uninsured in Medicaid states, the effect is economically very small (approximately 0.1 percentage increase) and statistically not significant. This indicates that the increase in enrollment was due to existing for-profit colleges absorbing more students in the less than two-year sector.

5.6 Are the Effects Driven by Differential Migration?

In this section, we investigate whether the effects obtained above were contributed to some extent by differential migration to (or from) the counties that were more exposed to the ACA (as captured by high baseline uninsurance rates and Medicaid expansion). For this purpose, we construct two measures of migration at the county-year level, the level at which our identifying variation lives. Our measures, in-migration and out-migration, draw heavily on the measures constructed in Schwartz and Sommers (2014). The first measure, in-migration, captures an individual who moved into a county i in a quarter but was in another county j in the previous quarter. The second measure, out-migration, captures an individual seen in county i in a quarter who has moved to another county k in the following quarter. Aggregating up these two measures to county-year level and using each of these as dependent variables in separate regressions, we estimate our specification (1) above (section 4). The results for in-migration and out-migration are respectively presented in Figure 19 Panels A-B in event-study style charts like our main results above. As we can see, there is no evidence of any differential in-migration or out-migration that covary with ACA exposure. This analysis rules out that our results are driven by differential migration of individuals into counties exposed more to the ACA.

5.7 Robustness Checks

In this section, we conduct a variety of robustness tests to check the sensitivity of our above results. The findings are presented in Figures 24-25. In Figure 24 Panel A, we sequentially add linear county time trends, state-year fixed effects, CBSA-year level fixed effects, and Commuting zone-year fixed effects, each to our baseline specification above. Figure 24 Panel B controls for unemployment dynamics by including interactions of 2009 unemployment rate with year dummies. We use 2009 unemployment rate because this measure captures the variations in unemployment (and labor market) across counties in 2009 in the depth of the recession, when unemployment rate hit its peak. Since the policy change we exploit took place soon after the recession we want to rule out that some of the effects are due to differences in impacts of the recession across states and counties.

Figure 25 includes county trends and a rich array of county-year level covariates: population, age distribution of population (%20-29, %30-39, %40 or over), baseline (2000) education distribution of the county population interacted with year dummies and baseline (2005) share of the population in poverty interacted with year dummies. Since the education distribution and poverty distribution of a county can potentially be endogenous

to our county-level time varying outcome measures, we use their baseline levels at the county level to exploit baseline variation in these measures across counties and interact them with year dummies to control for these covariates in the most flexible possible manner.

The results remain extremely robust in each of these iterations (the only two differences we see are discussed in the paragraph below). They barely budge from the baseline results either in terms of economic significance or statistical significance. In fact inclusion of each of CBSA-year fixed effects and Commuting zone-year fixed effects make the effects slightly stronger, as does the inclusion of county trends. While our baseline results showed a 1.5% increase in total enrollment for <2 year for-profit schools, the effect sizes in these various robustness exercises vary between 1.5% to 4%. The upper bound of the effects (between 2.5% and 4% are obtained when CBSA-year and Commuting zone-year effects are included. In other specifications the effects vary between 1.5% and 1.75%. The robustness of our results across a variety of specifications adds considerable confidence to our findings.

As outlined in section 5, inclusion of county trends and our comprehensive set of controls above makes the results for the minority groups (Black and Hispanic) and for Females stronger in <2 year for-profit institutions. We find that the effect for Black enrollment increases in size and persists over our three post-periods, varying between 1.2-1.4%. The effect on Hispanic enrollment continues to persist over time but the effects become stronger economically, varying between 1-2.2% during our post-period. This makes more sense as the minorities, and especially the Hispanics, benefited the most from the ACA and Medicaid expansion.

5.8 Average Effects

While in our above analysis, we were interested in understanding how the effects on education markets have varied over time due to exposure to the ACA, our objective in this section is to quantify an average effect due to ACA exposure pooling together all post-years. More specifically, we estimate specification (2) outlined in section 4. The results for this analysis are presented in Table 4 for our main variables (overall enrollment measures). Consistent with our event study baseline results, we find a 1% increase in enrollment in ACA exposed counties in <2 year for-profit institutions in the post-period. We also find that part of this effect is contributed by students enrolling during the academic year, after early fall. Once again, we do not find any evidence of enrollment changes in other institution types.

6 Conclusion

The strong connections between health insurance and employment in the U.S. imply that a major health insurance reform is likely to change individuals' incentives to invest in their human capital. We investigate how the Affordable Care Act impacted educational choices by exploiting the interaction of different baseline

uninsurance levels across areas of the U.S. before the ACA together with the state-level Medicaid expansion decisions that state governments made in the wake of the 2012 Supreme Court decision in *NFIB vs. Sebelius*. Our empirical strategy compares the differential in enrollment rates between high- and low-uninsurance counties in expanding states with the same differential in nonexpanding states, and interprets its evolution over time as caused exclusively by the differential Medicaid expansion.

We find that enrollment in <2 year for-profit institutions increases in high-uninsurance counties relative to low-uninsurance counties within states that expand Medicaid relative to those that don't, in the fall of the 2012 school year, immediately following the Supreme Court decision. An additional percentage point uninsured is associated with an approximately 1.25-1.5% higher <2-year for-profit college enrollment in county located in a Medicaid expansion state as compared with a nonexpansion state after 2012 relative to before. This result is robust, both in magnitude and in statistical significance, to controlling for differential county recovery dynamics from the Great Recession, unobserved county trends in enrollment that are linear over time, unobserved state or commuting zone time-varying effects of arbitrary form, and a variety of controls for dynamics related to county demographics, prior education levels and income. The enrollment increase in <2-year for-profit colleges is present for a variety of ways of measuring enrollment or defining which states were affected when by the Medicaid expansion. The enrollment effects are more prominent (economically and statistically more significant) for full-time rather than part-time enrollment, for minority students (Hispanics and Blacks, especially Hispanics), and for women. We do not find any similar statistically significant change in enrollment in other types of educational institutions besides the <2-year for-profit colleges. This is intuitive, because for-profit colleges tend to specialize in short programs aimed towards getting students jobs in particular trades, many of which tended to be conducted in small group settings that made insurance purchase different before the ACA.

We interpret our results as consistent with students and colleges acting rationally in anticipation of the ACA creating a better functioning insurance market for individuals outside the large employer setting by mitigating the adverse selection problem in small group and individual insurance markets, extending the eligibility for public insurance to individuals earning higher incomes than before, and subsidizing health insurance more heavily. Such reforms narrow the differential between jobs conducted in settings in which it is easy for employers to insure workers and jobs conducted in settings in which it is not easy to do so, allowing workers to allocate themselves to jobs where their marginal benefits are relatively high and their marginal costs of labor are relatively low. Our results are consistent with the idea that using firms as pools to reduce adverse selection creates considerable frictions both in the market for labor and in the market for human capital, which the ACA may partially have alleviated, but do not permit us to make a statement about the welfare consequences of the ACA as a whole.

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7 Tables and Figures

Figure 1

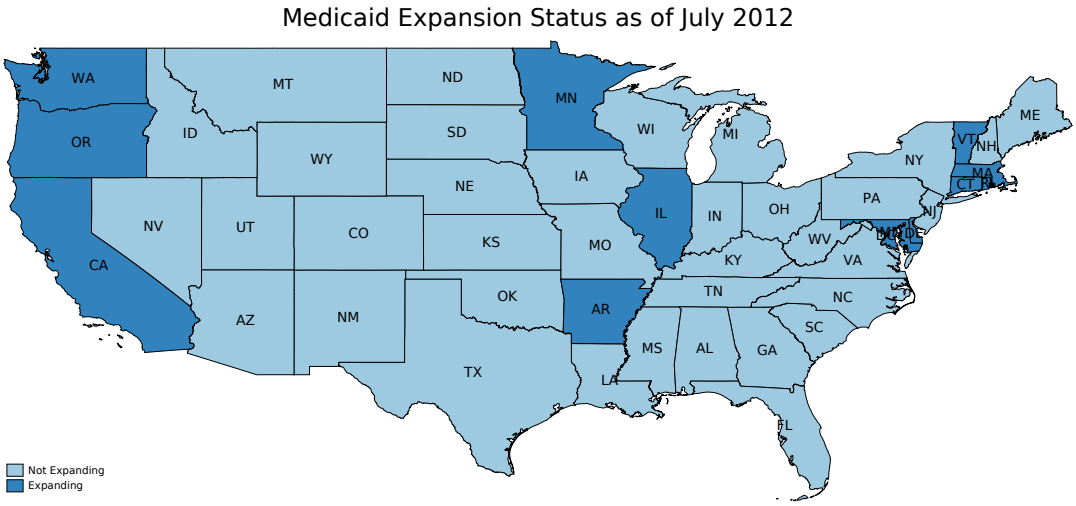


Figure 2

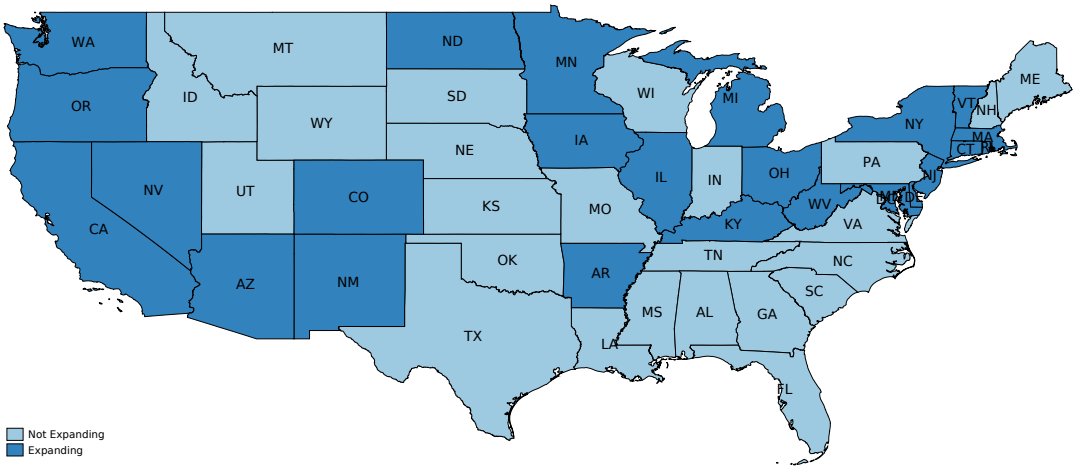


Figure 3

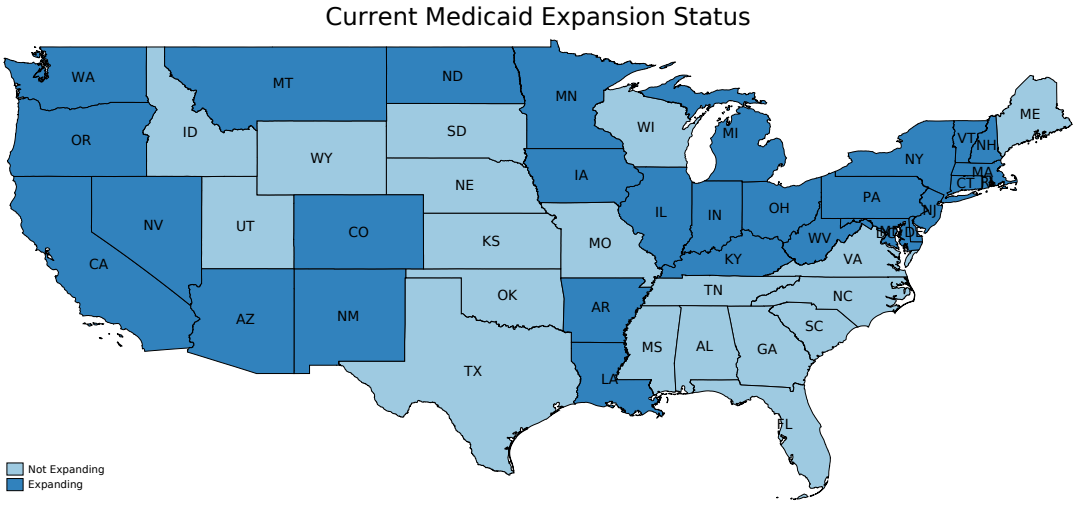


Figure 4: Timeline of Medicaid Events

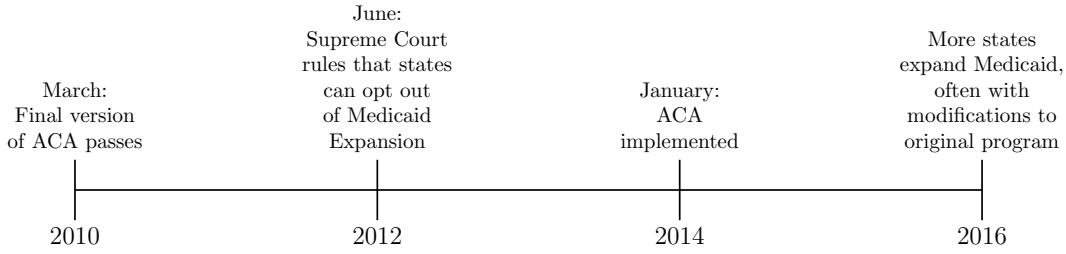


Figure 5: Uninsurance in 2009

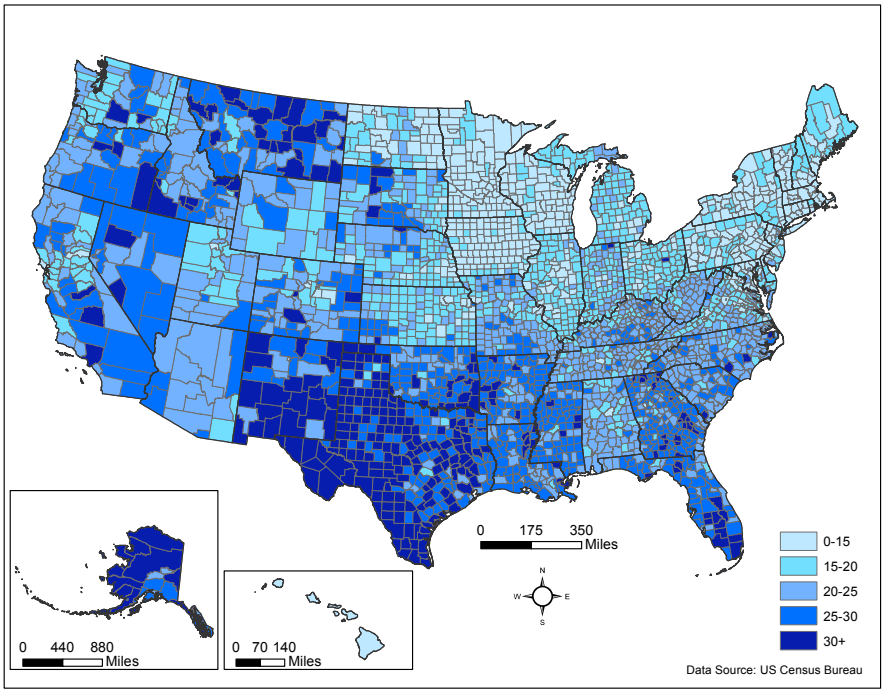


Figure 6: Distribution of Baseline Uninsurance

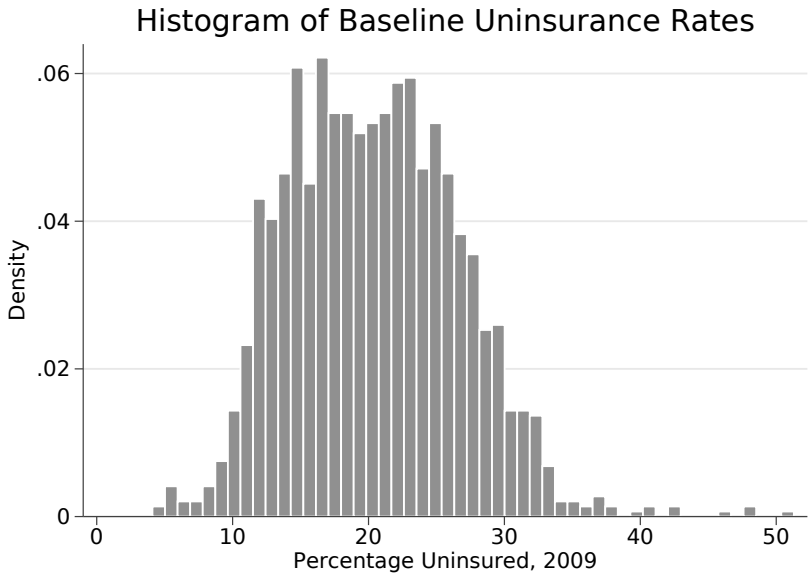


Figure 7: Uninsurance in 2014

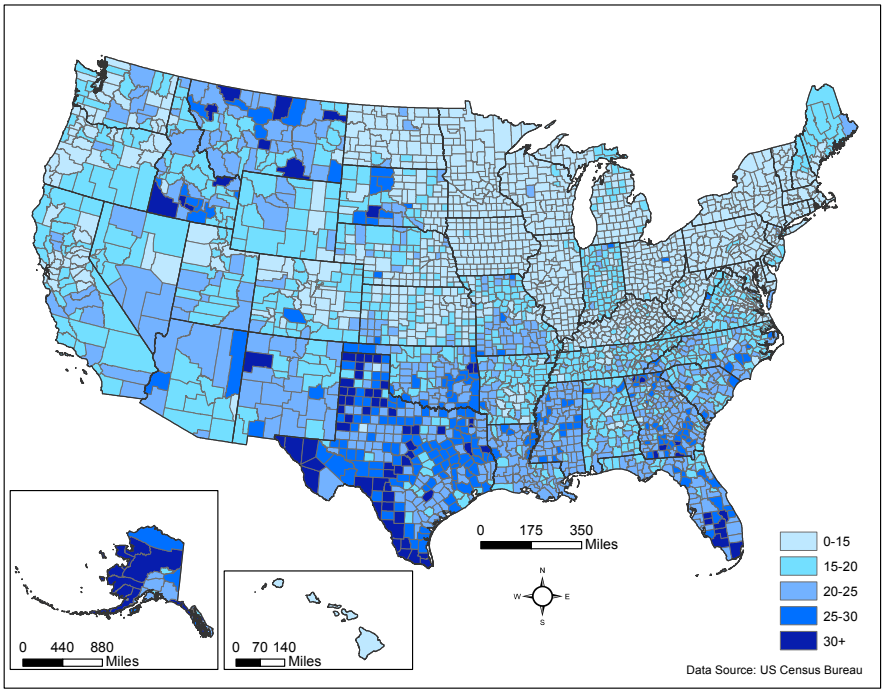


Figure 8: Change in Uninsurance from 2009 to 2014

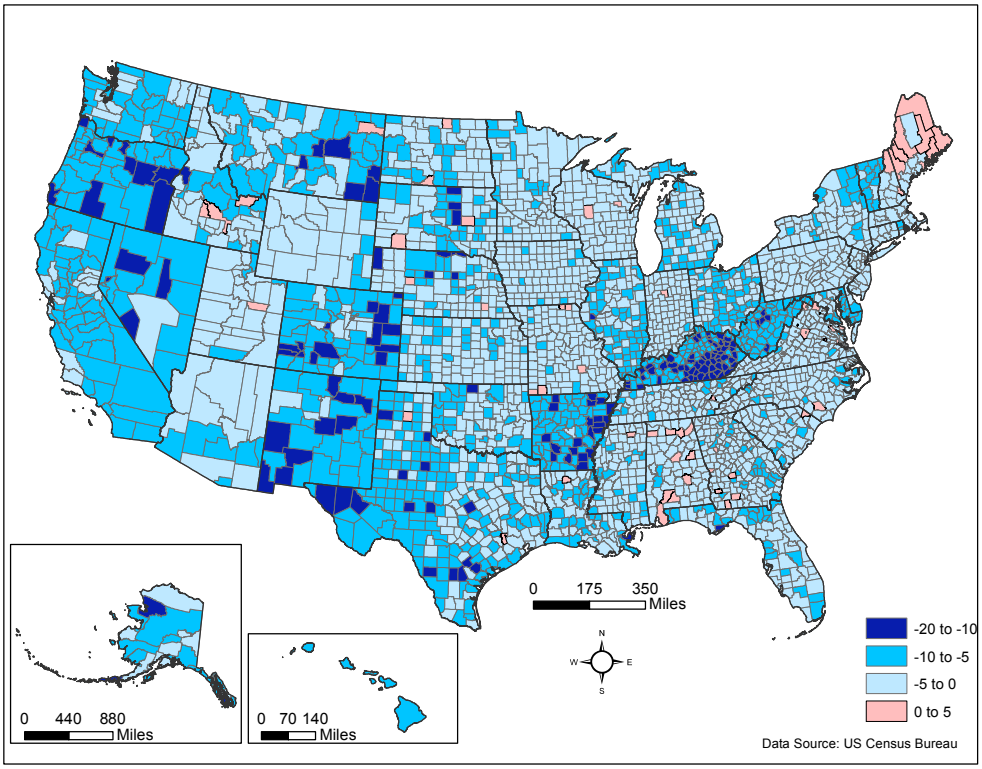
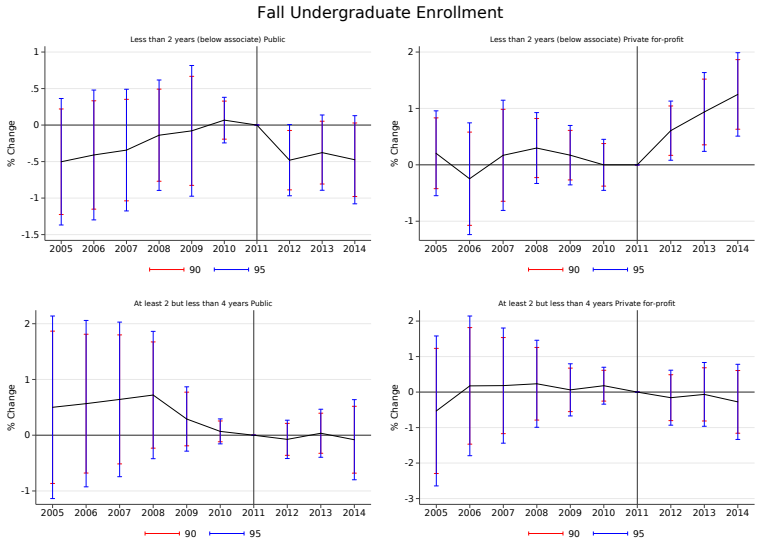
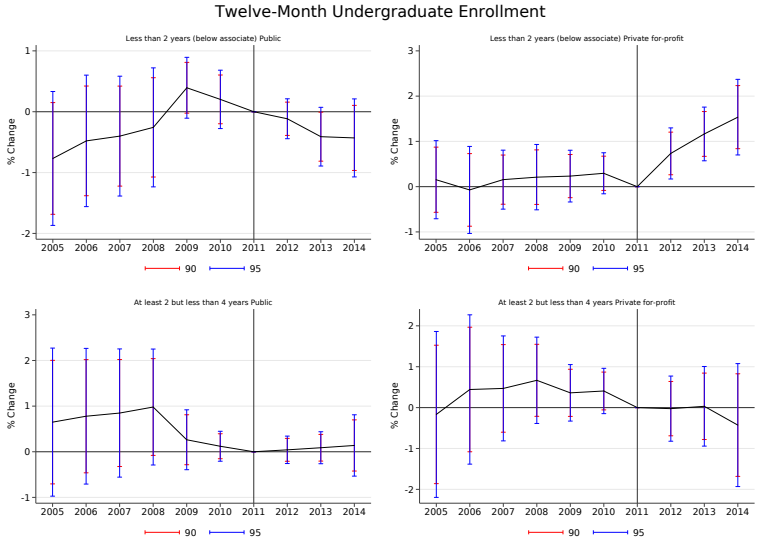


Figure 9

Panel A



Panel B



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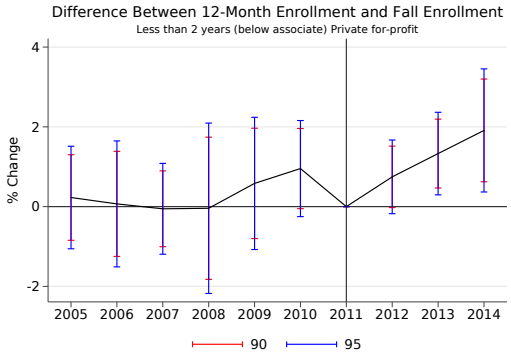
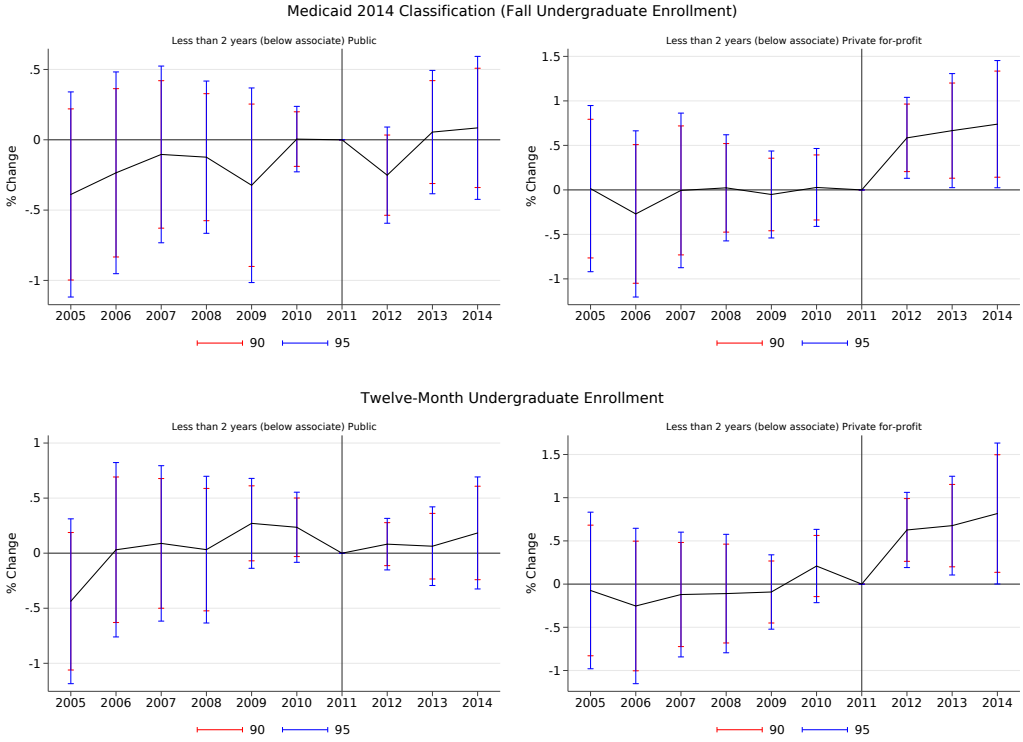


Figure 10

Panel A



Panel B

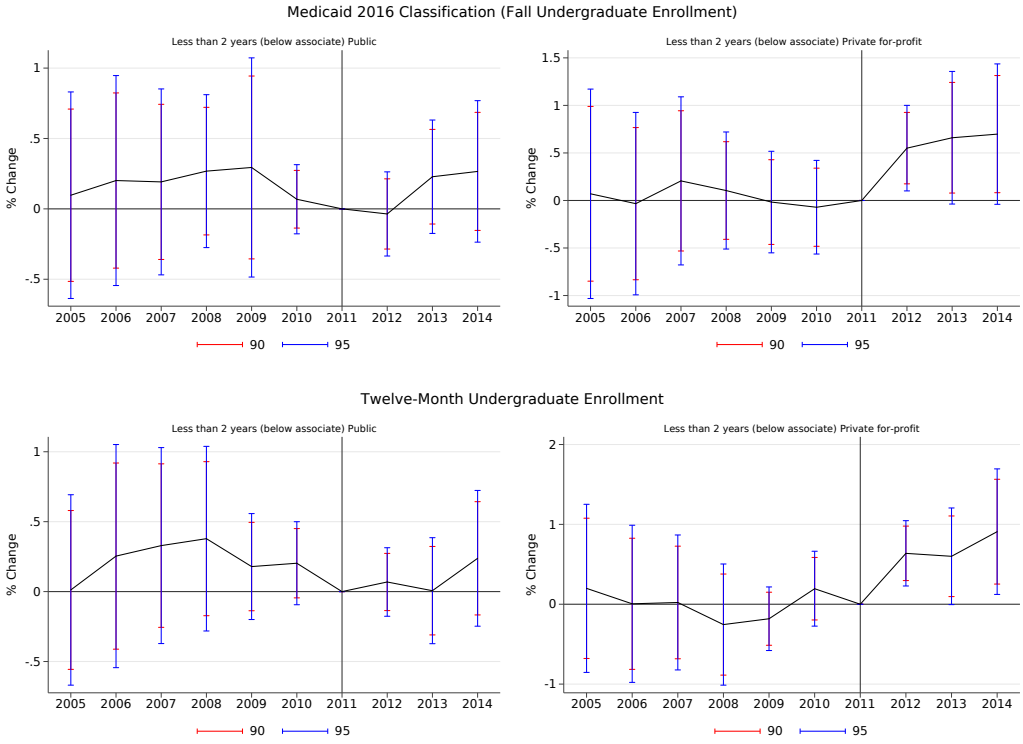


Figure 11

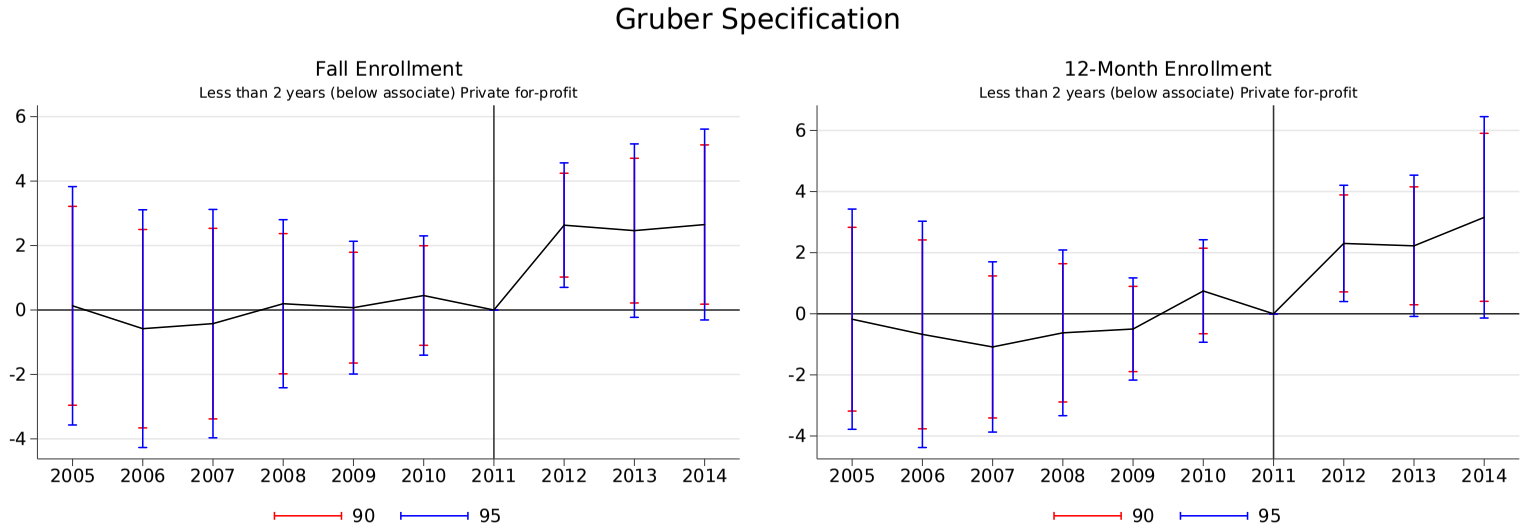
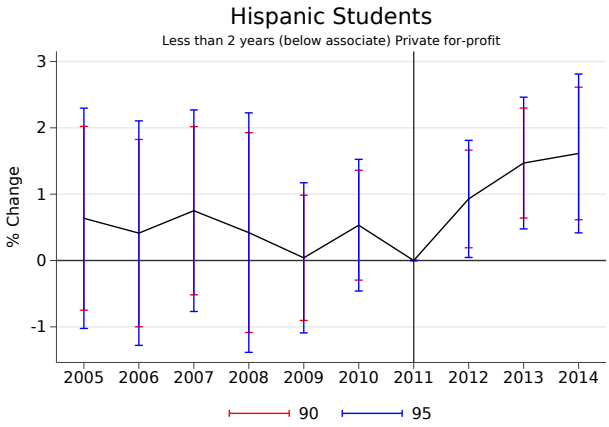
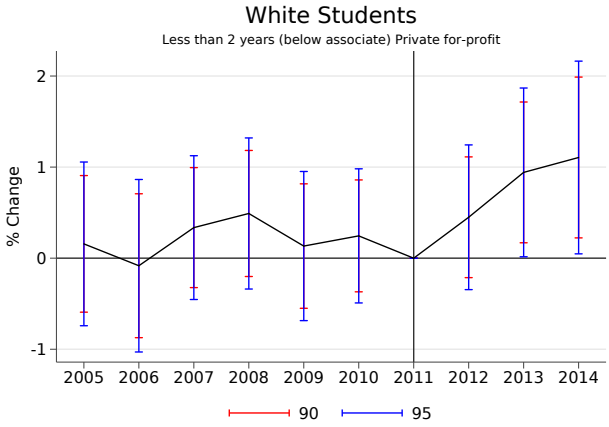

Gruber Specification
Fall Enrollment
Less than 2 years (below associate) Private for-profit
6
4
2
0
-2
-4
2005
2006
2007
2008
2009
2010
2011
2012
2013
2014
90
95
12-Month Enrollment
Less than 2 years (below associate) Private for-profit
6
4
2
0
-2
-4
2005
2006
2007
2008
2009
2010
2011
2012
2013
2014
90
95

Figure 12


Hispanic Students
Less than 2 years (below associate) Private for-profit
% Change
3
2
1
0
-1
2005 2006 2007 2008 2009 2010 2011 2012 2013 2014
90
95


White Students
Less than 2 years (below associate) Private for-profit
% Change
2
1
0
-1
2005 2006 2007 2008 2009 2010 2011 2012 2013 2014
90
95

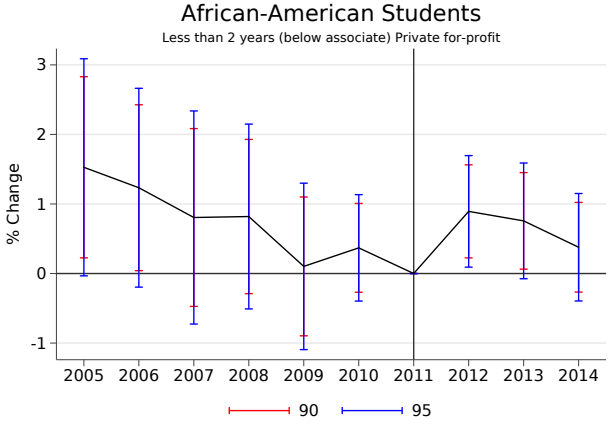

African-American Students
Less than 2 years (below associate) Private for-profit
% Change
3
2
1
0
-1
2005 2006 2007 2008 2009 2010 2011 2012 2013 2014
90
95

Figure 13

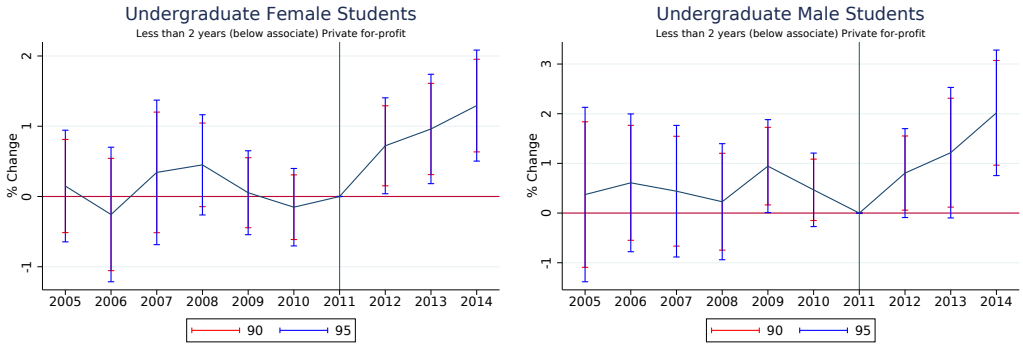


Figure 14

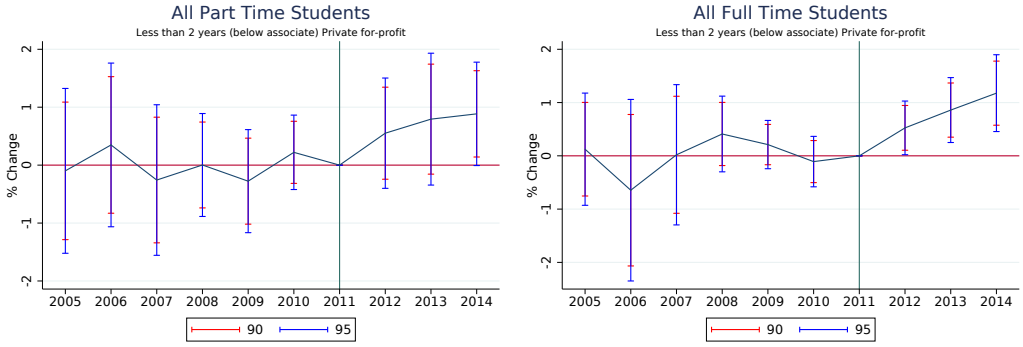


Figure 15

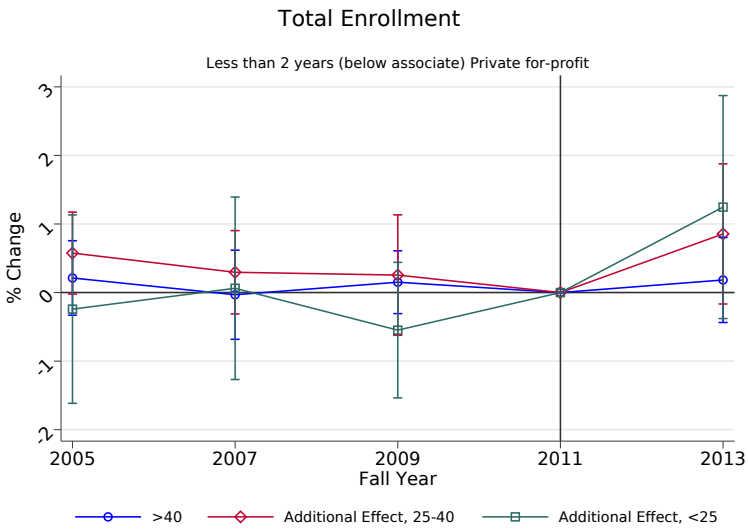

Total Enrollment
Less than 2 years (below associate) Private for-profit
% Change
3
2
1
0
-1
-2
2005
2007
2009
2011
2013
Fall Year
>40
Additional Effect, 25-40
Additional Effect, <25

Figure 16

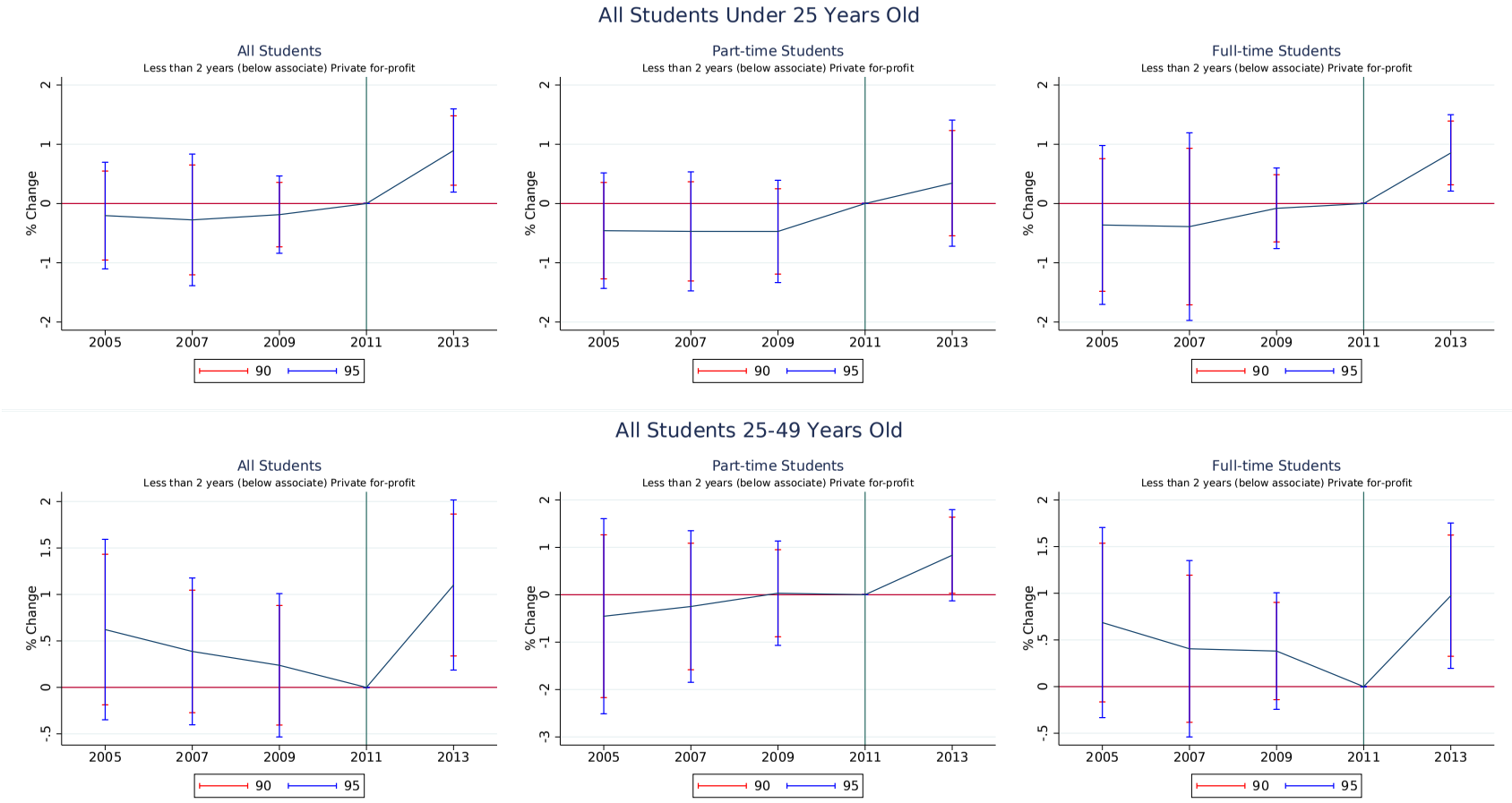

All Students Under 25 Years Old
All Students
Less than 2 years (below associate) Private for-profit
Part-time Students
Less than 2 years (below associate) Private for-profit
Full-time Students
Less than 2 years (below associate) Private for-profit
% Change
2005
2007
2009
2011
2013
90
95
All Students 25-49 Years Old
All Students
Less than 2 years (below associate) Private for-profit
Part-time Students
Less than 2 years (below associate) Private for-profit
Full-time Students
Less than 2 years (below associate) Private for-profit
% Change
2005
2007
2009
2011
2013
90
95

Figure 17

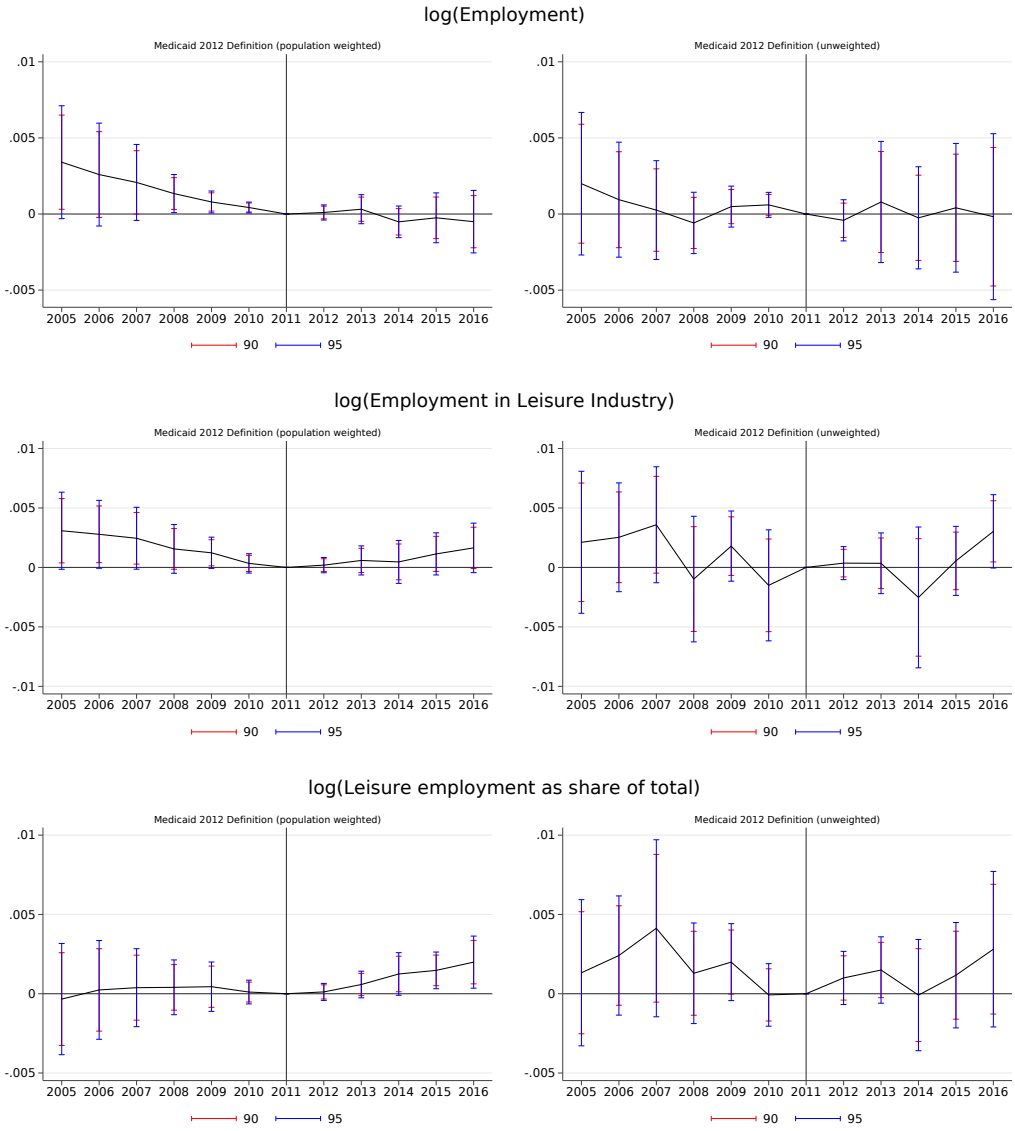

log(Employment)
Medicaid 2012 Definition (population weighted)
Medicaid 2012 Definition (unweighted)
log(Employment in Leisure Industry)
Medicaid 2012 Definition (population weighted)
Medicaid 2012 Definition (unweighted)
log(Leisure employment as share of total)
Medicaid 2012 Definition (population weighted)
Medicaid 2012 Definition (unweighted)
.01
.005
0
-.005
-.01
2005 2006 2007 2008 2009 2010 2011 2012 2013 2014 2015 2016
90
95

Figure 18: Degree Attainment

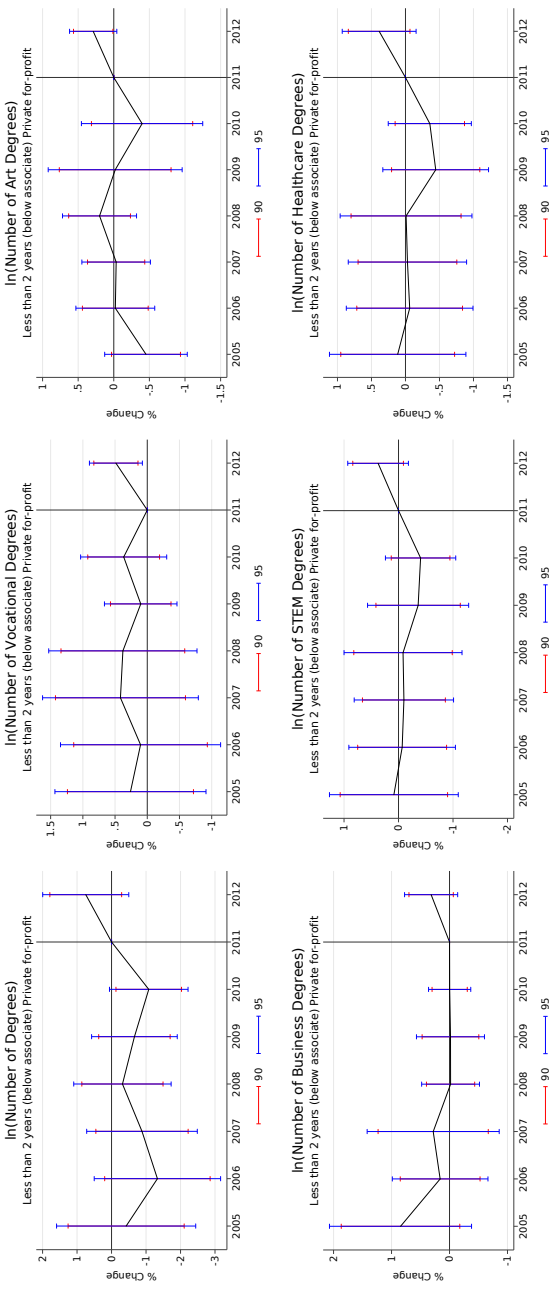


Figure 19: Normalizing Uninsurance (12-month Enrollment)

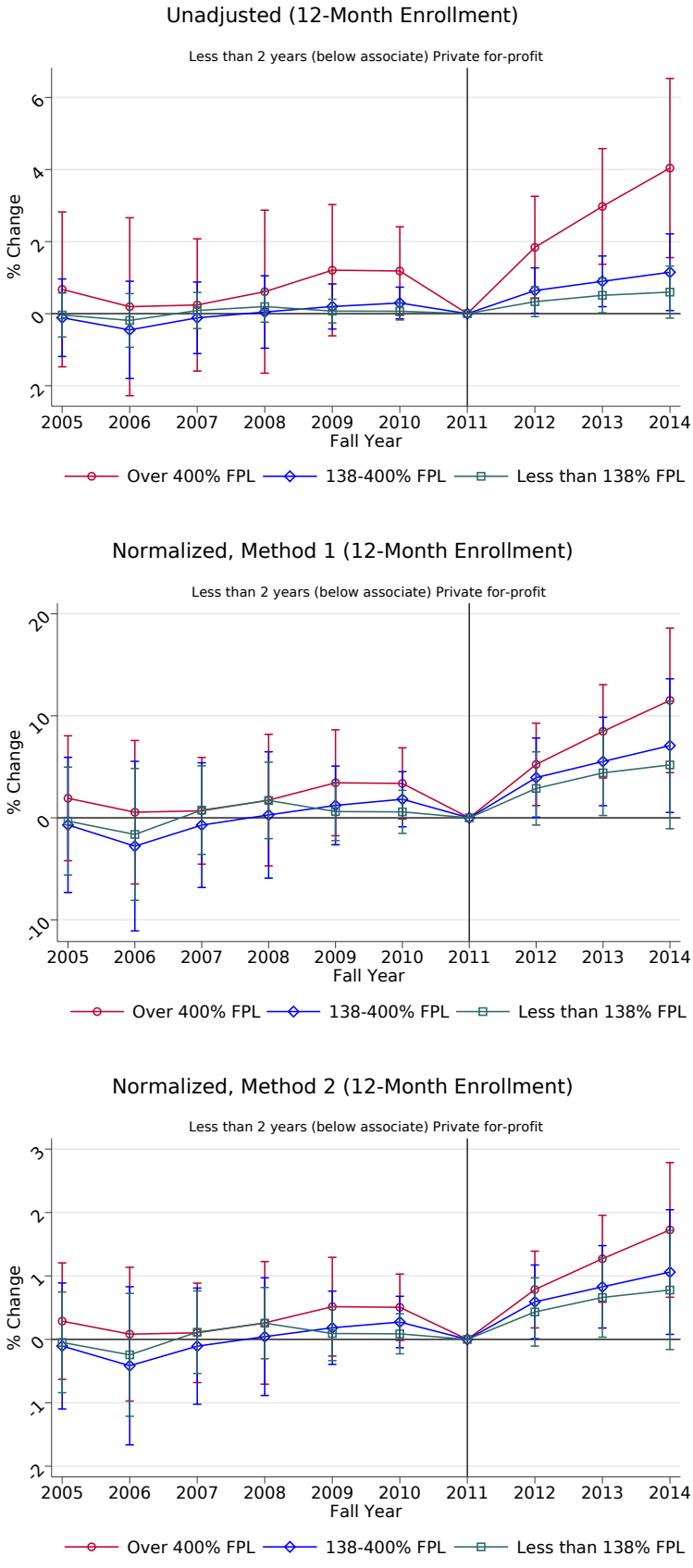


Figure 20

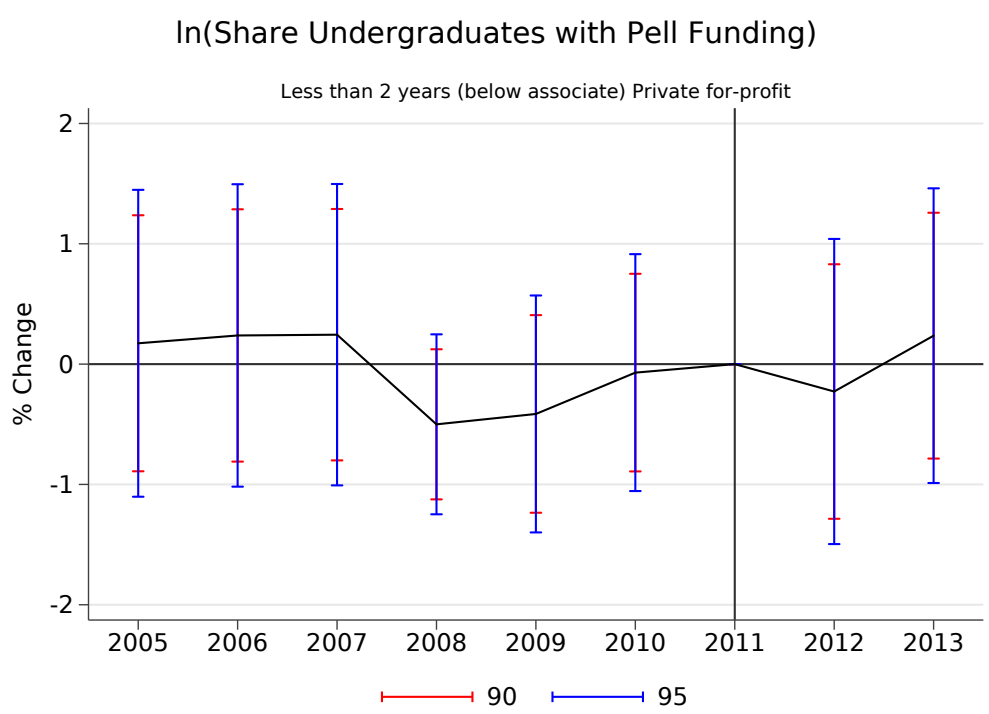
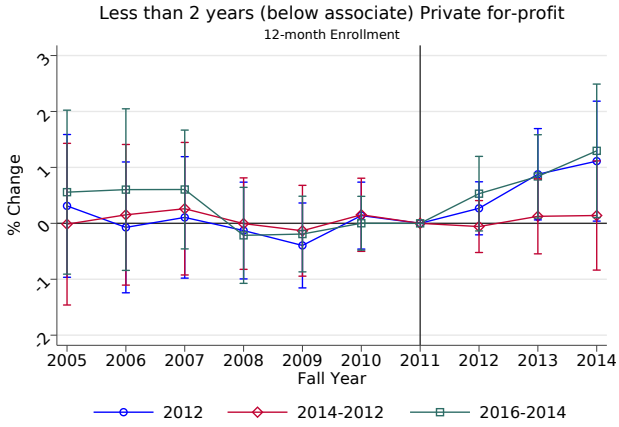
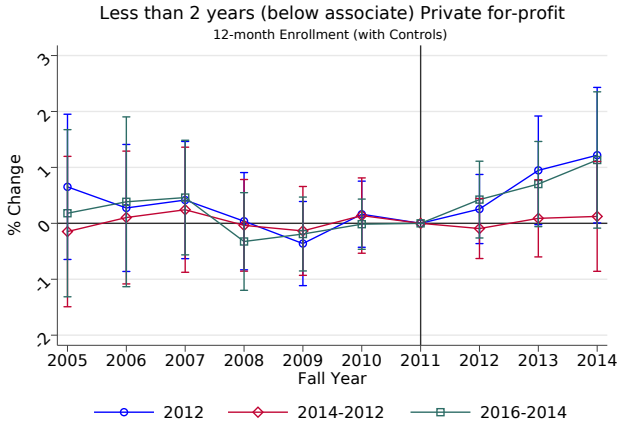
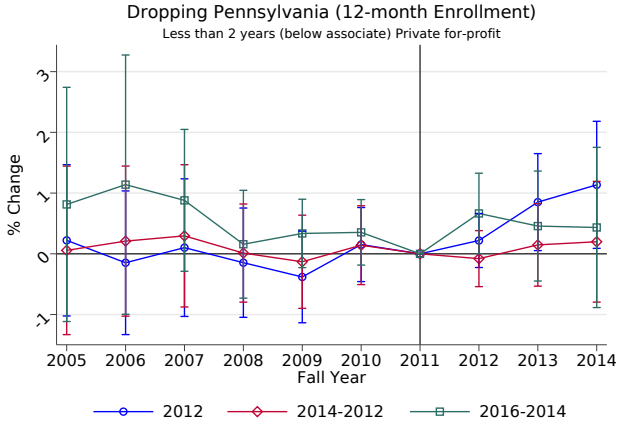
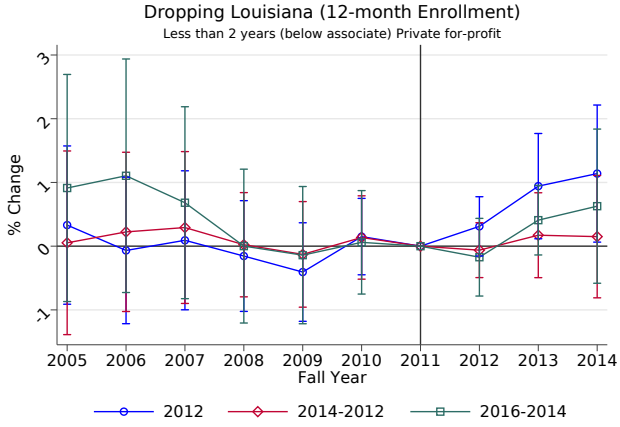

ln(Share Undergraduates with Pell Funding)
Less than 2 years (below associate) Private for-profit
% Change
2
1
0
-1
-2
2005
2006
2007
2008
2009
2010
2011
2012
2013
90
95

Figure 21


Less than 2 years (below associate) Private for-profit
12-month Enrollment
% Change
Fall Year
2012
2014-2012
2016-2014


Less than 2 years (below associate) Private for-profit
12-month Enrollment (with Controls)
% Change
Fall Year
2012
2014-2012
2016-2014


Dropping Pennsylvania (12-month Enrollment)
Less than 2 years (below associate) Private for-profit
% Change
Fall Year
2012
2014-2012
2016-2014


Dropping Louisiana (12-month Enrollment)
Less than 2 years (below associate) Private for-profit
% Change
Fall Year
2012
2014-2012
2016-2014

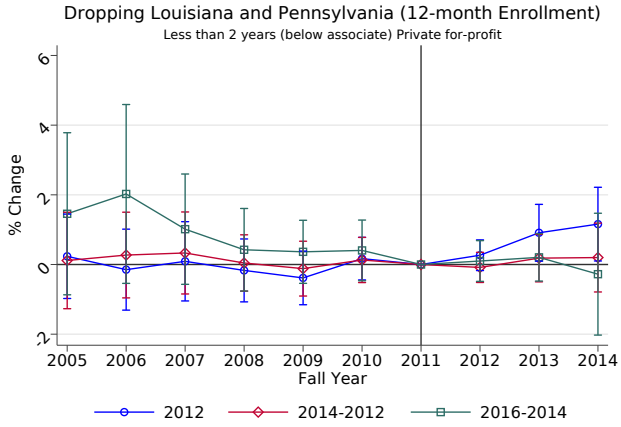

Dropping Louisiana and Pennsylvania (12-month Enrollment)
Less than 2 years (below associate) Private for-profit
% Change
Fall Year
2012
2014-2012
2016-2014

Figure 22: For-Profit Entry

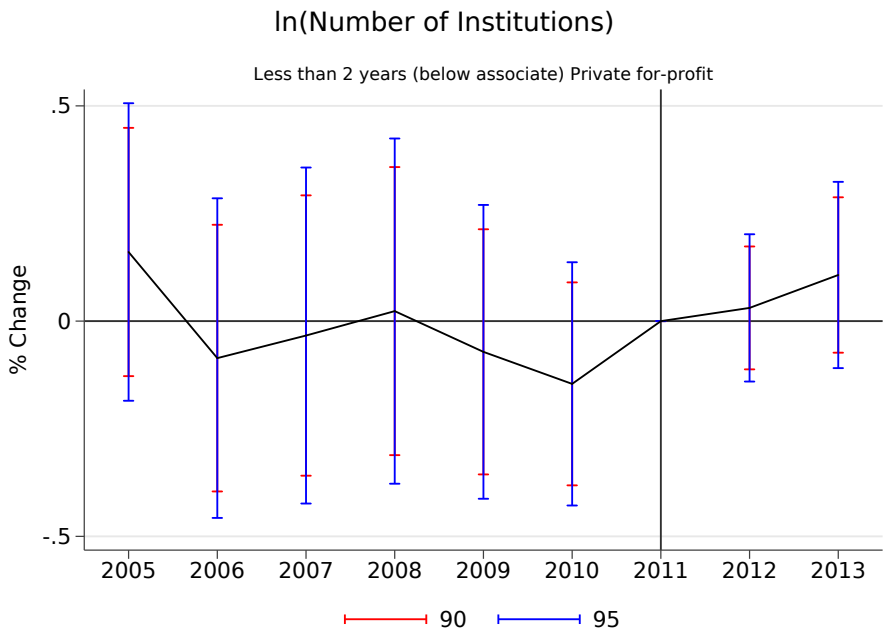
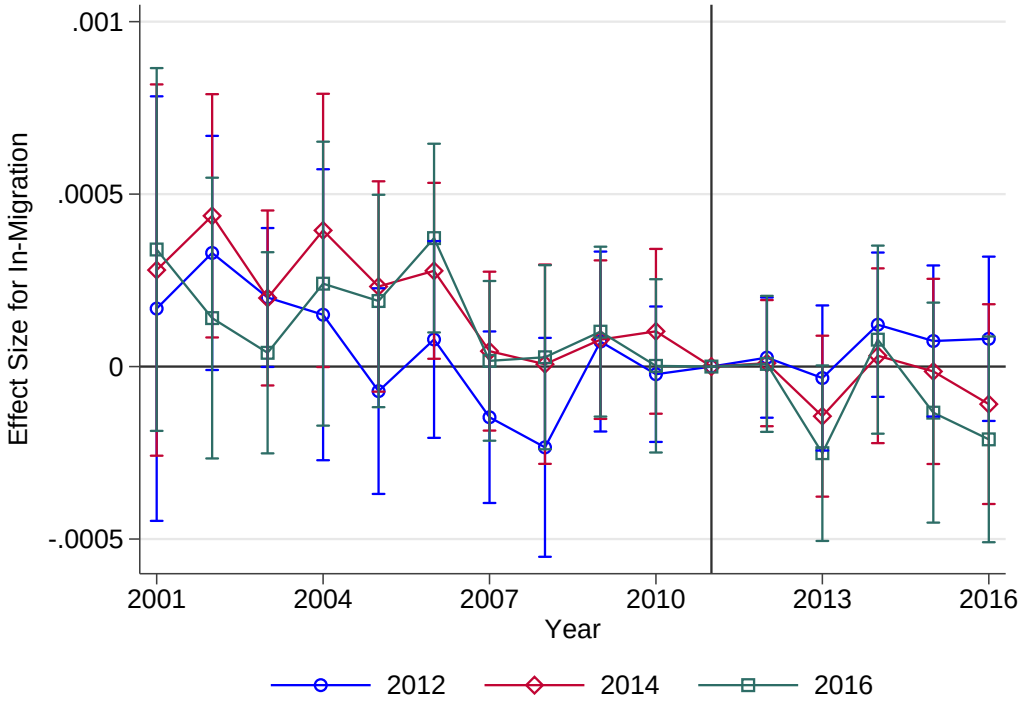


Figure 23

Effect on In-Migration



Effect on Out-Migration

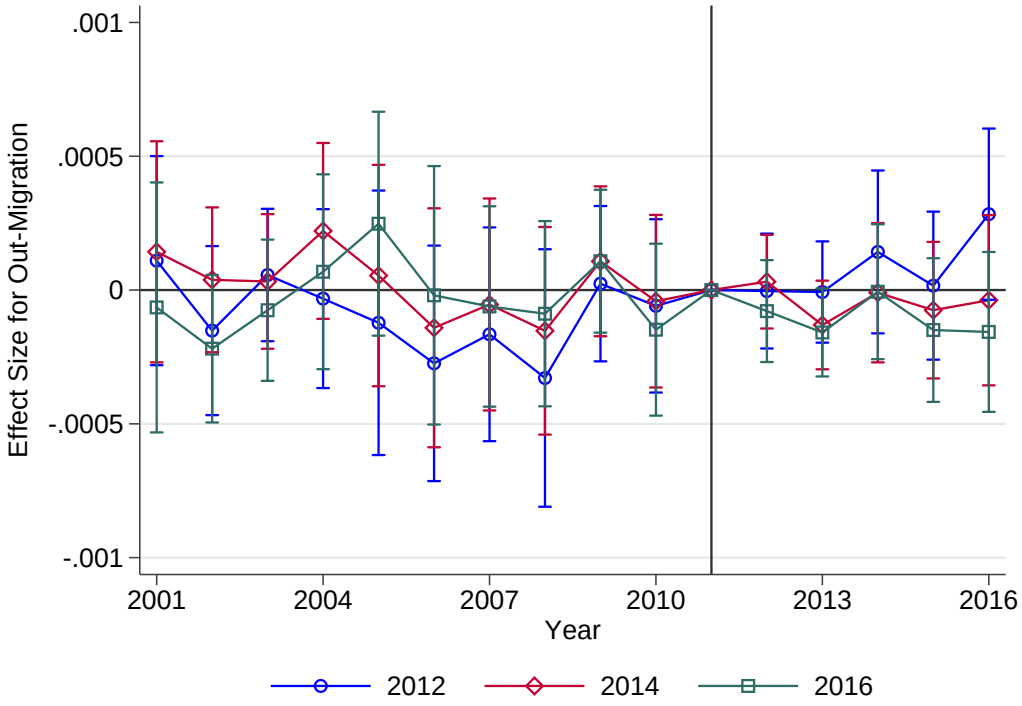
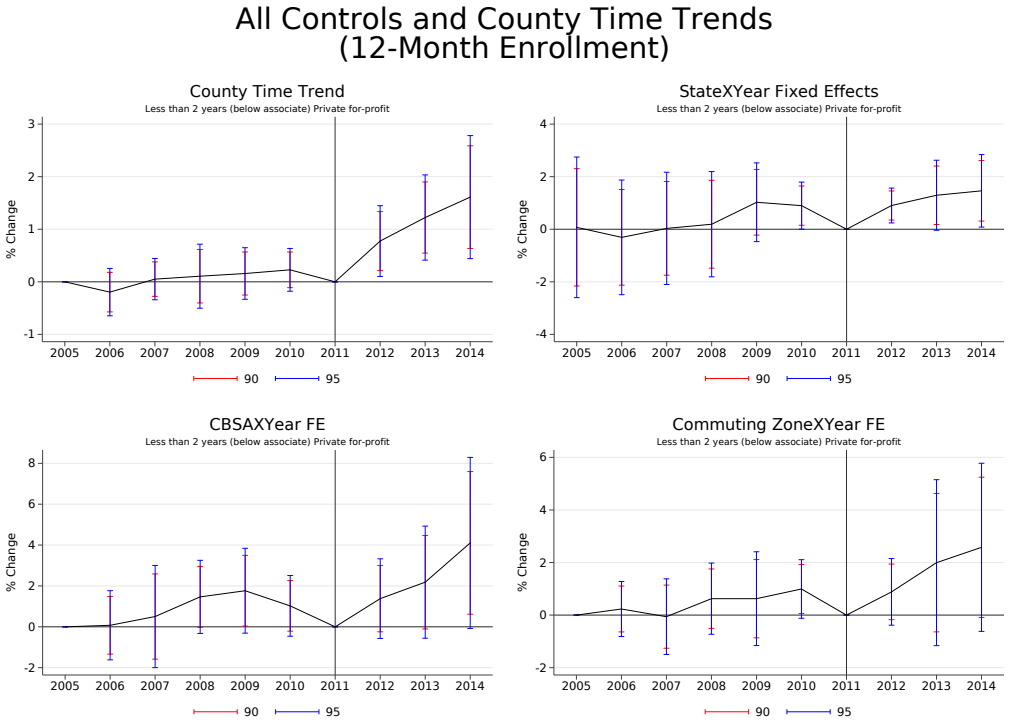


Figure 24

Panel A



Panel B

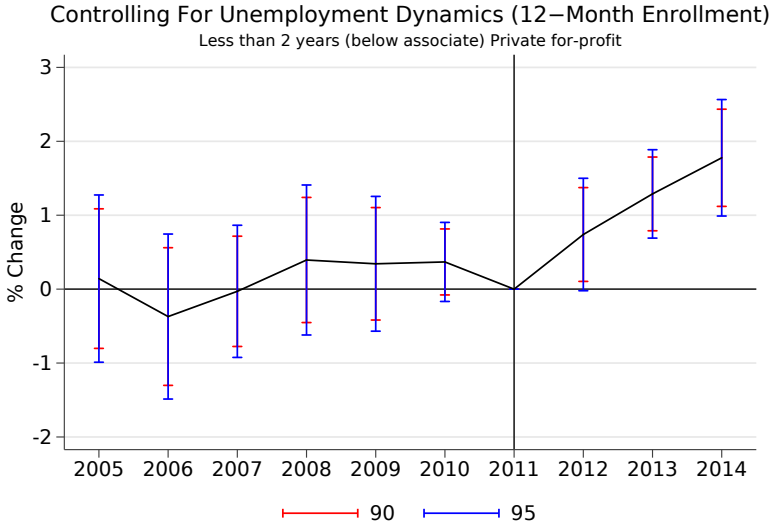


Figure 25

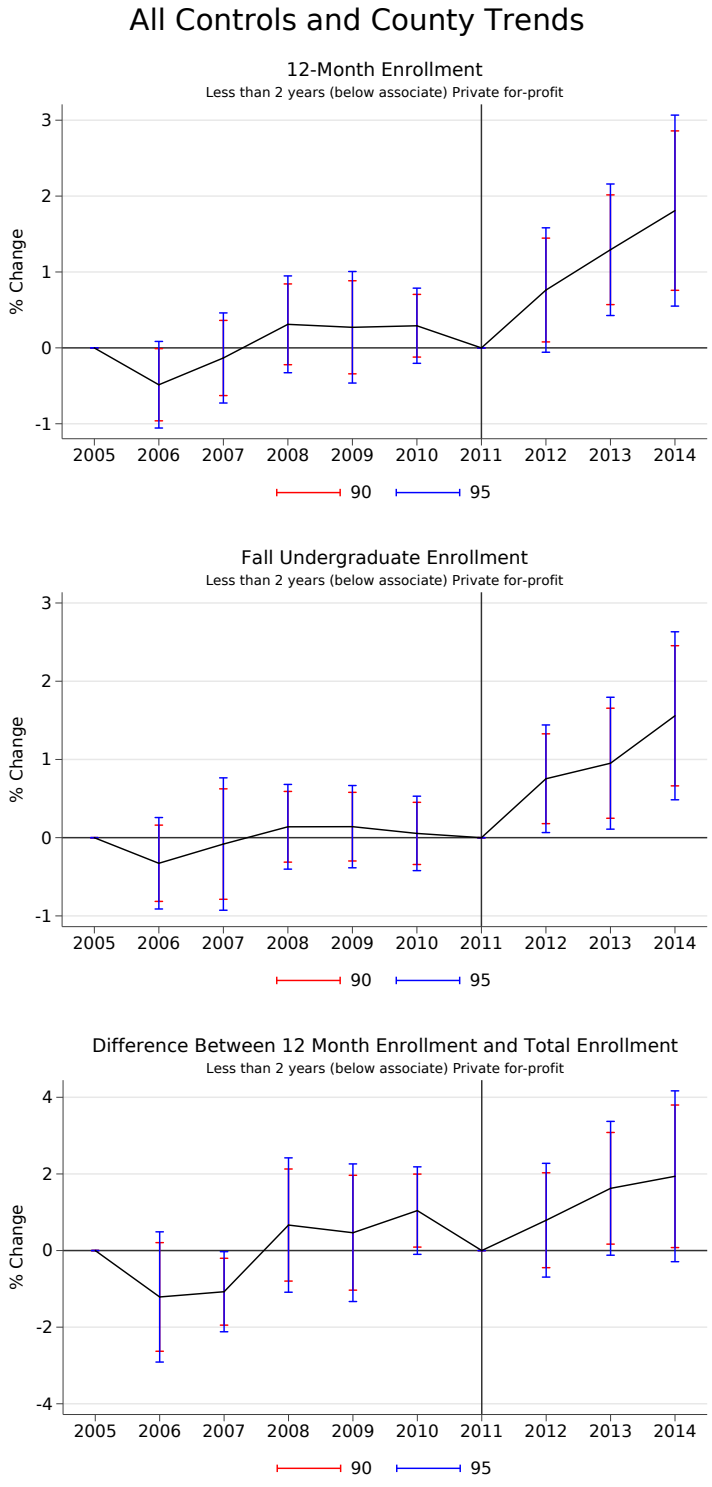

All Controls and County Trends
12-Month Enrollment
Less than 2 years (below associate) Private for-profit
% Change
3
2
1
0
-1
2005
2006
2007
2008
2009
2010
2011
2012
2013
2014
90
95
Fall Undergraduate Enrollment
Less than 2 years (below associate) Private for-profit
% Change
3
2
1
0
-1
2005
2006
2007
2008
2009
2010
2011
2012
2013
2014
90
95
Difference Between 12 Month Enrollment and Total Enrollment
Less than 2 years (below associate) Private for-profit
% Change
4
2
0
-2
-4
2005
2006
2007
2008
2009
2010
2011
2012
2013
2014
90
95

Table 1: Summary Statistics

Variable	N	mean	sd	25^{th} perc.	median	75^{th} perc.
Health Variables						
Percentage Uninsured, 2009	142380	20	6.3	16	20	25
Percentage Uninsured, 2009, Medicaid Expanding States	25,830	17.9	6.4	13	16.7	23.2
Expanded Medicaid, 2012	51	.27	.45	0	0	1
Expanded Medicaid, 2014	51	.49	.5	0	0	1
Expanded Medicaid, 2016	51	.63	.49	0	1	1
Gruber Simulated Medicaid Eligibility	51	.1	.12	0	.006	.23
Education Variables, Conditional on Any Enrollment						
Fall Enrollment	34837	4965	1.3e+04	264	1489	4999
12mo Enrollment	34607	6807	1.8e+04	409	2082	6924
Hispanic Students	32081	774	4281	15	70	338
White Students	34938	3230	6537	137	885	3420
African-American Students	33167	788	2270	27	136	634
Undergraduate Female Students	34750	2837	7169	172	898	2935
Undergraduate Male Students	34165	2177	5622	73	616	2115
Undergraduate Part Time Students	29213	2173	7675	89	509	1925
Undergraduate Full Time Students	34742	3152	7496	192	987	2962
Undergraduate Students Under 25	17374	3328	7967	121	917	3157
Full Time Undergraduate Students Under Age 25	17318	2479	5570	92	654	2218
Part Time Undergraduate Students Under Age 25	14422	1033	3892	37	207	900
Undergraduate Students Ages 25-49	17347	1436	4981	79	401	1346
Full Time Undergraduate Students Ages 25-49	17278	617	3124	54	215	617
Part Time Undergraduate Students Ages 25-49	14369	991	3334	41	239	842
Controls						
Population (in Thousands), 2005	142380	133	320	24	47	116
Delinquincies	133020	260	169	142	222	334
Unemployment Rate	142290	9.2	2.9	7.3	8.9	11

Source: Integrated Post Secondary Education Data System, Kaiser Family Foundation, and Census. Includes academic years 2005-2006 through 2014-2015, all institution types (conditional on any enrollment for enrollment variables), and, in the whole sample, 1582 counties. Enrollment figures by age group include only academic years for which the fall year is an odd number, since those are the only years in which institutions are required to report enrollment by age group.

Table 2: Summary Statistics By School Type

Variable	All	< 2-year	2-year	4-year	< 2-year FP	< 2-year NFP	< 2-year Pub.	2-year FP	2-year NFP	2-year Pub.	4-year FP	4-year NFP	4-year Pub.
							Conditional On Any Enrollment						
Fall Enrollment	4,965	446	5,262	7,292	543	213	279	1,046	391	7,791	4,552	4,357	11,613
	(12,633)	(1,147)	(15,249)	(12,662)	(1,295)	(345)	(854)	(2,003)	(515)	(18,645)	(18,149)	(7,973)	(12,973)
12mo Enrollment	6,807	758	8,148	8,948	939	355	439	1,715	555	12,017	7,314	5,168	13,746
	(18,396)	(2,026)	(23,624)	(16,805)	(2,263)	(548)	(1,583)	(3,265)	(782)	(28,877)	(28,390)	(9,545)	(15,682)
Hispanic Students	774	134	976	873	160	92	73	231	47	1,360	557	429	1,487
	(4,281)	(626)	(5,921)	(3,241)	(652)	(135)	(637)	(763)	(94)	(7,178)	(1,992)	(1,402)	(4,677)
White Students	3,230	179	2,802	5,380	200	61	162	437	220	4,194	2,182	3,678	8,586
	(6,537)	(296)	(5,335)	(8,426)	(326)	(158)	(232)	(718)	(295)	(6,269)	(9,226)	(7,106)	(8,404)
African-American Students	788	123	792	1,118	162	62	40	268	89	1,084	1,295	675	1,537
	(2,270)	(341)	(2,246)	(2,731)	(403)	(102)	(121)	(516)	(155)	(2,700)	(4,722)	(1,573)	(2,574)
Undergraduate Female Students	2,837	327	3,034	4,095	417	138	164	672	262	4,454	2,803	2,519	6,332
	(7,169)	(830)	(8,345)	(7,552)	(967)	(238)	(465)	(1,217)	(344)	(10,180)	(12,349)	(4,583)	(7,068)
Undergraduate Male Students	2,177	128	2,262	3,213	138	80	120	392	133	3,338	1,750	1,860	5,281
	(5,622)	(378)	(6,989)	(5,313)	(382)	(137)	(410)	(910)	(218)	(8,508)	(5,951)	(3,468)	(6,001)
Undergraduate Part Time Students	2,173	152	3,598	1,643	132	99	208	189	166	4,778	1,558	776	2,608
	(7,675)	(357)	(11,396)	(3,708)	(268)	(387)	(505)	(279)	(246)	(13,012)	(4,596)	(1,999)	(4,470)
Undergraduate Full Time Students	3,152	358	2,217	5,706	464	180	153	937	288	3,047	3,208	3,602	9,017
	(7,496)	(974)	(4,883)	(10,430)	(1,141)	(230)	(526)	(1,874)	(401)	(5,842)	(16,149)	(6,724)	(9,991)
							All Observations						
Fall Enrollment	1,215	73	1,508	2,063	174	8	38	249	30	4,245	617	1,629	3,945
	(6,603)	(494)	(8,502)	(7,494)	(776)	(78)	(328)	(1,074)	(177)	(14,298)	(6,859)	(5,310)	(9,350)
12mo Enrollment	1,654	123	2,317	2,523	299	13	59	405	42	6,505	981	1,925	4,662
	(9,528)	(864)	(13,124)	(9,789)	(1,349)	(125)	(598)	(1,745)	(261)	(22,073)	(10,688)	(6,339)	(11,214)
Hispanic Students	174	17	259	248	42	2.6	6.8	47	2.7	726	76	161	507
	(2,058)	(227)	(3,079)	(1,771)	(340)	(27)	(195)	(356)	(25)	(5,290)	(759)	(883)	(2,819)
White Students	792	29	802	1,547	63	2.2	22	104	17	2,284	302	1,409	2,930
	(3,524)	(136)	(3,122)	(5,132)	(206)	(32)	(102)	(396)	(1.0e+02)	(5,076)	(3,512)	(4,747)	(6,377)
African-American Students	183	18	215	318	46	1.8	4.6	57	5.7	582	177	254	523
	(1,145)	(136)	(1,222)	(1,541)	(228)	(20)	(43)	(262)	(45)	(2,051)	(1,800)	(1,019)	(1,668)
Undergraduate Female Students	692	53	869	1,155	133	5.1	22	160	20	2,427	380	934	2,151
	(3,746)	(357)	(4,672)	(4,414)	(581)	(53)	(179)	(659)	(118)	(7,834)	(4,646)	(3,045)	(5,095)
Undergraduate Male Students	522	20	639	908	41	2.9	16	89	10	1,818	237	694	1,794
	(2,907)	(156)	(3,851)	(3,174)	(217)	(30)	(154)	(464)	(69)	(6,496)	(2,270)	(2,302)	(4,300)
Undergraduate Part Time Students	446	15	873	449	27	1.4	17	26	8	2,585	182	283	882
	(3,585)	(121)	(5,821)	(2,072)	(131)	(47)	(156)	(123)	(65)	(9,862)	(1,649)	(1,264)	(2,878)
Undergraduate Full Time Students	769	58	635	1,614	148	6.6	20	223	22	1,660	435	1,345	3,063
	(3,942)	(414)	(2,799)	(6,114)	(679)	(56)	(199)	(997)	(135)	(4,571)	(6,044)	(4,464)	(7,221)
N	142380	47,460	47,460	47,460	5,028	582	2,113	3,760	1,213	8,618	2,144	5,910	5,374
Unique Institutions	8451	2481	2530	3120	306	114	2061	676	1681	763	1157	221	1152

Source: Integrated Post Secondary Education Data System. Includes academic years 2005-2006 through 2014-2015 and all institution types. Enrollment figures by age group include only academic years for which the fall year is an odd number, since those are the only years in which institutions are required to report enrollment by age group. Cells are means with corresponding standard deviations below in parentheses.

Table 3: Summary Statistics By School Type

Variable	All	< 2-year	2-year	4-year	< 2-year FP	< 2-year NFP	< 2-year Pub.	2-year FP	2-year NFP	2-year Pub.	4-year FP	4-year NFP	4-year Pub.
							Conditional On Any Enrollment						
Undergraduate Students Under 25	3,328	215	3,106	5,368	271	79	119	530	213	4,631	1,336	3,341	9,185
	(7,967)	(567)	(9,001)	(8,515)	(668)	(113)	(304)	(1,078)	(305)	(10,986)	(3,818)	(6,334)	(10,271)
Full Time Undergraduate Students Under Age 25	2,479	176	1,582	4,719	238	68	59	487	175	2,254	1,035	3,109	7,933
	(5,570)	(507)	(3,741)	(7,564)	(609)	(93)	(171)	(1,037)	(263)	(4,506)	(3,400)	(5,998)	(8,945)
Part Time Undergraduate Students Under Age 25	1,033	70	1,813	678	58	35	103	76	63	2,394	357	241	1,258
	(3,892)	(156)	(5,836)	(1,624)	(109)	(78)	(236)	(103)	(95)	(6,640)	(880)	(533)	(2,286)
Undergraduate Students Ages 25-49	1,436	205	1,871	1,708	250	120	121	481	164	2,712	2,896	859	2,176
	(4,981)	(541)	(5,361)	(5,822)	(614)	(247)	(384)	(909)	(236)	(6,549)	(13,215)	(2,020)	(3,163)
Full Time Undergraduate Students Ages 25-49	617	164	589	906	209	97	78	420	105	729	2,009	425	1,001
	(3,124)	(450)	(1,119)	(4,870)	(531)	(131)	(236)	(841)	(156)	(1,263)	(11,892)	(1,071)	(1,261)
Part Time Undergraduate Students Ages 25-49	991	74	1,522	836	73	72	76	107	98	2,001	1,042	450	1,180
	(3,334)	(193)	(4,736)	(2,070)	(152)	(340)	(243)	(148)	(165)	(5,395)	(3,329)	(1,331)	(2,100)
							All Observations						
Undergraduate Students Under 25	812	35	891	1,511	86	2.9	16	125	17	2,531	177	1,248	3,107
	(4,187)	(243)	(5,021)	(5,122)	(398)	(26)	(119)	(571)	(102)	(8,442)	(1,461)	(4,195)	(7,387)
Full Time Undergraduate Students Under Age 25	603	29	453	1,327	75	2.5	7.9	115	14	1,232	137	1,161	2,684
	(2,946)	(214)	(2,127)	(4,538)	(359)	(22)	(66)	(545)	(87)	(3,515)	(1,283)	(3,961)	(6,415)
Part Time Undergraduate Students Under Age 25	209	6.6	438	184	11	.43	8.3	10	3	1,299	40	88	423
	(1,800)	(52)	(2,970)	(897)	(53)	(9.5)	(72)	(46)	(25)	(5,035)	(316)	(342)	(1,453)
Undergraduate Students Ages 25-49	350	33	536	480	79	4.5	16	113	13	1,482	384	320	736
	(2,535)	(232)	(2,991)	(3,181)	(365)	(53)	(147)	(486)	(79)	(5,025)	(4,912)	(1,301)	(2,108)
Full Time Undergraduate Students Ages 25-49	150	27	168	254	66	3.5	10	99	8	398	267	158	338
	(1,561)	(191)	(655)	(2,612)	(313)	(31)	(90)	(445)	(51)	(1,002)	(4,384)	(684)	(873)
Part Time Undergraduate Students Ages 25-49	200	6.9	368	226	14	.94	6	15	4.6	1,084	118	162	397
	(1,550)	(63)	(2,417)	(1,138)	(72)	(39)	(71)	(66)	(41)	(4,093)	(1,166)	(827)	(1,340)
N	71,190	23,730	23,730	23,730	1,502	103	624	1,074	375	4,283	894	2,851	2,663
Unique Institutions	8291	2386	2511	3117	303	110	1973	682	1684	751	1157	221	1133

Source: Integrated Post Secondary Education Data System. Includes academic years 2005-2006 through 2014-2015 and all institution types. Enrollment figures by age group include only academic years for which the fall year is an odd number, since those are the only years in which institutions are required to report enrollment by age group. Cells are means with corresponding standard deviations below in parentheses. Statistics using by age enrollment are included only for odd years, as reporting is not required in even years.

Table 4: Effects on Baseline Enrollment

Medicaid 2012 Specification (Fall Enrollment)

	(1) 2-year Private for-profit	(2) 2-year Public	(3) <2-year Private for-profit	(4) <2-year Public
γ	-0.188	-0.362	0.943**	-0.190
	(0.340)	(0.548)	(0.324)	(0.377)
N	15820	15820	15820	15820
N Clusters	51	51	51	51

Medicaid 2012 Specification (12-Month Enrollment)

	(1) 2-year Private for-profit	(2) 2-year Public	(3) <2-year Private for-profit	(4) <2-year Public
γ	-0.467	-0.347	1.136***	-0.242
	(0.394)	(0.523)	(0.308)	(0.344)
N	15820	15820	15820	15820
N Clusters	51	51	51	51

Medicaid 2012 Specification (Difference Between 12â'Month Enrollment and Fall Enrollment)

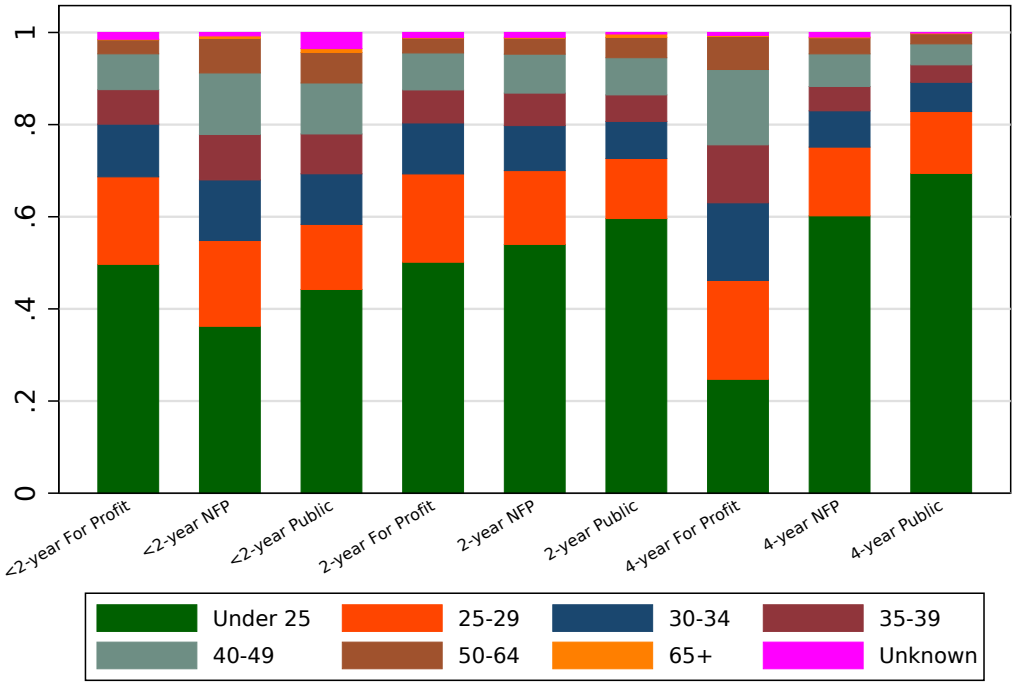
	(1) 2-year Private for-profit	(2) 2-year Public	(3) <2-year Private for-profit	(4) <2-year Public
γ	-0.508	-0.517	1.306**	0.056
	(0.578)	(0.578)	(0.461)	(0.381)
N	15820	15820	15820	15820
N Clusters	51	51	51	51

Table 5: My caption

Measure	Mean (MOE/value)	Std. Dev. (MOE/value)	Var(Value)
All	0.088344	0.016707	44.35839
Less than 138% FPL	0.124944	0.026596	75.55706
Less than 200% FPL	0.105666	0.021749	65.44018
Less than 250% FPL	0.097863	0.01951	60.34343
Less than 400% FPL	0.089149	0.017088	51.43775

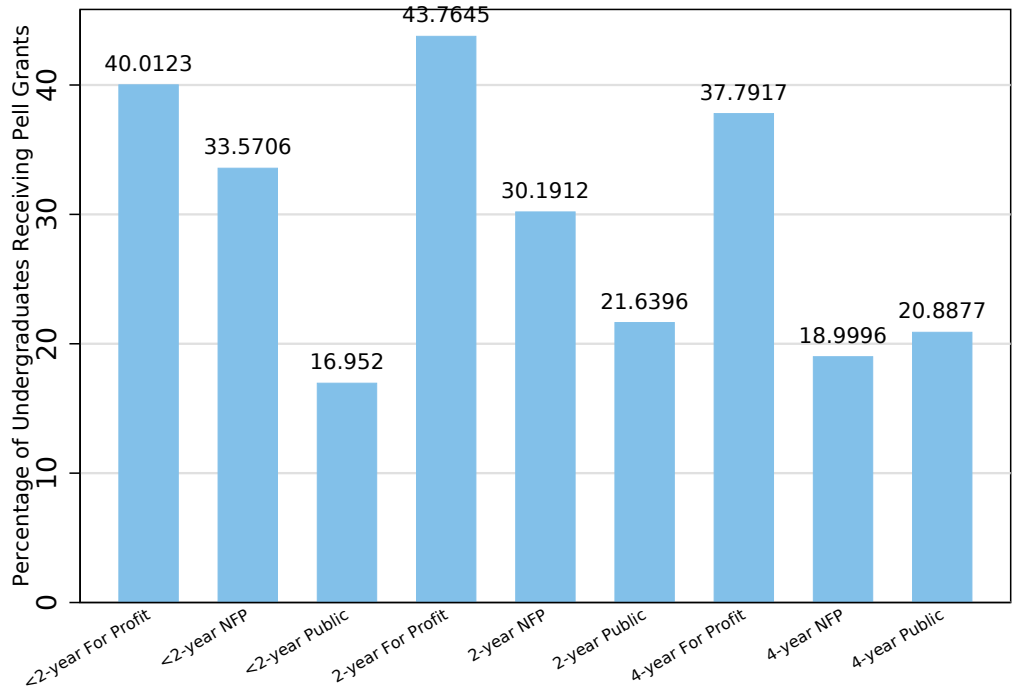
8 Appendix

Figure A.1: Age Distribution of Students by School Type



Source: IPEDS. Uses all data collected in odd fall years from 2005-2013.

Figure A.2: Pell Recipients by School Type



Source: IPEDS. Uses all data collected in academic years 2005-2006 through 2014-2015.