Three Essays on U.S. Social Policy’s Impact on the Human Capital Development of Young Adults At-Risk of Poverty

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Abstract:

Social welfare programs and policies can have a variety of anticipated and unexpected effects on the human capital investments of young adults at-risk of living in poverty in the United States. My dissertation investigates how three large-scale public programs – means-tested, cash welfare (e.g., Aid to Families with Dependent Children and Temporary Aid to Needy Families), Medicaid health insurance for children, and the Social Security Student Benefit Program – affected the educational attainment and work experience of vulnerable young adults.

In the first chapter, I examine how public policies encouraging labor force participation by low-skilled single mothers during welfare reform unintentionally led to labor supply declines by young, less-educated single males. While the labor market woes of low-skilled male workers over the past several decades have been well documented, the academic literature on the identification of causal factors leading to the decline in labor force participation (LFP) by young, low-skilled males is relatively scant. In this paper, I use a fixed-effects, instrumental variable research design to exploit the timing and characteristics of welfare reform policies to explore whether policies targeted to increase LFP rates for low-skilled single mothers inadvertently led to labor force exit of young, low-skilled males. Using data from the Current Population Survey and the series of work inducements enacted by states throughout the 1990s as a source of exogenous variation in a quasi-experimental design, I find that a welfare-reform-generated 10 percentage point (pp) increase in LFP for low-skilled single mothers resulted in a statistically significant 2.6 pp decline in LFP rates by young, low-skilled single males. Furthermore, after a series of alternative model specifications and robustness checks, I find that this result is driven

entirely by the decline in labor supply for white males; young black males and other groups of workers appear to be unaffected by the labor supply response of less-educated single mothers to welfare reform.

The second essay in my dissertation studies one of the long-term effects of the child Medicaid health insurance expansions. Prompted by the legislative decision to decouple child Medicaid benefits from cash welfare receipt, the number of young children qualifying for public health insurance grew markedly throughout the 1980s and early 1990s. This chapter extends the academic literature examining early childhood investments and longer-term human capital measures by exploring whether public health insurance expansions to low-income children led to a greater number of high school completers in the 2000s. Using a technique developed by Currie and Gruber (1994, 1996) to simulate the generosity of a state's Medicaid program during early childhood, I find large and significant effects on completion rates, which are examined in two forms: the dropout rate and the traditional four-year high school graduation rate. Intent-to-treat estimates range from a 1.9 to 2.5 pp decrease in the dropout rate for each 10 pp increase in early childhood years covered by the state-level Medicaid program. The same 10 percentage point increase in child Medicaid program generosity reveals increases of 1.0 to 1.3 pp when applied to four-year graduation rates, indicating that dropout reductions are propelled by increases in traditional diplomas. In addition, results appear to be driven by Hispanic and white students, the two groups which experienced the greatest within-group eligibility increases due to the decoupling of child Medicaid from the AFDC cash assistance program.

My final dissertation chapter investigates how a particular college fund guarantee affected achievements in higher education. Utilizing data from the National Longitudinal Survey of Youth (1979) and a difference-in-differences model, this work re-examines the impact of the

Social Security Student Benefits Program (SSSBP) on post-secondary educational attainment, a topic first studied by Dynarski (2003). By exploiting a larger panel of data and exploring degree attainment at various ages, my coauthor and I find that disadvantaged youth potentially qualifying for SSSBP funds – e.g., those losing a father before they turned 18 – were over 20 pp more likely to obtain higher education degrees beyond their high school diploma than similar students who would have qualified for benefits, but-for the program's termination in May 1982. Initial program impacts – i.e., those by age 23 – show an increase in Associate's degree attainment. As these respondents age, however, many go on to obtain four year degrees. Impacts are large and statistically significant, and suggestive that social programs seeking to reduce the financial costs of Associate's degrees – such as the one announced by President Obama in his 2015 State of the Union Address – could be well-targeted.

THREE ESSAYS ON
U.S. SOCIAL POLICY'S IMPACT ON THE
HUMAN CAPITAL DEVELOPMENT OF
YOUNG ADULTS AT-RISK OF POVERTY

by

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DISSERTATION

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I am either incredibly lucky or simply likeable. Regardless of the causal mechanism, I have benefitted tremendously from many wonderful mentors, advocates, and friends throughout the years, as well as from a family which has supported me unconditionally on my non-traditional route to date. Simply put, I am the sum of these relationships and am perpetually indebted to those who have supported me on this long journey towards PhD completion. This section formally acknowledges many of those who made this degree possible, while fully conceding that a few words on paper cannot fully capture the magnitude of these influences.

Similar to the issues that I study, I cannot consider my time in Syracuse in isolation. My success at the Maxwell School is the culmination of years of constant support, which date back to childhood. Success in higher education was far from certain for me as a child, a period when I was much more adept at disrupting classrooms than in actually learning something. Fortunately, I was blessed with a number of primary school teachers in Groton, NY, who patiently worked to channel my unbridled energy towards more constructive purposes. While teachers were an important part of my human capital accumulation function, the strongest influence in facilitating my transition from class-clown to a learned student was – without a doubt – my parents. My mother's mixture of drive, confidence, and desire for accountability has fundamentally shaped me as an individual. The strong work ethic which she and my father instilled in me at an early age provided the backbone of many of my future successes later in life. Constant support throughout the various chapters of my life from both of my parents, as well as from my sister Rhiannon, my Grammy Groves, and my Aunt Carol and Uncle Will, has made it much easier to dream big and to achieve many of my long-term goals. My family has made life much more

meaningful and interesting, and has always provided a much needed reprieve from the solitude of research. So, to them, I owe many thanks.

After leaving my childhood home, my strongest professional influences were economists. Economic training and mentorship permeates not only my academic work but, for better or worse, thinking in my personal life. At Binghamton University, Professor Clifford Kern spent countless hours imbuing me with microeconomic theory and assisting me with my senior honors thesis. His introductory economics course was fundamentally the reason I chose economics as a major, which sent me out along a much different career path than originally envisioned at the start of my freshman year. Moreover, his strong letter of recommendation helped me get my first "adult" job, at the Department of Justice (DOJ), Antitrust Division. While at DOJ, two staff economists – Ronald Drennan and Kenneth Danger – took me under their wings and helped lay a very solid foundation for my future work in SAS programming and statistical modeling. I was then able to parlay this skillset into a private sector consulting firm for not one, but two tours with Microeconomic Consulting and Research Associates (MiCRA), Inc. In the cumulative six plus years I spent with this DC-based firm – which also graciously funded my M.A. in Applied Economics at Johns Hopkins – my understanding of research designs, policy analysis/the public policy process, and statistical programming grew exponentially. I am certain that this degree would have taken much longer had it not been for the mentoring and support of a number of economists at the firm, including Rick Warren-Boulton, Steve Silberman, Hal Van Gieson, Michael Pelcovits, and Renee Duplantis.

Correspondingly, my time at the Maxwell School was replete with supporters. I would like to start with my excellent advisor Len Lopoo. He deserves a paragraph of his own for helping me transition from a private sector consultant (with, perhaps, a few bad habits) to a potential academic scholar. His foresight and vision helped push my work to greater heights, especially once I figured out a set of questions which were actually both (1) interesting and (2) answerable. His insightful feedback and accessibility was much appreciated and was the primary factor in me finishing this degree within 4 years. Moreover, Len embodies my upper bound as a professor: his statistics and program evaluation classes are the right mixture of interesting/challenging, and his lesson planning is first-rate. Finally, Len was – generally speaking – a wonderful role model for me and also represents the best that I could hope for in a work/family life balance. It was an absolute pleasure learning so much from him.

Two other groups at Syracuse deserve my gratitude. The first are professors. My committee members – which include Bob Bifulco, Len Burman, Sarah Hamersma, and Doug Wolf – provided helpful feedback which served to strengthen the analyses presented in this dissertation. Janet Wilmoth also did a great job serving as my oral chair for my dissertation defense. In addition, Sarah went above and beyond her committee duties by including me on a team of researchers investigating issues related to food insecurity and helping me to establish contact with the Institute for Research on Poverty – which is where I am headed next. While technically not a professor at the time, but acting very professorial, Kerri Raissian served as a wonderful "academic big sister" by constantly supporting me throughout my first two years and helping me through some dark early days. Unquestionably, she helped me retain my sanity by serving as a voice of reason and, for that, I am forever indebted. Finally, a countless number of Maxwell professors assisted me throughout this program, either through my coursework or through departmental programs such as Focus on the Faculty. Upon arrival, my expectations for professors teaching in the country's top MPA program were high. I am happy to report that these notions were vastly exceeded.

The second set of Syracusans are my peers. I am thankful to have spent so much time with my PA PhD cohort – Pengju Zhang, Tian Tang, Harrish Jagannath, and Dae Woo Kim – during our early stages. Moreover, the broader set of PA PhD students served to make our seminars enlightening, and our social events helped us to push through those long, dark upstate NY winters. My most influential and supportive peer group are my fellow “BoyBayers” in the Center for Policy Research (CPR). While sharing an office with 5 other guys initially sounded terrible, this was definitely not the case. To Pallab Ghosh, Judson Murchie, Edoardo Rainone, and, again, Pengju Zhang, thank you for the countless hours of stimulating conversations, the research support, and for the after-hours company – both inside and out of CPR.

Finally, this section cannot conclude without recognizing three other research-related items. First, I was very fortunate to be housed within the Center for Policy Research within the Maxwell School. The staff are top-notch. In particular, Peggy Austin, Kelly Bogart, and Candi Patterson and all assisted greatly in my research, whether it was to help make my work more readable, to help resolve computing issues, or to help me find external funding sources. Secondly, I am indebted to Professor Susan Dynarski for sharing the Stata code to her 2003 American Economic Review paper. This greatly simplified the replication portion of the third chapter and allowed Len and I to concentrate on more interesting, second-order questions. As the last formal thank you, I acknowledge that most of the Current Population Survey (CPS) data used in my first two chapters was obtained via IPUMS. Efforts conducted at the University of Minnesota to increase the accessibility of the CPS data and greatly decreased the amount of data cleaning required to produce these analyses. For saving me weeks of programming time, I conclude by gratefully recognizing IPUMS.

All errors in this dissertation began as mine. And they will forever remain mine alone.

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Chapter 1

Better Things to Do?
Welfare Reform and Labor Force Exit by Young, Low-Skilled Single Males

1.1. Introduction

A number of authors have noted the decline in labor force participation (LFP) rates for young, less-educated men in the United States since the early 1980s (Holzer et al. 2005; Holzer and Offner 2006; Blank 2009); however, few studies have presented compelling causal evidence to explain these trends. This paper seeks to fill this gap in the literature by exploiting the timing and characteristics of welfare reform within a fixed-effects, instrumental variable (FE-IV) design to examine whether policies enacted under state waivers and Temporary Assistance for Needy Families (TANF) implementation during the course of the 1990s led to labor force exit by young, low-skilled single males. This paper contributes to the welfare reform literature by showing that the subsidies and inducements for work embedded in reform policies targeting low-skilled single mothers led to an unintended decline in the labor supply of young, low-skilled single males.

After President Clinton's vow to "end welfare as we know it," states encouraged low-skilled single women with children to enter the labor force through their enactment of work requirements, time limits, and work incentives. The results were quite striking: U.S. caseloads fell by 56.5% from 1994 to 2000 and LFP rates for single mothers with children under the age of 18 increased from approximately 68% in 1994 to almost 78% in 2000 (Blank 2002). Given the comparatively weaker labor market attachment of young, less-educated men relative to other groups of working men, I investigate whether a significant number of males left the legitimate labor market due to government reform policies and, moreover, whether two occurrences – the increase in labor supply by single mothers and the decrease in labor supply for young, low-skilled single males – can be linked causally.

The theoretical underpinning for this analysis of low-skilled labor supply is the assumption that individuals respond to work incentives in two ways: (1) directly through the labor market, and (2) indirectly via responses to changes in behavior by groups targeted by public policy. Given that single mothers with education levels at or below a high school level are those most likely to apply for welfare benefits, I examine how changes in their labor supply attributable to welfare reform impacted the LFP rates for single males with equivalent levels of education. Typically these men were excluded from the package of welfare reform work incentives – which include cash benefits and much higher Earned Income Tax Credits (EITC), as well as potential childcare credits and health benefits. Conceptualizing this bundle as a direct subsidy to single mothers, welfare reform policies greatly fostered entrance into the labor market for low-skilled single mothers – which prompted a sharp increase in their LFP – while tightening the labor market and reducing the relative work incentives for young, low-skilled single male workers who competed directly with many of these women for low-wage, entry level positions.

While social policy scholars have devoted an enormous amount of attention to the legislative changes of the 1990s, the question of whether welfare reform policies accelerated labor supply declines by low-skilled male labor has been understudied. Using Bartik (2002) and Blank and Gelbach (2006) as guides, I employ two-stage least squares (2SLS) regression analysis, a wider set of instrumental variables characterizing changes to state-level Aid to Families with Dependent Children (AFDC) programs, and data from the Current Population Survey (CPS) to estimate the labor supply response of young, low-skilled male labor to welfare reform.

Labor supply declines among young, low-skilled males prompted by welfare reform are robust across a number of specifications. 2SLS models reveal a roughly 2.6 percentage point (pp) decrease in LFP rates for single males aged 16 to 29 for each 10 percentage point increase in LFP for single mothers. Estimates of declines in labor force participation are typically larger and more precise for younger, single white males – roughly 3.5 pp – while young black males appear to be unaffected by the labor supply response of less-educated single mothers to welfare reform. This last conclusion is consistent with the work of Holzer et al. (2005) who attribute much of the decline in labor supply for this group to increased incarceration rates and stronger child support enforcement. Additionally, there is no consistent evidence of labor supply declines for lower-skilled single males aged 30 to 49 who typically have higher levels of work experience and operate at different margins. Nor is there proof of a labor supply response by young, low-skilled single women without children. These tests indicate that the indirect effects of welfare reform induced declines in LFP were concentrated among young, single male workers.

While the methodology presented in this analysis cannot identify the exact mechanism of these labor supply declines – such as intra-household labor reallocation, increases in the number of discouraged workers, or reservation wages exceeding the market price for low-skilled labor – this paper presents the first robust estimates of young, male labor supply declines stemming from welfare reform policies, findings which are both statistically and economically significant.

1.2. Background and Previous Research

The 1990s were a remarkable period for the transition of individuals – mostly single mothers – from welfare to work. This decade witnessed the end of welfare programs as an entitlement, which had engendered and prolonged a long-term, and often intergenerational, transmission of poverty for a subset of individuals (Moffitt 1992; Moffitt 2002; DeParle 2004). Welfare became a program that was time-limited, full of sanctions and requirements, and, at its core, sought to eliminate many of the disincentives of the previous program by "making work pay" (Ellwood 1988; Danziger et al. 2002; DeParle 2004). While many aspects of welfare reform have been carefully investigated, see Blank (2002) and Grogger and Karoly (2005) for thorough reviews of this literature, the possibility that government welfare reform policies inadvertently led to labor force exit of another vulnerable population – namely young, low-skilled single males[1] – has largely been ignored.

Policymakers should be concerned with the labor supply responses of young, low-skilled single males because of their several decade decline in LFP[2,3] and the strong statistical relationship between less-educated males and outcomes generally detrimental to a society. Young males who exit the legitimate labor market are at an increased risk for a number of socially undesirable outcomes including higher probabilities of incarceration and delinquency (Blanchflower and Freeman 2000; Harlow 2003; Western and Pettit 2010; Bloom and Haskins 2010; Smeeding et al. 2011) and decreased suitability for marriage (McLaughlin and Lichter 1997; Edin and Lein 1997; Edin and Kefalas 2005; Cherlin 2010), the latter which has been linked to the decline in the traditional nuclear family within low-income communities (Wilson

[1] In this analysis, low-skilled workers are defined by those individuals with an education level of high-school or less. Furthermore, the phrases "low-skilled" and "less-educated" will be used interchangeably.

[2] As is common practice in this literature, I concentrate on labor force participation in this paper rather than employment for the following reason: LFP is arguably a more accurate depiction of labor supply because it captures the intent to provide labor. Employment, on the other hand, can be based upon a number of factors outside of the individual's control, especially the demand for labor.

[3] The general decline in LFP rates for low-skilled males since the early 1980s has been documented very thoroughly by Holzer and various colleagues. In Holzer and Offner (2006), they report declines in labor supply for young, less-educated white males from approximately 92% in 1979 to roughly 87% in 2000. Correspondingly, rates for black males have dropped from roughly 82% to 70% over the same time period. In Holzer et al. (2005), the authors report that at least half of the decline in employment among less-educated black males can be attributed to increases in incarceration rates and stronger child support enforcement laws.

1987; Edin and Kefalas 2005; Cherlin 2010). Consequently, young males who are no longer enrolled in school nor looking for work can create negative externalities not only for their families and local neighborhoods, but for the broader society as well.[4]

Two notable studies have investigated the possibility of adverse effects from welfare reform on male labor supply. Blank and Gelbach (2006) use data from the CPS, a variety of empirical tests to examine the substitutability of less-educated males and females within the labor market, and do not find consistent evidence of male labor supply crowd-out from welfare reform. This paper builds on their work in two ways. Rather than examining changes in male LFP using female LFP as a control in their initial modeling,[5] I use instrumental variables to uncover the exogenous increase in female labor supply stemming from welfare reform. While Blank and Gelbach use welfare reform policy implementation as instruments in their reduced-form specifications, their use of a single indicator variable for each construct ignores the large degree of heterogeneity in AFDC waiver and TANF programs across the states. I improve on these identification limitations through my use of a fixed-effects, instrumental variable research design with a wider set of instruments not examined by the authors. This FE-IV approach allows me to better exploit the variation in work incentives created for low-skilled workers across the different state plans, increases the precision of my estimates, and, furthermore, allows me to test the strength and validity of my instruments.

The second empirical guide is taken from Bartik (2002). In this work, he uses instrumental variables to control for welfare caseloads across the states and estimates the labor

[4] Recent estimates of the scale of the illicit drug trade is the United States indicate that it is a highly lucrative industry which is estimated to produce up to $150 billion in revenue each year (Bagley 2012; UNODC 2012). Given this scope, it does not seem unreasonable to contend that low-skilled males are more likely than other groups to enter these illegal professions given that they have fewer employment options.

[5] Without instruments, female LFP would be endogenous because factors affecting female LFP, e.g., a strong economy, also affect males.

market spillover effects of welfare reform. Using simulation, Bartik estimates the displacement and wage elasticities for less-educated workers given the increase in supply of less-educated workers, under the assumption that welfare reform led to an influx of 1.4 million individuals into the low-skilled labor market. Moreover, with his 2SLS model, Bartik concludes that welfare reforms, which led directly to decreased caseloads, may have led to employment losses for less-educated males and reduced the wages for single mothers and male high school dropouts. [6] While Bartik's modeling concentrates on employment rates and wages across all ages of workers, this paper is focused on estimating the impact of welfare reform on the labor supply decision of young, less-educated males.

Before proceeding to the theoretical framework used in this analysis, it is important to explain why young, single male labor supply *should not be directly affected by welfare policy changes*. As noted by Blank and Gelbach (2006), only about 10 percent of welfare recipients in 1990 were male and received benefits as either a head of family or as a single parent. Moreover, the authors report that 9% of all less-educated female workers reported receiving benefits in 1996, while only 1% of less-educated men did. Thus, males – especially young single men – should not directly qualify for government benefits before or after the series of welfare reform efforts of the 1990s. Furthermore, while a subset of unmarried young, low-income males may be part of informal households' choosing to reallocate labor in response to market incentives, the change in female labor supply within these loosely structured family units can also be perceived as a mediating variable – i.e., that males formulate their labor supply decisions based upon the choices of their cohabiting partner and not the policies of the government itself.

[6] Bartik's finding that real wages may have decreased during the welfare reform period for select categories of low-skilled workers will be a useful piece in my theoretical framework.

1.3. Theoretical Framework: Welfare Reform and Labor Supply

Two of the explicit, political goals of welfare reform were to end welfare as an entitlement and to make work pay for low-income individuals (Blank 2002; DeParle 2004). In this section, I outline three indirect mechanisms which can explain labor force exit by young, low-skilled males stemming from welfare reform policies, which include (1) intra-household labor reallocation, (2) an increase in the number of discouraged workers due to a tightened labor market and an "envy story" rooted in behavioral economics, and (3) a reservation wage story, whereby the market price for low-skilled labor fell below the reservation wage for many low-skilled single males.

All three of these mechanisms are founded upon the premise that welfare reform did not affect males directly, rather their behavior was influenced by the behavioral responses of those low-skilled single women with children who previously would have qualified for government benefits under the old AFDC entitlement system. In other words, the direction of the causal relationship for labor supply responses to welfare reform for the subset of individuals explored in this analysis is conceptualized as follows:

Note that for this instrumental variable approach to be valid, I am explicitly claiming that there was no direct impact of welfare reform on the labor supply by young, low-skilled single males and that the impact was channeled – or meditated – through the direct response by women potentially qualifying for welfare benefits. The remainder of this section will outline some potential mechanisms for these conjectures.

Public policies enacted at the state-level during welfare reform utilized a variety of mechanisms to increase the labor supply of single mothers. While some methods, such as time limits, family caps, and sanctions are, arguably, more indirect ways of influencing supply, the EITC, child care subsidies, and health care programs (e.g., Medicaid or CHIP) can be perceived as direct subsidies to working individuals with children under 18. Moreover, and critically, if work requirements are an explicit part of the welfare program, then benefit receipt is conditional on LFP and individuals must seek work to retain their monthly cash transfers from the government. Given this package of incentives seeking to increase labor supply, the post-welfare LFP decision by low-skilled single mothers should be a function of (1) potential income from work, (2) welfare cash benefits, (3) available EITC, (4) childcare credits, and (5) government provided health insurance.

On the other hand, work incentives for young, low-skilled single males would only be a function of their potential income from work, though a small EITC would be available in some years. Given that other factors which determine LFP are likely to affect both groups of workers, these policy differences suggest that a substantial number of single mothers receive benefits from LFP in excess of what young males competing for the same types of low-skilled, entry-level jobs would receive. For an informal household choosing to maximize income and benefits based upon the set of options provided to them, it is conceivable that the males would choose to exit the formal labor market to concentrate on more domestic duties while the corresponding female would choose to participate in the labor market.

Furthermore – and importantly for the broader set of young, low-skilled single males who are not cohabitating – to the extent that Bartik’s findings are valid and that real wages declined for low-skilled workers due to the influx of labor supply induced by welfare reform, then the importance of government transfers and subsidies increase in the work/no-work decision. Moreover, young, low-skilled males could be disproportionately affected by this trend in targeted supports. Under this set of circumstances, exit of low-skilled male labor facilitated by welfare reform could stem from one of two primary scenarios: via (1) the reservation wage or (2) due to an increase in the number of discouraged workers. Under the former, the influx of low-skilled female labor would drive down the effective market wage for overlapping industries[7] and encourage lower-skilled males with higher reservation wages to exit the market, especially if these individuals had better paying options in the informal or black market.

The second scenario occurs when the entrance of low-skilled single mothers – who have more incentives for work and, consequently, may be better motivated employees – leads to a disproportionate hiring of and retaining of these workers, which causes some unemployed young males to become discouraged and exit the formal labor force. Additionally, one could easily envision an "envy story" routed in behavioral economics whereby young males in overlapping industries learn about these target supports and realize that their effective wages are much lower than their female counterparts due to the bundle of goods offered by the government. Under this story, males may be more inclined to participate in black or informal markets to receive better effective wages, which leads to more discouraged workers.

Unfortunately, data and methodology used in this analysis cannot distinguish between these purported explanations. Future work seeks to disentangle these mechanisms.

[7] While a number of scholars have noted their skepticism regarding whether low-skilled men and women compete in the same labor markets (Blank 2002; Blank and Gelbach 2006), there is seemingly enough overlap in some low-skilled sectors such as fast food or custodial services, and security and retail jobs for this supposition. For example, Card and Krueger (1994) claim that fast-food franchises are a leading employer of low-wage workers and, seemingly, low-skilled workers of either gender seem equally qualified for these entry level positions.

1.4. Empirical Strategy

To isolate the causal impact of increased labor supply by single mothers stemming from welfare reform on young, low-skilled male labor, I use a fixed-effect, instrumental variable research design. Under this approach, the dependent variable in the outcome equation is the labor force participation rate for a given group of males (e.g., low-skilled single males, aged 16 to 29) at a given point in time (e.g., 1996, quarter 1). A welfare reform induced reduction in young, single male labor supply would be observed when an exogenous increase in the LFP rates for females leads to a decrease in the labor supply for males.[8] A strength of this FE-IV research design is that it can control for time-invariant factors affecting male and female labor supply across both states and time, as well as isolating the indirect effect of an exogenous change in female labor supply increase due to welfare reform as channeled through a single mediating variable.

The general estimation strategy used in this analysis can be written as:

(1) $(LFP_{males})_{syq} = \alpha + \beta(LFP_{females})_{syq} + \gamma\mathbf{X}_{syq} + \delta_s + \zeta_y + \psi_q + \varepsilon_{syq}$

where: an observation is defined by a combination of state (s), year (y), and quarter (q);
X is a vector of control variables containing measures of economic growth, state minimum wage rates, child support enforcement strictness, wages for low-skilled males, and male incarceration rates;
$\boldsymbol{\delta}$, $\boldsymbol{\zeta}$, and $\boldsymbol{\psi}$ are fixed effects for state, year, and quarter, respectively and,
$\boldsymbol{\varepsilon}$ is the robust standard error.

In this specification, the state, year, and quarter fixed-effects[9] are used to detrend the labor supply variables across both states and time. All regressions are weighted by the corresponding number of males residing in the state (s) in year (y) and quarter (q).

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[8] A positive relationship could indicate peer effects, which occurs when the welfare-reform work inducements create positive spillovers in the form of increased LFP for applicable males residing within the household or the community.

[9] Quarterly variables account for seasonality in the LFP rates for young males. The CPS considers university bound males on summer break – in other words those between their last year of high school and first year of college – as

As specified above, $LFP_{females}$ is most likely endogenous. The rationale is simple: LFP of men and women are influenced by many of the same economic factors, and failure to account for simultaneously determined LFP rates in an OLS specification leads to biased coefficients on the primary variable of interest, i.e., $\partial LFP_{males} / \partial LFP_{females}$, which is represented by the coefficient β in equation 1. To derive unbiased coefficients, I require a set of instrumental variables for $LFP_{females}$, which will allow me to use the exogenous portion of the increase in female LFP *stemming from welfare reform* to estimate the potential for labor force exit of young, single low-skilled male labor.

Under the classical definition of an instrumental variable, I need variables which are (1) uncorrelated with LFP_{males} conditional on other controls within the model (i.e., the exclusion restriction) but (2) explain changes in Female LFP (i.e., the relevance criteria). The welfare reform policies of the 1990s conveniently serve this purpose: legislation and new programs provided a series of incentives for welfare recipients – who were by and large single mothers – to enter the labor force, whereas these policies were designed to have little or no direct impact on young, single males simply because they did not qualify for these programs. Consequently, the attributes, timing, and variation of the state waivers and TANF programs can be used as identifying instruments for changes in female LFP rates. Furthermore, within this setup, I can explicitly test the relevance and exogeneity of the instrumental variables, the latter which will mitigate concerns over simultaneity in the second stage equation.

The first-stage equation, which models the trends in LFP for low-skilled single mothers between the ages of 16 and 44, is written as:

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potential labor force participants. This influx of short-term labor drives down the LFP rate for single males between the ages of 16 to 29 during the summer months.

$$(2) \qquad (\mathrm{LFP}_{\mathrm{females}})_{syq} = \omega + \chi\mathbf{X}_{syq} + \rho\mathbf{Z}_{syq} + \varphi(\text{Welfare Reform})_{syq} + \tau\left(\frac{\text{Maximum Cash Benefits}}{\text{Maximum EITC}}\right)_{syq} + \eta(\text{Welfare Reform X Cash Benefits })_{syq} + \kappa_s + \nu_y + \varsigma_q + \mu_{syq}$$

Where:

X is the same set of controls from equation 1;
Z is a vector of attributes of state-level welfare reform policies (e.g. waiver type) which are in effect in year y and in quarter q;
Welfare Reform is an indicator which is turned on after the first waiver implementation or the effective TANF program date;
Maximum Cash Benefits are the maximum state AFDC or TANF cash benefits for a family of 3 at a particular point in time;
Maximum EITC is the maximum state and federal earned income tax credit which could be earned in a state and year;
Welfare Reform X Cash Benefits allows for a change in LFP incentives when cash benefits become directly linked to labor supply;
κ, **ν**, and **ς** are vectors of fixed-effects for states, years, and quarters, respectively, and
μ is the first-stage error term.

In the above equation, the **Z** vector contains the state-level characteristics of waiver programs, such as the presence of work requirements, time limits, work incentives, and personal responsibility policies.[10] Since welfare reform is defined as a single indicator variable, it measures the average impact of waiver and/or TANF implementation, after accounting for the specific elements of these programs.

The EITC is now the largest cash transfer program in the United States (Moffitt 2007). Thus, in the construction of the maximum cash benefits to maximum EITC variable, I seek to more precisely capture the implicit tradeoff between cash transfers from no work (maximum AFDC/TANF cash benefits) and work (maximum state and federal EITC), which in some states – like New York – changes well before they implement either a welfare waiver or their state-

[10] Other instruments were considered in this analysis but were excluded due to their weak predictive power.

level TANF program.[11] Online Appendix A shows that the ratio between cash benefits and the total EITC is decreasing in all states dramatically over time. A large cash benefit to EITC ratio reveals that the gains to work are low if one is solely seeking to maximize the amount of transfers received from the government and bases the “work” or “no work” decision on the amount of cash transfers received from the government. A higher ratio should translate into lower LFP rates for qualifying individuals and a ratio below 1.00 indicates that a family of three can earn more money in transfers via the EITC than through cash benefits and no work. Thus, *a priori*, we should expect that the coefficient on this variable (τ) will be negative.

Another important component of these models is the interaction between cash benefits and the timing of welfare reform. This coefficient is constructed to capture the very different behavioral responses facilitated by welfare reform. In a pre-period of welfare entitlement, large cash benefits would be considered a work disincentive which would, in turn, decrease LFP rates for low-skilled single mothers. However, in a post-reform period where work requirements were now a critical portion of aid receipt, seeking work was now part of retaining benefits. So, as cash benefits grow, single mothers are greatly incentivized to increase their LFP rates in order to retain benefits. This interaction seeks to capture this complex, changing relationship between benefits and LFP and we should expect that η is positive.

Finally, under this fixed-effects framework, federal policies such as the Child Tax Credit, as well as the individual impacts of TANF programs once all states have adopted the new system, cannot be uniquely identified because they lack cross-sectional variation at a given point

[11] More specifically, New York implemented their TANF program in November 1997. However, the LFP rates for single mothers increased markedly before this point due, presumably, to the large increases in the federal EITC which started in 1994.

in time. Stated another way, the year fixed-effects subsume any variable for which the value is the same for all states in a particular time period.

1.5. Data Sources and Descriptive Statistics

All LFP variables used in this analysis were derived from monthly data collected by the Current Population Survey.[12] Within their nationally representative, rotating sample design, the CPS samples approximately 50,000 housing units each month across the United States. Despite this seemingly broad scope, the number of observations in some subgroups (such as black men between the ages of 16 and 29, living in Montana) can be extremely limited, so I aggregate data to the quarterly level to increase the precision of the estimates. Other important notes regarding the LFP calculations: (1) all individuals below the age of 25 who are enrolled in school or university full-time are omitted[13] and (2) the CPS excludes institutionalized individuals from their sampling, so these people cannot be included in the estimates. This latter issue is important because – to the extent that incarceration rates are increasing over time within a given state and subpopulation – the LFP rates derived are an upper-bound.

Data range from 1989 to 2002, which yields approximately five years of pre-data before the large increase in the maximum EITC and the initial mass of states implementing waivers in 1994, as well as roughly 5 years of data for the period after the last state implemented its TANF program.[14] Moreover, I use quarterly data as the unit of analysis to more precisely control for the timing of the welfare waiver or the state's TANF program implementation. Finally, it is important to reiterate that my analysis is based upon individuals with a completed education

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[12] The vast majority of CPS data used in this analysis was taken from the IPUMS-CPS databased compiled at the University of Minnesota. See www.ipums.org for more details.
[13] This is a common practice in the literature – see Holzer et al. (2005) and Holzer and Offner (2006).
[14] These periods are identified with vertical lines in the forthcoming graphs.

level of high school or less. While I am, admittedly, interested in a more refined group – high school dropouts – the number of cases reported in the CPS data are not sufficient to create robust estimates at a quarterly level.

Figure 1 displays the LFP rates for single males aged 16-29 and 30-49, and single mothers aged 16-44. These categories are used in the empirical modeling and were selected for the following reasons: 16-29 captures the range for “younger” workers who have relatively fewer years of work experience and who could be competing with single mothers for low-skilled or entry level positions. Moreover, the age range of 16 to 29 was chosen to facilitate more precise estimates of subgroups in smaller states, while the age range of 30 to 49 isolates workers with longer work histories and, presumably, have more consistent labor market attachment for individuals choosing to remain in the labor force. For females, 16 to 44 was chosen as the age range because welfare participation and qualification cannot be identified in the monthly CPS data but women of childbearing ages with low levels of education and children below the age of 18 are those most likely to apply for, and receive, welfare benefits.[15]

Returning to the graph, Figure 1 shows the aggregated trends for the United States.[16] Broadly speaking, the U.S. witnessed relatively flat LFP rates for young less-educated males in the earlier period, followed by a slight decline in LFP during welfare reform, and then another stabilization. While these trends foreshadow, at an aggregate level, the potential for only a small level of labor supply decline for young males stemming from welfare reform policies, this paper exploits state-level trends to derive more precise estimates of the hypothesized effect. Furthermore, for older, less-educated single males, rates are fairly consistent over time (hovering

[15] Upcoming modeling is not sensitive to the choice of using single mothers between the ages of 16 and 44. Single mothers between 16 and 30 were also examined and very similar estimates were obtained.
[16] As mentioned earlier, it is very important to control for the seasonality of LFP for young, single males given the influx of university-bound males during the summer months. Thus, the graph is seasonally-adjusted.

around 82%), while the LFP rates for single mothers across the U.S. increases dramatically from 1994 to 1999 as reported by Blank (2002).

Figure 2 contains trends in LFP rates by race which are similar to the aggregated trends presented in Figure 1. Again, the potential for labor supply declines appears to be relatively small when examined at the U.S. level. However, this aggregation masks a large degree in potential heterogeneity in response to the state-level policies which is critical for identification in this paper. Stated another way, a negative labor supply response can still be identified if states experiencing the greatest increases in LFP by low-skilled single mothers also witness the greatest decline in labor supply by young, low-skilled single males. The forthcoming empirical models will explicitly test this proposition.

Before proceeding to estimation, it is important to outline the instrumental and controls variables used in this analysis. Some important notes regarding the instruments:[17] the dates for the first major waivers and TANF implementation were obtained from the Department of Health and Human Services, and there was a wide degree of heterogeneity in the implementation of the waivers and the new state-level TANF programs. Using information provided by Ziliak et al. (2000), I isolated whether the state-level waiver incorporated work requirements, time limits, work incentives, or responsibility clauses[18] within their reform efforts. As with implementation there was a wide range of within- and across-state variation in these variables. Finally, the maximum cash benefits for a family of three under the state-level AFDC and TANF programs

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[17] A number of other characteristics of welfare reform were also analyzed but did not make it into the final first-stage regression models because of their weak predicative power, which violates the relevance criteria of an instrumental variable. Examined in this analysis but not shown are TANF attributes regarding the strictness of sanctions and time limits (Pavetti and Bloom 2001), state diversion policies under TANF (Urban Institute, Welfare Rules Database), and states with child care fee waivers available through the Child Care Development Fund (Blau 2003).

[18] While the first three are rather self-explanatory, the personal responsibility clauses include restrictions on benefits for increasing the family size (i.e., family caps), as well as regular school attendance and health check-ups for existing children.

and the maximum EITC – which includes both state and federal benefits – were taken from a comprehensive database complied by the University of Kentucky's Center for Poverty Research and, like all monetary variables in this analysis, were standardized to 2002 dollars.

Other state-level control variables used in the FE-IV models include (1) the percentage growth in gross state product, (2) the effective state minimum wage, and (3) a child support enforcement (CSE) index, (4) a lagged measure of the average weekly earnings for low-skilled males aged 16 to 29, working full time, and (5) incarceration rates by race and ethnic group. The first two covariates account for factors impacting labor market conditions for low-skilled workers. The third variable seeks to capture the differential behavioral responses by low-income parents not residing within the same household, and is also required because the enactment of CSE policies are correlated with welfare reform. While stricter child support enforcement may increase household income and make it less likely for single mothers to seek employment, it may also drive many low-income males away from the formal labor market – and towards informal opportunities – because of the relatively high marginal tax rates (Holtzer et al. 2005). Thus, the child support enforcement index used in this analysis was adopted from Huang, et al. (2002) and updated to fit the range in this series. This variable ranges from 0 to 8 with a higher number indicating the presence of more state programs to enforce child support payments.

Average weekly earnings for low-skilled males aged 16 to 29 working full time seeks to control for potentially omitted factors which have contributed to the well documented decline in wages for this particular subset of workers. Wages are estimated by state using the March CPS earnings data and two techniques are used to account for simultaneity and sampling issues in the construction of this variable. To address the concern that LFP rates and wages are jointly determined, earnings data are lagged by two years. Furthermore, to avoid identifying estimates based upon sampling variation stemming from the estimation of wages for sub-populations within a given state and March CPS, estimates are smoothed using a four year moving average. These adjustments create a measure which can capture important factors affecting male LFP rates, which may be also correlated with the timing of welfare reform policies.

Finally, incarceration rates by race and ethnic group are constructed similar to Holzer et al. (2005) using National Prisoner Statistics from the Bureau of Justice Statistics and population data from the Surveillance, Epidemiology, and End Results Program. Estimates represent the fraction of the adult population incarcerated at a particular point in time within a given state. This value is lagged by three years to reflect the fact that the average sentence during this period was roughly three years (Holzer et al., 2005) and, thus, this variable is an estimate of the reentrance of former prisoners into a local economy.

Table 2 presents the unweighted summary statistics for all variables utilized in the empirical models. The average LFP rate for all young, single males (16-29) is 0.852 and all single males (30-49) is approximately 0.82, and both are estimated with a relatively wide range of roughly 0.50 to 1.00. As indicated by the statistics under the Min and Max columns, I occasionally derive LFP rates of either 0 or 100%, which is reflective of measurement error due to the CPS sampling design and its inability to always reach select subpopulations. Regressions are weighted by the number of corresponding males in each category in each quarter and bias from mismeasurement should be attenuated if there is classic measurement error.

Other notes: the unweighted average LFP rates for single mothers is 0.695, which is much lower over the range of data than the rates for single males, though we know from the graphical analysis that it is increasing markedly over time. Additionally, a little more than half (54%) of the observations fall in either the post welfare waiver or TANF implementation period for a given state, which is indicated by the average of the Welfare Reform (Waiver or TANF) dummy variable. Summary statistics for the AFDC waiver attributes can be interpreted in a similar manner: the mean indicates the total fraction of the unweighted sample which is affected by that variable.

1.6. Empirical Findings

To explicate the findings, I start with the first-stage models as they are the key to any causal link between welfare reform, low-skilled single mother LFP, and young, low-skilled male labor supply. In Table 3, the middle column for each set of regression models contains the coefficients for the first stage models which estimate LFP rates for low-skilled single mothers aged 16 to 44. For the sake of clarity, I offer some brief commentary on the reported findings for a solitary case: all single males.

As displayed by the first-stage coefficients for all young, low-skilled single males in Table 3, a large portion of the within-state variation in labor supply by low-skilled single mothers, approximately 59%, can be explained by the first-stage models. Given that marginal effects are derived from several variables and my goal is simply to get one coefficient correct – the impact of the plausibly exogenous increase in LFP rates for low-skilled single mothers on the labor supply decisions of single, less-educated males – I bypass an interpretation of individual coefficients and comment on groups of covariates.[19] As displayed, variations in policies implemented under the state-level waiver programs appear to explain a significant share of the variation in female LFP during their period of enactment, all else equal. Moreover, the trio of Welfare Reform (Waiver or TANF), Max Cash Benefits to Max EITC credit, and Cash Benefits

[19] Note that the natural log has been applied to several control variables. This helps to account for any potential nonlinearities associated with the cash variables and to facilitate easier interpretation of these coefficients.

X Welfare Reform are all highly statistically significant and add a large amount of explanatory power to the model.

In terms of the strength and exogeneity of the instruments proposed in this analysis, the F-statistic for the identifying instruments is 12.76, which is above the empiricist minimum of 10 required to pass the weak instruments test (Angrist and Pischke 2009). Furthermore, I can formally test the exogeneity of the instruments since my model is over-identified. As shown, the Hansen J-Statistic indicates that one cannot reject the null hypothesis that the identifying instruments are exogenous at the 5% level of statistical significance.[20] Thus, the two crucial components required to perform an instrumental variable analysis – the relevance criteria and the exclusion restrictions – are met in this analysis.

Turning to the other models in Table 3, the mediated impact of welfare reform on the LFP rates for less-educated males aged 16 to 29 are presented for three groups of men: (1) all single males, (2) black single males, and (3) white single males. I begin with the single equation OLS coefficient, which indicates a positive and significant relationship between LFP rates for single mothers (16-44) and all single males (16-29). As noted, this estimate is biased. Two-stage modeling shown in the third column reveals a negative and statistically significant relationship. The coefficient of -0.2567 can be interpreted as follows: an exogenous 10 percentage point increase in LFP of low-skilled single mothers prompted by welfare reform policies led to an approximately 2.6 percentage point decline in labor supply by young, low-skilled single male laborers. Note that the sign of the estimated relationship has changed from positive to negative as

[20] This test reveals that the instruments in this analysis *do not directly influence* male labor supply, but that the impact is moderated through their influence on female LFP. In other words, they meet the exclusion restriction of a valid instrumental variable. This finding is critical to establish a valid IV research design and, as will be shown, is not typically found in the modeling with other groups.

the IV strategy addresses the endogeneity concerns highlighted earlier by removing a significant portion of the bias inherent within OLS estimation.

Examining the LFP rates for young, low-skilled black males reveals a different picture. Though the models also pass the weak instruments and exogeneity tests, the estimated effect of increases in labor supply by single mothers is statistically indistinguishable from zero. While somewhat surprising, these findings are in line with Holzer et al. (2005) who attribute the decline in LFP to factors other than welfare reform and increases in low-skilled female labor supply. More specifically, the authors find that CSE and incarceration rates drive a large portion of the drop in the labor supply of black men between the ages of 16 and 34. While the research design and data panel length used in this analysis do not seek to reproduce these findings,[21] it is simply important to note that black males within the period of welfare reform appear to be unresponsive to the influx of labor supplied by low-skilled single mothers.

The last group – single white males – is the group that is driving the findings of labor force exit for low-skilled men. As displayed, the estimated relationship between labor supply by single mothers and young, single white males is negative and statistically significant in the 2SLS modeling and, moreover, the point estimate indicates that a 10 percentage point increase in LFP rates by low-skilled single mothers led to approximately a 3.5 percentage point decline in LFP rates by young, low-skilled single males, holding all else equal. These values are both highly statistically and economically significant as this decline over a base LFP rate for white males of 87.6 pp can also be interpreted as a 4% decline in labor supply. In terms of the roughly 4 million young, low-skilled, single white males aged 16 to 29 in the March 2002 CPS, this represents a decline in supply of roughly 140,000 young men.

[21] Holzer et al. (2005) examine the period from 1979 to 2000 as well as a different set of age categories.

1.7. Robustness Checks

In this section, I present three additional robustness checks to examine the sensitivity of my findings. The first check is to introduce state-level time trends to the core models previously reported in Table 3. These specifications seek to account for time-variant omitted factors within a state which could either increase or decrease the propensity for LFP by low-skilled female and male workers. Table 4 contains the findings from this exercise. As displayed, the introduction of time trends diminishes the statistical significance of estimated decline in LFP rates for all single males aged 16 to 29. While the point estimates is still negative, the coefficients are not statistically significant at any conventional levels and the F-statistic on excluded instruments is well below the empiricist minimum of 10 to avoid the weak instruments critique. However, conclusions for the two subgroups of young males are similar to those reported in other tables. I find no evidence of labor supply changes for blacks, but statistically and economically significant labor force exit for young white males. Under this robustness check, the magnitude of the findings are very similar to those previously presented: I estimate an approximately 3.2 pp decline in labor supply for each 10 pp increase in LFP for single mothers.

Though not strongly supportive at the aggregated level, the results from the models with time trends do provide additional evidence for causal claims for the group which appears to be driving the finding of labor force exit: young, single white males. However, modeling with state-specific time trends in this analysis can be criticized on three grounds: (1) the majority of the data used to calculate the time trend resides in the post period and this distorts the inferences and value of establishing a “pre” trend because it is based primarily upon “post” data, (2) the margin with which labor supply decisions are changing is relatively small and, consequently, the use of a time trend does not leave much variation to be explained by other mechanisms, and (3) data at the aggregate level – recall Figure 1 – indicate a rather quick transition of LFP rates for young males during the welfare reform period then a subsequent stabilization. Stated differently, time trends may be too blunt of an instrument which absorbs much of the variation in LFP, which is directly attributable to welfare reform policies, especially given that the models already contain state, year, and quarter fixed effects. This statement is particularly true when thinking about the interpretation of the first stage tests for weak instruments. Given these critiques, coefficients derived from core models provide a more precise measurement of labor force exit stemming from an exogenous shift in low-skilled female labor supply.

The second set of robustness checks examines whether a similar subset of men who could have been affected by large increases in labor supply by low-skilled single mothers. As presented in Table 5, low-skilled single males aged 30 to 49 do not appear to be *negatively impacted* by the labor supply increases of single mothers aged 16-44. While eschewing a detailed analysis of these findings due to space constraints, I will simply make a few comments to explain the difference in patterns among the younger and older low-skilled male workers. Younger males may be more responsive to market conditions because they have less work history, labor force attachments, and are more likely to engage in criminal activity (Freeman 2000; Levitt 2001). Thus, they would be more influenced by changes in female labor supply, whereas many older males are ostensibly in the legitimate labor force, or not, a tendency which changed very little during the welfare reform years or are competing for positions unaffected by the influx of low-skilled female labor. Stated differently, the labor supply decision of older males appears to be operating at a different margin which is not negatively impacted by the increase in labor supply by low-skilled women stemming from welfare reform.

Finally, Table 6 contains a series of other robustness check where I apply the FE-IV strategy used in this paper to two overlapping *childless groups* which should not have been directly impacted by welfare reform under the theoretical framework established earlier: young, low-skilled single females aged 16 to 29 without children and low-skilled single women aged 16 to 44, again without children. When examining the responsiveness of single, childless women of each age group using the LFP responsiveness of single mothers aged 16-44 as the first-stage instrumented variable (i.e., equivalent to the second-stage model reported in Table 3), there is no evidence of labor force exit stemming from welfare reform.

Results from these robustness checks present strong evidence that the indirect effects of welfare reform were targeted specifically on low-skilled male – not female – workers. This important differentiation reveals that young, low-skilled males may be more sensitive to monetary incentives offered by the marketplace – especially if they have more lucrative employment opportunities in the informal or black market.

1.8. Discussion and Conclusions

Seeking to isolate another factor contributing to the decline in labor supply of young, low-skilled males, which has confounded a number of scholars (Holzer et al. 2005; Blank 2009), this paper presents compelling evidence that the welfare reform policies of the 1990s unintentionally led to an approximately 2.6 pp decline in the LFP rates for all young, low-skilled single male workers and a roughly 3.5 pp decline in LFP rates for single white males within this same subgroup. Though findings presented in this paper are robust across a number of alternative specifications, there are two limitations to this work which temper the claims for a truly causal relationship between the welfare-induced increases in labor supply by low-skilled single mothers and labor force exit by some low-skilled men. Both critiques are essentially routed in the fact that the fixed effects, instrumental variable design cannot control for unobserved or omitted factors which are changing over both time and region and which are correlated with the implementation of welfare waivers, TANF programs, and other work incentives enacted in a given state.

The first limitation of a FE-IV strategy is that it does not directly control for compositional changes in population over time. Since the range of data used in this analysis is fairly long (1989 to 2002), bias could be introduced into these models if the sub-population of interest is drastically different in the latter period from the initial one and that this variation is correlated with the state in which the individual resides and, more importantly, the timing of the bundle of welfare reforms. To be more concrete, suppose that the average white male in State A with education at or below a high school level is markedly less employable in 2002 than in 1989. If this decrease in "employability" is consistent across other states (B, C, etc.) then the year fixed-effects identifies the general decline across all regions over time. However, if the decrease in employability is both time and region variant, and correlated with reform, then this cannot be captured by a FE-IV model and – presumably – leads to overestimation of the decline in young, single male labor supply attributable to labor supply changes by low-skilled single women prompted by welfare reform. Under this occurrence, estimates presented in this paper would serve as an upper-bound of the true impact.

Secondly, other potentially omitted time-varying factors, such as the large increase in low-skilled individuals receiving disability insurance (DI) or supplemental security income (SSI) benefits, is problematic if, again, males in select states are more likely to apply and receive these benefits and changes in the generosity of these programs are correlated with the timing of welfare reform policies. While the inclusion of estimates of the number of DI or SSI recipients by state and year appears, at first, like a reasonable addition to the models, it is an outcome variable and, thus, meets the definition of a "Bad Control" (Angrist and Pischke 2009) and is in all likelihood endogenous.[22] What one really needs to control for in this critique is the time variant behavioral factors which lead to DI or SSI claims and which are correlated with the instruments, not to control for the outcome which can be jointly determined with LFP.

Those critiques aside, this paper presents evidence that young, low-skilled single males are responsive to government policies and that there was another serious unintended consequence of the large set of welfare reform policies enacted during the 1990s: the facilitation of labor force exit by young, low-skilled single male labor. Policy wise, this paper examines how resources concentrated on one particular set of disadvantaged individuals, e.g., low-skilled single mothers, may have adversely affected the behavior of another group, e.g., young, low-skilled males, which has struggled to incorporate itself into a changing American economy and is currently linked to a host of social ills, such as black market LFP, increased incarceration rates, and declines in low-income nuclear families. Thus, to the extent to which policymakers believe that the government has a role to play in steering this group towards more socially desirable outcomes, it would seem that the calls by many scholars to increase work incentives to other segments of low-wage workers – through policies such as an extension of the EITC (see Blank 2009; Smeeding et al. 2011; Western and Pettit 2010) – should be given more consideration in an attempt to reverse many of the negative trends associated with this potentially disadvantaged group of workers.

[22] Recall that – by definition – DI and SSI beneficiaries are not part of the labor market. Furthermore, note that this is also the rationale behind not including variables such as the unemployment rate in the models. Other modeling – not shown but available upon request – includes state and federal SSI generosity in both the first and second stages but results remain substantively unaffected.

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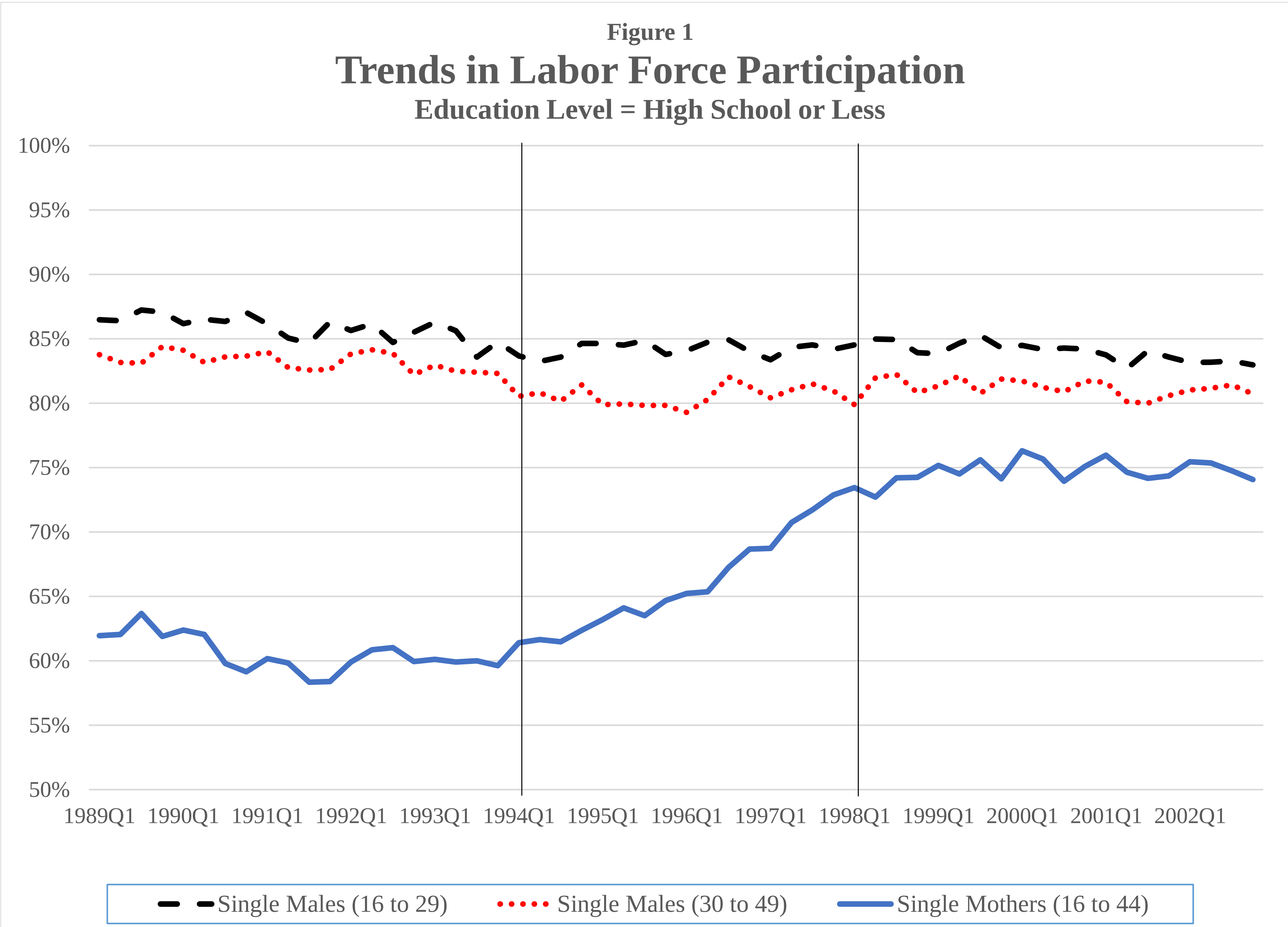
Figure 1
Trends in Labor Force Participation
Education Level = High School or Less
100%
95%
90%
85%
80%
75%
70%
65%
60%
55%
50%
1989Q1
1990Q1
1991Q1
1992Q1
1993Q1
1994Q1
1995Q1
1996Q1
1997Q1
1998Q1
1999Q1
2000Q1
2001Q1
2002Q1
Single Males (16 to 29)
Single Males (30 to 49)
Single Mothers (16 to 44)

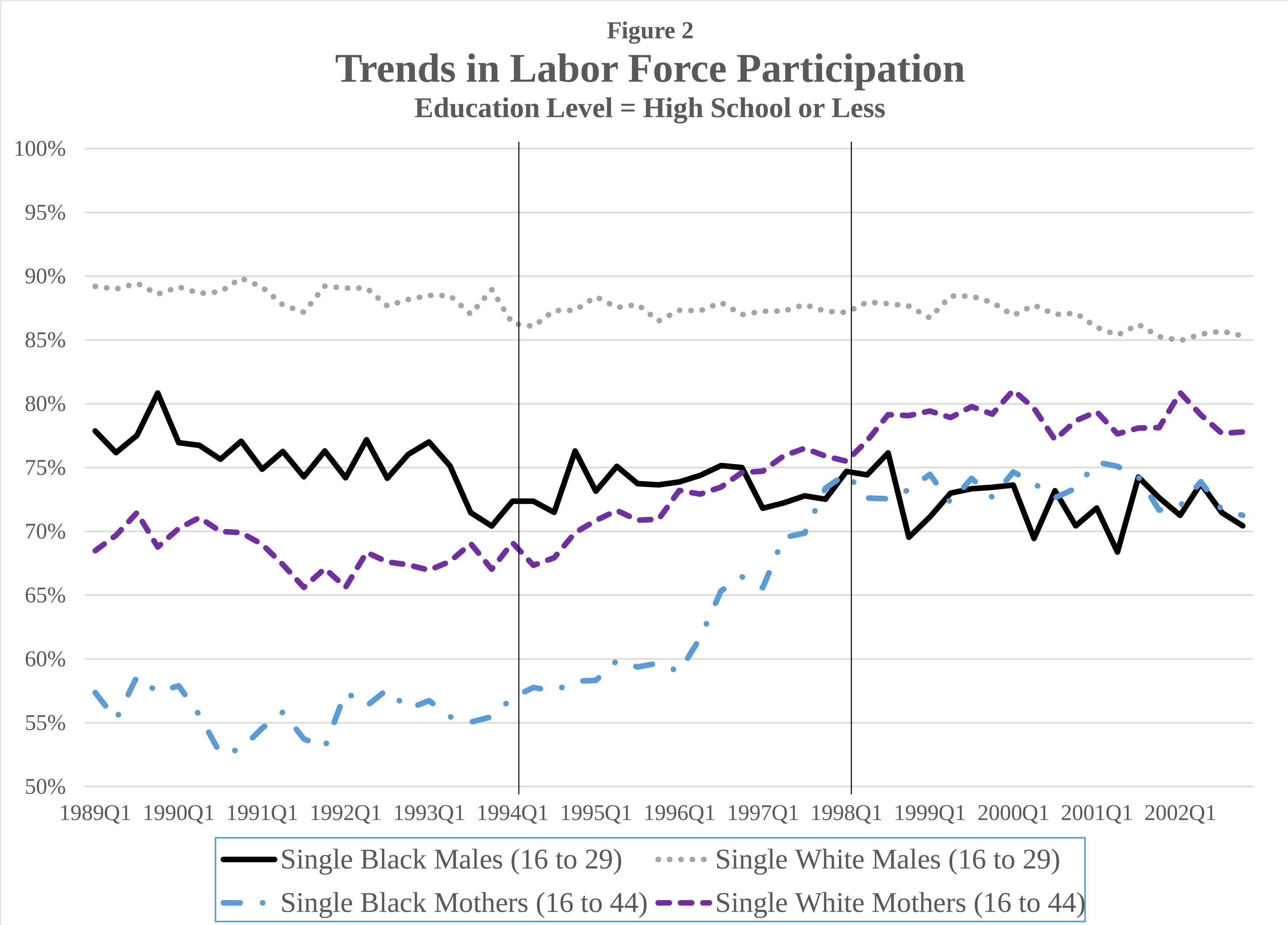
Figure 2
Trends in Labor Force Participation
Education Level = High School or Less
100%
95%
90%
85%
80%
75%
70%
65%
60%
55%
50%
1989Q1
1990Q1
1991Q1
1992Q1
1993Q1
1994Q1
1995Q1
1996Q1
1997Q1
1998Q1
1999Q1
2000Q1
2001Q1
2002Q1
Single Black Males (16 to 29)
Single White Males (16 to 29)
Single Black Mothers (16 to 44)
Single White Mothers (16 to 44)

Table 1

Instrumental Variables and Other Controls Examined in 2SLS Modeling

Variables Influencing the LFP of Low-Skilled Single Mothers

Variable	Source
Welfare Reform Dates	Department of Health and Human Services (http://aspe.hhs.gov/hsp/waiver-policies99/Table_A.PDF)
Implementation of a State Waiver Program	
Date of TANF Implementation	
State Waiver Components	Ziliak, Figlio, Davis, and Connolly (2000)
Work Requirements	
Time Limits	
Work Incentives	
Personal Responsibility Requirements	
Cash Benefits and the Earned Income Tax Credit	University of Kentucky Center for Poverty Research (UKCPR National Welfare Data, 1980-2013)
AFDC or TANF Maximum Monthly Cash Benefits - Family of Three	
State and Federal Earned Income Tax Credits - Family of Three	
Other Controls	University of Kentucky Center for Poverty Research (UKCPR National Welfare Data, 1980-2013)
Gross State Product - % Growth	
State Minimum Wage	
Weekly Earnings for Low-Skilled Males 16-29	March Current Population Survey Data
Incarceration Rates by race/ethnic group	National Prisoner Statistics, Bureau of Justice Statistics; Surveillance, Epidemiology, and End Results (SEER) Program
Child Support Enforcement	Huang, Kunz, and Garfinkel (2002)
Child Support Enforcement Index	

Table 2

Descriptive Statistics for Variables used in the Core Empirical Modeling

Variable	Obs	Mean	Standard Deviation	Min	Max
LFP - Single Males (16-29)	2856	0.852	0.063	0.574	0.994
LFP - Black Single Males (16-29)	2474	0.763	0.197	0.000	1.000
LFP - White Single Males (16-29)	2848	0.876	0.069	0.000	1.000
LFP - Single Males (30-49)	2856	0.823	0.069	0.478	1.000
LFP - Black Single Males (30-49)	2382	0.757	0.202	0.000	1.000
LFP - White Single Males (30-49)	2855	0.845	0.074	0.514	1.000
LFP - Single Mothers (16-44)	2856	0.695	0.112	0.336	0.980
Gross State Product Growth	2856	0.055	0.028	-0.110	0.173
State Minimum Wage	2856	4.48	0.89	1.60	7.15
Weekly Earnings for Low-Skilled Males 16-29 (MA4, Lag 2)	2856	397.63	42.99	296.55	567.78
Incarceration Rates - All Males - Lagged 3 Years	2856	0.009	0.006	0.002	0.051
Incarceration Rates - Blacks - Lagged 3 Years	2856	0.041	0.017	0.003	0.110
Incarceration Rates - Whites - Lagged 3 Years	2856	0.005	0.002	0.001	0.012
Child Support Index	2856	7.44	0.82	4.00	8.00
AFDC/TANF Benefits for a Family of Three	2856	469.35	186.18	129.58	1184.80
Ratio of Cash Benefits to the Max State + Federal EITC	2856	2.25	1.67	0.38	10.67
Welfare Reform (Waiver or TANF)	2856	0.540	0.498	0.000	1.000
AFDC - Work Requirement Waivers	2856	0.079	0.271	0.000	1.000
AFDC - Time Limit Waivers	2856	0.024	0.152	0.000	1.000
AFDC - Work Incentives Waivers	2856	0.080	0.272	0.000	1.000
AFDC - Responsibility Waiver	2856	0.078	0.268	0.000	1.000

Table 3

Impact of Welfare Reform on LFP for Single, Less-Educated Males Aged 16 to 29

	All Single Males			Single Black Males			Single White Males		
	OLS	IV First Stage	IV Second Stage	OLS	IV First Stage	IV Second Stage	OLS	IV First Stage	IV Second Stage
LFP: All Single Mothers (16-44)	0.0524***		-0.2567***	0.1145***		0.0538	0.0488***		-0.3455***
	[0.0136]		[0.0756]	[0.0414]		[0.1646]	[0.0135]		[0.0794]
Gross State Product - % Growth	-0.0358	0.0000	-0.0070	-0.0545	-0.0493	-0.0506	0.0031	-0.0302	0.0260
	[0.0412]	[0.0634]	[0.0463]	[0.1322]	[0.0872]	[0.1331]	[0.0457]	[0.0678]	[0.0536]
Log of State Minimum Wage	0.0086	0.0204	0.0199*	-0.0183	-0.0133	-0.0179	-0.0004	0.0260*	0.0132
	[0.0099]	[0.0139]	[0.0112]	[0.0308]	[0.0196]	[0.0310]	[0.0099]	[0.0144]	[0.0116]
Log of Average Weekly Earnings (MA4, Lag 2)	0.0199	-0.1292***	-0.0444*	0.0699	-0.1346***	0.0553	0.0353*	-0.1118***	-0.0317
	[0.0186]	[0.0279]	[0.0241]	[0.0543]	[0.0372]	[0.0652]	[0.0181]	[0.0286]	[0.0240]
Child Support Index	0.0003	0.0037	0.0015	-0.0051	0.0047	-0.0047	0.0033**	0.0045	0.0055***
	[0.0017]	[0.0027]	[0.0020]	[0.0052]	[0.0030]	[0.0053]	[0.0016]	[0.0027]	[0.0021]
Percent of Males Incarcerated (Lag 3)	1.0677*	-4.8319***	-1.3513	-0.0467	-0.9773***	-0.1274	3.5965**	-6.5874***	-0.5763
	[0.6452]	[0.8594]	[0.8824]	[0.4940]	[0.2289]	[0.5227]	[1.4567]	[1.8765]	[1.7820]
Welfare Reform (Waiver or TANF)		-0.2642***			-0.3501***			-0.2913***	
		[0.0442]			[0.0542]			[0.0434]	
AFDC - Work Requirements Waiver		0.0263***			0.0241**			0.0289***	
		[0.0081]			[0.0123]			[0.0093]	
AFDC - Time Limits Waiver		0.0289***			0.0267**			0.0178*	
		[0.0101]			[0.0128]			[0.0106]	
AFDC - Work Incentives Waiver		-0.0557***			-0.0572***			-0.0494***	
		[0.0100]			[0.0129]			[0.0118]	
AFDC - Responsibility Waiver		0.0087			0.0027			0.0139*	
		[0.0078]			[0.0098]			[0.0081]	
Log of Max Cash Benefits to Max EITC		-0.0603***			-0.0097			-0.0679***	
		[0.0187]			[0.0243]			[0.0196]	
Log of Cash Benefits X Welfare Reform		0.0478***			0.0648***			0.0514***	
		[0.0071]			[0.0087]			[0.0071]	
Number of Observations	**2856**	**2856**	**2856**	**2474**	**2474**	**2474**	**2848**	**2848**	**2848**
R-squared (within)	**0.386**	**0.592**		**0.084**	**0.575**		**0.366**	**0.567**	
F-statistic of the Excluded Instruments		**12.76**			**13.41**			**13.17**	
Hansen J-Statistics (P-Value)		**0.0869**			**0.8683**			**0.1602**	

Notes:

Regression are weighted by the number of relevant males residing in a given state/period. All models have state, year, and quarter fixed effects and robust standard errors are in brackets and statistical significance is indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

Table 4

Impact of Welfare Reform on LFP for Single, Less-Educated Males Aged 16 to 29

Modeling with State-Specific Time Trends

	All	Black	White
LFP: All Single Mothers (16-44)	-0.1076	0.0579	-0.3218**
	[0.1335]	[0.3183]	[0.1472]
Number of Observations	**2856**	**2474**	**2848**
First-Stage Tests:			
F-statistic of the Excluded Instruments	**2.77**	**2.96**	**2.82**
Hansen J-Statistics (P-Value)	**0.4694**	**0.3415**	**0.7343**

Notes:

Each cell represents a separate regression model which is estimated with state-specific time trends and all of the controls and instruments outlined in Table 3. Regressions are weighted by the number of individuals residing in a given state/period for the age range referenced. Robust standard errors are in brackets and statistical significance is indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

Table 5

Impact of Welfare Reform on LFP for Single, Less-Educated Males Aged 30 to 49

	All	Black	White
LFP: All Single Mothers (16-44)	0.0888	0.0136	-0.0811
	[0.0681]	[0.1567]	[0.0765]
Number of Observations	**2856**	**2257**	**2850**
First-Stage Tests:			
F-statistic of the Excluded Instruments	**14.33**	**13.12**	**14.71**
Hansen J-Statistics (P-Value)	**0.0013**	**0.0382**	**0.0000**

Include State-Specific Time Trends

	All	Black	White
LFP: All Single Mothers (16-44)	0.3976***	0.3571	0.4692***
	[0.1492]	[0.3139]	[0.1742]
Number of Observations	**2856**	**2257**	**2850**
First-Stage Tests:			
F-statistic of the Excluded Instruments	**3.29**	**3.12**	**3.35**
Hansen J-Statistics (P-Value)	**0.5119**	**0.2501**	**0.3506**

Notes:

Each cell represents a separate regression model and includes all of the controls and instruments outlined in Table 3. Regressions are weighted by the number of individuals residing in a given state/period for the age range referenced. Robust standard errors are in brackets and statistical significance is indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

Table 6

Impact of Welfare Reform on LFP for Single, Less-Educated Childless Women

	Childless Females Aged 16 to 29			Childless Females Aged 16 to 44		
	All	**Black**	**White**	**All**	**Black**	**White**
LFP: All Single Mothers (16-44)	-0.0423	0.0181	-0.1051	0.1046	0.2316	0.0600
	[0.1023]	[0.2127]	[0.1113]	[0.0807]	[0.1858]	[0.0896]
Number of Observations	**2856**	**2257**	**2850**	**2856**	**2364**	**2853**
First-Stage Tests:						
F-statistic of the Excluded Instruments	**12.65**	**11.26**	**13.28**	**12.87**	**11.91**	**13.29**
Hansen J-Statistics (P-Value)	**0.0090**	**0.5347**	**0.0036**	**0.0472**	**0.3984**	**0.0548**

Notes:

Each cell represents a separate regression model and includes all of the controls and instruments outlined in Table 3. Regressions are weighted by the number of individuals residing in a given state/period for the age range referenced. Robust standard errors are in brackets and statistical significance is indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

Appendix A
Ratio of AFDC/TANF Annual Cash Benefit to the Maximum State/Federal EITC Credit

State	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002
AK	10.67	10.65	8.66	8.01	7.33	4.38	3.56	3.11	3.03	2.95	2.90	2.85	2.76	2.68
AL	1.56	1.49	1.20	1.29	1.30	0.78	0.63	0.55	0.54	0.52	0.52	0.51	0.49	0.48
AR	2.69	2.57	1.98	1.77	1.62	0.97	0.79	0.69	0.67	0.65	0.64	0.63	0.61	0.59
AZ	3.86	3.69	2.85	2.90	2.76	1.65	1.34	1.17	1.14	1.11	1.09	1.07	1.04	1.01
CA	8.74	8.74	6.74	5.75	4.96	2.88	2.34	2.05	1.85	1.81	1.92	1.93	1.93	1.97
CO	4.69	4.48	3.46	3.09	2.83	1.69	1.37	1.20	1.17	1.14	1.03	1.00	0.97	1.03
CT	8.22	8.17	6.61	5.90	5.40	3.23	2.10	2.15	2.09	2.03	2.00	1.96	1.90	1.84
DC	5.18	5.15	4.16	3.55	3.25	1.99	1.62	1.42	1.31	1.21	1.19	0.94	0.91	0.88
DE	4.39	4.19	3.28	2.93	2.68	1.60	1.30	1.14	1.11	1.08	1.06	1.04	1.01	0.98
FL	3.78	3.70	2.86	2.63	2.41	1.44	1.17	1.02	0.99	0.97	0.95	0.94	0.91	0.88
GA	3.56	3.44	2.72	2.43	2.22	1.33	1.08	0.94	0.92	0.89	0.88	0.86	0.84	0.81
HI	7.35	7.58	6.14	5.77	5.50	3.38	2.75	2.40	2.34	1.82	1.79	1.76	1.71	1.65
IA	5.20	4.92	3.89	3.47	3.18	1.90	1.54	1.35	1.31	1.28	1.26	1.23	1.20	1.16
ID	4.01	3.99	3.08	2.73	2.50	1.50	1.22	1.07	1.04	0.88	0.87	0.90	0.88	0.85
IL	4.51	4.62	3.57	3.18	2.91	1.74	1.45	1.27	1.24	1.20	1.19	1.11	1.07	1.04
IN	3.80	3.63	2.80	2.50	2.29	1.37	1.11	0.97	0.95	0.92	0.91	0.89	0.86	0.83
KS	5.63	5.15	3.97	3.66	3.41	2.04	1.55	1.45	1.41	1.25	1.23	1.20	1.17	1.13
KY	2.87	2.87	2.22	1.98	1.81	1.08	1.01	0.88	0.86	0.84	0.82	0.81	0.78	0.76
LA	2.51	2.39	1.85	1.65	1.51	0.90	0.73	0.64	0.62	0.61	0.60	0.59	0.72	0.70
MA	7.11	6.79	5.24	4.67	4.28	2.75	2.23	1.91	1.69	1.68	1.66	1.62	1.65	1.56
MD	3.31	3.32	2.63	2.18	1.90	1.16	0.96	0.84	0.82	0.83	0.84	0.86	0.88	0.91
ME	5.78	5.70	4.40	3.93	3.60	1.98	1.61	1.41	1.37	1.34	1.38	1.36	1.31	1.34
MI	6.76	6.50	5.10	3.98	3.65	2.18	1.77	1.55	1.51	1.47	1.44	1.42	1.37	1.33
MN	7.02	6.70	4.70	4.19	3.67	2.20	1.78	1.56	1.52	1.36	1.35	1.31	1.21	1.16
MO	3.76	3.64	2.84	2.53	2.32	1.39	1.13	0.99	0.96	0.93	0.92	0.90	0.87	0.85
MS	1.58	1.51	1.17	1.04	0.95	0.57	0.46	0.40	0.39	0.38	0.38	0.52	0.51	0.49
MT	4.73	4.52	3.60	3.38	3.10	1.90	1.45	1.43	1.44	1.44	1.45	1.45	1.48	1.43
NC	3.51	3.42	2.64	2.36	2.16	1.29	1.05	0.92	0.89	0.87	0.86	0.84	0.81	0.79
ND	5.09	4.86	3.90	3.48	3.18	1.94	1.66	1.45	1.41	1.46	1.44	1.41	1.37	1.38
NE	4.80	4.58	3.54	3.16	2.89	1.73	1.40	1.23	1.19	1.16	1.14	1.12	1.09	1.06
NH	6.54	6.37	5.01	4.47	4.10	2.61	2.12	1.86	1.81	1.76	1.73	1.77	1.80	1.74
NJ	5.59	5.34	4.12	3.68	3.37	2.01	1.64	1.43	1.39	1.35	1.33	1.31	1.10	1.05
NM	3.48	3.32	3.01	2.81	2.57	1.69	1.47	1.31	1.28	1.24	1.54	1.35	1.31	1.27
NV	4.35	4.16	3.21	3.23	2.76	1.65	1.34	1.17	1.14	1.11	1.09	1.07	1.04	1.01
NY	7.11	7.27	5.61	5.00	4.58	2.55	2.02	1.62	1.58	1.54	1.51	1.48	1.38	1.31
OH	4.23	4.21	3.25	2.90	2.71	1.62	1.32	1.15	1.12	1.09	1.14	1.15	1.12	1.08
OK	4.29	4.09	3.31	2.96	2.57	1.54	1.18	1.04	1.01	0.93	0.92	0.90	0.87	0.81
OR	5.54	5.44	4.31	3.99	3.65	2.18	1.77	1.55	1.44	1.40	1.38	1.35	1.31	1.27
PA	5.30	5.30	4.09	3.65	3.34	2.00	1.55	1.42	1.38	1.35	1.32	1.30	1.21	1.22
RI	5.54	5.56	4.22	3.77	3.45	2.06	1.68	1.47	1.43	1.39	1.38	1.36	1.32	1.28
SC	2.72	2.59	2.04	1.82	1.59	0.95	0.77	0.67	0.66	0.64	0.63	0.63	0.61	0.59
SD	4.83	4.75	3.74	3.50	3.21	1.98	1.66	1.45	1.41	1.37	1.35	1.33	1.29	1.36
TN	2.28	2.32	1.89	1.60	1.47	0.88	0.71	0.62	0.61	0.59	0.58	0.57	0.55	0.54
TX	2.43	2.32	1.79	1.60	1.46	0.87	0.73	0.63	0.62	0.60	0.59	0.62	0.60	0.58
UT	4.96	4.87	3.91	3.49	3.19	1.97	1.64	1.44	1.40	1.36	1.42	1.39	1.35	1.37
VA	4.67	4.46	3.44	3.07	2.81	1.68	1.12	1.19	1.16	1.13	1.11	1.09	1.06	1.13
VT	6.48	6.51	5.15	4.56	4.09	2.42	2.02	1.77	1.68	1.56	1.54	1.75	1.61	1.56
WA	6.49	6.31	5.16	4.60	4.34	2.59	2.11	1.84	1.79	1.74	1.72	1.69	1.63	1.58
WI	5.45	5.21	4.02	3.59	3.28	2.12	1.72	1.53	1.49	1.89	1.86	1.82	1.77	1.71
WV	3.28	3.14	2.42	2.16	1.98	1.18	0.98	0.85	0.83	0.81	0.87	1.01	1.36	1.31
WY	4.75	4.53	3.50	3.12	2.86	1.71	1.39	1.21	1.18	1.09	1.07	1.05	1.02	0.99

Note: Cash and EITC benefits are for a three person family

Chapter 2

Still "Saving Babies"?
The Impact of Child Medicaid Expansions on High School Completion Rates

2.1. Introduction

Before the 1980s, qualification for public health insurance under state-level Medicaid programs was traditionally tied to the receipt of Aid to Families with Dependent Children (AFDC) benefits, although states could voluntarily choose to cover other low-income groups, such as the medically needy or single women pregnant for the first time. As the battle between conservatives and liberals over the direction of social welfare policy and government spending unfolded during the Reagan administration (Kaiser Family Foundation, 2014), a series of significant legislative changes from 1984 to 1989 led to a decoupling of the AFDC and the child Medicaid programs. As a result, millions of low-income children became eligible for public healthcare who would not have received benefits under the old rules.

This paper examines one of the long-term effects of these expansions and focuses on a singular question: did the expansion of health insurance benefits to low-income children throughout the 1980s and early 1990s increase state-level high school completion rates around the turn of the 21st century? Exploration of the other consequences of Medicaid expansions have received a considerable amount of attention in the academic literature, with studies examining the short-term impacts on child and maternal health (Aizer et al., 2007; Currie and Grogger, 2002; Currie and Gruber, 1994; Currie and Gruber, 1996a; Currie and Gruber, 1996b; Kaestner, 1999; Lykens and Jargowsky, 2002), the crowd-out of private health insurance (Blumberg et al., 2000; Busch and Duchovny, 2005; Cutler and Gruber, 1996; De La Mata, 2012; Gruber and Simon, 2008; Ham and Shore-Sheppard, 2005; Hamersma and Kim, 2013; Lo Sasso and

Buchmueller, 2004; Shore-Sheppard et al., 2000; Shore-Sheppard, 2008), the effects on academic achievement during early childhood years (Levine and Schanzenbach, 2009), and the impacts on fertility (DeLeire et al., 2011; Zavodny and Bitler, 2010). However, this present study is one of the first to explore whether Medicaid expansions helped to increase the high school completion rates – the other being the NBER working paper by Cohodes et al. (2014) – and, moreover, helps to assess whether governmental investments in the form of healthcare for low-income children can lead to improvements in long-term outcomes for this vulnerable population.

An investigation of the expansions of public health insurance to low-income families is substantively important due to the sheer size of these programs. In 1984, roughly 17% of all births in the United States were covered by Medicaid (Howell and Ellwood, 1991), while public insurance covered roughly 37% of all births after the full set of expansions was implemented in the early 1990s (MCH Update, 2003). More recently, this rate has grown to almost 48% of all U.S. births in 2010 (Markus et al., 2013). Thus, health insurance subsidized by the government covers a very significant proportion of all births in the United States and, moreover, provides access to healthcare in early childhood for a correspondingly large number of children. Access to care can allow medical professionals to diagnose and treat health issues in needy children before they become debilitating and could generate benefits beyond decreased child mortality and increased birth weight as noted in Currie and Gruber (1996b).

The link between governmental investments in the health of young, low-income children and the high school completion rates in America is an important one. As education levels and technological skills become increasingly valued in a specialized U.S. economy (Autor et al., 2008; Berman et al., 1998; Bresnahan et al., 2002), the long-term prospects for high school dropouts – both professionally and personally – are rather bleak. Not only are dropouts less likely than other workers to find stable employment (Apel and Sweeten, 2010; Rumberger and Lamb, 2003), they are also less prone to the formation of stable nuclear families (Carlson et al., 2004; Cherlin, 2010; Western and Wildeman, 2009), which can facilitate the intergenerational transmission of poverty (Western and Wildeman, 2009; Wilson, 1987). Moreover, those who fail to earn a degree – especially males – are much more likely to engage in criminal activities (Blanchflower and Freeman, 2000; Pettit and Western, 2004), which greatly diminishes long-term earning potential (Western et al., 2001) and contributes to the exceptionally high incarceration rates in the U.S. (Western and Wildeman, 2009). Thus, government investments in the form of early childhood health insurance for low-income children could conceivably lead to a population which is better-educated and less reliant upon social welfare programs as adults.

By exploiting the wide degree of heterogeneity in qualification standards for state-level Medicaid programs – as well as differences in the timing of Medicaid expansions and the implementation of federal mandates – this paper estimates the intent-to-treat (ITT) effect[1] of Medicaid expansions to low-income children on the subsequent educational attainment of all public high school students, measured by both the state-level dropout and four-year traditional graduation rates. More specifically, this paper uses a plausibly exogenous measure of the generosity of a state's Medicaid program to estimate the causal effect of increases in the percentage of child-years potentially covered by the state's public health insurance program from conception through age 5.[2] Using this simulated eligibility measure – the general form of which was first proposed by Currie and Gruber in 1994 and then subsequently adopted and adapted by a

[1] Like other papers in the literature, I consider this an intent-to-treat effect because the focus here is on eligibility and not the actual causal impact of public health insurance on the long-term graduation rates. The latter, producing treatment-on-the-treated estimates, would require a panel of individual-level data for all states, which does not exist.
[2] Medicaid eligibility is examined through age five for two reasons. First, this paper seeks to examine governmental investment in the form of public healthcare provided to young, low-income children *before they enter primary school*. Secondly, early legislative expansions to women and children in the late 1980s stipulated age 5 as the cutoff for mandatory Medicaid coverage.

number of other researchers (see Currie and Gruber, 1994; Yelowitz, 1995; Currie and Gruber, 1996a; Currie and Gruber, 1996b; Currie and Gruber, 2001; Ham and Shore-Sheppard, 2005; Gruber and Simon, 2008; DeLeire et al., 2011, Cohodes et al., 2014) – I find that a 10 pp increase in early childhood years potentially eligible for Medicaid coverage led to a decrease in long-term high school dropout rates by 1.9 to 2.5 pp and an increase in four-year graduation rates by 1.0 to 1.3 percentage points.

Findings are consistent across a number of alternative means to measure Medicaid eligibility and the number of years potentially covered during early childhood and, moreover, are driven by the two groups benefiting most from the public health insurance expansions: Hispanic and white students. Since the vast majority of states increased the generosity of their state-level programs by approximately 25 percentage points, this suggests that high school dropout rates decreased by roughly 4.75 to 6.25 pp, while traditional four-year graduation rates increased between 2.5 to 3.25 pp. Framing this last set of findings another way – and considering the base of roughly 3.8 million potential graduating seniors in the class of 2010 – public health insurance expansions to low-income children led to an increase of between 95,000 to 124,000 graduates per year in the U.S. Thus, of the 6 pp increase in the recent high school graduation rate reported by Murnane (2013), almost half of these gains can be attributed to child Medicaid expansions. These findings are both statistically and economically significant.

2.2. The Medicaid Program and Eligibility Expansions

A number of authors have detailed the history of the Medicaid program,[3] as well as the coverage expansions impacting eligibility across the United States throughout the 1980s and

[3] The Medicaid program dates back to 1965 when the program was officially enacted by Congress as part of President Johnson's Great Society Program. From its inception, Medicaid was a state and federal partnership,

early 1990s. Arguably, Gruber's 2003 book chapter, aptly titled "Medicaid", provides the most comprehensive overview. Given these resources, this section highlights the significant benchmarks and provisions of these public health insurance expansions that are most relevant to the fundamental research question of this paper.[4] Two notes regarding the evolution of Medicaid programs are particularly important to this paper. First, the bundle of goods and services provided by Medicaid are comprehensive and standardized across all states. Secondly, increases in eligibility stem from two key legislative changes: (1) the removal of the family structure restrictions from benefit receipt, and (2) the tying of income thresholds to some function of the federal poverty level rather than the AFDC payment standard established by the state.

2.2.1 The Scope of Medical Care Provided by Medicaid

As part of the agreement to receive federal funds, the government required that states provide a relatively standardized bundle of goods and services provided under their Medicaid program. Thus, potential medical treatment received during the early childhood years should have been roughly equivalent regardless of the state of residence for children evaluated in this analysis. This is important because the quality of "treatment" evaluated in this analysis should not be strongly dependent upon geography, conditional on time. Consequently, "generosity" in this paper refers to the number of children potentially eligible for public insurance and not the quality of medical treatment possibly received.

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whereby participating states received federal grants to help offset a portion of total program costs borne at the state-level. To receive federal funds, states were required to cover select sub-populations, such as individuals qualifying for AFDC, and states could choose to add other groups it deemed as medically needy. By 1972, all states except Arizona had created state-run Medicaid programs; Arizona opted into the program on a limited scale in 1982, only to expand coverage shortly thereafter.

[4] This overview draws heavily upon the historical overview provided in the Kaiser Family Foundation's publication "Medicaid: A Timeline of Key Developments" (2013) and reports published by the old U.S. General Accounting Office (1991)– a more detailed summary of the developments in Medicaid coverage can be found in Appendix A.

Concerning these legislated benefits over the duration of the program, medical coverage provided has been comprehensive: the wide range of services included physician care, inpatient and outpatient hospital procedures, laboratory and x-ray services, as well as access to skilled nursing facilities. A critical component of this coverage as it applies to health investments in low-income children are the Early and Periodic Screening, Diagnostic, and Treatment (EPSDT) services, which were enacted under the Social Security Mandates of 1967, and provide preventative and treatment services including dental, vision, hearing, and mental health. As the name implies, the goals of the EPSDT program are to identify health problems starting at birth, to keep monitoring the development of the child at regular intervals, and to treat the problems once they are discovered. So, where low-income children without Medicaid benefits may wait years to receive a diagnosis and treatment, children with coverage are more likely to receive help in their infancy. In turn, this could potentially eliminate or reduce the negative impact of debilitating conditions and increase cognitive development during the formative years of early childhood.

2.2.2 Determinants of Medicaid Eligibility

During Medicaid's early period, the vast majority of those covered by Medicaid received benefits based upon their qualification for AFDC benefits within a particular state. Due to the wide range of criteria used to determine AFDC qualification, a large number of poor children were excluded from public health insurance in the early period *because of family structure or income requirements legislated at the state level*.

Historically, qualification for AFDC typically precluded the presence of able-bodied males within the household. This means that low-income children residing within two-parent, nuclear families were typically not eligible for Medicaid benefits and that AFDC was essentially a program for low-income, single parents. Acknowledging the distortive effects of this policy, legislative changes sought to break this link between AFDC receipt and child Medicaid by expanding eligibility to all children below some multiple of the federal poverty guideline, *regardless of family structure type*. As Figure 1 notes, Hispanic and white children are most likely to reside in two-parent, married families during their early childhood years. Thus, they are the two groups most likely to benefit from the removal of the family structure restrictions on child Medicaid receipt.

Furthermore, since individual states determined the need and payment standards under the state-level AFDC programs, there was tremendous variation in the income level that qualified single-parent families for benefits during the early period of the Medicaid program. For example, Alabama's monthly need standard for a family of 3 in 1980 was $192 in nominal dollars, whereas the standard for a high-threshold state such as Vermont was $670. A comparison of these values to the federal poverty guideline of approximately $520 per month for a family of three at the same point in time reveal the potential for a significant number of poor children and families not qualifying for AFDC benefits and Medicaid simply because their states had chosen a low threshold to determine the "needy".

While minor changes to rules governing Medicaid eligibility occurred before the 1980s,[5] the bulk of the coverage expansions occurred during the mid to late 1980s and early 1990s – which were the early childhood years for students graduating after the turn of the 21st century. Under a number of legislative acts which sought to simultaneously limit federal expenditures and

[5] Despite the failure of President Carter's push to expand coverage to low-income children under the age of 6 who did not qualify for insurance under current state laws in the late 1970s, the notions of separating welfare receipt from Medicaid qualification and the expansion of coverage during early childhood – defined as conception through age 5 – help set the agenda for comprehensive expansions of the 1980s.

expand Medicaid coverage to needy populations during the Reagan administration,[6] Medicaid eligibility was extended to a large set of low-income children during early childhood and to their mothers during pregnancy. Details of these incremental expansions have been highlighted in a number of publications (in particular, see Currie and Gruber, 1994; Yelowitz, 1995; Currie and Gruber, 1996a; Currie and Gruber, 1996b) and, thus, I refer the interested reader to Appendix A for more information regarding the key developments in Medicaid expansions to low-income children which affected cohorts examined within this analysis. The key note is that – after the full enactment of the sweeping mandates throughout the 1980s – Medicaid for children in the United States had completed its transition from an optional state program, which was typically tied to AFDC receipt, to a stand-alone program which potentially covered all children at or below some federally mandated multiple of the federal poverty line, regardless of family structure type.

2.3. Theoretical Framework

This is an early childhood investments paper which examines governmental expenditures impacting children before they enter primary school. As such, the main mechanisms through which access to public health insurance for low-income children could raise the long-term human capital accumulation is a healthier childhood and increased cognitive and non-cognitive development during the formative years of early childhood. By being able to diagnose and treat aliments afflicting low-income children earlier in their development via Medicaid's EPSDT program, low-income children with access to Medicaid may not only be better prepared to enter

[6] Important measures included the Omnibus Budget Reconciliation Act of 1981 (OBRA81), the Deficit Reduction Act of 1984, the Consolidated Omnibus Budget Reconciliation Act of 1985 (COBRA85), the Omnibus Budget Reconciliation Act of 1986 (OBRA86), the Omnibus Budget Reconciliation Act of 1987 (OBRA87), the Medicare Catastrophic Coverage Act of 1988 (MCCA88), and the Omnibus Budget Reconciliation Act of 1989 (OBRA89).

school because of increased development in their early years, but they might miss fewer days of school once entering primary school relative to those without access to insurance. These two factors, in turn, should increase their long-term performance relative to equivalent students without insurance and, perhaps, increase their odds of obtaining a high school diploma, holding all else equal.

Several studies have linked healthcare access to health improvements.[7] Currie and Gruber (1996b) find that the Medicaid expansions that included pregnant women over the period 1979 to 1992 substantially decreased the incidence of infant mortality[8] and decreased the probability of a low birth weight baby. This finding was confirmed by Levin and Schanzenbach (2009). While the benefits of decreased infant mortality are clear, it is important to note that low birth weight has been linked to a host of long-term health issues for the child (Barker et al., 1989; Gluckman and Hanson, 2004), as well as lower reading and math scores during childhood (Chatterji et al., 2014) and decreased levels of education and employability as adults (Currie and Hyson, 1999). In another paper, Currie and Gruber linked Medicaid expansions to increases in healthcare utilization by the low-income population (Currie and Gruber, 1996a), a finding which was confirmed again in Currie and Gruber (2001). While they report that take-up of public insurance was less than 100% – e.g., a number of families qualified for Medicaid insurance but did not formally apply for benefits – they report high levels of medical care utilization, especially preventative care delivered in the offices of physicians. Thus, low-income children appeared to

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[7] In a recent literature review, Levy and Meltzer (2008) examine the causal link between health insurance coverage and health and conclude that "the evidence available to date conclusively demonstrates that health insurance improves the health of vulnerable subpopulations such as infants, children…"

[8] As noted by Currie and Gruber (1996b), Medicaid expansions to pregnant women and children stemmed, in part, from a desire of politicians to address the infant mortality rate in the U.S., which was among the highest in the industrialized world.

be using the care afforded to them under the Medicaid expansions and received treatments in excess of what they would have experienced in the absence of the eligibility extensions.

As a result of their access to care earlier in their lifecycle, low-income insured children experience fewer avoidable hospitalizations than children without insurance (Dafny and Gruber, 2005), which is presumably beneficial not only for the child's long-term development but can decrease the financial burden placed on the family (Gross and Notowidigdo, 2011; Finkelstein et al., 2012), as well as other consumers of healthcare services in the case of non-payment by the low-income family. Finally, a number of other studies and reviews have argued that access to medical care for low-income children improves their health during childhood. See Currie and Almond (2011), Gruber (1997), and Lykens and Jargowsky (2002) for further evidence supporting this link.

Comparatively fewer studies have examined the relationship between expansions of public health insurance and cognitive development during early childhood or other longer-term outcomes. This is due, in part, to the fact that many of the low-income children affected by Medicaid expansions are only now reaching adulthood. Levine and Schanenbach (2009) show that better health status at birth – as proxied by low birth weight and infant mortality – is related to improvements in 4th and 8th grade reading achievement. They use data from the National Assessment of Educational Progress (NAEP), a version of Currie and Gruber's simulated benefits, and a triple-difference identification strategy. Two other recent working papers have also investigated topics central to the theme in this one. Brown et al. (2014) use linked Internal Revenue Service data to report a positive impact of child Medicaid expansions on longer-term labor force earnings.

The current NBER working paper by Cohodes et al. (2014) is most similar in spirit to this work. They also utilize a form of Currie and Gruber's simulated Medicaid eligibility to study the effect of public health insurance expansions to low-income children aged 0 to 17 on high school and college completion rates. Using data from the 2005-2012 American Community Survey, the authors find that federal expansions led to declines in the high school non-completion rate of approximately 4.0 to 5.9% and, furthermore, that the gains were confined to non-whites. This analysis complements and extends Cohodes et al.'s work in a number of ways. First, this paper concentrates – and isolates – impacts of public health insurance expansions on early childhood only, as opposed to ages 0 to 17, and exploits a longer panel to produce more precise estimates of the impacts on the public high school completion rates. The longer panel is particularly important to establish a sufficient baseline before the family structure restrictions for Medicaid receipt were rescinded which, as noted, differentially affects individual race and ethnic groups.

This paper also contains two measures of public high school completion which were not analyzed in Cohodes et al.'s work: dropout rates using Current Population Survey (CPS) data and the traditional four-year high school graduation rate using data from the Common Core of Data (CCD). In particular, the restriction of the sample to individuals born in the U.S. increases the precision of the dropout estimates, because it isolates changes in trends only applicable to students who could have qualified for the public health insurance expansions throughout their entire early childhood. Analysis of CCD data reveals that increased completion rates applies to traditional diplomas, rather than simply increases in the number of General Education Development (GED) holders. This is important because GED holders do not fare better in the labor market relative to high school dropouts (Cameron and Heckman, 1993; Boesel et al., 1998), and, consequently, gains in completion rates reveal real improvements in human capital.

Finally, unlike Cohodes et al. (2014), I find that gains in completion rates are driven by Hispanics and whites. By estimating models by race and ethnic group, the identification strategy used in this paper explicitly addresses a potential limitation of the other study, which is that gains by “non-whites” are driven by increases in the proportion of Asian students over time – which have historically had completion rates more similar to whites. In other words, the authors may be missing a significant compositional change correlated with Medicaid expansions within their classification of a “non-white” group. Those caveats aside, the consistency in findings across these papers indicate that benefits from child Medicaid expansions are real and substantial.

2.4. Data

Data in this analysis come from three general sources: demographic information in the Current Population Survey, education statistics from the Common Core of Data, and a database of state rules used to determine Medicaid eligibility. The first source, the CPS, is a monthly survey of roughly 60,000 dwellings across the United States conducted by the U.S. Census Bureau for the Bureau of Labor Statistics.[9] While data collected in this survey serve as the basis of the government’s monthly estimate of the unemployment rate, researchers frequently use it to investigate issues pertaining to educational attainment, family structure, and family income. Data from the CPS are used in two segments of this analysis. Monthly CPS data are used to calculate the dropout rates for individuals aged 18 to 20. Estimates are examined from 1994 to 2010, which allows a number of years to establish a baseline in each state before the large-scale Medicaid eligibility expansions. March CPS data are used to simulate the generosity of a state’s Medicaid program by comparing family unit structure and income to eligibility rules established

[9] Monthly Current Population Survey data was downloaded from IPUMS-CPS. See www.ipums.org.

within a particular state. More details regarding this simulation are supplied shortly and technical details can be found in Appendix B.

The second source of data, the Common Core of Data comes from a repository of educational data maintained by the U.S. Department of Education's National Center for Education Statistics (NCES). NCES collects both fiscal and non-fiscal data from all public schools in the United States on an annual basis, including the number of traditional diplomas awarded and student enrollment by grade level. Data are supplied directly from state education agencies and uploaded to the CCD; I use the public-use, state-level data in the calculation of four-year high school graduation rates. Diploma and enrollment figures were first documented by the CCD in the early 1990s which means that, given the lag structure required to measure the four-year graduation rate, the first graduation cohort for which a rate can be estimated is 1997. This allows for the construction of a minimal pre-period before the large-scale Medicaid mandates begin impacting children during early childhood years.

Finally, a number of resources were used to compile a database of the rules used to determine Medicaid eligibility for pregnant women and children in each state from 1975 to 1997 (Currie and Gruber, 1994; Hill, 1992; Kaiser Family Foundation, various publications; The National Governors Association, various publications; U.S. Department of Health and Human Services, various publications). This 20-plus year period covers the early childhood years for the graduation cohorts from the class of 1994 to the class of 2010. As with the other variables, more details regarding this database are provided in the forthcoming sections.

2.5. Empirical Strategy

This section outlines three vital components of this empirical analysis. It starts with a general discussion of the requirements for the identification of a casual effect of increased access to public health insurance for low-income children on the long-term public high school completion rates. Other portions describe the construction and findings from the two variables of central importance in this paper: the simulation of the generosity of the state-level Medicaid program, and the estimation of public high school completion rates in the United States.

2.5.1. Identification of a Causal Effect

This paper builds off of literature which uses estimates of the generosity of a state's Medicaid program for children as a time-varying, exogenous source of variation in a quasi-experimental research design (Currie and Gruber, 1994; Yelowitz, 1995; Currie and Gruber, 1996a; Currie and Gruber, 1996b; Currie and Gruber, 2001; Ham and Shore-Sheppard, 2005; Gruber and Simon, 2008; DeLeire et al., 2011; Cohodes et al., 2014). Employing a form of the methodology adopted by these authors, I combine fixed-effects modeling with simulated Medicaid eligibility – using a nationally representative sample of CPS data and the eligibility requirements of state-level programs – to investigate the causal impact of healthcare expansions to low-income children on the subsequent high school completion rates. Exploiting the timing of Medicaid expansions to women and children, which varied significantly across geographic areas in terms of the percentage of the population potentially eligible, I estimate an intent-to-treat (ITT) effect of these expansions on the high school completion rates. The general estimation strategy can be written as follows:

$$\textbf{(1)}\ \ (\boldsymbol{Completion\ Rate})_{scg} = \boldsymbol{\alpha} + \boldsymbol{\beta}\begin{pmatrix}\%\ \boldsymbol{Early\ Childhood\ Years} \\ \boldsymbol{Eligibile\ for\ Medicaid}\end{pmatrix}_{scg} + \boldsymbol{\delta}_s + \boldsymbol{\zeta}_c + \boldsymbol{\xi}_g + \boldsymbol{\varepsilon}_{scg}$$

where: **Completion Rate** is measured by either the CPS dropout or CCD graduation rate for a given state (s), cohort (c), and race/ethnic group (g);
% Early Childhood Years Eligible for Medicaid is the percentage of all early childhood years potentially eligible for Medicaid under existing state laws for a particular race/ethnic group in a graduation cohort;

$\boldsymbol{\delta_s}$, $\boldsymbol{\zeta_c}$, and $\boldsymbol{\xi_g}$ are state, cohort, and race/ethnic group fixed effects, respectively,
$\boldsymbol{\varepsilon_{scg}}$ is the error term, which is clustered at the state level, and
all models are weighted by the number of relevant individuals residing in a state for a particular cohort and group.

The major challenge in this research is to construct a plausibly exogenous measure of the generosity of a state's Medicaid program during early childhood. Since this variable is the key to my identification strategy and any causal claims, I discuss issues in estimation and potential empirical solutions, as well as describe – in detail and in a separate section – the estimation procedure used to simulate this variable. As is common in quasi-experimental research designs, two major sources of bias in the estimation of β are particularly relevant: (1) simultaneity between the outcome and main explanatory variables, and (2) other forms of omitted variable bias.

The main concern with using *actual Medicaid use* rather than a measure of the generosity of the rules governing access to the state-level plan is that strategic behavior by local residents can lead to changes in Medicaid enrollment (e.g., local residents choose an income level to qualify for benefits), yet this does not represent a real change in access to public healthcare. Consequently, and considering the within-estimator specified in the fixed-effects model above, an "effect" could be attributed to this strategic behavior by the child's parents, which could be influenced by third factors impacting completion rates.[10] A more convincing independent variable is one which is *exogenously determined from the vantage point of the aggregated individuals within a state.* Therefore, a covariate based upon the series of federal mandates

[10] One example: parents' education level, which may be a function of the ability endowments they bestow to the child, affects their potential earnings level. This, in turn, could influence their choice of an income level, one which qualifies them for the public insurance program.

leading to legislative changes in access to state-level child Medicaid programs could provide an exogenous measure of program generosity.

Restating the problem more generally, actual Medicaid use is probably correlated with other factors impacting early childhood health, the probability of family income falling below specified income levels, and high school completion rates. Consequently, Medicaid utilization is likely endogenous; DeLeire et al. (2011) provide a comprehensive, recent discussion of why other techniques must be employed. Given this issue of endogeneity, I adopt a form of the methodology established in the literature and use individual-level data to simulate the percentage of all March CPS sample children who would have qualified under a state's eligibility requirements in a given year, regardless of where they reside. This procedure yields a measure of the state plan's generosity because it is not dependent upon the characteristics or choices of the residents currently living within that state but simply the eligibility requirements established by the state legislators,[11] which were determined, in part, by federal mandates. Details regarding these simulations are provided in the next subsection and, moreover, a host of alternative estimation strategies are examined in the robustness checks section to analyze the sensitivity of my estimates to different simulation choices.

Other types of omitted variables can result in biased estimates of the relationship between Medicaid expansions and the high school completion rates. To isolate a causal effect after constructing the plausibly exogenous measure of the generosity of a state's Medicaid program, other variables potentially linked with Medicaid eligibility during the formative early childhood years and graduation rates more than a decade later must be included. Unfortunately, it is

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[11] In addition, the values produced in the simulation are meaningful in a statistical sense, especially when considering a within-state analysis. For example, a simulated value of 20% means that the program is twice as generous as programs where only 10% of the early childhood years for a given cohort are potentially coverable by Medicaid.

theoretically unclear as to what variables could be correlated and when they should be measured. Given this conceptual ambiguity, I choose to address these other forms of omitted variable bias through a variety of econometric demeaning techniques – including fixed effects and time trends – and to test the sensitivity of my finding under a range of definitions of Medicaid generosity.

Fixed effects address a number of potentially relevant, unobserved factors in this analysis. Given that states can differ in their historical completion rates for a variety of reasons, state-specific fixed effects can be used to account for factors which are time-invariant within a given state (such as general levels of spending per pupil or general marginal propensities of graduation). Race/ethnic group fixed effects hold constant for historical gaps in high school completion rates which may affect black, Hispanic, and white students at an aggregated level (e.g., across the entire U.S.), regardless of the time period. Extending these two constructs, state-race fixed effects are an even more flexible form of state-specific and race/ethnic group fixed effects. They control for differential graduation levels by race/ethnic groups *residing within the same state*. In other words, this functional form allows whites in Alabama to have historically different graduation rates than black students in that same state and, importantly, this racial differential – if existing – can vary in magnitude by the individual state.

Cohort-specific fixed effects can be used to control for macro factors affecting graduation trends in a particular year, such as the economy or binding federal education mandates. Modeling with state, cohort, and race/ethnic group fixed effects – which are indicated by $\boldsymbol{\delta_s}$, $\boldsymbol{\zeta_c}$, and $\boldsymbol{\xi_g}$ in Equation 2 – imply that identification of an impact rests upon the comparison of graduation rates *within a state* for cohorts exposed to varying levels of Medicaid generosity during early childhood, while simultaneously controlling for (1) unobserved factors affecting all students at a macro level within a chosen cohort, and (2) general differentials in propensities to complete high school for each race/ethnic group. Stated differently, if all states are experiencing increases in both high school completion and Medicaid eligibility (which they generally are), then identification of a positive estimate of β occurs only if states with greater increases in the generosity of their state Medicaid programs also experience larger increases in their long-term high school completion rates. Modeling with state-race fixed effects is interpreted similarly, but identification now occurs from changes within a state-race group rather than only a state.

In addition to controlling for time-invariant unobservables, other strategies account for the possibility that graduation rates are evolving differently across states. State-specific time trends identify impacts of Medicaid expansions only when high school completion rates exceed the level which would have been expected after controlling for the existing trends in completion.[12] Secondly, state-cohort fixed effects fully drop the linearity assumption implicit in the use of time trends. Under this specification, an effect is identified when increases in Medicaid generosity to a particular race or ethnic group residing within a state result in greater than anticipated gains in the high school completion rates, after accounting for all other factors. In other words, it can test whether the group receiving the greatest gains in access to public healthcare also experience the largest increases in completion rates. When included with the other techniques discussed above, this specification is the most stringent test of an effect and, potentially, the most convincing estimate of a causal impact because it can capture time-varying, unobserved factors at the state-level. All of these fixed-effects methods can significantly reduce the probability of an important omitted variable biasing estimation relative to the form presented in equation 1 above.

[12] Since the panel of data used in this analysis is long, I allow for quadratic time trends. Results are similar in magnitude when estimated with linear time trends.

2.5.2. Medicaid Eligibility Simulations

Having addressed the challenges in estimating a causal relationship between increases in the generosity of state-level child Medicaid programs and longer-term high school completion rates, it is useful to discuss a few elements of the simulation process. Appendix B contains a number of technical details required to accurately estimate the generosity of the state-level Medicaid program – as proxied by the percentage of children in a graduation cohort who would have been eligible for Medicaid during their early childhood years. This section broadly covers two steps used in this process: (1) the construction of a Medicaid eligibility rules database, and (2) the simulation of program generosity using CPS sample data.

The first step in the Medicaid eligibility simulation process is to properly document and categorize the large volume of legislative changes affecting qualification for child Medicaid and Aid to Families with Dependent Children (AFDC) from 1975 to 1997, which covered the early childhood years for the graduation cohorts from 1994 to 2010.[13] Over the range analyzed, there was a large degree of heterogeneity in the laws governing qualification for Medicaid benefits for both pregnant women and children. Timing and stipulations governing the access to care appeared to be essentially random from the perspective of individuals living within a state until the federal mandates became binding at various junctures. And, as noted, the removal of the family structure restrictions is particularly important for certain race/ethnic groups. These differences provide the exploitable source of variation which can identify coefficients in a causal analysis.

Once this database of state-level requirements for Medicaid qualification is compiled, the second major phase is to use data from the March CPS to estimate the generosity of a state's

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[13] See Table 1 for more detail regarding the ages and years required to estimate eligibility for all cohorts in the sample.

Medicaid program during a cohort's early childhood years. Like other researchers in the academic literature – most notably Currie and Gruber (1994, 1996a, 1996b), I use a national sample of March CPS children age 0 to 5 – e.g., all children regardless of their original home state and early childhood age[14] – and statistically ask the question: *conditional on their family structure and family income level, would they have qualified for Medicaid had they lived in a particular state in a given year?*[15] As Table 1 outlines, I perform this exercise for seven different CPS years for a single cohort – from conception through age 5 – and then take the simple average of these seven years to define the variable *% of Early Childhood Years with Medicaid Eligibility*.[16] Mathematically, this calculation for a particular state (s) and graduation cohort (c) can be written as follows:

$$(2)\quad \begin{pmatrix} \%\ Early\ Childhood\ Years \\ with\ Medicaid\ Eligibility \end{pmatrix}_{sc} = \frac{1}{7}\left[\sum_{y=c-19}^{c-13} \frac{\sum_{i=1}^{n} CPS\ Weight_i * Medicaid\ Eligibility_s}{\sum_{i=1}^{n} CPS\ Weight_i}\right]$$

where: the simulation is estimated from cohort c=1994 to c=2010;
i represents an individual March CPS observations from year (y) for a child aged 0 to 5;
Medicaid Eligibility is an indicator variable which is 1 when the family unit or individual child qualified for Medicaid benefits under a particular state (s) legislative thresholds and 0 otherwise; and
CPS Weight are person weights reported by the March CPS.

The corresponding output from Equation 3 is the average number of child-years potentially coverable by a state Medicaid program for a nationally representative sample of children. This is

[14] Parents in the CPS data appear to become wealthier as their children age. Thus, to avoid eligibility changes resulting from a changing demographic, the same sample of children aged 0 to 5 are used to simulate eligibility for all early childhood years estimated from a single March CPS following the mapping outline in Table 1.
[15] Families were defined by the most disaggregated units identified within the CPS data. Total family income less certain time-varying disregards were compared to income thresholds established by the individual state.
[16] The CPS and CCD do not provide the individual-level data required to simulate early childhood eligibility. As such, I need to make the assumption that students graduate, on average, at age 18 and benefits during early childhood are covered by the March CPS years as outlined in Table 1. This assumption should not be problematic so long as the age composition of the graduation class is not changing greatly from the class of 1994 to the class of 2010 in a given state. Moreover, the size of the expansions in the latter period, the smoothing of the estimates over the seven early childhood years, and the use of the within-estimation in the fixed-effects estimation should further mitigate any concerns over this procedure.

a plausibly exogenous measure of the generosity of a state's Medicaid program during early childhood for reasons outlined earlier in this text. Moreover, the simulation methodology outlined above can be easily altered to estimate eligibility by race and ethnic group.

The simulation contains three assumptions which are important to disclose. To start, the use of equal weights for each early childhood year contains the implicit supposition that each year of potential Medicaid coverage is uniformly important to a child's development and long-term probability of high school completion. This enters equation 2 through the 1/7 term. Although insurance coverage could be more important earlier in a child's development, it is theoretically unclear how the years from conception through age 5 should be weighted. Due to this ambiguity, I examine other potential measures to test the sensitivity of my preferred estimation strategy.

Two other assumptions stem from the lack of administrative or individual-level data following the potential graduate from early childhood through their high school years. The first is that any potential distortions in estimation from individuals migrating from state to state are minimal. Selective migration towards states with more generous Medicaid programs would cloud the relationship between those with eligibility increases and those not benefiting from legislative changes. Most likely, this would lead to attenuation bias in estimation due to misclassification error. Secondly, as an important reminder, I make the additional assumption that potential graduates would have finished at age 18, on average, as outlined in Table 1. This allows me to match the early childhood years in a consistent manner across cohorts but could also lead to misclassification error and attenuation bias in estimation if this central tendency is changing over time.

Those caveats aside, the simulated percent of early childhood years with Medicaid eligibility are shown, by state and for all children, in Appendix Table C1. Some important items to recall when interpreting these numbers: simulated values are estimated by graduation cohort and the value reported is the number of child-years potentially covered by Medicaid from conception through age 5. Estimates are a quantifiable and comparable measure of a state Medicaid program's generosity over time. Examples can help clarify the interpretation of this variable: 10.9% for Alabama's class of 1997 indicates that 10.9% of the early childhood years for the national sample of CPS children would have been covered under Alabama's eligibility requirements for child Medicaid. Under the eligibility simulation method established in the literature, the same exact CPS children are also run through the eligibility requirements for all other states in the same year and, as in places like California at 20.2% or Arizona at 4.4%, the percent of child-years covered can be higher or lower depending upon the state-level eligibility requirements. Thus, these simulations quantify the generosity of coverage in the various state-level Medicaid programs for the same set of low-income children during early childhood. In this table, all states experience a marked increase in the percentage of early childhood years covered, which occur, in part, as the federal coverage minimums become binding.

Similar tables were generated by race and ethnic group and are shown in Tables C2 through C4 in the appendix. These are the simulated values used in the core empirical modeling.[17] Figure 2 summarizes these tables with an aggregated depiction of the increases in the generosity of the average state's Medicaid program during early childhood for all U.S. states and by race and ethnic group. Not surprisingly, access to public health insurance increases markedly over time. Another striking feature of this graph is the change in eligibility impacting

[17] Other methods of Medicaid eligibility simulation are examined to reveal the sensitivity of estimates to key modeling choices.

the average Hispanic student. Over the period examined, Hispanic students were often raised in families with marital patterns most resembling whites, but with incomes most closely characterized by blacks. Thus, their estimated Medicaid eligibility during early childhood begins closer to whites. However, as family structure restrictions from child Medicaid are lifted, the fraction of early childhood years increases markedly for Hispanics and converges toward blacks at the end of the sample. This is an important source of exploitable variation.

2.5.3. The Outcome Variables: High School Completion Rates in the United States

The primary goal of this paper is to investigate the causal impact of a single public policy decision – the expansion of health insurance coverage to low-income children – on long-term dropout and traditional four-year high school graduation rates. Given this singular objective, the next two sub-sections bypass the multitude of factors affecting completion trends over the past several of decades.[18] Instead, the first section describes important choices made in the construction of the two rates, as well as outlines the strengths and weaknesses of each measure. More technical details regarding the construction of both measures can be found in Appendix B. The second sub-section contains a general discussion of the trends in U.S. dropout and traditional four-year high school graduation rates from the mid-1990s into the 2000s.

2.5.3.1. Estimation of Dropout and Graduation Rates

Despite being a widely reported statistics used as a barometer for the effectiveness of the public school system, estimation of U.S. high school completion rates is not straightforward,

[18] For those interested in other factors affecting dropout rates in the United States, see the relatively recent, thorough review by Rumberger and Lim (2008). Murnane (2013) also provides a comprehensive analysis of the challenges and trends associated with the public high school graduation rate.

primarily due to conceptual ambiguities and data limitations.[19] Given these challenges, I present and discuss two measures of public high school completion, each of which has strengths and weaknesses. Analyzing both constructs together exposes the true nature of the relationship between child Medicaid expansions and the long-term human capital investments of low-income children.

As previously noted, cohort-specific dropout rates were computed using monthly data taken from the Current Population Survey. As with the Medicaid eligibility simulations, Table 1 outlines how individuals of a particular age were assigned to a graduation cohort, which is defined by when the average student would have turned 18. Two other conditions were used to estimate the dropout rate.[20] Instead of using only age 18 in the construction of dropout rates, the CPS estimates were smoothed by using all sample individuals aged 18 to 20. This approach yields a more accurate estimation of dropout rates for minority groups living in predominately white states because the sample size is greatly increased. Secondly, since the research objective in this paper is to explore the impact of increased access to public healthcare in early childhood, dropout rates are estimated only on CPS respondents *who were born in the United States*. Low-income children not born in the U.S. would most likely either (1) not qualify for public health insurance because of residency requirements, or (2) have some significant delay in access to care during early childhood. While estimates for black and white students are not impacted by this restriction, the magnitude, but not general trends, of dropout rates for Hispanics are. Again, please refer to the technical details in Appendix B for more information.

[19] For a comprehensive discussion of the challenges associated with the estimation of completion rates, please see Heckman and LaFontaine (2010).
[20] As is standard in the literature, a dropout is identified when the CPS respondent has less than a high school level of education and is no longer enrolled in school.

While CPS dropout rates have the advantage that one can exclude respondents not living in the U.S. at the time of their birth – and thus, those may not fully benefit from Medicaid expansions during early childhood – this measure has two other disadvantages. The first is that researchers cannot exclude the GED certificate. The GED is the most common alternative to a traditional high school diploma; however, studies have argued that GED holders do not fare any better in the labor market than high school dropouts (Cameron and Heckman, 1993; Boesel et al., 1998).[21,22] Consequently, care must be taken in the conclusions drawn from an analysis of dropout rates if the percentage of GED holders is increasing over time; this would indicate a decrease in the dropout rate which is not a real long-term gain in human capital.

The second limitation is that the CPS sampling design excludes institutionalized populations. This could be problematic if the sample captured by the CPS is changing significantly over time due to factors such as mass incarceration. If the boom in U.S. prison population differentially impacts racial groups or individuals on the margin of graduation, which it most likely does, then CPS estimates serve as an upper-bound of the true rates. Furthermore, rates could be artificially higher in the later period if dropouts are more likely to be excluded from the CPS sample due to these changing trends in incarceration.

Given the potential limitations of the CPS dropout rate due to the use of the 18-20 year old smoothing technique, the non-excludability of non-traditional diplomas, and the non-sampling of institutionalized populations, a second outcome variable is examined. This measure concentrates on diplomas awarded in the traditional manner: e.g., students who attended an

[21] This effect is generally attributed to the general lack of non-cognitive skills characteristically held by these individuals, such as perseverance and motivation, traits which are essential to success in the academic and professional arenas (Heckman and LaFontaine, 2010).
[22] Furthermore, the federal government has formally recognized the non-substitutability between GED and traditional high school diplomas by excluding GED holders from the count of high school graduates under No Child Left Behind (NCLB) measures.

accredited high school program and received a traditional high school diploma, as discussed in Heckman and LaFontaine (2010). Following Heckman and LaFontaine (2010), I estimate a four-year graduation rate using diploma counts and enrollment data from the Common Core of Data. In this calculation, a graduation cohort (e.g., the Class of 2000) is defined by the number of diplomas awarded in a state in a given year. Thus, diplomas awarded are the numerator. To estimate four-year graduation rates, the number of 8th graders enrolled in that same state 4 years earlier is used as the proxy for the maximum number of potential completers. These enrollees are used as the denominator from which a four-year graduation rate can be constructed. Please see the technical appendix for more details.

While addressing the GED issue, the traditional diploma measure introduces two other limitations. First, students born outside of the United States – and, thus, most likely not qualifying for Medicaid benefits during early childhood – cannot be excluded. Secondly, an implicit assumption of using the four-year graduation measure, especially while using fixed-effects regression modeling, is that any measurement error needs to remain constant over time. When students do not all finish in exactly four years, measurement error on the outcome variable is a potential problem.[23] Under this scenario, degree duration would be an omitted third factor. When correlated with the primary covariate of interest, regression estimates would be biased. Unfortunately, given data restrictions,[24] there is no way to explicitly test the assumption of a constant number of years required for completion within a particular state. Thus, I discuss the direction of the potential bias later in this paper.

[23] In other words, and illustrating via an example, so long as students take, on average, 4.10 years to graduate in Alabama over the period explored in this analysis, then the same level of mismeasurement occurs across each time period, which can be controlled for via standard econometric procedures. A concern would be that the average time towards high school completion is time-varying within a state – e.g., that the time spent towards graduation in the earlier period is statistically different from the amount required in the latter period.

[24] To test this proposition, one would need administrative-level data across all states over a long period of time. This data is not available at a national level.

Neither outcome variable flawlessly captures the trends in public high school completion rates which are most relevant to the child Medicaid expansions of the 1980s and early 1990s. However, the two measures are complementary, strengthening one where the other fails. Thus, consistency in findings from the two measures would establish whether a statistically significant and robust relationship exists between public healthcare expansions to low-income children in early childhood and long-term gains in the high school completion rates.

2.5.3.2. U.S. Trends in the Dropout and Four-Year Graduation Rates

Trends in 18 to 20 year old dropout rates by race and ethnic group are shown in Figure 3. As displayed, rates appear to be flat in the early period and then fall dramatically after the turn of the century. All groups experience large declines in their dropout rates. At an aggregated level, dropout rates for all students fall from approximately 14% in 1994 to 9% in 2010. This represents roughly a 35% decline relative to the original baseline established during the period before the large-scale increases in public healthcare access to low-income children.

Figure 4 presents trends in traditional four-year high school graduation rates for the 1997 to 2010 graduation cohorts for all U.S. students, and by race and ethnic groups. Graduation rates at the aggregate level for all students have generally experienced an upward trajectory in the 2000s, starting at roughly 76% in 2000 and exceeding 82% by 2010.[25] Like dropout rates, improvements were experienced by all groups: black, Hispanic, and white students all experienced marked gains in their graduation rates throughout this period. The primary objective of this paper is to measure the extent to which these advances in completion rates at state-specific levels can be attributable to early childhood Medicaid expansions.

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[25] These trends and estimates are consistent with those presented by Heckman and LaFontaine (2010).

2.6. Descriptive Statistics

Table 2 contains a series of descriptive statistics for the data used to estimate the empirical models. Results are presented for all U.S. students, as well as separately by race and ethnic group. As noted earlier, Medicaid eligibility is estimated by the group of students, which means that the fraction of black, Hispanic, and white students which would have qualified for a state's Medicaid program had they lived in a given state during early childhood varies markedly across both group and cohort. This time-varying measure of Medicaid program generosity at the state level is the identifying source of variation exploited in this analysis, and the fraction of CPS children qualifying for the average state's Medicaid program in early childhood is contained in the third column. Medicaid eligibility rises from approximately 15% of all child-years in the first graduation cohort (1994) to above 40% by the end of the period analyzed (2010). These generosity increases represent almost 2.8 times more child-years eligible for Medicaid.

Table 2 reveals the magnitude by which Medicaid eligibility increases vary across race and ethnic groups. At the start of the time-series, the average black student in this analysis had 40.4% of their early childhood years potentially coverable by Medicaid. By 2010, this number rose to 70.0%. While large in absolute magnitude, this change corresponds to less than a doubling of program generosity. Thus, the marked within-group increases in eligibility are driven by the Hispanics and white students, which were the two groups benefiting most from the decoupling of Medicaid from AFDC. In the CPS samples analyzed, the average Hispanic lived in a state where the generosity of the program increased more than threefold: from 20.7% of all early childhood years coverable in 1994 to 67.4% eligible in 2010. Though not nearly as high in magnitude, whites also experienced a near tripling of eligibility, going from 10.8% in 1994 to 32.1% in 2010.

As discussed in the last section, blacks, Hispanics, and whites all experienced large gains in high school completion rates over the period analyzed. This fact is confirmed by the trends shown in aggregated CPS Dropout Rates and the CCD Graduation Rates.[26] However, since the completion measures and simulated Medicaid eligibility estimates are both increasing over the period examined, it is important to use a variety of econometric techniques to de-trend the data to avoid attributing an effect to the Medicaid expansions when some other third factor is truly driving part of the relationship.

2.7. Empirical Models: High School Dropouts

To explicate findings from my empirical models, I start with the full analysis of the high school dropout rate, which constitutes the most consistent and robust finding of a causal link between child Medicaid expansions and long-term gains in high school completion rates. After dropouts, I discuss the modeling of four-year high school graduation rates, which can address whether gains in completion rates were driven by increases in traditional diplomas or by other, less valuable, forms of high school completion.

2.7.1. Core Modeling

Table 3 contains estimates of the impact of Medicaid expansions in early childhood on the subsequent high school dropout rates, which constitute the core modeling in this analysis.

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[26] One limitation of the CCD data is that states did not always provide complete information on diplomas awarded. For example, two states failed to report diploma counts for all students in 2004, while 3 did not report in 2006. This issue becomes more serious when examining the trends in graduation rates by race and ethnic group, where the earlier period experiences greater frequencies of non-reporting. Here, the maximum number of potential observations is 14 * 51 * 3 = 2142, while only 1875 observations have valid data. A similar issue exists in the CPS data which stems from the lack of a sufficient sample of 18 to 20 year olds to calculate dropout rates for blacks and Hispanics in select states in particular years. In both cases, the length of the panel examined should still facilitate reliable estimates from the unbalanced panel.

Model 1 estimates the functional form proposed in equation 1 above. The three other models are shown in this table are extensions of this base form: Model 2 adds state-race fixed effects, while Models 3 and 4 account for existing trends in state-level graduation rates by exploiting state-specific time trends and state-cohort fixed effects, respectively. All standard errors in estimation are clustered at the state-level to account for the fact that the state-level residuals are probably not independent and identically distributed even after conditioning on the other right-hand-side variables.

Starting with the baseline presented in Model 1, there is a negative and statistically significant relationship between Medicaid eligibility expansions during early childhood and the dropout rate. However, it is easily argued that estimates from Model 1 suffer from omitted variable bias, forms of which are addressed in the other three models. Adding the state-race fixed effects in Model 2 increases the size of the estimated coefficient of interest, as well as decreases the standard error. Once accounting for state-specific time trends in high school completion in Model 3, the statistical precision of the estimate increases even further. The point estimate of -0.2422 can be interpreted as follows: a 10 percentage point increase in the Medicaid generosity of a state-level program resulted in an approximately 2.4 pp decrease in high school dropout rates, holding all other factors constant. Moreover, using state-cohort fixed effects to account for even more of the unexplained variation in factors affecting graduation within a given state, the point estimate increases slightly to 2.5 pp. This last finding strongly suggests that the groups benefitting the most from the Medicaid expansions (e.g., Hispanics and whites) also experience the greatest decreases in the dropout rates because identification now rests upon deviations from the mean within a particular state and cohort.

Summarizing the findings from these models, estimates from the core modeling – which are all estimated with a high level of statistical precision – indicate that Medicaid eligibility expansions led to long-term decreases in the high school dropout rates, with estimates ranging from 1.9 to 2.5 pp for each 10 pp increase in the generosity of the state's Medicaid program. Extending this estimate to the roughly 25 percentage point increase in program generosity generally witnessed by all states during the expansion period reveals a decrease in the dropout rate of between 4.75 to 6.25 pp. Thus, relative to a dropout baseline of roughly 14% in 1994, this indicates a decline of at least one third in the dropout rate, which can be attributed to Medicaid expansions. These estimates are both large and economically meaningful.

2.7.2. Heterogeneity Tests

Findings from the core empirical models and the Medicaid eligibility graphs suggest that racial and ethnic groups may be differentially impacted by the magnitude of Medicaid expansions, because each group starts with different levels of Medicaid access.[27] Table 4 presents formal tests of this proposition by showing the results from group-specific modeling. As the reader may quickly note, the power of the regressions are significantly diminished in the non-pooled models because the number of observations decline by 2/3. However, modeling presented – which corresponds to the first two functional forms in Table 2 – confirms intuition: decreases in dropout rates are greatest for Hispanics, who benefit the most from Medicaid eligibility expansions. Blacks gain the least in terms of their completion rates. Whites reside somewhere in the middle, as with eligibility gains, while the large standard errors on the point estimates preclude the reporting of a statistically significant relationship at conventional levels. Moving

[27] This is shown most noticeably by the trends in Medicaid eligibility expansions by group (Figure 2) and from the models with state-cohort fixed effects (Model 4) in Table 3.

past the smaller sample and power issues, there are two other reasons why whites could gain from access to public health insurance despite this finding in the disaggregated modeling. To start, the additional fixed effects in the pooled modeling increase the precision of the estimates, yet this important source of variation cannot be identified within the single group model.[28] Moreover, since regressions are weighted by the relevant number of students, whites have a disproportionate weight in pooled modeling. Thus, if the true impact on whites was zero, the finding of a statistically significant result would not occur in the larger sample because results are driven by the central tendency for whites. These facts, when coupled with the issues previously established, indicate that whites also benefit significantly from the early childhood public health insurance expansions.

2.7.3. Alternative Measures of Medicaid Eligibility, Part I: Fixed Cohort Demographics

Given the consistency of coefficients presented in Table 3, concerns regarding estimation bias from unobserved omitted variables should be mitigated. The second major issue is to test whether choices and assumptions made while constructing the *% of Early Childhood Years with Medicaid Eligibility* inadvertently drives the statistically significant relationship between expansions in public health insurance and high school dropout rates. To meet this objective, I examine eight alternative estimates of a state's Medicaid program generosity during the early childhood years, analyses which investigate whether CPS sample selection or length of potential Medicaid exposure differentially impact the estimates presented thus far. To ensure that changes in sample composition over time are not driving the findings, the first series of models examine

[28] To be clearer, the state-cohort fixed effects identify unobserved factors which are changing over time within the same state. Examples would be per pupil spending or graduation requirements. This potentially important source of bias cannot be accounted for in the single group modeling because there is only one observation per state and year.

the impact of fixing CPS demographics to a single sample of individuals choosing their family structure and income levels. The second set tests whether the duration of Medicaid exposure during early childhood matters. Having established that the dropout results are driven by Hispanics and whites, all of these robustness checks exclude black students.

Table 5 contains estimates derived from fixing the sample to three distinct March CPS years: 1975, 1980, and 1985.[29] This set of analyses investigate whether the changing CPS sample impacts the relationship between Medicaid generosity and dropout rates by fixing the cohort demographics to a single CPS year and then using CPI adjustment factors to convert family earnings into the nominal dollars required to determine eligibility for AFDC or child Medicaid eligibility within a given state-year.[30] By choosing different fixed samples, I can potentially alleviate lingering concerns of strategic behavior by a subset of families who may choose their income level in order to qualify for public assistance programs in a particular state and year.

Table 5 starts with the core modeling estimated with Hispanic and white students only. Coefficients are larger than those presented in Table 3 because black students were driving the coefficient towards zero. As shown across a variety of specifications, results from the fixed CPS sample are consistent with the limited core modeling, although the point estimates are often larger than what was previously reported for the more highly specified models. Excluding the potentially biased estimates presented in Model 1, estimated impacts range from roughly a 1.7 to 4.0 pp decrease in the high school dropout rate for each 10 pp increase in the generosity of the state's Medicaid program.

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[29] When interpreting this table, please note that each cell represents a separate regression model.
[30] To inflate the fixed CPS year (e.g., 1975, 1980, or 1985) earnings to "contemporaneous" values, I use a composite CPI index created from the CPI-U-X1 and CPI-U-RS series constructed by the Bureau of Labor Statistics.

While this methodology leads to larger estimates of the impact of Medicaid expansions, it suffers from the primary criticism that the use of a CPI inflator tacitly contains an unreasonable assumption, namely that wages – especially those for low-wage workers – rose exactly by the amount of inflation in a given year. Adjusting income under this methodology understates generosity during a high inflationary period – which corresponds to the baseline period – because the CPI adjustment factor allocates more income to low-income families then they would have reasonably earned given market constraints.[31] Although limited, this approach lends support to the finding of an impact of public health insurance expansions during early childhood on the subsequent long-term completion rates; it indicates that the use of the contemporaneous CPS sample during early childhood is not arbitrarily driving the finding of a statistically significant relationship between Medicaid eligibility expansions and fewer high school dropouts. Fixing the demographics to a single year, if anything, would lead to larger estimates.

2.7.4. Alternative Measures, Part II: Tests of the Potential Exposure to Medicaid

The remaining five alternative definitions of Medicaid eligibility test what happens when the dose of Medicaid treatment is altered statistically or, in other words, as the cumulative duration of Medicaid eligibility "received" changes. Since it is theoretically unclear how much Medicaid exposure is required to produce an effect, I examine point estimates when eligibility is estimated (1) as the lower bound of coverage, which is defined as the minimum percentage of the cohort covered in any single year, (2) as the upper bound of coverage, which is the maximum percentage of the cohort covered in any single year of early childhood, (3) during the conception

[31] Inflation rates in the late 1970s and early 1980s often exceeded 10% in a single year and were above 5% in a number of other years in this analysis. To maintain the assumption required by use of the fixed sample from either 1975, 1980, or 1985, low-skilled wages would also need to rise by the same amount. This assumption is implausible given sticky wages and minimum wage regulations.

year only (e.g., prenatal care and birth), (4) from conception through age 2 in the traditional manner, and finally, (5) coverage from age 3 to age 5, also with the core methodology established earlier.

The latter cases are relatively straightforward in their construction and interpretation: by examining a subset of ages potentially covered during early childhood – conception year only, from conception through age 2 and from age 3 to age 5 – I examine whether eligibility in the earlier years is more important than eligibility in the latter ones. As other measures of the duration of Medicaid eligibility, I also estimate the lower- and upper-bound of any potential Medicaid coverage, which technically envelop the *% of Early Childhood Years with Medicaid Eligibility* variable, which has been the focal point of this entire analysis.[32] The lower-bound of any coverage is defined by the minimum percentage of the estimated eligibility for any single year of early childhood and seeks to proxy the maximum number of children within a state-cohort which could have received treatment *throughout the 7 years of early childhood*. The second measure – the upper-bound of any coverage – attempts to measure the maximum number of children within a state-cohort who could have ever qualified for coverage during their childhood, at any time.

The second series of findings in Table 6 contain estimates from the lower-bound of the estimated Medicaid eligibility percentage in any single year, which again, seeks to proxy the maximum number of children which could have received benefits in all seven years. This

[32] A numerical example should help clarify the calculation of the lower- and upper-bounds. As provided in the appendix, the estimated percent of early childhood years with Medicaid eligibility for all students in the graduating class of 1997 in Alabama was 10.9%. This number is constructed as the simple average of the simulated eligibility for the seven years from conception through age 5 or the CPS simulated estimates of 10.7%, 10.5%, 10.5%, 10.0%, 9.5%, 10.0%, and 15.2%, respectively. To estimate the lower-bound of coverage in any single year for a cohort, one simply takes the smallest value from the seven years; here it is 9.5%. To estimate the upper-bound of coverage, one uses the maximum number of CPS respondents covered in any single early childhood year, which is 15.2%. These are the lower- and upper-bounds of potential coverage because they envelop the simple average of all seven years which is used in the main modeling.

measure of the cohort "always covered" during early childhood produces statistics estimated with a high degree of statistical precision and which substantiate estimates presented in other sections. The models report impacts on high school dropout rates ranging from 1.8 to 2.3 pp for each 10 pp increase in Medicaid program generosity. Combined with the findings from the third estimation exercise – which is a proxy for the maximum percentage of the state-cohort ever potentially qualifying for Medicaid insurance – it appears that qualifying for Medicaid benefits at some point during early childhood leads to the health and cognitive development benefits outlined earlier. That stated, there is some evidence that there may be less of an impact as expansions reached the upper tail of the low-income distribution as indicated by the smaller and less precise estimates derived from the upper-bound exercise, especially in Models 1 through 3.

Finally, when potential eligibility is examined in the conception year only, from conception through age 2, and from age 3 to age 5, coefficients are essentially in line with the point estimates of 1.9 pp to 2.5 pp derived from core modeling. Given these findings, it does not appear that any of the periods differentially impact high school graduation rates, i.e., the choice of equal weighting to each of the early childhood years does not appear to be consequential. Thus, while the methodology presented in this paper cannot precisely identify exactly which early childhood period is most crucial – if there really is such a period – the link between eligibility expansions from conception through age 5 and the long-term dropout rates is strong and robust to a number of alternative estimation procedures.

2.8. Empirical Models: Traditional Four-Year Graduation Rates

Given the consistency and robustness of findings across the various models examining dropout rates, this section examines whether fewer high school dropouts translated into more traditional high school graduates. As noted, holders of non-traditional diplomas do not fare better in the labor market than high school dropouts. Thus, to have a real influence on the human capital accumulation of low-income children, Medicaid must alter the number of traditional diplomas instead of other vehicles to graduation, such as the GED.

Table 7 presents a simplified version of the core modeling outlined in Table 3. While not nearly as precise as the dropout modeling, coefficients on the Medicaid generosity variable indicate a significant and robust relationship between increases in the percentage of early childhood years with Medicaid eligibility and the long-term traditional high school graduation rate. Estimates from modeling with black, Hispanic, and white students range from a 1.0 to 1.3 pp increase in completion rates stemming from a 10 pp increase in state program generosity. Again, extending these point estimates to the over 25 pp increase in eligibility in the average state, this suggests an increase in the four-year graduation rates of between 2.5 to 3.25 pp which can be attributed to Medicaid expansions.

Findings for Hispanic and white students only are very similar to the coefficients reported for the three race/ethnic groups. Though similar in magnitude to the other point estimates, Model 4 coefficients under both specifications are no longer statistically distinguishable from zero. This fact indicates that the greatest beneficiaries of the Medicaid expansions – Hispanics – may not be experiencing the largest gains in four-year graduation rates. While contrary to the other findings, this is a reminder of one of limitations of the CCD data: one cannot exclude students likely to have been ineligible for the large increases in access to public healthcare during early childhood. Thus, to the extent to which graduation rates are diluted by recent immigrants for a particular group – which they almost certainly are for Hispanics – then the estimates presented serve as a lower-bound of the true impact. Consequently, it does not seem unreasonable to conclude that the gains in the decreased dropout rates translated into more traditional diplomas and that Hispanics and whites propel this finding.

The same set of robustness checks examined with the dropout models can be applied for the four-year graduation rates. For the sake of brevity, they are not presented in this paper. In general, coefficients are similar to those presented in Table 7, though estimates can be less statistically significant. This precision issue highlights another advantage of the CPS dropout rate measure: it has a much longer time series at baseline, as it starts in 1994 as opposed to 1997. Recalling Figure 3, this extended period is important to establish a baseline of Medicaid program generosity within a state before the large scale public insurance expansions.

2.9. Discussion and Conclusions

Seeking to examine the long-term impact of early childhood investments by the U.S. government in the form of increased healthcare access to low-income children before they enter primary school, this paper presents evidence that the Medicaid expansions to qualifying children throughout the 1980s and early 1990s led to an increase in the high school completion rates in the 2000s. By exploiting the large degree of heterogeneity in policy implementation of the public insurance expansion mandates, as well as econometric techniques to account for otherwise unobserved factors which cause certain states or race/ethnic groups to have differential trends in graduation, I find a positive, consistent, and statistically significant relationship between Medicaid eligibility expansions during early childhood and longer-term high school completion rates.

The results presented in this paper are economically significant. For dropouts, the 1.9 to 2.5 pp decline in dropout rates for each 10 pp increase in public insurance program generosity translates into approximately a 4.75 to 6.25 pp decline in overall dropout rates from 1994 to 2010. Relative to the estimated 14.4% dropout rate for all students in 1994, this suggests a 33 to 43% decrease in the number of students exiting high school without a diploma or equivalent degree. Furthermore, dropout impacts appear to be driven by Hispanic and white students, the two groups benefiting the most from increased within-group access to public health insurance.

To test whether these gains impacted traditional manners of high school graduation, and not imperfect substitutes such as the GED, I also examined four-year graduation rates using traditional diploma counts from the Common Core of Data. The intent-to-treat estimates of a 1.0 to 1.3 percentage point increase in four-year graduation rates for each 10 pp increase in child-years potentially covered by a state's Medicaid program implies that – on a base of roughly a 25 pp increase for the average state – there were 95,000 to 124,000 more graduates across the U.S. in 2010 due to public health insurance expansions and healthier low-income children. Moreover, improvements appear to be shared by all race and ethnic groups. This exercise confirms that gains from public healthcare access did not stem from non-traditional means of high school completion, which further indicate that these advances represent real improvements in long-term human capital accumulation for a potentially vulnerable population.

This paper corroborates findings from two other recent working papers in the literature which find substantial positive impacts on educational attainment and labor market outcomes stemming from the child Medicaid expansions of the late 1980s and early 1990s (Brown et al., 2014; Cohodes et al., 2014). In particular, it complements and extends Cohodes et al. (2014) by more precisely targeting the source of the completion rate gains (Hispanic and whites), as well as deriving more precise estimates of the effect by exploiting a longer data panel and other sources of data. However, work in this arena is not without its current limitations. Important items left for future research are to unpack the mechanisms prompting these positive effects and to better understand when public insurance interventions matter the most. Stated another way, current research has not identified what exactly facilitates these increases in performance. Is it from the general increase in child health, increases in cognitive and non-cognitive development before entrance into primary school, the potential increase in seat-time for students who otherwise would have been battling health issues in the absence of insurance, a more positive predisposition towards academics, or other factors related to the benefits of health insurance, including income effects? Furthermore, it is still unclear as to when public insurance matters the most: is it *in utero* as claimed by those prescribing the fetal origins hypothesis, throughout early childhood as supported by this paper, or throughout the entire childhood (e.g,. ages 0-17) as analyzed by Cohodes et al.? Other datasets, sources, and methodologies are required to unravel these mechanisms and to evaluate when these interventions have the greatest impacts.

Finally, there may be lingering concerns over the measures of completion explored in this analysis. Presumably, arguments would be rooted in a measurement error critique, one which would have to further assume non-classical error (since classical error on an outcome variable simply leads to larger standard errors, but no bias in estimation). In the construction of 18-20 year old dropout rates, the smoothing technique would be problematic if it fails to adequately account for some time-varying aspect of completion which is correlated with treatment (e.g., early childhood Medicaid expansions). While migration to other states after high school would influence the general completion levels within a state, it is still not obvious how a source of omitted variable bias would work under this scenario, especially given the other panel data controls in the modeling.

Critiques of the four-year graduation rate could be more valid. Some race and ethnic groups – such as black and Hispanics – may take longer, on average, to graduate from high school than the standard of four years (Murnane, 2013). Consequently, these students would not count as diploma holders in time period t (the numerator of the four-year graduation rate calculation) which is compared to the number of students enrolled in 8th grade at time period t-4 (the denominator). Like the dropout rates, this is not problematic so long as the marginal propensity of completion remains constant over the time period examined, as this constant measurement error is accounted for using the panel data techniques employed in this paper. However, it would be a concern if these tendencies are time varying and occur simultaneously with Medicaid expansions to low-income children. In other words, a biased coefficient results if blacks or Hispanics in states with large Medicaid expansions are increasingly finishing within four years and the sequence of these two events is highly correlated. Although it appears as though this issue is ignored by those using the CCD in the academic literature because there is no obvious solution – it would imply that the estimates derived in this analysis serve as an upper-bound of the effect of Medicaid expansions. That stated, the robustness of the findings across the two definitions of completion and the various constructs of Medicaid eligibility, concerns regarding measurement error on the outcome variable should be abated.

To conclude, academic accountability studies, early childhood investments, and the impact of Medicaid expansions have all received a considerable amount of attention in the academic literature. This paper extends this work by examining how government investments in the form of increased healthcare access in early childhood for low-income children impact longer-term outcomes. Findings from this research reveal a large decline in dropout rates and a complementary increase in the four-year completion rates. For the latter, the 2.5 to 3.25 pp increase in the high school graduation rate stemming from the increases in healthcare access, which explains the majority of the recent 6 pp increase in the U.S. graduation rates reported by Murnane (2013). Policy implications of these findings are also meaningful given the high correlation between education and outcomes deemed generally desirable to a society: as individuals become more educated they are less likely to become reliant upon governmental programs as adults, less likely to engage in criminal activities, and more likely to be attached to the labor market. Thus, it appears as though the Medicaid expansions to children throughout the 1980s and early 1990s generated social benefits well beyond “saving babies” and “free healthcare” for qualifying low-income children during early childhood.

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Figure 1
Trends in CPS Children Aged 0 to 5 Residing in Two Parent Families: By Group

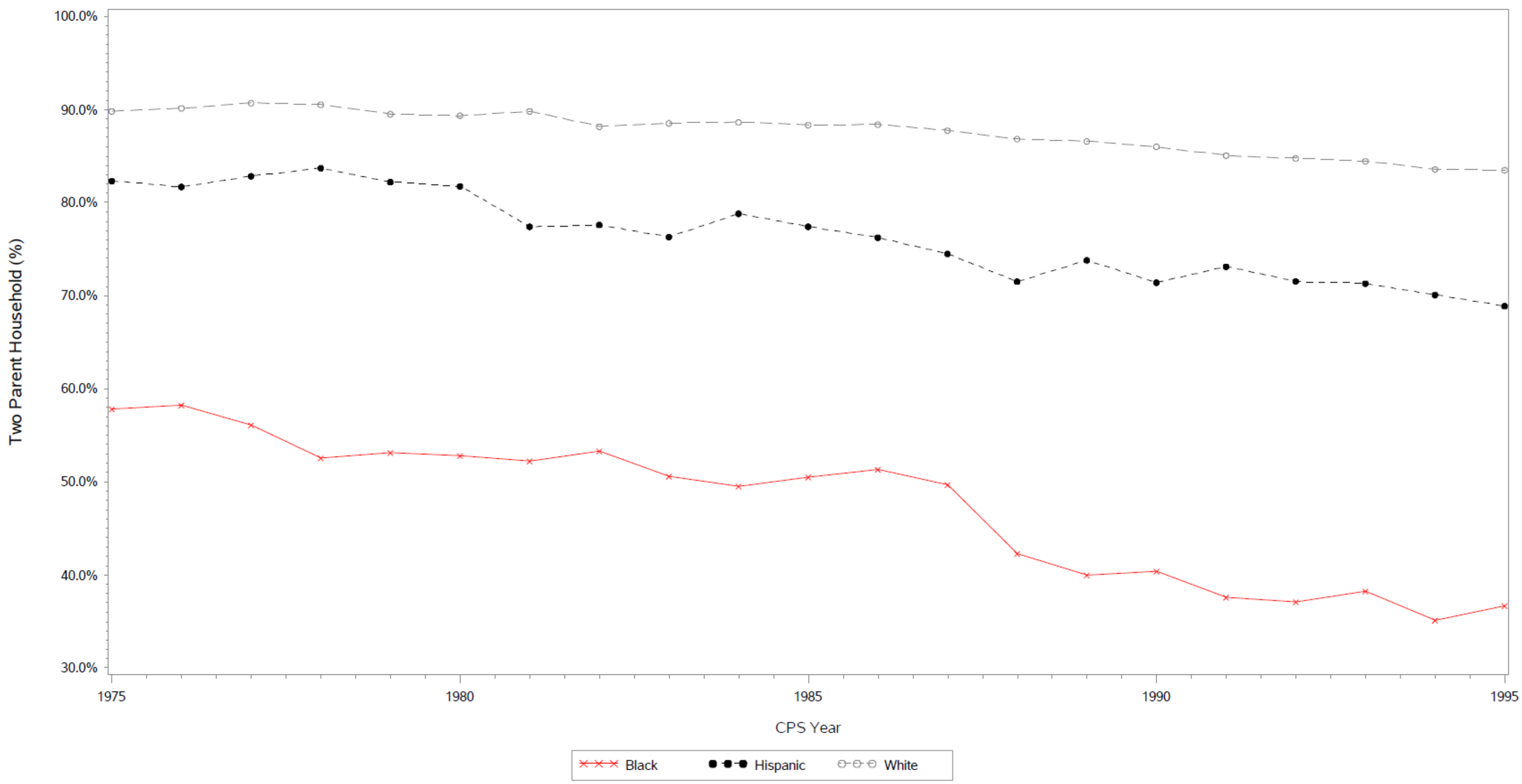

Figure 2

U.S. Trends in Medicaid Eligibility Expansions by Group

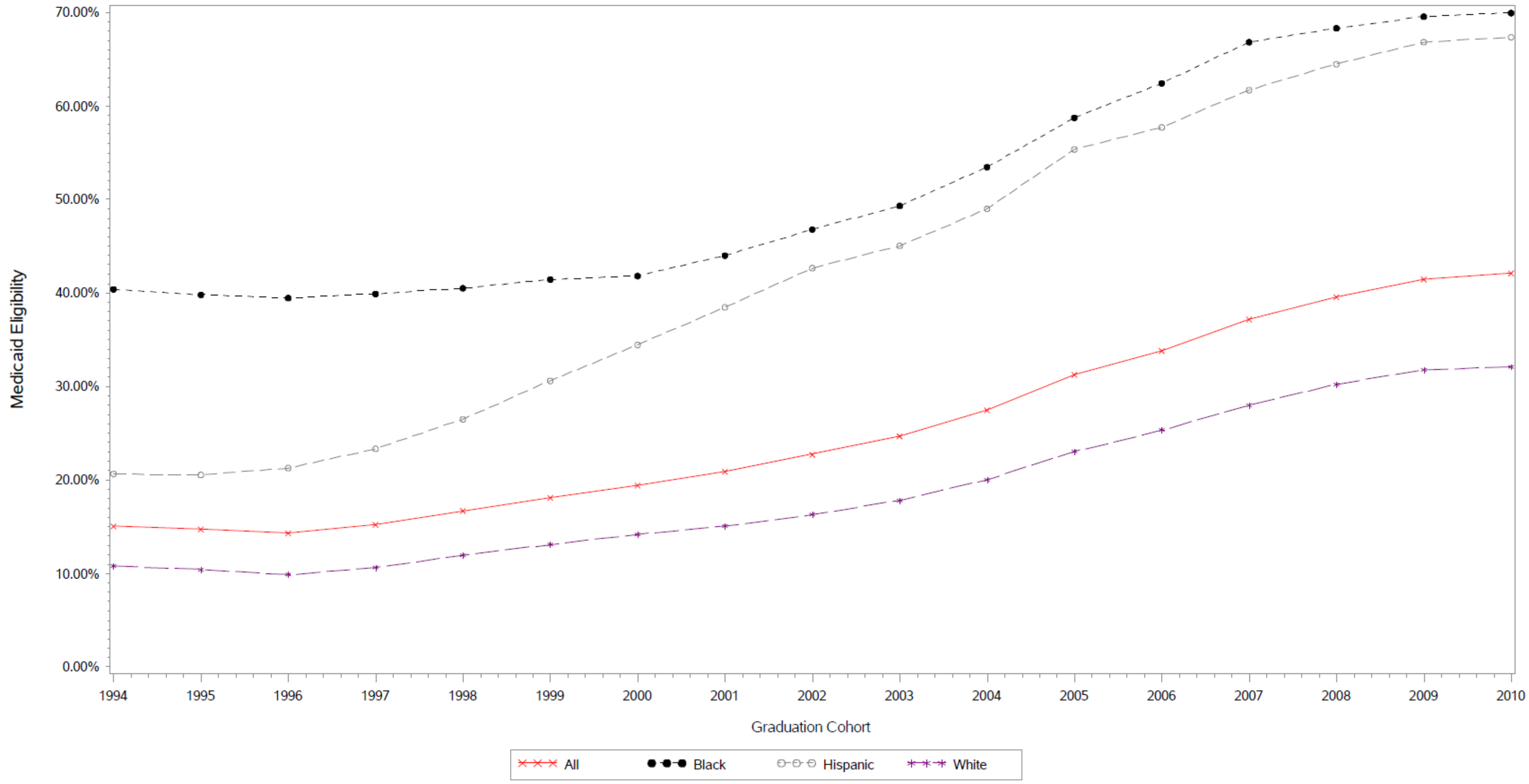

Figure 3

U.S. Trends in 18 to 20 Year Old Dropout Rate by Group

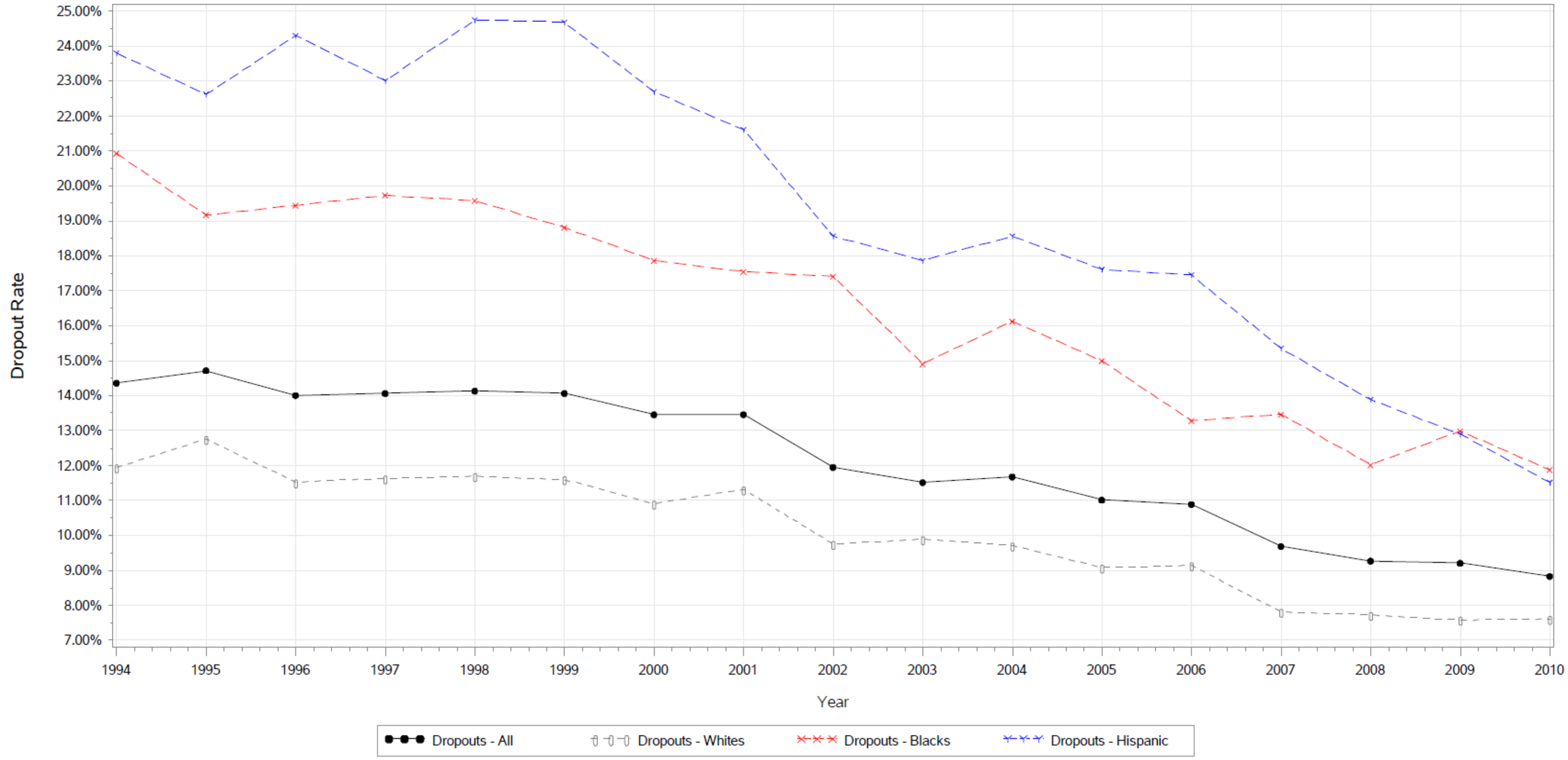

Figure 4

U.S. Trends in the Four-Year Graduation Rate by Group

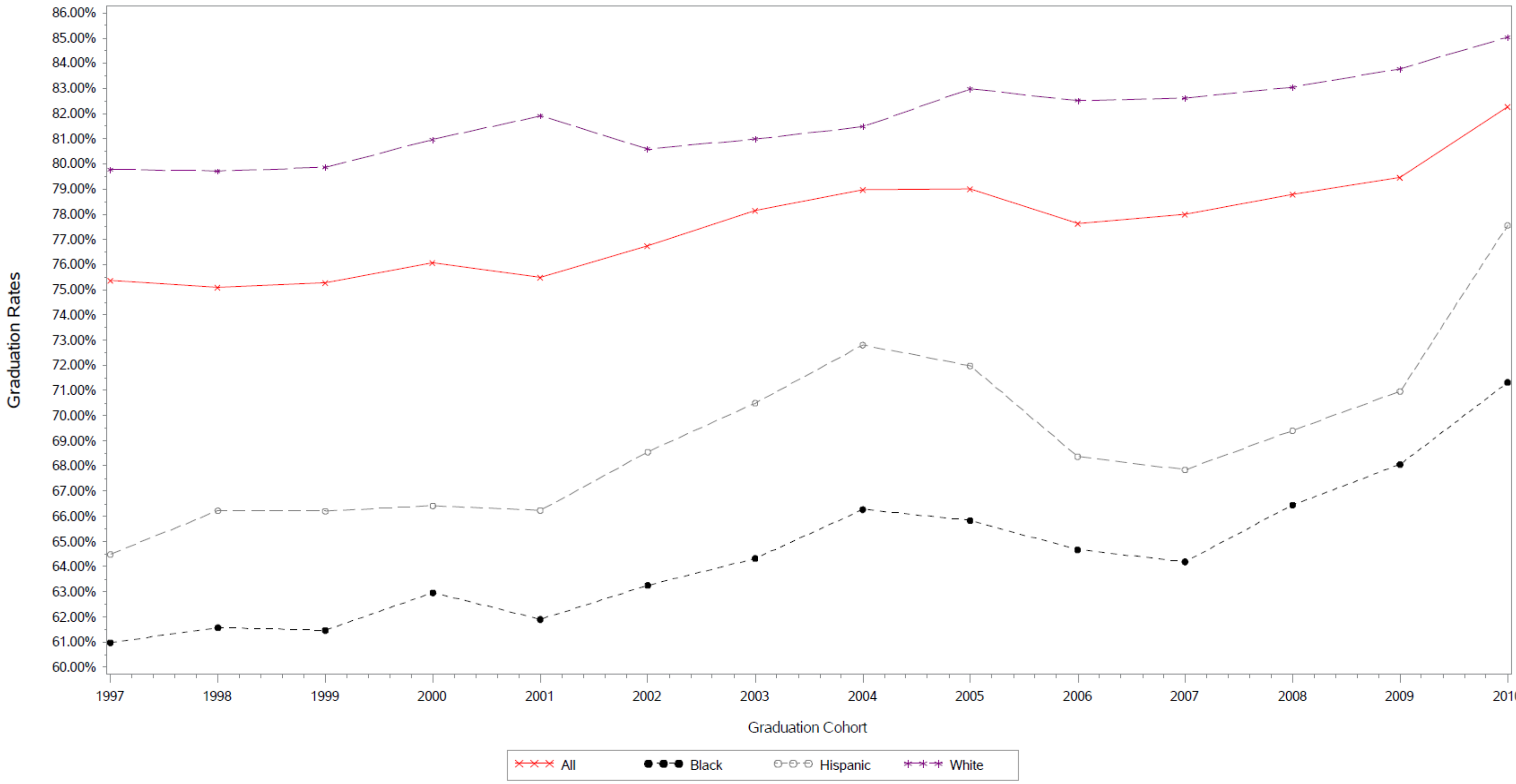

Table 1
Linking the March CPS Samples with the Early Childhood Years for a Given Graduation Cohort

Graduation Cohort	Conception	Age 0	Age 1	Age 2	Age 3	Age 4	Age 5	…	Age 18
1994	1975	1976	1977	1978	1979	1980	1981	…	1994
1995	1976	1977	1978	1979	1980	1981	1982	…	1995
1996	1977	1978	1979	1980	1981	1982	1983	…	1996
1997	1978	1979	1980	1981	1982	1983	1984	…	1997
1998	1979	1980	1981	1982	1983	1984	1985	…	1998
1999	1980	1981	1982	1983	1984	1985	1986	…	1999
2000	1981	1982	1983	1984	1985	1986	1987	…	2000
2001	1982	1983	1984	1985	1986	1987	1988	…	2001
2002	1983	1984	1985	1986	1987	1988	1989	…	2002
2003	1984	1985	1986	1987	1988	1989	1990	…	2003
2004	1985	1986	1987	1988	1989	1990	1991	…	2004
2005	1986	1987	1988	1989	1990	1991	1992	…	2005
2006	1987	1988	1989	1990	1991	1992	1993	…	2006
2007	1988	1989	1990	1991	1992	1993	1994	…	2007
2008	1989	1990	1991	1992	1993	1994	1995	…	2008
2009	1990	1991	1992	1993	1994	1995	1996	…	2009
2010	1991	1992	1993	1994	1995	1996	1997	…	2010

Note: Each highlighted year corresponds to the March CPS used in estimation.

Table 2
Completion Rates and Medicaid Expansions
Aggregated Analysis

			CPS		CCD	
Group	**Graduation Cohort**	**Medicaid Eligibility**	**CPS Dropout Rate (18 to 20 year olds)**	**States with Sufficient Obs for Estimation**	**CCD Graduation Rates**	**States Reporting Graduates in CCD**
All	1994	15.1%	14.4%	51		0
All	1995	14.7%	14.7%	51		0
All	1996	14.3%	14.0%	51		0
All	1997	15.3%	14.1%	51	75.4%	51
All	1998	16.7%	14.1%	51	75.1%	51
All	1999	18.1%	14.1%	51	75.3%	51
All	2000	19.4%	13.5%	51	76.1%	51
All	2001	20.9%	13.5%	51	75.5%	51
All	2002	22.8%	12.0%	51	76.8%	51
All	2003	24.7%	11.5%	51	78.2%	51
All	2004	27.5%	11.7%	51	79.0%	49
All	2005	31.3%	11.0%	51	79.0%	51
All	2006	33.9%	10.9%	51	77.6%	48
All	2007	37.2%	9.7%	51	78.0%	51
All	2008	39.6%	9.3%	51	78.8%	51
All	2009	41.5%	9.2%	51	79.5%	51
All	2010	42.2%	8.8%	51	82.3%	51
Black	1994	40.4%	20.9%	48		0
Black	1995	39.8%	19.2%	46		0
Black	1996	39.5%	19.4%	46		0
Black	1997	39.9%	19.7%	49	61.0%	43
Black	1998	40.5%	19.6%	48	61.6%	43
Black	1999	41.5%	18.8%	50	61.5%	46
Black	2000	41.8%	17.9%	50	63.0%	45
Black	2001	44.0%	17.6%	49	61.9%	46
Black	2002	46.8%	17.4%	50	63.3%	46
Black	2003	49.3%	14.9%	46	64.3%	49
Black	2004	53.5%	16.1%	46	66.3%	47
Black	2005	58.8%	15.0%	48	65.8%	49
Black	2006	62.4%	13.3%	49	64.7%	45
Black	2007	66.8%	13.5%	50	64.2%	48
Black	2008	68.3%	12.0%	50	66.5%	50
Black	2009	69.6%	13.0%	48	68.1%	51
Black	2010	70.0%	11.9%	49	71.3%	51
Hispanic	1994	20.7%	23.8%	46		0
Hispanic	1995	20.5%	22.6%	47		0
Hispanic	1996	21.3%	24.3%	48		0
Hispanic	1997	23.4%	23.0%	46	64.5%	43
Hispanic	1998	26.5%	24.7%	48	66.2%	43
Hispanic	1999	30.6%	24.7%	48	66.2%	46
Hispanic	2000	34.5%	22.7%	49	66.4%	45
Hispanic	2001	38.5%	21.6%	50	66.2%	46
Hispanic	2002	42.7%	18.6%	51	68.6%	46
Hispanic	2003	45.1%	17.9%	50	70.5%	49
Hispanic	2004	49.0%	18.6%	51	72.8%	47
Hispanic	2005	55.4%	17.6%	49	72.0%	49
Hispanic	2006	57.7%	17.5%	50	68.4%	45
Hispanic	2007	61.7%	15.4%	51	67.9%	48
Hispanic	2008	64.5%	13.9%	51	69.4%	50
Hispanic	2009	66.8%	12.9%	51	71.0%	51
Hispanic	2010	67.4%	11.5%	51	77.6%	51
White	1994	10.8%	11.9%	51		0
White	1995	10.4%	12.8%	51		0
White	1996	9.9%	11.5%	51		0
White	1997	10.6%	11.6%	51	79.8%	43
White	1998	12.0%	11.7%	51	79.7%	43
White	1999	13.1%	11.6%	51	79.9%	46
White	2000	14.2%	10.9%	51	81.0%	45
White	2001	15.1%	11.3%	51	81.9%	46
White	2002	16.3%	9.8%	51	80.6%	46
White	2003	17.8%	9.9%	51	81.0%	49
White	2004	20.0%	9.7%	51	81.5%	47
White	2005	23.0%	9.1%	51	83.0%	49
White	2006	25.3%	9.2%	51	82.5%	45
White	2007	28.0%	7.8%	51	82.6%	48
White	2008	30.2%	7.7%	51	83.1%	50
White	2009	31.8%	7.6%	51	83.8%	51
White	2010	32.1%	7.6%	51	85.0%	51

Note: Aggregated *Medicaid Eligibility* and *CPS Dropout Rates* are weighted by the number of the relevant 18 to 20 year olds residing in a particular state in a given year. *CCD Graduation Rates* are weighted by the relevant number of enrolled 8th graders for a given graduation cohort. Please see text for more detail.

Table 3

Estimated Impact of Medicaid Expansions in Early Childhood on High School Completion Rates

Outcome Variable = 18 to 20 Year Old Dropout Rate using CPS Data

Range Analyzed: 1994 to 2010

	Model 1	**Model 2**	**Model 3**	**Model 4**
% of Early Childhood Years with Medicaid Eligibility	-0.1727***	-0.1906***	-0.2422***	-0.2491***
	[0.0441]	[0.0411]	[0.0498]	[0.0694]
Black Students	0.1159***	0.2798***	0.2968***	0.2950***
	[0.0160]	[0.0142]	[0.0175]	[0.0243]
Hispanic Students	0.1436***	0.1662***	0.1778***	0.1806***
	[0.0146]	[0.0092]	[0.0113]	[0.0157]
Constant	0.1614***	0.1695***	0.2166***	0.2131***
	[0.0045]	[0.0041]	[0.0049]	[0.0041]
Number of obs.	**2526**	**2526**	**2526**	**2526**
R-Squared	**0.6308**	**0.6930**	**0.7180**	**0.7988**
Adjusted R-Squared	**0.6204**	**0.6710**	**0.6844**	**0.6735**
State Fixed-Effects	**X**	**X**	**X**	**X**
Cohort Fixed-Effects	**X**	**X**	**X**	**X**
State-Race Fixed Effects		**X**	**X**	**X**
State-Specific Time-Trends			**X**	
State-Cohort Fixed Effects				**X**

Notes: Early childhood years are defined by the seven years from conception through age 5. Regressions are weighted by the number of relevant CPS individuals aged 18 to 20 residing within a particular state for a given year and ethnic group. Standard errors are clustered at the state-level and are in brackets with statistical significance indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

Table 4

Estimated Impact of Medicaid Expansions in Early Childhood on High School Completion Rates

Outcome Variable = 18 to 20 Year Old Dropout Rate using CPS Data

Response Heterogeneity - Models by Race/Ethnic Group

	Black		***Hispanic***		***White***	
Explanatory Variable	**Model 1**	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**
% of Early Childhood Years with Medicaid Eligibility	0.1641	-0.0279	-0.1141	-0.2797**	-0.1054	-0.1397
	[0.1058]	[0.2135]	[0.1044]	[0.1356]	[0.0705]	[0.1237]
Number of obs.	**822**	**822**	**837**	**837**	**867**	**867**
R-Squared	**0.4826**	**0.5670**	**0.4896**	**0.5818**	**0.6919**	**0.7593**
Adjusted R-Squared	**0.4366**	**0.4564**	**0.4451**	**0.4774**	**0.6661**	**0.7018**
State Fixed-Effects	**X**	**X**	**X**	**X**	**X**	**X**
Cohort Fixed-Effects	**X**	**X**	**X**	**X**	**X**	**X**
State-Specific Time-Trends		**X**		**X**		**X**

Notes: Early childhood years are defined by the seven years from conception through age 5. Regressions are weighted by the number of relevant CPS individuals aged 18 to 20 residing within a particular state for a given year and ethnic group. Standard errors are clustered at the state-level and are in brackets with statistical significance indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

Table 5
Alternative Dropout Estimates - Fixed Cohort Demographics
Hispanics and White Students Only

Medicaid Eligibility Definition	Model 1	Model 2	Model 3	Model 4
Limited Core Modeling	-0.2083*** [0.0401]	-0.2149*** [0.0386]	-0.2579*** [0.0498]	-0.2624*** [0.0811]
Demographics at CPS Year = 1975	-0.1680*** [0.0604]	-0.1717*** [0.0606]	-0.2943*** [0.0671]	-0.3514** [0.1346]
Demographics at CPS Year = 1980	-0.1930*** [0.0684]	-0.2008*** [0.0692]	-0.3370*** [0.0746]	-0.3985*** [0.1428]
Demographics at CPS Year = 1985	-0.2014*** [0.0618]	-0.2140*** [0.0614]	-0.3348*** [0.0818]	-0.3786** [0.1534]
Number of obs.	**1704**	**1704**	**1704**	**1704**
State Fixed-Effects	**X**	**X**	**X**	**X**
Cohort Fixed-Effects	**X**	**X**	**X**	**X**
State-Race Fixed Effects		**X**	**X**	**X**
State-Specific Time-Trends			**X**	
State-Cohort Fixed Effects				**X**

Notes: Early childhood years are defined by the seven years from conception through age 5. Regressions are weighted by the number of relevant CPS individuals aged 18 to 20 residing within a particular state for a given year and ethnic group. Standard errors are clustered at the state-level and are in brackets with statistical significance indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

Table 6

Alternative Dropout Estimates - Tests of the Potential Exposure to Medicaid Insurance

Hispanic and White Students Only

Medicaid Eligibility Definition	Model 1	Model 2	Model 3	Model 4
Limited Core Modeling	-0.2083*** [0.0401]	-0.2149*** [0.0386]	-0.2579*** [0.0498]	-0.2624*** [0.0811]
Lower Bound of Any Coverage: Minimum % in any single year (from conception through age 5)	-0.1759*** [0.0320]	-0.1820*** [0.0310]	-0.1834*** [0.0421]	-0.2320*** [0.0626]
Upper Bound of Any Coverage: Maximum % in any single year (from conception through age 5)	-0.0972** [0.0375]	-0.0974** [0.0368]	-0.0982** [0.0420]	-0.2208** [0.0906]
Conception Only	-0.1560*** [0.0318]	-0.1620*** [0.0305]	-0.1760*** [0.0380]	-0.2400*** [0.0657]
From Conception Through Age 2	-0.2036*** [0.0331]	-0.2113*** [0.0314]	-0.2528*** [0.0445]	-0.2551*** [0.0742]
Age 3 to Age 5	-0.1364*** [0.0461]	-0.1377*** [0.0453]	-0.1711*** [0.0490]	-0.2390*** [0.0881]
Number of obs.	**1704**	**1704**	**1704**	**1704**
State Fixed-Effects	**X**	**X**	**X**	**X**
Cohort Fixed-Effects	**X**	**X**	**X**	**X**
State-Race Fixed Effects		**X**	**X**	**X**
State-Specific Time-Trends			**X**	
State-Cohort Fixed Effects				**X**

Notes: Early childhood years are defined by the seven years from conception through age 5. Regressions are weighted by the number of relevant CPS individuals aged 18 to 20 residing within a particular state for a given year and ethnic group. Standard errors are clustered at the state-level and are in brackets with statistical significance indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

Table 7

Estimated Impact of Medicaid Expansions in Early Childhood on High School Completion Rates

Outcome Variable = Four-Year Graduation Rates using Diploma Counts from the Common Core of Data

Range Cohorts Analyzed: 1997 to 2010

Modeling with all Race/Ethnic Groups	**Model 1**	**Model 2**	**Model 3**	**Model 4**
% of Early Childhood Years with Medicaid Eligibility	0.1061**	0.1294***	0.1004**	0.1203
	[0.0467]	[0.0464]	[0.0478]	[0.0971]
Number of obs.	**1875**	**1875**	**1875**	**1875**
R-Squared	**0.3052**	**0.3387**	**0.3525**	**0.4288**
Adjusted R-Squared	**0.2798**	**0.2744**	**0.2453**	**0.0668**

Modeling with Hispanic and Whites Only	**Model 1a**	**Model 2a**	**Model 3a**	**Model 4a**
% of Early Childhood Years with Medicaid Eligibility	0.1218***	0.1371***	0.0865**	0.1111
	[0.0413]	[0.0439]	[0.0404]	[0.1129]
Number of obs.	**1250**	**1250**	**1250**	**1250**
R-Squared	**0.2101**	**0.2294**	**0.2409**	**0.7572**
Adjusted R-Squared	**0.1667**	**0.1512**	**0.0831**	**0.4708**
State Fixed-Effects	**X**	**X**	**X**	**X**
Cohort Fixed-Effects	**X**	**X**	**X**	**X**
State-Race Fixed Effects		**X**	**X**	**X**
State-Specific Time-Trends			**X**	
State-Cohort Fixed Effects				**X**

Notes: Early childhood years are defined by the seven years from conception through age 5. Regressions are weighted by the number of enrolled 8th graders for a given graduation cohort residing within a particular state for a given year and ethnic group. Standard errors are clustered at the state-level and are in brackets with statistical significance indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

Appendix A

Summary of Key Benchmarks in Medicaid Expansions to Low-Income Children

affecting the Graduation Cohorts from the Class of 1997 to the Class of 2010

Year	Development
1965	The Medicaid and Medicare programs are signed into law in June and established as a volunteer federal-state partnership in which participating states receive grants to cover mandatory populations (e.g. AFDC recipients) and services.
1967	Social Security Amendments mandate Early and Periodic Screening, Diagnostic, and Treatment (EPSDT) services for all children up to age 21.
1972	Excluding Arizona, all states have established Medicaid programs.
1981	Despite the Reagan Administration's failure to convert Medicaid to a block grant, the Omnibus Reconciliation Act of 1981 (OBRA81) decreases federal matching payments. This affects fiscal years 1982 to 1984 and leads to coverage decreases in some states for single mothers pregnant for the first time.
1982	Arizona becomes the last state to establish a Medicaid program.
1984	The Deficit Reduction Act of 1984 (DEFRA84) affects coverage to children under two mechanisms. First, coverage for children born after September 20, 1983 is mandated for qualifying AFDC families, up through age 5. Secondly, Medicaid coverage for first-time pregnant women eligible for AFDC and pregnant women in two-parent unemployed families becomes mandatory. These policies take effect in 1985 and essentially eliminate the family structure restriction on Medicaid receipt for all pregnant women.
1985	Under the Consolidated Omnibus Budget Reconciliation Act of 1985 (COBRA85), coverage for all remaining AFDC eligible pregnant women is now mandatory. Moreover, this act extended DEFRA84 coverage for children up through age 5, effective immediately.
1986	Under the Omnibus Reconciliation Act of 1986 (OBRA86), the federal government allows states to cover pregnant women and infants (up to age 1) up to 100 percent of the federal poverty line (FPL). As another Medicaid option, insurance coverage for children up to age 5 is expanded to 100% of the FPL which can be phased in over time.
1987	The Omnibus Reconciliation Act of 1987 allowed states to again expand medical coverage to pregnant women and infants (up to age 1) for families with incomes up to 185 percent of the federal poverty line.
1988	The Medicare Catastrophic Coverage Act of 1988 (MCCA88) mandates that states begin phasing in coverage for pregnant women and infants from families with income levels equal to or below 100% of the federal poverty line.
1989	The Omnibus Reconciliation Act of 1989 (OBRA89) further mandated coverage for pregnant women and children under the age of 6 in families with income at or below 133 percent of the federal poverty line, *regardless of whether they also were receiving AFDC benefits* . Moreover, it required coverage up to age 6 for children in families below 133% of the FPL. This act effectively decoupled Medicaid for children from AFDC. Additionally - and importantly - the federal government mandated that states must treat any issues identified during EPSDT screening, even if these procedure were not traditionally covered under the state's Medicaid program.

Primary Sources:

Kaiser Family Foundation: http://kff.org/medicaid/timeline/medicaid-a-timeline-of-key-developments/
U.S. General Accounting Office (GAO) Reports: http://gao.gov/products/HRD-91-78

Appendix B - Technical Details

Medicaid Eligibility Simulation

Before the decoupling of child Medicaid and AFDC, the primary basis for Medicaid qualification was AFDC receipt. Given this, Medicaid eligibility determination in the early period is straightforward: only children in single-parent households qualified for care if their family income – less certain disregards – fell below the state's payment standards. As noted, these mandated thresholds varied greatly across states. During this early period, some states did make allowances for children in two parent households with an unemployed parent (AFDC-UP), as well as for "Ribicoff children" which, in this case, were typically teens who would have qualified for AFDC under their own income thresholds but did not qualify in the traditional manner due to family structure issues (e.g. they still lived with their parents). Archived documents outlined reveal states participating in these programs.

Another wrinkle in estimation during this early period was whether an unborn child counted in AFDC determination. Before DEFRA 1984, which mandated coverage of the unborn, states differed greatly in their positions especially when considering a single mother pregnant for the first time. When the unborn child did not count, these mothers typically failed to receive coverage during their pregnancy because single individuals without dependents rarely qualified for benefits. Preceding the federal mandate, a number of states incorporated programs to support single mothers pregnant for the first time at the point of verification by medical professionals. Again, there was wide variation in the implementation of these programs. All of these changes were documented and incorporated into the simulation procedure.

Finally, the last step in the collection of legislative procedures was to acquire all of the effective dates and poverty thresholds for the state Medicaid expansions to pregnant women, infants, and children in the late 1980s and early 1990s which effectively decoupled child Medicaid from AFDC. Documents outlining these transitions are obtainable through the variety of resources (see list in the data section). These documents, in turn, can then be used to compile a database of Medicaid eligibility requirements by state and year for young children in all states from 1975 to 1997.

Tables below disclose how specific rules governing qualification for either AFDC or Medicaid were handled in the simulation:

Issue: In the early period, AFDC receipt is the general basis for Medicaid receipt

Category	**Sub-Category**	**Details**	**Source(s)**
Definition of a family unit	General issues	The CPS contained detailed information intra-family relationships. Thus, it is typically possible to link the child to their parent(s), which can then be used in establishing the size of the family unit applicable for AFDC eligibility. To mitigate the issue of the endogeneity of family size due to social welfare policies, families with either 1, 2, or 3 children are used in simulations.	March Current Population Survey (various years)
	Unborn children	Before DEFRA 1984 - and effective in 1985 - a limited number of states counted the unborn child as part of the family unit in the determination of AFDC eligibility. Thus, the family size would be smaller by one for pregnant women in states not counting unborn children. This applies to the conception year only.	Analysis of State Medicaid Program Characteristics (various years)

Income requirements	Earnings Allowances	Before OBRA 1981: although there were no standardized allowances before 1981, Currie and Gruber assume that the levels were the same as those mandated under OBRA 1981.	Currie and Gruber (1994)
		OBRA 1981: starting in October 1981, the standardized allowances per month for work expenses was $75, while states allowed up to $160 per month per child for child care.	
		Family Support Act of 1988: effective October 1989, allowances were increased to $90 per month for work expenses and $175 dollars per child per month for child care.	
		30 and One-Third: at its inception, this work incentive feature allowed families to keep the first $30 of earned income, 1/3 of the remainder, while the remaining 2/3 lead directly to a reduction in AFDC benefits. See Currie and Gruber for details regarding the evolution of this program.	
	Binding Constraint for Qualification	Since the vast majority of the state's payment standards were well below the needs standards, the binding constraint for AFDC qualification was that a family unit's gross earnings - minus earnings allowances outlined in Currie and Gruber (1994) - were less than or equal to the state's payment standard.	Historical payment standards were available through state-level data provided by the University of Kentucky Center for Poverty Research.

Issue: As Medicaid becomes delinked from AFDC, other groups become eligible for coverage

Category	Sub-Category	Details	Source(s)
General expansions for all women, infants, and children.	DEFRA 1984	Medicaid coverage is mandated for children in AFDC qualifying families born after September 20, 1983 through age 5	Kaiser Family Foundation
	COBRA 1985	All pregnant women who meet income requirements were now eligible for Medicaid, regardless of family structure or the presence of other children. DEFRA coverage for children is expanded for all children at or below the age of 5 residing in AFDC families.	Currie and Gruber (1994) Kaiser Family Foundation
	OBRA 1986	States were given the option to expand the income thresholds for Medicaid eligibility regardless of family structure type. As an option, states are allowed to expand coverage to children up to age 5 residing in families at or below 100% of the federal poverty line.	Hill (1992); The National Governors Association MCH Updates (various years); Kaiser Family
	OBRA 1987	States were allowed to increase the income thresholds up to 185% of the poverty line for pregnant women and infants.	

	OBRA 1988	States were mandated to cover pregnant women, infants, and children up to 133% of the poverty line by April 1990, again regardless of family structure type. Some states choose thresholds above this mandated minimum.	Kaiser Family Foundation
Single mothers pregnant for the first time	Unborn children and benefits qualification	DEFRA 1984 mandated coverage for all pregnant women qualifying for AFDC under the typical mechanisms, regardless of whether she already had children. This policy became effective in 1985.	Currie and Gruber (1994)
Programs for married women below income requirements	DEFRA 1984	Coverage of all pregnant women in AFDC-UP type families now required. Before this mandate, states different in their timing and coverage of AFDC-UP type families.	Analysis of State Medicaid Program Characteristics (various years)
Minors	Ribicoff children	Since the goal was to estimate the number of child-years potentially covered by Medicaid, pregnant teens were considered as their own family unit and, consequently, the child qualified based upon the teenage mother's income (and not the larger family unit that they may have resided in). This simplifying assumption was made because historical details regarding state-level Ribicoff programs is limited.	Currie and Gruber (1994)
Other categories	Medically needy program	Lacking information on Medical expenditures at the household level, it is difficult to identify medically needy families. Consequently, they were not incorporated into the simulations.	

18-20 Year Old Dropout Rate using Current Population Survey Data

Sharing the same underlying data - the CPS - simulated Medicaid eligibility and the 18 to 20 year old dropout rates are estimated in a similar manner. Given the necessity of the smoothing technique already discussed, as well limiting the CPS respondents to only those individuals born in the United States, the 18-20 year old Dropout Rate in a single CPS month is calculated as:

$$(18-20\ Year\ Old\ Dropout\ Rate)_{sc} = \sum_{i=1}^{n} \frac{CPS\ Weight_{isc} \ *\ (No\ Degree, Not\ Enrolled)_{isc}}{CPS\ Weight_{isc}}$$

where: **i** represents a CPS observation for a relevant 18 to 20 year old;

No Degree, Not Enrolled identifies respondents who did not complete high school and are no longer enrolled in school - this defines a dropout; and

CPS Weight are the person weights reported by the individual CPS survey.

As noted in the primary text, dropout rates are estimated using monthly data from the Current Population Survey. Thus, instead of only a single month, 12 distinct CPS samples actually feed into a single cohort calculation. Since the traditional secondary school year usual ends around June, rates for a graduation cohort are estimated using the July CPS of a particular year through the June CPS of the next. For example, the sample used to calculate dropout rates for the class of 2000 are taken from the July 2000 CPS through the June 2001 CPS. These twelve individuals samples, along with the estimation using 18 to 20 year olds, ensures that a sufficient sample size produces the most reliable statistics.

Four-Year Graduation Rates using the Common Core of Data

Although it is one of the best measure currently available to researchers, this choice of four-year graduation rate using CCD data is not an uncontroversial because of two possible sources of measurement error. Before proceeding to the issues associated with the four-year graduation rate measure, it is useful to first discuss how a perfect measure would be constructed and then reveal how the four-year graduation rate potentially falls short. In an ideal thought experiment, all students would (1) enter 9th grade at the same age and (2) never repeat grades but simply drop out in a readily identifiable manner. Under this scenario and with accurate administrative data, once could construct a graduation rate measure for state (s) at time (t) as:

$$(Graduation\ Rate)_{st} = \frac{(\#\ Actual\ Grads)_{st}}{(\#\ Potential\ Grads)_{st}}$$

Unfortunately, the two conditions listed above are not met in practice. Estimation of high school graduation rates can be surprisingly challenging, due largely in part to some students taking longer than the standard of 4 years to finish their diploma – an issue of degree duration – and because other students remain in administrative systems longer than 4 years but never finish their degrees – a matter of grade retention. To simplify these issues, I follow Heckman and LaFontaine (2010) in their calculation of the four-year graduation rate.

While issues associated with degree duration are discussed in detail in the primary text, the second form of measurement error, *grade retention* , invokes less controversial assumptions. Importantly, it also relates to how a graduation cohort is determined in this analysis. Returning to the ideal equation above, calculation of a graduation rate takes some measure of completion as the numerator and some baseline measure of potential graduates as the denominator. While the exclusion of GED holders from the high school graduation calculation is simple – essentially one just subtracts these individuals from the numerator – the definition of the denominator is more challenging, given the problem of grade retention and the definition of a cohort. Since students who are held back in high school are much more likely to drop out, it is important to properly control for these individuals across cohorts so that they are not counted multiple times.[1]

To avoid the problems associated with grade retention, Warren (2005) proposed that the number of enrolled Grade 8 students be used as a proxy for the number of incoming Grade 9 students for a particular graduation cohort,[2] an approach was later employed by Heckman and LaFontaine (2010). I follow this approach in my analysis. *This implies that the cohort is defined by the year in which they graduate and not some other measure, such as the year they enter 9th grade* .[3] With the lag structure required to estimate the graduation rate under this process, the first cohort for which a graduation rate can be estimated using the CCD data is the class of 1997. Conveniently, this covers a minimal pre-period before the rules governing child Medicaid coverage were significantly expanded in all states, which means that I can establish a baseline of graduation rates before estimating the impacts of the marked increases in Medicaid eligibility during early childhood. Moreover, trends and estimates are consistent with those presented by Heckman and LaFontaine (2010).

Section Endnotes:

[1] As outlined by Warren (2005), a flawed estimation methodology using CCD data is to simply take the number of graduating seniors at time t and to divide by the number of freshman reported at time t-3. The problem with this approach is that students can stay registered in Grade 9 when they remain in the system, attend school sparingly, and do not progress past Grade 9; this is true especially with the end of social promotion policies. Thus, including these individuals in the Grade 9 calculation could lead to the double-counting of select individuals and a dilution of the graduation rate.

[2] Under this assumption, graduation rates are calculated as the number of high school graduates at time t divided by the number of 8th graders enrolled at time t-4, an estimation strategy which can reduce the bias from repeating students.

[3] Thus, for example, students graduating in 2010 are referred to as the class of 2010 even though some individuals may have originally had other anticipated graduation years (e.g. the class of 2009 for those repeating one year).

Appendix - Table C1
Estimated Percent of Early Childhood Years with Medicaid Eligibility
By Graduation Cohort - From Conception through Age 5
All Students

State	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
Alabama	10.7%	10.4%	10.2%	10.9%	11.6%	12.3%	12.7%	13.3%	14.1%	17.5%	20.7%	24.8%	30.4%	34.2%	37.5%	38.6%	38.7%
Alaska	12.7%	12.7%	12.9%	16.0%	20.0%	23.9%	27.2%	31.2%	34.8%	36.1%	37.3%	40.9%	41.6%	43.0%	44.0%	45.3%	45.3%
Arizona	0.0%	0.0%	1.6%	4.4%	7.3%	10.5%	13.4%	16.5%	20.3%	22.8%	27.1%	30.9%	33.4%	36.3%	37.7%	39.1%	39.4%
Arkansas	10.6%	10.4%	10.2%	11.0%	11.9%	12.9%	13.6%	14.9%	17.8%	20.7%	26.7%	30.7%	33.8%	36.3%	37.5%	38.6%	38.7%
California	19.3%	19.1%	18.7%	20.2%	22.5%	24.8%	26.9%	29.0%	31.4%	32.4%	33.0%	35.8%	36.4%	39.0%	41.3%	43.4%	43.3%
Colorado	17.2%	16.7%	16.2%	17.0%	18.3%	19.4%	20.2%	21.3%	22.5%	24.5%	26.6%	29.8%	31.9%	34.5%	36.6%	38.6%	38.7%
Connecticut	18.7%	18.4%	18.0%	19.4%	21.6%	23.8%	25.6%	28.2%	30.4%	31.4%	32.2%	34.7%	37.6%	40.5%	44.8%	46.3%	47.8%
Delaware	16.8%	16.3%	15.5%	16.0%	16.8%	17.8%	18.5%	19.4%	20.6%	23.0%	27.7%	31.0%	33.4%	36.3%	38.3%	40.3%	41.8%
District of Columbia	17.4%	16.8%	16.1%	16.6%	17.8%	18.7%	19.7%	21.0%	22.5%	22.9%	28.1%	31.4%	33.5%	35.3%	38.1%	40.8%	43.3%
Florida	11.4%	11.2%	11.1%	12.3%	13.6%	14.9%	16.0%	17.3%	20.2%	22.7%	27.1%	30.5%	33.2%	36.8%	38.5%	40.1%	41.2%
Georgia	10.4%	10.4%	10.4%	11.4%	12.7%	13.9%	14.9%	16.4%	17.8%	20.4%	25.1%	29.0%	31.8%	34.9%	37.5%	38.6%	39.2%
Hawaii	20.2%	19.7%	18.9%	19.9%	21.4%	22.8%	23.9%	25.4%	27.4%	29.2%	31.1%	34.7%	37.0%	41.4%	45.1%	48.0%	52.3%
Idaho	12.7%	12.4%	12.3%	13.6%	15.0%	16.3%	17.4%	18.8%	20.1%	22.3%	25.1%	28.4%	31.2%	34.1%	36.6%	38.6%	38.7%
Illinois	16.9%	16.4%	15.8%	16.2%	17.2%	18.2%	19.0%	20.0%	21.1%	23.2%	26.0%	29.3%	32.6%	35.3%	37.5%	38.6%	38.7%
Indiana	11.4%	11.3%	11.2%	12.4%	13.8%	15.1%	16.1%	17.6%	18.9%	21.3%	24.3%	28.1%	30.6%	33.6%	36.1%	39.2%	39.7%
Iowa	17.8%	17.4%	16.7%	17.3%	18.5%	19.6%	20.4%	21.6%	22.8%	24.7%	27.1%	31.1%	34.5%	38.6%	42.2%	43.4%	43.3%
Kansas	18.5%	17.9%	17.1%	17.7%	18.9%	20.0%	21.0%	22.0%	23.6%	25.6%	27.5%	31.3%	33.2%	36.1%	38.5%	40.1%	40.2%
Kentucky	14.2%	13.3%	12.3%	12.5%	13.1%	13.8%	14.6%	15.8%	17.0%	19.6%	24.4%	30.0%	33.7%	37.0%	39.9%	41.8%	43.3%
Louisiana	11.1%	10.9%	10.8%	11.8%	13.0%	13.9%	14.8%	15.9%	18.7%	21.4%	24.6%	28.2%	31.2%	34.6%	37.5%	38.6%	38.7%
Maine	12.5%	12.9%	13.5%	15.2%	17.4%	19.5%	20.8%	22.4%	24.5%	26.4%	28.2%	31.9%	36.2%	40.0%	43.3%	43.4%	43.3%
Maryland	16.7%	16.3%	15.7%	16.4%	17.5%	18.5%	19.5%	20.7%	22.1%	24.3%	26.5%	31.5%	33.6%	39.3%	43.6%	47.7%	49.2%
Massachusetts	18.6%	18.2%	17.5%	18.2%	19.5%	20.9%	22.4%	24.0%	26.0%	27.8%	29.7%	32.8%	37.1%	40.5%	43.3%	43.4%	43.3%
Michigan	19.3%	18.8%	18.2%	19.1%	20.6%	22.3%	23.5%	25.2%	27.0%	28.5%	30.2%	35.7%	39.6%	43.0%	43.8%	44.3%	44.8%
Minnesota	19.2%	18.8%	18.2%	19.5%	21.6%	23.6%	25.1%	26.8%	28.6%	29.9%	30.7%	33.6%	41.1%	48.0%	54.7%	58.5%	60.8%
Mississippi	9.9%	9.7%	9.7%	10.2%	10.8%	11.4%	12.0%	12.9%	13.8%	16.9%	22.8%	29.0%	35.1%	40.5%	43.3%	43.4%	43.3%
Missouri	15.2%	15.0%	14.6%	15.1%	16.0%	16.8%	17.4%	18.4%	19.3%	21.7%	26.9%	30.7%	33.3%	36.3%	37.5%	38.6%	38.7%
Montana	15.9%	15.0%	14.1%	14.9%	16.2%	17.3%	18.8%	20.6%	22.3%	24.2%	26.7%	29.9%	31.9%	34.5%	36.6%	38.6%	38.7%
Nebraska	17.7%	17.4%	16.8%	17.6%	18.7%	19.7%	20.5%	21.5%	22.6%	24.6%	27.5%	30.7%	32.8%	35.4%	37.5%	38.6%	38.7%
Nevada	12.1%	11.9%	11.8%	12.8%	14.0%	15.4%	16.5%	18.1%	19.7%	22.2%	25.3%	28.7%	31.2%	34.1%	36.4%	38.6%	38.7%
New Hampshire	12.2%	12.0%	11.9%	13.4%	15.4%	17.2%	18.7%	21.0%	23.2%	25.2%	27.6%	31.2%	33.2%	36.6%	39.3%	42.2%	44.3%
New Jersey	17.7%	17.2%	16.5%	17.1%	18.5%	19.8%	20.7%	22.1%	23.4%	23.6%	26.0%	30.3%	32.9%	35.3%	36.5%	39.2%	41.7%
New Mexico	11.6%	11.5%	11.4%	12.7%	14.1%	15.3%	16.4%	17.7%	19.1%	21.5%	27.2%	30.6%	33.2%	36.3%	37.5%	41.6%	44.7%
New York	19.4%	18.9%	18.3%	19.5%	21.2%	23.0%	24.4%	26.1%	27.9%	28.3%	29.5%	32.5%	33.6%	37.9%	40.6%	43.4%	43.3%
North Carolina	11.0%	10.8%	10.7%	11.6%	12.8%	13.9%	14.9%	16.3%	17.6%	20.3%	26.8%	30.3%	33.1%	36.8%	39.6%	42.3%	43.3%
North Dakota	12.6%	12.4%	12.4%	14.0%	16.1%	17.9%	19.4%	21.1%	23.0%	24.8%	27.2%	30.3%	32.3%	34.7%	36.8%	38.6%	38.7%
Ohio	16.8%	16.3%	15.7%	16.2%	17.1%	18.0%	18.7%	19.7%	20.9%	23.2%	25.6%	28.9%	32.3%	35.2%	37.5%	38.6%	38.7%
Oklahoma	11.6%	11.5%	11.4%	12.7%	14.3%	15.9%	17.1%	18.7%	20.2%	22.5%	26.4%	31.2%	33.5%	36.3%	37.5%	38.6%	39.2%
Oregon	17.7%	16.6%	15.2%	15.9%	17.2%	18.3%	19.9%	21.8%	23.8%	25.6%	28.7%	31.8%	33.5%	35.8%	37.5%	38.6%	38.7%
Pennsylvania	18.3%	17.7%	16.9%	17.6%	18.8%	20.0%	21.0%	21.9%	23.2%	25.1%	28.4%	32.0%	33.9%	36.3%	37.5%	38.6%	40.1%
Rhode Island	18.7%	18.2%	17.7%	18.8%	20.7%	22.5%	24.0%	25.4%	27.5%	28.9%	30.4%	33.3%	37.2%	40.5%	46.6%	49.8%	52.9%
South Carolina	10.8%	10.5%	10.5%	11.3%	12.4%	13.4%	14.3%	15.2%	18.1%	21.0%	24.7%	29.9%	32.9%	37.8%	40.7%	43.4%	43.3%
South Dakota	12.8%	12.5%	12.4%	13.8%	15.4%	16.8%	18.1%	19.3%	20.9%	23.0%	25.7%	29.4%	32.7%	35.3%	37.5%	38.6%	38.7%
Tennessee	10.8%	10.5%	10.4%	11.0%	11.8%	12.7%	13.4%	14.4%	17.3%	20.4%	25.7%	29.4%	32.6%	36.8%	38.5%	41.2%	42.2%
Texas	10.1%	9.8%	9.7%	10.5%	11.6%	12.6%	13.5%	14.7%	15.8%	18.8%	22.2%	28.1%	31.1%	34.6%	37.5%	40.2%	41.7%
Utah	16.8%	15.8%	14.7%	15.4%	16.6%	17.7%	19.2%	20.9%	22.6%	24.5%	27.0%	30.1%	33.0%	35.5%	37.5%	38.6%	38.7%
Vermont	19.6%	19.3%	18.8%	20.1%	22.2%	24.3%	25.9%	27.2%	29.0%	30.2%	31.3%	34.0%	39.9%	45.5%	51.0%	53.5%	54.6%
Virginia	11.8%	11.7%	11.6%	12.9%	14.7%	16.3%	17.7%	19.5%	21.1%	23.2%	25.9%	29.6%	32.7%	35.3%	37.5%	38.6%	38.7%
Washington	17.8%	16.8%	15.6%	16.7%	18.5%	20.3%	22.2%	24.5%	26.7%	27.9%	29.7%	32.6%	33.8%	37.2%	42.3%	47.1%	49.0%
West Virginia	16.2%	15.6%	14.8%	15.1%	15.6%	16.3%	17.0%	17.5%	19.9%	22.5%	26.6%	30.6%	34.7%	38.3%	40.0%	40.1%	40.2%
Wisconsin	19.5%	19.0%	18.5%	19.9%	22.0%	24.1%	25.7%	27.3%	29.0%	30.0%	31.6%	35.0%	37.2%	39.4%	41.3%	43.2%	44.8%
Wyoming	12.6%	12.4%	12.4%	13.8%	15.4%	16.9%	18.2%	19.7%	21.2%	23.3%	26.1%	29.3%	32.6%	35.3%	37.5%	38.6%	38.7%

Appendix - Table C2
Estimated Percent of Early Childhood Years with Medicaid Eligibility
By Graduation Cohort - From Conception through Age 5
Black Students

State	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
Alabama	36.1%	35.2%	34.9%	35.1%	34.9%	35.1%	35.0%	35.7%	37.0%	41.8%	45.4%	51.5%	58.6%	63.8%	67.1%	67.9%	67.9%
Alaska	40.6%	40.8%	41.2%	43.5%	47.1%	50.8%	53.4%	58.4%	62.5%	64.2%	65.4%	69.0%	70.1%	72.2%	72.5%	73.2%	73.3%
Arizona	0.0%	0.0%	5.2%	11.3%	17.9%	24.6%	30.8%	38.4%	45.5%	48.7%	53.7%	59.3%	62.7%	66.6%	67.1%	68.1%	68.3%
Arkansas	35.8%	35.4%	35.0%	35.1%	35.5%	36.2%	36.3%	38.3%	41.9%	45.5%	53.3%	59.1%	63.3%	66.6%	67.1%	67.9%	67.9%
California	47.8%	47.4%	47.2%	48.0%	49.8%	51.7%	53.2%	55.9%	58.9%	60.2%	61.7%	64.9%	66.1%	68.9%	69.9%	71.0%	70.9%
Colorado	44.7%	43.7%	43.4%	43.8%	44.3%	45.0%	45.2%	47.0%	48.8%	51.0%	54.1%	58.1%	61.1%	64.6%	66.1%	67.9%	67.9%
Connecticut	46.1%	45.8%	45.5%	46.0%	47.9%	50.1%	51.3%	55.1%	57.8%	59.0%	60.8%	63.9%	67.1%	70.2%	72.9%	73.3%	74.6%
Delaware	44.1%	42.9%	42.2%	42.2%	42.5%	42.9%	43.0%	44.9%	46.8%	49.4%	55.1%	59.3%	62.7%	66.6%	67.6%	69.0%	70.0%
District of Columbia	45.1%	44.0%	43.3%	43.3%	43.6%	44.2%	44.7%	46.9%	49.1%	49.6%	56.1%	60.1%	63.2%	65.7%	67.2%	69.1%	70.9%
Florida	38.0%	37.5%	37.4%	38.0%	38.7%	39.4%	39.9%	42.1%	45.8%	49.0%	54.3%	58.7%	62.5%	67.1%	67.9%	69.2%	69.7%
Georgia	35.4%	35.4%	35.4%	35.8%	36.9%	37.8%	38.4%	40.7%	42.7%	46.0%	51.3%	57.0%	60.9%	65.1%	67.1%	67.9%	68.3%
Hawaii	48.9%	48.1%	47.5%	47.9%	48.7%	49.8%	50.1%	52.4%	54.8%	56.8%	59.4%	63.4%	66.3%	70.7%	72.8%	74.8%	77.7%
Idaho	41.0%	40.4%	40.1%	40.8%	41.5%	42.0%	42.3%	44.4%	46.4%	49.1%	52.6%	56.7%	60.5%	64.3%	66.1%	67.9%	67.9%
Illinois	43.5%	42.9%	42.0%	41.8%	42.4%	43.3%	43.3%	45.2%	46.7%	49.1%	53.5%	57.6%	61.8%	65.4%	67.1%	67.9%	67.9%
Indiana	37.7%	37.6%	37.3%	37.7%	38.7%	39.5%	39.8%	42.2%	44.0%	46.9%	50.6%	56.1%	59.7%	63.8%	65.6%	68.3%	68.7%
Iowa	44.9%	44.4%	43.6%	43.5%	44.2%	44.9%	45.3%	47.4%	48.8%	50.9%	55.0%	59.8%	64.2%	68.4%	70.6%	71.0%	70.9%
Kansas	46.9%	45.8%	45.1%	45.1%	45.5%	45.9%	46.5%	48.2%	50.5%	52.7%	55.6%	60.1%	62.8%	66.4%	67.9%	69.2%	69.1%
Kentucky	40.1%	39.0%	38.0%	37.3%	37.6%	37.9%	38.0%	39.8%	41.6%	44.9%	50.6%	58.0%	63.0%	67.5%	68.9%	69.9%	70.9%
Louisiana	37.2%	36.6%	36.6%	37.1%	37.6%	38.1%	38.3%	40.0%	43.6%	47.0%	51.0%	55.6%	59.8%	64.4%	67.1%	67.9%	67.9%
Maine	38.7%	39.0%	39.5%	40.7%	42.6%	44.7%	45.6%	48.2%	50.7%	52.7%	55.5%	60.7%	65.6%	69.7%	71.6%	71.0%	70.9%
Maryland	44.0%	43.0%	42.6%	42.8%	43.3%	43.9%	44.3%	46.4%	48.5%	51.0%	54.0%	59.9%	63.0%	68.8%	71.5%	74.4%	75.7%
Massachusetts	46.9%	46.1%	45.5%	45.7%	46.3%	47.3%	48.2%	50.7%	53.1%	55.2%	57.9%	61.6%	66.5%	70.2%	71.6%	71.0%	70.9%
Michigan	47.8%	47.0%	46.5%	46.9%	47.5%	49.0%	49.6%	52.1%	54.4%	56.0%	58.5%	64.3%	68.5%	72.2%	72.2%	72.1%	72.4%
Minnesota	47.8%	47.1%	46.5%	47.3%	48.9%	50.5%	51.4%	53.9%	56.1%	57.6%	59.2%	62.6%	69.0%	75.0%	79.0%	81.1%	83.0%
Mississippi	33.6%	33.5%	33.2%	33.0%	32.9%	33.0%	33.2%	34.8%	36.3%	40.5%	48.1%	56.8%	64.0%	70.2%	71.6%	71.0%	70.9%
Missouri	40.9%	40.7%	40.4%	40.2%	40.8%	41.2%	41.3%	43.2%	44.6%	47.4%	53.5%	59.0%	62.7%	66.6%	67.1%	67.9%	67.9%
Montana	43.3%	42.2%	41.3%	41.8%	42.4%	43.1%	44.1%	46.7%	48.9%	51.2%	54.2%	58.3%	61.2%	64.7%	66.1%	67.9%	67.9%
Nebraska	45.6%	44.9%	44.3%	44.6%	44.8%	45.4%	45.6%	47.3%	49.0%	51.2%	55.0%	59.0%	62.0%	65.5%	67.1%	67.9%	67.9%
Nevada	39.6%	39.0%	38.8%	39.1%	39.6%	40.4%	40.9%	43.2%	45.5%	48.7%	52.5%	56.9%	60.5%	64.3%	66.0%	67.9%	67.9%
New Hampshire	39.5%	39.4%	39.1%	39.8%	41.3%	42.8%	43.8%	47.0%	49.4%	51.9%	54.9%	60.1%	62.9%	66.8%	68.5%	70.7%	72.5%
New Jersey	44.6%	44.1%	43.2%	43.2%	44.2%	45.1%	45.7%	47.9%	49.5%	49.8%	53.1%	59.0%	62.6%	65.7%	66.2%	68.2%	70.0%
New Mexico	38.7%	38.1%	38.1%	38.8%	39.5%	40.2%	40.7%	42.8%	44.7%	47.7%	54.4%	58.7%	62.5%	66.6%	67.1%	70.0%	72.4%
New York	48.0%	47.2%	46.7%	47.2%	48.2%	49.6%	50.5%	52.8%	55.2%	55.9%	58.1%	61.6%	63.3%	67.7%	69.1%	71.0%	70.9%
North Carolina	36.8%	36.6%	36.2%	36.4%	37.2%	38.1%	38.5%	40.7%	42.6%	45.9%	54.0%	58.5%	62.4%	67.1%	68.5%	70.4%	70.9%
North Dakota	40.9%	40.4%	40.4%	41.6%	42.8%	44.1%	45.0%	47.6%	49.8%	52.1%	55.1%	59.0%	61.9%	65.0%	66.3%	67.9%	67.9%
Ohio	44.2%	43.0%	42.5%	42.5%	42.8%	43.2%	43.3%	45.2%	47.1%	49.6%	53.1%	57.2%	61.6%	65.4%	67.1%	67.9%	67.9%
Oklahoma	38.2%	38.0%	37.9%	38.5%	39.7%	41.0%	41.5%	43.9%	45.9%	48.5%	53.2%	59.5%	62.8%	66.6%	67.1%	67.9%	68.3%
Oregon	46.2%	44.7%	43.5%	43.7%	44.3%	44.9%	45.7%	48.3%	50.7%	53.0%	56.7%	60.5%	63.1%	66.2%	67.1%	67.9%	67.9%
Pennsylvania	46.4%	45.4%	44.7%	44.9%	45.3%	45.9%	46.4%	47.6%	49.3%	51.4%	55.4%	60.7%	63.4%	66.6%	67.1%	67.9%	68.9%
Rhode Island	46.9%	46.1%	45.7%	46.3%	47.6%	49.2%	50.1%	52.4%	55.0%	56.6%	58.8%	62.3%	66.6%	70.2%	73.9%	75.7%	78.3%
South Carolina	36.5%	35.7%	35.6%	36.1%	36.8%	37.1%	37.5%	38.9%	42.5%	46.1%	51.2%	57.9%	62.0%	67.7%	69.1%	71.0%	70.9%
South Dakota	41.1%	40.6%	40.4%	41.2%	42.0%	42.7%	43.4%	44.8%	46.7%	49.3%	52.5%	57.9%	62.1%	65.6%	67.1%	67.9%	67.9%
Tennessee	36.3%	35.5%	35.3%	35.4%	35.4%	35.8%	35.9%	37.7%	41.6%	45.4%	52.2%	57.1%	61.6%	67.1%	67.9%	69.9%	70.4%
Texas	34.3%	33.9%	33.5%	33.7%	34.5%	35.5%	36.0%	37.9%	39.8%	43.6%	47.9%	55.5%	59.7%	64.4%	67.1%	69.0%	70.0%
Utah	45.0%	43.7%	42.7%	43.0%	43.4%	44.0%	44.8%	47.3%	49.3%	51.7%	54.8%	58.7%	62.5%	65.7%	67.1%	67.9%	67.9%
Vermont	48.3%	47.7%	47.4%	48.1%	49.7%	51.5%	52.4%	53.9%	55.8%	57.1%	59.9%	63.1%	68.3%	73.5%	77.1%	78.4%	79.6%
Virginia	38.6%	38.5%	38.2%	38.9%	40.2%	41.7%	42.4%	45.0%	46.9%	49.3%	52.6%	58.0%	62.0%	65.5%	67.1%	67.9%	67.9%
Washington	46.6%	45.4%	44.4%	45.0%	46.1%	47.6%	48.8%	51.6%	54.1%	55.7%	58.1%	61.6%	63.3%	67.0%	70.3%	74.0%	75.6%
West Virginia	43.4%	42.2%	41.5%	40.9%	40.8%	40.9%	41.0%	41.8%	44.7%	47.9%	53.0%	58.7%	64.0%	68.6%	69.5%	69.2%	69.1%
Wisconsin	48.1%	47.4%	47.1%	47.7%	49.3%	51.2%	52.0%	54.4%	56.4%	57.7%	59.9%	63.7%	66.5%	69.4%	70.6%	71.6%	73.0%
Wyoming	40.8%	40.4%	40.5%	41.3%	42.2%	43.2%	43.8%	46.0%	47.9%	50.5%	53.8%	57.9%	62.0%	65.5%	67.1%	67.9%	67.9%

Appendix - Table C3
Estimated Percent of Early Childhood Years with Medicaid Eligibility
By Graduation Cohort - From Conception through Age 5
Hispanic Students

State	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
Alabama	14.8%	14.6%	14.9%	16.6%	18.6%	20.6%	22.3%	23.9%	25.8%	31.0%	36.4%	43.1%	51.8%	57.5%	62.5%	63.7%	63.6%
Alaska	16.7%	16.9%	17.7%	23.0%	30.1%	37.2%	43.4%	50.3%	56.9%	58.4%	60.7%	66.3%	66.9%	68.6%	69.5%	70.9%	70.9%
Arizona	0.0%	0.0%	2.8%	7.5%	12.7%	18.7%	24.1%	29.9%	37.2%	41.1%	47.8%	54.0%	56.9%	61.0%	62.7%	64.2%	64.5%
Arkansas	14.8%	14.6%	14.9%	16.7%	19.1%	21.8%	23.9%	26.8%	32.5%	37.0%	47.1%	53.6%	57.7%	61.0%	62.5%	63.7%	63.6%
California	27.0%	27.1%	27.7%	30.7%	35.2%	40.3%	45.0%	49.3%	54.1%	55.4%	56.1%	61.0%	61.2%	64.1%	66.6%	68.6%	68.4%
Colorado	24.9%	24.6%	24.7%	26.3%	29.2%	32.4%	35.1%	38.0%	41.2%	43.9%	47.0%	52.3%	54.8%	58.3%	60.9%	63.7%	63.6%
Connecticut	26.4%	26.3%	26.9%	29.7%	34.0%	39.1%	43.3%	48.3%	53.0%	54.4%	55.4%	59.7%	62.4%	65.5%	70.1%	71.6%	73.2%
Delaware	24.5%	24.1%	24.1%	25.1%	27.2%	30.1%	32.5%	35.0%	37.9%	41.4%	48.5%	54.1%	57.1%	61.0%	63.4%	65.5%	67.0%
District of Columbia	25.0%	24.6%	24.6%	25.7%	28.4%	31.4%	34.3%	37.4%	41.0%	41.8%	49.9%	55.3%	57.9%	59.9%	63.1%	65.8%	68.4%
Florida	15.6%	15.6%	15.9%	18.4%	21.8%	25.0%	27.9%	31.1%	36.7%	40.6%	47.4%	53.1%	56.6%	61.6%	63.7%	65.5%	66.4%
Georgia	14.6%	14.6%	15.1%	17.4%	20.3%	23.3%	26.0%	29.4%	32.6%	36.7%	44.3%	50.6%	54.4%	58.9%	62.5%	63.7%	64.3%
Hawaii	27.8%	27.4%	27.7%	30.0%	33.5%	37.5%	40.9%	44.4%	48.9%	51.5%	53.9%	59.5%	61.8%	66.4%	70.0%	72.8%	75.9%
Idaho	16.8%	16.9%	17.2%	20.0%	23.5%	27.0%	30.1%	33.2%	36.4%	39.7%	44.2%	49.9%	53.5%	57.6%	60.9%	63.7%	63.6%
Illinois	24.6%	24.1%	24.3%	25.4%	27.7%	30.7%	33.1%	36.0%	38.9%	41.9%	45.9%	51.5%	55.9%	59.6%	62.5%	63.7%	63.6%
Indiana	15.7%	15.7%	16.2%	18.7%	22.1%	25.4%	28.0%	31.5%	34.6%	38.3%	43.0%	49.1%	52.3%	56.7%	60.0%	64.3%	64.8%
Iowa	25.6%	25.2%	25.5%	26.8%	29.5%	32.8%	35.5%	38.5%	41.6%	44.4%	47.9%	54.5%	58.3%	63.2%	67.6%	68.6%	68.4%
Kansas	26.2%	25.7%	25.8%	27.3%	30.0%	33.5%	36.5%	39.2%	43.0%	45.7%	48.6%	55.0%	57.0%	60.6%	63.7%	65.5%	65.4%
Kentucky	21.0%	19.9%	18.9%	19.3%	21.0%	23.1%	25.3%	28.2%	30.9%	35.2%	42.9%	52.0%	56.9%	61.8%	65.0%	66.9%	68.4%
Louisiana	15.2%	15.2%	15.5%	17.8%	20.6%	23.2%	25.5%	28.0%	33.7%	37.8%	43.1%	49.1%	53.2%	58.2%	62.5%	63.7%	63.6%
Maine	17.5%	18.2%	20.0%	23.4%	28.1%	33.0%	36.4%	40.2%	44.8%	47.6%	50.3%	56.0%	60.5%	64.9%	68.7%	68.6%	68.4%
Maryland	24.4%	24.1%	24.2%	25.5%	28.0%	31.1%	33.9%	36.8%	40.3%	43.6%	46.8%	54.9%	57.5%	64.3%	68.9%	73.2%	74.8%
Massachusetts	26.3%	26.1%	26.2%	27.9%	30.9%	34.8%	38.7%	42.4%	46.9%	49.4%	52.1%	57.3%	61.7%	65.5%	68.7%	68.6%	68.4%
Michigan	26.9%	26.6%	27.0%	28.9%	32.5%	36.9%	40.8%	44.4%	48.8%	50.8%	52.9%	60.8%	64.5%	68.3%	69.1%	69.5%	70.0%
Minnesota	26.8%	26.6%	26.9%	29.5%	33.5%	38.2%	42.5%	46.1%	50.6%	52.4%	53.5%	58.5%	65.4%	71.9%	78.0%	81.1%	82.6%
Mississippi	14.1%	13.8%	14.2%	15.7%	17.4%	19.3%	21.1%	23.3%	25.4%	30.2%	39.9%	50.5%	58.2%	65.5%	68.7%	68.6%	68.4%
Missouri	23.0%	22.7%	23.1%	23.9%	26.0%	28.7%	30.7%	33.2%	35.7%	39.3%	47.3%	53.5%	56.8%	61.0%	62.5%	63.7%	63.6%
Montana	22.9%	22.0%	21.0%	22.8%	25.6%	28.8%	32.6%	36.4%	40.4%	43.1%	47.1%	52.4%	54.9%	58.3%	61.0%	63.7%	63.6%
Nebraska	25.5%	25.3%	25.5%	27.1%	29.8%	32.9%	35.5%	38.2%	41.3%	44.0%	48.5%	53.8%	56.3%	59.7%	62.5%	63.7%	63.6%
Nevada	16.3%	16.4%	16.8%	19.2%	22.3%	25.8%	28.9%	32.3%	35.9%	39.8%	44.6%	50.4%	53.4%	57.6%	60.6%	63.7%	63.6%
New Hampshire	16.3%	16.4%	16.9%	19.8%	24.2%	28.7%	32.5%	37.1%	41.9%	45.0%	49.0%	54.7%	56.9%	61.2%	64.2%	67.5%	69.9%
New Jersey	25.4%	25.1%	25.2%	26.7%	29.6%	33.3%	36.1%	39.4%	42.7%	43.2%	47.0%	53.9%	56.9%	59.9%	61.3%	64.0%	66.7%
New Mexico	15.8%	15.9%	16.3%	18.9%	22.4%	25.7%	28.5%	31.5%	34.6%	38.3%	47.5%	53.2%	56.6%	61.0%	62.5%	66.8%	70.0%
New York	27.1%	26.8%	27.0%	29.4%	33.2%	37.8%	41.7%	45.4%	49.8%	50.7%	52.2%	57.2%	58.0%	62.8%	65.9%	68.6%	68.4%
North Carolina	15.2%	15.1%	15.6%	17.6%	20.4%	23.5%	26.1%	29.2%	32.1%	36.3%	46.8%	52.7%	56.4%	61.6%	64.8%	67.5%	68.4%
North Dakota	16.7%	16.9%	17.3%	20.8%	25.3%	29.8%	33.8%	37.6%	41.6%	44.3%	48.2%	53.4%	55.5%	58.6%	61.2%	63.7%	63.6%
Ohio	24.6%	24.2%	24.2%	25.3%	27.6%	30.4%	32.8%	35.3%	38.3%	41.6%	45.2%	50.8%	55.5%	59.4%	62.5%	63.7%	63.6%
Oklahoma	15.8%	15.8%	16.3%	18.9%	22.5%	26.4%	29.7%	33.4%	37.0%	40.5%	46.6%	54.5%	57.2%	61.0%	62.5%	63.7%	64.3%
Oregon	24.5%	23.4%	22.1%	23.7%	26.7%	30.3%	34.4%	38.5%	42.7%	45.4%	50.5%	55.7%	57.5%	60.4%	62.5%	63.7%	63.6%
Pennsylvania	25.8%	25.5%	25.5%	27.1%	30.0%	33.5%	36.6%	39.2%	42.5%	45.3%	50.3%	56.0%	58.0%	61.0%	62.5%	63.7%	65.2%
Rhode Island	26.4%	26.1%	26.5%	28.9%	32.7%	37.2%	41.4%	44.6%	49.3%	51.3%	53.1%	58.1%	61.9%	65.5%	71.2%	74.0%	76.7%
South Carolina	14.9%	14.8%	15.1%	17.0%	19.8%	22.5%	24.8%	27.2%	32.9%	37.6%	43.3%	52.0%	56.0%	62.7%	65.9%	68.6%	68.4%
South Dakota	16.9%	17.0%	17.4%	20.3%	24.0%	27.7%	31.3%	34.4%	37.9%	41.2%	45.6%	51.6%	56.2%	59.7%	62.5%	63.7%	63.6%
Tennessee	14.9%	14.8%	15.0%	16.7%	18.9%	21.2%	23.3%	25.8%	31.6%	36.3%	44.9%	51.2%	55.7%	61.6%	63.7%	66.4%	67.3%
Texas	14.3%	14.0%	14.3%	16.1%	18.8%	21.4%	23.6%	26.5%	29.1%	33.7%	39.2%	48.9%	53.1%	58.2%	62.5%	65.2%	66.7%
Utah	23.7%	22.8%	21.7%	23.3%	26.0%	29.4%	33.2%	37.0%	40.8%	43.6%	47.6%	52.9%	56.6%	59.8%	62.5%	63.7%	63.6%
Vermont	27.2%	27.3%	27.8%	30.4%	34.6%	39.5%	43.8%	47.0%	51.2%	53.1%	54.2%	59.1%	64.7%	70.2%	75.6%	78.0%	78.8%
Virginia	15.9%	15.9%	16.5%	19.3%	23.0%	27.1%	30.6%	34.6%	38.3%	41.6%	46.0%	52.0%	56.2%	59.7%	62.5%	63.7%	63.6%
Washington	24.5%	23.6%	22.5%	24.7%	28.6%	33.1%	38.1%	42.6%	47.7%	49.6%	52.4%	57.3%	58.2%	61.9%	67.3%	72.4%	74.3%
West Virginia	23.6%	23.2%	23.0%	23.7%	25.3%	27.8%	29.9%	31.7%	36.6%	40.5%	46.9%	53.2%	58.2%	63.2%	65.3%	65.5%	65.4%
Wisconsin	27.2%	27.0%	27.5%	30.1%	34.5%	39.3%	43.5%	47.0%	51.2%	52.7%	54.6%	60.1%	62.1%	64.6%	66.6%	68.9%	70.6%
Wyoming	16.7%	16.8%	17.4%	20.4%	24.0%	27.9%	31.3%	34.8%	38.2%	41.4%	45.8%	51.4%	55.9%	59.6%	62.5%	63.7%	63.6%

Appendix - Table C4
Estimated Percent of Early Childhood Years with Medicaid Eligibility
By Graduation Cohort - From Conception through Age 5
White Students

State	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
Alabama	6.5%	6.3%	6.2%	6.7%	7.3%	7.9%	8.4%	8.6%	9.0%	11.8%	14.5%	17.8%	22.3%	25.3%	28.0%	28.7%	28.4%
Alaska	8.3%	8.2%	8.3%	11.1%	14.9%	18.4%	21.4%	24.7%	27.6%	28.7%	29.5%	32.5%	32.9%	33.8%	34.7%	35.6%	35.2%
Arizona	0.0%	0.0%	1.0%	3.0%	5.1%	7.4%	9.5%	11.4%	14.1%	16.1%	19.7%	22.8%	24.7%	27.0%	28.2%	29.2%	29.1%
Arkansas	6.5%	6.3%	6.1%	6.8%	7.6%	8.4%	9.1%	9.9%	12.1%	14.5%	19.4%	22.6%	25.1%	27.0%	28.0%	28.7%	28.4%
California	14.3%	14.1%	13.5%	14.8%	17.1%	19.0%	20.9%	22.4%	24.2%	24.9%	25.1%	27.3%	27.6%	29.7%	31.9%	33.8%	33.4%
Colorado	12.3%	11.9%	11.3%	11.9%	13.1%	14.0%	14.8%	15.3%	16.0%	17.6%	19.2%	21.7%	23.4%	25.3%	27.2%	28.7%	28.4%
Connecticut	13.9%	13.5%	13.0%	14.3%	16.3%	18.2%	19.7%	21.6%	23.2%	23.9%	24.3%	26.2%	28.9%	31.3%	35.7%	36.9%	38.0%
Delaware	12.0%	11.5%	10.7%	11.1%	11.9%	12.6%	13.2%	13.6%	14.3%	16.2%	20.2%	22.8%	24.8%	27.0%	28.9%	30.5%	31.7%
District of Columbia	12.5%	12.0%	11.2%	11.6%	12.7%	13.5%	14.3%	15.0%	15.9%	16.1%	20.5%	23.0%	24.7%	26.0%	28.8%	31.2%	33.4%
Florida	7.1%	6.9%	6.8%	7.8%	9.0%	10.0%	11.0%	11.9%	14.0%	16.1%	19.8%	22.5%	24.6%	27.4%	28.9%	30.3%	31.0%
Georgia	6.4%	6.3%	6.2%	7.1%	8.2%	9.2%	10.2%	11.1%	12.0%	14.1%	18.0%	21.1%	23.4%	25.8%	28.0%	28.7%	28.9%
Hawaii	15.2%	14.7%	13.8%	14.6%	16.0%	17.1%	18.0%	18.9%	20.2%	21.7%	23.3%	26.2%	28.2%	32.3%	36.0%	38.7%	43.3%
Idaho	8.2%	8.0%	7.7%	8.8%	10.1%	11.2%	12.2%	13.1%	13.9%	15.7%	18.0%	20.6%	22.8%	25.0%	27.1%	28.7%	28.4%
Illinois	12.2%	11.7%	11.0%	11.4%	12.3%	13.1%	13.7%	14.2%	14.7%	16.5%	18.7%	21.3%	24.0%	26.1%	28.0%	28.7%	28.4%
Indiana	7.2%	7.0%	6.9%	8.0%	9.2%	10.2%	11.2%	12.1%	13.0%	14.9%	17.3%	20.3%	22.3%	24.6%	26.7%	29.3%	29.5%
Iowa	13.0%	12.5%	11.8%	12.3%	13.4%	14.3%	14.9%	15.5%	16.2%	17.8%	19.6%	22.9%	25.9%	29.4%	32.9%	33.8%	33.4%
Kansas	13.6%	13.0%	12.1%	12.6%	13.7%	14.6%	15.4%	15.9%	16.9%	18.5%	20.0%	23.1%	24.5%	26.8%	28.9%	30.3%	30.0%
Kentucky	9.7%	8.8%	7.8%	8.0%	8.6%	9.1%	9.8%	10.6%	11.3%	13.5%	17.5%	22.0%	25.1%	27.7%	30.5%	32.2%	33.4%
Louisiana	6.9%	6.7%	6.5%	7.4%	8.4%	9.3%	10.0%	10.7%	12.8%	15.0%	17.7%	20.5%	22.9%	25.6%	28.0%	28.7%	28.4%
Maine	8.2%	8.6%	9.0%	10.4%	12.5%	14.1%	15.2%	16.2%	17.7%	19.3%	20.7%	23.5%	27.6%	30.8%	34.1%	33.8%	33.4%
Maryland	11.9%	11.5%	10.8%	11.4%	12.4%	13.3%	14.1%	14.7%	15.6%	17.4%	19.1%	23.2%	25.0%	30.0%	34.4%	38.4%	39.4%
Massachusetts	13.7%	13.2%	12.4%	13.0%	14.2%	15.4%	16.6%	17.7%	19.0%	20.5%	22.0%	24.4%	28.3%	31.3%	34.1%	33.8%	33.4%
Michigan	14.3%	13.8%	13.1%	13.8%	15.2%	16.6%	17.6%	18.7%	19.9%	21.1%	22.4%	27.3%	30.9%	33.9%	34.5%	34.7%	34.8%
Minnesota	14.2%	13.9%	13.1%	14.3%	16.2%	17.8%	19.2%	20.2%	21.4%	22.4%	22.8%	25.1%	32.8%	39.6%	46.7%	50.6%	52.7%
Mississippi	6.0%	5.9%	5.7%	6.3%	6.8%	7.3%	7.8%	8.3%	8.8%	11.4%	16.2%	21.2%	26.7%	31.3%	34.1%	33.8%	33.4%
Missouri	10.6%	10.4%	9.9%	10.4%	11.1%	11.8%	12.3%	12.8%	13.2%	15.3%	19.6%	22.6%	24.7%	27.0%	28.0%	28.7%	28.4%
Montana	11.1%	10.3%	9.3%	10.0%	11.2%	12.1%	13.4%	14.6%	15.8%	17.3%	19.3%	21.8%	23.4%	25.3%	27.2%	28.7%	28.4%
Nebraska	12.8%	12.5%	11.9%	12.5%	13.5%	14.3%	15.0%	15.5%	16.1%	17.7%	20.0%	22.5%	24.2%	26.1%	28.0%	28.7%	28.4%
Nevada	7.7%	7.5%	7.3%	8.2%	9.3%	10.4%	11.4%	12.5%	13.5%	15.6%	18.1%	20.8%	22.8%	25.0%	27.0%	28.7%	28.4%
New Hampshire	7.8%	7.6%	7.4%	8.7%	10.5%	12.0%	13.3%	15.0%	16.6%	18.2%	20.1%	22.9%	24.4%	27.3%	29.8%	32.5%	34.1%
New Jersey	12.9%	12.4%	11.6%	12.2%	13.4%	14.4%	15.2%	15.9%	16.8%	16.7%	18.6%	22.1%	24.2%	26.0%	27.0%	29.5%	31.6%
New Mexico	7.3%	7.1%	7.0%	8.1%	9.3%	10.4%	11.4%	12.2%	13.0%	15.0%	19.9%	22.6%	24.6%	27.0%	28.0%	32.0%	34.7%
New York	14.5%	14.0%	13.2%	14.2%	15.9%	17.3%	18.5%	19.6%	20.8%	20.9%	21.7%	24.0%	24.8%	28.6%	31.3%	33.8%	33.4%
North Carolina	6.8%	6.6%	6.5%	7.3%	8.3%	9.2%	10.1%	11.0%	11.8%	14.0%	19.5%	22.3%	24.6%	27.4%	30.2%	32.6%	33.4%
North Dakota	8.1%	7.9%	7.8%	9.2%	11.0%	12.6%	13.9%	15.0%	16.3%	17.8%	19.7%	22.1%	23.7%	25.5%	27.3%	28.7%	28.4%
Ohio	11.9%	11.5%	10.8%	11.2%	12.1%	12.8%	13.4%	13.9%	14.5%	16.5%	18.4%	21.0%	23.8%	26.0%	28.0%	28.7%	28.4%
Oklahoma	7.3%	7.2%	7.0%	8.2%	9.6%	10.9%	12.0%	13.0%	14.0%	15.8%	19.1%	23.1%	24.9%	27.0%	28.0%	28.7%	28.9%
Oregon	12.8%	11.7%	10.3%	10.8%	12.1%	13.0%	14.3%	15.7%	17.1%	18.6%	21.0%	23.4%	24.8%	26.6%	28.0%	28.7%	28.4%
Pennsylvania	13.4%	12.8%	11.9%	12.4%	13.6%	14.5%	15.4%	15.8%	16.6%	18.2%	20.8%	23.7%	25.1%	27.0%	28.0%	28.7%	29.9%
Rhode Island	13.7%	13.3%	12.6%	13.6%	15.3%	16.8%	18.1%	19.0%	20.4%	21.5%	22.6%	24.9%	28.4%	31.3%	37.7%	40.9%	43.8%
South Carolina	6.6%	6.4%	6.3%	7.0%	8.0%	8.8%	9.6%	10.1%	12.3%	14.8%	17.8%	22.0%	24.4%	28.6%	31.4%	33.8%	33.4%
South Dakota	8.3%	8.0%	7.8%	9.0%	10.4%	11.6%	12.8%	13.5%	14.6%	16.3%	18.5%	21.4%	24.2%	26.1%	28.0%	28.7%	28.4%
Tennessee	6.6%	6.4%	6.2%	6.8%	7.5%	8.3%	8.9%	9.5%	11.7%	14.2%	18.6%	21.6%	24.2%	27.4%	28.9%	31.4%	32.1%
Texas	6.1%	5.9%	5.8%	6.4%	7.3%	8.2%	9.0%	9.8%	10.4%	12.9%	15.7%	20.4%	22.9%	25.5%	28.0%	30.4%	31.6%
Utah	12.0%	11.0%	9.8%	10.4%	11.5%	12.4%	13.7%	14.9%	16.0%	17.5%	19.5%	22.0%	24.3%	26.2%	28.0%	28.7%	28.4%
Vermont	14.6%	14.2%	13.6%	14.8%	16.7%	18.5%	19.9%	20.7%	21.9%	22.8%	23.4%	25.5%	31.4%	36.8%	42.3%	44.8%	45.5%
Virginia	7.5%	7.3%	7.2%	8.4%	9.9%	11.3%	12.5%	13.7%	14.8%	16.5%	18.7%	21.6%	24.1%	26.1%	28.0%	28.7%	28.4%
Washington	12.9%	11.9%	10.6%	11.6%	13.3%	14.7%	16.4%	18.0%	19.6%	20.6%	21.9%	24.2%	25.0%	28.0%	33.1%	37.8%	39.2%
West Virginia	11.4%	10.9%	10.0%	10.2%	10.7%	11.4%	12.0%	12.0%	13.8%	16.0%	19.4%	22.5%	26.1%	28.9%	30.5%	30.3%	30.0%
Wisconsin	14.5%	14.0%	13.4%	14.6%	16.6%	18.3%	19.7%	20.7%	21.8%	22.5%	23.7%	26.6%	28.5%	30.1%	31.8%	33.5%	34.7%
Wyoming	8.1%	7.9%	7.8%	9.0%	10.4%	11.7%	12.8%	13.9%	14.8%	16.5%	18.8%	21.3%	24.1%	26.1%	28.0%	28.7%	28.4%

Federal Financial Aid and Educational Attainment: Re-Examining the Social Security Student Benefit Program

with Leonard M. Lopoo

3.1. Introduction

For some time now, social scientists have described the United States as a country divided into two classes: the “haves” and “have-nots.” McLanahan (2004) argues that the disparity in resources between the children of these two classes has grown over time, with the low-income group suffering from even greater social isolation, the dissolution of family ties, and deeper poverty. She also shows that one of the primary factors that distinguishes these two groups is college completion, which comports with the common notion that higher education is one of the safest routes to the middle-class for low-income families.

Given the importance of obtaining a post-secondary degree for economic success, recent news media accounts have called attention to the increasing cost of higher education (see e.g., Campos 2015; Lewin 2013; Hildreth 2014). Figure 1 reports data from the College Board (in constant 2014 dollars) on the mean cost of tuition and fees in both private and public four-year colleges and universities from the 1971-72 academic year to the 2014-15 academic year. Both trends clearly show an increase in the advertised price of higher education, with the annual mean cost more than tripling for private colleges and universities and nearly quadrupling for public higher education.

With these increasing costs, one might worry about a reduction in access to higher education for less affluent Americans, which could limit their upward mobility. However, beginning with the Higher Education Act of 1965, the federal government has increasingly subsidized higher education for families across the income distribution through Pell Grants, tax credits, and a number of subsidized and unsubsidized loan programs. Figure 2 shows federal spending on several different programs that encourage post-secondary educational attainment. The Pell Grant program, which provides funding for low-income individuals who attend college, has increased considerably over the past twenty years, growing from awards of around $900 per full-time equivalent (FTE) in the 1993-94 academic year to over $2,200 by the 2013-14 academic year. The figure also shows that the Stafford loan program grew the most in dollar terms among the federal loan programs increasing from around $2,200 per FTE to over $5,000 per FTE during that same time span.

Simple economic theory dictates that, holding constant the cost, these subsidies should increase the likelihood that people will attend college. Recent research has consistently shown that these federal programs designed to reduce the cost of higher education have increased college access for many groups, including the low-income population, recent high-school graduates, middle-class families, and older students who start college in their twenties and thirties (Dynarski 2000; Seftor and Turner 2002; Dynarski 2003; Abraham and Clark 2006; Kane 2007; Turner 2011; Dynarski and Scott-Clayton 2013); however, much less is known about college completion. Among the low-income population, these programs may be encouraging students who are poorly prepared for college into a situation that is unsuccessful and potentially counterproductive to their human capital development. If true, these investments may be more efficacious if made earlier in the child's educational career. On the other hand, these programs may have given disadvantaged students the opportunity to obtain the human capital necessary to succeed in today's labor market.

The current paper is a follow-up to Dynarski's 2003 *American Economic Review* article on financial aid investments, college attendance, and years of education. In her analysis, Dynarski uses the National Longitudinal Survey of Youth, 1979 cohort (NLSY) and a difference-in-differences (DD) model to estimate the changes in college enrollment for children who finished high school near the phase-out of the Social Security Student Benefit Program (SSSBP), a program that provided large subsidies to qualified children who were in college.[1] By identifying potential program participants as those with a father passing before the NLSY respondent's 18th birthday, Dynarski shows that a $1,000 increase in higher education subsidies increased the likelihood of college attendance by about 3.6 percentage points by age 23. While receiving a lot less attention in that same article, Dynarski also estimates the effect on total education completed by age 28 writing that her result "suggests that aid eligibility did not simply speed up investment in schooling but also raised its optimal level (p. 284)."

This work re-investigates the latter claim. We use the same NLSY cohorts, as well as analyses with additional NLSY cohorts, to estimate respondents' educational attainment in their twenties through their forties. Our results show marginally significant effects of the SSSBP program on the educational attainment of recipients when measured as years of education. However, when we allow for differential impacts by degree type, we find strong evidence that the program increased the likelihood that high school graduates would go on to earn additional postsecondary degrees. Specifically, we find that access to the SSSBP for youth aged 18 to 22 created large increases in the likelihood of Associate's degree receipt by age 23 and 28 for individuals whose father died before they turned 18. Furthermore, the evidence suggests that many beneficiaries eventually went on to earn a Bachelor's degree.

[1] While targeted in the sense that only children with deceased, disabled, or retired parents paying into the Social Security system qualified for the SSSBP, the program was "universal" in that all students, once meeting the minimum requirements, qualified for aid regardless of the family's ability to pay for college. Thus, there could have been a significant fraction of children participating in this program who had the means to pay for college in the absence of the program. For these students, the SSSBP may have simply crowded out other forms of payment. As we show in our samples below, lower-income and minority children are much more likely to have a deceased parent; thus, we believe the results from this study are more likely generalizable to the low-income population.

Analysis of the SSSBP should provide information about the expected effects of higher education subsidies on educational attainment. The program was large in scope, provided generous benefits for students entering university full-time immediately after high school completion, had no conditions on the family's ability to pay, and contained no requirements for student performance. While this federal program no longer exists, lessons learned from the SSSBP could potentially inform the ongoing debate regarding the efficacy and cost-effectiveness of tuition assistance programs currently in place in the United States.

More specifically, findings from this study have important implications for recent policy discussions. In his 2015 State of the Union address, President Obama proposed a plan to provide two years of community college education at no cost to anyone who maintains a minimum grade point average and makes steady progress toward their degree. Our results suggest that federal investment in higher education could substantially increase Associate's degree completion rates among the disadvantaged population, i.e., this proposed program would be well targeted. Equally interesting is the finding that, for some, this opportunity could eventually lead to a Bachelor's degree.

3.2. The Social Security Student Benefit Program

Shortly after the enactment of the Social Security Program in 1935, it was amended to provide benefits to any dependent child of a disabled, retired, or deceased parent who qualified for the program, with a dependent child usually defined as a minor.[2,3] In the 1965 Social Security Amendments, Congress expanded the definition of a dependent child to include full-time

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[2] Except where noted, the legislative history described in this section comes from DeWitt (2001).

[3] Dynarski (2003) reports that 90 percent of student beneficiaries in 1982 qualified for benefits based upon their father's Social Security earnings history. Thus, survivor benefits are tied to the father's life course in this analysis.

students under the age of 22, under the premise that parents traditionally help pay their child's higher education expenses. This legislative change effectively extended the Social Security child benefits into early adulthood under a program commonly referred to as the Social Security Student Benefits Program. The program was very popular, and in 1981, the largest pay-out year, over 760,000 students received the benefit at a monthly cost of nearly $200 million. Dynarski (2003) reports that during its peak, around 12 percent of all full-time students between the ages of 18 and 21 were receiving the benefit.

For a variety of reasons, including reducing the cost of the Social Security program, President Reagan signed the Omnibus Reconciliation Act of 1981, which legislated that Social Security child benefits for full-time students aged 18 to 21 would be phased-out by 1985. Specifically, students who were enrolled in college by May 1982 would continue to receive benefits annually, but at a 25% reduction with benefits terminating in April of 1985. Younger, and previously qualified college students, would no longer receive any tuition supports in the form of the child benefit extensions. We use the elimination of the SSSBP for full-time college students, the policy change investigated by Dynarski (2003), to estimate the effect of federal subsidies for higher education on the educational attainment of several cohorts who would have completed high school around the time the benefits were eliminated.

In an effort to increase the higher education levels of at-risk young adults with a deceased, disabled, or retired wage earning parent, the SSSBP was very generous in a number of dimensions. Dynarski (2003) reported that the average annual benefit for a qualifying student was $6,700 in 1980. To put these nominal funds in perspective, she reports, the average Pell Grant was $2,000 while guaranteed student loans were approximately $4,500, on average, in the same year. From this, Dynarski concluded, average benefits were more than sufficient to cover tuition and fees at public four-year colleges and university, where costs averaged about $1,900 in 1980. Moreover, and importantly for low-income youth at the margin of college enrollment, these funds could have been sufficient to cover a significant portion, if not all, of their living expenses.

While there were large financial benefits, the barriers to SSSBP enrollment were minimal. The Social Security Administration (SSA) would contact child benefit recipients slightly before their 18th birthday to investigate whether the beneficiary would attend college full-time after high school graduation. If the recipient declared that s/he was college-bound, the SSA would continue mailing a lump-sum check directly to the child until they either (1) left school, (2) married, or (3) turned 22. To validate enrollment, colleges and universities would provide proof of full-time enrollment on an annual basis.[4] Thus, the SSSBP provided substantial benefits in terms of aid receipt and ease of enrollment, which explains the popularity of the program and its large number of beneficiaries. It also illustrates why, as Dynarski (2003) writes, "Except for the introduction of the Pell Grant program in the early 1970s, and the various G.I. bills, this is the largest and sharpest change in grant aid for college students that has ever occurred in the United States."

3.3. Literature Review

This section outlines the academic literature that investigates the relationship between federal higher education financial assistance and education outcomes. Given that this is an extension of Dynarski (2003), however, we first provide a detailed review of her article.

[4] Program details in this paragraph were taken from Dynarski (2003), Committee on Ways and Means (1979, 1982) and the Office of the Comptroller General (1979).

As mentioned, Dynarski (2003) uses data from the NLSY and the elimination of the SSSBP in 1982 to estimate the impact of the tuition benefits on college enrollment and completion. She finds that a $1,000 decrease in college costs increases college enrollment by 3.6 percentage points, which is within the 3 to 6 percentage points range established by other authors (Leslie and Brinkman 1988; Dynarski 2000; Seftor and Turner 2002; Dynarski 2003; Abraham and Clark 2006; Kane 2007). In addition, Dynarski reports that the SSSBP appears to have boosted full-time college attendance for qualifying students by over 20 percentage points.

While the academic literature has consistently shown that college enrollment increases as students receive more financial aid, the research investigating total education or degree attainment remains thin. Dynarski (2003) also examines the number of years of education completed at age 23 and age 28. She shows that students qualifying for the federal tuition supports under Social Security completed a little over a half year more of college than those who would have qualified for the program had it not been eliminated in 1982. Estimates rise to between 0.679 and 0.754 more years of education at age 28 depending on how attriters and misclassification error is handled. With this increase in education at age 28, Dynarski concludes that the educational gains are attributable to SSSBP benefits and not a delay in the timing of higher education by non-qualifying students.

Although the estimates for full-time college attendance are precisely measured in her paper, coefficients on the number of years of schooling are not. In fact, of the eight coefficients related to post-secondary education, none is statistically distinguishable from zero at the 0.05 level. In other words, while she estimates statistically significant impacts of the probability of full-time college attendance, Dynarski does not show that attendance translates into degree completion.

There are a number of plausible reasons to explain why Dynarski does not find an effect for total education. First, and perhaps most obvious, the program may be pushing students who are poorly prepared for higher education into college. These students may have higher rates of attendance but subsequently drop out of college before completing their degree. Second, there are relatively few “treated” cases in the NLSY data, and the small sample may be insufficient to identify an effect with precision, despite there being enough cases to identify impacts on college attendance. Third, Dynarski is estimating a mean effect on the program on years of education across all potential students. However, additional years of education are not necessarily rewarded by the labor market. Instead, degree completion typically translates into larger financial rewards. Consequently, her models may be misspecified: one should instead estimate changes in degree completion rather than years of education. Finally, the universal benefits provided under the SSSBP may have crowded out other sources of funding for more affluent students in the sample. While NLSY respondents are disproportionately from the low-income population, the more affluent students in the sample would have attended anyway, but financed their education through other sources, such as their parents. In other words, there could have been crowd out of private funding sources but no real impact on human capital accumulation. By pooling both types of students, the signal to noise ratio could be insufficient to produce significant results.

Other studies examine the impact of tuition subsidies on post-secondary years of education or degree attainment. Angrist (1993) and Bound and Turner (2002) explore changes in the G.I. education bill after World War II and the Korean War to show that grant aid greatly increased the probability of degree attainment by veterans. Bettinger (2002) utilizes discontinuities in the federal Pell Grant eligibility formulas to estimate the impact of grant size on the persistence rates of college students. Although his findings are sensitive to functional form, he also reports evidence of a positive link between aid receipt and increases in post-secondary educational attainment.

More recently, the academic literature has moved away from the analysis of large-scale, federal programs toward isolating impacts of state-level programs which typically tie aid to student performance. Dynarski (2008) uses the implementation of merit aid program in Arkansas and Georgia to analyze why students exit college without receiving a diploma. These state programs were similar in that they provided substantial scholarships for students meeting GPA and test standards established by the state. Exploiting the cross-cohort changes in probability of college completion in Arkansas and Georgia, she reports that the scholarship programs increased college completion by 3 percentage points or roughly an 11 percent increase in graduation rates for cohorts qualifying for the generous state tuition support programs.

Similar to the state merit aid programs in Arkansas and Georgia, which both tied aid receipt to academic performance, Scott-Clayton (2011) explores the impacts of the PROMISE program in West Virginia. With access to administrative data, she exploits discontinuities in eligibility rules and implementation timing to conclude that the state-level program led to statistically significant increases in the number of credits earned. Moreover, the West Virginia PROMISE program increased the probability of four-year BA completion rates by almost 60 percent (9.4 percentage points on a baseline of approximately16 percentage points). Five-year Bachelor's degree completion rates also increased by 12 percent.

Two MDRC randomized controlled trials (RCTs) also provide insights into the effects of performance-based scholarships and years of college completed, which can eventually result in completed degrees. Richburg-Hayes et al. (2009) report findings from MDRC's Opening Doors Study targeting low-income students in Louisiana; this demonstration tied relatively modest scholarships to enrollment and GPA minimums. Although follow-up of the program was disrupted by the aftermath of Hurricane Katrina, the authors still report a positive and significant impact of the program on college enrollment, persistence, and credits completed. MDRC extended this pilot program to six other states – each with varying target populations, eligibility requirements, and scholarship amounts – via the Performance-Based Scholarship demonstration. Analyzing data from these RCTs, Patel and Richburg-Hayes (2012) generally find significant increases in credits earned across the various state programs, with some evidence of positive impacts on student performance.

This paper contributes to the existing literature in two important ways. First, we expand Dynarski (2003) by utilizing more NLSY cases over a longer period of time. Secondly, we advance the literature by examining the influence of these subsidies on educational attainment measured both in years and degree attainment.

3.4. Data and Descriptive Statistics

The National Longitudinal Survey of Youth, 1979, (NLSY) is a cohort survey conducted by the Bureau of Labor Statistics. It began with a nationally representative sample of 12,686 American youth aged 14 to 22 in 1979, the survey baseline. Respondents were interviewed annually through 1994 and then biennially thereafter. Data collected by the BLS provide life-course information for respondents on a number of items, including school enrollment, educational attainment, and detailed family history information. This implies that, for respondents remaining in the survey through the most recent release in 2012, we have data through ages 47 to 55.

With the elimination of the SSSBP by Congress in May 1982, the NLSY sample includes a set of cohorts finishing high school directly before and after program cessation. Like Dynarski (2003), we are able to use enrollment information captured by surveyors to assign cohorts to pre- and post-periods by the spring in which they were enrolled in grade 12. In what we label the "Replication Sample," we follow her original analytic sample construction and assign a subset of NLSY respondents to cohorts around this threshold by limiting data to respondents meeting the following criteria:

(1) Enrolled in 12th grade or less in 1979,
(2) Lived with their parent(s) during their senior year, and
(3) Participated in the 1988 survey. This wave collected the only detailed set of life-history information on the respondent's parents during childhood.

With these restrictions, the sample of 12,686 original respondents drops to 3,987 matching Dynarski's original sample.[5] From the Dynarski sample, we further remove 126 cases. First, we exclude children not born in the United States and also not living in the U.S. at age 14 because they most likely do not qualify for the program based upon their father's work history. Secondly, we further restrict the data to respondents which have finished high school by age 23. This is done for three reasons: (1) to simplify interpretation in our empirical modeling, (2) to allow us to add cases of respondents graduating before 1979, and (3) because the SSSBP did not impact high school graduation.[6]

[5] The "Replication Sample" includes one more observation than the total reported in her original publication. Inclusion of this respondent – who falls into the post-comparison group – should have minimal impact on our estimates.
[6] In footnote 3, Dynarski (2003) reports that the SSSBP does not impact high school graduation rates. We have independently verified these claims with results available upon request.

In Table 3 (discussed below), we show how removing these cases affects our results. Ultimately, we end up with 3,861 cases after our two exclusions, which we call the "Limited Dynarski Sample." These observations are further split into four quasi-experimental groups based on two categories:

(1) Respondents whose senior years occurred before 1982 constitute the pre-period group (i.e., the potential graduation classes of 1979, 1980, 1981). These students could have been eligible for the SSSBP. Remaining individuals, with later graduation dates, were seniors during the post-period.

(2) "Treatment" – as in Dynarski's analysis – is defined by having a deceased father before the NLSY child turns 18. Again, following Dynarski, treatment status is identified using the detailed family history information provided in the 1988 survey. For consistency, we will refer to the students with a deceased father in the cohorts after the SSSBP was phased out as being in the "pseudo-treatment" or "pseudo-treated" group.

In her original sample, Dynarski has a relatively small number of "treated" observations used for identification in her DD design. To increase the statistical power of the modeling, therefore, we also utilize a larger sample, which we label the *Expanded NLSY Sample*. In addition to Dynarski's original sample, the Expanded NLSY Sample:

(1) Includes youth finishing high school before 1979. While we are unable to identify their exact senior year because we lack enrollment information before 1979, we know that this subset of individuals likely became high school seniors while the SSSBP was still in place, and are, therefore, observations from the pre-period. Importantly, parental information is still collected for these individuals in 1988, so we can identify respondents

with deceased fathers before age 18, which made them potentially eligible for SSSBP funds.

(2) Removes the restriction that the students must have lived with their parents during the spring of their senior years. This decision is made, in part, because the NLSY does not capture this information for respondents no longer living within their childhood homes at baseline (e.g., 1979).

Descriptive statistics for the Limited Dynarski Sample and the Expanded NLSY Sample are shown in Tables 1 and 2, respectively. These tables are modeled after Table 1 in Dynarski (2003). There are four general sets of variables used in Dynarski and our analysis. Appendix A outlines how these variables were created. It also discloses which covariates differ from the original Dynarski specification. Substitutions were required to expand our sample because historical information required to construct select controls was not recorded for respondents no longer living in their childhood homes in 1979.

As noted, the quasi-experimental design groups are constructed based upon the childhood family roster information collected in the 1988 survey. We were able to replicate and expand Dynarski's original categories for this analysis. Regarding individual controls, Dynarski used the AFQT score administered in 1979, race/ethnic group indicators, the respondent's age in 1988, and state of residence indicators (during the first survey year). Rather than age-adjusting AFQT as prescribed by Dynarski (2003), we use the NLSY age-adjusted measured released in 2006. Lacking access to the non-restricted data, we currently use Census Region dummies instead of the more specific state dummies employed by Dynarski. Two of the four family-level controls utilized by Dynarski – e.g., father/mother attended college – are also used in this analysis. In order to expand the NLSY sample to cohorts finishing before 1979, we use alternative definitions of family income and family structure.

The last set of variables are the outcomes. Our college attendance and years of schooling variables are exactly the same as Dynarski's. Utilizing the information collected in nearly every NLSY wave, we also construct a series of variables which indicate the highest degree attained at a particular age. High school degrees – which include both traditional diplomas and GEDs – are the minimum degree level analyzed in this analysis. The next level is "other credentials" which includes other qualifications such as certificates, licenses, or journeyman's card which were required in professions such as nursing, automotive repair, steel working, etc. Junior college and Associate's degrees are henceforth referred to as "Associate's degrees", while we combine Bachelor's and higher level degrees (e.g., Master's, professional, etc.) to simplify exposition.

As displayed at the bottom of Table 1, there are 134 individuals in the sample who could have received SSSBP funds before the program's termination in May 1982. In the post-period, there are 53 young men and women who could have qualified for tuition supports had they not been eliminated. Expanding the sample, as described and outlined in Table 2, increases the number of potentially qualifying individuals to 339, while the post pseudo-treated group also increased to 59. Sample sizes for the second comparison groups in our DD analysis are large – e.g., exceeding 1,000 individuals – in both the Limited Dynarski and Expanded samples, for both the pre- and post-periods.

The last column in both tables shows a t-test for the difference-in-differences coefficient in a model without controls. This set of simple tests identifies the covariates and outcome variables that differ across the time and treatment categories before entering the multivariate modeling. For the covariates, this serves as a balance check to determine whether there is a significant compositional change in respondent type from the pre- to post-period. Optimally, the treated groups would be identical to the control groups with the exception that the treatment group experienced a parental death. Though the death of a parent is not necessarily a random event, the DD model is still identified if the trend in educational outcomes for the control group appropriately serves as the counterfactual trend for the respondents with deceased parents. One way to test for this common trends assumption is to compare the DD estimates for factors exogenous to parental death to determine if there are differences in the patterns observed in the period before and after the SSSBP was phased out.

As displayed in the last column, the common trends assumptions appears to be supported for most of the covariates in Table 1 and 2, with the exception of the AFQT score. In both samples, we see a noticeable decline in the percentile AFQT scores among the respondents with a deceased father in the post-period. While we account for the AFQT scores in our models, if other unobservable factors are different between the treatment and control, then our results are potentially spurious. In the empirical section below, we explain adjustments we make to account for this potential source of bias.

Tables 1 and 2 also show results from t-tests for a select subset of outcomes. We provide these t-tests to initially identify potential program impacts from the SSSBP. Note that there appears to be a statistically significant difference in the probability of full-time college attendance by age 23, the maximum education ever reported, and select degree attainment at age 23.[7] Graphical analysis of degree attainment can provide further insights which are not obvious from the descriptive statistics. The bottom of Figure 3 contains two histograms, which display the distribution of degrees at age 23 by the four quasi-experimental groups. Degree attainment is

[7] Recall that the most robust finding in Dynarski (2000) is on the probability of full-time college attendance by age 23.

shown in a disaggregated form, ranging from a high school diploma only to those who completed a four year degree or more.

Starting with the Limited Dynarski sample, the distribution of diplomas is fairly constant when comparing educational attainment for the comparison group in the pre- to post- periods. For the respondents who have a deceased father, the proportion who do not achieve any other degrees by age 23 increases over 20 percentage points: from 66 to 85 percent from the pre- to the post-period. The likelihood for an Associate's degree drops from 11 percent to 2 percent for the treatment group and remains flat for the comparison group. The "other" credential shows similar results. The proportion receiving Bachelor's degrees falls slightly for the treatment group and has an even smaller decline for the comparison group. These patterns are similar for the Expanded NLSY Sample. This set of results suggests that the SSSBP may have increased educational degree attainment for some beneficiaries changing their degree attainment from high school to other credentials and Associate's degrees.

3.5. Empirical Strategy

Given that we observe cohorts of high school graduates in the NLSY immediately before and after the elimination of the SSSBP, we use the DD research design employed by Dynarski. Specifically, we estimate the following model:

$$(1)\ y_{i,a} = \alpha + \beta_1(PRE)_i + \beta_2(DeceasedDad)_i + \beta_3(PRE * DeceasedDad)_i + \boldsymbol{X}'\gamma + \varepsilon_{i,a},$$

where y is years of education for individual *i* at age *a*, PRE is an indicator variable signaling students who could have qualified for SSSBP funds before program termination in May 1982, DeceasedDad is an indicator for youth with a father who died before their 18th birthday, **X** is a matrix of other controls (the family and individual characteristics outlined in Appendix A), and $\boldsymbol{\varepsilon_{i,a}}$ is a robust standard error term which is clustered at the household level.

In addition, following Dynarski, we also interact all of the covariates in matrix **X** with the PRE variable and the DeceasedDad variable. We include these interactions to capture unobserved differences between the treated and control group (and for cohort differences) that we cannot explicitly account for in our models.

This DD model identifies the change in educational attainment of youth with deceased fathers who had access to federal supports relative to those who did not. The second difference, derived from the change in the outcome for a second group who never qualified for benefits, serves as the counterfactual difference in educational attainment had the program remained intact. Under the common trends assumptions implicit within this DD design, this second group yields more precise program effects because it nets out factors, such are the strength of the macroeconomy or changes in the cognitive ability of the cohorts, that would introduce omitted variable bias in a simple pre-post analysis.

The SSSBP provided benefits for students until age 22; therefore, we should expect educational differences to surface as early as age 23. Thus, like Dynarski, we report estimates for educational attainment at age 23 and age 28. In addition, we also examine education attainment at other points – including ages 30, 35, and 40, as well as the maximum age the individual ever reported in the NLSY – to determine the long-term impact of this program.

The DD framework utilized by Dynarski identifies the mean differences in the total years of education, and we start by attempting to duplicate her results. However, employers are typically interested in degree attainment because it serves as a stronger signal of labor market productivity than years of schooling (Hungerford and Solon 1987; Belman and Heywood 1991; Jaeger and Page 1996; Card 1999). This suggests that we should model postsecondary degree attainment as an outcome as well, while explicitly considering the distribution of post-secondary degrees in the United States.

Potential changes in the educational attainment continuum can be best illustrated via a series of graphs. Figure 4 contains two charts: the first shows the total years of education completed using a pooled sample of all waves from the Current Population Survey collected in 1990, and the second reports degree attainment from the 1990 Census micro sample. In both cases, educational attainment is limited to individuals aged 25 to 33 with at least a high school level of education.[8] These figures clearly illustrate that educational attainment is discrete and tends to cluster at degree completion points. In the case of years of education, almost half of the sample has exactly 12 years of education and there are jumps in the frequencies at two, four, and six years of college education which correspond roughly to Associate’s, Bachelor’s, and Master’s (plus) degrees. For the highest degree received, the two most common values, high school diploma or Bachelor’s degree, represent over 80% of sampled individuals.

We use a second set of linear probability models, building off of equation 1, to investigate the probability that a respondent potentially qualifying for SSSBP benefits obtains a particular terminal degree: e.g., having a high school diploma, an “other” credential, an Associate’s degree, or a Bachelor or graduate degree. We also examine attainment at several ages to measure if the program affects people differently at various points during their lives.

3.6. Results

The first panel in Table 3 reports the impact of the SSSBP on completed years of education by age 23 using several different samples. The first entry shows a 0.564 year increase

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[8] This matches the original sampling frame of the NLSY79, where respondents were 14 to 22. Moreover, the sample is limited to high school graduation because they should be most affected by the SSSBP.

in the mean years of education completed. This estimate was reported by Dynarski (2003) using her sample and variable construction. The next entry under the column titled "Replication Sample" uses nearly the same respondents as Dynarski with the variables we constructed to match her specification. The point estimate for the replication is 0.327, somewhat lower than Dynarski's estimate. Thus, our variable construction clearly affects the DD point estimates.[9] In the third column, we report coefficient estimates when we use the Limited Dynarski Sample, and the last column provides estimates when we use the Expanded NLSY Sample. All of the point estimates are similar, but those obtained using the Expanded NLSY Sample are closest to Dynarski's result and are the most precisely measured. The same is true when we measure years of education at age 28 (panel 2). In the last two panels, we report the results for educational attainment at age 35 and age 40. Dynarski does not report on educational attainment at these older ages. Our point estimates using the Expanded NLSY Sample range between 0.5 and 0.9 years of education depending on the age when educational attainment is measured. Dynarski's estimates ranged between 0.5 and 0.7. Given the extra sample size and the similarity in result, we report estimates for the Expanded NLSY Sample from this point forward.

Depending on the sample, variable construction, and age used, one estimates a mean increase in educational attainment of between 0.5 and 0.9 years. While this increase is potentially important, degree attainment is a stronger signal to employers about human capital accumulation. In Table 4, we report the DD estimate for the likelihood of having only a high school diploma in the first panel. By age 23, those who were eligible for the SSSBP program were much more likely to continue their education following high school than those who had a deceased father after the program ended. The DD estimate is nearly 22 percentage points, which on a sample

[9] One potential issue is that Dynarski uses the Restricted NLSY79 data to add state fixed-effects. We have applied for access to this data and will incorporate these variables in the forthcoming months.

weighted average baseline of roughly 60 percent, is very large. This difference persists as the potential beneficiaries aged and, notably, may have actually increased over time. By age 30, the DD estimate is nearly 30 percentage points. By 35 or 40, the difference is around 25 percentage points. Clearly, degree attainment declined for individuals with a deceased father, relative to those without a deceased father after the SSSBP was eliminated. Results are both large and statistically significant.

The next three panels use different degree attainment outcomes, which essentially decompose the results reported in the first panel. Panel 2 shows that individuals were 10 percentage point more likely to receive an “other” credential by age 23, and while estimate precision declines for the measure at different ages, the point estimate is fairly constant. In panel 3, we see an initial increase in the likelihood of an Associate’s degree. By age 23, there is a 7.6 percentage point increase in the probability of an Associate’s degree and, by age 30, there is an 8.6 percentage point increase. Both estimates are statistically significant at the 0.10 level. However, by age 35, the marginal change in the probability of obtaining an Associate’s degree declines to about zero.

Interestingly, in panel 4, we see little impact on a Bachelor’s degree before age 35, but we observe the DD estimates starts to increase around age 30 and becomes larger and more significant as the individual ages. By the maximum age in the NLSY, which is typically measured after the respondent turns 45, we observe a statistically significant 18.7 percentage point increase in the likelihood of a Bachelor’s degree.

Collectively, this set of results suggests a compelling story. Clearly, the program reduces the likelihood that a high school diploma was the terminal degree for SSSBP candidates. When one considers the type of degree obtained, we see a couple of patterns emerge. Apparently, some of the beneficiaries used the benefit for “other” credentials and cease their educations once they have finished their training. The other group appears to enter a track that uses the subsidy to complete his/her Associate’s degree. However, many of this group continued to pursue their educations after they no longer receive benefits from the program, which must terminate at age 22. Therefore, the SSSBP may have pushed these recipients on a different educational trajectory, one which ultimately ended in a Bachelor’s degree.

To test this interpretation of our results, in Table 5 we show a transition matrix for those who reported they had an other credential by age 23 (Panel 1) and those who reported that they had earned an Associate’s degree by age 23 (Panel 2) separating the cases into four groups: treatment in the pre-period, “pseudo-treatment” in the post-period, and the comparison group in the pre- and post-periods. If our hypothesis is correct, we should see flat proportions of attainment for the other credential for the pre-treated group and growth in the proportion who have a Bachelor’s degree for those with an Associate’s degree as the respondents age. This is exactly what we find.

Among the treated group who had other credentials by age 23, 94 percent remained at that degree level by age 30. By age 40, 88 percent had not earned more education. For those in the post “pseudo-treatment” group and those in the two comparison groups, this pattern was quite similar. Thus, while there may have been some growth in the likelihood of an other credential among those who received tuition supports while the SSSBP was in place, there was not a lot of additional educational attainment over the life course for this group.

Panel 2 conditions the Expanded NLSY sample to those with Associate’s degrees by age 23. This group follows a much different pattern over the life course than those in the first panel. Relative to the comparison groups, we find larger proportionate changes in Bachelor’s degree attainment for the treatment/pseudo-treatment cohorts finishing around the time of the SSSBP elimination in May 1982. By the maximum age education was measured, we find that nearly two in five in the pre-treated group – e.g., those qualifying for the tuition supports in early adulthood – had a Bachelor's degree. The change observed in Bachelor's degrees for those in the comparison group is somewhere between 20 and 25 percent. Stated differently, youth qualifying for SSSBP funds were substantially more likely to receive a Bachelor's degree or more by the end of the sample period than respondents who never would have qualified for the program and who had also obtained an Associate's degree by age 23.

3.7. Sensitivity Analysis

Earlier we showed that the post "pseudo-treated" group had much lower percentile AFQT scores than the pre-treated group. We included interactions between the covariates and treatment groups to potentially account for compositional changes in the treatment groups, but one should remain concerned that the composition change remains an issue. This difference could introduce an important source of bias into our findings.

Scholars frequently use a number of econometric techniques to reduce the heterogeneity between the treatment groups. One common approach is to match cases. Unfortunately, the "pseudo treatment" group is quite small in the post-period and the NLSY does not include many measures for the father of the NLSY respondents that are exogenous to the father's health or that could be linked to death. When we attempted to match "treatment" respondents to the comparison group, the common support was small and we could not achieve balance in the AFQT scores.

Instead, one easy approach to test the sensitivity of our results is to trim the sample by AFQT scores. Figure 5 shows that the individuals in the post "treatment" group are disproportionately in the bottom decile of the AFQT distribution. These differences could be driven by the procedure used to age-adjust the AFQT scores, the smaller sample size, shifts in the abilities of the youth with deceased fathers in the post-period, or simply sampling variation.

To create more homogeneous analytic samples in a straightforward manner, we trim all of the samples below the 10th percentile of the AFQT distribution. This choice clearly reduces the generalizability of the result: we no longer include those at the bottom of the AFQT distribution. Nevertheless, this simple approach creates balance in the AFQT scores for the four quasi-experimental design groups. Table 6 shows the same descriptive statistics previously reported in Table 2 with the trimmed sample. The t-test column shows that there is no longer a statistically significant differences in AFQT scores at any conventional level of significance, making the common trends assumption more likely.

In Table 7, we report the same results as shown in Table 4. While we lose some precision with the lost cases, the overall patterns are the same. We see a large increase in the likelihood that a high school diploma is an individual's terminal degree once the SSSBP ended. We also observe that the increase in the probability of other credentials is constant across all ages. For higher degree attainment, the trends match Table 4: we find an increase in the probability of earning an Associate's degree through age 30 and, starting at age 35, we see a large increase in the likelihood of a Bachelor's degree.

3.8. Discussion and Conclusions

In the 21st century labor market in the United States, post-secondary degrees are increasingly becoming the minimum requirement necessary to obtain financial and employment security in an economy no longer providing large numbers of well-paid manufacturing positions. However, the costs of higher education continue to skyrocket. To the extent that policymakers are concerned about addressing the growing income and life course divisions in the U.S., increasing post-secondary educational outcomes for youth at-risk of living in poverty as adults appears to be the surest path towards the middle class and self-sufficiency. This means not only increasing access to college but promoting diploma completion.

While research has consistently shown that federal programs have reduced the burden of these higher education costs and have increased college access for many groups – including the low-income population, recent high-school graduates, middle-class families, and older students – much less is known about college completion. This paper seeks to address this latter shortcoming by using the framework established by Dynarski (2003) to explore and isolate SSSBP's impact on post-secondary diploma attainment for a potentially disadvantaged group of students. Eligibility for the SSSBP occurred only after a child lost a parent before they turned 18, and low-income and minority children were much more likely to have a qualifying parent. Thus, the estimation strategy utilized by Dynarski provides useful insights into how generous supports, which could be used to cover tuition and living expenses, can influence the educational attainment of a particularly vulnerable group of students.

Like Dynarski, we begin by analyzing the programs impact on completed years of education. We find that program effects by age 23 and 28 were relatively small and marginally significant: roughly 0.5 years more of education, on average, and statistically significant only at the 10 percent significance level. By utilizing data collected after her publication, we see that program impacts persist, and actually increase in both size and statistical precision, as the respondents got older. By age 40, we observe that individuals with potential access to the SSSBP have accumulated nearly one more year of post-secondary education.

Conceding that 0.93 years of education, on average, does not necessarily translate into more degrees, we examine a series of linear probability models which estimate the likelihood of having a particular degree type as the highest level of education received at a given age. Unequivocally, these models show that youth potentially qualifying for the SSSBP had greatly increased probabilities of obtaining degree types beyond a high school diploma. This is true at all ages. Notably, it appears that the youth accumulated other credentials or Associate's degrees while qualifying for the SSSBP (i.e., before they turned 22) and, furthermore, a significant margin of those with Associate's degrees would finish their Bachelor's degree at later points in their lives. Relative to other groups of student who had obtained an Associate's Degree by age 23, SSSBP qualifiers were over 50% more likely to have completed Bachelor's degrees by their last recorded age in the NLSY survey. Moreover, sensitivity analysis confirms that these tendencies were not driven by compositional changes in the four quasi-experimental groups.

The findings from this study have important policy implications in light of recent political developments. In his 2015 State of the Union address, President Obama unveiled a proposal which would provide two years of community college education at no cost to students maintaining minimum grade point averages and making steady progress toward their degree. While this proposal does not exactly match the SSSBP – a program which provided no-string-attached funds which could cover both tuition and living expenses – our results suggest that federal investment in higher education via community college access could substantially increase Associate's degree completion rates by fostering an initial avenue for low-income youth to fund their higher education. If SSSBP beneficiaries identified in this analysis are in anyway representative of the youth today, students would be closer – and much more likely – to complete their four-year degrees as well. With these more advanced degrees, at-risk youth can further increase their human capital and better insulate themselves against structural changes in future labor markets. These benefits, in turn, could help policymakers begin addressing some of the systemic factors contributing to the diverging destinities for American youth described by McLanahan (2004).

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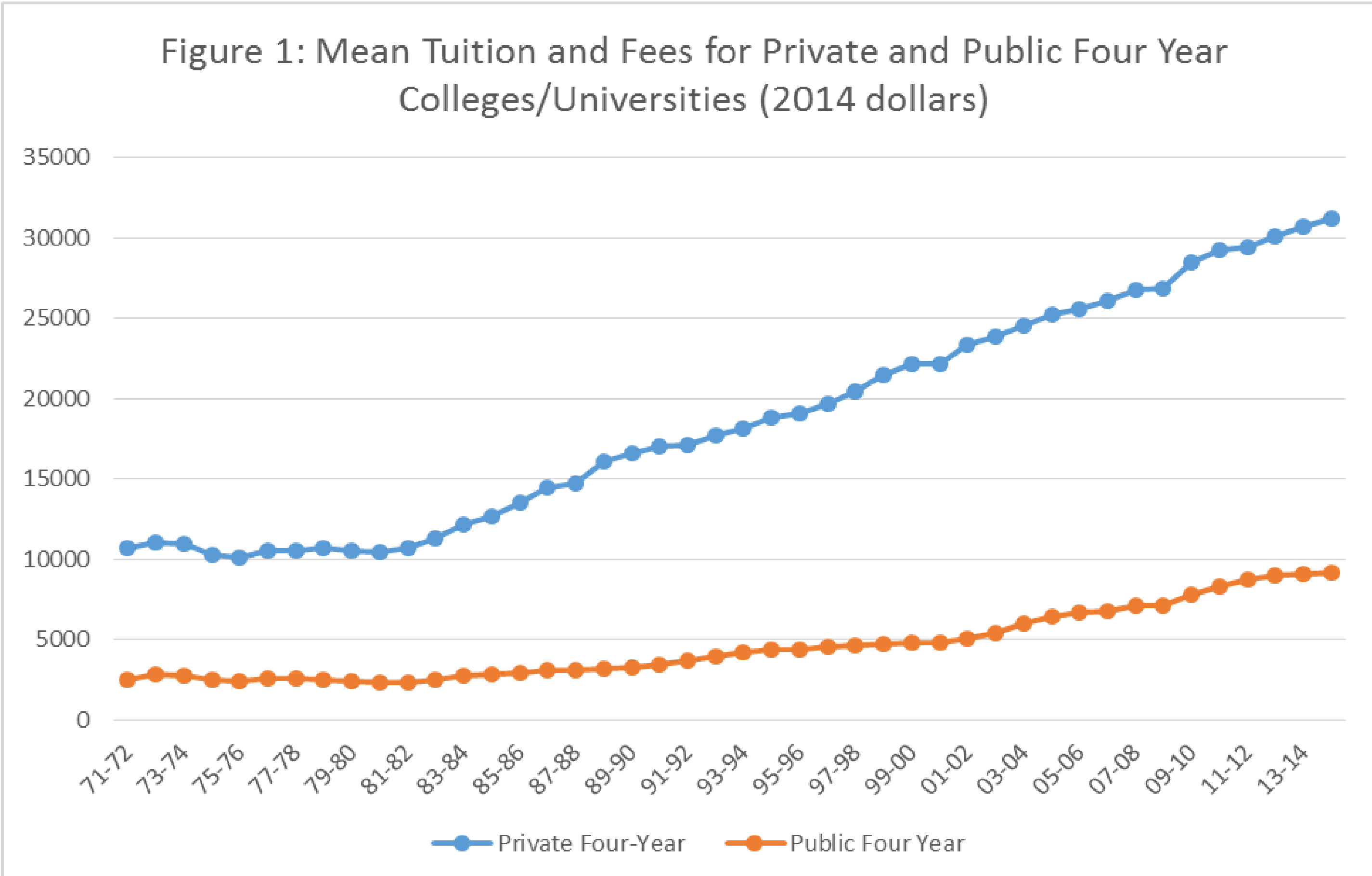
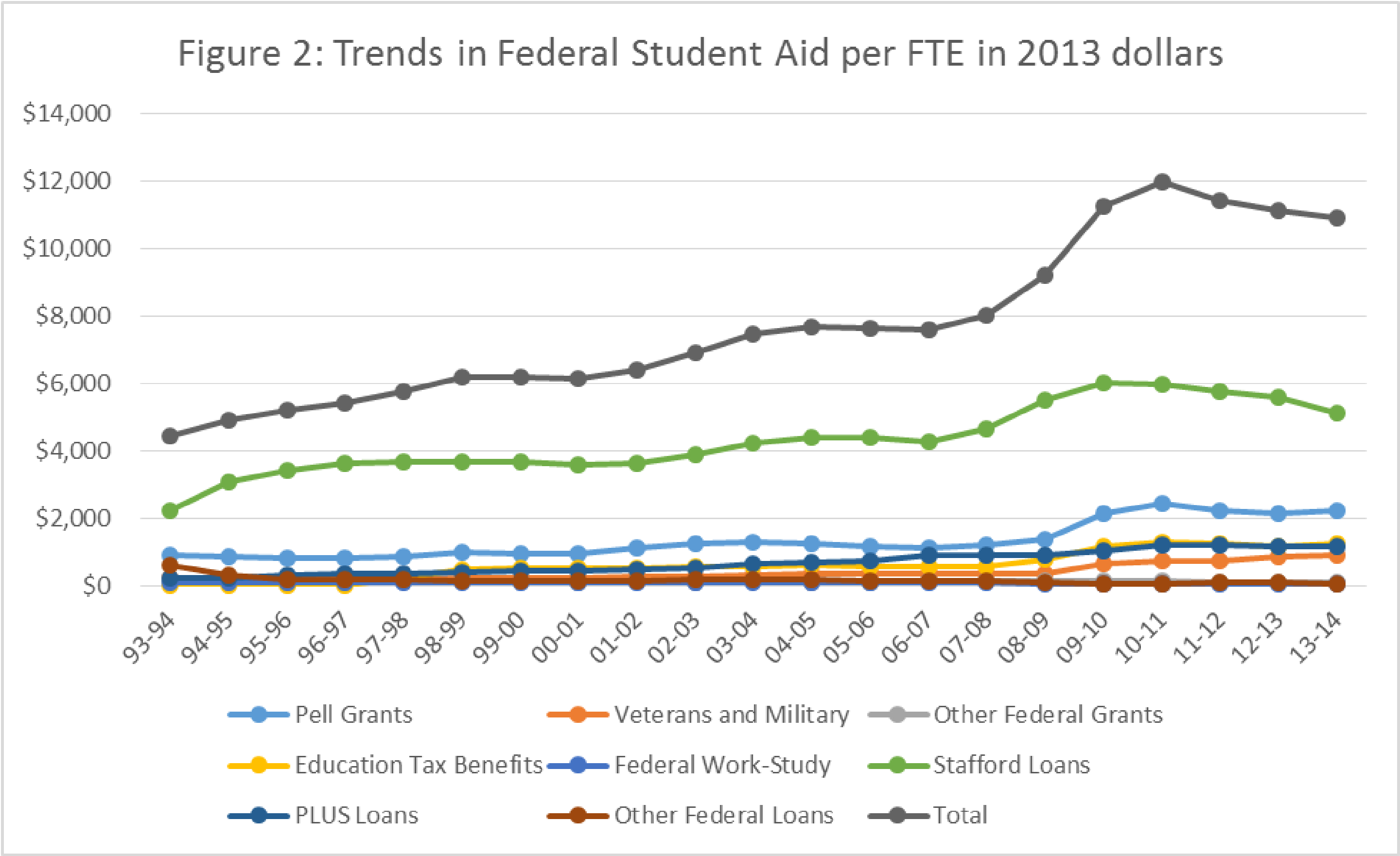


Figure 1: Mean Tuition and Fees for Private and Public Four Year Colleges/Universities (2014 dollars)

Source: College Board, Table 2: http://trends.collegeboard.org/college-pricing/figures-tables/tuition-fees-room-board-time-1974-75-2014-15-selected-years. Retrieved March 23, 2015.



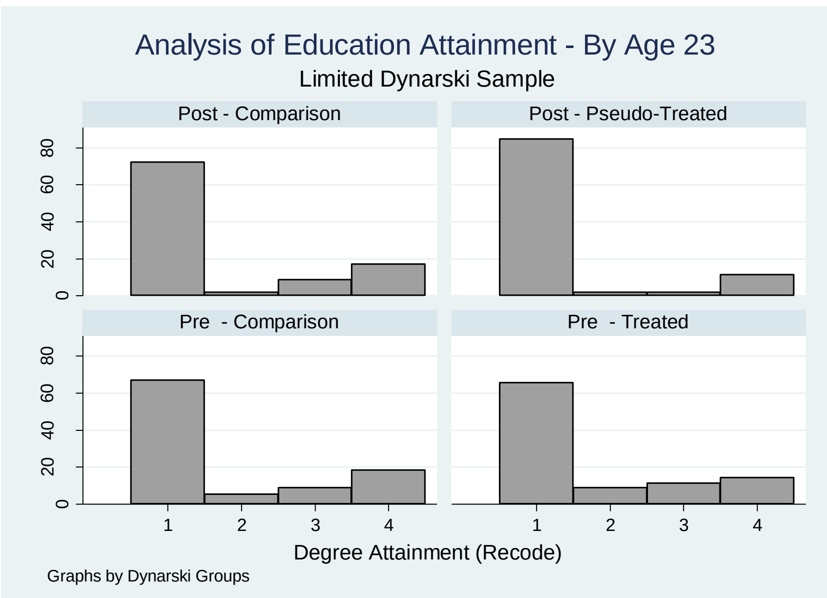
Source: College Board, Table 6. http://trends.collegeboard.org/student-aid/figures-tables/federal-aid. Accessed March 23, 2015.

Figure 3

Degree Attainment by Age 23 - NLSY79 Observations

		Limited Dynarski Sample					Expanded NLSY Sample				
Recode	**Degree Attainment by 23**	**Pre - Treated**	**Post Pseudo-Treated**	**Pre - Comparison**	**Post - Comparison**	**Total**	**Pre - Treated**	**Post Pseudo-Treated**	**Pre - Comparison**	**Post - Comparison**	**Total**
1	High School Diploma (Or Equivalent)	88	45	1,795	722	**2,650**	206	50	3,775	780	**4,811**
2	Other Credentials	12	1	146	18	**177**	34	1	572	19	**626**
3	Associate/Junior College	15	1	239	88	**343**	41	1	529	94	**665**
4	Bachelor's Plus	19	6	494	172	**691**	58	7	1,229	175	**1,469**
	Total	**134**	**53**	**2,674**	**1,000**	**3,861**	**339**	**59**	**6,105**	**1,068**	**7,571**

		Limited Dynarski Sample					Expanded NLSY Sample				
Recode	**Degree Attainment by 23**	**Pre - Treated**	**Post Pseudo-Treated**	**Pre - Comparison**	**Post - Comparison**	**Total**	**Pre - Treated**	**Post Pseudo-Treated**	**Pre - Comparison**	**Post - Comparison**	**Total**
1	High School Diploma (Or Equivalent)	66%	85%	67%	72%	**2,650**	61%	85%	62%	73%	**4,811**
2	Other Credentials	9%	2%	5%	2%	**177**	10%	2%	9%	2%	**626**
3	Associate/Junior College	11%	2%	9%	9%	**343**	12%	2%	9%	9%	**665**
4	Bachelor's Plus	14%	11%	18%	17%	**691**	17%	12%	20%	16%	**1,469**
	Total	**134**	**53**	**2,674**	**1,000**	**3,861**	**339**	**59**	**6,105**	**1,068**	**7,571**



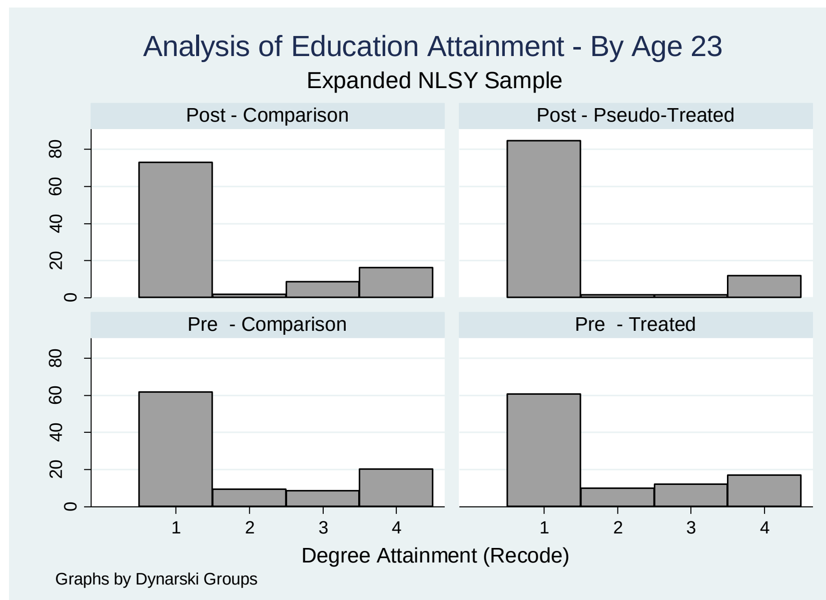
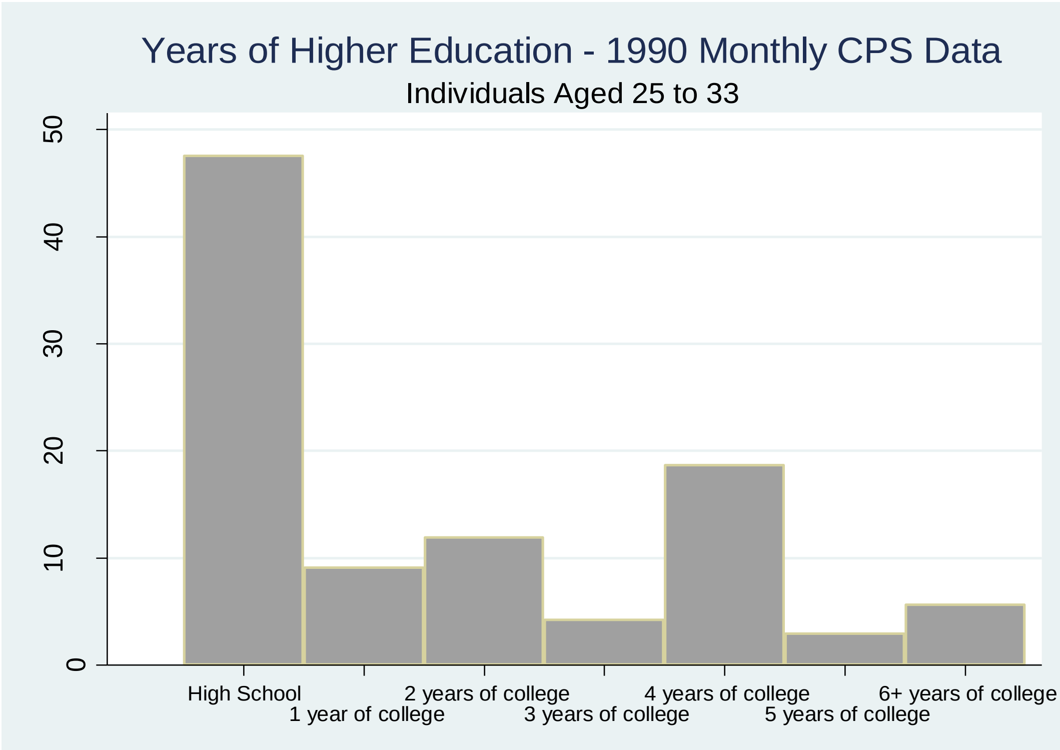
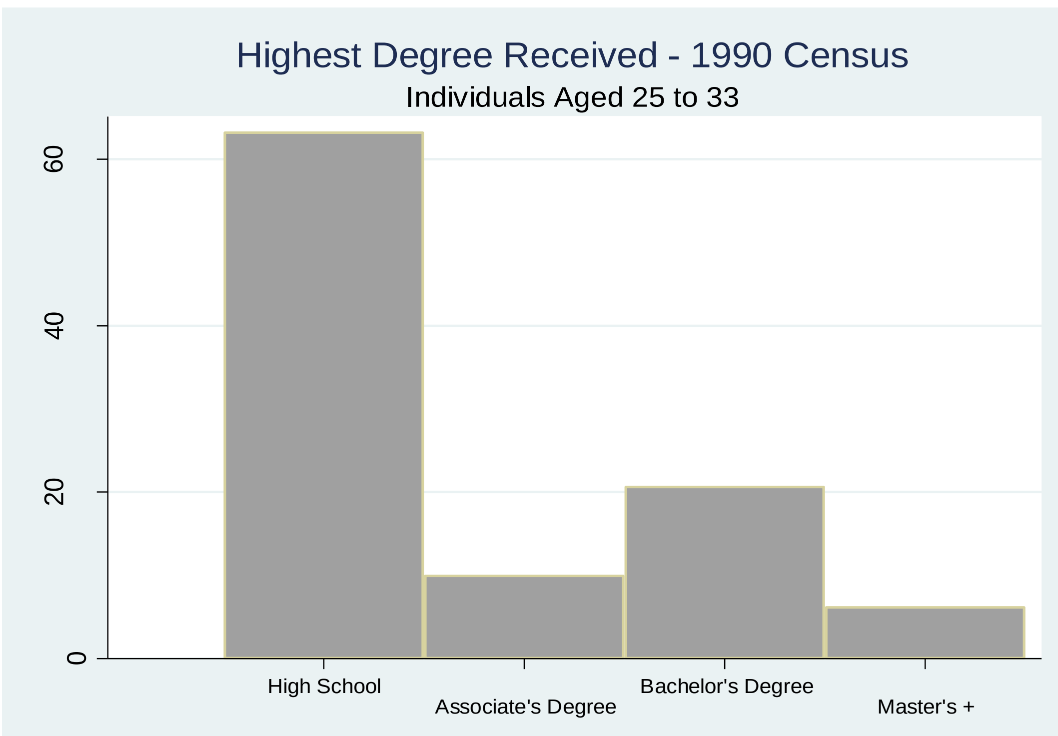


Figure 4





Sources: Authors calculations using Census and PSID micro sample data from IPUMS.

Figure 5

AFQT Score Distribution by Treatment Group

Table 1

Limited Dynarski NLSY Sample - Regression Data Set

	High School Seniors: 1979-1981		High School Seniors: 1982-1983			
Variable	**Father not Deceased**	**Father Deceased**	**Father not Deceased**	**Father Deceased**	**Difference-in-Differences**	**t-stat**
Mean Family Income 79-82 (2010$)	$61,401	$37,546	$60,652	$38,739	-$1,942	-0.36
AFQT Percentile (Age Adjusted)	56.30	55.63	52.74	41.09	10.98	2.08
Black	0.133	0.228	0.150	0.299	-0.054	-0.79
Hispanic	0.050	0.053	0.060	0.055	0.008	0.34
Two-Parent Household	0.874	0.400	0.850	0.307	0.069	0.74
Mother Attended College	0.239	0.123	0.209	0.167	-0.074	-0.87
Father Attended College	0.332	0.186	0.307	0.159	0.002	0.03
Number of Siblings in School	1.72	1.84	1.89	2.15	-0.15	-0.51
Age in 1988	25.95	25.90	23.93	23.94	-0.07	-0.51
Female	0.485	0.490	0.476	0.482	-0.001	-0.01
Attended College by 23	0.504	0.565	0.492	0.354	0.199	2.06
Completed Any College by 23	0.490	0.567	0.475	0.363	0.189	1.93
Years of Schooling at 23	13.43	13.47	13.33	12.91	0.46	1.53
Highest Grade Completed by 28	13.77	13.69	13.74	13.08	0.59	1.75
Max Education Ever Reported	14.20	14.28	14.11	13.23	0.97	2.62
Degree Attainment - Age 23						
High School Diploma (Or Equivalent)	0.628	0.586	0.656	0.809	-0.195	-2.20
Other Credentials	0.050	0.094	0.023	0.008	0.058	1.82
Associate/Junior College	0.090	0.108	0.091	0.007	0.101	2.86
Bachelor's Plus	0.232	0.213	0.230	0.175	0.036	0.44
N	**2,674**	**134**	**1,000**	**53**	**3,861**	

Notes: Analysis is weighted by the 1988 NLSY respondent weights. Attrition is not explicitly accounted for: the last reported value is assigned to all long-term outcomes.

Table 2

Expanded NLSY Sample - Regression Data Set

	High School Seniors: 1981 & Before		High School Seniors: 1982-1983			
Variable	**Father not Deceased**	**Father Deceased**	**Father not Deceased**	**Father Deceased**	**Difference-in-Differences**	**t-stat**
Mean Family Income 79-82 (2010$)	$55,928	$37,446	$60,291	$37,094	$4,714	1.12
AFQT Percentile (Age Adjusted)	56.23	55.68	52.58	42.28	9.74	1.94
Black	0.125	0.160	0.150	0.254	-0.069	-1.26
Hispanic	0.048	0.057	0.061	0.045	0.025	1.17
Two-Parent Household	0.883	0.427	0.842	0.319	0.067	0.79
Mother Attended College	0.246	0.189	0.212	0.138	0.017	0.25
Father Attended College	0.321	0.206	0.305	0.131	0.060	0.85
Number of Siblings in School	1.59	1.40	1.89	1.86	-0.16	-0.66
Age in 1988	27.86	28.14	23.95	23.93	0.29	1.87
Female	0.502	0.486	0.472	0.487	-0.030	-0.36
Attended College by 23	0.394	0.403	0.489	0.300	0.198	2.45
Completed Any College by 23	0.490	0.550	0.466	0.307	0.219	2.66
Years of Schooling at 23	13.40	13.43	13.29	12.78	0.54	2.15
Highest Grade Completed by 28	13.74	13.71	13.71	12.92	0.75	2.64
Max Education Ever Reported	14.19	14.20	14.10	13.13	0.98	3.24
Degree Attainment - Age 23						
High School Diploma (Or Equivalent)	0.590	0.580	0.665	0.835	-0.180	-2.60
Other Credentials	0.099	0.113	0.022	0.007	0.029	1.22
Associate/Junior College	0.086	0.115	0.091	0.006	0.113	4.53
Bachelor's Plus	0.225	0.193	0.223	0.152	0.038	0.58
N	**6,105**	**339**	**1,068**	**59**	**7,571**	

Notes: Analysis is weighted by the 1988 NLSY respondent weights. Attrition is not explicitly accounted for: the last reported value is assigned to all long-term outcomes.

Table 3
SSSBP Impacts on Completed Years of Education - NLSY79 Data

By Age 23	**Dynarski AER**	**Replication Sample**	**Limited Dynarski Sample**	**Expanded NLSY Sample**
Deceased Father X Before	0.564	0.327	0.3520	0.4954*
	[0.379]	[0.3583]	[0.3471]	[0.2933]
Observations	**3986**	**3987**	**3861**	**7571**

By Age 28	**Dynarski AER**	**Replication Sample**	**Limited Dynarski Sample**	**Expanded NLSY Sample**
Deceased Father X Before	0.727*	0.4030	0.4240	0.6276*
	[0.397]	[0.3727]	[0.3655]	[0.3356]
Observations	**3986**	**3987**	**3861**	**7571**

By Age 35	**Replication Sample**	**Limited Dynarski Sample**	**Expanded NLSY Sample**
Deceased Father X Before	0.7154*	0.7271*	0.8467**
	[0.4106]	[0.4002]	[0.3486]
Observations	**3987**	**3861**	**7571**

By Age 40	**Replication Sample**	**Limited Dynarski Sample**	**Expanded NLSY Sample**
Deceased Father X Before	0.8796**	0.8986**	0.9273**
	[0.4264]	[0.4201]	[0.3609]
Observations	**3987**	**3861**	**7571**

Note: Please see Appendix A for a thorough discussion of the controls utilized in this analysis.

Table 4

Exact Degree Attainment by Select Age Thresholds

Expanded NLSY Data

	Prob(Diploma = High School)				
	by Age 23	*by Age 30*	*by Age 35*	*by Age 40*	*Max Age*
Deceased Father X Before	-0.2191**	-0.2999***	-0.2584**	-0.2541**	-0.2618**
	[0.0962]	[0.1008]	[0.1024]	[0.1042]	[0.1037]

	Prob(Diploma = Other Credentials)				
	by Age 23	*by Age 30*	*by Age 35*	*by Age 40*	*Max Age*
Deceased Father X Before	0.1011**	0.0990*	0.0963	0.1076*	0.0937
	[0.0455]	[0.0591]	[0.0586]	[0.0579]	[0.0588]

	Prob(Diploma = Associates)				
	by Age 23	*by Age 30*	*by Age 35*	*by Age 40*	*Max Age*
Deceased Father X Before	0.0764*	0.0855*	0.0098	-0.0005	-0.0190
	[0.0452]	[0.0498]	[0.0745]	[0.0772]	[0.0770]

	Prob(Diploma = BA+)				
	by Age 23	*by Age 30*	*by Age 35*	*by Age 40*	*Max Age*
Deceased Father X Before	0.0415	0.1154	0.1523*	0.1471*	0.1871**
	[0.0797]	[0.0835]	[0.0864]	[0.0869]	[0.0884]

Notes: All regressions are limited to respondents with at least a high school level of education. "Other credentials" includes qualifications such as certificates, licenses, or journeyman's cards (see https://www.nlsinfo.org/content/cohorts/nlsy79/other-documentation/codebook-supplement/nlsy79-attachment-7-other-certificate for the full list of NLSY79 other certificate codes). The individual-level controls used in the above modeling were female, black, and Hispanic indicators, age-adjusted AFQT score, respondent Age in 1988, and their Census Region in 1979. Family controls include an indicator for those residing in a two-parent household at age 14, the number of siblings still enrolled in school at baseline, and the number of older siblings. Parental controls are the mean family income from 1979 to 1982 and dummy variables for whether the mother or father completed college. Indicator variables are also used to identify all imputed values. Standard errors are clustered at the family-level and are in brackets with statistical significance indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

Table 5

Transition Matrix - From Other Credential or Associate's Degree to Higher Level Degrees

Degree at Age 23 = Other Credential

		Degree at 30			Degree at 35			Degree at 40			Maximum Age Ever		
Treatment Group	**N @ 23**	**Other**	**Associates**	**Bachelors Plus**	**Other**	**Associates**	**Bachelors Plus**	**Other**	**Associates**	**Bachelors Plus**	**Other**	**Associates**	**Bachelors Plus**
Pre - Treated	34	94%	6%	0%	91%	9%	0%	88%	12%	0%	88%	12%	0%
Post - "Pseudo-Treated"	1	100%	0%	0%	100%	0%	0%	100%	0%	0%	100%	0%	0%
Pre - Comparison	572	86%	6%	8%	82%	9%	10%	80%	9%	12%	80%	9%	12%
Post - Comparison	19	95%	5%	0%	89%	5%	5%	89%	5%	5%	89%	5%	5%
	626												

Degree at Age 23 = Associates

		Degree at 30			Degree at 35			Degree at 40			Maximum Age Ever		
Treatment Group	**N @ 23**	**Other**	**Associates**	**Bachelors Plus**	**Other**	**Associates**	**Bachelors Plus**	**Other**	**Associates**	**Bachelors Plus**	**Other**	**Associates**	**Bachelors Plus**
Pre - Treated	41		83%	17%		76%	24%		76%	24%		63%	37%
Post - "Pseudo-Treated"	1		100%	0%		100%	0%		100%	0%		100%	0%
Pre - Comparison	529		87%	13%		84%	16%		82%	18%		79%	21%
Post - Comparison	94		85%	15%		79%	21%		78%	22%		76%	24%
	665												

Table 6
Expanded NLSY Sample - Regression Data Set
Exclude AFQT < 10th Percentile

	High School Seniors: 1981 & Before		High School Seniors: 1982-1983			
Covariate	**Father not Deceased**	**Father Deceased**	**Father not Deceased**	**Father Deceased**	**Difference-in-Differences**	**t-stat**
Mean Family Income 79-82 (2010$)	$56,855	$37,814	$62,163	$39,000	$4,121	0.83
AFQT Percentile (Age Adjusted)	58.67	58.05	56.35	51.95	3.79	0.79
Black	0.105	0.143	0.125	0.191	-0.029	-0.55
Hispanic	0.046	0.055	0.057	0.039	0.026	1.16
Two-Parent Household	0.889	0.425	0.850	0.357	0.029	0.30
Mother Attended College	0.254	0.191	0.226	0.174	-0.012	-0.14
Father Attended College	0.333	0.216	0.325	0.165	0.044	0.53
Number of Siblings in School	1.57	1.39	1.85	1.89	-0.23	-0.81
Age in 1988	27.85	28.12	23.92	23.91	0.28	1.67
Female	0.503	0.493	0.484	0.548	-0.074	-0.78
Attended College by 23	0.407	0.414	0.512	0.343	0.177	1.86
Completed Any College by 23	0.506	0.564	0.494	0.362	0.190	1.97
Years of Schooling at 23	13.46	13.49	13.39	12.94	0.47	1.59
Highest Grade Completed by 28	13.82	13.77	13.82	13.12	0.66	1.98
Max Education Ever Reported	14.27	14.28	14.23	13.38	0.86	2.50
Degree Attainment - Age 23						
High School Diploma (Or Equivalent)	0.577	0.575	0.648	0.800	-0.154	-1.87
Other Credentials	0.100	0.103	0.020	0.009	0.016	0.64
Associate/Junior College	0.089	0.120	0.092	0.008	0.116	4.34
Bachelor's Plus	0.235	0.202	0.240	0.184	0.023	0.29
N	**5635**	**311**	**943**	**44**	**6933**	

Notes: Analysis is weighted by the 1988 NLSY respondent weights. Attrition is not explicitly accounted for: the last reported value is assigned to all long-term outcomes.

Table 7

Exact Degree Attainment by Select Age Thresholds

Expanded NLSY Data - Exclude AFQT < 10th Percentile

	Prob(Diploma = High School)				
	by Age 23	***by Age 30***	***by Age 35***	***by Age 40***	***Max Age***
Deceased Father X Before	-0.1895*	-0.2701**	-0.2213*	-0.2176*	-0.2528**
	[0.1062]	[0.1131]	[0.1157]	[0.1180]	[0.1166]

	Prob(Diploma = Other Credentials)				
	by Age 23	***by Age 30***	***by Age 35***	***by Age 40***	***Max Age***
Deceased Father X Before	0.0737	0.0573	0.0522	0.0629	0.0608
	[0.0476]	[0.0648]	[0.0643]	[0.0635]	[0.0637]

	Prob(Diploma = Associates)				
	by Age 23	***by Age 30***	***by Age 35***	***by Age 40***	***Max Age***
Deceased Father X Before	0.0919*	0.0981*	0.0140	0.0081	-0.0011
	[0.0503]	[0.0540]	[0.0789]	[0.0812]	[0.0813]

	Prob(Diploma = BA+)				
	by Age 23	***by Age 30***	***by Age 35***	***by Age 40***	***Max Age***
Deceased Father X Before	0.0239	0.1147	0.1551	0.1467	0.1930*
	[0.0889]	[0.0937]	[0.0967]	[0.0973]	[0.1008]

Notes: All regressions are limited to respondents with at least a high school level of education. "Other credentials" includes qualifications such as certificates, licenses, or journeyman's cards (see https://www.nlsinfo.org/content/cohorts/nlsy79/other-documentation/codebook-supplement/nlsy79-attachment-7-other-certificate for the full list of NLSY79 other certificate codes). Regression controls are the same as those outlined in Table 4. Standard errors are clustered at the family-level and are in brackets with statistical significance indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

Appendix A

NLSY79 Variable Construction

Variable Type	Dynarski's Variable	Definition	Expanded NLSY Variable	Definition
Quasi-Experimental Group Construction	Deceased Father	Childhood family roster information collected in 1988 is used to identify when the sample child stopped living with their father before age 18. A one for this indicator variable signals that the NLSY child has their father passed before their 18th birthday.	Same as Dynarski	
	Before	This dummy variable identifies all students eligible to enroll in college before May 1982. It is based upon grade enrollment information.		
Individual Controls	AFQT Score	The original AFQT scores were not age adjusted to account for the fact that younger children were less developed in their abilities than older respondents. Dynarski uses a series of regression models to age-adjust these values.	Age Adjust AFQT Score (2006)	The NLSY now provides an age adjusted AFQT score.
	Black	Race of the child at baseline (1979).	Same as Dynarski	
	Hispanic	Ethnic group of the child at baseline.		
	Age in 1988	This variable allows for cohort-specific trends.		
	State Dummies	These variables identify the location of the respondent in the first NLSY survey.	Currently we use Census Region Dummies.	With access to the restricted data, we will incorporate the state-level indicators. For respondents finishing their degrees before 1979, we must assume that the current state is also the state in which they went to high school.
Family Controls	Senior-Year Family Income / 10,000 ($2000)	This variable measures the family income of the respondent during their senior year of high school. Values are normalized to 2000 dollars.	Average Family Income from 1979 to 1982 / 10,000 ($2010)	Rather than using income from a single year, we smooth family income over four years from 1979 to 1982. Since we lack childhood family income for respondents completing high school before 1979 and no longer living in their childhood household, we implicitly invoke the assumption that the family income from 1979 to 1982 is roughly the same as the income generated by the child's family while they were in high school. This should be a reasonable assumption for the low-income children analyzed in the sample,
	Father Attended College	Based upon the educational attainment of the respondent's father in 1979.	Same as Dynarski	
	Mother Attended College	Based upon the educational attainment of the respondent's mother in 1979.		
	Single-Parent Household	Construction unclear in Dynarski's Stata code.	Two-Parent Household at age 14	The baseline 1979 survey gathers family information on all respondents at age 14. Children identified as living with some combination of a mother/father (including step-parents) were coded as living in two-parent households.
Outcomes	Attended College by (age)	This indicator identifies any student which have attended college full-time by a particular age using enrollment information.	Same as Dynarski	
	Years of Schooling at (age)	This continuous variable is based upon reoccurring sample question which capture the highest grade completed.		
			Degree Attainment by (age)	In nearly every wave in the NLSY, information regarding the highest diploma awarded was recorded.

Notes: For variables with missing values, we follow the imputation method employed by Dynarski (2003). First, we assign the average value for the corresponding quasi-experimental group used in the regression sample. Next, we create an indicator to signal the values which have been imputed.

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