# Fostering a Gentler Flight from the Nest:

# Effects of Foster Care Reform on Labor Market Outcomes

Kate Musen*

October 15, 2025

Click Here for Current Version

**Abstract**

In the United States, six percent of children—and nearly 12 percent of Black children—experience a foster care placement before age 18. Youth who spend time in foster care, and particularly those who age out of foster care, attain less education and are less likely to be employed in adulthood than peers from similar socioeconomic backgrounds. In 2012, California introduced extended foster care, allowing eligible youth to remain in care until age 21 instead of exiting at 18. I exploit this reform using a difference-in-differences design comparing affected and unaffected cohorts to estimate the intent-to-treat effects of eligibility. Scaling reduced form effects with an IV-DiD design, I find that each additional year of extended care increases college enrollment by six percent and formal employment at ages 24–26 by 4 percent. These effects are concentrated among the most vulnerable youth, including those with more reports of maltreatment and those without relatives to provide care. Non-Hispanic white men are disproportionately represented among these high-vulnerability groups and experience correspondingly larger gains. Conservative estimates of the marginal value of public funds indicate that each dollar spent on extended foster care generates at least three dollars in benefits, suggesting that well-designed interventions can also yield meaningful returns in late adolescence—a stage of life where government investments are often thought to be less effective.

*khm2128@columbia.edu. I am grateful to Sandra Black, Janet Currie, and Ebonya Washington for their unwavering guidance and advice. I thank Doug Almond, Mark Anderson, Harris Eppsteiner, Itzik Fadlon, Michelle Jiang, Kate Kennedy-Moulton, Wojciech Kopczuk, Hani Mansour, Chris Mills, Suresh Naidu, Dan O'Flaherty, Roman Rivera, Adam Sacarny, Bernard Salanié, Hannah Solheim, Gabriel Swagel, Pietro Tebaldi, Jane Waldfogel, Yuci Zhou and participants at the Transition Age Youth Special Interest Group Annual Meeting, APPAM, the Oslo Economics Workshop on Marginalized Children and Long-term Outcomes, the Atlanta Workshop on Public Policy and Child Well-being, Janet Currie's Festschrift, the WEAI Graduate Student Workshop, and the Columbia and Princeton Graduate Labor Workshops for helpful comments and suggestions on the analysis presented in this manuscript. I thank Mark Courtney, Stephanie Cuccaro-Alamin, Andrea Lane Eastman, Anthony Gómez, Joe Magruder, Glenn Saika, Wendy Wiegmann, and the rest of the CCWIP staff for their feedback, technical support, and encouragement. I also thank the Center for Health and Wellbeing at Princeton University for hosting me as a visiting student during the 2024–2025 academic year. This research was funded by the National Science Foundation (Grant Number DGE2036197) and University of California, Berkeley via the Conrad N. Hilton Foundation. The findings reported herein were performed with the permission of CDSS. The opinions and conclusions expressed herein are solely those of the author and should not be considered as representing the policy of the collaborating department, agency or any department or agency of the California government. All errors are my own.

# 1 Introduction

At some point before their 18th birthday, six percent of children—and nearly 12 percent of Black children—in the United States will be placed in foster care, which provides around-the-clock support for children who have experienced abuse or neglect (Wildeman and Emanuel, 2014). Until the 2010s, foster care often ended abruptly at age 18, with youth who aged out of the foster care system experiencing high rates of poverty, homelessness, and incarceration (Courtney et al., 2007; Courtney and Hughes Heuring, 2005). Over 20,000 children reach the age of 18 while in foster care annually (National Foster Youth Institute). More recently, however, states across the country have introduced extended foster care programs wherein youth who reach the age of 18 while in care have the option to remain in care until age 21. Extended foster care represents a major policy shift toward extending public investment in foster youth into early adulthood, with the goal of improving long-term economic self-sufficiency.

Since at least Becker (1974), economists have acknowledged and understood that parents continue to invest in the human capital development of their children and provide social insurance in times of economic uncertainty well beyond the age of 18. As the real cost of college and the unemployment risk for young adults has risen dramatically over the past two and a half decades (Hipple, 2016), parents have played an increasingly important role in promoting the economic stability and growth of their children in young adulthood. Economists, in turn, have renewed their interest in modeling and estimating the social insurance role of families (Kaplan, 2012; Johnson, 2013; Hotz et al., 2023; Anstreicher and Venator, 2024). Former foster youth, however, often do not have parents who can provide a safe and stable place for them to live and to weather economic shocks as young adults. To address this pervasive challenge, the California Fostering Connections to Success Act (AB 12) extended foster care eligibility up to age 21 in California, potentially providing former foster youth with the insurance and the investment that non-foster youth have been increasingly relying on for decades. Extended foster care also creates a natural experiment to empirically study the value of investments in early adulthood for the most vulnerable adults.

Economics research on foster care has demonstrated that foster care is a heterogeneous bundle of supports whose effects depend on time and place. The effects of foster care placement are highly context dependent, with findings ranging from increased criminal justice involvement and reduced earnings in some settings to improved child safety and schooling outcomes in others (Doyle 2007; Doyle 2008; Baron and Gross, 2022; Baron and Gross, 2025). However, very little research has examined the causal effects of reforms that change the intensity or duration of care, especially during the transition to adulthood. Using both survey and administrative data, descriptive evidence from the social work literature suggests that reforms targeted at youth transitioning out of foster care may lead to improved well-being up through age 23. The California Youth Transitions to Adulthood Study, for example, documents better outcomes for youth staying in care beyond the age of 18, but the authors do not account for unobservable differences between foster youth who choose to stay in foster care and those who decide to live independently.[1] A challenge in creating effective policy to promote the welfare of vulnerable children is the need to identify and analyze empirical evidence that is robust to selection effects.

In this paper, I provide the first causal estimates of the long-term impact of extended foster care on the labor market outcomes of youth transitioning out of care. A large literature in economics has demonstrated that early childhood environments are critical determinants of human capital accumulation and later economic outcomes (e.g., Almond and Currie, 2011). Far less attention has been paid to adolescence, despite evidence from neuroscience that this life stage—extending into the early twenties—constitutes a second "critical period" of brain development (Giedd et al., 1999). As Almond, Currie, and Duque (2018) emphasize, this "missing middle" between early childhood and adulthood remains poorly understood. The relatively small literature on programs for young adults generally finds low value of programs targeting this age group. This study expands the evidence base beyond tuition assistance and job training programs that are the focus of this more limited literature. Extended foster care provides a bundle of broader support that more closely resembles the resources that parents often provide their children during the transition to adulthood, including housing,

[1] Also see Okpych and Courtney (2019) and Courtney and Hook (2017) for evaluations of data from the Midwest Evaluation of the Adult Functioning of Former Foster Youth (Midwest Study). Prettyman (2023) also uses national data from the National Youth in Transition Database (NYTD) to look at the relationship between extended foster care exposure and outcomes at age 21, but data limitations in the NYTD require a relatively strong section on observables assumption for causal interpretation.

money for necessary expenses, and the guidance of older adults in making educational and vocational decisions. Extended foster care also requires youth to be participating in activities to build human capital to ease the transition when extended care ends at age 21.

To evaluate the effects of extended foster care eligibility on labor market outcomes, I use detailed administrative data that include economic outcomes for foster youth before and after the implementation of extended foster care under AB 12, which provided funding for foster care up to age 21. I estimate a difference-in-differences model in which youth who leave foster care between age 16 and age 17 serve as a reference group for youth who remain in care after 17 and thus are at risk of aging out.[2] Although youth who leave care before their 17th birthday differ from those who remain in care with respect to outcomes, empirical evidence indicates that their trajectories would have likely been similar in the absence of AB 12. I also use the effect of extended foster care eligibility on time in foster care to measure take-up of the policy and rescale the intent-to-treat estimates to identify the treatment on the treated effects of one additional year of foster care on downstream outcomes. This identification strategy is complemented with a fuzzy regression kink design with an alternate set of identification assumptions that exploits the initial rollout of extended foster care eligibility. This alternate identification strategy is noisier but does not require assumptions about the stability of youth leaving care before age 17.

Extended foster care has had economically and statistically significant effects on the educational attainment and wages of former foster youth. Each additional year of extended foster care increases the likelihood that youth enroll in college by three percentage points (6% of the mean) and increases earnings between ages 24 and 26 by nearly five percent. These effects are largely driven by improved outcomes for the most vulnerable youth in foster care. Within the context of foster care, non-Hispanic white men are more likely than other demographic groups to have characteristics that indicate high vulnerability, with correspondingly larger treatment effects. These characteristics include coming from homes with a higher number of allegations of child maltreatment and not having the support of kin (relative) foster parents.

[2] At age 16, youth leaving care are fully eligible for Chafee transition services that have provided resources to transition age youth nationally since 1999.

Having documented sizable effects on education and earnings, especially for the most vulnerable youth, I next assess the broader policy significance of these results by evaluating the overall value of extended foster care as a public investment. Even under conservative assumptions, estimates of the marginal value of public funds suggest that each dollar invested in extended foster care yields more than three dollars in benefits. This result suggests that targeted support for young people approaching adulthood can yield economically significant gains, even in a stage of life typically associated with lower program effectiveness.

Extended foster care programs across the country have work and school requirements to stay in care, but California provides a particularly broad definition of activities to meet these requirements. In practice, caseworkers have considerable discretion in judging compliance with these eligibility requirements. This discretion raises a central policy question: How strict should eligibility requirements be to maximize youth outcomes? To answer this question, I examine how variation in enforcement ("leniency") shapes participation and downstream outcomes. Treating caseworker discretion as a policy parameter, both descriptive patterns and estimates from a more formal discrete-choice model suggest that higher leniency keeps more youth in care without reducing schooling or work for those who remain in care. These results suggest that both the generosity of support and the way it is implemented are central to improving outcomes for youth transitioning out of care.

The rest of the paper proceeds as follows: Section 2 discusses the background behind the introduction of extended foster care. Section 3 provides an overview of the data sources used in the analysis. Section 4 presents the empirical strategy. Section 5 presents the treatment effects of extended foster care, both on average and for more vulnerable subgroups of youth in foster care. Section 6 provides estimates of the marginal value of public funds (MVPF) and provides evidence that lenient eligibility requirements for extended foster care participation are welfare maximizing. Section 7 puts extended foster care in a broader policy context and concludes.

## 2 Background on Foster Care Reform

Before the introduction of extended foster care, federal funding for foster care ended when a child turned 18, forcing youth in most parts of the United States to immediately become largely self-sufficient.[3] On October 7, 2008, the Fostering Connections to Success and Increasing Adoptions Act, an Act of Congress with enormous bipartisan support, was signed into law by President George W. Bush, creating major reform to the child welfare system. One of the most important components of this act was the provision of federal funding to states to expand foster care coverage up until age 21 for children pursuing educational or vocational goals. Importantly, the Act incentivized states to create extended foster care programs but did not require them to do so. Today, 47 of 50 states now offer extended foster care in some form, although the length and requirements for extended foster care eligibility vary substantially by state (Juvenile Law Center, 2025).

California was an early adopter of this new source of funding with its Fostering Connections to Success Act (AB 12), which took effect on January 1, 2012. Extended foster care was phased in gradually. Foster care eligibility up to age 19 was effective starting on January 1, 2012. Eligibility expanded up to age 20 on January 1, 2013, and up to age 21 on January 1, 2014. Youth born in 1993 were deemed the "bubble children," as they were eligible for extended care at the beginning of each calendar year but would lose federal funding on their birthday. Counties supplemented federal funding to keep many of the bubble children in care, but access was not complete until the 1994 birth cohort came of age.

In order to be eligible for extended foster care in California, youth have to be engaged in activities aimed specifically at improving educational or vocational outcomes if able to do so. In order to meet the criterion of working toward educational goals, youth can be enrolled in high school, a high school equivalency program, college, or technical school, at least part time. In order to meet the criterion of working toward vocational goals, youth can be enrolled in a job training program or work at least 80 hours per month. Youth with a medical condition precluding school or work can also remain in care under AB 12. Extended foster care is designed to empower transition age youth—youth in foster care in late adolescence—to choose the support they need as they enter adulthood. Extended foster care beyond the age of 18 is voluntary for youth; they may choose to leave and re-enter care at any point before age 21 so long as they meet eligibility requirements. These non-

[3]California allowed some youth to stay in care beyond the age of 18 (but not beyond age 19) before the extension of federal funding, but this determination was made on a case-by-case basis and long extensions, in practice, were relatively rare prior to 2012.

minor dependents in extended care have several placement options available to them, ranging from more traditional foster homes to supervised independent living placements. Figure A1 shows the distribution of placement types by age for transition age youth prior to AB 12. Youth in foster care after age 16 are primarily living in congregate care, in foster family homes, or kin care arrangements wherein extended family members serve as foster parents. Figure A2 shows that shifting distribution after age 18 for transition age youth after the implementation of AB 12. Many youth in extended foster care stay in more traditional foster family homes, but many move to supervised independent living programs (SILPs), which provide more independence to youth while also continuing the structure of youth contact with caseworkers.

Youth transitioning out of foster care have had historically low educational attainment and poor labor market outcomes. Among youth in California in care after their 16th birthday born between 1991 and 1995, only three percent had a four-year college degree (Table 1A) and 38 percent had no formal employment between the ages of 24 and 26 (Table 1B). These statistics illustrate the economic vulnerability of this population.

# 3 Data

I use detailed administrative data on foster children from the California Department of Social Services Child Welfare Services/Case Management System (CWS/CMS) linked to quarterly wage and college enrollment data that allow me to observe economic outcomes for foster youth prior to and after the reform was enacted. These data include records for all children who interact with the foster care system starting in 1998, with data linkages available for all youth who spend at least a week in care after their 16th birthday. The California Department of Social Services (CDSS) data include every encounter a child has with child protective services (CPS), as well as detailed demographic data for each child.[4] These demographic data allow for the examination of heterogeneous effects by race, gender, and disability status. Quarterly wage data come from the Unemployment Insurance files from the Employment Development Department. College enrollment data come from the National Student Clearinghouse.

[4]See the Appendix A for more information on the foster care administrative data used.

The primary analysis restricts to the sample of youth born between 1991 and 1995 in order to observe wage outcomes at age 26. This restriction provides me with two cohorts of youth too old to be eligible for extended foster care under AB 12, one transition cohort born in 1993, and two cohorts of youth fully eligible for extended foster care.[5] Youth in Los Angeles County are omitted because Los Angeles had its own extended foster care policies in place before AB 12. Youth who did not spend at least 8 days in foster care after their 16th birthday are also omitted to ensure linkages with the National Student Clearinghouse data. The National Student Clearinghouse data are linked to the child welfare data through name and birthday, but the quarterly wage data are linked via Social Security number (SSN). For this reason, the three percent of youth who are missing an SSN or who have an invalid SSN are dropped from wage analyses. The implication of this restriction is that the wage analyses will not include most undocumented former foster youth, as these youth are most likely not to have an SSN.

These restrictions and linkages yield a well-defined sample that spans the period before and after the policy change. In these data, clear shifts emerge in foster care participation and early adult outcomes as extended foster care was introduced. Figure 1 shows the raw mean exit age from foster care, any employment at ages 22 through 24, and any college enrollment. Table 1 presents outcomes through age 26. Descriptively, the roll out of extended foster care and subsequent improved outcomes are visible in the data. Discrete changes in the slope of the average age of exit from foster care and in downstream outcomes are visible with the phase in of extended care with the 1993 birth cohort and with the full implementation of extended care in the 1994 birth cohort. These descriptive patterns provide preliminary evidence that AB 12 provides a successful natural experiment to estimate the effects of extended foster care reform.

[5]In supplementary analysis, youth born in 1996 are included to expand the sample and consider outcomes at age 25.

# 4 Empirical Strategy

## 4.1 Estimating Effects of Extended Foster Care Eligibility

Estimating the causal effects of extended foster care poses two main challenges. First, extended foster care is optional — youth who choose to stay in extended foster care are likely different from youth who choose to leave care at age 18. These youth may differ in personal characteristics such as motivation, but also in unobserved external factors such as the availability of a place and people to stay with after leaving care. Second, the characteristics of youth in foster care change over time. Transition-aged youth (i.e., youth in foster care after age 16) born too early for extended foster care may be different than youth born in later years.

AB 12 serves as a natural experiment that allows me to estimate the causal effects of extended foster care, accounting for selection and birth cohort effects. Notably, only youth born after after 1993 and who remained in care as they were approaching age 18 were affected by the introduction of extended foster care. Youth born before 1993 would turn 18 before extended care began (pre-policy) and youth born after 1993 are eligible (post-policy). Youth who left care after their 16th birthday but before their 17th birthday were not directly affected because they had already left care before they reached the age where the policy would apply (control group). Youth in care after their 17th birthday are at risk of aging out (treated group). The mandate that social workers face to prioritize minor dependents of the state achieving "permanency" (i.e, exiting foster care if possible) did not change after AB 12, so youth who had the opportunity to leave care before age 17 likely continued to do so even with the implementation of extended care. This cutoff of the 17th birthday is arbitrary, but is chosen to fall well before 17 years and 5 months, at which point caseworkers actively prepare youth for the transition to legal adulthood.

This context motivates the following difference-in-differences design to estimate the causal effect of extended foster care eligibility on outcomes:

$$Y_{ict} = \alpha + \beta \mathbb{1}(\text{Exit} > 17)_i + \gamma_t + \delta \mathbb{1}(\text{Exit} > 17)_i \times (\text{AB 12})_t + \mu_c + \omega X_i + \nu_{ict}. \qquad (1)$$

Here, $Y$ represents outcomes such as college attendance and earnings, $i$ indexes individuals, $c$ indexes counties, and $t$ indexes birth cohorts defined by quarter of birth. The variable $(\text{AB } 12)_t$ is the fraction of an individual's life between ages of 18 and 21 with guaranteed extended care. $\mathbb{1}(\text{Exit} > 17)_i$ is an indicator for whether the youth left care after age 17, $\gamma_t$ is a vector of quarter of birth dummies, and $\mu_c$ is a vector of county fixed effects, and $X_i$ is a vector of demographic controls. The coefficient $\delta$ is the estimated intent-to-treat effect of extended foster care eligibility on youth who are still in care on their 17th birthday and thus at risk of aging out.

#### 4.1.1 Identifying Assumptions

These estimates rely on three primary identifying assumptions. First, although youth leaving care before and after age 17 differ from each other, in absence of the policy change, the outcomes of the two groups are assumed to have trended similarly. The event study in Figure 2 suggests that this assumption is reasonable; in this event study the AB 12 eligibility variable is replaced with a full set of quarter-of-birth indicators with exit age from foster care as the outcome, still conditioning on quarter-of-birth indicators and the indicator for whether the youth left care after 17. Birth cohorts before 1993 were ineligible for guaranteed extended foster care, birth cohorts in 1993 were partially eligible, and birth cohorts after 1993 were fully eligible. Figure 2 reflects this policy variation. The coefficients on quarters of birth prior to 1993 are close to zero, consistent with the idea that, prior to the policy change, the groups were trending similarly. There is also no evidence of pre-trends in the other outcomes in Figure 4a and Figure 4b.

The second assumption is that individuals did not anticipate the policy change and respond prior to its implementation. Figure 3a provides evidence that this assumption is reasonable: For the 1992 birth cohort, age 18 appears to still be a binding exit age for most youth, with no youth leaving at or after age 19. Further, at the time that AB 12 was passed in September 2010, it was unclear whether or not youth would have access to extended care after age 19, so social workers and youth did not have full knowledge of the policy at the time that earlier birth cohorts were approaching 18.

The third assumption is that the relative composition of youth leaving before and after age 17 is stable across time, and that changes in time in care for youth treated by AB 12 do not affect the time in care for untreated youth (no spillovers). This assumption is tested by estimating the difference-in-differences model with observable demographic characteristics on the left hand side to see if the demographic characteristics of youth are changing.

The included characteristics are indicators for whether the individual is white, Black, Hispanic, and female. Figure A3 shows the corresponding event studies with these demographic variables as the outcome. These figures reassuringly show no evidence of any demographic shifts for youth around the age 17 threshold in response to extended foster care.[6]

As an additional test, I consider a larger sample of youth who exit care after age 15, and estimate the effect of AB 12 on the probability that youth in care after age 16 will stay after age 17.[7] This placebo test is specified as follows:

$$\mathbb{1}(\text{Exit} > 17)_{ict} = \alpha + \beta \mathbb{1}(\text{Exit} > 16)_i + \gamma_t + \delta \mathbb{1}(\text{Exit} > 16)_i \times (\text{AB } 12)_t + \mu_c + \omega X_i + \nu_{ict}. \quad (2)$$

In this test, $\hat{\delta} = .00008$, with a 95% confidence interval of [-.01, .01], essentially a precise zero. This result suggests that extended foster care did not increase the likelihood that youth stay in care after their 17 birthday and thus move into the treated group. Figure A5 shows the corresponding event study.

Finally, there is no evidence of changes in the density of youth exiting around the age 17 threshold before and after the implementation of AB 12 (see Figure 3b). This assumption also weakened in additional robustness checks in Section 5.3, where the sample is limited to youth who would have turned 17 before foster youth were fully informed about extended foster care policy. Lack of knowledge about extended care at age 17 limits the possibility that changes in youth's motivation to stay in care related to the policy change affected their likelihood of exiting care.

## 4.2 Estimating the Treatment on the Treated

Since participation in extended care is optional and many youth exit care between their 17th and 18th birthdays, it is also valuable to estimate the treatment on the treated of

[6] If one is concerned that the Great Recession affected the composition of removals, I further alleviate concerns with county-level placebo tests of county characteristics related to the effect of the Great Recession in Figure A4.

[7] Educational outcomes are only available for youth in care after age 16, limiting which youth are included in the primary estimates.

additional time in extended foster care on outcomes. In the reduced form estimates, an indicator for youth still being in foster care at age 17 is used as a proxy for risk of aging out of foster care at age 18—but many youth exit foster care between their 17th and 18th birthdays for reasons unrelated to extended foster care availability and may not actually be treated by the policy change.

Treatment on the treated of additional time in extended foster care on outcomes is estimated using as complementary IV-DiD design. These models use the increase in time spent in care attributable to the implementation of AB 12 (i.e., the difference-in-difference estimator with exit age as the outcome) as an instrument for exit age to estimate the effect of one additional year of extended care on labor market outcomes. These estimates are analogous to those generated by RCTs with imperfect compliance in which assignment to treatment is used to instrument actual treatment to estimate average treatment effects for compliers.

The baseline model for the first stage is Equation (1) with exit age from foster care as the dependent variable.

The second stage equation is as follows:

$$\text{Outcome}_{ict} = \lambda + \xi \mathbb{1}(\text{Exit} > 17)_i + \eta_t + \tau \widehat{\text{Exit Age}}_{ict} + \kappa_c + \theta X_i + \epsilon_{ict}. \tag{3}$$

Here, $\mathbb{1}(\text{Exit} > 17)_i$ is an indicator for whether the youth left care after age 17, $\eta_t$ is a vector of quarter of birth dummies, $\widehat{\text{Exit Age}}_{ict}$ is the predicted value of the increase in time in foster care induced by AB 12 estimated in the first stage, $\kappa_c$ is a vector of county fixed effects, and $X_i$ a vector of demographic controls.

The difference-in-differences estimator, which represents the estimated effect of AB 12 on exit age from foster care, is strongly related to the actual exit age observed. Table 2 shows this first stage for all youth and for subgroups of youth by race and gender. The effective F-statistic as described in Montiel Olea and Pflueger (2013) is 4,279 for youth overall, and over 420 for all subgroups analyzed, which is well above even the largest thresholds for a sufficiently strong instrument as proposed in the literature (Montiel Olea and Pflueger, 2013).

In order for this policy instrument to be valid, the exclusion restriction must also be satisfied. This restriction requires that the timing of the implementation of extended foster care is unrelated to other changes in policy or the social and economic environment that would differentially affect youth in care after age 17. This part of the assumption is not directly testable, but there are several institutional details that support this assumption.

The federal law that provides Title IV-E funds to states that choose to implement extended foster care was finalized the same day Lehman Brothers filed for bankruptcy. As discussed in the Chapin Hall Report on the implementation of AB 12, "if the committees had waited a few more weeks, passage of [the federal Fostering Connections to Success and Increasing Adoptions Act] would have been unthinkable" (Mosley and Courtney, 2012). Thus, the federal law that enabled the passage of AB 12 was constructed when policymakers could not have possibly anticipated the economic and social conditions that foster youth would experience when the law came into effect. Child welfare advocates in California had been advocating for extended foster care for years before the 2008 federal legislation, but extended care in California became financially feasible only with the support of federal funding. According to interviews with key parties in California government, AB 12 was first introduced almost immediately following the federal bill, and the fact that AB 12 did not come into effect until January 1, 2012 reflects the considerable time that was required to craft and implement the final legislation (Mosley and Courtney, 2012). Part of the delay between AB 12 being introduced in December 2008 and finally signed in September 2010 also reflects the time it took the incoming Obama administration to clarify aspects of the federal Fostering Connections to Success and Increasing Adoptions Act—rather than any desire to delay extended care due to economic conditions in 2009 and 2010 (Mosley and Courtney, 2012).

# 5 Results

## 5.1 Exit Age from Foster Care

I first evaluate the effect that AB 12 had on take-up of extended foster care. Table 2 provides results of estimating Equation (1) with exit age as the dependent variable, providing estimates of the effect of AB 12 on the exit age of youth previously at risk of aging out of foster care at age 18. Across all youth, AB 12 increased the exit age of youth still in care after age 17 by 1.4 years. Figure 3a suggests a bimodal distribution of exit ages at age 18 and age 21, however, indicating that another interpretation of these findings is that AB 12, which extended foster care eligibility by three years to age 21, was used by about half of eligible youth. The effect of AB 12 on time in foster care is substantial in all groups, although there is some variation in point estimates by race and gender. Estimates of the first stage by race and gender are shown in Columns 2 through 5 of Table 2A and for more detailed subgroups in Table 2B. Point estimates range from an increase in exit age from foster care of 1.2 years for non-Hispanic white women to 1.65 years for Black women. Taken together, these estimates indicate that AB 12 successfully increased time in foster care for youth previously at risk of aging out of care at age 18.

## 5.2 Treatment Effects on College and Earnings

Having established that AB 12 increased time in care for youth at risk of aging out, I evaluate the effects of the policy on downstream education and labor market outcomes. Table 3 shows the estimates of Equation (1) with college and earnings outcomes on the left hand side. Extended foster care eligibility increases the likelihood that youth enroll in college by 4.3 percentage points (Column 1). Estimates scaled by the take-up of extended foster care are presented in Table A2: Each additional year of care increases the likelihood that youth enroll in college by three percentage points, or six percent of a mean enrollment rate of 50 percent. This increase in college enrollment does not lead to an increase in college graduation, however, with null effects on two-year and four-year college graduation rates shown in Columns 2 and 3 of Table 3 and Table A2.

Despite the null effects on college graduation, extended foster care improves labor market outcomes at ages 24 through 26. Extended foster care eligibility increases the likelihood that youth are formally employed by 3.6 percentage points (Table 3, Column 4). Again scaling by the average uptake of extended foster care of 1.4 years, this implies that each additional year of care increases likelihood that youth are formally employed by 2.5 percentage points, or 4 percent of a mean employment rate of 62 percent. Including both employed and unemployed youth, each additional year of care increases the inverse hyperbolic sine of wages between ages 24 and 26 by 0.304 (which is 4.6 percent of the mean). These findings indicate meaningful improvements in the labor market outcomes for youth in extended foster care.

### 5.3 Robustness and Alternative Specifications

I conduct additional robustness checks to address two potential sources of concern: how treatment exposure is defined and whether the composition of youth remaining in care changed around the policy's implementation. By varying the measure of time in care, restricting to earlier cohorts, and exploiting the phased roll-out of extended care by birth date, I confirm that the main results are stable across these alternative specifications. The full details of these analyses are presented in Appendix B.

To account for the fact that youth may exit and re-enter foster care between the ages 18 and 21, I first re-estimate the first stage using total time in foster care rather than exit age. This alternative measure of program take up captures both continuous stays and re-entries into care. Although this specification is somewhat noisier, the first-stage relationship remains strong, and the resulting treatment-on-the-treated estimates for college enrollment and earnings are very similar to those in the preferred specification.

To further reduce concerns about endogenous selection into care after age 17, the analysis is repeated in a restricted sample of youth who turned 17 before October 2011, prior to the mass dissemination of information about AB 12 to foster youth. The consistency of results in this restricted sample indicates that foreknowledge of the policy did not materially affect which youth stayed in care past age 17.

Finally, I use a completely different (albeit noisier) estimation strategy that fully relaxes the assumption in the difference-in-difference model that the relative composition of youth leaving care before their 17th birthday does not change in response to extended foster care. More specifically, I estimate a fuzzy regression kink design that leverages the phased roll-out of AB 12 by birth date: eligibility was extended to age 19 beginning January 1, 2012, then to age 20 in 2013, and to age 21 in 2014. Youth born earlier in 1993 therefore had shorter guaranteed eligibility windows than those born later in the year, generating a piecewise-linear relationship between birth date and expected time in care. This design identifies sharp changes in the slope of exit age with respect to birth date at each policy threshold. The share of the 18–21 age window for which a youth was eligible for state and federal funding under AB 12 serves as a continuous measure of treatment intensity and there is a strong first-stage evidence of these slope changes. The second-stage estimates produce slightly larger but statistically comparable effects on college enrollment and earnings, reinforcing the causal interpretation of the difference-in-differences results.

Taken together, these robustness exercises provide evidence that the main findings are not sensitive to the way treatment exposure is measured or the possibility of selection into the treatment group.

### 5.4 Heterogeneous Treatment Effects and Youth Vulnerability

Foster youth constitute a vulnerable population, but some youth in care may be more vulnerable than others. In the absence of extended foster care, more vulnerable youth with fewer options upon leaving foster care may have worse counterfactuals, suggesting potential heterogeneous treatment effects of extended care.

With respect to the options youth have when aging out of foster care, there are two important considerations. First, will a youth's foster parent allow them to stay in the home without foster care payments? Youth who are in kin care are more likely to be able to continue living with their family. This implies that youth who have never been in kin care are likely to be more vulnerable at age 18. Youth who have never been in kin care may also lack family members who are capable or, in some cases, willing to care for youth when they enter foster care, and this inability to provide housing and tangible support may persist during the critical period of ages 18 to 21. In this case, extended foster care can provide alternative support systems.

The second important consideration for youth aging out of care is what the home environment be like if they return to the family from which they were originally removed. Historically, many youth have returned to their home of origin after emancipation. The number of allegations of maltreatment against a child can serve as a proxy for the underlying quality of the home environment from which youth were removed. Youth with a high number of allegations of maltreatment are therefore also more likely to be more vulnerable at age 18.

Thus, heterogeneity in treatment effects is captured by two key indicators of youth vulnerability: (1) a high number of allegations of maltreatment and (2) the absence of kin care providers. For youth in the birth cohorts 1991 and 1992 who were born too early for extended foster care, both of these characteristics are associated with worse outcomes. Youth with above the median number of allegations were 4 percentage points less likely to ever be formally employed between ages 24 and 26. Youth who had never been in kin care were 5 percentage points less likely to ever be formally employed between ages 24 and 26. These worse outcomes also point to a greater need to support these youth after age 18.

These vulnerability measures are not positively correlated. Instead, youth who have never been in kin care (and thus are less likely to continue staying with foster caregivers in absence of extended care) are actually less likely to have been removed from homes with indicators of underlying lower quality. In this sample, youth who have never been in kin care are 7 percentage points less likely to have above the median number of allegations of maltreatment.[8] The youth who are likely to have worse counterfactuals because they are more likely to need to relocate at age 18 are *different youth* than those with worse counterfactuals because they are less likely to be able to return to the home from which they were removed.

#### 5.4.1 Treatment Effects for More Vulnerable Youth

The two indicators of vulnerability—an extensive maltreatment history and the absence of kin caregivers—reflect different counterfactuals youth face upon aging out, and extended foster care operates through distinct channels for each, generating divergent treatment effects. Table 4 shows the effects of extended foster care on college outcomes by each of these three risk factors. Youth with above the median number of allegations of maltreatment are much more likely to attend college in response to extended foster care, but two- and four-year college graduation rates are unchanged. By contrast, youth who have never been in kin care are only marginally more likely to enroll in college, but these youth see precisely estimated increases in 4-year college graduation rates, which may reflect better sorting into college, better support by college educated adults, or both.

Differences in college enrollment and graduation effects for youth with above median allegations of maltreatment and youth who have never been in kin care translate into different patterns of labor force participation with age. Figure 5 shows the trajectory of employment

[8]This negative association between risk factors is driven in part by youth who have been in kin care having more allegations of maltreatment raised by family members.

effects for youth with above median allegations of maltreatment. This figure shows that treated youth experience lower employment rates at ages 18 and less precisely at 19—consistent with higher college enrollment but limited graduation effects, which would entail even more delayed labor force entry. By age 20, their employment levels converge to those of untreated peers, who likely accumulated more early work experience, but employment effects remain statistically indistinguishable from zero throughout. Figure 6 shows the trajectory of employment effects for youth who have never been in kin care. Here we see that youth who were never in kin care do not experience negative employment effects at ages 18 and 19, which is consistent with the smaller treatment effects on college entry. Starting at age 22, which is the age that most individuals start graduating 4-year college, youth who were never in kin care have positive and steady employment effects at each age measured. These estimates provide evidence that more vulnerable youth benefit more from extended foster care, but the benefits they incur are likely mediated by the types of support they receive while in extended foster care.

More specifically, youth who have never been in kin care may have greater benefits of extended foster care because extended care provides these youth with greater exposure to college-educated adults than youth who stay in extended care with kin foster parents. Youth in kin care are generally more likely to go to college, but since most kin foster parents have not been to college themselves, young people in kin care may have less access to adults who have gone to college to guide them than youth in other types of care.[9] Even compared to youth in Supervised Independent Living Programs (SILPs), which confer the greatest degree of independence in extended foster care, youth in kin care are less likely to have professional support from caseworkers, teachers, school counselors, and therapists (Okpych et al., 2018). Foster youth tend to believe they are more prepared for college than their caseworkers do (Torres-García et al., 2019), and caseworkers may have more weight in young people's college decisions in the absence of competing expectations and desires from kinship care providers.

Thus, when youth in extended foster care decide between educational and vocational

[9] Okpych and Courtney (2017) find that the probability that youth in foster care go to college increases as youth have more adults outsides of their biological families, or institutional agents—who are more likely to have a college degree themselves—who can provide tangible support and guidance. When controlling for the number of institutional agents that youth have access to, youth in kin care are much more likely to enroll in college.

activities to meet the eligibility requirements for extended care, youth who have never been in kin care may be more likely to turn to college-educated adults to decide if and where to attend college. This support may enable youth to better sort into education and vocational activities according to college preparedness. For youth not in kin care who attend college, access to college-educated adults may also support these youth in graduating college.[10]

By contrast, other vulnerable youth in extended care may not gain this same access to college-educated adults, especially since youth with a high number of allegations are more likely to have been in kin care. These youth may have less support to assess college preparedness before enrolling in college. Youth who are unprepared for college or who lack the support of college-educated adults to persist in college may be less likely to graduate. These students do gain some college experience, but they also lose labor market experience at ages 18 and 19, and fail to catch up to their peers by age 26.

An alternate explanation for these findings is that the presence or absence of kin care alone explains the heterogeneous treatment effects. This alternate hypothesis can be tested by looking at the outcomes of youth who have both been in kin care and have below median allegations of maltreatment in Table 5. These relatively less vulnerable youth experience no significant effect of extended foster care on educational or labor market outcomes. It is only the relatively more vulnerable youth (with respect to number of allegations of maltreatment) who have been in kin care who increase their college attendance. These findings indicate that heterogeneous treatment effects cannot be explained solely by kin care status; instead, the absence of kin care appears to be a key mediator of the effects of extended foster care among more vulnerable youth.

The heterogeneous treatment effects of extended foster care by placement type also shed light on the mechanisms behind the increase in earnings estimated on the full sample. In aggregate, extended foster care increased college enrollment and earnings at age 26 but did not significantly increase college completion. One might assume that these findings indicate that either a) taking classes at college campuses improved earnings profiles for youth who did

[10]There is also some evidence that youth who have never been in kin care are better able to avoid for-profit colleges. Six percent of youth in the sample attend a for-profit two- or four-year institution at some point before age 26, but youth who have never been in kin care are 12.5 percent less likely to do so than youth who have been in kin care.

not graduate or b) that increased educational attainment is not related to the mechanisms behind increased earnings—but this is an ecological fallacy. Youth with a high number of allegations of maltreatment drive effects on college enrollment, but these youth do not experience increases in earnings. Youth who have never been in kin care drive the aggregate increases in earnings for youth in extended care, and these youth do experience increases in college graduation. Youth who have never been in kin care are also only marginally more likely to enroll in college with extended care. These results suggest that extended foster care improves labor market outcomes through channels beyond increasing collage enrollment and that many youth who are induced to enroll in college do not complete a two- or four-year degree.

#### 5.4.2 Treatment Effects for Youth with Neuropsychological Disabilities

Beyond what options youth have when aging out of foster care, youth with neuropsychological disabilities (e.g., ADHD, PTSD, depression) may encounter more challenges than their peers without such conditions as they transition to adulthood.[11] These youth may need more support from social workers beyond age 18. Leaving foster care may also mark a greater loss of support for these youth, implying a potentially worse counterfactual. Youth with neuropsychological disabilities also have worse baseline outcomes: They are 7 percentage points less likely to ever be formally employed between ages 24 and 26.

Similar to youth with above the median number of allegations of maltreatment, youth with neuropsychological disabilities experience large treatment effects of extended foster care on college attendance in response to extended foster care, with no detectable effect on two- and four-year college graduation rates (Table A20). Figure A12 shows the trajectory of employment effects for youth with neuropsychological conditions. Treated youth with neuropsychological disabilities work less right after high school, catch up a year or two later, and never surpass their peers—suggesting delayed, not expanded, entry into the labor market.

[11]Neuropsychological conditions are recorded by caseworkers and may not be fully representative of a youth's health status. Conditions include ADHD, autism, mood disorders, intellectual disabilities, anxiety, emotional distress, addiction, antisocial personality disorder, eating disorders, psychotic disorders, conditions requiring psychiatric hospitalization or psychiatric medication, and other behavioral and learning disabilities not otherwise specified. Two important reasons for a broad approach to categorizing neuropsychological conditions are 1) the 2013 update to the Diagnostic and Statistical Manual of Mental Disorders and 2) changes in specificity over time in how caseworkers record conditions.

Youth with neuropsychological conditions face unique challenges after age 18 beyond those related to their counterfactual housing options, but in practice these youth are more likely to come from homes with indicators of underlying lower quality. Youth with neuropsychological conditions are 20 percentage points more likely to have above the median number of allegations. The large degree of colinearity between these risk factors preclude further disentanglement of underlying mechanisms.

## 5.5 Heterogeneous Treatment Effects by Race and Gender

### 5.5.1 Non-Hispanic White Men in Foster Care are Particularly Vulnerable

Non-Hispanic white men have characteristics that indicate that they are, on average, more vulnerable when aging out of foster care than other demographic groups. Non-Hispanic white men in the sample have more referrals to CPS, are more likely to have never received kin care, and have a higher likelihood of being diagnosed with a neuropsychological disability than other youth in foster care. Non-Hispanic white men also have lower mean college enrollment rates, lower 2- and 4-year college graduation rates, lower labor force participation rates, and lower earnings between 24 and 26 than other youth aging out of foster care at baseline (see Table A3 for details.)[12]

In the context of youth aging out of foster care, white youth, and especially white men, are likely to be relatively negatively selected on factors related to human capital accumulation and labor market productivity. The reason for this negative selection is likely two-fold. First, there is increasing evidence that racial disparities in CPS investigation and subsequent placement into foster care are created in part by white children who are at risk of further maltreatment being under-placed into foster care after a referral to child protective services (Baron et al., 2024a; Baron et al., 2024b; Grimon and Mills, 2024; Rittenhouse et al., 2024). A consequence of this disparity is that the average white child in foster care will have likely experienced more maltreatment than the average Black child in foster care, creating the first source of negative selection of white children. Second, Black teenagers (and even more so

[12] More specifically, Non-Hispanic white men are 3.4 percentage points (6.8 percent) more likely to have above median referrals, 8.5 percentage points (15.6 percent) more likely to have never been in kin care, and 13.6 percentage points (27.5 percent) more likely to have been diagnosed with a neuropsychological disability. For youth born before 1993, controlling for these factors reduces the gap in college attendance and labor force participation at ages 24 through 26 between Non-Hispanic white men and other foster youth by 13.7 and 19.7 percent, respectively.

Black teenage boys) are more likely to be diverted to the juvenile justice system by the age of 16 (Ryan et al., 2007). Thus, Black boys who are in foster care at age 16 may be relatively positively selected on factors that also allowed them to avoid the juvenile justice system.

Given the relative vulnerability of non-Hispanic white men in foster care and their implied worse counterfactuals in the absence of extended foster care, this group of youth would be expected to benefit relatively more from the policy change. This is indeed what I find: Despite the relatively small sample of non-Hispanic white men, non-Hispanic white men have the largest point estimates of the treatment effects of extended foster care across all outcomes measured and are often the only demographic subgroup with statistically significant effects.

### 5.5.2 Educational Outcomes

Table 6 shows the effect of extended foster care eligibility on the probability that youth enroll in either 2-year or 4-year college by race and gender. Only non-Hispanic white men have a statistically significant effect of extended foster care eligibility on college attendance. The point estimate of the effect for non-Hispanic white women is nearly as large but very noisily estimated. The point estimates for Black, Asian, Hispanic, and other race men and women are both much smaller in magnitude. There is no statistically significant effect of extended foster care on 2-year college for any of the subgroups tested, but Table 7 shows a positive effect of extended foster care eligibility on four-year college graduation for non-Hispanic white men which is undetectable in aggregate estimates.

### 5.5.3 Employment Outcomes

Table 8, Panel A shows the effect of extended foster care eligibility on the probability that youth are formally employed at any point between the ages of 24 and 26 by race and gender. Similar to the effects of extended foster care on educational outcomes, the treatment effects are the largest and most precisely estimated for non-Hispanic white men, who increase their formal employment by nearly 10 percentage points. No other subgroup has a statistically significant effect of extended foster care eligibility on employment.

Table 8, Panel B shows the effect of extended foster care eligibility on the inverse hyperbolic sine of wages between ages 24 and 26, and a similar pattern emerges, though now a marginally significant effect on the wages of non-Hispanic white women appears.

#### 5.5.4 Treatment Effects for More Vulnerable Youth by Race and Gender

The hypothesis that underlying vulnerability can explain the larger treatment effects that non-Hispanic white men experience from extended foster care can be tested by comparing treatment effects across demographic groups, restricting to youth who have the risk factors that in aggregate predict the largest treatment effects on each educational outcome. Given the evidence that vulnerable youth with above median allegations drive the college attendance effects in aggregate, I estimate the effect of extended foster care eligibility by race and by gender on college attendance, restricting to youth with above median allegations (Table 9, Panel A). This restriction essentially eliminates the treatment effect gap between non-Hispanic white men and men of color, and the treatment effect on women of color is less precise but also large. The treatment effect on non-Hispanic white women is a very noisy zero, suggesting other factors related to the intersection of race and gender may be important. As youth who have never been in kin care drive the aggregate effects on four-year college graduation rates, estimates from the analogous exercise are presented in Table 9, Panel B, restricting to youth who have never been in kin care. Here we yet again see a narrowing of the gap in treatment effects on four-year college graduation rates for non-Hispanic white men and men of color. The effect of extended care on four-year college graduation rates for non-Hispanic white women is also larger among those who have never been in kin care, but there is no change to the estimated treatment effect for women of color.

Since restricting to sub-samples of youth with these risk factors particularly reduces racial disparities in treatment effects on educational outcomes for men, the trajectories of employment effects for non-Hispanic white men and for men of color are estimated separately by whether or not they have ever been in kin care (Figure 7 and Figure 8). Here we see that non-Hispanic white men and men of color follow very similar trajectories with respect to employment effects when conditioning on this risk factor. Non-Hispanic white men have greater baseline risk factors than Black men in my sample, but Non-Hispanic white men and Black men with similar risk factors experience much more similar treatment effects of extended care.

The heterogeneous treatment effects of extended foster care highlight unresolved questions while also offering important policy insights. Taken at face value, the larger treatment effects for non-Hispanic white men could be interpreted as a failure of extended care to support youth of color. The convergence of treatment effects between non-Hispanic white men and men of color when restricting to more vulnerable youth, however, challenges this assumption by providing evidence that some of the heterogeneous treatment effects by race may be driven by differential underlying vulnerability. This finding reinforces the importance of studying racial gaps in foster care entry, as under-removal of vulnerable non-Hispanic white boys on the margin limits these youth access to support in young adulthood via extended care.

As shown in Table 9, the differences across groups with respect to allegations of maltreatment and kin care do very little to explain gender gaps. The lack of convergence of treatment effects between young men and young women with similar backgrounds indicates that other factors are also at play. In particular, the interaction between pregnancy and parenting and extended foster care policy is likely important for many young women. In recent years, California has developed additional programming for parents in extended foster care, which is likely to be particularly valuable to young women. The types of placements available to youth have also expanded, which may affect selection into extended foster care. The expansion of transitional housing programs may be particularly important, as these programs are designed to serve relatively disadvantaged youth within the foster care system. Future work should evaluate which supports most effectively improve outcomes for pregnant and parenting youth in extended care.

# 6 Evaluating and Strengthening Foster Care Policy

## 6.1 Cost Benefit Analysis

Extended foster care clearly has had large benefits for affected youth but is an expensive program for both the state and federal governments. One way to evaluate the value of extended foster care is through the marginal value of public funds (MVPF) framework popularized by Hendren and Sprung-Keyser (2020). The MVPF is defined as the ratio of the beneficiaries' willingness to pay for a policy per dollar of net government cost inclusive of fiscal externalities. Intuitively, the MVPF provides what Hendren and Sprung-Keyser (2020) call the "bang for buck" of a government dollar, accounting for the possible savings to the government that come from increased tax revenue or reductions in spending on other programs. The MVPF is defined for the marginal dollar of public funds spent on a policy change; depending on the policy, this can in theory affect both inframarginal and marginal participants. In this context, the MVPF is defined for the marginal beneficiary who is able to participate in the program through additional spending. For concreteness in the context of extended foster care, which can be provided for up to three years, the marginal value of public funds is defined for the average level of participation in extended foster care by the marginal participant. Conditional on youth being in foster care after age 18 for fully treated cohorts, the average youth is in care until age 20.2. Thus, both government expenditures and benefits are considered for approximately two years of participation. Extended foster care likely had benefits beyond those I can measure in this paper, as well as value to program participants that is difficult to quantify. Consequently, the MVPF calculations that follow constitute a lower bound on the true value of the program.

#### 6.1.1 Government Expenditures

The exact cost of providing extended foster care for a participant is not available, but an approximation can be computed from gross expenditures. For the fiscal year 2014–2015, California counties reported $130,135,000 in non-federal expenditures on care after age 18 (CDSS, 2016). For that same year, the California Department of Social Service reported that approximately 65% of their caseloads were eligible for federal matching (CDSS, 2017). The federal match rate of 50% implies that total direct expenditures on extended foster care in the fiscal year 2014–2015 was $172,428,875. Youth in extended foster care may move in and out of care, but point-in-time estimates of youth in extended foster care can be used to approximate the total number of participant full year equivalents were in care during that time. Approximately 7,100 youth were in extended care at any given time during that fiscal year, implying an approximate program cost of $24,075 per youth per year, or $52,966 total for the marginal participant.

I next consider the fiscal externalities of extended foster care from changes in spending on other government programs and changes in tax revenue. In the primary analysis, effects are scaled by one year of care, but to allow for potential nonlinearities in the effects for the average marginal participant who is in care up to age 20.2, I consider the treatment on the treated effects of staying in care beyond age 20.

Extended foster care increased government spending on college for participants in extended foster care who were induced to go to college by the program. The IV treatment effects of staying in care beyond age 20 is a 8.8% increase in the chance of going to college and an increase in time in college of 0.64 terms. Since the vast majority of youth enroll in 2-year public community colleges, increased government spending on college is approximated using costs associated with enrollment in the California Community College System. In 2014, the cost of educating a student for one year at a California Community College was approximately $5640, $1104 of which was paid by student fees and tuition (CCCCO, 2014). Two of the most common ways that youth in extended foster care finance their education is Pell Grants and Educational Training Vouchers (ETVs) for youth who have been in the foster care system. ETVs are nearly always in higher demand than supply, so increased spending on extended foster care only changes who receives ETVs instead of government spending on ETVs. For youth who enrolled in a full year of community college, the value of a Pell Grant in 2014 was $1,150 (Federal Pell Grant Program, 2013. Summing these additional costs, the marginal youth in extended foster care generated approximately $1484 in government spending on education.

Extended foster care also generated increased income for youth, increasing tax revenue and decreasing spending on other social safety net programs. Staying in foster care until at least age 20 increased earnings at age 26 by $2558. Using the income project methods in Hendren and Sprung-Keyser (2020), this increase in earning translates into a predicted increase in lifetime earnings of $170,115, or $100,278 discounted back to age 20 at a 3% discount rate. I also use Hendren and Sprung-Keyser's (2020) projection methods to estimate the marginal tax rate on this income, which is inclusive of reductions in expected government transfers to low-income individuals. This methodology is based on Congressional Budget Estimates from 2015. The effective marginal tax rate is predicted to be 33.8%, which is on par with expectations for low-income individuals. Using this effective tax rate, the expected increase in tax revenue from the marginal participant in extended foster care is $27,559.

### 6.1.2 Beneficiaries Willingness to Pay

One of the least expensive types of extended foster care is Supervised Independent Living Programs (SILPs), which involves payments directly to youth, so these payments serve as the basis of a conservative estimate of the direct value of extended foster care to youth. In 2015, the basic rate paid to youth in SILPs was $859 per month (CDSS, 2015). Youth in SILPs were also eligible to receive a clothing allowance, which averaged approximately $178 per year (CDSS, 2010). Parenting youth were eligible for an infant supplement of $411 a month. Approximately 12% of youth in extended foster care lived with their own children (Courtney et al., 2016). These sources of transfers sum to $24,371 for the marginal participant in extended foster care.

Net of Pell Grants, youth also spend $321 on college that they would not have attended without extended foster care. This spending is subtracted from the total value of direct transfers from extended foster care. Youth also earn more over their lifetime. Assuming a 3% discount rate and subtracting off taxes, the value of these increased earnings is $53,976.

### 6.1.3 MVPF

The net cost to the government of additional participation in extended foster care, which includes direct program costs, education costs, and tax revenue is $26,890. The value of extended foster care to the marginal participant, which includes transfers while in care, expenditures on college, and income effects, is $78,026. Thus, the MVPF $= \frac{\text{WTP}}{\text{Net Costs}} = \frac{78026}{26890} = 2.9$

This estimate is likely an underestimate of the true MVPF because it does not account for any effect of extended foster care on criminal justice involvement or health, both of which are likely to be improved based on survey evidence. Even so, an MVPF of 2.9 is relatively high for young adult beneficiaries. For comparison, the job training program targeting young adults Year Up has an MVPF of 0.43, and the similarly targeted program Job Start has an MVPF of 0.20 (Hendren and Sprung-Keyser, 2020). The Wisconsin Scholar Grant to Low-Income Students to assist with college tuition has an MVPF of 1.43 (Hendren and Sprung-Keyser, 2020).

## 6.2 Work and School Requirements in Extended Foster Care

Extending foster care past age 18 offers critical support during the transition to adulthood, but the design and enforcement of participation requirements determine how effectively the program balances support and accountability. Extended foster care in California is not unconditional: youth may remain in care after age 18 only if they meet certain participation requirements intended to promote progress toward independence. Because these requirements focus on keeping youth engaged in school or work, those who do neither—commonly referred to in the policy literature as "NEETs" (Not in Employment, Education, or Training)—face the highest risk of losing eligibility. Understanding how many youth are NEETs, and how strictly caseworkers enforce these requirements, is essential for interpreting the causal effects of extended care and for designing policy that maximizes its benefits.

California law specifies five pathways for a "nonminor dependent" to remain in care after turning 18:

> *[Non-minor dependents can remain in care] when one or more of the following conditions exist:*
> *(1) The nonminor is completing secondary education or a program leading to an equivalent credential.*
> *(2) The nonminor is enrolled in an institution which provides postsecondary or vocational education.*
> *(3) The nonminor is participating in a program or activity designed to promote, or remove barriers to employment.*
> *(4) The nonminor is employed for at least 80 hours per month.*
> *(5) The nonminor is incapable of doing any of the activities described in subparagraphs (1) to (4), inclusive, due to a medical condition, and that incapability is supported by regularly updated information in the case plan of the nonminor.*

Conditions (3) and (5) create important flexibility for youth who face barriers to school or work but also give caseworkers discretion to determine compliance. This discretion raises a central policy question: how lenient should caseworkers be in applying these requirements to maximize youth outcomes? Overly strict enforcement risks excluding youth who are unable to work or attend school, potentially increasing homelessness and economic hardship. Estimates suggest that 11–38 percent of youth experience homelessness during the transition to adulthood after leaving foster care (Feng et al., 2020). At the same time, excessive leniency might reduce incentives to invest in human capital, potentially leaving youth unprepared for financial self-sufficiency after age 21.

### 6.2.1 Evaluating School and Work Requirements via Caseworker Leniency

In the spirit of the literature that uses quasi-random case worker assignment to study the effects of foster care entry (Doyle 2007; Doyle 2008; Baron and Gross, 2022; Baron and Gross, 2025), I capture caseworker leniency using a leave-one-out measure: the share of other youth supervised by the same caseworker who are NEETs but allowed to remain in care after their 19th birthday.[13] At the county level, the fraction of youth who are NEETs in care after their 19th birthday serves as a proxy for overall county enforcement. Focusing on activities at age 19 avoids conflating leniency with youth still completing high school in their 18th year.

Youth who are induced to leave foster care by less lenient caseworkers have worse outcomes.[14] Outcomes at age 25 are reported in Table A21. However, conditional on staying in care, youth with stricter caseworkers and youth living in stricter counties are less likely to be NEETs and have higher earnings at age 25 than comparable youth with more lenient caseworkers.[15] This pattern suggests that caseworker discretion affects youth outcomes on two dimensions: leniency determines who is able to remain in care at all, while stricter enforcement can increase work and school participation among those who clear the bar to stay. Measuring the relative magnitude of these two offsetting effects is thus critical to understanding the relationship between leniency and youth welfare.

Descriptively, counties with higher NEET shares have lower exit rates from care but the percent of youth who stay in care while working or in school is not meaningfully related to county leniency (see Figure 9a). A similar pattern emerges using caseworker-level leniency

[13]Estimates of schooling are likely undercounted, since adult education program participation is only observable if it takes place in coordination with a community college. Employment is defined as earning enough that they worked at least three months during their 19th year for 80 hours a month at minimum wage.

[14]Since caseworkers have more repeat interactions with youth in extended foster care than in the context of initial maltreatment investigations, the strict exclusion restriction of the more traditional judge leniency literature is unlikely to hold. However, the local average treatment effect of more lenient caseworkers can still be identified if the weaker average exclusion restriction of Frandsen et al. (2023) holds. In my context, this assumption implies that caseworkers can affect outcomes through other channels than their discretion for youth to stay in care but that those affects cannot be systematically correlated with their leniency. Additional estimates in support of this asumption are available upon request.

[15]I use county-level NEET rates as an additional instrument for power in this smaller subsample of youth who remain in care. See the Appendix C for full details of this estimation.

(see Figure 9b). These descriptive patterns provide *suggestive* evidence that caseworker and county enforcement are binding margins for many youth and that relaxing enforcement keeps more youth connected to care without large changes in youth working or in school.

To formalize these findings I model youth choices as a function of caseworker leniency while conditioning on factors that shape the costs and benefits of staying in care.[16] I control for financial resources, measured as both the value of foster-care housing (proxied by the fair-market rent for half of a two-bedroom apartment in the county) and the predicted probability of any earnings at age 25 from the reduced-form model. Among youth still in care after age 19, I use the caseworker- and county-level NEET shares instruments to estimate the earnings penalty associated with remaining in care as a NEET, conditional on remaining in care.

I also include proxies for the disutility of work, measured as the labor-force participation rate of former foster youth born in 1990 in the same county, and the disutility of school, measured as the county-by-cohort average 9th-grade English Language Arts scores for socioeconomically disadvantaged students, which includes foster youth.[17] For computational tractability, all these variables are normalized into z-scores. The model also allows for varying intercepts by region as defined by the California Census, combining the smallest regions.

Regression results in Columns 1–3 Table 10 show that leniency is strongly predictive of staying in care, both for youth engaged in school or work and for NEETs, but the increase in NEET status is small compared with the reduction in early exits.

Because the regressions in Columns 1-3 treat choices as binary outcomes—when in reality there are three mutually exclusive and collectively exhaustive outcomes—I also estimate a multinomial probit model that allows youth to choose among three options: staying in care and working or going to school, staying in care as a NEET, or exiting care before 19. This estimation provides a more robust framework to account for all three options that youth face while relaxing the assumption of independence of irrelevant alternatives required for

[16] Here when I say choice, I mean choice subject to the constraints that youth may face.

[17] According to the California Department of Education, students are considered to be socioeconomically disadvantaged if one of the follow conditions is met: "1. neither of the student's parents has received a high school diploma; 2. the student is eligible for or participating in the Free Meal program or Reduced-Price Meal program; 3. the student is eligible for or participating in the Title I Part C Migrant program; 4. the student was considered Homeless; 5. the student was Foster Program Eligible; 6. the student was Directly Certified; 7. the student was enrolled in a Juvenile Course School; 8. the student is eligible as Tribal Foster Youth."

multinomial logit model. In this model, youth decide between staying in foster care while working or being in school, staying foster care without working or being in school, or leaving foster care before age 19 simultaneously, subject to the restrictions placed on youth by caseworkers. This model is then estimated via maximum likelihood estimation.

The marginal effects of increasing leniency on the choices of youth are presented in Columns 4–6 of Table 10. Increasing caseworker leniency by one standard deviation predicts a decrease in youth exiting care before age 19 by approximately 10 percentage points with no meaningful change in the likelihood that youth begin working or going to school while in care. These results corroborate the descriptive findings and suggest that leniency acts primarily on the retention margin rather than the productivity margin.

# 7 Conclusion

The effects of extended foster care in California provide an important benchmark for the rest of the country as states refine similar programs. By delaying the abrupt transition to independence, extended care gives youth more time to invest in human capital, stabilize housing, and build connections with supportive adults. These additional years of care increase college enrollment and improve employment outcomes on both the extensive and intensive margins, with the largest gains accruing to youth who are most vulnerable at baseline. The heterogeneity in effects suggests that the benefits operate partly through improved housing stability and greater access to college-educated mentors, which may facilitate better educational decisions and smoother entry into the labor market.

Compared to the wealth of literature demonstrating the effectiveness of government programs targeted at young children to improve economic mobility, many fewer programs have been shown to be good investments in vulnerable teenagers and young adults (Hendren and Sprung-Keyser, 2020). Many researchers attribute these weaker results to the expectation that the returns to human-capital investments decline as children age, in part because the brain becomes less plastic over time.[18] But in contrast to the economics literature on young children and on adults, we know much less about what Almond, Currie, and Duque have coined "the missing middle" between early childhood and adulthood.

[18]This idea is sometimes formalized as the "Heckman Curve," which posits that the returns to government investments in human-capital development are highest in early childhood and fall with age (Heckman, 2006).

The effectiveness of extended foster care—which provides supports that differ markedly from traditional job-training programs or tuition assistance—suggests that we should revisit assumptions about the potential payoffs of investing in older youth. Neuroscientific evidence reinforces this possibility: while it was once believed that brain development largely ended around age 20 (Giedd et al., 1999), advances in imaging have pushed that estimate to age 25 and, more recently, to age 30 (van Blooijs et al., 2023). This longer developmental window implies that adolescence and early adulthood may be a more productive period for well-designed interventions than previously thought.

Outcomes for foster youth have improved in California over the past decade but continue to lag significantly behind the general population. These gaps underscore the urgent need to better understand which policies most effectively support youth as they transition to adulthood. The first cohorts affected by extended foster care are only now old enough for meaningful analysis of long-run labor market outcomes, and the evidence presented here demonstrates that carefully structured support during this period can yield substantial gains. Continued investment in rigorous evaluation—tracking cohorts over time, refining program design, and testing complementary interventions—will be essential for translating these gains into sustained improvements in economic mobility for future generations of foster youth.

# References


Douglas Almond, Janet Currie, and Valentina Duque. Childhood circumstances and adult outcomes: Act ii. Journal of Economic Literature, 56(4):1360–1446, December 2018.

Garrett Anstreicher and Joanna Venator. To grandmother's house we go: Informal childcare and female labor mobility. 2024.

E. Jason Baron, Jr. Doyle, Joseph J, Natalia Emanuel, and Peter Hull. Unwarranted racial disparity in U.S. foster care placement. Working Paper 33154, National Bureau of Economic Research, November 2024.

E Jason Baron, Jr Doyle, Joseph J, Natalia Emanuel, Peter Hull, and Joseph Ryan. Discrimination in multiphase systems: Evidence from child protection*. The Quarterly Journal of Economics, 139(3):1611–1664, 02 2024.

E. Jason Baron and Max Gross. Is there a foster care-to-prison pipeline? evidence from quasi-randomly assigned investigators. The Review of Economics and Statistics, pages 1–46, 05 2025.

Jason Baron and Max Gross. Is There a Foster Care-To-Prison Pipeline? Evidence from Quasi-Randomly Assigned Investigators, April 2022.

Gary S. Becker. A theory of social interactions. Journal of Political Economy, 82(6):1063–1093, 1974.

David Card, David S. Lee, Zhuan Pei, and Andrea Weber. Inference on causal effects in a generalized regression kink design. Econometrica, 83(6):2453–2483, 2015.

CCCCO. State of the system report. `https://web.archive.org/web/20190426164750/https://californiacommunitycolleges.cccco.edu/Portals/0/FlipBooks/2014_StateOfSystem/2014_State_of_the_System_FINAL.pdf`, 2014.

CDSS. All county letter (acl) no. 15-58. `https://www.cdss.ca.gov/lettersnotices/entres/getinfo/acl/2015/15-58.pdf`, 2015.

CDSS. Realignment expenditures summary. `https://cdss.ca.gov/cdssweb/entres/pdf/Expenditures/RealignmentExpendatures2016.pdf`, 2016.

CDSS. California department of social services, 2017-18 governor's budget:reference documents. `https://www.cdss.ca.gov/cdssweb/entres/localassistanceest/Jan2017/ReferenceDocuments.pdf`, 2017.

Raj Chetty and Nathaniel Hendren. The Impacts of Neighborhoods on Intergenerational Mobility I: Childhood Exposure Effects*. The Quarterly Journal of Economics, 133(3):1107–1162, August 2018.

Mark Courtney, Amy Dworsky, Gretchen Cusick, Judy Havlicek, Alfred Perez, and Thomas Keller. Midwest Evaluation of the Adult Functioning of Former Foster Youth: outcomes at age 21. School of Social Work Faculty Publications and Presentations, December 2007.

Mark Courtney, Amy Dworsky, and Laura Napolitano. Providing Foster Care for Young Adults: Early Implementation of California's Fostering Connections Act.

Mark Courtney, Nathanael Okpych, and Sunggeun (Ethan) Park. Report from calyouth: Findings on the relationship between extended foster care and youth's outcomes at age 21. 2018.

Mark E. Courtney and Darcy Hughes Heuring. The Transition to Adulthood for Youth "Aging Out" of the Foster Care System. In On your own without a net: The transition to adulthood for vulnerable populations, The John D. and Catherine T. MacArthur Foundation Research Network on Transition to Adulthood, pages 27–67. The University of Chicago Press, Chicago, IL, US, 2005.

Mark E. Courtney and Jennifer L. Hook. The potential educational benefits of extending foster care to young adults: Findings from a natural experiment. Children and Youth Services Review, 72:124–132, 2017. Economic Causes and Consequences of Child Maltreatment.

Joseph J. Doyle Jr. Child Protection and Adult Crime: Using Investigator Assignment to Estimate Causal Effects of Foster Care. Journal of Political Economy, 116(4):746–770, 2008. Publisher: The University of Chicago Press.

Federal Pell Grant Program. Payment schedule for determining full-time scheduled awards in the 2013-2014 award period. `https://fsapartners.ed.gov/sites/default/files/attachments/dpcletters/201314PellGrantPaymentandDisbursementSchedules.pdf`, 2013.

Huiling Feng, Justin S. Harty, Nathanael J. Okpych, and Mark E. Courtney. Predictors of homelessness at age 21. Memo from CalYOUTH, 2020.

Brigham Frandsen, Lars Lefgren, and Emily Leslie. Judging judge fixed effects. American Economic Review, 113(1):253–77, January 2023.

Jay N. Giedd, Jonathan Blumenthal, Neal O. Jeffries, F. X. Castellanos, Hong Liu, Alex Zijdenbos, Tomáš Paus, Alan C. Evans, and Judith L. Rapoport. Brain development during childhood and adolescence: a longitudinal MRI study. Nature Neuroscience, 2(10):861–863, October 1999. Number: 10 Publisher: Nature Publishing Group.

Marie-Pascale Grimon and Chris Mills. The Impact of Algorithmic Tools on Child Protection: Evidence from a Randomized Controlled Trial. 2022.

Max Gross and Jason Baron. Temporary Stays and Persistent Gains: The Causal Effects of Foster Care. American Economic Journal: Applied Economics, 14(2):170–199, April 2022.

James J. Heckman. Skill formation and the economics of investing in disadvantaged children. Science, 312(5782):1900–1902, 2006.

Nathaniel Hendren and Ben Sprung-Keyser. A unified welfare analysis of government policies*. The Quarterly Journal of Economics, 135(3):1209–1318, 03 2020.

Steven F. Hipple. Labor force participation: what has happened since the peak? Monthly Labor Review, U.S. Bureau of Labor Statistics, 2016.

Matthew T. Johnson. Borrowing constraints, college enrollment, and delayed entry. Journal of Labor Economics, 31(4):669–725, 2013.

V. Joseph Hotz, Emily E. Wiemers, Joshua Rasmussen, and Kate Maxwell Koegel. The role of parental wealth and income in financing children's college attendance and its consequences. Journal of Human Resources, 58(6):1850–1880, 2023.

Doyle Jr and Joseph J. Child Protection and Child Outcomes: Measuring the Effects of Foster Care. American Economic Review, 97(5):1583–1610, December 2007.

Greg Kaplan. Moving back home: Insurance against labor market risk. Journal of Political Economy, 120(3):446–512, 2012.

David S. Lee and Thomas Lemieux. Regression discontinuity designs in economics. Journal of Economic Literature, 48(2):281–355, June 2010.

Asjad Naqvi. Stata package "sankey".

Nathanael Okpych and Mark Courtney. The relationship between extended foster care and college outcomes for foster care alumni. Journal of Public Child Welfare, 14:1–23, 04 2019.

Nathanael Okpych, Huiling Feng, Keunhye Park, Adrianna Torres-García, and Mark E. Courtney. Living situations and social support in the era of extended foster care: A view from the u.s. Longitudinal and Life Course Studies, 9, 2018.

Nathanael J. Okpych and Mark E. Courtney. Who goes to college? social capital and other predictors of college enrollment for foster-care youth. Journal of the Society for Social Work and Research, 8(4):563–593, 2017.

José Luis Montiel Olea and Carolin Pflueger. A robust test for weak instruments. Journal of Business  Economic Statistics, 31(3):358–369, 2013.

Alexa Prettyman. Happy 18th birthday, now leave: Estimating the causal effects of extended foster care. 2017.

Katherine Rittenhouse, Emily Putnam-Hornstein, and Rhema Vaithianathan. Algorithms, Humans, and Racial Disparities in Child Protective Services: Evidence from the Allegheny Family Screening Tool. 2024.

Joseph P. Ryan, Denise Herz, Pedro M. Hernandez, and Jane Marie Marshall. Maltreatment and delinquency: Investigating child welfare bias in juvenile justice processing. Children and Youth Services Review, 29(8):1035–1050, 2007.

Adrianna Torres-García, Nathanael Okpych, and Mark Courtney. Memo from CalYOUTH: Youths' and child welfare workers' perceptions of youths' educational preparedness. 2017.

Dorien van Blooijs, Max A. van den Boom, Jaap F. van der Aar, Geertjan M. Huiskamp, Giulio Castegnaro, Matteo Demuru, Willemiek J. E. M. Zweiphenning, Pieter van Eijsden, Kai J. Miller, Frans S. S. Leijten, and Dora Hermes. Developmental trajectory of transmission speed in the human brain. Nature Neuroscience, 26(4):537–541, April 2023.

Christopher Wildeman and Natalia Emanuel. Cumulative Risks of Foster Care Placement by Age 18 for U.S. Children, 2000–2011. PLoS ONE, 9(3):e92785, March 2014.

# 8 Tables

Table 1: Summary Statistics

Panel A: College Summary Statistics

| | All | Born 1991-1992 Exit before 17 | Born 1991-1992 Exit after 17 | Born 1993 Exit before 17 | Born 1993 Exit after 17 | Born 1994-1995 Exit before 17 | Born 1994-1995 Exit after 17 |
|---|---|---|---|---|---|---|---|
| Enroll in College | 0.50 | 0.39 | 0.50 | 0.37 | 0.49 | 0.44 | 0.58 |
| | (0.50) | (0.49) | (0.50) | (0.48) | (0.50) | (0.50) | (0.49) |
| 2-Year Degree | 0.032 | 0.028 | 0.029 | 0.025 | 0.021 | 0.039 | 0.046 |
| | (0.18) | (0.16) | (0.17) | (0.16) | (0.14) | (0.19) | (0.21) |
| 4-Year Degree | 0.033 | 0.023 | 0.027 | 0.017 | 0.027 | 0.036 | 0.050 |
| | (0.18) | (0.15) | (0.16) | (0.13) | (0.16) | (0.19) | (0.22) |
| $N$ | 24618 | 2273 | 9740 | 922 | 3968 | 1322 | 6393 |

Panel B: Earnings at Ages 24–26 Summary Statistics

| | All | Born 1991-1992 Exit before 17 | Born 1991-1992 Exit after 17 | Born 1993 Exit before 17 | Born 1993 Exit after 17 | Born 1994-1995 Exit before 17 | Born 1994-1995 Exit after 17 |
|---|---|---|---|---|---|---|---|
| Any Wages | 0.62 | 0.61 | 0.62 | 0.58 | 0.62 | 0.58 | 0.63 |
| | (0.49) | (0.49) | (0.48) | (0.49) | (0.49) | (0.49) | (0.48) |
| Total Wages | 29258 | 25968 | 26868 | 25562 | 30100 | 30172 | 33920 |
| | (46881.6) | (42913) | (43412) | (44131) | (47749) | (48222) | (52248) |
| IHS Wages | 6.59 | 6.41 | 6.55 | 6.07 | 6.59 | 6.22 | 6.84 |
| | (5.32) | (5.27) | (5.26) | (5.34) | (5.34) | (5.44) | (5.36) |
| $N$ | 24449 | 2208 | 9737 | 915 | 4010 | 1277 | 6302 |

Table 2: Effect of AB 12 Eligibility on Exit Age from Foster Care

Panel A: Broad Race and Ethnic Groups

| VARIABLES | (1)<br>All | (2)<br>NHW Men | (3)<br>NHW Women | (4)<br>BAH Men | (5)<br>BAH Women |
|---|---|---|---|---|---|
| Exit > 17 × AB 12 | 1.418*** | 1.209*** | 1.197*** | 1.418*** | 1.548*** |
| | (0.022) | (0.055) | (0.058) | (0.037) | (0.037) |
| Observations | 24,618 | 3,925 | 3,410 | 8,896 | 8,387 |
| R-squared | 0.547 | 0.526 | 0.541 | 0.545 | 0.564 |
| Effective F-stat | 4279.01 | 477.08 | 424.81 | 1430.35 | 1748.65 |

The first column shows the effect of extended foster care eligibility on exit age from foster care for all youth, the second column shows the estimate for non-Hispanic white men, the third column shows the estimate for non-Hispanic white women, the fourth column shows the estimate for Black, Asian, Hispanic, and other race men, and the fifth column shows the estimate for Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses. The effective F-stat on the DiD estimator, which is effective first-stage F statistic as in Montiel Olea and Pflueger (2013), is also shown since this difference-in-differences model serves as the first stage for IV-DiD estimates shown in robustness checks.

Panel B: Detailed Race and Ethnic Groups

| VARIABLES | (1)<br>Hispanic Men | (2)<br>Hispanic Women | (3)<br>Black Men | (4)<br>Black Women |
|---|---|---|---|---|
| Exit > 17 × AB 12 | 1.342*** | 1.535*** | 1.578*** | 1.656*** |
| | (0.052) | (0.049) | (0.064) | (0.074) |
| Observations | 4,761 | 4,654 | 3,066 | 2,675 |
| R-squared | 0.551 | 0.567 | 0.568 | 0.588 |
| Effective F-stat | 675.86 | 996.32 | 603.1 | 501.64 |

The first column shows the effect of extended foster care eligibility on exit age from foster care for Hispanic men, the second column shows the estimate for Hispanic women, the third column shows the estimate for Black men, and the fourth column shows the estimate for Black women. Robust standard errors in parentheses. The effective F-stat on the DiD estimator, which is effective first-stage F statistic as in Montiel Olea and Pflueger (2013), is also shown since this difference-in-differences model serves as the first stage for IV-DiD estimates shown in robustness checks.

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

Table 3: Aggregate ITT Estimates of AB 12 Eligibility on Outcomes

| VARIABLES | (1) Any College | (2) 2Y Grad | (3) 4Y Grad | (4) Any Earnings 24–26 | (5) IHS Earnings 24–26 |
|---|---|---|---|---|---|
| Exit $>$ 17 $\times$ AB 12 | 0.043** | 0.003 | 0.010 | 0.036** | 0.435** |
| | (0.017) | (0.006) | (0.006) | (0.018) | (0.191) |
| Observations | 24,618 | 24,618 | 24,618 | 24,449 | 24,449 |
| R-squared | 0.049 | 0.016 | 0.021 | 0.026 | 0.032 |

The first column shows the effect of extended foster care eligibility on college enrollment. The second column shows the effect on two-year college graduation. The third column shows the effect on four-year college graduation. The fourth column shows the effect on the probability of any formal employment between the ages of 24 and 26. The fifth column shows the effect on the IHS of earnings between the ages of 24 and 26. Robust standard errors in parentheses.

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

Table 4: ITT Estimates, AB 12 Eligibility, College Outcomes, Additional Heterogeneity

| VAR | (1)<br>High Alleg.<br>Any College | (2)<br>High Alleg.<br>2Y Grad | (3)<br>High Alleg.<br>4Y Grad | (4)<br>Never Kin<br>Any College | (5)<br>Never Kin<br>2Y Grad | (6)<br>Never Kin<br>4Y Grad |
|---|---|---|---|---|---|---|
| Exit $>$ 17 $\times$AB 12 | 0.082*** | 0.007 | 0.007 | 0.043* | 0.012 | 0.020*** |
| | (0.025) | (0.009) | (0.008) | (0.023) | (0.008) | (0.007) |
| Observations | 11,803 | 11,803 | 11,803 | 13,441 | 13,441 | 13,441 |
| R-squared | 0.048 | 0.016 | 0.016 | 0.051 | 0.019 | 0.020 |

The first three columns show the effects of extended care eligibility for youth with above median allegation of maltreatment on college outcomes. The next three columns show the effects of extended care eligibility for youth who have never been in kin care. Robust standard errors in parentheses.

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

Table 5: ITT Estimates, AB 12 Eligibility, Youth Ever in Kin Care with Below Median Allegations

| VARIABLES | (1)<br>Any College | (2)<br>2Y Grad | (3)<br>4Y Grad | (4)<br>Any Earnings 24–26 | (5)<br>IHS Earnings 24–26 |
|---|---|---|---|---|---|
| Exit > 17 × AB 12 | 0.054 | 0.000 | 0.001 | 0.045 | 0.352 |
| | (0.037) | (0.013) | (0.012) | (0.036) | (0.394) |
| Observations | 6,029 | 6,029 | 6,029 | 6,129 | 6,129 |
| R-squared | 0.052 | 0.021 | 0.025 | 0.032 | 0.038 |

These estimates restrict to youth who have been in kin care and have below median number of allegations of maltreatment. Robust standard errors in parentheses.

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

Table 6: ITT Estimates, AB 12 Eligibility, Any College Enrollment

Panel A: Broad Race and Ethnic Groups

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | All | NHW Men | NHW Women | BAH Men | BAH Women |
| Exit > 17 × AB 12 | 0.043** | 0.089** | 0.037 | 0.040 | 0.016 |
| | (0.017) | (0.044) | (0.047) | (0.029) | (0.031) |
| Observations | 24,618 | 3,925 | 3,410 | 8,896 | 8,387 |
| R-squared | 0.049 | 0.053 | 0.051 | 0.038 | 0.032 |

The first column shows the effect on all youth, the second column shows the effect on non-Hispanic white men, the third column shows the effect on non-Hispanic white women, the fourth column shows the effect on Black, Asian, Hispanic, and other race men, and the fifth column shows the effect on Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses.

Panel B: Detailed Race and Ethnic Groups

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| VARIABLES | Hispanic Men | Hispanic Women | Black Men | Black Women |
| Exit > 17 × AB 12 | 0.056 | 0.035 | -0.016 | -0.018 |
| | (0.037) | (0.040) | (0.051) | (0.063) |
| Observations | 4,761 | 4,654 | 3,066 | 2,675 |
| R-squared | 0.040 | 0.040 | 0.054 | 0.045 |

The first column shows the effect on Hispanic men, the second column shows the effect on Hispanic women, the third column shows the effect on Black men, and the fourth column shows the effect on Black women. Robust standard errors in parentheses.

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

Table 7: ITT Estimates, AB 12 Eligibility, College Graduation

Panel A: 2-Year College Graduation

| VARIABLES | (1) All | (2) NHW Men | (3) NHW Women | (4) BAH Men | (5) BAH Women |
|---|---|---|---|---|---|
| Exit > 17 × AB 12 | 0.003 | 0.018 | 0.014 | -0.015 | 0.007 |
| | (0.006) | (0.013) | (0.021) | (0.009) | (0.013) |
| Observations | 24,618 | 3,925 | 3,410 | 8,896 | 8,387 |
| R-squared | 0.016 | 0.026 | 0.033 | 0.010 | 0.015 |

The first column shows the effect on all youth, the second column shows the effect on non-Hispanic white men, the third column shows the effect on non-Hispanic white women, the fourth column shows the effect on Black, Asian, Hispanic, and other race men, and the fifth column shows the effect on Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses.

Panel B: 4-Year College Graduation

| VARIABLES | (1) All | (2) NHW Men | (3) NHW Women | (4) BAH Men | (5) BAH Women |
|---|---|---|---|---|---|
| Exit > 17 × AB 12 | 0.010 | 0.035*** | 0.022 | 0.013 | -0.015 |
| | (0.006) | (0.013) | (0.021) | (0.008) | (0.012) |
| Observations | 24,618 | 3,925 | 3,410 | 8,896 | 8,387 |
| R-squared | 0.021 | 0.033 | 0.037 | 0.012 | 0.014 |

The first column shows the effect on all youth, the second column shows the effect on non-Hispanic white men, the third column shows the effect on non-Hispanic white women, the fourth column shows the effect on Black, Asian, Hispanic, and other race men, and the fifth column shows the effect on Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses.

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

Table 8: ITT Earnings Estimates, AB 12 Eligibility, Broad Race and Ethnic Groups

Panel A: Any Formal Employment Ages 24–26

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | All | NHW Men | NHW Women | BAH Men | BAH Women |
| Exit > 17 × AB 12 | 0.036** | 0.099** | 0.078 | 0.019 | 0.020 |
| | (0.018) | (0.045) | (0.048) | (0.029) | (0.030) |
| Observations | 24,449 | 3,950 | 3,418 | 8,756 | 8,325 |
| R-squared | 0.026 | 0.031 | 0.030 | 0.016 | 0.020 |

The first column shows the effect on all youth, the second column shows the effect on non-Hispanic white men, the third column shows the effect on non-Hispanic white women, the fourth column shows the effect on Black, Asian, Hispanic, and other race men, and the fifth column shows the effect on Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses.

Panel B: Inverse Hyperbolic Sine of Wages Ages 24–26

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | All | NHW Men | NHW Women | BAH Men | BAH Women |
| Exit > 17 × AB 12 | 0.435** | 1.103** | 0.894* | 0.272 | 0.265 |
| | (0.191) | (0.489) | (0.518) | (0.321) | (0.327) |
| Observations | 24,449 | 3,950 | 3,418 | 8,756 | 8,325 |
| R-squared | 0.032 | 0.032 | 0.033 | 0.020 | 0.023 |

The first column shows the effect on all youth, the second column shows the effect on non-Hispanic white men, the third column shows the effect on non-Hispanic white women, the fourth column shows the effect on Black, Asian, Hispanic, and other race men, and the fifth column shows the effect on Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses.

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

Table 9: ITT Estimates, AB 12 Eligibility, By Race and Risk Factors

Panel A: Any College, Above Median Allegations

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| VARIABLES | NHW Men | NHW Women | BAH Men | BAH Women |
| Exit $> 17 \times$ AB 12 | 0.121** | -0.005 | 0.099** | 0.082* |
| | (0.059) | (0.061) | (0.046) | (0.044) |
| Observations | 1,982 | 2,050 | 3,434 | 4,337 |
| R-squared | 0.076 | 0.077 | 0.050 | 0.045 |

These estimates restrict to youth with above the median number of allegations of maltreatment. The first column shows the effect on non-Hispanic white men, the second column shows the effect on non-Hispanic white women, the third column shows the effect on Black, Asian, Hispanic, and other race men, and the fourth column shows the effect on Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses.

Panel B: 4-Year College Graduation, Never Kin

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| VARIABLES | NHW Men | NHW Women | BAH Men | BAH Women |
| Exit $> 17 \times$ AB 12 | 0.030** | 0.041* | 0.016* | 0.004 |
| | (0.014) | (0.023) | (0.010) | (0.016) |
| Observations | 2,423 | 1,778 | 5,282 | 3,958 |
| R-squared | 0.039 | 0.045 | 0.013 | 0.023 |

These estimates restrict to youth who have never been in kin care. The first column shows the effect on non-Hispanic white men, the second column shows the effect on non-Hispanic white women, the third column shows the effect on Black, Asian, Hispanic, and other race men, and the fourth column shows the effect on Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses.

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

Table 10: Caseworker Leave One Measures and Youth Choices

| VARIABLES | (1)<br>FC<br>Work/School | (2)<br>FC<br>No Work/School | (3)<br><br>Exit Before 19 | (4)<br>FC<br>Work/School | (5)<br>FC<br>No Work/School | (6)<br><br>Exit Before 19 |
|---|---|---|---|---|---|---|
| LOO | 0.09*** | 0.12*** | -0.21*** | -1.24e-06 | 0.11*** | -0.11*** |
| | (0.00) | (0.01) | (0.00) | ( 5.31e-06) | (0.00) | (0.00) |
| N Youth | 8,020 | 8,020 | 8,020 | 8,020 | 8,020 | 8,020 |
| R-squared | 0.05 | 0.08 | 0.20 | 0.20 | 0.20 | 0.20 |
| Model | LPM | LPM | LPM | Probit | Probit | Probit |

LOO is the leave one out estimator of the percent of youth supervised by the same social worker who are in care after age 19 and not working or in school. Cohorts 1994 through 1996 are included. Standard errors in parentheses.

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

# 9 Figures

Figure 1: Outcome Means for All Youth in Care After Age 16

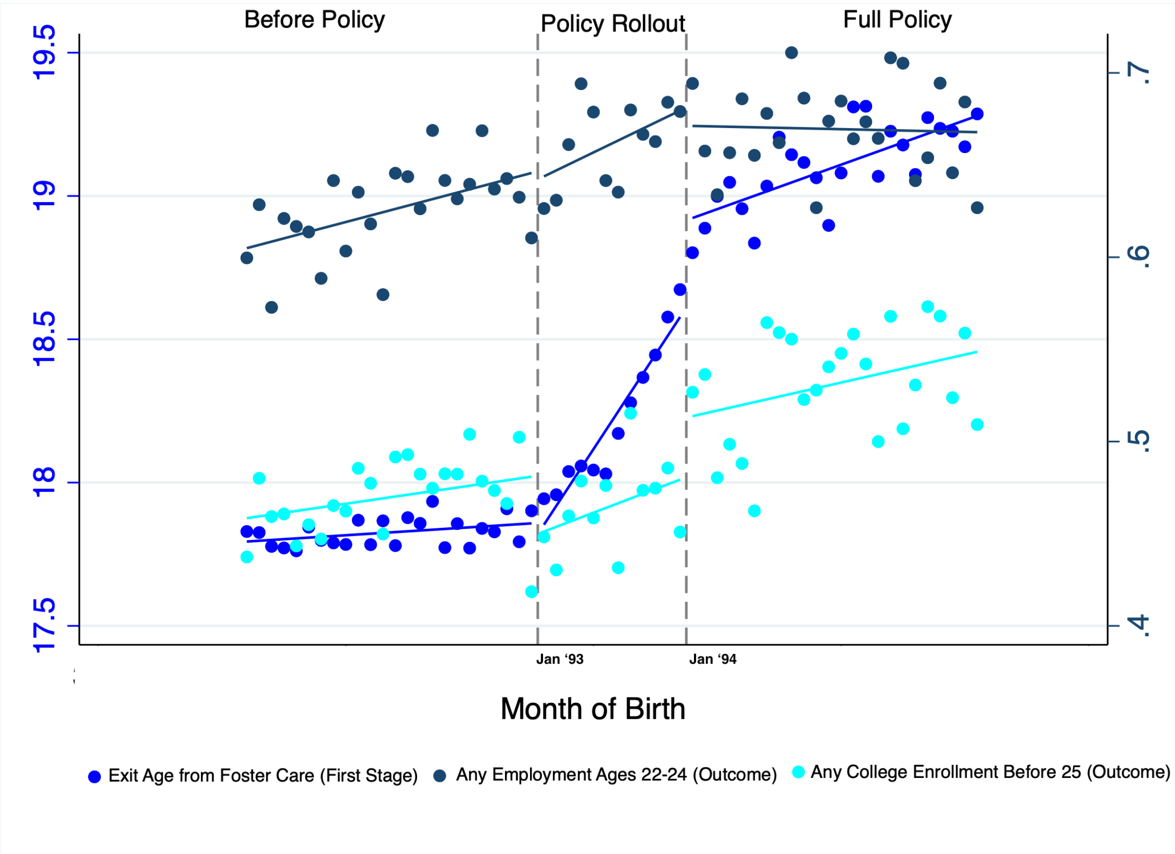


Figure 2: Event Study Showing Coefficients on (Quarter of Birth) x (Exit > 17)

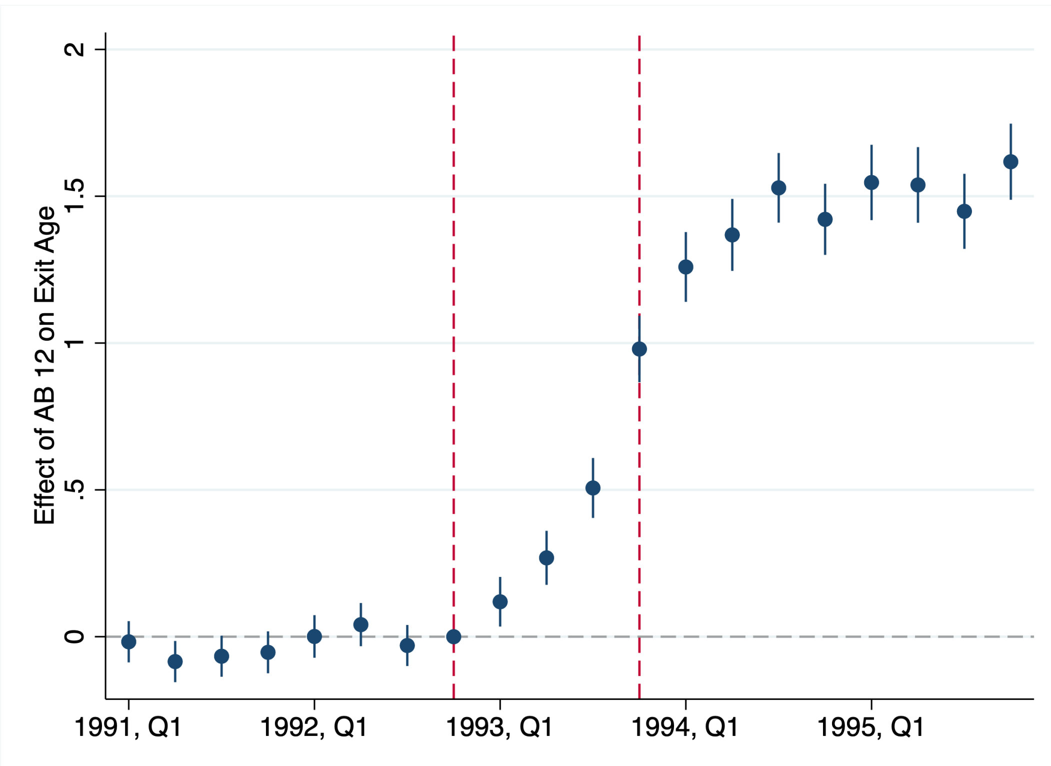
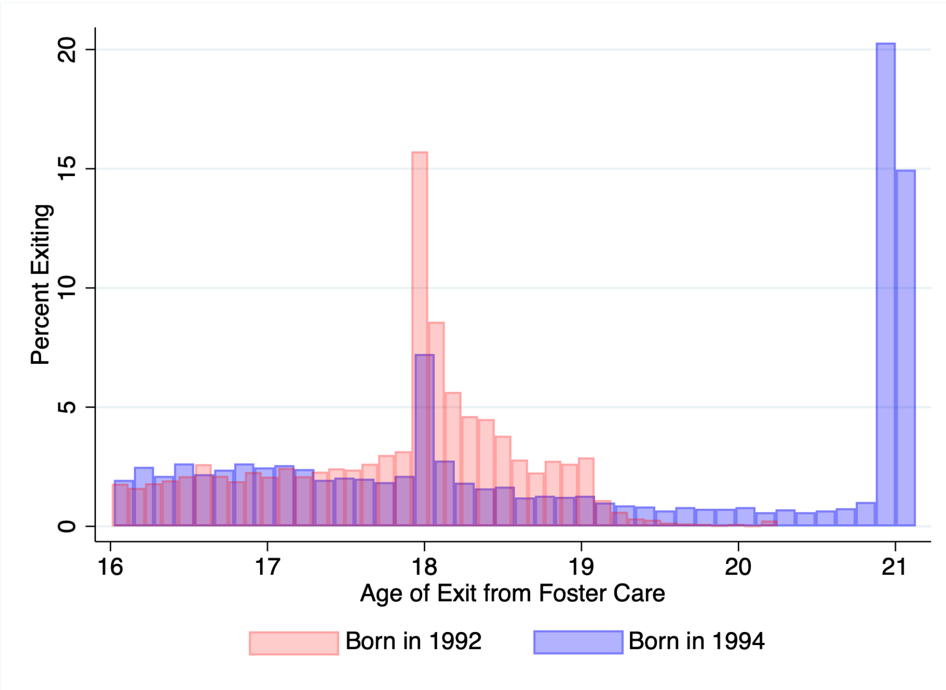



*Figure Notes*: The estimates shown in this figure are conditioning on quarter of birth, an indicator for Exit >17, county fixed effects, and demographics characteristics of youth.

Figure 3: Distribution of Exit Age for Youth in Care after 16th Birthday

(a) Histogram of Exit Age




(b) Histogram and Kernel Density Plot of Exit Age Zoomed in around Age 17

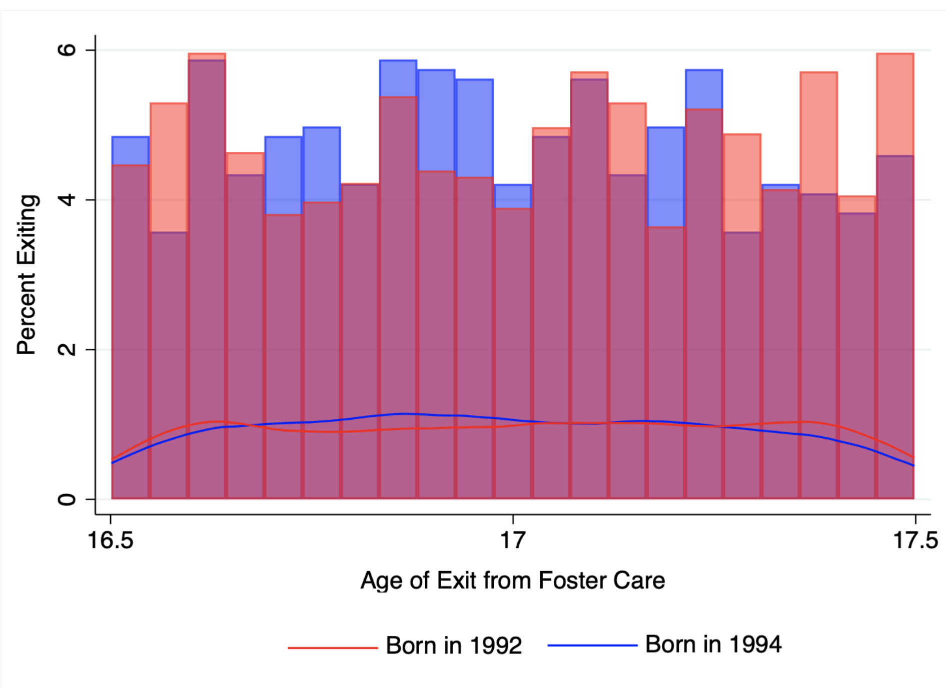



*Figure Notes*: Exit age is defined continuously.

Figure 4: ITT Event Studies on Downstream Outcomes

(a) Any College


(b) Formal Employment at Age 25

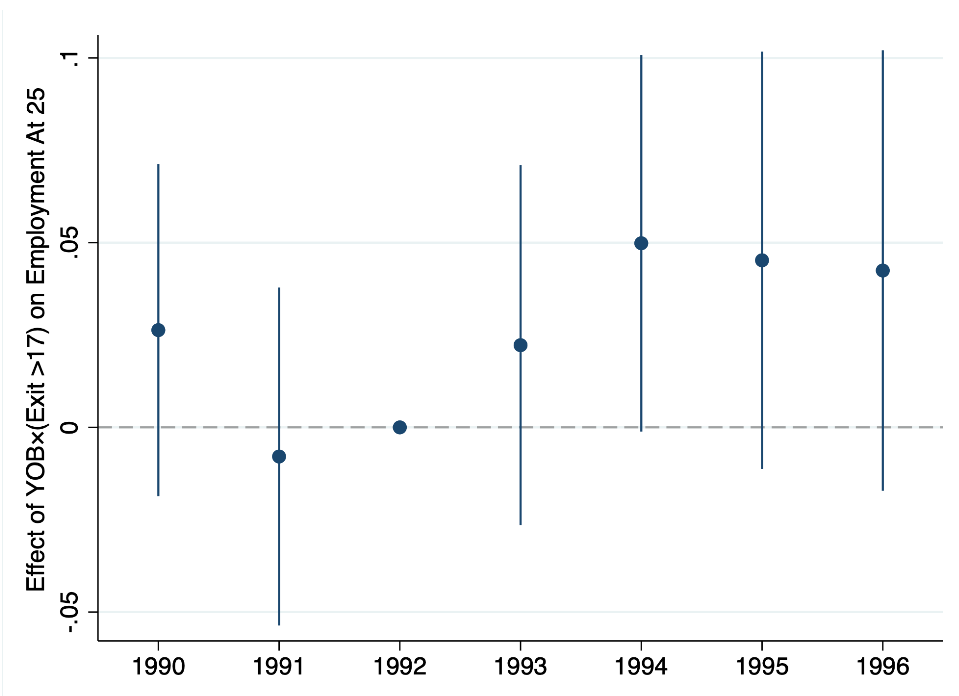



*Figure Notes*: The estimates shown in this figure are conditioning on quarter of birth, an indicator for Exit >17, county fixed effects, and demographics characteristics of youth.

Figure 5: Employment Effects for Youth with Above Median Allegations

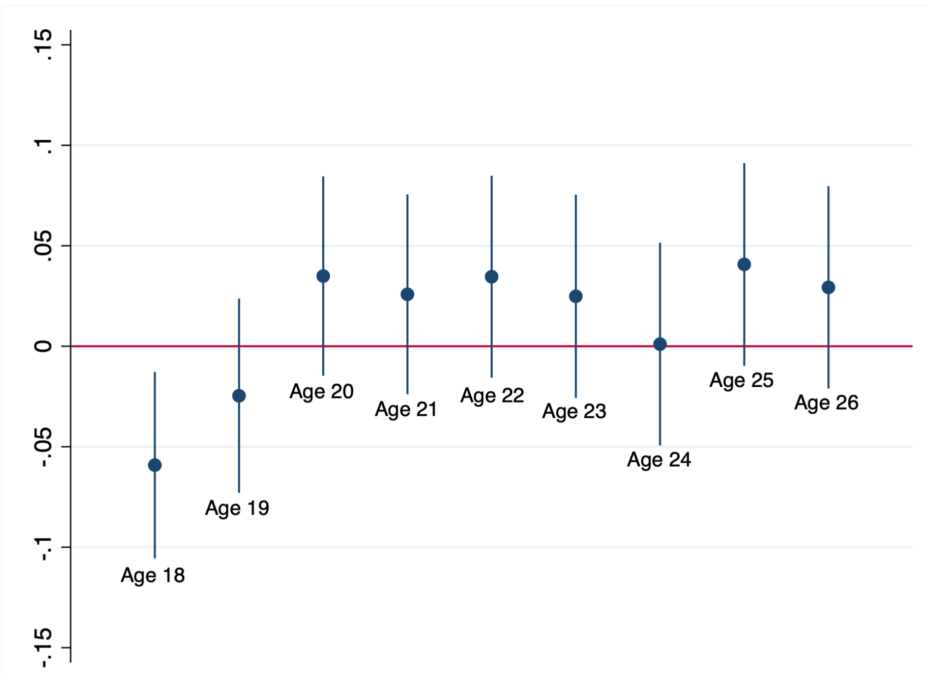



*Figure Notes*: This figure shows the effects of extended foster care eligibility on the probability of formal employment at ages 18 through 26 for youth with above median allegations of maltreatment.

Figure 6: Employment Effects for Youth Never in Kin Care

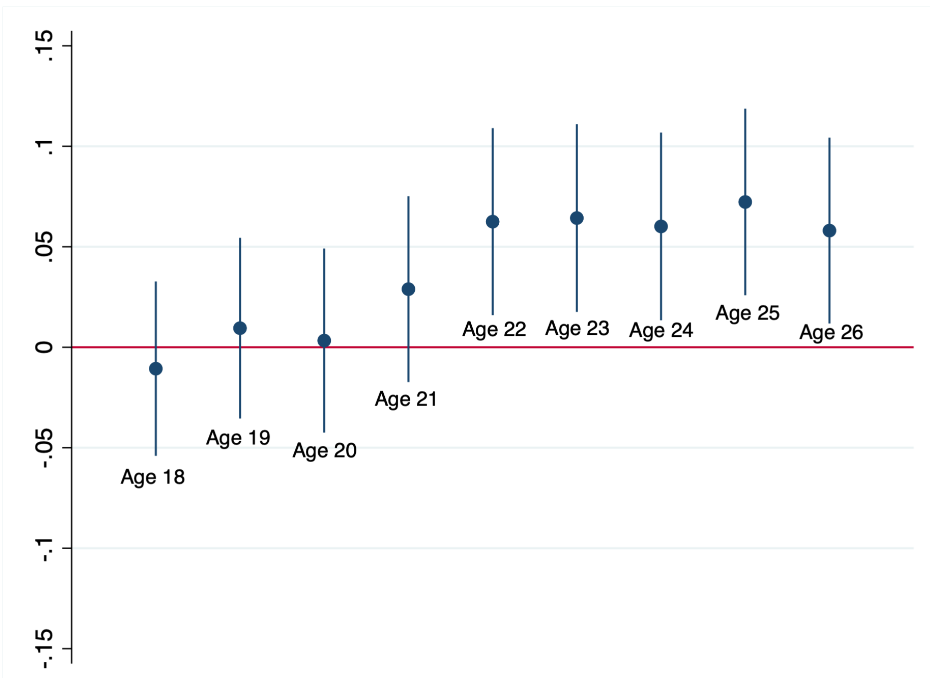



*Figure Notes*: This figure shows the effects of extended foster care eligibility on the probability of formal employment at ages 18 through 26 for youth who have never been in kin care.

Figure 7: Employment Effects for Non-Hispanic White Men, by Kin Care

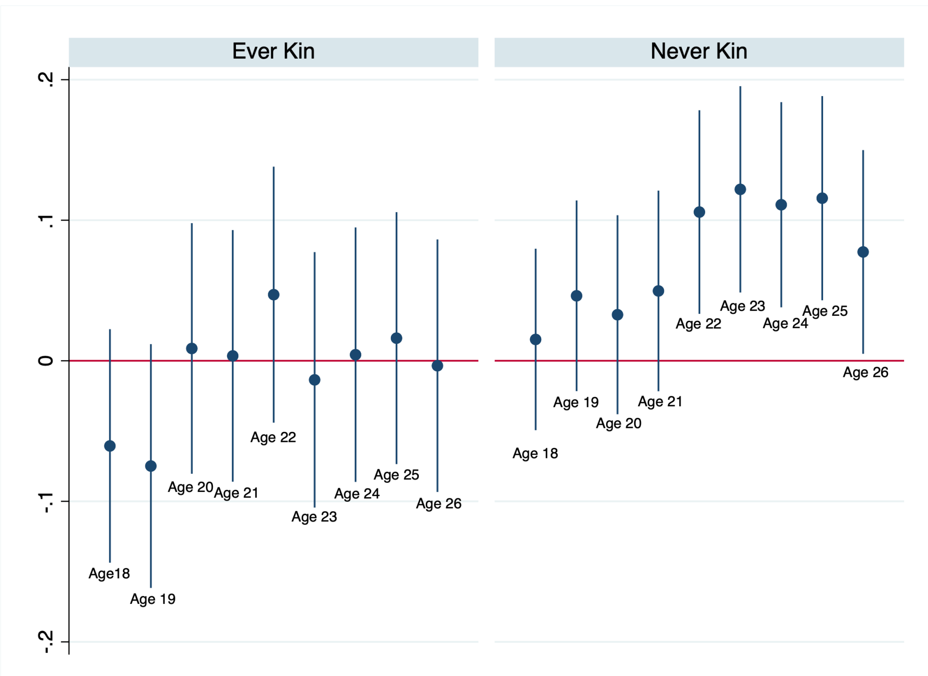



*Figure Notes*: This figure shows the effects of extended foster care eligibility on the probability of formal employment at ages 18 through 26 for non-Hispanic white men, split by whether they have ever been in kin care.

Figure 8: Employment Effects for Men of Color, by Kin Care

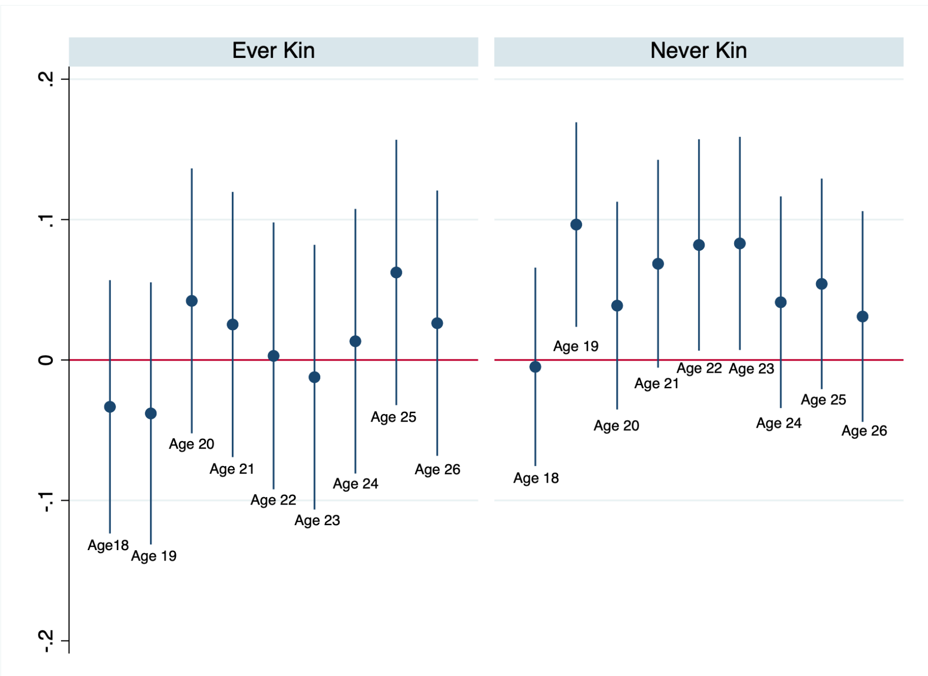
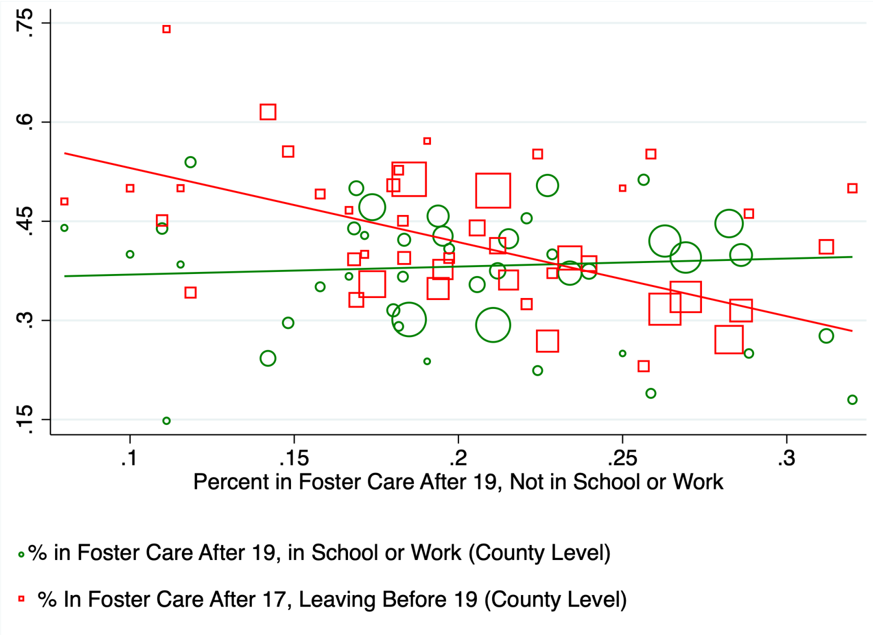


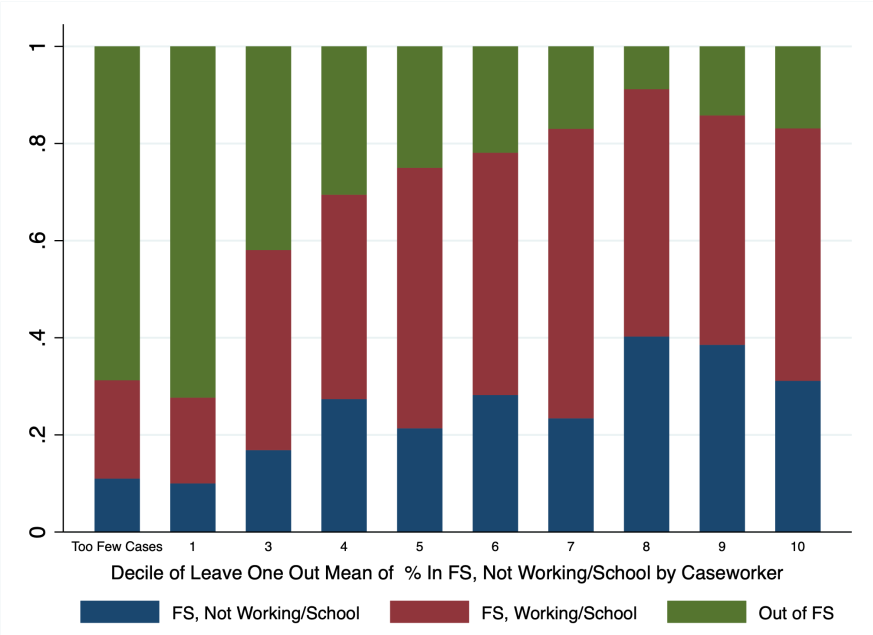

*Figure Notes*: This figure shows the effects of extended foster care eligibility on the probability of formal employment at ages 18 through 26 for men of color, split by whether they have ever been in kin care.

Figure 9: Relationship Between Leniency and Foster Care Exits




(a) County Level

*Figure Notes:* The squares and circles are proportional in size to the number of youth in the county. Estimates are conditional on youth still being in care at age 17. Birth cohorts 1994–1996 included.




(b) Caseworker Level

*Figure Notes:* Percent of youth in each category by the decile of their caseworker's other clients who are in foster care after age 19 but not working or in school. Birth cohorts 1994–1996 included.

# 10 Appendix

## 10.1 Appendix Tables

Table A1: Data Sources

| Data | Source |
|---|---|
| Child Welfare | California Department of Social Services |
| College Outcomes | National Student Clearinghouse |
| Employment and Earnings | Employment Development Department |

Table A2: Aggregate IV Estimates on Outcomes

| VARIABLES | (1) Any College | (2) 2Y Grad | (3) 4Y Grad | (4) Any Earnings 24–26 | (5) IHS Earnings 24–26 |
|---|---|---|---|---|---|
| Exit Age | 0.030** | 0.002 | 0.007 | 0.025** | 0.304** |
| | (0.012) | (0.005) | (0.004) | (0.012) | (0.134) |
| Observations | 24,618 | 24,618 | 24,618 | 24,449 | 24,449 |
| R-squared | 0.048 | 0.010 | 0.017 | 0.021 | 0.026 |

The first column shows the effect of one additional year of foster care on college enrollment. The second column shows the effect on two-year college graduation. The third column shows the effect on four-year college graduation. The fourth column shows the effect on the probability of any formal employment between the ages of 24 and 26. The fifth column shows the effect on the IHS of earnings between the ages of 24 and 26. Robust standard errors in parentheses.

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

Table A3: Summary Statistics by Race

Panel A: College Summary Statistics

| | All | NHW Men | NHW Women | BAH Men | BAH Women | Hisp. Men | Hisp. Women | Black Men | Black Women |
|---|---|---|---|---|---|---|---|---|---|
| Enroll in College | 0.50 | 0.46 | 0.56 | 0.44 | 0.55 | 0.40 | 0.50 | 0.49 | 0.62 |
| | (0.50) | (0.50) | (0.50) | (0.50) | (0.50) | (0.49) | (0.50) | (0.50) | (0.49) |
| 2-Year Degree | 0.032 | 0.023 | 0.045 | 0.018 | 0.047 | 0.018 | 0.044 | 0.015 | 0.039 |
| | (0.18) | (0.15) | (0.21) | (0.13) | (0.21) | (0.13) | (0.21) | (0.12) | (0.19) |
| 4-Year Degree | 0.033 | 0.022 | 0.052 | 0.019 | 0.044 | 0.017 | 0.035 | 0.019 | 0.046 |
| | (0.18) | (0.15) | (0.22) | (0.14) | (0.20) | (0.13) | (0.18) | (0.14) | (0.21) |
| *N* | 24618 | 3925 | 3410 | 8896 | 8387 | 4761 | 4654 | 3066 | 2675 |

Panel B: Earnings Summary Statistics

| | All | NHW Men | NHW Women | BAH Men | BAH Women | Hisp. Men | Hisp. Women | Black Men | Black Women |
|---|---|---|---|---|---|---|---|---|---|
| Any Wages Age 24–26 | 0.62 | 0.55 | 0.55 | 0.62 | 0.68 | 0.65 | 0.70 | 0.57 | 0.67 |
| | (0.49) | (0.50) | (0.50) | (0.49) | (0.47) | (0.48) | (0.46) | (0.50) | (0.47) |
| Total Wages Age 24–26 | 29258.2 | 27251.9 | 23446.0 | 29663.6 | 32170.1 | 33449.0 | 34807.6 | 22173.4 | 27030.3 |
| | (46881.6) | (50258.3) | (40133.7) | (47365.9) | (47028.2) | (49767.3) | (49184.9) | (40811.0) | (39858.9) |
| IHS of Wages Age 24–26 | 6.59 | 5.83 | 5.78 | 6.58 | 7.28 | 7.00 | 7.59 | 5.85 | 7.03 |
| | (5.32) | (5.41) | (5.38) | (5.31) | (5.15) | (5.27) | (5.08) | (5.27) | (5.09) |
| *N* | 24449 | 3950 | 3418 | 8756 | 8325 | 4604 | 4545 | 3113 | 2728 |

Table A4: ITT Earnings Estimates, AB 12 Eligibility, Detailed Race and Ethnic Groups

Panel A: Any Formal Employment Ages 24–26

| VARIABLES | (1) Hispanic Men | (2) Hispanic Women | (3) Black Men | (4) Black Women |
|---|---|---|---|---|
| Exit > 17 × AB 12 | 0.033 | -0.011 | -0.024 | 0.067 |
| | (0.039) | (0.037) | (0.052) | (0.059) |
| Observations | 4,604 | 4,545 | 3,113 | 2,728 |
| R-squared | 0.025 | 0.024 | 0.033 | 0.058 |

Panel B: Inverse Hyperbolic Sine of Wages Ages 24–26

| VARIABLES | (1) Hispanic Men | (2) Hispanic Women | (3) Black Men | (4) Black Women |
|---|---|---|---|---|
| Exit > 17 × AB 12 | 0.371 | -0.090 | -0.139 | 0.713 |
| | (0.430) | (0.415) | (0.552) | (0.626) |
| Observations | 4,604 | 4,545 | 3,113 | 2,728 |
| R-squared | 0.028 | 0.030 | 0.037 | 0.064 |

The first column shows the effect on Hispanic men, the second column shows the effect on Hispanic women, the third column shows the effect on Black men, and the fourth column shows the effect on Black women. Robust standard errors in parentheses.

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

Table A5: IV Estimates, Any College Enrollment

Panel A: Broad Race and Ethnic Groups

| VARIABLES | (1) All | (2) NHW Men | (3) NHW Women | (4) BAH Men | (5) BAH Women |
|---|---|---|---|---|---|
| Exit Age | 0.030** | 0.073** | 0.031 | 0.028 | 0.010 |
| | (0.012) | (0.036) | (0.039) | (0.020) | (0.020) |
| Observations | 24,618 | 3,925 | 3,410 | 8,896 | 8,387 |
| R-squared | 0.048 | 0.042 | 0.027 | 0.031 | 0.021 |

The first column shows the effect on all youth, the second column shows the effect on non-Hispanic white men, the third column shows the effect on non-Hispanic white women, the fourth column shows the effect on Black, Asian, Hispanic, and other race men, and the fifth column shows the effect on Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses.

Panel B: Detailed Race and Ethnic Groups

| VARIABLES | (1) Hispanic Men | (2) Hispanic Women | (3) Black Men | (4) Black Women |
|---|---|---|---|---|
| Exit Age | 0.041 | 0.023 | -0.010 | -0.011 |
| | (0.027) | (0.026) | (0.032) | (0.038) |
| Observations | 4,761 | 4,654 | 3,066 | 2,675 |
| R-squared | 0.031 | 0.034 | 0.016 | 0.015 |

The first column shows the effect on Hispanic men, the second column shows the effect on Hispanic women, the third column shows the effect on Black men, and the fourth column shows the effect on Black women. Robust standard errors in parentheses.

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

Table A6: IV Estimates, College Graduation

Panel A: 2-Year College Graduation

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | All | NHW Men | NHW Women | BAH Men | BAH Women |
| Exit Age | 0.002 | 0.015 | 0.012 | -0.010 | 0.005 |
| | (0.005) | (0.011) | (0.018) | (0.006) | (0.008) |
| | | | | | |
| Observations | 24,618 | 3,925 | 3,410 | 8,896 | 8,387 |
| R-squared | 0.010 | 0.008 | 0.001 | -0.006 | 0.005 |

Panel B: 4-Year College Graduation

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | All | NHW Men | NHW Women | BAH Men | BAH Women |
| Exit Age | 0.007 | 0.029*** | 0.018 | 0.009 | -0.010 |
| | (0.004) | (0.011) | (0.017) | (0.006) | (0.008) |
| | | | | | |
| Observations | 24,618 | 3,925 | 3,410 | 8,896 | 8,387 |
| R-squared | 0.017 | 0.005 | 0.017 | 0.007 | 0.002 |

The first column shows the effect on all youth, the second column shows the effect on non-Hispanic white men, the third column shows the effect on non-Hispanic white women, the fourth column shows the effect on Black, Asian, Hispanic, and other race men, and the fifth column shows the effect on Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses.

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

Table A7: IV Estimates, Any Formal Employment Ages 24–26

Panel A: Broad Race and Ethnic Groups

| VARIABLES | (1) All | (2) NHW Men | (3) NHW Women | (4) BAH Men | (5) BAH Women |
|---|---|---|---|---|---|
| Exit Age | 0.025** | 0.081** | 0.065* | 0.014 | 0.013 |
| | (0.012) | (0.037) | (0.039) | (0.021) | (0.019) |
| Observations | 24,449 | 3,950 | 3,418 | 8,756 | 8,325 |
| R-squared | 0.021 | 0.012 | 0.002 | 0.009 | 0.007 |

The first column shows the effect on all youth, the second column shows the effect on non-Hispanic white men, the third column shows the effect on non-Hispanic white women, the fourth column shows the effect on Black, Asian, Hispanic, and other race men, and the fifth column shows the effect on Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses.

Panel B: Detailed Race and Ethnic Groups

| VARIABLES | (1) Hispanic Men | (2) Hispanic Women | (3) Black Men | (4) Black Women |
|---|---|---|---|---|
| Exit Age | 0.024 | -0.007 | -0.015 | 0.040 |
| | (0.028) | (0.024) | (0.033) | (0.035) |
| Observations | 4,604 | 4,545 | 3,113 | 2,728 |
| R-squared | 0.012 | 0.002 | 0.006 | 0.019 |

The first column shows the effect on Hispanic men, the second column shows the effect on Hispanic women, the third column shows the effect on Black men, and the fourth column shows the effect on Black women. Robust standard errors in parentheses.

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

Table A8: IV Estimates, Inverse Hyperbolic Sine (IHS) of Wages Ages 24–26

Panel A: Broad Race and Ethnic Groups

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | All | NHW Men | NHW Women | BAH Men | BAH Women |
| Exit Age | 0.304** | 0.908** | 0.743* | 0.190 | 0.169 |
| | (0.134) | (0.399) | (0.426) | (0.224) | (0.208) |
| Observations | 24,449 | 3,950 | 3,418 | 8,756 | 8,325 |
| R-squared | 0.026 | 0.015 | 0.003 | 0.012 | 0.008 |

The first column shows the effect on all youth, the second column shows the effect on non-Hispanic white men, the third column shows the effect on non-Hispanic white women, the fourth column shows the effect on Black, Asian, Hispanic, and other race men, and the fifth column shows the effect on Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses.

Panel B: Detailed Race and Ethnic Groups

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| VARIABLES | Hispanic Men | Hispanic Women | Black Men | Black Women |
| Exit Age | 0.273 | -0.058 | -0.088 | 0.430 |
| | (0.314) | (0.265) | (0.345) | (0.373) |
| Observations | 4,604 | 4,545 | 3,113 | 2,728 |
| R-squared | 0.015 | 0.002 | 0.010 | 0.018 |

The first column shows the effect on Hispanic men, the second column shows the effect on Hispanic women, the third column shows the effect on Black men, and the fourth column shows the effect on Black women. Robust standard errors in parentheses.

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

Table A9: First Stage for Total Years in Foster Care

| VARIABLES | (1)<br>All | (2)<br>NHW Men | (3)<br>NHW Women | (4)<br>BAH Men | (5)<br>BAH Women |
|---|---|---|---|---|---|
| Exit > 17 × AB 12 | 1.589*** | 1.852*** | 0.780** | 1.854*** | 1.374*** |
| | (0.130) | (0.310) | (0.313) | (0.227) | (0.243) |
| Observations | 24,618 | 3,925 | 3,410 | 8,896 | 8,387 |
| R-squared | 0.163 | 0.134 | 0.137 | 0.155 | 0.147 |
| Effective F-stat | 148.45 | 35.77 | 6.21 | 66.63 | 32.02 |

The first column shows the first stage for all youth, the second column shows the first stage for non-Hispanic white men, the third column shows the first stage for non-Hispanic white women, the fourth column shows the first stage for Black, Asian, Hispanic, and other race men, and the fifth column shows the first stage for Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses. Effective F-stat is the effective first-stage F statistic as in Montiel Olea and Pflueger (2013).

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

Table A10: IV Estimates, Effect of Total Years in Foster Care, Any College Enrollment

| VARIABLES | (1) All | (2) NHW Men | (3) NHW Women | (4) BAH Men | (5) BAH Women |
|---|---|---|---|---|---|
| Total Years Foster Care | 0.027** | 0.048* | 0.047 | 0.021 | 0.011 |
| | (0.011) | (0.025) | (0.064) | (0.015) | (0.023) |
| Observations | 24,618 | 3,925 | 3,410 | 8,896 | 8,387 |
| R-squared | 0.008 | -0.143 | -0.104 | 0.018 | 0.025 |

The first column shows the effect on all youth, the second column shows the effect on non-Hispanic white men, the third column shows the effect on non-Hispanic white women, the fourth column shows the effect on Black, Asian, Hispanic, and other race men, and the fifth column shows the effect on Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses.

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

Table A11: IV Estimates, Effect of Total Years in Foster Care, College Graduation

Panel A: 2-Year College Graduation

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | All | NHW Men | NHW Women | BAH Men | BAH Women |
| Total Years Foster Care | 0.002 | 0.010 | 0.018 | -0.008 | 0.005 |
| | (0.004) | (0.007) | (0.028) | (0.005) | (0.010) |
| Observations | 24,618 | 3,925 | 3,410 | 8,896 | 8,387 |
| R-squared | 0.008 | -0.082 | -0.128 | -0.090 | -0.010 |

Panel B: 4-Year College Graduation

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | All | NHW Men | NHW Women | BAH Men | BAH Women |
| Total Years Foster Care | 0.006 | 0.019** | 0.028 | 0.007 | -0.011 |
| | (0.004) | (0.008) | (0.029) | (0.005) | (0.009) |
| Observations | 24,618 | 3,925 | 3,410 | 8,896 | 8,387 |
| R-squared | -0.003 | -0.292 | -0.235 | -0.042 | -0.070 |

The first column shows the effect on all youth, the second column shows the effect on non-Hispanic white men, the third column shows the effect on non-Hispanic white women, the fourth column shows the effect on Black, Asian, Hispanic, and other race men, and the fifth column shows the effect on Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses.

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

Table A12: IV Estimates, Effect of Total Years in Foster Care, Labor Market Outcomes

Panel A: Any Formal Employment Ages 24–26

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | All | NHW Men | NHW Women | BAH Men | BAH Women |
| Total Years Foster Care | 0.022** | 0.053** | 0.095 | 0.010 | 0.014 |
| | (0.011) | (0.025) | (0.067) | (0.016) | (0.021) |
| Observations | 24,449 | 3,950 | 3,418 | 8,756 | 8,325 |
| R-squared | -0.014 | -0.204 | -0.533 | 0.006 | -0.015 |

Panel B: Inverse Hyperbolic Sine of Wages Ages 24–26

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | All | NHW Men | NHW Women | BAH Men | BAH Women |
| Total Years Foster Care | 0.268** | 0.588** | 1.091 | 0.145 | 0.185 |
| | (0.120) | (0.274) | (0.745) | (0.171) | (0.232) |
| Observations | 24,449 | 3,950 | 3,418 | 8,756 | 8,325 |
| R-squared | -0.014 | -0.220 | -0.608 | 0.006 | -0.022 |

The first column shows the effect on all youth, the second column shows the effect on non-Hispanic white men, the third column shows the effect on non-Hispanic white women, the fourth column shows the effect on Black, Asian, Hispanic, and other race men, and the fifth column shows the effect on Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses.

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

Table A13: First Stage, Restricted Sample

| VARIABLES | (1)<br>All | (2)<br>NHW Men | (3)<br>NHW Women | (4)<br>BAH Men | (5)<br>BAH Women |
|---|---|---|---|---|---|
| Exit $> 17 \times$ AB 12 | 1.295*** | 1.082*** | 1.063*** | 1.274*** | 1.470*** |
| | (0.029) | (0.072) | (0.077) | (0.050) | (0.050) |
| Observations | 20,104 | 3,274 | 2,762 | 7,410 | 6,658 |
| R-squared | 0.536 | 0.522 | 0.544 | 0.524 | 0.561 |
| Effective F-stat | 1993.97 | 225.95 | 188.52 | 656.39 | 855.45 |

The first column shows the first stage for all youth, the second column shows the first stage for non-Hispanic white men, the third column shows the first stage for non-Hispanic white women, the fourth column shows the first stage for Black, Asian, Hispanic, and other race men, and the fifth column shows the first stage for Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses. Effective F-stat is the effective first-stage F statistic as in Montiel Olea and Pflueger (2013).

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

Table A14: IV Estimates, Restricted Sample, Any College Enrollment

| VARIABLES | (1)<br>All | (2)<br>NHW Men | (3)<br>NHW Women | (4)<br>BAH Men | (5)<br>BAH Women |
|---|---|---|---|---|---|
| Exit Age | 0.051*** | 0.092* | 0.064 | 0.059** | 0.019 |
| | (0.016) | (0.048) | (0.054) | (0.027) | (0.026) |
| Observations | 20,104 | 3,274 | 2,762 | 7,410 | 6,658 |
| R-squared | 0.048 | 0.032 | 0.030 | 0.035 | 0.022 |

The first column shows the effect on all youth, the second column shows the effect on non-Hispanic white men, the third column shows the effect on non-Hispanic white women, the fourth column shows the effect on Black, Asian, Hispanic, and other race men, and the fifth column shows the effect on Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses.

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

Table A15: IV Estimates, Restricted Sample, College Graduation

Panel A: 2-Year College Graduation

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | All | NHW Men | NHW Women | BAH Men | BAH Women |
| Exit Age | -0.001 | 0.028*** | 0.030 | -0.020** | -0.002 |
| | (0.006) | (0.011) | (0.023) | (0.009) | (0.011) |
| | | | | | |
| Observations | 20,104 | 3,274 | 2,762 | 7,410 | 6,658 |
| R-squared | 0.008 | -0.016 | -0.013 | -0.021 | 0.002 |

Panel B: 4-Year College Graduation

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | All | NHW Men | NHW Women | BAH Men | BAH Women |
| Exit Age | 0.004 | 0.027* | 0.033 | 0.008 | -0.017* |
| | (0.006) | (0.016) | (0.022) | (0.008) | (0.010) |
| | | | | | |
| Observations | 20,104 | 3,274 | 2,762 | 7,410 | 6,658 |
| R-squared | 0.009 | -0.004 | 0.006 | 0.004 | -0.001 |

Panel C: 2- or 4-Year College Graduation

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | All | NHW Men | NHW Women | BAH Men | BAH Women |
| Exit Age | 0.004 | 0.046** | 0.067** | -0.015 | -0.011 |
| | (0.008) | (0.019) | (0.030) | (0.012) | (0.014) |
| | | | | | |
| Observations | 20,104 | 3,274 | 2,762 | 7,410 | 6,658 |
| R-squared | 0.014 | -0.020 | -0.022 | -0.006 | 0.001 |

The first column shows the effect on all youth, the second column shows the effect on non-Hispanic white men, the third column shows the effect on non-Hispanic white women, the fourth column shows the effect on Black, Asian, Hispanic, and other race men, and the fifth column shows the effect on Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses.

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

Table A16: IV Estimates, Restricted Sample, Labor Market Outcomes

Panel A: Any Formal Employment Ages 24–26

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | All | NHW Men | NHW Women | BAH Men | BAH Women |
| Exit Age | 0.029* | 0.096* | 0.097* | 0.000 | 0.025 |
| | (0.016) | (0.050) | (0.057) | (0.028) | (0.025) |
| Observations | 20,018 | 3,300 | 2,775 | 7,302 | 6,641 |
| R-squared | 0.021 | 0.013 | -0.014 | 0.005 | 0.008 |

Panel B: Inverse Hyperbolic Sine of Wages Ages 24–26

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | All | NHW Men | NHW Women | BAH Men | BAH Women |
| Exit Age | 0.385** | 1.106** | 1.147* | 0.069 | 0.359 |
| | (0.179) | (0.544) | (0.612) | (0.302) | (0.270) |
| Observations | 20,018 | 3,300 | 2,775 | 7,302 | 6,641 |
| R-squared | 0.025 | 0.015 | -0.016 | 0.008 | 0.009 |

The first column shows the effect on all youth, the second column shows the effect on non-Hispanic white men, the third column shows the effect on non-Hispanic white women, the fourth column shows the effect on Black, Asian, Hispanic, and other race men, and the fifth column shows the effect on Black, Asian, Hispanic, and other race men. Robust standard errors in parentheses.

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

Table A17: Comparison of First Stage Models

| Model | (1)<br>Week Dummies | (2)<br>p=1 | (3)<br>p=2 | (4)<br>p=3 |
|---|---|---|---|---|
| F-stat | 14.15 | **398.9** | 251.3 | 183.5 |
| AIC | 50418 | 50312 | 50303 | **50300** |
| BIC | 51604 | **50357** | 50372 | 50384 |
| GCV | 1.765 | 1.752 | 1.751 | 1.751 |

Table A18: First Stage Regression Kink

| VARIABLES | (1)<br>Exit Age | (2)<br>Exit Age | (3)<br>Exit Age | (4)<br>Exit Age |
|---|---|---|---|---|
| Kink 1 | 0.0019*** | 0.0019*** | 0.0019*** | 0.0019*** |
| | (0.0002) | (0.0002) | (0.0002) | (0.0002) |
| Kink 2 | -0.0015*** | -0.0016*** | | |
| | (0.0003) | (0.0003) | | |
| Constant | 17.8527*** | 17.3857*** | 17.8527*** | 17.5744*** |
| | (0.0358) | (0.1593) | (0.0294) | (0.1596) |
| Observations | 14,804 | 14,804 | 10,600 | 10,600 |
| R-squared | 0.1188 | 0.1457 | 0.0427 | 0.0731 |
| Effective F-stat | 31.63 | 33.53 | 86.31 | 88.93 |
| Controls | No | Yes | No | Yes |
| Kinks | Both | Both | First Only | First Only |

Standard errors in parentheses

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

Table A19: Fuzzy Regression Kink Estimates

Panel A: Baseline Estimates Using Both Kinks

| Outcomes | Any College | 2Y Grad | 4Y Grad | Any Earnings 22–24 | IHS Earnings 22–24 |
|---|---|---|---|---|---|
| Exit Age | 0.079* | 0.027 | 0.008 | 0.072* | 0.760* |
| | (0.047) | (0.016) | (0.016) | (0.041) | (0.442) |
| | | | | | |
| Observations | 14,804 | 14,804 | 14,804 | 14,736 | 14,736 |
| R-squared | 0.036 | -0.031 | 0.002 | 0.004 | 0.010 |

Panel B: Estimates Using Both Kinks with Controls

| Outcomes | Any College | 2Y Grad | 4Y Grad | Any Earnings 22–24 | IHS Earnings 22–24 |
|---|---|---|---|---|---|
| Exit Age | 0.071 | 0.029* | 0.010 | 0.077* | 0.810* |
| | (0.045) | (0.016) | (0.016) | (0.040) | (0.428) |
| | | | | | |
| Observations | 14,804 | 14,804 | 14,804 | 14,736 | 14,736 |
| R-squared | 0.070 | -0.025 | 0.014 | 0.026 | 0.038 |

Panel C: Estimates Using First Kink Only

| Outcomes | Any College | 2Y Grad | 4Y Grad | Any Earnings 22–24 | IHS Earnings 22–24 |
|---|---|---|---|---|---|
| Exit Age | 0.101** | 0.022 | 0.003 | 0.074* | 0.794* |
| | (0.049) | (0.016) | (0.015) | (0.043) | (0.457) |
| | | | | | |
| Observations | 10,600 | 10,600 | 10,600 | 10,606 | 10,606 |
| R-squared | 0.017 | -0.020 | 0.000 | 0.002 | 0.006 |

Panel D: Estimates Using First Kink Only with Controls

| Outcomes | Any College | 2Y Grad | 4Y Grad | Any Earnings 22–24 | IHS Earnings 22–24 |
|---|---|---|---|---|---|
| Exit Age | 0.095** | 0.026* | 0.004 | 0.076* | 0.829* |
| | (0.048) | (0.015) | (0.015) | (0.042) | (0.448) |
| | | | | | |
| Observations | 10,600 | 10,600 | 10,600 | 10,606 | 10,606 |
| R-squared | 0.058 | -0.010 | 0.016 | 0.030 | 0.039 |

Standard errors in parentheses

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

Table A20: ITT Estimates, AB 12 Eligibility, College Outcomes, Youth with Neuropsycholgical Disabilities

| VAR | (1)<br>Neuropsych<br>Any College | (2)<br>Neuropsych<br>2Y Grad | (3)<br>Neuropsych<br>4Y Grad |
|---|---|---|---|
| Exit $> 17 \times$AB 12 | 0.077** | -0.007 | 0.013 |
| | (0.034) | (0.011) | (0.010) |
| Observations | 9,245 | 9,245 | 9,245 |
| R-squared | 0.048 | 0.021 | 0.019 |

This table show the effects of extended care eligibility for youth with neuropsycholgical disabilities. Robust standard errors in parentheses.

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

Table A21: Effect of Leaving Care Before 19 Induced by Caseworker Leniency

| VARIABLES | (1)<br>Any Earnings at 25 | (2)<br>Any Earnings at 25 | (3)<br>Any Earnings at 25 |
|---|---|---|---|
| Exit Before 19 | -0.061** | -0.080*** | -0.113*** |
| | (0.028) | (0.029) | (0.034) |
| # Clients of Caseworker in Care After 17 | 0.001*** | | |
| | (0.000) | | |
| First Stage F-Stat | 1231 | 1535 | 1193 |
| Observations | 9,017 | 7,150 | 5,952 |
| R-squared | 0.039 | 0.038 | 0.045 |
| Demographic Controls | YES | YES | YES |
| Quarter of Birth FE | YES | YES | YES |
| County County FE | Yes | Yes | Yes |
| Sample | All Exit Age >17 | Case Worker Has >5 Clients in Care > 17 | Case Worker Has >10 Clients in Care > 17 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

## 10.2 Appendix Figures

Figure A1: Placement Types by Age, 1991–1992 Birth Cohorts

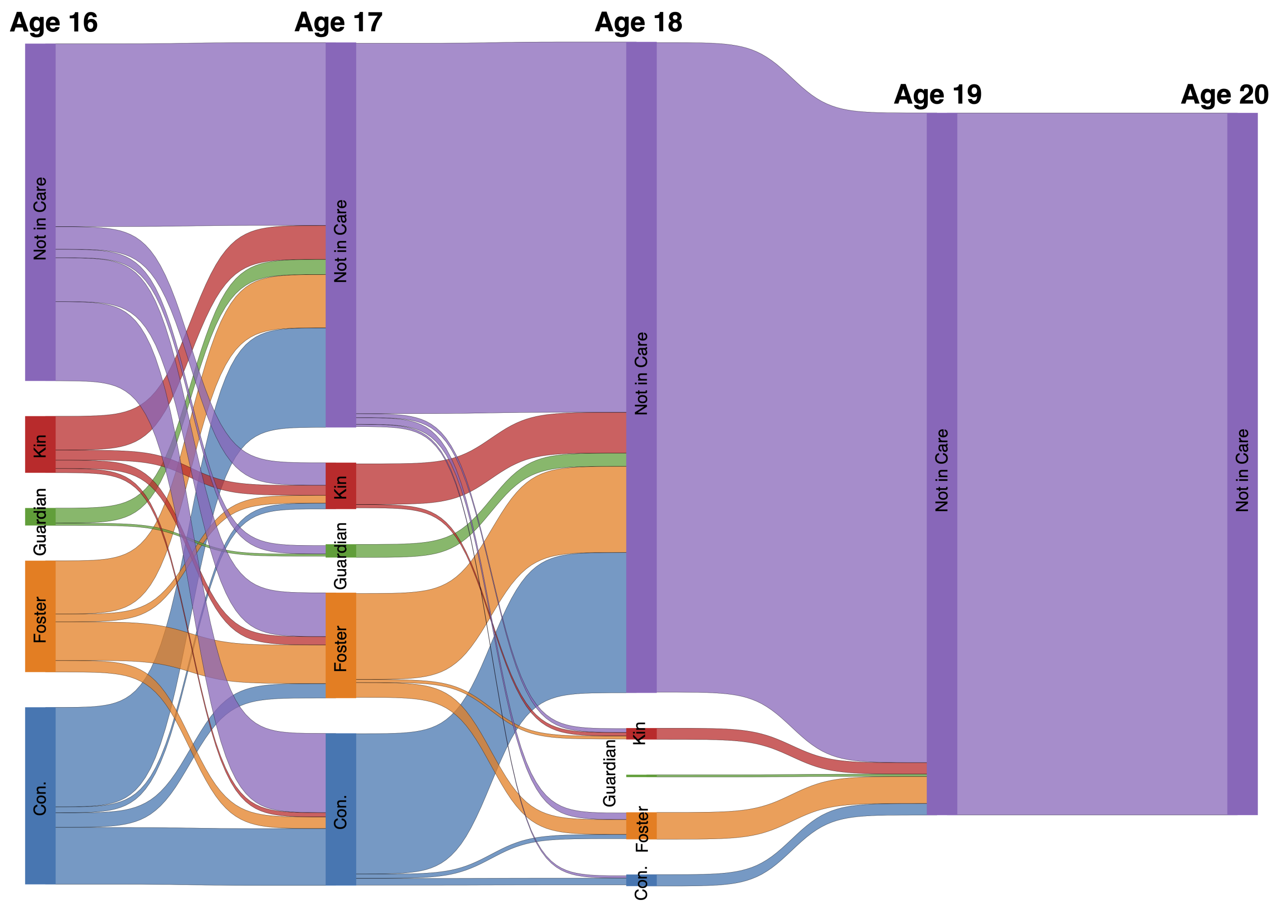



*Figure Notes*: This figure shows the distribution of and flow between the most common placement types that youth had between the ages of 16 and 20, with flows of fewer than 30 youth omitted for privacy. From top to bottom, purple represents youth not in foster care, red represents youth in kin care, green represents youth in care with a guardian, orange represents youth living in a foster family home, and blue represents youth in congregate care.

Figure A2: Placement Types by Age, 1994–1995 Birth Cohorts

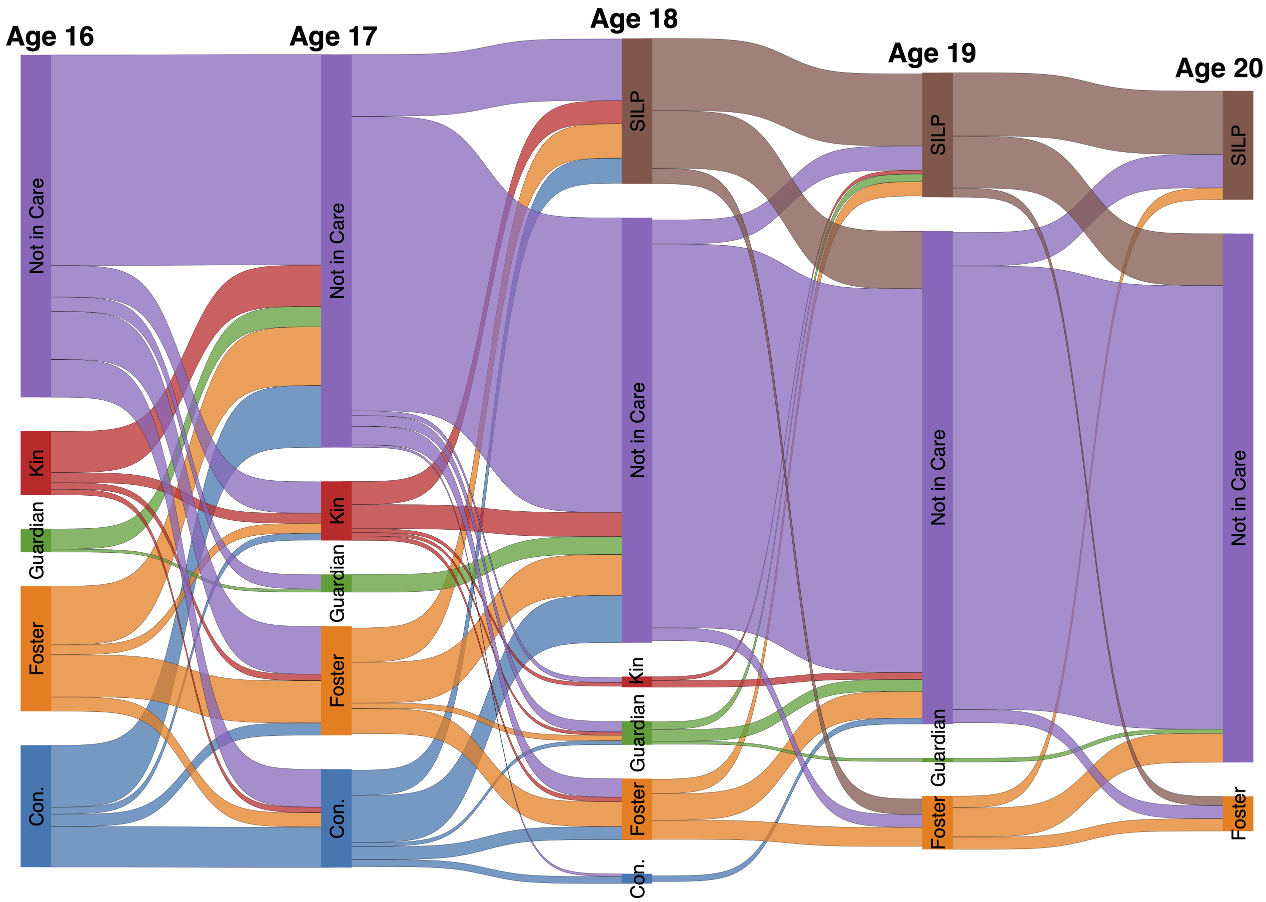
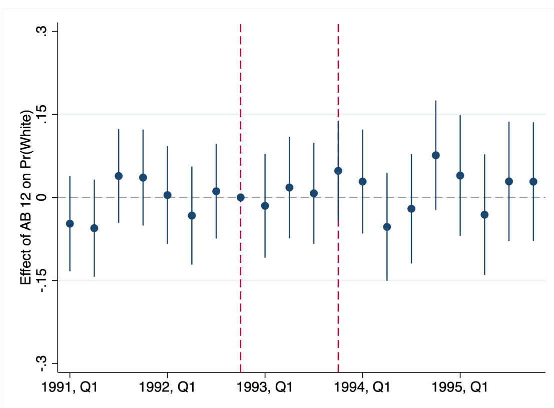
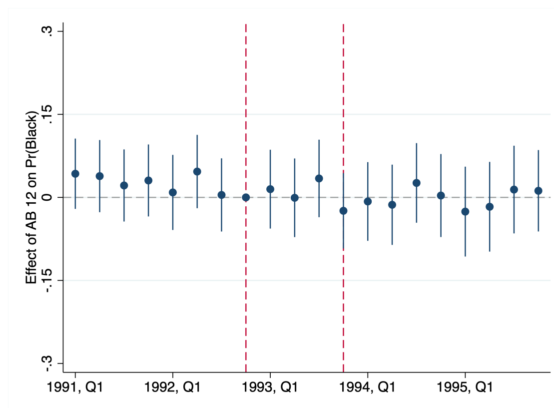



*Figure Notes*: This figure shows the distribution of and flow between the most common placement types that youth had between the ages of 16 and 20, with flows of fewer than 30 youth omitted for privacy. Purple represents youth not in foster care, red represents youth in kin care, green represents youth in care with a guardian, orange represents youth living in a foster family home, and blue represents youth in congregate care. Brown represents youth in supervised independent livings programs (SILPS), including college dormitories.

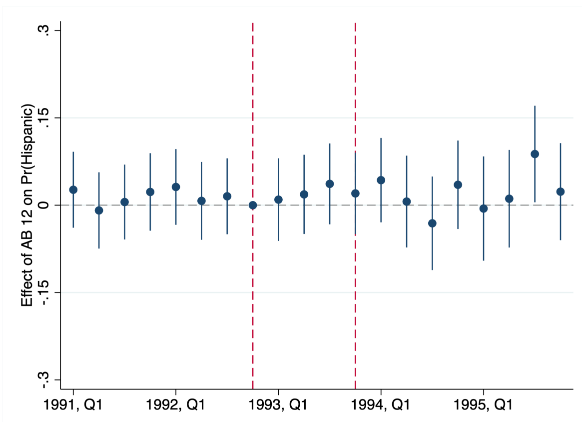
Figure A3: Placebo Tests Showing the Effects of AB 12 on Select Demographics



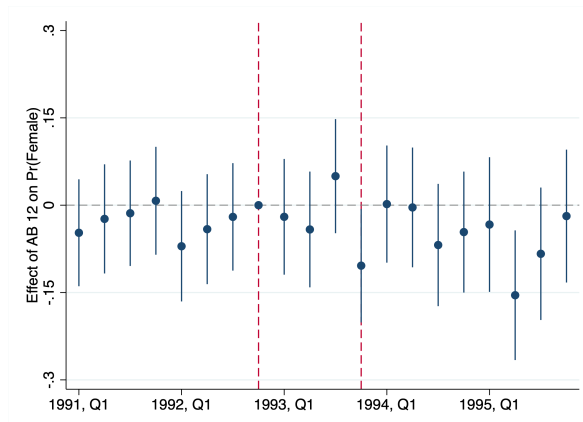

(a) Outcome: Prob. of Being White




(b) Outcome: Prob. of Being Black



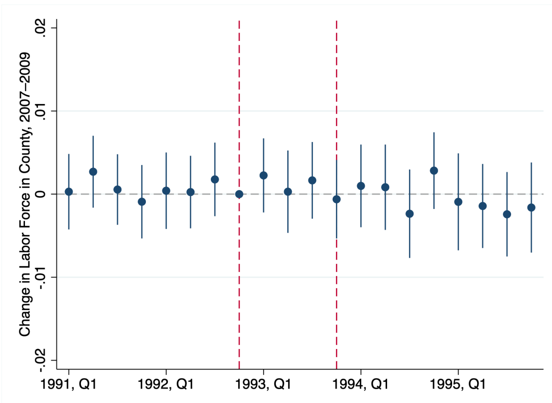

(c) Outcome: Prob. of Being Hispanic



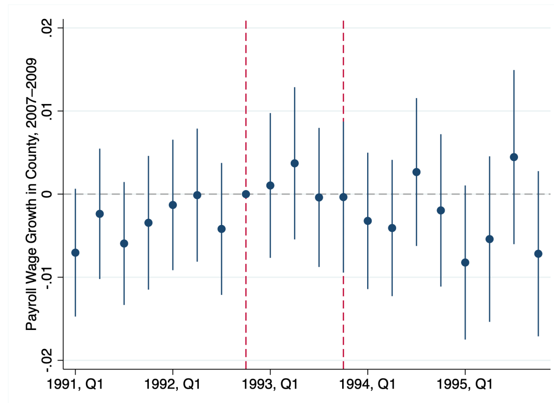

(d) Outcome: Prob. of Being Female

*Figure Notes*: The estimates shown in this figure are conditioning on quarter of birth, an indicator for Exit >17, and county fixed effects.

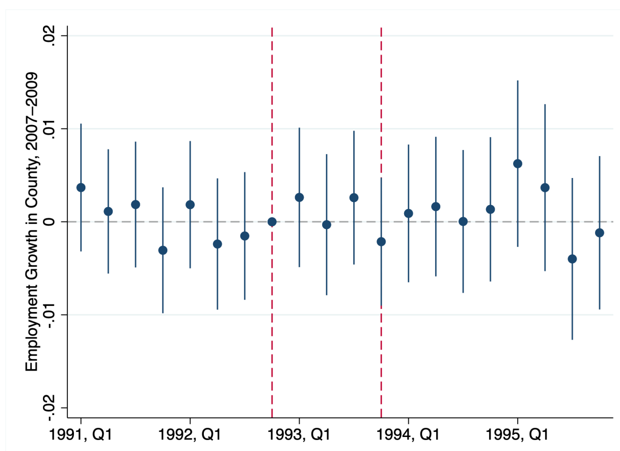
Figure A4: Placebo Tests Showing the Effects of AB 12 on County Composition as Related to the Great Recession




(a) Outcome: Change in Labor Force in County, 2007-2009




(b) Outcome: Payroll Wage Growth in County, 2007-2009




(c) Outcome: Employment Growth in County, 2007-2009

*Figure Notes*: The estimates shown in this figure are conditioning on quarter of birth, an indicator for Exit >17, and demographic variables.

Figure A5: Effect of AB 12 on Exiting After Age 17

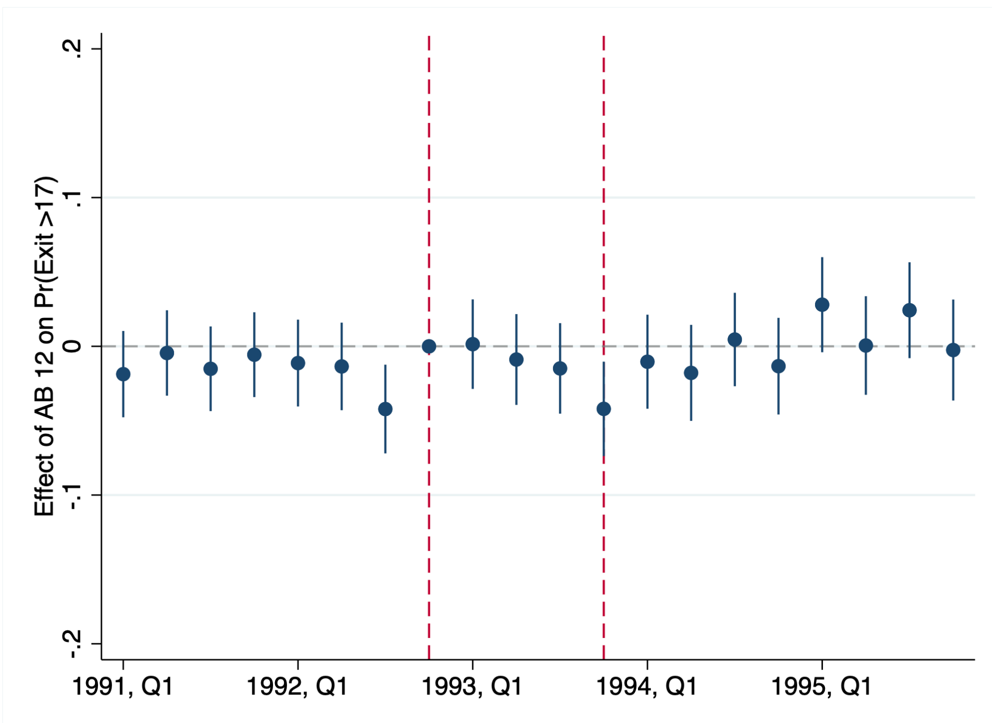



*Figure Notes*: The estimates shown in this figure are conditioning on quarter of birth, an indicator for Exit >16, county fixed effects, and demographics characteristics of youth.

Figure A6: Effect of AB 12 on Total Time in Foster Care

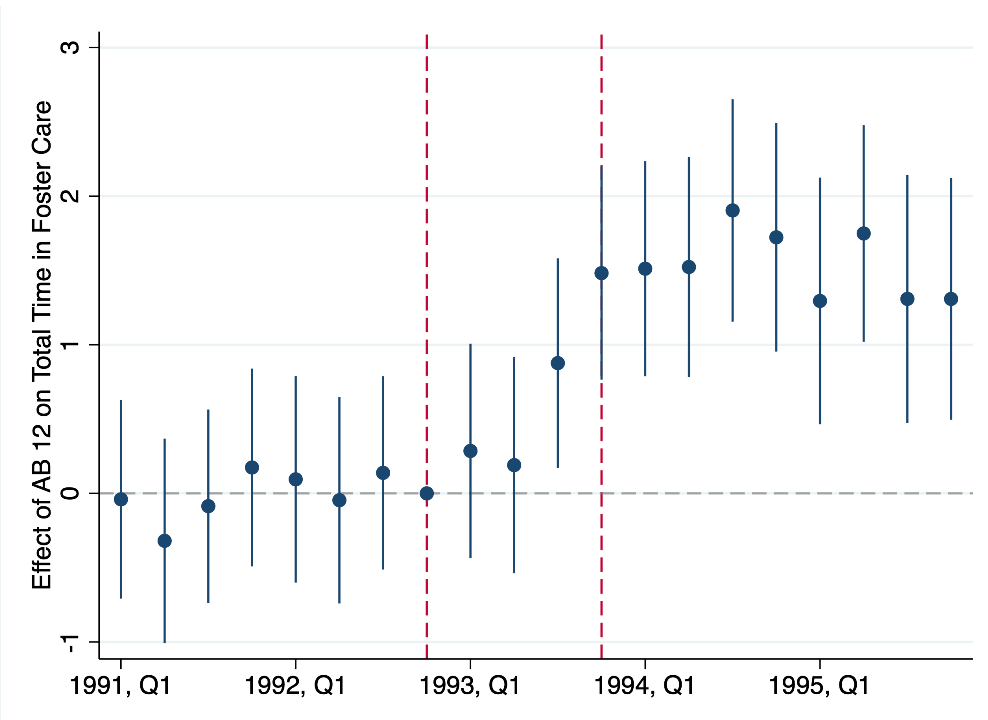



*Figure Notes*: The estimates shown in this figure are conditioning on quarter of birth, an indicator for Exit >17, county fixed effects, and demographics characteristics of youth.

Figure A7: CDF of Exit Age, 1992-Q4 and 1993-Q3

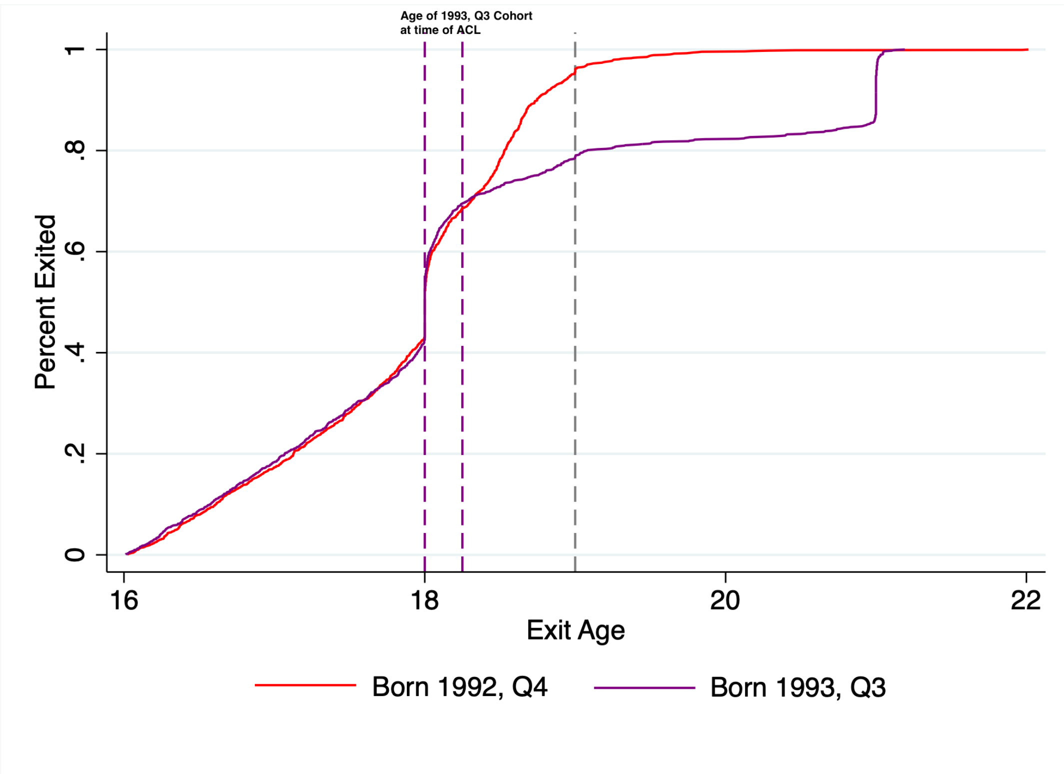



*Figure Notes*: This figure compares the CDF of exit rates for youth born in the last quarter of 1992 (not eligible for AB 12) to the CDF of exit rates for youth born in the third quarter of 1993 (partially eligible for AB 12).The hazard rate of exit for these two groups at each continuous age is the same up until October 2011, after which the younger cohort's hazard rate of exit decreases.

Figure A8: CDF of Exit Age, 1992-Q4 and 1993-Q2

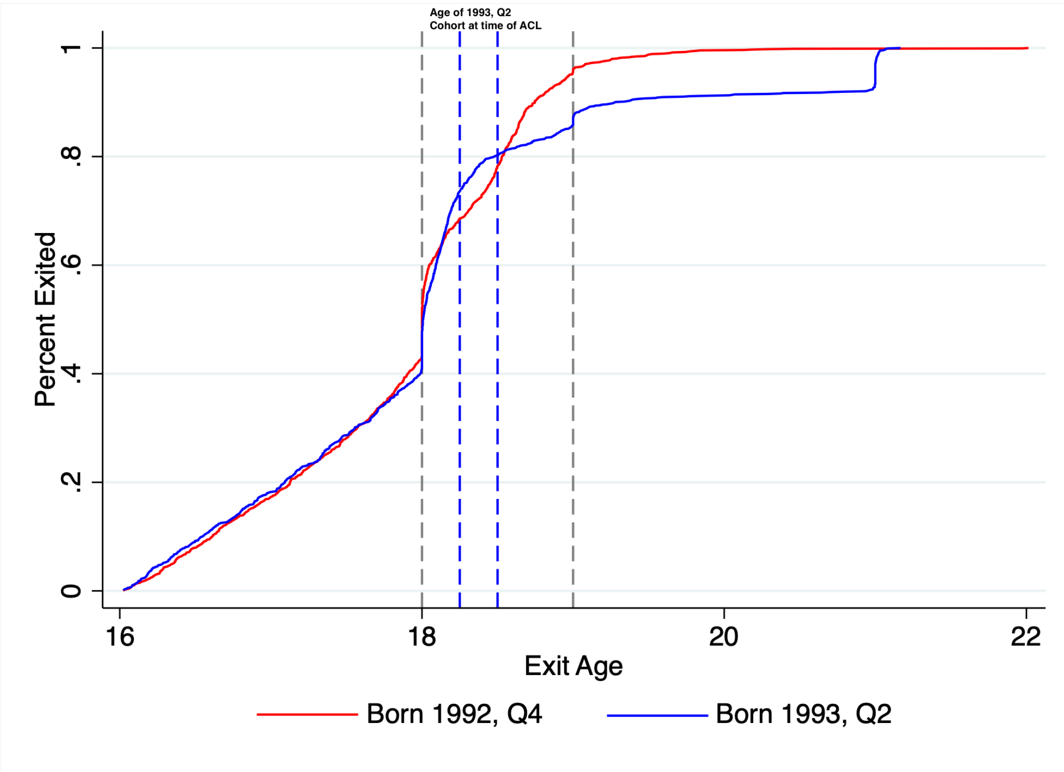



*Figure Notes*: This figure compares the CDF of exit rates for youth born in the last quarter of 1992 (not eligible for AB 12) to the CDF of exit rates for youth born in the second quarter of 1993 (partially eligible for AB 12).The hazard rate of exit for these two groups at each continuous age is the same up until October 2011, after which the younger cohort's hazard rate of exit decreases.

Figure A9: CDF of Exit Age, 1992-Q4 and 1993-Q1

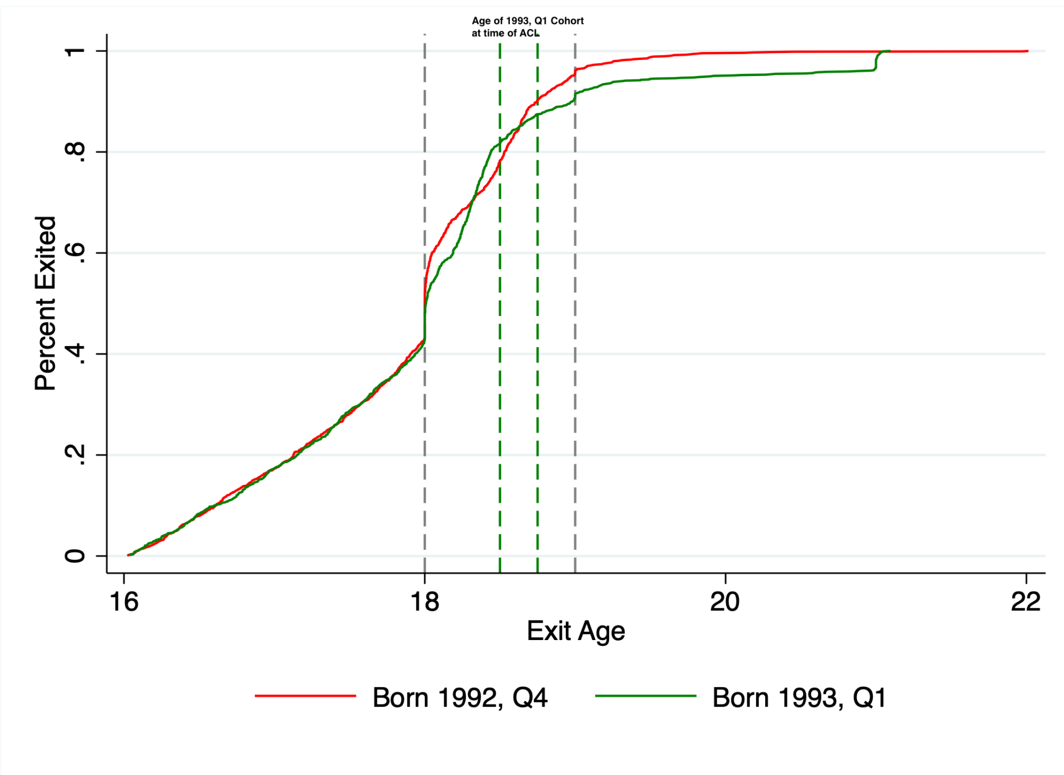



*Figure Notes*: This figure compares the CDF of exit rates for youth born in the last quarter of 1992 (not eligible for AB 12) to the CDF of exit rates for youth born in the first quarter of 1993 (partially eligible for AB 12).The hazard rate of exit for these two groups at each continuous age is the same up until October 2011, after which the younger cohort's hazard rate of exit decreases.

Figure A10: Sensitivity of Second Stage Estimates to Bandwidth—Any College

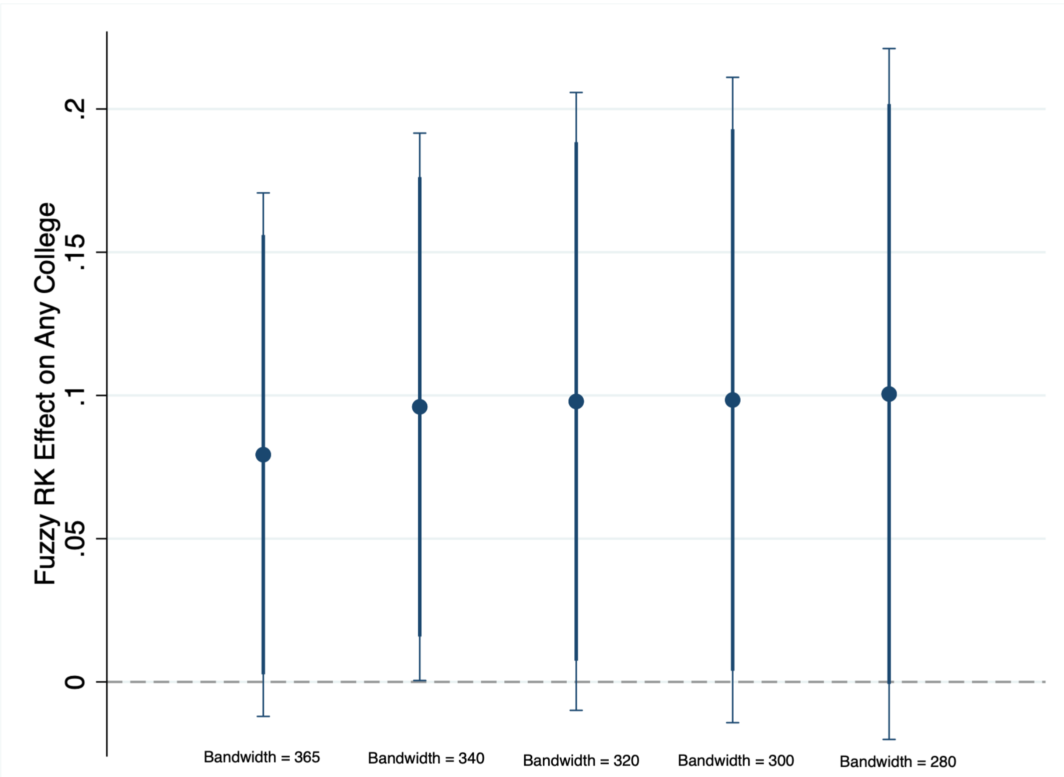


Figure A11: Sensitivity of Second Stage Estimates to Bandwidth—Any Earnings Ages 22–24

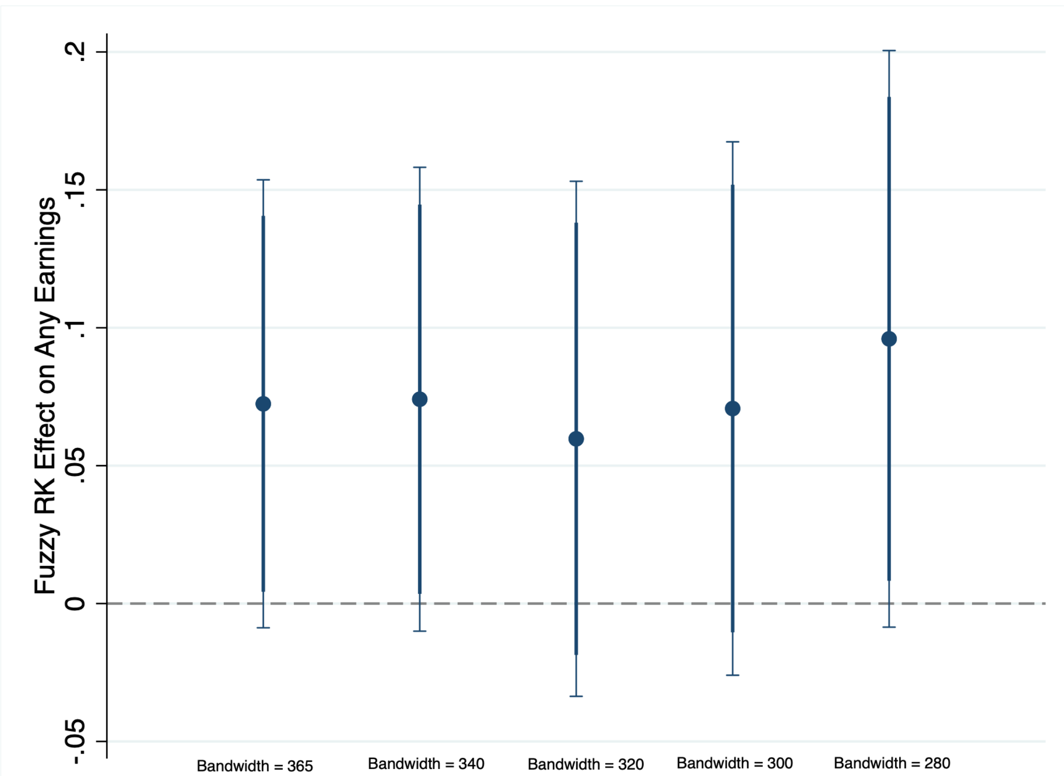


Figure A12: Employment Effects for Youth with Neuropsychological Conditions

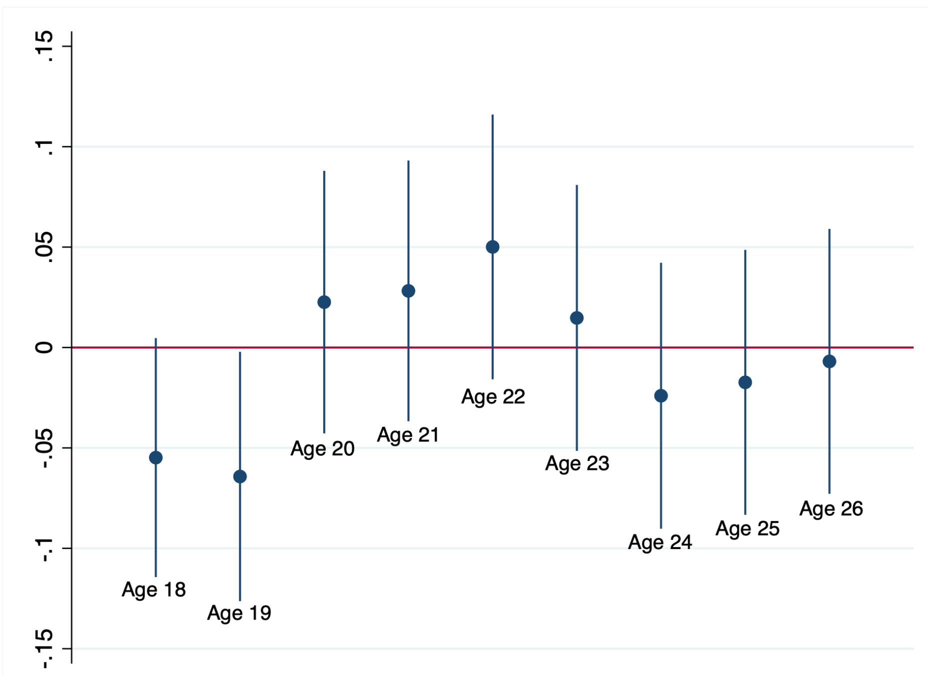



*Figure Notes*: This figure shows the effects of extended foster care eligibility on the probability of formal employment at ages 18 through 26 for youth with neuropsychological conditions.

# Appendix A: Data Appendix

## CWS/CMS

The primary data source used for administrative foster care records is derived from the Child Welfare Services/Case Management System (CWS/CMS). As CWS/CMS was not originally designed for the purpose of social science research, further description of its design is warranted. The development of CWS/CMS began in 1989, when California Senate Bill 370 provided funding for a statewide computer system to replace the decentralized systems that were operated on the county-level, often manually. The California Department of Social Services enumerates eleven specific functions of the system:

> *1. Intake – referral screening, investigation and cross reporting.*
> *2. Client Information – recording and accessing information on clients;*
> *3. Service Delivery – recording of services delivered to clients;*
> *4. Case Management – development of case plans, monitoring service delivery, progress assessment;*
> *5. Placement – placement management and matching of children to placement alternatives;*
> *6. Court Processing – hearing preparation, filing of petitions, generating subpoenas, citations, notices, recording court actions;*
> *7. Caseload – assignment and transfer of cases;*
> *Resource Management – information on resources available for CWS (services 8. providers, county staff resources, etc.)*
> *9. Program Management – caseload, county, program-level information for program management purposes;*
> *10. Adoptions – recording of information for reporting purposes; and 11. Licensing – information on licensees used in placement decisions.*

CWS/CMS was designed to help social workers record and manage data to best serve their clients and improve child welfare. CWS/CMS also collects data to comply with federal reporting requirements, including the Adoption and Foster Care Analysis and Reporting System (AFCARS). These aims have resulted in an incredibly rich data source that relies on accurate reporting by social workers and other dedicated civil servants. It should be noted, however, that CWS/CMS was not designed to collect data for research purposes.

## Match Rates

I start with 25,215 youth born between 1991 and 1995, excluding youth from Los Angeles County. The name and birth date for 24,618 of these youth (97.6%) were sent to the

National Student Clearinghouse to search for college records. The Employment Development Department Data are matched via Social Security Number. 766 youth have missing or invalid SSNs and are dropped from the sample, implying that 97.0% of youth could have been found in the EDD records. Among all youth in sample, 38% are Hispanic. Among the 766 youth who have missing or invalid SSNs, 69% are Hispanic, which is consistent with undocumented youth not having a valid SSN.

# Appendix B: Details of Robustness

## Alternate First Stage IV-DiD Estimates

Because youth can enter and exit care multiple times both before and after age 18, an alternate way to measure the effects of extended foster care take-up is to evaluate the effects of increased total time in foster care induced by AB 12. In these alternate specifications, exit age is replaced with total time in care in the first stage and estimate the following:

$$\text{Years in Care}_{ict} = \alpha + \beta \mathbb{1}(\text{Exit} > 17)_i + \gamma_t + \underbrace{\delta \mathbb{1}(\text{Exit} > 17)_i \times (\text{AB 12})_t}_{\text{Instrument for time in care}} + \mu_c + \omega X_i + \nu_{ict}. \quad (4)$$

As in the primary specification, $\mathbb{1}(\text{Exit} > 17)_i$ is an indicator for whether the youth left care after age 17, $\gamma_t$ is a vector of quarter of birth dummies, $\mathbb{1}(\text{Exit} > 17)_i \times (\text{AB 12})_t$ is the interaction term estimating the effect of AB 12 eligibility on time in care for those leaving care after age 17, $\mu_c$ is a vector of county fixed effects, and $X_i$ is a vector of demographic controls.

Again, increase in time spent in care induced by the policy change (i.e., the difference-in-difference estimator) is used as an instrument for total time in care to estimate the treatment on the treated of extended care on labor market outcomes. This alternate second stage is as follows:

$$\text{Outcome}_{ict} = \lambda + \xi \mathbb{1}(\text{Exit} > 17)_i + \eta_t + \tau \widehat{\text{Years in Care}}_{ict} + \kappa_c + \theta X_i + \epsilon_{ict}. \quad (5)$$

$\mathbb{1}(\text{Exit} > 17)_i$ is an indicator for whether the youth left care after age 17, $\eta_t$ is a vector of quarter of birth dummies, $\text{Years} \hat{\text{in}} \text{ Care}_{ict}$ is the increase in time in foster care induced by AB 12 estimated in the first stage, $\kappa_c$ is a vector of county fixed effects, and $X_i$ a vector of demographic controls.

Table A9 shows the first stage estimates for the effect of AB 12 on total time in foster care. Since AB 12 had no effect on foster care entry and other important determinants of total time in care, it is not surprising that the first stage is noisier than that in the main specifications. The effective F-statistic in the model including all youth is still 148, and the F-statistic is above 30 for all of the primary subgroups except for non-Hispanic white women, which has the smallest sample size. The second stage estimates of the effects of the increase in time in foster care created by AB 12 on college and earnings outcomes are remarkably similar to those estimated in the preferred specifications (see Table A10 through Table A12).

## Restricted Sample with Less Knowledge of AB 12 at Age 17 IV-DiD Estimates

As discussed in Section 4.1.1, I have to assume that AB 12 did not affect which youth leave foster care before or after age 17. If AB 12 influenced youth's decision to stay in care after 17 (instead of 18), the estimated treatment effects would be biased by non-random changes in selection into the group who is in care after age 17. In order to weaken this assumption, I re-estimate the IV-DiD models on a subset of youth who would have turned 17 before foster youth in California would have widely known about the details of AB 12. This restriction means that the $\mathbb{1}(\text{Exit} > 17)_i$ variable is determined prior to the time that the implementation of AB 12 could more plausibly influence youth's decisions.

Although AB 12 was passed in September 2010, many key details of the policy implementation were not settled until October 2011 with the passage of "clean-up legislation," and the AB 12 planning committees did not begin regularly meeting until April 2011 (Mosley and Courtney, 2012). In light of such uncertainty, counties were instructed to not begin informing youth about AB 12 until after the All County Letters outlining the policy were released beginning in October 2011. Counties complied with this instruction, and the Youth Engagement, Training, and Informing Team was "very diligent to not distribute training products prematurely until the All County Letters and County Fiscal Letters [had] been released to ensure that the information [...] disseminated for informing and training [was] accurate" (ACIN I-78-11). That said, the original passing of AB 12 was major state news and several advocacy organizations developed their own training materials—so youth would not have been completely unaware of AB 12 prior to October 2013 (Courtney et al., 2013).

October 2011 still appears to be a "tipping point" of sorts with respect to youth behavior in response to AB 12. The lack of knowledge about AB 12 among foster youth until October 2011 is supported empirically by the exit behavior of youth who turned 18 between the time that AB 12 was signed in September 2010 and October 2011. As discussed in Section 2.2., before AB 12, youth could petition to stay in care for a short period of time after their 18th birthday but not beyond their 19th birthday. Figure A7 compares the cumulative distribution function of exit ages for transition-age youth who turned 18 in the last quarter of 2010 (i.e., were born in the last quarter of 1992 and not eligible for AB 12) to the cumulative distribution function of exit ages for transition-age youth who turned 18 in the third quarter of 2010 (i.e., were born in the third quarter of 1993 and were partially eligible for AB 12), right before youth were notified by county agents about AB 12 starting in October 2011. The two purple dashed lines shows the age that youth born in the third quarter of 1993 would have been in October 2011. What can be seen in this figure is that youth who were ineligible for AB 12 and youth who were partially eligible for AB 12 followed the same trajectory with respect to exits until the age that partially eligible youth would have been at the time that county-driven mass information campaigns began. While noisier, Figures Figure A8 and Figure A9 depict a similar pictures for youth born in the quarter 2 and quarter 1 of 1993, respectively.

I thus define a restricted sample of youth who turned 17 prior to the third quarter of 2011. Table A13 shows the first stage estimates for the restricted sample. The second stage estimates on outcomes, again largely consistent with the primary specification, are presented in Table A14 through Table A16. These results provide further evidence that unobservable compositional changes to the treatment group do not drive results.

## Fuzzy Regression Kink Estimates

As an alternate way of relaxing the assumptions made in the difference-in-differences and IV-DiD models, I also also estimate fuzzy regression kink models, which impose different assumptions on the underlying data generating process.

The kinks in treatment by extended foster care are a result of the way that the policy was implemented. Starting January 1, 2012, foster care eligibility was extended to age 19, and then to age 20 and age 21 on January 1, 2013 and January 1, 2014, respectively. This policy implementation meant that youth born before 1992 had no eligibility for AB 12, youth born after 1993 were fully eligible for AB 12, and youth born in 1993 were partially eligible for AB 12, with the amount of AB 12 funding they received being directly and linearly proportional to their birth date. These kinks, both in exit age from foster care and downstream outcomes is visually apparent in Figure 1.

For example, consider a child born on January 31, 1993. This child would have been eligible for extended foster care between January 1, 2012 and January 31, 2012, at which point they would turn 19 and lose eligibility. Then, when extended foster care was extended to age 20 on January 1, 2013, they would regain eligibility, only to lose it again on their 20th birthday. This process would repeat one more time in 2014 when extended foster care was fully phased in. This child would be eligible for AB 12 for 30*3/(365*3) = 8.2% of the time between ages 18 and 21. In order for this child to stay in care, the county would have to provide funding for the other 91.8% of those three years, at the discretion of the county. Consider another child born later in the year on December 1, 1993. This child would be eligible for AB 12 for 335*3/(365*3) = 91.8% of the time between ages 18 and 21. Much less supplemental funding would be required for this child. A birthday later in the year also implies greater knowledge of the new funding that would be available beginning January 1, 2012.

These policy details motivate a linear parametric model[19] with kinks at birth dates January 1, 1993 and January 1, 1994. For completeness, however, I follow Lee and Lemieux

[19]Although nonparametric estimation of regression discontinuity designs is preferred in most contexts, there are limited circumstances under which parametric estimation may produce more reliable estimates. The bias of parametric regression discontinuity estimates primarily stems from the degree of model specification, a problem that is not ameliorated with larger sample sizes. With nonparametric estimates, by contrast, the bias tends toward zero as the sample size approaches infinity and the optimal bandwidth approaches zero (Lee and Lemieux, 2010). The sample size required for informatively precise estimates is even larger when estimating regression kinks, which require the estimation of derivatives (Card et al, 2015). In this context, the implementation of extended foster care induces two (fuzzy) kinks in treatment, the sample size is limited, and the institutional details behind the policy provide clear guidance on the functional form of the kink. For these reasons, instead of providing noisy nonparametric estimates that are sensitive to local modeling choices, I estimate a more traditional parametric fuzzy regression kink.

(2010) and evaluate the goodness of fit of this model (including the Akaike information criterion and the generalized cross-validation statistic) compared to alternative higher order models as well as a more general model with week of birth dummies. These estimates are presented in Table A17. Higher order polynomials perform better according to the AIC but worse according to the BIC, and models are essentially the same with respect to GCV. Since the linear model is not clearly dominated by alternate more complex specifications and is indicated by the underlying funding function for AB 12, the models are estimated using this specification. In the primary estimates, I use the full 365 days before and after each kink for power, but more restricted bandwidths that use a narrower interval before the first kink and after the second kink provide comparable results. Estimates, shown in Figure A10 and Figure A11 are broadly similar but with a wider confidence interval.

In addition to the models that use both kinks—with and without demographic controls—I estimate models using just the first kink, since there is more ambiguity in the evolution of policy take-up after the second kink where funding is complete. In all models, the first stage effect of the kinks on exit age from foster care is strong (see Table A18). In the second stage fuzzy regression kink models, the effects of extended foster care on education and labor market outcomes are similar to those estimated using the primary empirical strategy. These effects, presented in Table A19, are somewhat larger, but also more nosily estimated, so overall effects are quite comparable.[20]

# Appendix C: Counterfactual Employment Calculations for Probit Model

To obtain estimates of the expected probability of employment for each youth under each alternative, I use the following procedure. First, I use estimate an IV model in the form of the main analysis, but instead of estimating the effect of an additional year in foster care, I estimate the effect of staying in care beyond age 20. Conditional on being in care at age 19, the vast majority of youth stay beyond 20, so this provides a better estimate of predicted employment effects.

[20] Estimates using the kinks also use a slightly modified version of outcomes, restricting to outcomes observed before age 25, since the lack of comparison group in these models means that I am not controlling for cohort-specific effects of the 2020 pandemic.

More specifically, I estimate the following first stage:

$$\mathbb{1}(\text{Exit} > 20)_{ict} = \alpha + \beta \mathbb{1}(\text{Exit} > 17)_i + \gamma_t + \underbrace{\delta \mathbb{1}(\text{Exit} > 17)_i \times (\text{AB } 12)_t}_{\text{Instrument for time in care}} + \mu_c + \omega X_i + \nu_{ict}. \quad (6)$$

Here, $i$ indexes individuals, $c$ indexes counties, and $t$ indexes birth cohorts defined by quarter of birth. The variable $(\text{AB } 12)_t$ is the fraction of an individual's life between ages of 18 and 21 with guaranteed extended care. $\mathbb{1}(\text{Exit} > 17)_i$ is an indicator for whether the youth left care after age 17, $\gamma_t$ is a vector of quarter of birth dummies, $\mathbb{1}(\text{Exit} > 17)_i \times (\text{AB } 12)_t$ is the interaction term estimating the effect of AB 12 eligibility on time in care for those leaving care after age 17, $\mu_c$ is a vector of county fixed effects, and $X_i$ is a vector of demographic controls.

And then the second stage equation is:

$$\text{Any Earnings at } 25_{ict} = \lambda + \xi \mathbb{1}(\text{Exit} > 17)_i + \eta_t + \tau \mathbb{1}(\widehat{\text{Exit} > 2}0)_{ict} + \kappa_c + \theta X_i + \epsilon_{ict}. \quad (7)$$

Here, $\mathbb{1}(\text{Exit} > 17)_i$ is an indicator for whether the youth left care after age 17, $\eta_t$ is a vector of quarter of birth dummies, $\mathbb{1}(\widehat{\text{Exit} > 2}0)_{ict}$ is the predicted likelihood of still being foster care after age 20 induced by AB 12 estimated in the first stage, $\kappa_c$ is a vector of county fixed effects, and $X_i$ a vector of demographic controls.

The, I can use this estimated equation to get predicted earnings for each individual if they stay in care beyond their 20th birthday and if they do not stay in care beyond their 20th birthday.

Next, I need a way to account for differential earnings by whether or not youth are working or in school while in extended foster care. Youth's activities in extended foster care are endogenous to their employment potential, creating a selection one problem. I can only observe what youth do in extended care for those who are actually in care, which generates another selection problem. I can address the first selection problem with an instrumental variables approach, but the second selection requires imposing an assumption about how out of sample youth would respond to the IV used to address the first selection problem. More specifically, I address selection into not working or being in school (NEET) while in extended foster care by instrumenting for this with the leave-one-estimate of the activities of other youth supervised by the same social worker and with county level averages of the proportion of youth in extended foster care who are NEETs. Since I use county-level averages to strengthen the first stage, I use region fixed effects, which are groups of counties, instead of county fixed effects. For youth who are actually in extended care after their 20th birthday, I estimate the following first stage:

$$\text{NEET}_{ict} = \alpha + \rho\text{caseworker LOO}_i + \zeta\text{Prop. NEET}_c + \gamma_t + \mu_r + \omega X_i + \nu_{ict}. \tag{8}$$

And the second stage is:

$$\text{Any Earnings at } 25_{ict} = \lambda + \tau\widehat{\text{NEET}}_{ict} + \eta_t + \kappa_r + \theta X_i + \epsilon_{ict}. \tag{9}$$

Then for each youth in the sample, I can predict the difference in likelihood of employment at age 25 when the youth is working or in school in extended care versus when they are not. In order to get the predictions for the whole sample, however, I need to assume that youth who actually left care before age 20 would respond to the instruments in a similar way to those for those in care in the counterfactual that they did stay in care. This is a relatively strong assumption but one that is likely second order in the full model.

To get the predicted employment for each alternative, I then weight this expected difference by the actual share of NEETs in the sample. This gives a system of two equations and two unknowns from which to solve for the predicted employment probabilities.