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A Benefit-Cost Analysis of the Tulsa Universal Pre-K Program

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ABSTRACT

In this paper, benefits and costs are estimated for a universal pre-K program, provided by Tulsa Public Schools. Benefits are derived from estimated effects of Tulsa pre-K on retention by grade 9. Retention effects are projected to dollar benefits from future earnings increases and crime reductions. Based on these estimates, Tulsa pre-K has benefits that exceed costs by about 2-to-1. This benefit cost ratio is far less than the much higher benefit-cost ratios (ranging from 8-to-1 to 16-to-1) for more targeted and intensive pre-K programs, such as Perry Preschool and the Chicago Child-Parent Center (CPC) program. Comparing benefit-cost results from different studies suggests that our more modest estimates are due to two factors: 1) smaller percentage effects of pre-K on future earnings and crime in Tulsa than in Perry and CPC, and 2) smaller baseline crime rates in Tulsa than in the Perry and CPC comparison groups.

JEL Classification Codes: I28, I26, H43

Key Words: Universal pre-K, benefit-cost analysis, grade retention, crime reduction effects of education, increased earnings effects of education

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A BENEFIT-COST ANALYSIS OF TULSA'S PRE-K PROGRAM

In recent years, many states and some local governments (e.g., New York City, Seattle, San Antonio) have significantly expanded pre-K funding. Are these short-term costs justified by long-term benefits? In this paper, we address this question by predicting the long-term benefits of the universal pre-K program in Tulsa, Oklahoma, through earnings increases and crime reductions, and comparing these benefits to the intervention's costs.

We focus on grade retention as a mechanism by which pre-K alters long-term outcomes. The connections are clear: as we show, Tulsa pre-K reduces grade retention, and grade retention reduces earnings and increases crime. Grade retention is also an indicator for both "hard skills" (cognitive skills) and "soft skills" (social skills and other personality traits). If both are important, then grade retention is perhaps a more versatile indicator than most.

Our new Tulsa pre-K estimates are combined with national data for this benefit-cost analysis (BCA). We then compare our results with previous BCAs of pre-K: Tulsa pre-K, projected from kindergarten test scores (Bartik, Gormley, and Adelstein 2012); the Perry Preschool program (Belfield et al. 2006; Heckman et al. 2010); the Chicago Child-Parent Centers (CPC) program (Reynolds, Temple, White et al. 2011); and a California universal pre-K proposal (Karoly and Bigelow 2005).

In our new Tulsa analysis, benefits exceed costs, but only modestly. Our benefit-cost ratios are much lower than more intensive and targeted pre-K programs, such as Perry and Chicago CPC. These lower ratios are due in part to Tulsa pre-K having lower predicted percentage effects on adult outcomes compared to Perry and CPC. In addition, Perry and CPC

targeted disadvantaged groups with high crime rates decades ago, allowing crime reduction benefits to be larger.

The next section of this paper reviews prior research. Our new estimates are then presented for Tulsa pre-K's effects on grade retention. Our methodologies are described for projecting grade retention effects on earnings increases and crime reductions, as well as for measuring program costs. Our overall BCA is then summarized and compared with previous studies. The conclusion considers implications for future research and policy.

LITERATURE REVIEW

Evidence on the Benefits of Preschool Participation

Many studies have examined the short-term effects of "hothouse" preschool programs (the Perry Preschool and Abecedarian programs), Head Start, and state pre-K programs. These studies, which include experimental or quasi-experimental evidence, have converged on a firm conclusion—that high-quality preschool programs improve school readiness, sometimes dramatically (Camilli et al. 2010; Gormley 2007; Yoshikawa et al. 2013).

Most pre-K research on school readiness effects has focused on disadvantaged students because many programs target this group. Some programs have shown solid test score gains (Frede et al. 2007; Peisner-Feinberg, et al. 2015; Puma et al. 2005) while others have shown stunning gains (Ramey and Campbell 1984; Reynolds 2000; Weikart, Bond, and McNeil 1978). A minority of studies have examined universal pre-K in Tulsa (Gormley et al. 2005), Boston (Weiland and Yoshikawa 2013), Georgia (Henry et al. 2003), and Florida (Bassok and Miller 2014). The Tulsa and Boston studies both find that pre-K enhances school readiness not just for

disadvantaged students but also for middle-class students; the Georgia and Florida studies do not address this issue.

A few studies examine pre-K's long-term effects and reach three conclusions: first, short-term effects on test scores diminish over time; second, short-term effects on test scores do not disappear over time; and third, positive effects for other outcomes are discernible for both adolescents and adults among the earliest high-intensity programs. Studies in New Jersey (Barnett et al. 2013), Texas (Andrews et al. 2012), Florida (Bassok and Miller 2014), North Carolina (Dodge et al. 2014; Peisner-Feinberg et al. 2015), and Tulsa (Hill, Gormley, and Adelstein 2015) have found that pre-K participants performed better in elementary school than nonparticipants, including higher test scores. An exception to these findings is a Tennessee study (Lipsey, Farran, and Hofer 2015), which finds no lasting benefits from pre-K after initial gains in kindergarten. Quasi-experimental studies of Head Start find evidence of lasting benefits, including increased earnings and reduced crime, though more for some subgroups than others (Deming 2009; Garces, Thomas, and Currie 2002). An experimental study of Head Start finds fade-out of short-term positive effects after kindergarten (Puma et al. 2010), although this conclusion has been challenged because many members of the control group also participated in preschool (Feller et al. 2016; Kline and Walters 2015). However, there is limited evidence of the effects of universal pre-K on adult outcomes because such programs are relatively new and scarce.

Long-term studies of the earliest and most intense pre-K programs point to diverse benefits. The Perry Preschool study finds that treatment group members were more likely to graduate from high school, have a job, have higher earnings, and own a home, and were less likely to receive cash welfare and be arrested (Schweinhart et al. 2005). The Abecedarian study

finds that treatment group adults were more likely to graduate from high school, attend a four-year college, and have a job (Campbell et al. 2012), but no crime or substance abuse effects were detected. The Chicago CPC study finds that program participants were more likely to graduate from high school, have a higher income, and have health insurance, and were less likely to engage in substance use and be arrested (Reynolds, Temple, Ou et al 2011). However, these programs are not characteristic of contemporary pre-K programs, nor are the counterfactuals like today's due to the growing availability of social supports for four-year-old children.

Finally, many studies have found a negative relationship between pre-K and grade retention, a precedent for this paper. The Perry Preschool study (Schweinhart et al. 2005), the Abecedarian Project (Campbell and Ramey 1995; Campbell et al. 2002), and the CPC study (Reynolds et al. 2007; Reynolds et al 2001) all find that pre-K reduced grade retention. Comparisons of Head Start participants and sibling nonparticipants have found that Head Start reduced grade retention (Deming 2009; Garces, Thomas, and Currie 2002). Other more recent studies have also found that pre-K lowers grade retention, as of elementary school (Andrews et al. 2012; Barnett et al. 2013; Bassok and Miller 2014; Dodge et al. 2014) or middle school (Gormley, Phillips, and Anderson 2016).

In sum, evidence suggests a link between pre-K and favorable elementary, middle school, and adult outcomes. However, most studies have focused on targeted programs, not universal programs. Because disadvantaged students have fewer resources outside of preschool than middle-class students, more modest impacts might be expected from universal programs, which serve a mix of students. Our study helps fill this gap in the literature by linking universal pre-K to long-term effects via its middle-term effects on grade retention.

Previous Benefit-Cost Analyses of Pre-K Programs

A growing number of studies use pre-K's estimated effects to compare program benefits and costs. Most pre-K benefits are due to earnings increases and crime reductions. BCAs of Perry Preschool from Belfield et al. (2006) and Heckman et al. (2010) show benefit-cost ratios of 16-to-1 and 8.5-to-1, with over 90 percent of benefits due to earnings increases or crime reductions. The BCA of CPC from Reynolds et al. (2002) finds a benefit-cost ratio of about 11-to-1, with over 75 percent of benefits due to earnings increases or crime reductions.

These jaw-dropping benefit-cost ratios may be of limited relevance to today's programs. Perry Preschool and CPC were well-funded and well-staffed and included home visits or other efforts to engage parents. Most garden-variety pre-K programs do not reach such standards (Mashburn et al. 2008). Also, the counterfactual for comparison group students has changed. In the 1960s and 1970s, child care subsidies for disadvantaged children were rare and Head Start enrollments were low. Today, child care subsidies are more common and Head Start enrollments have climbed. Today, unlike three to five decades ago, comparison group children are more likely to have educational experiences as four-year-olds. In short, the educational exposure gap of treatment versus control children has narrowed over time.

Fewer BCAs have examined large-scale contemporary pre-K programs, including universal pre-K. Karoly and Bigelow (2005) estimate the costs and benefits if California were to adopt high-quality universal pre-K and conclude that the benefits would outweigh costs by 3-to-1. Bartik, Gormley, and Adelstein (2012) examine earnings benefits of Tulsa universal pre-K for different income groups by using kindergarten test scores to predict future earnings based on Chetty et al. (2011). Earnings benefits outweighed program costs by 3- or 4-to-1 for all income groups. The Washington State Institute for Public Policy (2014) concludes from a research

review that benefits outweigh costs by 4-to-1 for state and local programs (including both contemporary programs and earlier programs such as Perry and Abecedarian), and by 2.5-to-1 for Head Start. The evidence suggests that current, larger-scale pre-K programs have benefits exceeding costs but by much less than earlier, smaller programs of exceptionally high quality.

EFFECTS OF TULSA PRE-K PROGRAM ON GRADE RETENTION

The Tulsa Public Schools (TPS) pre-K program is universal, so all four-year-olds whose families reside in the district can enroll for free. Tulsa pre-K is high quality: every teacher has a BA degree, is early-childhood certified, and is on the same pay scale as other TPS teachers. Classroom observations suggest that, compared to 11 other states, Tulsa pre-K instructional quality is higher, with teachers performing better at providing students with feedback, language modeling, and higher-order concept development (Phillips, Gormley, and Lowenstein 2009). Benefits of Tulsa pre-K extend beyond academic achievement to social-emotional skills (Gormley et al. 2011).

Our BCA of Tulsa pre-K relies on its estimated effects on grade retention. These estimates are presented in more detail elsewhere (Gormley, Phillips, and Anderson 2016) but are summarized here.

Tulsa Sample and Model

Our sample was derived from the fall 2006 TPS kindergarten entrants (N = 4,033). The dependent variable was whether these students were retained in grade as of 2015–2016, when students typically enter ninth grade. This grade retention outcome was explained using a linear regression model, in which the key independent variable indicated whether a student was

enrolled in Tulsa pre-K in 2005–2006. Students who attended Head Start were excluded from the estimates as they were considered their own treatment group. Of the original sample of students, we were able to track 3,045 longitudinally through state administrative records, with 1,283 having attended Tulsa pre-K and 1,410 not (the remainder were in Head Start).

Observable variables were controlled for using propensity score weighting: independent variables were used to predict the probability of a student attending pre-K, and then propensity score weights were formed to weight the control group observations so that the control group was as similar as possible to the treatment group in the predicted probability of attending pre-K. Independent variables were derived from school administrative records, parent surveys, and the Census Bureau. They included gender, race, lunch status, mother's education and marital status when the child was in kindergarten, whether or not the child lived with the father, an indicator of current school district (with TPS as the omitted referent), and neighborhood median income.

Using these propensity score weights, we estimated the effect of Tulsa pre-K on grade retention using linear probability regression models. We ran models for all pre-K students together and by gender, race/ethnicity (white, black, and Hispanic), and free lunch status (free, reduced, and paid). Models were also run by full- and half-day pre-K and then by subgroup (see Appendix A). However, estimates by both subgroup and full-day versus half-day pre-K are not reported in the main paper due to limited sample sizes.

Pre-K and Grade Retention: Results

The results indicate that Tulsa pre-K reduced grade retention, both overall and for most subgroups (Table 1). Effects were stronger for more disadvantaged groups, such as blacks,

Hispanics, and lower income groups. Effects were also stronger for males. However, effects only occurred for full-day pre-K (the majority of enrollees) and not for half-day.

ESTIMATING THE EFFECTS OF GRADE RETENTION ON EARNINGS AND CRIME

The next step in assessing the effects of Tulsa pre-K on earnings and crime, as mediated through grade retention, is to estimate the relationship between grade retention and these long-term outcomes. Previous research suggests grade retention has some impacts on earnings and crime via its effects on educational attainment (Jacob and Lefgren 2009). However, research is limited on the direct evidence between grade retention and these outcomes. Eide and Showalter (2001) find a negative relationship between retention and earnings, though Babcock and Bedard (2011) determine that retention during early grades (first and second) actually led to wage increases. Further, retained students are arrested more than nonretained students, but McCoy and Reynolds (1999) find no relationship between grade retention and delinquency at age 14. For this project, we chose to follow the direct approach and expand upon the existing research.

Our Tulsa data cannot be used to examine long-term earnings and crime outcomes, as the 2005–2006 Tulsa pre-K cohort is in ninth grade in 2015–2016. Therefore, we use data from the 1997 National Longitudinal Survey of Youth (NLSY97), which allows for direct estimates of how retention through eighth grade affects earnings and crime in young adulthood. We then project earnings and crime over an individual's lifetime.

NLSY97 Data and Model

The NLSY97 is a longitudinal survey of 8,984 youth aged 12–17 in 1997, with an over-sample of black and Hispanic youth, and with follow-up surveys through 2013 to date. Our

analysis relates annual earnings and number of crimes committed to whether an individual had been retained in grade, both for the overall NLSY97 sample and for groups defined by gender, race/ethnicity, and family income status at ages 12–17.

The dependent variables include, for each individual: 1) earnings for each age separately from 18 to 31; 2) the number of violent crimes committed between 1998 and 2013; and 3) the number of property crimes committed over the same time period.[1] Earnings are the self-reported total income from wages, salary, commissions, or tips from all jobs, before taxes. Crimes committed are based on self-reports of arrests leading to police charges between 1998 and 2013. Violent crimes include assault, rape, murder, and robbery. Property crimes include burglary and theft.

The independent variable of interest is whether a student has been retained at least once from first through eighth grade. The models also include other control variables, including gender, race/ethnicity, urban residence, birth cohort, English language learner status, free and reduced-price lunch status, whether from a single mother or two-parent family, and the average highest grade completed of the respondent's parents.

An ordinary least squares model is used to predict the relationship between grade retention and earnings at each age, the number of violent crimes committed, and the number of property crimes committed. To check robustness, Poisson models are run for the skewed crime outcomes, but these estimates reveal no differences in statistical or substantive significance. We ran models for youth overall, and by gender, race/ethnicity (white, black, and Hispanic), and

[1] Because crime is a rarer event for individuals than obtaining earnings, obtaining any significant results for crime requites pooling data for individuals over multiple ages, whereas earnings determinants can be readily examined on an annual basis.

lunch status (free, reduced, and paid). Overall sample sizes for the earnings regressions range between 3,866 at age 18 to 1,363 at age 31; the sample size for the crime regressions is 4,324. The grade retention rate through grade eight in the overall sample averages 15 percent. Sample size reduction occurred due to missing data for dependent and independent variables, a decline in response rate to 80 percent by 2013, and later ages having data only for older birth cohorts.

Effects of Grade Retention on Earnings and Crime: Summary of Results

As expected, grade retention is associated with lower earnings for each age between 18 and 31. Negative effects for most ages and subgroups are statistically significant; for the 126 grade retention effects on earnings estimated (9 groups by 14 ages), 104 are statistically significant at least at the 5 percent confidence level. Grade retention also is associated with higher crime, although crime effects are often statistically insignificant. For the overall sample, grade retention was significantly associated (5 percent level) with increased violent and property crime. For violent crime, grade retention's positive association was significant for white youth and marginally significant (10 percent level) for the male and paid lunch groups. For property crime, grade retention's positive association was only marginally significant for males. See Appendix B for full results.

Figures 1 and 2 interpret the earnings results. These figures convert estimated dollar effects on earnings into percentage effects at mean earnings of each group/age. For the overall sample, Figure 1 shows percentage earnings effects by age. The underlying dollar earnings effects are all statistically significant at the 1 percent level. Grade retention reduces overall earnings by 20–30 percent, with no obvious trend. For each group, Figure 2 shows the average percentage effect (simple average over all ages from 18 to 31). For each group, grade retention

reduces earnings by 15–35 percent. Percentage earnings effects are somewhat greater for more disadvantaged groups, such as racial minorities and lower-income groups.

Figure 3 shows the effects of retention on crime, overall and by group. To aid in interpretation, estimated effects on self-reported crimes from 1998–2013 are converted to percentage effects at the mean of each group's reported crimes. For the overall sample, grade retention increases self-reported arrests by 60–70 percent. This is an increase of about 0.08 arrests per person separately for both violent and property crimes. Percentage effects of grade retention on crime are somewhat larger for more advantaged groups, such as whites and the paid lunch group; this is also true for the raw crime numbers. Percentage effects on crime are similar for males and females, although this corresponds to a larger absolute effect for males than females (e.g., an increase of 0.1 arrests for violent crime of males versus 0.04 for females).

While these regressions cannot prove that grade retention causally reduces earnings and increases crime, these estimates are plausible. Holding constant other important determinants of earnings and crime, grade retention seems to produce worse outcomes in adulthood.

PROGRAM BENEFITS

The NLSY97 results only show earnings and crime effects of grade retention through early adulthood. For benefit-cost analysis, effects must be projected over a lifetime. This section discusses these projections. In addition, the crime reduction benefits require one additional step: assigning crime reductions a dollar value, which we also discuss in this section.

Predicting Earnings Benefits

To project NLSY97 retention effects on young adult earnings to a lifetime, we first determine baseline lifetime earnings for each group, using data from the American Community Survey (ACS), 2009–2013, for persons in the Tulsa metropolitan area. Using Tulsa ACS data, adjusted to 2013 national prices, age-earnings profiles are calculated for the overall sample, and for the gender and racial groups.[2] Mean earnings by single year of age for ages 18–79 are calculated for the overall sample, separately for men and women, and for three racial groups: white non-Hispanic, black non-Hispanic, and Hispanic.

However, cross-sectional ACS data cannot differentiate how the age-earnings profiles vary by family income of an individual when growing up. Individuals who grew up in a lower-income family would tend to have lower career earnings. To project how future earnings vary with family income background, ACS data on Tulsa earnings are combined with NLSY97 data on early career earnings and past family income. In NLSY97, we calculate for individuals at ages 12–17 whether their families' incomes would qualify them for a free or reduced-price lunch or put them in the paid lunch group. For these three family income groups, as well as for the overall NLSY97 sample, we calculate average earnings for each single year of age from 18 to 31. These group and overall means are used to calculate the ratio of each group's earnings to the overall average, for each year of age from 18 to 31.

We assume these NLSY97-estimated ratios show how earnings vary by an individual's family income background in the ACS. Therefore, we take average overall earnings in the ACS,

[2] The 2009–2013 ACS data are already adjusted to 2013 prices, which implicitly means 2013 Tulsa prices. We adjust to national prices by using data from the U.S. Bureau of Economic Analysis (2016) on regional price parities.

for each year of age from 18 to 31 and multiply these by the NLSY-derived ratios to predict earnings for the different income background groups. For ages beyond 31, we project earnings by multiplying the ACS overall earnings for each age from 32 to 79 by the average NLSY97 ratio of ages 25–31 for each income background group (65 percent for free lunch, 85 percent for reduced lunch, 117 percent for paid lunch). Averaging over ages 25–31 reduces estimate volatility; starting with age 25 reflects that more individuals have completed education at 25 than earlier ages.

These age-earnings profiles only show how earnings vary during a given time period (2009–2013). However, our BCA of Tulsa pre-K needs to adjust for mortality since age 4 and secular future earnings increases. To adjust for mortality, we use U.S. Life Tables from 2011 (Arias 2015). We calculate ratios of the expected number of persons alive at each age to the persons alive at age 4.[3] We adopt the midrange assumption of the Social Security Trustees that the long-run growth in U.S. real wages will be 1.17 percent per year over the next 75 years (U.S. Social Security Administration, 2015).[4]

To compare earnings benefits with pre-K costs at age 4, we discount future earnings back to age 4. We use a 3 percent annual real discount rate, which is commonly used in BCAs.[5]

Given recent trends, future earnings growth of 1.17 percent could be questioned. However, what matters for comparing the present value of earnings benefits with costs is the difference between the discount rate and the earnings growth rate. It is hard to rationalize a real

[3] Such projections only reflect current medical technologies, and so may overstate mortality. Life Tables are not available for family background so we use black non-Hispanic survival rates for the free and reduced-price lunch groups, and white non-Hispanic survival rates for the full price lunch group.

[4] Because our sample will be age 18 in the year 2019, we adjust our 2013 age 18 ACS data by multiplying by 1.0117 raised to the sixth power, and adjust each subsequent age *x* by 1.0117 raised to *x* minus 12.

[5] See discussion of discount rate issues as they relate to early childhood programs of Bartik (2011, pp. 177–181), and also see Moore et al. (2004).

social discount rate as great as 3 percent if future earnings are not increasing by at least 1 percent per year. Discounting of future earnings is justified because the future will be wealthier, reducing the value of future income increases. If real earnings will be stagnant, the discount rate should be lowered below 3 percent, with little net effect on the present value of earnings.[6]

To calculate how retention affects lifetime earnings, the age-earnings profiles of each group are combined with the percentage effects of retention on earnings, from the previous section. For ages 18–31, we multiply the NLSY97-percentage effect times the adjusted ACS earnings for that age/group to get the retention effect for that age/group. For ages 32–79, we multiply the adjusted ACS earnings for that age/group by that group's NLSY97-derived average percentage effects ages 25–31. When discounted and summed over all ages, this calculation gives the present value reduction in lifetime earnings from grade retention.

This present value of the earnings reduction from being retained in grade for each group is then multiplied by the estimated pre-K effects on grade retention to give the estimated pre-K effects on the present value of earnings. This gives us the earnings benefits of Tulsa pre-K participation mediated through its effects on grade retention, overall and by group.

Predicting Crime Benefits

Our estimates in previous sections showed negative effects of pre-K on grade retention, and positive effects of retention on crime in young adulthood. To translate these estimates into

[6] As discussed in Bartik (2011, Appendix 7A), the Ramsey equation for the optimal real discount rate is that it should equal the sum of the "pure" rate of time preference, plus the income growth rate times the elasticity of utility with respect to income. Moore et al. (2004) argue for a pure rate of time preference of 1 percent and an elasticity of utility with respect to income of 1. Under these assumptions, with an annual income increase of 1.17 percent, the discount rate should be 2.17 percent. If we instead assume zero secular income increases, the discount rate should be lowered to 1 percent. The lower discount rate would just offset the lower secular earnings increase.

dollar benefits of crime reduction, we need two more pieces of information: baseline crime rates at different ages and the social costs per crime committed.

To project baseline Tulsa crime rates, we use data from the 2012 FBI Uniform Crime Reports (UCR), which reports data on arrests and total crimes reported, broken down by type of crime: murder, rape, assault, robbery, theft and burglary. The first four are "violent crimes" and the last two are "property crimes." The arrest data are further broken down by age and gender and are used to allocate crime counts for each type of crime by age and gender. The crime counts are then combined with census population data to calculate crime rates by age, gender, and type of crime.[7] As expected, crime rates are highest for persons in their late teens and early twenties, and are much higher for males than females.

To derive crime rates by race and age, overall crime counts by age are multiplied by relative Tulsa crime rates by race. Relative crime rates for whites and blacks are calculated from the Tulsa UCR data on arrests and Census population data.[8] The UCR does not report arrests by Hispanic status. Hispanic crime rates are assumed to be the same as the overall population.[9]

To calculate crime rates by age for the different family income background groups, the overall age-crime profiles, calculated above, are multiplied by relative arrest rates by family income background group, derived from the NLSY97. Using NLSY97 data, relative arrest rates are calculated from 1998 to 2013, for violent and property crimes separately, broken down by the

[7] Because both the UCR data and the census population data are reported by age ranges, the calculation of crime rates by single years of age requires some interpolation.

[8] The reported arrest data by race are used to allocate crimes by race and age, and individuals were counted in the census data as white if they indicated no other race, and as black even if they included multiple races.

[9] There is controversy but little data about Hispanic crime rates. The available evidence suggests that first-generation immigrants have lower crime rates, but that crime rates converge to the overall U.S. population for subsequent generations. See Brame et al. (2014), Morin (2013), and Steffensmeier et al. (2010).

individual's family income at ages 12–17.[10] Crime rates are 50–75 percent higher for individuals from a free lunch background than for the average individual, and about half as high for individuals from a paid lunch background than for the average individual; individuals from a reduced-price lunch background are close to the overall average.

For the benefit-cost calculations, these crime rate figures need to be adjusted for mortality since age 4. We use the same mortality assumptions as was done for earnings.

Social costs per each type of crime are chosen to be in the middle range of the research literature. This middle range relies on McCollister, French, and Fang (2010, Table 1), which reports crime cost estimates from six other studies and provides its own estimates. From these seven studies, we choose the median social cost for each type of crime.[11]

To calculate present values of crime costs by group, we first multiply the expected number of each type of crime per person from each subgroup for each single year of age, adjusted for mortality, by the median social cost of that crime. These costs are discounted by 3 percent back to age 4.

For each group, present values of crime costs by type are summed into the broader violent crime and property crime categories. For violent crime and property crime separately, and for each group, percentage effects of Tulsa pre-K are estimated by multiplying effects of pre-K on retention by percentage effects of retention on crime. The present value of expected future crimes in each category for a typical 4-year-old in each group is then multiplied by the

[10] Arrest rates for family income groups are used to calculate ratios of income group arrest rates to the Tulsa average using census data on the proportion of the city of Tulsa population from ages 12 to 17 in each income group. The relative "violent crime" arrest rates are applied to the overall crime-age distribution of the four "violent crimes," and the relative "property crime" arrest rates to the overall crime-age distribution of the two "property crimes."

[11] In Appendix C, the sensitivity of our benefit-cost calculations to alternative crime costs is considered.

percentage effects of crime reduction for that category and group. We then sum the violent and property crime categories. This yields estimated benefits of reduced crime from participation in the Tulsa pre-K program, mediated through grade retention, for the overall sample and subgroups.

PROGRAM COSTS

Our BCA requires an estimate of Tulsa pre-K program costs at all levels of government. We estimate state aid to Tulsa pre-K by applying the state aid formula to the number of pre-K children and to those students with characteristics that triggered added state aid, such as lunch price status and English language learner status. We then add in federal Title I funds used for Tulsa pre-K. Next, we conferred with the TPS treasurer, who estimated that in 2005–2006 TPS contributed $0.87 in local funds for every $1.00 of state aid (Stoeppelwerth 2011).

Conversations with TPS officials also suggested no differences in pre-K spending for children from different income groups. More low-income children were in full-day pre-K, but for a given type of program, there are believed to be no systematic differences in pre-K spending across income groups. TPS officials asserted that the costs of full-day pre-K were simply twice the costs of half-day pre-K. The resulting total cost of Tulsa pre-K in the 2005–2006 school year, in 2013 U.S. dollars, is $5,544 for half-day pre-K and $11,089 for full-day pre-K. These half-day and full-day cost figures are directly used in our BCA of half-day and full-day pre-K. For the overall sample, and for the different subgroups, the cost figure per child is adjusted based on the observed mix of half-day versus full-day enrollment.

BENEFIT-COST ANALYSIS RESULTS

Main Results

Combining the benefit and cost results from the previous sections gives us BCA calculations, shown in Table 2. Tulsa pre-K for most groups appears to pass a benefit-cost test, but only modestly. The overall program benefit-cost ratio is 1.89. This contrasts with studies of older, smaller, targeted programs with benefit-cost ratios as great as 16-to-1 (Heckman et al. 2010).

Earnings benefits are much greater than crime reduction benefits. For example, in the overall results, the ratio of earnings benefits to crime reduction benefits is over 4-to-1. This contrasts with other studies, where crime reduction benefits are much larger.

The results in Table 2 provide mixed evidence as to whether pre-K's net benefits are more favorable for the disadvantaged. Benefit-cost ratios and net benefits tend to be higher for pre-K participants from lower income groups compared to the highest income group. They are also relatively high for Hispanics. However, the ratios are somewhat higher for the average white pre-K participant than for the average black participant. Net benefits are much greater for males than for females.

Results are more favorable for full-day pre-K than for half-day pre-K. Estimated net benefits of half-day pre-K are negative. Note that overall results are not an average of the full-day and half-day results. This occurs because of differences in the matching algorithm when matches are done separately for full-day versus half-day, versus all pre-K participants together.

Comparing Overall Results to Previous Studies

Comparisons with previous studies are in Table 3. Our current results are compared with an earlier study of Tulsa pre-K, which projected benefits based on kindergarten test scores (Bartik, Gormley, and Adelstein 2012). These Tulsa results are compared with two BCAs of Perry Preschool, Belfield et al. (2006) and Heckman et al. (2010); a BCA of Chicago CPC (Reynolds, Temple, White et al. 2011); and a projected BCA for a proposed California universal pre-K program (Karoly and Bigelow 2005).

The Perry and CPC studies have much higher benefit-cost ratios. Our current study has an overall benefit-cost ratio for Tulsa of 1.89; the prior Tulsa study had a benefit-cost ratio of 3.20. In contrast, the Perry Preschool benefit-cost ratio is over 8-to-1 in Heckman et al. (2010) and over 16-to-1 in Belfield et al. (2006). For Chicago CPC, the benefit-cost ratio is over 11-to-1.

In the California study of universal pre-K, overall net benefits and the benefit-cost ratio are modestly higher than the current study. Benefits are similar in the current study and the California study, both focusing on universal programs. However, the California study has a lower cost per student because it assumes that these benefits can be achieved with half-day pre-K, whereas our current study suggests that high benefits require full-day pre-K.

In sum, our current study suggests real but modest support for Tulsa pre-K as a good investment. In contrast, the prior studies of Perry and CPC suggest extraordinarily high rates of return. The California study gets stronger results by assuming half-day pre-K works.

Differences in benefits between the current study and previous the Tulsa, Perry, and CPC studies are in part due to earnings benefits. Earnings benefits in the CPC and the prior Tulsa

study are twice as great as the current study. For Perry, overall earnings benefits are five or six times as great as the current study.

However, most differences between the current study and the Perry and CPC studies are due to crime reduction benefits. CPC crime reduction benefits are over 15 times greater than the current study, and Perry crime reduction benefits are over 25 times as great (Heckman et al. 2010) or over 70 times as great (Belfield et al. 2006).

What is behind these large differences across studies? In addition, why do the Karoly and Bigelow (2005) study of universal pre-K and the current study of universal pre-K get such similar benefit results? The next subsections drill a little deeper.

Comparing Earnings Benefits across Studies

In Table 4, for each study overall earnings benefits are broken into two components: the comparison group's baseline present value of earnings and the pre-K program's percentage effect on earnings. Earnings benefits equal these two components' mathematical product.

The current study has baseline earnings that are somewhat larger compared to other studies. If percentage earnings effects were similar, the current study would have *larger* earnings benefits. Instead, percentage earnings effects are much larger in most other studies, leading to larger dollar earnings benefits. Overall percentage earnings effects are 2 percent in the current study, 7 percent in the previous Tulsa study, 8 percent for CPC, and 17–25 percent for Perry.

One exception is Karoly and Bigelow (2005). This study scales back the CPC study's estimates so that universal pre-K has 23 percent of CPC's effects. With this scale-back, the percentage earnings effects are similar to the current study, where lower percentage effects on earnings come from estimated effects on retention, not by assumption.

Baseline earnings differentials across studies are due to different target groups and assumptions. Perry and CPC target more disadvantaged groups than the universal Tulsa pre-K program, which lowers baseline earnings. In addition, baseline earnings for Perry and CPC are mostly in the past, whereas the current Tulsa results project baseline earnings into a future with earnings growth.[12] The prior Tulsa study assumed zero secular growth in future earnings. Karoly and Bigelow assume lower annual earnings growth at 0.5 percent.

Different percent earnings effects could be due to differences in programs and the source of estimates. Perry is the most expensive program, costing over twice as much as Tulsa or CPC. These extra dollars buy smaller class sizes, more trained teachers, and up to two years of services versus one year. Perhaps extra dollars also lead to greater effects. CPC students generally participated in the program for a longer time period than was true in Tulsa; many CPC children attended two years of pre-K and had elementary school supports (Reynold, Temple, White et al. 2011).

Another explanation is differences in studies' estimation procedures. The Perry studies rely on direct estimates of earnings effects and educational attainment effects through age 40; the CPC studies rely on educational attainment and earnings effects through age 26. In contrast, the current study relies on effects on retention until grade 9, and the prior Tulsa study relied on kindergarten test scores. Earnings benefits may be transmitted through other areas than just grade retention. If so, our estimates of Tulsa pre-K's long-term impacts may be conservative. Better estimates of Tulsa pre-K's effects on adult outcomes require data on such outcomes.

[12] Going in the other direction, Perry and CPC studies include fringes, whereas the two Tulsa studies do not. Apparently, the focus on the past and the disadvantaged target groups outweigh the inclusion of fringes.

Comparing Crime Reduction Benefits across Studies

For the various studies, Table 5 breaks down crime reduction benefits into two components: average lifetime crime costs for the comparison group and program-induced percentage reduction in crime costs. We explore two issues: why are crime reduction benefits in the present study so similar to Karoly and Bigelow (2005), and why are crime reduction benefits so much higher in the Perry and CPC studies?

The similarity of crime reduction benefits between the current study and Karoly and Bigelow reflects two offsetting differences. Karoly and Bigelow's estimates assume a larger negative percentage effect of pre-K on crime. But baseline crime costs are much lower in Karoly and Bigelow. Their percent reduction in crime is relatively large because the study extrapolates from the CPC study. Karoly and Bigelow scale back the CPC effects but still assume that preschool will reduce crime by 18 percent, over twice the present study. For baseline crime costs, Karoly and Bigelow have low estimates because they exclude "intangible" victim costs.[13]

The larger crime reduction benefits in Perry and CPC are due to both components. For the overall sample, the percentage crime reduction in Perry and CPC is 6–8 times that of the current study. Average baseline crime costs are 2–3 times that of the current study in the CPC study and the Heckman et al. (2010) Perry study, and 12 times greater in the Belfield et al. (2006) Perry study.

For the percentage crime reduction benefits, larger effects for Perry and CPC could be due to these programs being more intensive. On the other hand, Perry and CPC have actual data on crime, and grade retention may not capture all avenues by which pre-K reduces crime.

[13] In sensitivity tests, Karoly and Bigelow (2005) report that if they include intangible costs, their crime reduction benefits increase by a factor of 2.25 (compare their Tables 3.4 and 3.2).

Differences in baseline crime costs could be due to differences in baseline crime rates or costs per crime. Because of differences in crimes included in various studies, it is impossible to exactly compare these studies along these dimensions, but some differences are due to higher baseline crime rates in Perry and CPC. For example, in Belfield et al. (2006), the lifetime numbers of rapes per male comparison group member is 1.32 versus 0.11 in Tulsa. In Heckman et al. (2010), as shown in Table 6, average lifetime crime rates for each of the six crimes for males are at least six times higher than the current study, and crime rates are also higher for females. Finally, CPC assumes that average baseline rates of "serious crime" are 5 times higher than the current study.[14] These higher crime rates in Perry and CPC might be plausible because the programs targeted a more disadvantaged group.

CONCLUSION

This study of Tulsa's universal pre-K program finds evidence that the program's future benefits exceed its costs. The policy implication is that pre-K programs that are universal and high quality will provide net benefits to society.

On the other hand, the benefit-cost evidence for Tulsa's universal pre-K is not as overwhelmingly favorable as was the case for small, intensive, and highly targeted programs such as Perry and CPC. At least on current evidence, one cannot promise policymakers that large, less-intensive, and more universal programs will deliver the extraordinary results of

[14] Reynolds, Temple, White et al. (2011) assume that the CPC "comparison group" in Chicago has a 44 percent probability of an adult criminal career, and that the average career criminal commits 9.41 "serious crimes" from ages 19 to 44 (Greenwood et al. 1998), which implies the CPC comparison group averages 4.14 "serious crimes" from ages 19 to 44. In our Tulsa sample, the average comparison group member commits only 0.84 "serious crimes" over this same age range. (Serious crimes in Greenwood et al. [1998] are murder, rape, robbery, assault, and 60 percent of burglary.)

programs such as Perry. However, it is perhaps unreasonable for policymakers to expect large-scale universal programs to deliver such extraordinary benefits, and extraordinary benefits are not needed for a program to be worthwhile.

One key to the more modest benefits of universal programs like Tulsa's pre-K program, compared to more targeted pre-K programs, is the more modest baseline crime rates among potential pre-K participants. In designing pre-K, as well as other educational interventions, the baseline crime rate of the targeted group can have large implications for eventual benefits.

Although our findings on Tulsa's universal pre-K program are less dramatic than findings from some early iconic programs, the Tulsa program does have the advantage of being a large-scale contemporary program that reaches a large and diverse cross-section of children. Thus our research has higher external validity than many other studies. At the same time, we should caution that we have examined only two pathways for pre-K impacts: from pre-K to grade retention to adult earnings, and from pre-K to grade retention to crime. If pre-K produces benefits through other pathways or influences other outcomes, then our estimate of program impact is a lower boundary for the actual impact. With longer-run follow-up of pre-K participants into adulthood, future research may be able to provide more direct and accurate estimates of pre-K's adult benefits.

Appendix A
More Details on the Estimated Effects of Tulsa Pre-K on Grade Retention

Table A.1 provides estimates of the effects of TPS pre-K on grade retention, by half-day and full-day pre-K participation and by group. These estimates are derived using propensity score weighting and were calculated separately by pre-K participation status and group.

Appendix B
More Details on the Estimated Effects of Grade Retention on Earnings and Crime

This appendix provides more detail on the NLSY97 estimated effects of grade retention on earnings and crime.

Table B.1 provides descriptive statistics for the NLSY97 sample used to estimate grade retention's effects on earnings and crime. In addition to providing mean values for earnings and crime, by grade retention status, the table also provides statistics on the independent variables used as controls in this analysis.

Table B.2 provides estimates of the dollar reduction in annual earnings due to grade retention, for the various groups and ages. These estimates are derived from separate regressions for each age and group. The control variables described in Table B.1 are also in the regressions.

Table B.3 translates these dollar effects of grade retention on earnings into percentage effects, by dividing the effects by the corresponding means.

Table B.4 provides estimates of the effects of grade retention on self-reported arrests for violent crimes and property crimes over the 1998–2013 period for the various groups. The control variables listed in Table B.1 are included.

Table B.5 translates these absolute retention effects on the number of arrests over a 15-year period into percentage effects on arrests, by dividing the effects by the sample mean for each group and type of crime. The numbers in Table B.5 are used to generate Figure 3 in the text.

Appendix C
Sensitivity of Results to Alternative Assumptions and Estimates

This appendix considers how our benefit estimates change when we use alternate baseline assumptions and estimates. We consider three alternatives: zero secular earnings growth, rather than the Social Security Trustee's predicted secular earnings growth of 1.17 percent annually; estimates of seventh grade test score pre-K effects on future earnings, rather than grade retention pre-K effects; social costs of crime that are lower or higher, compared to the median cost from various estimates of social crime costs.

Zero Earnings Growth

As described in the main text, the baseline estimates assume that annual earnings grow over time. Specifically, we assume that from 2013 on, annual real earnings for any particular group at a particular age grows by 1.17 percent per year. This secular growth estimate is based on the "mid-range" long-run assumptions of the Trustees of the Social Security Administration. These official estimates are used to help assess the Social Security system's financial position in the long-run.

Because our benefit-cost analysis (BCA) is comparing costs incurred in the 2005–2006 school year, with earnings benefits that occur decades later, such secular earnings growth assumptions have the potential to significantly alter benefit-cost ratios. Therefore, a prudent course is to consider earnings growth assumptions that are more conservative, which will cause future earnings benefits to be scaled back. Our conservative earnings growth assumption is zero growth, almost surely causing the present value of future earnings to be understated, and thereby causing future earnings benefits to be understated.

Table C.1 shows both the baseline earnings estimates, and earnings estimates under the assumption of zero earnings growth. Compared to 1.17 percent annual earnings growth, zero earnings growth reduces the earnings benefit estimates by around 30 percent, which is due to the present value of lifetime earnings for various groups being scaled back by that amount. The zero earnings growth assumption very slightly modifies the percentage effects of retention on earnings, because our percentage effects of retention on earnings differ slightly at different ages based on our NLSY estimates.

The modest reduction in earnings benefits from the zero growth assumption reduces the benefit-cost ratio for Tulsa pre-K by a modest amount. However, the finding that this eighth grade evidence predicts pre-K benefits that are somewhat greater than costs, but not overwhelmingly so, is not altered by the zero growth assumption. The overall benefit-cost ratio changes from the baseline assumption's value of 1.89 to 1.44 under the zero growth assumption.

Test Score Effects of Tulsa Pre-K Used to Predict Future Earnings

Our baseline estimates use the predicted effects of Tulsa pre-K on grade retention, together with the estimated effects of grade retention on adult earnings, to project Tulsa pre-K's earnings benefits. Even if the estimates of pre-K "effects" and retention "effects" are interpreted as causal, these estimates reflect the effects of retention, together with whatever other student outcomes accompany retention, on adult earnings. For example, these baseline estimates would include any tendency for retention to be accompanied by lower test scores.

An alternative to using pre-K's grade retention effects is to use pre-K's test score effects to predict pre-K's earnings effects. To do so, estimates of how seventh grade test scores affect future earnings are needed. Perhaps the best recent study of test score earnings effects is by

Chetty et al. (2011), who find that test scores, measured in percentile terms, had a roughly linear relationship with average earnings at age 27. We interpret this as, for all groups and regardless of starting test scores, a given percentile improvement in test scores will have the same effect when measured as a percentage of overall mean earnings. We assume that the percentage effect at age 27 will be maintained over the life-cycle.

Chetty et al. use multiple regression to estimate adult earnings at age 27 based on a particular grade's test scores, for all grades from kindergarten through eighth grade. For kindergarten only, Chetty et al. also estimate test scores' true causal effects on earnings. These causal estimates scaled back the regression estimates by about 16 percent. For our analysis, we assume that this scale-back of regression estimates, when applied to regression estimates of test score effects on earnings at other grades, will roughly capture the true causal effects of test scores on earnings. For seventh grade test scores, the resulting estimate is that each 1 percentile improvement in average test scores will increase adult earnings by 0.729 percent of the overall population's average earnings.[15] This percentage increase in overall average earnings is assumed to apply to percentile improvements for all groups, regardless of their average earnings. A similar procedure was used in the projections made in Bartik, Gormley, and Adelstein (2012).

Using the Chetty et al. procedure required estimates of Tulsa pre-K's effects stated as percentile effects on average test scores in terms of the overall student population distribution. We estimate Tulsa pre-K's effects on seventh grade reading and math test scores for all students, including all the different groups considered in this study (gender, racial, income, and full-day

[15] The raw regression estimate in Chetty et al. is that the effect of a 1 percentile increase in average seventh grade test scores will increase earnings at age 29 by 0.869 percent of overall mean earnings of the population at age 28. Scaling back by 16.1 percent gives us the 0.729 percent.

versus half-day pre-K groups). These test score effects were stated in effect size units. The overall state test score standard distribution was used to estimate effect sizes. To derive percentile estimates from these effect size estimates, we combine the following information: the overall state means for seventh grade reading and math test scores, effect size estimates of Tulsa pre-K on seventh grade test scores in reading and math for the various groups, seventh grade test score means in reading and math for former Tulsa pre-K participants in the various groups, and an assumption that seventh grade reading and math test scores are normally distributed. From this information, estimated percentile effects were derived for how Tulsa pre-K affected reading and math test scores separately, and then averaged for each group to get an average percentile effect. This average percentile effect was then combined with the estimates derived from Chetty et al.—that each percentile improvement in average test scores at seventh grade will increase adult earnings by 0.729 percent of overall population mean earnings—in order to derive estimates of earnings effects for each group in present value dollar terms. These present value dollar estimates can then be compared with the expected present value of lifetime earnings for the relevant comparison group to get an implied percentage effect of pre-K for each group.

In Table C.2, the baseline earnings effects, based on pre-K's effects on grade retention by eighth grade, are compared with the alternative estimates, based on pre-K's effects on seventh grade test scores. The estimated test score effects for the overall population are slightly higher. However, there is no obvious pattern for different groups. Compared to the retention-effect derived estimates, these estimates are higher for females, lower for males; higher for whites and Hispanics, lower for blacks; higher for the free lunch and paid lunch groups, lower for the reduced-price lunch groups; lower for the full-day group, less negative for the half-day group.

It should be noted that there are fewer test score estimates that are statistically significant compared to the retention estimates. Only 6 of the 11 groups rely on at least one estimate that is statistically significant at a 10 percent level. In contrast, 9 of the 11 group estimates of retention effects are statistically significant at the same level.

Overall, using test-score-based estimates does not drastically change the BCA findings. For the overall population, the benefit-cost ratio for Tulsa pre-K changes from 1.89 for the baseline retention-derived estimates to 2.03 for the test-score-derived estimates.

These results go against the notion that the fading of test score effects, but later recovery of adult earnings effects, was due to unobserved soft skills. One might hypothesize that "sleeper effects" of pre-K on adult earnings are mediated through hard-to-observe soft skills and might be more fully reflected in behavioral middle school outcomes, such as retention. However, there is no sign that the retention-based effects systematically predict larger adult earnings effects than test-score-based effects. From either retention or test scores in middle school, the benefit-cost ratio for Tulsa's universal pre-K program is less than that of some of the more targeted programs, and less than that of the previous BCA examining the effects of Tulsa pre-K on kindergarten entrance test scores. It appears that fading effects of pre-K are found in retention-based estimates, not just test-score based estimates. It is of course possible that there are other soft skills, not reflected in retention effects, that might result in Tulsa pre-K having larger effects on adult outcomes than we predict based on these middle school outcomes. Alternatively, perhaps the universal Tulsa pre-K program has somewhat lower benefits than programs such as Perry and CPC, which were both more intense and more highly targeted.

Test Score Effects Controlling for Retention

Another alternative was to combine the retention-based projections of pre-K's earnings effects with test score effects. To do so, we calculated estimates of pre-K's effects on test scores, controlling for retention. The estimates that separately link either retention or test scores to adult earnings do not control for the other variable. In our NLSY97 estimates, part of the estimated effect of retention on adult earnings is due to test score decreases that tend to accompany grade retention. In Chetty et al.'s estimates of how test scores affect adult earnings, part of the test score effects may be due to their effects on grade retention. A plausible hypothesis is that if either of these analyses controlled for the other variable, the separate effects of each variable would be somewhat reduced. Therefore, adding together the retention-based estimates of pre-K's effects on adult earnings with the test-score-based estimates of pre-K's effects on adult earnings is likely to overstate the combined effect of the two variables on adult earnings. However, such an analysis may at least provide a possible upper bound for pre-K's earnings effects via retention and test scores in middle school.

Table C.3 reports the results. First, it should be noted that after controlling for grade retention, only 2 out of 11 groups have test-score-based pre-K effects that are statistically significant at a 10 percent level. In other words, after controlling for grade retention, there is no strong evidence of statistically significant effects of pre-K on seventh grade test scores. Furthermore, the estimated test-score effects of pre-K are only positive and significant for the paid lunch group.

Due to our estimation methodology, the combined effects of pre-K on earnings, due to effects on both retention and test scores, are forced to exceed the retention-only effects unless pre-K has negative effects on test scores. For some groups, these increases in projected total

earnings effects are large. However, for the overall pre-K population, the increased earnings effects are modest. Adding in test-score effects to retention effects increases earnings benefits for the overall pre-K population by only 38 percent, with the overall benefit-cost ratio going up from the baseline of 1.89 to 2.49.

Lower or Higher Social Costs of Crime

A final sensitivity test is to consider the implications of assuming different social costs of crime. Our baseline predictions used the median estimate of the social costs of six different crimes in seven different studies. As an alternative, we considered using the lowest and highest estimates from these studies.

Table C.4 reports the social costs of these six types of crime from the seven different studies. It also reports the median, minimum, and maximum social costs of crime for each crime type and study. As can be seen, there are considerable differences in costs, with the social costs of crime varying from around 3-to-1 to over 10-to-1.

Using these various social costs of crime, Table C.5 reports how the benefit estimates vary under these different scenarios. The alternative crime cost scenarios affect the expected lifetime "average" costs of crime for each group considered.[16] In addition, these alternative crime cost scenarios slightly alter the percentage effects of pre-K on crime costs, as retention's percentage effects on crime rates vary marginally with age and type of crime. Multiplied together, these changes in comparison group average crime costs, and the percentage crime effects yield different benefits of pre-K in reducing crime.

[16]As noted in the text, one must recognize that this average is based on a small number of individuals in each group that commit most of the crimes and should not be interpreted as the "typical" costs of crime for an individual in each group. This average almost surely exceeds "median" costs of crime in each group.

As shown in the table, compared to the baseline "median" crime cost estimates, the low crime cost estimates lessen the estimated benefits of pre-K due to crime reduction by about 40 percent. The high-cost crime estimates, compared to the baseline "median cost" estimates, yield crime reduction benefits that are approximately tripled.

However, even these extreme crime cost assumptions do not cause the overall benefit-cost ratio to be changed by a large amount. For the overall population, the benefit-cost ratio for Tulsa pre-K changes from 1.89 using baseline median costs to 1.77 using low crime costs and 2.52 using high crime costs.

Why don't lower or higher crime costs make a larger difference? For the current study, the estimated percentage effects of pre-K on crime are modest. For example, the percentage effects for the overall population translate to around a 6 percent crime reduction. In contrast, as shown in Table 5 in the main text, the percentage of crime reduction for the overall pre-K population due to pre-K ranges from 34 percent to 52 percent in the Perry and CPC studies, and is 18 percent for the California universal pre-K study. In our current study, quite different results would have been obtained if the combination of pre-K's effects on grade retention, and grade retention's effects on crime, had yielded larger percentage effects of pre-K on crime.

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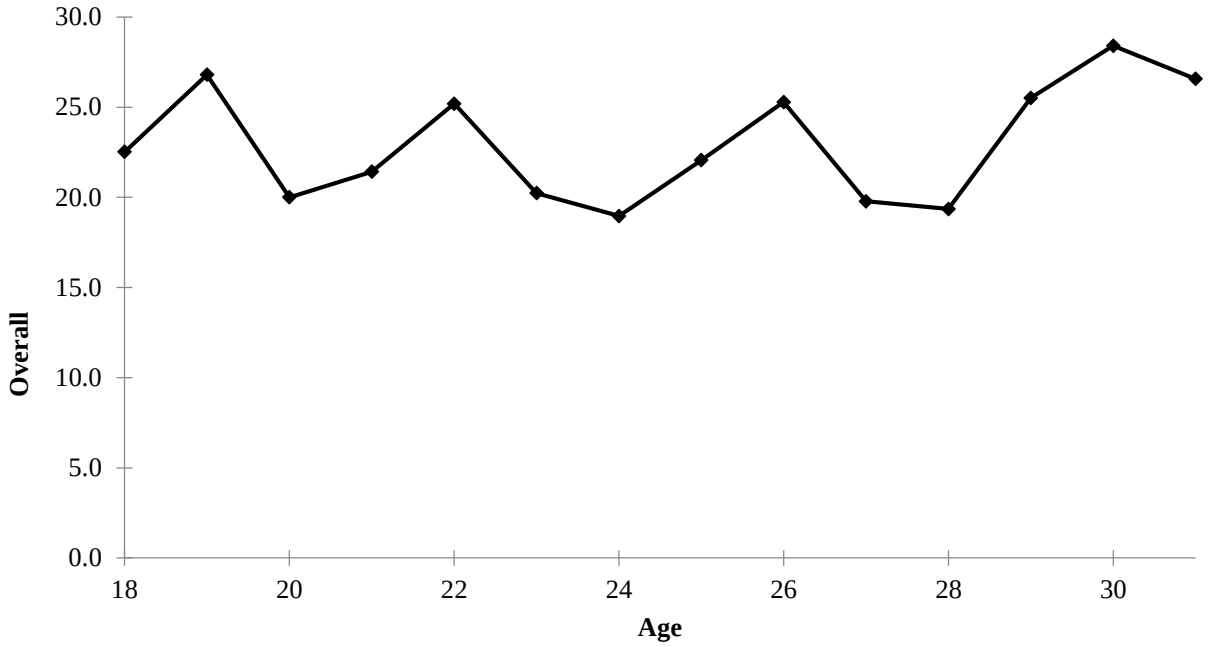
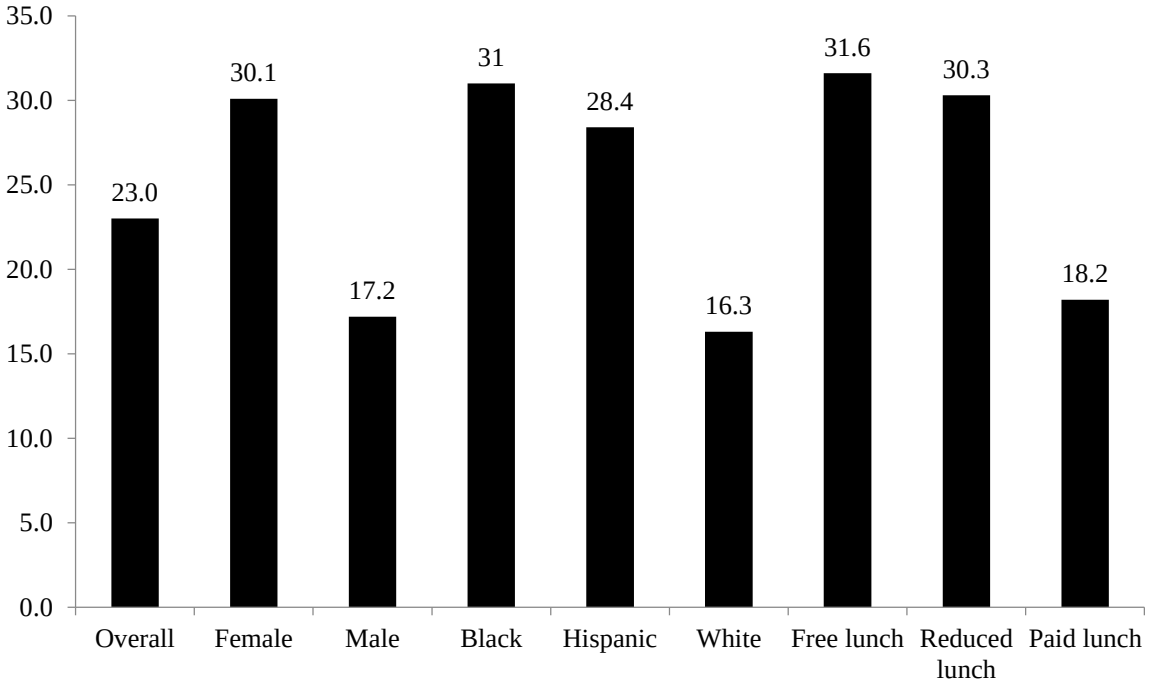
Figure 1 Earnings Reductions Due to Grade Retention (%)



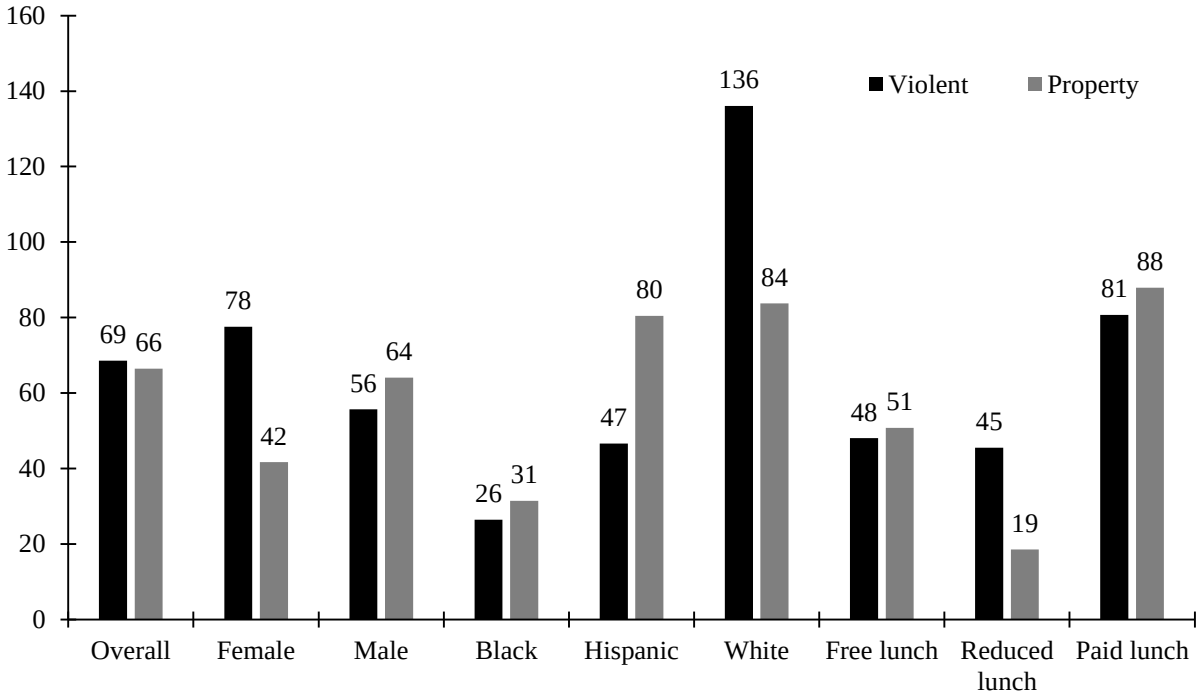
NOTE: Derived from NLSY97 estimates, as described in text, and more fully presented in Appendix B. The percent results are for the overall NLSY sample. They are derived by dividing the dollar reduction estimates for each age by estimated mean earnings by age. All estimates are statistically significant at 1 percent level.
SOURCE: Authors' estimates.

Figure 2 Average Percent Reduction in Earnings Due to Grade Retention, Ages 18-31



NOTE: Derived from estimates described in text, based on NLSY97. Estimates are based on taking reduction in dollar earnings for single years of age and group, and dividing by mean earnings for that age and group. Average is simple average of those percent reductions over ages 18-31.
SOURCE: Authors' estimates.

Figure 3 Percent Increase in Crime Due to Grade Retention, by Group and Type of Crime



NOTE: This figure is derived from NLSY97 estimates, as described in text. Percent increase in crime due to grade retention is the estimated effect on arrests from 1998 to 2013, divided by mean arrests for that group over that time period.
SOURCE: Authors' estimates.

Table 1 Tulsa Pre-K Effects on Grade Retention

All	Female	Male
−0.0915***	−0.0617**	−0.1082***
(0.0197)	(0.0275)	(0.0278)
White	Black	Hispanic
−0.0642**	−0.0990***	−0.1632***
(0.0299)	(0.0358)	(0.0485)
Free	Reduced	Paid
−0.0989***	−0.1397**	−0.0481
(0.0255)	(0.0562)	(0.0345)
Full-day	Half-day	
−0.0617**	0.0102	
(0.0251)	(0.0254)	

NOTE: * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Standard errors in parentheses. The reported coefficients are, for the indicated group, for the effects of Tulsa Pre-K on retention in grade as of when the student would have been in ninth grade, for kindergarten students in 2006–2007 who did or did not attend pre-K in 2005–2006. Coefficients come from a linear probability model and hence can be interpreted straightforwardly as the change in the probability of grade retention for a kindergarten student who attended Tulsa pre-K versus a kindergarten student who did not participate in either Tulsa pre-K or Head Start. Regression includes large numbers of controls and propensity score weighting, as described in the text and more fully in Gormley, Phillips, and Anderson (2016). All effects are for students in either full-day or half-day pre-K, except for results in bottom rows.

Table 2 Summary Benefit-Cost Analysis of Tulsa Pre-K, Based on Projecting Grade Retention Results to Earnings Gains and Crime Reductions over the Life-Cycle

Group	Earnings benefit ($)	Crime $ benefit ($)	Total benefits (earnings gains plus crime reductions) ($)	Pre-K costs ($)	Net benefits ($)	BC ratio
Overall	14,415	2,963	17,378	9,183	8,196	1.89
Female	8,168	645	8,813	9,153	(339)	0.96
Male	18,420	4,854	23,274	9,209	14,065	2.53
Black	10,237	3,663	13,900	10,232	3,668	1.36
Hispanic	19,062	3,763	22,825	9,702	13,123	2.35
White	9,751	2,883	12,634	7,871	4,763	1.61
Free lunch	13,072	3,898	16,969	9,792	7,177	1.73
Reduced lunch	24,577	2,658	27,235	8,773	18,461	3.10
Paid lunch	7,818	996	8,814	7,654	1,160	1.15
Full-day	9,721	1,998	11,719	11,089	630	1.06
Half-day	(1,607)	(330)	(1,937)	5,544	(7,482)	−0.35

NOTE: Table reports present value of benefits and costs per Tulsa pre-K participant, overall or in subgroups. Present value is measured in 2013 U.S. national prices. Present value is calculated based on 3 percent real annual discount rate. Derivation of earnings benefits, crime benefits, and costs are discussed in text. Net benefits are simply equal to total benefits measured in this study (earnings and crime reduction benefits) minus pre-K costs. Benefit-cost ratio is simply a ratio of total benefits to costs.

Table 3 Comparing This Study with Previous Benefit-Cost Analyses of Pre-K

Study	Group	Earnings benefit ($)	Crime reduction benefit ($)	Other benefits ($)	Total benefits ($)	Pre-K costs ($)	Net benefits ($)	BC ratio
This study (Tulsa)								
	Overall	14,415	2,963		17,378	9,183	8,196	1.89
	Female	8,168	645		8,813	9,153	(339)	0.96
	Male	18,420	4,854		23,274	9,209	14,065	2.53
	Black	10,237	3,663		13,900	10,232	3,668	1.36
	Hispanic	19,062	3,763		22,825	9,702	13,123	2.35
	White	9,751	2,883		12,634	7,871	4,763	1.61
	Free lunch	13,072	3,898		16,969	9,792	7,177	1.73
	Reduced lunch	24,577	2,658		27,235	8,773	18,461	3.10
	Paid lunch	7,818	996		8,814	7,654	1,160	1.15
	Full-day	9,721	1,998		11,719	11,089	630	1.06
	Half-day	(1,607)	(330)		(1,937)	5,544	(7,482)	−0.35
Bartik, Gormley, and Adelstein (Tulsa)								
	Overall	29,866			29,866	9,339	20,527	3.20
	Free lunch	31,779			31,779	9,892	21,887	3.21
	Reduced lunch	31,007			31,007	9,230	21,778	3.36
	Paid lunch	24,547			24,547	8,028	16,519	3.06
	Full-day	34,309			34,309	11,089	23,220	3.09
	Half-day	20,228			20,228	5,544	14,683	3.65
Belfield et al. (Perry)								
	Overall	83,834	222,782	11,449	318,065	19,704	298,361	16.14
	Female	91,745	16,201	2,305	110,252	19,704	90,547	5.60
	Male	75,922	429,363	20,594	525,879	19,704	506,175	26.69
Heckman et al. (Perry)								
	Overall	89,794	76,867	9,236	175,897	20,442	155,455	8.60
	Female	146,743	19,757	(52)	166,448	20,442	146,006	8.14
	Male	49,455	117,320	15,814	182,590	20,442	162,148	8.93
Reynolds et al. (Chicago CPC)								
	Overall	30,916	45,513	22,415	98,845	9,123	89,721	10.83
Karoly and Bigelow (Universal in California)								
	Overall	9,154	2,723	3,883	15,761	5,003	10,758	3.15

NOTE: All columns except benefit-cost ratio column show benefits or costs in terms of present value of pre-K benefits or costs in 2013 U.S. dollars, as of age four, using a 3 percent real discount rate. Adjustments from the dollars used in the particular study, to 2013 U.S. dollars, use various combinations of Bureau of Economic Analysis Regional Price Parities and changes in regional and national price deflators. All earnings numbers are for gross earnings, including taxes. All crime results sum both juvenile and adult crime and include all crime costs that are counted in that study.

SOURCE: BGA results are from their Table 5. The Belfield et al. figures are derived from their Table 9. Heckman et al. estimates are taken from their Table 8. Reynolds et al. results are from their Appendix 3 table. Karoly and Bigelow results are from their Table 3.2.

Table 4 Comparing Earnings Benefits across Pre-K Studies

Study	Group	Baseline earnings	% effects of pre-K on earnings	Earnings benefit
This study (Tulsa)				
	Overall	664,906	2.2	14,415
	Female	480,445	1.7	8,168
	Male	856,410	2.2	18,420
	Black	410,916	2.5	10,237
	Hispanic	438,315	4.3	19,062
	White	728,195	1.3	9,751
	Free lunch	428,758	3.0	13,072
	Reduced lunch	558,796	4.4	24,577
	Paid lunch	771,907	1.0	7,818
	Full-day	664,906	1.5	9,721
	Half-day	664,906	−0.2	(1,607)
Bartik, Gormley, and Adelstein (Tulsa)				
	Overall	402,965	7.4	29,866
	Free lunch	329,040	9.7	31,779
	Reduced lunch	431,954	7.2	31,007
	Paid lunch	570,960	4.3	24,547
	Full-day	381,615	9.0	34,309
	Half-day	449,274	4.5	20,228
Belfield et al. (Perry)				
	Overall	501,536	16.7	83,834
	Female	403,248	22.8	91,745
	Male	599,824	12.7	75,922
Heckman et al. (Perry)				
	Overall	356,780	25.2	89,794
	Female	311,346	47.1	146,743
	Male	388,963	12.7	49,455
Reynolds et al. (CPC)				
	Overall	381,678	8.1	30,916
Karoly and Bigelow (Universal for California)				
	Overall	518,456	1.8	9,154

NOTE: All earnings figures are in 2013 U.S. dollars and are present values as of age 4 using 3 percent real discount rate. Baseline earnings is present value of lifetime earnings of comparison group. This study's estimates are described in text. BGA results are from their Table 5. The Belfield et al. baseline earnings estimates are derived from extrapolating from Profile A2 in their Table 2, and their present value results in their Table 9, by assuming that the ratio of present value by age range and gender to undiscounted earnings can be extrapolated from the benefit calculations to baseline earnings. The Heckman et al. figures are extrapolated from their Table 3 figures on undiscounted earnings by gender and age, and combined with ratios for each range calculated from comparing undiscounted to discounted earnings by age and gender using this study's data from the ACS. Reynolds et al. baseline earnings is taken from their Appendix 3. The Karoly and Bigelow percent effect of pre-K on earnings figure is taken from replicating their research by directly calculating using their methodology the earnings effect of getting a high school degree. This percent effect uses the CPS-ORG to calculate earnings by age and gender for high school dropouts versus graduates, extrapolates that the effect on high school dropout rates from universal pre-K will be 23 percent of the 11 percentage point effect found in the CPC study (this is Karoly and Bigelow's assumption), and then uses this to calculate the present value of the difference in earnings due to universal pre-K. Mean earnings in these calculations is calculated using information from the National Center on Education Statistics that the mean regular high school completion rate is 85.3 percent. http://nces.ed.gov/pubs2015/2015015.pdf (accessed August 16, 2016).

Table 5 Comparing Crime Reduction Benefits across Pre-K Studies

Study	Group	Baseline crime costs per potential pre-K participant ($)	% effect of pre-K on reducing crime	Crime $ benefit ($)
This study (Tulsa)				
	Overall	47,290	−6.3	2,963
	Female	14,187	−4.5	645
	Male	79,952	−6.1	4,854
	Black	138,898	−2.6	3,663
	Hispanic	47,425	−7.9	3,763
	White	33,938	−8.5	2,883
	Free lunch	81,801	−4.8	3,898
	Reduced lunch	43,637	−6.1	2,658
	Paid lunch	25,499	−3.9	996
	Full-day	47,290	−4.2	1,998
	Half-day	47,290	0.7	(330)
Belfield et al. (Perry)				
	Overall	602,184	−37.0	222,782
	Female	181,415	−8.9	16,201
	Male	1,022,953	−42.0	429,363
Heckman et al. (Perry)				
	Overall	148,114	−51.9	76,867
	Female	26,869	−73.5	19,757
	Male	233,996	−50.1	117,320
Reynolds et al. (CPC)				
	Overall	133,272	−34.2	45,513
Karoly and Bigelow (Universal for California)				
	Overall	15,432	−17.6	2,723

NOTE: Baseline crime costs are the costs that would be imposed on society by a typical pre-K participant if they had been in the comparison group. Baseline crime costs and crime benefits are present value figures as of age four, using a 3 percent discount rate, stated in 2013 dollars. This study's methodology is described in text. Belfield et al. figures for baseline costs are extrapolated from their Tables 7 and 9. Undiscounted crime costs for the control group by age range and gender are adjusted to present value terms using the ratio of the present value of crime benefits to the undiscounted crime benefits. Heckman et al. baseline crime costs are calculated by extrapolating from their Tables 3 and 8. The ratio of the present value of crime reduction benefits to undiscounted crime reduction benefits by gender is used to adjust the control group's undiscounted crime reduction benefits to present value terms. Reynolds et al. percentage effect is derived by combining information from their Appendix C and their Table 3 results that report that CPC reduced their indicator of juvenile crime (number of petitions to juvenile court) from 0.78 to 0.45 and their indicator of adult crime (number of felony arrests) from 0.44 to 0.32. Together with the present value of estimated benefits by juvenile crime and adult crime in Appendix C, this allows an inference for the implied baseline crime costs in their sample and the percentage average crime cost reduction in their study. Karoly and Bigelow calculations are based on assumptions about the implication of their division of universal preschool into nine groups (three groups defined by counterfactual preschool status—public preschool, private preschool, no preschool—and three groups defined as high risk, medium risk, and low risk) and their relative effectiveness assumptions for each of these nine groups. Our baseline assumption is that the percentage effect of crime generates the difference in relative effectiveness of a new universal preschool program across different counterfactual enrollment groups, and that baseline crime levels generates the differences in relative effectiveness of preschool across different risk groups, with one exception: the zero effectiveness they assume of universal preschool for low risk students otherwise must be generated by zero effects of preschool on crime. Karoly and Bigelow state (p. 162) that their baseline for the 100 percent effectiveness group (high risk, no preschool counterfactual) is the CPC estimate that juvenile crime is reduced by 42 percent, and they assume adult crime is reduced 80 percent of that 42 percent. Applying this to the benefit figures by type of crime in Karoly and Bigelow gives a weighted average crime reduction for the high risk no prior preschool group of 37.5 percent. We then calculate what baseline crime costs and percentage crime reductions for all nine groups are consistent with the above assumptions.

Table 6 Lifetime Crime Rates of Typical Comparison Group Member in Perry versus Tulsa (%)

	Heckman et al. Perry study		This Tulsa Study		Ratios, Perry over Tulsa	
Types of crime	Male	Female	Male	Female	Male	Female
Murder	0.09	0.08	0.01	0.00	7.1	54.3
Rape	1.91	0.00	0.11	0.00	17.6	
Robbery	2.76	0.31	0.33	0.04	8.4	7.0
Assault	4.32	0.00	0.68	0.19	6.3	0.0
Burglary	12.12	0.85	1.98	0.25	6.1	3.4
Larceny	21.70	4.09	1.89	2.44	11.5	1.7

NOTE: These tables show an undiscounted sum of the number of different types of crimes committed for a typical control or comparison group member. The Heckman et al. numbers are reported in their Appendix Tables H.8 and H.9. The particular numbers are the estimated number of victimizations caused by an "average" control group member over his or her lifetime, when the ratio of victimizations to arrests is estimated separately for each type of crime using data from urban Midwestern areas. These numbers are used because they are the basis for the benefit-cost analysis results that Heckman et al. present for comparison purposes in their text Table 8. For the present study, the methodology for calculating lifetime crimes committed per comparison group member is described in text. These calculations correct for mortality since age four.

Table A.1 Pre-K Effects on Grade Retention, Full- and Half-Day Estimates, across Subgroups

	All	Female	Male	White	Black	Hispanic	Free	Reduced	Paid
Full-day pre-K	−0.0617**	−0.0420	−0.0748**	0.0337	−0.0927**	−0.1463**	−0.0778***	−0.1308*	0.0776
	(0.0251)	(0.0353)	(0.0345)	(0.0441)	(0.0392)	(0.0567)	(0.0297)	(0.0739)	(0.0578)
Half-day pre-K	0.0102	0.0163	0.0050	−0.0495	0.0734	−0.0219	0.0480	−0.0667	−0.0512
	(0.0254)	(0.0333)	(0.0364)	(0.0349)	(0.0582)	(0.0678)	(0.0362)	(0.0746)	(0.0395)

NOTE: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are in parentheses.
SOURCE: Authors' estimates.

Table B.1 Descriptive Statistics for NLSY97 Sample Used to Estimate Earnings and Crime Effects of Grade Retention, By Grade Retention Status

	Retained			Non−Retained					
Variable	Mean	SE	N	Mean	SE	N	Diff.	p	Description
Dependent variables									
earnings	7975.06	240.416	1,313	12400.32	125.707	7,442	−4425.26	0.000	Average annual earnings in 2013 dollars from 18 to 31 years of age.
Violent crimes	0.284	0.025	1,229	0.116	0.007	6,846	0.168	0.000	Average number of times respondent was charged with a violent crime between 1998 and 2013.
Property crimes	0.269	0.028	1,229	0.097	0.006	6,846	0.172	0.000	Average number of times respondent was charged with a property crime between 1998 and 2013.
Primary independent variable									
grade retention	—	—	1,337	—	—	7,647	—	—	Dummy variable for whether a respondent was retained between first and ninth grade.
Gender									
Female	0.384	0.013	1,337	0.506	0.005	7,647	−0.122	0.000	Dummy variable indicating gender.
Ethnicity									
Black	0.409	0.013	1,336	0.234	0.005	7,628	0.175	0.000	Dummy variable indicating black.
Hispanic	0.222	0.011	1,336	0.210	0.005	7,628	0.012	0.358	Dummy variable indicating Hispanic.
White	0.350	0.013	1,336	0.517	0.006	7,628	−0.167	0.000	Dummy variable indicating white.
Other	0.020	0.004	1,336	0.038	0.002	7,628	−0.018	0.001	Dummy variable indicating non−white, other race.
Socioeconomic status									
Free lunch	0.471	0.015	1,079	0.265	0.006	5,482	0.206	0.000	Dummy variable indicating eligibility for free school lunches.
Reduced lunch	0.136	0.010	1,079	0.104	0.004	5,482	0.032	0.002	Dummy variable indicating eligibility for reduced−price school lunches.
Paid lunch	0.393	0.015	1,079	0.631	0.007	5,482	−0.238	0.000	Dummy variable indicating no eligibility for subsidized school lunches.
Urbanicity									
Urban	0.758	0.012	1,258	0.765	0.005	7,346	−0.007	0.556	Dummy variable indicating urban household.
Family status									
Single mother	0.390	0.013	1,335	0.264	0.005	7,618	0.126	0.000	Dummy variable indicating single mother household.
Two parents in household	0.484	0.014	1,335	0.650	0.005	7,618	−0.166	0.000	Dummy variable indicating two parent household.
ELL status									
English language learner	0.242	0.014	966	0.116	0.004	5,938	0.126	0.000	Dummy variable indicating English language learner.

Table A2.1 (Continued)

	Retained			Non−Retained					
Variable	Mean	SE	N	Mean	SE	N	Diff.	p	Description
Birth cohort									
1980	0.212	0.011	1,337	0.184	0.004	7,647	0.028	0.017	Dummy variable indicating born in 1980.
1981	0.215	0.011	1,337	0.207	0.005	7,647	0.008	0.506	Dummy variable indicating born in 1981.
1982	0.207	0.011	1,337	0.205	0.005	7,647	0.002	0.824	Dummy variable indicating born in 1982.
1983	0.180	0.011	1,337	0.205	0.005	7,647	−0.025	0.039	Dummy variable indicating born in 1983.
1984	0.185	0.011	1,337	0.199	0.005	7,647	−0.014	0.246	Dummy variable indicating born in 1984.
Parent education (highest grade)	11.421	0.075	1,232	12.772	0.034	7,119	−1.351	0.000	Average highest grade completed between both residential parents.

NOTE*:* This table compares the characteristics of retained and nonretained youths in the NLSY97 sample. The included variables are the variables used as control variables in the regressions. The mean and the standard error of the mean is reported, along with sample sizes for each variable.
SOURCE: Authors' estimates.

Table B.2 Grade Retention Effects on Earnings by Age, across Subgroups

Age	All	Female	Male	White	Black	Hispanic	Free	Reduced	Paid
18	−671.2***	−741.6***	−629.8*	−1,089***	−222.3	−619.2*	−825.7**	−231.0	−628.2*
	(215.0)	(228.6)	(336.7)	(283.8)	(545.2)	(347.2)	(324.2)	(584.8)	(331.0)
	3,866	*1,894*	*1,972*	*871*	*728*	*2,137*	*991*	*432*	*2,443*
19	−1,405***	−1,474***	−1,262***	−1,801***	−2,110***	−469.6	−1,018**	−2,537***	−1,332***
	(320.2)	(407.3)	(479.3)	(426.1)	(552.1)	(621.4)	(474.0)	(850.9)	(506.5)
	3,581	*1,786*	*1,795*	*825*	*642*	*1,987*	*915*	*404*	*2,262*
20	−1,646***	−1,925***	−1,302**	−2,015***	−2,416***	−718.4	−1,345*	−3,074***	−1,486**
	(455.1)	(594.4)	(663.3)	(609.3)	(930.5)	(847.3)	(694.7)	(1,133)	(711.8)
	3,455	*1,698*	*1,757*	*776*	*620*	*1,948*	*882*	*383*	*2,190*
21	−2,388***	−2,951***	−1,820**	−2,759***	−3,994***	−880.6	−3,921***	−3,687**	−612.4
	(543.6)	(674.1)	(822.2)	(806.2)	(1,093)	(938.1)	(739.8)	(1,493)	(878.4)
	3,417	*1,696*	*1,721*	*753*	*615*	*1,928*	*849*	*376*	*2,192*
22	−3,365***	−4,919***	−1,896**	−2,386**	−4,970***	−2,859***	−4,079***	−3,986***	−2,386**
	(629.1)	(757.7)	(953.7)	(1,031)	(1,261)	(1,006)	(946.9)	(1,520)	(994.2)
	3,412	*1,708*	*1,704*	*745*	*639*	*1,908*	*868*	*372*	*2,172*
23	−3,330***	−5,345***	−1,418	−3,361***	−5,455***	−1,592	−4,339***	−4,452**	−1,894*
	(753.2)	(926.5)	(1,121)	(1,229)	(1,330)	(1,298)	(985.2)	(1,828)	(1,267)
	3,328	*1,640*	*1,688*	*736*	*601*	*1,886*	*851*	*365*	*2,112*
24	−3,825***	−5,206***	−2,524**	−3,881***	−5,433***	−2,310*	−4,758***	−4,479**	−2,610*
	(819.1)	(968.4)	(1,232)	(1,235)	(1,462)	(1,439)	(1,079)	(1,849)	(1,390)
	3,401	*1,689*	*1,712*	*746*	*603*	*1,944*	*856*	*380*	*2,165*
25	−5,283***	−5,745***	−4,851***	−5,494***	−6,800***	−3,207**	−6,489***	−4,204	−4,265***
	(915.1)	(1,237)	(1,293)	(1,359)	(1,783)	(1,564)	(1,117)	(2,972)	(1,507)
	3,511	*1,733*	*1,778*	*774*	*645*	*1,977*	*888*	*372*	*2,251*
26	−6,457***	−6,098***	−6,476***	−5,690***	−6,565***	−5,921***	−6,767***	−7,302***	−5,658***
	(872.2)	(1,121)	(1,263)	(1,366)	(1,608)	(1,477)	(1,217)	(2,009)	(1,432)
	3,571	*1,779*	*1,792*	*805*	*644*	*2,015*	*901*	*407*	*2,263*
27	−5,421***	−5,184***	−5,320***	−4,085***	−4,607**	−5,352***	−4,314***	−4,780**	−6,170***
	(979.4)	(1,232)	(1,434)	(1,429)	(1,979)	(1,637)	(1,288)	(2,332)	(1,633)
	3,655	*1,816*	*1,839*	*837*	*672*	*2,031*	*953*	*398*	*2,304*
28	−5,537***	−4,874***	−5,580***	−3,147*	−3,528*	−6,934***	−3,798**	−8,021***	−6,369***
	(1,121)	(1,413)	(1,651)	(1,775)	(2,099)	(1,853)	(1,507)	(2,830)	(1,833)
	2,962	*1,510*	*1,452*	*655*	*555*	*1,658*	*765*	*324*	*1,873*
29	−7,745***	−7,939***	−7,284***	−3,719*	−10,014***	−8,399***	−6,216***	−8,634***	−8,614***
	(1,217)	(1,332)	(1,868)	(1,933)	(2,153)	(2,065)	(1,623)	(2,825)	(2,006)
	2,907	*1,479*	*1,428*	*680*	*538*	*1,600*	*751*	*315*	*1,841*
30	−8,678***	−7,770***	−9,011***	−9,162***	−8,131***	−7,118***	−6,885***	−11,734***	−9,230***
	(1,364)	(1,581)	(2,127)	(2,060)	(2,226)	(2,553)	(1,722)	(3,192)	(2,348)
	2,196	*1,099*	*1,097*	*493*	*412*	*1,217*	*563*	*241*	*1,392*
31	−8,451***	−7,352***	−9,030***	−2,689	−6,705*	−12,751***	−4,591*	−9,223**	−10,627***
	(1,859)	(1,971)	(3,059)	(3,269)	(3,404)	(3,031)	(2,905)	(3,575)	(2,824)
	1,363	*727*	*636*	*345*	*244*	*738*	*345*	*153*	*865*

NOTE: N in italics. * p < 0.1; ** p < 0.05; *** p < 0.01. Robust standard errors in parentheses. Outcome is earnings for ages 18–31 received from wages, salary, commissions, or tips from all jobs before taxes. All regressions include the full set of control variables reported in Table B.1; full results are available on request.
SOURCE: Authors' estimates.

Table B.3 Average Percentage Effects of Grade Retention on Annual Earnings, by Age and Group (%)

Age	Overall	Female	Male	Black	Hispanic	White	Free lunch	Reduced lunch	Paid lunch
18	−22.5	−29.3	−18.4	−56.5	−8.3	−17.4	−38.4	−7.9	−18.9
19	−26.8	−32.6	−21.2	−54.8	−47.1	−7.3	−24.6	−46.7	−23.6
20	−20.0	−27.6	−13.8	−38.2	−29.6	−7.5	−19.7	−36.5	−17.0
21	−21.4	−32.3	−13.8	−34.8	−34.4	−7.1	−42.0	−31.9	−5.2
22	−25.2	−42.7	−12.5	−24.2	−35.4	−19.7	−37.2	−28.5	−16.8
23	−20.2	−37.7	−7.6	−28.6	−32.3	−8.7	−32.8	−27.0	−10.7
24	−19.0	−29.9	−11.0	−26.3	−29.2	−10.3	−32.7	−23.7	−11.5
25	−22.1	−27.8	−17.9	−31.5	−31.3	−11.9	−39.3	−20.0	−15.6
26	−25.3	−27.8	−22.2	−30.9	−28.4	−20.5	−39.3	−32.0	−19.3
27	−19.8	−22.0	−17.0	−21.2	−19.2	−17.0	−24.2	−20.0	−19.3
28	−19.4	−20.1	−16.9	−15.2	−13.8	−21.4	−20.6	−31.4	−19.1
29	−25.5	−31.5	−20.4	−17.5	−36.0	−24.5	−32.0	−36.0	−24.0
30	−28.4	−31.5	−24.7	−41.9	−29.2	−20.8	−36.0	−44.6	−25.7
31	−26.6	−28.2	−23.5	−11.9	−23.9	−34.5	−22.9	−38.0	−28.1
Average	−23.0	−30.1	−17.2	−31.0	−28.4	−16.3	−31.6	−30.3	−18.2
Average 25+	−23.8	−27.0	−20.4	−24.3	−25.9	−21.5	−30.6	−31.7	−21.6
Average 28+	−25.0	−27.8	−21.4	−21.6	−25.7	−25.3	−27.9	−37.5	−24.2
Average 25–27	−22.4	−25.9	−19.0	−27.9	−26.3	−16.5	−34.3	−24.0	−18.0

NOTE: This table simply takes the dollar effects from Table B.2 and divides by the corresponding sample mean for that age and group.
SOURCE: Authors' estimates.

Table B.4 Grade Retention Effects on Crimes Committed by Type, across Subgroups

Crime type	All	Female	Male	White	Black	Hispanic	Free lunch	Reduced lunch	Paid lunch
Violent crimes	0.0806**	0.0388	0.103*	0.0484	0.0570	0.119***	0.110	0.0519	0.0582*
	(0.0344)	(0.0296)	(0.0570)	(0.0809)	(0.0623)	(0.0436)	(0.0698)	(0.0860)	(0.0340)
Property crimes	0.0721**	0.0201	0.109*	0.0451	0.100	0.0772	0.0923	0.0203	0.0670
	(0.0343)	(0.0252)	(0.0586)	(0.0686)	(0.0798)	(0.0483)	(0.0656)	(0.0764)	(0.0410)
Observations	4,324	2,176	2,148	1,003	810	2,377	1,129	478	2,717

NOTE: * p < 0.1; ** p < 0.05; *** p < 0.01. Robust standard errors in parentheses. Outcome is charges for violent or property crimes between 1998 and 2013. Violent crimes are assault (including rape and murder) and robbery. Property crimes are burglary and theft. All regressions include the full set of control variables reported in Table B.2; full results are available on request.
SOURCE: Authors' estimates.

Table B.5 Percentage Effect of Grade Retention on Crime, by Type of Crime and Group (%)

	Violent	Property
Overall	68.6	66.4
Female	77.5	41.7
Male	55.7	64.1
Black	26.4	31.5
Hispanic	46.6	80.5
White	136.0	83.7
Free lunch	48.0	50.7
Reduced lunch	45.5	18.6
Paid lunch	80.7	87.9

NOTE: These figures are derived by dividing the absolute arrest effects in Table B.4 by the mean arrests for the corresponding group and type of crime.
SOURCE: Authors' estimates.

Table C.1 Effects of Substituting More Modest Future Earnings Growth Assumptions

	Baseline estimates			Zero earnings growth estimates			
Group	Comparison group estimated lifetime earnings	% effects of pre-K on earnings	Earnings benefit	Comparison group lifetime earnings	% effects	Earnings benefit	Earnings benefit with zero growth as % of baseline
Overall	664,906	2.17	14,415	472,661	2.16	10,228	70.9
Female	480,445	1.70	8,168	345,490	1.71	5,901	72.3
Male	856,410	2.15	18,420	604,688	2.14	12,916	70.1
Black	410,916	2.49	10,237	296,656	2.51	7,455	72.8
Hispanic	438,315	4.35	19,062	315,712	4.38	13,821	72.5
White	728,195	1.34	9,751	517,899	1.33	6,870	70.5
Free lunch	428,758	3.05	13,072	306,906	3.06	9,376	71.7
Reduced lunch	558,796	4.40	24,577	399,763	4.39	17,538	71.4
Paid lunch	771,907	1.01	7,818	547,987	1.01	5,511	70.5
Full-day	664,906	1.46	9,721	472,661	1.46	6,897	70.9
Half-day	664,906	(0.24)	(1,607)	472,661	(0.24)	(1,140)	70.9

NOTE: Baseline estimates are as reported in text tables. Zero earnings growth estimates are described in this appendix. Figures are in present value 2013 national dollars, except for percentage effects.
SOURCE: Authors' estimates.

Table C.2 Effects of Substituting Test Score Effects of Pre-K for Retention Effects of Pre-K in Predicting Adult Earnings

Group	Baseline earnings (2013 Tulsa $)	Retention % effect	Retention $ effects	w/o ret test score effect size of pre-K	w/o ret test score % effects on earnings	w/o ret $ effect of test scores on earnings	$ earnings benefit due to test scores as % of baseline estimates based on retention
Overall	664,906	**2.17**	14,415	**0.09**	**2.36**	15,665	108.7
Female	480,445	**1.70**	8,168	**0.09**	**3.05**	14,666	179.6
Male	856,410	**2.15**	18,420	0.09	1.84	15,778	85.7
Black	410,916	**2.49**	10,237	**(0.03)**	**(0.87)**	(3,556)	(34.7)
Hispanic	438,315	**4.35**	19,062	**0.16**	**7.61**	33,362	175.0
White	728,195	**1.34**	9,751	0.13	3.24	23,604	242.1
Free lunch	428,758	**3.05**	13,072	**0.09**	**3.36**	14,422	110.3
Reduced lunch	558,796	**4.40**	24,577	0.01	0.39	2,181	8.9
Paid lunch	771,907	1.01	7,818	**0.14**	**3.31**	25,520	326.4
Full-day	664,906	**1.46**	9,721	0.05	1.18	7,819	80.4
Half-day	664,906	(0.24)	(1,607)	(0.01)	(0.15)	(971)	60.4

NOTE: Baseline estimates come from text tables. Alternative estimates described in this appendix. Bold percentage effects on earnings means that at least one of the underlying estimated effects of pre-K on retention or test scores is statistically significant at least at the 10 percent level.
SOURCE: Authors' estimates.

Table C.3 Effect of Adding Test Score Effects of Pre-K Conditional on Retention to Retention Effects of Pre-K in Predicting Earnings

Group	Baseline earnings (2013 Tulsa $)	Retention % effect	Retention dollar effects	ES of test score controlling for retention	w/ret test % effect on earnings	w/ret test $ effect on earnings	tot% (retention + test score effects)	tot$ (retention + test score effects)	Earnings benefit from adding both retention and test score effects as percent of original retention only earnings effect
Overall	664,906	**2.17**	14,415	0.03	0.82	5,454	2.99	19,869	137.8
Female	480,445	**1.70**	8,168	0.05	1.68	8,050	3.38	16,218	198.6
Male	856,410	**2.15**	18,420	0.02	0.41	3,529	2.56	21,949	119.2
Black	410,916	**2.49**	10,237	**(0.08)**	**(2.33)**	(9,563)	0.16	674	6.6
Hispanic	438,315	**4.35**	19,062	0.07	3.24	14,220	7.59	33,283	174.6
White	728,195	**1.34**	9,751	0.08	2.05	14,953	3.39	24,704	253.4
Free lunch	428,758	**3.05**	13,072	0.03	1.22	5,228	4.27	18,300	140.0
Reduced lunch	558,796	**4.40**	24,577	(0.11)	(3.78)	(21,123)	0.62	3,454	14.1
Paid lunch	771,907	1.01	7,818	**0.11**	**2.65**	20,456	3.66	28,274	361.6
Full-day	664,906	**1.46**	9,721	0.00	0.11	707	1.57	10,427	107.3
Half-day	664,906	(0.24)	(1,607)	(0.00)	(0.13)	(855)	−0.37	(2,462)	153.2

NOTE: Baseline estimates come from text tables. Alternative estimates described in this appendix. Bold percentage effects on earnings indicates that at least one of underlying estimates of pre-K on retention, or on test scores conditional on retention, is significant at least at the 10 percent level.
SOURCE: Authors' estimates.

Table C.4 Comparisons of Costs of Different Types of Crime from Different Studies ($)

	Aos (2001)	Cohen (1988)	Cohen et al. (2004)	Miller, Cohen, and Rossman (1993)	Millern Cohen, and Wiersema (1996)	Rajkumar and French (1997)	McCollister et al. (2010)	Median	Maximum	Minimum
Murder	4,305,801	—	11,048,388	4,034,293	4,263,893	—	8,743,668	4,305,801	11,048,388	4,034,293
Rape	359,892	95,353	278,653	78,262	121,105	—	234,363	177,734	359,892	78,262
Assault	102,734	22,412	82,303	24,322	20,880	74,783	104,170	74,783	104,170	20,880
Robbery	213,446	23,524	272,774	32,156	18,096	32,260	41,183	32,260	272,774	18,096
Larceny	—	335	—	—	515	1,075	3,438	795	3,438	335
Burglary	—	2,506	29,393	—	2,088	1,921	6,290	2,506	29,393	1,921

NOTE: All crime costs come from McCollister, French, and Fang (2010). These figures are adjusted to 2013 U.S. dollars.

Table C.5 Effects of Alternative Assumptions about Social Costs of Crime

	Baseline			Low crime costs				High crime costs			
Group	Comparison group crime costs per potential participant ($)	% effect of pre-K on reducing crime (%)	Reduced crime $ benefit ($)	Comparison group crime costs per potential participant ($)	% effect of pre-K on reducing crime (%)	Reduced crime $ benefit ($)	Reduced Crime $ benefits as % of baseline (%)	Comparison group crime costs per potential participant ($)	% effect of pre-K on reducing crime (%)	Reduced crime $ benefit ($)	Reduced crime dollar benefits as percent of baseline (%)
Overall	47,290	−6.27	2,963	28,620	−6.26	1,793	60.5	139,859	−6.24	8,729	294.6
Female	14,187	−4.55	645	7,443	−4.56	339	52.6	38,515	−4.25	1,638	253.8
Male	79,952	−6.07	4,854	49,544	−6.08	3,011	62.0	239,852	−6.18	14,822	305.3
Black	138,898	−2.64	3,663	88,841	−2.64	2,344	64.0	436,203	−2.69	11,716	319.9
Hispanic	47,425	−7.94	3,763	28,688	−7.95	2,282	60.6	140,185	−8.60	12,059	320.4
White	33,938	−8.50	2,883	19,630	−8.48	1,664	57.7	93,841	−7.99	7,501	260.2
Free lunch	81,801	−4.76	3,898	49,528	−4.77	2,360	60.6	238,189	−4.79	11,418	292.9
Reduced lunch	43,637	−6.09	2,658	26,451	−6.08	1,607	60.5	132,213	−5.57	7,367	277.2
Paid lunch	25,499	−3.91	996	15,443	−3.91	603	60.6	77,195	−3.95	3,052	306.5
Full-day	47,290	−4.23	1,998	28,620	−4.22	1,209	60.5	139,859	−4.21	5,886	294.6
Half-day	47,290	0.70	(330)	28,620	0.70	(200)	60.5	139,859	0.70	(973)	294.6

SOURCE: Authors' estimates.