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# Parents' Incomes and Children's Outcomes: A Quasi-Experiment Using Transfer Payments from Casino Profits†

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*We examine the role an exogenous increase in household income, due to a government transfer unrelated to household characteristics, plays in children's long-run outcomes. Children in affected households have higher levels of education in their young adulthood and a lower incidence of criminality for minor offenses. Effects differ by initial household poverty status. An additional $4,000 per year for the poorest households increases educational attainment by one year at age 21, and reduces the chances of committing a minor crime by 22 percent for 16 and 17 year olds. Our evidence suggests improved parental quality is a likely mechanism for the change.* (*JEL* D14, H23, I32, I38, J13)

Household conditions and characteristics play an important role in determining the outcomes of children. The strength and nature of that role has been an important research area for social scientists. One characteristic is of special importance for economists—household incomes. Does having more money in the household produce better child outcomes over time? Alternatively, does growing up in poverty produce worse outcomes for children? It is difficult to answer these

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questions because household incomes are not exogenously given. Income depends crucially on parental characteristics, both observed and unobserved. Therefore, simply observing that children from high- (low-) income families tend to have positive (negative) educational, income, and employment outcomes in young adulthood tells us little about the actual causation. Parents transmit to their genetic offspring some of their innate abilities, and the observed correlation between parental incomes and child outcomes later in life may simply reflect this intergenerational transfer and not the effect of income, per se.

Researchers have sought to overcome this endogeneity problem by using a number of instrumental variables and fixed effects techniques that attempt to isolate the difference in household incomes that are not due to parental characteristics or ability. Using father's union and occupational status as instruments for income, John Shea (2000) finds that income has no effect on child outcomes while Arnaud Chevalier et al. (2005) find that permanent income matters in children's educational attainment. Eric Maurin (2002) uses grandparent socioeconomic status as a predictor of parental incomes, a measure which is then used to explain a child's performance in early education. He finds that a child is much less likely to be held back in school the higher the household income. Katrine Loken (2007) uses the Norwegian oil boom of the 1970s and 1980s, which only affected a few regions of the country, as an instrument for increases in household income that is unrelated to parental characteristics. She finds that there is no effect of family income on child educational attainment. For these instruments to be valid, we must assume that there is no choice involved in union or occupational status or selection in the job loss instruments. Alternatively, we must assume that there is no transmission of abilities across generations in order for the grandparent socioeconomic instrument to be valid. Finally, in the oil boom scenario, we must assume no endogenous movement across regions, but also that all industries within the affected regions were not differentially affected.

Other researchers have used more permanent income measures, such as household assets. Susan E. Mayer (1997) uses household assets and child support payments as measures of household income (these are taken to be less closely related to parental characteristics), and she finds that income has a positive and significant effect on educational attainment and wages. David M. Blau (1999) uses child fixed effects in the National Longitudinal Study of Youth data and finds that parental income (at least the transitory component) does not affect child test scores. Bruce Sacerdote (2007) finds that parental income matters less than parental education for young adult educational, income, and health outcomes for Korean-American adoptees in his data. This research design is particularly useful. However, the obvious drawback is that there is selection with regard to families willing to adopt children. Households that adopt children are not representative of the population at large.

While previous research has found conflicting results with regard to the effect of household income on the young adult outcomes of household children, none of the studies have been able to identify a truly exogenous income change at the household level. Recent work by Gordon B. Dahl and Lance Lochner (2005), using panel data and changes in the Earned Income Tax Credit (EITC) in the United States, has shown that reading and math scores improved in households with increased earnings—especially for the most disadvantaged households. In their intergenerational data, Philip

Oreopoulos, Marianne E. Page, and Ann Huff Stevens (2005) find that children who come from households where fathers were displaced from their jobs have, on average, 9 percent lower earnings than children whose fathers were not displaced in childhood. Once again, they find the effect to be driven by the most disadvantaged households. This will hold, generally, in our data as well. Our empirical strategy most closely match those of Esther Duflo (2003). In her paper, Duflo (2003) examines the effect of pension extension to the black South Africans, by gender, on the anthropometric status of grandchildren in these households. Similar to Duflo (2003), we find that an exogenous increase in household income matters for child outcomes and that there is a gendered effect. Women have a large effect on child educational attainment.

Our approach attempts to overcome the standard household income endogeneity problem in a direct manner. We observe households in which incomes are increased exogenously and permanently through a governmental transfer program without regard to parental human capital, ability, or other household characteristics. In our study, we follow children that reside in households with and without exogenously increased incomes. The children are sampled in three age cohorts. The youngest children reside as minors in households with higher incomes for a longer period of time than the oldest children in this study. We compare educational attainment and criminality outcomes from the youngest age cohort to the oldest age cohort to determine the effect of residing in a household with exogenously higher incomes. The children from households without additional household income serve as a control for any changes in local labor market opportunities that may have arisen between the age cohorts.

Our study uses data from the Great Smoky Mountains Study of Youth (GSMS). In this longitudinal study of child mental health in rural North Carolina, both American Indian and non-Indian children were sampled. Halfway through the data collection, a casino opened on the Eastern Cherokee reservation. A portion of the profits from this new business operation is distributed every six months on an equalized, per capita basis to all adult tribal members regardless of employment status, income, or other household characteristics. No choice is involved here. Individuals are eligible based on preexisting American Indian status. Therefore, we can observe the treatment effect on an entire distribution of household types. Non-Indian households are not eligible for these cash disbursements. Figure 1 provides a clear depiction of the change in household incomes over the first eight survey waves of our study. A marked increase is noted in the number of households with incomes above $30,000 for the treatment (American Indian) households after the disbursement of casino payments in 1997.[1] No long-run change is observed for non-Indian households.

On the one hand American Indians are a particular group in the United States, with real per capita income of $8,000 in the 2000 US census and poverty rates in excess of 37 percent (compared to the US average of $21,000 and a 9 percent poverty rate). Decades of failed policies have plagued American Indian reservations from

[1] We use the percentage of households by group (American Indian versus non-Indian) that have household incomes greater than $30,000. This corresponds to the median value of non-Indian households in survey wave three that was just conducted prior to the opening of the casino.

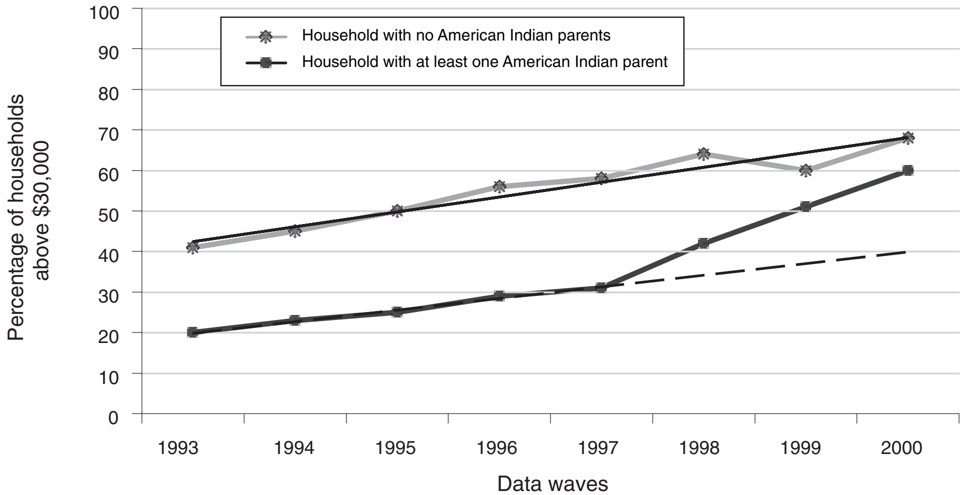


Figure 1. Household Income by American Indian Parent Status in Waves 1–8

land reform policies to natural resource extraction and business development.[2] In this regard, the advent of casino operations has been hailed as a viable means of creating prolonged economic development. On the other hand, this particular American Indian reservation is fairly well integrated into the local regional economy of western North Carolina. There is only about a $10,000 difference in average household incomes between the American Indian households and non-Indian households in our survey prior to the start of casino operations. This is still a large number, but smaller than national averages would suggest. Additionally, the reservation is not particularly isolated, nor is it large.[3] Our research question is a general one that is of interest for other high poverty groups in the United States: how effective are anti-poverty, cash-transfer programs in improving the outcomes of household children? While the particular circumstances associated with the casino are unusual, the government transfer payment is not. This study examines the effect of a cash transfer on children from poor American Indian households, and these findings could also be instructive for other poor, semi-rural communities in the United States. Our research design allows us to evaluate the effect along an entire distribution of household incomes—a rarity in these sorts of studies.

We find that children who reside the longest in households with exogenously increased incomes tend to do better later in life on several outcome measures. The children in these households are more likely to have graduated from high school by age 19 as compared to the children from untreated households. By age 21, the

[2] See David E. Wilkins (2002) or Eric Henson et al. (2007) for a good description of past American Indian policies and programs, both successes and failures.

[3] The reservation is less than 100 square miles in size and is less than an hour from Asheville, NC, less than two hours from Knoxville, TN, and less than three hours from Atlanta, GA, all of which are large metropolitan areas.

treated children from the poorest households have an additional year of schooling.[4] A rough estimate indicates that an average of $4,000 additional household income for the poorest families results in an additional year of education for the child from a treated household. Additionally, we find, using administrative records on criminal arrests, that these same children have statistically significantly lower incidence of criminal behavior for minor offenses. The additional household income reduces the indcidence of ever having committed a minor crime by 22 percent at ages 16 and 17 for these children from treated households. These children also self-report that they have a lower probability of having dealt drugs than children from households unaffected by the additional income.

As expected, the poorest households in the survey experience the largest gains in terms of child outcomes. Separating the data according to prior poverty status, we find that many of these results are driven by the poorer households. The findings also indicate that mothers who receive the exogenous increase in incomes affect the the child's educational outcome, while fathers who receive the income affect the child's criminal behavior.[5]

There are numerous mechanisms that may translate higher household incomes into better child outcomes. We explore two potential mechanisms: parental quality and parental time. The additional income may allow the poorer households to move away from full-time employment toward part-time employment, thus allowing for more child care. This does not appear to happen in our data. Parents do not reduce their working time. On the other hand, we find that parental interactions and experiences with the children in the affected households tend to improve dramatically. Both child and parent report improved behavioral effects and parent-child interactions relative to unaffected households. We observe that parent behavior, similar to that of the child, tends to improve with regard to criminality.[6] Previous research has found a direct relationship between poverty and parenting ability (Jane D. McLeod and Michael J. Shanahan 1993; Robert J. Sampson and John H. Laub 1994; Nicole E. Ennis, Stevan E. Hobfoll, and Kerstin E. E. Schröder 2000), and we confirm this result in our research. There is at least some indication that one of the mechanisms responsible for translating higher household incomes into better child outcomes is through increased parental quality, while parenting time does not appear to have been an important causal factor.

[4] William Evans and Wooyoung Kim (2006) use the 1990 and 2000 US census data to examine changes in educational attainment at a more aggregate level for American Indian reservations. In their study, they find that having a casino on a reservation tends to increase high school drop out rates and reduce college enrollment. The census data do not allow one to know whether the same individuals are being followed over time. The census only asks individuals where they resided five years prior. Therefore, it is possible and highly likely (see Evans and Julie H. Topoleski 2002) that there was significant in-migration by low-skilled individuals after the casinos opened up on these American Indian reservations, which has led to a decrease in overall educational attainment. Our data follows the same group of people over time on a single reservation before and after the opening of the casino.

[5] These findings are similar to those of Duflo (2003) on the effect of cash transfers in South Africa. Females who receive a cash transfer (via pension extension) affect child health investment, while there is no similar finding when males receive a cash transfer.

[6] Similar results were found in the Moving to Opportunity program (Jeffrey R. Kling, Jens Ludwig, and Lawrence F. Katz 2005; Kling, Jeffrey B. Liebman, and Katz 2007). In this case, low-income households were given the means to move into lower poverty neighborhoods. Incidence of mental illness decreased for parents and youth. Additionally, in previous research utilizing the GSMS data, Costello et al. (2003) found decreased mental illness for children from households that were lifted out of poverty as a result of the casino income.

The next section describes the data from the GSMS and our empirical methods. Section II provides our estimation results. We explore some potential mechanisms which may play a role in translating increased incomes into better child outcomes in Section III. Section IV concludes.

## I. The Great Smoky Mountains Study of Youth, Empirical Methods, and Data Description

The GSMS is a longitudinal survey of 1,420 children aged 9, 11, and 13 years at the survey intake who were recruited from 11 counties in western North Carolina. The children were selected from a population of approximately 20,000 school-aged children using an accelerated cohort design.[7] American Indian children from the Eastern Band of Cherokee Indians were over sampled for this data collection effort. Survey weights are used in the child outcome regressions that follow. The federal reservation is situated in 2 of the 11 counties within the study. The initial survey contained 350 American Indian children and 1,070 non-American Indian children. Proportional weights were assigned according to the probability of selection into the study. Therefore, the data is representative of the school-aged population of children in this region. Attrition and nonresponse rates were found to be equal across ethnic and income groups.

The survey began in 1993 and has followed these three cohorts of children annually up to the age of 16, and then re-interviewed them at ages 19 and 21.[8] Additional survey waves are scheduled for these children when they turn 24 and 25 years old. Both parents and children were interviewed separately until the child was 16 years old. After that, interviews were conducted with the child alone.

After the fourth wave of the study, a casino was opened on the Eastern Cherokee reservation. The casino is owned and operated by the tribal government. A portion of the profits are distributed on a per capita basis to all adult tribal members.[9] Disbursements are made every six months and have been since 1996. The average annual amount per person has been approximately $4,000. This income is subject to the federal income tax requirements.

### A. *Empirical Specifications*

*Difference-in-Difference Regression.*—We compare young adult outcomes for children that resided, as minors, in households with increased incomes for six years to children who resided, as minors, in households with exogenously increased incomes for two years. We employ a difference-in-difference methodology. This specification

[7] See Costello et al. (1996) for a thorough description of the original survey methodology.

[8] Individuals are interviewed regardless of where they are living (whether on their own, in college, or still living with their parents). No child is dropped from the survey because they moved out of their parent's home. We find no statistically significant difference in selection between the treatment and control groups. American Indians comprise 24 percent of the sample in the first survey wave and approximately 27 percent of the sample at age 21.

[9] All adult tribal members received these per capita disbursements. If there were any noncompliers (American Indian parents that either did not receive or refused the additional income), then any estimates found here would be an underestimation of the true effects of additional income. All enrolled, American Indian children were eligible for the casino disbursements at age 18 if they completed high school. If they did not complete high school they would receive the casino transfers at age 21.

allows us to compare the effect of four additional years of higher household incomes on young adult outcomes for these children. The two youngest age cohort variables (age 9 and age 11 at survey intake) function as the "after-treatment" cases and the oldest age cohort (age 13 at survey intake) functions as the "before-treatment" case. We focus explicitly on the effect of the per capita transfer on children's outcomes. An examination of the effect of the treatment on household income indicates that almost the entirety of the additional cash transfer shows up as additional household income in each survey wave.[10]

The size of the exogenous increase in household incomes can take on two different values depending upon the number of American Indian parents in each household. It is possible for there to be zero, one, or two American Indian parents in each household.[11] Clearly households with two American Indian parents will have double the amount of exogenous income of households with only a single American Indian parent. Households without an American Indian parent serve as a control household. We treat the number of parents as a continuous variable, and we have two interaction variables that are of interest. The equation below details the specification:

$$(1) \quad Y_i = \alpha + \beta_1 \times Age9_i + \beta_2 \times Age11_i + \delta \times NumParents_i + \gamma_1 \times Age9_i \times NumParents_i + \gamma_2 \times Age11_i \times NumParents_i + \mathbf{X}_i'\theta + \varepsilon_i.$$

In the equation above, $Y$ is the outcome variable of interest for the child at ages 19 or 21. We will examine educational attainment, high school completion variables, and criminal arrests at various ages (16–21). In the equation above, the *Age*9 and *Age*11 variables indicate whether or not the child is drawn from the initial age 9 or age 11 cohorts, respectively. The age 13 cohort is the omitted category in this regression. The variable *NumParents* indicates the number of American Indian parents in that child's household. The two coefficients of interest for this research are $\gamma_1$ and $\gamma_2$, which measure the effect of receiving the casino disbursements and being in either the age 9 or age 11 cohorts, relative to the 13-year-old cohort, and not receiving any household casino disbursements. The vector **X** controls household conditions prior to the opening of the casino and includes household poverty status, average household income over the four years, the sex of the child, the race of the child, and the education levels of both parents. All results presented for the child outcomes are robust to inclusion of the number of siblings in the household. Survey weights are employed

[10] We find that the effect of the treatment (household eligibility for the casino per capita transfer) results in approximately \$3,900 of additional household income at each survey wave. The average amount distributed per person has been about \$4,000 per year. This suggests that households do not alter their labor participation in response to this additional household income.

[11] In some cases, the biological parent does not live in the same household. In these cases, while the child is not necessarily living in a household with the additional income, he or she still has a parent with exogenously increased income. The inclusion of these households should actually reduce the effect of household incomes on child outcomes if there is no direct effect of the additional income for nonresident parents on their children. We have excluded these households and find that, in general, while the sample size is reduced and standard errors increase, the results tend to hold for most of the reported outcomes.

in all of these difference-in-difference regressions. In Web Appendix Table 1, we provide placebo tests, where possible, for the outcome variables described above.

Identification of equation (1) relies on the fact that the different age cohorts of children were randomly sampled within American Indian and non-Indian groupings. The next section provides evidence for this, and also indicates that the two groups of households (American Indian and non-Indian) faced similar conditions in the labor market and with regard to social conditions. It is also important to note that there were no new health or educational programs created immediately after the advent of casino disbursements by the tribal government. This is important in establishing the fact that time variant characteristics that were related only to American Indians (such as tribally-funded, anti-crime programs or tutoring programs) are not the causal factor here. In later years, new programs have been developed, but for the crucial period in which these children were minors in their parents' households, there is little evidence of new programs. Anecdotal evidence suggests that the revenues from the casino operations were, at least in the short run, spent on per capita disbursements to membership of the tribe. Spending on large-scale construction (such as a new community gym, diabetes center, and other affiliated offices) and programs did not occur until 2001–2002. Therefore, the children in this study were not minors when these new programs and facilities were operational, and were not likely to have been affected. Schools on the reservation were constructed in the 1970s and a new elementary school opened in 2007, and a new middle and high school opened in the fall of 2009.

Another point worth mentioning is that the effect of this new industry, casino operations, may have a rather large effect on the demand for labor in the local labor market. This increase in demand may affect the employment opportunities and wages in the region. In fact, this change in labor demand may be directly driving all of the observed results, and the actual cash transfer program may be inconsequential. There are a number of reasons why that probably does not hold. First, at all survey waves, we know if parents are employed. There does not appear to be a dramatic increase in parental employment after the casino begins operating. Second, we would have to assume that the labor supply in this region was relatively inelastic in order to get large increases in wages. Others have shown (Evans and Topoleski 2002) that labor supply in these communities is highly elastic, and there has been large in-migration when casinos open up on American Indian reservations between 1990 and 2000. Therefore, we do not expect there to be a large change in wages even with a large increase in labor demand for the region. There are several rather large towns and cities in the region, and this argues against a very inelastic supply of labor.

Finally, we use global positioning system data (GPS) to compute a distance measure that serves as a proxy for other noncash transfer related effects of the casino operations on households. The average household is 32 miles (median is 36 miles) away from the casino, with a minimum distance of 5 miles and a maximum distance of 75 miles. We find that inclusion of this measure (which is available for all survey households) and an interaction variable with treatment households does not diminish the effects reported in later tables. [12]

[12] We include a measure of distance from each household to the casino (using GPS data) in level and interacted with household eligibility for casino payments in Web Appendix Table 2. One can think of this distance measure

*Fixed-Effects Panel Regression*.—Given the panel nature of the data, we are also able to utilize individual fixed effects for one of the outcome variables—child's school attendance. This educational measure is meaningful at various points throughout the child's life, not just at young adulthood, as is the case with the other educational attainment measures. Therefore, we employ a fixed effects regression for the number of days a child was present at school in the last three months prior to the interview. The regression is given of the form:

$$Y_{it} = \mathbf{X}'_{it}\beta + \alpha_0 + \alpha_i + \varepsilon_{it}. \tag{2}$$

In this regression, $\alpha_i$ is the individual fixed effect and $\mathbf{X}$ is the vector of control variables, including whether the individual child, $i$, belongs to a household that is eligible for casino payments. This indicator variable is always zero for households without American Indian parents. For households with American Indian parents the variable is zero for the first four survey waves and then takes the value of one thereafter. We employ a similar model when testing for changes in parental arrests and relationship with their children in the second half of the paper which investigates the mechanisms through which additional household income affects young adult child outcomes. We use a random effects probit model for changes in parental employment status over time.[13]

## B. *Data Description*

*Data Means*.—Table 1 provides the means for the data used in this analysis by the type of household. The first panel provides the variables used primarily in the difference-in-difference regressions, while the second panel provides the data used in the fixed effects regressions. In panel A, the first set of columns provides the means for the households with at least one American Indian parent, and the second panel contains the means for households that do not have any American Indian parents.

**Educational Variables:** It is worth noting that children from households with at least one American Indian parent have statistically significantly different educational

as a proxy for the other noncash transfer effects of the casino on households. The estimated coefficient on the first interaction variable for years of education regression is 0.42, while the probability of high school graduation becomes 0.125—which is qualitatively similar to our results in Table 4. Inclusion of these variables closely resembles our results in Table 5 in which we restrict analysis to households previously in poverty. The estimated coefficient on the first interaction variable is 1.199 and is significant at the 1 percent level; and the coefficient for the probability of high school graduation regression is 0.343 and statistically significant at the 5 percent level.

[13] The random effects probit model is described in detail in Jeffrey M. Wooldridge (2002) and based on a model described initially by Gary Chamberlain (1980) and Yair Mundlak (1978). These models are useful in panel situations when the outcome variable is binary, and there are important unobserved effects that are not directly controllable in the data. The key to this method is that the distribution of the endogenous variables is computed using the initial values of these variables; joint distribution for all outcomes need not be computed. In these models, the relationship between the unobserved heterogeneity and the observed explanatory variables is assumed to be linear and has a conditional normal distribution. In practice, this means that the unobserved heterogeneity is modeled as a linear combination of all of the means of the observed explanatory variables over all time periods, as well as their initial values. Additionally, the random effects probit model includes a lagged value of the dependent variable (see Wooldridge 2005 for a series of three models and an empirical example). Under the crucial assumption of conditional normality of the unobserved heterogeneity term, this method produces average partial effects for the variables of interest.

Table 1—Mean Values for Panel Variables for All Survey Waves

| Variable | At least one American Indian parent household Mean | No American Indian parent household Mean | t-statistics for difference in group means |
|---|---|---|---|
| *Panel A. difference-in-difference regressions* | | | |
| Education variables | | | |
| Years of education | 11.21 | 11.96 | −4.10** |
| High school graduation probability at age 19 | 0.62 | 0.69 | −2.12** |
| Received a GED or graduated from high school at age 19 | 0.76 | 0.82 | −2.26** |
| Age, parents, and interaction variables | | | |
| Age cohort initially 9-year-olds | 0.39 | 0.35 | 1.26 |
| Age cohort initially 11-year-olds | 0.33 | 0.34 | −0.51 |
| Age cohort initially 13-year-olds | 0.28 | 0.31 | 0.43 |
| Number of American Indian parents | 1.34 | 0.00 | 20.63** |
| Interaction age 9 cohort × number of American Indian parents | 0.52 | 0.00 | 17.98** |
| Interaction age 11 cohort × number of American Indian parents | 0.45 | 0.00 | 79.58** |
| Household characteristics | | | |
| Male child indicator | 0.52 | 0.53 | −0.29 |
| Mother has a high school degree/GED | 0.36 | 0.29 | 2.31** |
| Father has a high school degree/GED | 0.21 | 0.17 | 1.53 |
| Mother has more than a high school degree | 0.35 | 0.49 | −4.06** |
| Father has more than a high school degree | 0.2 | 0.31 | −3.51** |
| Average years household in poverty over initial 3 years | 1.40 | 0.66 | 9.60** |
| Average household income (by category) for first 3 years | 4.58 | 6.65 | −8.79** |
| Average household income (in dollars using mid point of each category) for first 3 years | 20,919 | 30,377 | −3.96** |
| Crime variables | | | |
| Any crime ages 16–17 | 0.10 | 0.14 | −1.72 |
| Any crime ages 18–19 | 0.17 | 0.22 | −1.81 |
| Any crime ages 20–21 | 0.16 | 0.15 | 0.28 |
| Any minor crime by age 21 | 0.25 | 0.29 | −1.10 |
| Any moderate crime by age 21 | 0.09 | 0.14 | −1.79 |
| Any violent crime by age 21 | 0.04 | 0.05 | −0.86 |
| Ever dealt drugs by age 21 | 0.06 | 0.06 | −0.47 |

| Variable | At least one American Indian parent household Mean | No American Indian parent household Mean | t-statistics for difference in group means | Total observations |
|---|---|---|---|---|
| *Panel B. Fixed effect regressions* | | | | |
| Education variable | | | | |
| Days present at school in last quarter | 39.64 | 39.15 | 1.27 | 3,317 |
| Mother's characteristics | | | | |
| Labor force participation rate | 0.88 | 0.87 | 1.14 | 6,780 |
| Arrest status | 0.12 | 0.06 | 7.51** | 5,333 |
| Supervision of child | 1.81 | 1.79 | 0.89 | 5,758 |
| Activities spent with child | 1.87 | 1.88 | −1.15 | 6,673 |
| Father's characteristics | | | | |
| Labor force participation rate | 0.90 | 0.93 | −3.95** | 4,161 |
| Arrest status | 0.27 | 0.13 | 9.18** | 3,309 |
| Supervision of child | 1.11 | 1.12 | −0.27 | 5,758 |
| Activities spent with child | 1.90 | 1.92 | −1.23 | 3,829 |

*Note:* Sample size differs across these variables due to missing information.
**Significant at the 5 percent level.

attainment, on average, compared to children from households with no American Indian parents.[14] On all measures, children from the first type of household have lower recorded educational attainment or completion.

**Age Cohorts:** The next group of variables indicates the distribution among the different age cohorts and the number of American Indian parents. There is a slightly higher proportion of children found in the 9-year-old age cohort for the American Indian parent household than for the non-American Indian parent household, but this difference is not statistically significant. The second age cohort is much closer in number distribution between the two types of households. The number of American Indian parents and the interaction terms differ between the two household types by design.

**Household Characteristics:** The third set of variables provides a look at the household conditions prior to the opening of the casino for both groups of children. There are level differences between all of the initial household conditions except for the gender distribution for children from both types of households. The parental education variables, unlike the education measures for the child, are given in categories, not in years. It appears that parents from households with at least one American Indian parent tend to be overrepresented in the "high school degree" category as compared to households without American Indian parents. Additionally, households without American Indian parents tend to be overrepresented in the "more than high school education" category. The omitted category is "less than a high school or GED degree." All categories are mutually exclusive for the parental education variables.

The last two variables under household characteristics in Table 1 provide insight into the economic conditions of the households. On average, households with at least one American Indian parent have spent at least one year in poverty in the first three years of the study, while the figure is 0.66 years for the households with no American Indian parents. Income is also given in categories, and the value of 4.58 corresponds to an annual income between $15,001 and $20,000. For households with no American Indian parents, the average household income value of 6.65 falls in the $25,001 to $30,000 annual income category. Using the midpoint of income categories, give an average household income of about $20,000 for American Indian households and an average household income of about $30,000 for non-Indian households in our survey.

**Criminality Measures:** The final set of variables in this panel provide the criminal activity of the sample children. These data are gathered independently from the GSMS data. Searches of public databases in the North Carolina Administrative Office of the Courts produced these data. All counties in North Carolina are covered by these data including arrests made on the American Indian reservation. Arrests after the sixteenth birthday fall under the jurisdiction of the adult criminal justice

[14] The other races in this dataset are white and African American. The African American children make up less than 6 percent of the total observations. Therefore, using non-Indians refers to these two other groups, but whites make up the highest proportion of that group.

system. Arrest records were found for juvenile arrests with the permission of the juvenile court judges. We have classified the arrest records into three broad categories: minor arrests, which includes arrests for disorderly conduct, trespassing and shoplifting; moderate arrests, which are primarily property crimes that do not involve serious harm to a person such as simple assault, felony larceny, and drug-related offenses and violent arrests, which include sexual assault, armed robbery, and assault with deadly weapons. The first set of variables reports whether a child has committed any crime in the years indicated. The categories are not cumulative and are independent of one another. Therefore, we see that a child from a household with at least one American Indian parent had a 10 percent chance of committing any type of crime (minor, moderate, or violent) between the ages of 16 and 17. A child from an American Indian household had a 17 percent chance of committing any type of crime between the ages of 18 and 19. The next set of variables measures whether a child has committed any crime by age 21, by arrest category. The first variable indicates that a child from a household with at least one American Indian parent had a 25 percent chance of having committed a minor crime by age 21, while the same figure for a household with no American Indian parents was 29 percent. Interestingly, children from American Indian households are less likely to have been arrested for all crimes across the board, and statistically significantly less likely to have been arrested (at the 10 percent level) for moderate crimes, by age 21. The final variable is found within the GSMS survey and indicates the child's self-reported drug dealing behavior at each survey wave. The mean of this variable indicates that 6 percent of children from both types of households report ever having dealt drugs.

**Fixed Effects Data:** Panel B of Table 1 provides the data used primarily in the fixed-effects regressions for changes in parental behavior. The first variable gives the number of days the child was present in school in the last quarter. This question is asked at every survey wave while the child is less than 18 years old. There is no statistically significant difference between children in the two types of households.

**Panel Data Characteristics for Parents:** The next set of variables provides characteristics of the mother at each stage over the survey time period. The first variable is coded one for individuals who are in the labor force (working outside of the home) and zero otherwise. There is no statistically significant difference between the labor force participation of mothers by household type. Labor force attachment is a categorical variable that measures (on a scale of zero to four) an individual's degree of involvement in the labor force. A zero indicates no work whatsoever (student, retired, or disabled). One indicates work only in the home. Two indicates currently unemployed. Three indicates part-time employment, and four indicates full-time employment. For mothers, it does not appear that there is any difference in the attachment to the labor force. Mothers who are working tend to be employed less than full time. Arrest status is simply an indicator variable for whether the mother was arrested since the last survey wave. Once again, there is a statistically significant difference here, with mothers from non-American Indian households slightly more likely to have been arrested.

The child supervision variable measures the adequacy of parental supervision of the child. There are three options here: a value of zero indicates that the parent does not have adequate control or knowledge of the child's whereabouts at least 50 percent of the time; a value of one indicates that the parent does not have adequate control or knowledge of the child's whereabouts at least once a week; a value of two indicates that the parent has age appropriate supervision or control over the child. The average value for both groups of households is approximately one, which indicates, on average, for all survey waves, that parents did not know where their children were at least once in the previous reference week. The final variable is a measure of the percentage of parent-child activities and interactions that are categorized as enjoyable by the child at each survey wave. The previous measure of parental supervision was only asked of the parents. The three options possible here are: a value of zero indicates that less than 25 percent of all activities with the parent are enjoyable to the child; a value of one indicates that between 25 percent and 74 percent of all activities are a source of tension, worry, or disinterest to the child; and a value of two indicates that at least 75 percent of all activities are enjoyable. We observe that there is no statistically significant difference between household types for these last two variables. The results for fathers are presented in the next section. There is a statistically significant difference for fathers by type of household for labor force participation, labor force attachment, and arrest status.

*Differences Across Age Cohorts by Observed Characteristics.*—We use the oldest age cohort of children as the control group for the two younger age cohorts. In order for this to be a valid strategy, the different age cohorts must be reasonably similar to one another, on average. We would assume this to be the case as the survey design employed a randomized selection process. Nevertheless, we present evidence of this fact using observable characteristics of the childrens' households. Table 2 presents a comparison of these initial household characteristics by age cohort for each of the two types of households. This table provides information on the suitability of the third age cohorts to serve as controls for the two other age cohorts in this study. In this table, *t*-statistics are presented for a test of a mean difference between the indicated age cohorts for a given variable. In panel A of Table 2, we show the differences in age cohorts for households that have no American Indian parents. There are statistically significant differences in the number of American Indian children in these households for cohorts 2 and 3 (age 11 and age 13 initially) and cohorts 1 and 3 (age 9 and age 13 initially). The difference is driven by the relatively large amount of American Indian children in the third age cohort (7 percent). There is no difference in the gender distribution for any of the three cohorts. We observe little difference in education levels for the parents by age cohorts. We do find statistically significant differences for household income levels for cohorts 1 and 2 as well as for cohorts 1 and 3. The mean difference between income categories is very small here 0.7 and 0.6 for each, respectively. Each income category represents a step of $5,000 each. Therefore, the difference represented here, on average, is between $3,000 and $3,500 per year.

The panel B Table 2 provides a similar analysis for the households with at least one American Indian parent. There appears to be very little difference between these age cohorts. In sum, it appears that the data are reasonably similar across age cohorts for both types of households. While there are some statistically significant

TABLE 2—*t*-SCORES OF MEAN DIFFERENCES BY AGE COHORT AND AMERICAN INDIAN PARENT STATUS

| | Difference between cohort 1 and 2 | Difference between cohort 2 and 3 | Difference between cohort 1 and 3 |
|---|---|---|---|
| *Panel A. Households with no American Indian parent* | | | |
| Number of American Indian parents | N/A | N/A | N/A |
| American Indian indicator | −1.43 | −2.00** | −3.35** |
| Male child indicator | −0.93 | 1.84 | 0.95 |
| Mother has a high school degree/GED | 0.81 | −0.25 | 0.52 |
| Father has a high school degree/GED | <−0.001 | 1.49 | 1.50 |
| Mother has more than a high school degree | −1.51 | 1.21 | −0.23 |
| Father has more than a high school degree | −0.83 | 0.49 | −0.30 |
| Household income | −2.47** | 0.36 | −2.04** |
| *Panel B. Households with at least one American Indian parent* | | | |
| Number of American Indian parents | −0.49 | 1.29 | 0.84 |
| American Indian indicator | −1.89 | 1.86 | 0.04 |
| Male child indicator | −0.56 | 0.05 | −0.46 |
| Mother has a high school degree/GED | 1.06 | −0.05 | 0.93 |
| Father has a high school degree/GED | 1.00 | −1.66 | −0.65 |
| Mother has more than a high school degree | −0.63 | 0.45 | −0.14 |
| Father has more than a high school degree | −0.30 | 0.62 | 0.34 |
| Household income | 0.34 | −1.60 | −1.29 |

*Note:* Each cell provides *t*-statistics for a test of difference in means.
** Significant at the 5 percent level.

differences, the magnitude of these differences for most variables is, in fact, quite small.

*Time Trends*.—It is extremely important in a difference-in-difference framework that we control for any changes that may have occurred in these communities unrelated to the casino disbursements over time. Children from households that are not eligible for casino disbursements allow us to control for these changes. We show that the two types of households (those with and without American Indian parents) are affected similarly by the general macroeconomic and social conditions in this region. While, in general, the American Indian households tend to perform slightly worse on most measures, the rate of change over time is indistinguishable from that of non-American Indian households in the region. Absolute differences in average conditions or characteristics are permissible in the difference-in-difference framework as long as the rate of change prior to the intervention was stable across both groups. We provide some evidence on the similarity of the time trends of the two types of households in the time period prior to the opening of the casino. It is not, of course, possible to show how the unobserved heterogeneity effect evolves over time for the two types of households. However, we do show that the households have similar trends in a number of dimensions. Figure 1 provides the trend in household incomes for the two types of households, and we have already noted that there is a significant difference after the opening of the casino. Prior to the opening of the casino, however, the growth in the percentage of households with incomes greater than $30,000 was similar between the two groups. A simple test comparing the two trend lines in the first three survey years (prior to casino operations) results in a $p$-value of 0.178, indicating that we fail to reject the null hypothesis that these two trend lines are similar. After the first three survey waves, the proportion of American Indian households earning more than $30,000 increases, and we see a convergence to that of non-American Indian households in later years. The two straight lines in the figure indicate the long-run trend line for both types of households. There is a marked change for American Indian households.

Table 3 shows the unemployment rate for mothers and fathers in the first three survey waves. The unemployment rate was generally decreasing for both household types. The $p$-values associated with these tables are 0.78 and 0.176, respectively, which fails to reject the null hypothesis that these two groups (American Indian and non-Indian households) experience similar changes in unemployment rates in the first three survey waves. The bottom part of Table 3 shows the difference in reported incidence of alcohol or drug abuse problems for the father as reported by the mother.[15] While there is a slight decrease in the incidence between periods 1 and 2, the incidence of alcohol or drug abuse is a relatively constant distance between waves 2 and 3. The $p$-value for this table is 0.39, which fails to reject the null hypothesis of similarity of the two trends.

Taken together, Figure 1 and Table 3 indicate that the two types of households, while differing in levels, appear to be equally affected by the same social conditions, macroeconomic conditions, and labor market experiences. The Eastern Cherokee reservation is located in the middle of the 11 counties surveyed in this research. There is little evidence to support that the two household types are affected differently by changes at the local level in the period prior to the casino opening.

Additionally, testing between the nature of household types across time, it appears that there is no statistically significant difference in the composition of households across time. In Web Appendix Table 3, we provide $t$-tests of differences in marital status for the household types after the casino begins operations. The additional casino funds do not appear to affect the marital status of couples included in this data. This finding indicates that the casino payments are not creating incentives for the dissolution or the creation of new partnerships, which may directly affect the young adult outcome of children.

[15] We take the report of the first parent about the second parent's drug and alcohol abuse to be more accurate than the self-reported information about the first parent's own drug and alcohol problems. There is reason to suspect that there would be problems with a self-reported measure of drug and alcohol abuse, but less so with regard to the other parent.

Table 3—Time Trends for Parents' Unemployment Rates and Father's Drug and Alcohol Use in the First Three Survey Waves

| Year | At least one American Indian parent in household | No American Indian parent in household |
|---|---|---|
| *Panel A. Mother's unemployment rate* | | |
| 1993 | 17 | 13 |
| 1994 | 12 | 11 |
| 1995 | 11 | 8 |

*Notes:* The $p$-value for the hypothesis that the changes in the unemployment rate are the same for each type of household is 0.78. We fail to reject the null hypothesis that the trends are the same.

| Year | At least one American Indian parent in household | No American Indian parent in household |
|---|---|---|
| *Panel B. Father's unemployment rate* | | |
| 1993 | 8.5 | 3.97 |
| 1994 | 7.1 | 3.6 |
| 1995 | 4.95 | 1.8 |

*Notes:* The $p$-value for the hypothesis that the changes in the unemployment rate are the same for each type of household is 0.176. We fail to reject the null hypothesis that the trends are the same.

| Year | At least one American Indian parent in household | No American Indian parent in household |
|---|---|---|
| *Panel C. Father's reported drug and alcohol incidence by data waves as reported by mother in percent* | | |
| 1993 | 12.2 | 4.73 |
| 1994 | 9.5 | 4.91 |
| 1995 | 8.8 | 3.9 |

*Notes:* The $p$-value for the hypothesis that the change in drug and alcohol incidence is the same for each type of household is 0.39. We fail to reject the null hypothesis that the trends are the same.

## II. The Effects of Exogenous Change in Income on Young Adult Educational Attainment and Criminal Behavior

In this section, we present the results from the difference-in-difference regression described in equation (1) and the fixed effects regression described in equation (2). All of the results control for robust standard errors or clustered standard errors at the individual level in the fixed effects regressions, and employ survey weights. Where the outcome variables are indicator variables, we use a probit specification and report marginal coefficients. For continuous outcome variables, such as years of education, we use a simple ordinary least squares regression for our analysis.[16]

### A. *Education Outcome Variables*

Table 4 presents the results from regressions for the educational outcome variables. The first column presents the regression of years of completed child's education at age 21 on the level and interaction variables previously described. The two interaction

[16] In the following regressions, the sample sizes vary primarily because of missing information in the outcome variables. We take advantage of the maximum number of observations possible for each outcome variable and do not restrict our analysis to a smaller subset. Reducing the sample size does not appear to affect the sign or magnitude of results, however, the standard errors do increase somewhat, as expected.

Table 4—Effect of Cash Transfer on Children's Educational Achievement

| Independent variables | Years of education, age 21<br>Coefficient | Probability of HS graduate, age 19<br>Marg. eff. | Probability of HS graduate/GED, age 19<br>Marg. eff. |
|---|---|---|---|
| Interaction 1: age cohort 1 × number of American Indian parents | 0.379<br>(0.447) | 0.156**<br>(0.073) | 0.086<br>(0.054) |
| Interaction 2: age cohort 2 × number of American Indian parents | 0.117<br>(0.304) | 0.042<br>(0.066) | 0.033<br>(0.044) |
| Age cohort 1 (9 years old) | −0.269<br>(0.294) | −0.025<br>(0.060) | −0.019<br>(0.0457) |
| Age cohort 2 (11 years old) | 0.072<br>(0.275) | −0.010<br>(0.055) | −0.016<br>(0.041) |
| Number of American Indian parents in household | −0.503<br>(0.350) | −0.156<br>(0.068) | −0.131***<br>(0.047) |
| American Indian | 0.003<br>(0.472) | 0.081<br>(0.063) | 0.075<br>(0.038) |
| Sex | −0.639***<br>(0.227) | −0.123***<br>(0.043) | −0.081***<br>(0.033) |
| Mother has a high school degree/GED | 0.557<br>(0.399) | 0.103*<br>(0.051) | 0.079**<br>(0.034) |
| Father has a high school degree/GED | −0.164<br>(0.396) | 0.001<br>(0.067) | 0.026<br>(0.044) |
| Mother has more than a high school degree | 0.924**<br>(0.367) | 0.117**<br>(0.058) | 0.129***<br>(0.045) |
| Father has more than a high school degree | 0.757**<br>(0.306) | 0.053<br>(0.056) | 0.051<br>(0.040) |
| Household previously in poverty indicator variable | −0.120<br>(0.174) | −0.045<br>(0.028) | −0.026<br>(0.019) |
| Average household income in first three survey waves | 0.214**<br>(0.048) | 0.031***<br>(0.010) | 0.022***<br>(0.007) |
| Constant | 10.554<br>(0.532) | | |
| Observations | 1,045 | 1,060 | 1,060 |

*Notes:* Years of education regressions are ordinary least squares. The next two regressions are probit regressions with marginal effects calculated. Robust standard errors are given in parentheses below the estimated coefficients.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

variables presented in the first two rows indicate that there is a positive, but not statistically significant, effect of residing in a household with exogenously increased incomes for six or four years relative to two years. The coefficient on the first interaction variable indicates that children who reside in treatment households and come from the youngest age cohort have, on average, about four months more education at age 21 than their untreated counterparts, although this coefficient is not statistically significant.

The other variables of interest in the regression are the parental education variables. The more than high school education variables are positive and statistically significant for both parents. The average household income in the first three survey waves variable is also positive and statistically significant in this and the other two regressions. Column two presents the probability of a child being a high school graduate by age 19. The marginal effect on the first interaction variable indicates that the effect of having four more years of exogenously increased household income increases a child's probability of finishing high school by age 19 by almost 15 percent. The second interaction coefficient is positive, but smaller in absolute magnitude, and not statistically significant. The third column outcome variable measures whether an individual has a high school diploma or a general equivalency degree. The first interaction coefficient is, once again, positive but not statistically significant at conventional levels. It is important to note that the American Indian children had an incentive to finish high school by age 18 as they became eligible for payment of the semi-annual casino payments themselves; otherwise they would have to wait until age 21. In that sense, we should interpret the changes in high school graduation rates as similar to an outcome from a traditional conditional cash transfer program (see work by T. Paul Schultz 2000 or Jere R. Behrman, Susan W. Parker, and Petra E. Todd 2005). After age 21, however, all of these American Indian children receive the transfers regardless of high school completion.

### B. *Educational Outcome by Previous Poverty Status, Child Gender, and Parental Gender*

We now investigate whether the exogenous increase in incomes has differing impact by the prior poverty status of households. The first four columns of Table 5 present the same analysis as Table 4, except that the sample has been divided according to whether the household was previously in poverty, prior to casino operation.[17] We find in the first two regressions, for households previously in poverty, that the coefficient on the first interaction term is always statistically significant at the 5 percent level and larger in magnitude than in Table 4. The coefficient for the interaction variable for the years of education regression triples in size and implies that the treatment of four additional years of exogenously increased income increases educational attainment at age 21 by a full year (1.1 years).[18] The first interaction variable coefficient for the high school graduation regression increases in magnitude and is highly statistically significant. The next two columns present the results from the subsample of households that were never in poverty in the first three survey waves. None of the coefficients on the interaction variables are statistically significant. These results explain the results for the full sample, which yielded statistically insignificant results for the years of education regression. The additional household income does not have a noticeable effect in households not previously in poverty.[19]

[17] Uses the US poverty levels adjusted for household size.

[18] Future survey waves will collect data on educational attainment when the children are 24 and 25 years old. This will allow for an additional look at the educational attainment as well as college completion rates.

[19] As a robustness check, we create a predicted poverty rate for the sample using parent's education and employment status, county dummy variables, and household size; this robustness check is presented in Web

Table 5—Effect of Cash Transfer on Educational Achievement by Previous Household Poverty Status and Child Gender

| | Household previously in poverty | | Household not previously in poverty | | Male child | | Female child | |
|---|---|---|---|---|---|---|---|---|
| | Years of education, age 21 | Probability of HS graduation, age 19 | Years of education, age 21 | Probability of HS graduation, age 19 | Years of education, age 21 | Probability of HS graduation, age 19 | Years of education, age 21 | Probability of HS graduation, age 19 |
| Independent variables | Coefficient | Coefficient | Marginal effects | Marginal effects | Coefficient | Coefficient | Marginal effects | Marginal effects |
| Interaction 1: age cohort 1 × number of American Indian parents | 1.127*** | 0.391*** | −0.166 | 0.129 | 0.586 | 0.164 | 0.809 | 0.196*** |
| | (0.449) | (0.135) | (0.722) | (0.085) | (0.421) | (0.100) | (0.597) | (0.086) |
| Interaction 2: age cohort 2 × number of American Indian parents | 0.451 | 0.298** | −0.058 | 0.011 | 0.470 | 0.053 | 0.100 | 0.047 |
| | (0.436) | (0.140) | (0.422) | (0.075) | (0.384) | (0.099) | (0.448) | (0.082) |
| Observations | 438 | 444 | 607 | 616 | 548 | 553 | 497 | 507 |

*Notes:* Includes American Indian indicator, gender, mother's highest educational attainment, father's highest educational attainment, average household income prior to casino operation, age cohorts, and a constant. The years of education regressions are ordinary least squares, the probability of high school graduation regressions are probit regressions with marginal effects calculated. Robust standard errors are given in parentheses below the estimated coefficients.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

The next four columns of Table 5 divide the data according to the sex of the child in the survey, and provide the same analysis for the educational outcome variables. In the first set of columns, the sample is restricted to male children, and the next set of columns presents only the female children's regressions. Examining the years of education regressions for each gender, it does not appear that years of education is differentially affected by restricting the sample by gender. Females appear to have a higher likelihood of finishing high school on time than males.[20] The results here are not as clear as the division by previous poverty status.

Table 6 disaggregates the data by the gender of the parent receiving the additional household income in order to investigate whether the additional household income has differential effects by the gender of recipient. In the two regressions, mothers who receive the additional household income have a positive and statistically significant effect on the total years of education and high school graduation rates for

Appendix Table 4. This poverty measure increased the number of non-American Indian households in poverty. Therefore, when we restrict analysis to only the households previously in predicted poverty, the sample size increases by about 150 to 600. With the predicted poverty rate, there is a greater balance of households in the poverty subsample between American Indian and non-American Indian households in the ratio of 1.25:1. The results from the regressions do not change substantially. In the regression of years of education on the first interaction variable, the estimated coefficient is 1.273 and statistically significant at the 1 percent level. In the probability of high school completion regression, the estimated coefficient for the interaction variable is 0.266 and, again, is statistically significant at the 5 percent level.

[20] Others have found that increasing household incomes in developing countries can have a differential impact on children depending upon their gender. Different household responsibilities along gender lines imply that additional income will change the composition of work or duties for the household children. See, for example, Joyce J. Chen (2006).

Table 6—Effect of Cash Transfer on Educational Achievement by Parental Gender

| Independent variables | Years of education, age 21<br>Coefficient | Probability of HS graduation, age 19<br>Marginal effects |
|---|---|---|
| Interaction 1: age cohort 1 | 1.48** | 0.148* |
| × American Indian mother | (0.606) | (0.053) |
| Interaction 2: age cohort 2 | 0.724 | 0.0141* |
| × American Indian mother | (0.507) | (0.052) |
| Interaction 3: age cohort 1 | −0.915 | 0.114 |
| × American Indian father | (1.158) | (0.076) |
| Interaction 4: age cohort 2 | −0.886 | −0.180 |
| × American Indian father | (0.699) | (0.161) |
| Observations | 1,044 | 1,059 |

*Notes:* Includes American Indian indicator, gender, mother's highest educational attainment, father's highest educational attainment, average household income prior to casino operation, age cohorts, and a constant. Robust standard errors are given in parentheses below the estimated coefficients. The years of education regressions are ordinary least squares. The probability of high school graduation regressions are probit regressions with marginal effects calculated.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

their children. Fathers, on the other hand, appear to have no noticeable impact when they receive additional household income. These results are qualitatively similar to Duflo (2003). In her paper, Duflo (2003) discusses the incentives for grandmothers to invest in their grandchildren as they have longer life expectancies than grandfathers, and therefore reap the benefit of this grandchild health investment. It is not possible to conduct a similar analysis in our data without information on household expenditures. Nonetheless, it is still plausible that mothers spend more on their own children as they anticipate reaping the most benefit and assistance from their children later in life.

### C. *School Attendance in the Past Three Months*

A secondary check on a child's educational achievement is a simple measure of school attendance. We investigate whether additional income affects school attendance rates throughout childhood. The dataset contains a variable that indicates the number of days present in school in the three months prior to the survey interview date. This particular question is asked at all of the childhood surveys. We remove all time-invariant household characteristics (both observed and unobserved), and control for the time-varying characteristics directly in our fixed effects regression. Table 7 presents these fixed effects results. In the first column, we regress the number of days present in school in the last three months on the household's casino payment eligibility, household income, parental ages, child's age, and the number of children less than six years old in the household. The results indicate that casino payment eligibility increases school attendance by almost two and a half days per quarter. Dividing the data by households that previously were in poverty, we find that the

TABLE 7—EFFECT OF CASH TRANSFER ON CHILD'S SCHOOL ATTENDANCE IN DAYS FOR THE PREVIOUS QUARTER

| Independent variables | Number of days present within the last 3 months | Number of days present within the last 3 months if household previously in poverty | Number of days present within the last 3 months if household never in poverty |
|---|---|---|---|
| | Coefficient | Coefficient | Coefficient |
| Household eligible for casino disbursement | 2.43* | 3.85** | 2.420 |
| | (1.280) | (1.943) | (1.720) |
| Age of child | 0.105 | −0.768 | 0.295 |
| | (0.169) | (0.342) | (0.195) |
| Number of children less than 6 years old | 0.447 | 1.156 | −0.591 |
| | (0.614) | (0.794) | (0.946) |
| Observations | 3,317 | 1,120 | 2,197 |
| Number of groups | 1,110 | 444 | 666 |

*Notes:* All three regressions are ordinary least squares regressions with fixed effects. Standard errors are clustered at the individual level and are given in parentheses below the estimated coefficients. Includes parents' ages, income and income squared, and a constant.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

effect almost doubles in size. Children from the poorest households with this additional income are present at school for almost four moredays than their untreated counterparts.[21] The effect is still positive, however, it is not statistically significant, for the households that previously were not in poverty. Overall, the additional household income appears to have a very strong effect on the child's school attendance at each survey wave.

## D. *Criminal Behavior During Young Adulthood by Age and Offense Type*

Table 8 examines the criminal behavior of all of the sample children. Administrative data have been merged with the GSMS data at the individual level, with information on the number and nature of each crime for all of the survey children. In the final column of Table 8, we utilize self-reported data on drug dealing activities by the child. We classified the arrests into three broad categories: minor, moderate, and violent offenses. Minor offenses include disorderly conduct, trespassing, and shoplifting. Moderate offenses are property crimes such as felony larceny, drug-related crimes, and simple assault. Major offenses include sexual assault, armed robbery, and assault with a deadly weapon. Additionally, information about when the arrests occurred allows us to identify the age (16–21) of arrest for each person.

We report marginal effects from the difference-in-difference regressions in Table 8. The first three columns of Table 8 indicate that children from households

[21] Using a predicted poverty measure (see note above) to restrict our sample, we find that children from households receiving the additional casino household income attend 3.4 more days of school (significant at the 10 percent level) per quarter than their untreated counterparts.

TABLE 8—EFFECT OF CASH TRANSFER ON DRUG DEALING AND CRIMINAL ARRESTS BY AGE AND OFFENSE TYPE

| | Any crime by age | | | Ever committed a crime by type | | | Self-reported drug dealing |
|---|---|---|---|---|---|---|---|
| | Committed any crime, age 16–17 | Committed any crime, age 18–19 | Committed any crime, age 20–21 | Ever committed a minor crime by age 21 | Ever committed a moderate crime by age 21 | Ever committed a violent crime by age 21 | Ever dealt drugs by age 21 |
| Independent variables | Marginal effects | Marginal effects | Marginal effects | Marginal effects | Marginal effects | Marginal effects | Marginal effects |
| Interaction 1: age cohort 1 × number of American Indian parents | −0.224*** | −0.068 | 0.051 | −0.179** | −0.002 | 0.002 | −0.065* |
| | (0.078) | (0.072) | (0.075) | (0.089) | (0.065) | (0.012) | (0.033) |
| Interaction 2: age cohort 2 × number of American Indian parents | −0.108* | −0.026 | 0.008 | −0.078 | −0.022 | −0.005 | −0.005 |
| | (0.064) | (0.069) | (0.062) | (0.088) | (0.049) | (0.014) | (0.020) |
| Age cohort 1 (9 years old) | 0.076* | −0.011 | −0.068** | −0.051 | −0.017 | −0.003 | 0.000 |
| | (0.043) | (0.052) | (0.033) | (0.055) | (0.026) | (0.009) | (0.016) |
| Age cohort 2 (11 years old) | −0.017 | −0.047 | −0.056 | −0.097* | −0.044* | 0.009 | 0.023 |
| | (0.036) | (0.049) | (0.033) | (0.053) | (0.022) | (0.011) | (0.017) |
| Number of American Indian parents | 0.136 | −0.043 | 0.091 | 0.096 | 0.114* | −0.011 | −0.019 |
| | (0.091) | (0.063) | (0.078) | (0.094) | (0.068) | (0.010) | (0.019) |
| Observations | 1,093 | 1,061 | 1,045 | 1,045 | 1,045 | 1,045 | 1,045 |

*Notes:* All regressions are probit regressions with marginal effects estimated. The robust standard errors are given in the parentheses below the estimated coefficients. Includes American Indian indicator, gender, mother's highest educational attainment, father's highest educational attainment, average household income prior to casino operation, and a constant.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

that receive casino payments are 22 percent less likely to have been arrested at ages 16–17 than their untreated counterparts.[22] Examining the effect on criminality in later years, ages 18–21, the additional household income has no direct effect on criminal arrests for either the first age cohort or the second age cohort. This result is somewhat puzzling but may be due to the fact that the children are no longer under their parents direct control after age 18. Therefore, the diversion in criminal behavior and arrests appears to be directly related to the child's minor status. The reduction in these criminal arrests is due to a reduction in male criminal activity.There is very little female criminal activity in general.

The next three columns in Table 8 present the effect of additional household income on the child's criminal behavior by the type of crime committed. These three columns indicate that the reduction in criminal behavior occurs only in minor

[22] The results are also robust to the inclusion of a distance measure between the household and casino (see Web Appendix Table 2). As mentioned in a previous note, one may view this variable as a proxy for noncash related effects of the casino on households. In the regression of commiting a crime at ages 16–17, the coefficient on the first interaction variable is negative and statistically significant at the 1 percent level with a point estimate of −0.225. Adding in an interaction term of distance with casino payment eligibility does not significantly change the results. The estimated coefficient is −0.299 and remains statstically significant at the 1 percent level.

crimes. By age 21, a child who resided in a household with the additional casino income has an almost 18 percent lower probability of having ever committed a minor crime than a similar child from an untreated household. Further regressions that examined the effect of additional household income on the number of crimes (by category) did not yield significant results. This indicates that the additional income affected whether an individual entered into criminal behavior, but not the number of crimes once they had entered into criminality. Conducting a separate analysis for males alone, we find that the results hold up for minor crimes, if slightly diminished in significance, and become rather strong for moderate crimes.

A final measure of child criminal behavior is provided in the final column of Table 8. The child's self-reported drug dealing activities are regressed on the same set of explanatory variables used in the previous regression. The first interaction term indicates that children from households with exogenously increased incomes are almost 7 percent less likely to have reported dealing drugs in their youth. Restricting this to households that were previously in poverty, we do not find that there are any differential effects by previous poverty status. Additional exogenous household income reduces the incidence of drug dealing for all types of households equally.

## III. Potential Mechanisms

The previous section provided evidence that the exogenous increase in household income has positively affected young adult outcomes for children from these households. The results indicate that children from households with additional income have better educational attainment and reduced criminal behavior. In this section, we discuss a few of the potential mechanisms that may be contributing to the observed changes in child outcomes.

There are several potential explanations for why increased incomes may affect the young adult child outcomes. One potential explanation is that the additional household income is used to purchase better quality educational inputs. Unfortunately, the data does not contain consumption or expenditure data.[23]

### A. *Parental Labor Force Participation Rates*

A second potential explanation is that parents use their additional income to substitute away from full-time employment and into more childrearing. We have information on both parents' labor force participation rates for each interview wave. Because we have panel data with regard to the parental labor force participation, we employ a random effects probit model (Wooldridge 2005) for the mother and father. In the first two columns of Table 9, we regress mother's labor force participation on whether the household was eligible for casino disbursements, a lag of household income, number of children less than six years old in the household, and mother's

[23] It is not possible to ascertain the degree to which parents are spending in the casino in this data. However, we have examined the child's gambling behavior and there does not appear to be any differential effect of the additional household income on the child's gambling behavior in young adulthood.

Table 9—Effect of Cash Transfer on Parental Labor Force Participation

| Independent variables | Mother's labor force participation (FT, PT, UE) Marginal effects | Mother's labor force participation (FT) Marginal effects | Father's labor force participation (FT, PT, UE) Marginal effects | Father's labor force participation (FT) Marginal effects |
|---|---|---|---|---|
| Household eligible for casino disbursement | 0.069 | −0.089 | −0.013 | 0.044 |
| | (0.196) | (0.287) | (0.385) | (0.392) |
| Lag of household income | 0.020 | −0.011 | 0.072 | −0.046 |
| | (0.028) | (0.370) | (0.072) | (0.073) |
| Number of children less than 6 years old | 0.031 | −0.03 | −0.236 | 0.054 |
| | (0.096) | (0.125) | (0.285) | (0.296) |
| Mother's age | 0.011 | 0.021 | | |
| | (0.017) | (0.023) | | |
| Father's age | | | −0.102** | 0.122*** |
| | | | (0.044) | (0.047) |
| Observations | 3,318 | 3,318 | 1,988 | 1,988 |
| Number of groups | 1,076 | 1,076 | 643 | 643 |

*Notes:* Random effects probit regression specification for all four models as suggested by Wooldridge (2005). The regressions all include mother's (father's) initial labor force status, a lagged variable for mother's (father's) labor force status, a constant and the mean over all time periods for the following variables: household eligibility for casino, mother's (father's) age, the lag of household income, and number of children below age 6. Robust standard errors are indicated below each estimated coefficient. A linear probability model with standard errors clustered at the individual level provides qualitatively similar results. These results are robust to omitting the lagged dependent variable as an explanatory variable.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

age. We also include additional control variables as suggested by the random effects probit model: the mean of all explanatory variables over all time periods, initial values of the explanatory variables, and a lag of the dependent variable.[24] The outcome variable is binary with one indicating either full-time employment, part-time employment, or currently unemployed. A zero indicates the individual is out of the labor force—either retired, disabled, or a household worker for no pay. A positive marginal effect for the casino eligibility variable indicates that the additional household income increases the labor force participation. The second regression uses a slightly more restrictive labor force participation binary variable. A value of one indicates full-time participation only with zero being all other possibilities. Our results for both measures of labor force participation rates indicate that the additional household income does not affect the mother's labor force participation. A similar analysis is carried out for men in the next two columns. The point estimates are small in size and are also statistically insignificant. Therefore, it appears that households affected by cash transfers are not reducing their labor force participation.

[24] The results presented in Table 8 are robust to removal of the lagged dependent variable; we find no change in parental employment status in this alternative specification of the model (see the Web Appendix Table 12). Additionally, a linear probability model provides qualitatively similar results.

### B. *Parental Behavior and Quality Measures*

A third explanation is that parental quality improves with additional income. Increased household incomes may translate into lower levels of household stress and disruption. There is existing research that indicates moving out of poverty may improve parental quality. Using the National Longitudinal Study of Youth (NLSY) data, McLeod and Shanahan (1993) find that currently poor mothers are more likely to spank their children and are less responsive to child needs. They also find that the persistence of poverty increases the direct internalization symptoms in children. Sampson and Laub (1994) find that poverty decreases adult stability and good decision making. Ennis, Hobfoll, and Schröder. (2000) have found that poverty can adversely affect mental health and depression among parents. Rand D. Conger et al. (1994) find direct evidence that not having sufficient income produces stresses on individual parents.

We explore the possibility that the additional household income affects parental behavior and parent-child relationships. Additional information is available with regard to the two parents' arrests since the last interview at each survey wave. In Table 10, we examine the effect of the per capita transfer on parental arrests. The first two columns present a linear probability model estimate for whether the mother or the father was arrested in the previous year at each survey wave.[25] The results indicate that both mothers and fathers have a reduced probability of being arrested when they come from households with the casino payments. This effect is intensified for the households that were previously in poverty, however, the sample size falls dramatically and is not shown here.

The results for parental arrests indicate that parents are engaging in less destructive behavior as a result of the increased income. This improvement in parental behavior and choices also tends to spill over into parent-child interactions and supervision. The GSMS data contain measures of parental supervision that ask the parent, at each interview wave, the percentage of time they know their child's whereabouts and activities. In the next three columns of Table 10, we conduct a fixed effects regression of the mother's and father's reported supervision of their child on the household's eligibility for casino payments, the child's age, household income, parental ages, and the number of children below age six in the household. The positive coefficient on the casino disbursement indicates an improvement in mothers' and fathers' supervision separately, as well as jointly, in households receiving the additional income. These variables are given in categories and the mean is about 1.9 (where 2 represents age-appropriate knowledge of child's whereabouts) for both mothers and fathers. Therefore, there is a 3 percent and 5 percent improvement in the parental supervision of their children over time. We find these to be moderate to large effects. They are larger in magnitude than even the coefficient on the child's age, which should be an important determinant of parental supervision.

Finally, the last two columns of Table 10 present a direct measure of parental quality as reported by the child. Previous parental behavioral information was provided

[25] An analysis using a random effects probit model yielded very similar results.

Table 10—Effect of Cash Transfer on Parenting Measures and Parental Arrests

| | Parental arrests | | Parental supervision | | | Parental activities with child | |
|---|---|---|---|---|---|---|---|
| | Mother arrest since last interview | Father arrest since last interview | Mother's supervision | Father's supervision | Parental supervision | Activities with mother | Activities with father |
| Independent variables | Coefficient | Coefficient | Coefficient | Coefficient | Coefficient | Coefficient | Coefficient |
| Household eligible for casino disbursement | −0.039** | −0.107*** | 0.062*** | 0.096*** | 0.179** | 0.069*** | 0.035 |
| | (0.019) | (0.039) | (0.023) | (0.032) | (0.067) | (0.024) | (0.036) |
| Mother's age | −0.008*** | | −0.001 | | −0.003 | −0.003 | |
| | (0.002) | | (0.004) | | (0.010) | (0.004) | |
| Father's age | | −0.021*** | | 0.003 | 0.003 | | −0.007 |
| | | (0.003) | | (0.003) | (0.008) | | 0.004 |
| Age of child | | | −0.014*** | −0.023*** | −0.045*** | −0.007 | −0.009 |
| | | | (0.005) | (0.006) | (0.016) | (0.005) | (0.006) |
| Number of children less than 6 years old | 0.032** | 0.010 | −0.018 | −0.045 | −0.067 | −0.014 | 0.002 |
| | (0.014) | (0.029) | (0.012) | (0.030) | (0.060) | (0.018) | (0.017) |
| Observations | 3,483 | 2,169 | 3,802 | 2,365 | 2,025 | 3,802 | 2,367 |
| Number of groups | 1,139 | 723 | 1,163 | 745 | 637 | 1,163 | 745 |

*Notes:* The parental arrests regressions are a linear probability fixed effects regression. The regressions include mother's (father's) labor force status in each period, and a lag of household income for each period, and a constant. Clustered standard (at the individual level) errors are indicated below each estimated coefficient. A random effects probit regression provides qualitatively similar results for the parental arrest outcomes (which are binary outcomes). The parental supervision and activities with parents regressions are linear probability fixed effects regressions. The clustered standard errors are provided below the estimated coefficients. These regressions include controls for income, income squared, labor force status of the mother and father, and a constant.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

by the parent at all survey waves. The variable we consider measures the amount of positive interactions between the child and parent from the child's perspective in the previous reference week. In both cases, the estimated coefficient is positive which indicates an improvement in parent-child interactions. The results indicate that there is a large improvement in the relationship between the child and the mother, and that this improvement is statistically significant. The results are not statistically significant with regard to the father, even though the estimated coefficient is of the same sign as the mother. Once again, we find these effects to be moderate to large relative to the other explanatory variables. The effect of the casino eligibility improves parent-child relationships by about 4 percent for mothers.

Overall, the results indicate that parents in households with additional incomes make better choices in their personal behavior and with regard to criminal behavior. They do not appear to make significant changes in their labor force participation efforts. Children report better relationships over time in the households with additional income, and parents report better supervision of their children over time in these same households. While there are many potential causal mechanisms at work here, it is useful to learn that parental time is not responsible for the observed changes in child outcomes. Parental quality and interaction with their children appears to be an important factor for explaining how additional household income translates into better child outcomes.

## IV. Discussion and Conclusion

Our results indicate that changes in the permanent income of a household can have permanent effects. The effect on children continues on into young adulthood in our sample. We have seen that an exogenous treatment of increasing income tends to improve the overall child outcomes in terms of educational attainment at ages 19 and 21 and in terms of reduced criminal behavior at ages 16 and 17. Given the unique design of the research, we are able to control for several important confounding factors that might otherwise be the cause of the observed changes. We have been able to control for cohort differences by using a control group of nontreated households in our sample. Additionally, the comparison between the age 9 and age 13 cohorts provides us with the counterfactual observations of a household in which incomes were unchanged for a shorter period of time (6 years versus 2 years). We find that, in general, there is an overall improvement in the outcomes of the American Indian children, while those of the non-American Indian children have remained mostly stable. We see, for the educational outcomes, that American Indians have made big strides and have converged to that of the non-American Indians. On the other hand, with regard to the criminal arrests, American Indians have diverged and now are less likely than the non-American Indians (whose rates of arrests remained constant over time) to commit these minor crimes.[26]

We have also explored a couple of the potential mechanisms that transform additional household income into better child outcomes. While it is not possible, in this analysis, to definitively identify the true causal mechanism responsible for the improvement in young adult outcomes, we have been able to identify a few changes in parental behavior (parental quality) that are suggestive of a mechanism. Parents have a better overall relationship with their children after the additional household income is introduced, as evidenced by responses from both the parent and child. Additionally, parents appear to have less problems over time once the exogenous income is introduced. We see that fathers are less likely to be arrested themselves over time. On the other hand, we do not have much evidence that the additional income is used by parents to make a dramatic shift from labor force participation toward more child care (parental quantity). While our data is not perfect, it appears

[26] The high school graduation rate for non-American Indians at age 19 is 0.70, and 0.66 for the age 9 and age 13 cohorts, respectively. The rates are 0.67 and 0.57 for the American Indians in the age 9 and age 13 cohorts, respectively. The age 9 cohorts have higher graduation rates than their age 13 counterparts, and this amount has increased most dramatically for the American Indian cohort. With regard to the education variable, the years of educational attainment at age 21 is 11.6 for the age 9 cohort, and 12.18 for the age 13 cohort of non-American Indians. However, after dropping a few extreme outliers, the average years of educational attainment is 12.08 for the age 9 cohort with no change in the age 13 cohort. For American Indians, the average years of educational attainment is 11.3 for the age 9 cohort and 10.9 for the age 13 cohort. Omitting a few extreme outliers, the average years of educational attainment is 11.4 for the age 9 cohort and 11.1 for the age 13 cohort. Again, we see that there is a positive gap between the age 9 and age 13 cohort for American Indians and almost no difference (or even a slight negative one) for the non-American Indian children. The probability of having ever committed a minor criminal act is 0.31 for both age cohorts of non-American Indians. It is only 0.19 for the age 9 cohort of American Indians, while it is 0.28 for the age 13 cohort. In this case, the youngest age cohort of American Indians have decreased their criminal behavior dramatically relative to both the age 13 cohort of American Indians and the non-American Indian children as a whole. Overall the effects suggest that the American Indian children are moving, in all cases, in the beneficial direction, while the non-American Indian children are basically remaining stable (with slight increases/decreases over time in a couple outcomes) relative to the American Indian children.

that neither mothers nor fathers are leaving the labor force because of the additional household income. More research that focuses on the mechanisms that translate household incomes into child well-being is certainly needed.

It is important to note the differences from this research and previous efforts. The program described here differs in at least two dimensions: size and duration. The size of the casino payments is large relative to other income augmentation programs, and certainly with regard to other quasi-experimental policies. The additional $4,000 dollars per year represents anywhere from one-fourth to one-third of the income for many of these households. Second, this casino disbursement program has no foreseeable end date. While it is contingent upon successful and continued operations of the casino, there has been no indication that there would be a change in the program or that profits have decreased over time. Therefore, people treat these changes in their income as permanent and spend accordingly. These two effects are probably responsible for the large effects found in this research, which are not often evident in studies with smaller amounts and temporary income changes.

Future work will allow us to explore the effect of this additional income on the geographic mobility of the children. The casino payments are not limited by geographic proximity to the Eastern Cherokee reservation. Therefore, in future work, we anticipate evaluating how this additional income has increased the geographic distribution of these children from American Indian households. Individuals may move out of state, and they will still be eligible for casino payments. In future survey waves, we will also have additional employment information for the children at ages 24 and 25, which will allow us to explore whether they enter into different occupations and industries, and any resulting wage differentials.

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