Rich Dad, Poor Dad: Short- and Long-Term Effects of Quasi-Experimental Income Transfers on Children's and Young Adult Well-being

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Abstract: We make use of a quasi-experimental program to examine the effects of unconditional cash transfers on children's health and wellbeing later in life. Our longitudinal data set follows the same children from age nine to their mid-twenties. A subset of the children's households receive an exogenous unconditional income transfer. This is the first research to show that unconditional cash transfers can have lasting long-term impacts on both labor market and non-labor market outcomes for the children of the affected families. The increase in household incomes changes child trajectories in an overwhelmingly beneficial direction – in terms of inprovements in education, income, employment, marital status, and a reduction in drug usage and drunkenness. Our results indicate that the biggest gains are experienced by households that were previously in poverty. Cost-benefit analysis shows that the program recoups initial outlays within two years of realized adult outcomes.

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I. Introduction

There has been a strong interest among economists in studying the effects of in-utero and early life shocks to health and wellbeing on long-term health and socio-economic outcomes for the affected individuals. A consensus has emerged that adverse shocks to maternal health and wellbeing during the fetal development period have long-term negative consequences on children's health (van den Berg, Lindeboom and Portrait, 2006; Almond, 2006), and that early life insults to health affect long-term economic and social outcomes (Currie et al, 2010). At the same time, a number of studies have shown that early intervention school-based programs such as the Perry preschool program and Head Start may have long-term beneficial effects on children's socio-economic success (for a recent review, see Barnett, 2011).

Public policy could also work to counterbalance any such adverse shocks by providing extra resources to affected families. Some work has been done to identify the long-term effects of welfare programs such as Food Stamps (Almond at al, 2010) and Head Start (e.g. Currie and Thomas, 1995; Carneiro and Grinja, 2011; Frisvold, 2007). However, a recent review of the literature identifies few studies that have attempted to test how pure income transfers to families affect the short- and long-term wellbeing of their children (Almond and Currie, 2009). Pure cash transfers to the entire distribution of household types are rare; most programs are aimed directly at very poor households or have extensive requirements for eligibility.[1] Furthermore, the existing literature has focused on studying the short-term effects of income transfers (Dahl and Lochner, 2005; Milligan and Stabile, forthcoming). Studies that evaluate long-term effects of public policy interventions have concentrated on the small set of pre-school or primary-school based programs that affected children at very young ages (for a comprehensive review see Cunha et al, 2006). We know little about the persistence of these effects into adulthood, whether the age of intervention matters, and whether the affected outcomes are cognitive (as measured by test scores), behavioral, or health-related.

[1] There is a separate literature evaluating conditional cash transfers, which tie extra income receipt to fulfilling different conditionalities.

We study the effects of a quasi-experimental cash transfer program on the long-term health and wellbeing of children from affected families. Children are followed from age nine to their mid- to late twenties. The cash transfers to the families begin when the children are in their early-to-mid teenage years. We consider wage income, employment and educational attainment, as well as criminality, alcohol and drug consumption at three later stages – at age 19, age 21, and ages 23-28. We find persistent positive effects of cash transfers on long-term wellbeing of the affected individuals. Therefore, program evaluations based on short-term outcomes may miss significant gains that might accrue in the longer-term. Our findings also demonstrate that policy interventions at ages beyond infancy and early childhood can have a positive impact on long-term outcomes and so the "window of opportunity" extends to the early teenage period. Finally, we show that some of the social problems that characterize an unequal society can be alleviated by directly reducing household income inequalities.

In the next section we discuss some of the relevant background literature. Section 3 introduces the data and explains the empirical strategy. Next, we report and comment on the results from the empirical estimations and conduct a basic cost-benefits analysis based on our findings. The last section concludes.

II. **Background**

There are many ways in which poverty can affect child wellbeing. Families with lower incomes make different investment decisions, and face more rigid short-term constraints in their optimization decisions. They may face different production technologies and have worse access to health inputs. Pure cash transfers might improve access to inputs, without directly affecting the production function facing poor households. Programs such as Head Start and the Perry school project alter the technologies available to disadvantaged households. It's an empirical question whether providing access to better (or more) inputs without necessarily changing the production technology could improve outcomes for children coming from poor households. A somewhat related question is whether the effects of permanent transfers differ significantly from the impact of temporary (perceived as transitory) cash assistance.

Studies that make use of plausibly exogenous changes in income that have little direct effect on child outcomes are hard to come by. Dahl and Lochner (2005) use variation in the amount of the EITC households are eligible for over time and household type to identify the effects of household income and find that each $1000 of income improves children's test scores by 0.02% to 0.04% of a standard deviation. This result implies that it would take on the order of a $10,000 transfer to have an educationally meaningful effect on test scores.

Milligan and Stabile (2008) use a social experiment resulting from changes in Canadian child benefits to study how exogenous changes in family income affects children's school performance and psychological wellbeing. An advantage of their research is that the changes in income were not tied to other changes in family behavior, in contrast to programs like the EITC. They find that an extra $1000 of child benefits leads to an increase of about 0.07 of a standard deviation in the math scores and in the Peabody Picture Vocabulary Test, a standardized test of language ability for four to six year old children. If we think of a change of a third or a half a standard deviation in test scores as a meaningful educational effect, then these results suggest that an increase of as little as $5000 in family income has a meaningful effect. The study also considers effects on other indicators. They find that higher child benefits lower aggression in children and decrease depression scores for mothers. They do not find evidence that income transfers directly affect short-run physical health measures.

Almond et al (2010) examine the impact of introducing the Food Stamps program on long-term economic and health outcomes for the affected children. Their results suggest that the strongest long-term effects are in the area of health. Weaker long-term benefits are found also in the area of economic success. These findings underscore an important question that is central to the literature, namely whether shocks at certain ages matter more than others. The idea is that certain functionalities must be acquired at a particular point in life, and if they are not acquired at that point, they will either not be acquired at all, or will not be acquired properly. We still have little evidence of critical periods in humans.

Using the same quasi-experimental transfers that we analyze here, Akee et al (2010, 2011) find that exogenous income transfers received by the household during a child's teenage years have positive short-run effects on educational attainment. Receipt of extra income was also linked to reduced criminality and better health as measured by BMI in young adulthood. An interesting feature of their results is that the education and criminality benefits seem to affect mostly children coming from poor families, while the health benefits accrue to children coming from better-off households. This suggests that the channels through which extra income affects children's wellbeing differ depending on the outcome. In this research we extend the time frame to the children's mid-20s and examine additional measures of wellbeing and socio-economic success later in life.

III. Data and Empirical Methodology

The Great Smoky Mountains Study of Youth (GSMS) is a longitudinal survey of 1420 children aged 9, 11 and 13 years at the survey intake that 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.[2] 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 two of the 11 counties within the study. The initial survey contained 350 Indian children and 1070 non-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 non-response 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.[3] Additional survey waves were carried out for these children when they turn 25 and 26 years old. Both parents and children were interviewed separately up until the child was 16 years old; interviews after that were conducted with the child alone.

[2] See Costello E. Jane, Adrian Angold, and Barbara Burns, and Dalene Stangl, and Dan L. Tweed, and Alaatin Erkanli, and Carol M. Worthman (1996) for a thorough description of the original survey methodology.

[3] 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% of the sample in the very first survey wave and comprise approximately 27% of the sample at age 21.

After the fourth wave of the study, a casino was opened on the Eastern Cherokee reservation; the survey children were approximately 13, 15 and 17 years of age at that time. 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.[4] Disbursements are made every six months and have occurred since 1996. The average annual amount per person has been approximately $4000. This income is subject to the federal income tax requirements.

Data Set Description

Table 1 provides the means of our outcomes of interest. The first panel provides summary statistics from the pre-casino period and the outcome variables for the survey subjects at age 19. The average pre-casino household income is around thirty thousand US, which is close to the number reported by the Census for this rural area in North Carolina. One in every three households spent at least one year below the poverty line before the casino transfers commenced. One in three mothers has high school education only, and almost half of them have attended some college. The average age at intake for the children is 10.8 years. On average only 9 percent of the survey sample repeated a grade by age 19. The average years of education was a little over 11 years, with a maximum of 16 years and a minimum of 1 year.[5] Approximately 63 percent are high school graduates by age 19. Survey respondents indicate that 43% have ever tried marijuana and almost 30% had been drunk in the past 3 months. The average number of drinks per week that survey respondents reported was almost 3. The incidence of self-reported crimes is very low at less than 1 %. Very similar results are provided in the second panel for the age 21 and 23 outcome variables.

Empirical Specification

[4] All adult tribal members received these per capita disbursements. If there were any non-compliers (American Indian parents that either did not receive or refused the additional income) then any estimates found here would be an under estimate of the true effects of additional income. All enrolled, American Indian children were eligible for the casino disbursements themselves at age 18 if they completed high school; even if they did not complete high school they would receive the casino transfers at age 21. While they initially did not know exactly how much the transfers will amount to, tribal members had every reason to believe that this was a permanent positive change in their incomes.

[5] There are a few outliers who report less than 6 years of education at age 19. Omitting these observations does not change our results.

We compare young adult outcomes for children that resided for a total of six years as minors in households with increased incomes to children who resided for just two years as minors in households with exogenously increased incomes. We employ a difference-in-difference methodology. This specification 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.

In the equation above, Y is the outcome variable of interest for the child at ages 19, 21 or 23. We will examine educational attainment, high school completion variables and criminal arrests at various ages (16-21). The Age9 and Age11 variables indicate whether or not the child is drawn from the youngest or second-youngest cohorts respectively -- the age 13 cohort is the omitted category (oldest at intake) 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 γ□and γ□, 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 average household income over those years, mother's education levels, the sex of the child, and the race of the child.[6]

In the analysis that follows, we also divide our data by households that prior to the casino opening were in poverty and those that weren't. We conduct the difference in difference

[6] In previous research (Akee, et al, 2010) we discuss the characteristics of this particular situation which provide us with a very well specifid identification of the effect of the cash transfer on the long-run outcomes. Specifically, there is no choice involved in the receipt of the casino payments. Additionally, no other systematic household, political or economic changes occurred during the children's time in the household that could be driving our results. We test for these potential mechanisms in the previous paper and do not find evidence for these alternatives driving our results.

regressions for each of these subsamples in order to examine the effect of the additional household income on these two different types of households.

IV. Results

Before we proceed with the estimation results from the difference-in-differences model in (1), we present the entire distribution of educational attainment and labor market earnings for tribal members and non-members by cohort in figures one and two. Our working hypothesis is that the casino transfers were more beneficial for Native American children in the youngest and middle cohorts. We also assume that the casino transfers did not have a direct effect on non-tribal members, therefore differences between the three cohorts of white children can be used as a counterfactual.

In figure 1 we plot the distribution of years of obtained education at age 21. There are two clearly identifiable groups – those who dropped out of high school and those who went to college. While the distributions are fairly similar among non-members of different cohorts, there is a marked increase in the education of the youngest group of Native Americans. They are much less likely to drop out of high school and much more likely to continue to college than either of the older cohorts of tribal members. We interpret this as suggestive evidence that spending more time as a minor in a household receiving extra income is beneficial for educational attainment in early adulthood.

In figure 2 we plot the corresponding distributions of income at ages 24-27. The oldest cohort of children were interviewed somewhat later (average age 26.4) than the middle cohort (average age 25.3) and the youngest age group (average age 24.6). Therefore the oldest cohort would earn on average more than the second and the youngest cohorts because of more experience in the labor market. This is exactly what we see in the distribution of non-tribal members' earnings. However, more of the Native American children from the youngest cohort earn over 35 thousand dollars and fewer of them earn between 10 and 25 thousand than the middle and the oldest cohort. This is consistent with the evidence on educational attainment and implies that the casino transfers could improve labor market outcomes for the children of the initially treated households.

Next, we turn to our regression results. We first present the difference in difference results for the complete sample and we do not differentiate between children whose households were ever in poverty and their better-off neighbors. We separate outcomes into labor market and non-labor market outcomes. Outcomes that are potentially relevant for the labor market include: educational attainment, income, and employment. Non-labor market outcome include criminality, drug and alcohol consumption, and obesity status. We first show empirical results for labor market outcomes in Table 2. The results in the first column indicate that an individual from the youngest age cohort is least likely to have ever having repeated a grade relative to his older counterparts. The probability that a child would repeat a grade is more than 8% lower for an individual from the youngest cohort relative to the oldest age cohort. The effects on the second oldest cohort is about half the size in magnitude but does not attain statistical significance. The corresponding coefficients for the high school graduation rate are of the expected sign but do not attain statistical significance either. We see no corresponding effect for the total years of education at ages 19 and 21 in columns 3 and 4. [7]

Finally in columns 5 and 6 we examine economic outcomes at ages 23 and older. We find that current income is higher and statistically significant for both the youngest and the middle cohorts of Native American children relative to the oldest age cohort. The survey respondent's income is given in categories of \$5000, so the first coefficient in column 5 translates to approximate \$8000 more in annual income than the oldest counterparts. The middle cohort attain on average \$3700 more than the oldest cohort. This may be partially explained by the results found in column 6. In this regression we find that individuals who come from the youngest age cohort are more likely to be employed in the past three months. This employment result may help explain the relatively higher levels of annual income for the youngest age cohorts. The effect on the middle cohort appears to be around half of the coefficient on the youngest cohort, however it is not statistically precise. It is important to note that the effects of exogenous income transfers on later-life labor market-related outcomes appear to increase monotonically with each cohort. Unfortunately we are not able

[7] Previous research (Akee, et al 2010) found a similar result for the effect on total years of education; separating the analysis by household poverty status, however, indicated a strong effect of the casino transfer payment on years of education for the poorest households. We repeat part of this analysis in the next section.

to distinguish whether the age at initial treatment or the dosage (duration) of treatment is the driving factor behind the differences between cohorts.

Next we examine criminal activity and marijuana use at different ages. Table 3 reports the results. Individuals from the youngest cohort of Native American children are less likely to have been involved in criminal behavior by ages 21 and 23. The survey question asks whether the individual has ever committee any crime, so it is indicative that the coefficient on the longest treatment group increases over time. The effects of growing up in households receiving extra income persist and grow in significance with age.

In column 6, we find that additional household income has a negative effect on a child ever using marijuana. The coefficient is large and statistically significant. The effect is also negative for the middle age cohort, but it is both smaller in magnitude and lacking in statistical significance. Interestingly, these effects disappear in the longer term. There is no statistically significant difference between marijuana use by ages 21 and 23 among the three treatment cohorts. Taken together with the previous results on criminal behavior, we take this to mean that additional household income works to reduce the child's bad behaviors in the short run (while they are still living in the parents' household), but not on a longer horizon.

Turning to alcohol consumption and obesity in Table 4, we find that there are significant effects on the incidence of obesity by ages 21 and 23, but not at age 19. In previous research (Akee et al, 2011) we reported that the effects of extra income transfers on obesity vary with initial income. We return to this analysis by poverty status in the next section. The casino transfers also had significant negative effects on alcohol consumption at all ages. The youngest cohort of treated children are between 12 and 15 percent less likely to report having been drunk in the past three months at ages 19 and above. The number of drinks consumed in the last 3 months also steadily decreases over time relative to the oldest cohort. Again, the coefficients on the middle cohort are of the expected sign and smaller in magnitude than the youngest cohort effects, however they lack statistical significance.

Overall, the casino transfer payments had significant positive effects on both labor market and non-labor market outcomes for the children who spent the longest time in treatment households and were treated at the youngest ages. Previous research suggests that the transfers had different effects depending on the level of initial household income pretreatment. A commonly used marker is whether the household was ever below the Federally mandated poverty level. In the next section we investigate whether the beneficial effects of exogenous income worked differently among children who grew up in poorer households.

Results by Initial Household Poverty Status

In this section, we separate the sample by initial poverty status and reexamine all previous outcome variables. Table 5 provides the regression results from the difference in difference regressions for labor-market related outcomes. In the first two columns we see that the effect of the additional household payment reduces the likelihood that a child from the youngest age cohort repeats a grade relative to the oldest age cohort of children. The effect is statistically significant for households that were previously in poverty (column 2) and much larger in magnitude than in column 1. For poor children, the casino transfer results in a 23 percentage point reduction in the probability of repeating a grade. High school graduation is close to 32 percentage points more likely among the youngest children from poor households, and 23 percentage points more likely among the middle cohort. These results accord with expectations that additional household income should have an effect for those households that were previously the most budget constrained.

The next set of educational variables indicate the total years of education at ages 19 and 21 is greater for only the youngest individuals from households that were previously in poverty. The coefficient in column 7 indicates that the extra income resulted in more than one more year of education by age 21. This is additional evidence that household income has an important effect on the years of educational attainment. We find that the results are large and statistically significant relative to the middle age cohort and the oldest age cohort of children. There are no statistically significant results for the households that were previously not in poverty. Overall these results provide substantial evidence that the exogenous change in income has an effect on several measures of educational attainment and achievement.

As columns 9-12 make apparent, this extra educational attainment mostly likely affected incomes and job retention at ages 23 and above. We see a sizeable effect on incomes of children coming from better-off families, up to a $6,000 increase relative to the oldest cohort in annual earnings. But the effect for children from poor backgrounds is much larger at $10,000 for both the youngest and the middle cohorts! Job retention is also much better among these individuals – up to 38 percentage points better for the youngest children, and up to 17 percentage points better for the middle cohort. Overall, these findings imply that lifting children out of poverty has large and long-lasting effects on their educational and labor market success.

The next set of outcome variables examines health outcomes by initial poverty status. The effects on obesity are mixed – while at age 21 the youngest cohort is less likely to be obese across both poverty levels, at age 23 only individuals coming from better-off households are within normal body weight. The effects on alcohol consumption are similar. Both groups benefit from the income transfers, but the effects vary over time. At age 19 those from poorer households experience much stronger reductions in drinking than their richer peers. By age 23 the trend reverses, and individuals coming from better-off households report lower incidence of drunkenness in the past 3 months. This is supported by the differences in the number of drinks consumed over the same period. Overall, these results suggest that the non-labor market effects of extra income transfers are stronger in the short-term for children coming from poorer households, but they disappear at older ages. On the contrary, children coming from better off households are more likely to experience increasing long-term health effects. This stands in contrast with our findings on education and labor market outcomes, and suggests that extra income works differently across socio-economic markers depending on the individual's socio-economic background.

Finally we turn to criminality and drug use in table 6. Individuals from the youngest age cohort are statistically less likely to report ever having used marijuana by age 19 than their older (less treated) counterparts. These results are large in magnitude and statistically significant. The effect is smaller in magnitude and of the same sign for the middle age cohort, however the results are not statistically significant. There are no corresponding results for the households that previously were not in poverty. We find that the additional household income does not affect the number of drinks that individuals consume in the past week for any household type or treatment type.

There effect of additional household income does diminish the self-reporting of being drunk in the past three months for individuals from the poorest households. The coefficient for individuals from the youngest age cohort that receive the casino payment in column 12 is negative and statistically significant. The age and SES patterns that emerge from the criminality and drug use results are largely in line with the results on health and drinking reported in Table 5. While we are restricted by small sample sizes, we interpret these results as suggestive evidence that socio-economic background, and in particular income, affect children's long-term wellbeing differentially across labor and non-labor market outcomes. The beneficial effect on labor market outcomes are cumulative, i.e. once the extra human capital investment is in place, it creates long-lasting positive returns. On the other hand, effects on health and social behavior such as criminality and alcohol consumption appear shorter-lived among those coming from poor backgrounds. Nevertheless, even such short-term improvements are significant and large enough to positively affect societal welfare.

V. Discussion and Cost/ Benefit Analysis

In the previous analysis we provided evidence that a cash transfer intervention can have short term and long term effects on affected children. In this section, we seek to quantify at least a few of the benefits in monetary terms to compare it to the costs of the program. Benefits accrue both privately to the individual and to society. For example, increases in human capital translate into higher earnings for the individual and higher taxes that benefit the general public. We concentrate the analysis on private benefits from earnings and public benefits from reduced crime.

We find that by age 19 there are decreases in the marijuana use and incidence of drunkenness by children who grew up in households that received the casino transfer payment. There were also fewer criminal arrests. We will attempt to calculate both short run and long-run monetary benefits to the cash intervention. The long run benefits by age 23 indicate that the treated children are more likely to be employed, earn more and have higher levels of education.

Non-labor Market Outcomes

There are several changes in the behaviors of the children directly affected by this cash transfer program. One of the most striking results is that there is an increase in the education levels of the treated children and completion of high school. Also, there is a reduction in the amount of criminal arrests up to age 18 for the treated children. Additionally, there is a reduction in the use of both marijuana and alcohol abuse for these children as minors. Finally, we find that there is a reduction in obesity rates for treated children from the initially wealthier households.

In general it is difficult to quantify the monetary benefit of these effects. Specifically, many of these observed improved behaviors will interact directly with an individual's health as well as labor market success. We focus on the short run outcomes of two outcomes that are well-specified – criminal arrests and obesity. By focusing on just these two short run outcomes, we provide a conservative estimate of the benefits from the cash transfers.

We focus also on the reduction in the number and type of criminal arrests. The benefits of such a reduction in crimes are not easily defined; however, we follow previous researchers. We use an updated version of the Lochner and Moretti (2004) cost of burglaries which is estimated at $1,314 per arrest. We find that the probability of ever committing and crime by age 23 is about 10% lower for the youngest group of Native American children. The total number of crimes committed by this group by age 23 is 213. If we assume that all crimes result in arrests, this indicates that there were approximately 21 less minor crimes due to this cash transfer program. The total savings due to reduction in costs in the administrative justice system are$27,600.

The Center for Disease Control estimates that obese people spent about $1500 more in health care costs each year. We find that by their mid-20s tribal members of the youngest cohort are about 12% less likely to be obese than their older brothers. This translates into 5 fewer obese individuals in the youngest cohort due to longer exposure to the casino transfers. The implied yearly savings from reduced health care costs only amount to $7500.

Long Run Benefits

Tables 3 and 5 indicate that there are improvements in marital status, employment and education for children from the youngest age cohort that receive the casino transfer payments. We believe that these characteristics contribute to better economic outcomes such as wages. We have a measure of the individual's annual income (inclusive of transfers) and examine the annual gains to income in dollar amounts for these individuals. In Table 3, we see that individuals from the youngest age cohort on average have incomes that are 1.181 categories higher than their least treated counterparts; these income categories corresponds to $5000 unit interval. Therefore, if we attribute the marginal gain to each of the treated individuals in each age cohort, we get the following results:

	N individuals	Gain per individual	Total
Annual gain in income for individuals in middle cohort	117	$3700.00	$432,900.00
Annual gain in income for individuals in youngest cohort	109	$5909.00	$644,181.00
Total income gain per year			$1,076,981.00

In the table above, the second column provides the monetary benefits of differences in income categories, for instance $5000 x 1.181 = $5909. We then multiply this amount by the number of affected individuals in each of the age cohorts to get the number in the final column (109x $5909). In total, the average private annual benefit from the cash transfer payment is over one million dollars per year.

Our benefits analysis was conducted relative to children from the oldest age cohort (age 17 at first treatment). Therefore we also calculate the costs relative to the oldest age cohort. If we compare the benefits to the actual costs of the program, which was approximately $4000 per year while the child was a minor in the household (5 years for this youngest age cohort; 3 more years relative to the oldest age cohort) for these affected children the net costs are the following:

<u>Youngest cohort</u>

4 x $4000 = $16,000

<u>Middle cohort</u>

2x $4000 = $8,000

$16,000 x 109= $1,744,000 $8,000 x 117 = 936,000

Therefore, the program is not justifiable on purely economic terms in the first year of benefits. However, it would just take a little over thirty months for the program to worthwhile. Accumulated annual incomes for treated individuals would then exceed the costs of the program.

VI. Conclusion

We have provided evidence that unconditional cash transfer programs can have long-run effects on young adult outcomes. In a longitudinal data set we have found effects for children who resided in households with exogenously increased incomes during they adolescence.

Public policies that aim to reduce the long-term effects of childhood deprivation have traditionally focused on interventions in educational settings. We show that household income transfers without any attached conditionalities achieve similar improvements in education and long-term earnings and decrease criminality. The positive effects are stronger for children coming from worse socio-economic backgrounds. Thus, unearned exogenous household income transfers work to reduce inequality in the long run. The only outcome that exhibits the opposite SES gradient is obesity. We find that initial income is negatively associated with the marginal effects of extra income on BMI. This suggests that the channels linking deprivation and obesity are different from the ones that affect later-life socio-economic status.

We believe this is the first documentation of such long run effects. This is also the first evaluation of casino-related income transfers on individual health and SES outcomes of tribal member children. Poverty is the underlying cause of many social inequalities later in life. One way to reduce inequality is to provide policy interventions aimed at reducing the effects of poverty on human capital development. Another is to reduce poverty itself.

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Tables and Figures

Table 1. Means and standard deviations of key variables and outcomes

	Pre-casino	Age 19	Age 21 and 23
Average pre-casino household income	$30658		
	[17245]		
Mother has high school education	30%		
Mother has college	47%		
Age	10.8		
Below the poverty line	33%		
Ever Repeat a Grade?		0.090	
		[0.287]	
Total Years of Education		11.375	
		[2.058]	
High School Graduate		0.630	
		[0.483]	
Ever use Marijuana?		0.432	0.649
		[0.496]	[0.478]
Number of Drinks Per Week		2.700	4.235
		[8.880]	[11.335]
Ever Drunk in the past 3 months?		0.284	0.233
		[0.451]	[0.423]
Obese?		0.233	0.353
		[0.423]	[0.478]
Ever Committed any Crime?		0.003	0.221
		[0.053]	[0.415]
Note: Total number of observations is 1076 for age 19 outcomes			
Currently Married?			0.264
			[0.441]
Current Income			4.563
			[2.793]
Had a job in the past 3 months?			0.849
			[0.358]

Note: Total number of observations is 952 for age 21 outcome and 963 for age 23 outcomes.
Standard deviations in square brackets under the mean

Figure 1: Distribution of educational attainment by cohort

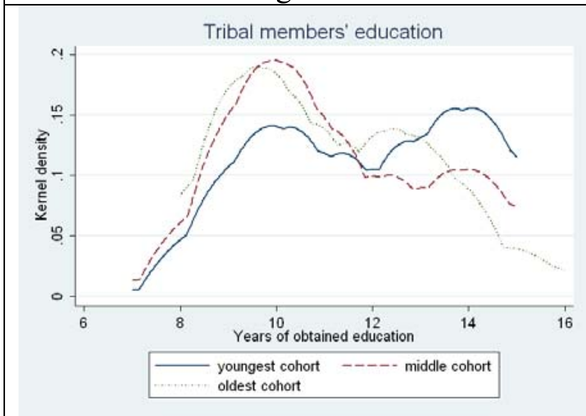
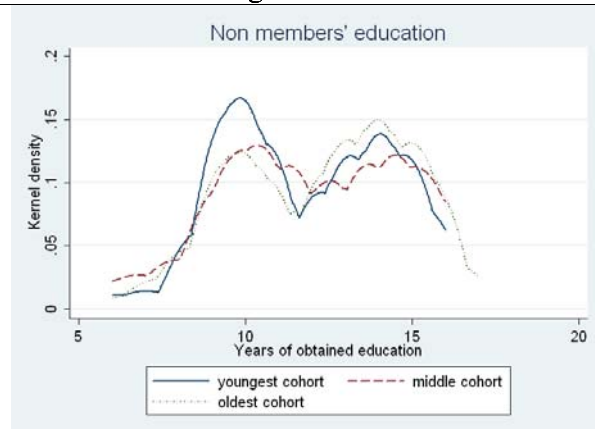
Tribal members at age 21	Non-members at age 21
	

Figure 2: Distribution of labor income by cohort at ages 24-27

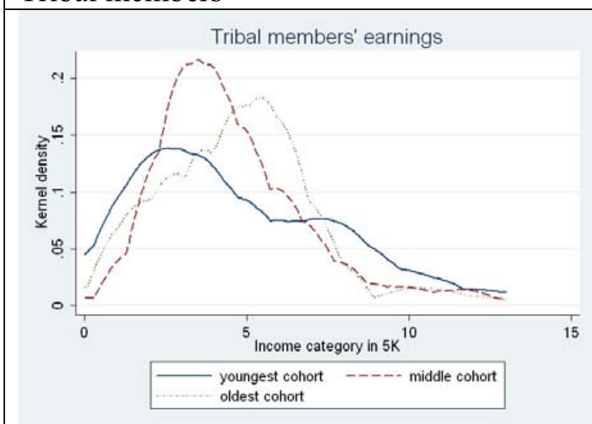
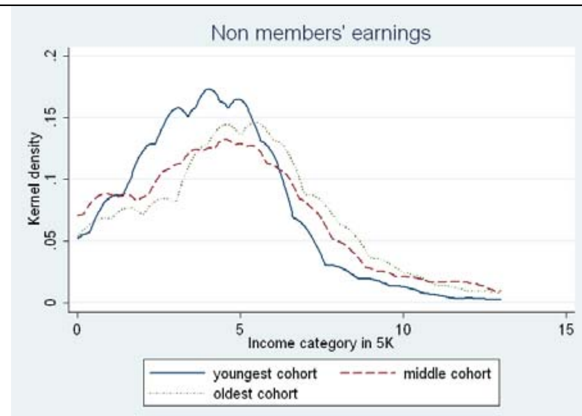
Tribal members	Non-members
	

Table 2 : Difference in Difference Regressions for Labor Market Outcomes

VARIABLES	(1) Ever Repeat a Grade?	(2) High School Graduate at 19	(3) Years of Education at 19	(4) Years of Education at 21	(5) Current Income at Age 23+	(6) Had a job in the past 3 months Age 23+?
Age Cohort 1 x Number of American Indian Parents	-0.0827**	0.0921	-0.0649	0.600	1.181***	0.130**
	(0.0378)	(0.0789)	(0.373)	(0.538)	(0.456)	(0.0595)
Age Cohort 2 x Number of American Indian Parents	-0.0396	0.0308	0.243	-0.283	0.739**	0.0638
	(0.0388)	(0.0741)	(0.313)	(0.338)	(0.372)	(0.0504)
Average HH Income	-0.00645**	0.0422***	0.0978***	0.315***	0.185***	0.0146***
	(0.00309)	(0.00673)	(0.0325)	(0.0348)	(0.0530)	(0.00519)
Age Cohort 1 (13 yo)	0.000839	-0.0201	-0.204	-0.751***	-1.188***	-0.127***
	(0.0241)	(0.0564)	(0.212)	(0.276)	(0.419)	(0.0436)
Age Cohort 2 (15 yo)	-0.0172	0.00846	-0.0851	0.523	-0.985**	-0.0641**
	(0.0216)	(0.0508)	(0.200)	(0.343)	(0.391)	(0.0309)
Number of AI Parents	0.0422	-0.214***	-0.468	-0.480*	-0.001	-0.0647
	(0.0335)	(0.0707)	(0.298)	(0.268)	(0.398)	(0.0494)
Sex	0.0204	-0.123***	-0.531***	-0.745***	1.094***	0.129***
	(0.0183)	(0.0396)	(0.163)	(0.236)	(0.314)	(0.0312)
Mother has a High School Diploma	-0.0205	0.133**	0.701**	0.0779	0.399	-0.0175
	(0.0296)	(0.0613)	(0.306)	(0.381)	(0.469)	(0.0500)
Mother has Some College or More	-0.0139	0.0978	0.963***	0.839***	0.427	0.0296
	(0.0293)	(0.0598)	(0.292)	(0.321)	(0.330)	(0.0403)
Father has a High School Diploma	0.0746*	0.0482	-0.209	-0.845**	-0.666	0.0273
	(0.0412)	(0.0638)	(0.293)	(0.417)	(0.482)	(0.0435)
Father has Some College or More	-0.00336	0.0424	0.461**	0.797***	0.0552	-0.0324
	(0.0225)	(0.0533)	(0.213)	(0.289)	(0.366)	(0.0405)
Observations	1,084	1,084	1,085	952	1,023	1,043
R-squared	0.044	0.175	0.171	0.347	0.122	0.097

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

Table 3: Difference in Difference Regressions for Non-Labor Market Outcomes - Crime

VARIABLES	(1) Ever Committed a Crime Age 21?	(2) Ever Committed a Crime Age 23?	(3) N Crimes Age 21	(4) N Crimes Age 23	(6) Ever use Marijuana Age 19?	(7) Ever use Marijuana at 21?	(8) Ever use Marijuana 23?
Age Cohort 1 x Number of American Indian	-0.0550*	-0.102*	-1.418	-1.279*	-0.134**	-0.0882	-0.0511
Parents	(0.0302)	(0.0593)	(0.889)	(0.733)	(0.0566)	(0.0630)	(0.0695)
Age Cohort 2 x Number of American Indian	-0.0294	-0.00669	-1.098	-0.342	-0.0521	0.0374	0.0458
Parents	(0.0231)	(0.0568)	(0.860)	(0.713)	(0.0590)	(0.0645)	(0.0593)
Average HH	0.000697	-0.0108**	0.0323	-0.168***	-0.0132**	-0.00509	-0.0110**
Income	(0.00236)	(0.00473)	(0.0415)	(0.0601)	(0.00546)	(0.00575)	(0.00548)
Age Cohort 1 (13 yo)	0.0499***	-0.0670*	-0.108	-0.816*	0.0273	-0.0979**	-0.107**
	(0.0169)	(0.0395)	(0.395)	(0.457)	(0.0439)	(0.0403)	(0.0471)
Age Cohort 2 (15 yo)	-0.0108	-0.0940***	-0.771**	-0.895**	0.0507	0.0373	-0.100***
	(0.0122)	(0.0331)	(0.368)	(0.397)	(0.0420)	(0.0473)	(0.0385)
Number of AI Parents	0.0170	0.0933	0.726	0.948	0.0346	-0.0519	-0.0203
	(0.0290)	(0.0597)	(1.045)	(0.763)	(0.0546)	(0.0583)	(0.0639)
Sex	0.0234*	0.181***	0.275	2.168***	0.135***	0.194***	0.0962***
	(0.0122)	(0.0247)	(0.284)	(0.293)	(0.0300)	(0.0309)	(0.0300)
Mother has a High School	-0.0144	-0.00432	-0.281	0.124	-0.0653	-0.0519	-0.0148
Diploma	(0.0191)	(0.0379)	(0.435)	(0.439)	(0.0419)	(0.0409)	(0.0413)
Mother has Some College or	-0.00570	-0.0703**	0.0157	-0.228	-0.00542	0.0711*	-0.00285
More	(0.0184)	(0.0334)	(0.428)	(0.390)	(0.0410)	(0.0418)	(0.0388)
Father has a High School	-0.00587	-0.0267	0.0198	-0.0947	-0.0142	0.0444	-0.0634
Diploma	(0.0189)	(0.0383)	(0.467)	(0.483)	(0.0434)	(0.0448)	(0.0454)
Father has Some College or	-0.0148	-0.0190	-0.396	-0.0326	-0.000645	0.00378	0.00586
More	(0.0189)	(0.0342)	(0.329)	(0.433)	(0.0410)	(0.0438)	(0.0407)
Observations	952	1,050	952	1,050	1,085	952	1,050
R-squared	0.024	0.086	0.023	0.080	0.036	0.077	0.028

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

Table 4: Difference in Difference Regressions for Non-Labor Market Outcomes - Health

	(10)	(11)	(12)	(14)	(15)	(16)	(18)	(19)	(20)
		Obese			Ever Drunk in the past 3 months			N Drinks in the past 3 months	
VARIABLES	At 19?	At 21?	At 23?	At 19?	At 21?	At 23?	At 19	At 21	At 23
Age Cohort 1 x Number of American Indian Parents	0.0286	-0.166***	-0.120*	-0.158***	-0.156***	-0.121**	-0.988	-1.298	-5.061***
	(0.0559)	(0.0605)	(0.0672)	(0.0498)	(0.0557)	(0.0602)	(0.857)	(0.957)	(1.756)
Age Cohort 2 x Number of American Indian Parents	0.0288	0.0197	0.0117	0.00944	-0.0631	-0.0143	0.201	0.603	-1.786
	(0.0572)	(0.0635)	(0.0554)	(0.0574)	(0.0613)	(0.0544)	(1.101)	(1.203)	(1.964)
Average HH Income	-0.00615	-0.0104**	-0.00541	-0.000883	0.0169***	-0.00576	-0.0440	-0.0708	-0.111
	(0.00416)	(0.00515)	(0.00501)	(0.00496)	(0.00564)	(0.00492)	(0.0941)	(0.0937)	(0.151)
Age Cohort 1 (13 yo)	0.0449	0.00182	0.0721	-0.00340	0.0716*	0.0309	-0.487	0.363	-0.490
	(0.0343)	(0.0355)	(0.0472)	(0.0395)	(0.0408)	(0.0432)	(0.650)	(0.693)	(0.733)
Age Cohort 2 (15 yo)	-0.00343	0.0112	-0.0195	-0.00176	0.0369	-0.0651*	-0.352	-0.158	0.358
	(0.0313)	(0.0411)	(0.0358)	(0.0385)	(0.0467)	(0.0338)	(0.745)	(0.768)	(0.918)
Number of AI Parents	-0.00745	0.0232	0.143**	0.0884*	-0.00135	0.0420	0.0164	-0.773	3.342*
	(0.0530)	(0.0559)	(0.0580)	(0.0521)	(0.0546)	(0.0571)	(0.843)	(0.969)	(1.980)
Sex	0.0438*	-0.0247	-0.0241	0.120***	0.151***	0.182***	3.299***	2.910***	5.059***
	(0.0247)	(0.0283)	(0.0286)	(0.0272)	(0.0315)	(0.0265)	(0.497)	(0.504)	(0.712)
Mother has a High School Diploma	0.0490	-0.0256	0.0595	-0.0126	-0.0387	-0.0542	1.245*	-0.483	-1.378
	(0.0361)	(0.0403)	(0.0417)	(0.0370)	(0.0429)	(0.0391)	(0.672)	(0.604)	(1.303)
Mother has Some College or More	0.00590	-0.0616	-0.0314	0.0197	0.0869**	-0.0400	1.388**	1.429**	-2.249**
	(0.0332)	(0.0387)	(0.0378)	(0.0362)	(0.0424)	(0.0354)	(0.608)	(0.700)	(1.144)
Father has a High School Diploma	-0.0230	-0.0192	0.0136	-0.0228	0.0330	0.0279	0.518	1.836**	2.167
	(0.0363)	(0.0388)	(0.0429)	(0.0382)	(0.0450)	(0.0389)	(0.932)	(0.907)	(1.323)

Father has Some College or More	-0.0708**	-0.0180	-0.0516	0.0432	-0.0119	0.0210	0.105	1.135	1.002
	(0.0316)	(0.0403)	(0.0383)	(0.0383)	(0.0439)	(0.0362)	(0.575)	(0.735)	(1.191)
Observations	1,083	950	1,050	1,079	921	1,023	1,068	922	1,029
R-squared	0.113	0.084	0.107	0.039	0.071	0.063	0.042	0.054	0.074

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

Table 5: Difference in Difference Regressions for Labor Market Outcomes by Initial Household Poverty Status

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Ever Repeat a Grade?		High School Graduate at Age 19		Total Years of Education at Age 19		Total Years of Education at Age 21		Current Income at Age 23+		Had a job in the past 3 months at Age 23+?	
Age Cohort 1 x Number of American Indian Parents	0.0166	-0.228***	0.0586	0.317***	-0.0231	0.710*	0.00904	1.380***	1.204**	2.175**	-0.0270	0.379***
	(0.0375)	(0.0734)	(0.0885)	(0.103)	(0.520)	(0.391)	(0.814)	(0.505)	(0.505)	(0.977)	(0.0723)	(0.109)
Age Cohort 2 x Number of American Indian Parents	-0.0133	-0.0760	-0.0118	0.233**	0.469	0.102	-0.530	0.255	0.471	2.184***	0.0313	0.173*
	(0.0344)	(0.0669)	(0.0964)	(0.101)	(0.437)	(0.340)	(0.465)	(0.520)	(0.388)	(0.812)	(0.0507)	(0.0889)
Ever in Poverty?	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
Observations	638	446	638	446	638	447	554	398	600	423	611	432
R-squared	0.073	0.072	0.089	0.155	0.179	0.207	0.359	0.164	0.148	0.184	0.083	0.174

Robust standard errors in parentheses. Note additional covariates identical to previous regression are included in all models.
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Non Labor Market Outcomes by Poverty Status

Panel A: Obesity						
VARIABLES	Obese at Age 19?	Obese at Age 19?	Obese at Age 21?	Obese at Age 21?	Obese at Age 23?	Obese at Age 23?
Age Cohort 1 x Number of American Indian Parents	-0.0280	0.0854	-0.161**	-0.159*	-0.213**	0.0127
	(0.0858)	(0.0773)	(0.0800)	(0.0916)	(0.0895)	(0.109)
Age Cohort 2 x Number of American Indian Parents	0.0375	0.0215	-0.0362	0.0991	-0.116	0.166*
	(0.0881)	(0.0809)	(0.0949)	(0.0934)	(0.0749)	(0.0914)
Ever in Poverty?	N	Y	N	Y	N	Y
Observations	637	446	552	398	613	437
R-squared	0.110	0.094	0.031	0.138	0.110	0.141
Panel B: Alcohol Consumption						
	Ever Drunk in the past 3 months by Age 19?		Ever Drunk in the past 3 months by Age 21?		Ever Drunk in the past 3 months by Age 23?	
Age Cohort 1 x Number of American Indian Parents	-0.0830	-0.206***	-0.163**	-0.207***	-0.248***	0.0158
	(0.0725)	(0.0678)	(0.0803)	(0.0768)	(0.0730)	(0.0840)
Age Cohort 2 x Number of American Indian Parents	0.0502	-0.0180	-0.0486	-0.0971	-0.147**	0.120
	(0.0810)	(0.0801)	(0.0873)	(0.0889)	(0.0672)	(0.0737)
Ever in Poverty?	N	Y	N	Y	N	Y
Observations	634	445	531	390	602	421
R-squared	0.041	0.066	0.104	0.064	0.135	0.051
Panel C: Alcohol Consumption						
	N of Drinks in the past 3 months Age 19		N of Drinks in the past 3 months Age 21		N of Drinks in the past 3 months Age 23	
Age Cohort 1 x Number of American Indian Parents	-1.038	-0.695	-0.411	-2.125**	-8.302***	-0.371
	(1.222)	(1.270)	(1.721)	(1.072)	(2.629)	(1.448)
Age Cohort 2 x Number of American Indian Parents	-0.473	1.064	-0.340	0.972	-6.742**	4.198*
	(1.375)	(1.639)	(1.698)	(1.634)	(2.665)	(2.260)
Ever in Poverty?	N	Y	N	Y	N	Y
Obs	628	440	533	389	600	429
R-squared	0.027	0.075	0.036	0.112	0.112	0.101

Robust standard errors in parentheses. Note additional covariates identical to previous regression are included in all models.

Table 6: Non-labor Market Outcomes by poverty status - criminality and drug use

Panel A: Criminality

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Ever Committed any Crime up to Age 21?		Ever Committed any Crime up to Age 23?		Number of Crimes Committed up to Age 21		Number of Crimes Committed up to Age 23	
Age Cohort 1 x Number of American Indian Parents	-0.00820	-0.101**	-0.142**	-0.0494	-0.359	-2.732**	-1.525**	-0.559
	(0.0526)	(0.0401)	(0.0720)	(0.0991)	(1.057)	(1.376)	(0.761)	(1.404)
Age Cohort 2 x Number of American Indian Parents	0.00276	-0.0694*	-0.00760	0.0285	-0.263	-2.397*	-0.526	0.322
	(0.0275)	(0.0373)	(0.0777)	(0.0907)	(0.946)	(1.385)	(0.803)	(1.346)
Ever in Poverty?	N	Y	N	Y	N	Y	N	Y
Observations	554	398	613	437	554	398	613	437
R-squared	0.030	0.051	0.085	0.095	0.024	0.064	0.085	0.090

Panel B: Drug use

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Ever use Marijuana up to Age 19?		Ever use Marijuana up to Age 21?		Ever use Marijuana up to Age 23?	
Age Cohort 1 x Number of American Indian Parents	-0.104	-0.202***	-0.0557	-0.113	-0.140	0.0624
	(0.0852)	(0.0769)	(0.101)	(0.0864)	(0.0912)	(0.108)
Age Cohort 2 x Number of American Indian Parents	-0.0549	-0.0773	-0.00627	0.0796	-0.119	0.231**
	(0.0865)	(0.0816)	(0.0973)	(0.0915)	(0.0802)	(0.0929)
Ever in Poverty?	N	Y	N	Y	N	Y
Observations	638	447	554	398	613	437
R-squared	0.032	0.081	0.062	0.139	0.045	0.038

Robust standard errors in parentheses. Note additional covariates identical to previous regression are included in all models; *** p<0.01, ** p<0.05, * p<0.1