Online Appendix for: Human Capital and Voting Behavior Across Generations: Evidence from an Income Intervention
[*Not to be included in printed versions*]

Contents

List of Figures

List of Tables

A Additional Relevant Literature

One strand of research gets close to studying the causal effect of income on voting: studies exploring the political consequences of conditional cash transfers (CCT). This body of work leverages random (or as-if random) variation in exposure to CCT programs—linking participants (or heavily exposed geographic areas) to political outcomes data (e.g. Baez et al. 2012; De La O 2013, 2015; Galiani et al. 2016; Imai, King, and Rivera 2017; Linos 2013; Pop-Eleches and Pop-Eleches 2012; Zucco 2011). While these studies speak to an important topic, this approach may not be ideally situated to answer the question of whether income has a causal effect on voter turnout. On a very basic level, this program of study has faced data challenges in linking CCT participants and voting outcomes. In the largest and most comprehensive work on this topic, De La O (2013, 2015) provides evidence that suggests that CCT exposure increases turnout substantially (by about 5-15 percentage points, depending on the subsample used). However, the conclusions in this work have been strongly challenged as being a product of data errors (Imai, King, and Rivera, 2017).

More generally, CCT programs face fundamental difficulties in using their design to examine the pure effects of income. Importantly, many CCT programs require that *before* receiving the income transfers recipients make changes to their behavior that may actually be driving any effect on voter turnout. For example, Progressa—one of the largest and most-studied conditional cash transfer programs—required that participants enroll their children in school, ensure that they show up to school, and make a certain number of visits to healthcare providers (De La O 2013, p. 3). These behavioral changes, rather than income, may be the primary mover in any effect on turnout as educational and health are strong inputs of voting (Sondheimer and Green 2010; Burden et al. 2017).[h] Overall, though CCT studies deliver important findings about political participation, it is unclear whether income is indeed the driving force in any voting gains; the unique components of CCT programs contaminate this instrument from eliciting the pure downstream effects of income.[i]

To our knowledge, there is only one study of the effects of unconditional

[h] Another potential issue especially salient to CCTs is that these programs may come with source or demand effects because there are "ample opportunities for incumbents to claim the credit for positive program results" (De La O 2013, 1). Indeed, for this reason, scholars have tended to study whether CCTs have persuasive effects rather than mobilizing effects. Hence, any effect CCTs have on voter turnout may actually be the result of credit-claiming campaigns on the part of highly motivated politicians, rather than of income per-se.

[i] To be clear, we are not arguing that education and health are not potential mechanisms. We are arguing, instead, that in using CCTs these are likely not mechanisms, but primary movers.

cash transfers on voting.[j] Using an innovative approach that leverages data from the annual Spanish Lottery, Bagues and Esteve-Volart (2016) show that areas that realize an exogenous increase in lottery income substantially shift their incumbent voting patterns, but do not change their levels of voter turnout. While this unique work clearly speaks to the topic at hand, it remains unclear whether this null effect holds in the U.S. Further, winning the lottery is a rare occurrence and the behavioral responses to such an event are likely different than how individual would react to a permanent change in future income (which is the nature of the exogenous income change that we study here). Another difference between our study and theirs is that in their case any resource gains individual winners achieve may be muted by a decreased likelihood of retrospective voting. That is, in providing a huge transfer of wealth, the Spanish lottery not only enhanced citizen income at a micro level, but it fundamentally improved local economic conditions (a point Bagues and Esteve-Volart readily admit). Abundant research has shown that voters respond to a poorly performing economy (e.g. Brunner, Ross, and Washington 2011; Healy and Malhotra 2013; Healy and Lenz 2014; Healy and Lenz 2017; Lewis-Beck and Stegmaier 2007). Hence, while the income effect may increase voters' capacity to vote, it may decrease their incentive to do so as a means of holding low performing public officials accountable, thus resulting in a null effect on turnout.[k] Finally, Bagues and Esteve-Volart (2016) do not explore potentially important heterogeneities in income's effect on turnout—including across socioeconomic status and the life course. Their work focuses exclusively on effects on adults, but there are strong reasons to suspect that income obtained in childhood may matter a great deal (Ojeda, 2018).

[j] Brunner, Ross and Washington (2011) have shown that exogenous increases in income due to exogenous labor demand shocks tend to decrease the support for redistributive policies at the census-tract levels in California. They do not identify whether these positive economic shocks affect the probability of voting as they are not examining individual-level data.

[k] In the application we study, income is disbursed by the tribal government whose elections are held in different years than the elections we study, thus making retrospection much less likely.

B More Information About the GSMS Sample

For the counties covered in the GSMS survey, see Figure A1.

Figure A1: Location of the GSMS Study Participants

Note: Figure displays the counties included in the GSMS study. The Eastern Cherokee reservation (where the casino is located)is in Cherokee, NC (which is split between Swain and Jackson County, NC).

Figure A2 shows the design of the follow ups for the GSMS survey. Children were interviewed at the same time as their parents (but in separate interviews) until they turned 16. After that, only children were surveyed. For more details on the sampling framework, see Costello et al. 1996 and Costello et al. 1997. The casino itself opened up in 1996 (after Wave 4 of the survey). The process for approving the casino started in 1988, with the federal passage of the Indian Gaming Regulatory Act, which (among other things) clarified the sovereignty of Native tribes to open and operate casinos. For more information on the context of the casino's opening, see Johnson, Kasarda, and Appold (2011).

The GSMS contains information on a host of baseline characteristics for parents and children, including name, current location, date of birth, poverty status, educational attainment, race/ethnicity, marital status and labor force participation. Parents and children are linked by a common, de-identified, number.

The first three variables presented in Table A1 show that the survey selection was balanced across the cohorts by Native American race. The survey was

Figure A2: Design of Follow up Surveys of the GSMS

Wave	1	2	3	4		5	6	7	8	9	10	11	12	13	14	15	16	17
Age	1993	1994	1995	1996	Casino Opening	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009
9	C1																	
10		C1																
11	C2		C1															
12		C2		C1														
13	C3		C2															
14		C3		C2			C1											
15			C3			C2		C1										
16				C3			C2		C1									
17																		
18																		
19								C3		C2		C1						
20																		
21										C3		C2		C1				
22																		
23																		
24													C3		C2		C1	
25														C3		C2		C1

Note: Figure displays the structure of the GSMS data. C1=cohort 1, C2=cohort 2, C3=cohort 3. On the vertical access are children's ages. On the horizontal access are survey wave and year. Survey data collection began in 1993, with the three age cohorts all being interviewed. These interviews continued until the 4th wave (1996) right before the casino was opened. Following the casino opening, cohorts were interviewed in a staggered manner (for reasons unrelated to the casino opening; see Costello et al. 1996 and Costello et al. 1997.). Contact information is continuously maintained and updated up until the present.

also balanced along gender lines. There is a statistically significant difference in levels of average household incomes prior to the intervention; Native American households earned incomes of approximately $23,000 while non-Native American households earned incomes that were almost nine thousand dollars higher for an average of $32,000. Marital status also appears to be well balanced across the groups. There is a difference in mother's educational attainment by race. In general, non-Native American mothers tend to have higher educational attainment (more than a high-school degree) than Native American mothers prior to the start of the intervention. Mothers appear to work in similar proportions across the two groups. Native American parents are less likely to vote as compared to non-Native American parents over the entire time period by about thirty percentage points. Our identification strategy accounts for these initial differences in voting probabilities and other differences in starting characteristics.

Table A1: Table of Means for Outcomes at Initial Survey Wave

Variable	Native American Mean	Native American Std. Dev.	Non Native American Mean	Non Native American Std. Dev.	Test of Equality of Means Diff in means	SE of Diff	T-Statistic
Age cohort initially 9-year olds	0.370	0.484	0.355	0.479	0.015	0.032	0.471
Age cohort initially 11-year olds	0.357	0.480	0.345	0.476	0.012	0.032	0.382
Age cohort initially 13-year olds	0.273	0.446	0.300	0.458	-0.027	0.030	-0.914
Age	10.80	1.595	10.89	1.616	-0.084	0.105	-0.797
Male child indicator	0.532	0.500	0.563	0.496	-0.031	0.033	-0.942
Average Household Income Over First 3 Years	23156	15217	32361	16907	-9204	1035	-8.90
Parents are Married	0.503	0.501	0.486	0.500	0.017	0.033	0.514
Mother has a high school degree/GED	0.357	0.480	0.282	0.450	0.074	0.031	2.391
Mother has more than a high school degree	0.391	0.489	0.484	0.500	-0.094	0.032	-2.896
Mother Employed Full Time?	0.852	0.356	0.857	0.351	-0.005	0.023	-0.206
Parents' Voting	0.216	0.412	0.492	0.500	-0.276	0.028	-9.697

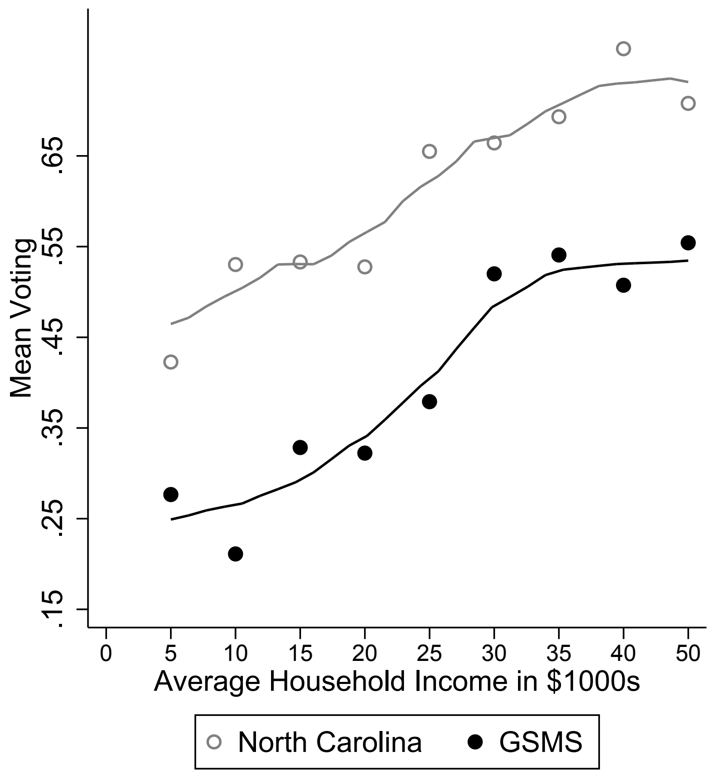
Notes: Table shows sample summary statistics broken by Native American, Non-Native American. Table shows means (columns 2 and 4) and standard deviations (columns 3 and 5). Difference of means columns shows the results from a simple t-test. The number of observations for non-Native American ranges between 1028-1041 except for Mother Employed Full Time which is 879. The number of observations for Native Americans ranges between 292-297 except for Mother Employed Full Time which is 270.

Figure A3 benchmarks how the propensity to vote varies by baseline income levels in the Great Smoky Mountain Study of Youth (GSMS)—the main dataset we use for our analyses (and which we describe in greater detail below)—to that from the Current Population Survey (CPS) November 1992 extract for the state of North Carolina. (The CPS is widely used in voting research and this state-year subgroup situates us as close to our primary sample as possible.) We plot the average voter turnout within these income bin categories and show a local polynomial fit for each sample plot. The top line and corresponding points provide the income-voting relationship for all of North Carolina for the 1992 U.S. Presidential election based on the CPS. The bottom provides the relationship for the parents from the GSMS data for the same election using validated voter turnout. As Figure A3 demonstrates, there is a positive relationship between income and voting probabilities in both groups. Despite some differences in these two samples, the income-voting gradient appears to be quite similar.[l]

It is also clear from the figure that although higher incomes correlate with a higher propensity to vote, the relationship is not the same across the entire income distribution. For some income brackets, the gradient is steeper than for others; the gradient is flattest in the highest income categories for both groups shown here. This suggests that beyond a threshold level of average household income additional increases in income are not associated with as large changes in voting probability. This is consistent with previous theoretical work in that any exploration of the effect of income on voting should consider the possibility that there are diminishing marginal returns.

[l] The level differences across the two populations shown here this may be explained by differences in average characteristics between the average North Carolina adult and that of the GSMS parents. Further, the voting rates in the CPS are self-reported while in the GSMS sample they are based on official voting records; self-reported voting rates are always higher than actual voting rates.

Figure A3: Income Gradient for Voting Average for Different Groups and Income Bins for 1992 Election



Notes: The data for the GSMS is restricted only to the subject parents for the U.S. Presidential election in 1992 (before the casino transfers began). Validated voting data in the GSMS come from the North Carolina voter file. Data for North Carolina are drawn from the Current Population Survey 1992 November file. Voting in the CPS is measured through survey self reports. We plot the average voting turnout by income bins as given and show a polynomial fit for each of the two groups.

B.1 Match of GSMS Participants to Voter Files

The GSMS benefits from having all of the matching inputs available for all children in the dataset.[m] The availability of matching inputs did vary somewhat across parents, with some of these not having date of birth.[n] Fortunately, however, the number of matching inputs available was balanced across the treatment and the control samples.[o]

Overall, our match reveals that 47.2% of children and 45.4% of parents were registered to vote. This difference in match rates across generations is not statistically significant (p=0.28)— suggesting that our match found about the same number of children and parents in the voter files. Comfortingly, this registration rate is similar for individuals in the general population of a similar demographic profile.[q] As we would expect given the (somewhat limited) evidence in other studies of transmission of votes (or non-votes) from one generation to the next, the bivariate correlation between parents' voting and children's voting is high ($r=0.8$; $\beta=0.76$, $p<0.001$).[r] Following previous best practice, the participants who we could not locate in the voter records were coded as

[m] We could not use nationwide voter file vendors like Catalist, L2, or the Data Trust because of privacy and data security concerns from the guardians of the GSMS data. Given that we only had access to the North Carolina voter file and the online registration voter portal in other states (which forces an exact match) we did exact matching to be consistent across states. This decision is consistent with other work in this area and will not bias our results.

[n] For these individuals, we added a search condition to include county of residence.

[o] Tests for balance across the number of matching inputs available across the child cohorts and casino eligibility (our identification strategy for the children and parents respectively) are provided in Appendix Table A2. Our approach avoids many of the issues that come with matching to administrative records.[p] For example, in seeking to match to other data files, the Census struggles with questions like: "should you clean names using NYSIIS or use exact spelling?" and "should you allow some lenience on age or require exact age match?" (These issues frequently come up in matches to voter records, see Ansolabehere and Hersh (2012) and Berent, Krosnick, and Lupia (2016).) We avoid the problems associated with the first question by having actual, validated first names among our entire sample; and we avoid the problems associated with the second by having exact date of births rather than age.

[q] According to data from the Current Population Survey November Supplement, the self-reported registration rate from 2000-2012 among citizens with incomes of less than $25,000 is 54.7%. This rate is likely artificially inflated because of the social desirability of social acts like registering to vote that arises in survey-based measures of registration.

[r] Theory predicts a strong transmission of voting from parent to child (Dawson and Prewitt 1968; Langton 1969; Searing, Schwartz, and Lind 1973; Plutzer 2002; Miller and Saunders 2016). However, few credible datasets exist to estimate this transmission. The most-commonly used exception—the Youth-Parent Socialization Panel Study (Jennings et al. 2005)—comes from a select cohort that came of age in the 1960s. As many have noted (e.g. Plutzer 2002), this sample has its limitations. For example, this cohort had especially high rates of self-reported voter turnout (children's voter turnout rate: 84% and parents' voter turnout rate: 87%). Among this group where ceiling effects are clearly in play, there still remains a strong bivariate relationship between parents' voting and children's voting ($r=0.3$; $\beta=0.22$, $p<0.001$), but one that is clearly muted by the sample composition and the voting measure being self-reported, rather than validated.

having not registered nor voted (Sondheimer and Green 2010; Holbein 2017; Ansolabehere and Hersh 2012; Grimmer et al. Forthcoming).[s]

Robustness checks reveal that match quality is similar across our identifying variation (Appendix Table A2). We find little evidence that those exposed to the casino transfers for a longer period of time as minors are different in terms of the propensity of parents to move out of the state or to change their last name, or to have missing residential information in any of the survey waves—all measures that could substantially hinder match quality from being similar across our identifying variation. This suggests that our results are unlikely to be biased by the match procedure itself.

[s]It may be tempting to argue that we should, instead, condition on registration. However, such an approach risks introducing post-treatment bias that would significantly skew our results. Controlling for registration would block one of the primary channels by which income may increase voting.

Table A2: Differences in Characteristics Affecting Matching Rates for Parents

	Initial HH Income			Initial HH Income			Initial HH Income		
	Pooled	<Median	>Median	Pooled	<Median	>Median	Pooled	<Median	>Median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Moved Out of North Carolina			Ever Changed Last Name			Number of Matching Inputs Available		
Interaction 1: Age Cohort 1	-0.00471	-0.0629	0.118	0.0220	-0.0218	0.0882	-0.046	-0.069	-0.016
$\times NativeAmerican$	(0.0561)	(0.0771)	(0.0914)	(0.0373)	(0.0517)	(0.0581)	(0.042)	(0.059)	(0.062)
Interaction 2: Age Cohort 2	-0.00891	-0.0248	-0.0168	0.0949**	0.0731	0.122*	-0.067	-0.050	-0.038
$\times NativeAmerican$	(0.0577)	(0.0836)	(0.0726)	(0.0407)	(0.0560)	(0.0641)	(0.043)	(0.062)	(0.057)
Observations	1,328	648	680	1,332	651	681	1,233	595	638
R-squared	0.014	0.030	0.020	0.012	0.013	0.015	0.074	0.066	0.106

Notes: *** $p < 0.01$, ** $p < 0.05$, *$p < 0.10$. The outcomes are binary indicator variables. Robust standard errors in parentheses.

We also provide results from sensitivity analyses which weight the parent observations based on the uniqueness of their match in the North Carolina voting registration data. There are potential duplicate matches for parents given incomplete information on parental birth date in the GSMS records. This missing information is balanced across our identifying variation.[t] This is not an issue for matching of the children, since their data is much more complete. Finally, for completeness and direct comparability, we also show results for the parents using the cohort comparison framework that we use for the children in Equation 2.

To further check for pre-treatment differences across the two groups, Table A3 provides checks of variable means for a variety of baseline characteristics across the three age cohorts of children broken by race prior to the start of the unconditional cash transfer. As can be seen, there are very few statistically significant differences across the various cohorts by race. Out of the 36 statistical tests run, only 4 show signs of imbalance. Moreover, if we include these pre-treatment measures in the regressions, they do not affect the results. This indicates that the different age cohorts can serve as appropriate controls for estimating the effect of the casino transfer.[u]

[t] Fortunately, the rate of missing observations of this matching information is balanced across our identifying information (Cohort 1, β=-0.34 (matches), p $<$0.369; Cohort 2, β=0.11 (matches), p $<$0.795. For parents, the median number of matches is 0; conditional on matching at all, the median is 1 match.). This makes it unlikely that these matches are biasing our results. To go one step further, however, we assign lower weights to those observations that have multiple matches using the inverse of the number of matches as weights and repeat the analysis using these weights. Intuitively, this approach places less emphasis on observations that have many matches, and, thus, less certainty of whether the match is right. As can be seen below, when we conduct these checks, the results do not change substantially. Fortunately, the potential bias that Solon, Haider, and Wooldrige (2015) explain appears to be of little concern in our application, as these weights do little to change our effect estimates.

[u]Table A4 also provides a comparison of characteristics of the GSMS Native American population to that of other Native American populations and rural African American groups; we show that there is similarity across these groups in several important categories. Appendix Table A5 provides a correlation of voting and education for rural Americans, African Americans and our GSMS sample. The results show that the education gradient, similar to the income gradient, for the GSMS population is largely in line with that of these other groups as well.

Table A3: Mean Differences by Age Cohort and Native American Parent Status at Survey Wave 1

Differences Between Cohort 1 and Cohort 2	Cohort 1 Mean	Cohort 2 Mean	Difference	SE of Difference
Number of Native American Parents	N/A	N/A		
Native American Indicator	0.019	0.036	-0.017	0.012
Male Child Indicator	0.562	0.596	-0.034	0.037
Mother Has a High School Degree/GED	0.297	0.27	0.027	0.033
Father Has a High School Degree/GED	0.184	0.184	0	0.029
Mother Has More than a High School Degree	0.462	0.518	-0.056	0.037
Father Has More than a High School Degree	0.281	0.309	-0.028	0.034
Initial Household Income	29367.98	32652.17	-3284.19*	1331.824

Differences Between Cohort 2 and Cohort 3	Cohort 2 Mean	Cohort 3 Mean	Difference	SE of Difference
Number of Native American Parents	N/A	N/A		
Native American Indicator	0.036	0.071	-0.034*	0.017
Male Child Indicator	0.596	0.526	0.07	0.038
Mother Has a High School Degree/GED	0.27	0.279	-0.009	0.035
Father Has a High School Degree/GED	0.184	0.141	0.043	0.029
Mother Has More than a High School Degree	0.518	0.471	0.047	0.039
Father Has More than a High School Degree	0.309	0.292	0.018	0.036
Initial Household Income	32652.17	32154.88	497.29	1399.523

Differences Between Cohort 1 and Cohort 3	Cohort 1 Mean	Cohort 3 Mean	Difference	SE of Difference
Number of Native American Parents	N/A	N/A		
Native American Indicator	0.019	0.071	-0.052**	0.015
Male Child Indicator	0.562	0.526	0.037	0.038
Mother Has a High School Degree/GED	0.297	0.279	0.018	0.035
Father Has a High School Degree/GED	0.184	0.141	0.043	0.028
Mother Has More than a High School Degree	0.462	0.471	-0.009	0.038
Father Has More than a High School Degree	0.281	0.292	-0.011	0.035
Initial Household Income	29367.9	32154.88	-2786.9*	1364.668

Notes: *** $p < 0.01$, ** $p < 0.05$, *$p < 0.10$.

Table A4: Comparison of Economic Characteristics with other Native American Tribes and relevant demographic groups

Data Source:	1990 Census for Native Americans	Social Explorer			IPUMS 1990		
Group:	Eastern Cherokee	All 11 NC Counties	All Native Americans	Rural Native Americans	Rural African Americans	All of US	Rural US
Rural status	99%*	65%	54%	100%	100%	32%	100%
Median Family Income	$17,778	$27,275	$20,000	$18,000	$17,000	$32,030	$29,400
Family size	2.95		3.86	4.17	4.11	3.28	3.4
Own house	70%	75%	58%	68%	70%	69%	80%
Married	50%	60%	47%	49%	41%	58%	66%
% of Age 25+ HS Degree	70%	69%	69%	64%	53%	79%	75%
Unem Rate	12%*	6%	15%	18%	12%	6%	6%
Per Capita Income	$6,543	$11,691	$11,362	$9,905	$9,165	$17,922	$15,677

Source: *Taylor and Akee (2014); 1990 Census Report on Native Americans; Social Explorer, 1990 County Data; IPUMS 1990, 1% Sample.

Table A5: Correlation of Education and Voting

Rural:	0.2153
Rural African American:	0.1801
GSMS	0.2014

C Casino Transfers and Household Income

The GSMS contains information on household income in total and does not have information on the size of the various components of income flows, such as earnings from labor, child support, pensions, and others. As a result, we cannot pinpoint the change in incomes to a source such as tribal government transfers, as it should be recorded. We are able to estimate the overall change in total income, as we report below as well as rule out changes in employment and marital status as potential channels. Note, also, that the cash transfers are disbursed to adult members of the tribe only; children's cash transfers are banked for them until age 18 so the family receives no additional money for the children during our study period.

Here we demonstrate that the casino transfers increased household income for Native American families substantially.[v] In Table A6 in the Online Appendix, we show how household income was affected by eligibility for casino transfer payments in a regression framework. The first two columns provide the pooled ordinary least squares results and the estimates from models incorporating household fixed-effects regressions respectively. The dollar amounts are all converted to year 2000 dollar values and indicate that, on average, annual incomes increased by approximately $4,700 per recipient household, which accords with unofficial reports. This effect is large: being equivalent to a 20-30% increase in household levels (enough to pull many families out of poverty). In the next two columns, we interact the variable for casino transfer eligibility with survey wave (with the intervention year omitted) for the ordinary least squares regression and the individual fixed-effects regression. We use the estimated coefficients from column 3 to produce the event-analysis plot in Figure A4. The coefficients plotted in Figure A4 are based on the following triple difference equation

$$\begin{aligned} Y_{it} = \alpha + \beta_1 YoungestCohorts_i + \beta_2 AfterCasino_t \\ + \beta_3 NativeAmerican_i + \gamma_1 YoungestCohorts_i \times AfterCasino_t + \\ \gamma_2 YoungestCohorts \times NativeAmerican_i + \\ \sum_t^T \lambda_t \times YoungestCohorts_i \times NativeAmerican_i \times Year_t + X'\theta + \epsilon_{it} \end{aligned} \tag{3}$$

The figure shows that there was no statistically significant change in household income prior to the income intervention (in survey waves 1-3)—which is

[v] We note that the changes in household income indicated on the graph are not concurrent with the elections. The income measures are taken during annual survey waves until the household children turn 16, the latest data point recorded in 2000.

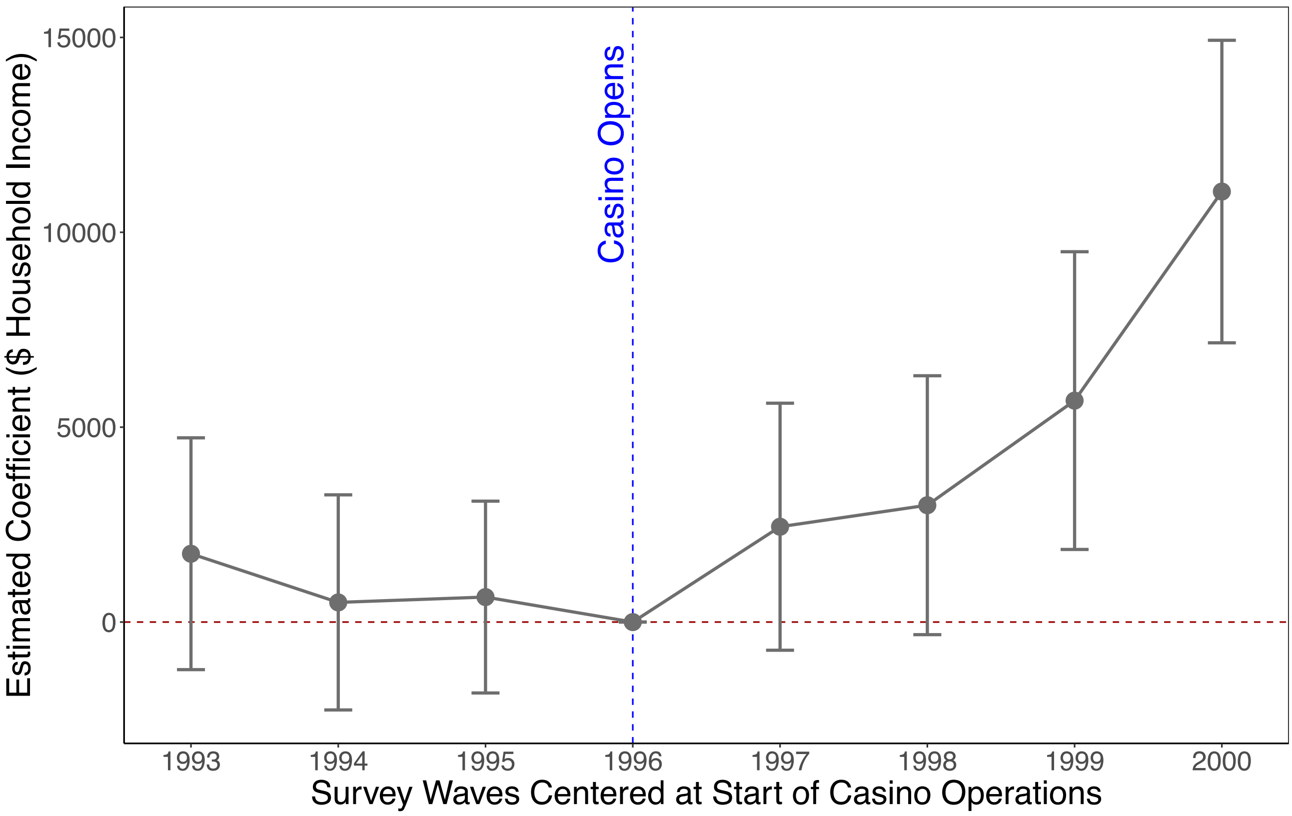
again reassuring of the validity of our research design—and a large and statistically significant increase in household incomes for Native American households subsequent to the transfer initiation.

Table A6: The Effect of the Casino Transfer on Household Income For Children Up to the Age of 18

	(1)	(2)	(3)	(4)
VARIABLES		Household Income in 2000 US $		
Receipt of Cash Transfer?	4,690***	4,730***		
	(998.5)	(950.2)		
Survey Wave 1 Interaction			1,753	910.2
			(1,517)	(1,416)
Survey Wave 2 Interaction			504.5	35.61
			(1,408)	(1,314)
Survey Wave 3 Interaction			641.3	105.4
			(1,255)	(1,138)
Survey Wave 4 Interaction			Omitted Category	Omitted Category
Survey Wave 5 Interaction			2,446	2,023
			(1,617)	(1,511)
Survey Wave 6 Interaction			2,998*	2,731*
			(1,695)	(1,466)
Survey Wave 7 Interaction			5,682***	5,033***
			(1,949)	(1,884)
Survey Wave 8 Interaction			11,045***	10,431***
			(1,980)	(1,939)
Constant	35,012***	34,914***	34,969***	34,738***
	(1,024)	(286.0)	(1,044)	(414.9)
Fixed-Effects?	N	Y	N	Y
Total N	6,674	6,674	6,674	6,674
Number of GSMS children	1,420	1,420	1,420	1,420

Notes: *** p <0.01, ** p <0.05, * p <0.1. Receipt of Cash Transfer is the triple difference coefficient from our empirical specification. It is an interaction of race * age cohort* wave. Casino payments began after wave 4 for only Native American children (the baseline category). All regressions include all secondary interactions and level variables. Standard Errors clustered at the individual level. In columns 3 and 4, Survey Wave Interaction variables are the Receipt of Cash Transfer variable interacted with each wave dummy variable and the fourth survey wave interaction is omitted. Coefficients are in 2000 US $.

Figure A4: Effect of Cash Transfers on Household Income around Start of Casino Operations



Notes: Receipt of Cash Transfer is the triple difference coefficient from our empirical specification. It is an interaction of race * age cohort * wave. Casino payments began after wave 4 for only Native American children. All regressions include all secondary interactions and level variables. Standard Errors clustered at the individual level. In columns 3 and 4, Survey Wave Interaction variables are the Receipt of Cash Transfer variable interacted with each wave dummy variable and the fourth survey wave interaction is omitted. Figure shows point estimates (dots) and corresponding 95% confidence intervals (bars).

D Additional Tables

Table A7 provides the coefficient estimates from the difference-in-difference specification that leverages pre- and post-casinodifference by transfer eligibility status for the parents. Here the coefficient of interest is the interaction coefficient between Native American household and a binary variable indicating the time period after the start of casino operations. Column 1 provides the results from the simple difference-in-differences specification testing changes in the voting patterns of the same household over time. Here we find that the increase in household income has no economically substantive or statistically significant effect on parents' voting probabilities. This null effect is precise. Using equivalence testing, our 95% confidence intervals allow us to confidently rule out effects as large or small as 2.5 percentage points. In column 2 we test the hypothesis that additional income has decreasing returns in terms of voting probability in this adult population. We interact the treatment variable with initial household income (the average of household income in the first three survey waves) and include all relevant double interactions. The triple difference coefficient is small and not significant at conventional levels. The null effects on parental voting that we found in column (1) are thus unlikely to be masking differences in treatment effects across the initial socioeconomic distribution.

Table A10 in the shows the exact coefficients from Equation 2. The identification for this analysis comes from differences in the length of treatment of living in a household with exogenously increased incomes. The companion Appendix Table A11 reports the results from the most basic models that exclude all covariates except for the cohort indicator variables and the race indicator variables. The coefficients of interest are very similar to those reported in Table A10.

In columns 1 and 2 of Panel A, we present the results for the full sample controlling for baseline characteristics—average household income in the pre-transfer period and the parental voting propensity in the pre-transfer period. The estimated interaction coefficients in rows one and two provide the difference-in-difference coefficients as shown in Equation 2. Under the assumptions outlined above, these coefficients estimate the differences in voting propensity of AI relatively to non-AI children from the two youngest cohorts as compared to the oldest cohort. The oldest cohort was 17 at the time of the first transfers, and thus we consider the AIs from this cohort treated to the extra income for the shortest period of time while they were minors in the affected households (and in general, at any fixed age). The two outcome variables are measures of child voting behavior over the time period when all three cohorts were eligible to vote

Table A7: The Effect of Casino Transfer on Parents' Voter Turnout (Probability of Voting)

	Pooled	Pooled Triple Difference	Below Median HH Income at Baseline	Above Median HH Income at Baseline
	(1)	(2)	(3)	(4)
VARIABLES	Voted	Voted	Voted	Voted
AI x After Casino	-0.00492	-0.0432	-0.0250	0.00673
	(0.0148)	(0.0265)	(0.0201)	(0.0217)
AI x After Casino x Initial HH Income		0.00637		
		(0.00426)		
Year FE	Y	Y	Y	Y
Household FE?	Y	Y	Y	Y
Mean of Dependent Variable	0.435	0.435	0.328	0.536
Observations	15,984	15,984	7,812	8,172
R-squared	0.054	0.055	0.069	0.045
Number of newid	1,332	1,332	651	681

Notes: *** p <0.01, ** p <0.05, *p <0.10. Models include a race indicator variable, an indicator for post-casino operations, age fixed effects, year fixed effects and a constant; we control for average initial household income for the first three survey waves in columns 1 and 2 . Column 2 provides a triple difference with initial household income prior to the casino operations. The 95% confidence intervals are based on cluster robust standard errors (family level) are given below the estimated coefficients. Additional regressions using matching weights produce qualitatively similar results.

(2002-2014). The outcome variables measure whether these children (in adulthood) ever voted in a State or Federal election and the proportion of elections that they voted, respectively.

We find in row three that parents' prior voting probability in the 1992 and 1994 elections is strongly correlated with children's voting probability in the future. Comparing parents who always voted before the income intervention began to those who did not vote reveals a correlation on the order of 11-16 percentage points ten to twenty years later. This is evidence in favor of strong inter-generational transmission of voting.[w]

The estimated difference-in-difference coefficients in the two pooled regression equations in columns 1 and 2 are both positive but they are not statistically significant at conventional levels. Given the strong income gradient found in both North Carolina data and the GSMS parental data (Figure A3) and the clear theoretical predictions from voter turnout theories, we again examine in columns 3 and 4 whether there is a differential impact of the cash transfers on child voting

[w]The coefficients in row six demonstrate that initial household income is correlated with children's voting probabilities as well. Here we include a control for household income in $5,000 bins. On average, children raised in households with incomes that are $5,000 higher are about 2 percentage points more likely to vote as adults.

Table A8: The Effect of Casino Transfer on Parents' Voter Turnout Interacted with Parental Age (Probability of Voting)

VARIABLES	(1) Voted	(2) Voted	(3) Voted
AI x After Casino	-0.00572	-0.00792	-0.000987
x Age	(0.00507)	(0.00570)	(0.0127)
After Casino x	0.000973	0.00142	0.00162
Age	(0.00131)	(0.00214)	(0.00159)
AI x Age	0.000712	0.00415	0.00143
	(0.00493)	(0.00600)	(0.00963)
Age	0.00390	0.00112	0.00511
	(0.00244)	(0.00364)	(0.00330)
After Casino x	0.220	0.281	0.0504
After	(0.201)	(0.225)	(0.504)
After	0.00960	0.0290	-0.0403
	(0.0581)	(0.0890)	(0.0741)
AI	-0.219	-0.254	-0.367
	(0.195)	(0.233)	(0.388)
Observations	14,292	6,804	7,488
R-squared	0.104	0.069	0.061

Notes: *** p <0.01, ** p <0.05, *p <0.10. Models include a race indicator variable, an indicator for post-casino operations, age fixed effects, initial household income, year fixed effects and a constant. The 95% confidence intervals are based on cluster robust standard errors (family level) are given below the estimated coefficients.

Table A9: Parents Event Analysis Regression Tables

		Initial HH Income	
	Pooled	Below Median	Above Median
	(1)	(2)	(3)
VARIABLES	Voted	Voted	Voted
Interaction 1: AI x 1992	-0.00164	0.00895	-0.00110
	(0.0192)	(0.0228)	(0.0392)
Interaction 2: AI x 1994	0.0331*	0.0177	0.0485
	(0.0198)	(0.0255)	(0.0342)
Interaction 3: AI x 1996	Omitted Category	Omitted Category	Omitted Category
Interaction 4: AI x 1998	0.0200	0.00667	0.0372
	(0.0185)	(0.0217)	(0.0378)
Interaction 5: AI x 2000	-0.00405	-0.0324	0.0319
	(0.0180)	(0.0242)	(0.0286)
Interaction 6: AI x 2002	0.0399*	0.0269	0.0489
	(0.0213)	(0.0265)	(0.0397)
Interaction 7: AI x 2004	-0.0279	-0.0538**	0.00712
	(0.0206)	(0.0271)	(0.0332)
Interaction 8: AI x 2006	0.0256	-0.000942	0.0528
	(0.0202)	(0.0264)	(0.0324)
Interaction 9: AI x 20008	-0.0179	-0.0416	-0.00977
	(0.0218)	(0.0297)	(0.0332)
Interaction 10: AI x 2010	0.00232	-0.0127	0.0112
	(0.0215)	(0.0290)	(0.0311)
Interaction 11: AI x 2012	-0.00997	-0.0358	-0.00471
	(0.0241)	(0.0331)	(0.0336)
Interaction 12: AI x 2014	0.0221	-0.00110	0.0281
	(0.0232)	(0.0298)	(0.0386)
Observations	15,984	7,812	8,172
R-squared	0.097	0.059	0.044

Notes: *** $p < 0.01$, ** $p < 0.05$, *$p < 0.10$. Robust standard errors in parentheses.

Table A10: The Effect of Casino Transfer on Children's Voter Turnout (Years 2002-2014)

Panel A: Pooled and Initial HH Income	Pooled		Pooled	
	(1)	(2)	(3)	(4)
Independent Variables	Ever Voted	Prop Voted	Ever Voted	Prop Voted
Interaction 1: Age Cohort 1	0.0828	0.0428	0.575***	0.317***
$\times NativeAmerican$	(0.0741)	(0.0401)	(0.128)	(0.0698)
Interaction 2: Age Cohort 2	0.0743	0.0451	0.321**	0.228***
$\times NativeAmerican$	(0.0720)	(0.0398)	(0.126)	(0.0697)
Parents Prior Voting	0.162***	0.107***	0.172***	0.112***
	(0.0419)	(0.0250)	(0.0417)	(0.0249)
Triple Interaction Cohort 1			-0.0878***	-0.0486***
(Age Group 1 x AI			(0.0243)	(0.0145)
x Initial Income)				
Triple Interaction Cohort 2			-0.0398*	-0.0311**
(Age Group 2 x AI			(0.0241)	(0.0136)
x Initial Income)				
Initial Household Income	0.0214***	0.0135***	-0.00158	-0.000460
	(0.00406)	(0.00230)	(0.00845)	(0.00463)
Mean of Dep Variable	0.3273	0.1541	0.3273	0.1541
Observations	1,332	1,332	1,332	1,332
R-squared	0.051	0.063	0.066	0.077

Panel B: By Median HH Income	Below Median HH Income		Above Median HH Income	
	(1)	(2)	(3)	(4)
Independent Variables	Ever Voted	Prop Voted	Ever Voted	Prop Voted
Interaction 1: Age Cohort 1	0.289***	0.128***	-0.115	-0.0233
$\times NativeAmerican$	(0.0817)	(0.0409)	(0.142)	(0.0857)
Interaction 2: Age Cohort 2	0.231***	0.124***	-0.0382	-0.0219
$\times NativeAmerican$	(0.0792)	(0.0433)	(0.141)	(0.0785)
Parents Prior Voting	0.125*	0.0609	0.185***	0.131***
	(0.0659)	(0.0373)	(0.0538)	(0.0327)
Mean of Dep Variable	0.2412	0.0974	0.4097	0.2083
Observations	651	651	681	681
R-squared	0.049	0.041	0.033	0.041

Note: *** p<0.01, ** p<0.05, * p<0.1 Regressions include parents' voter turnout rate before the transfer as a control, Native American indicator, gender, average household income prior to casino operation, age cohort indicator variables, age, number of children in the household below age 6 and a constant. Robust standard errors employed, but the significance thresholds remain the same if we cluster by family or use the small-N clusters approach shown by Cameron, Gelbach, Miller (2008): available upon request.

by initial household income. The regressions in columns 3 and 4 include initial household income, all relevant double interactions, and the triple interaction of initial household income with cohort and Native American race. The interaction effects in the first two rows are now larger and statistically significant indicating that the effects differ across initial household income for the children. (Recall that we found no such effects in the parents' population.) In rows 4 and 5 of columns 3 and 4 we present the triple interaction coefficients. The estimated coefficients are negative and statistically significant. These negative coefficients indicate that a child from the same race and from the youngest birth cohort who resides in a household with $5,000 lower income would realize an 8.7 percentage point increase in having ever voted over the 2002-2014 election cycles relative to another child from the same cohort and race coming from a richer household. A similar result is found for the middle cohort in row 5 but it is smaller in size and less precisely estimated. We note that parents' prior voting probability remains approximately similar in size and statistical significance in these specifications.

It is not immediately clear how to interpret the heterogeneity in outcomes across the initial income distribution; in particular the linear extrapolation of the triple interaction coefficients to the entire income distribution may be problematic. To aide in interpreting these results, we present additional analyses in the appendix and in Panel B of Table A10. First, in Appendix Figure A6 we plot the coefficients for the effect of the cash transfer on our voting outcomes in four separate partitions of the data by initial household income quartiles. Our intention here is to identify whether and where potential non-linearities in the effect may exist across the initial income distribution. Examining the results, there appears to be much larger effects for those observations from initially poorer households. In fact, we see some evidence that there is a break in the estimated effects on both of the outcome variables around the median of initial household income. The estimated coefficients are positive and statistically significant in the first and second income quartiles at the 90% level and approach zero and are slightly negative (but not statistically significant) in the third and top quartiles of pre-transfer income. In a second analysis provided in Appendix Table A12 we present the results from models in which we interact the percent change in initial household income due to the cash transfers with AI race and cohort variables. These regression estimates tell a complementary story to those found in Appendix Figure A6 that the initially poorest households experience the biggest impact of the casino transfer on the observed voting outcomes.

Based on these findings we again test for differential effects of the transfers on children from households below and above the median initial household income. Panel B of Table A10 separates the observations by individuals from households initially below and initially above the median household income (which is approximately $27,000). In the first two columns, we present similar analysis to that in Panel A columns 1 and 2 except the observations are restricted to those households that were initially below the median household income. The estimated coefficients on the interaction variables are all positive and statistically significant. These results indicate that a child from a below median income household who is exposed to exogenously higher incomes during adolescence for 2 or 4 years has about 23-29 percentage point increase in their likelihood of ever voting as compared to the control group of children who were not treated to the additional income as minors; it increases their proportions of elections voted by 12-13 percentage points (shown in column 2 of Panel B). This increase in voting probability is relatively large. However, it is important to remember the scale of the intervention being tested. The income transfers here are large enough to pull many families out of poverty. This intervention is an order of magnitude larger than previous get-out-the-vote programs. We show in Figure A3 that there is a non-linear relationship between initial household income and parental voting in the period prior to the casino payments. In particular, we find that there is a large jump in average voting probability (a steeper income gradient) for moving from incomes in the range of $20,000 to $35,000 in the GSMS population. A similar relationship seems to hold in the self-reported voting data from North Carolina in the CPS. Further, we note that the correspondence between income and concurrent voting among adults may be different than the impact of additional household income on children's voting propensities in adulthood. We are not aware of any prior research that would inform our priors about the size of the coefficients we estimate. Still, the evidence we provide from the CPS and the GSMS parents' voting propensities is broadly consistent with our results.

The next two columns in Panel B provide similar analysis for the observations that were above the median household income level prior to the income intervention. The estimated coefficients of interest are negative, smaller in absolute size than the estimated coefficients in columns 1 and 2, and not statistically significant. As predicted by the regressions in columns 3 and 4 in Panel A above, there are heterogeneous effects of extra income depending on the households pre-casino financial standing. Income transfers in early adolescence appear to narrow participatory gaps considerably helping to shrink the pre-treatment gap in voting for the youngest cohorts. We also note that for children from above median household incomes the estimated coefficient on parental prior voting is still positive and statistically significant while it was not as strongly significant in the first two columns of Panel B.

These results are remarkably robust to various alternative specifications. (We conduct a full series of robustness checks in Appendix G.) In Table A14 we conduct a difference-in-difference analysis where we combine the youngest two age cohorts and compare them to the oldest age cohort in exactly the same specification as in Table A10. Our results largely mirror the results found in Table A10. The median household income in this sample is about $27,000. The unconditional cash transfers add up to about an additional $20,000 in the first four years of treatment (see Figure A6). If we interpret the evidence in Figure A3 as a causal relationship between household income and voting, we would expect about 20 percentage points increase in voting propensity if we moved households with initial income around $20,000 up to about $40,000 in annual income (which would be comparable to receiving additional $20,000 in unearned income). Our estimates are somewhat higher, but not statistically different from an increase of 20 percentage points. Further, Figure A3 is based on parental voting, while the effects on children may be different, depending on the mechanisms at play.

Table A11: The Effect of Casino Transfer on Children's Voter Turnout (Years 2002-2014) with No Covariates

Panel A: Pooled and Initial HH Income	Pooled		Pooled	
Independent Variables	(1) Ever Voted	(2) Prop Voted	(3) Ever Voted	(4) Prop Voted
Interaction 1: Age Cohort 1	0.0921	0.0439	0.530***	0.314***
$\times NativeAmerican$	(0.0719)	(0.0403)	(0.120)	(0.0646)
Interaction 2: Age Cohort 2	0.0768	0.0356	0.330***	0.237***
$\times NativeAmerican$	(0.0704)	(0.0400)	(0.117)	(0.0636)
Parents Prior Voting	0.162***	0.107***	0.172***	0.112***
	(0.0419)	(0.0250)	(0.0417)	(0.0249)
Triple Interaction Cohort 1			-0.0804***	-0.0491***
(Age Group 1 x AI			(0.0223)	(0.0133)
x Initial Income)				
Triple Interaction Cohort 2			-0.0413*	-0.0343***
(Age Group 2 x AI			(0.0223)	(0.0128)
x Initial Income)			0.00603	0.00323
			(0.00822)	(0.00448)
Mean of Dep Variable	0.325	0.153	0.325	0.153
Observations	1,400	1,400	1,400	1,400
R-squared	0.008	0.005	0.055	0.061

Panel B: By Median HH Income	Below Median HH Income		Above Median HH Income	
Independent Variables	(1) Ever Voted	(2) Prop Voted	(3) Ever Voted	(4) Prop Voted
Interaction 1: Age Cohort 1	0.258***	0.124***	-0.0853	-0.0241
$\times NativeAmerican$	(0.0787)	(0.0386)	(0.133)	(0.0837)
Interaction 2: Age Cohort 2	0.233***	0.127***	-0.0625	-0.0474
$\times NativeAmerican$	(0.0763)	(0.0402)	(0.134)	(0.0804)
Mean of Dep Variable	0.236	0.094	0.412	0.210
Observations	692	692	708	708
R-squared	0.029	0.024	0.007	0.006

Note: *** p<0.01, ** p<0.05, * p<0.1 Regressions include Native American indicator, age cohort indicator variables and a constant. Robust standard errors employed, but the significance thresholds remain the same if we cluster by family or use the small-N clusters approach shown by Cameron, Gelbach, Miller (2008): available upon request.

Table A12: The Effect of Casino Transfer as a Percent of Initial Household Income on Children's Voter Turnout (Years 2000-2014)

VARIABLES	(1) Ever Voted	(2) Proportion Elections Voted
Interaction 1: Age Cohort 1	0.353**	0.145**
Transfer as % of Initial Income	(0.154)	(0.0730)
Interaction 2: Age Cohort 2	0.339**	0.156**
Transfer as % of Initial Income	(0.154)	(0.0763)
Initial Household	0.0201***	0.0131***
Income	(0.00423)	(0.00240)
Parents Prior	0.160***	0.105***
Voting	(0.0414)	(0.0247)
Observations	1,330	1,330
R-squared	0.053	0.064

Note: *** $p < 0.01$, ** $p < 0.05$, *$p < 0.10$. Parent's prior voting is the probability of the parents voting in the period prior to the casino operations; initial household income refers to the average household income in the period prior to the casino operations. Regressions include Native American indicator, gender, age cohort indicator variables, age, number of children in the household below age 6 and a constant. Robust standard errors employed.

Table A13: Children's Voting Probability Pooled by Initial Household Income

			Initial HH Income		
	Pooled	Below Median	Above Median	Below Median	Above Median
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Voted	Voted	Voted	Voted	Voted
Interaction 1: Age Cohort 1	0.0440	0.131***	-0.0268		
$\times NativeAmerican$	(0.0400)	(0.0409)	(0.0857)		
Interaction 2: Age Cohort 2	0.0452	0.124***	-0.0253		
$\times NativeAmerican$	(0.0395)	(0.0401)	(0.0802)		
Interaction 1: Age Group				Omitted	Omitted
$\times AI$ x 2002				Category	Category
Interaction 2: Age Group				0.131***	-0.0644
$\times AI$ x 2004				(0.0505)	(0.110)
Interaction 3: Age Group				0.0921*	0.0487
$\times AI$ x 2006				(0.0487)	(0.0835)
Interaction 4: Age Group				0.161***	0.0193
$\times AI$ x 2008				(0.0601)	(0.116)
Interaction 5: Age Group				0.103**	-0.0750
$\times AI$ x 2010				(0.0505)	(0.0952)
Interaction 6: Age Group				0.202***	-0.0380
$\times AI$ x 2012				(0.0576)	(0.114)
Interaction 7: Age Group				0.126***	-0.0124
$\times AI$ x 2014				(0.0469)	(0.0953)
Parents Prior Voting	0.108***	0.0660*	0.139***	0.0652*	-0.0124
	(0.0250)	-0.0376	-0.033	(0.0377)	(0.0953)
Observations	9,324	4,557	4,767	4,557	4,767
R-squared	0.064	0.040	0.054	0.043	0.056

Notes: *** p < 0.01, ** p < 0.05, *p < 0.10. The outcomes are binary indicator variables. Robust standard errors in parentheses.

Table A14: Children's Voting Probability by Combined Cohorts (1 and 2) Relative to Cohort 3

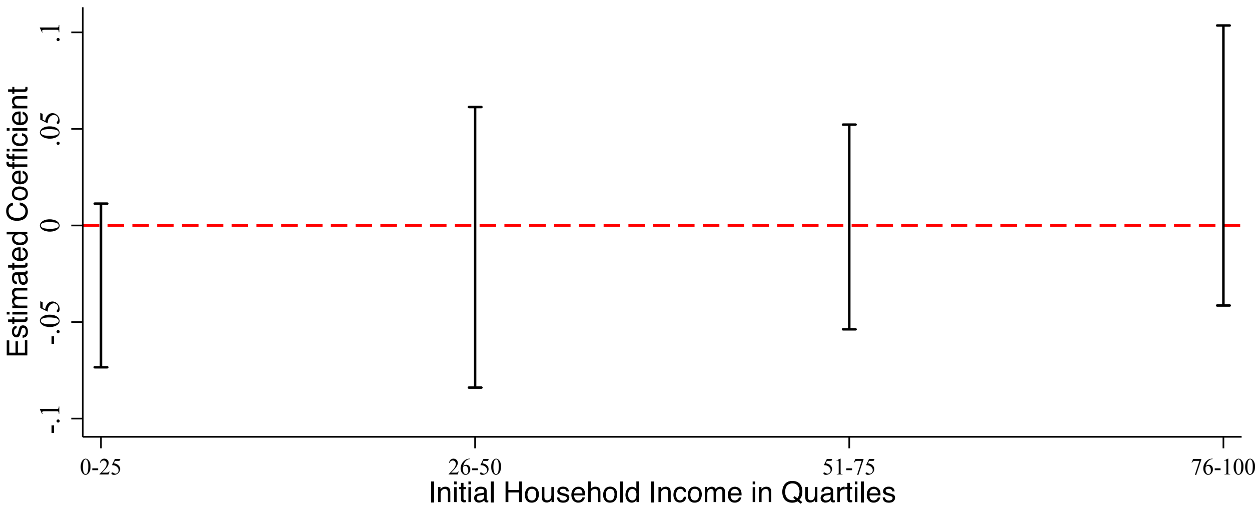
	Pooled		Below Median HH Income		Above Median HH Income	
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Ever Voted	Prop Voted	Ever Voted	Prop Voted	Ever Voted	Prop Voted
Interaction 1: Age (Cohorts 1 or 2)	0.0789	0.0437	0.259***	0.126***	-0.0813	-0.0245
x Native American	(0.0648)	(0.0348)	(0.0701)	(0.0368)	(0.125)	(0.0710)
Parent Prior Voting	0.162***	0.107***	0.128*	0.0633*	0.193***	0.135***
	(0.0418)	(0.0250)	(0.0660)	(0.0378)	(0.0535)	(0.0326)
Observations	1,332	1,332	651	651	681	681
R-squared	0.051	0.064	0.042	0.034	0.029	0.038

Notes: *** $p < 0.01$, ** $p < 0.05$, *$p < 0.10$. Robust standard errors in parentheses.

E Additional Figures

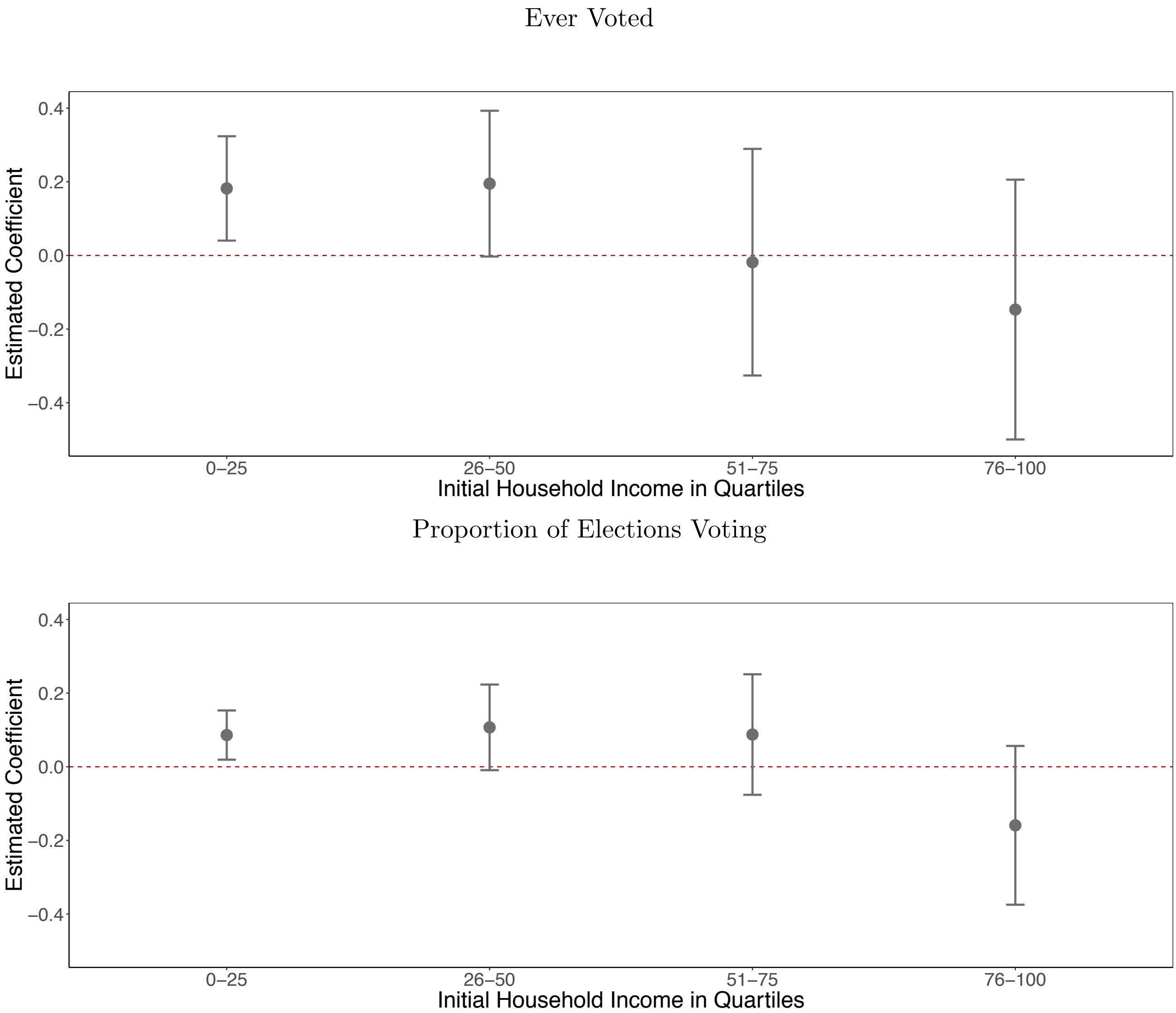
We test for different effects of the unearned income transfers across the four initial income quartiles. Figure A5 plots the coefficients and 95 percent confidence intervals by initial income quartile. Again, we find null effects across the entire income distribution. We run separate analyses on the samples by above and below initial median household incomes in the last 2 columns of Table A7 to confirm the findings in Appendix Figure A5. There are no large or statistically significant effects on parental voting probabilities in either subsample.

Figure A5: Effect of Cash Transfers on Voting for Parents by Initial Income Quartiles



Notes: This analysis separates the data into four initial income quartiles and examines the effect of the increased income on parental voting. Standard errors are given by the horizontal bars at the 95% confidence level.

Figure A6: Effect of Cash Transfer on Voting by Children by Initial Household Income Quartiles



Notes: Figure displays the impact of the casino transfers on our two voting outcomes for children by the family's initial household income at baseline in quartiles. Each coefficient is estimated on a separate partition of the data set and thus we are able to estimate coefficients for each quartile in the analysis. We combine the two youngest age cohorts together for this presentation. Robust standard errors employed and confidence intervals are given at the 90% level.

F Potential Mechanisms

We have focused on, up to now, the effect of an exogenous increase in household income on both parental and child voting probabilities. The results indicate that children from households with incomes initially below the median are most likely to increase their voting behavior as adults. There is no evidence that voting is affected for any of the parents.

There are several reasons why unconditional cash transfers may have a noticeable effect on the voting outcomes of recipients from disadvantaged childhood backgrounds. Our results are consistent with foundational voter turnout theories, which predict that extra income will have diminishing returns (Wolfinger and Rosenstone 1980) and that family income during critical periods in the lifecourse affects voting (Ojeda 2018). The recipients of the cash transfer could have realized higher levels of human capital due to the income transfers. This additional human capital may have encouraged them to vote. Alternatively, they could have experienced enhanced social networks that help mobilize them through the social component of voting. Both of these indirect channels are consistent with a model of voting based on the human capital formation framework.

Unfortunately, eliciting compelling causal mechanisms is virtually impossible for reasons discussed in the literature on this topic (Bullock, Green, and Ha 2010; Montgomery, Nyhan, and Torres 2017; Imai, Keele, and Tingley 2010; Acharya, Blackwell, and Sen 2016).We, therefore, provide suggestive evidence regarding a few of the potential mechanisms that may be behind the effects we observe.

One important mechanism that could explain the effects on disadvantaged children's eventual voting probabilities is their parents' pattern of voting (Plutzer 2002). We found that the unconditional cash transfers had precisely estimated null effects on parents; thus making it unlikely that the change in voting among children is the result of changes in voting among parents.

In theory, the cash transfers may have also increased parental education or changed their employment levels, which may play a role in affecting children's voting probability after the casino payments began. In Appendix Table A15 we show mother's educational attainment and full time employment status when the child is 16 years of age (the last survey wave that contains data about the parents). The estimating equation is the same as Equation 1. There are no statistically significant or substantively meaningful changes either in mother's educational attainment or her employment status as full time employed. We also decompose our analysis for mother's employment and educational attainment by below and above the median initial household income; we find no significant results in this analysis either.[x]

[x] We do not provide a similar analysis for fathers as there is a substantial amount of missing observations for these characteristics.

Table A15: Mother's Educational Attainment and Employment

	Pooled	Below Median HH Income	Above Median HH Income	Pooled	Below Median HH Income	Above Median HH Income
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	High School Diploma or Some College			Mother Works Full Time?		
Interaction 1: Age Cohort 1	0.0679	0.0963	0.108	0.00582	0.155	-0.169
× Native American	(0.0830)	(0.110)	(0.135)	(0.0890)	(0.128)	(0.123)
Interaction 2: Age Cohort 2	0.104	0.122	0.0169	-0.00885	0.0468	-0.0643
× Native American	(0.0844)	(0.112)	(0.138)	(0.0854)	(0.123)	(0.117)
Mean of Dep Variable	0.403	0.442	0.366	0.660	0.597	0.715
Observations	1,331	650	681	972	449	523
R-squared	0.020	0.028	0.009	0.043	0.036	0.024

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$ Regressions include parents' voter turnout rate before the transfer as a control, Native American indicator, gender, average household income prior to casino operation, age cohort indicator variables, age, number of children in the household below age 6 and a constant. Outcomes measured when children are 16 years old. Robust standard errors employed.

Moving household locations may also play an important role in changing child voting behavior in the long run. Research shows that moving negatively affects who votes, especially among young people (Ansolabehere, Hersh, and Shepsle 2012). However, the effects of income on moving in our context are unclear. On the one hand, a parent may, as a result of the increased casino transfers, move to a better community and the resulting change in peers who are more likely to vote may lead directly to the increase in child voting behavior. Or, on the other hand, those who stay in a single location over time may build up social connections that promote voting that people who move do not receive—a fact corroborated by studies that show the negative effect Moving to Opportunity had on voter participation (Gay 2012). Given previous research, we dedicate substantial effort to test if our effects are driven by movers or non-movers.

First, in Appendix Table A16, we use data on the households geographic location to show that there are no systematic differences (by receipt of the income transfer) on whether a child's parents ever moved during their childhood.[y] In Table A17 the first two columns divide the observations to households that moved and didn't move during childhood, respectively. The regression results indicate that there is no difference in the effects of cash transfers on the propensity to vote. In columns 3-6 we further separate the observations into below and above initial median household. We find a positive and statistically significant impact of the casino payment on ever voting for those households that were initially below the median income and did not move during childhood (column 4). This is consistent with a social capital formation mechanism, wherein individuals who stay in their current community build stronger connections that reinforce the importance of voting.

In the next four columns (7-10) we investigate whether the child moves as an adult and whether that is related to the observed effects on their own voting outcomes. We separate the data by whether an individual resides outside of North Carolina or within North Carolina currently (as an adult) and by their initial household income status. The results are driven by those individuals that did not leave North Carolina and who come from households that were below the median initial household incomes, which is again consistent with a social capital formation mechanism.[z]

[y] The GSMS data contain household panel location data in longitude and latitude and we used that to identify the household (and subsequently the child) location of residence.

[z] In unreported results, we find similar outcomes for whether an individual lives in a different county in North Carolina (as an adult) than the one that they grew up in during childhood. Again, the results indicate that the effects are strongest for those who remain in the same county and come from households that were below the median initial household incomes.

Table A16: Moving across Counties During Adolescent Years

		Initial HH Income	
	Pooled	Below Median	Above Median
	(1)	(2)	(3)
VARIABLES	Change County of Residence		
Interaction 1: Age Cohort 1	0.0157	0.0303	-0.0386
Native American	(0.0770)	(0.106)	(0.116)
Interaction 2: Age Cohort 2	-0.0587	-0.133	0.0828
Native American	(0.0798)	(0.108)	(0.129)
Constant	-0.219	0.242	-0.590
	(0.512)	(0.795)	(0.666)
Mean of Dep Var	0.328	0.238	0.413
Observations	1,322	646	676
R-squared	0.059	0.041	0.045

Notes: *** $p < 0.01$, ** $p < 0.05$, *$p < 0.10$. Robust standard errors in parentheses. Outcome variable measures whether a household changes their county of residence during the child's adolescent years.

Table A17: Probability of Moving During Childhood and Adulthood

			Initial HH Income				Initial HH Income			
			Below Median		Above Median		Below Median		Above Median	
	Moved in Childhood?	Did not move in Childhood?	Moved in Childhood?	Did not move in Childhood?	Moved in Childhood?	Did not move in Childhood?	Lives Outside NC Currently	Lives in NC Currently	Lives Outside NC Currently	Lives in NC Currently
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	Ever Vote?	Ever Vote?	Ever Vote?	Ever Vote?	Ever Vote?	Ever Vote?	Ever Vote?	Ever Vote?	Ever Vote?	Ever Vote?
Interaction 1: Age Cohort 1	0.0253	0.119	0.149	0.390***	-0.00661	-0.169	0.111	0.298***	-0.263	-0.101
$\times NativeAmerican$	(0.111)	(0.0948)	(0.111)	(0.113)	(0.292)	(0.161)	(0.103)	(0.0897)	(0.213)	(0.154)
Interaction 2: Age Cohort 2	0.101	0.0584	0.158	0.245**	-0.0213	-0.0199	-0.0193	0.295***	0.506	-0.137
$\times NativeAmerican$	(0.109)	(0.0918)	(0.113)	(0.105)	(0.271)	(0.173)	(0.108)	(0.0892)	(0.357)	(0.153)
Constant	1.618*	0.204	-0.418	-0.531	3.850**	0.151	0.980	-0.650	-0.162	1.461*
	(0.886)	(0.633)	(1.015)	(0.906)	(1.545)	(0.888)	(1.408)	(0.765)	(1.434)	(0.863)
Mean of Dep Var	0.287	0.347	0.198	0.265	0.420	0.410	0.126	0.265	0.199	0.467
Observations	422	900	253	393	169	507	111	540	146	535
R-squared	0.080	0.048	0.060	0.057	0.070	0.035	0.152	0.060	0.050	0.034

Notes: *** $p < 0.01$, ** $p < 0.05$, *$p < 0.10$. The first two columns separate all observations by whether the household moved during childhood or not. The next four columns add an additional separation as to whether the household was initially below or above the median household income. The next set of columns separates the observations by the initial household income and whether the individual currently lives outside of North Carolina (in adulthood). The outcome variable is a measure of whether the individual ever voted in their adulthood. Robust standard errors in parentheses

Overall, these results indicate that the effect of the cash transfers is found primarily among children from households below the median initial household income level that did not relocate during childhood or as adults. This is consistent with a social capital story, where individuals who received the cash transfers and stayed in their communities were much more likely to form the social bonds that mobilized them to vote.

An additional mechanism might be the effect of increased educational attainment due to the increase in household income for the affected children. Sondheimer and Green (2010) have shown a strong connection between education and voting.

In Table A18 we show the effect of the cash transfer on whether an individual has a high school degree by age 19. The first column provides the pooled analysis and indicates the children from the two treated cohorts have positive estimated coefficients and the effect is large and statistically significant for the youngest cohort. In columns 2 and 3 we separate out the analysis by initial household income above and below the median as in our previous analysis. The results in column 1 appear to be driven by those who receive the cash transfer from the initially poorer households. We believe that this high school completion measure is a useful indicator of a potential mechanism for the voting results. High school completion is the highest level of education one can achieve before they are first eligible to vote, and so differences in the rate of high school completion that mirror the differences in voting probability may suggest a mediating effect of education on voting.

A final mechanism that we investigate is whether the child is investing more in developing pro-social skills and associations while in adolescence. Given the findings that the largest effects of the cash transfer are concentrated among those who did not move during childhood (or adulthood), it seems reasonable to examine the social connections of the children. There are several variables that exist in the GSMS survey which indicate some measures of affability or social connectivity. We provide the results in Table A19. Each set of regressions are based on the empirical model in our main analysis and are separated by initial household income at the median. We explore three variables and take the combined reporting from both the parent and the child for these characteristics; the characteristics are binary indicator variables indicating whether the characteristic is ever present in either report or not. We also note that these characteristics are measured while the child is still residing in the household at ages 15 and 16 and both parent and child are interviewed separately.

The first outcome we explore is whether a child is reported as having difficulty making friends due to failure to approach other children; this is reported

Table A18: Child's High School Completion at Age 19

		Household Income	
	Pooled	Below Median	Above Median
	(1)	(2)	(3)
VARIABLES	High School Graduate at Age 19?		
Interaction 1: Age Cohort 1	0.326***	0.435***	0.124
Native American	(0.0852)	(0.118)	(0.130)
Interaction 2: Age Cohort 2	0.0979	0.122	0.0653
Native American	(0.0882)	(0.120)	(0.136)
Constant	0.698	0.592	1.003
	(0.584)	(0.938)	(0.750)
Observations	1,014	482	532
R-squared	0.138	0.075	0.026

Notes: *** $p < 0.01$, ** $p < 0.05$, *$p < 0.10$. Regressions include Native American indicator, gender, age cohort indicator variables, age, number of children in the household below age 6 and a constant. Columns 1 controls for initial average household income in the first three survey waves. Robust standard errors in parentheses.

in columns 1 and 2. This outcome provides an important dimension of social connections and interactions with peers. The estimated coefficients in columns 1 and 2 are negative indicating that children in households receiving the transfers are less likely to report difficulty making friends due to shyness. The magnitudes of the estimated coefficients are much larger for children from households initially below the median income level and marginally statistically significant.

We also test whether a child is considered to be a rule breaker in columns 3 and 4. This measure is coded as one if the child violates rules at school or elsewhere in society; this variable explicitly excludes rule breaking in the home and is meant to measure rule breaking in society as a whole. The first estimated coefficient in column 3 is large, negative and marginally statistically significant. The estimated coefficients in column 4 are small in magnitude and do not reach statistical significance at conventional levels.

The results presented in Table A19 provide some suggestive evidence that social connections may be marginally stronger for children who resided in households receiving the cash transfer and from initially lower income households. There is some evidence that these children exhibit slightly stronger societal connections (less difficulty making friends) and are less likely to be considered antisocial (less of a rule breaker) relative to the control groups. These connections may translate into the civic domain, given the strong connection between social skills and networks in adolescence and participation in adulthood (McFarland and Thomas 2006; Holbein 2017). We do acknowledge, however, that the results are not nearly as precise as we would like in order to be able to determine the channel through which additional household income affects child voting behavior.

Table A19: Child's Social Characteristics At Age 15 or 16

	Initial HH Income		Initial HH Income	
	Below Median	Above Median	Below Median	Above Median
	(1)	(2)	(3)	(4)
VARIABLES	Refuses or Unable to be involved or talk with peers?		Violates rules at school or elsewhere outside the home?	
Interaction 1: Age Cohort 1	-0.114*	-0.0284	-0.290**	0.0211
Native American	(0.0588)	(0.0612)	(0.120)	(0.128)
Interaction 2: Age Cohort 2	-0.0631	-0.0669	-0.0750	0.0532
Native American	(0.0599)	(0.0625)	(0.116)	(0.143)
Constant	-0.378	0.542	-1.813**	0.243
	(0.535)	(0.393)	(0.879)	(0.846)
Mean of Dep Variable	0.087	0.054	0.563	0.455
Observations	482	541	474	539
R-squared	0.028	0.021	0.155	0.100

Notes: *** $p < 0.01$, ** $p < 0.05$, *$p < 0.10$. Outcome variables combine both parental and child reporting for these characteristics. The outcomes are binary indicator variables at ages 15 or 16 for the children. Regressions include Native American indicator, gender, age cohort indicator variables, age, number of children in the household below age 6 and a constant. Robust standard errors in parentheses.

While we do not claim to be able to identify definitive mechanisms for how the increased household income affects the long-run voting probability for low income children, we have provided some evidence against potential channels and some tentative evidence in favor of other channels. Initial parental voting probabilities and changes to parental education or employment do not appear to be driving the results.

The child's own high school completion rates by age 19 does suggest a potential channel via human capital accumulation as an important mechanism. We do find some suggestive evidence that children report more connection to their friends and peers as adolescents as a result of the cash transfers. These relationships in conjunction with remaining in place over a longer time period may increase the social capital for these individuals and thus drive the higher voting probabilities as adults. While this clearly may not explain the whole effect, it is likely an integral part of the story.

G Additional Robustness Checks

In Appendix Table A20 we restrict individuals to be of a comparable age and compare their voting behavior in different elections. The empirical specification here is the same as in Equation 2, but we change the outcome variable to be the probability of voting in the first election in which all children are above 18 and eligible to vote. This analysis compares different duration of treatment effects for individuals who are the same age in a particular election. It is intended to address the potential issue that we cannot hold both age and election cycle constant in the cohort analysis. Specifically, here we compare individuals from the oldest and the youngest cohort voting in US Congressional elections in 2002 and 2006, respectively; individuals in these cohorts were approximately 21-22 years old during those elections. We omit the middle age cohort since they were 21 years of age in 2004 which was a US Presidential election which typically has a higher voter turnout than Congressional elections. We find, in columns 1-3, that the youngest cohort and those from the poorest households show the largest effects on voting probabilities. These results indicate that age differences at various election cycles are not driving the main results.

Table A20: Children's Voting Probabilities at Similar Ages and in 2002 Election

	Pooled	Initial HH Income Below Median	Initial HH Income Above Median	Pooled	Initial HH Income Below Median	Initial HH Income Above Median
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES		Voted at Age 21?			Voted in 2002 Election?	
Age Cohort 1 or 2	-0.0109	0.0747*	-0.101	0.0173	0.0792***	-0.0528
x Native American	(0.0444)	(0.0382)	(0.0999)	(0.0355)	(0.0272)	(0.0819)
Observations	864	424	440	1,332	651	681
R-squared	0.037	0.013	0.039	0.027	0.018	0.027

Notes: In columns 1-3 the variable in row one is only Age Cohort 1 x Native American. In columns 4-7, the variable in row one is Age Cohort 1 or 2 x Native American. We omit the second age cohort for the regressions in columns 1-3 in order to compare similar midterm elections at age 21. *** $p < 0.01$, ** $p < 0.05$, *$p < 0.10$. Robust standard errors in parentheses.

In the next three columns of Appendix Table A20, we test whether the age at intervention has an effect on the observed results. We consider the difference in the propensity to vote in the 2002 election for all cohorts. By 2002, everyone in the AI population had been treated to 6 years of transfers in 2002. Holding the duration of treatment constant allows us to explore whether the age at intervention matters differentially for the observed results on voting probability. The assumption here is that age-by-race-specific differences in the propensity to vote 2002 are not large enough to impact the findings. We find, again, that there are positive and statistically significant results for the youngest cohort from the poorest households – indicating that the age at first treatment matters.

The results presented are remarkably robust to alternate specifications. For example, in Appendix Table A21 we estimate the same cohort based difference-in-difference specification for parents that we used for children (see equation 1). Our intention here is to compare whether parents of different cohorts were more likely to vote. For example, if the parents of a particular cohort had greater voting preferences or were differentially affected themselves somehow, they may have been more likely to transmit this habit to their children. We explore whether some of the effects that we observed in Table A10 are attributable to cohort-based differences that emerge already across parents (in their respective cohorts). In part, this is also helpful to explore potential mechanisms driving our effect among children.

Table A21: The Effect of Casino Transfer on Parents' Voter Turnout (Probability of Voting)

Panel A: Unweighted		Initial HH Income		Unweighted		Initial HH Income	
	Pooled	Below Median	Above Median		Pooled	Below Median	Above Median
	(1)	(2)	(3)		(4)	(5)	(6)
Independent Variables	Voted	Voted	Voted		Voted	Voted	Voted
Age Cohort 1	-0.0447	-0.125	0.0595	Age Cohort 1 or 2	-0.00653	-0.0235	0.00110
x Native American	(0.0631)	(0.0787)	(0.114)	x Native American	(0.0331)	(0.0464)	(0.0465)
				x Avg Init Income			
Age cohort 2	-0.0132	-0.0509	-0.0249				
x Native American	(0.0664)	(0.0851)	(0.114)				
Year FE	Y	Y	Y	Year FE	Y	Y	Y
Ind FE	N	N	N	Ind FE	Y	Y	Y
N (families)	1332	651	681	N (families)	1332	651	681
R-squared	0.097	0.061	0.045	R-squared	0.056	0.072	0.046

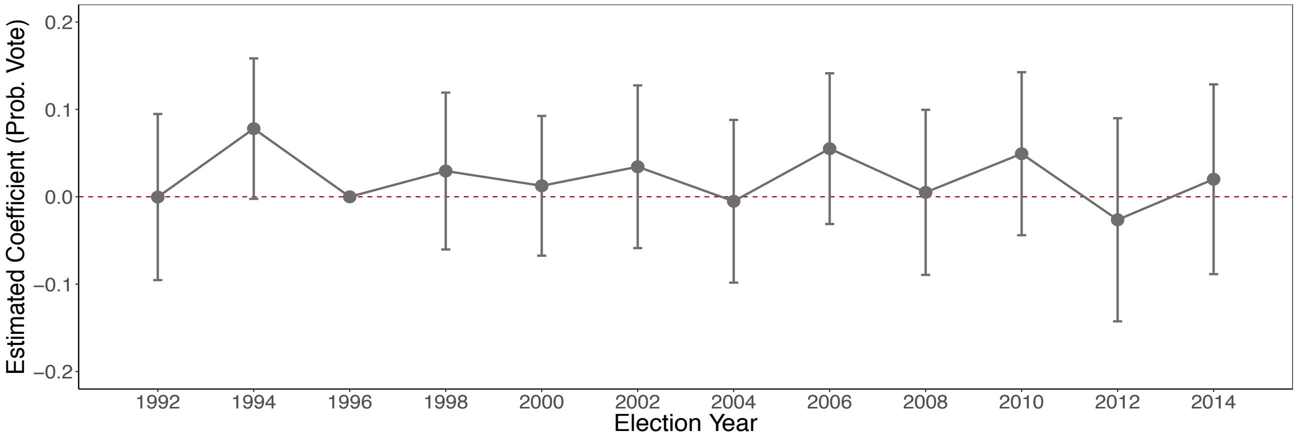
Panel B: Weighted		Initial HH Income		Weighted		Initial HH Income	
	Pooled	Below Median	Above Median		Pooled	Below Median	Above Median
	(1)	(2)	(3)		(4)	(5)	(6)
Independent Variables	Voted	Voted	Voted		Voted	Voted	Voted
Age cohort 1	-0.00813	-0.0707	0.0565	Age cohort 1 or 2	-0.00480	-0.0173	-0.00696
x Native American	(0.0580)	(0.0683)	(0.110)	x Native American	(0.0333)	(0.0465)	(0.0462)
				x Avg Init Income			
Age cohort 2	-0.0125	-0.0430	-0.0428				
x Native American	(0.0601)	(0.0728)	(0.107)				
Year FE	Y	Y	Y	Year FE	Y	Y	Y
Ind FE	N	N	N	Ind FE	Y	Y	Y
N (families)	1332	651	681	N (families)	1332	651	681
R-squared	0.104	0.052	0.043	R-squared	0.055	0.069	0.047

Notes: *** $p < 0.01$, ** $p < 0.05$, *$p < 0.10$. Robust standard errors in parentheses.

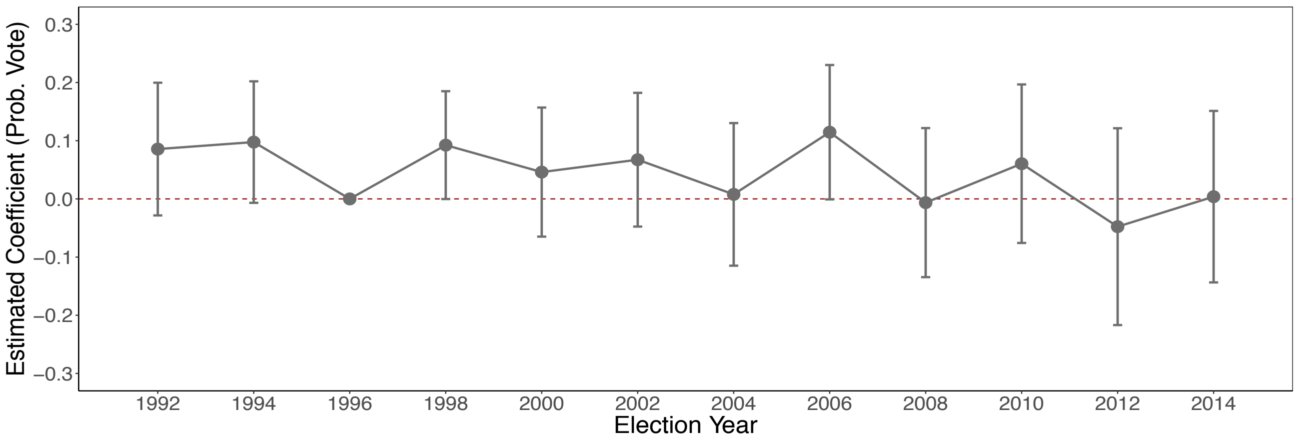
We present this difference-in-difference in Appendix Table A21. Figure A7 provides the event analysis for these same results and Appendix Table A22 provides the regressions used in the figure. We estimate the model among the pooled sample as well as split by initial median household incomes. The difference-in-difference results are presented in Panel A of Appendix Table A22 and the weighted models are presented in Panel B. There are no statistically significant results in these analyses, which accords with our earlier findings in Table A7. This suggests that the effect on children that we observe may be unrelated to parents socializing their children to the norm of voting through their own example.

Figure A7: Effects of Casino on Parents Voting, Event Analysis

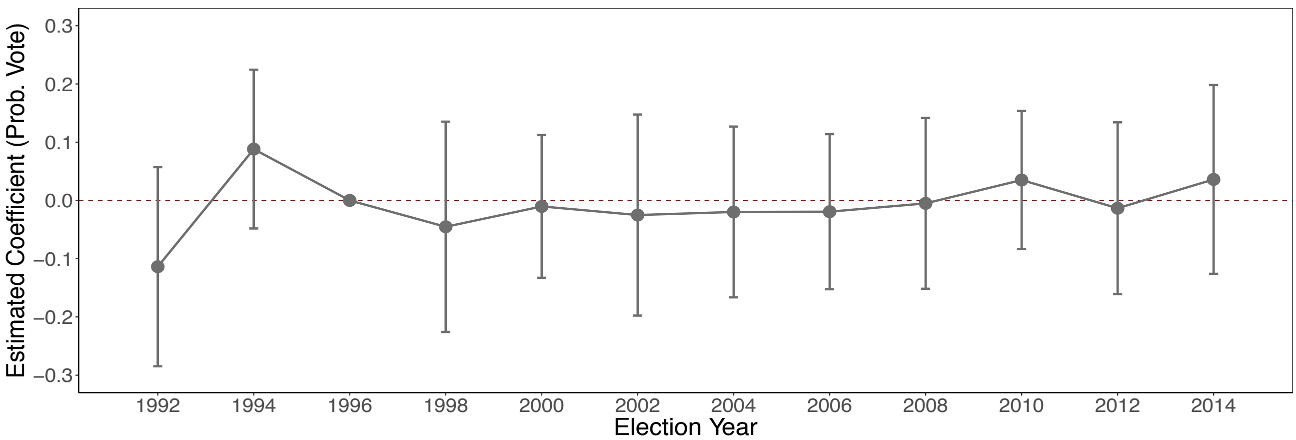
Pooled Parents Estimates



Parents Below Median Income at Baseline



Parents Above Median Income at Baseline



Notes: Figure displays coefficients from event analysis model for parents' voter turnout in the 1992-2014 elections. The estimates are split by median family income levels at baseline. Standard errors are clustered at the individual level.

Table A22: Parents Event Analysis Regression Tables by Age Combined Age Cohorts

		Initial HH Income	
	Pooled	Below Median	Above Median
	(1)	(2)	(3)
VARIABLES	Voted	Voted	Voted
Interaction 1: Age Group	-0.000278	0.0856	-0.114
$\times AI$ x 1992	(0.0485)	(0.0582)	(0.0872)
Interaction 2: Age Group	0.0780*	0.0976*	0.0881
$\times AI$ x 1994	(0.0410)	(0.0533)	(0.0695)
Interaction 3: Age Group	Omitted Category	Omitted Category	Omitted Category
$\times AI$ x 1996			
Interaction 4: Age Group	0.0295	0.0924*	-0.0452
$\times AI$ x 1998	(0.0458)	(0.0473)	(0.0921)
Interaction 5: Age Group	0.0126	0.0461	-0.0103
$\times AI$ x 2000	(0.0408)	(0.0566)	(0.0625)
Interaction 6: Age Group	0.0344	0.0673	-0.0251
$\times AI$ x 2002	(0.0475)	(0.0586)	(0.0881)
Interaction 7: Age Group	-0.00513	0.00776	-0.0198
$\times AI$ x 2004	(0.0475)	(0.0625)	(0.0748)
Interaction 8: Age Group	0.0551	0.115*	-0.0193
$\times AI$ x 2006	(0.0440)	(0.0589)	(0.0679)
Interaction 9: Age Group	0.00508	-0.00639	-0.00503
$\times AI$ x 2008	(0.0482)	(0.0654)	(0.0748)
Interaction 10: Age Group	0.0493	0.0605	0.0351
$\times AI$ x 2010	(0.0476)	(0.0695)	(0.0605)
Interaction 11: Age Group	-0.0263	-0.0477	-0.0134
$\times AI$ x 2012	(0.0593)	(0.0863)	(0.0753)
Interaction 12: Age Group	0.0201	0.00383	0.0361
$\times AI$ x 2014	(0.0554)	(0.0752)	(0.0827)
Observations	15,984	7,812	8,172
R-squared	0.097	0.060	0.045

Notes: *** $p < 0.01$, ** $p < 0.05$, *$p < 0.10$. Robust standard errors in parentheses.

H Appendix References

1. Baez, Javier E., Adriana Camacho, Emily Conover, and Romn Andrs Zrate. Conditional Cash Transfers, Political Participation, and Voting Behavior. No. 6870. Institute for the Study of Labor (IZA), 2012.

2. Bagues, Manuel, and Berta Esteve-Volart. "Politicians luck of the draw: Evidence from the Spanish Christmas lottery." Journal of Political Economy 124, no. 5 (2016): 1269-1294.

3. Berent, M. K., Krosnick, J. A., and Lupia, A. (2016). Measuring Voter Registration and Turnout in Surveys Do Official Government Records Yield More Accurate Assessments? Public Opinion Quarterly, 80(3), 597-621.

4. Bullock, John G., Donald P. Green, and Shang E. Ha. "Yes, but whats the mechanism?(dont expect an easy answer)." Journal of personality and social psychology 98, no. 4 (2010): 550-558.

5. Brunner, Eric, Stephen L. Ross, and Ebonya Washington. "Economics and policy preferences: causal evidence of the impact of economic conditions on support for redistribution and other ballot proposals." Review of Economics and Statistics93, no. 3 (2011): 888-906.

6. Burden, Barry C., Jason M. Fletcher, Pamela Herd, Donald P. Moynihan, and Bradley M. Jones. "How different forms of health matter to political participation." The Journal of Politics 79, no. 1 (2017).

7. Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller. "Bootstrap-based improvements for inference with clustered errors." The Review of Economics and Statistics 90, no. 3 (2008): 414-427.

8. Costello, E. Jane, Adrian Angold, Barbara J. Burns, Dalene K. Stangl, Dan L. Tweed, Alaattin Erkanli, and Carol M. Worthman. "The Great Smoky Mountains Study of Youth: goals, design, methods, and the prevalence of DSM-III-R disorders." Archives of general psychiatry53, no. 12 (1996): 1129-1136.

9. Costello, E. Jane, E. M. Farmer, Adrian Angold, Barbara J. Burns, and Alaattin Erkanli. "Psychiatric disorders among Native American and white youth in Appalachia: the Great Smoky Mountains Study." American journal of public health87, no. 5 (1997): 827-832.

10. De La O, Ana L. “Do conditional cash transfers affect electoral behavior? Evidence from a randomized experiment in Mexico.” American Journal of Political Science 57, no. 1 (2013): 1-14.

11. De La O, Ana L. Crafting Policies to End Poverty in Latin America. Cambridge, MA: Cambridge University Press, 2015.

12. Galiani, Sebastian, Nadya Hajj, Pablo Ibarraran, Nandita Krishnaswamy, and Patrick J. McEwan. Electoral reciprocity in programmatic redistribution: Experimental Evidence. No. w22588. National Bureau of Economic Research, 2016.

13. Gay, Claudine. “Moving to opportunity: The political effects of a housing mobility experiment.” Urban Affairs Review 48, no. 2 (2012): 147-179.

14. Grimmer, Justin, Eitan Hersh, Marc Meredith, Jonathan Mummolo, and Clayton Nall. ”Comment on Voter Identification Laws and the Suppression of Minority Votes.” Journal of Politics (Forthcoming).

15. Healy, Andrew, and Neil Malhotra. “Retrospective voting reconsidered.” Annual Review of Political Science16 (2013): 285-306.

16. Healy, Andrew, and Gabriel S. Lenz. “Substituting the End for the Whole: Why Voters Respond Primarily to the ElectionYear Economy.” American Journal of Political Science 58, no. 1 (2014): 31-47.

17. Healy, Andrew, and Gabriel S. Lenz. “Presidential Voting and the Local Economy: Evidence from Two Population-Based Data Sets.” The Journal of Politics 79, no. 4 (2017).

18. Imai, Kosuke, Gary King, and Carlos Velasco Rivera. ”Do nonpartisan programmatic policies have partisan electoral effects? Evidence from two large scale randomized experiments.” Unpublished manuscript, Princeton University and Harvard University Retrieved from https://gking. harvard. edu/files/gking/files/progpol. pdf (2016).

19. Imai, Kosuke, Luke Keele, and Dustin Tingley. “A general approach to causal mediation analysis.” Psychological methods 15, no. 4 (2010): 309-334.

20. Jennings, M. Kent, Gregory B. Markus, Richard G. Niemi, and Laura Stoker. “Youth-parent socialization panel Study, 19651997: Four waves combined. ICPSR04037-v1.” Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor] 11 (2005).

21. Johnson, James H., John D. Kasarda, and Stephen J. Appold. "Assessing the Economic and Non-Economic Impacts of Harrahs Cherokee Casino, North Carolina." Frank Hawkins Kenan Institute of Private Enterprise Technical Report (2011).

22. Lewis-Beck, Michael S. and Mary Stegmaier. 2007. "Economic Models of Voting," in Russell Dalton and Hans-Dieter Klingemann (eds.), The Oxford Handbook of Political Behavior. Oxford: Oxford University Press, pp. 518-37.

23. Linos, Elizabeth. "Do conditional cash transfer programs shift votes? Evidence from the Honduran PRAF." Electoral studies32, no. 4 (2013): 864-874.

24. Pop-Eleches, Cristian, and Grigore Pop-Eleches. "Targeted government spending and political preferences." Quarterly Journal of Political Science 7, no. 3 (2012): 285-320.

25. Solon, Gary, Steven J. Haider, and Jeffrey M. Wooldridge. "What are we weighting for¿' Journal of Human resources 50, no. 2 (2015): 301-316.

26. Zucco, Cesar. "Conditional cash transfers and voting behavior: Redistribution and clientelism in developing democracies." Working Paper (2011).