Family Income and Voting Behavior Across Generations: Evidence from an Income Intervention

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Abstract

Despite clear evidence of a sharp income gradient in voting participation, it remains unclear whether income truly causes voting. In this paper, we investigate how exogenous increases in unearned income affect voting in U.S. elections for two generations (parents and children) from the same household. In contrast to expectations made by current models of voting, we find that this income shock had precisely no effect on parents' voting behaviors. However, we also show that increasing household income has heterogeneous effects on the civic participation of children from different socioeconomic backgrounds. It increases children's voting propensity among those raised in initially poorer families but has no effect on children from initially richer families—resulting in substantially narrowed voting participation gaps. Our results indicate that positive changes in household income can have substantially differential effects on individual civic participation depending on when they happen during the life course.

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1 Introduction

In the United States, only 60 percent of eligible citizens vote in presidential elections and only 40 percent turn out in midterm elections (turnout in local elections being even lower still). As a result, the U.S. has the dubious honor of falling in the bottom third of worldwide voter turnout distribution. Moreover, there are stark differences in voter participation by income level. People who are more affluent are much more likely to participate in politics than those who are less affluent, as documented by a large number of observational studies (Frey 1971; Leighley and Nagler 2013; Ojeda 2018; Schlozman, Brady, Verba 2018; Smets and van Ham 2013; Verba, Schlozman, and Brady 1995; Verba and Nie 1987). In light of the recent increases in income inequality (Piketty 2014), income gaps in political participation are troubling as they may fundamentally shape who gets elected and the types of policies implemented (Griffin and Newman 2005; Schlozman, Verba, Brady 2012, 2018).

While the presence of a participatory gap between high and low-income individuals is well established, scholars know less about whether income is the driving force behind these gaps, or instead, income gradients in voting reflect some other unobserved social or contextual factor. As a result, we do not know whether increasing household income would actually improve overall levels of civic engagement and narrow gaps in voting behavior. This question is inherently difficult to answer, as incomes are (typically) not exogenously distributed. Moreover, there has been little research into how income interacts with the life course and whether children's propensities to vote are affected by the family environment, by family income, or both (except for recent work by Ojeda 2018, which we discuss below). We also have surprisingly little contemporary research exploring if political participation is transmitted across generations (like socio-economic status).

In this paper, we explore whether income has an effect on civic participation, whether the propensity to vote is similar across generations from the same household, and whether the intergenerational transmission of political participation can be affected by changes in family income. To do so, we investigate the effects of a quasi-experimental unconditional cash transfer program. We examine a unique longitudinal dataset from the Great Smoky Mountains Study (GSMS): a study of children in rural western North Carolina, which began in 1993 and consisted of both American Indian (AI) and non-American Indian families in the area. Halfway through the

initial 8-year survey time frame, a casino opened on the Eastern Cherokee reservation located in this region. Upon its opening, a portion of the profits were distributed to all adult tribal members independent of employment status, income, or other characteristics relevant to political engagement. This exogenous unconditional income transfer, along with the unique longitudinal nature of the data, allow us to use various panel techniques to explore the effects of positive changes in household incomes on the political participation of parents and children from the same household.

We first test whether a positive change in income has an effect on parents' voting probability. As we describe below, the Resource Model of Voting (RMV)—and other similar voting models—would suggest that positive income shocks should have a noticeable effect (Verba, Schlozman, and Brady, 1995). However, we find a precisely-estimated null effect for parental voter turnout. These results are robust to examining different parts of the income distribution prior to the income intervention. They help shine light on a research question that has previously shown decidedly mixed results (Smets and Van Ham 2013)—showing evidence that exogenous unearned income, bestowed in adulthood does not impact overall rates of or gaps in voter turnout.

Second, we test whether there is any impact of the additional household income on the household children. We first find a strong positive correlation between parents' and children's validated voter turnout, suggesting substantial intergenerational transmission of voting behavior in the absence of external shocks such as the income transfers we study here. We find this additional unearned income is associated with an increase in individual voting as adults for affected children from the initially poorest households. These results are consistent with theories that predict diminishing marginal returns of income and with theories that additional resources have differential effects across the lifecourse. The increase in voting behavior closes the participatory gap between high and low-income individuals of this rising generation. Average annual unconditional transfers of approximately $4,700 (in 2000 dollars) increase the initially poorer group's voter turnout by about 8-20 percentage points, depending on the age of the recipients and the type of measures of voting one considers. Our checks (found in the Appendix) suggest that this effect is unlikely to be driven by the transmission of voting from parents to children, changes in

mother's education and employment, or movement to new residential locations out of state. We provide some suggestive evidence that the results may, indeed, be driven by previously theorized channels—educational attainment and the dynamics of social capital/social skill acquisition over the life course.

Our work makes several important contributions to theory and to policy. Conceptually, our study helps answer the question of whether income contributes to underlying levels of voter participation. In so doing, it adds nuance to the foundational resource model of voting developed by Verba, Schlozman, and Brady (1995). Our findings are consistent with a more complex voter turnout model that allows for childhood resources to affect future civic participation in a manner consistent with the human capital formation literature from economics (e.g. Almond and Currie 2011; Currie 2011; Becker and Tomes 1986; Heckman et al 2013). We find support for the two-gaps hypothesis recently put forth in political science (see Ojeda, 2018) by showing that a person's economic history during childhood and young adulthood matter a great deal.

Second, given the intergenerational element to our analyses, the results also contribute to our understanding of political socialization. In seeking to understand why some people develop into active citizens, while others do not, social scientists have tended to focus almost exclusively on adulthood experiences—when citizens are just coming of age or are already eligible to vote—rather than on those that occur in childhood or early adolescence. Political socialization research once focused on childhood in hopes of discovering the roots of political participation (e.g. Dawson and Prewitt 1968; Langton 1969; Searing, Schwartz, and Lind 1973), with early research arguing that "the more important a political orientation is in the behavior of adults, the earlier it will be found in the learning of the child" (Greenstein 1965, p. 12). Though various theoretical models have postulated that resources allocated earlier in the life course may matter more than those delivered later, little to no contemporary research has explored this possibility.[1] And the research that has scratched this topic has generally struggled to elicit causal estimates. Our research provides compelling evidence that early life experiences—in this case, the receipt of additional income—have a greater effect on participation than *the same experiences* among members of

[1]We are not the first to identify this gap in scholarly research (see also Sapiro 2004, 1; Niemi and Hepburn 1995, 7; Miller and Saunders 2016).

the same family later in life. This implies that voting propensities are not a heritable trait that is transmitted from one generation to the next, but, instead, can be influenced by malleable aspects of the household environment during childhood.

Third, our paper draws attention to a minority group that has been largely ignored in previous voting research. There are very few studies of voter turnout among Native Americans (Frymer 2016). Existing studies have shown that turnout rates among this group are low, with scholars speculating that this is the case as a result of low socioeconomic status, distrust in the federal government, exposure to demobilizing electoral rules, and a lack of contact from mobilization campaigns (De Rooij and Green 2017; Peterson 1997; Schroedel and Hart 2015; Schroedel et al. 2017). However, we have little sense of patterns of validated voting among this population, much less how to increase their levels of participation. Our work is a step forward in closing this gap in the literature.

Finally, our results have implications for public policy. Discussions about the merits of various income distribution schemes are at the heart of a multitude of policy reforms: from debates over progressive taxation, income inequality, various forms of social welfare, minimum wages, to more recent discussions of unconditional cash transfer programs and those surrounding universal basic income (UBI). Our results suggest that unconditional income transfers may have broader effects than previously realized. In particular, they may affect levels of civic engagement or social capital. Simply put, income augmentation programs might be an effective means of closing the income gap in voting, as long as they are targeted early in the life course. In a context of soaring levels of income inequality, this finding is of critical import given that civic participation plays a vital role in preserving democratic values and institutions, connects individuals in communities to one another, and promotes democratic accountability.

2 Background and Conceptual Framework

What drives people to participate in politics? Various theories have been put forth to answer this question. These include rational choice models—wherein citizens consider the various costs and benefits of voting—psychological models—wherein citizens' voting decisions are shaped by their

internal motivational attachments—and sociological models—wherein citizens' voting decisions are shaped by their social networks (e.g. Downs 1957; Riker and Ordeshook 1968; Rosenstone and Hansen 1993; Miller and Saunders 2016).

Regardless of the framework used, each of these models typically starts from the point that voting is costly. To vote, citizens face a number of obstacles: registering before pre-set deadlines, locating and traveling to polling locations, waiting in line at the ballot box, navigating inclement weather on Election Day, and (hopefully) learning about the candidates and issues in advance of the election (e.g. Corvalan and Cox 2013; Burden et al. 2014; Leighley and Nagler 2013; Wolfinger and Rosenstone 1980; Atkeson et al. 2010). Together, these obstacles exert a non-trivial strain on citizens' time, energy, and cognitive resources.

One theory that stands out as explaining why some citizens, but not others, overcome these voting costs is the resource model of voting (RMV). The RMV states that because voting is costly, the resources that individuals possess play a key role in determining who votes and who stays home—simply put, resources help people overcome voting obstacles (Almond and Verba 1963; Verba and Nie 1972; Verba, Schlozmann, and Brady 1995; Schlozman, Brady, Verba 2018). Resources theorized to be important for voting include education, health, information, skills, time, and income (Sondheimer and Green 2010; Burden et al. 2017; Lassen 2005; Holbein 2017; Holbein and Schafer 2017). Importantly, under the RMV these resources act to increase the chances one turns out and votes *regardless of the timing* of their accumulation in the life course.

2.1 Income and Political Participation

Among voting resources, income has been thought to play an especially important role. Indeed, foundational models of voting behavior place income as a core voting resource—along with time and skills (Verba, Schlozman, and Brady 1995; Schlozman, Brady, Verba 2018). At first glance this relationship yields a puzzle: despite having a higher opportunity cost for engaging in acts like voting, affluent citizens are much more likely to vote than the less affluent (Frey 1971; Leighley and Nagler 2013; Milbrath 1965; Verba and Nie 1987; Schlozman, Brady, Verba 2018). Many attempts have been made to provide a theoretical rationale for this positive relationship.

These revolve around two primary channels: human capital acquisition and social norms.[2]

Some have argued that income increases individual investments in education, skills, and health that make it easier for one to participate in politics. For example, Frey argues that "citizens with high paying jobs are more used to deal with political questions which are in principle of the same character as their daily work, and which are therefore done much more efficiently" (1971, 104-105). Consistent with this view, Wolfinger and Rosentsone (1980, 20) argue, "well-to-do people are likely to acquire in their jobs the interests and skills that lead to political involvement and voting." Wolfinger and Rosenstone (1980, 20) further argue, "Desperately poor people are preoccupied by the struggle to keep body and soul together ... They have no time or emotional energy for nonessentials such as voting." The skills gained when someone is endowed with great wealth may include cognitive abilities such as the ability to read and write, which make consuming political information easier (Denny and Doyle 2008; Verba, Schlozman, and Brady 1995; Schlozman, Brady, Verba 2018), the so-called non-cognitive abilities that help citizens follow-through on their intention to participate in politics (Holbein 2017), and the personality traits thought to be important for voting (Mondak 2010; Gerber et al. 2011; Akee et al. 2018).

Alternatively, some have argued that income increases the likelihood of voting by enhancing citizens' social status and social connections. Under this framework, income makes it more likely that citizens are socialized to a norm of voting. For example, in their seminal work on voting, Wolfinger and Rosenstone (1980, p. 21) argue that "income determines one's neighborhood and, perhaps, avocational companions and thus exposure to a variety of norms and pressures." Income enhances social capital: helping build connections that did not exist before. In this way, income increases political motivation and inculcates values that orient citizens toward participating in politics.

Importantly, the voting literature makes clear predictions that increasing income will exhibit diminishing returns—that is, that income may only matter up to a point (Wolfinger and Rosentstone 1980, 21; Leighley and Nagler 2013; Verba and Nie 1987; Verba, Schlozman, and

[2]Ojeda (2018) argues that the theoretical channels linking income and voting can be grouped into four likely mechanisms, which include: resources, recruitment, mobilization, and psychology.

Brady 1995). For those who are poor, income may matter a great deal for voting; for those who are well-off already, additional income may matter very little. While this theoretical prediction has some face validity and some observational empirical support, it has yet to be fully tested.

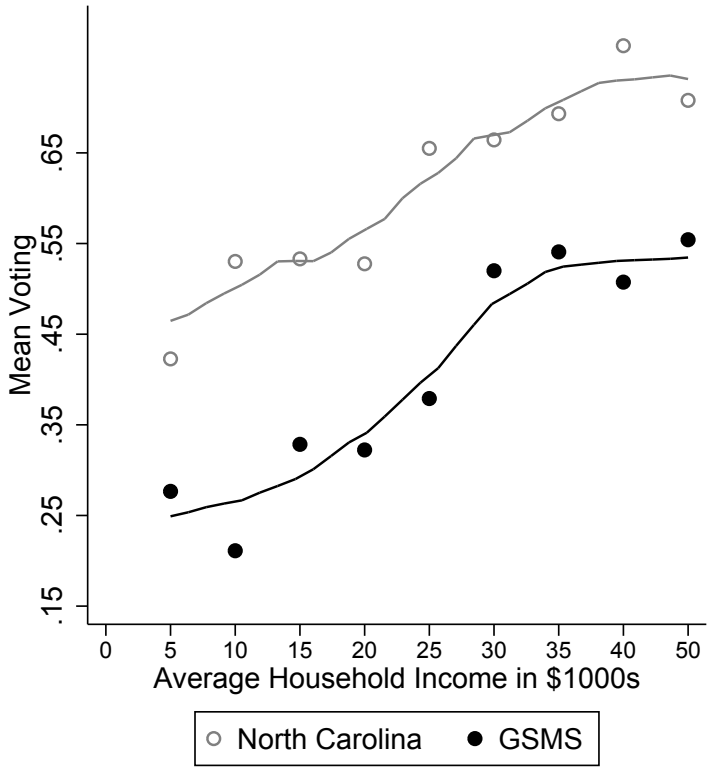
2.2 Empirical Evidence Linking Income and Political Participation

There is empirical evidence to support the theoretical prediction that voter turnout increases with income. Virtually all data sources that have measures of income and voting indicate that they exhibit a strong positive association (Ojeda 2018). This is true in the data we use as well. Figure 1 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 1 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.[3]

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

[3]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 1: 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). Data for North Carolina are drawn from the Current Population Survey 1992 November file. We plot the average voting turnout by income bins as given and show a polynomial fit for each of the two groups.

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.

While there is clearly an income gradient in voting, this does not mean that there is a causal relationship between income and voting. Indeed, this relationship could be spurious. Acknowledging this possibility, a host of researchers have dug deeper than the bivariate relationship we show in Figure 1. These studies condition on observable individual and contextual characteristics. From this group of studies, the evidence of the relationship between income and participation is decidedly mixed. A recent meta-analysis of 90 studies shows that about half of

studies find that income is an important predictor for voting, while the other half do not (Smets and Van Ham 2013). (As a reference, Smets and Van Ham (2013) report that educational attainment and age showed signs of being significant predictors in about 70% of studies/tests.) For a very thorough examination of the conditional-on-observables relationship between income and voting, see Ojeda (2018).

Overall, the empirical research on income and civic participation is inconclusive. Here we argue that these mixed findings occur, in large part, because of lack of good identification. In systematically reviewing the studies included in Smets and Van Ham's (2013) meta-analysis, it is clear that none leverages exogenous variation in income. One strand of research gets close to so doing: 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).[4] 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.[5]

To our knowledge, there is only one study of the effects of unconditional cash transfers on voting.[6] 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

[4]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.

[5]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.

[6]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.

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.[7] 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).

Indeed, there is some suggestive evidence that the attitudes, skills, and identities that govern political behavior harden by late adolescence. For example, Prior (2010, 2017) shows that after late adolescence (i.e. when one turns 18), one's interest in politics—one of the strongest predictors of whether one votes (Veba, Schlozman, and Brady 1995)—tends to exhibit remarkable levels of intertemporal stability and rigidity to targeted intervention. Furthermore, the cognitive and non-cognitive skills important for voting may solidify over time (Holbein 2017); as a result, resource investments designed to target these skills may have less of an effect on voter turnout than earlier investments. Consistent with this view, some research has shown that voting patterns tend to be persistent over time (Gerber, Green, and Shachar 2003; Coppock and Green 2015). However, this habitual model of voting only considers adulthood—once individuals are eligible to vote—and only explores the role of voting in one period on voting in the next. It has little to say about what gets people to vote the first time, or about the effect of resources accumulated before individuals are eligible to register and vote.

Consistent with human capital models, one might expect that if income does matter for voting it may matter more-so for income accumulated earlier in the life course rather than later. In this paper we test whether this is the case.

[7] 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.

3 Data

To test the effect of income on voter turnout across generations, we use data from a unique quasi-experiment from Western North Carolina. Specifically, we employ survey data from the Great Smoky Mountain Study (GSMS)—a longitudinal study of 1,420 children and their parents that began in 1993—matched to administrative data records on voting.[8] The survey was originally designed as a means of studying the mental health and well-being of children (Costello et al, 1996).

At the beginning of the survey, the children of the three participating cohorts were 9, 11, and 13 years old. The sample was designed to be representative of the school-aged population of children in the region studied. Families were recruited from 11 counties with an oversample of children from the Eastern Band of Cherokee Indians.[9] In the original sample, 25% of the children were American Indians living on the Eastern Cherokee Reservation or in the rest of the 11 counties. Children and parents have been followed over time, with attrition and non-response rates being statistically the same across ethnic and income groups as well as across the exogenous variation we leverage in this study. The survey is still ongoing and follows the original subjects, with the latest survey wave completed in 2015.[10]

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. We include descriptive summary statistics for the GSMS sample in Table A.1 in the Online Appendix.

After the fourth wave of the survey in 1996, a casino opened on the Eastern Cherokee reser-

[8]For the counties covered in the GSMS survey, see Appendix Figure A1 in the Online Appendix.

[9]For more details on the sampling framework, see Costello et al. 1996 and Costello et al. 1997

[10]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 an overview of the survey wave structure, see Figure A2 in the Online Appendix.

vation.[11] Upon the casino's opening, all adult enrolled tribal members—regardless of whether they were living on the reservation or not—were eligible to receive bi-annual cash transfers from casino revenues. These unconditional cash transfers were sizable and gradually increased during the first years of casino operation. Comparing the estimated change in household income to the average incomes in the affected group before the casino opened reveals an increase in income of about 20-25%. (We will discuss this in more depth in the Methods section below.) Based on the experiences of other tribes and the regulatory framework, tribal members had strong reasons to believe that the transfers would continue in perpetuity and their size would depend in part on the success of the casino.

To explore the effect of casino transfers on voter turnout, in early 2016 (not long after the most recent survey-based follow up with GSMS participants) we matched GSMS participants to public use voter files. This match was possible, in part, because the GSMS data has been actively maintained over time, being continuously updated to incorporate new information on subjects who have changed their names, moved, died, or gotten married. This approach involved scraping voter registration and voter history information off publicly available statewide voter portals.[12] To do so, we followed common best practice and matched parents and children based on their name (first and last), date of birth, and, in some instances, their current location. We looked for subjects in North Carolina voting records and, for those who had moved, in the state of their current address (overall, only a small minority had moved out of state: with about 80% of participants remaining in state even 20 years later). This matching technique mirrors that

[11]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).

[12]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.

used in matching other survey data (e.g. Pew, CCES, ANES), academic work (Ansolabehere and Hersh 2012), and social interventions to voter records (Sondheimer and Green 2010; Holbein 2017). When all of these matching inputs are available, duplicate matches and matching errors are very rare. More details about this match to voting records and checks of the quality of the subsequent matched data can be found in the Online Appendix.

4 Methods

In order to test the effect of exogenously increased household incomes on parents and children, we employ different estimation strategies for the two populations. These methods follow the exact approach taken in previous studies that have used the casino intervention to look at the causal effect of unearned income transfers, which have done extensive work to document the exogenous assignment of individuals to the additional income or no additional income conditions (see Akee et al. 2010, 2013, 2018; Costello et al. 1996, 1997).

First, for the parents, because they are eligible to vote prior to the income intervention, we conduct a standard difference-in-difference analysis where we are able to use the same household's voting record over time. We do this by coding the household as "voted" if either parent voted in the election.[13] The comparison in the case of parents is across the same household before and after the transfers began, and across AIs (eligible) and non-AI households (not eligible). Since the treatment is on the household level, we run the analysis at that level as well. We consider a household to be American Indian if at least one parent is a member of the Eastern Cherokee tribe.

For the children, there is no "before" period as they are not eligible to vote as minors. Thus, we conduct a difference-in-difference analysis where we compare the voting records across cohorts of children who are exposed to the income intervention for different amounts of time. Younger children live in households with exogenously increased households for a longer time period than their older counterparts because the income intervention started at a single point in time in this community.

The parents in our analysis were eligible to vote prior to the income transfers and can

[13]We have done the analysis taking the mean across the two parents and the results are the same.

be found in the public-use voter file in the pre-transfer election years 1992 and 1994. We are thus able to use a standard difference-in-difference analysis for the parents as we have "before" observations and "after" observations for the same individual that can be aggregated on the household-election level, as well as a well-specified set of treatment (AI) and control(non-AI) household groups.

Equation 1 formalizes this model—with γ_1 being the coefficient of interest. In this case, the treatment of interest is an indicator for household exposure to the casino transfer in the time period after the start of the casino intervention. We include a control for American Indian household status and a binary variable for whether the observation is drawn from the time period after the intervention. The variable $AmericanIndian_{\text{i}} \times AfterCasino_{\text{i}}$ is simply the interaction between those two binary indicator variables. We also include an individual household fixed-effect α_i since we observe the same household over multiple periods in our strongly-balanced panel; note that this implies that we will not be able to separately identify the level effect of American Indian household in the regression equation as it will be captured in the individual household fixed-effect. Finally, we include election year fixed-effects to account for potentially different average voter turnout for Presidential versus Congressional-only elections θ_t and an error term. Our estimation equation is given below:

$$\begin{aligned} Y_{it} = \alpha_i + \gamma_1 AmericanIndian_i \times AfterCasino_t + \delta_1 AmericanIndian_i \\ + \lambda_1 AfterCasino_t + \theta_t + \epsilon_{it} \end{aligned} \tag{1}$$

Identification in Equation 1 is based on the assumption that the parallel trends assumption holds. We show pre-treatment trends based on the parents' voting records across age cohorts by race and year in Figure 3 below. In this figure we interact the treatment variable (eligible for the cash transfer) with a year dummy variable and plot the estimated coefficient in the figure. Voting data for the years 1992 and 1994 serve as the pre-treatment observations. We find no statistical or substantial differences between the two groups. This standard check provides supportive evidence of the internal validity of our quasi-experimental design—among other things, it provides evidence that before the income transfers began the treated and untreated groups voted at indistinguishable rates. (It is not possible to test a similar pre-trends analysis for the children since none of them were eligible to vote in the pre-treatment time period.)

Our empirical analysis as regards children from treated households is designed to compare outcomes across age cohorts and race; this design is necessary since there are no pre-treatment observations for the children as none of them were eligible to vote prior to the casino transfers. For the GSMS children, we run a difference-in-difference specification that leverages two differences—the first difference is between American Indian (eligible for the transfer) and non-American Indian children (not eligible) and the second difference is across age cohorts of AI children who were exposed to the income transfers at different points in the life course.

This approach leverages the fact that children from different cohorts were of different ages when their parents first started receiving the transfers. Specifically, the transfers for the younger cohorts started when they were 13 (cohort 1) or 15 (cohort 2). Compared to individuals who were in cohort 3 (17 at the time of first receipt), thus, individuals from cohorts 1 and 2 were exposed at younger ages and for a longer time period to the income intervention while minors in their parents' households and this may have had a differential effect on their voting attitudes, skills, and identities discussed earlier. Our hypothesis, consistent with Ojeda's model (2018) and with the human capital model from economics, is that income transfers will have larger effects on the younger children in the survey. (We expand further upon the mechanisms that might drive timing-based effects in the Online Appendix.)

Equation 2 formalizes the difference-in-difference model that we estimate using data on the children in the GSMS sample:

$$Y_i = \alpha + \beta_1 YoungestCohort_i + \beta_2 MiddleCohort_i + \delta_1 AmericanIndian_i + \gamma_1 YoungestCohort_i \times AmericanIndian_i + \gamma_2 MiddleCohort \times AmericanIndian_i + X'\theta + \epsilon_i \quad (2)$$

Following previous practice (Holbein 2017; Sondheimer and Green 2010), in Equation 2, we specify the outcome variable in several ways—first, as a binary variable indicating whether an individual has ever voted in a Federal or State election (in adulthood over the period 2002-2014) and second, as a continuous variable measuring the proportion of eligible Federal elections that a person voted in. We also examine the effects on specific elections in supplementary models. In Equation 2, $YoungestCohort_{\text{i}}$ is an indicator variable for the child belonging to the youngest cohort (age 9 at intake, age 13 at first cash transfer), $MiddleCohort_{\text{i}}$ is an indicator that the child belongs to the second youngest cohort (age 11 at intake, age 15 at first cash transfer).

The omitted group is the third (oldest) cohort, so all coefficients are interpretable as differences from that cohort. The variable $AmericanIndian_{\mathrm{i}}$ is a dummy equal to one for American Indians and zero otherwise. We start out with a very parsimonious specification that does not include any additional controls. The vector X is a set of baseline covariates that include the parents' voter turnout rate before the casino opened, child age, number of children below age six in the household and gender. The identification relies on differences between the three cohorts and across American Indian race. The coefficients β_1 and β_2 identify differences in the propensity to vote between the youngest two cohorts and the oldest cohort. The coefficients of interest are γ_1 and γ_2, which capture the difference-in-difference results. It is important to note that as specified these coefficients may potentially capture differences in voting propensities arising from the differences at the age of first transfer treatment or differences in the duration of treatment before reaching maturity. We offer further discussion and some additional analysis aimed at disentangling these two potential channels below.

There are several reasons to expect heterogeneities in the estimated effects by income levels in our analyses. First, there is the high correlation of voting and household incomes and the observational pattern of diminishing returns displayed in Figure 1. Second, existing theory predicts additional income will have diminishing returns (Frey 1971; Wolfinger and Rosentstone 1980; Leighley and Nagler 2013; Verba and Nie 1987; Veba, Schlozman, and Brady 1995; Ojeda 2018). Therefore, we specifically examine whether there are heterogeneities in the effect of the unconditional cash transfers across the initial household income distribution for both parents and children.

We interact household income before the transfers with the transfer treatment dummy variable and show in a regression framework that the additional income is positively associated with a higher propensity to vote but at a diminishing rate with respect to initial (pre-trasfer) income for the children, but not for the parents. We follow up with additional analysis aimed to identify possibly heterogeneous effects of the additional income across the initial income distribution. We find evidence of such heterogeneities across children's subsequent propensity to vote as adults roughly below and above the median of initial income, as measured before the transfers began (about $27,000).

To ease interpretation, we conduct the analysis separately on the subsamples of individuals below and above the median in the initial household distribution. We then supplement all our empirical analyses with separate estimations for these two sub-groups throughout the rest of the analysis. This allows to us to explore not only whether income transfers help to raise overall turnout, but also the complementary (perhaps more normatively interesting) question of whether positive income changes help narrow income-based participatory gaps in political participation.

To further check for pre-treatment differences across the two groups, Appendix Table A.4 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.[14]

[14]Appendix Table A.5 also provides a comparison of characteristics of the GSMS American Indian population to that of other American Indian populations and rural African American groups; we show that there is similarity across these groups in several important categories. Appendix Table A.6 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.

5 Results

5.1 Casino Transfers and Household Income

Here we demonstrate that the casino transfers increased household income for American Indian families substantially.[15] In Table A.2 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 2. The coefficients plotted in Figure 2 are based on the following triple difference equation

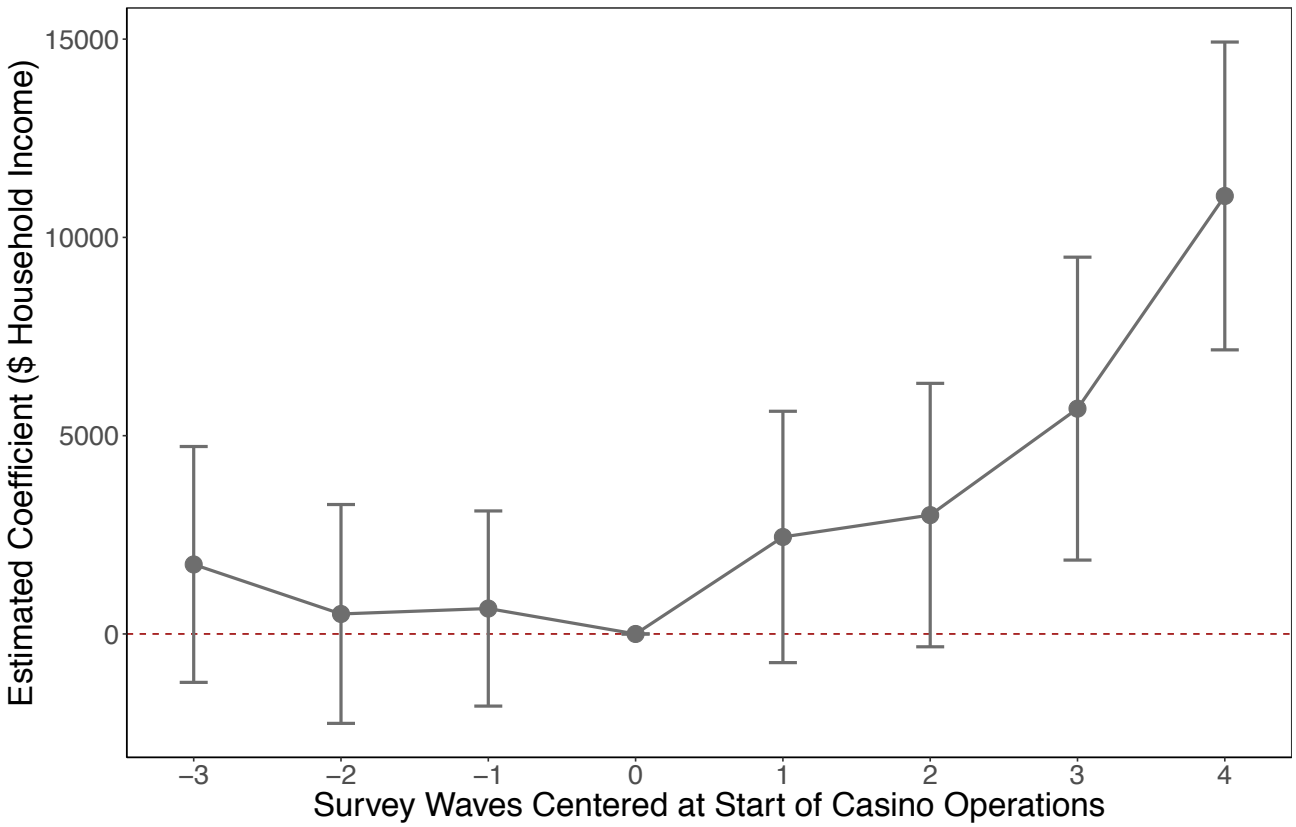
$$\begin{aligned} Y_{it} = \alpha + \beta_1 YoungestCohorts_i + \beta_2 AfterCasino_t + \beta_3 AmericanIndian_i \\ + \gamma_1 YoungestCohorts_i \times AfterCasino_t + \\ \gamma_2 YoungestCohorts \times AmericanIndan_i + \\ \sum_t^T \lambda_t \times YoungestCohorts_i \times AmericanIndian_i \times Year_t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

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 again reassuring of the validity of our research design—and a large and statistically significant increase in household incomes for American Indian households subsequent to the transfer initiation.[16]

[15] 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.

[16] 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,

Figure 2: 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 American Indian 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).

5.2 Parents' Voting Outcomes

We first turn our attention to the effects of the casino transfer on parents' voting rates. In Table 1 we report estimates from Equation 1. As mentioned earlier, we compare the voting behavior of parents before and after the casino opened for the same household as we have voter participation data starting in 1992. Given this additional flexibility, we examine the impact of the exogenous change in household income among parents in two ways. First, we compare the voting probabilities of treated and untreated households in a simple difference-in-difference setting. Second, we test for potentially heterogeneous effects of the extra income across the initial income distribution.

Table 1 provides the results from the difference-in-difference specification that leverages pre- and post-casino difference by transfer eligibility status for the parents. Here the coefficient of interest is the interaction coefficient between American Indian 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: our 95% confidence intervals allow us to confidently rule out effects as large or small as 2.5 percentage points (a small effect based on the voting literature, and also relative to the mean in the GSMS parents' sample). 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

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.

found in column (1) are thus unlikely to be masking differences in treatment effects across the initial socio-economic distribution.

To investigate even further, we test for different effects of the unearned income transfers across the four initial income quartiles. Appendix Figure A3 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 1 to confirm the findings in Appendix Figure A3. There are no large or statistically significant effects on parental voting probabilities in either subsample.

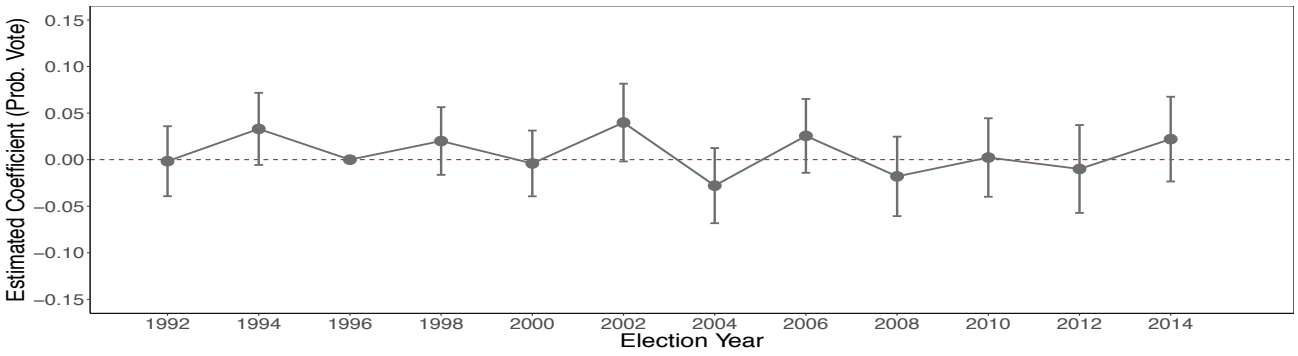
Figure 3 provides the event analysis for parents for the pooled sample and separately by initial household income above and below the median. Note that the first two years (1992 and 1994) serve as a test of the parallel trends assumption. We plot the coefficient on the AI household dummy for all elections starting in 1992, which is before the income transfers began in 1996 (Appendix Table A.7 provides the regression results used to construct this figure.) The figure confirms the findings in Table 1 for the parents voting probabilities and also shows that there were no significant differences in the trends in voting probabilities before the transfers started. Regardless of how we specify the model, unconditional cash transfers have no effect on parents' voting.

These precisely-estimated null effects are vitally important in their own right. They suggest that resource-based models of voter turnout may be oversimplified. Resources (in this case income) when distributed in adulthood do not uniformly increase adult turnout. Our results are consistent with a more nuanced conceptual model of voting in which individual propensity to participate in civic society is set by the adult years. An important implication is that efforts in adulthood to change woefully low, unequal, and (by some accounts) declining rates of civic participation may not be likely to succeed *even if* these interventions fundamentally change the living conditions of treated adults (as is the case in our large income intervention).[17]

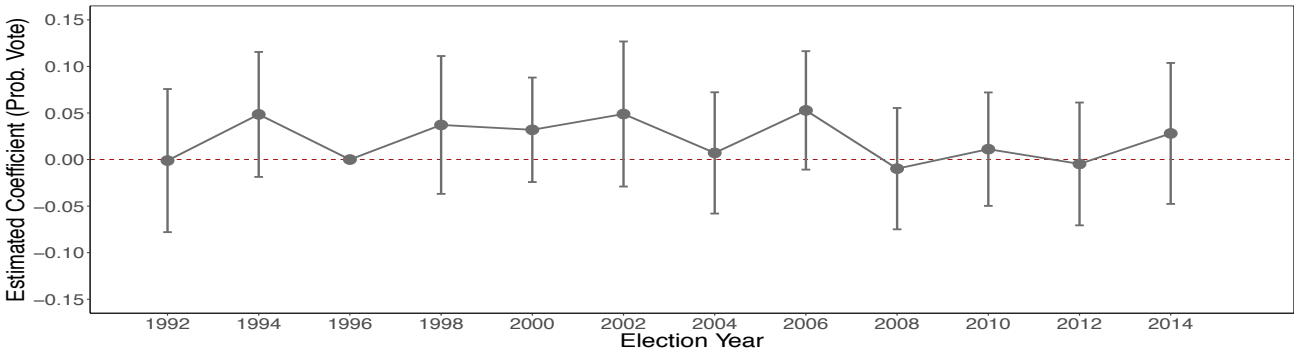
[17]This conclusion is corroborated by the overwhelming small effects of adult-targeted get-out-vote campaigns; see Green, McGrath, and Aronow 2013).

Figure 3: Effect of Casino Transfers on Parental Voting by Initial Household Income Status around the Start of Casino Operations

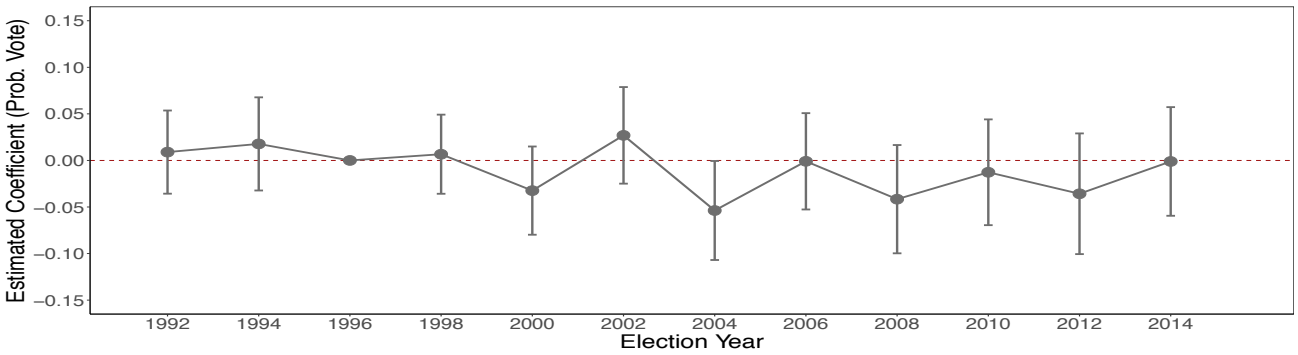
Pooled Parents Estimates



Parents Above Median Income at Baseline



Parents Below 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. Top panel N = 15,984 (1,332 GSMS individuals); Middle panel N = 8,172 (681 GSMS individuals); Bottom panel N = 7,812 (651 GSMS individuals).

Table 1: 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		0.00637		
x Initial HH Income		(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.

5.3 Children's Voting Outcomes

Table 2 shows our results which estimate the effect of casino transfers on the voter turnout of children using Equation 2.[18] 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 A.8 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 2.

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

[18]Our final analysis sample is around 1,300 individuals due to missing baseline characteristics. The results do not change if we do not use these baseline characteristics and increase the number of observations.

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 (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.[19]

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 1) 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 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 American Indian 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

[19]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.

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 2. First, in Appendix Figure A4 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 A.9 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 A4 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 2 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 1 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.

Figure 4 provides a graphical depiction of the differences in the probability of voting across all elections. Here we combine cohorts 1 and 2 for readability, so the plotted coefficients represent the average effect of the transfers on the voting propensities of these two cohorts relative to the oldest cohort. The estimation setup is exactly the same as in equation [3], except for the combined cohorts 1 and 2. In the top panel, we plot the estimated coefficients for observations below the baseline median household income. The effect of the casino transfers on the younger cohorts is positive, substantively large, and (in virtually all elections) statistically significant. In the bottom panel, we provide the same analysis for individuals from above the baseline median household income level. The effect among this group of children is smaller and not statistically significant.[20]

These results are remarkably robust to various alternative specifications. (We conduct a full series of robustness checks in Appendix D.) In Appendix Table A.11 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 2. Our results largely mirror the results found in Table 2. 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 A.2). If we interpret the evidence in Figure 1 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 1 is based on parental voting, while the effects on children may be different, depending on the mechanisms at play.

These results are consistent with the predictions based on a human capital perspective of voting and with the implications of Ojeda's two-gaps conceptual model (Ojeda, 2018). They

[20] Appendix Table A.10 provides the regression coefficients for Figure 4. Here we pool across all potential election years. Appendix Table A.11 also provides the analysis in a simple difference-in-difference setup.

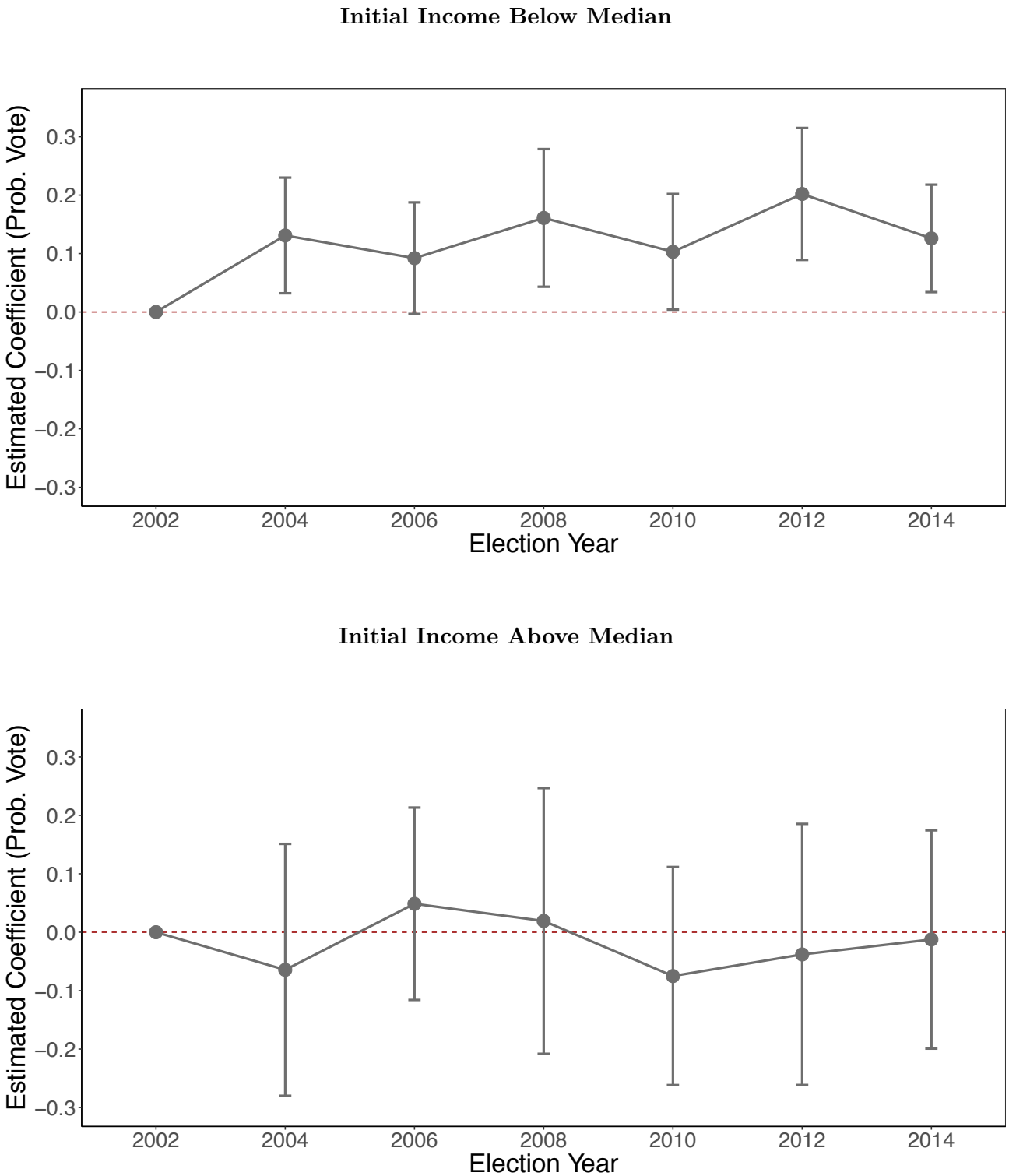
Table 2: The Effect of Casino Transfer on Children's Voter Turnout (Years 2002-2014)

Panel A: Pooled and	Pooled		Pooled	
Initial HH Income	(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***
$\times AmericanIndian$	(0.0741)	(0.0401)	(0.128)	(0.0698)
Interaction 2: Age Cohort 2	0.0743	0.0451	0.321**	0.228***
$\times AmericanIndian$	(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	Below Median HH Income		Above Median HH Income	
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
$\times AmericanIndian$	(0.0817)	(0.0409)	(0.142)	(0.0857)
Interaction 2: Age Cohort 2	0.231***	0.124***	-0.0382	-0.0219
$\times AmericanIndian$	(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, American Indian 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.

Figure 4: Effect of Casino Transfers on Child Voting by Initial Household Income Status



Notes: Figure displays coefficients from event analysis model for children's voter turnout in the 2004-2014 elections. The estimates are split by median family income levels at baseline. To make visualization easier, cohorts 1 and 2 are collapsed together and compared to cohort 3. Standard errors are clustered at the individual level. Top panel N = 4,557 (651 GSMS individuals); Bottom panel N = 4,767 (681 GSMS individuals)

suggest that income transfers in relatively early life narrow participatory gaps considerably. For young people who are in their formative years and who have yet to finish high school, household incomes matter a great deal in determining whether they become active voters or fail to do so—with these effects concentrated among those who have the lowest initial household incomes. This suggests that voting rates are not pre-determined due to household characteristics or rigidly transferred from one generation to the next. Elevating families out of poverty has potentially sizable effects on children's levels of civic participation.

Because the younger cohorts were treated at younger ages, but were also treated for a longer period of time while in their parent's households (we assume at least up to age 18), we cannot categorically say whether the effects of the additional income are stronger in those cohorts because of the former or the latter differences. One fact that should be emphasized is that we only consider elections starting in 2002, in which all cohorts were eligible to vote. Because the income transfers started in 1996, the total duration of exposure to the transfers is identical across cohorts at every election. The age at which they were first treated is not. This lends some credibility to the hypothesis that the income effects are different across cohorts due to the differences in initial age of treatment.

We provide suggestive tests for potential mechanisms in the Appendix. Our results suggest that this effect is unlikely to be driven by the transmission of voting from parents to children, changes in mother's education and employment, or movement to new residential locations out of state. We provide some suggestive evidence that the results may, indeed, be driven by previously theorized channels—educational attainment and the dynamics of social capital/social skill acquisition over the life course. These channels help to explain why the timing of the income intervention mattered so much for children from the youngest cohort at receipt, whose pre-voting attitudes and skills were more malleable to targeted intervention than their elder counterparts.

6 Conclusion

Decades of social science research has established that there is an income gradient in voter turnout and that such patterns may have distortionary effects on representative democracy. In

this paper we provide some of the first causal evidence exploring whether pure income shocks increase turnout and narrows participatory gaps. The results suggest that increasing household income does, indeed, have a substantial impact on participatory inequality, but only if it is received at early ages. Cash transfers help disadvantaged children catch up with their more advantaged peers. However, the unconditional cash transfers have little to no effect on parents' generation nor on the children raised in initially better-off households. The null effect on parents is notable because it implies that remedial programs that improve financial standing in adulthood would not affect voter turnout.

Our results contribute to a broader framework for understanding what drives people to participate in politics. The Resource Model of Voting predicts that resources uniformly increase participation, but our results suggest that a model of voting formation that is more closely aligned with existing models of human capital formation are more accurate. Regardless of the exact terminology one uses for this updated model, our results clearly suggest that the resource model needs to take into account developmental dynamics—that is, that resource models should go hand in hand with socialization models. Consistent with the implications of such a framework, our results show that additional resources' impact on civic participation depends on the life course. This makes a substantial contribution to our understanding of political socialization. At present, the political socialization literature has been woefully underdeveloped; with few examples of studies exploring the causal impact of childhood inputs on later life voting. Such a gap is unfortunate given the abundance of evidence that voting patterns (and the inputs that predate them) harden long before many analyses even start. Our work helps to begin to fill that gap.

Our further analyses suggest that the theorized key mechanism of income eliciting a direct intergenerational transmission of voting behavior from parent to child may be overstated. Our results are, instead, consistent with a human capital framework of voting: wherein income enhances skill development that, in turn, promotes later-life voting.

From a practical perspective, our results suggest that unconditional cash transfer programs may have broader effects than previously realized. Not only do they affect individuals labor, health, and schooling outcomes, but may also influence citizens' levels of civic engagement

or social capital. This finding has special meaning given the dismal returns of many interventions designed to increase voter turnout (Gerber and Green 2008, 2015; Bedola and Michelson 2012; Green, McGrath, and Aronow 2013; Enos, Fowler, and Vavreck 2013). Our results—combined with this previous research—suggest that there may be some merit to the argument that in order to get meaningful increases in voter turnout, larger-scale programmatic interventions might be required. Such an approach is more expensive, but is of critical import. Inasmuch as voting participation determines representation and policy, additional resources may in this way affect the perpetuation of the inter-generational transmission of economic inequality.

Many efforts have been made to increase voter participation among low SES families. They usually provide citizens with various information or social nudges. Sadly, most of these interventions have negligible effects on disadvantaged populations or have even backfired and made participatory gaps worse. Our results suggest that social policies aimed at reducing economic disadvantage may have the additional benefit of increasing voter turnout in the next generation. Without these early life human capital enhancements, voting differences persist across generations and likely contribute to intergenerational patterns of social inequality.

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