Family Income and the Intergenerational Transmission of Voting Behavior: Evidence from an Income Intervention

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Abstract

Despite clear evidence of an income gradient in political participation, research has not been able to isolate the effects of income on voting from other household characteristics. Does higher income exacerbate participatory inequality and do these effects span across multiple generations? We investigate how exogenous unconditional cash transfers affected voting in US elections across two generations from the same household. We show that household receipt of unconditional cash transfers increases children's voting participation in adulthood among children from initially poorer families. The income transfers have no effect on adult-aged recipients, regardless of initial income levels. However, income transfers have no effect on adult-aged recipients (the household parents). These results suggest that family circumstance during childhood—income levels in particular—play a key role in influencing levels of political participation in the United States. Further, in the absence of outside shocks, income differences are transmitted across generations and likely contribute to the intergenerational transmission of social and political inequality.

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1 Introduction

Voting is the foundational act of democracy. Philosophers, theorists, and other important public figures have long argued that in order for democracies to survive, citizens need to be actively engaged in the political process (Green and Gerber 2008; Leighley and Nagler 2013; Verba, Schlozman, and Brady 1995; Verba and Nie 1987; Wolfinger and Rosenstone 1980). Greater levels of citizen participation allow for a better aggregation of citizen interest, enhance social connectivity, and help achieve the foundational values underlying democracy. Despite this fact, in many contemporary democracies voter turnout is perpetually low and vastly unequal. In particular, in the United States, levels of turnout hover around 60 percent in presidential elections, 40 percent in midterm elections, and much lower still in local elections (putting the U.S. in the bottom third of worldwide voter turnout).

Inequality in voter turnout is ubiquitous and perhaps even more troubling. Comparing those who vote to those who dont reveals a particularly large inequality in citizen participation; simply put, people who are more affluent are much more likely to participate in politics than those who are less affluent (Blais 2006; Frey 1971; Leighley and Nagler 2013; Smets and van Ham 2013; Verba, Schlozman, and Brady 1995; Verba and Nie 1987). This form of social inequality is troubling on a number of levels. In practical terms, this pattern appears to have distortionary downstream effects on representative government—reinforcing patterns that bias public policy towards the wealthy (Schlozman, Verba, Brady 2012; Griffin and Newman 2005; Gilens 2012; Bartels 2009). The most compelling empirical research on this topic tends to show that who participates affects who gets elected and the policies they implement (Anzia 2013; Berry et al. 2011; Bertocchi et al. 2017; Fowler 2013; Griffin and Newman 2005; Lee et al. 2004; Leighley and Nagler 2013; Madestam et al. 2013; Verba and Nie 1972). Brunner, Ross and Washington (2013) have shown that there is evidence that politicians vote in line with their constituents about 75% of the time; however, this means that there is a "politically disadvantaged" group (those who are represented by a politician of an opposing party). This political disadvantage may be especially acute among low-income communities with low voter turnout.

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 reflect some other unobserved social or contextual factor.[1] As a result, there is little understanding of how to address this form of social inequality. We do not know whether providing disadvantaged citizens with greater levels of income would actually increase overall levels of civic engagement and narrow gaps in these types of civic behaviors. 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

[1] Brunner, Ross and Washington (2011) have shown that exogenous increases in income due to exogenous labor demand shocks tends 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.

whether children's propensities to vote are affected by the family environment, by family income, or both.

In this paper, we explore whether income has an effect on civic participation 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. Specifically, we examine data from the Great Smoky Mountains Study (GSMS), a longitudinal study of children's in rural western North Carolina, which began in 1993 and consisted of both American Indian 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. Non-Indian households surrounding the casino were not eligible for these cash disbursements. 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 exogenously increasing household incomes on the political participation of parents (who were adults when they began receiving it) and their children's voting behavior as adults (who were adolescents when their parents began receiving transfers). These identification strategies build on research using the GSMS data, which show that these casino transfers are, indeed, exogenously disbursed (Akee et al. 2010, Akee et al. 2013, Akee et al. 2018; Copeland et al. 2011; Costello et al. 2010; Foley et al. 2006).

Matching GSMS parents and children to public-use voter files based on their identifying information, we find that income has long-term civic benefits; however, these gains are not uniformly distributed across all recipients.[2] Income transfers increase the turnout levels of children in the initially poorer families substantially: closing 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 this groups voter turnout by about 8-20 percentage points, depending on the age of the recipients and the type of measures of voting one considers.

We also find, however, that unconditional income transfers have precisely estimated null effects on parents, regardless of their starting income levels. This result suggests that adults voting patterns may be locked-in, being non-responsive to later-life transfers: perhaps as a result of political socialization and voting habituation (Fujiwara, Meng, and Vogl 2016; Gerber, Green, and Shachar 2003; Coppock and Green 2015; Meredith 2009; Plutzer 2002; Holbein 2017; Mullainathan and Washington 2009).

We explore a host of potential mechanisms for this effect, concluding that it is unlikely to be driven by the transmission of voting from parents to children, changes in mother's education and employment, movement to new locations out of state, or children's educational attainment

[2] The information included is name, date of birth, and location, the standard inputs to match individuals to voter files, see Ansolabehere and Hersh 2012, 2016.

at age 30. We provide some suggestive evidence that the results may be driven by the dynamics of social capital and social skill acquisition over the life course. Regardless of the exact mechanisms, however, these effects on childhood recipients serve to narrow the turnout gap between advantaged and disadvantaged children.

Our work makes several important contributions. Conceptually, our study helps answer the vital question of whether income contributes to underlying levels of voter participation. In so doing, it adds nuance to the foundational resource model of voting (RMV) developed by Verba, Schlozman, and Brady (1995). Rather than income mattering universally (as the RMV might predict) incomes effect appears to be moderated by other important factors. This suggests a more nuanced model for voting, consistent with what we term a human capital model for voting (HCMV). Consistent with the predictions of the HCMV, our results show that resources like household income matter for those with the lowest levels of baseline resources. Further, resources appear to be more beneficial for children than for adults.

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.[3,4] The HCMV that we put forth helps draw attention back on the life course dynamics associated with political participation. 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 the same family experienced later in life. This implies that voting propensities are not etched in stone at birth, like a heritable trait, but, instead, can be shaped

[3] We are not the first to identify this gap in scholarly research. Some have lamented the "abandonment" of studies exploring the role of childhood experience for voting (Sapiro 2004, 1). Others have readily acknowledged that political behavior studies in recent years have "eschew[ed] ... young children" and have instead "focus[ed] on the political learning years [of early adulthood]" (Niemi and Hepburn 1995, 7), justifying this focus by arguing that "the degree of activity or involvement in politics ... seem[s] to be best explained in terms of [adult] experiences" (Verba and Almond 1987, 267-268).

[4] Plutzer (2002, 41) argues that from the resource model we are left without "a good sense of ... when [in the life course] ... variables will matter most." While the habitual model of voting presented by Plutzer (2002) allows for resources to vary in salience over time, we argue that this model starts too late in the life course—only starting the examination of political development "when a cohort of young citizens becomes eligible to vote for the first time." Our argument is that experiences that far predate the voting experience play an under-appreciated role.

by well-targeted investments early in the life course.

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). The studies that have been done 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). We have little sense of patterns of validated voting among this indigenous population, much less how to increase their levels of participation. Our work is a step forward in closing that gap in the literature.

Finally, our results have implications for both policy and practice. Discussions about the merits of various income distribution schemes are at the heart of a multitude of policy reforms: from debates over progressive taxation, 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. Not only may these transfers affect individuals labor, health, and schooling outcomes (e.g. Akee et al. 2010 and 2013; Aguero, Carter, and Woolard 2006; Baird et al. 2012; Baird, De Hoop, and zler 2013; Blattman, Fiala, and Martinez 2013; Cunha 2010; De Mel, McKenzie, and Woodruff 2012; Paxson and Shady 2010; Haushofer and Shapiro 2016), but they may also affect levels of civic engagement or social capital. Inasmuch as civic participation plays a vital role in preserving democratic values and institutions, connects individuals in communities to one another, and promotes democratic accountability, such a finding is vitally important.

Our results suggest that income plays a role in narrowing stubborn participation gaps. From a practical perspective, millions of dollars are expended each election cycle by political campaigns and nonpartisan entities to increase turnout (Gerber and Green 2008, 2015; Bedola and Michelson 2012). However, meta-analyses show that many of these get-out-the-vote (GOTV) efforts fail to have noticeable effects, with some even making participatory gaps worse (Green, McGrath, and Aronow 2013; Enos, Fowler, and Vavreck 2013). Our results suggest that unconditional income transfers can reverse this phenomenon.

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 (Downs 1957; Riker and Ordeshook 1968; Campbell et al. 1960; Fiorina 1976; Rosenstone and Hansen 1993; Gerber, Green, and Larimer 2008; Washington 2006).

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, such as 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 (Cascio and Washington 2013; Corvalan and Cox 2013; Leighley and Nagler 2013; Wolfinger and Rosenstone 1980; Holbein and Hillygus 2016;Brady and McNulty 2011; Pettigrew 2016; Fujiwara, Meng, and Vogl 2016; Gomez, Hansford, and Krause 2007; Lassen 2005; Fujiwara 2015). 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). Resources theorized to be important for voting include education, health, information, skills, time, and income (Sondheimer and Green 2010; Burden et al. 2017; Adena et al. 2015; DellaVigna and Kaplan 2007; Enikolopov, Petrova, and Zhuravskaya 2011; Gentzkow 2006; Holbein 2016; Lassen 2005; Kendall, Nannicini, and Trebbi 2014; Martin and Yurukoglu 2017; Holbein 2017; Holbein and Schafer 2017). 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. 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). 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.

Some have argued that income increases individual investments in education, skills, and health that make it easier for one to participate in politics.[5] These skills 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), the so-called non-cognitive abilities

[5] 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."

that help citizens follow-through on their intention to participate in politics (Hillygus, Holbein, and Snell 2016; Holbein 2017), and the personality traits thought to be important for voting (Akee et al. 2018; Mondak 2010; Gerber et al. 2011).

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 ones neighborhood and, perhaps, avocational companions and thus exposure to a variety of norms and pressures." Income enhance 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.

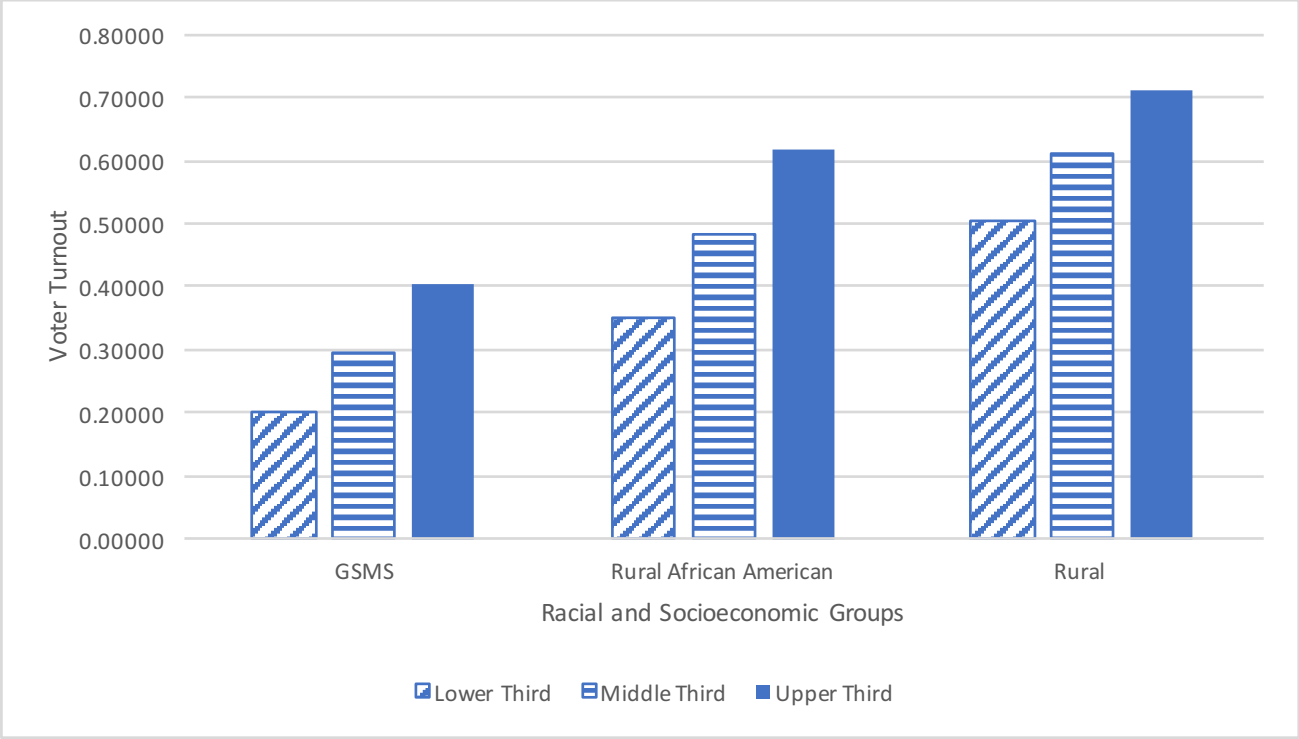
Importantly, income may exhibit diminishing returns, that is, income may only matter up to a point (Wolfinger and Rosentstone (1980, 21); Leighley and Nagler 2013; Verba and Nie 1987; Veba, Schlozman, and Brady 1995). For those who are poor, income may matter a great deal for voting; for those who are well-off already, additional income may matter very little. While this prediction has some face validity and some observational empirical support, this theoretical prediction has yet to be fully tested.

2.2 Empirical Evidence Linking Income and Political Participation

At first glance, empirics support the theoretical prediction that income increases voter turnout. Virtually all data sources that have measures of income and voting indicate that there is a positive relationship between the two. This is validated in the data we use as well. Figure 1 shows how the propensity to vote changes with income using data from the American National Election Study (ANES) and the Great Smoky Mountains Study of Youth (GSMS), which is the survey instrument we use in this study.[6] In the figure, respondents are separated into three income bins—lowest, middle, and highest. For the ANES, these bins are based on how respondents self-identify in the income distribution, not on any actual income amounts. For the GSMS, the income bins are based on the actual income amounts reported by the respondents. The first set of columns provides the parental voting probability for the GSMS data from the baseline period across the three income bins. The second and third sets of columns provide a similar set of data for rural African Americans and rural Americans respectively using data from the ANES. We use these two groups as benchmarks to give context to our GSMS sample because their financial and social circumstance are the closest to the GSMS population in nationally representative

[6] The American National Election Study (ANES) is one of longest running and most respected nationally representative surveys of voters in the United States. It has been conducted around Federal Elections ever two years since 1948. For more information on the ANES sampling framework and measures, see www.electionstudies.org.

Figure 1: Income Gradient for Voting Average for Different Groups and Income Terciles



Notes: The data for the GSMS is restricted only to the subject parents for the years 1992-1996 (before the casino transfers began). Data for the other two groups are drawn from the American National Election Study (ANES) Time Series Cumulative Data File (1948-2012) In both sources, income measured using a question where individuals are asked to label where they fall in the income distribution. In the ANES, voter turnout measured using self-reports of voting in national elections in the given year (available in all years excepting 2002). The same pattern holds with validated voting in the limited years that it is available.

data that report actual income levels, such as the Census.[7] As Figure 1 demonstrates, there is a positive relationship between income and voting probabilities in all three groups. Within the GSMS sample, the gap between the top and bottom of the income distribution is 20.1 percentage points; in the other two similar ANES subsamples the gap is similar, at 20.6 percentage points (for all rural) and 26.8 percentage points for rural African Americans. We note that average participation in voting across income levels differs slightly across these groups, which may in part be due to the differences in how the income bins are formed. However, the relationship across income bins holds within each subsample and is of a similar magnitude.

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

[7] If we look across the entire population in the ANES, the income gradient is 22.3 percentage points from the bottom tercile to the top tercile ($p < 0.001$).

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).[8]

Overall, the 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 Hams (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 an 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 (Imai, King, and Rivera, 2017).

More generally, CCT programs face two fundamental difficulties in using their design to examine the pure effects of income. First, CCT 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 see 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. Second, 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 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 (Sondheimer and Green 2010; Burden et al. 2017). Overall, with CCTs it is unclear whether income is indeed the driving force in any income gains; simply put, the unique components of CCT programs contaminate this instrument from eliciting the pure downstream effects of income.[9]

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

[8] 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.

[9] 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.

on voting. Using an innovative approach that leverages data from the annual Spanish Lottery, Bagues and Esteve-Volart (Forthcoming) 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 innovative 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. Moreover, 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; Feigenbaum and Hall 2015; Healy and Malhotra 2013; Healy and Lenz 2014; Healy and Lenz 2017; Lewis-Beck and Stegmaier 2007). Hence, while the income effects 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. An important difference between this research and their paper is that the unconditional income transfers we study are disbursed by the tribal government, while the voter participation is measured in federal and local elections. The tribal elections, which arguably may have been impacted by the cash transfers, are held in different years and different months from the elections we analyze. Finally, Bagues and Esteve-Volart (Forthcoming) do not explore potentially important dynamics in incomes effect on turnout—including across socioeconomic status and the life course.

2.3 The Human Capital Model of Voting

The RMV predicts that resources, like income, matter regardless of when they are acquired in the life course. This approach stands in contrast to a host of work on how childhood investments and experiences affect adult behaviors, including an entire body of research on critical periods in childhood development (Currie, 2011; Becker and Tomes 1986; Chetty et al. 2011; Currie and Thomas 1995; Heckman references REFS). While it is common to see models of human capital acquisition applied for education, labor, and health outcomes, we are not aware of any work that links human capital formation concepts to civic or social behavior like voting and participation in the political process. Here we briefly articulate what a human capital model of voting (HCMV) would imply about this fundamental form of democratic participation. (For a more thorough explication of the HCMV, see Appendix Section I.)

There are compelling reasons to suspect that early life resource investments may be more important for voting than later life investments. Income is likely to matter for voting because it encourages investments in skills that affect civil participation and socializes people towards the norm of voting. If the norms, skills, and attitudes required to engage in the political process lock-

in at a certain point in the life course, then later investments may not affect voting behaviors. Indeed, according to what some have termed the "impressionable years hypothesis", young people's political behavior may be more malleable because they have yet to form a hardened set of attitudes and identities that govern that behavior (Krosnick and Alwin 1989; Sears and Funk 1999). Early adolescence may be especially critical, as young people are making decisions about their future (such as how long they should stay in school) that have clear implications for whether they will become active voters (Sondheimer and Green 2010).

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—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; as a result, resource investments designed to target these skills may have less of an effect on voter turnout than earlier investments (Holbein 2017). Consistent with this view, some research has shown that voting patterns tend to be persistent over time (Fujiwara, Meng, and Vogl 2016; Gerber, Green, and Shachar 2003; Coppock and Green 2015). However, the habitual model of voting only starts in adulthood—once individuals are eligible to vote—and only focuses on 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 of other adult behaviors, 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. Alternatively, resources may matter when the act of voting is closest—a view consistent with many get-out-the-vote interventions that bestow citizens with resources (such as information) when one is eligible to vote and elections are close.

In this paper we can empirically test how these two broader competing models of voting perform in actual data by exploring the impact of exogenous income transfers across two generations. The older generations received these investments in adulthood and the second generation received these in late childhood. Further, we present evidence that the timing of income increases during the formative years matters as well, by exploring the effects of income transfers across birth cohorts within eligible children.

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 unique longitudinal study of 1,420 children and their

parents that began in 1993—matched to administrative data records on voting.[10] The survey was originally designed as a means of studying the mental health and well-being of children; however, this sample has been used in a number of different contexts, including those studying other aspects of health, education, and personality, to name a few (Akee et al. 2010, Akee et al. 2013, Akee et al. 2018; Copeland et al. 2011; Costello et al. 2010; Foley et al. 2006).

At the beginning of the survey, the children 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.[11] 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 (Akee et al. 2018). The survey is still ongoing and follows the original subjects, with the latest survey wave completed in 2016.[12]

The GSMS contains information on a host of baseline characteristics for parents and children, including 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 1.

The first three variables presented in Table 1 show that the survey selection was balanced across the cohorts by American Indian and non-Indian status. The survey was also balanced along gender lines. There is a statistically significant difference in levels of average household incomes prior to the intervention; American Indian households earned incomes of approximately $23,000 while non-Indian 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-Indian mothers tend to have higher educational attainment (more than a high-school degree) than American Indian mothers prior to the start of the intervention. Mothers appear to work in similar proportions across the two groups. American Indian parents are less likely to vote as compared to non-American Indian parents over the entire time period by about thirty percentage points. Our identification strategy accounts for these initial differences in voting probabilities.

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

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

[12] 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.

Table 1: Table of Means for Outcomes at Initial Survey Wave

	American Indian		Non Indian		Test of Equality of Means		
Variable	Mean	Std. Dev.	Mean	Std. Dev.	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
Parent's Voting	0.216	0.412	0.492	0.500	-0.276	0.028	-9.697

Notes: Table shows sample summary statistics broken by American Indian, Non American-Indian. 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-Indians ranges between 1028-1041 except for Mother Employed Full Time which is 879. The number of observations for American Indians ranges between 292-297 except for Mother Employed Full Time which is 270.

After the fourth wave of the survey in 1996, a casino opened on the Eastern Cherokee reservation.[13] 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.)

3.1 Match of GSMS Participants to Voter Files

To explore the effect of casino transfers on voter turnout, we matched GSMS participants to public use voter files. This approach involved scraping voter registration and voter history information off publicly available statewide voter portals.[14] 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 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.

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. As a result of the quality of this dataset, the GSMS has been successfully matched to other public records before (for example, Akee et al. 2010 used a match to crime records). The GSMS benefits from having all of the matching inputs available for all children in the dataset. The availability of matching inputs did vary somewhat across parents, with some of these not having date of birth.[15] Fortunately, however, the number of matching inputs available was balanced across the treatment and the control samples.[16]

[13] 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 gaming compact agreement between the State of North Carolina and the Eastern Band of Cherokee Indians was signed in August 1994.

[14] 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 is consistent with other work in this area (e.g. Holbein 2017) and will not bias our results.

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

[16] Tests for balance across the number of matching inputs available across the child cohorts and casino eligibility (our identification strategy) are provided in Appendix Table 1. Our approach avoids many

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.[17] 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; β=0.76, p <0.001).[18] Following previous best practice, the participants who we could not locate in the voter records were coded as having not registered nor voted (Sondheimer and Green 2010; Holbein 2017; Ansolabehere and Hersh 2012).

Robustness checks provided in the Online Appendix reveal that match quality is similar across our identifying variation (Appendix Table 1). We find little evidence that those exposed to the casino transfers for a longer period of time as minors are different in terms of children or parents moving out of the state, getting married, dying, or children or parents changing their last name—all measures that could substantially hinder match quality from being similar across our identifying variation. As we outline in much greater detail in the Online Appendix, all of this suggests that our results are unlikely to be biased by the match procedure itself.

4 Methods

Our identification strategy relies on techniques that make use of the individual panel nature of the data for parents and the cohort design of the survey for children. For the GSMS children, we run a difference-in-difference specification that leverages two differences—the first difference

of the issues that come with matching to administrative records. 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.

[17] 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.

[18] Theory predicts a strong transmission of voting from parent to child (Dawson and Prewitt 1968; Langton 1969; Searing, Schwartz, and Lind 1973; Plutzer 2002). 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 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; β=0.22, p<0.001), but one that is clearly muted by the sample composition.

is between American Indian (eligible for the transfer) and non-American Indian children (not eligible) and the second difference is across cohorts of AI children who were exposed to the income transfers at different points in the life course. This approach has been used in previous studies of the effect of the casino transfer (e.g., Akee et al. 2010).

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), these younger individuals were exposed for a longer time period to the income intervention and this may have had had a differential effect on children's voting attitudes, skills, and identities discussed earlier. Our hypothesis, informed by the HCMV, is that income transfers will have larger effects on the younger children in the survey. Our empirical analysis is designed to compare outcomes across age cohorts and race; this design is necessary since there are no credible pre-treatment observations for the children as none of them were eligible to vote prior to the casino transfers.

Equation 1 formalizes the difference-in-difference model that we estimate using data on the children in the GSMS sample:

$$Y_i = \alpha + \beta_1 YoungestCohort_i + \beta_2 MiddleCohort_i + \delta_1 AmericanIndian_i + \gamma_1 YoungestCohort_i \times AmericanIndian_i + \gamma_2 MiddleCohort \times AmericanIndian_i + X'\theta + \epsilon_i \quad (1)$$

Following previous practice (Holbein 2017; Sondheimer and Green 2010), in equation 1, we specify the outcome variable in two ways—first, as a binary variable indicating whether an individual has ever voted in a Federal or State election and second, as a continuous variable measuring the proportion of eligible Federal elections that a person voted in.[19] In equation 1, $YoungestCohort_{\mathrm{i}}$ is an indicator variable for the child belonging to the youngest cohort (age 9 at

[19] Whereas individuals typically only register once, they are free to vote multiple times. Hence, voting propensity is more appealing as an outcome, being more precisely estimated than registering. Increasing precision also motivates our decision to look beyond individual elections. Additionally, in North Carolina, Gubernatorial elections occur in the same year as US Presidential elections. According to North Carolina State Board of Elections website (https://www.ncsbe.gov/Voters/Registering-to-Vote), requirements to vote in North Carolina are that the person: "Must be a U.S. citizen; must be a resident of the county, and prior to voting in an election, must have resided at his or her residential address for at least 30 days prior to the date of the election; must be at least 18 years old, or will be at the time of the next general election, or be at least 16 years old and understand that you must be at least 18 years old on Election Day of the general election in order to vote; must not be serving a sentence for a felony conviction (including probation or on parole); must rescind any previous registration in another county or state." The individual must meet all of these requirements, turn in an application by mail and then an accepted registration notification will be mailed to the persons mailing address when they are successfully registered to vote in North Carolina. This is a substantially different process than what is required for tribal election. Registration for tribal elections requires that a person (according to Chapter 161: Elections Code of Ordinances for the Eastern Band of Cherokee Indians): "Be an enrolled member of the Eastern Band of Cherokee Indians; and be at least 18 years of age on the date of the applicable election; and be registered with the Cherokee Board of Elections as set forth in Section 161-11 prior to the applicable election." Registration can occur at tribal offices or by mail.

intake), $MiddleCohort_{\text{i}}$ is an indicator that the child belongs to the second youngest cohort (age 11 at intake). The omitted group is the third (oldest) cohort, so all coefficients are interpretable as differences from that cohort. The variable $AmericanIndian_{\text{i}}$ is a dummy equal to one for American Indians and zero otherwise. The vector X is a set of baseline covariates that include parents' voter turnout rate before the casino opened, 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 register 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 these coefficients may potentially capture differences in voting propensities arising from the differences at the age of first treatment or differences in the duration of treatment. We offer further discussion and some additional analysis aimed at disentangling these two potential channels in the Results section.

Given the high correlation of voting and household incomes displayed in Figure 1, that 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), and that some previous work on the casino transfers shows important heterogeneities by initial income levels, we specifically examine whether there are heterogeneities in the effect of the unconditional cash transfers across the initial household income distribution. We interact initial household income 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.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 income helps narrow income-based participatory gaps in political participation.

For the parents, we employ a difference-in-difference analysis that uses observations from before and after the income intervention and differences across AI (eligible) and non-AI (non-eligible) parents. The parents in our analysis were eligible to vote prior to the income transfers and can be found in the voter file in the pre-transfer election years 1992, 1994 and 1996. 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 as well as a well-specified set of treatment and control groups.

Our identification strategy for the parents is also based on the exogenous nature of the income transfer. Equation [2] formalizes this model—with γ being the coefficient of interest. In this case, our treatment of interest is an indicator for being exposed to the casino transfer in the time period after the start of the casino intervention. We include a control for American Indian status and a binary variable for whether the observation is drawn from 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 a constant α and an individual fixed-effect α_i since we observe the same individual 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 in the regression equation as it will be captured in the individual fixed-effect. Finally, we include voting 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:

$$Y_{it} = \alpha + \gamma_1 AmericanIndian_i \times AfterCasino_t + \delta_1 AmericanIndian_i + \lambda_1 AfterCasino_t + \theta_t + \epsilon_{it} \quad (2)$$

Identification in equation [2] is based on the assumption that the parallel trends assumption holds. We show pre-treatment trends for parents across age cohorts by race and year in Figure 4 below.[20] 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, 1994 and 1996 serve as the pre-treatment observations. We find that parents eligible for the casino transfer voted at a rate that was equivalent to those who were not eligible for the casino transfer over that pre-treatment period. This is reassuring that there were no differential time trends prior to the treatment. 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 (they were all less than age 18).

To further check for pre-treatment differences across the two groups, Appendix Table 2 in provides checks of variable means for a variety of baseline characteristics across the three age cohorts 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.[21]

We estimate the parental models both with the entire sample and then separated by above and below the median household income measured in the first three survey waves. We also provide results from sensitivity analyses which weight the parent observations based on the

[20] The rationale behind this test is that if treatment were truly orthogonal to other factors influencing voting, we would not expect to see treatment effects before the cash transfers began. If our identification strategy were able to isolate the effect of unconditional cash transfers from other factors, we would expect to see balanced rates of voter turnout across the different cohorts and American Indian tribal status before the transfers began.

[21] Appendix Table 3 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 4 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.

uniqueness of their match in the North Carolina voting registration data. As we discuss further in the Online Appendix, there are potential duplicate matches for parents given incomplete information on parental birthdate in the GSMS records. This missing information is balanced across our identifying variation.[22] 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 [1].

5 Results

5.1 Casino Transfers and Household Income

In Table 2, we show how household income was affected by eligibility for casino transfer payments. 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 $4,700 per recipient household, which accords with unofficial reports. 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 figure shows that there was no statistically significant change in household income prior to the income intervention (in survey waves 1-3) and a large and statistically significant increase in household incomes for American Indian households subsequent to the transfer initiation. The coefficients plotted in Figure 2 are based on the following triple difference equation

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

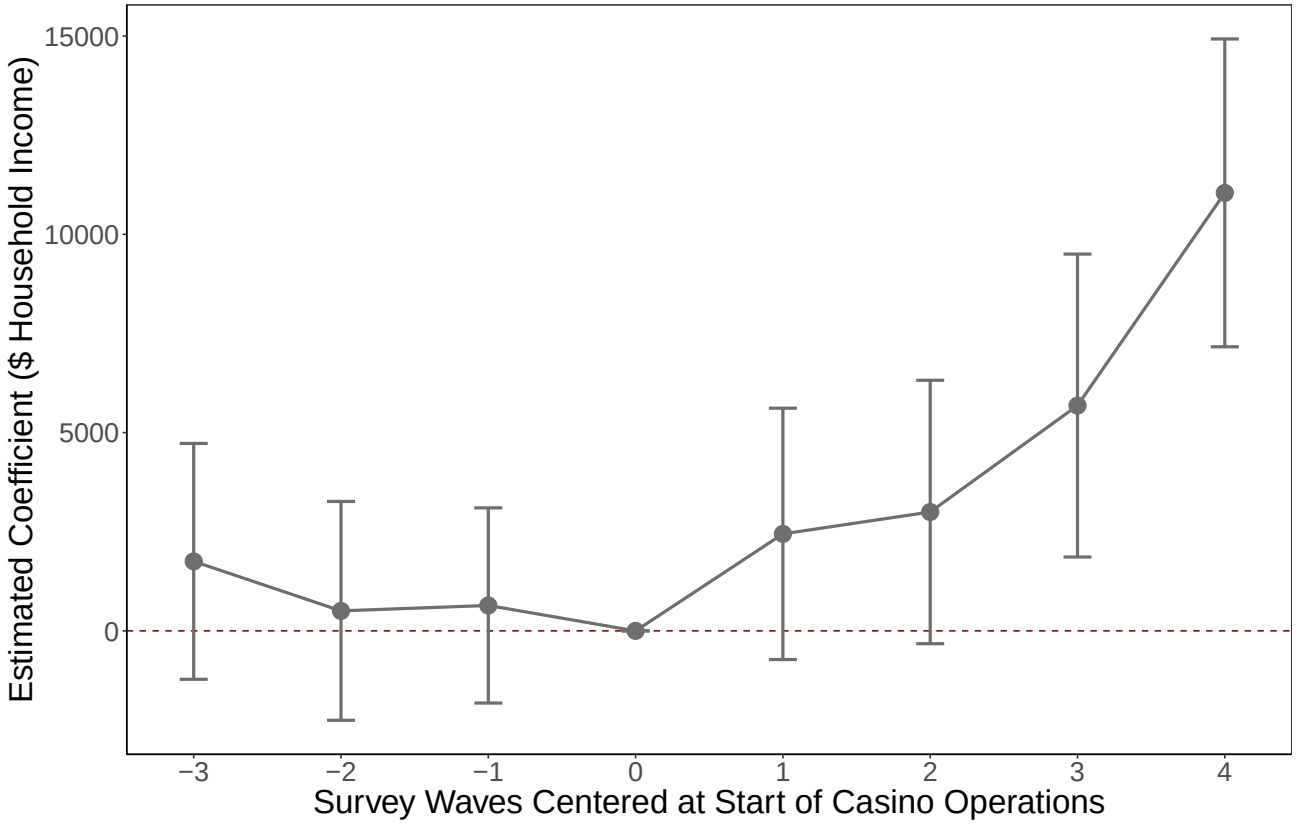
[22] Fortunately, the rate of missing observations of this matching information is balanced across our identifying information (Cohort 1, β=-0.34 (matches), p <0.369; Cohort 2, β=0.11 (matches), p <0.795. For parents, the median number of matches is 0; conditional on matching at all, the median is 1 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.

Table 2: First Stage Regression Using Individual Fixed Effects Regression Age Less Than 18 for Household Income

	(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 American Indian children (the baseline category). All regressions include all secondary interactions and level variables as well as the number of children less than age 6, Year and Month of Interview controls and a constant term. 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 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 as well as the number of children less than age 6 residing in the household, Year and Month of Interview controls and a constant term. 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 Children's Voting Outcomes

Table 3 shows our first set of voting results which estimate the effect of casino transfers on the voter turnout of children using equation [1].[23] The identification for this analysis comes from differences in the propensity to vote between AIs and non-AIs in the oldest cohort, which was treated for the shortest amount of time at the latest age, versus the youngest two cohorts, which were treated for 2 and 4 years longer, respectively. In columns 1 and 2 of Panel A, we present the results for the full sample. The estimated interaction coefficients in rows one and two provide the difference-in-difference coefficients as shown in equation [1]. The two outcome variables are measures of child voting behavior over the time period where all three cohorts were eligible to vote (2002-2014). The outcomes measures whether children ever voted in a State or Federal election and the proportion of elections that they voted, respectively.[24] We find in row three that parents' prior voting probability is strongly correlated with children's voting probability in the future. A one percentage point increase in parental prior voting probability increases own-child's voting by 0.11-0.16 percentage points. The coefficients 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. Children raised in households with incomes that are $5,000 higher are 1-2 percentage points more likely to vote.

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. Given the strong income gradient found in both national and the GSMS parental data (Figure 1) as well as the positive and statistically significant estimated coefficients in the regression, we next examine in columns 3 and 4 whether there is a differential impact of the cash transfers on child voting by initial household income (prior to the cash transfers). 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. 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 birth cohort but 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. Taken at face value, these estimates suggest that, ceteris

[23] Our final analysis sample is around 1,300 individuals due to missing baseline characteristics.

[24] Our analysis does not focus on voting in tribal elections, given the sporadic data that is available. Tribal elections do not line up with State or Federal elections. According to the "Charter And Governing Document Of The Eastern Band Of Cherokee Indians" ($https://library.municode.com/nc/cherokee_indians_eastern_band/codes/code_of_ordinances?nodeId=THCHCO$), the tribal elections generally take place every two years on odd-numbered years. Tribal council members hold terms for two years while the Principal and Vice Chief hold office for four years. Tribal elections are held on the first Thursday in September.

paribus, children from the youngest cohort with household incomes below $33,000 (0.575/0.088 * 5000) before the casino transfers increased their voting participation relative to children from the oldest cohort with similar household incomes. Those above the $33,000 initial household income threshold experienced a decrease in their voting participation. A similar result is found for the second Triple Interaction in row 5 but it is smaller in size and statistical significance. We note that parents' prior voting probability remains approximately similar in size and statistical significance as in the previous two columns.

It is not immediately clear how to interpret the heterogeneity in outcomes across the initial income distribution, and 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 3. First, in Appendix Figure 4 we plot the coefficients from triple interactions in which we combine cohorts 1 and 2 and interact that indicator with dummies for quartiles of relative increase in income due to the transfers as compared to initial income distribution. The lowest quartile is omitted and the coefficients on all other quartiles are interpretable as relative to that the effect on that lowest quartile. As the figure suggests, the effects are concentrated in the top two quartiles, and thus we expect that the effects should be much stronger in households located in the lower portion of the initial household distribution. APPENDIX TABLE WWWW 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. These estimates paint a similar picture, that the size of the cash transfer relative to the initial household income, has an important effect on the probability of children's subsequent voting. Second, and in light of the results presented above, we split the sample along the median initial household income. Panel B separates the observations by individuals from households initially below and initially above the median household income. In the first two columns, we present a 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 (column 1 of Panel B); it increases their proportions of elections voted by 12-13 percentage points (shown in column 2 of Panel B).

The next two columns in Panel B provide a 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 life appear to narrow participatory gaps

considerably helping to shrink the pre-treatment gap in voting for the youngest cohorts.[25] We also note that for children from the 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. This may suggest that the influence of parental voting probabilities is less influential for lower income households but plays a larger role for initially wealthier households.

Figure 3 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 these two cohorts. The estimation setup is exactly the same as in equation [3], except for the combined cohorts 1 and 2. We have no pre-treatment outcomes as the children were not eligible to vote in that time period. In the top panel, we plot the estimated coefficients for observations below the baseline median household income. The reference group is the oldest age group. The effect the casino transfers on the younger cohorts in 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.[26]

These results are remarkably robust to various alternative specifications. In Appendix Table 6 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 3. Our results largely mirror the results found in Table 3.

These results are consistent with the expectations that we described in the conceptual framework based on a human capital perspective of voting. They suggest that income transfers in 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 for nontrivial portions of the population voting rates are not locked-in at birth or rigidly transferred from one generation to the next. Elevating families out of poverty has 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 duration of the transfers is identical

[25] As a reference, recall that Figure 1 shows that the income gradient between GSMS (and similar individuals in the ANES) is around 20-26 percentage points.

[26] Appendix Table 5 provides the regression coefficients for Figure 3. Here we pool across all potential election years. Appendix Table 5 also provides the analysis in a simple difference-in-difference setup.

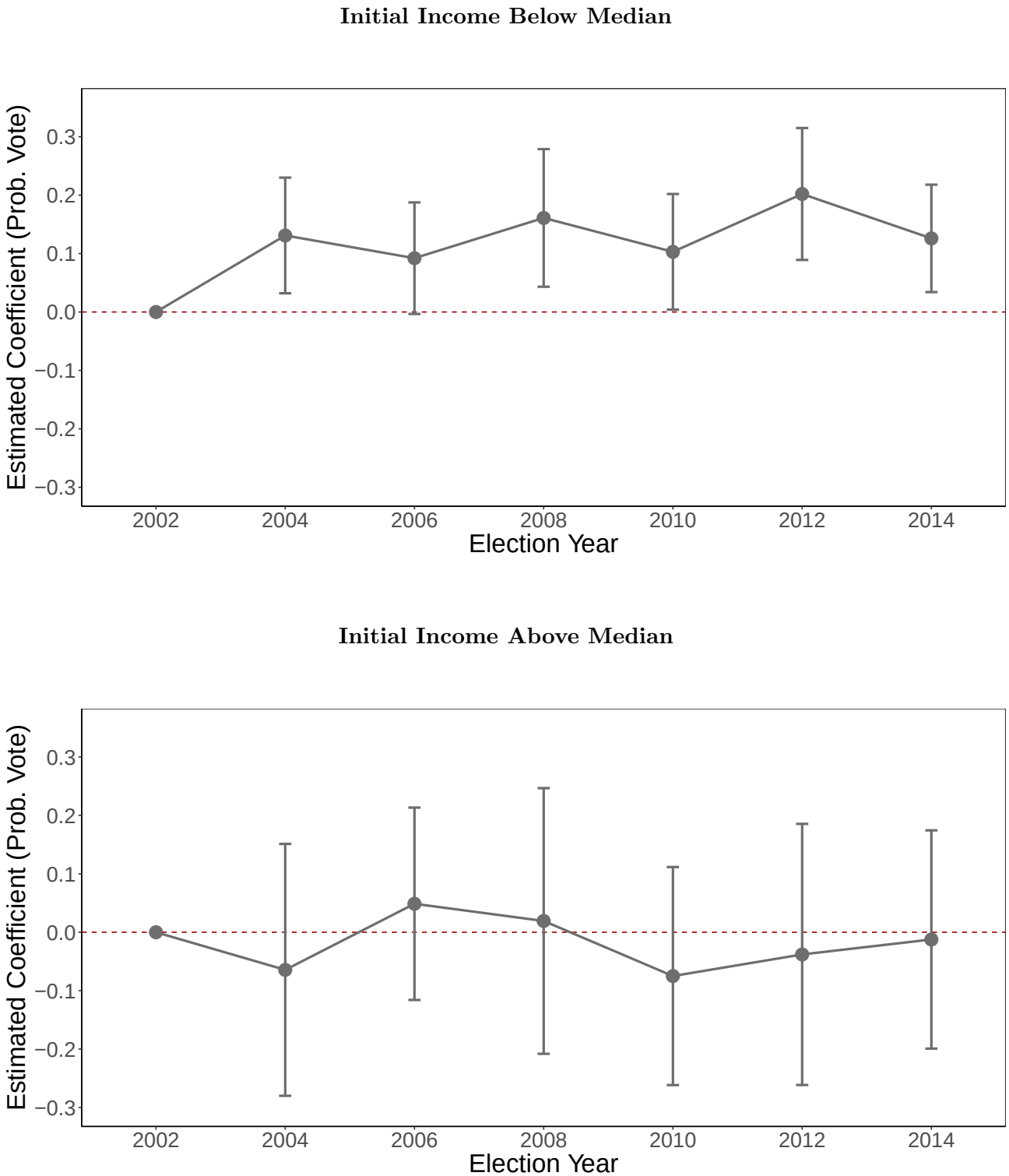
Table 3: The Effect of Casino Transfer on Children's Voter Turnout (Years 2000-2014)

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***
$\times American Indian$	(0.0741)	(0.0401)	(0.128)	(0.0698)
Interaction 2: Age Cohort 2	0.0743	0.0451	0.321**	0.228***
$\times American Indian$	(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
$\times American Indian$	(0.0817)	(0.0409)	(0.142)	(0.0857)
Interaction 2: Age Cohort 2	0.231***	0.124***	-0.0382	-0.0219
$\times American Indian$	(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, mother's highest educational attainment, fathers highest educational attainment, 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 3: 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)

across cohorts at every election. The age at which they were first treated is not. This lends some credibility to the hypothesis that the effects are different due to the differences in initial age of treatment. We perform one more set of analyses to provide additional evidence. In Appendix Table 10 we consider the difference in the propensity to vote in the 2002 election. The empirical specification here is the same as in equation 1, 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. Again, everyone in the AI population has been treated for 6 years of transfers in 2002, and so we hold duration of treatment constant. Still, there are differences in the age across cohorts, but we have no reason to believe these differences to be different across AIs and non-AIs. In any case, in this setup we see that the younger cohorts AI are more likely to participate relative to the non-AIs and relative to the oldest cohort. Finally, we restrict individuals to be of a comparable age and compare their voting behavior in different elections. Specifically, we compare individuals from the oldest and the youngest cohort voting in US Congressional elections in 2002 and 2006, respectively; each of those cohorts were approximately 21-22 years of age 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. The results provided in Appendix Table 10 show that the results are qualitatively similar to our main results. Taken together, these findings give somewhat be

5.3 Parents' Voting Outcomes

We next turn our attention to the effects of the casino transfer on parents' voting rates. In Table 4 we estimate equation [2], which focuses on parents' voting. As we mentioned earlier, for the parents we are able to compare the period before and after the casino opened for the same person as we have reliable voter participation data starting in 1992. Given this additional flexibility, we examine the impact of the exogenous change in household income among parents in several ways. First, we compare them in a simple difference-in-difference setting, then we compare across the age cohorts (of their children) as a robustness check and a direct comparison to the children's estimation setup.

The first three columns of Table 4 provide the results from the difference-in-difference specification that leverages pre- and post-casino difference by transfer eligibility status. Here the coefficient of interest is the interaction coefficient between American Indian race and a binary variable indicating the time period after the casino operations began. Column 1 provides the results for the pooled sample. Here we find that the increase in household income has no economically substantive or statistically significant effect of on parents' voting probabilities. This null effect is precise: with our 95% confidence intervals allowing 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). Given the heterogeneity in the effect on children's voting by initial household income, we run separate analyses on the samples

Table 4: The Effect of Casino Transfer on parents' Voter Turnout (Probability of Voting)

	Pooled	Below Median HH Income at Baseline	Above Median HH Income at Baseline	Pooled	Below Median HH Income at Baseline	Above Median HH Income at Baseline
	(1)	(2)	(3)	(4)	(5)	(6)
Variables	Voted	Voted	Voted	Voted	Voted	Voted
Casino Payment	-0.00492	-0.0250	0.00673	-0.00501	-0.0203	0.000564
	(0.0148)	(0.0201)	(0.0217)	(0.0147)	(0.0201)	(0.0207)
Mean of Dependent Variable	0.4346	0.3285	0.5361	0.4346	0.3285	0.5361
Year FE	Y	Y	Y	Y	Y	Y
Weighted ?	N	N	N	Y	Y	Y
N (parent-years)	15,984	7,812	8,172	15,984	7,812	8,172
R-squared	0.097	0.057	0.044	0.104	0.050	0.042

Notes: *** $p < 0.01$, ** $p < 0.05$, *$p < 0.10$. Models include individual fixed effects, age fixed effects, year fixed effects and a constant. 95% confidence intervals based on cluster robust standard errors (family level) are given in brackets below the estimated coefficients.

below and above initial median household income in columns 2 and 3. There are no large or statistically significant effects on parental voting probabilities in either subsample, contrary to our findings about the children. The next three columns in Table 4 provide similar difference-in-difference models with probability weights based on the quality of the match to public records. We find no large differences.

Figure 4 provides the event analysis for parents for the pooled sample and separately by initial household income. We plot the coefficient on the AI dummy for all elections starting in 1992, which is before the income transfers began in 1996 (Appendix Table 7 provides the regression results used for this figure.) The figure confirms the findings in Table 4 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 levels.

These results are consistent with a conceptual model of voting in which individual propensity to participate in civic society is set by the adult years, which is consistent with our proposed conceptual framework. They suggest that voting preferences are set earlier in life and are unchanged in later adult years. An important implications is that efforts in adulthood to change woefully low, unequal, and (by some accounts) declining rates of civic participation are not likely to succeed (a fact corroborated by the overwhelming small effects of adult-targeted get-out-vote campaigns; see Green, McGrath, and Aronow 2013).

Figure 4: Effect of Casino Transfers on Parental Voting by Initial Household Income Status around the Start of Casino Operations

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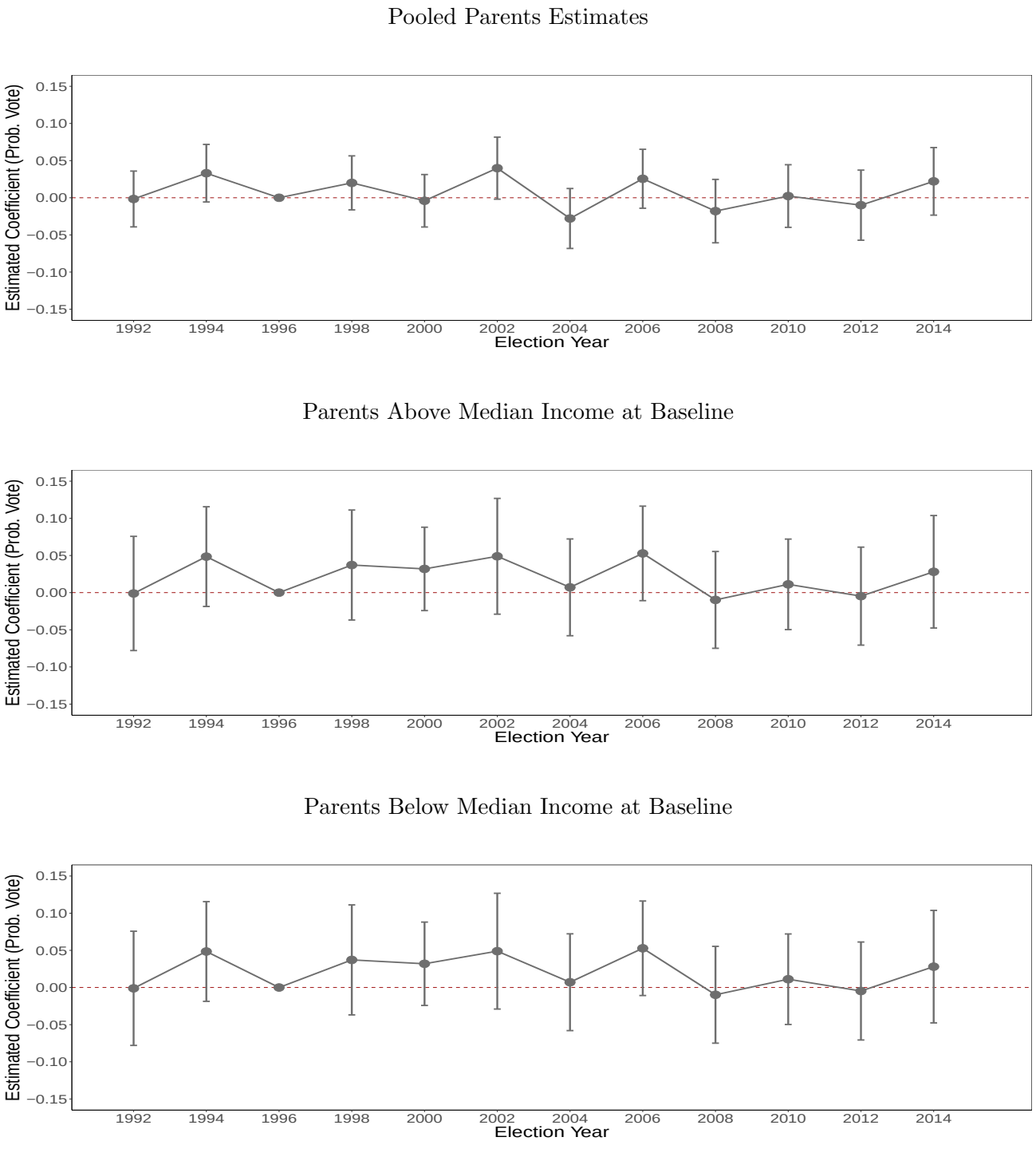

Notes: Figure displays coefficients from event analysis model for parents' voter turnout in the 1992-2014 elections. The estimates are split by median family income levels at baseline. Standard errors are clustered at the individual level. Top panel N = 15,984 (1,332 GSMS individuals); Middle panel N = 8,172 (681 GSMS individuals); Bottom panel N = 7,812 (651 GSMS individuals).

5.4 Robustness Checks

The results presented are remarkably robust to alternate specifications. For example, in Appendix Table 8 we conduct the same cohort based difference-in-difference for parents that we used for children (see Table 3). Our intention here is to compare whether parents of difference child cohorts were more likely to vote. Fr example, if the parents of a single cohort had greater voting preferences or were differentially affected themselves somehow, they may have been more likely to transmit to this habit to their children. That is, here we are exploring whether some of the effects that we observed in Table 3 are attributable to cohort-based differences that emerge across parents. In part, this is helpful to explore potential mechanisms driving our effect among children.

We present this difference-in-difference in Appendix Table 8. Figure A3 provides the event analysis for these same results and Appendix Table 9 provides the regressions used in the figure. Again, 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 and the weighted models are presented in Panel B. As can be seen, there is no statistically significant results in these analyses, which accords with our earlier findings in Table 4. This suggests that the effect on children that we observe probably has very little to do with parents' directly socializing their children to the norm of voting through their example in voting. Something else is probably driving these results.

6 Potential Mechanisms

Our analysis up to now has investigated the effect of an increase in household income on both parental and child voting probabilities. The results indicate that there is evidence that for children from households with incomes initially below the median that they are most likely to increase their voting as adults. There is no evidence that voting is affected for parents at all.

As we outlined in the conceptual framework portion of our paper, there are several reasons to suspect why unconditional cash transfers had such a noticeable effect on the voting outcomes of recipients from disadvantaged childhood backgrounds. These individuals could have seen higher levels of human capital (education, cognitive ability, non-cognitive ability) due to the income transfers, that acted as additional resources down the road that encouraged them to vote; or they could have experienced enhanced social networks that help mobilize them through votings social component. Both of these indirect channels are consistent with a model of voting based in 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). In short, with mechanism testing it is hard to 1.) incorporate post-treatment variables (as mediation models

all require) without introducing bias, 2.) know whether unobserved mediators are actually doing the heavy lifting, and 3.) understand the complex relationship between interconnected mechanisms. For these reasons, documenting exact mechanisms is hard (if not impossible).Still, we can provide suggestive evidence regarding a few of the potential mechanisms driving 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). Our analyses in Table 3 included parental voting probability prior to the start of the cash transfer payments and we observe that this probability does not substantially impact the results. Moreover, we found that the cash transfers had precisely estimated null effects on parents; thus making it unlikely that the change in voting among children is the result of (non-existent) changes in voting among parents. Therefore, it seems unlikely that changes in parental voting patterns are a likely mechanism in our analysis.

The cash transfer 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 A11 we show mother's educational attainment and employment status over the period of 1993-2000 (while the child is still in the household). There do not appear to be any statistically significant or substantively meaningful changes in either mother's educational attainment or her employment status as full or part-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.[27] Hence, this too is unlikely to explain changes in disadvantaged children's voting probabilities.

Moving household locations may also play an important role in changing child voting behavior in the long run. Research shows that moving affects who votes (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.

In Table 5, we use data on the households geographic location to examine whether children's parents ever moved during their childhood or whether the child herself moved during adulthood.[28] The first two columns divide the observations to households that moved and didn't

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

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

move during childhood, respectively. The regression results indicate that there is no difference in observed results for the cash transfer. In columns 3-6 we further separate the observations into below and above initial median household. We find that the impact of the casino payment is found only for those households that were initially below the median income and did not move during childhood (column 4).

In the next four columns (7-10) we investigate whether moving as an adult is related to the observed effects on voting for the children. 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.[29]

[29] 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 5: 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
$\times American Indian$	(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
$\times American Indian$	(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)
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 that did not residentially relocated during childhood or as adults. This result 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. Thus, we have little evidence to suggest that moving to a new community that may have different voting patterns is an important mechanism for explaining the improvement in voting probability for the study subjects as adults.

It is also plausible that as adults the subject children have higher levels of education and this could be an important determinant of adult voting behavior. In previous work, Akee et al. (2010) show that the casino transfer increased the probability of graduating high school on time (by the time one was 19) and education attainment at 21 substantially, with these effects being centered among those previously in poverty prior to the cash transfer. Hence, educational attainment may be a likely mediator (especially given its strong connection to voting in previous research; e.g. Sondheimer and Green 2010).

In Appendix Table A12 we show the effect of the cash transfer on whether an individual has a college degree at age 30. The first column provides the pooled analysis and indicates the children from the two treated cohorts have positive estimated coefficients, however, neither are statistically significant. We also separate the analysis by initial household income and do not find statistically significant differences in college degree attainment by age 30 across these study subjects. Therefore, it does not appear that differences in educational attainment at age 30 are driving the long-term effects we observe 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 6. Each set of regressions are similar to 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 aggressive behavior. None of the coefficients are statistically significant, though the signs are negative, suggesting that receiving the casino transfers reduced the likelihood that young people from the youngest and second youngest cohorts had difficulties making friends.

The second outcome we explore is whether a child is reported as having difficulty making friends due to failure to approach other children; this is reported in columns 3 and 4. We view

this outcome as complementary to the previous one and explaining an additional dimension of social connections and interactions with peers. The estimated coefficients in columns 3 and 4 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 larger for children from households initially below the median income level. Overall, we fail to reject the hypothesis that the estimated coefficients between column 3 and 4 are equal to one another.

Finally, we test whether a child is considered to be a rule breaker in columns 5 and 6. 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 5 is large, negative and statistically significant. The estimated coefficients in column 6 are small in magnitude and do not reach statistical significance at conventional levels. The results presented in Table 6 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 anti-social (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 for which additional household income affects child voting behavior. We take these results as likely candidates to explain the mechanisms, but acknowledge that we cannot provide definite evidence in this setting without additional data.

Table 6: Child's Social Characteristics At Age 15 or 16

	Initial HH Income		Initial HH Income		Initial HH Income	
	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Difficulty making friends due to aggressive behavior?		Refuses or Unable to be involved or talk with peers?		Violates rules at school or elsewhere outside the home?	
Interaction 1: Age Cohort 1	-0.0357	-0.0321	-0.114*	-0.0284	-0.290**	0.0211
American Indian	(0.0476)	(0.0504)	(0.0588)	(0.0612)	(0.120)	(0.128)
Interaction 2: Age Cohort 2	0.0674	-0.0300	-0.0631	-0.0669	-0.0750	0.0532
American Indian	(0.0549)	(0.0493)	(0.0599)	(0.0625)	(0.116)	(0.143)
Constant	0.688	0.244	-0.378	0.542	-1.813**	0.243
	(0.466)	(0.417)	(0.535)	(0.393)	(0.879)	(0.846)
Observations	481	541	482	541	474	539
R-squared	0.022	0.014	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 American Indian 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.

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 several 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 educational attainment beyond high school does not increase in a manner consistent with our results. We do find some weak evidence, however, 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.

7 Conclusion

Decades of social science research has established that income bias exists in voter turnout and that these patterns may have distortionary effects on representative democracy. Here we have taken the next step to explore whether income transfers are able to raise turnout and narrow participatory gaps; that is, we have examined whether income has an effect on this foundational social act of democracy. Results from our unique quasi-experiment suggest that unconditional cash transfers do, indeed, have a substantial impact on participatory inequality. Cash transfers help disadvantaged children catch up with their more advantaged peers. However, they have little to no effect on parents nor on more advantaged childhood recipients.

Our results make both conceptual and practical contributions. In establishing that this foundational resource plays an especially important role earlier in the life course, our results contribute to a broader framework for understanding what drives people to participate in politics. Rather than relying alone on a Resource Model of Voting, which predicts that resources uniformly increase participation, our results suggests a more nuanced Human Capital Model for Voting may be more accurate. Consistent with the predictions of the HCMV, our results show that resource effects appear to be constrained by powerful life course forces. That is, voting resources (like income) appear to be more beneficial for children than for adults.

From a practical perspective, our results suggest that unconditional cash transfer programs may have broader effects than previously realized. Not only may these affect individuals labor, health, and schooling outcomes, these may also influence citizens' levels of civic engagement or social capital. As civic participation plays a vital role in preserving democratic values and institutions, such a finding is important.

Future work would do well to consider the effects of exogenous unconditional income transfers in the context of a randomized-control trial targeted towards families. To our knowledge, such a set up with intergenerational elements currently does not yet exist. But, in future years, unconditional cash transfer programs could feasibly be linked to voting records as we have done here. Further, future work would do well to consider the effect of other resource transfers over the life course and across generations. In our view, at present too little political behavior research looks at the effects of early life experiences.

Many efforts have been made to increase voter participation among disadvantaged low SES families. These provide citizens with various information or social nudges. Sadly, most of these interventions have negligible effects on disadvantage populations or have even backfired and made participatory gaps worse. Our results suggest that a more straightforward approach may be helpful. To narrow socioeconomic gaps in voter turnout, income transfers to disadvantaged children are viable.

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A Appendix Tables

Table A.1: Differences in Characteristics Affecting Matching Rates for Parents

		Initial HH Income			Initial HH Income	
	Pooled	Below Median	Above Median	Pooled	Below Median	Above Median
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Moved Out of North Carolina			Ever Changed Last Name		
Interaction 1: Age Cohort 1	-0.00471	-0.0629	0.118	0.0220	-0.0218	0.0882
$\times AmericanIndian$	(0.0561)	(0.0771)	(0.0914)	(0.0373)	(0.0517)	(0.0581)
Interaction 2: Age Cohort 2	-0.00891	-0.0248	-0.0168	0.0949**	0.0731	0.122*
$\times AmericanIndian$	(0.0577)	(0.0836)	(0.0726)	(0.0407)	(0.0560)	(0.0641)
Observations	1,328	648	680	1,332	651	681
R-squared	0.014	0.030	0.020	0.012	0.013	0.015

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

Table A.2: Mean Differences by Age Cohort and American Indian 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 American Indian Parents	N/A	N/A		
American Indian 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 American Indian Parents	N/A	N/A		
American Indian 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 American Indian Parents	N/A	N/A		
American Indian 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 A.3: Correlation of Education and Voting

Rural:	0.2153
Rural African American:	0.1801
GSMS	0.2014

Table A.4: Comparison of Economic Characteristics with other American Indian Tribes and relevant demographic groups

Data Source:	1990 Census for American Indians	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 American Indians; Social Explorer, 1990 County Data; IPUMS 1990, 1% Sample.

Table A.5: Children's Voting Probability Pooled by Initial Household Income

	(1)	(2)	(3)	(4)	(5)
		Initial HH Income			
		Below Median	Above Median	Below Median	Above Median
VARIABLES	Voted	Voted	Voted	Voted	Voted
Interaction 1: Age Cohort 1	0.0440	0.131***	-0.0268		
$\times American Indian$	(0.0400)	(0.0409)	(0.0857)		
Interaction 2: Age Cohort 2	0.0452	0.124***	-0.0253		
$\times American Indian$	(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 A.6: 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 American Indian	(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.

Table A.7: 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	Omitted	Omitted
	Category	Category	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 A.8: 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 American Indian	(0.0631)	(0.0787)	(0.114)	x American Indian	(0.0331)	(0.0464)	(0.0465)
Age cohort 2	-0.0132	-0.0509	-0.0249				
x American Indian	(0.0664)	(0.0851)	(0.114)				
Year FE	Y	Y	Y	Year FE	Y	Y	Y
N (families)	1332	651	681	N (families)	1332	651	681
R-squared	0.097	0.061	0.045	R-squared	0.097	0.059	0.044

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 American Indian	(0.0580)	(0.0683)	(0.110)	x American Indian	(0.0333)	(0.0465)	(0.0462)
Age cohort 2	-0.0125	-0.0430	-0.0428				
x American Indian	(0.0601)	(0.0728)	(0.107)				
Year FE	Y	Y	Y	Year FE	Y	Y	Y
N (families)	1332	651	681	N (families)	1332	651	681
R-squared	0.104	0.052	0.043	R-squared	0.104	0.052	0.043

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

Table A.9: 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.

Table A.10: Children's Voting Probabilities at Similar Ages and in 2002 Election

	(1)	(2)	(3)	(4)	(5)	(6)
	Pooled	Initial HH Income Below Median	Above Median	Pooled	Initial HH Income Below Median	Above Median
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 American Indian	(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: *** p < 0.01, ** p < 0.05, *p < 0.10. Robust standard errors in parentheses.

Table A.11: Mother's Educational Attainment and Employment

	(1)	(2)	(3)	(4)	(5)	(6)
	Pooled	Below Median HH Income	Above Median HH Income	Pooled	Below Median HH Income	Above Median HH Income
VARIABLES	Mother Has a High School Diploma or More			Mother Has a College Degree or More		
Received Casino Payment?	0.00342	0.0120	0.0354	-0.0341	-0.00549	-0.0339
	(0.0308)	(0.0429)	(0.0455)	(0.0365)	(0.0515)	(0.0618)
Constant	0.720***	0.570***	0.857***	0.385***	0.186***	0.573***
	(0.0292)	(0.0457)	(0.0382)	(0.0392)	(0.0626)	(0.0511)
Observations	7,414	3,416	3,998	7,414	3,416	3,998
R-squared	0.006	0.018	0.009	0.012	0.021	0.010
Number of Individuals	1,276	612	664	1,276	612	664

	(1)	(2)	(3)	(4)	(5)	(6)
	Pooled	Below Median HH Income	Above Median HH Income	Pooled	Below Median HH Income	Above Median HH Income
VARIABLES	Mother Works Full Time?			Mother Works Part Time		
Received Casino Payment?	-0.0192	-0.0423	0.0836	0.0134	0.00517	0.0748
	(0.0480)	(0.0693)	(0.0678)	(0.0498)	(0.0748)	(0.0568)
Constant	0.391***	0.200**	0.567***	0.614***	0.468***	0.754***
	(0.0617)	(0.0964)	(0.0778)	(0.0541)	(0.0867)	(0.0679)
Observations	7,402	3,409	3,993	7,402	3,409	3,993
R-squared	0.024	0.048	0.014	0.018	0.034	0.010
Number of Individuals	1,276	612	664	1,276	612	664

Notes: *** $p < 0.01$, ** $p < 0.05$, *$p < 0.10$. Models include individual fixed effects, age fixed effects, year fixed effects and a constant. 95% confidence intervals based on cluster robust standard errors (family level) are given in brackets below the estimated coefficients.

Table A.12: Child's Educational Attainment by Age 30

VARIABLES	Pooled (1)	Initial HH Income Below Median (2)	Initial HH Income Above Median (3)
	College Degree by Age 30		
Interaction 1: Age Cohort 1	0.0618	0.0516	0.129
$\times AmericanIndian$	(0.0533)	(0.0603)	(0.112)
Interaction 2: Age Cohort 2	0.0212	0.0147	0.00576
$\times AmericanIndian$	(0.0587)	(0.0705)	(0.120)
Constant	0.0123	0.593	-0.110
	(0.449)	(0.521)	(0.742)
Observations	1,152	554	598
R-squared	0.140	0.035	0.044

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

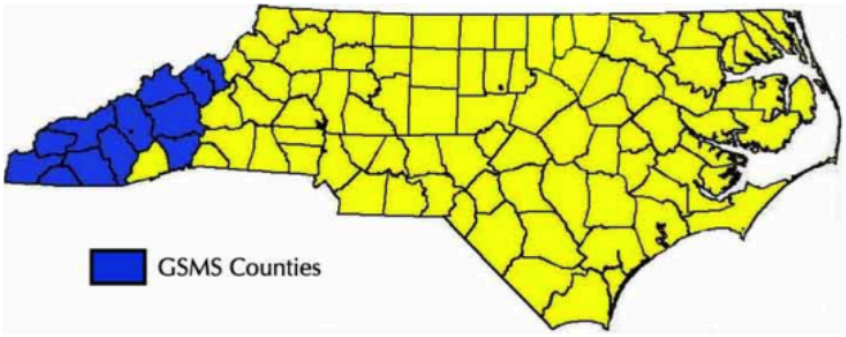
Table A.13: The Effect of Casino Transfer as a Percent of Initial Household Income on Children's Voter Turnout (Years 2000-2014)

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 American Indian indicator, gender, age cohort indicator variables, age, number of children in the household below age 6 and a constant. Robust standard errors employed.

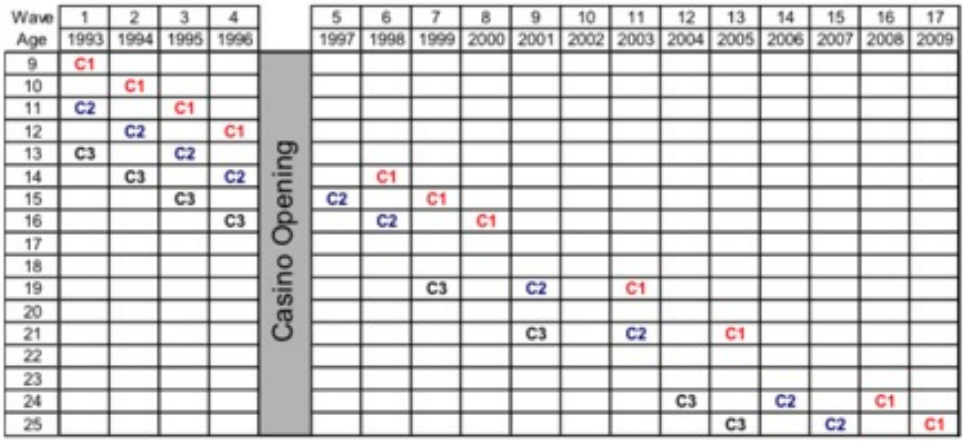
B Appendix Figures

Appendix Figure 1: graphic 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).

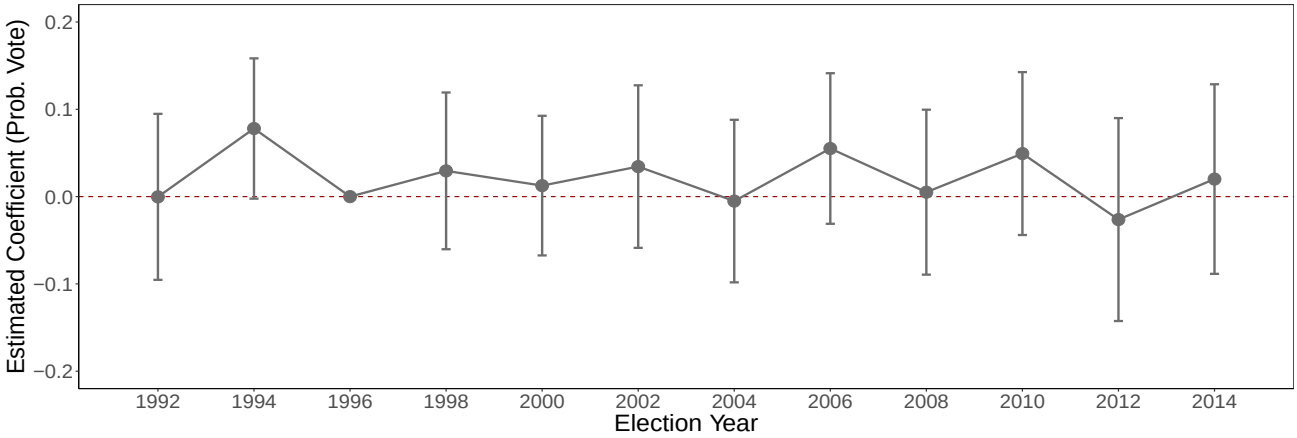
Appendix Figure 2: Design of Follow up Surveys of the GSMS



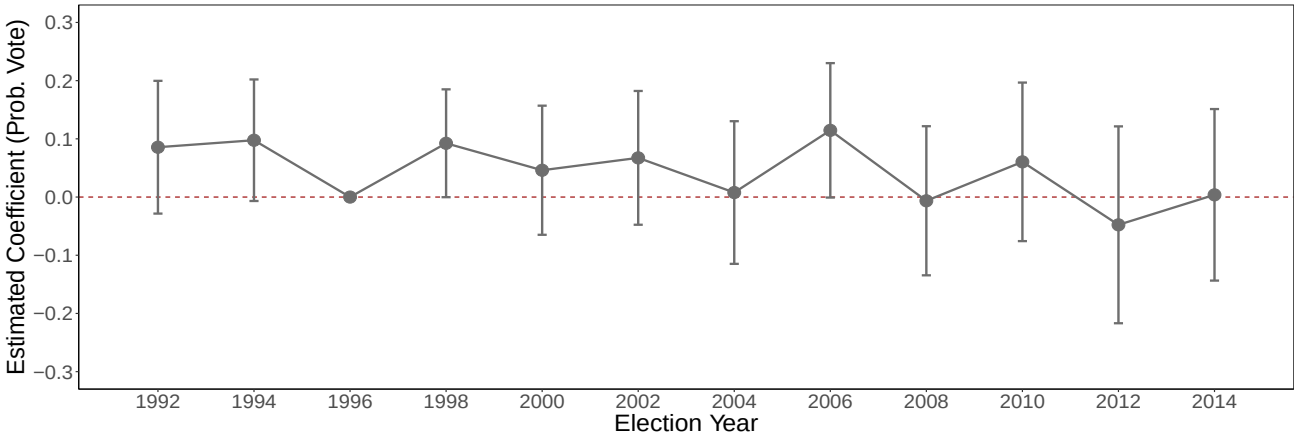
Note: Figure displays the structure of the GSMS data. C1=cohort 1, C2=cohort 2, C3=cohort 3. On the vertical access are children's ages. On the horizontal access are survey wave and year. Survey data collection began in 1993, with the three age cohorts all being interviewed. These interviews continued 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.

Appendix Figure 3: Effects of Casino on Parents Voting, Event Analysis

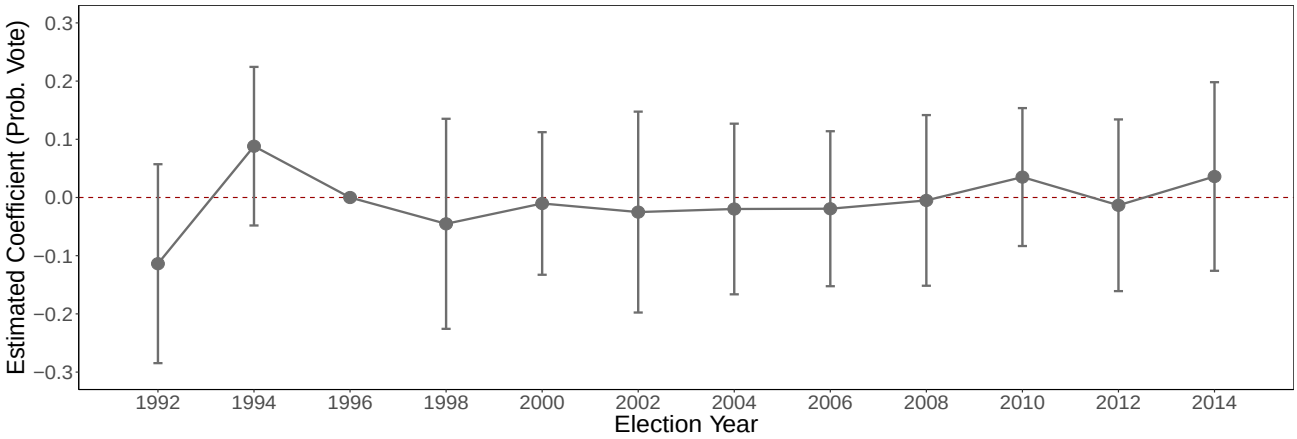
Pooled Parents estimates



Parents Below Median Income at Baseline

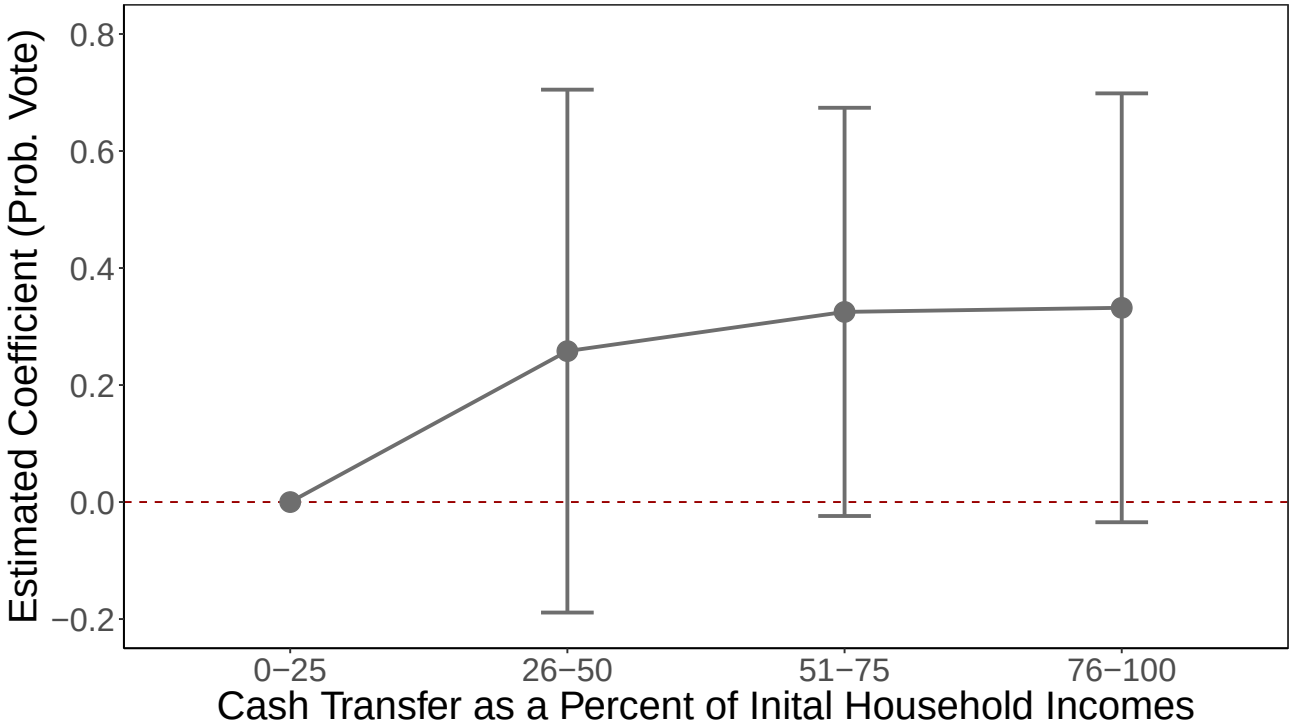


Parents Above Median Income at Baseline



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.

Appendix Figure 4: Impact of Cash Transfer on Voting by Relative Size of Transfer



Notes: Figure displays the impact of the casino transfers on voting by the size of the transfer relative to the family's income at baseline. Sample restricted to American Indians, since non-Indians are all automatically at 0%. Robust standard errors employed.