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

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People who have more money are more likely to participate in politics. Despite clear evidence of this income gradient in political participation, few have been able to isolate the effects of income from other household characteristics. There is little work showing whether income itself narrows or exacerbates participatory inequality or has effects that span multiple generations. Even less is known about how and when children's participation rates are formed and whether family financial circumstance plays a role. In this paper, we begin to fill these gaps by exploring the effect of exogenous unconditional cash transfers across two generations from the same household. Our approach employs a quasi-experimental income intervention. Using various panel techniques, we show that household receipt of unconditional cash transfers increases children's voter turnout (in adulthood) noticeably among the children from initially poorer households. Thus, the additional income narrows participatory inequality across generations. However, income transfers have no effect on adult-aged recipients (the household parents), whose voting patterns appear to be locked-in. These results suggest that childhood conditions—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, these differences are transmitted across generations and likely contribute to the intergenerational transmission of social inequality.

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I. 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 more troubling. Comparing those who vote to those who don't 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). Indeed, 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. But see also Wolfinger and Rosenstone 1980 on this point).

While the presence of a participatory gap between high and low-income individuals—and its broader implications—is well established, scholars have much less understanding of whether income is the driving force behind these gaps, or instead income gradients reflect some other unobserved social or contextual factor. As a result, there is little understanding of how to address this form of participatory 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 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 a meaningful 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 mental health in rural western North Carolina, which began in 1993 and consisted of both American Indian and non-American Indian families in the area. Partway during the collection of the GSMS data, a casino opened on the nearby Eastern Cherokee reservation. 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 (who were in late childhood 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.[1] As theory would predict, income transfers increase the turnout levels of children in the initially poorer families noticeably—substantially closing the participatory gap between high and low-income individuals of this rising generation. Average annual unconditional transfers of approximately $3,500 increase this group's later turnout by about 8-20 percentage points, depending on the age of the recipients and measure of voting one considers. However, unconditional income transfers have precisely estimated null effects on parents, regardless of their starting income levels: with our 95% confidence intervals allowing us to confidently rule out effects as small as 3 percentage points. 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).

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

[1] 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.

predict—income's 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.[2,3] The HCMV that

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[2] 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).

[3] 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

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 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 the 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

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that experiences that *far predate* the voting experience play an underappreciated role in influencing civic participation.

(e.g. Akee et al. 2010 and 2013; Aguero, Carter, and Woolard 2006; Baird et al. 2012; Baird, De Hoop, and Özler 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.

II. 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).

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 including 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). 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.

IIa. 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.[4] 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 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. Under this framework, income makes it more likely that citizens are socialized to a norm of voting. For example, in their seminal work on voting, Wolfinger and Rosenstone (1980, p. 21) argue that "income determines one's neighborhood and, perhaps, avocational companions and thus exposure to a variety of norms and pressures." 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

[4] 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.

prediction has some face validity and some observational empirical support, this theoretical prediction has yet to be fully tested.

IIb. Empirical Evidence Linking Income and Political Participation

At first glance, empirics support the theoretical prediction that income increases voter turnout. Indeed, it is clear from virtually all data sources that have measures of income and voting that there is a positive relationship between the two. Figure 1 provides the correlation between household income and voting using data from the American National Election Study (ANES)[5] and the Great Smoky Mountains Study of Youth (which sample we explain in greater detail below, as we use it for identification). In the figure, we separate the different groups into three income bins. The first column in all three bins is for the GSMS parental voting data from the baseline period. The second and third columns in each of the clusters provide voter turnout rates for rural African Americans and rural Americans respectively using data from the ANES. We use these two as benchmarks to give context to our GSMS sample and to show that the income gradient is present across social context.[6] As can be seen, there is a noticeable 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, being 20.6 percentage points (for rural) and 26.8 percentage points for rural African Americans. We note that absolute income levels differ slightly across these groups, however, the relationship holds in all three cases. Simply put, people with higher incomes are substantially more likely to

[5] 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.
[6] 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 ($p<0.001$).

engage in the fundamental act of democracy.

[*Figure 1 here: Income Gradient in Voting*]

While there is clearly an income gradient in voting, this does not mean that there is a causal relationship between income and voting. Indeed, this relationship could be spurious. Acknowledging this possibility, a host of researchers have dug deeper than the bivariate relationship we show in Figure 1. These studies condition on observable individual and contextual characteristics. From this group of studies, the evidence of the relationship between income and participation is decidedly mixed. A recent meta-analysis of 90 studies shows that about half of studies find that income is an important predictor for voting, while the other half do not (Smets and Van Ham 2013).[7]

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 Ham's (2013) meta-analysis, it is clear that none leverages exogenous variation in income. One strand of research gets close to so doing: studies exploring the political consequences of conditional cash transfers (CCT). This body of work leverages random (or as-if random) variation in exposure to CCT programs—linking participants (or heavily exposed geographic areas) to political outcomes data (e.g. Baez et al. 2012; De La O 2013, 2015; Galiani et al. 2016; Imai, King, and Rivera 2017; Linos 2013; Pop-Eleches and Pop-Eleches 2012; Zucco 2011). While these studies speak to an important topic, this approach may not be ideally situated to answer the question of whether income has an effect on voter turnout. On a very basic level, this program of study has faced data challenges in linking CCT participants and

[7] 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.

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.[8]

To our knowledge, only one study of the effects of *unconditional* cash transfers exists. Using an innovative approach that leverages data from the annual Spanish Lottery, Bagues and

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[8] 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.

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. Finally, Bagues and Esteve-Volart (Forthcoming) do not explore potentially important dynamics in income's effect on turnout—including across socioeconomic status and the life course. For these reasons, the effect of income on voter turnout remains an important object of study.

III. 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 and administrative data from the Great Smoky Mountain Study (GSMS)—a unique longitudinal study of 1,420 children and their parents that began in 1993.[9] The survey was originally designed as a means of studying

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

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. Families were recruited from 11 counties with an accelerated cohort design and an oversample of children from the Eastern Band of Cherokee Indians (for more details on the sampling framework, see Costello et al. 1996 and Costello et al. 1997). In the original sample, 25% of the children were American Indians living on (or near) the Eastern Cherokee Reservation. The sample was designed to be representative of the school-aged population of children in the region studied. 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).[10]

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 characteristics are averaged over the first three survey waves prior to the start of the intervention. The first five characteristics show that the survey selection was balanced across the three age cohorts across the American Indian and non-Indian population in these 11 counties. There is a statistically significant difference in levels of average household incomes prior to the intervention;

[10] Children were interviewed at the same time as their parents (but in separate interviews) until they turned 16. After that, only children were surveyed. For an overview of the survey wave structure, see Figure A2 in the Online Appendix.

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. There is also a difference in parental educational attainment by race in this data. In general, non-Indian parents (both mothers and fathers) tend to have higher educational attainment (more than a high-school degree) than American Indian parents prior to the start of the intervention. We also find that American Indian parents are less likely to vote at all as compared to non-American Indian parents over the entire time period by about thirty percentage points. Hence, our identification strategy must take into account these raw mean differences.

[Table 1 here: Table of Means by Race]

After the fourth wave of the survey, a casino opened on the Eastern Cherokee reservation.[11] Upon the casino's opening, all adult enrolled tribal members—regardless of whether they were living on the reservation or not—were eligible to receive bi-annual cash transfers. 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%.

IIIa. Match of GSMS Participants to Voter Files

To explore the effect of casino transfers on voter turnout, in July 2016 we matched GSMS participants to public use voter files. This approach involved scraping voter registration and voter history information off publically available statewide voter portals.[12] To do so, we followed

[11] The process for approving the casino started in 1988, with the federal passage of the Indian Gaming Regulatory Act, which (among other things) clarified the sovereignty of Native tribes to open and operate casinos. For more information on the context of the casino's opening, see Johnson, Kasarda, and Appold (2011). The gaming compact agreement between the State of North Carolina and the Eastern Band of Cherokee Indians was signed in August 1994.
[12] We could not use nationwide voter file vendors like Catalist, L2, or the Data Trust because of privacy and data security concerns from the owners of the GSMS data. Given that we only had

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.[13] Fortunately, however, the number of matching inputs available was balanced across the treatment and the control samples.[14]

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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.

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

[14] Tests for balance across the number of matching inputs available across the child cohorts and casino eligibility (our identification strategy) reveals balance (Cohort 1, p = 0.38; cohort 2, p=0.57). Still, as with any data match, this process comes with error. Fortunately, this approach avoids many 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;

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.[15] As we would expect given the (somewhat limited) evidence in other studies of transmission of votes (or non-votes) from one generation to the next, the bivariate correlation between parents voting and children's voting is high ($r=0.8$; $\beta=0.76$, $p<0.001$).[16] 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

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and we avoid the problems associated with the second by having exact date of births rather than age.

[15] 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.

[16] 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 ($r=0.3$; $\beta=0.22$, $p<0.001$), but one that is clearly muted by the sample composition.

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.

IV. 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 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 different durations of income transfers starting at different points in the life course.

This approach leverages the fact that for the youngest and middle cohorts, income transfers to their households began when individuals were younger than those in the third cohort. 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 may have a higher degree of susceptibility to intervention given the comparative malleability of attitudes, skills, and identities discussed earlier (and to which we return in the potential mechanisms section). Our hypothesis, informed by the HCMV, is that income transfers will have the largest effects on the youngest children in the survey. Our empirical analysis is designed to compare outcomes across age cohorts and race; this design is necessary since there is 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:

$$
\begin{aligned}
Y_i = \alpha_i + \beta_1 \times Youngest\_cohort_i + \beta_2 \times Middle\_cohort_i + \delta_1 \times American\_Indian_i \\
+ \gamma_1 \times Youngest\_cohort_i \times American\_Indian_i + \gamma_2 \times Middle\_cohort_i \\
\times American\_Indian_i + X'\theta + \varepsilon_i
\end{aligned} \qquad [1]
$$

Following previous practice (Holbein 2017; Sondheimer and Green 2010), in equation [1], we specify the outcome variable (Y_i) 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.[17] In equation [1], *Youngest_cohort* is an indicator variable for the child belonging to the youngest cohort (age 9 at intake), *Middle_cohort* is an indicator that the child belongs to the second youngest cohort (age 11 at intake). The variable *American_Indian* is a dummy equal to one for American Indian race and X is a set of baseline covariates that include parents' voter turnout rate before the casino opened, and gender. The omitted group is the third (oldest) cohort, so all coefficients are interpretable as differences from that cohort. The identification relies on differences between the three cohorts

[17] Whereas individuals typically only register once, they are free to vote multiple times. Hence, voting propensity is more conducive 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 person's mailing address when they are successfully registered to vote in North Carolina. 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.

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.

Given the high correlation of voting and household incomes (see Figure 1) and 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), we specifically examine whether there are heterogeneities across the initial household income distribution. We interact initial household income with the transfer treatment 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. In order to identify whether this is being driven by the upper or lower parts of the income distribution, we separate the analysis at the median of the initial household incomes. We then conduct our standard analyses for these two sub-groups for both the children and also on the parents. This type of analysis informs us on the importance of intergenerational transfers of voting probabilities and the role that household income plays in that behavior.

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 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 x After* 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 year fixed-effects to account for potentially different average voter turnout for Presidential versus Congressional-only elections (θ_t) and an error term.

$$Y_{it} = \alpha + \alpha_i + \gamma\ AmericanIndian\ x\ After_{it} + \delta AmericanIndian_i + \qquad [2]$$

$$\lambda After_t + \theta_t + \varepsilon_{it}$$

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.[18] In this figure we interact the treatment variable (receipt of the cash transfer) with a year dummy variable and plot the estimated coefficient in the figure. In the first three survey waves (1992, 1994 and 1996) serve as the pre-treatment observations. We find that parents eligible for the casino

[18] 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 program 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.

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 in place for these different groups 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 out 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 45 statistical tests run, only 4 show signs of imbalance—only marginally above what we would expect by random chance. Moreover, if we include these pre-treatment measures in our results, they do not affect our results. This indicates that the different age cohorts are appropriate controls for estimating the effect of the casino transfer.[19]

We estimate the parental models both with the entire sample and then separated by above and below the initial median household income as was done previously for the children. We also provide results from sensitivity analyses which weight the parent observations based on the uniqueness of their match in the North Carolina voting registration data. 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

[19] 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.

across our identifying variation.[20] This is not an issue for matching of the children, however, since the data for them 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].

V. Results

Va. 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 ordinary least squares and household fixed-effects regressions respectively. The dollar amounts are inflated 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. We run the following triple difference equation on household

[20] 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, to make sure that our results are not being biased by these multiple matches, we assign lower weights to those observations that have multiple matches using the inverse of the number of matches as 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.

income and plot the estimated coefficients for the lambda coefficients for all survey years that the child is a minor in the household.

$$\begin{aligned} Y_{it} = \alpha_0 + \beta_1 \times Youngest\_cohorts_i + \beta_2 \times After_t + \beta_3 \times American\_Indian_i + \gamma_1 \\ \times Youngest\_cohorts_i \times After_t + \gamma_2 \times Youngest\_cohorts_i \\ \times American\_Indian_i \\ + \sum_{t=1}^{T} \lambda_t \times Youngest\_cohorts_i \times American\_Indian_i \times Year_t + X'\theta + \varepsilon_i \end{aligned}$$

[3]

[Figure 2 here]

[Table 2 here]

Vb. 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].[21] 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 in 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), which measure whether children ever voted in a State or Federal election and the proportion of elections that they voted, respectively.[22]

[21] Our final analysis sample is around 1,300 individuals due to missing baseline characteristics.
[22] 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

The estimated difference-in-difference coefficients in the two pooled regression equations are both positive but they are not statistically significant. Given the strong income gradient found in both national and the GSMS parental data (Figures 1 and 2) 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. The regressions include income, all relevant double interactions, and the triple interactions with cohort and American Indian race; the estimated coefficient on the initial household income variable indicates that having $5,000 more in initial household income increases the probability of voting by 17 percentage points on average. The interaction effects in rows one and two are now larger and statistically significant. In rows 4 and 5 we present the triple interaction coefficients. The estimated coefficients are negative and statistically significant. These negative coefficients indicate a child from an otherwise similar household at the outset but who has $5,000 lower household income would realize about a 5.7 percentage point increase in having ever voted over the 2002-2014 election cycles than a child from the older cohort.

It is not immediately clear how to interpret the heterogeneity in outcomes across the initial income distribution—do high income individuals vote less or vice versa? To aide in interpreting these results, Panel B separates the observations by those 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 indicate that a low-income child who is exposed to

years. Tribal council members hold terms for two years while the Principal and Vice Chief hold office for four years. Elections are held on the first Thursday in September.

exogenously higher incomes during adolescence has about 8-20 percentage point increase in their likelihood of voting (depending on how voting is measured and which cohort we use as the treatment group) as compared to the control group.

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 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 household's 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.[23]

Figure 3 provides a graphical depiction of these results. This analysis is based on equation [3], however, we have no pre-treatment outcomes as the children were not eligible to vote in that time period. In the top panel, we create a dummy variable equal to one if the child belongs to the combined first and second age cohorts with election year and race and plot those estimated coefficients for observations below the baseline median household income; note that the first two cohorts of American Indian children are the ones that are treated to increased household incomes due to the casino transfer payment and the third age cohort is the control group. The effect of casino transfers in young adulthood (older than 18 years old) 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 of

[23] 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.

earlier cash is smaller and not statistically significant.[24]

These results are remarkably robust to various alternative specifications. For example, they hold whether we re-specify our results using baseline poverty status, rather than median income levels (available upon request). In Appendix Table A6 we conduct a difference-in-difference analysis where we combine the youngest two age cohorts and compare them to the oldest age cohort. Our results largely mirror the results found in Table 3.

[Table 3 here: Children's Voting Outcomes]

[Figure 3 here: Child Event Analysis]

These results are consistent with the expectations that we laid out in the HCMV above. 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.

Vc. Parent's 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 as we have reliable voter participation data starting in 1992. Given this additional flexibility, we examine the impact

[24] 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.

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 3 percentage points (a small—modest effect based on the voting literature). Given the heterogeneity in the effect on children's voting by initial household income, we run separate analyses below and above median household income in columns 2 and 3. There is no large or statistically significant effects on parental voting probabilities in either the total data or the data separated by initial household income. 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. (Table A4 provides the regression results used for this figure.) Our regression is similar to equation [3] for parent's voting as the outcome variable. We plot in Figure 4 the estimated coefficients from the triple-difference coefficients in that equation. The figure provides similar conclusions found in Table 4 for the parents voting probabilities. Regardless of how we specify the model, unconditional cash transfers have no effect on parents' voting levels.

[Figure 4 here: Parental Event Analysis]

[Table 4 here: Parental Voting by Income Status and FEs]

These results are consistent with the expectations laid out by the HCMV. They suggest that voting preferences are set earlier in life and are thus unchanged in later adult years. This implies that efforts to change woefully low, unequal, and (by some accounts) declining rates of civic participation in adulthood are not likely to succeed.

Vd. Robustness Checks

The results just presented are remarkably robust to alternate specifications. For example, in Appendix Table 7 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 differentially exposed by cohort were more likely to vote. This allows us to explore whether the parents of a single cohort had greater voting preferences and were thus 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. This helps us to (in part) explore potential mechanisms driving our effect among children.

We present this difference-in-difference in Appendix Table 7. Figure A3 provides the event analysis for these same results and Appendix Tables 8 and 9 provide the regressions used in the figure. Here 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.

We also conduct a robustness analysis where we compare the voting probability of the three age cohorts in the first election where they are all eligible to vote. We regress the probability of voting in the 2002 US Congressional elections on whether or not the child received the additional household income in childhood. Our results, shown in Appendix Table 10, are very similar to our main results—with income transfers increasing the turnout of low income individuals substantially.

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 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.

VI. Potential Mechanisms

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 disadvantaged childhood recipients. These individuals could have seen higher levels of human capital (education, cognitive ability, non-cognitive ability) that acted as additional resources down the road that encouraged them to vote. These indirect channels are explicitly allowed under the human capital model of voting. Put differently, if individuals voted because income increased educational attainment, personality, health, cognitive ability, non-cognitive ability, or some other unlisted mediator, this would be part of our story, rather than a threat to identification. That is, as long as our instrument is, indeed, a positive income shock, the spillover to effect other capacities is fine—it's part of our theoretical 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 and 2.) to know whether unobserved mediators are actually doing the heavy lifting.

For these reasons, documenting exact mechanisms is hard (if not impossible). This is especially the case with the casino transfer as we know from previous research that this was a bundled treatment that had multiple effects on multiple outcomes potentially related to voting. We are very sensitive to this fact and for this reason leave discussion of potential causal mechanisms to future studies better designed to elicit these. Identifying the exact causal pathways is not our primary purpose: here we focus on the first-order question of whether unconditional income transfers affect turnout and who these affect across generations. That being said, we can provide one piece of *very suggestive* evidence as to one of the potential mechanisms driving the effects we observe.

Given previous research on the GSMS, the only potential mediator that follows the pattern we observe—of effects among children who are low socioeconomic status, but not others—is educational attainment. 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. We think this has a high degree of face validity as a potential mechanism; those who were 13 (cohort 1) and 15 (cohort 2) when the cash transfers began still had the opportunity to make decisions about how long they would stay in high school, whereas those who were 17 (cohort 3) had already made their decisions about how long they would stay in high school. Given the strong role education plays in voting[25]—and the absence of other similar patterned effects on health, non-cognitive ability, or crime rates—this suggests that part of the reason why unconditional cash transfers increased turnout among young people is that it increased their investment in schooling—thus linking them up with the skills, values, and experiences known to be linked to voting. Under this story, poor individuals who received the transfer when they were 13/15 (cohorts 1/2) had enough time to change their human capital investments, while individuals who were older (cohort 3, who were 17, and parents, who were even older) may have already made these educational decisions; thus, making the income transfer less efficacious. While education clearly may not explain the whole effect, it is likely an integral part of the story.

VII. 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.

[25] Campbell 2006; Dee 2004; Denny and Doyle 2008; Henderson 2014; Nie, Junn, and Stehlik-Barry 1996; Sondheimer and Green 2010; Verba, Scholzman, and Brady 1995; Wolfinger and Rosenstone 1980; Verba and Nie 1987

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—which allows for variation in the importance of resources across the life course—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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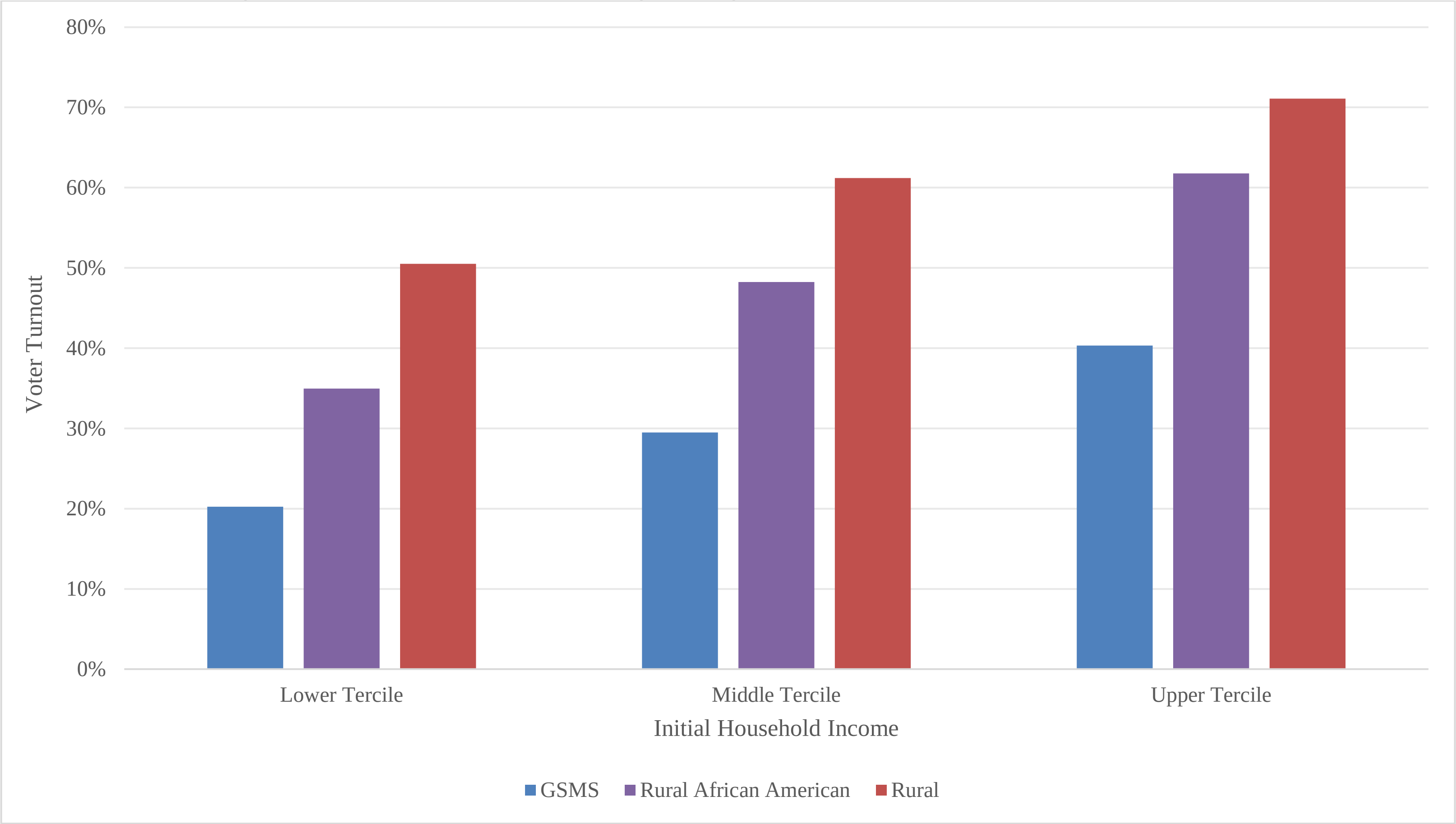
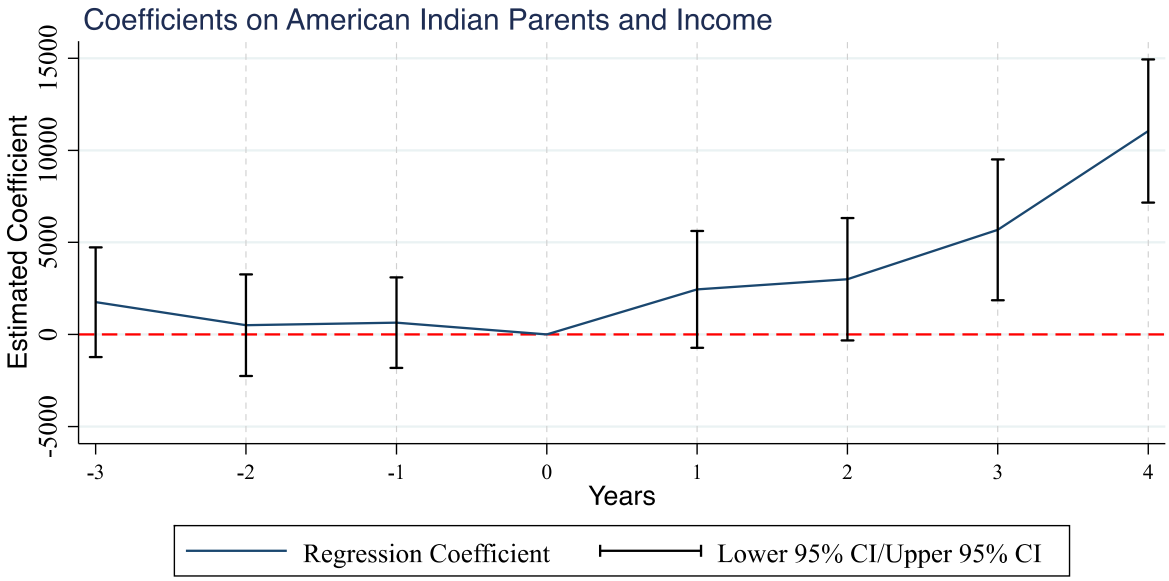
Figure 1. Income Gradient for Voting Average for Different Groups and Income Terciles



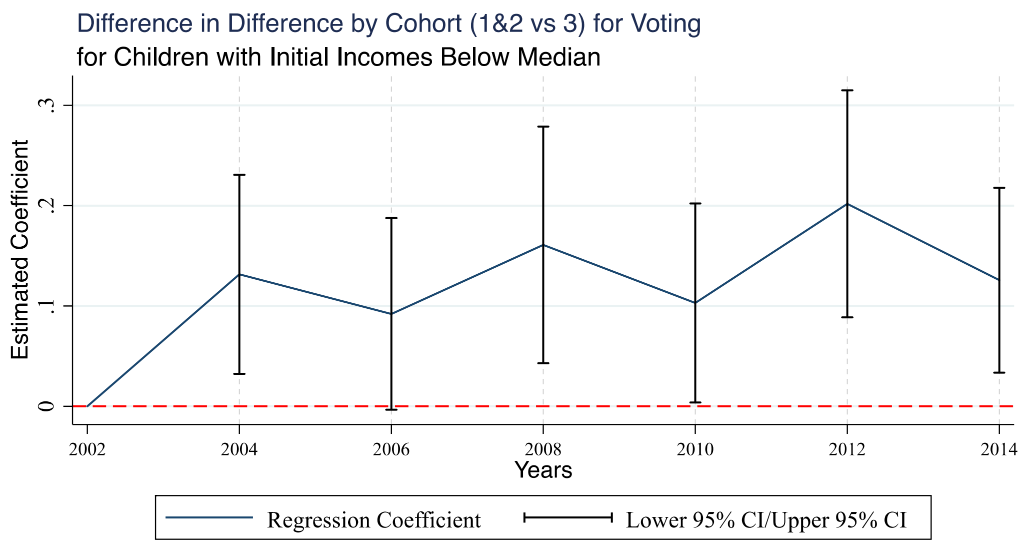
Notes: The data for the GSMS is restricted only to the subject parents for the years 1992-1996 (before the casino transfers began). Data for the other two groups are drawn from the American National Election Study (ANES) Time Series Cumulative Data File (1948-2012) In both sources, income measured using 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.

Figure 2. The Effects of Unconditional Transfers on Household Income around the Start of Casino Operations



Notes: Note: Receipt of Cash Transfer is the triple difference coefficient from our empirical specification. It is an interaction of race x age cohort x 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, 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).

Figure 3. The Effects of Casino Transfers on Child Voting by Initial Household Income Status around the Start of Casino Operations



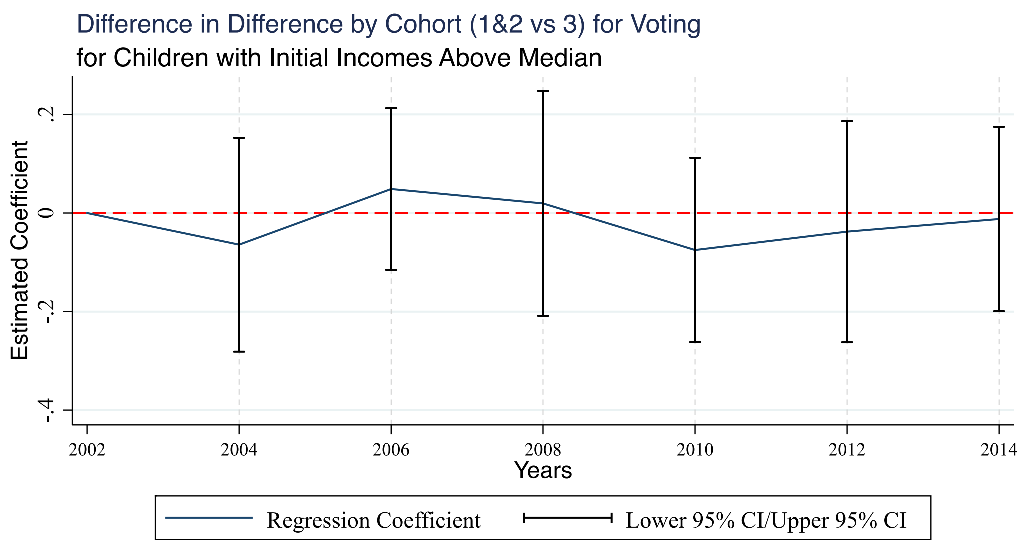
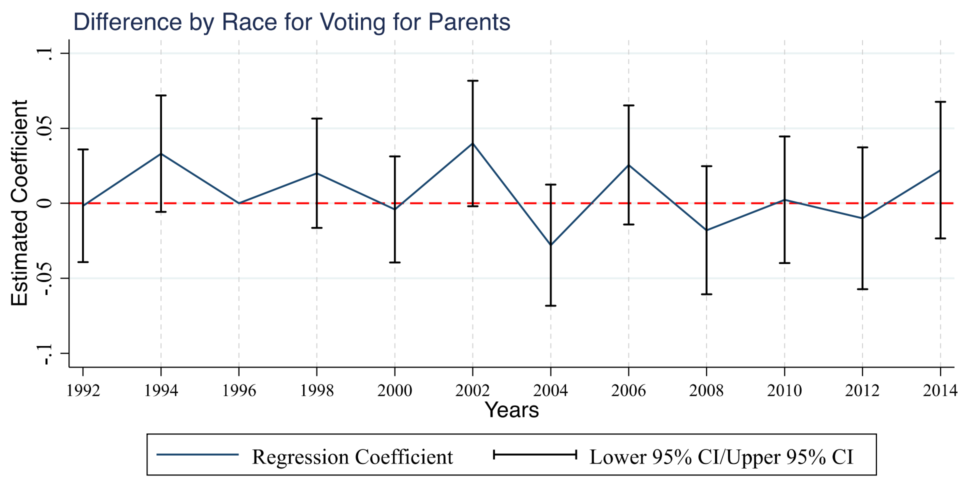
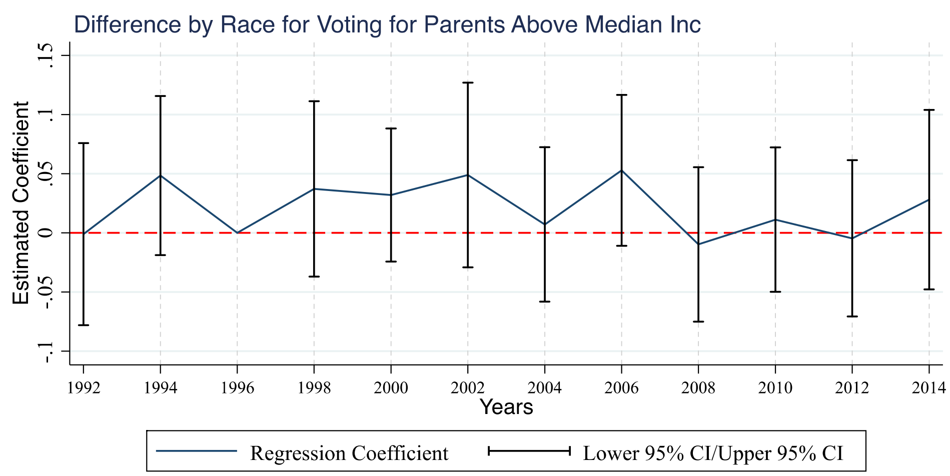


Figure 4. The Effects of Casino Transfers on Parental Voting by Initial Household Income Status around the Start of Casino Operations





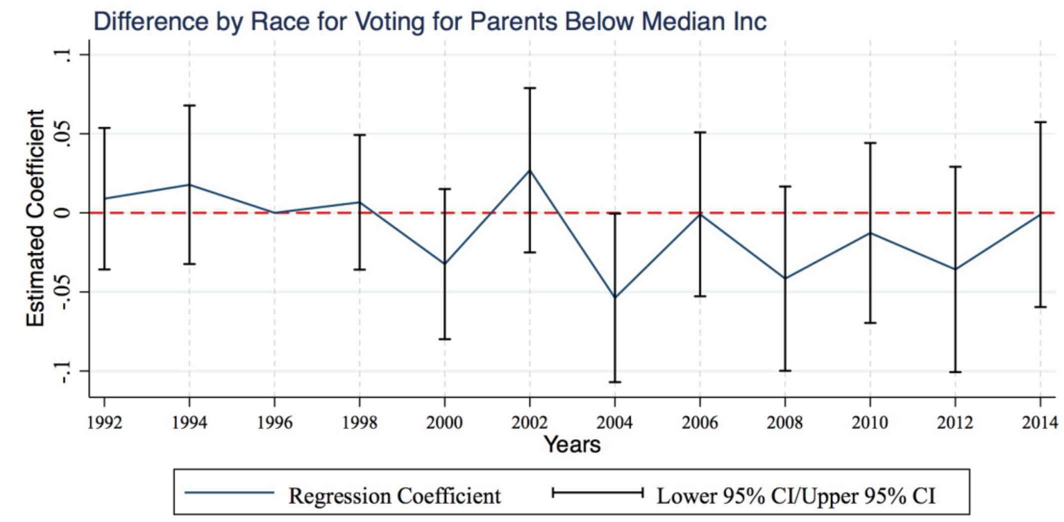

Difference by Race for Voting for Parents Below Median Inc
Estimated Coefficient
.1
.05
0
-.05
-.1
1992
1994
1996
1998
2000
2002
2004
2006
2008
2010
2012
2014
Years
Regression Coefficient
Lower 95% CI/Upper 95% CI

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

Variable	American Indian		Non-Indian		Test of Equality of Means		
	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: The number of observations for non-Indians ranges between 1028-1041 except for Mother and Father Employed Full Time which is 879 and 539 respectively. The number of observations for American Indians ranges between 292-297 except for Mother and Father Employed Full Time which is 270 and 165 respectively.

Table 2: First Stage Regression Using Individual Fixed Effects Regression for Household Income

VARIABLES	(1) Household Income in 2000 US $	(2) Household Income in 2000 US $	(3) Household Income in 2000 US $	(4) 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
# GSMS kids	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 x age cohort x 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, 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.

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

Panel A: Pooled and By Initial HH Income	Pooled		Pooled	
	(1)	(2)	(3)	(4)
Independent Variables	Ever Voted	Proportion Elections Voted	Ever Voted	Proportion Elections Voted
Interaction 1: Age Cohort 1 × Number of American Indian Parents	0.0415	0.0280	0.374***	0.211***
	(0.0530)	(0.0290)	(0.0954)	(0.0544)
Interaction 2: Age Cohort 2 × Number of American Indian Parents	0.0310	0.0140	0.240***	0.145***
	(0.0513)	(0.0260)	(0.0903)	(0.0466)
Parents Prior Voting	0.161***	0.106***	0.170***	0.110***
	(0.0418)	(0.0251)	(0.0417)	(0.0250)
Triple Interaction Cohort 1 (Age Group 1 x AI Parent x Initial Income)			-0.0573***	-0.0316***
			(0.0195)	(0.0114)
Triple Interaction Cohort 2 (Age Group 2 x AI Parent x Initial Income)			-0.0336*	-0.0218**
			(0.0172)	(0.00894)
Initial Household Income	0.0214***	0.0134***	0.170***	0.110***
	(0.00404)	(0.00228)	(0.0417)	(0.0250)
Mean of Dependent Variable	0.3273	0.1541	0.3273	0.1541
Observations	1,332	1,332	1,332	1,332
R-squared	0.051	0.063	0.064	0.074

Panel B: By Median HH Income	Below Median HH Income at Baseline		Above Median HH Income at Baseline	
	(1)	(2)	(3)	(4)
Independent Variables	Ever Voted	Proportion Elections Voted	Ever Voted	Proportion Elections Voted
Interaction 1: Age Cohort 1 × Number of American Indian Parents	0.197***	0.0915***	-0.0764	-0.00903
	(0.0529)	(0.0275)	(0.0987)	(0.0607)
Interaction 2: Age Cohort 2 × Number of American Indian Parents	0.166***	0.0766***	-0.0726	-0.0387
	(0.0520)	(0.0250)	(0.0917)	(0.0486)
Parents Prior Voting	0.119*	0.0574	0.184***	0.130***
	(0.0655)	(0.0372)	(0.0536)	(0.0327)
Mean of Dependent 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 American Indian indicator, number of American Indian parents, gender of the child, average household income prior to casino operation, age cohort indicator variables, age of the child and a constant. Robust standard errors employed, but the significance thresholds remain the same if we cluster by family or use the small-N clusters approach shown by Cameron, Gelbach, Miller (2008): available upon request.

Table 4: The Effect of Casino Transfer on Parents' Voter Turnout (Probability of Voting) by Race and After

	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)
Independent Variables	Voted	Voted	Voted	Voted	Voted	Voted
Receipt of Cash Transfer	-0.00492	-0.0250	0.00673	-0.00501	-0.0203	0.000564
	(0.0148)	(0.0201)	(0.0217)	(0.0147)	(0.0201)	(0.0207)
Initial Household Income	0.0275***			0.0312***		
	(0.00349)			(0.00343)		
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 Regressions?	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	0.059	0.044	0.104	0.052	0.043

Notes: *** p < 0.01, ** p < 0.05, *p < 0.10. Models include age fixed effects, year fixed effects, race by year, race by age group effects and a constant. Weights in columns 4, 5 and 6 are probability weights that are inversely related to the number of parental duplicate matches that were found in our data search.

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

[Not to be included in printed versions]

I. More Details on 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 other 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 theoretically 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 required for voting and socializes people towards the norm of voting. If the norms, skills, and attitudes required to engage in politics lock-in at a certain point in the life course, then later investments may have trouble moving 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—the period during which we observe the children we study below—may be especially critical, as young people are making decisions about their future—e.g. 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. Resources bestowed earlier in the life course may be more likely to socialize young people to the norm of voting. 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 (i.e. the so-called voting as a habit effect) (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 (i.e. information) when one is eligible to vote and elections are close.

In this paper, we are able to explicitly test these two broader competing models of voting by exploring the impact of exogenous income transfers across two generations—with the older generations receiving these investments in adulthood and the second generation receiving these in late childhood.[26] We further unpack this by exploring the effects of income transfers across cohorts within eligible children—leveraging variation in when the children's households begin receiving income in the life course as our primary identification strategy for child recipients.

[26] The HCMV explicitly allows for diminishing returns in resource investments as predicted by Wolfinger and Rosenstone (1980).

II. Match Diagnostics

To ensure that our results are unbiased by the match, what we want to know is whether match quality is balanced across our "treatment" and "control" conditions (i.e. those exposed to the casino transfer and those not). However, match quality is an amorphous and unobservable construct impossible to measure. Given that our outcome is strongly related to one's presence in the voter files, simply comparing match rates across our "treatment" and "control" conditions is not informative of match quality. It's possible that income makes it more likely that a person would register to vote, as well as actually vote.[27] Hence, we cannot infer very much about biased matching based on differential match rates. Simply put, there is no direct way to check whether match quality varies in ways that would bias our results. Still, we can run some informative checks suggested in the literature that get us close to seeing whether differential matching errors are biasing our results.

These checks revolve around comparing variation in the inputs of match quality. While match quality itself is hard to observe, we know what inputs are likely to influence this construct.[28] For example, it is not hard to imagine that if someone has died, married (and changed their name), moved, is missing a current address, or is not responding to requests to be surveyed, that person is much more difficult to match successfully to voter records. Fortunately, these inputs of match quality are observable. Comparing whether these observable inputs vary across our treatment and control groups gives us a view into whether matching errors are biasing our results. While none of these alone proves that match quality is equal across our treatment and control groups, together they provide assurance that matching to voter files is not biasing our results.

As we outline in greater detail in the Methods section below, our identification strategy leverages two differences: first, across individuals eligible (American Indians—AI) to receive unconditional cash transfers and those who ineligible to do so (non-American Indians) and second, across cohorts that were younger (those in Cohorts 1 and 2) when they were exposed to transfers, and hence were exposed to more transfers over their life course than those who were older when the transfers begin (those in Cohort 3). This approach results in two difference-in-difference coefficients: one for American Indians in Cohort 1 and the other for American Indians in Cohort 2. Estimating this difference-in-difference specification on the inputs of match quality allows us to see if match quality is likely to bias our results.

Across our two treatment groups, individuals are balanced in their likelihood of displaying several characteristics. In Table A1, we present the results on the differences in matching characteristics across race and cohort groups.

[27] We use voting, rather than registration, as our outcome of interest as it is more conducive to our panel methods we use, given that individuals (typically) only register once but have the opportunity to vote many times. This allows us to reduce residual variance in our voting scales and to leverage individual fixed effect models (with parents), which require temporal variation in our outcome over multiple periods (something registration doesn't do).

[28] We note that some approaches construct measures of matching quality. However, these, by and large, use the exact inputs we check below.

Simply put, it appears that our key identification strategy produces groups that equal in the quality of matching inputs. This makes it very unlikely that the match itself biases the results we present below.

Appendix Table 1: Differences in Characteristics Affecting Matching Rates for Parents

	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	Pooled	Below Median HH Income at Baseline	Above Median HH Income at Baseline
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Moved Out of North Carolina	Moved Out of North Carolina	Moved Out of North Carolina	Ever Changed Last Name	Ever Changed Last Name	Ever Changed Last Name	Found In Voter Data Bases	Found In Voter Data Bases	Found In Voter Data Bases
Interaction 1: Age Cohort 1 × Number of American Indian Parents	0.00173	-0.0279	0.0822	0.0139	-0.0220	0.0604	0.0254	0.0415	-0.00446
	(0.0312)	(0.0438)	(0.0573)	(0.0285)	(0.0420)	(0.0436)	(0.0513)	(0.0621)	(0.0918)
Interaction 2: Age Cohort 2 × Number of American Indian Parents	0.0168	0.0160	0.00430	0.0571*	0.0417	0.0693*	0.0315	0.0111	0.0735
	(0.0346)	(0.0532)	(0.0362)	(0.0299)	(0.0452)	(0.0380)	(0.0504)	(0.0606)	(0.0792)
Observations	1,328	648	680	1,332	651	681	1,351	651	681
R-squared	0.016	0.038	0.022	0.011	0.015	0.014	0.005	0.004	0.004

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Moreover, as another check of the quality of our match, we can assess whether match patterns and baseline voting (of parents before the casino opened) follow well-known patterns in registering/voting from other samples. When we look at match rates and baseline turnout by socioeconomic status—comparing those not in poverty at baseline to those that were in poverty at baseline—we can see that less affluent individuals were 14.1 percentage points less likely to register to vote ($p<0.001$) and had voter turnout at baseline that was 17.9 percentage points lower ($p<0.179$) than their more affluent counterparts. This is consistent with broader patterns in income bias in voting we discussed earlier. Further, match rates and baseline voting propensities vary as we would expect by race/ethnicity, given the extensive literature on racial gaps in registration and voting (Leighley and Nagler 2013; Wolfinger and Rosenstone 1980; Verba and Nie 1987;). Similarly, we can do a simple comparison of voter turnout rates across children and parents. Though these matches were done independently, parents and kids voting rates are highly correlated ($r=0.75$). That is, according to our match, children who voted were likely to have parents who voted—a result consistent with political socialization research (Plutzer 2002). This provides some additional reassurance that our match was identifying people correctly and that match quality is similar across generations.

One might be concerned that periodic state purges of registered voters from the voter lists might bias our results. Fortunately, we can check and see if purges from the voter file bias our results. To do so, we compare voter records marked "inactive" across treatment and control group in our difference-in-difference specifications. Being labeled "inactive" is the first step in purging individuals; as such, it serves as a proxy to see if purges are biasing our results. When we run our difference-in-difference models with inactive status as our dependent variable, we can see that there is balance across our identifying variation (AI * Cohort 1: $p = 0.886$; AI * Cohort 2: $p = 0.405$). This suggests that purges from the voter records are unlikely to bias our results.

Finally, we note that unlike the children—for whom, given the thoroughness of matching inputs, there were no duplicate matches—the parents' data does have some duplicate matches. This arises because the parents' data are sometimes missing date of birth.[29] While duplicates are somewhat undesirable, this actually offers us another way to explore whether match quality varies across our identifying variation. If we make the assumption that the number of duplicate matches is strongly correlated with match quality (a reasonable assumption in our view), examining the number of duplicates across our treatment and control groups gives us a powerful check of the findings' robustness to matching error. When we conduct this check, we can very clearly see that the number of duplicates is balanced for both the first cohort (AI * Cohort 1: $p = 0.225$; AI * Cohort 2: $p = 0.250$) and second cohort parents (AI * Cohort 1: $p = 0.691$; AI * Cohort 2: $p = 0.065$). This provides us with additional evidence that match quality is not biasing our results.

Among parent match duplicates, it is inherently hard to distinguish which match is correct. As a result, in our results below we average voter turnout among parent matches. We are also able to mitigate any potential problem this may present in several ways. First, we can run our models just among parents with one or no matches—based on the assumption that these are matched with a higher degree of precision. Second, we can assign lower weights those observations that have

[29] For these individuals, we supplement the match with their county of residence to narrow down the search.

multiple matches using the inverse of the number of matches as weights. Intuitively, this approach places less emphasis on observations that have many matches, and, thus, less certainty of whether the match is right. When we conduct these checks, the results don't change.[30]

Given balanced attrition, movement rates, quality of matching inputs, duplicates, and our performance on the other match diagnostics we perform here, it seems highly likely that the match itself is unlikely to bias the results outlined below.

[30] We do not present these results as our main effects given the difficulties associated with including fixed effects and weights in the same model as a means of estimating causal effects (Solon, Haider, and Wooldridge 2015). Fortunately, in our application, these weights do little to change our effect estimates.

III. Appendix Tables

Appendix Table 2: Mean Differences by Age Cohort and American Indian Parent Status at Survey Wave 1

Non-American Indian Cohorts

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	0.270	0.027	0.033
Father Has a High School Degree/GED	0.184	0.184	0.000	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	0.070	0.038
Mother Has a High School Degree/GED	0.270	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	497.290	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	29367.90	32154.88	-2786.9*	1364.668

Note: * indicates significance at the 5% level, ** indicates significance at the 1% level.

American Indian Cohorts

Differences Between Cohort 1 and Cohort 2	Cohort 1 Mean	Cohort 2 Mean	Difference	SE of Difference
Number of American Indian Parents	1.355	1.387	-0.032	0.066
American Indian Indicator	0.927	0.981	-0.054	0.028
Male Child Indicator	0.509	0.547	-0.038	0.068
Mother Has a High School Degree/GED	0.400	0.330	0.070	0.066
Father Has a High School Degree/GED	0.218	0.160	0.058	0.053
Mother Has More than a High School Degree	0.373	0.415	-0.042	0.067
Father Has More than a High School Degree	0.218	0.236	-0.018	0.057
Initial Household Income	21952.38	21212.12	740.260	2179.163

Differences Between Cohort 2 and Cohort 3

	Cohort 2 Mean	Cohort 3 Mean	Difference	SE of Difference
Number of American Indian Parents	1.387	1.296	0.090	0.070
American Indian Indicator	0.981	0.926	0.055	0.030
Male Child Indicator	0.547	0.543	0.004	0.074
Mother Has a High School Degree/GED	0.330	0.333	-0.003	0.070
Father Has a High School Degree/GED	0.160	0.259	-0.099	0.059
Mother Has More than a High School Degree	0.415	0.383	0.032	0.073
Father Has More than a High School Degree	0.236	0.198	0.038	0.061
Initial Household Income	21212.12	25000.00	-3787.880	2373.339

Differences Between Cohort 1 and Cohort 3

	Cohort 1 Mean	Cohort 3 Mean	Difference	SE of Difference
Number of American Indian Parents	1.355	1.296	0.058	0.069
American Indian Indicator	0.927	0.926	0.001	0.038
Male Child Indicator	0.509	0.543	-0.034	0.073
Mother Has a High School Degree/GED	0.400	0.333	0.067	0.071
Father Has a High School Degree/GED	0.218	0.259	-0.041	0.062
Mother Has More than a High School Degree	0.373	0.383	-0.010	0.071
Father Has More than a High School Degree	0.218	0.198	0.021	0.060
Initial Household Income	21952.38	25000.00	-3047.620	2366.745

Note: * indicates significance at the 5% level, ** indicates significance at the 1% level.

Appendix Table 3: Comparison of Characteristics with other American Indian Tribes and Relevant Demographic Groups

	1990 Census Report on American Indians	Social Explorer		IPUMS 1990				
	Eastern Cherokee (reservation)	All 11 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%
Percent of Age 25+ with a high school degree	70%	69%		69%	64%	53%	79%	75%
Unemployment Rate	12%*	6%		15%	18%	12%	6%	6%
Voter Turnout Rate	NA	NA		41.98%	33.7%	39.6%	62.5%	58.8%
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

Source: Social Explorer, 1990 County Data. IPUMS 1990, 1% Sample. Voter turnout comes from the ANES.

Appendix Table 4: Correlation of Education and Voting

Rural:	0.2153
Rural African American:	0.1801
GSMS	0.2014

Note: This is a simple correlation of educational attainment with voting for the Adults in the GSMS with data from ANES. Education Categories: 1 Less than HS degree; 2 HS degree or equivalent; 3 Post HS education / some college; 4 BA degree; 5 More than a BA degree.

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

	(1)	(2)	(3)	(4)	(5)
		Below Median HH Income at Baseline	Above Median HH Income at Baseline	Below Median HH Income at Baseline	Above Median HH Income at Baseline
VARIABLES	Voted	Voted	Voted	Voted	Voted
Interaction 1: Age Cohort 1 × Number of AI Parents	0.0440	0.131***	-0.0268		
	(0.0400)	(0.0409)	(0.0857)		
Interaction 2: Age Cohort 2 × Number of AI Parents	0.0452	0.124***	-0.0253		
	(0.0395)	(0.0401)	(0.0802)		
Interaction 1: Age Group × Number of AI Parents x 2002				Omitted Category	Omitted Category
Interaction 2: Age Group × Number of AI Parents x 2004				0.0921*	-0.0644
				(0.0487)	(0.110)
Interaction 3: Age Group × Number of AI Parents x 2006				0.161***	0.0487
				(0.0601)	(0.0835)
Interaction 4: Age Group × Number of AI Parents x 2008				0.103**	0.0193
				(0.0505)	(0.116)
Interaction 5: Age Group × Number of AI Parents x 2010				0.202***	-0.0750
				(0.0576)	(0.0952)
Interaction 6: Age Group × Number of AI Parents x 2012				0.126***	-0.0380
				(0.0469)	(0.114)
Interaction 7: Age Group × Number of AI Parents x 2014				0.126***	-0.0124
				(0.0469)	(0.0953)
Parents Prior Voting	0.108***	0.0660*	0.139***	0.0652*	0.140***
	(0.0250)	-0.0376	-0.033	(0.0377)	(0.0330)
Initial Household Income	0.0140***				
	(0.00227)				
Observations	9,324	4,557	4,767	4,557	4,767
R-squared	0.064	0.040	0.054	0.043	0.056

Notes: Columns 1, 2 and 3 include Age cohort dummy variables, Number of American Indian parent controls, year fixed effects and a constant. Columns 5 and 6 include the same controls and provide interaction effects where the youngest two age cohorts are combined and compared to the oldest age cohort. Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 6: Children's Voting Probability by Combined Cohorts (1 and 2) Relative to Cohort 3

	Pooled	Pooled	Below Median HH Income at Baseline		Above Median HH Income at Baseline	
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Ever Voted	Proportion Elections Voted	Ever Voted	Proportion Elections Voted	Ever Voted	Proportion Elections Voted
Interaction 1: Age (Cohort 1 or Cohort 2)× Number of AI Parents	0.0364	0.0210	0.182***	0.0849***	-0.0774	-0.0268
	(0.0470)	(0.0246)	(0.0457)	(0.0235)	(0.0832)	(0.0467)
Parent Prior Voting	0.161***	0.106***	0.124*	0.0599	0.193***	0.134***
	(0.0418)	(0.0251)	(0.0657)	(0.0376)	(0.0533)	(0.0326)
Observations	1,332	1,332	651	651	681	681
R-squared	0.051	0.063	0.042	0.033	0.029	0.038

Notes: Regressions include number of American Indian parents, age cohort control, initial average household income (in columns 1 and 2 only), gender of child, age of child and a constant. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 7: The Effect of Casino Transfer on Parents' Voter Turnout (Probability of Voting)

Panel A: Unweighted	Pooled	Below Median HH Income at Baseline	Above Median HH Income at Baseline	Unweighted	Pooled	Below Median HH Income at Baseline	Above Median HH Income at Baseline
	(1)	(2)	(3)		(4)	(5)	(6)
Independent Variables	Voted	Voted	Voted		Voted	Voted	Voted
Age cohort 1 X Native American	-0.0447	-0.125	0.0595	Age cohort 1 or 2 X Native American	-0.00653	-0.0235	0.00110
	(0.0631)	(0.0787)	(0.114)		(0.0331)	(0.0464)	(0.0465)
Age cohort 2 X Native American	-0.0132	-0.0509	-0.0249				
	(0.0664)	(0.0851)	(0.114)				
Year FE	Y	Y	Y	Year FE	Y	Y	Y
N (families)	15,984	7,812	8,172	N (families)	15,984	7,812	8,172
R-squared	0.097	0.061	0.045	R-squared	0.097	0.059	0.044

Panel B: Weighted	Pooled	Below Median HH Income at Baseline	Above Median HH Income at Baseline	Weighted	Pooled	Below Median HH Income at Baseline	Above Median HH Income at Baseline
	(1)	(2)	(3)		(4)	(5)	(6)
Independent Variables	Voted	Voted	Voted		Voted	Voted	Voted
Age cohort 1 X Native American	-0.00813	-0.0707	0.0565	Age cohort 1 or 2 X Native American	-0.00480	-0.0173	-0.00696
	(0.0580)	(0.0683)	(0.110)		(0.0333)	(0.0465)	(0.0462)
Age cohort 2 X Native American	-0.0125	-0.0430	-0.0428				
	(0.0601)	(0.0728)	(0.107)				
Year FE	Y	Y	Y	Year FE	Y	Y	Y
N (families)	15,984	7,812	8,172	N (families)	15,984	7,812	8,172
R-squared	0.104	0.052	0.043	R-squared	0.104	0.052	0.043

Notes: All regressions include a binary for after the casino payments, age cohort controls, number of Native American parents, race by year effects, race by age group effects, year by age group effects initial household income (in columns 1 and 4 only) and a control. *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the household level.

Appendix Table 8: Parents Event Analysis Regression Tables

	Pooled	Below Median HH Income at Baseline	Above Median HH Income at Baseline
	(1)	(2)	(3)
VARIABLES	Voted	Voted	Voted
Interaction 1: Number of AI Parents x 1992	-0.00164	0.00895	-0.00110
	(0.0192)	(0.0228)	(0.0392)
Interaction 2: Number of AI Parents x 1994	0.0331*	0.0177	0.0485
	(0.0198)	(0.0255)	(0.0342)
Interaction 3: Number of AI Parents x 1996	Omitted Category	Omitted Category	Omitted Category
Interaction 4: Number of AI Parents x 1998	0.0200	0.00667	0.0372
	(0.0185)	(0.0217)	(0.0378)
Interaction 5: Number of AI Parents x 2000	-0.00405	-0.0324	0.0319
	(0.0180)	(0.0242)	(0.0286)
Interaction 6: Number of AI Parents x 2002	0.0399*	0.0269	0.0489
	(0.0213)	(0.0265)	(0.0397)
Interaction 7: Number of AI Parents x 2004	-0.0279	-0.0538**	0.00712
	(0.0206)	(0.0271)	(0.0332)
Interaction 8: Number of AI Parents x 2006	0.0256	-0.000942	0.0528
	(0.0202)	(0.0264)	(0.0324)
Interaction 9: Number of AI Parents x 20008	-0.0179	-0.0416	-0.00977
	(0.0218)	(0.0297)	(0.0332)
Interaction 10: Number of AI Parents x 2010	0.00232	-0.0127	0.0112
	(0.0215)	(0.0290)	(0.0311)
Interaction 11: Number of AI Parents x 2012	-0.00997	-0.0358	-0.00471
	(0.0241)	(0.0331)	(0.0336)
Interaction 12: Number of AI Parents 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: Regressions include year fixed effects, number of American Indian parents, age group controls, initial household income (for column 1 only) and a constant. Standard errors clustered at the household level.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 9: Parents Event Analysis Regression Tables by Age Combined Age Cohorts

	Pooled	Below Median HH Income at Baseline	Above Median HH Income at Baseline
VARIABLES	(1) Voted	(2) Voted	(3) Voted
Interaction 1: Age Group × Number of AI Parents x 1992	-0.000278	0.0856	-0.114
	(0.0485)	(0.0582)	(0.0872)
Interaction 2: Age Group × Number of AI Parents x 1994	0.0780*	0.0976*	0.0881
	(0.0410)	(0.0533)	(0.0695)
Interaction 3: Age Group × Number of AI Parents x 1996	Omitted Category	Omitted Category	Omitted Category
Interaction 4: Age Group × Number of AI Parents x 1998	0.0295	0.0924*	-0.0452
	(0.0458)	(0.0473)	(0.0921)
Interaction 5: Age Group × Number of AI Parents x 2000	0.0126	0.0461	-0.0103
	(0.0408)	(0.0566)	(0.0625)
Interaction 6: Age Group × Number of AI Parents x 2002	0.0344	0.0673	-0.0251
	(0.0475)	(0.0586)	(0.0881)
Interaction 7: Age Group × Number of AI Parents x 2004	-0.00513	0.00776	-0.0198
	(0.0475)	(0.0625)	(0.0748)
Interaction 8: Age Group × Number of AI Parents x 2006	0.0551	0.115*	-0.0193
	(0.0440)	(0.0589)	(0.0679)
Interaction 9: Age Group × Number of AI Parents x 20008	0.00508	-0.00639	-0.00503
	(0.0482)	(0.0654)	(0.0748)
Interaction 10: Age Group × Number of AI Parents x 2010	0.0493	0.0605	0.0351
	(0.0476)	(0.0695)	(0.0605)
Interaction 11: Age Group × Number of AI Parents x 2012	-0.0263	-0.0477	-0.0134
	(0.0593)	(0.0863)	(0.0753)
Interaction 12: Age Group × Number of AI Parents x 2014	0.0201	0.00383	0.0361
	(0.0554)	(0.0752)	(0.0827)
Observations	15,984	7,812	8,172
R-squared	0.097	0.060	0.045

Notes: Regressions include year fixed effects, number of American Indian parents, age group controls, initial household income (for column 1 only) and a constant. Standard errors clustered at the household level.
*** p<0.01, ** p<0.05, * p<0.1

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

	(1)	(2)	(3)	(4)	(5)	(6)
	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
VARIABLES	Voted at Age 21?	Voted at Age 21?	Voted at Age 21?	Voted in 2002 Election?	Voted in 2002 Election?	Voted in 2002 Election?
Casino Payment	-0.0206	0.0588**	-0.105	-0.00864	0.0470***	-0.0624
	(0.0366)	(0.0288)	(0.0745)	(0.0308)	(0.0176)	(0.0638)
Observations	864	424	440	1,332	651	681
R-squared	0.038	0.014	0.044	0.027	0.014	0.031

Notes: Regressions include number of American Indian parents, age cohort control, initial average household income (in columns 1 and 4 only), gender of child, age of child, parent's probability of voting and a constant. Standard errors clustered at the household level.

*** p<0.01, ** p<0.05, * p<0.1

Figure A1: Geographic Location of the GSMS Study Participants

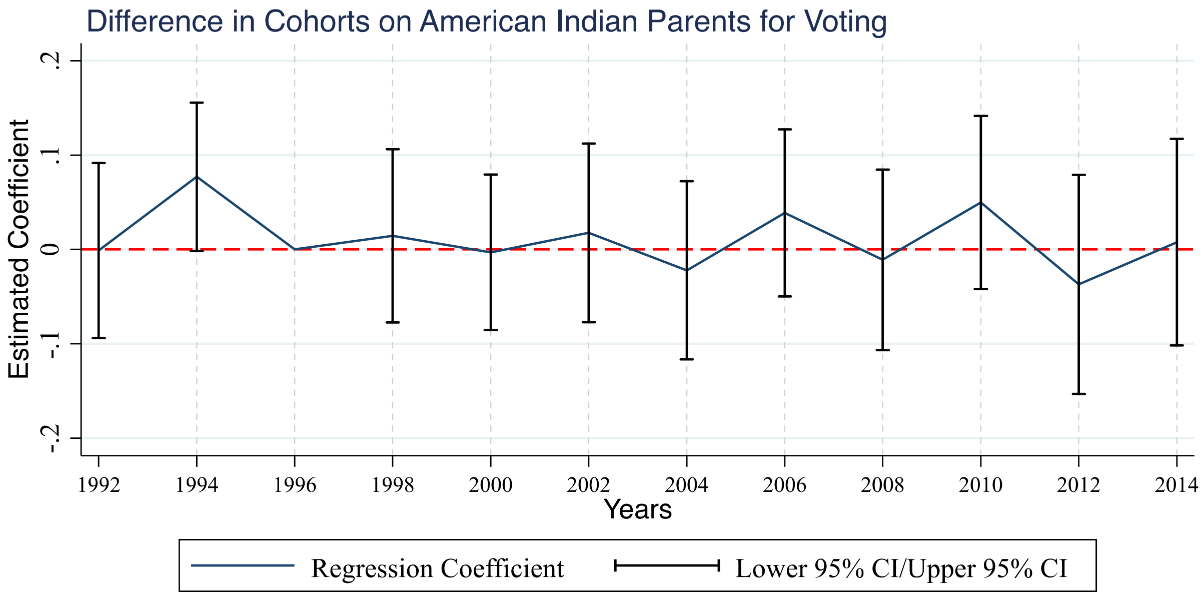
Note: Figure displays the counties included in the GSMS study. The Eastern Cherokee reservation—where the casino is located—is in Cherokee, NC (which is split between Swain and Jackson County, NC).

Figure A2: Design of Follow up Surveys of the GSMS

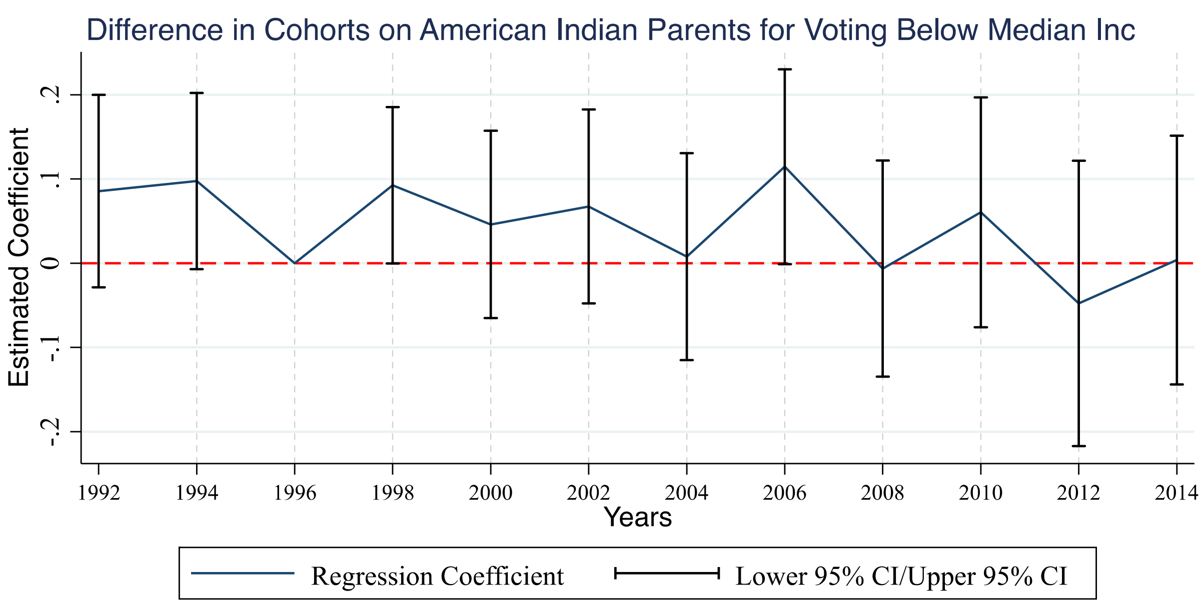
Wave	1	2	3	4		5	6	7	8	9	10	11	12	13	14	15	16	17
Age	1993	1994	1995	1996		1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009
9	C1				Casino Opening													
10		C1																
11	C2		C1															
12		C2		C1														
13	C3		C2															
14		C3		C2			C1											
15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

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

Figure A3



Difference in Cohorts on American Indian Parents for Voting
Estimated Coefficient
-.2
-.1
0
.1
.2
1992
1994
1996
1998
2000
2002
2004
2006
2008
2010
2012
2014
Years
Regression Coefficient
Lower 95% CI/Upper 95% CI



Difference in Cohorts on American Indian Parents for Voting Below Median Inc
Estimated Coefficient
-.2
-.1
0
.1
.2
1992
1994
1996
1998
2000
2002
2004
2006
2008
2010
2012
2014
Years
Regression Coefficient
Lower 95% CI/Upper 95% CI