Family Income and the Intergenerational Transmission of Civic Participation: Evidence from a Cash Transfer Program and Parent and Child Voting Behaviors [*]

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February 9, 2018

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 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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We wish to thank Gordon Keeler and Jurgen Henn for their assistance with accessing the GSMS data. Holbein wishes to thank the National Science Foundation for funding support (SES #1416816 and SES #1657821). We also wish to thank audiences at Tulane University, Brigham Young University, and the Association for Public Policy Analysis and Management (2017) for their invaluable feedback. We also wish to thank Kevin Collins for the encouragement to start this project.

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.[1] 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. 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[2]. 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.[3]

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

[1] Green and Gerber 2008; Leighley and Nagler 2013; Verba, Schlozman, and Brady 1995; Verba and Nie 1987; Wolfinger and Rosenstone 1980.
[2] Blais 2006; Frey 1971; Leighley and Nagler 2013; Smets and van Ham 2013; Verba, Schlozman, and Brady 1995; Verba and Nie 1987.
[3] 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.

income is the driving force behind these gaps, or instead they 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 political participation and whether the intergenerational transmission of political participation can be affected by changes in family income. To do so, we utilize a naturally occurring quasi-experiment. This approach leverages 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 (located in Cherokee, NC). 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.[4] 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 this transfer on the political participation of parents (who were adults when they began receiving this exogenous influx of family income) and their children (who were in late childhood when their parents began receiving transfers). These

[4] Children were eligible for the casino disbursements once they turned 18 if they completed high school. If they did not complete high school, they received transfers once they turned 21.

identification strategies build on research using the GSMS data, which shows that these casino transfers are, indeed, exogenously disbursed.

Matching GSMS parents and children to public-use voter files based on their identifying information, we find that annual unconditional cash transfers have some long-term civic benefits, but maybe less than some have imagined.[5] 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. However, unconditional income transfers have no effect on parents, regardless of their starting income levels. Adults' voting patterns appear to be locked-in: 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).

Our work makes several important contributions. Conceptually, our study helps answer the question of whether income contributes to underlying levels of participation. In so doing, it adds nuance to the foundational resource model of voting (RMV) (Verba, Schlozman, Brady 1995). Rather than income mattering universally—as the RMV might 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 income matter for those with the lowest levels of baseline resources. Put differently, voting resources face diminishing marginal returns. Further, voting resources appear to be more beneficial for children than for adults.

Second, given the intergenerational element to our analyses, our results also contribute to our understanding of political socialization. In seeking to understand why some people develop

[5] 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

into active citizens, while others do not, social scientists have tended to focus solely 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, 12). Though various theoretical models have postulated that resources allocated earlier in the life course may matter more than those delivered later (e.g. Plutzer 2002), little to no contemporary research has explored this possibility.[6,7] The HCMV that we put forth helps draw attention back on the life course dynamics associated with political participation. Our research provides compelling evidence that early life experiences—in this case, the receipt of additional income—have a greater effect on participation than the same experiences 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 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

[6] 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).

[7] 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."

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 helps to begin to close 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 taxation, welfare, minimum wages, to universal basic income (UBI). Our results suggest that unconditional income transfers may have broader effects than previously realized. Not only may these affect individuals' labor, health, and schooling outcomes (e.g. Akee et al. 2010 and 2013; Aguero, Carter, and Woolard 2006; Baird et al. 2012; Baird, De Hoop, and Özler 2013; Blattman, Fiala, and Martinez 2013; Cunha 2010; De Mel, McKenzie, and Woodruff 2012; Paxson and Shady 2010; Haushofer and Shapiro 2016), but also their 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 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 (Green, McGrath, and Aronow 2013), with some even making participatory gaps worse (Enos, Fowler, and Vavreck 2013). Our results suggest that unconditional income transfers can buck this trend.

The rest of the paper proceeds as follows. Section II provides background and our conceptual framework, including an articulation of our human capital model of voting. Section III outlines the GSMS dataset and its match to public-use voter records. Section IV lays out the methods we employ along with our identification assumptions and tests of these. Section V outlines our results. Section VI provides a discussion of likely mechanisms and Section VII concludes.

II. Background and Conceptual Framework

What drives people to participate in politics? Various theories have been put forth to answer this broader question. These include rational choice models—wherein citizens consider the various costs and benefits of voting in making their decision of whether to turn out (e.g. Downs 1957; Riker and Ordeshook 1968)—psychological models—wherein citizens' voting decisions are shaped by their internal motivational attachments (e.g. Campbell et al. 1960; Fiorina 1976)—and sociological models—wherein citizens voting decisions are shaped by their social networks (e.g. 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 (Cascio and Washington 2013; Corvalan and Cox 2013; Leighley and Nagler 2013; Wolfinger and Rosenstone 1980; Holbein and Hillygus 2016), locating and traveling to polling locations (Brady and McNulty 2011), waiting in line at the ballot box (Pettigrew 2016), navigating inclement weather on Election Day (Fujiwara, Meng, and Vogl 2016; Gomez, Hansford, and Krause 2007), and (hopefully) learning about the candidates and issues (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 (Sondheimer and Green 2010), health (Burden et al. 201), information (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), skills (Holbein 2017), time (Holbein and Schafer 2017), and income. 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.[8] Consistent with this view, Wolfinger

[8] 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).

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."[9] 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. forthcoming; Mondak 2010; Gerber et al. 2011).

Alternatively, some have argued that income increases the likelihood of voting by enhancing citizens' social status. Income may make it more likely that citizens are socialized to a norm of voting. For example, Wolfinger and Rosenstone (1980, 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 (Wolfinger and Rosentstone (1980, 21); Leighley and Nagler 2013; Verba and Nie 1987; Veba, Schlozman, and Brady 1995)—. That is, income may only matter up to a point. For those who are poor, income may matter a great deal for voting; for those who are well-off already, additional income may matter very little. While this prediction has some face validity and some observational empirical support, this theoretical prediction has yet to be fully tested.

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

[9] 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."

is a positive relationship between the two. Figure 1 shows the income gap in voting, using data from the American National Election Study (ANES).[10,11] Regardless of whether one uses data from survey self-reports of voting or from validated public use voter file data, the difference in the propensity to vote ranges between 19 and 23 percentage points between the bottom and top thirds ($p < 0.001$). To put that in perspective: the gap between white at non-white citizens—one that has drawn a substantial degree of interest and discussion over the period of study (Leighley and Nagler 2013)—ranges from about 12 to 17 percentage points over that time period ($p < 0.001$). People with higher incomes are substantially more likely to engage in the fundamental act of democracy. We term this gap the *income bias* in voting.

[Figure 1 here: National Data on Income and Voting Gradient]

While there is clearly income bias 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).[12]

[10] 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.
[11] The same pattern can be seen in the Census data from the CPS (1994-2012). Here, those in the top third report voting at a rate 14 percentage points higher than those in the lowest third ($p < 0.001$). The gap can also be seen in the Cooperative Congressional Election Study (CCES), where income bias is 11 percentage points between the top and bottom thirds ($p < 0.001$).
[12] 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.

Overall the research on the predictive power of income above and beyond observable controls is inconclusive. These mixed results leave us without a clear understanding of income's relative contribution for voting. Here we argue that these mixed findings occur, in large part, because of lack of good identification. Without a study with exogenous variation in income, it is hard to disentangle the role of income from the host of observed and unobserved factors that influence whether a person shows up to vote.

At present, little to no work of this type exists. Indeed, 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: 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 per se has an effect on voter turnout. On a very basic level, this program of study has faced data challenges in linking CCT participants and voting outcomes. In the largest and most comprehensive work on this topic, De La O (2013, 2015) provides evidence that suggests that CCT exposure increases turnout substantially (by about 5-15 percentage points, depending on the subsample used). However, the conclusions in this work have been 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, 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). That is, with CCTs it is unclear whether income is indeed the driving force in any income gains; the unique components of CCT programs contaminate this instrument from eliciting the pure downstream effects of income.[13]

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 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 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. Perhaps more importantly, though, 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

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

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 1990; 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 heterogeneities 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.

IIc. 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 sharp contrast to work on other adult behaviors, which suggests that earlier life investments have a larger impact on adult outcomes than later life investments (e.g., Becker and Tomes 1986; Chetty et al. 2011; Currie and Thomas 1995). While it is common to see models of human capital acquisition applied for education, labor, and health outcomes, this model has yet to be applied to prosocial behaviors like voting. Here we briefly articulate what a *human capital model of voting* (HCMV) would imply about this fundamental form of democratic participation.

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; see Fujiwara, Meng, and Vogl 2016; Gerber, Green, and Shachar 2003; Coppock and Green 2015; Meredith 2009). 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.[14] 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.

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.[15] The survey was originally designed as a means of studying the mental health and well-being of children; however, this sample has been used in a number of different contexts (Akee et al. 2010, Akee et al. 2013, Akee et al. 2015; 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

[14] The HCMV explicitly allows for diminishing returns in resource investments as predicted by Wolfinger and Rosenstone (1980).
[15] For the counties covered in the GSMS survey, see Figure A5 in the Online Appendix.

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. Forthcoming).[16]

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 baseline 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 average household incomes prior to the intervention; American Indian households earned incomes of approximately $23,000 while non-Indian households earned incomes that were almost nine thousand dollars higher for an average of $32,000. 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.

[Table 1 here: Table of Means by Race]

[16] 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 A4 in the Online Appendix.

After the fourth wave of the survey, a casino opened on the Eastern Cherokee reservation.[17] Upon the casino's opening, all adult enrolled tribal members 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%--a sizeable increase as a result of the casino transfers. (We also note the pre-treatment balance on income shown in this figure.) In these models, the coefficients of interest associated with the casino opening are all highly significant. This exogenous change in income levels provides us a tool for exploring the effects of income on voter participation across generations.[18]

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 history information off publically available statewide voter portals.[19] To do so, we followed common best practice and matched parents and children based on their name (first and last), date of birth, and,

[17] 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).

[18] A competing hypothesis is that the casino increased individuals' levels of hope (independent of income's indirect effects through hope). That is, the casino may have generated a greater sense that the future would be better than the present. To be clear, this would only be a threat to our analysis if this hope effect is independent of voting. After all, hope could be a mechanism by which income increases voting. This is not a threat to identification, but a potential channel that could be driving any effects. Ultimately, we think this explanation highly unlikely given the results we present below—null effects for parents. If there were a hope effect, we would expect to see it across parents and children.

[19] 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 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.

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 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 social interventions (e.g. Sondheimer and Green 2010; Holbein 2017) and survey data (e.g. Pew, CCES, ANES; see Ansolabehere and Hersh 2012) to voter records. 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.[20] Fortunately, however, the number of matching inputs available was balanced across the treatment and the control samples.[21]

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. Comfortingly, this registration rate is similar for individuals in the general population of a similar demographic

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

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

profile.[22] 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 correlation between parents voting and children's voting is high ($r=0.8$). Following previous best practice (e.g., Ansolabehere and Hersh 2012; Holbein 2017; Sondheimer and Green 2010; and that recommended by the CCES, Pew, and ANES surveys), the participants who we could not locate in the voter records were coded as having not registered nor voted.[23]

Robustness checks provided in the Online Appendix reveal that match quality is similar across our identifying variation (Table A1). We find little evidence that those exposed to the casino transfers for a longer period of time as minors are different in terms of children or parents moving out of the state, getting married, dying, or children or parents changing their last name—all measures that could substantially hinder match quality from being similar across our identifying variation. As we outline in much greater detail in the Online Appendix, all of this suggests that our results are unlikely to be biased by the match procedure itself.

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 between

[22] 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%. Of course, this rate is artificially inflated because of the social desirability of social acts like registering to vote.

[23] As an additional check, we can run the models just among those who are registered to vote. This check allows us to see if our coding of the vote outcome biases our results. This, understandably, decreases our precision, but does little to alter our substantive results. We do not include this as our standard specification as it would involve conditioning/stratifying on a post-treatment covariate, which is generally a very bad idea (Angrist and Pischke 2008).

American Indian (eligible for the transfer) and non-American Indian children (not eligible) and the second across cohorts of AI children who were exposed to different durations of income transfers.

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 the educational mechanism that we deem most likely based on evidence we present below. Simply put, our identification strategy is built around our expectation, informed by the HCMV, that income transfers will be of greater efficacy when they begin when individuals are younger. It is also built of necessity, given that children were not eligible to vote before the transfers began.

An important assumption behind this strategy is that non-AI children from the same cohorts provide valid controls groups to the AI children. We show some evidence in favor of this assumption in Figure 4.

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

$$Y_i = \alpha_i + \beta_1 \times Youngest\_cohort_i + \beta_2 \times Middle\_cohort_i + \delta \times American\_Indian_i + \gamma_1 \times Youngest\_cohort_i \times American\_Indian_i + \gamma_2 \times Middle\_cohort_i \times American\_Indian_i + X'\theta + \varepsilon_i \quad [1]$$

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 election and second, as a continuous variable measuring the proportion of eligible Federal elections that a person voted in.[24]

In equation [1], *American_Indian* is an indicator for American Indian race, *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), and X is a set of baseline covariates that include parents' voter turnout rate before the casino opened, and gender. The identification relies on differences between the three cohorts 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 these differences also across American Indian race.

Based on existing information that voting is highly correlated with household incomes (Figure 1), we interact the variables with initial household income in order to investigate whether the exogenous increase in household income has a differential effect across household types. In Figure 2 below, we show that for the GSMS parents there was a strong income gradient with respect to voting probability in the period prior to the start of the income intervention. In the figure below we provide the entire distribution of adults across three income bins and show that there is a monotonically increasing relationship between the initial household income and civic participation.

Following that analysis, we interact initial income with the transfer treatment variable and show in a regression framework that income is positively associated with higher propensity to vote. This observation does not allow us to gauge whether initially better off people vote less after

[24] 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.

the cash transfers or initially poorer people vote more. We then separate out observations by households that were below the median household income in the first three (pre-intervention) survey waves and those that were not to test whether the additional income has differential effects by baseline income. This is in line with the predictions outlined previously in the literature (by Wolfinger and Rosenstone 1980, and others, Frey 1971; Verba, Schlozman, and Brady 1995; Verba and Nie 1987) and with previous studies using the GSMS data (Akee et al. 2010, Akee et al. 2013, Akee et al. forthcoming; Copeland et al. 2011; Costello et al. 2010; Foley et al. 2006). This heterogeneity in the impact of extra income allows us to answer a substantively interesting question in the literature on income transfers (and voter turnout): do transfers not only rise overall levels, but do they narrow inequalities? [25]

[Insert Fig 2 here: GSMS Parental Voting Probability by Income Groups Prior to Intervention]

For the parents, we employ difference in difference analysis using observations from before and after the income intervention and comparing AI to non-AI parents. Since we are concerned with voting probabilities, the parents in our analysis were eligible to vote and can be found in the voter rolls before being exposed to the transfer in the election years 1992, 1994 and 1996. We are

[25] One might be tempted to argue that we should also try and break apart elections results in two additional ways: first, considering voting experiences at the same age (say, voting when one is 18) across the three cohorts and the second, examining the converse setup—voting experiences in the same election (say, 2010) across the three cohorts. Unfortunately, our identification strategy makes these comparisons unpalatable. By looking with age at the voting experience held constant (i.e. the first alternate route), we are inherently looking across different elections (given that we have three cohorts with varying ages). That is, for those in the first and third cohorts, voting when they are 18 was in a midterm election, while voting for the second cohort voting at 18 for the second cohort occurred in a presidential election. It has long been known that election context has a large effect on turnout rates (e.g. Anzia 2011, 2013). Hence, the coefficient of interest in this specification amounts to something perfectly collinear with an election context fixed effect interacted with race—something interesting, but probably unrelated to the casino transfer.

A similar issue holds for a model that holds election constant; here, the problem becomes the reverse of the first—we would be holding election context constant, but looking at voting by different ages; again, this is unrelated to the casino transfer given massive gaps in voting by age (Leighley and Nagler 2013; Holbein and Hillygus 2016). Previous studies that have involved multi-cohort long-term evaluations have acknowledged these complexities and, as such, have stuck to exploring average turnout rates (Holbein 2017; Sondheimer and Green 2010). We follow this approach as it also has the advantage of increasing residual precision with a scale-based measure of voting propensity.

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 still based on the exogenous nature of the income transfer. Equation [2] formalizes this model—with γ being the coefficient of interest. *Treatment* 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 *Treatment* is simply the interaction between those two other 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. 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.

$$Y_{it} = \alpha + \alpha_i + \gamma\, Treatment_{it} + \delta AmericanIndian_i + \lambda After_t + \theta_t + \varepsilon_{it} \quad [2]$$

Identification in equation [2] is based on the assumption that the parallel trends assumption holds. Because we are estimating this specification with the parents' voting probability, we can examine data from the periods prior to the casino operations. This test is shown in all three panels of Figure 5 below. Here it can be seen that prior to treatment, parents eligible for the casino transfer voted at a rate that was equivalent to those who were not eligible for the casino transfer.

As with children, we estimate our models both with the entire data set and then separated by above and below the initial median household income. We also provide results 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 is not an issue for matching of the children since the data for them is much more complete.[26] For completeness and direct comparability, we also show results using the cohort comparison framework we use for the children in eq [1].

Difference in difference regression analyses require showing evidence that pre-treatment trends across the treatment and control groups progress at similar rates. Figure 5 provides some evidence that this parallel trend assumption likely holds for adults in our data. We are unable to provide a similar analysis, however, for the children in our data as they were not eligible to vote during those years (all were less than 18 years old). Instead, we show the parallel trends for the parents across age cohorts and by race. We believe this provides a useful placebo check for our analysis.

In order to implement this, we estimate the same specification as in equation [1] and use parents' voting rates for all years as the outcome variables.[27] However, the years of interest are

[26] 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.

[27] 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.

the election years 1992, 1994, and 1996 as they pre-date the introduction of the casino cash transfers. These results are presented in Appendix Tables and event analyses figures.

To further check for pre-treatment differences across out two groups, Table A2 in the Online Appendix provides checks of baseline variable means 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, these do not change. This indicates that our different age cohorts are appropriate controls for estimating the effect of the casino transfer.

V. Results

Va. First stage – the effect of cash transfers on family income

In this section we show that household income increased for the eligible American Indian households over time after the introduction of the casino cash transfer program. 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 $2000 values and indicate that on average each household received about $4,700 per year which accords with existing 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 in Figure 3. 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 cash disbursement.

[Insert Figure 3 here]

[Insert Table 2]

Vb. Child Voting Outcomes

Table 3 shows our first set of results, estimating the effect of casino transfers on the voter turnout of children using equation (1).[28] Again, the identification 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, versus the youngest two cohorts, which were treated for 2 and 4 years longer, respectively. In Panel A, we present in columns 1 and 2 the results for our 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) and they are ever voted in an election or proportion of elections voted, respectively. The estimated difference in difference coefficients in the two 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), 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 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 which has $5,000

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

lower household income 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.

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 child from the youngest age cohort that is exposed to exogenously higher incomes during adolescence has about 20 percentage point increase in their likelihood of having ever voted over the period 2002-2014 as compared to a non-treated child.

The next two columns in Panel B provide a similar analysis for the observations that were initially above the median household income level prior to the income intervention. The estimated coefficients are negative, smaller in absolute size and statistical significance than the estimated coefficients in columns 1 and 2. As predicted by the regressions in columns 3 and 4 in Panel A above, there appears to be a relationship between initial household income and the effect of the exogenous change in household income on the child's likelihood of voting as an adult.

Figure 4 provides a graphical depiction of the results from Panel B. In the top panel, we combine the youngest two cohorts and interact that with election year and race and plot those estimated coefficients for observations initially from below the median household income. The probability of voting in a particular election is positive and statistically significant as compared to a child from the older age cohort. In the bottom panel, we provide the same analysis for individuals from above the initial median household income levels and see that the likelihood of voting in a particular election is not statistically significant across the age cohorts. Appendix Table 1 provides the regression coefficients for this analysis where we pool across all potential election years. We

provide the analysis in a simple difference in difference analysis as well as in the event analysis presented in Figure 4.

In Appendix Table 2 we conduce 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 4 : Child Event Analysis]

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 for parents' voting. It is important to note that for the parents we are able to compare the period prior to the cash intervention and have reliable voter participation data starting in 1992, as they were adults and able to vote in those early years. Therefore, we examine the impact of the exogenous change in household income on 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 a simple OLS difference in difference analysis. 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 everyone and we find that there is no statistically significant

effect of the increase in household income on parents' voting probabilities. Given that there was heterogeneity in results by initial household income for children we run separate analyses below and above median household income in columns 2 and 3. There does not appear to be large or statistically significant effects of the increase in household income on parental voting probabilities in either the total data or the data separated by initial household income. The results may be somewhat surprising. They suggest that voting preferences are set earlier in life and are thus unchanged in later adult years. The next three columns in Table 4 provide similar OLS analysis with probability weights based on the quality of the match to public records. We find no large differences.

Figure 5 provides the event analysis for parents for the full data, by below median household income and by above median household income. The figures provide similar conclusions found in Table 4 for the parents voting probabilities. Appendix Table 3 provides the regression results used for this figure.

[Figure 5 here: Parental Event Analysis]

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

Vd. Robustness Checks and Placebo Analysis

In Appendix Table 4 we conduct an analysis similar to that for the children in Table 3. Our intention here is to compare whether parents were more likely to vote by cohort. Perhaps the parents of a single cohort had greater voting preferences and were thus more likely to transmit to their children; therefore, we are identifying a cohort fixed-effect and not an effect related to the increase in household incomes.

We present the difference in difference analysis where the data is separated by the three age cohorts of children and interacted with race. We have included the pooled analysis as well as the analysis where the data is split by initial median household incomes. The OLS results are presented in Panel A, the individual fixed-effects regressions in Panel B and the weighted OLS regressions in Panel C. There is no statistically significant results in these analyses which accords with our earlier findings in Table 4. Appendix Figure 1 provides the event analysis for these same results; Appendix Table 4 provides the estimated coefficients.

In Appendix Table 5 we collapse the two treated cohorts and (similar to Appendix Table 2 for the children) find that there are no statistically significant results.

VI. Conclusion

Decades of social science research has established that income bias exists in voter turnout (Blais 2006; Frey 1971; Leighley and Nagler 2013; Verba, Schlozman, and Brady 1995; Verba and Nie 1987) and that these patterns may have distortionary effects on representative democracy (Anzia 2013; Berry et al. 2011; Fowler 2013; Griffin and Newman 2005; Leighley and Nagler 2013; Schlozman, Verba, Brady 2012; Verba and Nie 1972). Here we have taken the next step to explore whether income transfers are able to raise turnout and narrow participatory gaps; that is, we have examined whether income has an effect on this foundational social act of democracy. Results from our unique quasi experiment suggest that unconditional cash transfers do, indeed, have a substantial impact on participatory inequality. Cash transfers help disadvantaged children catch up with their more advantaged peers. However, they have little to no effect on parents nor on more advantaged childhood recipients.

Our results make both conceptual and practical contributions. In establishing that this foundational resource plays an especially important role earlier in the life course, our results contribute to a broader framework for understanding what drives people to participate in politics. Rather than relying alone on a Resource Model of Voting—which predicts that resources uniformly increase participation—our results suggests a more nuanced Human Capital Model for Voting—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 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 (Bedola and Michelson 2012) or have even backfired and made participatory gaps worse (Enos, Fowler, and Vavreck 2013). 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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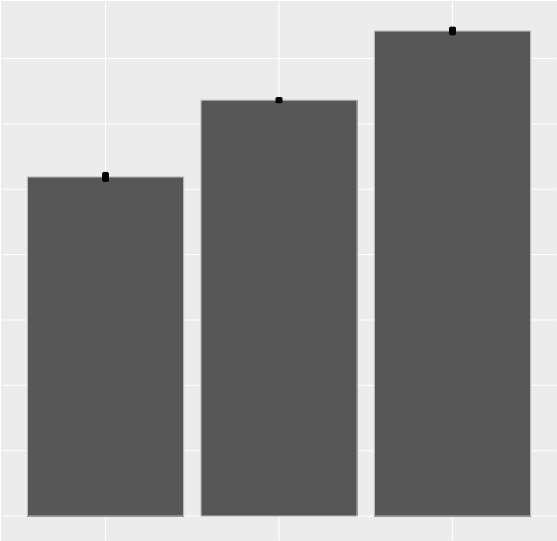
Figure 1: Voter Turnout by Income, 1948-2012 (ANES)



Notes: Figure 1 displays voter turnout rates by income thirds. Graph displays turnout estimates (points) and corresponding 90 (thicker bars) and 95% (narrower bars) confidence intervals. Data are drawn from the American National Election Study (ANES) Time Series Cumulative Data File (1948-2012). Income measured using the standard ANES question in which individuals are asked to label where they fall in the income distribution. Voter turnout measured using self-reports of voting in national elections in the given year (available in all years excepting 2002) and validated voting reports from matches to state voter files (available in 1964, 1976, 1978, 1980, 1984, 1986, and 1990).

Figure 2: Parental voter registration by initial income

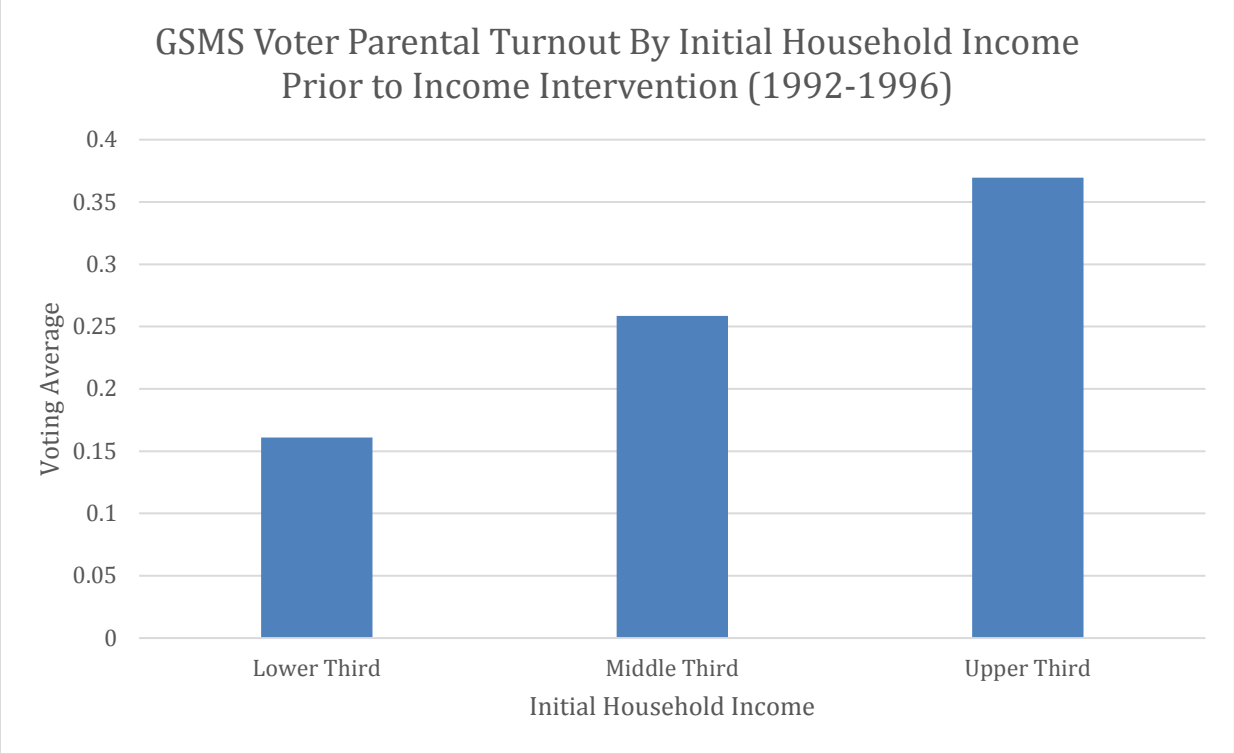


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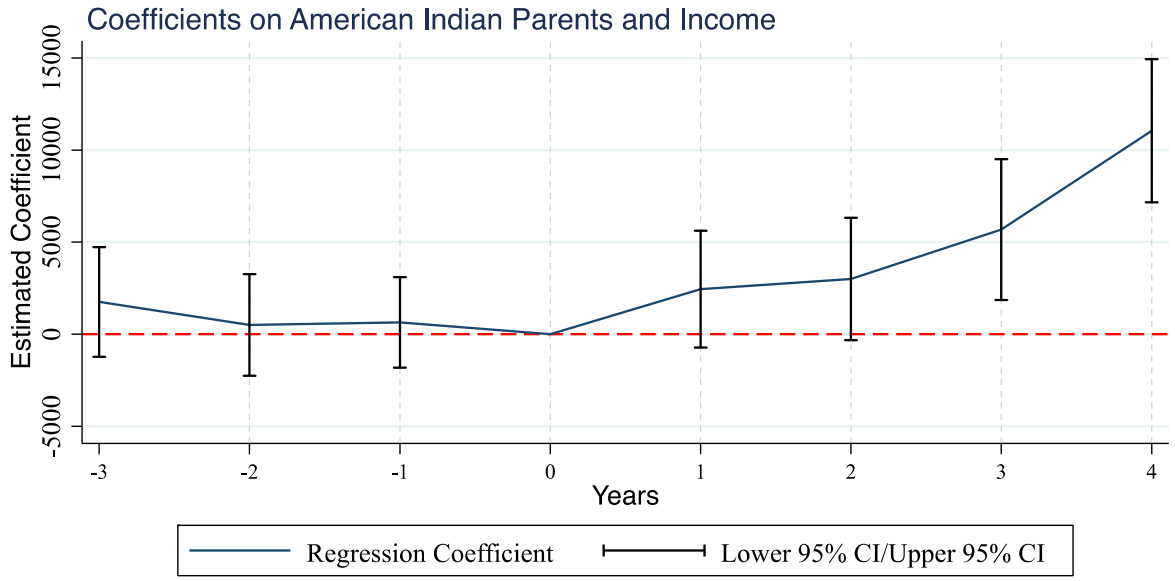
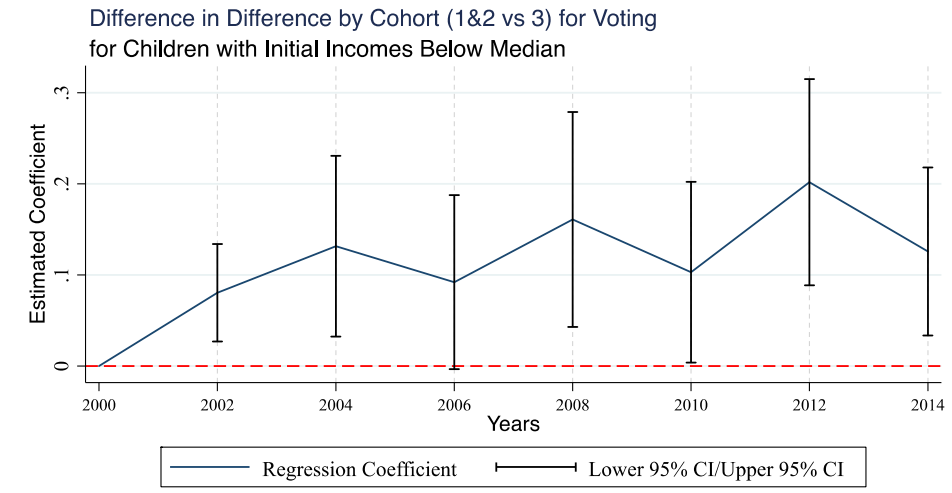
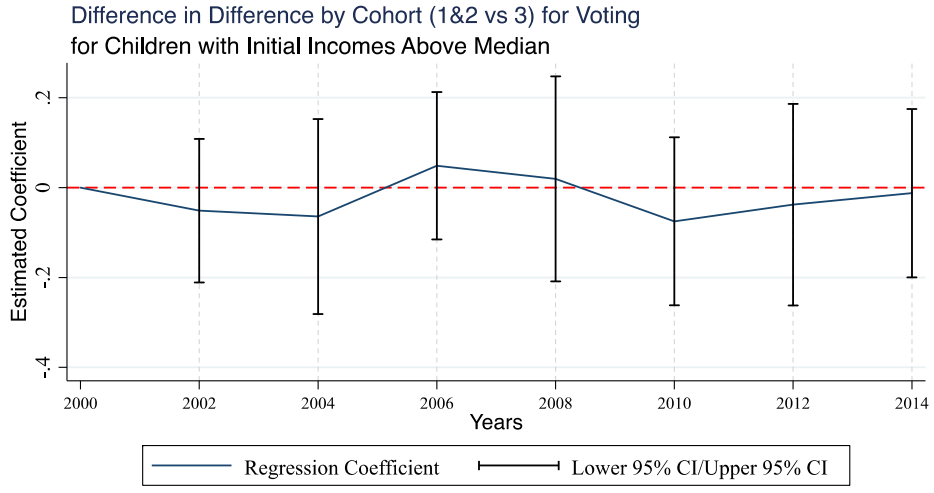
Figure 3: The effects of Casino Transfers on Household Income



Notes: 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 (wave 4 in the centered figure above) 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. Figure shows point estimates (dots) and corresponding 95% confidence intervals (bars).

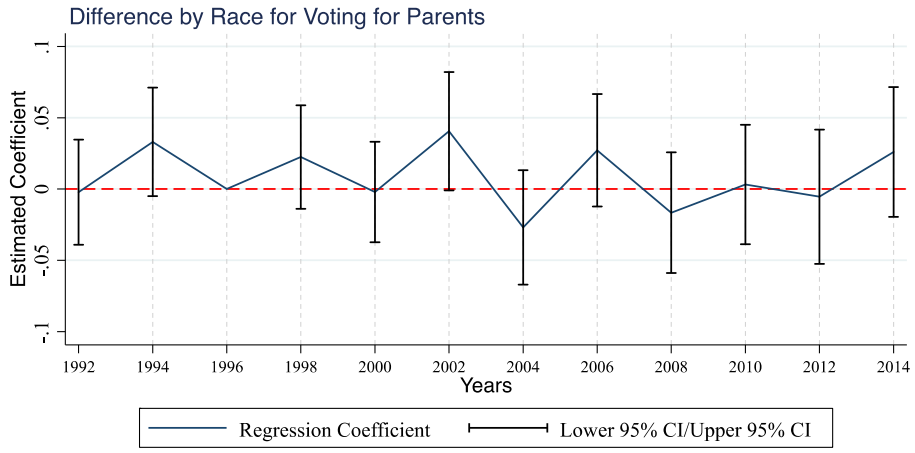
Figure 4: The Effect of Casino Transfer on Children's Voter Turnout

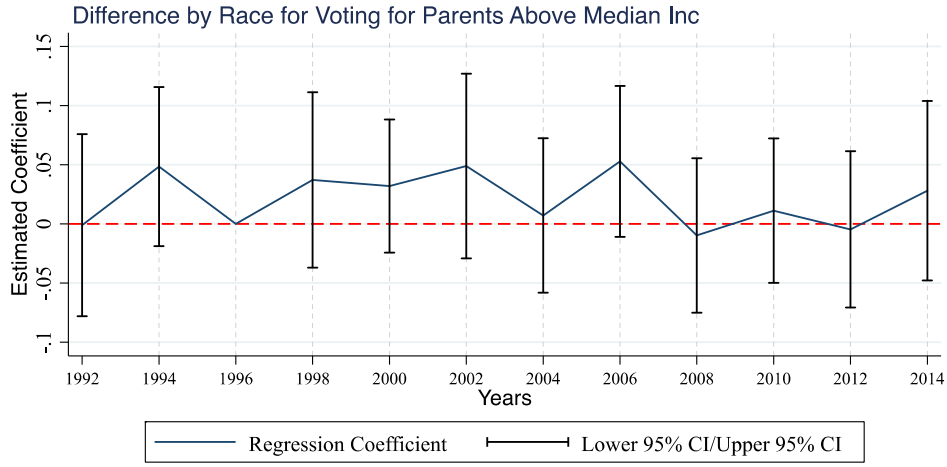




Notes: figure shows coefficient plot estimates from our "event study" regression models for children. Figure shows point estimates (circles) and corresponding 90/95% confidence intervals (bars). X-axis starts with election year 2002, which was the first year in which all children were eligible to vote.

Figure 5 Parents' Event Analysis





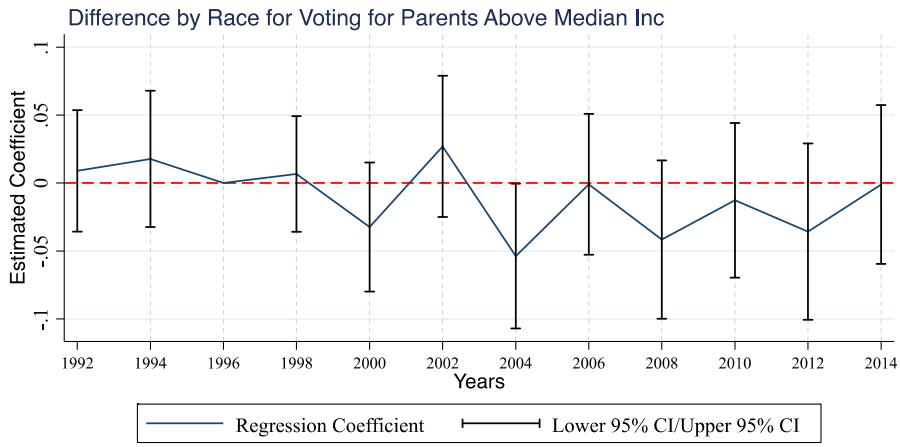

Difference by Race for Voting for Parents Above Median Inc
Estimated Coefficient
-.1
-.05
0
.05
.1
1992
1994
1996
1998
2000
2002
2004
2006
2008
2010
2012
2014
Years
Regression Coefficient
Lower 95% CI/Upper 95% CI

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

	American Indian		Non-Indian		Tests of Equality of Means		
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Diff in means	SE of Diff	T-Statistic
Age cohort initially 9-year olds	0.370	0.484	0.355	0.479	0.015	0.032	0.471
Age cohort initially 11-year olds	0.357	0.480	0.345	0.476	0.012	0.032	0.382
Age cohort initially 13-year olds	0.273	0.446	0.300	0.458	-0.027	0.030	-0.914
Age	10.805	1.595	10.889	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	23156.390	15216.620	32360.710	16907.050	-9204.320	1034.774	-8.895
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
Father has a high school degree/GED	0.209	0.407	0.171	0.377	0.038	0.026	1.433
Mother has more than a high school degree	0.391	0.489	0.484	0.500	-0.094	0.032	-2.896
Father has more than a high school degree	0.219	0.414	0.294	0.456	-0.075	0.028	-2.694
Mother Employed Full Time?	0.852	0.356	0.857	0.351	-0.005	0.025	-0.195
Father Employed Full Time?	0.921	0.270	0.942	0.233	-0.021	0.023	-0.913

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

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Survey wave 4 (when the casino transfers began) is the omitted category.

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

Panel A: Pooled and By Initial HH Income	Pooled		By Initial HH Income	
	(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)
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)
Observations	651	651	681	681
R-squared	0.049	0.041	0.033	0.041

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

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
Casino Payment	-0.00267	-0.0250	0.00673	-0.00346	-0.0203	0.000564
	(0.0148)	(0.0201)	(0.0217)	(0.0147)	(0.0201)	(0.0207)
Year FE	Y	Y	Y	Y	Y	Y
Weighted Regressions?	N	N	N	Y	Y	Y
N (parent-years)	16,212	7,812	8,172	16,212	7,812	8,172
R-squared	0.065	0.057	0.044	0.059	0.050	0.042

Notes: *** $p < 0.01$, ** $p < 0.05$, *$p < 0.10$. Models include individual fixed effects, age fixed effects, year fixed effects and a constant. 95% confidence intervals based on cluster robust standard errors (family level) are given in brackets below the estimated coefficients. The N for the heterogeneity models do not quite add up to the pooled estimate do to a small number of families missing household income at baseline. Results do not change if we leave these families out of the pooled estimate.

Online Appendix: The Effect of Unconditional Cash Transfers on Intergenerational Patterns of Voter Participation

[Not to be included in printed versions]

I. Descriptive Patterns in the Dependent Variable

Figure A1: Distribution and Trends in Voter Turnout by Generations

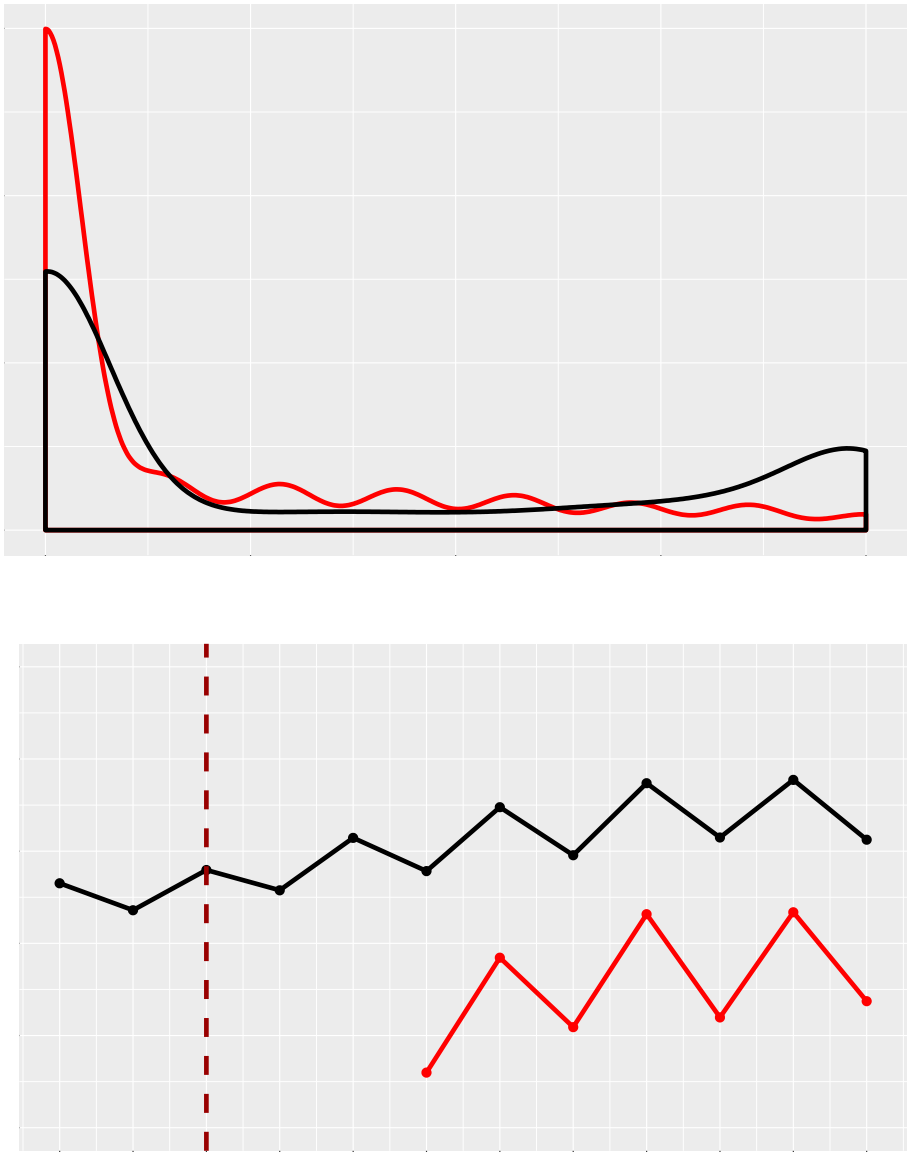


Figure A1 provides descriptive patterns in our outcome variable—voter turnout—across our two generations. The upper panel displays the distribution of turnout across parents and children. The bottom displays mean rates of voter turnout across elections. In both panels children are colored as red and parents as black. As can be seen, parents (being older) tend to vote at higher rates, though both generations show signs of aging into higher levels of voting over time. As in many other studies, voter turnout in midterm elections is lower than in presidential.

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.[29] 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.[30] 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.

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.

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[29] 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).

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

Table A1: Differences Across Cohorts for Various Characteristics that Affect Match Rates by Race and Age Cohort

VARIABLES	(1) Child Moves Out of North Carolina	(2) Parent Moves Out of North Carolina	(3) 1 Parent Moves Out of North Carolina	(4) 2 Child Married	(5) is Child Deceased	(6) is Child Changed Their Last Name	(7) Parent Changed Their Last Name	(8) 1 Parent Changed Their Last Name	2
Interaction 1: Age Cohort 1 × Number of American Indian parents	-0.0334	0.00915	0.0315	-0.0308	-0.00184	-0.0573	0.0153	-0.0355	
	(0.0312)	(0.0308)	(0.0541)	(0.0608)	(0.0242)	(0.0370)	(0.0303)	(0.0291)	
Interaction 2: Age Cohort 2 × Number of American Indian parents	-0.0304	0.0144	0.0709	0.124*	0.00278	-0.0749*	0.0565*	-0.0216	
	(0.0360)	(0.0351)	(0.0552)	(0.0686)	(0.0262)	(0.0389)	(0.0325)	(0.0302)	
Observations	1,190	1,186	1,067	952	1,190	1,190	1,190	1,068	
R-squared	0.066	0.023	0.050	0.086	0.016	0.187	0.024	0.012	

Note: *** p<0.01, ** p<0.05, * p<0.1. Regressions include parents' voter turnout rate before the transfer, American Indian indicator, gender, mother's highest educational attainment, father's highest educational attainment, average household income prior to casino operation, age cohort indicator variables, age, number of children in the household below age 6 and a constant. Robust standard errors in parentheses.

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.[31] 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 multiple matches using the inverse of the number of matches as weights. Intuitively, this approach

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

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.[32]

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.

[32] 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. Alternate Specifications

Appendix Table 1: 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	0.0484	0.131***	-0.0268		
	(0.0408)	(0.0409)	(0.0857)		
Interaction 2: Age Cohort 2 × Number of AI Parents	0.0408	0.124***	-0.0253		
	(0.0396)	(0.0401)	(0.0802)		
Interaction 1: Age Group × Number of AI Parents x 2000				Omitted Category	Omitted Category
Interaction 2: Age Group × Number of AI Parents x 2002				0.0805***	-0.0515
				(0.0273)	(0.0813)
Interaction 3: Age Group × Number of AI Parents x 2004				0.132***	-0.0644
				(0.0505)	(0.110)
Interaction 4: Age Group × Number of AI Parents x 2006				0.0921*	0.0487
				(0.0487)	(0.0835)
Interaction 5: Age Group × Number of AI Parents x 2008				0.161***	0.0193
				(0.0601)	(0.116)
Interaction 6: Age Group × Number of AI Parents x 2010				0.103**	-0.0750
				(0.0505)	(0.0952)
Interaction 7: Age Group × Number of AI Parents x 2012				0.202***	-0.0380
				(0.0576)	(0.114)
Interaction 8: Age Group × Number of AI Parents x 2014				0.126***	-0.0124
				(0.0469)	(0.0953)
Parents Prior Voting	0.143***	0.0660*	0.139***	0.0655*	0.140***
	-0.0251	-0.0376	-0.033	(0.0377)	(0.0330)
Observations	9,457	4,557	4,767	4,557	4,767
R-squared	0.049	0.040	0.054	0.043	0.056

Robust standard errors in parentheses

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

Appendix Table 2: 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	0.181***	0.0839***	-0.0749	-0.0256
	(0.0470)	(0.0246)	(0.0453)	(0.0233)	(0.0832)	(0.0466)
Parent Prior Voting	0.161***	0.106***	0.120*	0.0576	0.184***	0.130***
	(0.0418)	(0.0251)	(0.0656)	(0.0372)	(0.0535)	(0.0326)
Observations	1,332	1,332	651	651	681	681
R-squared	0.051	0.063	0.046	0.040	0.033	0.040

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

Appendix Table 3: Parents Event Analysis Regression Tables

	Race				Cohorts		
	Pooled	Below Median HH Income at Baseline	Above Median HH Income at Baseline		Pooled	Below Median HH Income at Baseline	Above Median HH Income at Baseline
	(1)	(2)	(3)		(4)	(5)	(6)
VARIABLES	Voted	Voted	Voted	VARIABLES	Voted	Voted	Voted
Interaction 1: Number of AI Parents x 1992	-0.00225	0.00895	-0.00110	Interaction 1: Age Group × Number of AI Parents x 1992	-0.00113	0.0856	-0.114
	(0.0188)	(0.0228)	(0.0392)		(0.0473)	(0.0582)	(0.0872)
Interaction 2: Number of AI Parents x 1994	0.0330*	0.0177	0.0485	Interaction 2: Age Group × Number of AI Parents x 1994	0.0770*	0.0976*	0.0881
	(0.0194)	(0.0255)	(0.0342)		(0.0401)	(0.0533)	(0.0695)
Interaction 3: Number of AI Parents x 1996	Omitted Category	Omitted Category	Omitted Category	Interaction 3: Age Group × Number of AI Parents x 1996	Omitted Category	Omitted Category	Omitted Category
Interaction 4: Number of AI Parents x 1998	0.0224	0.00667	0.0372	Interaction 4: Age Group × Number of AI Parents x 1998	0.0143	0.0924*	-0.0452
	(0.0185)	(0.0217)	(0.0378)		(0.0468)	(0.0473)	(0.0921)
Interaction 5: Number of AI Parents x 2000	-0.00210	-0.0324	0.0319	Interaction 5: Age Group × Number of AI Parents x 2000	-0.00308	0.0461	-0.0103
	(0.0180)	(0.0242)	(0.0286)		(0.0420)	(0.0566)	(0.0625)
Interaction 6: Number of AI Parents x 2002	0.0405*	0.0269	0.0489	Interaction 6: Age Group × Number of AI Parents x 2002	0.0176	0.0673	-0.0251
	(0.0212)	(0.0265)	(0.0397)		(0.0483)	(0.0586)	(0.0881)
Interaction 7: Number of AI Parents x 2004	-0.0269	-0.0538**	0.00712	Interaction 7: Age Group × Number of AI Parents x 2004	-0.0221	0.00776	-0.0198
	(0.0204)	(0.0271)	(0.0332)		(0.0481)	(0.0625)	(0.0748)
Interaction 8: Number of AI Parents x 2006	0.0272	-0.000942	0.0528	Interaction 8: Age Group × Number of AI Parents x 2006	0.0387	0.115*	-0.0193
	(0.0201)	(0.0264)	(0.0324)		(0.0451)	(0.0589)	(0.0679)
Interaction 9: Number of AI Parents x 20008	-0.0166	-0.0416	-0.00977	Interaction 9: Age Group × Number of AI Parents x 20008	-0.0110	-0.00639	-0.00503
	(0.0216)	(0.0297)	(0.0332)		(0.0487)	(0.0654)	(0.0748)
Interaction 10: Number of AI Parents x 2010	0.00321	-0.0127	0.0112	Interaction 10: Age Group × Number of AI Parents x 2010	0.0498	0.0605	0.0351
	(0.0213)	(0.0290)	(0.0311)		(0.0468)	(0.0695)	(0.0605)
Interaction 11: Number of AI Parents x 2012	-0.00537	-0.0358	-0.00471	Interaction 11: Age Group × Number of AI Parents x 2012	-0.0370	-0.0477	-0.0134
	(0.0240)	(0.0331)	(0.0336)		(0.0592)	(0.0863)	(0.0753)
	0.0260	-0.00110	0.0281		0.00771	0.00383	0.0361

Interaction 12: Number of AI Parents x 2014	(0.0232)	(0.0298)	(0.0386)	Interaction 12: Age Group × Number of AI Parents x 2014	(0.0558)	(0.0752)	(0.0827)
Observations	16,212	7,812	8,172		16,212	7,812	8,172
R-squared	0.065	0.059	0.044		0.065	0.060	0.045

Robust standard errors in parentheses

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

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

	Pooled	Below Median HH Income at Baseline	Above Median HH Income at Baseline
	(1)	(2)	(3)
Independent Variables	Voted	Voted	Voted
Age cohort 1 X Native American	-0.0558	-0.125	0.0595
	(0.0624)	(0.0787)	(0.114)
Age cohort 2 X Native American	-0.0314	-0.0509	-0.0249
	(0.0657)	(0.0851)	(0.114)
Individual FE	N	N	N
Year FE	Y	Y	Y
N (families)	16,212	7,812	8,172
R-squared	0.065	0.061	0.045

Notes:

Weighted Below:

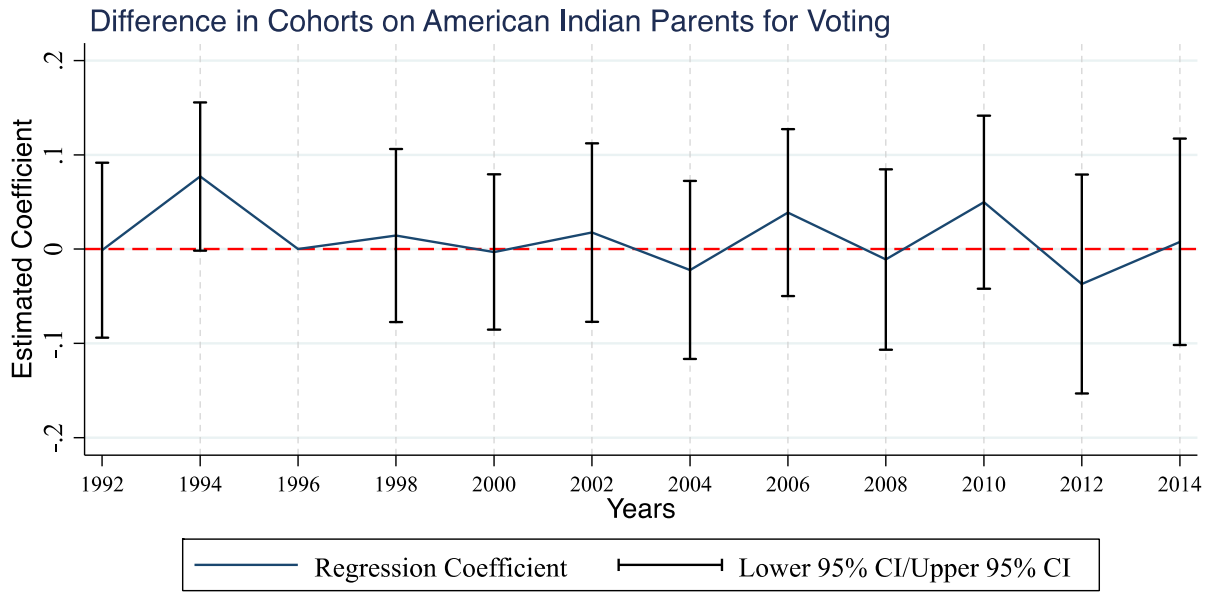
	Pooled	Below Median HH Income at Baseline	Above Median HH Income at Baseline
	(1)	(2)	(3)
Independent Variables	Voted	Voted	Voted
Age cohort 1 X Native American	-0.0232	-0.0707	0.0565
	(0.0579)	(0.0683)	(0.110)
Age cohort 2 X Native American	-0.0357	-0.0430	-0.0428
	(0.0597)	(0.0728)	(0.107)
Individual FE	N	N	N
Year FE	Y	Y	Y
N (families)	16,212	7,812	8,172
R-squared	0.061	0.052	0.043

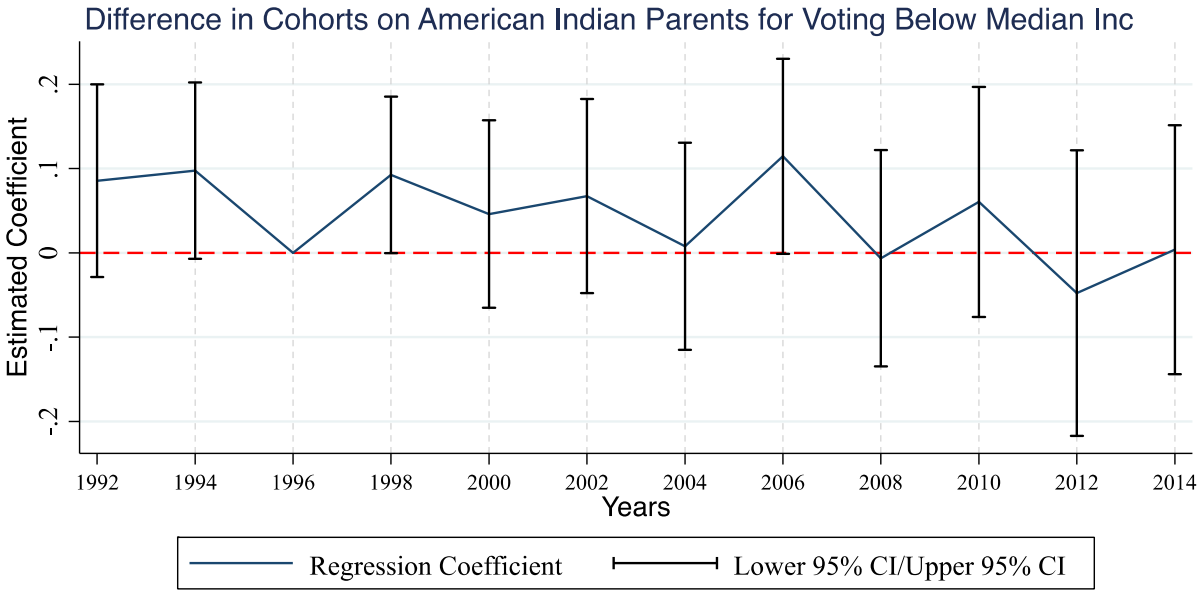
Appendix Table 5: The Effect of Casino Transfer on Parents' Voter Turnout (Probability of Voting) Comparison Across Cohorts (1&2 vs 3)

	Pooled	Below Median HH Income at Baseline	Above Median HH Income at Baseline
	(1)	(2)	(3)
Independent Variables	Voted	Voted	Voted
Casino Payment	-0.0192	-0.0235	0.00110
	(0.0342)	(0.0464)	(0.0465)
Individual FE	N	N	N
Year FE	Y	Y	Y
N (parent-years)	16,212	7,812	8,172
N (families)	1,351	651	681
R-squared	0.065	0.059	0.044

Weighted	Pooled	Below Median HH Income at Baseline	Above Median HH Income at Baseline
	(1)	(2)	(3)
Independent Variables	Voted	Voted	Voted
Casino Payment	-0.0152	-0.0173	-0.00696
	(0.0339)	(0.0465)	(0.0462)
Individual FE	N	N	N
Year FE	Y	Y	Y
N (parent-years)	16,212	7,812	8,172
N (families)	1,420	692	708
R-squared	0.060	0.052	0.043

Appendix Figure 1




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

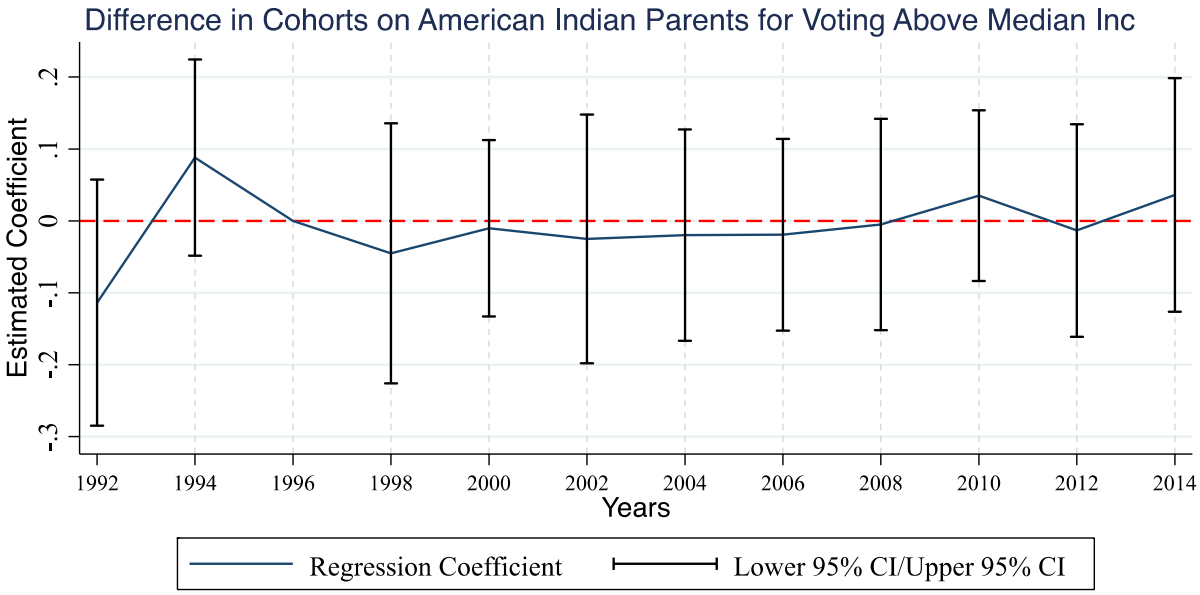

Difference in Cohorts on American Indian Parents for Voting Above Median Inc
Estimated Coefficient
-.3
-.2
-.1
0
.1
.2
1992
1994
1996
1998
2000
2002
2004
2006
2008
2010
2012
2014
Years
Regression Coefficient
Lower 95% CI/Upper 95% CI

Table A2: 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

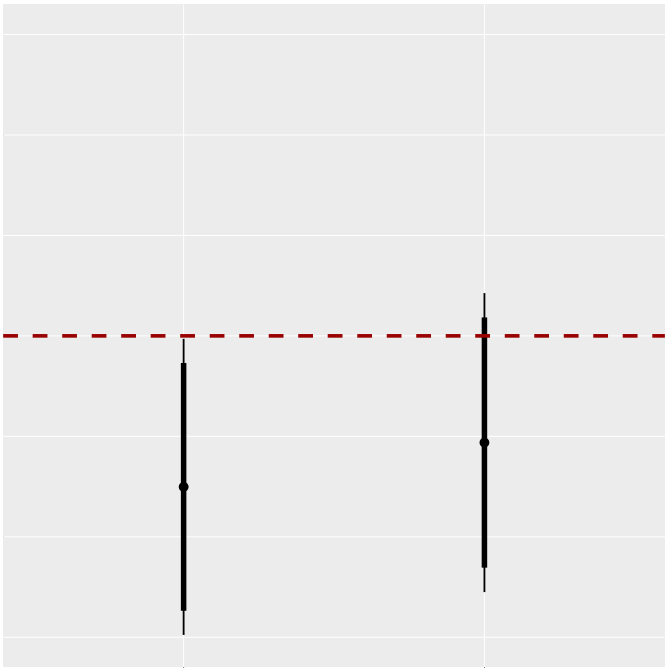
	Cohort 2 Mean	Cohort 3 Mean	Difference	SE of Difference
Number of American Indian Parents	1.387	1.296	0.090	0.070
American Indian Indicator	0.981	0.926	0.055	0.030
Male Child Indicator	0.547	0.543	0.004	0.074
Mother Has a High School Degree/GED	0.330	0.333	-0.003	0.070
Father Has a High School Degree/GED	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

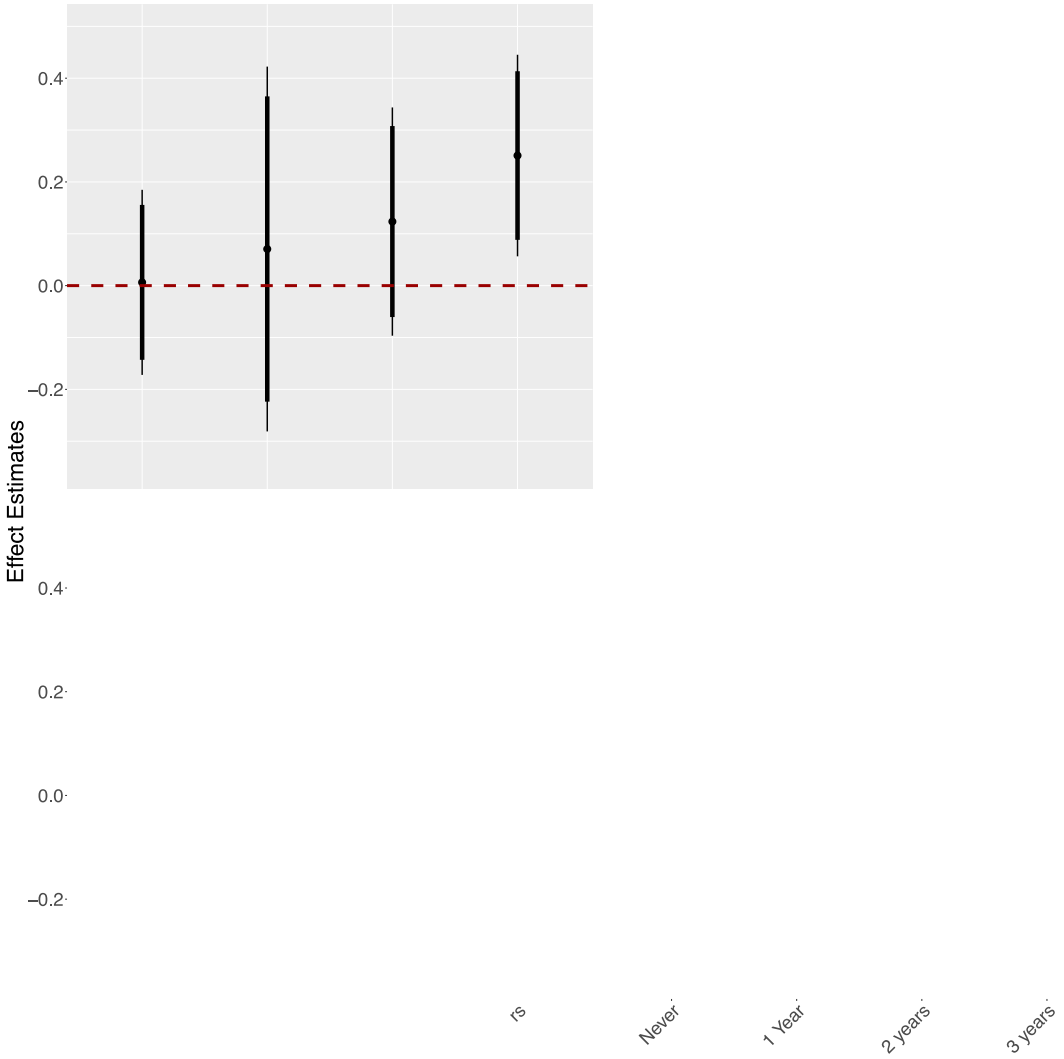
Note: * indicates significance at the 5% level, ** indicates significance at the 1% level.

Figure A2: The Effect of Casino Transfer on Children's Voter Turnout, Ordered Probit Estimates (Never Vote)



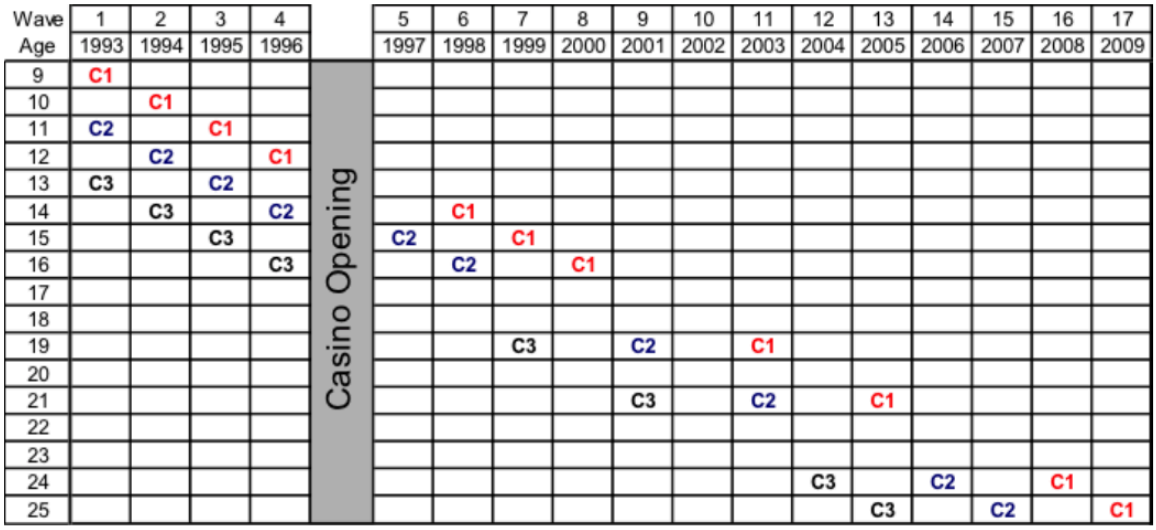
Notes: Coefficient plots for the effect of casino transfers on the voting of children. Figure shows the coefficient estimates (points) and corresponding 90% (thicker line) and 95% confidence intervals (thinner line). Models are ordered probit with marginal effects holding controls at their median levels. The first shows the interaction term for Cohort 1 (p=0.046) and Cohort 2 (p=0.162). Models include pre-treatment measures of parents' voter turnout rate before the transfer, American Indian indicator, gender, mother's highest educational attainment, father's highest educational attainment, average household income prior to casino operation, age cohorts, and a constant. Robust standard errors employed.

Figure A3: The Effect of Casino Transfer on Children's Voter Turnout, by the Number of Years in Poverty Before the Casino Opened



Notes: Coefficient plots for the effect of casino transfers on the voting of children. Figure shows the coefficient estimates (points) and corresponding 90% (thicker line) and 95% confidence intervals (thinner line). Models broken by the number of years (out of 3 before the casino) that individuals were in poverty. Models include pre-treatment measures of parents' voter turnout rate before the transfer, American Indian indicator, gender, mother's highest educational attainment, father's highest educational attainment, average household income prior to casino operation, age cohorts, and a constant. Robust standard errors employed. As can be seen, the positive effects on turnout accrue mostly on individuals perpetually in poverty before the casino opened.

Figure A4: Design of Follow up Surveys of the GSMS



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

Figure A5: 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).

Appendix Table 3: Comparison of Economic 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%
Per Capita Income	$6,543	$11,691	$11,362	$9,905	$9,165	$17,922	$15,677

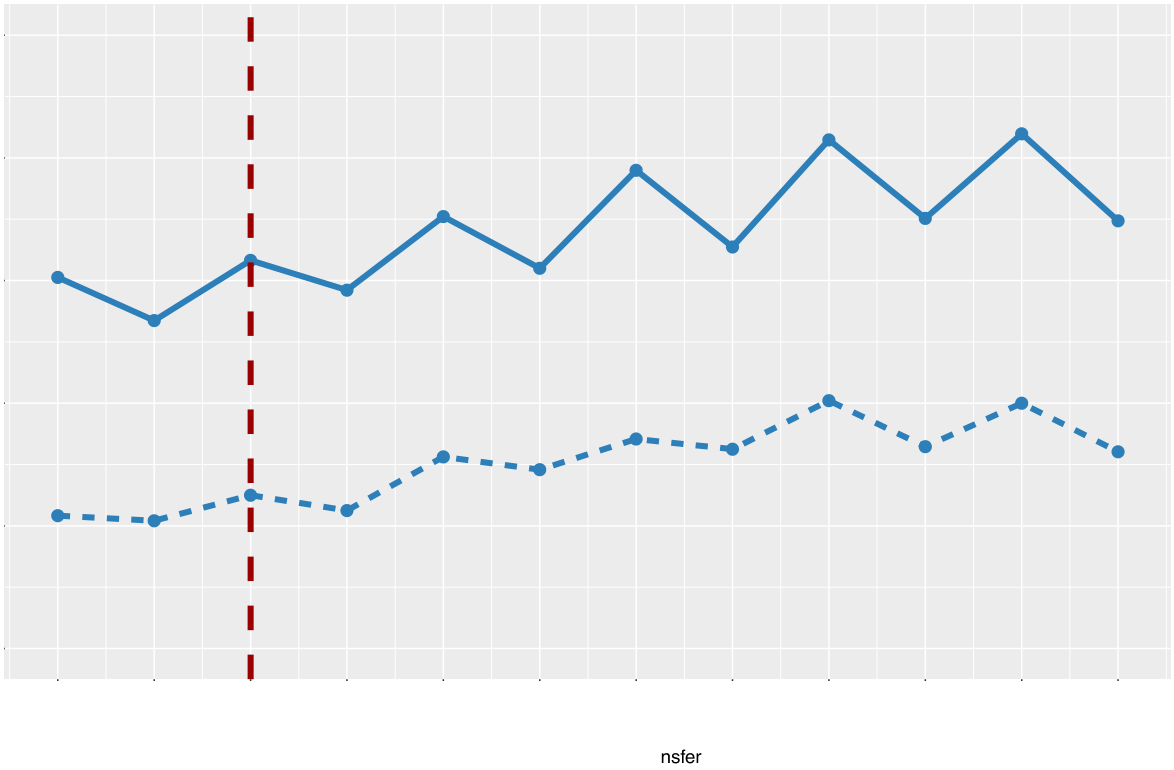
* Source: Taylor and Akee (2014)

1990 Census Report on American Indians

Social Explorer, 1990 County Data

IPUMS 1990, 1% Sample

Figure ???: The Effect of Casino Transfer on Parents' Voter Turnout



Notes: figure shows raw voter turnout rates of parents by transfer status (no casino transfer = solid; casino transfer = dashed). The maroon vertical dashed line shows the year the casino transfers began. Identification for parents leverages differences in casino eligibility and before and after the transfer and an individual fixed effects specification.