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The Two Income-Participation Gaps

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Abstract: *Scholars have long attributed the income-participation gap—which is the observation that the rich participate in politics more than the poor—to income-based differences in the resources, recruitment, mobilization, and psychology underpinning political behavior. I argue that these explanations require a longer time horizon than the empirical evidence permits. Education, for example, typically ends in young adulthood and so cannot logically mediate the effect of income on participation in late adulthood. To resolve this temporal problem, I propose that there are two income-participation gaps: one based on current economic status and another on childhood economic history. I situate this argument in a developmental framework and present evidence for it using six studies. The results, while mixed at times, indicate that there are two gaps, that the size of each gap changes over the life course, and that their joint effect creates a larger income-participation gap than estimated by prior research.*

Replication Materials: The data, code, and any additional materials required to replicate all analyses in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network, at: https://doi.org/10.7910/DVN/XU8ZWB.

One well-documented finding in the study of political behavior is that the rich participate in politics more than the poor (Blais 2006; Rosenstone 1982; Schlozman, Verba, and Brady 2012; Verba, Schlozman, and Brady 1995), a stylized fact that I refer to as the income-participation gap. Due in part to the gap's empirical prevalence and normative implications, the American Political Science Association commissioned a task force to report on how economic inequality affects democracy. They concluded that "the voices of American citizens are raised and heard unequally. . . . Citizens with lower or moderate incomes speak with a whisper that is lost on the ears of inattentive government officials" (American Political Science Association 2004, 1). Although subsequent research shows that the effect of income is smaller than other predictors of participation, such as education or interest in politics (Smets and Van Ham 2013), income remains one of the most consequential. As Leighley and Nagler note, "income is the most widely used and recognized demographic criteria by which government distributes benefits. . . . [I]f poor people do not vote, they could find government policy explicitly written to disadvantage them" (2013, 24). The repercussions for representation are not hypothetical, however: Parties support positions and policy makers craft legislation that differentially benefits the rich (Bartels 2008; Gilens 2012; Rigby and Wright 2013). Simply put, even modest income-based differences in participation can have large ramifications for who gets what, when, and how.

Empirically, research on the income-participation gap has coalesced around the notion that current income is the appropriate measure of economic status. Researchers using survey data typically ask respondents about their household income in the year preceding the interview and then include the responses in models of political participation. This approach, however, overlooks the fact that individuals have diverse childhood backgrounds. Might economic history also be an appropriate measure of economic status and one that matters to political participation? Growing up in or even briefly experiencing poverty as a child has negative consequences in adulthood—poor health, reduced cognitive ability, mental health disorders, and limited educational opportunities, to name just a few (Brooks-Gunn and Duncan 1997). It seems obvious then that if poor and rich "youth arrive at their late teens with vastly different capacities and resources to navigate the various transitions, such

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as education, leaving home, entering the workforce, and forming relationships and families" (Furstenberg 2008, 4), they would also exhibit differences in how they engage in the political process. While the idea that childhood experiences matter to adult political behavior is not new (Campbell 2006; Jennings, Stoker, and Bowers 2009; Sears 1983), demonstrating this in the context of the income-participation gap has been difficult because of the limitations of survey data. As a result, we know little about whether or how childhood economic history affects participation.

Combining the foundational work on the income-participation gap with developmental models of voting (Plutzer 2002) and longitudinal data from sociology and economics, I refine our understanding of the connection between economic status and political participation through a two-pronged argument. My first and central claim is that there are two income-participation gaps: one based on current economic status and another based on economic history. I call this the two gaps hypothesis and provide evidence for it using six studies: the General Social Survey, the 1979 National Longitudinal Survey of Youth, the Youth-Parent Socialization Study, the Children of the 1979 National Longitudinal Survey of Youth, the 1997 National Longitudinal Survey of Youth, and the Panel Study of Income Dynamics. The results, although mixed at times, indicate that the overall size of the income-participation gap has been understated by focusing on current economic status. Once economic history is incorporated into models of voting, the cumulative difference in turnout between the rich and the poor grows substantially.

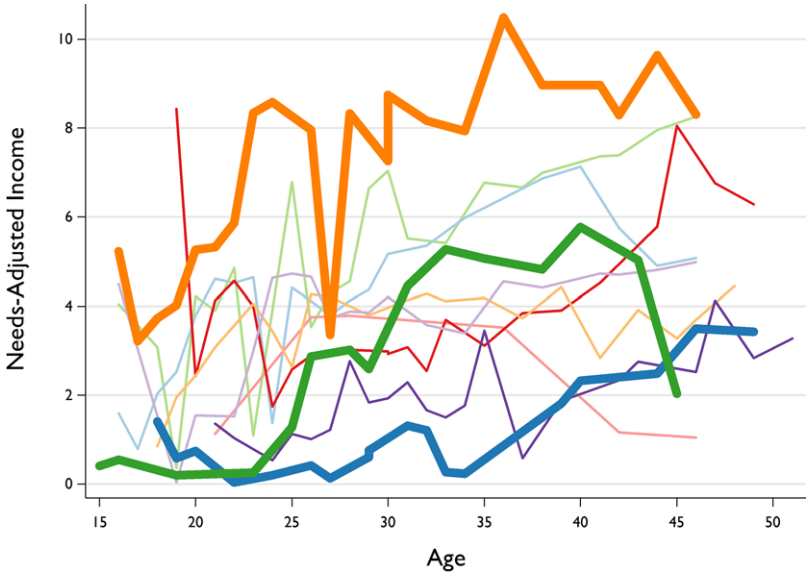
Importantly, the identification of two gaps changes how we think about the causes of income-based differences in participation. Extant explanations point to disparities in the resources, mobilization, recruitment, and psychology underpinning political participation (Soss and Jacobs 2009). These explanations are illuminating and useful, but they also contain strong development components and are therefore better suited to explain the effect of economic history on participation than the effect of current economic status. Education, for example, usually ends in young adulthood and so cannot logically mediate the effect of income on participation later in life. It could, however, explain differences in participation based on economic history. In this way, my argument for two income-participation gaps advances research by bringing clarity to the origins of this phenomenon.

My second argument extends the two gaps hypothesis by proposing that the size of each gap fluctuates over the life course. Specifically, economic history will be important in young adulthood as voting habits are forming, but it will be less important in middle and late adulthood once habits have formed. In contrast, current economic status will be of modest importance to habit formation but will have a larger effect in middle and late adulthood by disrupting the inertia underpinning voting or nonvoting habits. I call this argument the life course hypothesis and use a mixture of statistical modeling and intrastudy comparisons to provide some, albeit limited, evidence in its favor. I conclude that the income-participation gap has two faces and requires both developmental and contemporaneous explanations and evidence.

The Problem of Temporal Ordering

That the poor participate in politics less than the rich has become a stylized fact in the study of American political behavior. Three aspects of the income-participation gap reveal its breadth, complexity, and persistence. The first is that the rich participate more than the poor across a variety of political acts, including voting, volunteering, running for office, or donating to a campaign (Schlozman, Verba, and Brady 2012). The second truism is that the shape of the income-participation gap is curvilinear—the effect of income on turnout is largest at the lower end of the income distribution and smallest at the higher end (Rosenstone 1982). Pacheco and Plutzer (2008) explain this logic by pointing out that the difference between $10,000 and $30,000 is qualitatively greater than the difference between $110,000 and $130,000. This phenomenon is known in economic terms as the diminishing marginal utility of income; in its application here, the diminishing return of each dollar is on the probability of voting. Third, it is well documented that the income-participation gap has persisted over time (Leighley and Nagler 1992, 2013). Working from these truisms, scholars have articulated four causal mechanisms to explain the income-participation gap: (1) the poor have fewer skills and resources necessary for participation (Brady, Verba, and Schlozman 1995), (2) mobilization efforts by campaigns and other organizations tend to ignore the poor (Rosenstone and Hansen 1993), (3) the policy agenda does not typically emphasize issues relevant to the poor (Piven and Cloward 1988), and (4) institutional restrictions on voting disproportionately affect the poor (Hershey 2009). These explanations are widely accepted by mainstream media (Weeks 2014) and academic scholars (Soss and Jacobs 2009), thus seeming to put to rest the question of why the income-participation gap exists. Rather, efforts on this front have focused on chipping away at the gap with special attention to matters like

FIGURE 1 A Random Sample of 10 NLSY79 Respondents Shows Income Fluctuates Year to Year



mobilization (LeVan 2011) and electoral laws (Fullmer 2015). Laws about voter identification and registration, workday elections, felony disenfranchisement, and early voting in particular have entered the public spotlight because they are thought to disenfranchise the poor and people of color.

Nevertheless, this line of research remains incomplete due to what I call the problem of temporal ordering. The proposed explanations of the gap are predicated on the assumption that current income is a good indicator of economic status, even though the microprocesses of each explanation contain strong developmental elements.[1] The idea that the poor lack the skills and resources for participation points to a model of skill development and resource acquisition that occurs over a longer time horizon than is captured by a measure of current economic status. Education, language skills, and political knowledge are relatively stable in adulthood and are unlikely to fluctuate in response to changes in current economic status. In this way, the "lack of skills and resources" explanation, as is true of the other explanations, requires a differentiation between short- and long-term changes in economic status. For example, it stands to reason that people living in *chronic* poverty are less likely to participate than are those *temporarily* unemployed because both have probably acquired different resources, social networks, and interest in politics over their life courses, even if the economic statuses of these two groups may be the same in any given year.

Current economic status would capture skill development and resources acquisition *if* economic status were stable across the life course; in this case, current economic status would be a perfect substitute for economic history. However, sociological and economic research show that income often fluctuates from year to year even as intergenerational economic immobility is entrenched (Sandoval, Rank, and Hirschl 2009). Figure 1 illustrates this point using a spaghetti plot of needs-adjusted income for 10 randomly selected respondents from the 1979 National Longitudinal Study of Youth.[2] The graph offers

[1] Economic status refers to a person's ability to use labor, land, or capital to shape an economy to his or her own benefit. There are several ways to operationalize economic status, including wealth (i.e., the accumulation of resources), income (i.e., the flow of cash), or consumption (i.e., the purchase of goods and services). I use an income-based approach because it reflects prior research on political participation and because survey data rarely ask about wealth or consumption. One distinction I make throughout the article is between current economic status and economic history. Current economic status refers to a person's family income from the present year. Economic history refers to a person's family income from childhood. General references to income or economic status are used when temporality is irrelevant to the idea being conveyed.

[2] Needs-adjusted income is the ratio of family income to the family needs standard as defined by the Census Bureau's poverty measure. For example, if a family of four earns $50,000 a year and has a need of $25,000, then it has a needs-adjusted income of 2. Families with needs-adjusted income below 1 are said to be in poverty (i.e., needs exceed income). Families with a needs-adjusted income of 1 or greater are not in poverty (i.e., income meets or exceeds needs). I use needs-adjusted income in this article as a more precise measure of economic status than unadjusted income; however, the results reported here and later in the analyses hold for either measure.

several important observations. First, needs-adjusted income takes on a range of values at any given age. This variation reflects the distribution of needs-adjusted income among any random cross-section of the population. Second, needs-adjusted income generally declines following adolescence—as children leave the home and start a life of their own—and then steadily increases over young and middle adulthood—as individuals reach their peak purchasing power through job advancement, the accumulation of wealth, and marriage. If this sample extended into late adulthood, then we would see another decline as individuals retire and lose work as an important source of income.

Most germane to this study is the fact that needs-adjusted income undulates from year to year in addition to the broader life course trends noted above. The respondents represented by the thick lines illustrate this point by revealing how needs-adjusted income rises or falls seemingly at random in any given year, even as each respondent displays a long-term upward trend. It is clear that there is no "typical" needs-adjusted income for these or other respondents. Such variability is not uncommon: The correlation between an individual's median needs-adjusted income over the life course and his or her needs-adjusted income in a randomly selected year is 0.39 among this sample. This fact alone challenges research that exclusively measures economic status as current economic status, especially if there is no theoretical reason to do so.

For all that the lines in Figure 1 have in common—namely, that there is variability in needs-adjusted income over the life course—they also exhibit stark and meaningful differences. The respondent represented by the thick blue line is trapped in a system of economic immobility; this respondent's needs-adjusted income in adulthood looks like his or her family's needs-adjusted income from childhood. Constrained mobility is also observed with the thick orange line but on the opposite end of the income distribution. Indeed, the needs-adjusted income of nearly half of the respondents does not deviate by more than 1 point on average over the life course. Contrast this immobility to the "success" of the thick green line—a respondent who grew up in poverty but quickly left it behind in young adulthood. These differences point to still another observation: There is variability in the degree to which needs-adjusted income varies over the life course. Some individuals encounter large year-to-year fluctuations, whereas others maintain a steady needs-adjusted income over time.

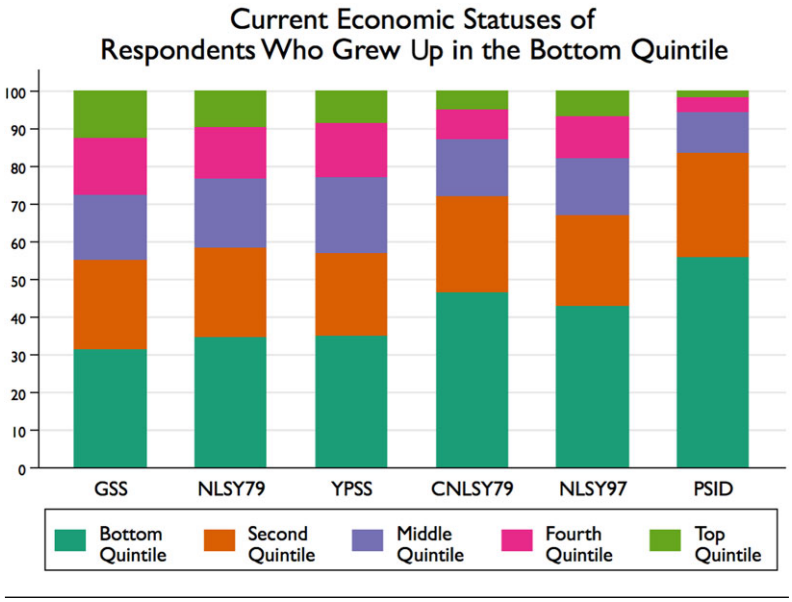
Of course, it would be misleading to imply that economic history and current economic status are independent of one another. That income fluctuates from year to year does not mean that changes in income are untethered from a political economy that tends to reify classes rather than disrupt them (Hacker and Pierson 2010). The temporal stickiness of income—what is commonly known as economic immobility—means that my best guess of an individual's current economic status is his or her economic history. Figure 2 quantifies this stylized fact through the perspective of the poorest respondents in the six studies that I analyze below. A majority of individuals who grow up in the bottom economic quintile will end up either there or in the second-to-last quintile. Less than 10% on average will make it to the top quintile. All told, the picture of economic status over the life course is a complex one with seemingly contradictory brushstrokes: Yearly changes in income are common but do not often result in economic mobility.

If economic history and current economic status are distinct but related aspects of an individual's economic profile, then what are the consequences for models of political participation that exclusively use current economic status? In short, why should we care about the problem of temporal ordering? The likely empirical consequences are twofold: Current economic status will appear larger because it will capture some of the effect of economic history, and the overall size of the income-participation gap will appear smaller because of the failure to model the full effect of economic history. While these dual overestimating and underestimating errors are unlikely to be enormous because of the tempering effects of economic immobility, they are still far-reaching because they suggest that the economic roots of political inequality are larger and more fortified than previously thought. Perhaps more concerning than the statistical bias are the substantive problems that emerge. Accounting for other temporal dimensions brackets the power of each of the five explanations of the income-participation gap. To say that a lack of skills is a barrier to voting for the *chronically* poor is to make a much smaller claim than to say that a lack of skills is a barrier for *all* who are poor in any particular year. The chronically poor represent only a modest percentage of individuals who experience poverty and an even smaller percentage of the population in general. Similar remarks could be made about social capital, mobilization, the policy agenda, and institutional restrictions on voting as explanations of the income-participation gap.

The Theory of Two Gaps

If extant explanations of the income-participation gap do not fit the evidence, then either new explanations, new evidence, or both are required. Here, the developmental

FIGURE 2 Despite Year-to-Year Fluctuations in Income, Economic Mobility Is Still Entrenched



model of voting offers one fruitful avenue for hypothesizing about the relationship between economic history, current economic status, and political participation. At the core of the developmental model is the idea that voting and nonvoting are habits that develop over the life course (Plutzer 2002). This development is characterized by three concepts: starting levels, growth, and inertia. *Starting levels* refer to differences in the propensity to vote upon entering the political process. Many adolescents-turned-newly-eligible-voters do not vote in their first election because they are still learning about politics and have not yet incurred many of the one-time costs required for voting (e.g., registering to vote, learning about the parties, and so forth). Over the course of young adulthood, many nonvoters become voters. They acquire the resources required for participation, are recruited into politics, and develop interest in political issues. This period of habit formation represents a time of *growth*. Finally, by the time individuals reach early middle adulthood, they have settled into voting and nonvoting habits. Turnout in future elections is thereafter largely driven by *inertia*.

The premise of the developmental model is that changes throughout the life course—starting in early childhood and continuing up until the present day—affect how, when, and why individuals participate in politics. Whether individuals are rich or poor in childhood will set them on a path toward participation or nonparticipation just as much as their current economic status will determine their level of participation. I call this argument the *two gaps hypothesis*. However, the extent to which economic history and current economic status matter will depend in part on age. Economic history will matter most in young adulthood when respondents are still developing a voting habit and least in late adulthood once voting and nonvoting habits have formed, whereas the opposite is true of current economic status. I call this argument the *life course hypothesis*.

Regarding economic history, the resources, socialization experiences, and mobilization required to vote in an individual's first election and to grow into a habitual voter accumulate over childhood and adolescence. Consider the development of cognitive ability. Cognitive ability is required for individuals to have knowledge about voter registration procedures, where and when to go to the polls, political preferences, the policy positions of candidates, and how their preferences map onto the candidates. The formative years of cognitive development are childhood and adolescence, and it is therefore what happens in these years that equips individuals with the cognitive ability necessary for participation (Sapiro 2004). Given that poverty can retard cognitive development (Hazzouri et al. 2017), it stands to reason that adults coming from poor childhood homes will be less equipped to vote compared to their rich counterparts.

A similar story could be told about economic history and the other resources, socialization experiences, and social connections that facilitate the development of a voting habit. The ramifications of childhood poverty are many and often politically relevant: lower-quality education, college attendance, and adult income (Duncan,

Ziol-Guest, and Kalil 2010); poorer physical and mental health (Chen, Matthews, and Boyce 2002); housing instability (Desmond 2016); and higher criminal arrest (Valentina, Spatz, and Sally 2011), to name just a few. Disparities like these shape participation and reinforce the idea that economic history matters for participation. Complementing these private roots of public action are the political sources of political activity (Burns, Schlozman, and Verba 2001). Here too, differences between poor and rich children abound. Studies in political socialization reveal economic discrepancies in efficacy, interest, discussion of current events, knowledge of political terms and processes, cognitive representations of government, and perceptions of parental interest (Easton and Dennis 1969; Hess and Torney 1967; Hyman 1959). Although research has yet to link economic history to political interest and partisan strength, such a connection seems plausible given that these attitudes form in young adulthood and remain stable through life (Green and Palmquist 1994; Prior 2010).

If childhood poverty is cumulatively disadvantaging because its negative consequences reinforce the initial poverty, then starting levels in turnout should exhibit large income disparities. Negative consequences—because they are cumulative and reinforcing—will have residual effects even when individuals escape poverty. Economic history should thus continue to have downstream effects on the growth into habitual voting. Once a voting habit has been formed, however, there are few reasons to expect that economic history will exert a *direct* effect. Its influence may still shape the more proximate factors that foster participation, but the inertia of voting is its limiting factor. Inertia means that individuals continue to vote or abstain until they are disrupted by an external force. Economic history cannot logically be this external force because it remains unchanged as individuals move into the future. I therefore hypothesize that economic history will be important in young adulthood when voting habits are forming but that its direct effect on turnout will dissipate in middle and late adulthood once habits have formed.

Next, consider the gap created by current economic status. Irrespective of economic history, current economic status can create conditions that impair starting levels and growth and disrupt the inertia of habit. Bouts of poverty or income loss resulting from unemployment, divorce, or unexpected medical expenses result in stress that distracts from activities like political participation. In contrast, a windfall of income can be a catalyst into politics. A pay raise leading to a bump in the tax bracket can create a newfound interest in the system that is taking a cut of the paycheck. A successful business venture can connect individuals to more politically minded business leaders, which results in peer-to-peer recruitment into politics. In these ways, a person's current economic status is relevant to his or her interest in politics and the decision to participate or abstain. I therefore expect that current economic status affects all aspects of habit formation—starting levels, growth, and inertia.

Nevertheless, I do not expect that the effect of current economic status is uniform across the life course. Young adults will be guided less by their current economic status in the process of habit formation than older adults will be once voting or nonvoting habits have formed. Given the anticipated weight of economic history in affecting the formation of a voting habit, it seems unlikely that current economic stress will also have an especially large effect during this period. Even among respondents who are not poor at the time of their first election, there will still be substantial nonvoting due to differences in economic history and the resources and socialization individuals bring to the political process. The demarcation between voters and nonvoters in young adulthood therefore has less to do with who is *currently* poor and rich, but rather with who comes from a poor and rich *background*. Even so, I expect that current economic status will increase in its relevance as economic history fades in importance and inertia sets in. The bouts of poverty or the windfalls in income act here as the external forces that disrupt inertia.

Data and Measures

To test my hypotheses, I analyze data from six studies: the General Social Survey, the 1979 National Longitudinal Survey of Youth, the Youth-Parent Socialization Study, the Children of the 1979 National Longitudinal Survey of Youth, the 1997 National Longitudinal Survey of Youth, and the Panel Study of Income Dynamics. Below, I briefly describe each study and its relevant measures. More information about the studies, including the question wording, response options, and descriptive statistics of the variables, is described in the supporting information. Table 1 summarizes the key features and highlights important similarities and differences between studies.

The General Social Survey (GSS) is a nationally representative pooled cross-sectional survey of Americans on political, economic, and social issues from 1978 to 2012. Current economic status is measured by asking respondents about the total value of their family's pretax income in the year prior to the interview and then

TABLE 1 Features of the Data Sets Used in the Analyses

Data Source	Data Type	Age of Sample	Elections Analyzed	Additional Notes
GSS	Pooled cross-section (1978– 2012)	18–89	1976, 1980, 1984, 1988, 1992, 2000, 2004, 2008	Models include election fixed effects, odd-year dummy, and sampling weights
NLSY79	Longitudinal survey of individuals	41–50	2004	Models include sampling weight
YPSS	Longitudinal survey of parent-child dyads	26–50	1972, 1980, 1996	Models include individual random effects and year fixed effects
CNLSY79	Longitudinal survey of individuals in families	18–35	2004, 2006	Models include individual random effects and midterm dummy
NLSY97	Longitudinal survey of individuals	19–31	2004, 2006, 2008, 2010	Models include family and individual random effects, election counter, and midterm dummy
PSID	Longitudinal survey of individuals in families	18–27	2004, 2006, 2008, 2010	Models include family and individual random effects, election counter, and midterm dummy

allowing them to choose from a set of ordinal income categories. Because the categories and value of money change over time, I use a pseudo-continuous inflation-adjusted measure created by the GSS (for details, see GSS Methodological Report No. 101). This income value is then divided by a need standard, commonly called the poverty threshold or the Orshansky needs standard, as set forth by the U.S. Census Bureau (Fisher 1992). This value yields an income-adjusted measure of current economic status that is commensurable across time. Economic history is measured retrospectively by asking respondents, "Thinking about the time when you were 16 years old, compared with American families in general then, would you say your family income was—far below average, below average, average, above average, or far above average?" Voter turnout is measured as self-reported voting in the most recent election, which for these data are the 1976, 1980, 1984, 1988, 1992, 2000, 2004, and 2008 presidential elections.

Two studies capture respondents primarily in middle adulthood. The 1979 National Longitudinal Survey of Youth (NLSY79) is a nationally representative longitudinal sample of 12,000 Americans collected by the Bureau of Labor Statistics. Current economic status is measured as the pretax total of each family member's income in the year prior to the election, which is adjusted for the family's needs standard. Economic history is measured as the respondent's median needs-adjusted income of the preadult years. Voter turnout is measured for the 2004 and 2006 elections. The Youth-Parent Socialization Study (YPSS) is a nationally representative sample of over 1,000 parent-child dyads sampled in four waves from 1965 to 1997. The 1973, 1982, and 1997 waves contain information on self-reported voter turnout in the 1972, 1980, and 1996 presidential elections. Income in the YPSS is measured using income categories that change from wave to wave. To make economic status comparable across waves, I measure current economic status as the respondent's income percentile within a wave and economic history as the parent-reported income percentile from the 1965 wave when respondents were 18 years of age.

Three studies capture respondents primarily in young adulthood. The Children of the 1979 National Longitudinal Survey of Youth (CNLSY79), also a data set collected by the Bureau of Labor Statistics, is a nationally representative longitudinal sample of American children born to mothers from the NLSY79. It tracks over 6,000 respondents and includes questions about turnout in the 2004 and 2006 elections. The economic information comes from the NLSY79 and therefore reflects the income of the respondent's parents. The 1997 National Longitudinal Survey of Youth (NLSY97) is a nationally representative sample of Americans that follows about 9,000 respondents from childhood to young adulthood from 1997 to present. The study, also conducted by the Bureau of Labor Statistics, contains detailed information on economic status as well as voter turnout in the 2004, 2006, 2008, and 2010 elections. The Panel Study of Income Dynamics (PSID) is an annual and nationally representative longitudinal study of over 5,000 American families from 1968 to present. Voter turnout is measured as self-reported voting in the 2004, 2006, 2008, and 2010 elections. In each of these three data sets—the

CNLSY79, the NLSY97, and the PSID—the measurement of current economic status, economic history, and voter turnout is comparable to that of the NLSY79, although more detail on these measures, including information about income nonresponse, is provided in the supporting information.

Analytic Strategy

The analysis is composed of two parts. The first part examines whether there are in fact two income-participation gaps as my argument predicts. The second part analyzes whether the effects of current economic status and economic history on voter turnout change over the life course. I describe the strategy for testing these hypotheses in the first two subsections below. I then provide additional detail about the model estimation and the selection of control variables.

Testing the Two Gaps Hypothesis

To assess the possibility of two income-participation gaps, I use regression analyses of nationally representative survey data that include information on respondents' current and past economic status. First, I estimate the effect of current economic status on voter turnout. This model captures the standard approach to modeling the effects of economic status. Second, I estimate the effect of both current economic status and economic history on voter turnout. This model tests my theory of the two income-participation gaps. If there are in fact two gaps as I propose, then a positive and statistically significant effect of both current economic status and economic history on voter turnout should be consistently observed across analyses of the five studies. Because the structure of the data varies by data set, the model specification, including how I handle repeated cases in longitudinal data sets or election-specific changes in turnout over time, also varies. I elaborate on the model specification and estimation in more detail below.

Testing the Life Course Hypothesis

Two different tests assess whether age conditions the size of each income-participation gap. First, I expand the regression models to include interaction terms between age and each of the measures of economic status. If my hypothesis is correct, then the interaction term coefficient between age and current economic status should be positive, whereas the interaction term coefficient between age and economic history should be negative. Recent research on the best practices for testing conditional hypotheses when the dependent variable is binary indicates that a product term (e.g., current economic status × age) should be included in the regression model. Rainey (2016) argues that the product term should *always* be included, whereas Berry, Demerrit, and Easery (2010) argue that it should only be included when the theory underpinning the conditional hypothesis does not depend on the statistical compression that occurs in a logistic regression. Since my theory is not contingent on compression, past scholarship uniformly suggests that a product term rather than a second differences approach is more appropriate for testing my hypotheses.

Unfortunately, the interaction between age and economic status will be an imperfect test that is at risk of producing errors of the second kind. Because the NLSY79, CNLSY79, NLSY97, and PSID only capture a snapshot of the life course—including a small range of age values—the change in the effects of economic status over the life course within any of these studies is likely to be small and difficult to detect, potentially resulting in false negatives. I therefore employ a second test that compares the results within studies. One implication of my argument is that the size of the effect of economic history should be *larger* than the size of the effect of current economic status within the studies of younger respondents (i.e., CNLSY79, NLSY97, and PSID), but it should be *smaller* than the size of the effect of current economic status within the studies of older respondents (i.e., NLSY79 and YPSS). I therefore examine whether the results comport to this pattern within the longitudinal studies and use the results as additional evidence for or against my hypothesis.

Estimating the Models

The structure of the data varies across studies, so the estimation and specification of the regression models also vary across studies. Table 1 summarizes the details of the models for each data set. Since the GSS data are pooled cross-sections, the model is estimated using a logistic regression with weights to account for the unequal probability of selection into the sample and election fixed effects in the form of election dummy variables. Because respondents are asked about turnout in interviews occurring 2 and 4 years after the election, I also include a dummy variable ("odd year") to indicate whether the respondent was asked about turnout 4 years after the election or otherwise. The NLSY79 is a cross-section of data, so the models are logistic regressions that are weighted

to account for unequal probability of selection into the sample. The YPSS is a longitudinal data set of parent-child dyads with one child per family, so models are estimated as mixed effects logistic regressions with random effects for the individual (i.e., child) and election dummy variables. The CNLSY79 is a longitudinal data set of individuals nested in families over time, so the models are estimated as mixed effects logistic regressions with random effects for both individuals and families. The CLSY79 model also includes a midterm dummy since respondents were asked about turnout in both a presidential and midterm election. The NLSY97, like the CNLSY79 and the YPSS, is a longitudinal data set of individuals but without the family structure. Models are estimated as mixed effects logistic regressions with random effects for the individual as well as an election counter variable and a midterm dummy since respondents were asked about turnout in four consecutive elections—two presidential and two midterm. Finally, the PSID is similar to the NLSY97 but with the family structure, so the models are identical except that the PSID also includes random effects at the family level.

Selecting the Control Variables

The analyses control for several important predictors of voter turnout, including age, age squared, gender, race, parental education, region of residence, religious attendance, residential mobility, marital status, education, health, political interest, and partisan strength. These control variables were selected because they capture important demographic features of the respondent or because they represent different models of turnout and have been shown to consistently predict voting across studies, as found by a recent meta-analysis (Smets and Ham 2013). Specifically, the effect of resources on turnout is captured by age, age squared, gender, race, region, residential mobility, marital status, education, and health; the effect of mobilization is captured by frequency of religious attendance and partisan strength; the effect of socialization is captured by parental education; and the psychological effect on turnout is captured by political interest. Notably, I do not include a variable that represents the rational choice model of turnout (e.g., caring who wins, civic duty) because no suitable candidate variable is consistently available across data sets. A description of each variable is reported in the supporting information, along with its descriptive statistics and correlations with other variables for each data set.

Importantly, *how* these variables factor into my argument about the two income-participation gaps depends on how the variable relates to economic history. Because my argument is not that economic history has an effect above and beyond resources, mobilization, socialization, or psychology, but rather that its effect occurs through these things, I expect that the coefficient for economic history will shrink, nearing zero in many instances, once these factors are included in the model. I therefore make a distinction between what I call "precursor" variables, or variables that are contemporaneous with or orthogonal to economic history, and what I call "mediator" variables, or variables that are influenced by and account for the effect of economic history on turnout. If economic history does in fact matter to turnout, then its coefficient should be positive and statistically significant even when controlling for precursor variables. If economic history is working through the mediator variables, then controlling for them should render the effect of economic history statistically insignificant. To account for these differential effects, I estimate models that include only the precursor variables as well as models that include the precursor and mediator variables.

The proposed division between variables raises questions about how they should be categorized. Theory points to current economic status, religious attendance, residential mobility, marital status, education, health, political interest, and partisan strength as potential mediator variables, whereas age, gender, race, parental education, and region of residence are unlikely to mediate the relationship between economic history and turnout. However, rather than rely only on a theoretically motivated categorization of the variables, I also empirically model the relationship between economic history and each of the eight potential mediators to aid in the determination of categories. Only in instances in which the potential mediator is statistically significantly predicted by economic history in the expected direction and with some consistency across studies do I designate it to have "mediator" status. The results of this analysis, the details of which are reported in the supporting information, reveal that current economic status, education, health, political interest, and partisan strength should be categorized as mediator variables, whereas religious attendance, residential mobility, and marital status are more appropriately categorized as precursor variables.

Results

The results are divided into two sections that mirror the analytic strategy described above. First, I start by examining the possibility of two income-participation gaps. Then I look at whether and how age conditions the effects of economic status on voting.

TABLE 2 Both Current Economic Status and Economic History Affect Voting

	Model 1: Standard	Model 2: Two Gaps	Model 3: Precursors	Model 4: Mediators
Current Economic Status	0.612*	0.600*	0.465*	0.246*
	(.021)	(.021)	(.029)	(.058)
Economic History		0.092*	0.080*	−0.066
		(.020)	(.029)	(.060)
Precursors				
Age			0.062*	0.048*
			(.009)	(.019)
Age × Age			−0.0002*	−0.00004
			(.0001)	(.0002)
Gender: Female			0.097*	0.160
			(.046)	(.096)
Race: Black			0.143	−0.246
			(.080)	(.149)
Race: Other			−0.683*	−0.642*
			(.105)	(.228)
Parental Education			0.095*	0.063*
			(.007)	(.016)
Region: South			−0.114*	0.062
			(.050)	(.107)
Religious Attendance			0.724*	0.480*
			(.048)	(.100)
Residential Mobility			−0.288*	−0.319*
			(.049)	(.101)
Marital Status: Married			0.163*	0.262*
			(.048)	(.101)
Mediators				
Education				0.200*
				(.020)
Health				0.051
				(.061)
Political Interest				0.147*
				(.039)
Partisan Strength				0.600*
				(.050)
Election Years				
1980	0.186*	0.190*	0.277*	−0.161
	(.084)	(.084)	(.103)	(.172)
1984	0.169*	0.165	0.203	−0.049
	(.086)	(.086)	(.105)	(.218)
1988	−0.021	−0.014	0.089	−0.464*
	(.105)	(.105)	(.132)	(.208)
1992	0.143	0.141	0.148	−0.563*
	(.103)	(.103)	(.129)	(.275)
2000	−0.088	−0.086	−0.058	−0.279
	(.081)	(.081)	(.100)	(.178)

(Continued)

Table 2 Continued

	Model 1: Standard	Model 2: Two Gaps	Model 3: Precursors	Model 4: Mediators
2004	0.151	0.157	0.145	0.029
	(.083)	(.084)	(.104)	(.158)
2008	0.249*	0.253*	0.164	
	(.085)	(.085)	(.107)	
Odd Year	−0.016	−0.011	−0.082	−0.242
	(.041)	(.041)	(.052)	(.136)
Constant	0.186*	−0.060	−3.377*	−6.073*
	(.080)	(.097)	(.253)	(.570)
Observations	19,875	19,875	14,689	3,939

Note: Data come from the General Social Survey. Cell entries are coefficients from a logistic regression; standard errors are reported in parentheses. *$p < .05$ (two-tailed).

The Two Gaps Hypothesis

Table 2 reports the results of four regression analyses using the General Social Survey. Model 1 examines the bivariate relationship between current economic status and voter turnout and shows a positive and statistically significant effect. This finding is consistent with the standard specification of the income-participation gap. Model 2 adds economic history as an independent variable and finds that it also has a positive and statistically significant effect on turnout. This finding provides initial evidence for the two gaps hypothesis.

Model 3 includes the precursor variables, or those variables expected to affect voting and co-occurring with or theoretically orthogonal to economic history, which attenuates the size of the economic status coefficients but does not change the fact they are still positive and statistically significant. Adding the full set of control variables in Model 4 further attenuates the effect of current economic status, although it remains positive and statistically significant. The effect of economic history, in contrast, ceases to be statistically significant. This finding comports with my expectation that economic history influences the resources, socialization, mobilization, and psychology required for voting, such that the effect of economic history on turnout should dissipate once these variables are included in the model.

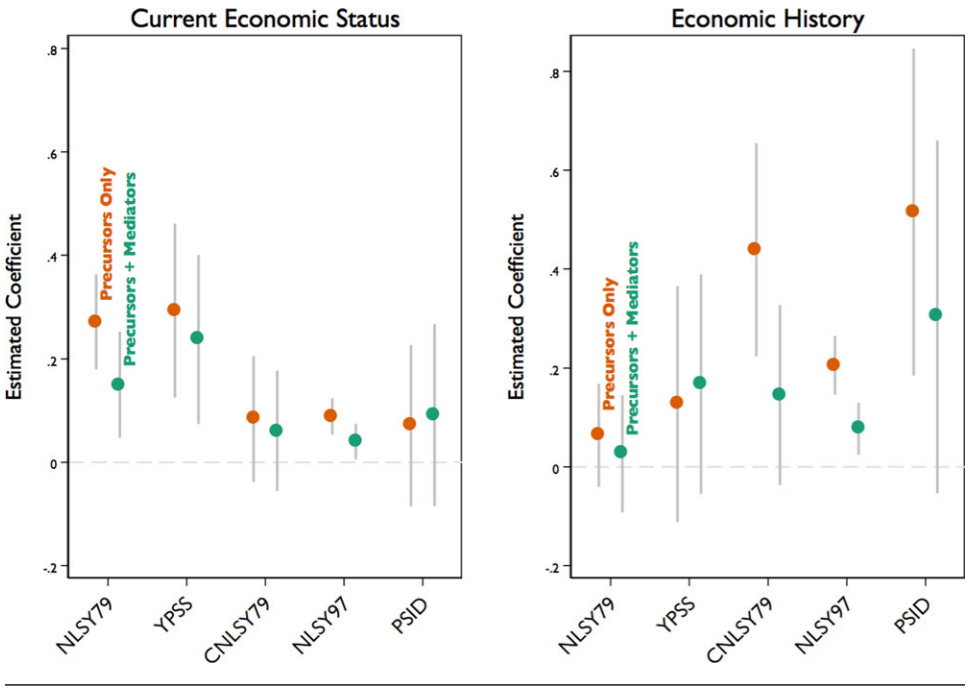
Figure 3 plots the coefficients for current economic status and economic history from Model 3 and Model 4 for the remaining five studies. The full results of these regressions, along with Models 1 and 2, are reported in the supporting information. The left panel of Figure 3 shows a positive and statistically significant effect of current economic status for the NLSY79, YPSS, and NLSY97 when only precursor control variables are included and when precursor and mediator control variables are included. The results from the CNLSY79 and PSID models show that the effect of current economic status is not statistically significant in either model, findings that are consistent with the idea that current economic status should matter less for young adults. Overall, four of the six studies show that individuals are more likely to vote when they have a better current economic status even after controlling for their economic history. This "rate of success" is higher than findings from a recent meta-analysis of voting that found income had a statistically significant effect in 21 of 40 published studies (Smets and Van Ham 2013).

The right panel of Figure 3 shows that economic history has a positive and statistically significant effect in the CNLSY79, NLSY97, and PSID when controlling for only precursor control variables. This effect disappears in the PSID and the CNLSY79 when the mediator control variables are included. This pattern is consistent with my argument, as I expect the coefficient for economic history to substantially decline once controlling for the resources, socialization, mobilization, and psychology that underpin voting. In the cases of the NLSY79 and YPSS, economic history does not have a statistically significant effect on voting in either model. This pattern is consistent with my life course hypothesis, as I predict that the effect of economic history will be small, and perhaps even negligible, in middle and late adulthood. Altogether, four of the six studies show that individuals are more likely to vote when they grow up in wealthier homes even after controlling for their current economic status.

The Life Course Hypothesis

The second part of my argument is that the size of each income-participation gap changes over the life course. Specifically, I argued that the effect of current economic

FIGURE 3 The Effect of Current Economic Status and Economic History Are Consistent across Studies



status on voting increases with age, whereas the effect of economic history does just the opposite. To test these arguments, I begin by expanding the GSS regression models to include interaction terms between the two measures of economic status and age. The results from these analyses are reported in Table 3 and provide evidence for my hypotheses. In Models 5 and 6, the interaction term coefficients are statistically significant in the expected direction. Consistent with my expectations, the conditional effect of economic history goes away once the mediator control variables are added in Model 6.

These results are depicted graphically in Figure 4 for current economic status and Figure 5 for economic history. Both graphs show the probability of voting over the life course for respondents of varying economic statuses. Figure 4 compares the average respondent in the top and bottom deciles of the current economic status distribution and shows that there is only a small difference in the probability of voting between these two respondents when they are a young age. In fact, the model that includes the mediator control variables shows that the predicted probability of voting for these two respondents is not statistically different until around the age of 30. By the time these respondents reach the age of 65, however, there is a statistically significant 20-point difference in the probability of voting.

Figure 5 compares the average respondents who report far above and far below average family incomes at the age of 16. In contrast to the results comparing current economic status, the results for economic history reveal substantial and statistically significant differences in the probability of voting between those who grew up rich and those who grew up poor when respondents are young but not when they are old. Differences in economic history persist until respondents are in their 40s, at which point differences in economic history no longer affect voting. As expected, however, the differences between those with a rich and poor economic history disappear once the mediator control variables are included in the model.

Is this pattern of change over the life course observed in the other studies? The answer is mixed. The inclusion of interaction terms produces inconsistent effects across the five longitudinal studies. The results of the NLSY79, YPSS, and PSID produce no statistically significant interaction effects, whereas the results of the CNLSY79 produce a statistically significant interaction between current economic status and age that is in the opposite direction of what is hypothesized. Still, the NLSY97 results are statistically significant and in the expected direction for both current economic status and economic history. The results, which are reported in the supporting information, are clearly inconsistent and at times contradictory.

One possible explanation for the null findings is that the models are not well suited to test life course hypotheses given the restricted age variation within each study. Notably, the GSS is the exception, and its results support

TABLE 3 The Size of Each Income-Participation Gap Changes over the Life Course

	Model 5: Two Gaps	Model 6: Precursors	Model 7: Mediators
Current Economic Status	0.110	0.001	−0.127
	(.066)	(.075)	(.145)
Current Economic Status × Age	0.010*	0.012*	0.009*
	(.001)	(.002)	(.003)
Economic History	0.392*	0.304*	0.206
	(.065)	(.086)	(.178)
Economic History × Age	−0.004*	−0.005*	−0.006
	(.001)	(.002)	(.004)
Precursors			
Age	0.064*	0.066*	0.058*
	(.009)	(.011)	(.024)
Age × Age	−(.0002)*	−(.0002)	−(.00003)
	(.0001)	(.0001)	(.0002)
Gender: Female		0.100*	0.161
		(.046)	(.096)
Race: Black		0.150	−0.246
		(.079)	(.150)
Race: Other		−0.675*	−0.647*
		(.104)	(.226)
Parental Education		0.093*	0.063*
		(.007)	(.016)
Region: South		−0.112*	0.058
		(.050)	(.107)
Religious Attendance		0.731*	0.491*
		(.048)	(.101)
Residential Mobility		−0.304*	−0.323*
		(.049)	(.101)
Marital Status: Married		0.149*	0.247*
		(.049)	(.101)
Mediators			
Education			0.197*
			(.020)
Health			0.042
			(.061)
Political Interest			0.144*
			(.039)
Partisan Strength			0.600*
			(.050)
Election Years			
1980	0.198*	0.259*	−0.142
	(.085)	(.103)	(.172)
1984	0.144	0.187	−0.038
	(.086)	(.105)	(.218)
1988	−0.040	0.074	−0.459*
	(.106)	(.131)	(.209)

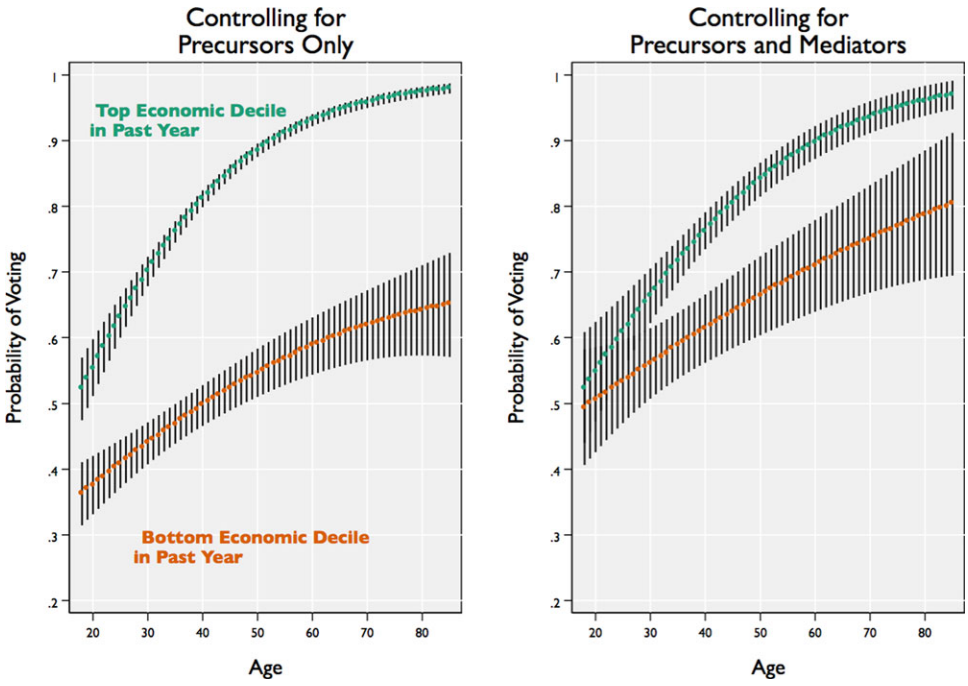
(Continued)

TABLE 3 Continued

	Model 5: Two Gaps	Model 6: Precursors	Model 7: Mediators
1992	0.082	0.120	−0.541*
	(.105)	(.128)	(.274)
2000	−0.079	−0.084	−0.287
	(.081)	(.100)	(.179)
2004	0.079	0.118	0.024
	(.084)	(.104)	(.159)
2008	0.159	0.135	
	(.086)	(.107)	
Odd Year	−0.074*	−0.081	−0.234
	(.042)	(.052)	(.137)
Constant	−2.524*	−3.574*	−6.482*
	(.271)	(.357)	(.798)
Observations	19,841	14,689	3,939

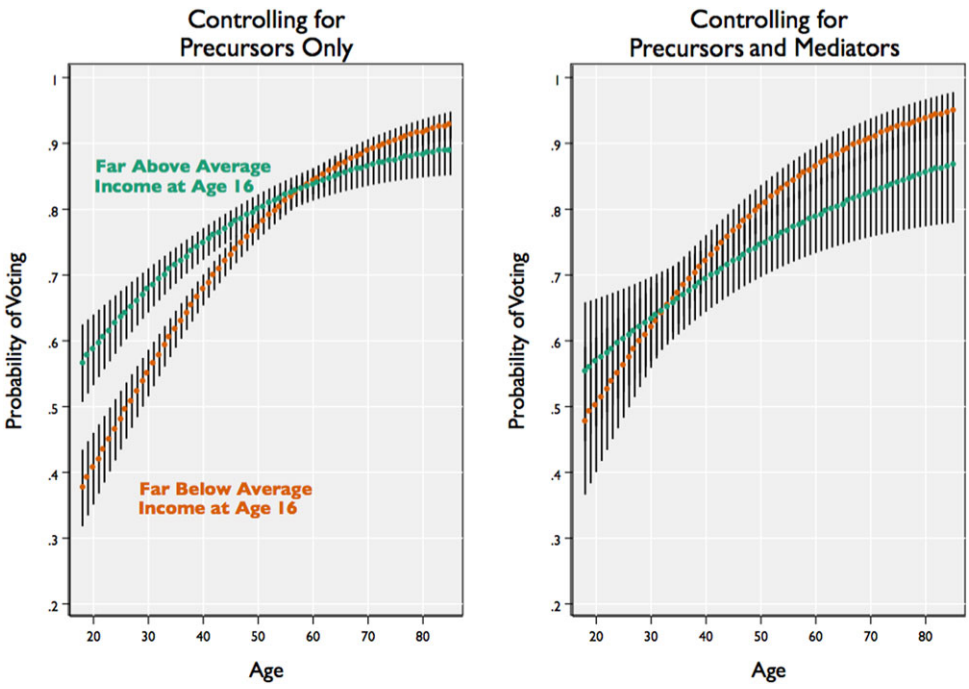
Note: Data come from the General Social Survey. Cell entries are coefficients from a logistic regression; standard errors are reported in parentheses. *p < .05 (two-tailed).

FIGURE 4 The Effect of Current Economic Status on Voter Turnout Grows over the Life Course (GSS)



my hypotheses. Still, alternative evidence for or against my hypotheses can be gleaned from within-study comparisons of the results from Figure 3. If my life course hypotheses are correct, then the effect of current economic status should be larger than the effect of economic history in the studies of older respondents and vice versa in the studies of younger respondents. In fact, just this pattern can be found in Figure 3. In the studies composed of younger respondents—PSID, CNLSY79, and NLSY97—economic history consistently has a larger effect than current economic status. The opposite is true of the NLSY79 and YPSS—studies that are composed of middle-aged adults on average—where the effect of economic history is not even statistically significant in the

FIGURE 5 The Effect of Economic History on Voter Turnout Shrinks over the Life Course (GSS)



precursor model. This finding is consistent with the GSS results that show economic history stops affecting voting in middle adulthood.

I also examine whether economic history conditions the effect of current economic status as a robustness check on the life course hypothesis. The strength of the results, which are reported in the supporting information, are consistent with those reported above. Taken together, the evidence for the life course hypothesis is mixed but suggestive. The hypothesis is supported with clear evidence from the GSS and is bolstered by a within-study comparison of the longitudinal data. Nevertheless, the findings should thus be treated cautiously and with an eye toward future testing and scrutiny.

Discussion

Due to its persistence, magnitude, and consequentiality, the existence of the income-participation gap has become a matter of fact in the study of American politics. In this article, I build on this canonical work by identifying the temporal incompatibility between the proposed explanations of the income-participation gap and the evidence used to prove its existence and by devising a measurement strategy for economic status that better reflects theories of political behavior. The findings show that there are two income-participation gaps and that these gaps together reveal larger income disparities in participation than prior research estimated. Secondarily, I provide evidence suggesting that these gaps manifest differently over the life course.

These findings magnify the troubling aspects of the connection between economic status and participation because they show that poor children's capabilities for political participation are diminished through no fault of their own. If the political cycle of poverty is the ways in which poverty reduces participation, degrades representation, and leads to policy outcomes that reinforce poverty (Piven and Cloward 1988), then what are the implications when poverty originates in childhood? One likely consequence is the absence of policies that are sensitive to the needs of poor children or that promote the American dream. Reality abounds in illustrating this possibility, ranging from the toxic levels of lead in the water in Flint, Michigan, to the deteriorating quality of public education in the nation's poorest communities. This study is thus important for understanding unequal representation as much as it is for understanding voter turnout.

The analyses also bring greater clarity to the origins of the income-participation gap. I show that the purported causes of the gap are primarily explanations of the effect of economic history and only marginally explanations of the effect of current economic status. Earlier,

I proposed that the stress, anxiety, and depression brought about by poverty and financial insecurity might contribute to the effect of current economic status, but future research should also consider the broader material and physical consequences of poverty and social immobility, including housing instability, food insecurity, mental health disorder, poor physical health, problems of crime and policing, educational inequalities, and employment difficulties. Additionally, identifying the effect of economic history reveals several points of investigation and intervention. More work should determine the degree to which the income-participation gap emerges because of the link between economic history and current economic status, education, health, political interest, and partisan strength *or* because of the link between these factors and political participation. The findings thus expand the number of avenues by which scholars can study this problem.

Still, however much these findings advance our understanding of income-based differences in participation, their contributions are bound by the limitations of what they can reveal. Economic status, for example, is interwoven with race, gender, disability, and geography in American society, so analyzing it without regard to its intersectional structure means our understanding of the two income-participation gaps is incomplete. The shortcomings of the data also limit the contributions of the study. The data skew younger, with the oldest respondents in the longitudinal studies only reaching middle adulthood. Poverty levels tend to rise in late adulthood as individuals retire from work, experience declining health, lose a spouse, or see their government benefits stagnate because of inflation. Studying these age-related changes in economic status is important for capturing the experience of this expanding portion of the electorate. The findings are also limited by the observational nature of the data. While the panel structure of the data sets and the multiplicity of studies allay some concerns, these strengths alone cannot overcome the barriers to causal inference inherent in observational data. The challenge here is that experimental designs are not appropriate when treating individuals' incomes or economic histories. Future research should look for ethical opportunities to draw on experimental research to confirm or refute the results I presented.

Despite years of research on the income-participation gap, income-based differences in participation persist. I brought this phenomenon into greater focus by showing that there are two income-participation gaps and that the size of these gaps fluctuate, over the life course. The findings confirm, magnify, and clarify the concerns expressed by previous scholars about this important problem. As Schlozman, Verba, and Brady recently concluded in *The Unheavenly Chorus*, "To listen more carefully to the accent of the unheavenly chorus and to take measures to include a more representative set of singers—would be a step toward delivering on the promise of the American Dream" (2012, 601). Like these scholars, my hope is that the findings reported here will inspire research that tackles the issues of poverty, social immobility, and unequal voice in a way that promotes political equality and representation for all.

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Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

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3 Variable Descriptive Statistics
4 Variable Correlations
5 Testing the Mediator Variables
6 Full Results
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8 The Conditional Effect of Economic Status
9 Income Nonresponse