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Income Changes Do Not Influence Political Participation: Evidence from Comparative Panel Data

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Income changes do not influence political participation. Evidence from comparative panel data

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Abstract
The income gradient in political participation is a widely accepted stylized fact. This article asks how income effects on political involvement unfold over time. Using nine panel datasets from six countries, it analyzes whether income changes have short-term effects on political involvement, whether effects vary across the life-cycle, and whether parental income has an independent influence. Irrespective of indicator, specification, and method (hybrid models, inclusion of lags and leads, error-correction models), we find neither significant short-term effects of income changes nor life-cycle variation in these effects. However, parental income does seem to affect political socialization. Descriptive evidence and latent-growth-curve modeling based on household panels show that participatory inequality by parental income is already large before voting age. Poorer voters do not catch up with their richer peers in their twenties. This implies an urgent need for research on (political) inequality in youth and childhood.

Keywords
Participation, political inequality, panel data, socialization, income

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Introduction

The income gradient in political participation is an important research topic and a pressing societal concern. A large literature has reported lower political involvement among voters with low income or other socio-economic problems (Aytaç et al. 2020; Erikson 2015; Dalton 2017; Gallego 2015; Lawless & Fox 2001; Marx & Nguyen 2018; Pacheco & Plutzer 2008; Solt 2008; Schlozman et al. 2012).

Although the underlying theoretical models differ, studies often assume that low income triggers social and psychological mechanisms that *situationally* inhibit political involvement (e.g., Rosenstone 1982).[1] However, recent scholarship has started addressing the related questions of whether income effects on political involvement are causal and how these effects unfold over time (Margalit 2019; Prior 2019). Based on the observation that political participation often becomes habitual with age and therefore resilient to external influences (Plutzer 2002), an emerging literature places economic hardship in the life-cycle (Akee et al. 2020; Emmenegger et al. 2017; Ojeda 2018; Prior 2019). A key implication is that cross-sectional income gaps tend to be confounded with previous experiences and, hence, do not reflect a direct causal effect. That said, the underlying theoretical arguments differ in important ways and the associated empirical evidence remains patchy. Moreover, experimental research outside political science has documented impressive short-term effects of economic scarcity on mental capacities (Haushofer & Fehr 2014; Mani et al. 2013; Schilbach et al. 2016; Vohs 2013) that are also crucial for political involvement (Denny & Doyle 2008; Fowler & Kam 2006; Holbein & Hillygus 2020; Ojeda & Pacheco 2019). Although this research has not yet been linked explicitly to political participation, it suggests that income changes could indeed have immediate effects on participation.

In sum, the question of whether short-term changes in income (or socio-economic position in general) are able to trigger short-term reactions in political involvement is theoretically ambiguous and still awaits comprehensive empirical assessment. A problem in the existing literature is that the few longitudinal studies typically rely on just one dataset (and rarely more than three). Given the idiosyncratic features of some panel datasets and contexts, this provides a weak basis for knowledge accumulation. We therefore analyze income changes in nine panel datasets from six countries (Germany, Netherlands, Spain, Switzerland, UK, USA). While all advanced capitalist democracies, these countries do provide reasonable variation in socio-economic patterns and political institutions. Consistent findings in this group, hence, are likely to be generalizable to Western democracies. We also wish to highlight this study's achievements in identifying a comparatively large number of panel datasets with political information as an independent contribution to political behavior research.

Our analysis of individual income trajectories, by far the most comprehensive of its kind, produces an important insight that is applicable across all contexts. Irrespective of method and operationalization, income changes have hardly any predictive power for short-term changes in the propensity to participate in politics. This holds for reported voting or voting intentions

[1] We use "political involvement" as an overarching concept capturing individuals' propensity to cognitively, emotionally, or behaviorally engage with politics. This helps us to summarize results and literature based on diverse indicators, such political interest, efficacy, or participation. When we discuss specific results, we do so with reference to the precise indicator.

as well as for attitudinal measures of political involvement. It is also robust to different ways of measuring income changes.

How can we make sense of this nonfinding? As mentioned above, some studies have explained the socio-economic gradient in participation with political habits that already emerge during socialization. Most prominently, Schlozman et al. (2012: 177) suggested that parental influences contribute to pronounced participatory inequality "at the starting line" of voting age. Hence, cross-sectional income gaps might reflect an early influence of parental income on political socialization that crystalizes into stable patterns of involvement or apathy. While a full treatment of this conjecture is beyond the scope of this article, we provide a first assessment based on two analyses. First, by merging parents and children in household panels in four countries, we descriptively show that "inequality at the starting line" is a generalizable phenomenon. Moreover, we observe that children from poorer families typically do not catch up in their twenties. Second, a latent-growth-curve analysis in the United Kingdom adds nuance to this picture. The multivariate analysis confirms that parental socio-economic status influences the starting level and development of political participation. However, parental education and political interest appear to be more relevant in determining socialization patterns. This shows the need to unpack parental socio-economic background into different components.

In sum, our analyses yield key insights into unequal political participation. While short-term income dynamics do not influence political involvement, youth and childhood experiences are a key source of inequality. We hence point to the urgent need for future research on socio-economic problems during these life stages.

Theoretical arguments: Income, voting, and the political life-cycle

By now, there should be little doubt that income correlates with political participation and that the poor in particular tend to abstain from voting. Yet, research has not conclusively answered whether we can think of this link as causal and *how* causality may operate (Akee et al. 2020). These questions are inseparable from the question of how income effects unfold over time. Three possibilities appear likely.[2]

First, income might be seen as a summary indicator of background factors unfavorable to political participation (Pacheco & Plutzer 2008). Income could relate to differences in, inter alia, health, housing conditions, education, class background, quality of social relationships, experiences of discrimination, ethnicity, personality, values, civic skills, or cognitive ability. In most cases, the effects of such background factors are likely to be cumulative and to contribute to a rather stable disposition to abstain or participate. To the extent that the income-participation link is "spurious" and really caused by these background factors, year-to-year fluctuations in income should have no short-term impact on political participation.

[2] We restrict the discussion to theoretical explanations relating to voters' individual situation. There is an additional literature linking participatory inequality to political elites and communication (Leighley & Nagler 2013; Marx & Nguyen 2018; Piven & Cloward 1988; Solt 2008). While the temporality of these mechanisms is far from obvious, they provide an alternative possibility to theorize dynamic links between income and participation.

Second, income could be directly causally related to participation. This would be the case if socio-economic problems create cognitive "opportunity costs" for engagement with politics, as argued by Rosenstone (1982) or Brody & Sniderman (1977). Going even further, several studies from psychology and behavioral economics have demonstrated that situational economic scarcity *immediately* impairs mental capacities through stress, cognitive load, and ego depletion (Haushofer & Fehr 2014; Mani et al. 2013; Schilbach et al. 2016; Vohs 2013). Short-term changes in income then could be decisive for how much time and attention potential voters allocate to politics, how easy they find it to process and recall political information, and how efficacious they feel (Marx & Nguyen 2018). If such effects are strong enough, they might situationally influence for example whether habitual political involvement is translated into actual voting (Holbein & Hillygus 2020).

Third, income might only have a direct causal effect at certain stages of the life-course. According to the "impressionable years" hypothesis, political behaviors and orientations are comparatively malleable until early adulthood and become increasingly resilient afterwards (Dinas 2013; Stoker & Jennings 2008). Accordingly, economic shocks could have more immediate effects at young ages but lose explanatory power during habituation (Emmenegger et al. 2017; Hassell & Settle 2017). Another possibility, linked to an even earlier life-cycle stage, is that socio-economic factors operate through parental transmission during youth and childhood (Akee et al. 2020; Schlozman et al. 2012: 177-198). Although studies rarely make explicit links to income effects (an important exception is Ojeda 2018), several do confirm that political inequality is a) already large at voting age and b) strongly influenced by parental characteristics (Cesarini et al. 2014; Jennings et al. 2009; Plutzer 2002; Prior 2019). To the extent that adults' current income and participatory inclination are jointly influenced by early (and, hence, typically unobserved) socio-economic experiences, income gaps in political behavior would be inflated in cross-sectional data.

State of existing research on the temporality of income effects

We are not the first to study the temporal dimension of the link between socio-economic hardship and political involvement. A number of recent studies have explicitly addressed the issue. Emmenegger et al. (2017) argued and showed with difference-in-difference matching based on German panel data a) that unemployment only depresses political interest during the impressionable years of early adulthood and b) that the negative effect of youth unemployment on interest and turnout lasts well into prime age. This confirms the suspicion that people's resilience to economic shocks grows over the life cycle and that present socio-economic variables can be biased by earlier experiences. However, Emmenegger et al. (2017) did not consider parental background as a preceding influence.

Ojeda (2018) goes in a similar direction, but his theory differs in crucial ways. He argued that family income during childhood influences turnout inequality at young age. The relevance of family background is crowded out by the effect of personal (current) income as people get older. This implies that current income should have direct and more or less immediate effects, at least from prime age onwards—which is the opposite of what Emmenegger and colleagues argued. Unfortunately, the empirical results are impossible to compare because Ojeda largely relied on age-income interactions in cross-sectional data and because the dependent variables and countries differed. Moreover, Ojeda's random-effects estimators cannot be used to isolate short-term variability, because they do not decompose within- and between-variation. Nonetheless, a key insight of Ojeda's study (which we follow in the second part of the paper) is that socio-economic family background should be modeled explicitly, in particular regarding young citizens.

Closely related to our work, but with a narrower focus on political interest, is Prior's (2019) comprehensive study. He used fixed-effects and first-difference models with distributed lags based on British, German, and Swiss panel data to show that income changes have no systematic influence on political interest (chapter 12). This is true for year-to-year fluctuations and two-year long-run effects. Although he separately demonstrated that children are strongly influenced by parents' political interest (chapter 11) and education (chapter 8), he did not explicitly address the influence of parental income on young voters' political involvement. According to Ojeda's (2018) reasoning, he hence missed one potential channel through which income might matter. Crucially, we also do not know whether Prior's results hold for other dependent variables than political interest.

Lahtinen et al. (2019) used Finnish register data and a sibling design to isolate effects of family characteristics on voting turnout. While they found strong effects of socio-economic background and of parents' voting in an earlier election, socio-economic background largely seemed to matter in the form of parental education and occupation. Parental income had a significant but small effect. This suggests that the effects of parental income should not be taken at face value (as in Ojeda 2018) when other parental characteristics are not controlled for. Support for this conclusion comes from the natural experiment of comparing adopted and biological children in Cesarini et al. (2014), which suggests that parental-income effects are partly attributable to prebirth factors. However, another natural experiment recently confirmed that positive income shocks for children from low-income families can have lasting positive effects on voting if they occur early in the life course (Akee et al. 2020).

Research questions

In sum, income effects on political participation are a broadly studied topic. The literature has only recently sought to causally understand this link by studying its temporal ordering. To date, the literature is far from conclusive. Studies on the topic have often considered individual cases and do not always build on each other; they thus provide little knowledge accumulation. Consequently, we lack clear theoretical and empirical knowledge on whether income gaps are causal and how they unfold over time. Many studies have indicated life-cycle variation, but we know little about a) how much short-term variability remains after the "impressionable years," b) how strong the influence of parental socio-economic status is, and c) which aspect of parental socio-economic status might matter. In our view, these knowledge gaps lead to three pressing research questions (RQ):

RQ1: Do income changes trigger short-term effects on political involvement?

RQ2: Do income effects on political involvement vary across the life cycle?

RQ3: How strong is the influence of parental income on the starting level and development of political involvement?

In some cases, it would be possible to formulate concrete hypotheses. For example, an *opportunity-cost hypothesis* would predict negative short-term effects of personal income drops. An *impressionable-years hypothesis* would predict that these effects are restricted to early adulthood, while a *two-income-gaps hypothesis* (based on Ojeda 2018) would restrict them to prime age and later years. The latter would overlap with a *starting-line hypothesis* in predicting early participatory inequality by parental income. As these examples illustrate, existing research allows a large number of nuanced but partly contradictory hypotheses. It also leaves some aspects under-theorized such as the influence of parental income on the development of political involvement. Against this background, we believe that the formulation of open research questions is more appropriate at this stage.

Research strategy

Addressing the research questions requires individual panel data. In an ideal situation, we would base our analysis on panels that a) are large enough to have a sufficient number of respondents in different income and age groups; b) sample households so that parental influences can be modelled explicitly; c) stem from different countries to allow for generalizable statements. Panels that fulfill criteria a) and b) *and* consistently include political dependent variables over time are rare. By screening several international studies, we identified nine panels from six countries that fulfilled our criteria to different degrees (Table 1). Unfortunately, we had to exclude a number of high-quality datasets that did not sufficiently cover politics (for instance, the "Household, Income and Labour Dynamics in Australia Survey"). That said, we have used a substantially larger number of datasets than comparable studies. We see this as an independent contribution beyond our concrete research findings. With data from Germany, Netherlands, Spain, Switzerland, UK, and the United States we can study income effects in diverse contexts. The included countries differ, for instance, in their party systems, welfare-state generosity, and levels of income inequality and unemployment. If the relationship between income and political involvement is similar across these contexts, it can be assumed to be a general feature of advanced capitalist democracies.

As already stressed, we know little about the precise temporal logic through which income effects on political involvement unfold. This is a theoretical lacuna, but it matters for methodological choices, because modeling strategies entail different assumptions about underlying dynamics. In any case, answering RQ1 requires us to decompose within-person changes over time and inter-individual changes. If political involvement is largely explained by cross-sectional income differences and unresponsive to income changes, we would have to conclude that any link is likely driven by spurious correlations (which would arise if personal income and involvement are influenced by family background, for instance). Effects due to changes within respondents can usually be isolated with fixed-effects (FE) estimation. We prefer hybrid models (Bell & Jones 2015), because they allow an explicit comparison of within- and between-estimators. The model has the form

$$POL_{it} = \beta_0 + \beta_1\left(INC_{it} - \overline{INC_i}\right) + \beta_2(x_{it} - \overline{x}_i) + \beta_3\overline{INC_i} + \beta_4\overline{x}_i + \beta_5 z_i + u_i + e_{it} \quad (1)$$

where the political involvement of individual i in period t (POL_{it}) is a function of income (changes). β_1 indicates the effect of (time-variant) income (INC_{it}) after a de-meaning transformation. This within-effect is identical to an FE model (Bell and Jones 2015) and hence does not suffer from heterogeneity bias. β_3 is the between-effect of the (time-constant) intra-personal mean income ($\overline{INC_i}$), which might be inflated by such bias. While the within-effect shows how changes of income within a person's lifetime are related to political participation, the between-effect shows whether those who have always earned more are also those who were always more politically involved. β_2 and β_5 are the effects of additional time-variant and time-invariant predictors x_{it} and z_i.

A key advantage of hybrid models is that they are relatively undemanding in terms of data structure. Because they only require information about political involvement and income over few waves, we could include all datasets in Table 1. An important downside pertains to the modelling of the underlying dynamics, which is restrictive and not necessarily realistic. The model in Equation 1 assumes that income changes between t_1 and t_2 fully exert their effect at t_2. Yet, this might not be the case if changes in political involvement predate income changes or follow them with a delay. For instance, income changes may be anticipated or influence political habits slowly. To account for these possibilities, we ran additional FE models including lag and lead variables, which would pick up any preceding or delayed effect:

$$POL_{it} = \alpha_i + \beta_1 INC_{it} + \beta_2 INC_{it-1} + \beta_3 INC_{it-2} + \beta_4 INC_{it-3} + \beta_5 INC_{it+1} + \beta_6 z_{it} + e_{it} \quad (2)$$

The model, which includes individual FE α_i, and time-varying control variables z_{it} captures the effect of income changes up to three years after (β_2, β_3, β_4) and a year before (β_5) they occur. The more lags and leads of income are included, the more informative the model. However, the number is limited by the need to observe enough panelists with a sufficient number of subsequent waves. We pragmatically chose a specification with three-year lags of INC_{it} and a one-year lead (assuming that income changes are rarely anticipated far into the future). This reduced the number of datasets we could include in the analysis (Table 1). Note that the model in Equation 2 can be estimated with income as a continuous variable and in the form of a binary "shock" variable (the operationalization is discussed below). The latter is useful to assess how relatively large income drops (as opposed to gradual changes) influence political involvement as discrete events. It also avoids the assumption that positive and negative changes have uniform effects.

An alternative way to model dynamic links between income and political involvement is provided by error correction models (ECMs), which are particularly useful for capturing effects distributed over several periods (De Boef & Keele 2008; Prior 2019). We included these models as robustness checks. Taken together, the three analytical strategies should be sufficiently flexible to capture various temporal dynamics through which income changes might influence political involvement.

Data

Based on the above-criteria, we used data from the British Household Panel Study and its follow-up Understanding Society (BHPS and UKHLS), the German Longitudinal Election Study (GLES) and Socio-Economic Panel (GSOEP), the Dutch Longitudinal Internet Studies for the Social Sciences (LISS), the Spanish Political Attitudes Panel (POLAT), and the Swiss Household Panel (SHP), as well as the Panel Study of Income Dynamics (PSID), the National Longitudinal Survey of Youth (NLSY97), and the General Social Survey Panels (GSS) for the United States. However, not all datasets provide sufficient information for all models. All surveys allowed us to estimate the hybrid models in Equation 1. Models with lags and leads (Equation 2) and error correction models are more demanding in terms of data structure, because they require researchers to observe individuals across more consecutive waves. We therefore restricted the estimation of these models to the BHPS/UKHLS, LISS, POLAT, SHP, and SOEP. These datasets measure political involvement in (almost) every wave and cover relatively long time periods.

We measured political involvement flexibly with a wide range of variables, depending on availability in the datasets. This included (intended) voting as well as political interest, political efficacy, knowledge, and news consumption. In addition, we used party identification when available (Table 1). Our focus was on voting (intention) for numerous reasons. It is the central form of participation in democracies, it plays a dominant role in existing research, and it is the variable most consistently included across datasets. We included attitudinal indicators of involvement, because they might be more responsive than political behavior, thus allowing us to detect more subtle changes. Moreover, as political variables are typically limited anyway in large panels, it would not have made sense to disregard this information.

As explanatory variables, we used both objective and subjective income measures. The objective situation was measured as deciles of household income. Deciles are easy to interpret and facilitate comparisons across time and countries. As a needs adjustment, we used a simple equivalence scale and divided income by the square root of the number household members. In Equation 2 we additionally modelled income shocks as dummy variables that take the value of 1 if a person experiences an income reduction between t_{-1} and t_0 of at least two deciles. Furthermore, we used individuals' subjective assessments of their personal financial or economic situation to check whether the role of socio-economic problems was based on the person's financial situation per se or their evaluation of it. Due to space concerns we present these models in the Supplementary Information. Finally, we controlled for age, education, and labor force status in all models, and for sex and migration status in the hybrid models. We only controlled additionally for race in the US data. With the exception of age and objective income, all quasi-metric variables were recoded to a scale from 0 to 10 in order to make results comparable across models. Age in years has been centered at 18 years of age. For binary dependent variables, we show the results of linear probability models. Results from logistic models indicate similar findings and can be found in the Supplementary Information (Table A-2 to Table A-10).

Table 1: Variables for Political Involvement and Modelling Strategies by Dataset

	BHPS	GLES	LISS	POLAT	SHP	SOEP	GSS	NLSY97	PSID
Dependent variables									
Vote at next election	X^{26a}*	X^{3}*	X^{11}*		X^{20}*	X^{3}*		X^{4}*	X^{7}*
Intention to vote	X^{4}	X^{3}	X^{3}	X^{6}		X^{2}			
Political Interest	X^{22}	X^{3}	X^{11}*	X^{6}	X^{20}	X^{34}	X^{4b}*	X^{4}	
Internal Efficacy	X^{3}	X^{3}	X^{11}	X^{6}					
Political Knowledge		X^{3}							
Media Use for Political Purpose		X^{3}		X^{6}					
Importance of Elections & Campaigns		X^{3}							
(Strength of) Party Identification				X^{6}		X^{35}			
Duty to Vote	X^{4}			X^{6}					
Benefits of Voting	X^{4}								
Participation in Polls					X^{14}				
Participation Index			X^{11}	X^{6}					
Modelling strategy	X								
Hybrid model	X	X	X	X	X	X	X	X	X
Lags-and-leads model	X		X		X	X			
Error correction model	X		X		X	X			
Latent-growth curve model	X				X				

Note: Superscripted numbers refer to the number of waves the respective variable was surveyed.
[*] Dummy variable.
[a] Combined measure of voting at the next election and support of a political party.
[b] Interest in international affairs and in military policy.

Findings

Subjective and objective income

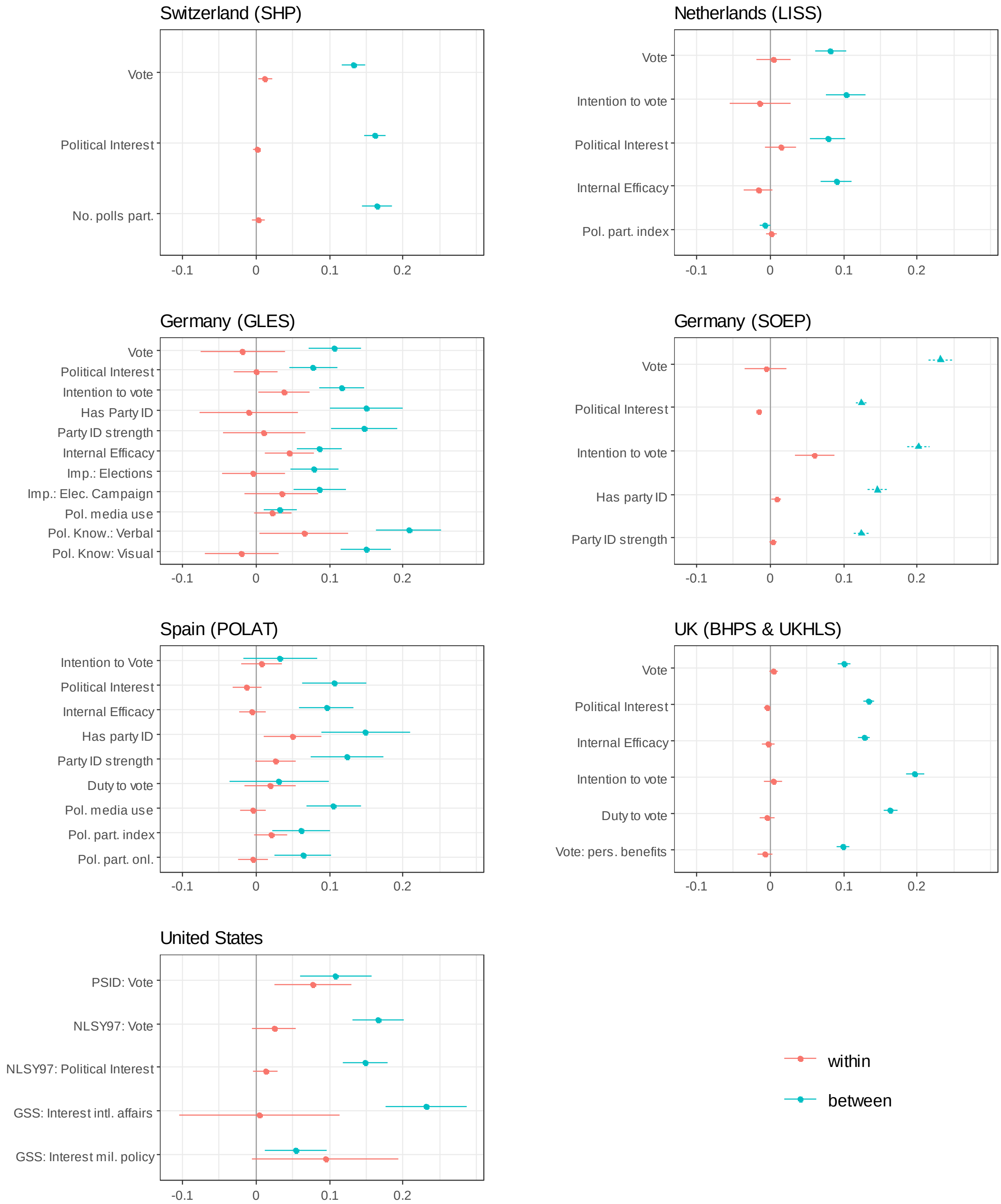
As a plausibility probe, we began by regressing subjective on objective income using the models presented in Equations 1 and 2. This preliminary analysis allowed us to assess the extent to which these models are able to capture the intuitive link between income changes and changes in income satisfaction. Put briefly, the results (presented in Supplementary Information Table A-1) show significant effects in the expected direction in most cases. Hence, it appears that our operationalizations and specifications are suitable, in principle, to study the effects of income changes.

Within- and between-effects of income on political involvement

The first step of our main analysis was to investigate the effect of income on indicators of political involvement within and between individuals. Figure 1 graphically displays models based on Equation 1 in which income is operationalized in deciles of equivalized household income.

Although there is some variation across countries and variables, the decomposition yields a clear picture. The income gradient is driven by differences between individuals, while there are little to no effects for within-person change.

Figure 1: Within- and between effects of income on political participation



Note: Results for "Vote" (all data sets), "Political Interest" (LISS), "Has party ID" (GLES, SOEP, POLAT), and all variables in the GSS are performed using linear probability models. For readability, we rescaled their coefficients so that they indicate the effect of going from the lowest to the highest income decile. All other coefficients show the effect of a one-income-decile change on dependent variables scaled 1 to 10.

To begin with voting participation, all seven coefficients indicate significant and substantial between-effects. For better readability, we rescaled the coefficients for voting (and all other binary variables) so that they indicate changes from the lowest to the highest decile. Respondents from the highest income decile are between eight (LISS) and 23 percentage points (SOEP) more likely to vote than respondents from the lowest income decile. At the same time, a within-person change from the lowest to highest income decile has either no effect or a very small one (e.g., a 1.2 percentage point increase in the SHP). The same pattern holds for *intention* to vote in the BHPS/UKHLS and the LISS. In both German datasets, however, there are significant but small within-effects on voting intention of 0.04 (GLES) and 0.06 (SOEP) point changes for a one-decile change (recall that all continuous dependent variables are rescaled to a range from zero to ten). Again, these within-effects are smaller than the corresponding between-effects. The only dataset showing no significant between-effect is the Spanish POLAT.

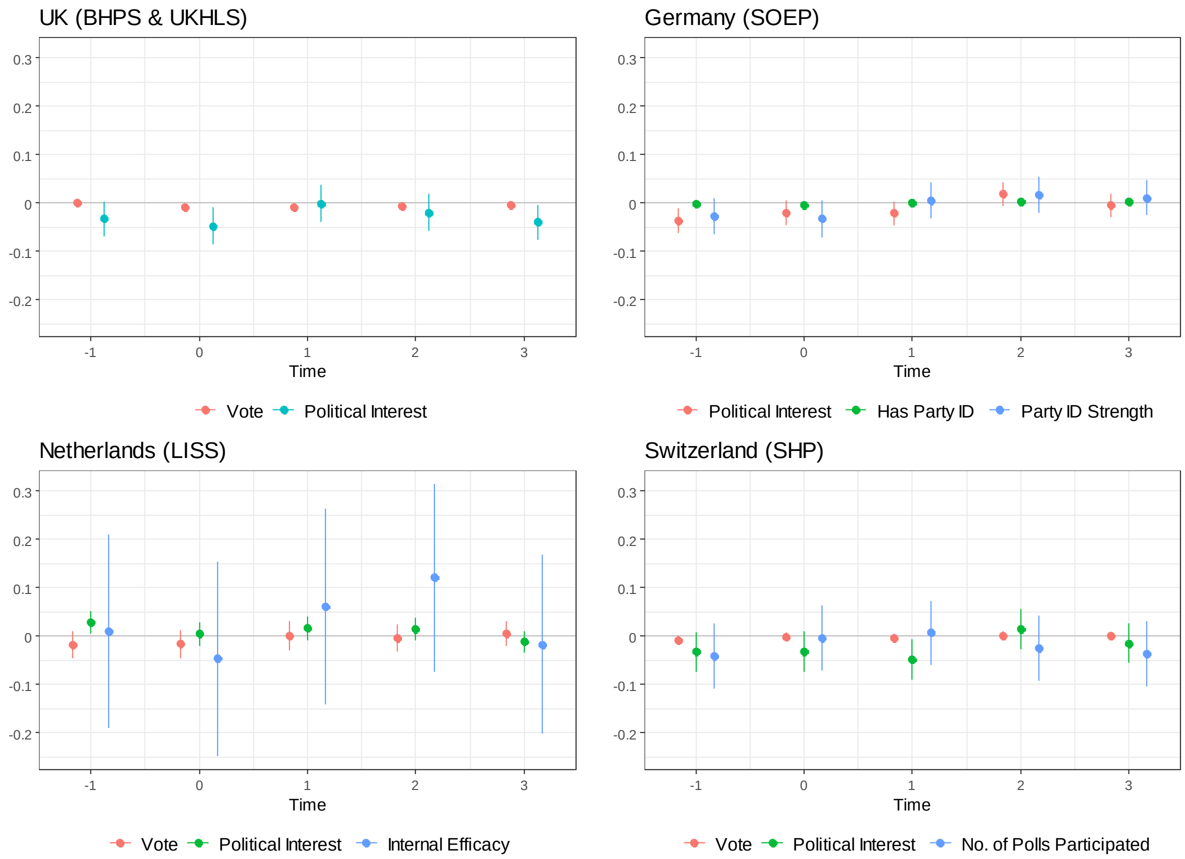
Political interest likewise shows substantial between-effects, ranging from a 0.08 (GLES) to 0.16 (SHP) point change for a one-decile income change. There are similar effects in the GSS, where respondents were asked about their interest in international and military affairs. Respondents in the highest income decile are about 23 percentage points more likely to be at least moderately interested in international affairs than those from the lowest income decile. Yet again, there is virtually no effect for income changes over time. Only in the case of the SOEP do we find a small and negative significant effect (-0.02).

This pattern continues for attitudinal indicators of political involvement, like internal efficacy, having a party identification and party identification strength. We also included a number of less common indicators, like the number of polls someone usually participated in during the previous year (SHP), an index of nonelectoral political participation (LISS), the subjective importance of elections and election campaigns (GLES), the subjective duty to vote, and the personal benefits of voting (BHPS/UKHLS). In all cases, we find broadly the same patterns described above. The same is true for indices of political media use (GLES and POLAT) and political knowledge (GLES).

Lags and leads of income changes

In a next step, we relaxed the (possibly unrealistic) assumption that the effect of income change unfolds fully in the following wave. To this end, we calculated FE models presented in Equation 2 that include three lags and one lead of income shocks (i.e., a decrease of income by two or more deciles compared to the previous period). The results in Figure 2 show that even if we account for anticipation and gradually unfolding effects, there is no evidence that would challenge our findings from the previous hybrid models. The experience of an income shock has a substantially negligible effect in the BHPS and UKHLS data; it merely decreases the probability to vote by one percentage point in the same period (t_0) and by even less in t_{+1} (0.7 percentage points). There is no anticipation effect in the sense that citizens' voting probability decreases when they expect a substantial drop in income. We also find no effect across all periods in the LISS and SHP.

Figure 2: FE-Models including lagged and leaded predictors



Note: The figure shows the effects of an income-drop of at least two deciles compared to the previous period. Results for the binary variables "Vote" (all data sets), "Political Interest" (LISS) and "Has party ID" (SOEP) are based on linear probability models.

This pattern largely holds for political interest. While there are a few significant effects, they do not add up to any consistent pattern across countries. In the UK, there is a drop of 0.05 units in t_0 followed by another 0.04 units in t_{+3}. In the SOEP, by contrast, we find a 0.04 drop in political interest in t_{-1}. Nevertheless, there are no changes in political interest when or after such a shock occurs. Analyses of the LISS data show that individuals experienced a slight increase of 2.8 percentage points in the probability of being fairly or very interested in politics in t_{-1}. Finally, analyses of the Swiss data show only a minor effect in t_{+1} (-0.05).

We find similar patterns with mostly null effects for a wide range of other indicators of political involvement. An income shock does not impact internal efficacy (LISS) in a meaningful way. It likewise has no effect on the probability of having a party identification and its strength (SOEP) or on expressions of support for a political party, e.g., by donating money or performing voluntary work (LISS). Overall, there is no evidence that income shocks have a substantial impact on political involvement. The few effects that we found are inconsistent and small given that the continuous dependent variables are measured on scales from zero to ten.

Robustness checks

As robustness checks, we ran the models based on Equation 1 and 2 using income deciles without needs adjustment (not shown) and income satisfaction as explanatory variables.[3] The results are substantially similar in magnitude and the patterns are again quite inconsistent (see Supplementary Information Figure A-1). For all binary dependent variables we ran logit models, which indicated roughly the same patterns as described here (not shown). We also tested an additional operationalization of shocks by including a dummy variable for job loss since the last wave (because of case numbers, this is only possible in the BHPS/UKHLS and the SOEP).[4] In the SOEP, job loss is linked to consistent but small changes in political interest in t_0 (-0.09), t_{+1} (-0.06) and t_{+2} (-0.11), but has no effect on having a party identification or party identification strength (Table A-11). We find no such effects in the British data.

Finally, we ran error correction models, which are first-difference models including a lagged dependent variable. Such models provide an additional way of investigating the dynamic relationship between political involvement and socio-economic problems. Yet, we again find only small effects, if any (see Supplementary Information Table A-12 to Table A-14).

Individual heterogeneity by age, income, and political involvement

Could our non-findings be explained by diverging patterns across sub-groups? As formulated in RQ2, income shocks might differ by age group. While Emmenegger et al. (2017) suggest that their importance should decrease with age, Ojeda's (2018) argument implies that they should increase. To assess the interaction of age and income, we included the product of both variables in our hybrid models following the de-meaning transformation for time-varying variables specified in Equation 1. While this is a standard procedure, the de-meaned age-income product does not yield a pure within-effect (Giesselmann & Schmidt-Catran 2020). Although the coefficient thus might partly reflect unobserved time-constant heterogeneity correlated with age, it allows a reasonable assessment of whether income effects differ by age. In almost all cases the interactions proved to be insignificant or substantially negligible (Table 2). Hence, based on this operationalization, there is no direct evidence that younger and older individuals differ in their short-term responsiveness of political involvement to income changes.

In addition, income shocks might be more relevant for respondents with already low income (Akee et al. 2020; Pacheco & Plutzer 2008; Rosenstone 1982). We analyzed this possibility by breaking down the "shock" dummy into a multi-category variable. Compared to the previous period, respondents in this operationalization can either experience an income increase, stability, or a one-decile drop (irrespective of previous income). In addition, we added separate outcomes in the form of a two-decile drop (or more) from a) the upper half of the income distribution, b) decile four and c) decile three. Stronger effects at the bottom of the income distribution should be captured by categories b) and c). Because of the smaller categories, we needed a larger sample and therefore restricted the analysis to the British data,

[3] Material not included in official Supplementary Information will be made available via the Harvard dataverse.

[4] The number of respondents experiencing job loss varies in each wave between 87 and 176 in the BHPS, 267 and 561 in the UKHLS and 87 and 453 in the SOEP.

with its large number of waves and respondents. As shown by the simple FE model in Table A-15 in the Supporting Information, large income drops at the bottom of the distribution have equally small and insignificant effects on voting and political interest.

Table 2: Overview of Interaction Effects for Age and Income on Vote in Hybrid Models

	BHPS	GLES	LISS	NLSY97	PSID	SHP	SOEP
W: Income	-0.007*	0.00	-0.00	0.00	0.00	0.01***	0.01***
	(0.003)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
W: Age	0.000	0.00	-0.00**	-0.01**	0.01**	0.00***	0.00***
	(0.001)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
W: Income*Age	0.000	-0.00	0.00	0.00	0.00	-0.00***	-0.00***
	(0.000)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
B: Income	0.107***	0.02***	0.01***	0.00	0.01*	0.02***	0.04***
	(0.006)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)
B: Age	0.032***	0.00***	0.00***	-0.01*	0.01*	0.00***	0.01***
	(0.001)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)
B: Income *Age	0.001***	-0.00*	-0.00*	0.00**	0.00	-0.00***	0.00***
	(0.000)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)

Note: Income is measured as equivalized household income in deciles from 1 to 10. Age in years is centered at the age of 18. W indicates within effects, B refers to between effects. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Finally, the detrimental effects of income shocks might be restricted to respondents with low initial political involvement, who lack the stabilizing force of habituation (Hassell & Settle 2017). To address this possibility, we followed the same procedure as for the age-income interaction, but this time multiplying income with political interest lagged by two periods. We used this model to predict voting in a hybrid model, again, only in the British data. Lagging political interest by two waves should ensure that we condition on pre-shock involvement. Again, the interaction effect turned out to be insignificant (Table A-16 in the Supporting Information).

To sum up the empirical evidence so far, we clearly find that income changes do not influence political involvement. The answer to RQ1, hence, is a resounding *no*. Based on an interaction term of age and income change, we also have to answer RQ2 in the negative. We find no evidence that young people are more or less responsive to income changes. The same is true for low-income earners and respondents with low initial interest in politics.

Does this mean that income is irrelevant for political involvement? We believe this conclusion would be premature. Between-effects of income are likely inflated due to spurious correlations. But, as discussed in the theory section, the omitted unobserved factors driving the spurious correlations could themselves be shaped by income differences. This would be the case if, for instance, parental income in adolescence influences political socialization, which crystalizes into stable patterns with age. In other words, our previous analysis might suffer from an "initial conditions problem" (Denny and Doyle 2009) and the unobserved initial conditions might very well be related to (parental) income. To address this issue and RQ3, we analyze political-involvement trajectories in early adulthood.

Trajectories in political involvement

To give an overall impression for the development of political involvement over the course of early adulthood, Figure 3 plots descriptively the probability to vote by age and by parental household income. It is restricted to four datasets that contain parental data. To create large enough samples of young adults, we pooled available waves and grouped respondents by age. We then grouped respondents by parental equivalized household income into three categories: lowest two deciles (bottom 2), fifth and sixth decile (mid 2) and top two deciles (top 2).

In three countries there is a considerable income gap in voting propensity already at the age of 18. In the UK, there is a roughly 15-percentage-point gap in voting propensity for first time voters depending on whether they come from a low- and medium- (around 60 percent) or high-income household (around 75 percent). Although there are some fluctuations over time, citizens from low-income households never catch up. A major gap remains after the first ten years of vote eligibility.

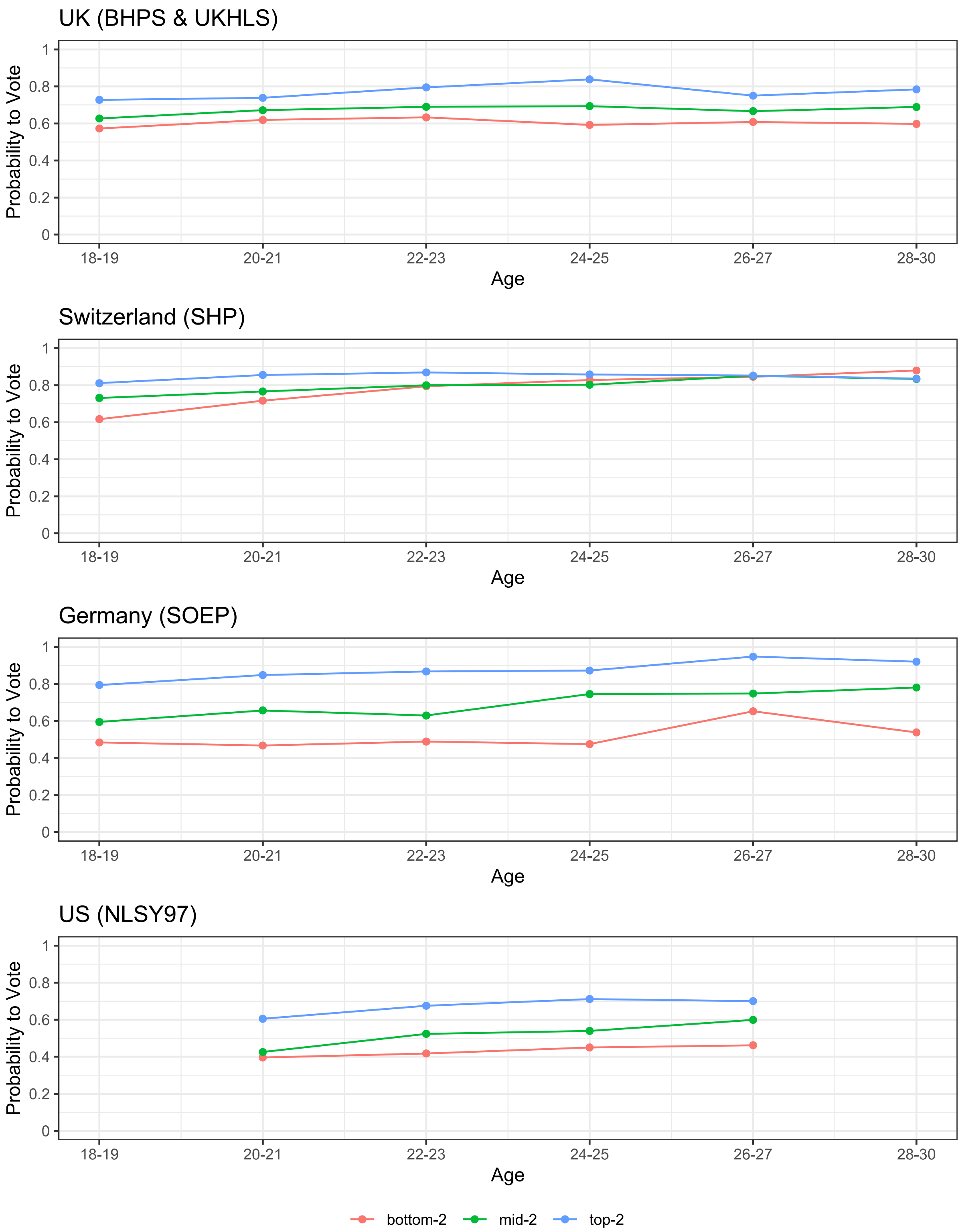
The initial gap between low- (50 percent) and high-income backgrounds (80 percent) is even higher in Germany. Although the development is somewhat more positive for medium-income backgrounds, there is little upward progress for the poor. Coming from a wealthy background, on the other hand, makes it almost certain that someone will vote ten years later.

In Switzerland, there is a slightly different pattern. The differences at the starting line are again substantial, with a gap of around 20 percentage points. However, these differences disappear over time. After ten years, citizens from low-income backgrounds have caught up, reaching an almost 90-percent probability to vote. These patterns are, however, almost certainly biased by design artefacts in the SHP that lead to enormous over-reporting of voter participation.[5]

Finally, there are substantial differences in the American cohort data, too. The probability to vote in a presidential election is around 40 percent for individuals from low- or medium-income backgrounds. For the top deciles, it is 60 percent. This gap widens further over time. While the curve is nearly flat for citizens from poorer families, it climbs to around 70 percent for those with high parental income.

[5] It is difficult in the SHP coding to distinguish voters from non-voters. Furthermore, interviewers were specifically instructed to ask non-voters again about who they would potentially vote for. This might contribute to the overestimation of voting in Switzerland, which usually has a turnout of only between 40 and 50 percent.

Figure 3: Probability to vote by age and parental income



Note: Pooled cross-sectional data. Parental income is measured as equivalized household income deciles. Because of the design of the NLSY97 cohort study, there are no or few respondents aged 18-19 and 28-30.

These figures indicate a strong influence of parental income on the political development of their offspring. They suggest that political inequality by income is, indeed, already highly unequal at the "starting line." This is a potentially powerful explanation for why our previous analyses failed to yield significant effects: The processes underlying political inequality seem to occur prior to entering the panels our results are based on. That said, the descriptive results could obviously result from spurious correlations. Specifically, they can say little about whether parental income causes unequal participation by first-time voters or whether it is confounded by other factors, such as parents' education or political involvement. They do not account for the fact that individuals from rich and poor families may differ in consequential ways, most notably regarding education.

To at least partially remedy this problem, we ran latent growth curve models (LGCMs) to see how income gaps unfolded within individual life courses when applying individual and parental controls. LGCMs were a suitable method, because they allowed us to separately estimate the effect of predictors on the intercept (starting level) and slope (growth) of trajectories. Moreover, they are an established tool in political socialization research (Plutzer 2002; Prior 2019). We focused on respondents who entered the panel at or below the age of 18 and for whom there was parental data available, either on the mother or father. This allowed us to capture respondents' conditions prior to their first act of voting. As such a data structure is quite demanding, the UKHLS is the only panel dataset that fulfills all these criteria and contains a large enough sample size for our analyses. We also considered the SHP, which qualified in principle, but coding problems made the results difficult to interpret (see Note 5 above). We present the SHP results in the Supporting Information. The models thus include UKHLS waves 1 through 9 and a total number of 13533 respondents. Technically, we ran all models in Mplus via the *MplusAutomation* package for R (Hallquist and Wiley 2018). Using an ML estimator for calculation, we predicted trajectories for our binary dependent variable (voting).

In the most basic model, we estimated the effect of parental income on respondents' starting level of voting propensity and the slope of any increase or decrease over time. In later models, we controlled additionally for parental political interest and education. Again, we measured parental income in household income deciles, political interest as the highest level of the mother's or father's political interest on a scale from zero to ten, and education as the highest educational qualification obtained by either the mother or father. In all models, we controlled additionally for sex, education, and migration status.

The results are shown as probabilities to vote by age and parental variables. Individual control variables are held constant at the values for a nonmigrant woman with medium education (Figure 4). Each panel shows the starting levels of voting propensity at timepoint 0, i.e., the time when respondents enter the panel between ages 16 and 18. The upper panel shows that our hypothetical individual from a low/high income family has a 55/73 percent probability to vote. Hence, there is a gap of almost 20 percentage points depending on whether parents are in the second or ninth income decile. As indicated by the similar slopes, there is little change in this gap. After eight years, the difference is still at around 16 percentage points.

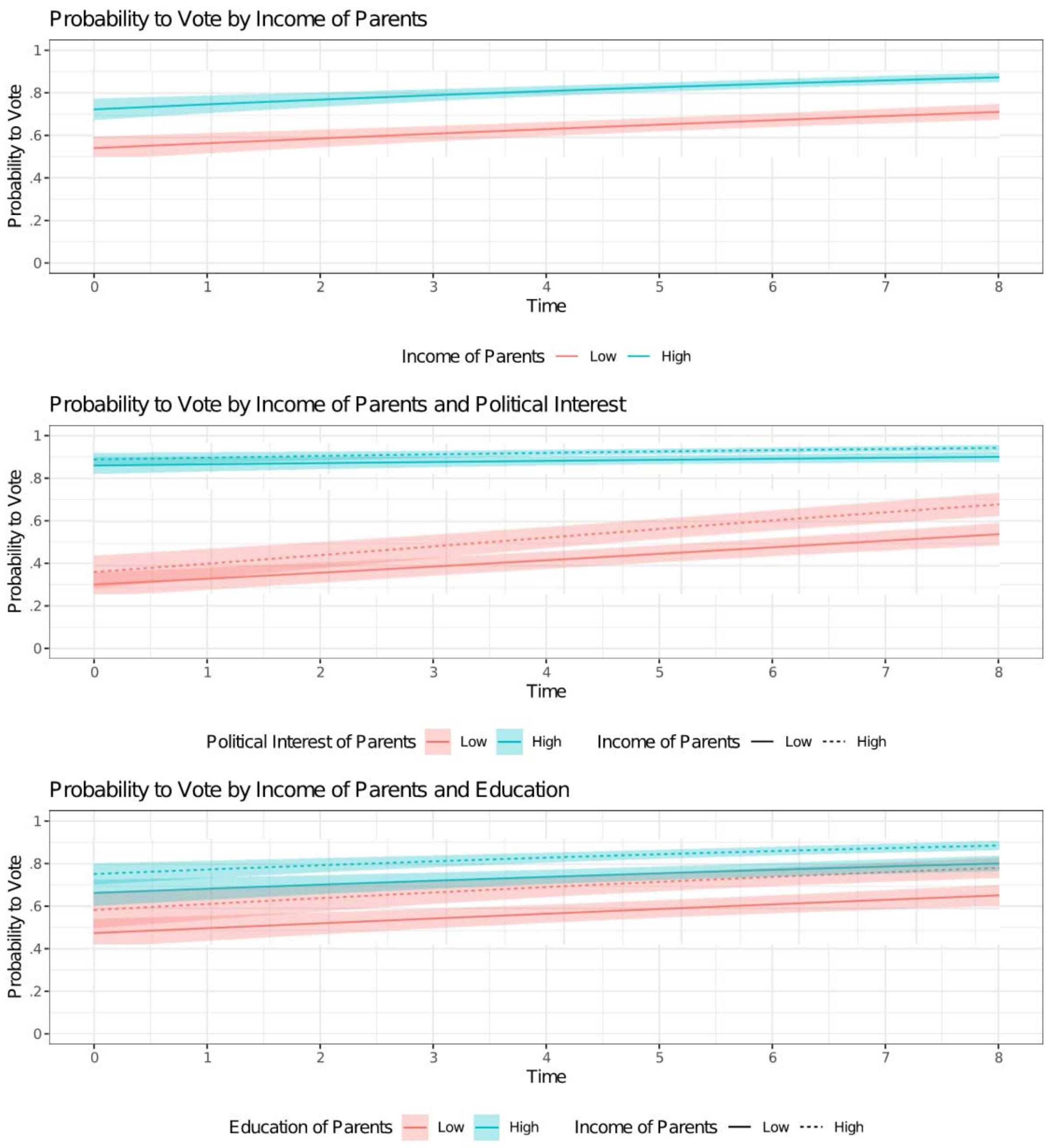
Importantly, the effects of parental income might be inflated because of a correlation with the more directly relevant variables of parental education and political interest. The center panel shows that parental political interest plays a much larger role than income. Low parental interest is linked to a voting probability of only about 35 percent, *regardless of whether the family is poor or rich*. If parental political interest is high, the propensity jumps to a whopping 87 percent. However, parental income has an independent influence on the slope at low levels of parental interest. Hence, high income helps to at least partly compensate for low parental interest. Still, income is a considerably weaker predictor than interest (which is not entirely surprising, because it should be further away from participation in the causal chain). Although the turnout difference between high and low-interest families decreases substantially over time, it remains at around 35 percentage points (25 percentage points for high income families). These gaps are substantially higher than the differences between poor and rich families (13/4 percentage points for families with low/high political interest).

Finally, the bottom panel shows that parental education also has a slightly stronger impact on voting propensity than parental income. The educational starting gap is roughly 19/16 percentage points for low/high income. At the same time, the difference by income is merely around 10 percentage points at both levels of parental education. However, another way to look at the results is that parental income retains a substantial effect on participatory inequality when education is held constant. Again, this pattern continues over early adulthood.

In all models the differences by parental background are similar for young men (not shown), although the intercepts are on average higher and the slopes smaller.

We ran additional models for voting intention in the British data (Figure A-3) and voting in Switzerland (Figure A-4). Overall, the patterns are quite similar to those presented here for vote intention. Given the above-mentioned overreporting of voting in the SHP, the effects are less strong. But also here, the parental influence is clearly visible and political interest emerges as the strongest predictor. Thus, we have to conclude that parental income plays a significant but comparatively small role in the development of political involvement. Parental interest in politics is far more important and seems to translate into strong political participation from an early age.

Figure 4: Latent growth curve models for probability to vote in the United Kingdom



Note: Plots are based on predictions from latent growth curve models for non-migrant women with medium education. “Low” and “high” income of parents refers the second and the ninth equivalized household income deciles. For political interest these values represent either being “not at all interested” (low) or “very interested” (high) in politics. For parental education “low” indicates having no degree or a degree lower than GCSE and “high” indicates having achieved A-Levels or higher.

Conclusions

This article has provided the most comprehensive analysis to date of how income and political involvement are related in longitudinal data. Our study uses many datasets, specifications, and operationalizations, which helps us to avoid over-interpreting chance findings and provides a stronger basis for generalization. Indeed, we found a number of effects for single variables that might look relevant *in isolation*. However, our encompassing analysis has mostly revealed them to be outliers in the broader picture. In sum, we see this article as a step towards consolidating knowledge on important research questions that so far have received a rather disparate treatment. While it confirms some previous analyses, it challenges others.

Most clearly, our results confirm Prior's (2019: 270) verdict that "Income really does not affect political interest, no matter how we look at it" but generalizes them to a considerably larger number of countries and, crucially, measures of political involvement. Taken together, our results and Prior's (2019) results strongly support the theoretical position that political involvement is habitual and hardly influenced by short-term income changes. The often-reported negative correlation between income and voting is most likely spurious.

Regarding RQ2, we could not generalize the argument by Emmenegger et al. (2017) about the "impressionable years" as a period of heightened responsiveness to socio-economic shocks. We did not detect a significant interaction between young age and income changes as predictors of political involvement. A possible reason for the diverging findings is that Emmenegger et al. (2017) focused on youth unemployment, which might have distinct socio-emotional repercussions. Our findings are also inconsistent with Ojeda's (2018) argument that the importance of personal income increases over the life course. We do acknowledge, however, that the question of life-course variation deserves treatment in a separate paper in which finer-grained methods can be explored (such as the difference-in-difference matching in Emmenegger et al. 2017). It might also be necessary to explicitly model age-specific experiences, such as economic problems in conjunction with labor-market entry or family formation.

Importantly, we cannot rule out the possibility that income-related processes within the family prior to voting age interfere with political socialization. Indeed, our analysis of socialization trajectories shows that the income gradient in political participation is already large among first-time voters. Moreover, voters from low-income families do not, on average, catch up. At least tentatively, we can generalize the important finding by Schlozman et al. (2012) of inequality at the starting line beyond the US case. That said, we also reaffirm concerns that their notion of "socio-economic status" is too broad a concept when seeking to understand income effects (Lahtinen et al. 2019). In fact, differences by parental income are partly confounded with parental education and politicization (although an independent income effect remains when controlling for these variables).

Taken together, our findings demonstrate the need for future research to focus on the influence of socio-economic experiences during childhood and adolescence. Again, our approach certainly does not exhaust possible and useful research strategies. First and foremost, an effort comparable to the one in this paper will be necessary to compile panel data on younger respondents. As shown by Akee et al. (2020), we have to observe respondents as early as possible to capture all effects of socio-economic variables. As several household panels have started to use youth questionnaires and as education panels often include political variables, this appears increasingly feasible. Moreover, there is a need to develop clearer theoretical guidelines on the underlying psychological dynamics. For example, socio-economic problems might hamper political learning in the family because of stressed parents. But they might also operate through an impact on students' performance at school and other indirect mechanisms. Relatedly, researchers will have to develop designs that separate different aspects of socio-economic family characteristics and possibly study their interaction. In any case, understanding the socialization processes underlying political inequality should be a major research goal for political behavior scholars in the coming years.

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SUPPLEMENTAL INFORMATION

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Table A-1: Overview Link Between Subjective and Objective Income

	Hybrid-Models				Lags-and-Leads Models				
	HHINC		*Shock*		*Shock*				
	W	B	W	B	t_{-1}	t_0	t_{+1}	t_{+2}	t_{+3}
BHPS	0.09^{***}	0.19^{***}	-0.15^{***}	-0.33^{***}	0.088^{***}	-0.293^{***}	-0.161^{***}	-0.133^{***}	-0.100^{***}
GLES	0.18^{***}	0.37^{***}	-0.19	-0.23	-	-	-	-	-
GSS	0.03^{***}	0.05^{***}	-0.08^{***}	-0.12^{***}	-	-	-	-	-
LISS	0.09^{***}	0.20^{***}	-0.19^{***}	-0.26^{*}	-0.187^{***}	-0.286^{***}	-0.167^{***}	-0.119^{**}	-0.101^{*}
POLAT	0.15^{***}	0.13^{***}	-0.42^{***}	-0.35	-	-	-	-	-
SHP	0.14^{***}	0.28^{***}	-0.22^{***}	-0.35^{***}	0.073^{***}	-0.265^{***}	-0.223^{***}	-0.144^{***}	-0.087^{***}
SOEP	0.26^{***}	0.41^{***}	-0.41^{***}	-0.85^{***}	0.106^{***}	-0.461^{***}	-0.290^{***}	-0.189^{***}	-0.132^{***}

Note: Subjective satisfaction with income (DV) is measured on a scale from 0 to 10, HHINC refers to the equivalized household income deciles ranging from 1 to 10 in all datasets, except for the POLAT where individual income deciles are used. Shock is operationalized as a dummy that takes 1 if respondents experienced a drop in income by at least two income deciles compared to the previous period. Effects for the GSS are based on linear probability models, as satisfaction with income was coded as a binary variable. W indicates within-effects, B stands for between-effects. Entries for lags-and-leads models represent the effect of periods t-1, t0, t+1, t+2 and t+3. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A-2: Logit Hybrid Models (BHPS)

	Vote	Vote
Female	-0.508***	-0.511***
	(0.019)	(0.021)
Migrant	-0.112***	0.055
	(0.028)	(0.031)
W: Income	0.005	
	(0.003)	
W: Age	-0.016***	-0.006***
	(0.001)	(0.001)
W: Unemployed	0.076**	0.074*
	(0.027)	(0.030)
W: Not in labor force	0.068***	0.043*
	(0.017)	(0.019)
W: Edu: med	0.019	-0.046
	(0.056)	(0.069)
W: Edu: high	0.151**	0.075
	(0.056)	(0.069)
B: Income	0.107***	
	(0.004)	
B: Age	0.039***	0.039***
	(0.001)	(0.001)
B: Unemployed	-0.627***	-0.810***
	(0.056)	(0.059)
B: Not in labor force	0.477***	0.337***
	(0.026)	(0.026)
B: Edu: med	0.312***	0.387***
	(0.029)	(0.032)
B: Edu: high	0.940***	1.122***
	(0.026)	(0.028)
W: Sat. Income		0.014***
		(0.002)
B: Sat. Income		0.072***
		(0.005)
Constant	-0.122**	-0.004
	(0.040)	(0.044)
Var(W)	5.193***	5.454***
	(0.059)	(0.067)
N	545256	450220

W indicates within effects, B refers to between effects. Variables with no prefix indicate time-constant variables. Income is measured as equivalized household income in deciles from 1 to 10. Satisfaction with income ranges from 0 (not satisfied) to 10 (very satisfied). Age is centered at the age of 18. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A-3: Logit Hybrid Models (GLES)

	Vote	Vote	Has Party ID	Has Party ID
Female	-0.79**	-0.81**	-1.01***	-1.00***
	(0.30)	(0.30)	(0.14)	(0.14)
Edu: medium	0.78*	1.08**	0.38*	0.40*
	(0.34)	(0.34)	(0.18)	(0.18)
Edu: high	2.70***	4.68***	1.13***	1.15***
	(0.52)	(0.67)	(0.20)	(0.20)
W: Income	-0.06		-0.01	
	(0.09)		(0.04)	
W: Age	0.03	0.03	0.02	0.02
	(0.04)	(0.04)	(0.02)	(0.02)
W: Unemployed	-0.10	0.19	0.17	0.26
	(0.72)	(0.75)	(0.35)	(0.35)
W: Not in labor force	0.82	1.27*	0.04	0.05
	(0.54)	(0.56)	(0.23)	(0.23)
B: Income	0.39***		0.17***	
	(0.08)		(0.03)	
B: Age	0.05***	0.07***	0.05***	0.05***
	(0.01)	(0.01)	(0.01)	(0.01)
B: Unemployed	0.18	0.41	0.68	1.15**
	(0.54)	(0.58)	(0.38)	(0.40)
B: Not in labor force	0.38	-0.01	0.49**	0.34*
	(0.36)	(0.35)	(0.18)	(0.17)
W: Sat. Income		0.08		0.09*
		(0.09)		(0.04)
B: Sat. Income		0.35***		0.30***
		(0.07)		(0.04)
Constant	0.82	-0.02	-0.59*	-1.62***
	(0.52)	(0.59)	(0.27)	(0.32)
Var(W)	8.74***	8.90***	7.40***	7.46***
	(1.82)	(1.39)	(0.94)	(0.93)
N	4010	4073	5790	5885

W indicates within effects, B refers to between effects. Variables with no prefix indicate time-constant variables. Income is measured as equivalized household income in deciles from 1 to 10. Satisfaction with income ranges from 0 (not satisfied) to 10 (very satisfied). Age is centered at the age of 18. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A-4: Logit Hybrid Models (GSS)

	Int. international affairs	Int. international affairs	Int. military affairs	Int. military affairs
Female	-0.94***	-1.02***	-0.78***	-0.89***
	(0.15)	(0.15)	(0.14)	(0.14)
Edu: college	1.70***	2.14***	0.20	0.40**
	(0.18)	(0.18)	(0.15)	(0.14)
Race: black	-0.47*	-0.78***	-0.50**	-0.69***
	(0.20)	(0.19)	(0.18)	(0.17)
Race: other	-0.22	-0.27	-0.34	-0.36
	(0.28)	(0.28)	(0.24)	(0.24)
Migrant	0.24	0.13	-1.03***	-1.02***
	(0.26)	(0.25)	(0.21)	(0.21)
W: Income	0.00		0.11	
	(0.06)		(0.06)	
W: Age	-0.07	-0.08*	-0.01	-0.00
	(0.04)	(0.03)	(0.04)	(0.04)
W: Unemployed	0.03	-0.20	-0.07	-0.11
	(0.35)	(0.32)	(0.37)	(0.34)
W: Not in labor force	0.15	0.09	0.16	-0.13
	(0.30)	(0.27)	(0.33)	(0.30)
B: Income	0.25***		0.07*	
	(0.03)		(0.03)	
B: Age	0.03***	0.04***	0.02***	0.02***
	(0.01)	(0.00)	(0.00)	(0.00)
B: Unemployed	-0.28	-0.42	-0.17	-0.36
	(0.33)	(0.33)	(0.31)	(0.31)
B: Not in labor force	0.18	-0.25	-0.20	-0.34*
	(0.19)	(0.17)	(0.17)	(0.16)
W: Sat. Income		0.12		-0.44
		(0.21)		(0.23)
B: Sat. Income		0.60***		-0.02
		(0.17)		(0.16)
Constant	-0.52*	0.27	2.35***	2.73***
	(0.24)	(0.21)	(0.25)	(0.24)
Var(W)	5.38***	5.66***	2.59***	2.85***
	(0.84)	(0.81)	(0.57)	(0.56)
N	4227	4602	4231	4609

W indicates within effects, B refers to between effects. Variables with no prefix indicate time-constant variables. Income is measured as equivalized household income in deciles from 1 to 10. Satisfaction with income is a dummy coded 0 if respondents are not at all satisfied with the financial situation and 1 if they are (more or less) satisfied. Age is centered at the age of 18. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A-5: Logit Hybrid Models (LISS)

	Vote	Vote	Political Interest	Political Interest
Female	-0.47***	-0.46***	-1.60***	-1.61***
	(0.10)	(0.10)	(0.08)	(0.08)
Migrant	-0.87***	-0.78***	-0.10	-0.16
	(0.14)	(0.14)	(0.11)	(0.11)
W: Income	0.01		0.02	
	(0.03)		(0.02)	
W: Age	-0.03*	-0.02	-0.06***	-0.07***
	(0.01)	(0.01)	(0.01)	(0.01)
W: Unemployed	0.40	0.51*	-0.06	-0.16
	(0.21)	(0.20)	(0.13)	(0.13)
W: Not in labor force	0.03	0.09	-0.14	-0.12
	(0.17)	(0.16)	(0.10)	(0.09)
W: Edu: med	-0.07	-0.23	0.95***	1.09***
	(0.33)	(0.30)	(0.15)	(0.15)
W: Edu: high	0.50	0.19	1.13***	1.29***
	(0.45)	(0.42)	(0.23)	(0.22)
B: Income	0.17***		0.14***	
	(0.02)		(0.02)	
B: Age	0.04***	0.04***	0.05***	0.05***
	(0.00)	(0.00)	(0.00)	(0.00)
B: Unemployed	0.09	-0.05	0.41	0.30
	(0.32)	(0.32)	(0.28)	(0.30)
B: Not in labor force	0.57***	0.48***	0.73***	0.62***
	(0.12)	(0.12)	(0.10)	(0.10)
B: Edu: med	0.64***	0.76***	1.38***	1.35***
	(0.12)	(0.12)	(0.10)	(0.10)
B: Edu: high	2.02***	2.16***	2.99***	3.05***
	(0.15)	(0.15)	(0.12)	(0.12)
W: Sat. Income		0.08**		-0.00
		(0.03)		(0.02)
B: Sat. Income		0.28***		0.12***
		(0.03)		(0.03)
Constant	1.63***	0.54*	-0.04	0.02
	(0.20)	(0.26)	(0.15)	(0.21)
Var(W)	12.44***	12.09***	11.72***	11.56***
	(0.88)	(0.83)	(0.53)	(0.52)
N	31846	32151	48035	47676

W indicates within effects, B refers to between effects. Variables with no prefix indicate time-constant variables. Income is measured as equivalized household income in deciles from 1 to 10. Satisfaction with income ranges from 0 (not satisfied) to 10 (very satisfied). Age is centered at the age of 18. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A-6: Logit Hybrid Models (NLSY97)

	Vote
Female	0.34***
	(0.06)
Edu: College	1.29***
	(0.06)
US Citizen	-1.18***
	(0.14)
Race: black	0.70***
	(0.07)
Race: other	-0.32***
	(0.07)
W: Income	0.02
	(0.01)
W: Age	-0.06***
	(0.01)
W: Job status	0.00
	(0.00)
W: Income	0.12***
	(0.01)
W: Age	-0.00
	(0.01)
W: Job status	0.01***
	(0.00)
Constant	-2.24***
	(0.14)
Var(W)	2.61***
	(0.14)
N	18863

W indicates within effects, B refers to between effects. Variables with no prefix indicate time-constant variables. Income is measured as equivalized household income in deciles from 1 to 10. Age is centered at the age of 18. Job status refers to the cumulative weeks worked at all civilian jobs until the data of the interview. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A-7: Logit Hybrid Models (POLAT)

	Has Party ID	Has Party ID
Female	-0.43***	-0.51***
	(0.12)	(0.12)
W: Income	0.05*	
	(0.02)	
W: Age	-0.27***	-0.28***
	(0.03)	(0.03)
W: Unemployed	0.14	0.10
	(0.13)	(0.13)
W: Not in labor force	0.05	0.07
	(0.17)	(0.17)
W: Edu: high	0.08	0.11
	(0.25)	(0.25)
B: Income	0.16***	
	(0.03)	
B: Age	0.00	0.02
	(0.01)	(0.01)
B: Unemployed	0.03	-0.28
	(0.21)	(0.20)
B: Not in labor force	0.36	0.29
	(0.21)	(0.21)
B: Edu: high	0.70***	0.85***
	(0.14)	(0.14)
W: Sat. Income		0.01
		(0.01)
B: Sat. Income		0.07*
		(0.03)
Constant	0.60*	0.91***
	(0.26)	(0.26)
Var(W)	7.16***	7.31***
	(0.49)	(0.50)
N	10959	11057

W indicates within effects, B refers to between effects. Variables with no prefix indicate time-constant variables. Income is measured as personal income in deciles from 1 to 10. Satisfaction with income is a dummy coded 0 when the financial situation stayed the same or got worse compared to the previous year and 1 for when it increased. Age is centered at the age of 18. Standard errors in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A-8: Logit Hybrid Models (PSID)

	Vote
Female	0.30***
	(0.08)
W: Income	0.05**
	(0.02)
W: Age	0.12***
	(0.01)
W: Unemployed	0.13
	(0.10)
W: Not in labor force	-0.22*
	(0.10)
W: Edu: med	0.86***
	(0.16)
W: Edu: high	1.27***
	(0.18)
B: Income	0.07***
	(0.02)
B: Age	0.09***
	(0.02)
B: Unemployed	-0.06
	(0.15)
B: Not in labor force	-0.11
	(0.13)
B: Edu: med	0.37*
	(0.15)
B: Edu: high	1.36***
	(0.15)
Constant	-2.12***
	(0.20)
Var(W)	2.40***
	(0.19)
N	9186

W indicates within effects, B refers to between effects. Variables with no prefix indicate time-constant variables. Income is measured as equivalized household income in deciles from 1 to 10. Age is centered at the age of 18. Standard errors in parentheses. $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$.

Table A-9: Logit Hybrid Models (SHP)

	Vote	Vote
Female	-0.88***	-0.93***
	(0.05)	(0.04)
Migrant	-1.25***	-1.18***
	(0.05)	(0.04)
W: Income	0.02**	
	(0.01)	
W: Age	0.05***	0.05***
	(0.00)	(0.00)
W: Unemployed	-0.12	-0.10
	(0.08)	(0.07)
W: Not in labor force	-0.35***	-0.36***
	(0.04)	(0.03)
W: Edu: med	1.06***	1.05***
	(0.06)	(0.06)
W: Edu: high	1.12***	1.17***
	(0.09)	(0.08)
B: Income	0.18***	
	(0.01)	
B: Age	0.04***	0.03***
	(0.00)	(0.00)
B: Unemployed	-1.65***	-1.26***
	(0.28)	(0.22)
B: Not in labor force	-0.07	-0.42***
	(0.07)	(0.06)
B: Edu: med	1.14***	1.16***
	(0.08)	(0.06)
B: Edu: high	1.97***	2.09***
	(0.09)	(0.07)
W: Sat. Income		0.00
		(0.01)
B: Sat. Income		0.25***
		(0.01)
Constant	0.42***	-0.50***
	(0.09)	(0.10)
Var(W)	5.35***	5.19***
	(0.15)	(0.12)
N	121590	153415

W indicates within effects, B refers to between effects. Variables with no prefix indicate time-constant variables. Income is measured as equivalized household income in deciles from 1 to 10. Satisfaction with income ranges from 0 (not satisfied) to 10 (very satisfied). Age is centered at the age of 18. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A-10: Logit Hybrid Models (SOEP)

	Vote	Vote
Female	-0.24***	-0.31***
	(0.05)	(0.04)
Edu: med	0.59***	0.76***
	(0.06)	(0.05)
Edu: high	1.70***	1.97***
	(0.07)	(0.07)
Migrant	-0.99***	-1.13***
	(0.10)	(0.10)
W: Income	-0.01	
	(0.02)	
W: Age	-0.00	-0.01
	(0.01)	(0.01)
W: Unemployed	0.15	0.12
	(0.13)	(0.13)
W: Not in labor force	-0.17	-0.12
	(0.09)	(0.09)
B: Income	0.28***	
	(0.01)	
B: Age	0.04***	0.05***
	(0.00)	(0.00)
B: Unemployed	-0.82***	-1.24***
	(0.12)	(0.12)
B: Not in labor force	0.15*	-0.14*
	(0.06)	(0.06)
W: Sat. Income		-0.00
		(0.02)
B: Sat. Income		0.22***
		(0.01)
Constant	-1.23***	-1.02***
	(0.09)	(0.10)
Var(W)	5.37***	5.58***
	(0.26)	(0.26)
N	50745	52474

W indicates within effects, B refers to between effects. Variables with no prefix indicate time-constant variables. Income is measured as equivalized household income in deciles from 1 to 10. Satisfaction with income ranges from 0 (not satisfied) to 10 (very satisfied). Age is centered at the age of 18. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A-11: FE-Models including lagged and leaded predictors (job loss)

	BHPS & UKHLS		SOEP		
	Vote	*Pol. Interest*	*Pol. Interest*	*Has Party ID*	*Party ID strength*
Job loss$_{t0}$	-0.020	-0.087	-0.09**	-0.01	-0.04
	(0.010)	(0.063)	(0.03)	(0.01)	(0.05)
Job loss$_{t-1}$	-0.003	-0.066	-0.06*	-0.00	-0.06
	(0.008)	(0.050)	(0.03)	(0.01)	(0.04)
Job loss$_{t+2}$	-0.008	0.020	-0.11***	-0.01	-0.03
	(0.008)	(0.048)	(0.02)	(0.01)	(0.04)
Job loss$_{t+3}$	0.009	-0.011	-0.03	0.00	-0.05
	(0.008)	(0.053)	(0.02)	(0.01)	(0.04)
Job loss$_{t-1}$	-0.002	-0.088	-0.03	-0.00	-0.10*
	(0.008)	(0.050)	(0.03)	(0.01)	(0.04)
Age	-0.003***	0.003*	0.01***	-0.00***	-0.01***
	(0.000)	(0.001)	(0.00)	(0.00)	(0.00)
Unemployed	0.025***	0.132**	0.13***	0.01*	0.07*
	(0.007)	(0.041)	(0.02)	(0.00)	(0.04)
Not in LF	0.015***	0.051**	0.07***	0.01***	0.04*
	(0.003)	(0.019)	(0.01)	(0.00)	(0.01)
Edu: medium	-0.005	-0.237**	0.11**	-0.04***	-0.06
	(0.013)	(0.078)	(0.04)	(0.01)	(0.06)
Edu: high	-0.009	0.049	0.28***	-0.00	0.09
	(0.013)	(0.079)	(0.05)	(0.01)	(0.07)
Constant	0.869***	4.265***	4.06***	0.60***	6.40***
	(0.010)	(0.060)	(0.03)	(0.01)	(0.04)
N	228079	199664	329399	325220	149048

"Vote" and "Has Party ID" are binary variables. "Pol. Interest" and "Party ID Strength" are measured on scales from 0 (low) to 10 (high). Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A-12: Error Correction Models (BHPS & UKHLS)

	Vote			Political Interest		
	exogeneous	predetermined	endogeneous	exogeneous	predetermined	endogeneous
DV_{t-1}	0.183***	0.176***	0.177***	0.177***	0.174***	0.174***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
DV_{t-2}	0.062***	0.057***	0.057***	0.074***	0.073***	0.073***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Income	0.000	-0.002**	-0.007**	0.002	-0.013*	-0.052**
	(0.001)	(0.001)	(0.002)	(0.004)	(0.005)	(0.018)
Age	-0.001*	-0.001***	-0.000	0.042***	0.037***	0.043***
	(0.000)	(0.000)	(0.000)	(0.003)	(0.003)	(0.004)
Unemployed	0.018**	0.014*	0.007	0.165***	0.135***	0.078
	(0.006)	(0.007)	(0.008)	(0.039)	(0.039)	(0.048)
Not in LF	0.011**	0.008	0.002	0.077**	0.057*	0.017
	(0.004)	(0.004)	(0.005)	(0.026)	(0.026)	(0.034)
Edu: med	-0.059*	-0.049*	-0.049*	0.083	0.132	0.257
	(0.025)	(0.024)	(0.024)	(0.145)	(0.146)	(0.148)
Edu: high	-0.053*	-0.036	-0.037	0.084	0.138	0.280
	(0.024)	(0.023)	(0.023)	(0.144)	(0.145)	(0.146)
Constant	0.644***	0.673***	0.673***	1.769***	1.994***	1.957***
	(0.020)	(0.020)	(0.020)	(0.140)	(0.139)	(0.146)
N	310407	310407	310407	261773	261773	261773
ABT(1)	-90.326	-89.132	-89.294	-102.070	-101.115	-101.058
ABT(2)	-0.970	-0.327	-0.315	-0.171	-0.171	-0.047

DV indicates dependent variable. Income represents equivalized household income in deciles. ABT displays z-values from Arellano-Bond test for zero autocorrelation in first-differenced errors for AR(1) and AR(2). Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A-13: Error Correction Models (SHP)

	Vote			Political Interest			Number of Participation in Polls		
	exogeneous	predetermined	endogeneous	exogeneous	predetermined	endogeneous	exogeneous	predetermined	endogeneous
DV_{t-1}	0.07^{***}	0.07^{***}	0.07^{***}	0.19^{***}	0.19^{***}	0.19^{***}	0.25^{***}	0.25^{***}	0.25^{***}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)	(0.03)
DV_{t-2}	0.04^{***}	0.04^{***}	0.04^{***}	0.07^{***}	0.07^{***}	0.07^{***}	0.09^{***}	0.09^{***}	0.09^{***}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Income	-0.00^{*}	0.00	0.01^{**}	-0.01	-0.00	-0.02	0.00	-0.01	-0.06
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.04)
Age	0.00^{***}	0.00^{***}	0.00^{**}	-0.00	-0.00	-0.00	0.01^{*}	0.01^{*}	0.02^{*}
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Unemployed	-0.00	-0.01	-0.00	0.22^{***}	0.23^{***}	0.22^{**}	-0.08	-0.08	-0.07
	(0.01)	(0.01)	(0.01)	(0.07)	(0.07)	(0.07)	(0.16)	(0.16)	(0.16)
Not in LF	-0.01	-0.00	-0.00	0.09^{**}	0.09^{**}	0.08^{*}	-0.05	-0.04	-0.07
	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)	(0.03)	(0.06)	(0.06)	(0.06)
Edu: med	0.03^{*}	0.03^{*}	0.03	-0.12	-0.13	-0.13	0.02	-0.03	0.06
	(0.02)	(0.02)	(0.02)	(0.07)	(0.08)	(0.08)	(0.35)	(0.38)	(0.36)
Edu: high	0.04	0.04	0.03	0.07	0.02	0.05	0.22	0.15	0.25
	(0.02)	(0.02)	(0.02)	(0.10)	(0.10)	(0.10)	(0.36)	(0.38)	(0.37)
Constant	0.69^{***}	0.68^{***}	0.64^{***}	4.29^{***}	4.38^{***}	4.47^{***}	4.73^{***}	4.92^{***}	5.03^{***}
	(0.03)	(0.03)	(0.03)	(0.18)	(0.18)	(0.21)	(0.52)	(0.55)	(0.55)
N	85046	85046	85046	84857	84857	84857	26134	26134	26134
ABT(1)	-37.77	-37.78	-37.99	-40.81	-40.59	-40.56	-19.81	-19.50	-19.71
ABT(2)	-0.38	-0.60	-0.50	-3.13	-3.21	-3.40	-0.76	-0.61	-0.52

DV indicates dependent variable. Income represents equivalized household income in deciles. ABT displays z-values from Arellano-Bond test for zero autocorrelation in first-differenced errors for AR(1) and AR(2). Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A-14: Error Correction Models (SOEP)

	Political Interest			Has Party ID			Party ID Strength		
	exogeneous	predetermined	endogeneous	exogeneous	predetermined	endogeneous	exogeneous	predetermined	endogeneous
DV_{t-1}	0.12***	0.12***	0.12***	0.16***	0.16***	0.16***	0.18***	0.18***	0.18***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
DV_{t-2}	0.05***	0.04***	0.05***	0.06***	0.06***	0.06***	0.06***	0.07***	0.06***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Income	0.00	-0.00	0.00	0.00	-0.00	-0.01***	0.01	-0.01	-0.10***
	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)
Age	0.00	0.00*	0.00	-0.00***	-0.00***	-0.00*	-0.01***	-0.01***	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Unemployed	0.08***	0.08**	0.09***	0.01*	0.01	-0.00	0.10**	0.08*	0.01
	(0.03)	(0.03)	(0.03)	(0.00)	(0.01)	(0.01)	(0.03)	(0.04)	(0.04)
Not in LF	0.05**	0.05**	0.06**	0.01*	0.01	-0.00	0.06*	0.05	-0.01
	(0.02)	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)	(0.03)	(0.03)	(0.03)
Edu: med	0.32**	0.34**	0.25*	-0.07**	-0.13***	-0.09***	-0.54**	-0.85***	-0.58**
	(0.12)	(0.11)	(0.11)	(0.03)	(0.03)	(0.03)	(0.19)	(0.18)	(0.18)
Edu: high	0.69***	0.77***	0.67***	-0.08**	-0.14***	-0.10***	-0.64**	-1.03***	-0.64**
	(0.13)	(0.13)	(0.13)	(0.03)	(0.03)	(0.03)	(0.22)	(0.21)	(0.21)
Constant	3.18***	3.20***	3.23***	0.46***	0.52***	0.53***	2.93***	3.28***	3.35***
	(0.09)	(0.08)	(0.08)	(0.02)	(0.02)	(0.02)	(0.13)	(0.13)	(0.13)
N	376683	376683	376683	363914	363914	363914	340336	340336	340336
ABT(1)	-97.54	-97.10	-97.11	-96.38	-95.23	-95.35	-92.48	-91.25	-91.41
ABT(2)	-0.37	-0.10	-0.20	-1.20	-1.23	-0.79	-1.42	-1.70	-1.22

DV indicates dependent variable. Income represents equivalized household income in deciles. ABT displays z-values from Arellano-Bond test for zero autocorrelation in first-differenced errors for AR(1) and AR(2). Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A-15: FE-Models including type of decile change (BHPS & UKHLS)

	Vote	Political Interest
Income: Increase (any)	0.000	0.015^{*}
	(0.001)	(0.007)
Income: 1 decile drop	-0.004^{**}	-0.014
	(0.002)	(0.010)
Income: 2 decile drop (t_{-1}: decile 5 or higher)	-0.007^{***}	-0.020
	(0.002)	(0.012)
Income: 2 decile drop (t_{-1}: decile 4)	-0.002	-0.011
	(0.005)	(0.033)
Income: 2 decile drop (t_{-1}: decile 3)	-0.013	0.067
	(0.007)	(0.043)
Age	-0.002^{***}	0.000
	(0.000)	(0.001)
Unemployed	0.009^{**}	0.072^{***}
	(0.003)	(0.018)
Not in labor force	0.008^{***}	0.019
	(0.002)	(0.011)
Edu: medium	-0.000	0.012
	(0.007)	(0.037)
Edu: high	0.015^{*}	0.430^{***}
	(0.006)	(0.037)
Constant	0.797^{***}	4.000^{***}
	(0.005)	(0.031)
N	548487	509249

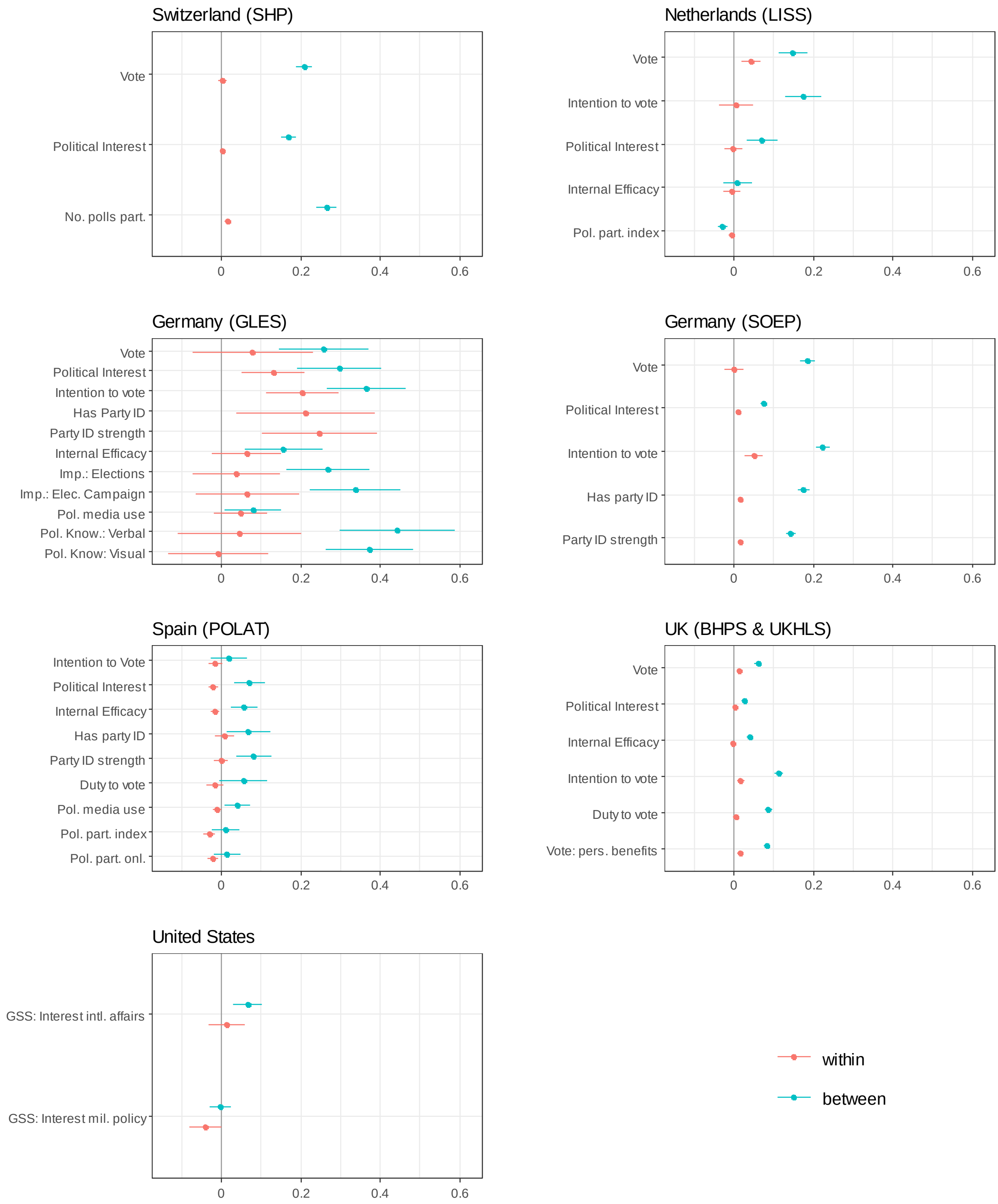
Reference category of changes in household income refers to no change between t_{-1} and t_0.
Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A-16: FE-Model controlling for initial political involvement (BHPS & UKHLS)

	Vote	Vote
Political Interest$_{t-2}$	0.015***	0.014***
	(0.001)	(0.001)
Income Shock	-0.007***	-0.012*
	(0.002)	(0.005)
Age	-0.002***	-0.002***
	(0.000)	(0.000)
Unemployed	0.016***	0.015***
	(0.004)	(0.004)
Not in labor force	0.011***	0.011***
	(0.002)	(0.002)
Edu: medium	-0.006	-0.006
	(0.011)	(0.011)
Edu: high	0.001	0.001
	(0.011)	(0.011)
PI$_{t-2}$*Income Shock		0.002
		(0.002)
Constant	0.791***	0.791***
	(0.009)	(0.009)
N	303344	303344

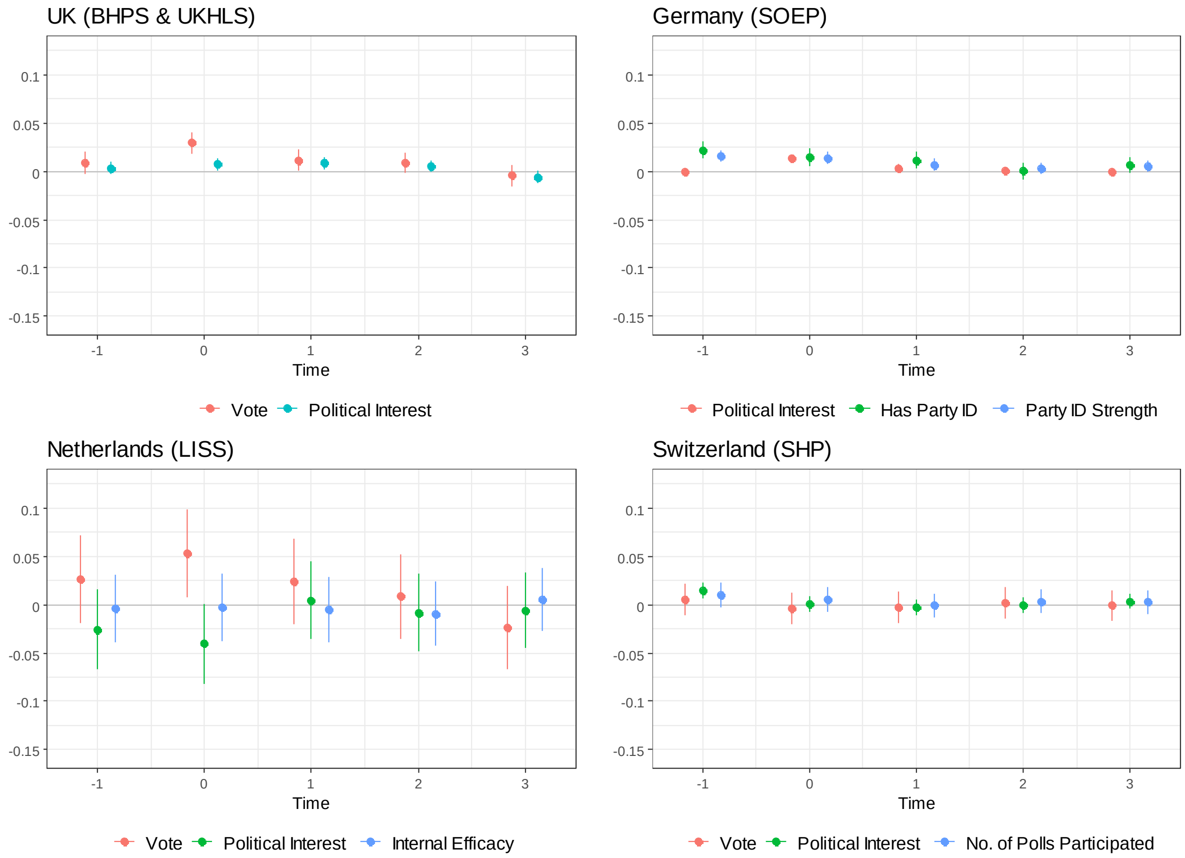
Income shock is operationalized as a dummy that takes the value of 1 if respondents experienced a drop in income by at least two income deciles compared to the previous period. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure A-1: Within- and between effects of income on political participation (subj. income)



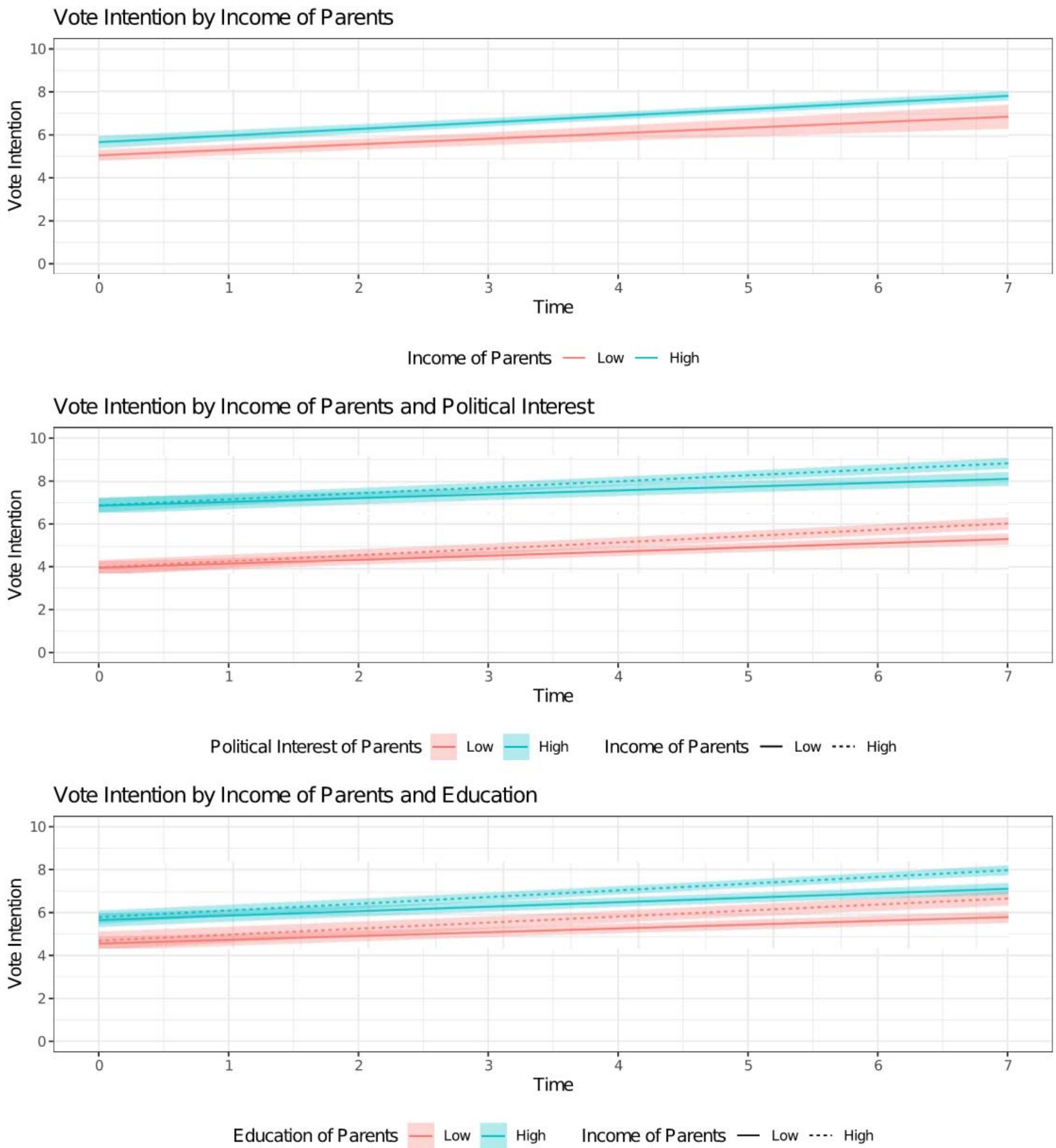
Note: Results for "Vote" (all data sets), "Political Interest" (LISS), "Has party ID" (GLES, SOEP, POLAT), and all variables in the GSS are performed using linear probability models. For readability, we rescaled the coefficients of the binary variables so that they indicate the effect of going from the lowest to the highest subjective income category. The remaining dependent variables are measured on a scale from 0 to 10. Subjective income is also measured on a scale from 0 (low) to 10 (high).

Figure A-2: FE-Models including lagged and leaded predictors (subj. income)



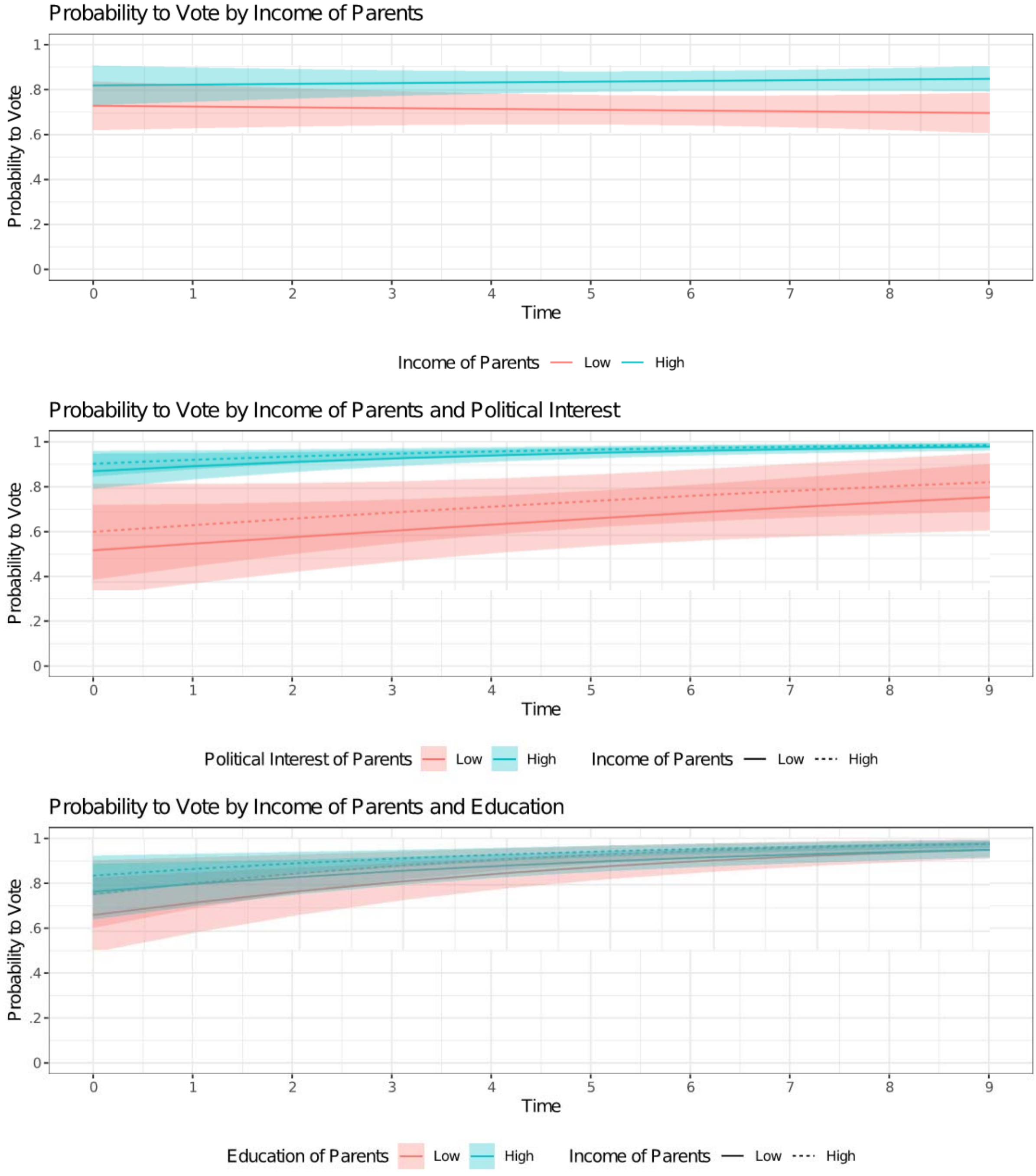
Note: Effects of a change in subjective income. Results for the binary variables "Vote" (all data sets), "Political Interest" (LISS) and "Has party ID" (SOEP, POLAT) are performed using linear probability models. For all variables estimates refer to a one income decile change. Subjective income is measured on a scale from 0 (low) to 10 (high), except for the POLAT where it is measured as a binary variable with 1 indicating satisfaction with personal income.

Figure A-3: Latent growth curve models for vote intention in the United Kingdom



Note: Plots are based on predictions from latent growth curve models for non-migrant women with medium education. “Low” and “high” income of parents refers the second and the ninth equivalized household income deciles. For political interest these values represent either being “not at all interested” (low) or “very interested” (high) in politics. For parental education “low” indicates having no degree or a degree lower than GCSE and “high” indicates having achieved A-Levels or higher.

Figure A-4: Latent growth curve models for probability to vote in Switzerland



Note: Plots are based on predictions from latent growth curve models for non-migrant women with medium education. “Low” and “high” income of parents refers the second and the ninth equivalized household income deciles. For political interest these values represent either being “not at all interested” (low) or “very interested” (high) in politics. For parental education “low” indicates having no degree or a degree lower than lower secondary education (ISCED level 2) and “high” indicates post-secondary education (ISCED level 4A) or higher.

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