

Does poverty reduce turnout? Causal evidence from the sequencing of bank working days

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Abstract

Does income poverty reduce political participation? The negative link between low socio-economic status and all forms of political participation is a classic finding dating back to the early days of empirical political science. However, producing causal evidence demonstrating that poverty *per se*—rather than the low education levels or general lack of resources that usually accompany poverty—causes the observed drop in participation has proven exceedingly difficult. This paper revisits the debate, drawing on new research that highlights the deleterious psychological effects of poverty. A natural experiment causing variation in income poverty allows for causal inference. I exploit the fact that the effective length of months varies in an unpredictable fashion depending on the distribution of bank working days throughout the year. This means that individuals in certain months will have to make do with the same salary for up to three days longer. Among the relatively poor, the additional days cause a marked increase in financial difficulties, especially towards the end of the month. I analyze the causal effect of these acute income shortages. Drawing on data from over 3 million individuals and 1,100 elections in Germany, I document reductions in both turnout intentions and turnout. Effect sizes are substantial, ranging between 2 and 6 percentage points. Qualitative evidence from personal interviews confirm the debilitating effect of income shortages on participation: income deprivation is most extreme at the end of the month, and leaves individuals stressed, unmotivated, and socially isolated, taking away their perceived capability to politically engage. The findings have important implications for the scheduling of election days and the political representation of the poor.

Keywords: Poverty; Political participation; Turnout; Germany.

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Fin du mois, fin du monde, même combat!

'End of the month, end of the world, same struggle', slogan of the French yellow vests

1 Introduction

Political scientists have long observed that poverty is associated with lower levels of political participation, most notably lower turnout (Jahoda et al., 1971; Rosenstone, 1982; Verba et al., 1995). This evidence is usually explained with reference to the voter resource model (Verba and Nie, 1972): political participation is driven by time, money and skills, all of which the poor possess less of. While intuitive, the resource model was built on observational evidence only, and has drawn the criticism of other scholars. Yes, poverty is associated with lower political participation—the line of critique goes—but this effect is driven almost exclusively by the fact that poor people tend to be less educated (Wolfinger and Rosenstone, 1980; Leighley and Nagler, 1992). Evidence that demonstrates that income deprivation *as such* can cause lower levels of political engagement is still lacking, and so are studies that exclude the threat of confounding by other factors through rigorous causal identification (Blais, 2006; Geys, 2006). This paper revisits the debate, drawing on new research that highlights the deleterious psychological effects of poverty. A natural experiment causing variation in income poverty allows for causal inference. Evidence comes from micro- and macro-level data from Germany. Data sources include observations from over 3 million individuals interviewed in several large-scale opinion surveys since the 1990s, and turnout figures from over 1,100 elections that took place in Germany since 1945.

As in other industrialized countries, in Germany there is marked negative relationship between poverty and all forms of political participation, especially turnout. I set out to demonstrate that this relationship is causal. In order to do so, I exploit a natural experiment causing short-term shifts in individuals' income poverty status. I rely on the fact that the effective length of months varies in an unpredictable fashion depending on the distribution of bank working days throughout the year. In Germany, salaries and other monthly payments are consistently made at the end of month, or, more precisely, on the last bank working day of the month. In short months where the last day fall on a

weekend (i.e. not on a bank working day), payments are made earlier than when the last day of the month falls on a normal weekday (resulting in a long month). A particular situation arises where a long month comes after a short, a pattern here referred to as LMAS. LMAS lead to periods between payments that are three days longer than short months. In other words, in LMAS, the same monthly income has to do for a month that is effectively 10% longer. Especially for poor people, LMAS can therefore lead to acute financial difficulties, providing short-term variation in income poverty. Crucially, the pattern at which LMAS occur is hard to predict in the short run, making preparing for financial shortages at the end of LMAS difficult. Checks demonstrate excellent balance between LMAS and non-LMAS for both the individual- and the polity-level data.

I then go on to show that LMAS-induced income poverty cause both turnout intentions and actual turnout to drop. The negative effect of LMAS on turnout intentions shows up in all included surveys, and is especially pronounced among the poor, and when interviews are conducted towards the end of month—when turnout intentions are an average 1 to 4 percentage points lower. The effect of income shortages on turnout is even more pronounced. In elections held during LMAS at the end of a month, turnout is on average 6 percentage points reduced. The negative effects shows up in all types of elections, but is strongest in regional and local elections.

I interpret my findings in light of new research in economics and psychology that deepens our understanding of the manifold effects that poverty and other forms of scarcity may have (Banerjee and Duflo, 2011; Mullainathan and Shafir, 2013; Haushofer and Fehr, 2014). This scholarship points to intimate links between scarce incomes, high levels of stress, increased present bias, and reduced cognitive function. The stress, poor mental health, and social isolation that come along with poverty, I argue, make people less interested in politics, reduce their feelings of being able to exert influence, and make them focused on the present, discarding benefits that might accrue from having their voice heard. Evidence from the surveys and from personal interviews confirm this intuition, but also show how institutional embeddedness can help to cushion these effects.

This paper makes three contributions. First, I revisit the literature on voting and poverty and link it to emerging research on the psychological effects of poverty. Second, I introduce a natural experiment that helps to avoid the endogeneity problems that typically mar the analysis of the relationship between socio-economic factors and voting. The setting exploits the timing of election surveys in relation to bank-working-days. In principle, the same pattern should be observable in many different context—and can hence be transferred to all survey studies where the interview date is available. Third, based on this natural experiment, the paper demonstrates a causal, detrimental effect of income poverty on several dimensions of political engagement, most notably turnout. Importantly, these reductions are fully concentrated in the lower end of the income distribution. Income poverty, even in the short-term, thus effectively leads to a disenfranchisement of the poor. I conclude by drawing out implications for political representation and suggest ways of addressing the problem.

2 Poverty, income scarcity and political engagement

The negative link between political participation and poverty has been observed since the early days of modern political science. Some of the earliest empirical work pointed to the negative psychological effects of poverty. Income deprivation, these authors argued, over time leads to poor mental health, lower political interest, lower participation in associations and lower community cohesion (Merriam and Gosnell, 1924; Jahoda and Zeisel, 1933; Jahoda et al., 1971). The classic rational choice approaches that followed these early explorations instead focused on the costs and benefits of voting. The focal point here is the decisiveness of the vote, which is conceptualized largely independently of an individual's socio-economic status, with two notable exceptions: first, the poor, especially when belonging to a minority group, have less power and may face stronger negative repercussions if they turn out due to their low societal position; and, second, the poor are assumed to have higher discount rates, so that if voting is costly and benefits accrue in the future, they will have a lower tendency to go to the polls (Downs, 1957; Riker and Ordeshook, 1968; Aldrich, 1993; Blais, 2000).

The consequences of poverty for political participation were made the primary focus of inquiry by Sidney Verba, Kay Lehman Schlozman, and co-authors (Verba and Nie, 1972; Schlozman and Verba, 1979; Verba et al., 1995): their research demonstrates the close link between higher socio-economic status and various domains of political engagement, ranging from voting to participation in voluntary organizations. In their model, poverty affects participation mainly through its psychological effects, like a decreased interest in politics, a lower sense of political efficacy, a weaker sense of citizen duty, and poor mental health.¹ The close link between income and turnout is confirmed in quasi-experimental work showing that *increasing* income via cash transfers can lead to higher turnout (De La O, 2013; Layton and Smith, 2015). While the basic tenants of the resource model have thus repeatedly been confirmed (cp. Smets and van Ham, 2013), other research questioned the relative importance of high income levels. These authors argue that while income only affects political engagement up to the point where a modestly comfortable standard of living has been attained, education increases individuals' capacity for "understanding and working with complex, abstract, and intangible subjects like politics" (Wolfinger and Rosenstone, 1980, 18, cp. Gallego, 2010; Sondheimer and Green, 2010).

The essential role of the ability to acquire and process political knowledge for all forms of political engagement was further elaborated in work by Delli Carpini and Keeter (1996), and also shows in get-out-the-vote experiments. Assistance in voter registration and face-to-face explanation of the voting process has been shown to increase turnout in France (Braconnier et al., 2017), although the mere provision of information appears to have little effect in the U.S. (Green and Gerber, 2015). The extent to which these information is processed, in turn, plausibly relies on cognitive ability, which may explain the overall negative correlation between such ability and turnout (Denny and Doyle, 2008; Mondak et al., 2010). These latter findings create a link to a growing body

¹I here limit myself to discussing individual-level determinants of voter turnout. Early cross-national research produced findings broadly in line with the standard socio-economic model, stressing that economic developments, especially economic growth and the formation of a white collar class, enable political participation (Lipset, 1960). Other classic work stressed institutional factors such as the competitiveness of districts, voting rules, and the depth of the party system (Key, 1949; Jackman, 1987; Cox, 2015), at the same time casting doubt on the strong link between economic factors and turnout (Blais, 2006; Geys, 2006). More recent investigations have focused on socio-economic inequality as a factor driving down all forms of political participation, arguing that with rising inequality, participation becomes the monopoly of the rich (Solt, 2008; Cancela and Geys, 2016).

of research in economics and psychology that provides a fascinating new perspective on the effects of poverty. This research demonstrates that poverty tends to go along with increased time-discounting/a stronger present-bias, leading to more short-sighted decision making (Banerjee and Duflo, 2011). These effects are arguably due to increased cognitive load and ‘tunneling’ among the poor causing myopic and erratic decision-making (Mani et al., 2013; Mullainathan and Shafir, 2013). Poverty has also been shown to be associated with high stress levels and poor mental health (Haushofer and Fehr, 2014). On the flipside, field experimental evidence demonstrates that even moderate boosts in income can improve psychological wellbeing, relax high time discounting rates, and improved cognitive functioning (Haushofer and Shapiro, 2016; Ong et al., 2019).

While these studies look at effects of poverty in medium-run—usually over the course of several months—lab and lab-in-the-field studies have found related, short-term effects when manipulating scarcity experimentally (Shah et al., 2012). Individuals on tighter budgets show stronger focus on the immediate task at hand, but less concern for future outcomes. A study that exploits short-term changes in income levels before and after payday confirms increased levels of time discounting among the income-deprived, but does not find any effects of income-deprivation on cognitive ability (Carvalho et al., 2016). A related study finds that the poverty status of others makes people hesitant to cooperate with them, especially when they are poor themselves (Schaub et al., 2019). This works points at potential second-order effects of poverty in terms of reduced sociability that also operate immediately. Many of the effects that are believed to negatively affect participation among the poor—stress, mental health issues, and a discounting of the future—therefore re-appear in this separate literature as direct effects of both medium and short-run poverty. As elaborated in the next section, these findings provide the micro-foundations for a theory linking income poverty to lower political engagement.

3 Mechanisms linking income poverty to lower levels of political participation

Should we expect acute income poverty to have similar effects on political participation as the long-term or structural poverty that previous research has focused on? And if so, what are the mechanisms linking income shortages to reduced turnout? While some of the proposed linkages—especially between poverty and education—are unlikely to be moved by income squeezes towards the end of the month, many of the psychological effects may in fact be impacted. Plausible mechanisms include reduced efforts to acquire political knowledge, lowered (internal) political efficacy, higher time discounting/present bias, and worse psychological wellbeing. I test each of these factors in the analysis section below.

Another set of mechanisms results from conceptualizing turnout and other forms of political engagement not only as individual behavior but as a collective endeavor. If people believe that their friends and family will vote, and if they know that others will learn if they voted or not, this makes them more likely to go to the polls (Quattrone and Tversky, 1988; Knack, 1992; Blais, 2000; Gerber et al., 2008). In this perspective, voting is as much about the individual herself as it is about her relationship to others (Huckfeldt and Sprague, 1995; Fowler, 2006). Expectations about the behavior of others may therefore shape engagement. However, scholars have suggested that these very expectations are lower among the poor. In his classic *The other America*, Harrington (1962, 133) described the ‘personality of poverty’ as “suspicious . . . and [with] a lack of trust in others.” New experimental evidence shows that the poor tend show lower levels of cooperation, especially with other poor, and that these lower levels are driven by the expectation that other poor will not reciprocate (Schaub et al., 2019). This mechanism may extend to forms of political engagement, especially voting. If individuals feel that their vote counts only insofar as others participate, and if financial difficulties drive down this expectation, the outcome may be lower rates of voting among the income-deprived.

4 Background and data sources

I study the case of Germany. Even though Germany is among the richest countries in the world, substantial parts of the population must be considered poor, in some cases severely so. Individuals are considered at risk of poverty when they earn less than 60% of the median means-adjusted equivalent income. This applies in particular to single-parent households, pensioners, individuals living off unemployment benefits, and poorly educated individuals employed in the low-income sector (Statistisches Bundesamt, 2018). For Germany as a whole, in 2017 the ‘at risk’ population was between 15.8 percent according to figures from the German Statistical Office (Statistisches Bundesamt, 2018) and 16.8 percent according to the German Socioeconomic Panel—up from around 12% in the early 1990s (Aust et al., 2018). And, as in other industrialized societies, in Germany there is a gap in political participation between the social classes, with individuals of lower socio-economic status less likely to be party members, to engage in political discussions, and to vote (Bödeker, 2012). What is more, this gap has been growing over the years (Faas and Siri, 2017). While until the 1980s, there were no discernible differences in turnout between higher and lower social classes, the turnout gap between high- and low socio-economic-status individuals had increased to 20 percentage points by 2013 (Schäfer, 2015). I demonstrate this negative correlation when introducing my data sources below.

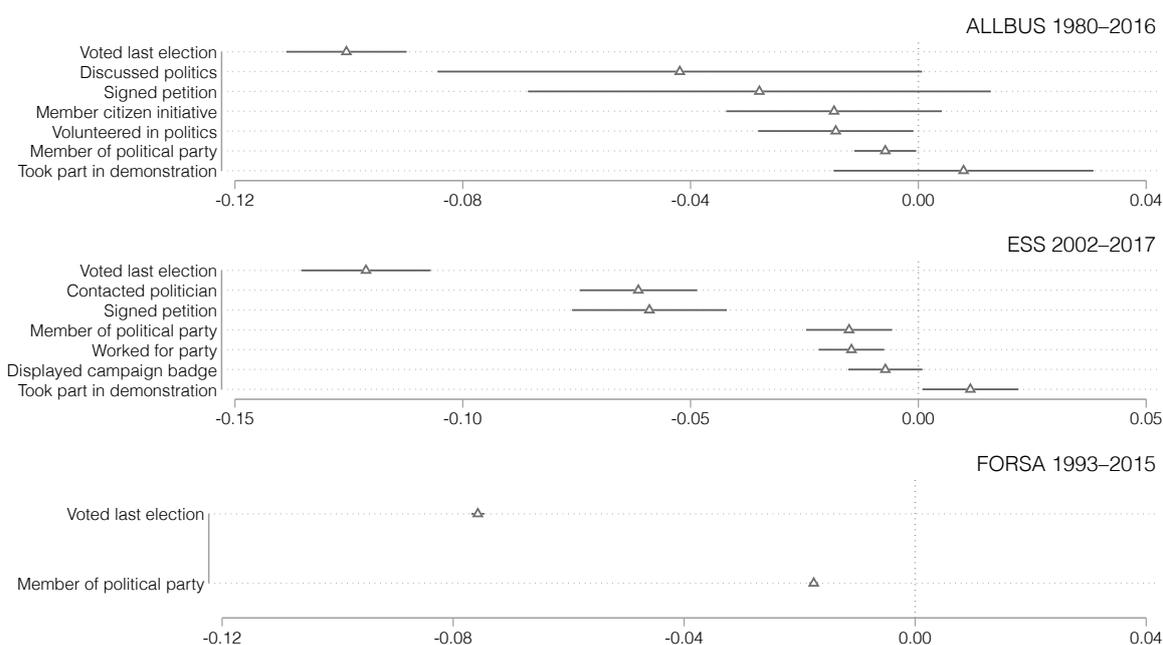
The case of Germany is ideal to study the effects of income poverty because of the way the labor market and social security system are organized, and because of the strong conventions surrounding the payment of wages and other transfers. At least three points are of importance. Most importantly, salaries, wages, and welfare benefits (pensions and unemployment support) are paid for the whole month rather than being disbursed weekly or bi-weekly, as common in other countries. In a poll conducted among 60 randomly chosen firms representing the 20 most common professions, 97% of firms confirmed that their employees are paid on a monthly basis (see Section E of the Appendix for details on the poll). Second, the majority of employees and workers—66% in the poll—receives their wages and salaries at the end of the month, and the same is true for recipients of welfare payments (Conrad, 2016). Third, in Germany in 2019, 78%

of low-income households lived in rented accommodation (Kempermann et al., 2019), and there is a strong convention for rent payments for the upcoming month to be made in the final days of the month. Therefore, rental payments—for most households the single largest monthly expense—tend to accrue at the end of the month. Taken together, these factors can lead to situations where money becomes tight towards the end of the month, especially for the poor, and especially in months that are effectively longer than others. This creates the ebb and flow in incomes that the quasi-experiment (introduced below) exploits.

4.1 Data sources

I use data from a range different sources. For the individual-level analysis, I draw on the full universe of large-scale political opinion datasets available in Germany. Datasets were included in the study if they a) contain measures of voting intentions or plausible mechanisms, b) include an income measure to allow me to identify the poor, c) show month-to-month variation in when people were interviewed, and d) explicitly state the interview date. The last two criteria are crucial for the definition of the instrument (see Section A in the Appendix for further information). Four large-scale political opinion data sets fit these criteria: the German General Social Survey (ALLBUS) covering $n=64,684$ observations collected between 1980 and 2016, the European Social Survey (ESS, 2002–2017, $n=23,342$), the *Deutschland Trend* (DT, 2008–2018, $n=143,542$) polling dataset, and the FORSA polling data set. The FORSA dataset is unique in that it contains data from daily polls for the years 1993–2015, adding up to no fewer than $n=3,050,039$ observations. The data cover a wide range of outcome measures that allow me to assess the overall relationship between poverty and political participation. These include past turnout, associational membership and participation in discussions. In all datasets, I identify individuals who are poor according to the standard definition by the OECD, i.e. individuals earning less than 60% of the means-adjusted median household income. The share of poor individuals ranges between 12% and 26% (compared to 16% in the general population), depending mainly on how precisely this group can be identified in the data. Figure 1 shows the correlation between the indicator for being poor and different forms of political participation.

Figure 1: Correlation of measures of political participation with poverty



Note: Coefficient plot from regressions of indicated outcomes on indicator for poverty, defined as earning less than 60% of the means-adjusted median income. All outcomes standardized to range from 0 to 1. OLS regression controlling for age, sex, education, parents' education and including month, year, and state fixed effects. Markers are point estimates, horizontal lines 95% confidence intervals.

We see that across all datasets, poverty is negatively correlated with 'hard' forms of participation such as party membership, but also with 'softer' forms of engagement such as contributions to political campaigns or participation in political discussions, mirroring the findings by German political scientists studying inequality and unemployment (Schäfer, 2015; Bödeker, 2012; Faas and Siri, 2017). The single strongest negative correlation, however, is that with voting. Poor people are between 4 and 12 percent less likely to indicate that they voted in the last general election.

The individual-level outcome of focus are turnout intentions. This measure is ideal because it is highly correlated with actual voting on election day—arguably the most important form of political participation. It is also a measure that is widely available, allowing me to test my hypotheses across different data sources. And, crucially, turnout intentions are time variant, in the sense that they can plausibly change during a limited time span, and can hence be affected by income poverty even in the very short run. ALLBUS, FORSA and the Deutschland Trend ask respondents the so-called 'Sunday

question' (*Sonntagsfrage*): respondents are asked to indicate which party they would vote for if elections were to take place the coming Sunday, the traditional voting day in Germany. I re-code the answers to this question into a binary variable that takes the value 1 if the respondents names a party, and 0 if s/he answers 'don't know', says she won't vote, or does not offer an answer at all. As shown in the robustness check section, the results are robust to alternative formulations of intended turnout, e.g. excluding intentional non-voters. Where useful, I amend the analysis of turnout intentions with that of other forms of political participation. However, these are usually measured over longer time-spans ('past month', 'past year') so that the measures are unlikely to be affected by short-term changes in the poverty status, even though the underlying behavior might well be.²

As a second outcome measure, I look at actual turnout. I assembled an original dataset of all general, European, state-level and municipal elections that took place in Germany since WWII, and for which electronic records are kept by the federal elections officer or the election officers of the sixteen German states—a total of 1,122 elections. Unlike in the individual-level analysis of turnout intentions, the unit of analysis here is the election. For all elections, I record the date at which they took place and the reported turnout, hence allowing me to test for an effect of short-changes in the electorate's income status on actual turnout.

Finally, in order to probe deeper into the lived reality of short-term poverty and political participation, I conducted semi-structured interviews with individuals living below the poverty line. Interviews were conducted in person in low-status neighborhoods in the German capital Berlin. I recruited interview partners in two ways so to allow for variation in terms of political engagement. First, potential respondents were contacted through the German National Conference on Poverty, a network of NGOs working on poverty and related issues. This recruitment channel allowed me to speak to individuals, who, despite their poverty, maintain high levels of political engagement. Second, with the help of student assistants I distributed flyers in low-status neighborhoods in Berlin,

²Ideally, we would measure short-term political engagement with broader range of items, including participation in political discussions during the last few days, sharing of political content, and participation in campaign and other political activities. Unfortunately, such measures are virtually never included in large-scale political opinion datasets. In a related project (Schaub, Gereke, and Baldassarri (n.d.)) we therefore defined and collected data that captures political participation in the short run.

asking for participation in the interviews. This allowed me to recruit individuals who are not part of any institutional structure, and who often showed very low levels of political participation. A total of 31 respondents took part in the interviews: 15 women and 16 men, with an average age of 52 years. 31 percent of respondents were in regular employment, 44 percent lived off unemployment benefits, and 25 percent were pensioners. Interviews lasted about 60 min each, and all respondents received a monetary compensation for their time amounting to about two times the Federal minimum wage.

5 The sequencing of bank holidays and changes in income status

5.1 The Long months after short (LMAS)

Causal identification relies on the sequencing of bank working days, which produce an interplay of what is here referred to as ‘long’ and ‘short’ months. I define a month as ‘long month’ when the last day of the month falls on a bank working day. Bank working days are, with very few exceptions, all Mondays to Fridays of the year. A ‘short month’, in contrast, is one where the last day of the month falls on Saturday or Sunday. February is always a short month. From the interchange between short and long months result ‘long months after short’ (LMAS). A LMAS occurs whenever a short month is followed by a long, which is the case in 30 percent of all months. The interplay between short and long months creates variation with regard to how long a household has to manage with the same amount of money.

In short months, wages and salaries are paid before the end of the month. Companies and the state are required by law to deposit payments on the recipient’s bank account by the end of the calendar month. As banks do not make transfers on weekends, this means that in short months, the transfer is done on the last Friday before the weekend that ends the month. In long months, transfers are usually made on the last day of the week. The length between two dates of payment is therefore longest when a short

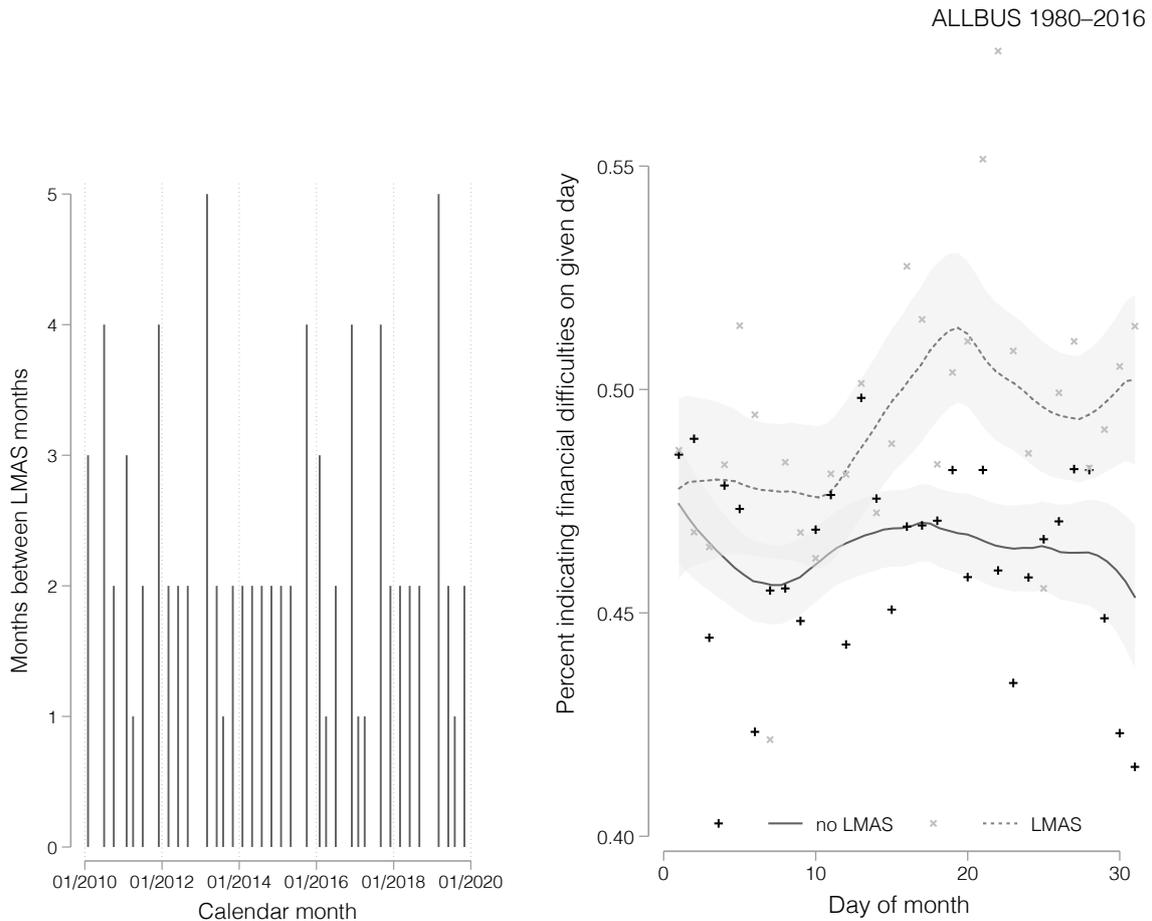
month is followed by a long. This is shown in Table 2A in the Appendix, which lists average month lengths for all months between January 1950 and December 2019. In the Table we see that in short months, the average number of days since a person was last paid is 29.1 days, and in other non-LMAS days, it is 29.8. For LMAS, in contrast, this figure stands at 32 days. This means that people have to make do with the same income for two to three more days. Especially for poor individuals, these additional days with expenses but no salary can imply that they may end the month cash-deprived or even in debt (as shortly demonstrated). What is interesting about LMAS months is that their occurrence is highly irregular to the extent to be no better than random in the short run. This is demonstrated in Figure 2a, which plots the number of months between the occurrence of LMAS for the time period 2010 to 2020.

We see that little in terms of a pattern can be observed. This intuition can be formalized with the Wald–Wolfowitz runs test, a test of the statistical independence of sequences. It tests the hypothesis that in a given series of numbers, the sequence of ‘runs’—appearances of the same number in a row—could have occurred by chance. In other words, it tests the null hypothesis that each element in the series is independently drawn from the same distribution. I constructed a test to check how likely it is that any series of months with length n drawn from the time period 1945 to 2020 differs significantly from chance in terms of the runs of LMAS it produces. The runs test only starts picking up (at the 10% level) non-randomness with regard to the LMAS from a series length of $n=34$ months upwards. In other words, when looking into the future, at any point in time, the occurrence of LMAS in the proceeding 2 years and 10 months look no better than random. This means that for individuals it is exceedingly hard to develop an intuition with regard to the sequence of cash-deprived end-of-months.

5.1.1 LMAS and financial situation

The claim is that LMAS negatively affect household finances, which then causes a drop in participation rates. Can we show the causal effect of LMAS on household’s financial situation empirically? Unfortunately, most public opinion surveys, including the ESS and the FORSA study, do not include measures of immediate economic and financial wellbeing. This said, the ALLBUS includes one item that can be used to evaluate the instrument. In ALLBUS, individuals were asked to rate their own economic situation

Figure 2: Long-months after short (LMAS)



(a) Spacing of LMAS in period 2010-2020 (b) Effect of LMAS on individual financial situation

Note: Figure 2a plots the number of months that lie between one long-month after short (LMAS) and the next for the years 2010 to 2010. Figure 2b shows the percentage share of respondents indicating financial difficulties on the given day against the day of the week. The lines are kernel density plots (Epanechnikov kernel with optimal bandwidth) of financial difficulty for respondents interviewed during a long-month-after short (LMAS, dashed line) or non-LMAS months (solid line). Markers are day-of-month averages of financial difficulties. The shaded areas are 95% confidence intervals. ALLBUS 1980-2016 data, $n = 56,907$.

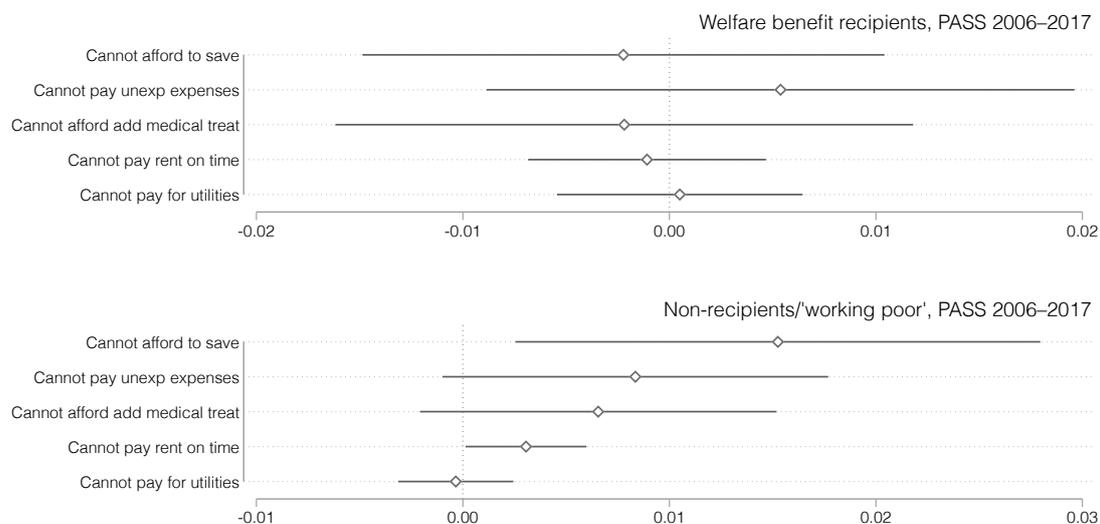
“today” (rather than the more conventional “nowadays”), with answer options ranging from “Very bad” to “Very good”. I recode this variable to take the value 1 for individuals indicating some financial difficulties, and 0 for those who do not. Figure 2b plots the share of respondents indicating financial difficulties at a given day of a month, separately for LMAS and non LMAS. In non-LMAS months, the line is basically flat: at any time during the month, respondents indicate about the same level of financial difficulties. Not so for LMAS: here we see a marked increase in the share of respondents indicating financial difficulties towards the end of the month.

In order to get more detailed information, I consulted the Panel on Household Finances (PHF panel) collected by the German Central Bank.³ The results, plotted in Figures 12Aa and 12Ab in the Appendix, show that poor people during LMAS report a higher number of unpaid bills, a reduced ability to save, and more difficulties to get by. A further dataset, the Panel Study Labour Market and Social Security (PASS) provided by the German Institute for Employment Research (IAB) allows to probe even deeper into the effects of LMAS. The PASS is exceptional in that it specializes on low income populations. About 60% of the 27,000 households in the dataset were recruited among current or erstwhile recipients of unemployment benefits, while the other 40% are from the general population, with lower status individuals oversampled. The dataset therefore allows us to more closely analyze *who* among the poor is most negatively affected by LMAS-induced short-term poverty. For example are those living off unemployment benefits affected the same way as the working poor? The analysis presented in Figure 3 suggests that this is not the case.

Instead, the effect on LMAS on financial difficulties appears to be confined to the working poor/individuals *not* living off benefits. This group finds it harder to cover unexpected costs, medical expenses, to save, and to pay their rent and utility bills in time during LMAS. In contrast, the effect of short-term income poverty does not show among benefit recipients. What explains this surprising finding? Possible answers were provided during my interviews, and are also reflected in the quantitative data. For one, unemployment, more than other drivers of poverty, is associated with high levels of financial prudence. This might be because welfare recipients are pressed harder by the labor agency (that in charge of disbursing benefits) to take care of their finances, or because the income of the unemployed tends to be even lower than that of other poor—so that they have come to take extra care. Many statements during the interviews supported these arguments. For example, one respondent, an unemployed former mechanic, stated that “throughout the years [of unemployment] we have come to learn very well how to manage our finances. We know how much we are allowed to spend every day, and virtually never go beyond these amounts.” Similarly, a female

³Access to the micro-level dataset including the date variable is possible through the Secure Data Center and on-site only.

Figure 3: Effect of short-term poverty on measures of financial difficulties among welfare recipients



Note: Coefficient plot from pooled regressions of indicated outcomes on the indicator for long-month-after short (LMAS). OLS regression controlling for age, sex, education, and including month, year, and state fixed effects. Markers are point estimates, horizontal lines 95% confidence intervals. Standard errors clustered at the level of the respondent.

respondent, living off a welfare payments, stated that “every month I start my month by drawing up a list of things I will need, and plan how much money I will have to spend on these necessities. Only after I have bought these items I allow myself to spend money on luxuries like having a coffee out.”

The higher level of financial prudence is also reflected in the panel data. In comparison to other poor, the unemployed report stronger financial worries, but also state that they take care more strongly, and regularly compare all prices before making acquisitions (see Figure 8A in the Appendix). A second reasons why the effect should be stronger among the working poor than those on benefits is a different flow of expenses. In particular, while the working poor have to make rent payments at the end of each month, for welfare beneficiaries, this cost is usually covered by the employment agency. Concern about being able to pay for the rent was a recurrent theme during the interviews. One interviewee, divorced father of one, reported how paying the rent of the flat where his ex-wife and son lived put so much strain on his finances that he himself ended up homeless, having to live with varying friends and family members. The importance

of rent payments for causing short-term poverty is also reflected in the quantitative analysis: the ability to pay rents remains negatively affected by LMAS among the working poor even when introducing individual-level fixed effects i.e. exploiting *within-household* variation in the timing of interviews, as shown in Figure 7A in the Appendix.

5.1.2 Balance

For the LMAS instrument to be valid, it has to be orthogonal to all potential confounders. While this assumption is not fully testable (since there might be unobservable confounders), an important implication of this assumption is that treated and untreated individuals should be indistinguishable in terms of observable covariates. This assumption is tested in Tables 3A to 5A in the Appendix. The tables demonstrate excellent balance in all datasets. Very few differences between individuals recruited during LMAS and those recruited during non-LMAS reach statistical significance at conventional levels, even though sample sizes are large. And where they do, substantive differences are miniscule.

6 Results

6.1 Individual-level results

The next section analyzes the effect of income poverty on political behavior. I start by analysing the effect of income changes on turnout intentions. I estimate a regression model in the following form:

$$Y_{imys} = \beta_0 + \beta_1 LMAS_i + \beta X_i + \alpha_m + \delta_y + \gamma_s + \mu_{imys} \quad (1)$$

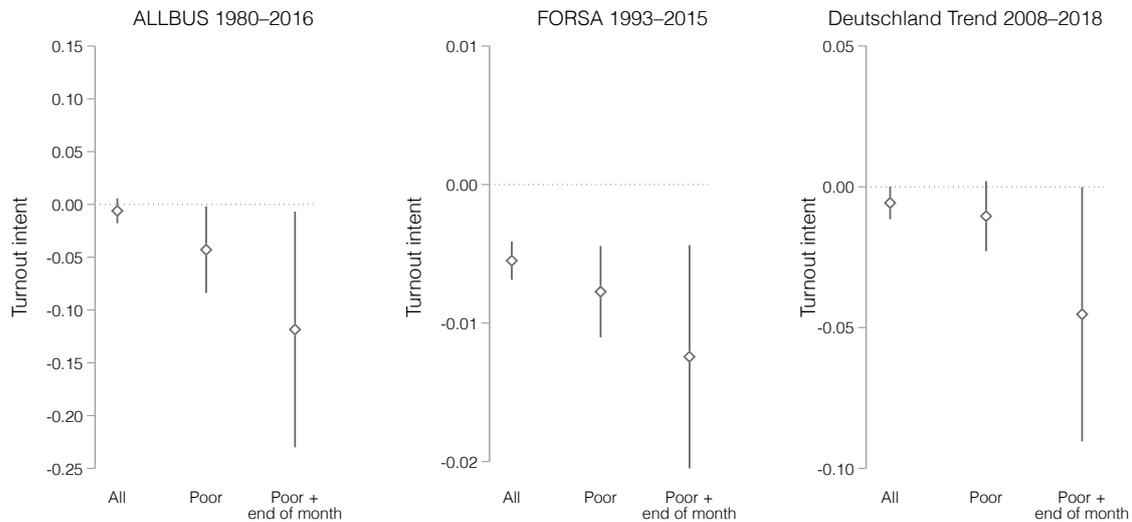
where Y stands for the outcomes, $LMAS$ records whether individuals were interviewed in a 'long month after short' or a non-LMAS; X is a vector with the pre-treatment control variables age, sex, education, and parents' education, and m , y , and s are month-of-the-year, year, and federal state fixed effects, respectively. The fixed effects ensure that results do not simply reflect idiosyncrasies related to the year, time of the year, or local area where they are recorded.

Figure 4 sums up the result for the the ALLBUS, FORSA and Deutschland Trend datasets. For each dataset, three estimates for the effect of short-term poverty are given: i) on the whole sample, ii) on the poor population only, and iii) the poor population at the end of the month. We see that while there are only small overall effects, among the poor LMAS-induced income shortages cause significantly lower turnout intentions. This trend is reinforced is reinforced towards the end of the month. Effect sizes for the poor population range between one and five percentage points—a relative difference of one to six percent relative to the average turnout for this group.⁴ For the poor at the end of the month, effect sizes range between 1.5 and 11 percentage points lower turnout intentions (two to seventeen percent relative to the baseline).⁵ Assuming an effect size of 5 percentage points and given that 15.8% of the German electorate of roughly 65 million is poor, this translates into a loss of half a million intended votes at the end of a LMAS as compared to a non-LMAS.

⁴The average intended turnout rates for the poor population are 71% in the ALLBUS data, 70% in the FORSA data, and 76% in the Deutschland Trend data.

⁵While effects are negative throughout, they vary in scale, with effect sizes in the FORSA data being much smaller than in the other datasets. One possible explanation are the different interviewing methods used. ALLBUS and ESS rely on personal interviews, arguably the gold standard in opinion research. FORSA and Deutschland Trend, on the other hand, use telephone interviewing, which has been found to produce lower response rates and data of less reliable quality, especially among the poor (e.g. ?).

Figure 4: Causal effect of LMAS-induced income poverty on turnout intentions (individual-level)



Note: Figure 4 plots the coefficients for individual-level regressions of turnout intentions on the indicator for long-month-after short (LMAS) months. OLS regressions controlling for age, sex, education, and parents' education, and including month, year, and state fixed effects. ALLBUS 1980–2016, FORSA 1993–2015, and Deutschland Trend 2008–2018 data. Markers are point estimates, vertical lines 95% confidence intervals.

6.2 Robustness

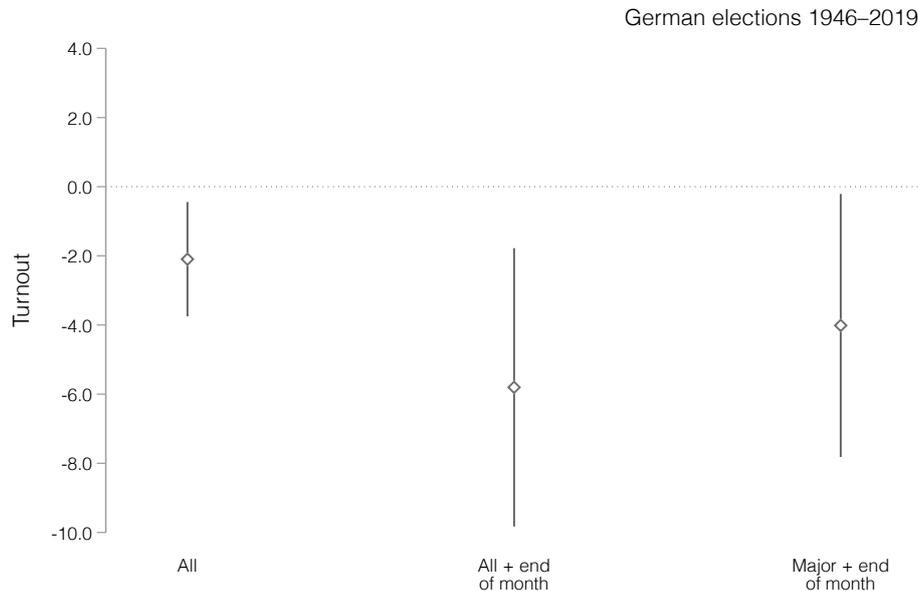
In order to probe further the plausibility of the results, we may ask where we would *not* expect to see an effect on LMAS on political participation. For one, we can think of groups of individuals for whom the interchange between LMAS and non-LMAS should matter little. Arguably the biggest group are the non-poor. For individuals with disposable savings and an income flow that tends to exceed their monthly expenditures LMAS should not matter. And this is indeed what we see. Figure 13A in the Appendix replicates Figure ?? for the non-poor population. Among this group (with the exception of the FORSA data, where some effect can be detected among the non-poor as well) the effect of LMAS is indistinguishable from zero. A second test can be derived from the fact that not all professional groups should be affected equally by the LMAS instrument. Self-employed individuals and farmers, for instance, have an irregular income flow that does not depend on the sequencing of bank working days. In line with the theory, there is no effect of LMAS among these groups (see Figure 11A in the Appendix). In fact, the effect is only unambiguously visible among workers and employees—groups

that typically depend on regularly paid wages or salaries for their living. As a third test, we can ask which outcomes should not be affected. If the effect of LMAS on voting intentions runs through short-term income poverty at the time of the interview, we should not see an effect time invariable traits. For example, no effect should be visible on party membership. Figure 10A in the Appendix demonstrates that this is indeed the case.

6.3 Election-level results

Next we assess the effect of LMAS-induced income poverty on past turnout using the German electoral turnout dataset (Figure 5). Elections that took place during an LMAS have a 2 percentage point lower turnout rate than those that took place in a non-LMAS. This overall effect is driven by differences between elections taking place in the last week of the month (22% of all elections), where the difference is 6 percentage points. When restricting the dataset to major (federal-, state, and European) elections, the gap narrows to 4 percentage points, but comfortably stays statistically significant. Acute financial difficulties caused by LMAS make income poverty more severe, and in turn cause political participation to drop.

Figure 5: Causal effect of LMAS-induced income poverty on observed turnout (elections)



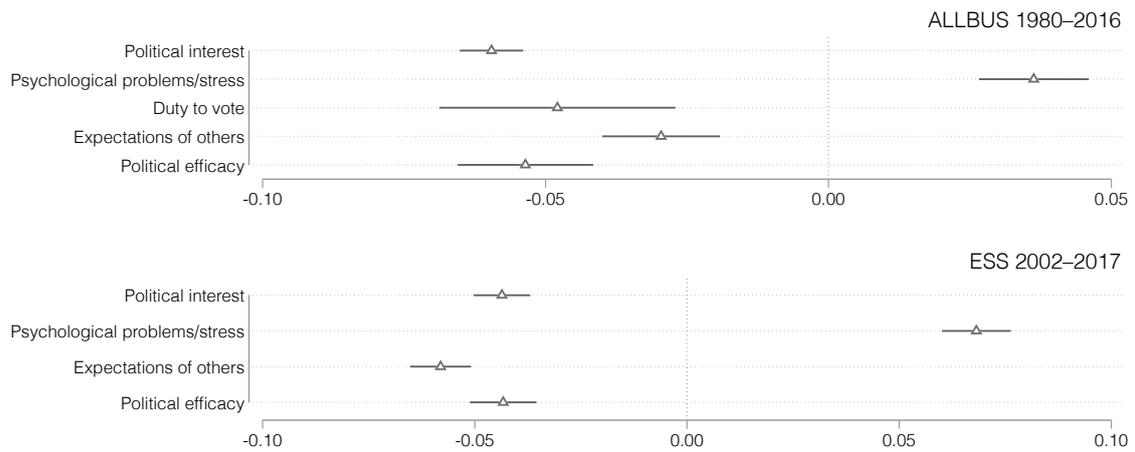
Note: Figure 5 plots the coefficients from a multi-level regression of turnout on the indicator for LMAS, with intercepts allowed to vary on the level of the decade and the geographic unit (the states or the federal state), and controlling for monthly fixed effects, an indicator for the length of the month, and the turnout in the previous election. German electoral turnout dataset (compiled by author). Markers are point estimates, vertical lines 95% confidence intervals.

7 Mechanisms

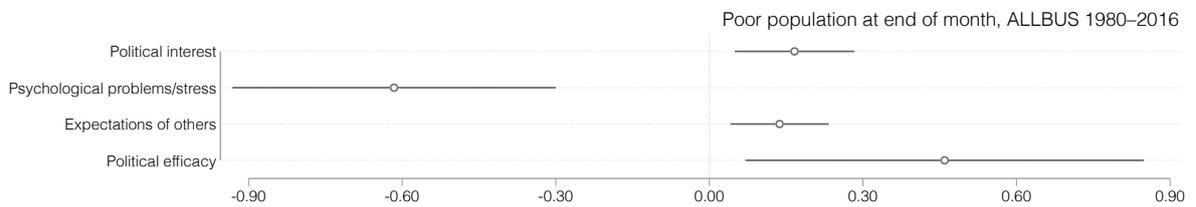
What are the mechanisms through which acute income poverty lowers political participation? Above I cited lower political interest, psychological distress, lowered expectations in others, and lower levels of political efficacy as plausible mechanisms. The ALLBUS and ESS surveys include a range of questions allowing us to capture these mechanisms. For ease of presentation, I summarize these items into scales by standardizing and averaging over items, and present effects on the scales.⁶ Unfortunately, only the ALLBUS Survey includes measures for both mechanisms *and* turnout intentions, and some items (on psychological wellbeing and efficacy) were only included in two of the rounds. These data limitations make it impossible to conduct formal causal mediation tests as suggested by Imai and Yamamoto (2013) or Acharya et al. (2016). I therefore follow a three-step procedure that draws on varying samples to make full use of the available data. I start by showing the correlation between mechanisms and income poverty.

⁶Figure 9A in the Appendix presents results for individual items.

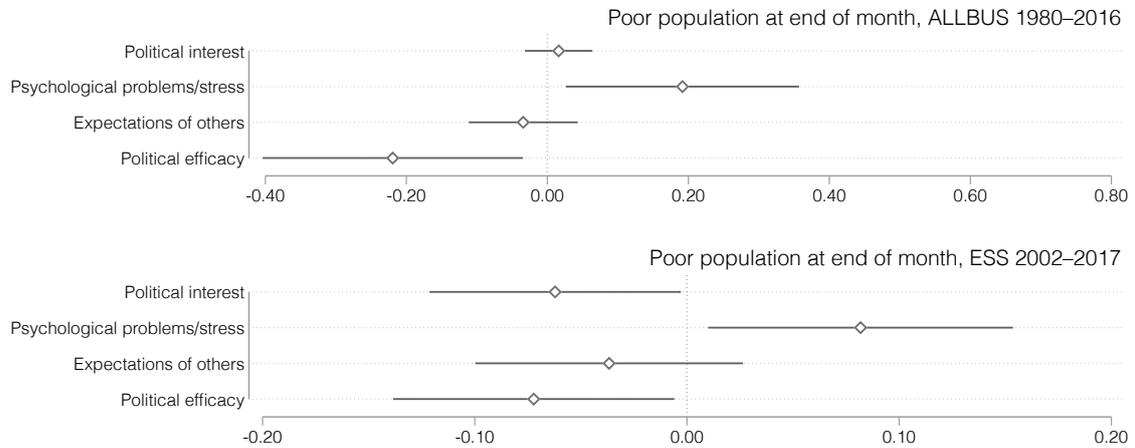
Figure 6: Mechanisms theorized to cause lower levels of political participation



(a) Correlation of poverty and theorized mechanisms



(b) Correlation of mechanisms and turnout intentions



(c) Causal effect of LMAS-induced income shortages on mechanisms

Note: Figure 6 plots the coefficients for regressions i) of the indicated outcome on the indicator for poverty, defined as earning less than 60% of the means-adjusted median income (Figure 9Aa), ii) of turnout on mechanisms (only available for the ALLBUS data, Figure 9Ab), and iii) of mechanisms on the indicator for long-month-after short (LMAS) months (Figure 9Ac). ALLBUS 1980-2016 and ESS 2002-2017 data. Markers are point estimates, horizontal lines 95%/90% confidence intervals. Results for the individual components of the indices are shown in Figure 9A.

As can be seen in Figure 6a, all correlations are as expected: poverty is associated with lower political interest, higher psychological distress, lowered expectations of

others, and lower levels of political efficacy. Second, Figure 6b demonstrates that in the ALLBUS data, the mechanisms also predict lower turnout intentions, especially when restricting the sample to poor individuals at the end of the month.⁷ Associations are strong: in particular, psychological distress and political efficacy appear to be closely linked with turnout intentions.

In a last step, in Figure 6c I present the causal effect of the instrument on the mechanisms. We can see that not unlike overall poverty, LMAS-induced income shortages are associated with a decline in psychological wellbeing and political efficacy—only that these effects are fully causally identified. On the other hand, no consistent effect on political interest and expectations is visible. It therefore appears that acute income poverty depresses turnout by causing stress, other forms of psychological unwellbeing, and lowered perceptions among the respondents' of their own ability to take part in politics.

7.1 Effects on party vote

If income poverty depresses electoral participation among the poor, does this have consequences in terms of party votes? If the poor support certain parties more than others, but go to the polls less, does this mean that their favored type of party suffers? In the case of lower turnout among the poor, we would expect left-leaning parties to suffer, as these tend to have their voter base in the low-income segment of society (cp. Pacek and Radcliff, 1995; Lijphart, 1997; Hansford and Gomez, 2010). For the analysis I coded indicators recording whether a person intended to vote for one of the left-leaning parties parliament (the Social Democrats (SPD) and the Left Party (PDS/Die Linke). As can be seen in Figure 14A in the Appendix, in all datasets LMAS-induced income shortages indeed appear to cause a drop in voting intentions for the left. This said, effect sizes are small and statistically insignificant at conventional levels in two of three cases. Similarly, in the elections dataset, short-term poverty is associated with a 1.5 percentage points lower support for the political left—but again this effect is only marginally statistically significant ($p=0.09$).

⁷In fact, the correlations also hold for the full sample; the restricted sample is used here because substantive effect sizes are larger, and to ensure comparability with the causal analysis.

7.2 Qualitative evidence

As mentioned, in order to check the plausibility of the quantitative findings and to put faces to the figures, I conducted qualitative interviews among 31 respondents living below the poverty line. The interviews centered around two themes: i) respondents' experience of poverty, income shocks, and methods of coping; and ii) respondent's political attitudes, past and current forms of political engagement, and attitudes to formal political processes, especially elections.

Speaking about how they experience poverty, respondents regularly stressed the tight constraints on consumption they are facing, especially towards the end of the month, when money runs out—which also compels them to reduce their social contacts. For example, a retired bookkeeper now living off a meager pension, stated "... towards the end of the month, money gets tight. The last 10 days of the month, you will often have to do with 30 Euros for all those days. In those days, I shouldn't go out and meet people, because this always costs." While most respondents did not perceive a clear regularity as to when money runs out beyond an end-of-month effect, one respondent said she closely watches the calendar to see if her money arrives before the end of the actual month (i.e. in a short month). She then tries to control her spending so to retain enough for the remaining days of that month and the whole of the following month. However, she also admitted that such rationing is extremely difficult. "You always have a list with things that you desperately need, like washing powder or other small things. Then when the money arrives early, you will start buying these things. Of course this means that the money goes more quickly. You start the month, and your budget is already down."

In terms of political engagement, most respondents stated that they occasionally followed the news, but generally showed little interest in current affairs, and largely made disapproving comments about politics, which they saw as taking place in a realm different from their lived reality. These feelings alienation were shared by those respondents engaged with the NCP: they followed politics only insofar as it dealt with social issues, often in the realm of housing or support for the handicapped or child poverty (cp. Han,

2009). These issues also motivated them to take part in elections—something other respondents had at best done occasionally.

A recurrent theme of the interviews were psychological difficulties. Many respondents reported of regularly feeling down and depressed. An unemployed butcher, for example, stated mentioned how “on days where I can’t see my children [who live elsewhere], I often fall into a deep hole. I sit at home, do nothing . . .”. This situation also caused the respondent to loose interest in the outside world. He had not been formally participating in politics in years. A lack of money also prevent engagement for very practical reasons. Not being able to afford tickets for public transport, one respondent chose to stay away from political events so not to risk being caught without a ticket—despite being strongly interested in current affairs and willing to engage. Another respondent, who sometimes takes part in discussion forums to report of his poverty, recounted his fear of being asked ‘to have lunch together’, not knowing whether the conversation partner would cover the cost for the food—something he could would not be able to afford himself. As a consequence, he often would forego opportunities for personal conversations, and hence relinquish influence a non-poor person would find easy to exert. However, the most commonly cited factor preventing individual to participate in political or other activities was the social isolation that comes along with even short stints of poverty. Respondents reported how, towards the end of the month, they would often choose to stay at home alone so to avoid spending money. This retreat in the domestic sphere in turn, has a range of effects. The lack of contact takes away conversation partners for political discussions, and the social pressures to participate, which those embedded in social relations may feel. And isolation may negatively affect psychological wellbeing, reinforcing the stress induced by having too little.

In fact, another common theme of the interviews was a reverse causal path, from engagement to psychological wellbeing. Individuals engaged with the National Conference on Poverty stressed that the meetings they go to often help them to “get out of the hole”, as one respondent put it. Interviewees thus hinted at a circular relationship between political engagement and psychological wellbeing: especially in times of severe financial constrain, engagement helps those engage in political structures to improve their psychological wellbeing, increasing their feelings of efficacy, and making them

more motivated to engage. In at least one case, embeddedness in formal structures directly affected voting. One respondent stated that she volunteered as poll worker, since this would earn her a small monetary compensation and a hot meal. Being at the polling station anyway, she would then cast her vote.

8 Conclusion

This paper has demonstrated a negative causal effect of acute income poverty on political participation. Income shortages go along with lower rates of various forms of participation, especially among the already poor. In particular, turnout intentions and actual turnout are negatively affected. Effect sizes are substantial, ranging between 2 to 6 percentage points. The paper thereby demonstrates that income poverty *as such* is sufficient to depress levels of political engagement, challenging previous arguments that saw the effect of poverty mainly operating through structural factors such as lower levels of education, and a lower embeddedness in social networks. In contrast, the changes observed here appear to be driven by negative psychology that takes immediate effect. This also implies that even short stints of poverty can have a depressing effect on political participation. As shown by studies in the U.S. and Europe, at most 60% of the poor population is poor long-term (Bane and Ellwood, 1986; Fouarge and Layte, 2005). The remaining poor are a diverse set of people who slip in and out of poverty. The population affected by the engagement-depressing effects of short-term income poverty is hence likely much larger and more diverse than official poverty quotas may suggest. These findings are of added importance in light of research demonstrating the habit-forming nature of voting. Voting in prior elections has been shown to have a strong and causal effect on turnout in subsequent elections (Gerber et al., 2003; Aldrich et al., 2011; Dinas, 2012; Coppock and Green, 2016). This means that small events can have long-term—and hence cumulative—effects. Even one off events such as a short spell of unemployment early on in life or a rainy election day can have downstream consequences, permanently reducing political participation among the affected (Fujiwara et al., 2016; Emmenegger et al., 2016). This papers shows that the same likely holds true for short-term income poverty.

The disenfranchising effect of acute income poverty seems as concerning for the well-functioning of democratic representation as that of poverty in general. This paper therefore echoes the conclusion of Verba and Nie (1972), who almost 50 years ago argued that governments should do what is possible to increase the participation of the poor. What could be measures to counter the effects of financial difficulties among the poor towards the end of the month? First, as an immediate measure, election dates that fall at the end of long months following short months should be avoided. This suggestion is especially pertinent in Germany, where general elections are often held at the end of month. Shifting election dates to the beginning of a month when financial resources tend to be more plentiful would avoid disenfranchising those struggling towards the end. Second, material obstacles to participation should be minimized, e.g. by making public transportation free on election day, as already practiced by some U.S. cities and in Israel, by providing additional welfare benefits to those who engage, or even by providing additional cash transfers at times of important political decisions. Third, the findings presented in this paper may support the case for mandatory voting. As argued and shown by an important body of scholarship (cp. Hill, 2006; Singh, 2015; Elliott, 2017), turnout tends to be more equal across social classes in democracies where voting is mandated by law. The same effect may with regard to acute income poverty, although this would need to be subject to further research. Finally, a further way to reduce disenfranchisement caused by short-term poverty would be to increase the institutional and social embeddedness of those potentially affected. The qualitative data showed that organizational structures helped those engaged in them to overcome the negative psychological effects and practical constraints caused by acute income poverty. Beyond what governments can do, recruiting the poor into formal associations is a task that civil society as a whole can contribute to.

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Appendix for 'Income poverty and political participation'

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A Data sources

For this study, the full universe of opinion and survey data in Germany was considered. A list of criteria was used to determine which datasets to include: 1) the dataset should contain data for a variety of months of the year so to provide variation in terms of the effective length of months. This rules out pre-electoral surveys, since these tend to be conducted during the course of only 2-3 months around the time of the national elections, which in Germany always take place in September. The dataset should include both measures of political engagement, and/or a measure of financial wellbeing, information on income, and the precise date of the interview. The first criterion excludes the German Socio-Economic Panel (SOEP), which only contains information on retrospective voting behavior, and the latter two criteria exclude the ‘Politbarometer’ which only—and inconsistently—indicates the week of the interview and contains no information on income. Furthermore, the dataset should have a rigorous sampling scheme that does not leave it to respondents when to answer the survey, and collects information on all dates of the months (shows a largely flat distribution with regard to what dates surveys are answered). This latter criterion rules out self-administered online surveys such as the GLES Online Tracking, since the timing of participation may be affected by the instrument.

Table 1A: Datasets considered for inclusion in study

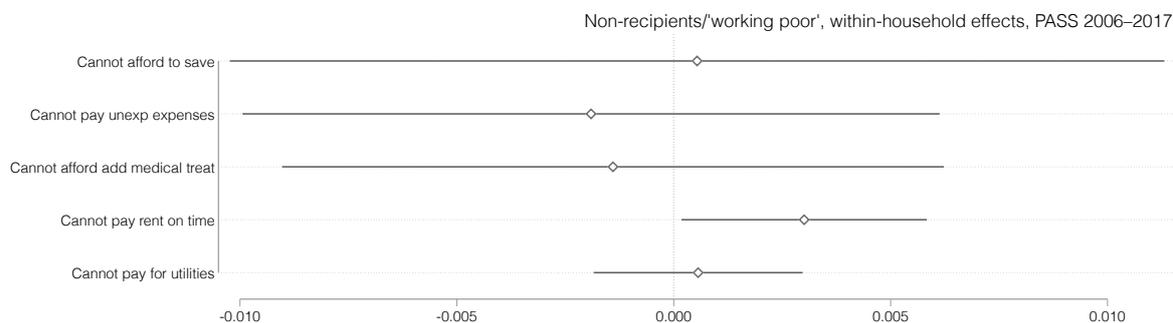
	Multiple months	Turnout	Interview date	Financial wellbeing	Rigorous sampling	Income data
Standard electoral surveys (GLES)	no	yes	yes	yes	yes	yes
SOEP / SOEP SILC	yes	no	yes	yes	yes	yes
Politbarometer	yes	yes	no	yes	?	yes
Forsa-Bus	yes	yes	yes	no	yes	yes
Deutschland Trend	yes	yes	yes	no	yes	yes
ESS	yes	yes	yes	yes	yes	yes
GLES-Online-Tracking	yes	yes	yes	yes	no	yes
Elections dataset	yes	yes	n.a.	n.a.	n.a.	yes (polity level)
State-level-surveys 1960–2004	yes	yes	no	?	yes	yes

B Describing the instrument

Table 2A: Length and shares of different types of months 1950-2019

Month	Effective length short months	Effective length Long months	Share long	Effective length non-LMAS	Effective length LMAS	Difference	Share LMAS
January	29.5	31.6	0.72	30.4	32.5	2.06	0.28
February	28.3	—	0	28.3	—	—	0
March	29.5	31.6	0.69	29.5	31.6	2.06	0.69
April	28.5	30.6	0.72	29.4	31.5	2.07	0.31
May	29.5	31.6	0.72	30.4	32.5	2.05	0.28
June	28.5	30.6	0.69	29.4	31.5	2.11	0.28
July	29.5	31.6	0.72	30.4	32.5	2.09	0.31
August	29.5	31.6	0.71	30.4	32.5	2.13	0.28
September	28.5	30.6	0.71	29.4	31.5	2.14	0.29
October	29.5	31.6	0.73	30.4	32.5	2.11	0.29
November	28.5	30.6	0.71	29.4	31.5	2.10	0.27
December	29.5	31.6	0.72	30.4	32.5	2.08	0.29
Average	29.1	31.0	0.66	29.8	32.1	2.09	0.30

Figure 7A: Effect of LMAS-induced income shortages on measures of financial difficulties among welfare recipients and non-recipients, household-level fixed effects model



Note: Coefficient plot from regressions of indicated outcomes on the indicator for long-month-after short (LMAS). OLS regression including month, year, and household fixed effects. Markers are point estimates, horizontal lines 95% confidence intervals.

B.1 Balance

Table 3A: Balance ALLBUS data

	Non-LMAS			LMAS			Diff
	n	mean	sd	n	mean	sd	
Female	45,358	0.516	0.500	16,174	0.522	0.500	-0.019
Age/100	45,285	0.474	0.174	16,149	0.483	0.177	-0.001
Married, live together	45,328	0.591	0.492	16,164	0.589	0.492	-0.034*
Married, live apart	45,328	0.015	0.123	16,164	0.016	0.125	0.004
Widowed	45,328	0.095	0.294	16,164	0.101	0.301	0.004
Divorced	45,328	0.067	0.251	16,164	0.067	0.250	-0.001
Single	45,328	0.231	0.421	16,164	0.227	0.419	0.027*
Without degree	45,259	0.019	0.138	16,133	0.020	0.140	0.002
Basic high school degree	45,259	0.432	0.495	16,133	0.440	0.496	0.003
High school degree	45,259	0.295	0.456	16,133	0.292	0.454	0.005
Advanced technical college	45,259	0.056	0.229	16,133	0.054	0.226	-0.008
A-Levels	45,259	0.185	0.389	16,133	0.183	0.386	0.001
Other school-leaving degree	45,259	0.005	0.067	16,133	0.003	0.055	-0.000
In education	45,259	0.008	0.088	16,133	0.008	0.091	-0.003
Member of armed forces	45,358	0.001	0.039	16,174	0.001	0.038	0.000
Legislator, senior official or manager	45,358	0.027	0.163	16,174	0.025	0.155	0.000
Professional	45,358	0.059	0.236	16,174	0.053	0.225	-0.004
Technician or associate professional	45,358	0.073	0.261	16,174	0.074	0.262	-0.005
Clerk	45,358	0.043	0.203	16,174	0.044	0.205	-0.002
Service worker or shop and market sales worker	45,358	0.036	0.185	16,174	0.035	0.185	0.007
Skilled agricultural or fishery worker	45,358	0.009	0.097	16,174	0.008	0.091	-0.001
Craft or related trades worker	45,358	0.067	0.249	16,174	0.062	0.241	-0.002
Plant and machine operator or assembler	45,358	0.026	0.160	16,174	0.022	0.148	0.001
Elementary occupation	45,358	0.019	0.137	16,174	0.017	0.130	-0.003

* $p < 0.05$, ** $p < 0.01$.

Table 4A: Balance ESS data

	Non-LMAS			LMAS			Diff
	n	mean	sd	n	mean	sd	
Female	17,325	0.494	0.500	6,017	0.501	0.500	-0.007
Age/100	17,185	0.481	0.182	5,972	0.486	0.180	-0.010
Lives with husband/wife/partner at household	17,246	0.625	0.484	5,985	0.627	0.484	0.028
Does not live with partner	17,246	0.375	0.484	5,985	0.373	0.484	-0.028
Less than lower secondary	17,255	0.029	0.169	5,992	0.028	0.165	-0.004
Lower secondary	17,255	0.118	0.323	5,992	0.112	0.316	-0.022
Lower tier upper secondary	17,255	0.449	0.497	5,992	0.459	0.498	0.045*
Upper tier upper secondary	17,255	0.039	0.194	5,992	0.039	0.194	0.001
Advanced vocational, sub-degree	17,255	0.163	0.369	5,992	0.164	0.371	-0.008
Lower tertiary education, BA level	17,255	0.080	0.271	5,992	0.075	0.263	-0.010
Higher tertiary education, >= MA level	17,255	0.120	0.325	5,992	0.121	0.326	0.002
Member of armed forces	17,325	0.004	0.060	6,017	0.003	0.058	0.001
Legislator, senior official or manager	17,325	0.058	0.234	6,017	0.056	0.229	0.002
Professional	17,325	0.147	0.354	6,017	0.150	0.357	0.006
Technician or associate professional	17,325	0.182	0.386	6,017	0.183	0.387	0.009
Clerk	17,325	0.108	0.310	6,017	0.121	0.327	0.012
Service worker or shop and market sales worker	17,325	0.123	0.328	6,017	0.120	0.325	-0.010
Skilled agricultural or fishery worker	17,325	0.023	0.149	6,017	0.024	0.153	-0.004
Craft or related trades worker	17,325	0.129	0.335	6,017	0.127	0.333	0.006
Plant and machine operator or assembler	17,325	0.065	0.247	6,017	0.061	0.240	0.004
Elementary occupation	17,325	0.068	0.252	6,017	0.064	0.245	-0.017

* $p < 0.05$, ** $p < 0.01$.

Table 5A: Balance FORSA data

	Non-LMAS			LMAS			Diff
	n	mean	sd	n	mean	sd	
Female	2,136,640	0.465	0.499	913,384	0.464	0.499	-0.000
Age/100	2,131,249	0.447	1.250	911,071	0.453	1.038	0.007**
Single/lives alone	1,547,257	0.238	0.426	657,916	0.238	0.426	-0.003
Lives with partner/spouse	1,547,257	0.593	0.491	657,916	0.593	0.491	0.003
Divorced	1,547,257	0.081	0.273	657,916	0.082	0.274	0.001
Widowed	1,547,257	0.084	0.277	657,916	0.084	0.277	-0.001
Without degree	1,901,240	0.016	0.125	811,449	0.016	0.124	-0.001
Primary school	1,901,240	0.273	0.445	811,449	0.275	0.446	0.003
High school degree (West)	1,901,240	0.277	0.448	811,449	0.277	0.447	-0.001
High school degree (East)	1,901,240	0.049	0.215	811,449	0.049	0.216	0.001
Advanced technical college	1,901,240	0.062	0.240	811,449	0.061	0.240	-0.001
A-Levels	1,901,240	0.317	0.465	811,449	0.316	0.465	-0.002
Other school leaving degree	1,901,240	0.007	0.083	811,449	0.007	0.082	-0.000
Farmer	1,057,469	0.015	0.122	450,758	0.015	0.120	0.000
Self-employed professional	1,057,469	0.021	0.143	450,758	0.021	0.143	-0.001
Self-employed in trade/industry	1,057,469	0.103	0.304	450,758	0.103	0.304	0.003*
State servant, incl. military	1,057,469	0.086	0.281	450,758	0.087	0.281	-0.001
Employee	1,057,469	0.596	0.491	450,758	0.596	0.491	-0.002
Worker	1,057,469	0.148	0.355	450,758	0.149	0.356	0.000
In education	1,057,469	0.020	0.142	450,758	0.020	0.141	0.001
Employed in family business	1,057,469	0.003	0.051	450,758	0.002	0.049	-0.000

* $p < 0.05$, ** $p < 0.01$.

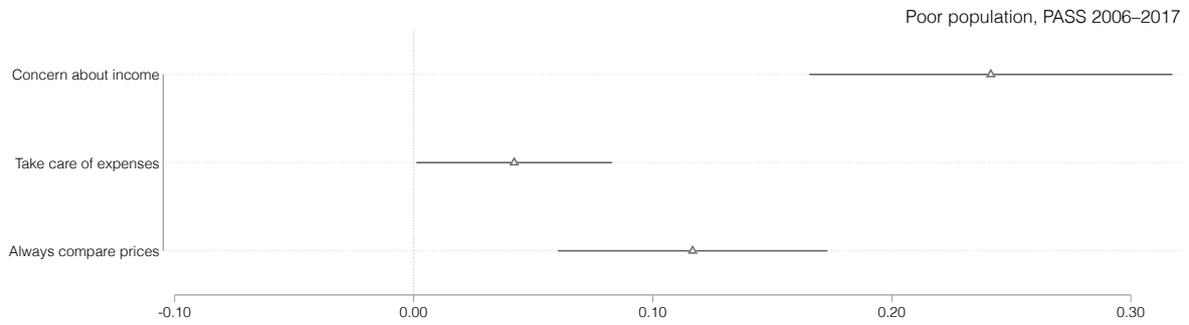
Table 6A: Balance PHF panel

	Non-LMAS			LMAS			Diff
	n	mean	sd	n	mean	sd	
Female	834	0.494	0.500	426	0.507	0.501	0.042
Age/100	834	0.502	0.181	426	0.483	0.183	-0.026
Single	834	0.336	0.473	426	0.362	0.481	-0.001
Divorced	834	0.195	0.397	426	0.181	0.385	-0.000
Widowed	834	0.080	0.272	426	0.047	0.212	-0.038*
Married/partnership, live together	834	0.360	0.480	426	0.373	0.484	0.022
Married, live apart	834	0.029	0.167	426	0.038	0.190	0.017
Without degree	834	0.044	0.206	426	0.042	0.201	-0.005
In education	834	0.001	0.035	426	0.007	0.084	0.003
Basic high school degree	834	0.404	0.491	426	0.385	0.487	-0.063
Higher secondary school	834	0.205	0.404	426	0.200	0.400	0.004
East German standard school	834	0.094	0.291	426	0.103	0.305	0.011
Advanced technical college	834	0.050	0.219	426	0.047	0.212	0.017
A-Levels	834	0.201	0.401	426	0.216	0.412	0.033
Member of armed forces	834	0.000	0.000	426	0.000	0.000	0.000
Legislator, senior official or manager	834	0.006	0.077	426	0.002	0.048	-0.006
Professional	834	0.037	0.189	426	0.026	0.159	0.003
Technician or associate professional	834	0.062	0.242	426	0.056	0.231	-0.006
Clerk	834	0.029	0.167	426	0.031	0.172	-0.008
Service worker or shop and market sales worker	834	0.080	0.272	426	0.063	0.244	-0.016
Skilled agricultural or fishery worker	834	0.026	0.160	426	0.009	0.097	-0.013
Craft or related trades worker	834	0.032	0.177	426	0.052	0.222	0.016
Plant and machine operator or assembler	834	0.022	0.145	426	0.045	0.207	0.016
Elementary occupation	834	0.065	0.246	426	0.063	0.244	-0.004

* $p < 0.05$, ** $p < 0.01$.

B.2 Differential effects among welfare recipients and 'working poor'

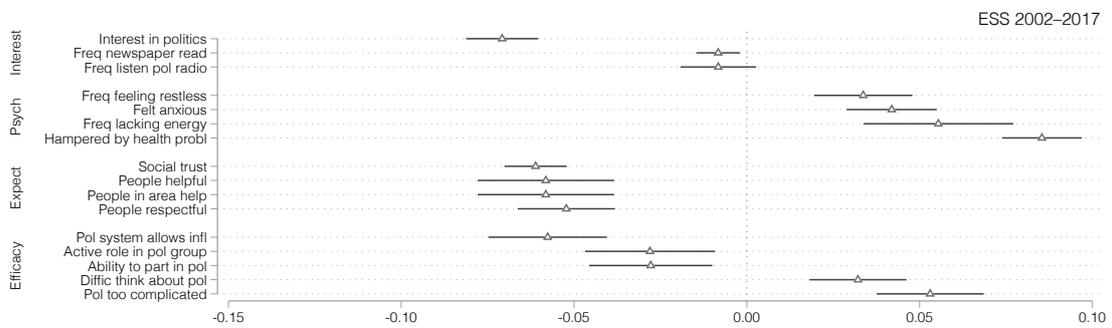
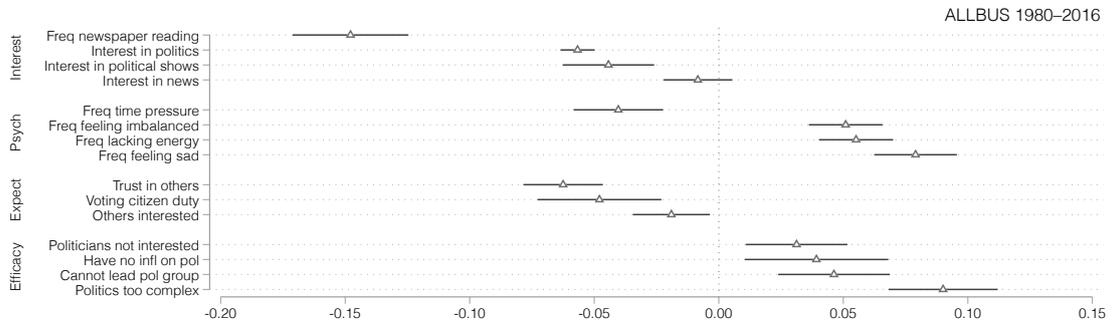
Figure 8A: Financial concerns and financial prudence among welfare recipients and non-recipients



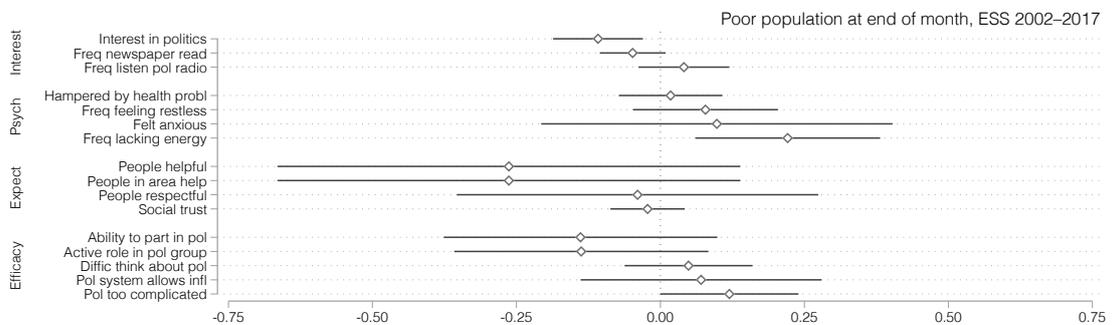
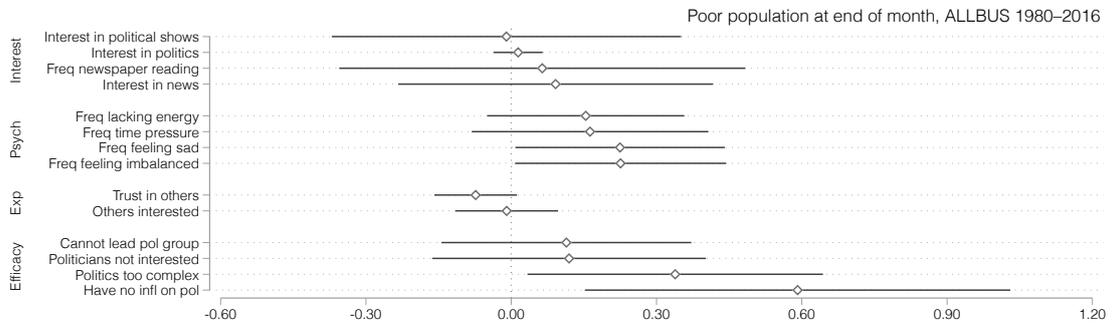
Note: Coefficient plot from regressions of indicated outcomes on indicator recording if individual is welfare recipient. OLS regression controlling for age, sex, education, parents' education and including month, year, and state fixed effects. Markers are point estimates, horizontal lines 95% confidence intervals.

C Mechanisms

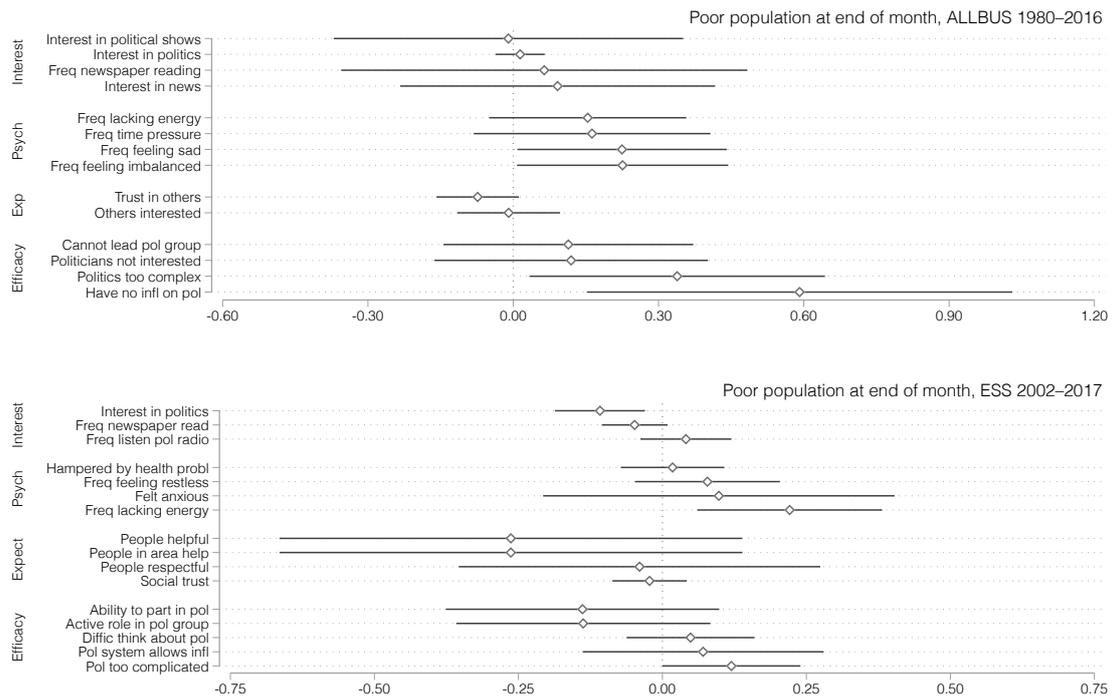
Figure 9A: Mechanisms theorized to cause lower levels of political participation, individual indicators



(a) Correlation of poverty with theorized mechanisms



(b) Correlation of mechanisms and turnout



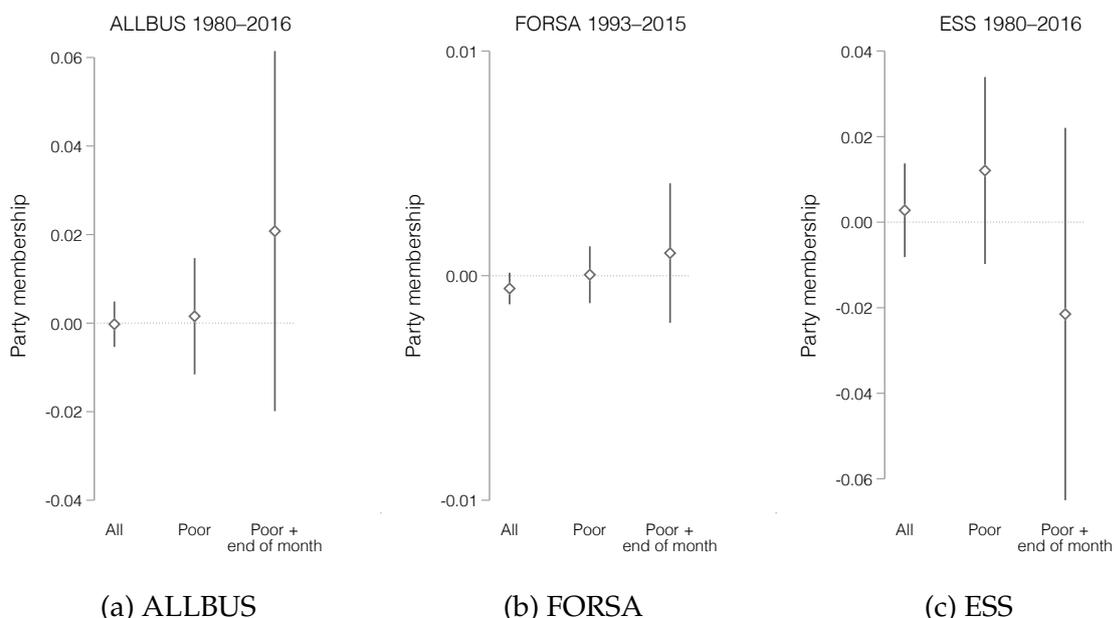
(c) Causal effect (LMAS) on mechanisms

Note: Figure 9A plots the coefficients for regressions i) of the indicated outcome on the indicator for poverty, defined as earning less than 60% of the means-adjusted median income (Figure 9Aa), ii) of turnout on mechanisms (only available for the ALLBUS data, Figure 9Ab), and iii) of mechanisms on the indicator for long-month-after short (LMAS) months (Figure 9Ac). OLS regressions controlling for age, sex, education, and parents' education, and including month, year, and state fixed effects. Results for individual indicators forming the composite scales in Figure 6 in the main text. ALLBUS 1980–2016 and ESS 2002–2017 data. Markers are point estimates, horizontal lines 95%/90% confidence intervals.

D Robustness

D.1 Falsification/placebo test

Figure 10A: Effect of LMAS-induced income shortages on party membership



Note: Figures 10Aa, 10Ab, and 10Ac plot the coefficients for individual-level regressions of party membership on the indicator for long-month-after short (LMAS) months. OLS regressions controlling for age, sex, education, and parents' education, and including month, year, and state fixed effects. ALLBUS 1980–2016, FORSA 1993–2015, and ESS 2007–2017 data. Markers are point estimates, vertical lines 95% confidence intervals.

D.2 Possible response bias

The main threat to inference comes from the instrument affecting other variables a) by making people less likely to respond in the first place, b) through its effects on cognition, time discounting etc., that might affect the recall of information. To address challenge a), I carefully check for signs that survey response is lower in LMAS than at other times, esp. among the poor; I find some evidence that this is indeed the case. While the bias introduced is unfortunate and hard to address, it should be noted that lower response rates almost certainly create biases our results upwards, i.e. making it harder for us to detect an effect. Those that do not participate in LMAS due to short-poverty are plausibly less politically engaged. The effects reported are therefore likely conservative estimates. Challenge b) is most likely to bias the analysis by way of

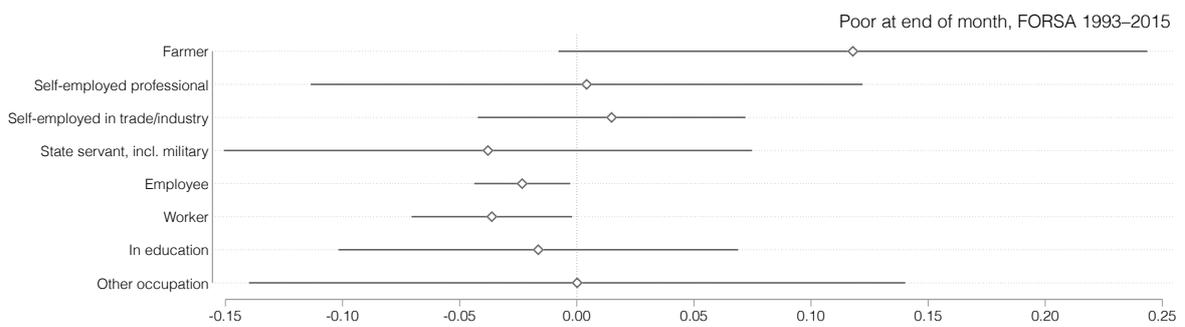
control variables. For example, poverty might affect the recall of past turnout. Even though past turnout in theory is pre-treatment and hence could be safely included in the list of covariates, this potential for recall bias urges a more careful approach. In the standard models I hence only control for variables that are easily remembered—namely basic demographics.

D.3 Imputation of missing values

In the Forsa data, for 5% of observations the value for education was missing, and for 20% of observations the value for income. In order to check the robustness of my findings, I imputed missing values with chained multiple imputation using predictive models (ordered logit for education and income categories, and predictive mean matching for absolute income levels) with sex, age, education, income and state as predictors. The same procedure was used for the ESS data, where 18% of observations for income were missing. Re-estimating the results with imputed data, including a poverty threshold computed based on imputed incomes, leaves outcomes largely unaffected—both in terms of effect sizes and statistical significance. I therefore decided to present results based on un-imputed data only. All results using imputed data are available upon request.

D.4 Causal effect of LMAS for different occupational groups

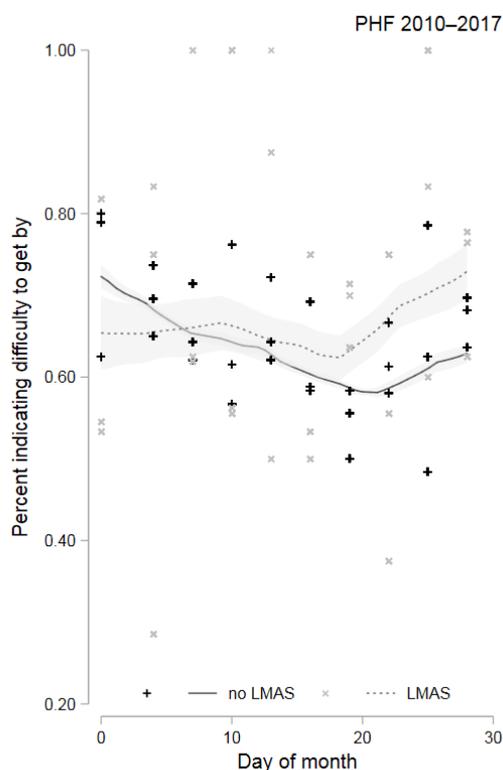
Figure 11A: Effect of LMAS-induced income shortages on turnout intentions, by occupation (FORSA data)



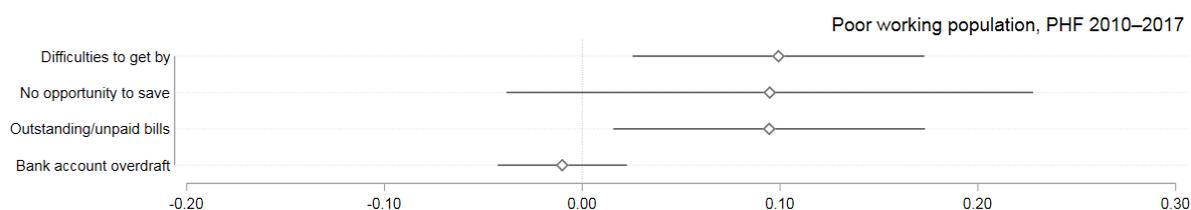
Note: Figure 11A plots the coefficients for regressions of turnout intentions on the indicator for long-month-after short (LMAS) months for eight occupational groups. OLS regressions controlling for age, sex, education, and parents' education, and including month, year, and state fixed effects. Forsa 1993–2015 data. Markers are point estimates, horizontal lines 95% confidence intervals.

D.5 Replication of first-stage results with Panel on Household Finances (PHF) data

Figure 12A: Relationship between LMAS and financial difficulties in Panel on Household Finances (PHF)



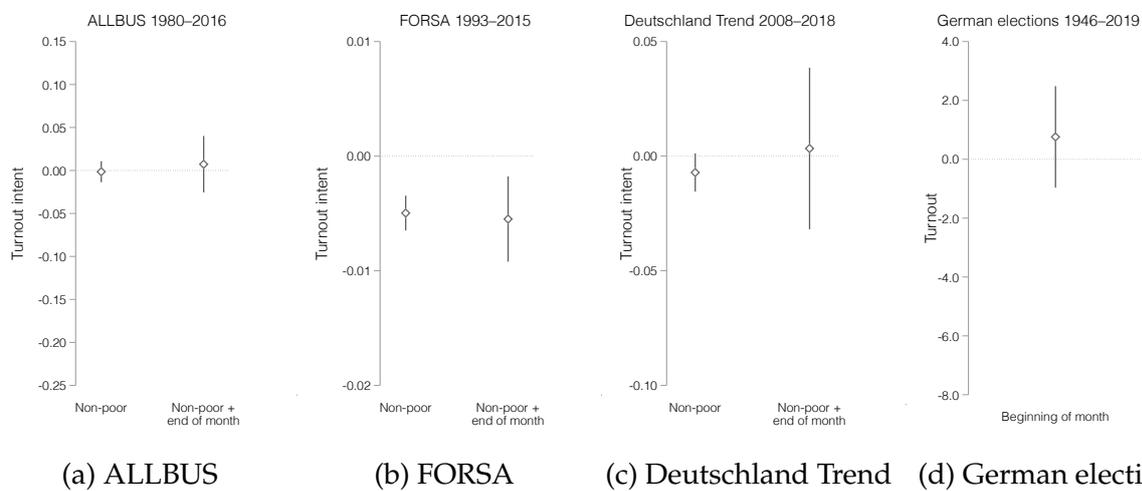
(a) Share of poor respondents indicating financial difficulties in LMAS and non-LMAS



(b) Effect of long-months-after-short (LMAS) on measures of financial difficulties

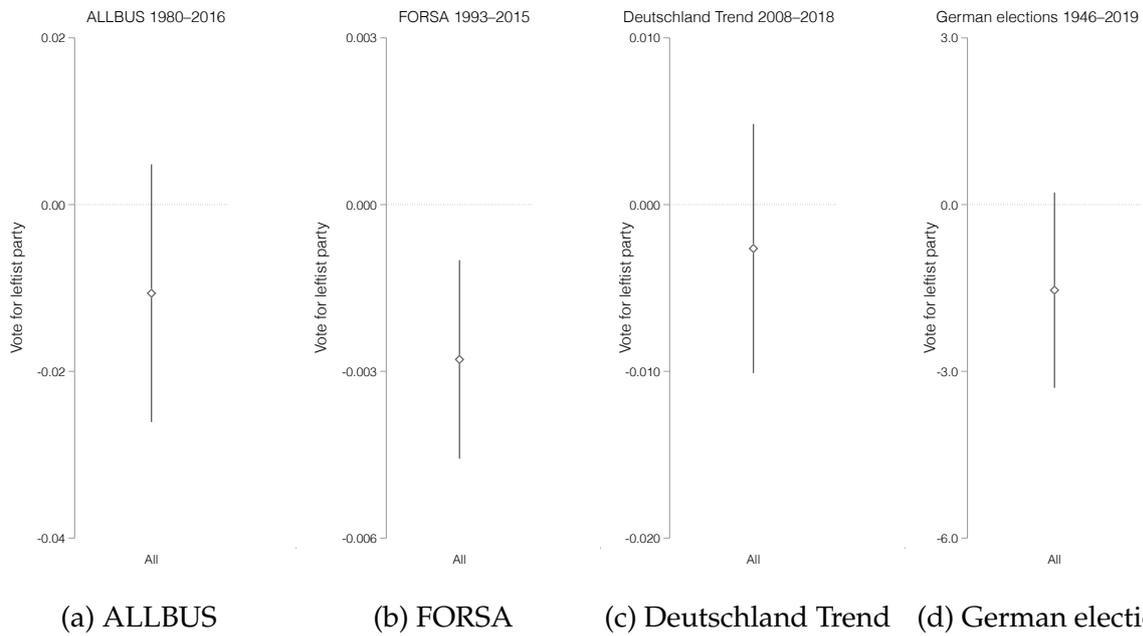
Note: Figure 12Aa plots the percentage share of respondents indicating difficulties to get by on their monthly income against the day of the week. The lines are kernel density plots (Epanechnikov kernel with optimal bandwidth) for respondents interviewed during a long-month-after short (LMAS, dashed line) or non-LMAS months (solid line). Markers are day-of-month averages of financial difficulties. The shaded areas are 95% confidence intervals. Figure 12Ab shows the coefficients for a regression of the indicated measures of financial difficulty on the LMAS instrument. OLS regression controlling for age, sex, and education, and including month, year, and state fixed effects. Markers are point estimates, horizontal lines 95% confidence intervals. PHF 2010-2017 data, $n=834$.

Figure 13A: Causal effect of LMAS-induced income shortages on turnout intentions and observed turnout, non-poor population



Note: Figures ??, ??, and ?? plot the coefficients for individual-level regressions of turnout intentions on the indicator for long-month-after short (LMAS) months. OLS regressions controlling for age, sex, education, and parents' education, and including month, year, and state fixed effects. ALLBUS 1980–2016, FORSA 1993–2015, and Deutschland Trend 2008–2018 data; Figure 13Ad plots the coefficients from a multi-level regression of turnout on the indicator for LMAS, with intercepts allowed to vary on the level of the decade and the geographic unit, and controlling for monthly fixed effects, an indicator for the length of the month, and the turnout in the previous election. German electoral turnout dataset (compiled by author). Markers are point estimates, vertical lines 95% confidence intervals.

Figure 14A: Effect of LMAS-induced income shortages on voting for the left



Note: Figures 14Aa, 14Ab, and 14Ac plot the coefficients for individual-level regressions of intentions to vote for the leftist parties *SPD* and *Die Linke* on the indicator for long-month-after short (LMAS) months. OLS regressions controlling for age, sex, education, and parents' education, and including month, year, and state fixed effects. ALLBUS 1980–2016, FORSA 1993–2015, and Deutschland Trend 2008–2018 data; Figure 14Ad plots the coefficients from a multi-level regression of vote share for *SPD* and *Die Linke* on the indicator for LMAS, with intercepts allowed to vary on the level of the decade and the geographic unit (the states or the federal state), and controlling for monthly fixed effects, an indicator for the length of the month, and turnout and SPD-vote share in the previous election. German elections dataset (compiled by author). Markers are point estimates, vertical lines 95% confidence intervals.

E Poll on salary payment conventions in Germany

In order to ascertain payment conventions for salaries, with the help of a student assistant I conducted a poll among a random sample of firms representing the 20 most common professions in Germany. Firms were selected and contacted according to the following procedure:

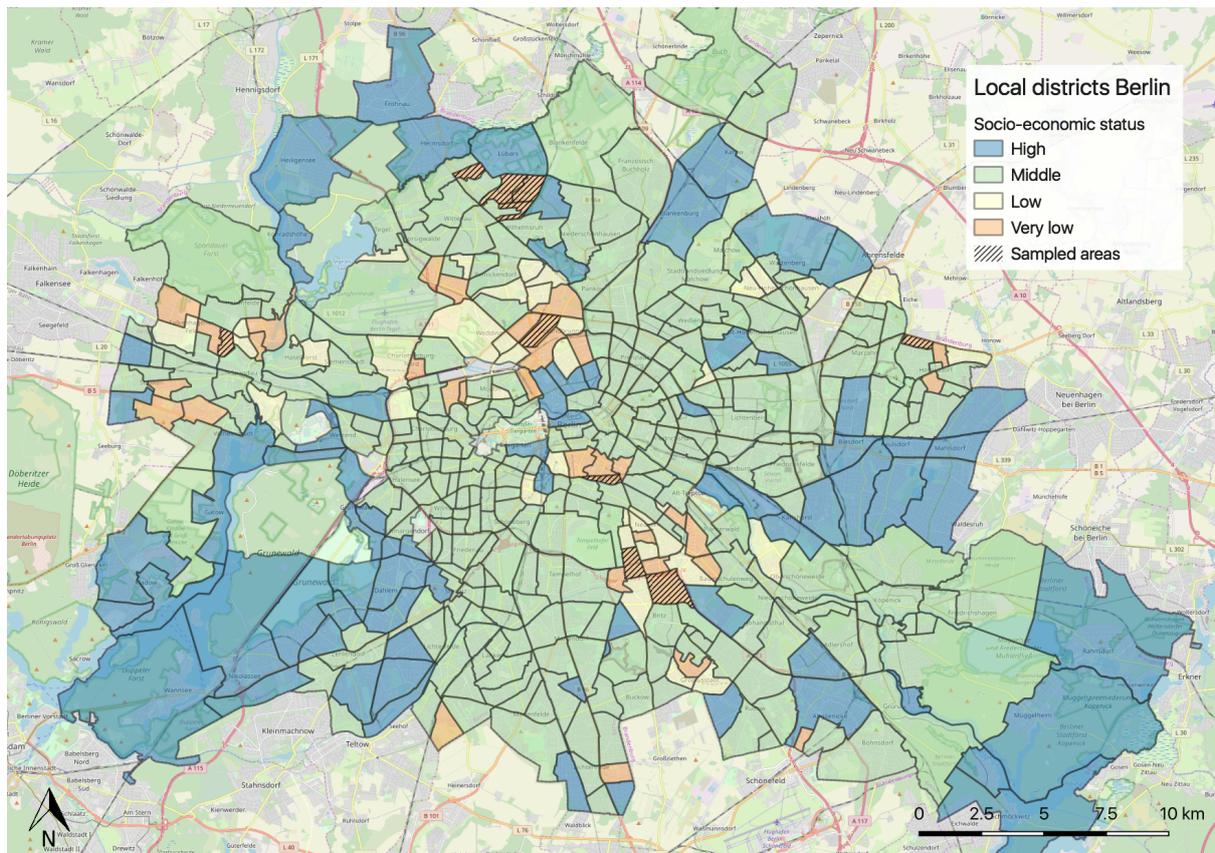
1. Based on data from the European Social Survey (ESS, 2018) for Germany, we identified the 20 most common professions in Germany
2. Concrete job descriptions were assigned to each professions to facilitate our search on 'Gelbe Seiten', a telephone directory listing virtually all firms in Germany
3. Using R's random function, each job position got assigned three different random zip codes which we used for calling a company in that region
4. The online version of 'Gelbe Seiten' displays a certain amount of hits for the searched category within the zip code area
5. Using excels random number function we received a random number within the range of the number of hits.
6. The company belonging to that number was called and informed about the study before being asked on which date someone in the corresponding job position is paid. If the person could not be reached, we tried to call again later. If nobody was reached after trying three times, a different company within that zip code area was called.

In total, 29 firms volunteered to take part in the poll. Out of these, 28 (97%) stated that they pay their employees on a monthly basis, while one said that their employees are paid bimonthly. 19 firms (66%) indicated that they pay their salaries at the end of the month, 8 said they paid at the beginning or in the middle of a month, and 2 firms stated that payment conditions depend on how individual contracts are negotiated.

F Qualitative Interviews — Sampling and recruitment

I recruited participants through two channels. First, I contacted the National Conference on Poverty, an organization that brings together charitable organizations from all over Germany with engaged individuals, who are themselves affected by poverty. I carried out a total of 10 interviews with individuals recruited from this group. These are highly engaged—and hence self-selected—individuals, who are unlikely to be representative for the general poor population. I therefore, second, also recruited interview partner through a second channel. Notably, I recruited participants by distributing flyers in deprived neighborhoods of the German capital Berlin. I sampled recruitment areas among the 42 (out of 447) local administrative areas in Berlin classified as ‘very low’ socio-economic status by official administrative data. A total of 10 districts were randomly sampled (with sampling weights corresponding to population size, see Figure 15A). In those districts flyers were distributed that described, in general terms, the purpose of the research, and provided contact details (telephone and email) of the researcher. Potential respondents would then contact the researcher if they wanted to take part in the study. A compensation of 20 Euros per interview hour was offered. Interviews were conducted either at the institute where the author was based or in a café close to where respondents lived, depending on what respondents considered more convenient.

Figure 15A: Sampled districts for qualitative interviews



Note: Figure 15A shows the local districts (*Lebensweltlich orientierte Räume*) of the German capital Berlin by official socio-economic status. Districts with 'very low' socio-economic status shaded in red. Sampled districts shaded.