

Off to a Bad Start: Unemployment and Political Interest during Early Adulthood

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It is often argued that unemployment depresses political involvement because unemployment deprives workers of important workplace-related resources. We challenge this argument from two sides. Theoretically, we argue that the resource approach neglects life cycle stages. Socialization theory suggests that workplace-related resources influence political involvement mostly during adolescence and early adulthood but not later in life. Ignoring life cycle stages therefore risks underestimating unemployment effects on young workers and overestimating them for older workers. Our second criticism is methodological. Existing literature on unemployment and political involvement largely uses cross-sectional data, which makes it impossible to eliminate unobserved heterogeneity. Using German panel data, we show that unemployment depresses the growth of political interest in early adulthood, while it does not have an effect later in life. Moreover, we find that early unemployment experiences have scarring effects that reduce the long-term level of political interest and participation.

The adverse consequences of unemployment is a classical topic in social science research as well as a pressing social concern. The latter is particularly true for young citizens. In the United States, as well as in Europe, youth unemployment rates are twice as high as for older workers.¹ Particularly in the aftermath of the Great Recession, youth unemployment has reached dramatic levels. The International Labour Office (ILO 2012, 1) even speaks of a youth employment crisis of “unprecedented proportions,” and the Organization for Economic Cooperation and Development (OECD 2010, 123) worries about a “lost generation.”

While there is a broad consensus in labor economics that unemployment has important and long-lasting “scarring effects” on socioeconomic outcomes (Arulampalam 2001), there is surprisingly little research in recent years on the effect that unemployment—and in particular youth unemployment—has on political involvement. Given that labor market entrance typically coincides with a crucial stage

in political socialization, this aspect deserves more attention. This paper therefore addresses two questions. Does youth unemployment have a stronger depressing effect on political involvement than unemployment experienced later on in life? And does youth unemployment have lasting effects on political involvement similar to the scarring effects on socioeconomic outcomes?

Political sociology scholarship provides important tools to analyze the effect of unemployment on political involvement. Classical contributions have argued and shown that unemployment experience has a negative effect on political involvement (Brody and Sniderman 1977; Jahoda, Lazarsfeld, and Zeisel 2002; Rosenstone 1982). The underlying theory states that unemployment deprives workers of important resources for political involvement: exposure to political discussions and recruitment, income, and a site to “practice” civic skills (Brady, Verba, and Schlozman 1995). Yet recent research has raised doubts about the proclaimed negative effect of unemployment on political involvement

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Data and supporting materials necessary to reproduce the numerical results in the paper are available in the *JOP* Dataverse (<https://dataverse.harvard.edu/dataverse/jop>). An online appendix with supplementary material is available at <http://dx.doi.org/10.1086/688226>.

1. Organization for Economic Cooperation and Development, Labor Force Statistics. <http://stats.oecd.org> (accessed October 19, 2015).

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(Burden and Wichowsky 2014; Charles and Stephens 2013; Prior 2010).

We identify two reasons why the scholarly literature is inconclusive. Theoretically, the existing political sociology approach to unemployment is incomplete. In particular, the resource theory neglects life cycle stages. Based on the political socialization literature, we argue that workplace-related resources have their strongest effect on political involvement during the “impressionable years” of adolescence and early adulthood (Dinas 2013; Schuman and Scott 1989). The influence of workplace-related resources declines with age because they produce a ratchet effect: through socialization, political involvement becomes habitual so the loss of resources due to unemployment no longer depresses involvement. However, unemployment experienced during early adulthood can impede this socialization process and depress involvement in the short term as well as in the long term. Ignoring life cycle stages therefore risks underestimating political behavior effects of unemployment on young workers and overestimating them for those past early adulthood.

Our second criticism is methodological. Existing literature on unemployment effects is largely based on cross-sectional data and therefore cannot account for unobserved heterogeneity due to selection into unemployment (Schlozman and Verba 1979). This is a serious problem because it has been argued that “unemployment is in large part a proxy for other factors that dampen participation rates” (Burden and Wichowsky 2014, 895). While longitudinal designs are therefore preferable, it is far from obvious which estimation strategy is appropriate in the context of such an analysis. As we show, fixed effects regressions are not suitable to capture (youth) unemployment effects because early adulthood is, on average, characterized by strong growth of political involvement. If unemployment only dampens this growth rather than reduces the level of political involvement, estimation strategies based on within-case variation are biased toward finding a null effect. We therefore rely on difference-in-difference propensity score matching. By using as our baseline the trend of political involvement among respondents without unemployment experience, we provide a more appropriate counterfactual for unemployment effects than would be possible based on within-case variation only.

Concretely, we analyze how unemployment spells interact with life cycle stages in depressing political interest. Political interest can be seen as a basic orientation tapping citizens’ curiosity about and attentiveness to political processes (van Deth 1990; Zaller 1992). As such, it is a core dimension of political involvement (Prior 2010). Although

political interest is conceptually different from political behavior, it is an important determinant of different forms of participation and other positive orientations toward the political system (Brady et al. 1995; Powell 1986). An advantage of political interest as a dependent variable therefore is that we do not have to restrict our analysis to certain forms of political behavior. Moreover, political interest is considered to be comparatively stable (Prior 2010), which makes our analysis a conservative test of unemployment effects on political involvement.

Using data from the uniquely suitable German Socio-Economic Panel (SOEP) for the years 1985–2013, we show that unemployment has a negative effect on political interest. However, this negative effect is exclusively driven by young labor market entrants with little previous working experience, while it cannot be observed for more experienced workers. Although the immediate effect of unemployment is restricted to young workers, it can have negative long-term effects. Even after more than 10 years, we observe a negative effect of early unemployment on political interest and participation.

EMPLOYMENT AND POLITICAL PARTICIPATION: A RESOURCE PERSPECTIVE

Although scholarly interest has been surprisingly limited in recent years, there is a long tradition in political sociology that links (un)employment to political involvement. In their classical Marienthal study, Jahoda et al. (2002) observe how major unemployment shocks erode political interest and activity in a community. Also, in more recent literature, unemployment has been shown to have a negative effect on political involvement (Anderson 2001; Emmenegger, Marx, and Schraff 2015; Gallego 2007; Lim and Sander 2013; Marx and Nguyen 2016; Rosenstone 1982; Rosenstone and Hansen 1993; Schur 2003).

However, the unemployment-involvement link is not borne out by all analyses (Charles and Stephens 2013; Schlozman and Verba 1979; Smets and van Ham 2013), and the underlying theory is rather unclear (Burden and Wichowsky 2014). Usually negative unemployment effects are explained with reference to resources attached to a job. This argument comes in two versions. The first focuses on income. It turns on the vague notion that economic hardship diverts attention from social to personal problems. Because of the income loss and the financial worries associated with unemployment, citizens are forced to focus their resources on “holding body and soul together—surviving—not on remote concerns like politics” (Rosenstone 1982, 26).

The influential civic voluntarism model by Brady et al. (1995) provides a second, more elaborate, resource-based explanation. Accordingly, core workplace-related resources for political participation (besides money) are time and civic skills, the latter of which are most relevant for our topic. Civic skills are communication and organizational skills, such as articulating opinions, leading meetings, and influencing decision-making processes. Brady et al. (1995, 273) argue that “those who have an opportunity to do these things in a nonpolitical setting would, presumably, be more willing and able to do them in a political context.” The workplace is the most important, but not the only, “training ground” in which civic skills can be acquired.

Thus, Brady et al.’s (1995) argument is akin to the “spill-over model” (Pateman 1970; Sobel 1993), according to which participation in workplace decision making provides competencies and orientations that facilitate political involvement (Adman 2008; Schur 2003). However, the workplace not only offers opportunities to improve individual skills. It also provides resources in the form of social networks by exposing workers to political discussions in informal groups and to political recruitment through unions (Schlozman, Burns, and Verba 1999). Network size and frequency of political discussions are often argued to foster political interest and participation, for instance, because they provide easy access to political information or through social norms (Campbell 2013; Klofstad 2010; McClurg 2003). The workplace has been shown to provide ample opportunity for such discussions in an extended network (Mutz and Mondak 2006), while unemployment is often characterized by a lack of social contact (Brand 2015). In line with these arguments, Highton and Wolfinger (2001) show that being in the labor force increases young citizens’ propensity to vote.

In sum, the civic voluntarism model offers a powerful argument as to why employment is important for political involvement. Being in work is associated with a number of resources—in particular, civic skills and politically relevant networks—that foster interest and participation. By implication, being out of work deprives workers of these resources. Does this mean that we can explain the negative effect of unemployment on political involvement with reference to workplace-related resources? Indeed, existing research often relies on the civic voluntarism or spill-over models as a theoretical starting point (Adman 2008; Burden and Wichowsky 2014; Lim and Sander 2013; Schur 2003).

In our view, a problem of the resource argument—when applied to the unemployed—is its failure to specify how exactly resources and civic skills in particular adjust to unemployment shocks (which, to be clear, is also not the

explicit goal of the theory). The civic voluntarism model explicitly acknowledges the role of socialization:

The acquisition of civic skills begins early in life—at home and, especially, in school. However, the process need not cease with the end of schooling but can continue throughout adulthood. Adult civic skills relevant for politics can be acquired and honed in the nonpolitical institutions of adult life. (Brady et al. 1995, 273)

However, unemployment research in the civic voluntarism tradition has largely neglected the role of socialization. It has therefore overlooked the possibility that socialization may produce a ratchet effect: once workers have acquired and honed civic skills in the workplace, and once they are socialized by workplace-related networks into political involvement, unemployment is unlikely to reverse this process. As we argue below, the unemployed should be expected to sustain their political involvement if their political socialization is largely completed. Hence, the civic voluntarism model is useful to explain the involvement gap of groups that are systematically excluded from certain workplace experiences (e.g., women in Schlozman et al. [1999]), but to explain the effect of an unemployment shock it has to be complemented with a life cycle perspective accounting for political socialization stages.

EMPLOYMENT AND POLITICAL PARTICIPATION: A SOCIALIZATION PERSPECTIVE

Commenting on the literature on voting turnout, Plutzer (2002, 42) criticizes that “Scholars in the resource or SES tradition rarely pay much attention to age.” He contrasts this tradition with a life cycle framework that “takes aging and development as the starting point rather than an afterthought.” Indeed, political socialization studies show that events have different effects on political orientations and behavior depending on when they occur during the life course. For instance, the socialization literature stipulates that orientations are still malleable in adolescence and early adulthood but become resilient later on (Dinas 2013; Schuman and Scott 1989; Sears and Funk 1999; Stoker and Jennings 2008). Accordingly, both political participation and political interest have been shown to become habitual and hence resilient to change over the life course (Gerber et al. 2003; Plutzer 2002; Prior 2010).

Socialization theory has three important implications for unemployment effects on political involvement. First, after early adulthood, workers should experience a ratchet effect when they lose their jobs; that is, they stick to the “habit” of

participating or being attentive to politics. Put differently, they do not have to exercise skills acts or regularly discuss politics in the workplace any longer to sustain their involvement. In line with this argument, Rosenstone (1982) shows that unemployment only affects participation immediately after job loss, while the effect starts to fade after some weeks. This “rebound” illustrates that a habituation effect typically kicks in after a period of adjustment to unemployment (Plutzer 2002).

Second, unemployment has potentially devastating effects if it occurs in early adulthood. If the workplace is as critical a site for political socialization as the literature suggests, unemployment among young labor market entrants obstructs this process. Young unemployed are cut off from networks in which politics are discussed and civic skills are practiced. Instead, they are forced into idleness and, potentially, isolation (Brand 2015). Early in the socialization process, civic skills and involvement may not be sufficiently resilient to “survive” such shocks (Dinas 2013). As a result, it may be that previous studies based on pooled age groups have underestimated the effect of youth unemployment on political involvement while they have overestimated the effect for older workers.

Third, the socialization perspective implies that events occurring early in life may have a lasting effect on political involvement (Klofstad 2010). For instance, Firebaugh and Chen (1995) show for US women that being socialized before the introduction of female suffrage has a negative effect on turnout that endures for decades. Although unemployment is probably less influential than gender norms, a similar mechanism could be at play in the case of youth unemployment. For citizens whose workplace-based socialization during formative years has been impeded by persistent or recurrent unemployment, the level of political interest or the participation tendency may never reach the levels of those citizens with uninterrupted work biographies. The result would be a sustained negative effect of early unemployment on political involvement, similar to the scarring effect observed in labor economics.

In sum, our theoretical argument for how unemployment affects political involvement combines the civic voluntarism model with insights from the political socialization literature. We agree with Brady et al. (1995) and others that the workplace is a crucial site for acquiring civic skills, and we also agree that being deprived of employment can have negative effects on political involvement. But, based on the socialization literature, we expect that the disruptive potential of unemployment declines over the life course. This decline justifies a focus on unemployment experience among young citizens.

The socialization literature does not clearly specify why the young are more susceptible to attitude change. Dinas (2013) distinguishes two lines of argument. First, there could be a greater density of significant life events in early adulthood compared to later on in life. Getting a first job is an example for a significant event that affects political involvement of the young (Highton and Wolfinger 2001). The second argument stresses the greater weight young individuals attach to new information, so that similar events could make a stronger impression at a young age. For the purpose of our paper, we are agnostic about the relative importance of events and age in the socialization process. Since getting a first job typically occurs at a young age, it is beyond the scope of this paper to disentangle the two effects.

We examine three hypotheses. The *civic skills hypothesis* expects unemployment to have a negative effect on political interest irrespective of age. The *socialization hypothesis*, in contrast, expects unemployment to have a negative effect on the growth of political interest during early adulthood. On average, political interest follows a pattern of strong growth at young age and flattening growth as people become older (Glenn and Grimes 1968; Neundorf et al. 2013). Such a growth pattern has important implications when investigating the effect of unemployment on political interest because the adverse effect of early unemployment moves in parallel with the rather robust increase over the life cycle.

Figure 1 provides a stylized example of how we expect early unemployment experiences to affect political interest. We hypothesize that such experiences depress the growth of political interest, thereby creating an “involvement gap” between respondents with and without early unemployment experiences. Unless there is a catch-up process, this gap could be sustained over the life course. Hence, our third hypothesis, the *scarring hypothesis*, expects unemployment during early adulthood to have a persistent negative effect on the level of political interest net of current employment status among older respondents.

METHODOLOGICAL ADVANCEMENT: STUDYING UNEMPLOYMENT WITH PANEL DATA

The fact that most research linking labor market situation and political behavior is based on cross-sectional analyses has attracted considerable criticism (Adman 2008; Margalit 2013; Owens and Pedulla 2014). Most important, cross-sectional data do not allow disentangling causal effects of unemployment experience from selection into unemployment (Schlozman and Verba 1979). Obviously, the unem-

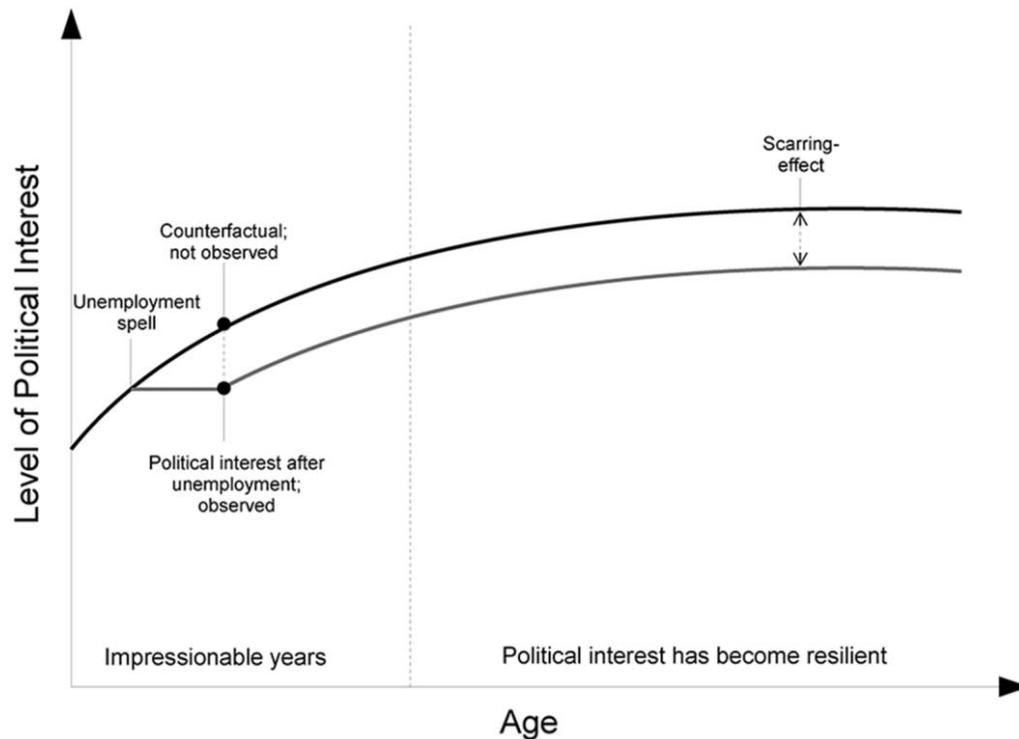


Figure 1. Hypothesized effects of early unemployment over the life cycle. Source: authors' own illustration

ployed differ in important ways from workers with secure jobs, many of which cannot be measured with surveys.

Moreover, cross-sectional analyses risk falling victim to the “fallacy of assumed reversibility” of political orientations (Plutzer 2002, 44).² Cross-sectional analyses yield estimates based on observed unemployed respondents. The interpretation of coefficients therefore rests on the assumption that a hypothetical job loss leads employed respondents to adjust to the average observed among the unemployed. But this is an artificial counterfactual, which is not based on actually observed cases of job loss and which therefore cannot capture habituation effects as described above. The within-case variation provided by panel data allows us to observe the same persons under employment and unemployment, avoiding extrapolations from the observed unemployed.

A third problem of cross-sectional data relates to policy implications. Given our overarching interest in whether

youth unemployment is a substantive problem in terms of political participation, we have to assess the longevity of effects. Longevity is impossible to analyze in cross sections. By relying on panel data, we can observe how the effect of unemployment unfolds over time and empirically observe its persistence.

In sum, the analytical advantages offered by panel data (removal of selection effects, proper counterfactuals, and temporal perspective) outweigh their disadvantages in terms of scope of available data sets. This is even more so as we can rely on a large sample representative of the German population (see below). One disadvantage is that political information is typically scarce in household panels. To take full advantage of the longitudinal structure, we need a dependent variable measured on a yearly basis. This limits the choice to political interest in our data set. It also means that we are not able to examine the role of important mediating variables, such as political efficacy. Another problem of panel data is bias because of nonrandom attrition. However, this can be corrected by weighting cases accordingly (see below).

DATA AND METHOD

We use data from the German Socio-Economic Panel (SOEP; provided by the German Institute for Economic Research)

2. As Plutzer (2002, 44) elaborates: “Consider two individuals whose earnings at age 30 place them in the 99th percentile. Based on current research, each is expected to have higher than average political participation. Suppose that four years later, one’s earnings plummet to the median. If voting is an acquired habit, it makes no sense to predict that her participation will fall to average levels. Nevertheless, this is exactly what most cross-sectional models force the researcher to assume.”

collected between 1984 and 2013. The SOEP is uniquely suitable for our purposes because it provides both sufficient observations as well as broad coverage of the relevant variables to examine the long-term effect of early unemployment experience on political interest.

We are primarily interested in people who entered the labor market and experienced unemployment in some of the subsequent years. Using all available SOEP waves, we follow respondents on a yearly basis from their first observation in employment onward. We code a respondent's first observation in employment as the first person-year (t_0) and investigate individual change in political interest over the subsequent observations t_1, t_2, t_3 , etc.). This allows us to draw on respondents from all available time periods. As in all panel studies, attrition reduces the sample over time, but the scope is acceptable in our case (app. III; apps. I–III are available online). Due to regular resampling in the SOEP, observations are distributed rather evenly over the whole time period (see fig. A1 in the appendix; figs. A1–A8 are available online).

We conduct two separate analyses using propensity score matching (PSM), an approach still underutilized in political science (Kam and Palmer 2011). We first investigate the immediate effect of unemployment on political interest for different age groups based on a specific radius matching strategy (Huber, Lechner, and Wunsch 2013). This approach effectively results in a two-wave panel design in which we analyze changes in political interest between pre-treatment and post-treatment waves. Second, scarring effects are analyzed in a cross-sectional PSM in which early unemployment serves as a treatment variable.

There are three main reasons for selecting a two-wave PSM (rather than a multi-wave fixed effects) approach in the first part of our analysis.³ First, early adulthood is, on average, characterized by strong growth of political interest (Glenn and Grimes 1968), and we expect unemployment to only dampen this growth rather than to reduce the overall level of political interest. To detect a negative effect of unemployment on political interest, fixed effects regressions would require a decrease in political interest from t_x to t_{x+1} , treating a respondent's observation in t_x as counterfactual. Yet, as figure 1 shows, the time before the unemployment spell is not the adequate counterfactual if unemployment is expected to only depress growth rather than reduce the absolute level of political interest. By relying on difference-

in-difference PSM, our baseline is the trend of political interest among respondents without unemployment experience, which clearly provides a more appropriate counterfactual.

Second, political interest shows low temporal variance (Prior 2010). While multiple-waves fixed effects models usually assume that shocks have immediate effects, we expect yearly intervals to be too short to affect such a resilient variable (see Adman [2008] for a similar discussion). A two-wave model (e.g., estimating change from t_0 to t_3) allows us to flexibly set time intervals without making assumptions about the timing of the effect.

Third, parametric fixed effects models make the restrictive assumptions that effects are linear and that the individual fixed effect is constant over time. PSM, on the other hand, allows for arbitrary effect heterogeneity and does not require linearity (Lechner, Miquel, and Wunsch 2011). Moreover, matching, unlike fixed effects regression models, does not have to assume strict exogeneity of all independent variables and is advantageous in case of rare treatment, which may lead to inflated standard errors in standard regression models (Smith 1997).

Panel data greatly strengthen inferences drawn from PSM estimators. They allow balancing treatment and control groups on pre-treatment information as well as differencing the dependent variable (Aassve et al. 2007; Lechner 2014). Just as in fixed effects regression, this differencing eliminates time-invariant sources of bias (Smith and Todd 2005).

We are interested in the unemployment effect on political interest for people losing their jobs, which is captured by the average treatment effect on the treated (ATT; Aassve et al. 2007):

$$\begin{aligned} \text{ATT} &= E(Y_{1i} - Y_{0i} | D_i = 1) \\ &= E(Y_{1i} | D_i = 1) - E(Y_{0i} | D_i = 1). \end{aligned} \quad (1)$$

Here $E(Y_{0i} | D_i = 1)$ can be identified only by imposing assumptions on the selection process. Most important, we have to assume conditional mean independence under the condition that the treatment group and the control group are balanced on a propensity score and/or further covariates:

$$\begin{aligned} \text{ATT} &= E_{p(X_i)} [E\{Y_{1i} | D_i = 1, p(X_i)\} - \\ &E\{Y_{0i} | D_i = 0, p(X_i)\}]. \end{aligned} \quad (2)$$

There are several PSM methods (Caliendo and Kopeinig 2008). We use radius matching with bias adjustment, a new semi-parametric matching method that is robust and reli-

3. Autoregression and Nickell bias make two-wave fixed effects models unattractive. And indeed, we do not detect unemployment effects in fixed effects regressions.

able in small as well as big samples (Huber et al. 2013, 2015). The estimator uses the propensity score and the Mahalanobis metric for distance-weighted radius matching. In a second step, the weights from the matching step are used in a weighted linear regression to remove biases due to bad matches (see Huber et al. [2015] for details).

Our dependent variable, political interest, is measured on an ordinal four-point scale (seven-point scale in the differences-in-differences PSM), which we treat as continuous in the analysis below.⁴ The scale favors temporal stability and induces ceiling and floor effects (because many respondents are inevitably located at the poles of the distribution). We therefore balance treatment and control groups on the initial level (t_0) of political interest.

We capture our treatment (unemployment) with a dummy variable, derived from respondents' labor force status. The underlying variable captures the job situation at the time of the survey in 11 categories distinguishing unemployment from other nonworking states (e.g., education or maternity leave; see table A1 in the appendix; tables A1–A8 are available online).⁵ In our design, all respondents in t_0 are employed. The unemployment dummy becomes one if a respondent experiences unemployment after t_0 . It is important to keep in mind that the dummy remains one over the following time periods if the respondent was unemployed at least once. If we investigate, for example, changes in political interest from t_0 to t_2 , the treatment is one if a respondent was unemployed in t_1 and/or t_2 . This strategy increases the number of treatment events. But it also restricts us in that we cannot increase the time gap between the pre- and post-treatment points too much, because the effect of a single unemployment spell in t_1 might have vanished in t_5 . As result of this strategy, the share of treated respondents increases over the time points from 4% in t_1 to 8% in t_2 , to 12% in t_3 , and 14% in t_4 .

We balance treatment and control groups on a number of theoretically relevant covariates that are likely to be related to the treatment as well as the outcome. An important restriction is that the covariates need to be surveyed over all waves, but the SOEP provides a relatively complete coverage of important variables. First, as mentioned already, we balance on respondents' level of political interest in t_0 . Second, we balance on respondents' level of net household

income in t_0 (Rosenstone 1982). Third, we use the International Standard Industrial Classification of all economic activities (one-digit level) in t_0 to account for different baseline probabilities of treatment in the different industries (Schur 2003). Moreover, we balance on a number of time-invariant variables to account for the possibility that the individual rate of change in political interest depends on such factors. We use the standard variables gender, age, and education level (Highton and Wolfinger 2001; Prior 2010; Scholzman et al. 1999). We also account for migrant background (Togeby 1999), wave-fixed effects (wave dummies), and a dummy for residence in the former German Democratic Republic (Prior 2010). Table A1 describes the coding of all the variables.

Our socialization argument (hypothesis 2) implies that unemployment should be especially harmful to the political interest of young labor market entrants. We use different age cut-off points as indicators for early adulthood. In addition, the SOEP provides a variable indicating whether a respondent's new job is the first job in her or his employment career. Out of the 45,800 respondents in t_0 , 5,212 are such labor market entrants. This information is provided only for respondents starting a new job and not for those who are employed already in their first wave. As a result, we might wrongly classify some entrants as nonentrants. If anything, this should lead us to underestimate the effect of being a labor market entrant. The large majority of labor market entrants are very young: 66% of all labor market entrants are 20 years or younger, and 97% are 30 years or younger.

We use four strategies to assess the robustness of immediate unemployment effects. First, we vary time gaps between post- and pre-treatment period to ensure that our results do not depend on choosing a specific period. Second, we vary the specification of the matching model to show that the results are not contingent on less relevant covariates. Third, we show that alternative matching methods produce similar results. Fourth, results could be biased if panel attrition is related to our variables of interest (see appendix III for a discussion). We therefore estimate our models with longitudinal weights that correct for a possible influence of political interest or unemployment on wave-specific dropout probabilities. Results on the last three points are provided in the appendix.

In the second part of our empirical analysis, we investigate the scarring effect of early unemployment experience on the level of political interest using PSM. We use the panel structure of the SOEP to measure whether respondents had unemployment experiences during early adulthood, defined as unemployment experience when aged less

4. The scarring effects analysis is robust using ordered probit (see table A8 in the appendix).

5. Note that respondents who are unemployed between surveys but employed at the time of the survey are not counted as unemployed. Moreover, we disregard unemployment duration. Both decisions bias against finding an effect of unemployment on political interest.

than 30 years.⁶ We then estimate the effect of early unemployment on the level of political interest in our last wave (2013) for respondents aged between 40 and 65. Using this restricted cross section, we can compare the political interest of respondents with and without unemployment experiences during early adulthood. The age restriction ensures that there is a sufficiently long time gap between early unemployment experience and current levels of political interest to capture scarring effects. Of the 6,805 observations in the restricted 2013 cross section, 442 experienced unemployment below the age of 30.⁷ If our scarring effect argument holds, this dummy variable should have a significant negative effect on political interest.

To account for potential confounders, we match on unemployment experiences made after the age of 30. Additional matching variables (see table A4) include years of education, gender, social class (Erikson-Goldthorpe), household income, dummy variables for East German respondents, and migration background, church attendance, marital status, and the presence of children in the household as prominent predictors of political interest (Highton and Wolfinger 2001; Prior 2010; Rosenstone 1982; Schlozman et al. 1999). Table A1 describes the coding of all the variables.

Finally, we extend the scarring effects model to turnout. The dependent variable is a dummy equaling one if respondents state that they participated in the 2009 general election. We use the same scarring effects model but allow for a logit bias correction to account for the dichotomous dependent variable.

EMPIRICAL RESULTS

To align our expectations about the relationship between political interest and age, we first investigate average levels of interest over age. Figure 2 displays the growth pattern that also formed the basis for figure 1. Political interest increases rapidly until the early thirties, after which this growth stagnates. On average, growth between the ages of 18 until 33 is twice as big as growth between ages 34 and 60. Figure 2 indicates that the initial level of political interest is rather low among young respondents. Indeed, 79% of respondents below age 26 say their political interest is “not that strong” or “not strong at all.”

We begin with an analysis of the immediate effects of unemployment using the difference-in-difference radius match-

6. We coded early unemployment experience zero when no information was available for a respondent, which makes our analysis a conservative test of scarring effects.

7. We choose a high age threshold to maximize observations. As unemployment effects decline with age (see below), a high threshold works against the scarring hypothesis.

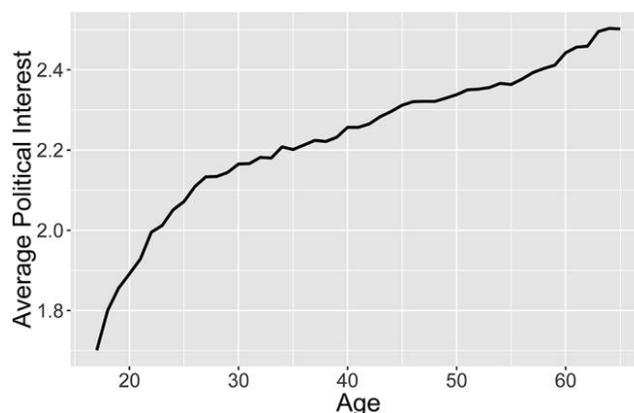


Figure 2. Average levels of political interest over age. The figure plots the average levels of political interest over age in the pooled sample. Source: German Socio-Economic Panel (SOEP.v30), own calculations.

ing with bias adjustment explained in the previous section.⁸ A crucial question when using the Huber et al. (2013, 2015) estimator concerns the covariates to use for Mahalanobis matching besides the propensity score. We follow Huber et al. (2015) in re-entering the most important covariates from the propensity score estimation into the Mahalanobis matching. Therefore, the results of table 1 are balanced on the propensity score as well as on the Mahalanobis distance derived from the initial level of political interest, the initial level of household income, and the level of education.⁹ Results for the propensity score estimation (table A2) as well as common support and bias reduction plots (figs. A3, A4) are provided in the appendix. We do not have any common support problems. Also, the bias reduction plots show that balancing works well.¹⁰

Table 1 presents estimates of the ATT for different subsamples of our data. We present results for t_2 , which capture the effects of unemployment on changes in political interest for respondents who experienced unemployment in t_1 and/or t_2 . We prefer this rather short time gap, as panel attrition and new information become a bigger threat for our results at later time points. We cannot provide a sensible estimation for t_1 because the number of treatment observations is too small at this early stage (especially if we break up the sample into different age groups).

8. Figure A2 shows that the yearly increases in political interest for treatment and control groups support the parallel trend assumption.

9. The Mahalanobis matching further balances on three crucial covariates, which have been included in the estimation of the propensity score.

10. Caliendo and Kopeinig (2008, 48) regard a bias of up to 5% as acceptable, and our matching procedure performs well in this regard (figs. A4, A6). Our bias reduction mechanism (Huber et al. 2013, 2015) further reduces sensitivity to bad matches.

Table 1. The Effect of Unemployment on Changes in Political Interest (t_2)

	All Respondents		Age ≤ 35		Age ≤ 30		Age ≤ 25		Entrants	
	ATT	SE	ATT	SE	ATT	SE	ATT	SE	ATT	SE
Unemployment	-.011	.026	-.095***	.034	-.122***	.037	-.166***	.048	-.166***	.054
Observations	16,720		7,937		6,196		4,045		2,646	

Note. ATT = average treatment effect on the treated. SE = standard error.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

Table 1 shows that there is no significant effect of unemployment on political interest for the full sample. This finding is in line with previous research that highlights the stability of political interest (Prior 2010). The picture changes if we run our matching estimator on subsamples of different age groups. Table 1 shows that the coefficient of the ATT increases as we restrict the sample to younger respondents. Particularly among respondents aged 25 or younger, we observe a more substantial and highly significant effect of unemployment on political interest. If we restrict the sample to labor market entrants, we observe a similar effect. These findings support our socialization hypothesis, which expects unemployment to be particularly harmful for young labor market participants' propensity to be interested in politics.

The effect sizes might appear modest at first sight. Due to the nonparametric nature of the PSM approach, coefficients can be interpreted as mean differences in political interest change between continuously employed entrants versus entrants who experience unemployment. On average, change in political interest among labor market en-

trants is depressed by 0.17. It is important to recall that the original political interest scale ranges only from one to four, so that every one unit change indicates a substantive qualitative shift. As shown in figure 2, the average growth in political interest over the life cycle amounts to roughly 0.8, and the growth in the crucial years between 17 and 30 years is less than 0.5. Compared to these benchmarks, a negative effect of 0.17 indicates a nontrivial disturbance in the development of political interest.

The results in table 1 are based on analytical standard errors, which do not account for the fact that the estimator is effectively a two-stage estimation procedure. To account for the estimation of the propensity score, table 2 presents a replication of table 1 based on bootstrapped standard errors and p -values using 100 replications. Table 2 confirms that coefficient sizes and significance level remain robust under this more demanding procedure.

In a second robustness test, we varied the propensity score model. Results should not change under minor changes to the propensity score model. We receive very similar results if we replace the one-digit industry code by the one-digit

Table 2. The Effect of Unemployment on Changes in Political Interest (t_2), Using Bootstrap

	All Respondents		Age ≤ 35		Age ≤ 30		Age ≤ 25		Entrants	
	ATT	SE	ATT	SE	ATT	SE	ATT	SE	ATT	SE
Unemployment	-.019	.024	-.106***	.030	-.128***	.031	-.173***	.040	-.158***	.049
Observations	16,720		7,937		6,196		4,045		2,646	

Note. Bootstrap using 100 replications, p -values calculated by bootstrapping the t -statistic. ATT = average treatment effect on the treated. SE = standard error.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

ISCO classification (table A3). In addition, we have also replicated our analysis using five nearest neighbors matching (table A7).

An important robustness test is the replication with longitudinal weights that account for nonrandom attrition. Our results could, for instance, be biased if the unemployed have a higher dropout probability. As explained in appendix III, we address this problem by weighting cases based on their inverse dropout probability (Owens and Pedulla 2014). Tables A5 and A6 show that our results are substantively similar with longitudinal weights, which increases our confidence that panel attrition is not a major concern.

Finally, we investigate ATTs for later time points. As explained above, increasing the time gap between pre- and

post-treatment periods increases panel attrition and leads to a more heterogeneous treatment group. Figure 3 therefore presents ATTs for the different age groups and for the periods t_2-t_5 .

Figure 3 shows that the empirical pattern presented in tables 1 and 2 holds for different time periods. The results for t_3 deviate the most, as we do not get clear-cut results for the age groups ≤ 30 , but they do confirm the strong negative effect among labor market entrants. Figure 3 also relates to our second analytical interest, namely, whether unemployment creates scarring effects among young labor market participants. As expected, political interest among the treated remains consistently below the level of the control group, particularly for labor market entrants, even up to

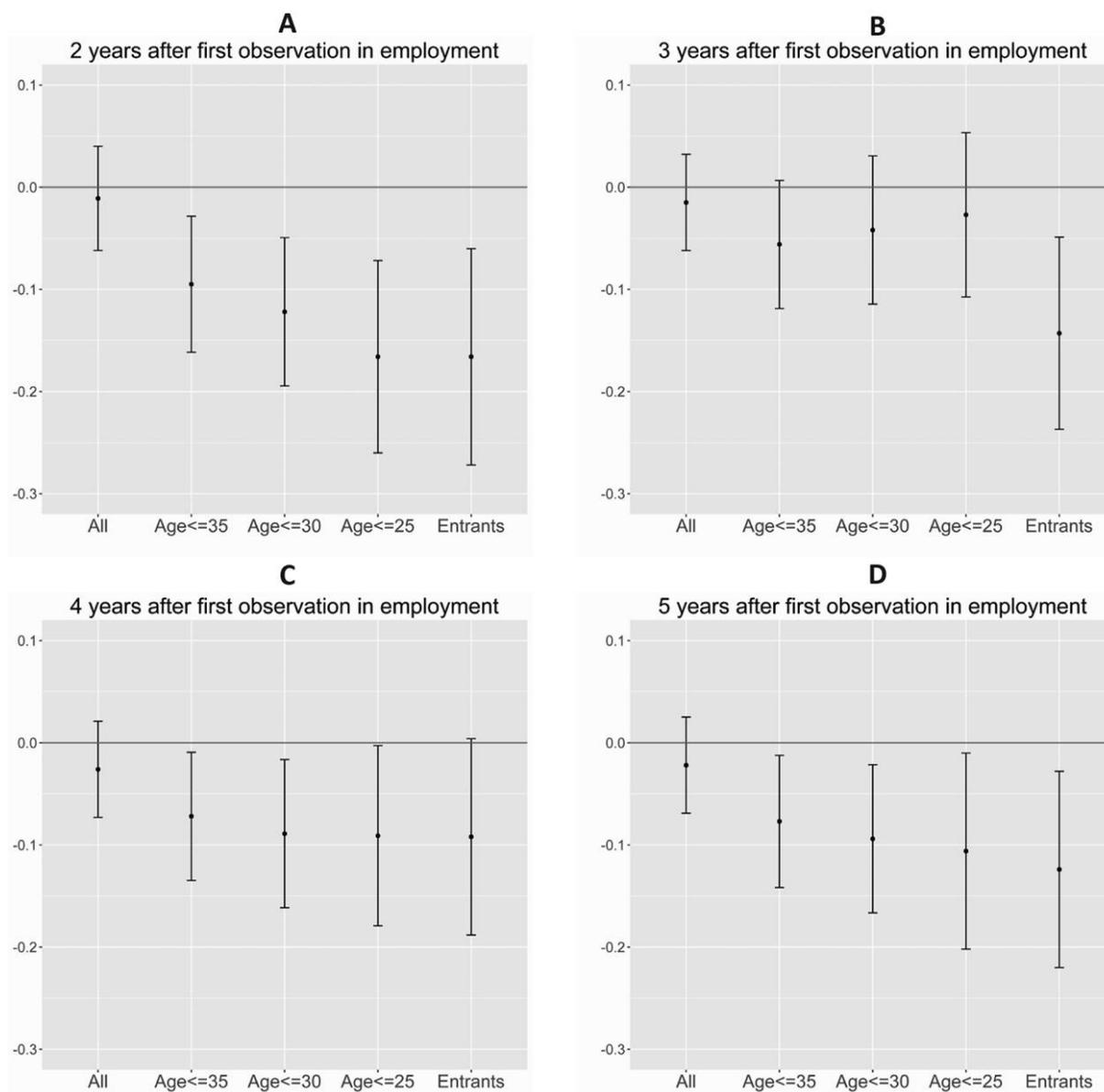


Figure 3. Effect of unemployment on political interest over different age groups and time gaps

four years after the unemployment experience. Put differently, labor market entrants exposed to unemployment spells in the early stages of their working lives struggle to build up comparable levels of political interest as respondents with no such unemployment experiences.

In the next step, we present a more direct test of the scarring hypothesis by comparing levels of political interest among workers above age 40 with and without experience of unemployment in early adulthood. The matching model includes a comprehensive set of covariates for the estimation of the propensity score as described in the methods section. For the Mahalanobis matching, we include three additional covariates: education, a dummy variable for unemployment experiences after age 30, and a dummy for East German respondents. Results for the propensity score estimation (table A4) as well as common support and bias-reduction plots (figs. A5–A8) are again provided in the appendix.

Table 3 and figure 4 present the estimated ATT for “early unemployment” (experienced below the age of 30). Importantly, the outcome for the estimation of the scarring effect is no longer differenced. The results therefore can no longer be interpreted as differences in temporal changes but instead as the absolute average difference between treatment and control group. The estimated ATT shows that there is a significant difference in levels of political interest between respondents who experienced unemployment during early adulthood and respondents who did not. Hence, early unemployment experiences scar people for life.

In the group with early unemployment experience, political interest is on average roughly 0.10 points lower (on a scale from 1 to 4). This effect size might appear moderate, but it should be kept in mind that political interest is considered to be very stable (Prior 2010), and we control

Table 3. Scarring Effect of Early Unemployment on Political Interest, Cross Section, 2013

	Standard		Bootstrap	
	ATT	SE	ATT	SE
Early unemployment	-.098**	.042	-.102**	.043
Observations	6,805		6,805	

Note. Bootstrap using 100 replications, *p*-values calculated by bootstrapping the *t*-statistic. ATT = average treatment effect on the treated. SE = standard error.

* *p* < .10.

** *p* < .05.

*** *p* < .01.

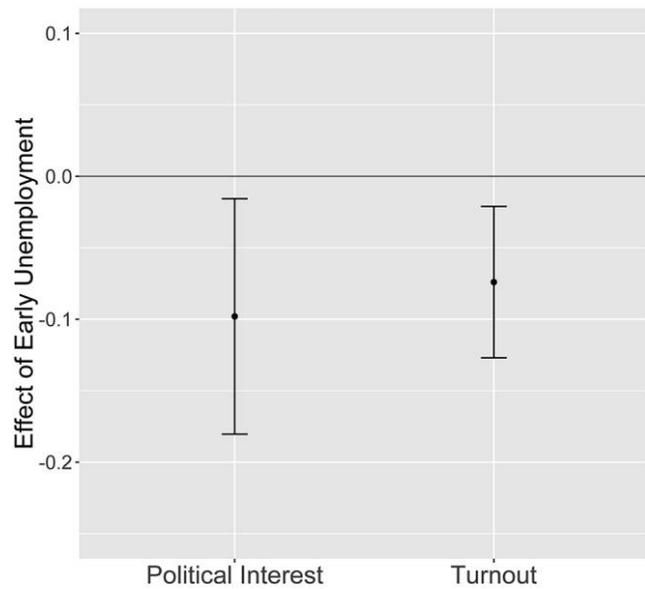


Figure 4. Scarring effects of unemployment under the age of 30 on political interest and turnout when aged 40 or older.

for a host of factors that arguably mediate the effect of early unemployment (most notably income and later unemployment experience). While this is important to isolate the causal effect of early unemployment, it might mask the “true” extent to which scarring effects are a social and political problem.

Finally, we analyze the scarring effects of early unemployment on political participation. The SOEP includes a retrospective measure of electoral participation in years after general elections. We use the most recent available question from 2010, which refers to the 2009 elections. Recall questions are of course susceptible to overreporting because of social desirability. Indeed, while in our sample 79.8% report participation, official turnout was only 70.8%. However, this is a universal problem in post-election surveys, and the gap in the SOEP is even comparatively modest (Selb and Munzert 2013). Theoretically, we expect that, on average, respondents suffering from early unemployment experiences have a lower propensity to participate, which table 4 and figure 4 confirm. After more than 10 years, unemployment in early adulthood is still associated with a 7% lower probability to turn out.

CONCLUSION

In this paper, we have taken up a classical but recently neglected topic in political sociology: the political involvement of the unemployed. Based on insights from socialization studies, we have challenged the dominant resource perspective—or, more precisely, the implicitly assumed reversibility of employment-related resources. Although we agree with Rosenstone (1982) that unemployment usually

Table 4. Scarring Effect of Early Unemployment on Turnout (Retrospective), Cross Section, 2009

	Standard		Bootstrap	
	ATT	SE	ATT	SE
Early unemployment	-.074***	.027	-.055**	.029
Observations	5,572		5,572	

Note. Bootstrap using 100 replications, p -values calculated by bootstrapping the t -statistic. Linear and logit bias correction. ATT = average treatment effect on the treated. SE = standard error.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

means economic hardship and stress, we argue that its potential to disrupt political involvement dramatically decreases once such involvement has become habitual. We also agree with Brady et al. (1995) that employment provides important skills and networks fostering political involvement but also that these employment-related resources become resilient in a process of socialization. Once acquired, the civic skills resulting from employment will not just vanish if employment ends. We have therefore proposed that the negative effect of unemployment on political involvement is relevant only among labor market participants in early adulthood. We have also argued that the effects of unemployment shocks should be studied with longitudinal designs to better control for selection effects and to model the ratchet effect of political involvement.

Empirically, we have examined changes in political interest following experiences of unemployment in a nationally representative sample of the German population. Our results strongly confirm the argument that unemployment effects are conditioned by socialization stage. From middle age onward, unemployment no longer depresses political interest. We therefore discard the simple civic skills hypothesis and interpret these findings as strong support for our socialization hypothesis. However, unemployment has the potential to impede the building up of interest among young workers. From a normative perspective, a disconcerting finding is that early unemployment produces scarring effects on political involvement. Given the scope of youth unemployment in many democracies, the possibility of persistently lowering political interest certainly is alarming and warrants more research.

Our study has important policy implications. German Chancellor Angela Merkel has called youth unemployment “the most pressing problem facing Europe at the present

time.”¹¹ Although she was mostly referring to the well-documented material and psychological repercussions of youth unemployment, such experiences, as we have shown, can also lower political involvement. This means that those who need public policies most are the least likely to make their voices heard. This finding reinforces arguments that fighting youth unemployment should be a top priority for policy makers—even in times of tight budgets.

Comparative research suggests that generous labor market policies can boost the health (Paul and Moser 2009), social integration (Anderson 2009), and political efficacy (Marx and Nguyen 2016) of the unemployed. In our view, the most promising approach is expanding active labor market policies and training programs in particular. These policies not only have positive long-term effects on socioeconomic outcomes (Card, Kluve, and Weber 2010); they potentially also help to counter some of the typical psychological repercussions of unemployment, such as social withdrawal, depressed self-efficacy, and loss of time structure (Brand 2015; Jahoda et al. 2002). Recent research shows indeed that the positive effects of labor market programs extend to noneconomic outcomes, including subjective well-being (Andersen 2008), and, if properly designed, social integration (Gundert and Hohendanner 2015). Hence, well-designed training programs can serve as alternative sources of social contacts and civic skills with potentially beneficial effects on political involvement.

There is one important limitation for the generalizability of our research, namely, the macroeconomic context. With its developed systems of vocational training, Germany is among the best performing OECD countries with regard to youth unemployment. It may be that the effects of youth unemployment are different in countries where labor market conditions are significantly worse or in periods of sustained economic crisis. Comparing the effects of youth unemployment across economic conditions will therefore be an important task for future research.

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