sociological science

Life-Course Transitions and Political Orientations

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Abstract: Do life-course transitions in adulthood shape political orientations? One framework suggests that life events expose people to new information, allowing actors to assess their political beliefs and preferences in response to these social experiences. An alternative framework suggests that the link between one's life-course position and personal politics may be ambiguous, and early experiences should be more informative for political orientations. In this article, I use four household surveys across three countries and 40 items on political beliefs and preferences to test whether life-course transitions change one's political orientations. In doing this, I employ difference-in-differences models to identify the effects of six life transitions across family and work domains on a wide variety of propositional survey items. I find that life-course transitions have no substantive influence on political orientations, and the general findings are not sensitive to differences in political interest or the age at which individuals experience these life events.

Keywords: political ideology; life course; difference-in-differences

Reproducibility Package: The code to reproduce the full set of analyses and instructions on how to access the household surveys are provided at https://osf.io/hu3yj/

WHAT role, if any, should life transitions play in explaining political orientations? This question poses several challenges for contemporary sociology. Studies find that personal culture—culture made manifest in people's beliefs, preferences, and self-descriptions (Lizardo 2017)—is relatively durable across adulthood, though the reasons why we observe this durability differ across competing perspectives. One stylized model suggests that early socialization experiences shape how individuals process new information based on their political dispositions, whereas another framework suggests that life-course transitions can reliably change people's political orientations.

In this article, I evaluate these two frameworks by examining whether life-course events in work and family domains have substantive effects on people's political beliefs and preferences. Using four long-running panel surveys across three countries and 40 unique outcomes, I adopt a difference-in-differences (DID) design to identify the effects of life events on people's responses to propositional survey items. I find that life-course transitions have no substantive influence on political orientations.

This finding substantiates the ongoing conversations about whether social positions or political dispositions inform the trajectory of one's political beliefs and preferences. Given the significance of life-course experiences for understanding the trajectory of adult lives across a variety of outcomes (Mayer 2009), the assertion that life events must have an influence on political orientations seems, at first, noncontroversial. Yet, if early life experiences can override or simply shape people's life

Citation: Keskintürk, Turgut 2024. "Life-Course Transitions and Political Orientations" Sociological Science 11: 907-933.

Received: July 3, 2024

Accepted: September 10, 2024

Published: September 27, 2024

Editor(s): Arnout van de Rijt, Jeremy Freese

DOI: 10.15195/v11.a33

trajectories, this would mean that the causal ties from social transitions to political change might not be so clear-cut. Therefore, answering the question of whether life transitions change political orientations has important implications for conceptualizing the dynamics of large-scale cultural change, life-course progression, and social and political psychology.

This article contributes to several subfields in sociology by evaluating the claim that one's social position is informative about their political preferences. I provide evidence for whether life-course events influence people's political orientations on multiple domains and whether we should expect to see changes in political culture after important events, speaking to common perceptions about, for example, people getting more conservative when they get older (Peterson, Smith, and Hibbing 2020). In doing this, these results extend the previous research in cultural sociology and studies on life course (Kiley and Vaisey 2020; Lersch 2023) by explicitly identifying the effects associated with life-course transitions. The general findings suggest that studies need credible counterfactuals and reliable longitudinal designs to understand political belief formation over the life course.

Theoretical Framework

Sociologists invoke two stylized models to understand change and stability in cultural orientations,[1] with competing empirical implications for the evolution of personal culture across the life course: *life-course adaption model* (LCAM) and *settled dispositions model* (SDM).[2] I claim that, once we consider the political field and political psychology at large, these models imply radically different conceptions about how life experiences are related to politics. Although the LCAM proposes that life events inform and subsequently shape political beliefs and preferences, the SDM proposes that political orientations are strongly mediated by early life experiences. These differences lead to competing empirical expectations on the trajectory of one's political orientations.

Competing Generative Models for Political Orientations

Both the LCAM and the SDM start with a basic empirical expectation: adults rarely make substantial cultural changes. Changes in one's cultural orientations are primarily concentrated in early periods of life, whereas adulthood tends to be more stable—an idea often referred to as the *impressionable years hypothesis* (Alwin and Krosnick 1991; Krosnick and Alwin 1989). Studies have provided substantive support to this view (Kiley and Vaisey 2020), the general conclusions being that people do change, though these changes are rather small when accounting for broad cultural differences (Lersch 2023).

The disagreements arise, however, regarding the *sources* of this cultural stability. The SDM argues that people go through "sensitive windows" early in life—a period involving long-lasting formative experiences. During this period, people are exposed to varying political communication in family (Jennings and Niemi 1968) and among friends (Settle, Bond, and Levitt 2011), with an enduring imprint on their subsequent beliefs and preferences. Once this window closes, one's susceptibility

to change drops substantially, leading to baseline dispositions that inform one's subsequent political orientations. Hence, a process of political socialization where people acquire their basic dispositions and navigate who their "political ingroup" is functions as the primary engine of political learning.

In contrast, the LCAM argues that stability in adulthood is "sociogenic" in character (Dannefer 1984), relying on a person's social location and distributed networks. In this view, people are situated in specific social positions with influential personal ties (Rawlings and Friedkin 2017; Visser and Mirabile 2004), information environments (DellaPosta, Shi, and Macy 2015), and institutionally defined social locations in the broader social structures (Longest, Hitlin, and Vaisey 2013). Hence, political orientations are broadly reflective of one's position in society (Lersch 2023). If this social position is somehow disrupted, the implication is that people's information environments change and individual beliefs fluctuate, leading to changes in political orientations.

The central disagreement between the SDM and the LCAM is that the former favors an *ontogenetic* paradigm that emphasizes political development in early life and the latter proposes a *sociogenic* paradigm that views political culture as a reflection of social position and long-term life-course experiences (Dannefer 1984). This effectively means that both theories characterize adulthood as a form of "political equilibrium." Individual baselines provide adequate bases for understanding political differences among people, either because we often converge on certain political positions right before adulthood (SDM) or because scientific studies miss important changes in life (LCAM).

Social Experiences and Belief Change

These two frameworks were developed for understanding the over-time trajectories of personal culture (Lizardo 2017), but their respective theoretical assumptions on political belief formation are undertheorized. I claim that the divide boils down to *the role of adult experiences in explaining personal politics*. Although the LCAM underscores a person's "novel interactions, experiences, considerations, and different sets of resources and constraints" (Lersch 2023:225) in informing their political outlook—effectively tying experiential information associated with one's social position to their political orientations—the SDM emphasizes that the tie from life experiences to beliefs and preferences must be mediated by underlying ideological dispositions formed early in life. This means that, for SDM, life experiences in adulthood can be *mostly irrelevant* for belief and preference formation.

Accordingly, research on whether life events in adulthood shape political orientations has produced a variety of findings, asking whether economic shocks across one's life course can change people's voting choices (Margalit 2019) and political preferences (Gilderbloom and Markham 1995; Owens and Pedulla 2014), or whether one's political identification is susceptible to having a child (Lee and Conley 2016). The central intuition among these studies is the expectation that an individual's life experiences should provide phenomenologically valid information about politics. For example, marriage exposes us to new information about gendered division of labor in the household, whereas an experience of unemployment can inform us

about government welfare policies. If, the argument goes, we observe changes in social position, the chances of having informative experiences go up. Because individuals go through a sequence of institutionally defined and relationally organized stages across life (Mayer 2009), this implies that life processes are informative about political orientations:

Premise 1: *The progression of the life course in general and people's specific social positions in particular are the determining factors that explain political differences among a population.*

This premise offers a specific expectation: we should expect to see changes in political orientations as a function of life events, given that one's social position at $t = 1$ differs from the one at $t = 0$.

In contrast, change marks a distinctive cognitive process for the SDM, which proposes that most change we observe is highly local around one's baseline—thus, a mix of quickly waning fluctuations, ambivalences resulting from the survey context, or measurement error in attitude reporting. When belief change happens, the reason is often not one's personal experiences, but changes in large-scale alliance structures and disruptions in the larger political field (Slothuus and Bisgaard 2021). This is because a person's political orientations are less dependent on *epistemic beliefs*, that is, beliefs aiming to represent the world in an "accurate" fashion, and more on *symbolic beliefs*, that is, beliefs that orient people to certain groups and worldviews in affective ways (Westra 2023).[3]

These trade-offs between symbolic beliefs and the epistemic function of personal life experiences are thus significant in understanding the trajectory of political orientations across the life course. Starting with early adulthood, people begin roughly situating themselves in an *alliance structure* (Pinsof, Sears, and Haselton 2023), select into information consumption according to their political convictions (Williams 2023), and develop a basic *political ontology* about the political field (Martin 2015; Martin and Desmond 2010), all of which help mediating personal experiences to politics. Hence, we should observe no systematic change during adulthood as a function of one's life-course experiences. Of course, salient events might be instrumental in facilitating belief change (Kiley and Vaisey 2020), but this salience requires political ideology to mediate[4] these experiences:

Premise 2: *Political orientations organize around hard-to-change stable baselines, and the change in one's social environment is primarily a process of cultural selection rather than treatment.*

Empirical Expectations on Political Orientations

These models propose two competing expectations, though both frameworks have to tackle the same *descriptive* fact: substantial individual change, once looked at the aggregate, is rather rare and small in magnitude. Thus, it is impossible to adjudicate these two perspectives by looking at the overall level of change in the population, which ultimately leads to arguments about change being *low* (Kiley

and Vaisey 2020) or simply *exists* (Lersch 2023). The former attributes the low amount of change in political orientations to the validity of the SDM, although the same set of expectations can easily be predicted by the LCAM as well. Similarly, claiming that there is some change does not explicitly specify why a statistically significant change in beliefs would be a confirmation of life-course processes.[5] Hence, empirical strategies that use the existence or non-existence of change are underdetermined to provide an insight into the underlying political processes.

Let me call one's political orientation P, with $P_{i,t}$ capturing person i's particular political position at time t. We expect that political positions are observed with varying and potentially changing realizations across different time periods. Let me also define, for the sake of simplicity, one life event L, serving as a treatment assignment for each individual.

Premise 1 suggests that life transitions should persistently change one's value of P, whereas Premise 2 suggests that life events should have no effect on P. This leads to the following expectation:

Expectation: *If the LCAM is correct, political orientations would change with life-course transitions, whereas if the SDM is correct, life events should have no persistent effect on political orientations.*

If we follow the notation above, we can identify the theoretical estimand (Lundberg, Johnson, and Stewart 2021) for this expectation as the difference in one's orientations associated with one life transition across the life course, with the counterfactual that they did not go through that transition:

$$\tau_{ATT} = \mathbb{E}(P_i^1 - P_i^0 \mid L = 1), \tag{1}$$

where $L = 1$ indicates the existence and $L = 0$ indicates the non-existence of this transition, and the difference captures the theoretical difference associated with the life event.

The Pathways from Adult Trajectories to Political Change

We can posit two general processes tying adult experiences to political outcomes, focusing on what Elder (1994) calls "work careers" and "family pathways" (5). The former refers to one's experiences in employment, including positional transitions from school to labor market and disruptive experiences such as unemployment, whereas the latter refers to one's age-graded trajectories on marriage, parenthood, and marriage dissolution. In all these transitions, the central mechanisms proposed by LCAM and SDM differ: LCAM argues that, once people go through these events, their social positions change among their social networks (Marsden 2018), leading to greater susceptibility to exploration, whereas SDM argues that most of these events are essentially selection. Hence, I argue that, even though the specific theoretical expectations associated with each broad transition—work careers and family events—might differ, the overall trends should be fairly comparable.

Yet, it is possible to observe heterogeneity for at least two reasons, each associated with one position. LCAM argues that, because life-events are dependent on

"social timing," that is, "the incidence, duration, and sequence of roles, and [. . .] relevant expectations and beliefs based on age" (Elder 1994:6), people's transition experiences might imply different social experiences depending on the *timing* of these transitions. For instance, if someone gets married quite late compared to societal expectations, that marriage experience might signal different social processes than a "modal" process. Therefore, if LCAM is correct, I expect that the effects associated with life-course transitions should be sensitive, that is, heterogeneous, to age at which people go through these transitions.

Similarly, the mechanisms proposed by SDM have theoretical components suggesting heterogeneity, particularly based on one's *political awareness*, that is, "the extent to which an individual pays attention to politics and understands what he or she has encountered" (Zaller 1992:21). If individuals show changes associated with life-course transitions, these effects should be most pronounced among people with low political awareness, given that the mediating role of politics should be the highest among high political awareness people. Hence, if the SDM is correct, I expect that the dispositional differences in political awareness should be effective in the trajectory of political orientations, and there are heterogeneities in individual responses to life transitions based on awareness.

Analytic Strategy

Data

To evaluate the central expectation, I use data from four long-running panel surveys across three countries: Germany, Switzerland, and the United Kingdom—each representative of their respective populations. These sources cover extended time periods ranging from 11 to 36 years, provide repeated observations on thousands of individuals and multiple measures, and contain detailed information about people's life-course processes. While preparing the data sources for analyses, I restricted the samples such that people who were already "treated" in respective life-course transitions during the observation window were dropped from treatment groups, and those outside the age range of 16 and 79 were excluded to mitigate potential concerns about differential mortality. In the end, the data provide, on average, seven observations for each individual—with median 6—ranging from 3 in some cases to 36 in others. Table 1 documents a basic description of these data sources.[6]

One challenge in conducting a cross-national analysis is to make sure that the measures at hand refer to the same constructs, particularly on issues such as employment status and educational attainment. Fortunately, the panels I use are part of the Comparative Panel File (CPF), an open-science initiative to harmonize cross-national life-course data using long-running household surveys (Turek, Kalmijn, and Leopold 2021). I used the most recent version of the CPF at the time of analysis (CPF version 1.5) to compile relevant information on life-course processes and several pretreatment covariates—sex, age, year of birth (coded as birth cohorts), education, and migration status—from the raw files of each of these four panel surveys. This ensured comparable classification schemes in all analyses.

Table 1: Description of the data sources

Country	Survey	Period	Items
Germany	German Socio-Economic Panel (SOEP)	1984–2020	10
Switzerland	Swiss Household Panel (SHP)	1999–2021	25
United Kingdom	Understanding Society/ UK Household Longitudinal Study (UKHLS)	2009–2020	5
United Kingdom	British Household Panel Survey (BHPS)	1991–2008	14

Measuring Political Orientations

I follow previous studies in operationalizing political outcomes by using a comprehensive set of survey items that capture one's beliefs and preferences on political issues. While selecting these items, I excluded (1) items that could be verified using external indicators, for example, personal practices or factual information (Alwin 2007), (2) items that were not asked in at least two survey waves, and (3) items with strong external referents—particularly, party sympathy and voting decisions.[7] This procedure resulted in 54 unique survey items. To facilitate tractability and ease of interpretation, I extracted two principal components from an 11-item set of spending attitudes in the SHP and one principal component from a five-item set of generalized trust in the SOEP, reducing the number of outcomes for analyses to 40.[8] These 40 items capture a variety of issues, ranging from redistributive spending and immigration attitudes to political identification, trust, and attitudes on breadwinners. I normalized all items such that they go from 0 to 1, where higher values refer to conservative positions if the items have such valence and higher "levels" for some others, for example, generalized trust. The online supplement material A provides details about all items used in the analyses.

Measuring Life-Course Transitions

I analyze six life-course transitions, three from the family domain and three from the work domain:

1. *Marriage*: the transition from a single or never-married status to a married status;
2. *First parenthood*: the transition to parenthood, that is, a person having a child for the first time in their life, whether through natural birth or adoption;
3. *Marriage dissolution*: the transition from a married status to a non-married status, particularly through divorce, separation from the spouse, or the death of one's spouse;
4. *Entry to labor market*: the entrance to labor market, that is, the first time a person who is inactive, at home, or in education starts holding a full- or part-time job;

5. *Unemployment*: the transition from a full- or part-time job to unemployment; and

6. *Retirement*: the transition from working status to retirement status.

I coded these life events such that once a participant gets *treated*—for example, a participant loses their job for a specific period of time—they get one in the subsequent periods as well. This achieves two things. First, it allows me to assume that the event is irreversible, which is necessary for effect identification. Substantively, though, this ensures that events are influential over subsequent times—commonly described as the "scarring" effect of life events, particularly in the context of unemployment (Clark, Georgellis, and Sanfey 2001).[9] In this sense, all life transitions are organized around a *pretreatment period* and a *treatment period* for each individual. The online supplement material A provides information about the coding of these life events as well as their comparability across surveys.

Identification Strategy

Because the main objective of this study is to adjudicate whether life-course transitions reliably change personal politics, I need to account for two basic issues. First, people often get exposed to important political events—changes in alliance structures, elections, or political communication—that severely confound any within-person observation across time. These might involve simple period shocks that can be controlled out using a variety of fixed-effect estimators, though it is highly plausible that these shocks can induce time-varying processes. Second, these time-varying processes might depend on various pretreatment characteristics, for example, a person's sex or age, which simply means that life events for particular groups might imply radically different social experiences. Therefore, I need *credible counterfactuals* to tease out the effects associated with life transitions.

In this study, I follow a DID design, with life transitions operationalized as "treatment" assignments. In its canonical form, a two-way fixed effects (TWFE) model involves two outcomes: $Y_{it}(0)$, person i's untreated potential outcome at time t, and $Y_{it}(1)$, person i's treated potential outcome at time t. If we observe two groups across two periods, where groups are defined according to their treatment status D, the main parameter of interest—average treatment effect on the treated (ATT)—in a two-wave design can be identified with

$$\text{ATT} = \mathbb{E}[Y_t - Y_{t-1}|D = 1] - \mathbb{E}[Y_t - Y_{t-1}|D = 0], \tag{2}$$

where ATT denotes the changes in outcomes among the treated adjusted by the changes in outcomes among the untreated. Note how this setup resolves time-varying period effects by employing a comparison of within-group trends with the trends of a counterfactual group.

There are two important problems preventing the use of TWFE in this case. First, studies showed that TWFE is severely biased in the existence of staggered treatment timing, that is, the case where different groups enter the treatment in different periods (Baker, Larcker, and Wang 2022). Second, life transitions are not random, which is why there is high probability that parallel trends assumption—the

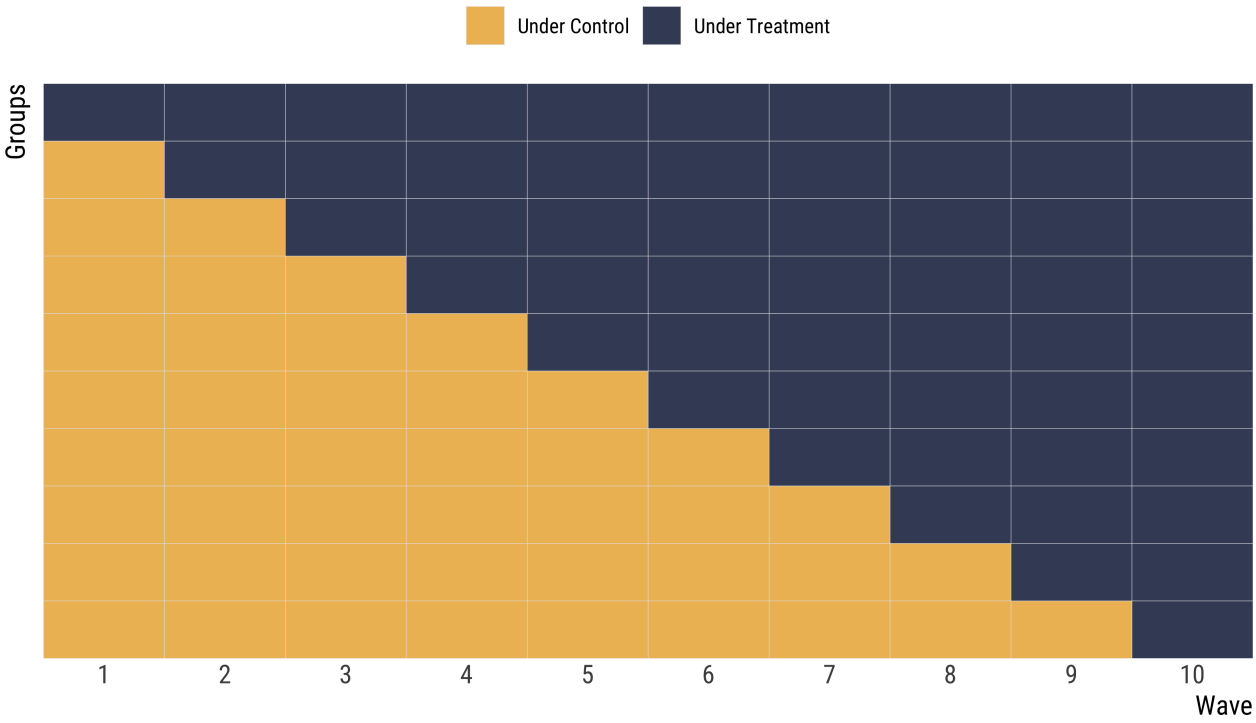

Figure 1: A hypothetical panel study with staggered treatment.

assumption that the trajectory of outcomes for the treated groups would have been the same with the untreated groups if they had not participated in the treatment—will be violated. This is why, as mentioned above, the use of counterfactual groups with wildly different pretreatment characteristics might result in a situation where groups do not have credible counterfactuals.

To see why thinking clearly about "effects" in this case requires strong care about the definition of what a "counterfactual" is, see Figure 1, which presents a hypothetical dataframe with a 10-wave study window. Suppose that we have 10 different groups, represented as rows, which select into treatment in different waves. Who would serve as a counterfactual for group *G*? In this hypothetical example, all blue tiles—those who get "treated"—will have all yellow tiles—those who are "under control"—as their comparison units. Yet, note that the composition of the control units changes over time, and the extent to which different groups get exposed to treatment differs across groups as well. Considering that the composition of the tiles is unlikely to be random, it is very important to think about who serves as under treatment and who serves as under control.

I employ a new DID estimator proposed by Callaway and Sant'Anna (2021) to circumvent these problems. This estimator modifies the canonical TWFE design by defining a new average parameter of interest, *group-time average treatment effect*, or $ATT_{g,t}$. This parameter estimates ATTs across *G*, defined as the time period when a unit *i* first becomes treated (hence indexing the group's "cohort"), and *t*, the specific time period under consideration. Using these basic constructs, it is possible

to identify group-time treatment effects associated with a particular g and t:

$$\text{ATT}_{g,t} = \mathbb{E}[Y_t(g) - Y_t(0)|G = g], \tag{3}$$

where $Y_t(g)$ is the expected outcome for group $G = g$ at time t and $Y_t(0)$ is the counterfactual untreated potential outcome at time t (Callaway and Sant'Anna 2021:203–4).

The intuition is simple: take one cohort and a time window—that is, those who get treated at a specific time—and estimate treatment effects *for that particular cohort in that particular treatment time*. This procedure allows us to resolve the chronic-weighting problems that cause misleading estimates in the staggered TWFE settings (Goodman-Bacon 2021), while at the same time we can disaggregate the full TWFE estimates into their heterogeneous components.

The second modification extends this basic building block to cases with more reliable comparisons. To do this, Callaway and Sant'Anna (2021) propose a non-parametric identification strategy:

$$\text{ATT}_{g,t} = \mathbb{E}\left[\left(\frac{G_g}{\mathbb{E}[G_g]} - \frac{\frac{p_g(X)C}{1-p_g(X)}}{\mathbb{E}\left[\frac{p_g(X)C}{1-p_g(X)}\right]}\right)\left(Y_t - T_{g-1}\right)\right], \tag{4}$$

where ATT for group $G = g$ at time $T = t$ is calculated using the long distance with respect to the base period, where control groups are weighted according to a propensity score model, which, in this case, involves estimating the probability of being in the treated group conditional on sex, age, the square of age, cohort, educational attainment, and migration status. The modification of Equation (4) to accommodate not-yet-treated units is presented in Callaway and Sant'Anna (2021:206, E2.5).

This specification achieves two fundamental things. First, we can use $t = g - 1$ as an appropriate reference time for each participant who selects into treatment in different windows. Second, we can use various identification strategies—for example, outcome regression or inverse probability weighting—to assume *conditional* parallel trends (Callaway and Sant'Anna 2021:205–6). Note that this is important for relaxing the parallel trends assumption across different, potentially incompatible, groups. In principle, I am imposing parallel trends on different covariate levels, allowing me to recruit credible counterfactuals, conditional on the validity of pretreatment covariates.

After recovering group-time average treatment effects, it is relatively straightforward to aggregate these estimates to have an effect estimate that is an analog to the canonical TWFE design:

$$\delta^*_{Aggregate} = \sum_{g=2}^{g=n} \delta_{Aggregate}(g)P(G = g), \tag{5}$$

where $\delta^*_{Aggregate}$ is the average effect for all groups for the time periods that they get treated, which is summing across different group-specific aggregate effects

$\delta_{Aggregate}(g)$:

$$\delta_{Aggregate}(g) = \frac{1}{\Gamma - g + 1} \sum_{t=2}^{\Gamma} 1\{g \leq t\} \text{ATT}_{g,t}, \tag{6}$$

where Γ represents the total number of time periods a group is treated and the value quantifies the overall effect of participating in treatment for each group g.

In summary, conditional DID design allows me to resolve three problems. First, I use pretreatment covariates to find credible counterfactuals for those who go through life transitions, which helps alleviate problems associated with time-varying period effects. Second, I am able to use unbalanced panels with heterogeneous treatment timing, which is unilaterally the case in all the data sets in this study.[10] Finally, I aggregate multiple observations to a single estimate using all the observations available to a person to mitigate small fluctuations that might result from measurement issues in opinion data (Alwin 2007). In what follows, I use this strategy for all analyses. The online supplement material B provides more details on model specifications and alternative robustness checks.

Results

I present the empirical analyses in five steps. First, I start by showing individual trends in outcomes to demonstrate that there is indeed meaningful change in political beliefs and preferences within the study window. Second, I present the aggregated DID models estimating the effects of six life events on 40 outcomes. Third, I disaggregate these estimates into effects at different lengths of exposure to the treatment to explore dynamic heterogeneity across time. Fourth, I analyze the differences between groups with high and low political interest. Finally, I examine whether the age at which respondents experience the treatment the first time matters for general findings.

Change in Political Orientations Across Time

We know that political orientations are less transient than cultural views, meaning that the variation we can potentially analyze might be low for an analysis of this kind. Therefore, the first question we need to answer is whether there is *enough* political change in these windows at the individual level to justify a theoretical comparison between two data generation processes. To answer this question, I analyzed over-time trajectories of individual responses by estimating varying fixed-effect models at the individual level (Rüttenauer and Ludwig 2020).[11] More specifically, I quantified the absolute movement observed for a person in an outcome. Figure 2 presents the distribution of these scores.

The majority of these change scores localized around 0, confirming the previous findings that there is indeed strong stability in response trajectories. However, we see a fair amount of change as well. The median value occurs at 0.14 on a $0-1$ scale, though the mass of response trajectories with slopes above 0.25—representing a moderate change in public opinion—occurs for 33 percent of the responses. Moreover, a substantive change in public opinion, with slopes above

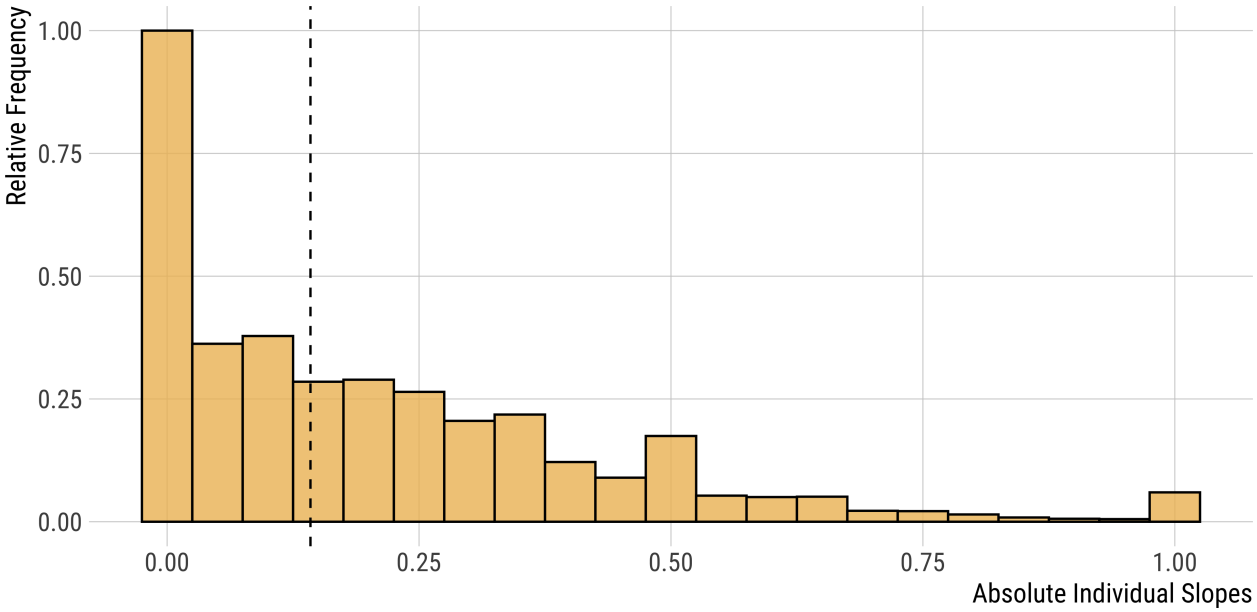

Figure 2: Change in political orientations across time.
Notes: The distribution of absolute change scores obtained from individual regression models across 40 political outcomes is shown. The median value occurs at 0.14, indexed as the dashed line.

0.50, represents nearly 10 percent of all the observations. Therefore, even though the modal trajectory suggests that the trajectories are, in general, stable, there is enough heterogeneity across people showing sizable changes over time.

The Effects of Life-Course Transitions

Figures 3 and 4 show estimates associated with each political outcome, distributed across family and work events. The overall results suggest that life-course transitions, on average, have no substantive influence on political views. Once we look at the effects in absolute terms, the aggregated treatment estimates range from 0 to 0.15, with a median of 0.01 and an interquartile range of 0.01 and 0.02 in a $0-1$ scale.[12] To put it differently, a median effect associated with each life-course transition is less than $1/50$th of the outcome scale. In conventional statistical significance tests—where $\alpha < 0.05$—43 out of 240 (17.9 percent) of these model estimates are reliably different than 0, though, again, those that *passed* the significance tests have a distribution with a median value of 0.03. Overall, these findings provide a substantive picture: life transitions might induce certain adjustments to political beliefs, though these adjustments are highly small in explaining the political differences among people. The online supplement material E provides the full set of DID estimates across items and life events.[13]

In addition to this overall finding, there are several important results about specific events and items. First, the effect sizes presented for family transitions are consistently smaller than the effect sizes presented for work transitions—approximately 32 percent on average. This is strongly driven by the entry to labor market, which

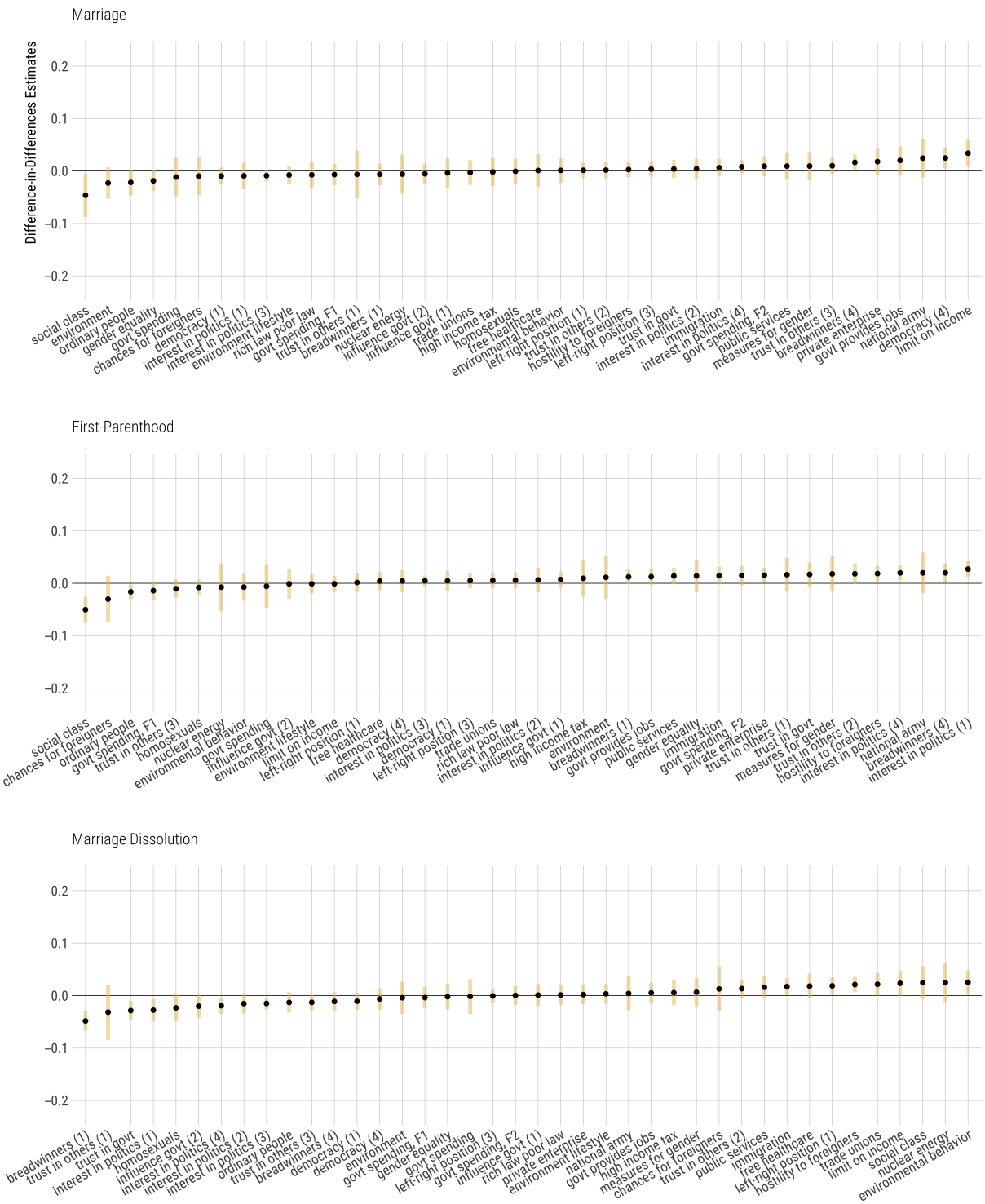

Figure 3: The DID estimates in the family domain.
Notes: The DID point estimates distributed across 40 political beliefs and three life-course transitions. Shaded lines represent 95 percent confidence intervals. Because several survey questions were asked in multiple panels, the figure represents the relevant estimates from BHPS as (1), SHP as (2), SOEP as (3), and UKHLS as (4) in the labels, as needed.

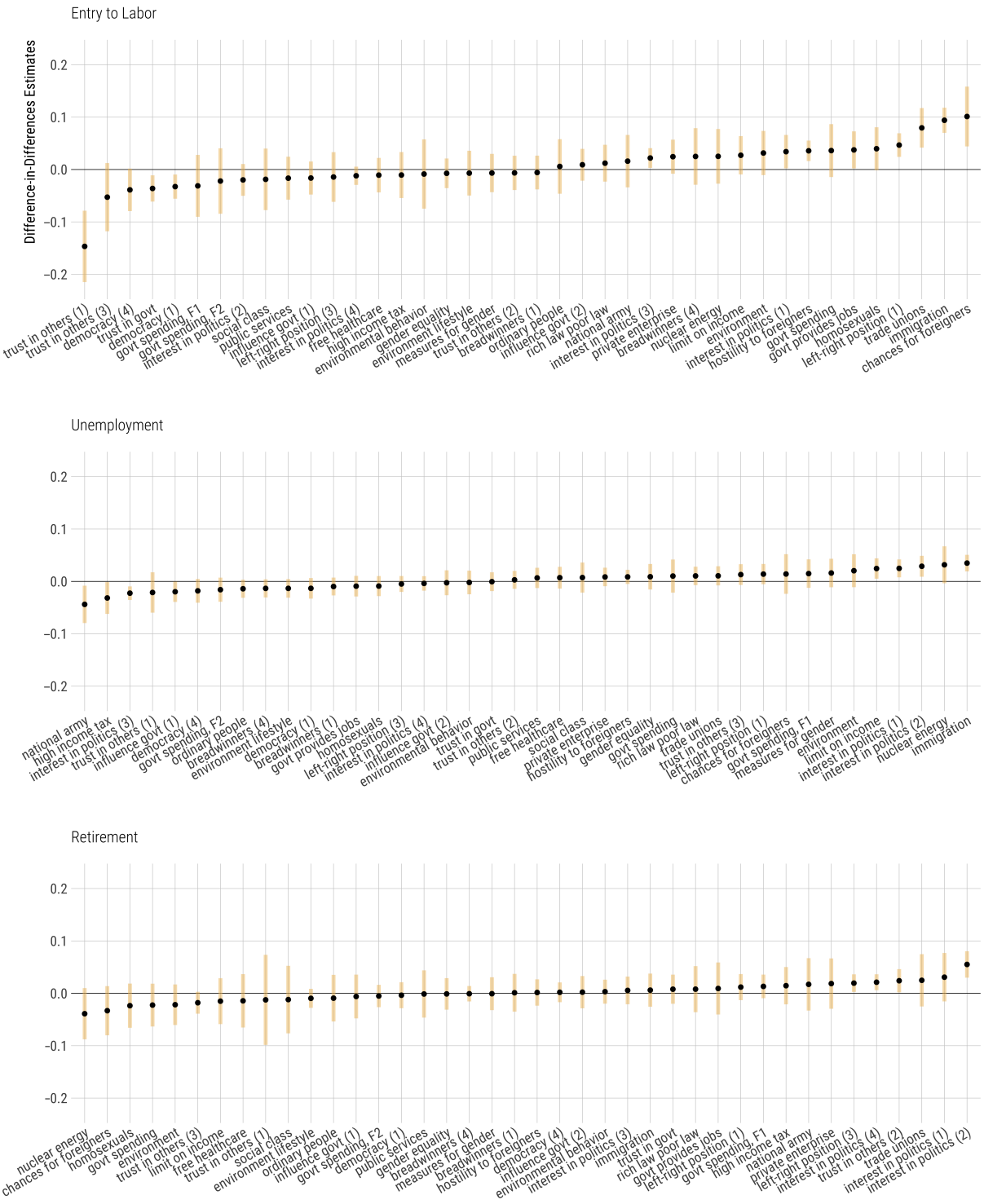

Figure 4: The DID estimates in the work domain.
Notes: The DID point estimates distributed across 40 political beliefs and three life-course transitions. Shaded lines represent 95 percent confidence intervals. Because several survey questions were asked in multiple panels, the figure represents the relevant estimates from BHPS as (1), SHP as (2), SOEP as (3), and UKHLS as (4) in the labels, as needed.

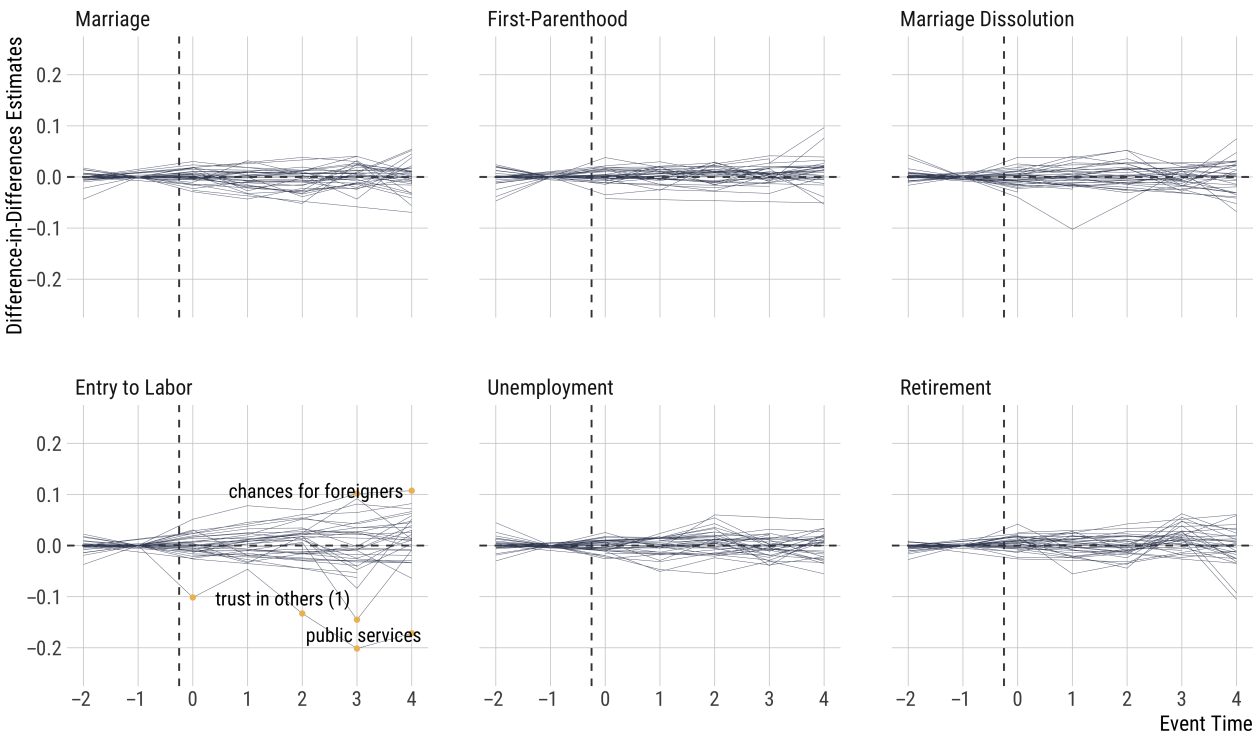

Figure 5: The dynamic effects of life transitions.
Notes: The figure presents the point estimates in different lengths of treatment exposure. The $t = 0$ indexes the first time respondents are recorded as "treated," $t = 1$ and onwards show the associated effect t years after the event, and $t = -2$ shows the pseudo-effect before the event happens. Due to event study estimation, $t = -1$ is constrained to be 0. The point estimates that are statistically significant at $\alpha < 0.05$ and above 0.10 are highlighted in yellow.

suggests that economic experiences might have a slight advantage in moving political positions compared to family experiences. Second, there are notable outliers concentrated in specific issues. Specifically, questions about immigration—see *chances for foreigners* and *immigration* for entry to labor—move 0.08−0.10 after each transition, suggesting moderate and politically significant effect sizes. Similarly, entry to labor market predicts substantial reductions in generalized trust. Finally, note that the changes show both left- and right-wing characteristics, though changes in immigration attitudes are usually in a conservative direction.

Note that there are only seven point estimates above 0.05, suggesting that there might be domain-specific effects of life events, though these effects are outliers to the general trends. Overall, these results broadly suggest that most change in political positions associated with life-course events is highly small, and people's baseline orientations are more prevalent in understanding political differences than their trajectories of change across time. In contrast to the expectations that people's personal life-course experiences can shed light on their general political preferences, the findings underscore a relative lack of connection between life-course developments and people's political orientations.

Heterogeneity Across Different Lengths of Treatment Exposure

What about event trajectories? It is highly plausible that there are heterogeneities across treatment exposure, such that individuals going through the events change their beliefs and preferences at first and then jump back to their previous positions, or, alternatively, the full effects of transitions need a relative amount of time to materialize. Looking at the observations, the data allow me to see, at the median, the first $3-5$ years after the event for most respondents—though there are cases where this goes up to 20 years. To explore these dynamic effects, I aggregated the group-time average treatment effects at length of exposure, approximating to an event study design. In Figure 5, I show the point estimates from $t = -2$ to $t = 4$, covering the first five years after the treatment, as well as the pseudo-window right before the event, while I discuss the general results below.

Figure 5 confirms the general trends observed in Figures 3 and 4: event estimates remain relatively flat. Looking at the point estimates, the median absolute effect size is, once again, 0.01. Significant outliers noted in the previous section—*trust in others (1)*, *changes for foreigners*, and *immigration*—along with a short-lived effect on *public services* are once again significant both statistically and substantively, though just like before, these are all associated with entry to labor. Similarly, roughly 16 percent of these point estimates are statistically significant at $\alpha = 0.05$; however, the median estimate associated with these significant cases is 0.03, with an interquartile range of 0.02 and 0.05.

If we extend the analyses to include *all* exposure lengths, overall, I estimated 3,232 event $\times$ outcome $\times$ event exposure estimates associated with this aggregation; 525 (16.2 percent) of these estimates were statistically significant at $\alpha < 0.05$. Once we extend the length of exposure to subsequent years, we start observing a handful of effects to emerge much later.[14] More particularly, the effects of entry to labor on *immigration*, *chances for foreigners*, *trust in others (1)*, and *trade unions* start intensifying after year 5—though observed in aggregate estimates as well—while the effects of retirement on *interest in politics* go up and subsequently down. Similarly, we observe over-time emergence of effects of first parenthood, marriage, and marriage dissolution on *measures for gender* and *breadwinners*. However, it is important to emphasize that the median estimate associated with these significant results is 0.06—with an interquartile range of 0.03 and 0.10—with no clear trajectory. Finally, only 135 of these 3,232 estimates (4.1 percent) are significant and above 0.10 and only 27 of these 135 are observed within the first 10 years after the event occurs. In the online supplement material B, I show that the simple aggregations using all estimates have a correlation of 0.99 with aggregations using the first 10 years and a correlation of 0.94 using the first five years after the event.

Heterogeneity Across Political Interest

If political orientation is indeed a function of early socialization to political groups and contemporary sensitivity to political communication from the elites, it is plausible to think that these findings are sensitive to political awareness (Zaller 1992), such that those with high political awareness are *less* likely to be sensitive to personal life experiences. To test this possibility, I repeated the same analyses reported

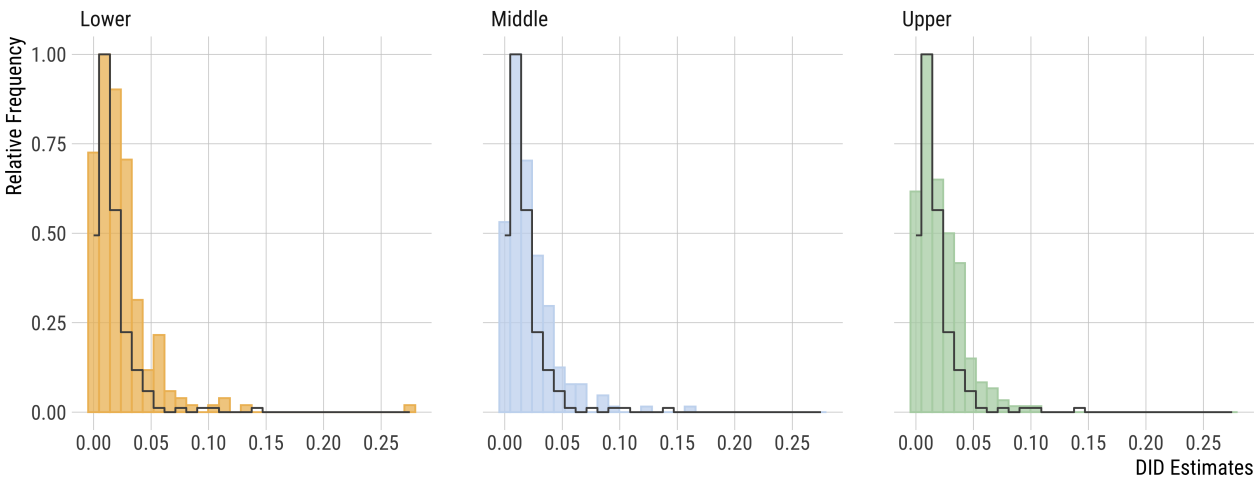

Figure 6: The DID estimates across levels of political interest.
Notes: The figure shows the distribution of DID estimates in three groups with different levels of political interest. The step plots show the full population estimates represented in Figures 3 and 4 for comparison.

above, but this time separating the sample into three groups, with different levels of political interest. In all surveys, respondents answered a question about their overall level of interest in politics, modeled as outcomes in the previous analyses. In what follows, I use these responses—pretreatment responses for treated groups and all responses for the control groups—to disaggregate the samples into groups with varying levels of political interest.

To do this, I estimated linear varying intercepts and slopes models in the multilevel context (Gelman and Hill 2007), where time $t_{i,t}$ is centered at the midpoint of each respondent:

$$y_{i,t} = \beta_0 + u_{0,i} + \beta_1 t_{i,t} + u_{1,i} t_{i,t} + \epsilon_{i,t}. \tag{7}$$

This procedure helps me extract a *modal* interest score for each respondent, captured in the varying intercepts at the individual level—$\beta_0 + u_{0,i}$—given that the time predictor t is centered at respondent midpoint. Theoretically, this approximates to one's general disposition of political interest.[15]

After extracting the individual scores, I divided the sample into three groups: those who are at the bottom one-third of the distribution ("Lower"), those who are at the top one-third of the distribution ("Upper"), and those who are in-between ("Middle"). The analyses below compare these three groups to evaluate whether the expectation of treatment heterogeneity is indeed true.[16] Due to the natural reduction in sample size with this disaggregation, I excluded several item and event combinations from the analyses, reducing the model counts from 720 to 642, and I only used gender in the propensity score model to avoid common support problems.

Figure 6 presents these results, showing little heterogeneity with notable exceptions. On average, there are no differences among these groups, with several outliers showing strong effect sizes for the low-interest group. More particularly, estimates for entry to labor show the biggest size differences, and the differences observed

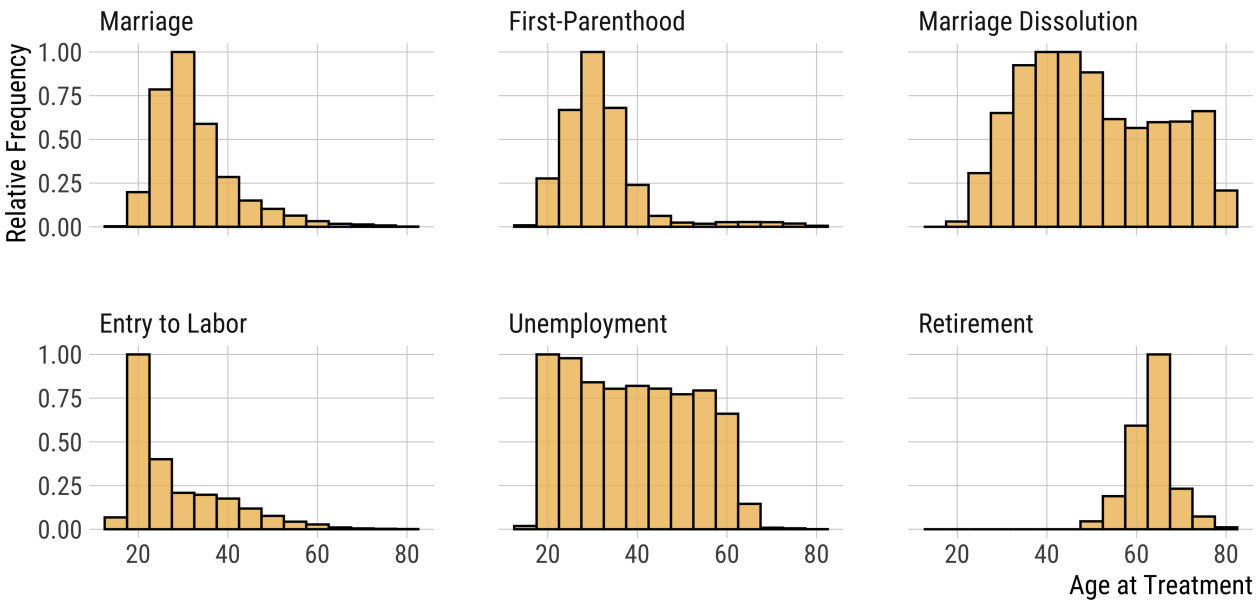

Figure 7: The distribution of age at treatment.
Notes: The distribution of age at which respondents went through the specific life event is shown. Each bin in the histograms represents five-year age intervals.

above for various measures—for example, *chances for foreigners*—are mostly concentrated for those on the lower one-third. As an example, those with low and middle political awareness changed their positions on *chances for foreigners* by 0.13 and 0.16, respectively, whereas those with high political interest were estimated to have a precise 0, which is not significant at the $p < 0.05$ level. Similarly, *immigration* changed by 0.10 for the lower group, 0.08 for the middle group, and 0.06 for the upper group, whereas *trust in others (1)* changed by an outstanding 0.28 among low-interest groups, while it was 0.06 and 0.04 for the middle and upper interest groups, respectively.

Therefore, conditional on observing *any* evidence of change in the aggregate, people with low or middle political awareness might be responsible for overall effects observed, compared to null effects on the higher end. Set against the baseline of non-existent or very minuscule movement, changes in political orientations we observe are likely to be driven by groups with low political awareness, as predicted by the SDM and theories that emphasize political mediation.

Rethinking Counterfactuals with Age Heterogeneity

Of course, all these life transitions are localized around certain ages. Figure 7 shows the distribution of "age at treatment," the age at which a person went through a transition. The figure documents how, for example, family formation is an early adulthood phenomenon, whereas unemployment or marriage dissolution can happen throughout one's life. These age-gradient differences are what life-course theory would predict: because adult life is institutionally organized and people go through specific life-stages, we expect that these events localize in specific

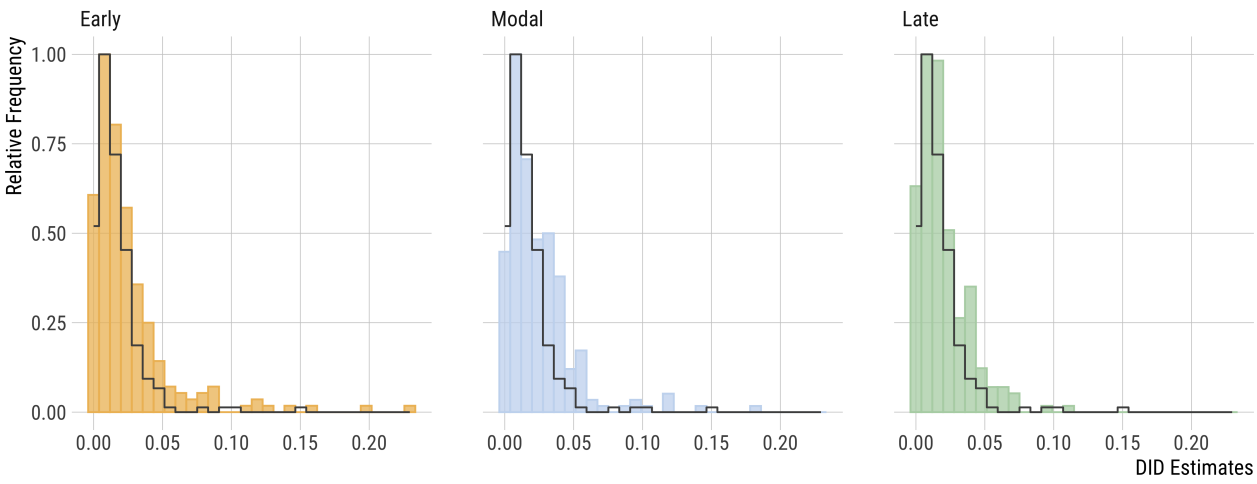

Figure 8: The DID estimates across different age groups.
Notes: The distribution of DID estimates in three groups with different levels of political interest is shown. The step plots show the full population estimates represented in Figures 3 and 4 for comparison.

periods (Elder 1994), with social expectations regulating on-time transitions and generating what Elder calls "normative timetables" (7).

Yet, there are important variations in age at treatment. See, for instance, how marriage is mostly concentrated in a 10-year window between the ages of 25 and 35, whereas marriage dissolution has a larger window. This heterogeneity might induce several problems to the aggregated estimates shown before. Even though the DID analyses used propensity scores to weight respondents based on age, we might still observe heterogeneous effects across age groups that get cancelled out or suppressed in the aggregate. After all, marrying at age 25 is a different experience than marrying at age 35, and these varying windows might be highly consequential. Hence, I explore whether such heterogeneities can emerge if we look at these normative timetables across the life course, and analyze whether people's timing into events have heterogeneous impacts.

Figure 8 shows the results from these heterogeneity analyses, with patterns similar to the discussion on political interest. In estimating these effects, I separated the samples into three: those who went through the specific life events at the bottom one-third age quantile in each country ("Early"), those who went through the specific life events at the top one-third age quantile ("Late"), and those in-between ("Modal"). Then, due to the reduction in sample size, I estimated 699 models, and I only used gender in the propensity score model to avoid common support problems.

The biggest variation in these comparisons is concentrated in early experiences, once again, mostly driven by changes in employment, where people who went through these events in modal or earlier times made significantly higher changes in their political positions. For instance, once employed, people changed their positions on generalized trust by 0.23, trade unions by 0.20, and class and law by 0.16 if they went through the employment earlier than the modal time. However, the fact that there is no corresponding pattern for late entries suggest that these

experiences might reflect developmental processes. Note that the lower bound for the modal age for entry to labor force is 21, meaning that these experiences occur before the usual developmental window of age 25.

Broadly, these analyses have three central implications. First, one's life-course transitions have little substantive influence on political orientations, once we look at the patterns with counterfactual comparisons. There are several, domain-specific, effects, though these effects are, once again, highly small. Second, once we separate respondents to groups with different levels of political interest, slightly varying but relatively small effect sizes emerge, suggesting that the well-established stylized fact in political science on the moderating role of awareness might be at play (Zaller 1992). Finally, the aggregate analyses reveal several age heterogeneities, though these heterogeneities may reflect early experiences and thus be confounded by developmental processes.

Discussion

The central objective of this article was to substantiate whether adult experiences reliably move one's political orientations. Using four panel surveys across three countries and a wide battery of political beliefs and preferences, I estimated the effects of six life events on the trajectory of survey responses. I found that life events have no substantive influence on political orientations. These results suggest that, in contrast to LCAM, individual differences in political beliefs and preferences at the start of most longitudinal surveys do not move reliably in response to life-course transitions.

Theoretical and Empirical Implications

This article speaks to two sets of questions that have been increasingly tackled in cultural sociology: the change and stability in cultural orientations across time and the role of preadult socialization over the life course. Because the findings from the former have started to converge on the idea that cultural preferences in adulthood are relatively hard to change, the natural implication of these studies was to propose socialization as the pathway for understanding cultural differences. The findings of this article suggest that the socialization period might indeed be highly consequential in setting the baseline for political orientations, but an exclusive focus on adulthood stability might not be enough for understanding the mechanisms of political belief formation. Given that individuals *do* change on a variety of political issues, as documented in Figure 2, understanding the extent to which political beliefs and preferences might change in response to personal experiences, local encounters, and political processes requires substantive scholarly interest.

Moving forward, these findings suggest that sociology needs an integrated theory of socialization and adult belief change. Even though the synthesized DID estimates suggest relatively null findings, there was variation in the estimates, showing no effect in most cases and substantive effects in the remaining few. This means that the tension between ontogenic and sociogenic explanations cannot be resolved *tout court*, and sociologists need to specify the conditions under which one

model or the other is powerful. The mixed evidence about political interest might be one starting point: even though personal culture can easily be conceptualized as a system ("the kind of person you are"), the fact that individuals vary over their political awareness can give us certain clues about what kinds of preferences across what kinds of people are more amenable to change after certain events.

Considering that one popular empirical strategy in sociology is to estimate the effects of certain life events on politics within relatively short time windows (e.g., Owens and Pedulla 2014), these results suggest that estimates that do not reliably identify causal effects—for example, those that do not follow sufficiently comparable counterfactuals, or studies that exclusively rely on two or three data points—might have been overestimating the actual processes. Once the issue is about political change in general, the comparability of groups, as well as a sufficient set of observation points, seem to be needed for reliably arguing whether an effect exists or not, and we need robust identification strategies to do that—especially when the meaning of change is elusive.

It is possible that life-course events do not change one's average responses but increase or decrease the response *variation*. Put differently, these life-course changes might reinforce preexisting views, thereby reducing "error" around one's true scores, or they might increase one's doubts about their positions, making them more volatile across time. However, these possibilities imply that empirical predictions associated with the LCAM and the SDM need different theoretical mechanisms to understand opinion reporting in more detail. The fact that mean responses do not change does not mean that there is no change in *variation*, and life events might be consequential for the latter.

Problems and Questions

There are, of course, several limitations for this article. Most importantly, the findings hinge on how political change is measured. Studies that attempt to understand change in cultural preferences emphasize that survey responses are highly error-prone, and make several assumptions about how to define change (Kiley and Vaisey 2020; Lersch 2023). We do not know what counts as *true change* and what counts as *measurement error*, which occupies most modeling challenges. In this study, I used mean changes associated with a pre-post design to operationalize change. This cannot rule out the possibility that the amount of change might be inflated or deflated depending on what we count as actual change. Similarly, the operationalization of change in this article may capture changes in central tendency, but not dynamic changes: individuals may change in response to a series of events, rather than one event, and my design was limited to address this issue.

Similarly, the general DID strategy presented in this article estimates conditional average changes, though there might be distributional effects in different quantiles of the outcomes (Callaway and Li 2019). Particularly, the effects of life events might be more pronounced in the middle or in the extremes, though these potential heterogeneities are most likely to be washed away in the aggregate. Hence, the null results presented in this article are subject to change if there are such heterogeneities across the initial political beliefs and preferences.

The substantive effects associated with life-course transitions notwithstanding, this article did not allocate the variation attributable to intraindividual change to the total set of life-course transitions. Obviously, following only six life transitions cannot capture the full set of adult experiences, though we do not know the extent to which life-course processes can explain *total* change in adulthood. Lersch's (2023) strategy of employing "age" as an index might be helpful in understanding over-time change in personal culture, though it cannot attribute within changes to specific expectations from life-course theory. This question is consequential, because the theoretical expectations require us to partition the role of personal trajectories, local experiences, and higher political processes. Hence, we need studies that explicitly identify the sources of overall change in political orientations.

One important assumption of this article was to conceptualize political orientations as a subset of personal culture, though it is highly plausible that political positions are more amenable to SDM than they are to LCAM. Of course, this was the central claim to begin with: I argued that political mediation masks phenomenological information one can learn from day-to-day life. That said, it is important to note that these findings cannot adjudicate SDM and LCAM *in general*. It might be that some life domains are more amenable to phenomenological information than others, and future research should quantify the susceptibility to life events across different cultural domains.

One final question concerns interpretive heterogeneity. The DID models summarized individual change trajectories *on average*, meaning that if life-course transitions make some people move in one direction and other people in another direction depending on an unobserved trait, these movements would be cancelled out in DID models. Because the theoretical question concerns political *displacement* rather than political *movement*, the heterogeneity in how people respond to life events might underestimate the overall effects. This suggests that we need heterogeneity-robust estimators that take these response differences across groups more seriously.

Data Note

This article uses data from BHPS, UKHLS, SHP, and SOEP as follows:

- The BHPS data were made available through the UK data archive. The data were originally collected by the Economic and Social Research Council (ESRC) research center on micro-social change at the University of Essex, now incorporated within the Institute for Social and Economic Research. The author bears full responsibility for the analyses and interpretation of the data presented in this article.
- Understanding Society is an initiative funded by the Economic and Social Research Council and various Government Departments, with scientific leadership by the Institute for Social and Economic Research, University of Essex, and survey delivery by NatCen Social Research and Kantar Public. The research data are distributed by the UK Data Service.

- This study has been realized using data collected by the SHP, which is based at the Swiss Centre of Expertise in the Social Sciences FORS. The project is supported by the Swiss National Science Foundation.
- The SOEP is administered at the German Institute for Economic Research (DIW Berlin) and funded by the Federal Ministry for Education and Research (BMBF) and the state governments under the umbrella of the Leibniz Association.

Notes

1 In what follows, I use political orientations to denote various political constructs observed at the personal level, such as beliefs, preferences, and self-descriptions. Because I use propositional survey items, I hold no position whatsoever as to the differences—or lack thereof—among these constructs. I use *cultural* and *political* interchangeably while discussing models from cultural sociology, with the understanding that the latter is a special case of the former. I will discuss the implications of this assumption on the theoretical framework and general results in the final section.

2 I follow Kiley and Vaisey (2020) and Lersch (2023) in using the *SDM* and the *LCAM* as heuristic devices for two broad sets of frameworks for understanding cultural differences among the populace.

3 This is why conspiracy beliefs, beliefs with strongly irrational or epistemically problematic components (Uscinski and Parent 2014), are hard to change even with "true" information. Symbolic beliefs, sometimes irrespective of their truth, thus have a social function in sustaining group cohesion (Williams 2021).

4 As Martin (2015:29) emphasizes, political ideology might be related to social position "insofar as this is mediated by alignment with a particular political side, especially a political party." The main point, *mediation*, still holds.

5 In their study on cultural change, Lersch (2023) used person age as an index to capture the life-course trajectory, with the benefit of strongly simplifying the estimation process. That being said, age may capture multiple effects, including, but not limited to, political communication, personal encounters with high-prestige opinion leaders, or simple personal reflection on public cues. This makes life-course experiences equal to any experience an individual can have over time, which is too big of a claim to refute empirically. Thus, I equate the expectations from life-course theory with specific and institutionally defined changes across life (see Elder, Johnson, and Crosnoe 2003; Marsden 2018).

6 The detailed information for these data sources is provided in survey documentations. See, particularly, Goebel et al. (2019) for Socio-Economic Panel (SOEP), Voorpostel et al. (2016) for Swiss Household Panel (SHP), and University of Essex (2019) for British Household Panel Survey (BHPS) and UK Household Longitudinal Study (UKHLS).

7 Because the composition of political parties changes over these long periods, the referents of those specific questions—for example, the parties one has sympathy for—changes over time, making these items non-consistent for over-time comparison.

8 Of course, it would be desirable to reduce the items more to decrease measurement error. However, a lot of these items were asked in different years, making dimensionality reduction impossible in most measures.

9 To test whether the findings are sensitive to this assumption, the online supplement B provides alternative analyses where the "effect window" is restricted to 5 and 10 years after the event. All substantive results hold.

10 One benefit of estimating group-time average treatment effects is that the aggregation can easily be achieved across cohorts—capturing treatment timing—or calendar time—capturing period effects. Because the estimation occurs in such a granular level, it is possible to attack the age-period-cohort problem in reasonable ways. That said, the sample size considerations do not allow me to aggregate the estimates using these alternative specifications, with low counts leading to high standard errors and low precision. Hence, the findings average over differences in periods and cohorts. To mitigate the problems associated with the latter, I include cohort in the propensity score models.

11 In practice, I estimated regression models by regressing the political outcome on a time variable, normalized at the individual level. This procedure helped me quantify a predicted change in outcomes for each individual.

12 Using random effects meta analyses to pool estimates for aggregation gives substantively the same results.

13 I conducted several analyses to test the robustness of these estimates. The online supplement B provides alternative DID specifications and aggregation schemes. The online supplement material C analyzes whether treatment anticipation—that is, the possibility that respondents might have private signals as to when they will get treated, and adjust their outcomes accordingly—changes the substantive conclusions. The online supplement D performs an alternative analysis to see whether non-random assignment based on prior political orientations have implications for the main results.

14 One caveat with these extended results is that, due to panel data, these estimates become compositionally different than the previous ones in the majority of cases. Although standard errors naturally increase due to smaller sample sizes, I observe "significant" effects in few time periods much later than the event, suggesting random deviations due to a combination of low sample size and imprecise estimates, which are washed away once we aggregate the event estimates.

15 Of course, if people move in substantive ways over time, this measure deflates or inflates the actual level of interest at any given time. That said, one's "chronic" interest might be much more relevant than specific calendar time interest, given that the latter might be highly sensitive to exogenous events, for example, elections or salient political developments.

16 I estimated the multilevel models within each country separately without pooling the data in any fashion. Once we compare these interest scores, respondents in the SHP, on average, are more politically interested compared to BHPS, SOEP, and UKHLS—on a $0-1$ scale, this translates into a mean difference of 0.11.

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Acknowledgments: I thank Stephen Vaisey, Craig Rawlings, and Christopher Wildeman for their extensive feedback on different versions of this manuscript, and Andrés Castro Araújo, Kevin Kiley, and the participants of the Worldview Lab at the Kenan Institute for Ethics at Duke University for their thoughtful comments on the project.

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