Supplement to:

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Supplemental Materials for

Life-Course Transitions and Political Orientations

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Contents

1 Supplemental Materials A: Measurement

1.1 Measuring Political Orientations

Table A1 provides the complete list of political belief variables used in the analyses, along with the survey references and descriptions associated with each item.

Table A1: The Descriptives of Political Belief Variables Used

Survey	Labels	Description	Item
BHPS	social class	Social class affects opportunities	opcls3
BHPS	breadwinners (1)	Husband should earn, wife should stay at home	opfamf
BHPS	homosexuals	Homosexual relationships are wrong	opfamr
BHPS	free healthcare	All healthcare should be free	ophla
BHPS	influence govt (1)	People cannot influence government policy	oppolb
BHPS	limit on income	There should be a limit on income	oppolc
BHPS	ordinary people	Ordinary people share nation's wealth	opsoca
BHPS	rich law poor law	One law for rich and one law for poor	opsocb
BHPS	private enterprise	Private enterprise solves economic problems	opsocc
BHPS	public services	Public services ought to be state-owned	opsocd
BHPS	govt provides jobs	Government has an obligation to provide jobs	opsoce
BHPS	trade unions	Strong trade unions protect employees	opsocf
BHPS	trust in others (1)	Trustworthiness of others	trust
BHPS	interest in politics (1)	General level of interest in politics	vote6
SHP	interest in politics (2)	General level of interest in politics	pp01
SHP	democracy (1)	Overall satisfaction with swiss democracy	pp02
SHP	influence govt (2)	Feeling about government influence	pp03
SHP	trust in govt	Trust in the federal government	pp04
SHP	left-right position (1)	Political identification: left-right position	pp10
SHP	national army	Opinion on the swiss army	pp12
SHP	govt spending	Opinion on social expenses	pp13
SHP	chances for foreigners	Opinion on chances for the foreigners	pp15
SHP	environment	Opinion on environmental protection	pp16
SHP	high income tax	Opinion on taxes on high income	pp17
SHP	nuclear energy	Opinion on nuclear energy	pp18
SHP	gender equality	Women in swiss society are generally penalized	pp20
SHP	measures for gender	In favor of measures for gender equality	pp22
SHP	trust in others (2)	Trustworthiness of others	pp45
SHP	govt spending, F1	Spending factor: big versus small government	spendgover
SHP	govt spending, F2	Spending factor: desired substantive shares	spendshare
SOEP	left-right position (3)	Political identification: left-right position	plh0004
SOEP	interest in politics (3)	General level of interest in politics	plh0007
SOEP	trust in others (3)	Trustworthiness of others	trustscale
SOEP	immigration	Worried about immigration to germany	plj0046
SOEP	hostility to foreigners	Worried about hostility to foreigners	plj0047
UKHLS	democracy (4)	Overall satisfaction with uk democracy	demorient
UKHLS	environmental behavior	Opinion on personal lifestyle and environment	scenv_ftst
UKHLS	environment lifestyle	Being green is an alternative lifestyle, not for the majority	scenv_grn
UKHLS	breadwinners (4)	Husband should earn, wife should stay at home	scopfamf
UKHLS	interest in politics (4)	General level of interest in politics	vote6

As noted in the article, I constructed two items ("govt spending, F1" and "govt spending, F2") in the SHP by extracting the first two principal components of 11 items on government spending: protection of the environment, health, day nursery, police and public order, research and education, military and defense, retirement, culture and art, unemployment benefit, aid to developing countries, and social aid. The first component shows positive loadings on all items, capturing a general disposition about big versus small government (with a correlation of $r = 0.28$ with left-right position), while the second component differentiates spending on military and defense, and police and public order from the others (with a correlation of $r = 0.51$ with left-right position).

Similarly, *trust in others* measure in the SOEP is the first principal component from 5 items, where respondents evaluated the following statements: on the whole trust people, nowadays can't trust

anyone, caution when dealing with strangers, most people are exploitative, and most people act in their own interest. As a test for the item's construct validity, I checked whether this item correlates with the worry about immigrants to Germany, and found a correlation of $r = -0.27$, i.e., those who are more trustful to people are less likely to be worried about immigration.

1.2 Measuring Life Transitions

To measure life-course transitions, I used harmonized datafiles from the Comparative Panel File initiative [CPF; see Turek, Kalmijn, and Leopold (2021)]. Of course, there are certain limitations to harmonization given that items differ considerably across panels. That said, CPF (2021) was highly instrumental to ensuring comparable survey measures as much as possible. The readers can visit the CPF webpage for the relevant documentations and the CPF syntax files for running the harmonization script. In what follows, I document the relevant procedure for each transition.

Marital Status. To operationalize one's marriage status, I used CPF's `nvmarr` variable, which captures whether respondents were formally married until the interview time.[1] To operationalize marriage dissolution, I used three items—`widow`, `divor`, and `separ`—where `widow` refers to being "widowed," `divor` to being "divorced," and `separ` to being "separated." Since any of these events have highly low counts, I generated a new indicator if a respondent reported at least one of these events.

Parenthood. CPF measures the number of children that respondents ever had—including those who are not living in the household, those who are older than age 18, and/or those who are adopted or deceased—to construct a `kids_any` variable, which measures whether respondent had *any* children. This variable provides within-person information across time for SHP and UKHLS, though not for BHPS and SOEP. In order to have the same information for BHPS and SOEP, I used additional data files: the British Household Panel Survey Consolidated Marital, Cohabitation and Fertility Histories for BHPS and the Birth Biography of Female and Male Respondents for SOEP. I used this variable as an indicator for measuring transition to parenthood.

Employment Characteristics. I used three items to capture relevant information about one's employment characteristics: entry to labor, unemployment, and retirement. In all cases, I used the baseline item `emplst5`, where respondents report their employment status at the time of the survey:

- To define entry to the labor market, I used `emplst5` and coded the changes from "not active/at home" and "in education" categories to "employed" category.

- I define "unemployed" as someone who is not currently working and actively looking for work after they were employed for a certain time. Hence, those who report unemployment after education or retirement are not counted as unemployed in this measure.

- To construct the final transition, "retirement," I used CPF's `retir`, defining retirement as a combination of those who self-categorize as retired, those who are not working and aged 65 and above, and those who are not working and receive old-age pension.

Note that the main sample does not include respondents who were *always-treated* throughout the survey window—e.g., those who are married throughout the survey.

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[1]According to the CPF documentation, in some cases, being "single" and "never married" could be bundled together. Thus, this variable contains a certain amount of measurement error with regard to the actual biographical details.

2   Supplemental Materials B: Difference-in-Differences Specifications

I used the "did" R package in all difference-in-differences analyses presented in the manuscript. There are three central decisions in the preparation of these models. The first involves whether the researcher fits a conditional or unconditional model, where the former uses pre-treatment covariates to constrain the parallel trends assumption across levels, while the latter assumes parallel trends between all treated and control cases. The second decision involves the composition of the control group: one choice is to use those who never got treated—"never treated"—and the other choice is to use those who never got treated as well as those who will get treated but for the purposes of group estimations, not yet treated—"not-yet-treated." The final decision involves how to present the results: since the main estimation involves group-time average treatment effects, researchers should decide on the level at which they aggregate these small estimates.

2.1   Decision 1: Conditioning

In all analyses—except otherwise noted—I estimated difference-in-differences models using conditional treatment assignments to alleviate concerns about parallel trends. In doing this, I used sex, age and the square of age, educational attainment, birth cohort, and migration status as the pre-treatment covariates, all harmonized in the Comparative Panel File.

2.2   Decision 2: Counterfactual Groups

In all specifications, I estimated the models using conditional DIDs with not-yet-treated groups, i.e., for each group-time average treatment estimate, I pooled the never treated groups and not-yet-treated groups together. Substantively, I proposed that this is the best combination: it is hard to believe that parallel trends hold unconditionally over the life-course, and the exclusion of not-yet-treated groups do not have a theoretical reason. Of course, we do not need complete balance in DID estimations—the assumption is not about levels, but the fact that individuals need to experience *time* in the same way. That said, it is still warranted to see different model specifications.

Figure B1 shows the comparison between the main estimates and coefficient estimates from two alternative models. In the first panel, I present an alternative model that uses *never treated* as the control group. In the second panel, I present an alternative propensity score specification, where I interacted cohort and age with gender.[2] As can be seen, I substantively arrived at the same results.

2.3   Decision 3: Aggregation

The third decision involves the aggregation of group-time average treatment effects. In the main estimates reported in the article, I aggregated group-time average effects to simple estimates by using all observations associated with each individual. This means that, if a group of people has observations 15 years after the said event, those observations, however small, are included in the weighted estimates. This, effectively, allows every observation in the data to be included. That said, it is plausible to argue that some effects might require only few years to materialize, and after a certain period of time, we cannot be sure about the real-world causal processes.

Figure B2 shows that this criticism does not hold. In the first panel, I compare the main estimates with estimates aggregated using a 5-year observation window, i.e., I used observations 5 years after

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[2]Note, however, the estimation for interaction models ended up with problems, given that the propensity score model had to extrapolate strongly without suitable weighting.
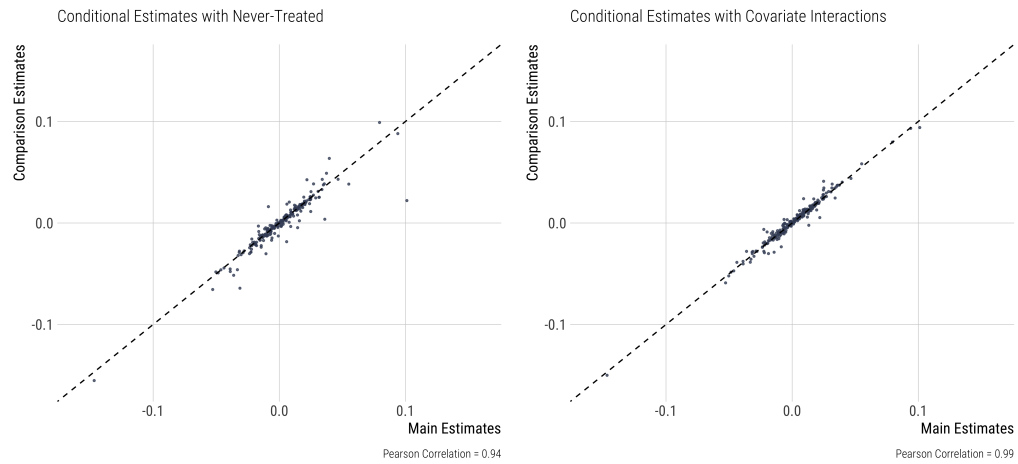
Conditional Estimates with Never-Treated

Conditional Estimates with Covariate Interactions

Pearson Correlation = 0.94

Pearson Correlation = 0.99

Figure B1: The Coefficient Estimates Across Model Choices
Notes: The figure presents coefficient comparisons between the main model and two alternative models. The first model compares never treated and not-yet-treated counterfactuals, while the latter changes the propensity score specification by adding female x cohort and female x age interactions to the main pre-treatment covariates.

the treatment event and 5 years before the treatment event. Panel 2 presents the same comparisons, this time with a 10-year observation window. As can be seen, the estimates correlate very strongly.
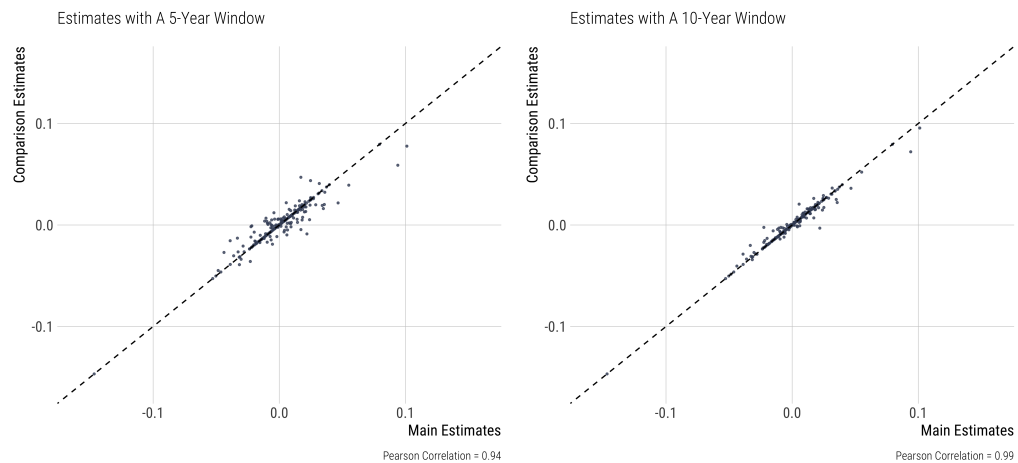
Estimates with A 5-Year Window

Estimates with A 10-Year Window

Pearson Correlation = 0.94

Pearson Correlation = 0.99

Figure B2: The Coefficient Estimates Across Aggregation Choices
Notes: The figure shows coefficient comparisons between the main aggregation and two alternative aggregations. The first panel shows a 5-year aggregation window while the second panel shows a 10-year aggregation window.

3   Supplemental Materials C: Treatment with Anticipation

One potential issue with the estimation of DID models is that individuals can "anticipate" the fact that they are going to get treated, and this can induce people to update their orientations *before* the treatment. This is a violation of the main DID assumptions (Callaway and Sant'Anna 2021).

In order to test whether this possibility changes the main coefficient estimates, we can modify our identification strategy by changing the baseline year. As stated in the manuscript, the baseline year for each group is one year before the treatment happens; changing the baseline year means that we include the observation at $t = -1$ to count *in the treatment time*, while revising the identification such that $t = -2$ becomes the new baseline year for calculations. This procedure allows us to move the window one year, and if anticipation is an issue, changing the window should account for it.

In Figure C1, I present the results from this robustness check. As can be seen, the main conclusions remain the same, even though there are slight changes in the main estimates.
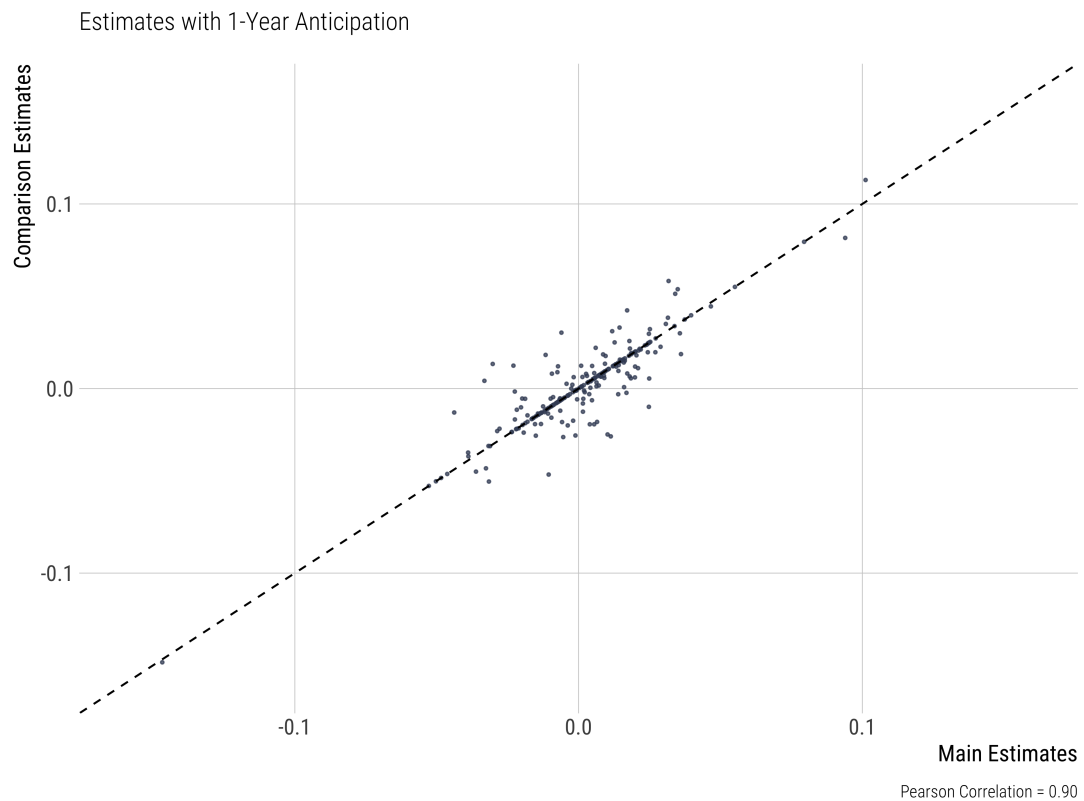

Figure C1: The Coefficient Estimates with Treatment Anticipation
Notes: The figure shows coefficient comparisons between the main aggregation and an alternative specification where 1-year anticipation window is included in the main DID estimations.

4 Supplemental Materials D: Non-Random Selection into Treatment Assignment

It is possible that political outcomes examined in the article are at least slightly consequential in one's probability of treatment—e.g., one's breadwinner attitudes might influence marriage timing.

The solutions to this, of course, might occur only under strong assumptions. One common strategy is to utilize lagged outcomes in the regression specification to adjust for potential differences. This comes with strong assumptions; most particularly, this inclusion changes the conditional parallel trends assumption to a conditional mean independence assumption, which makes sense only in cases where conditioning on the lagged outcomes and the covariates makes the treatment as good as random (Roth et al. 2023). This is obviously too strong of an assumption. That said, provided that we are cautious, we can explore the extent to which the estimates are sensitive to this specification.

To do this, I calculated the average pre-treatment responses of all individuals at each question, and estimated the models once more by including these scores in the DID models. Figure D1 shows these results. Again, the main findings are not sensitive to lagged outcomes.

Conditional Estimates with Lagged Outcomes
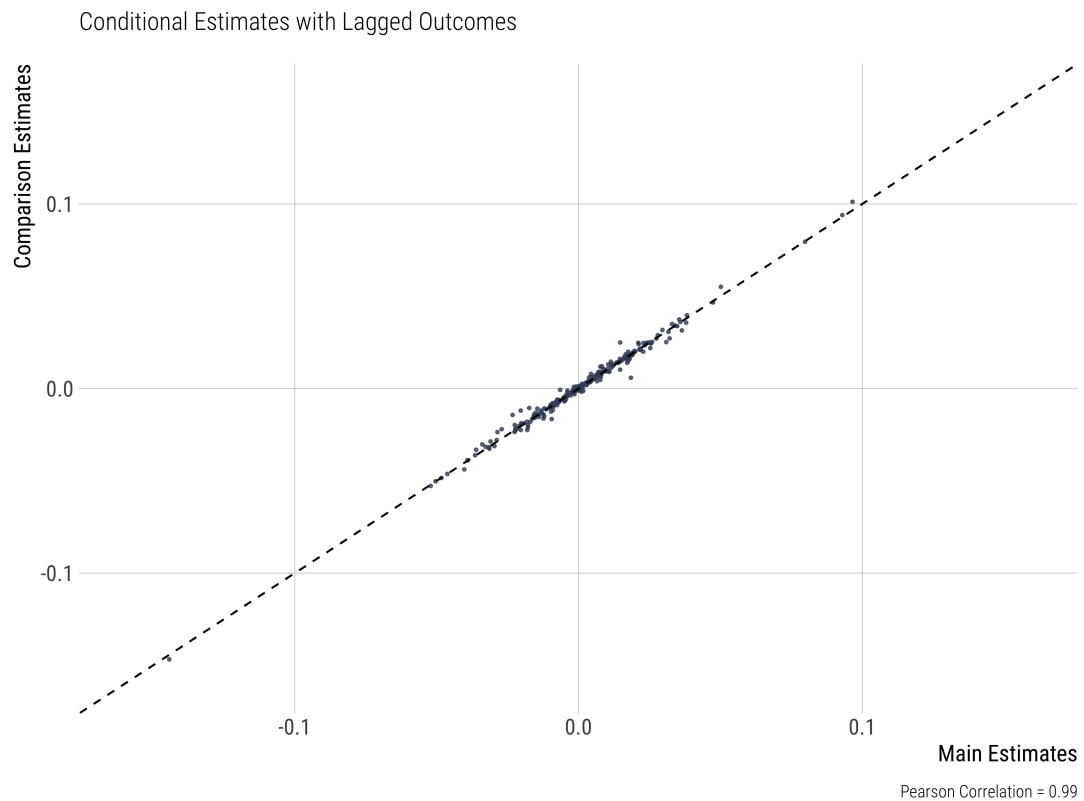
Pearson Correlation = 0.99

Figure D2: Estimates with Lagged Outcomes
Notes: The figure shows coefficient comparisons between the main aggregation and an alternative aggregation with lagged outcomes, where the latter refers to one's "central tendency" in respective political beliefs.

5   Supplemental Materials E: Aggregated DID Estimates

Table E1: The Difference in Differences Estimates

Survey	Variable	Marriage	Parenthood	Dissolution	Labor Entry	Unemployment	Retirement
BHPS	social class	-0.05	-0.05	0.02	-0.02	0.01	-0.01
BHPS	breadwinners (1)	-0.01	0.01	-0.05	-0.01	-0.01	0.00
BHPS	homosexuals	-0.00	-0.01	-0.02	0.04	-0.01	-0.02
BHPS	free healthcare	0.00	0.00	0.02	-0.01	0.01	-0.01
BHPS	influence govt (1)	-0.00	0.01	0.00	-0.02	-0.02	-0.01
BHPS	limit on income	0.03	-0.00	0.02	0.03	0.02	-0.02
BHPS	ordinary people	-0.02	-0.02	-0.01	0.01	-0.01	-0.01
BHPS	rich law poor law	-0.01	0.01	0.00	0.01	0.01	0.01
BHPS	private enterprise	0.02	0.02	0.00	0.02	0.01	0.02
BHPS	public services	0.01	0.01	0.02	-0.02	0.01	-0.00
BHPS	govt provides jobs	0.02	0.01	0.01	0.04	-0.01	0.01
BHPS	trade unions	-0.00	0.01	0.02	0.08	0.01	0.02
BHPS	trust in others (1)	-0.01	0.02	-0.03	-0.15	-0.02	-0.01
BHPS	interest in politics (1)	-0.01	0.03	-0.03	0.03	0.02	0.03
SHP	interest in politics (2)	0.00	0.01	-0.02	-0.02	0.03	0.06
SHP	democracy (1)	-0.01	0.00	-0.01	-0.03	-0.01	-0.00
SHP	influence govt (2)	-0.01	-0.00	-0.02	0.01	-0.00	0.00
SHP	trust in govt	0.00	0.02	-0.03	-0.04	-0.00	0.01
SHP	left-right position (1)	0.00	0.00	0.02	0.05	0.01	0.01
SHP	national army	0.02	0.02	0.00	0.02	-0.04	0.02
SHP	govt spending	-0.01	-0.01	-0.00	0.04	0.01	-0.02
SHP	chances for foreigners	-0.01	-0.03	0.01	0.10	0.01	-0.03
SHP	environment	-0.02	0.01	-0.00	0.03	0.02	-0.02
SHP	high income tax	-0.00	0.01	0.01	-0.01	-0.03	0.01
SHP	nuclear energy	-0.01	-0.01	0.02	0.03	0.03	-0.04
SHP	gender equality	-0.02	0.01	-0.00	-0.01	0.01	-0.00
SHP	measures for gender	0.01	0.02	0.01	-0.01	0.02	-0.00
SHP	trust in others (2)	0.00	0.02	0.01	-0.01	0.00	0.02
SHP	govt spending, F1	-0.01	-0.01	-0.00	-0.03	0.01	0.01
SHP	govt spending, F2	0.01	0.01	0.00	-0.02	-0.02	-0.01
SOEP	left-right position (3)	0.00	0.01	-0.00	-0.01	-0.00	0.02
SOEP	interest in politics (3)	-0.01	0.00	-0.02	0.02	-0.02	0.01
SOEP	immigration	0.01	0.01	0.02	0.09	0.03	0.01
SOEP	hostility to foreigners	0.00	0.02	0.02	0.04	0.01	0.00
SOEP	trust in others (3)	0.01	-0.01	-0.01	-0.05	0.01	-0.02
UKHLS	democracy (4)	0.02	0.00	-0.01	-0.04	-0.02	0.00
UKHLS	environmental behavior	0.00	-0.01	0.03	-0.01	-0.00	0.00
UKHLS	environment lifestyle	-0.01	-0.00	0.00	-0.01	-0.01	-0.01
UKHLS	breadwinners (4)	0.02	0.02	-0.01	0.02	-0.01	-0.00
UKHLS	interest in politics (4)	0.01	0.02	-0.02	-0.01	-0.00	0.02

References

Callaway, Brantly, and Pedro HC Sant'Anna. 2021. "Difference-in-Differences with Multiple Time Periods." *Journal of Econometrics* 225(2):200–230.

Roth, Jonathan, Pedro HC Sant'Anna, Alyssa Bilinski, and John Poe. 2023. "What's Trending in Difference-in-Differences? A Synthesis of the Recent Econometrics Literature." *Journal of Econometrics* 235(2):2218–44.

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