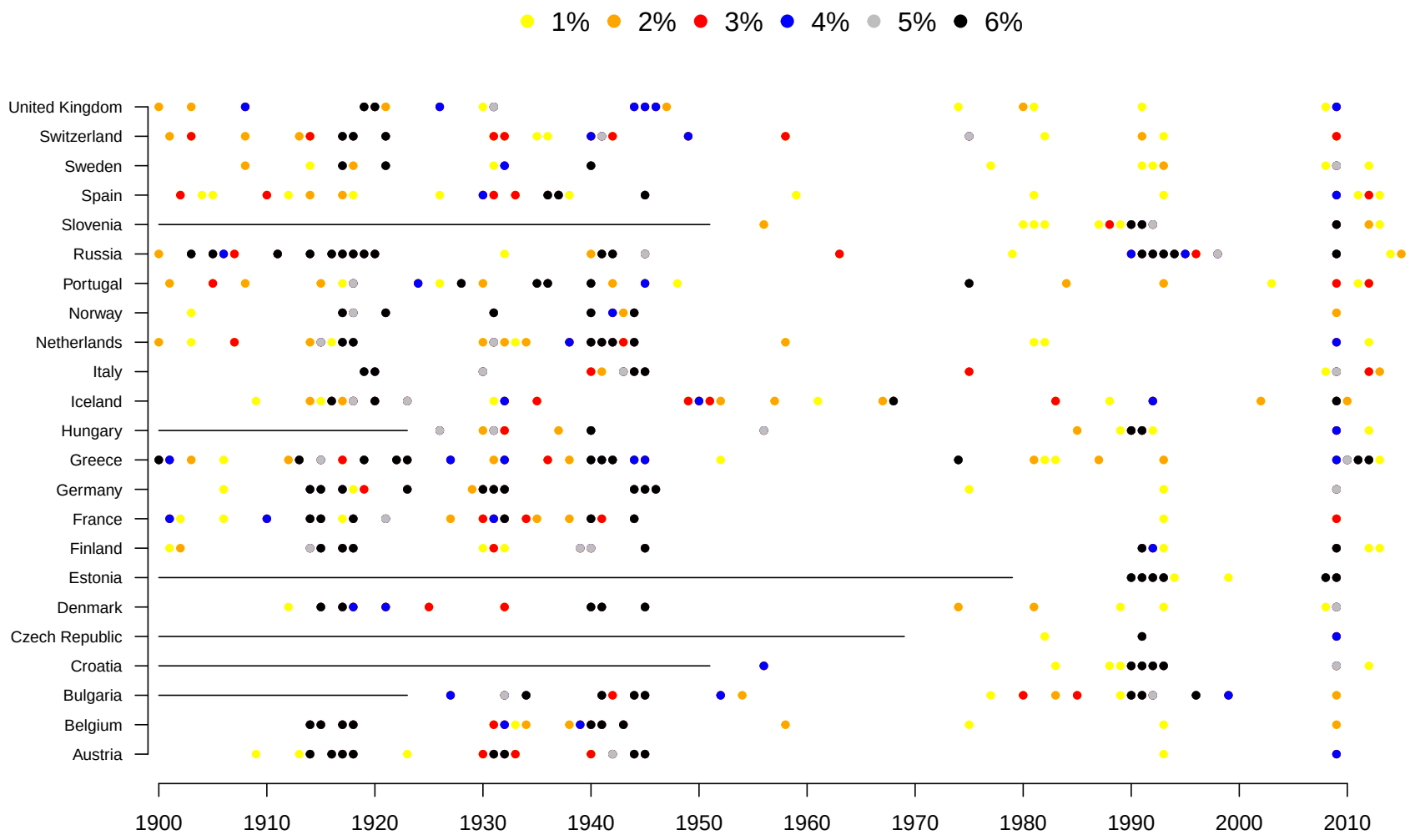
# Appendix: Economic Shocks and the Development of Immigration Attitudes

## Contents

Figure A1: Distribution of shocks across countries.




The points represent annual GDPpc declines of at least the specified threshold. Black lines represent missing GDPpc data for the specific country.

# Appendix A European Social Survey

## A.1 Operationalization of Outcome Variables

I measure anti-immigration attitudes with six survey items included in the ESS (Table 1 of Main Text). I closely follow Cavaillé and Marshall (2018) with only two minor deviations.[1] Cavaillé and Marshall (2018) argue that simply using the raw responses is problematic for two reasons. First, there is likely variation in responses to specific questions because the interpretation of non-extreme categories and reference points varies across countries and time. Second, attitudes toward different types of immigrants cannot be examined separately because opposition can be expressed differently depending on the specific context. In other words, opposition to immigration can come in many different flavors depending on the specific context.

To solve the first issue, Cavaillé and Marshall (2018) recode survey responses as binary variables that are anchored around references points that have similar interpretations across time and countries. For the countries in this study, respondents' answers tend to cluster around 5, which suggests this is interpreted as a key reference point that differentiates opposition from indifference or support. Thus, I create binary variables for each question that equals one if the response is between 0 and 4 and zero otherwise. For questions 4 to 6, Cavaillé and Marshall (2018) create three indicator variables that equal one if respondents answered with "none." They also created an additional three variables equal to one if respondents answered with either "none" or "few" to avoid any potential social desirability bias.

To address the second issue, Cavaillé and Marshall (2018) collapse the six binary vari-

[1]The first deviation is that I do not include a seventh variable that measures closeness to anti-immigrant parties. I do not include this variable for two reasons. First, the definition of an anti-immigrant party varies depending on the country and context. Second, there are some countries with no anti-immigrant party. The second deviation is that I do not standardize within countries because it would cause the interpretation of the results to be dependent on the specific country. Since I gain leverage across countries and time, this is not appropriate.

ables created for questions 4 to 6 into two binary variables that capture whether respondents opposed at least one type of immigrant. Specifically, the first variable equals one if respondents answered "none" to at least one of the three questions. The second variable equals one if respondents answered either "none" or "few" to at least one of the three questions.

Finally, I aggregate across the five binary variables to create an additive scale that captures an individual's latent anti-immigration attitude. The Cronbach's alpha equals 0.78, which suggests that the survey items are tapping into the same underlying latent immigration attitude. Importantly, I test alternative definitions of anti-immigration attitudes to ensure that the main results are not driven by these coding decisions. The results from these robustness checks are substantively similar to the main results (see section A.5).

## A.2 Description of Control Variables

The baseline specification includes several individual level controls that measure demographic information of the respondent and their parents and regional time-varying variables. Table A1 provides a brief description of the pre-treatment controls and mediator variables. Table A2 provides summary statistics for the restricted and full sample.

First, I include several variables about the respondents' parents. I create two variables ranging from 0 to 4 that measure the education levels of respondents' parents. The *Father's Education* (*Mother's Education*) variable equals 0 if the father (mother) completed less than lower secondary education, 1 if the father (mother) completed lower secondary education, 2 if the father (mother) completed upper secondary education, 3 if the father (mother) completed post-secondary non-tertiary education, and 4 if the father (mother) completed tertiary education. The two variables are highly correlated (Spearman's Rank Order Correlation = 0.654). Thus, to reduce potential issues with multi-collinearity, I create an additive variable that ranges from 0 to 8 (*Parental Education*). This variable helps account for the respondent's potential income, skill-level, and cultural values since parental characteristics are often predictive of their children's future prospects. If respondents' parents are highly

Table A1: Description of Control and Mediator Variables

| Variable | Description | Coding |
|---|---|---|
| Female | Gender of respondent | 0-Male; 1-Female |
| Minority | Minority status of the respondent | 0-Not Minority; 1-Minority |
| Parental Citizenship | Whether the respondent's parents are citizens of country | 0-Parents Non-citizens; 1-Both Parents Citizens |
| Parental Education | Sum of father's and mother's educations | Range: 0(min)-8(max) |
| Secondary Educ. | Whether the respondent completed at least 12 years of schooling | 0-Respondent completed less than 12 years of schooling; 1-Respondent completed at least 12 years of schooling |
| Avg. Education | Average years of education in the total population aged 15 years and older | Continuous Measure |
| Educational Quality | The extent that high quality basic education is guaranteed to all. | 0 (high equality) to 5 (low equality) |
| Ideology | Ideology of respondent | 0-Left; 10-Right |
| Income | Measures the respondent's net total income | 1-lowest income bracket; 10-highest income bracket |
| Education Years | Respondent's years of schooling | Continuous variable: 0-56 |

educated, it increases the likelihood respondents have more advanced skill-levels, higher incomes, and more progressive cultural values, all of which suggest a more favorable opinion toward immigration.

I also control for whether respondents' parents were born in the specific country. *Parental Citizenship* is coded as 1 if both parents were born in the respective country and 0 otherwise. This variable also helps account for economic and cultural factors likely to influence immigration attitudes. Children whose parents are of different nationalities are likely to experience different economic experiences and hold different cultural values.

Next, I create several categorical variables that capture the skill-level of the parents' occupations when the respondent was 14 years old. The ESS includes several questions across all waves that measure the employment status and the broad type of work each parent did when the respondent was 14 years old. I use this data to classify each parents' occupation into three broad categories: high, moderate, and low-skilled. Table A3 identifies the occupations assigned to each skill-level category. The ESS slightly changes the categories after Wave 3, which partially influences the assignment of occupations. For example, clerical occupations

Table A2: Summary Statistics

| Variable | Mean | SD | Min | Max | N |
|---|---|---|---|---|---|
| **Restricted Sample** | | | | | |
| Anti-Immigration Attitude | 1.63 | 1.65 | 0.00 | 5.00 | 115184 |
| Economic Shock (18-25) | 0.11 | 0.31 | 0.00 | 1.00 | 122131 |
| Economic Shock Count (18-25) | 0.18 | 0.64 | 0.00 | 6.00 | 122131 |
| Father High-Skilled | 0.16 | 0.37 | 0.00 | 1.00 | 122131 |
| Father Mod-Skilled | 0.45 | 0.50 | 0.00 | 1.00 | 122131 |
| Father Low-Skilled | 0.39 | 0.49 | 0.00 | 1.00 | 122131 |
| Mother High-Skilled | 0.06 | 0.24 | 0.00 | 1.00 | 122131 |
| Mother Mod-Skilled | 0.22 | 0.42 | 0.00 | 1.00 | 122131 |
| Mother Low-Skilled | 0.71 | 0.45 | 0.00 | 1.00 | 122131 |
| Female | 0.51 | 0.50 | 0.00 | 1.00 | 122131 |
| Minority | 0.02 | 0.14 | 0.00 | 1.00 | 122131 |
| Parental Citzenship | 0.94 | 0.24 | 0.00 | 1.00 | 122131 |
| Parental Education | 2.49 | 2.44 | 0.00 | 8.00 | 122131 |
| Age | 52.22 | 15.57 | 26.00 | 102.00 | 122131 |
| Cohort | 1956.79 | 15.95 | 1900.00 | 1991.00 | 122131 |
| Education 12 years or more | 0.63 | 0.48 | 0.00 | 1.00 | 122131 |
| Educational Equality (10-17) | 2.01 | 1.02 | -2.00 | 3.53 | 122131 |
| Education Level (10-17) | 8.96 | 2.26 | 1.84 | 13.17 | 122131 |
| Education Years | 12.77 | 4.39 | 0.00 | 56.00 | 122131 |
| Income | 6.03 | 2.88 | 1.00 | 10.00 | 122131 |
| Ideology | 5.10 | 2.10 | 0.00 | 10.00 | 122131 |
| **Full Sample** | | | | | |
| Anti-Immigration Attitude | 1.69 | 1.66 | 0.00 | 5.00 | 125515 |
| Economic Shock (18-25) | 0.13 | 0.34 | 0.00 | 1.00 | 134465 |
| Economic Shock Count (18-25) | 0.21 | 0.67 | 0.00 | 6.00 | 134465 |
| Father High-Skilled | 0.15 | 0.36 | 0.00 | 1.00 | 134465 |
| Father Mod-Skilled | 0.45 | 0.50 | 0.00 | 1.00 | 134465 |
| Father Low-Skilled | 0.40 | 0.49 | 0.00 | 1.00 | 134465 |
| Mother High-Skilled | 0.07 | 0.25 | 0.00 | 1.00 | 134465 |
| Mother Mod-Skilled | 0.23 | 0.42 | 0.00 | 1.00 | 134465 |
| Mother Low-Skilled | 0.70 | 0.46 | 0.00 | 1.00 | 134465 |
| Female | 0.51 | 0.50 | 0.00 | 1.00 | 134465 |
| Minority | 0.02 | 0.15 | 0.00 | 1.00 | 134465 |
| Parental Citzenship | 0.94 | 0.24 | 0.00 | 1.00 | 134465 |
| Parental Education | 2.55 | 2.41 | 0.00 | 8.00 | 134465 |
| Age | 51.82 | 15.60 | 26.00 | 102.00 | 134465 |
| Cohort | 1957.27 | 16.02 | 1900.00 | 1991.00 | 134465 |
| Education 12 years or more | 0.63 | 0.48 | 0.00 | 1.00 | 134465 |
| Educational Equality (10-17) | 2.00 | 1.00 | -2.00 | 3.53 | 134465 |
| Education Level (10-17) | 8.93 | 2.21 | 1.84 | 13.17 | 134465 |
| Education Years | 12.74 | 4.30 | 0.00 | 56.00 | 134465 |
| Income | 6.05 | 2.90 | 1.00 | 10.00 | 134465 |
| Ideology | 5.12 | 2.14 | 0.00 | 10.00 | 134465 |

had its own category for Waves 4 through 8 but was combined with intermediate occupations for Waves 1 through 3.

This measure is certainly a rough approximation of parental skill-level when the respondent was 14 years old. Importantly, the results from the main analysis provide evidence that these categories are appropriate (see Figure A2). Respondents whose parents held lower-skilled occupations were more likely to hold anti-immigration attitudes.

Second, I include several control variables that measure demographic information of the respondent. I control for gender and minority status. *Female* is coded as 1 if the respondent is a female and 0 if the respondent is a male. The previous literature typically finds a gender divide on immigration. *Minority* equals 1 if the respondent indicated he/she belongs to a minority ethnic group in the specific country and 0 otherwise. If a respondent belongs to a minority group, they are likely to be more favorable to immigration. Finally, I include a binary measure that captures whether the respondent completed 12 or more years of schooling. While there is a small chance this variable is capturing education that occurs post-treatment, this is unlikely given 12 years is the approximate years of schooling to complete secondary education. Further, including controls that capture some element of the respondent's education before their impressionable years is critical to eliminate confounding explanations. Importantly, even when treating education as a post-treatment mediator, the results indicate that an economic shock during young adulthood causes an increase in anti-immigration attitudes (See Section 4.4 in the main text).

Finally, the baseline specification includes controls for the country's average level of education and the educational equality. The measure for education level is from the Vdem dataset (e_peaveduc), which measures the average years of education among citizens older than 15 (Coppedge et al., 2021). The measure for educational equality is from the Vdem dataset (v2peedueq) (Coppedge et al., 2021). The measures captures the extent that high quality basic education is guaranteed in the country for a given year. Both variables are averages for when the respondent was between the ages of 10 and 17. This ensures they are

Table A3: Coding of Parental Skill Categories

| Skill-level | Occupation | Description used in survey |
|---|---|---|
| **Waves 4-8** | | |
| High-skilled | Professional and technical | such as: doctor – teacher – engineer– artist – accountant |
| | Higher administrator | such as: banker – executive in big business – high government official – union official |
| Mod-skilled | Clerical | such as: secretary – clerk – office manager – book keeper |
| | Sales | such as: sales manager – shop owner – shop assistant – insurance agent |
| | Skilled worker | such as: foreman – motor mechanic – printer – tool and die maker – electrician |
| | Farm worker | such as: farmer – farm labourer– tractor driver– fisherman |
| Low-skilled | Unemployed | Unemployed or deceased/not around |
| | Service | such as: restaurant owner – police officer – waiter – caretaker – barber – armed forces |
| | Semi-skilled worker | such as: bricklayer – bus driver – cannery worker – carpenter – sheet metal worker – baker |
| | Unskilled worker | such as: labourer – porter – unskilled factory worker |
| **Waves 1-3** | | |
| High-skilled | Traditional professional | such as: accountant – solicitor – medical practitioner – scientist – civil/mechanical engineer |
| | Modern professional | such as: teacher – nurse – physiotherapist – social worker – welfare officer – artist – musician – police officer (sergeant or above) – software designer |
| | Senior manager or administrators | (usually responsible for planning, organising and co-ordinating work and for finance) such as: finance manager – chief executive |
| Mod-skilled | Clerical and intermediate | such as: secretary – personal assistant – clerical worker – office clerk – call centre agent7 – nursing auxiliary – nursery nurse |
| | Technical and craft | such as: motor mechanic – fitter – inspector – plumber – printer – tool maker – electrician – gardener – train driver |
| | Middle or junior managers | such as: office manager – retail manager – bank manager – restaurant manager – warehouse manager – publican |
| Low-skilled | Unemployed | Unemployed or deceased/not around |
| | Semi-routine manual and service | such as: postal worker – machine operative – security guard – caretaker – farm worker – catering assistant – receptionist – sales assistant |
| | Routine manual and service | such as: HGV8 driver – van driver – cleaner – porter – packer – sewing machinist – messenger – labourer – waiter/waitress – bar staff |

before the impressionable years.

## A.3 Alternative Definitions of the Economic Shocks

In this section, I report the results from a series of additional models that use alternative definitions of economic shocks (Table A4). First, I show the main results are robust when only analyzing the post-WWII era. Second, I show the main results are robust when excluding adults socialized during the 1960s and 1970s. Third, I use different definitions for the pre-WWII and post-WWII periods. Specifically, I use 5.7 (2.6) for the pre-WWII (post-WWII) period.[2] Finally, similar to Barro and Ursúa (2008), I define an economic shock when the contraction in GDPpc from peak to trough exceeds 4 percent. Across the models, the results are similar to the main analysis. These results along with the results reported in Section 4.2 demonstrate the main findings are robust to alternative definitions of an economic shock.

## A.4 Alternative Definitions of the Impressionable Years

Given differences in mental and physical development, the impressionable years is difficult to pinpoint and may vary. The evidence reported in the main text suggests that the impressionable period may be longer. In Table A5, I report the results when using alternative definitions of the impressionable years. Three points are worth emphasizing. First, the main results are robust to different specifications of the impressionable years. Second, the results suggests that the time period between 18 and 25 is the most important for the socialization process. Finally, while Figure 2 in the main text provides some evidence that the period between 26 and 33 might be consequential, it appears these results are not robust to alternative specifications.

## A.5 Alternative DV Specifications

The following analysis shows that alternative definitions of the dependent variable produce similar results to the main analysis. I use four alternative approaches. First, I use the first factor the components for the main dependent variable. Second, I exclude each survey

[2]This corresponds to the average percentage GDPpc contraction for each period

Table A4: Results when excluding pre-WWII adults and adults socialized during the 1960s and 1970s

| | Estimate | 95% C.I. | N | Sample |
|---|---|---|---|---|
| **Excluding pre-WWII adults** | | | | |
| Economic Shock | 0.113 | (0.064, 0.162) | 103774 | Restricted |
| Economic Shock Count | 0.102 | (0.042, 0.162) | 103774 | Restricted |
| Economic Shock | 0.078 | (0.034, 0.123) | 113583 | Full |
| Economic Shock Count | 0.069 | (0.019, 0.119) | 113583 | Full |
| **Excluding adults socialized in 1960s and 1970s** | | | | |
| Economic Shock | 0.109 | ( 0.055, 0.163) | 74000 | Restricted |
| Economic Shock Count | 0.078 | ( 0.018, 0.138) | 74000 | Restricted |
| Economic Shock | 0.064 | ( 0.015, 0.114) | 81163 | Full |
| Economic Shock Count | 0.047 | (-0.005, 0.099) | 81163 | Full |
| **Different definitions for pre/post WWII** | | | | |
| Economic Shock | 0.074 | ( 0.031, 0.117) | 115184 | Restricted |
| Economic Shock Count | 0.063 | ( 0.012, 0.114) | 115184 | Restricted |
| Economic Shock | 0.049 | ( 0.009, 0.088) | 125515 | Full |
| Economic Shock Count | 0.043 | (-0.002, 0.087) | 125515 | Full |
| **4% GDPpc contraction from peak to trough** | | | | |
| Economic Shock | 0.096 | (0.057, 0.135) | 115184 | Restricted |
| Economic Shock Count | 0.073 | (0.041, 0.106) | 115184 | Restricted |
| Economic Shock | 0.066 | (0.030, 0.102) | 125515 | Full |
| Economic Shock Count | 0.055 | (0.025, 0.086) | 125515 | Full |

Standard errors clustered at the country-cohort level. Models include pre-treatment controls, cohort fixed effects, survey wave fixed effects, and country fixed effects.

item individually from the scale. Third, I estimate models using each separate survey item. Finally, I examine several alternative scale constructions. Importantly, as Cavaillé and Marshall (2018) note, these are not preferred to the main analysis due to measurement error and social desirability bias.

### A.5.1 Factor Analysis

The first alternative dependent variable that I examine is using the first factor of a factor analysis of the five binary variables used for the main dependent variable. The measure has range from -0.859 to 1.751 with a standard deviation of 0.894. The results are reported in

Table A5: Alternative Definitions of the Impressionable Years

| | **Estimate** | **95% C.I.** | **N** | **Sample** |
|---|---|---|---|---|
| Economic Shock (10-25) | 0.102 | (0.063, 0.140) | 115184 | Restricted |
| Economic Shock (14-25) | 0.116 | (0.075, 0.157) | 115184 | Restricted |
| Economic Shock (16-25) | 0.113 | (0.069, 0.157) | 115184 | Restricted |
| Economic Shock (17-25) | 0.100 | (0.054, 0.146) | 115184 | Restricted |
| Economic Shock (19-26) | 0.102 | (0.056, 0.147) | 115184 | Restricted |
| Economic Shock (18-21) | 0.103 | (0.047, 0.159) | 115184 | Restricted |
| Economic Shock (22-25) | 0.100 | (0.041, 0.159) | 115184 | Restricted |
| Economic Shock (26-29) | 0.021 | (-0.032, 0.075) | 115184 | Restricted |
| Economic Shock (30-33) | 0.062 | (0.005, 0.118) | 115184 | Restricted |
| Economic Shock Count (10-25) | 0.102 | (0.060, 0.145) | 115184 | Restricted |
| Economic Shock Count (14-25) | 0.110 | (0.064, 0.156) | 115184 | Restricted |
| Economic Shock Count (16-25) | 0.099 | (0.049, 0.149) | 115184 | Restricted |
| Economic Shock Count (17-25) | 0.090 | (0.038, 0.141) | 115184 | Restricted |
| Economic Shock Count (19-26) | 0.103 | (0.052, 0.155) | 115184 | Restricted |
| Economic Shock Count (18-21) | 0.095 | (0.026, 0.164) | 115184 | Restricted |
| Economic Shock Count (22-25) | 0.090 | (0.024, 0.155) | 115184 | Restricted |
| Economic Shock Count (26-29) | 0.028 | (-0.031, 0.088) | 115184 | Restricted |
| Economic Shock Count (30-33) | 0.070 | (0.006, 0.134) | 115184 | Restricted |
| Economic Shock (10-25) | 0.079 | (0.044, 0.113) | 124812 | Full |
| Economic Shock (14-25) | 0.081 | (0.044, 0.118) | 125204 | Full |
| Economic Shock (16-25) | 0.073 | (0.034, 0.112) | 125389 | Full |
| Economic Shock (17-25) | 0.066 | (0.025, 0.107) | 125452 | Full |
| Economic Shock (19-26) | 0.061 | (0.019, 0.102) | 125515 | Full |
| Economic Shock (18-21) | 0.060 | (0.008, 0.113) | 125515 | Full |
| Economic Shock (22-25) | 0.083 | (0.031, 0.135) | 125515 | Full |
| Economic Shock (26-29) | 0.009 | (-0.040, 0.058) | 125515 | Full |
| Economic Shock (30-33) | 0.065 | (0.014, 0.116) | 125515 | Full |
| Economic Shock Count (10-25) | 0.075 | (0.038, 0.112) | 124812 | Full |
| Economic Shock Count (14-25) | 0.076 | (0.037, 0.116) | 125204 | Full |
| Economic Shock Count (16-25) | 0.065 | (0.022, 0.107) | 125389 | Full |
| Economic Shock Count (17-25) | 0.059 | (0.014, 0.103) | 125452 | Full |
| Economic Shock Count (19-26) | 0.067 | (0.022, 0.111) | 125515 | Full |
| Economic Shock Count (18-21) | 0.058 | (-0.004, 0.120) | 125515 | Full |
| Economic Shock Count (22-25) | 0.073 | (0.015, 0.131) | 125515 | Full |
| Economic Shock Count (26-29) | 0.010 | (-0.044, 0.064) | 125515 | Full |
| Economic Shock Count (30-33) | 0.068 | (0.010, 0.126) | 125515 | Full |

Standard errors clustered at the country-cohort level. Models include pre-treatment controls, cohort fixed effects, survey wave fixed effects, and country fixed effects.

Table A6: Using the first factor to compute the anti-immigration attitudes scale.

| | Estimate | 95% C.I. | N | Sample |
|---|---|---|---|---|
| Economic Shock | 0.062 | (0.037, 0.086) | 115184 | Restricted |
| Economic Shock Count | 0.056 | (0.027, 0.084) | 115184 | Restricted |
| Economic Shock | 0.043 | (0.021, 0.066) | 125515 | Full |
| Economic Shock Count | 0.039 | (0.014, 0.064) | 125515 | Full |

Standard errors clustered at the country-cohort level. Models include pre-treatment controls, cohort fixed effects, survey wave fixed effects, and country fixed effects.

Table A6 and are substantively similar to the results in the main analysis. For instance, for the binary measure and restricted sample, growing up in a recession causes about 2.4 percentage point increase in anti-immigration attitudes, which is about 0.069 standard deviation change in the measure.

### A.5.2 Item-by-Item Exclusion From DV

A potential concern is that the results in the main analysis are caused by a single survey item. I assess this possibility by estimating the models while excluding each survey item from the scale. The results are reported in Table A7. The results suggest that none of the individual items are driving the main results. Specifically, all estimates are still positive and statistically significant ($p < 0.05$). Thus, it does not appear that the results are driven by a particular item in the scale.

### A.5.3 Separately Testing Each Survey Item

Table A8 reports the results of models when the dependent variable is each separate survey item on its original scale. The results are substantively similar to the main analysis. Of the 24 estimates, only four have 95 percent confidence intervals that include values below zero. Importantly, as noted in Section 4.3 of the main text, the insignificant estimates are for questions about immigrants of the same race and the economic consequences of immigration. One interpretation of these results, is that growing up in a recession causes a more xenophobic response that focuses on the racial and ethnic dimensions of immigration. See Section 4.3

Table A7: Excluding each item separately.

| | Estimate | 95% C.I. | N | Sample |
|---|---|---|---|---|
| **Excluding anti-immigration ("none" only)** | | | | |
| Economic Shock | 0.087 | (0.050, 0.124) | 115184 | Restricted |
| Economic Shock Count | 0.078 | (0.034, 0.122) | 115184 | Restricted |
| Economic Shock | 0.064 | (0.029, 0.098) | 125515 | Full |
| Economic Shock Count | 0.055 | (0.017, 0.093) | 125515 | Full |
| **Excluding anti-immigration ("none" or "few")** | | | | |
| Economic Shock | 0.102 | (0.063, 0.142) | 115184 | Restricted |
| Economic Shock Count | 0.093 | (0.048, 0.137) | 115184 | Restricted |
| Economic Shock | 0.071 | (0.035, 0.106) | 125515 | Full |
| Economic Shock Count | 0.065 | (0.027, 0.104) | 125515 | Full |
| **Excluding immigration is bad for the economy** | | | | |
| Economic Shock | 0.100 | (0.063, 0.137) | 116481 | Restricted |
| Economic Shock Count | 0.091 | (0.049, 0.133) | 116481 | Restricted |
| Economic Shock | 0.068 | (0.035, 0.102) | 126985 | Full |
| Economic Shock Count | 0.064 | (0.027, 0.100) | 126985 | Full |
| **Excluding immigration undermines culture** | | | | |
| Economic Shock | 0.091 | (0.053, 0.129) | 116049 | Restricted |
| Economic Shock Count | 0.084 | (0.040, 0.128) | 116049 | Restricted |
| Economic Shock | 0.058 | (0.024, 0.092) | 126716 | Full |
| Economic Shock Count | 0.054 | (0.016, 0.091) | 126716 | Full |
| **Excluding immigration reduces quality of life** | | | | |
| Economic Shock | 0.092 | (0.056, 0.128) | 116017 | Restricted |
| Economic Shock Count | 0.081 | (0.039, 0.123) | 116017 | Restricted |
| Economic Shock | 0.063 | (0.030, 0.095) | 126681 | Full |
| Economic Shock Count | 0.055 | (0.018, 0.091) | 126681 | Full |

Standard errors clustered at the country-cohort level. Models include pre-treatment controls, cohort fixed effects, survey wave fixed effects, and country fixed effects.

in the main text for additional discussion.

### A.5.4 Alternative Latent Anti-Immigration Specifications

I examine three alternative approaches to capture respondents' latent anti-immigration attitudes. First, rather than coding questions 4 to 6 equal to one if the respondent answered

Table A8: Testing each item separately.

| | Estimate | 95% C.I. | N | Sample |
|---|---|---|---|---|
| **Same Race** | | | | |
| Economic Shock | 0.043 | (0.020, 0.067) | 120164 | Restricted |
| Economic Shock Count | 0.048 | (0.022, 0.073) | 120164 | Restricted |
| Economic Shock | 0.020 | (-0.001, 0.042) | 132046 | Full |
| Economic Shock Count | 0.021 | (-0.002, 0.045) | 132046 | Full |
| **Different Race** | | | | |
| Economic Shock | 0.066 | (0.043, 0.090) | 120168 | Restricted |
| Economic Shock Count | 0.060 | (0.033, 0.087) | 120168 | Restricted |
| Economic Shock | 0.035 | (0.014, 0.056) | 132038 | Full |
| Economic Shock Count | 0.034 | (0.009, 0.058) | 132038 | Full |
| **Poorer Countries** | | | | |
| Economic Shock | 0.056 | (0.032, 0.080) | 119913 | Restricted |
| Economic Shock Count | 0.054 | (0.028, 0.081) | 119913 | Restricted |
| Economic Shock | 0.031 | (0.010, 0.052) | 131712 | Full |
| Economic Shock Count | 0.030 | (0.007, 0.053) | 131712 | Full |
| **Economy** | | | | |
| Economic Shock | 0.084 | (0.020, 0.148) | 119741 | Restricted |
| Economic Shock Count | 0.079 | (0.012, 0.146) | 119741 | Restricted |
| Economic Shock | 0.035 | (-0.023, 0.094) | 131409 | Full |
| Economic Shock Count | 0.023 | (-0.040, 0.086) | 131409 | Full |
| **Cultural Life** | | | | |
| Economic Shock | 0.111 | (0.051, 0.172) | 120288 | Restricted |
| Economic Shock Count | 0.107 | (0.047, 0.166) | 120288 | Restricted |
| Economic Shock | 0.088 | (0.033, 0.142) | 131732 | Full |
| Economic Shock Count | 0.083 | (0.027, 0.139) | 131732 | Full |
| **Quality of Life** | | | | |
| Economic Shock | 0.134 | (0.072, 0.195) | 120323 | Restricted |
| Economic Shock Count | 0.128 | (0.062, 0.194) | 120323 | Restricted |
| Economic Shock | 0.078 | (0.022, 0.135) | 131729 | Full |
| Economic Shock Count | 0.072 | (0.013, 0.132) | 131729 | Full |

Standard errors clustered at the country-cohort level. Models include pre-treatment controls, cohort fixed effects, survey wave fixed effects, and country fixed effects.

Table A9: Alternative Latent Immigration Attitudes.

| | Estimate | 95% C.I. | N | Sample |
|---|---|---|---|---|
| **"None" and "None"/"Few" to All Three Questions** | | | | |
| Economic Shock | 0.095 | (0.052, 0.137) | 115184 | Restricted |
| Economic Shock Count | 0.087 | (0.037, 0.137) | 115184 | Restricted |
| Economic Shock | 0.065 | (0.026, 0.104) | 125515 | Full |
| Economic Shock Count | 0.056 | (0.013, 0.099) | 125515 | Full |
| **Sum of Dichotomous Measures for Each Question** | | | | |
| Economic Shock | 0.106 | (0.059, 0.153) | 115184 | Restricted |
| Economic Shock Count | 0.099 | (0.039, 0.158) | 115184 | Restricted |
| Economic Shock | 0.079 | (0.035, 0.122) | 125515 | Full |
| Economic Shock Count | 0.069 | (0.018, 0.120) | 125515 | Full |
| **Sum of Questions Using Original Survey Scales (Standardized)** | | | | |
| Economic Shock | 0.055 | (0.033, 0.077) | 115184 | Restricted |
| Economic Shock Count | 0.054 | (0.030, 0.079) | 115184 | Restricted |
| Economic Shock | 0.032 | (0.012, 0.052) | 125515 | Full |
| Economic Shock Count | 0.030 | (0.007, 0.052) | 125515 | Full |

Standard errors clustered at the country-cohort level. Models include pre-treatment controls, cohort fixed effects, survey wave fixed effects, and country fixed effects.

"none" or "none/few" for at least one type of immigrant, I code the two variables equal to one if the respondent answered with "none" or "none/few" to *all* three types of immigrants. Like the dependent variable used in the main analysis, I aggregate across the five binary variables to create the dependent variable. Second, I code all six survey items as binary variables and then take the sum. Specifically, for questions 1 to 3, I code each variable equal to one if a respondent's answers were between 0 and 4 and zero otherwise. For questions 4 to 6, I code each variable equal to one if the respondent answered "none" and zero otherwise. To create the dependent variable, I then aggregate across the six binary variables. For the final alternative approach, I create a dependent variable that is the average of individuals' standardized responses to the six survey items. The measure has a range between -1.821 and 2.073 with a standard deviation of 0.799. The results are reported in Table A9. Overall, the main results are robust to these alternative specifications. Substantively, the effect sizes

Table A10: Country-cohort analysis

| | Estimate | 95% C.I. | N | Cohort FE | Sample |
|---|---|---|---|---|---|
| Economic Shock | 0.113 | (0.076, 0.151) | 1330 | No | Restricted |
| Economic Shock | 0.092 | (0.043, 0.141) | 1330 | Yes | Restricted |
| Economic Shock Count | 0.121 | (0.080, 0.161) | 1330 | No | Restricted |
| Economic Shock Count | 0.083 | (0.028, 0.139) | 1330 | Yes | Restricted |
| Economic Shock | 0.106 | (0.070, 0.141) | 1483 | No | Full |
| Economic Shock | 0.064 | (0.019, 0.109) | 1483 | Yes | Full |
| Economic Shock Count | 0.110 | (0.073, 0.147) | 1483 | No | Full |
| Economic Shock Count | 0.062 | (0.014, 0.110) | 1483 | Yes | Full |

Standard errors clustered at the cohort level. Estimates are from WLS regressions with the country-cohort sample size (appropriately weighted by the sampling weights) as weights. Country fixed effects, average level of education, and educational equality are included in all models.

are also similar. For example, for the binary measure for the restricted sample when using the standardized responses (bottom panel), growing up in a recession causes about a 1.4 percentage point increase in anti-immigration attitudes, which is about a 0.069 standard deviation change in the measure.

## A.6 Country-Cohort Analysis

Since variation in economic shocks only occurs at the country-cohort level, an alternative empirical strategy is to aggregate individual responses to the country-cohort level and estimate models with weighted least squares (WLS) regression with the within country-cohort sample size as weights. Table A10 reports the results from these WLS models. The estimated coefficients are positive, statistically significant, and similar to the main analysis. For instance, for the model specification estimated in Row (2) of Table A10, growing up in a recession causes about a 1.8 percentage point increase in anti-immigration attitudes, which is about a 0.11 standard deviation change in the dependent variable.

## A.7 Country-by-Country Exclusion

To ensure the results are not driven by a specific country, I estimate separate models while excluding each country. The results are reported in Table A11. The results suggest no single

Table A11: Results when excluding each country separately

| | Estimate | 95% C.I. | N |
|---|---|---|---|
| **Economic Shock (Restricted)** | | | |
| Excluding Austria | 0.119 | (0.073, 0.164) | 110243 |
| Excluding Belgium | 0.116 | (0.069, 0.163) | 107191 |
| Excluding Denmark | 0.121 | (0.075, 0.166) | 108066 |
| Excluding Finland | 0.103 | (0.050, 0.157) | 103557 |
| Excluding France | 0.108 | (0.062, 0.153) | 106592 |
| Excluding Germany | 0.123 | (0.077, 0.169) | 103379 |
| Excluding Greece | 0.117 | (0.068, 0.165) | 110573 |
| Excluding Iceland | 0.122 | (0.075, 0.169) | 113834 |
| Excluding Italy | 0.119 | (0.074, 0.165) | 113393 |
| Excluding Netherlands | 0.115 | (0.068, 0.161) | 105642 |
| Excluding Norway | 0.124 | (0.078, 0.171) | 105872 |
| Excluding Portugal | 0.098 | (0.051, 0.145) | 110245 |
| Excluding Russia | 0.112 | (0.065, 0.159) | 111504 |
| Excluding Spain | 0.121 | (0.076, 0.167) | 108930 |
| Excluding Sweden | 0.121 | (0.074, 0.168) | 108966 |
| Excluding Switzerland | 0.147 | (0.095, 0.199) | 108113 |
| Excluding Uk | 0.118 | (0.072, 0.164) | 106844 |
| **Economic Shock (Full)** | | | |
| Excluding Austria | 0.082 | (0.040, 0.123) | 120574 |
| Excluding Belgium | 0.079 | (0.036, 0.121) | 117522 |
| Excluding Bulgaria | 0.098 | (0.054, 0.142) | 122920 |
| Excluding Czech | 0.087 | (0.046, 0.127) | 123298 |
| Excluding Denmark | 0.084 | (0.043, 0.126) | 118397 |
| Excluding Estonia | 0.082 | (0.040, 0.124) | 125003 |
| Excluding Finland | 0.062 | (0.016, 0.108) | 113888 |
| Excluding France | 0.070 | (0.029, 0.112) | 116923 |
| Excluding Germany | 0.083 | (0.041, 0.125) | 113710 |
| Excluding Greece | 0.078 | (0.034, 0.122) | 120904 |
| Excluding Hungary | 0.089 | (0.045, 0.132) | 120508 |
| Excluding Iceland | 0.083 | (0.041, 0.126) | 124165 |
| Excluding Italy | 0.082 | (0.040, 0.123) | 123724 |
| Excluding Netherlands | 0.081 | (0.040, 0.123) | 115973 |
| Excluding Norway | 0.085 | (0.043, 0.127) | 116203 |
| Excluding Portugal | 0.063 | (0.020, 0.106) | 120576 |
| Excluding Russia | 0.072 | (0.030, 0.115) | 121835 |
| Excluding Spain | 0.084 | (0.042, 0.125) | 119261 |
| Excluding Sweden | 0.082 | (0.040, 0.125) | 119297 |
| Excluding Switzerland | 0.097 | (0.051, 0.143) | 118444 |
| Excluding Uk | 0.081 | (0.040, 0.123) | 117175 |

Standard errors clustered at the country-cohort level. Models include pre-treatment controls, cohort fixed effects, survey wave fixed effects, and country fixed effects.

country is driving the estimated effects in the main analysis, which provides evidence that the effect of an economic shock occurs in each country.

Table A12: Results when excluding each ESS round separately

| | Estimate | 95% C.I. | N |
|---|---|---|---|
| **Economic Shock (Restricted)** | | | |
| Excluding Round 1 | 0.120 | (0.072, 0.169) | 99922 |
| Excluding Round 2 | 0.116 | (0.066, 0.166) | 99709 |
| Excluding Round 3 | 0.126 | (0.076, 0.175) | 99064 |
| Excluding Round 4 | 0.120 | (0.072, 0.169) | 100672 |
| Excluding Round 5 | 0.119 | (0.070, 0.168) | 102184 |
| Excluding Round 6 | 0.123 | (0.077, 0.168) | 102216 |
| Excluding Round 7 | 0.125 | (0.077, 0.172) | 101654 |
| Excluding Round 8 | 0.093 | (0.044, 0.141) | 100867 |
| **Economic Shock (Full)** | | | |
| Excluding Round 1 | 0.088 | (0.043, 0.132) | 109456 |
| Excluding Round 2 | 0.069 | (0.024, 0.114) | 109054 |
| Excluding Round 3 | 0.086 | (0.041, 0.130) | 108151 |
| Excluding Round 4 | 0.080 | (0.036, 0.123) | 110052 |
| Excluding Round 5 | 0.091 | (0.047, 0.134) | 110281 |
| Excluding Round 6 | 0.091 | (0.047, 0.134) | 110248 |
| Excluding Round 7 | 0.086 | (0.043, 0.129) | 111610 |
| Excluding Round 8 | 0.063 | (0.019, 0.107) | 109753 |

Standard errors clustered at the country-cohort level. Models include pre-treatment controls, cohort fixed effects, survey wave fixed effects, and country fixed effects.

## A.8 Round-by-Round Exclusion

To ensure the results are not driven by a specific ESS round, I estimate models while excluding each round. Table A12 reports the results. Overall, the results suggest no single survey round is driving the estimated effects in the main analysis, which provides evidence that the effect of an economic shock does not depend on the specific contemporary context.

## A.9 Including Additional Regional Time-Varying Controls

Table A13 reports the results when including additional regional time-varying controls. Specifically, the models include controls for democracy (Coppedge et al., 2021), a measure for resource inequality (Coppedge et al., 2021), GDP, and an interaction between age and country fixed effects. The measures for democracy, resource inequality, and GDP are

Table A13: Results when including measures for democracy, resource inequality, GDP, and country age trends.

| | **Estimate** | **95% C.I.** | **N** | **Sample** |
|---|---|---|---|---|
| Economic Shock | 0.110 | (0.062, 0.158) | 114133 | Restricted |
| Economic Shock Count | 0.113 | (0.057, 0.170) | 114133 | Restricted |
| Economic Shock | 0.098 | (0.052, 0.143) | 123867 | Full |
| Economic Shock Count | 0.111 | (0.059, 0.162) | 123867 | Full |

Standard errors clustered at the country-cohort level. Models include pre-treatment controls, additional regional time-varying controls (democracy, resource inequality, and GDP), cohort fixed effects, survey wave fixed effects, country fixed effects, and an interaction between age and country fixed effects.

averages when the respondent was between the ages of 10 and 17. The results are similar to the main analysis. Moreover, the estimated effects when using the full sample increase and are closer to the effects for the restricted sample. This makes sense considering that countries with incomplete GDPpc data (Bulgaria, Croatia, the Czech Republic, Estonia, and Slovenia) have very different economic and political histories compared to the mainly Western European countries that have complete economic data. Overall, these results provide evidence that omitted regional time-varying factors are not driving the results in the main analysis.

## A.10 Effect of Current Economic Conditions on Immigration Attitudes

To provide a benchmark for the effect of current economic conditions on immigration attitudes, I estimate models that include the change in GDP per capita the year before the survey was conducted. The results are reported in Table A14. The estimated effects are in the right direction, negative growth corresponds to an increase in anti-immigration attitudes, but are not statistically significant ($p > 0.10$). The estimated effects suggest that a two standard deviation decrease in $\Delta GDPpc_{y-1}$ increases anti-immigration attitudes by 0.038. This roughly equates to a 0.79 percentage point increase or a 0.02 standard deviation

Table A14: Effect of current economic conditions on immigration attitudes.

| | **Estimate** | **90% C.I.** | **N** | **Sample** |
|---|---|---|---|---|
| $\Delta GDPpc_{y-1}$ | -0.720 | (-1.524, 0.084) | 115184 | Restricted |
| $\Delta GDPpc_{y-1}$ | -0.729 | (-1.518, 0.060) | 125515 | Full |

Standard errors clustered at the country-cohort level. Models include Economic Shock, pre-treatment controls, cohort fixed effects, survey wave fixed effects, and country fixed effects.

Table A15: Results when including individuals 25 and younger.

| | **Estimate** | **95% C.I.** | **N** | **Sample** |
|---|---|---|---|---|
| Economic Shock | 0.109 | (0.066, 0.151) | 128004 | Restricted |
| Economic Shock Count | 0.112 | (0.062, 0.162) | 128004 | Restricted |
| Economic Shock | 0.093 | (0.053, 0.132) | 140460 | Full |
| Economic Shock Count | 0.091 | (0.047, 0.135) | 140460 | Full |

Standard errors clustered at the country-cohort level. Models include pre-treatment controls, cohort fixed effects, survey wave fixed effects, and country fixed effects.

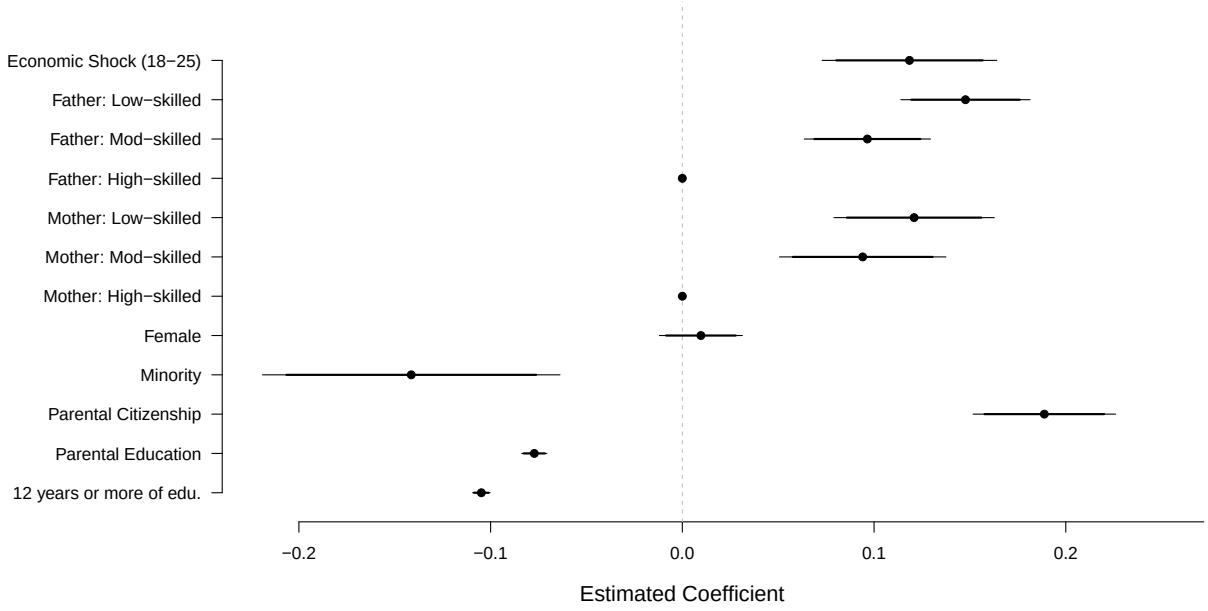
change in the immigration index. When comparing the estimated effects from Table 2 in the main text, the effect of an economic shock during the impressionable years on immigration attitudes is 3 times larger.

## A.11 Additional Results from the Main Analysis

In this section, I report a series of additional results referenced in the main text. Table A15 reports the results when including individuals younger than 26. The estimated effects are similar to those from the main analysis.

Figure A2 reports the estimated effects of Economic Shock and the controls from Row (1) of Table 2. For easier comparison, the effect of *12 years or more of edu.* is divided by 6. A few points are worth emphasizing. First, consistent with the previous literature, education has the largest effect on immigration attitudes. Obtaining 12 or more years of education decreases anti-immigration attitudes by about 12 percentage points. Further, higher levels of parental education also decrease anti-immigration attitudes. Second, the estimated coefficients of parental skill-level are in the expected directions. Respondents

Figure A2: Estimated coefficients of economic shock (18-25) and main control variables from Row (1) of Table 2.

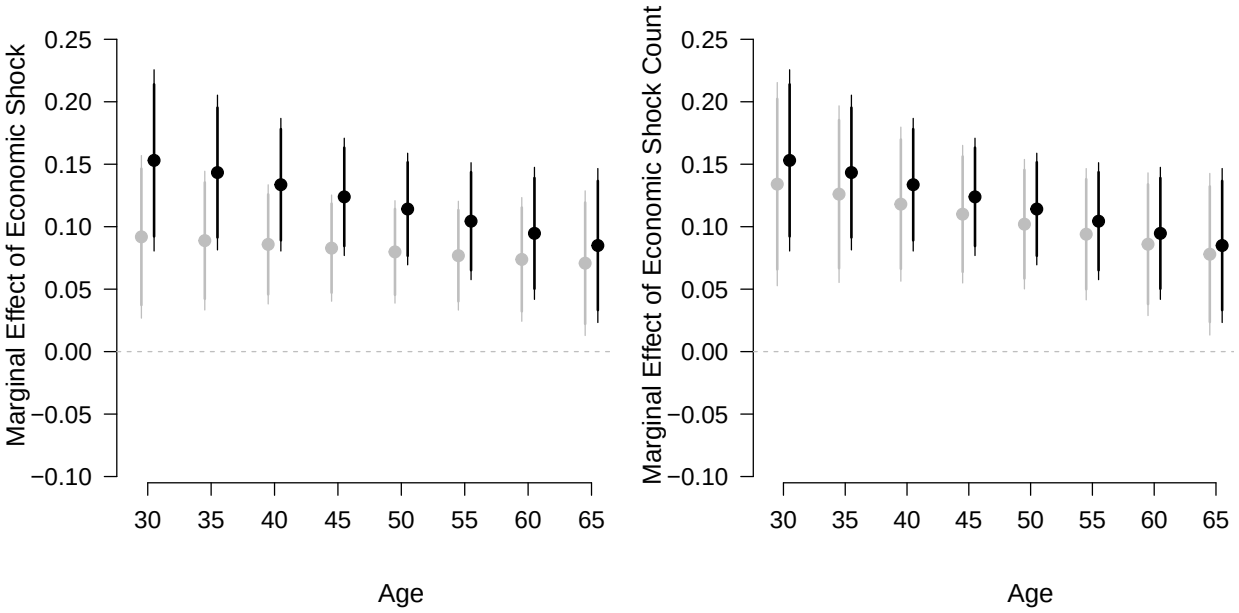



Horizontal lines are 95 and 90 percent confidence intervals with standard errors clustered at the country-cohort level. Model includes pre-treatment covariates, cohort fixed effects, survey wave fixed effects, and country fixed effects. The estimated effect for "12 years or more of edu" is divided by 6 for easier comparison.

whose parents were employed in lower skilled occupations when they were 14 years old have higher levels of anti-immigration attitudes. Specifically, respondents whose fathers were employed in low-skilled (mod-skilled) occupations have anti-immigration attitudes that are about 3 (2) percentage points higher compared to respondents whose fathers were employed in high-skilled occupations. These results provide additional evidence that the economic context when respondents grew up are important. Finally, the estimated effect of growing up in a recession is about one-fifth of the effect of education. It is important to emphasize the estimation strategy is very demanding and likely a lower-bound on the effect of growing up during a recession.

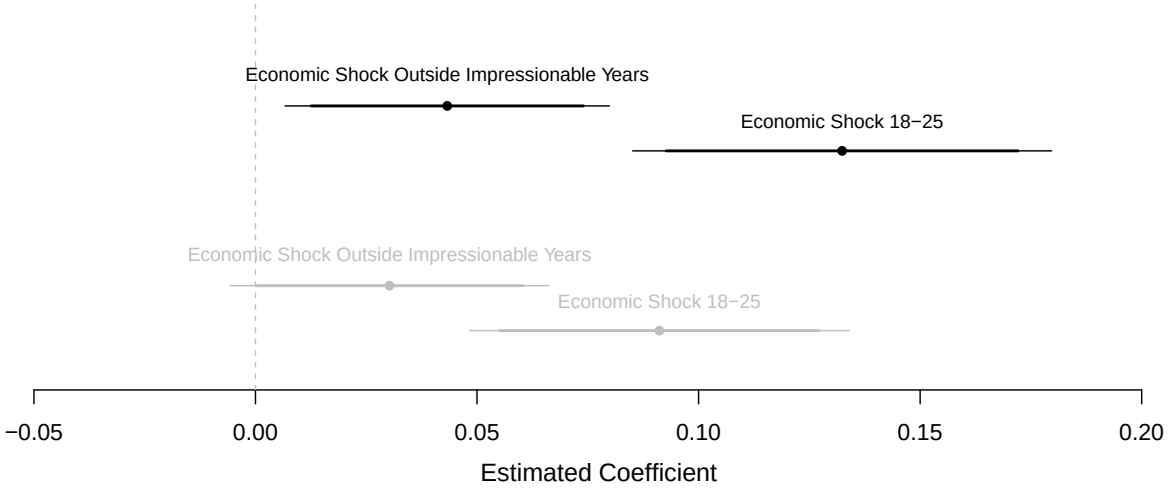
Figure A3 reports the marginal effect of an economic shock across respondents' age. The interaction terms between economic shock and age are insignificant across the models. This result demonstrates that the effect of an economic shock does not decrease as respondents

Figure A3: The marginal effect of an economic shock during the impressionable years (18-25) at different ages.



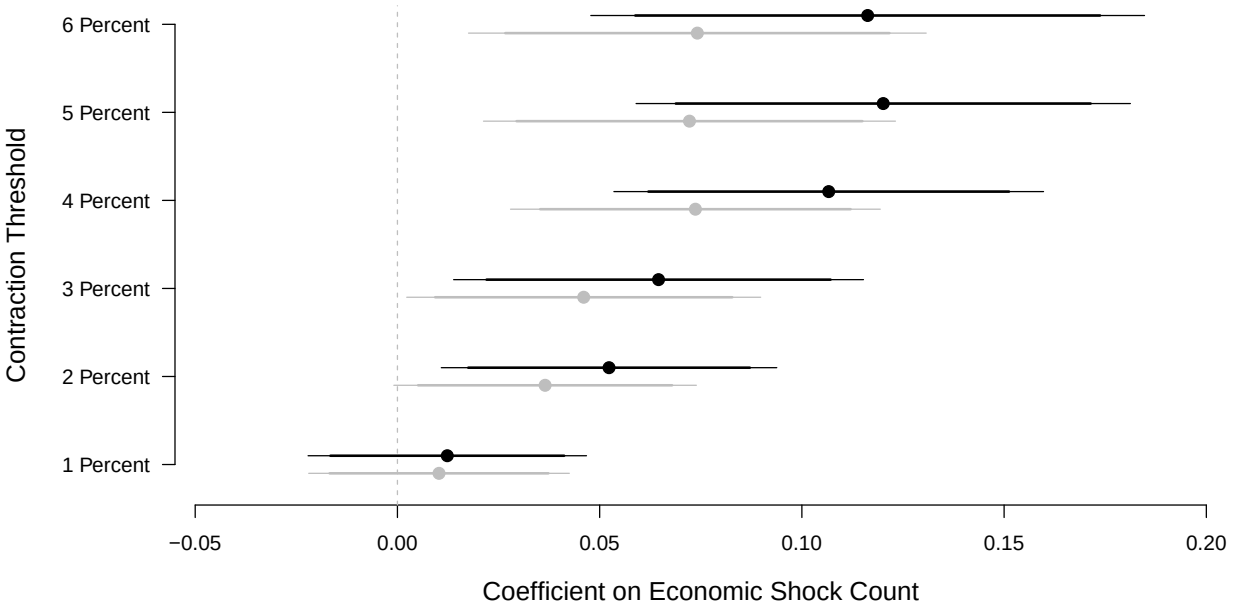

Horizontal lines are 95 and 90 percent confidence intervals with standard errors clustered at the country-cohort level. Estimates in black (gray) are based on the restricted (full) sample. Model includes pre-treatment covariates, cohort fixed effects, survey wave fixed effects, and country fixed effects.

Figure A4: The effect of an economic shock during the impressionable years (18-25) and an economic shock outside of the impressionable years.




Horizontal lines are 95 and 90 percent confidence intervals with standard errors clustered at the country-cohort level. Estimates in black (gray) are based on the restricted (full) sample. Model includes pre-treatment covariates, cohort fixed effects, survey wave fixed effects, and country fixed effects.

Figure A5: Effect of an economic shock count during the impressionable years (18-25) on immigration attitudes using alternative contraction thresholds.



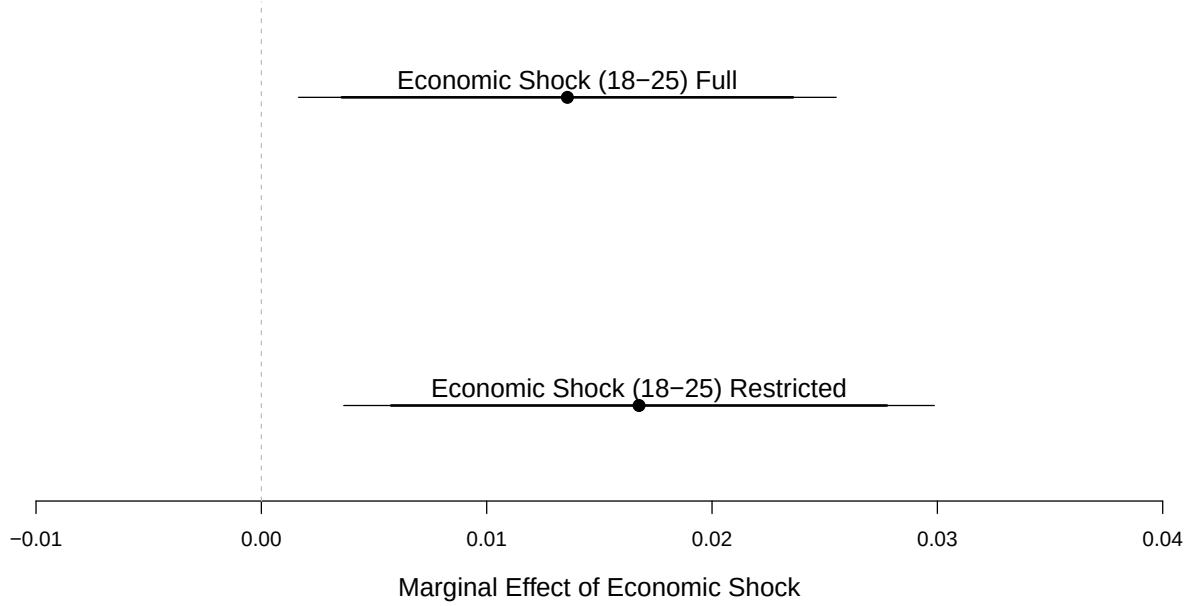

Horizontal lines are 95 and 90 percent confidence intervals with standard errors clustered at the country-cohort level. Estimates in black (gray) are based on the restricted (full) sample. Pre-treatment covariates, cohort fixed effects, survey wave fixed effects, and country fixed effects are included in all models.

grow older.

Figure A4 reports the results when directly testing the relative importance of experiencing an economic shock during the impressionable years compared to other periods. In addition to pre-treatment controls, cohort fixed effects, country fixed effects, and survey wave fixed effects, the models include the main independent variable and a binary measure that equals 1 if the respondent experienced an economic shock outside of the impressionable years. The results indicate that economic shocks outside of the impressionable have a substantially smaller impact on immigration attitudes. This provides evidence consistent with the political socialization literature that emphasizes the importance of young adulthood for the development of political and economic beliefs (Kustov et al., 2021).

Figure A5 reports the estimated effects of the count variable at different GDPpc contraction thresholds. The results are similar when using the binary measure in the main text.

Figure A6: Marginal effect of an economic shock during the impressionable years (18-25) on racialized immigration attitudes.




Estimates are from logit models. Horizontal lines are 95 and 90 percent confidence intervals with standard errors clustered at the country-cohort level. Model includes pre-treatment covariates, cohort fixed effects, survey wave fixed effects, and country fixed effects.

Figure A6 reports the marginal effect of an economic shock on racialized immigration attitudes from logistic regressions. Racialized immigration attitudes equals 1 if the respondent held more restrictive preferences for immigrants of a different race compared to immigrants of a similar race. For the restricted sample, the marginal effects suggest that an economic shock causes a 1.7 percentage point increase in the likelihood the respondent holds racialized immigration preferences.

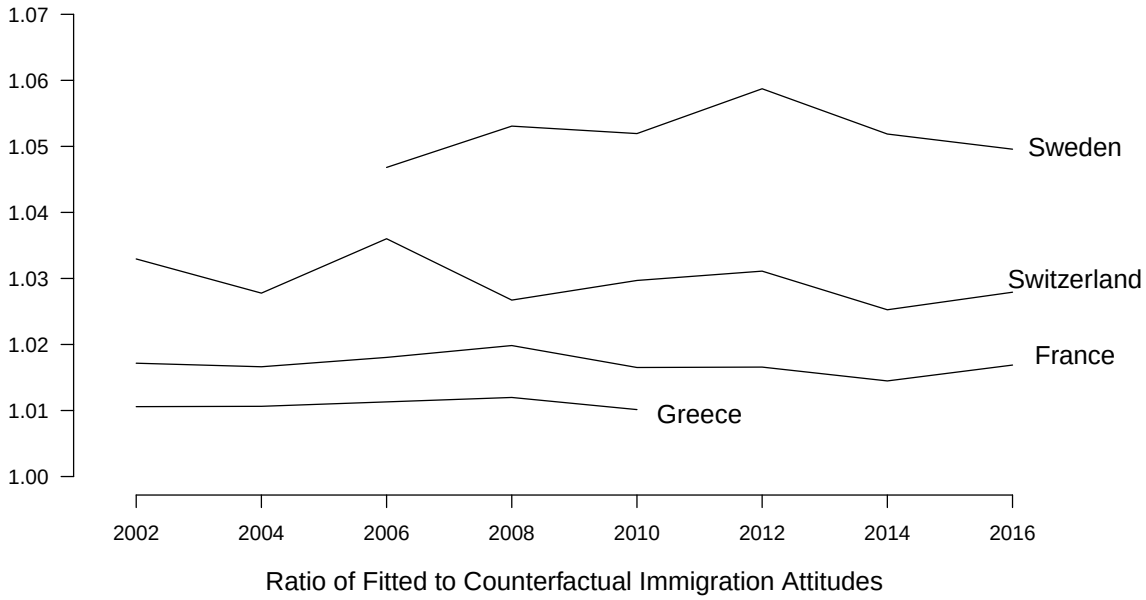
## A.12 Counterfactual Analysis

In this section, I provide an aggregate perspective on the importance of economic shocks on immigration attitudes by conducting a counterfactual exercise. What would immigration attitudes look like across countries if the Golden Years during the 1960s and 1970s had been different? Specifically, if countries had experienced a recession in 1965 and 1975. To do this, I first construct fitted values using the baseline specification for the dichotomous shock

variable and the restricted sample. I then conduct a counterfactual series by falsely assuming recessions occurred in 1965 and 1975. That is, I assume $A$ from equation (1) equals 1 for respondents who grew up during this time. Summing across individuals within each country allows me to compare the differences in immigrations attitudes if recessions had occurred in 1965 and 1975. I then compare the ratio of fitted immigration attitudes to the counterfactual immigration attitudes. The results suggest that immigration attitudes would have been 2.8 percentage points higher across Europe if recessions occurred in 1965 and 1975. Depending on the survey-round and country, the increase is between from 1 and 6 percentage points.

In Figure A7, I plot the ratio of fitted to counterfactual immigration attitudes for France, Greece, Switzerland, and Sweden to illustrate these changes. In line with expectations, the analysis shows that the increase is larger for countries that have experienced fewer recessions, which shows that historical economic conditions have affected aggregate opinion toward immigration. Given the stability of immigration attitudes (Kustov et al., 2021), recessions in 1965 and 1975 likely would of had a meaningful impact on a country's broader political climate. Specifically, Halikiopoulou and Vlandas (2020) show that anti-immigration attitudes are strongly linked to the success of far-right parties.

Figure A7: Ratio of fitted vs counterfactual immigration attitudes when assuming recessions in 1965 and 1975




# Appendix B European Value Study

In Appendix B, I demonstrate the results are robust to using data from the European Value Study (EVS). The section proceeds as follows. First, I discuss the limitations of the EVS data. Second, I provide details on the research design for the analysis. Finally, I present the estimated effects and several robustness checks. Overall, the results are consistent with the main analysis.

## B.1 Limitations of the European Value Study

The EVS has three main limitations compared to the ESS. First, while the ESS conducts a wave every two years (8 waves), the EVS only conducts surveys about once every ten years (5 waves). Thus, the ESS has a substantially larger sample size compared to the EVS.

Second, the EVS only started collecting data on the citizenship of the respondent in Wave 3 and the country where the respondent was born in Wave 4, which further limits the

sample size and the period analyzed because this data is required to ensure the respondent was in the country during the impressionable years. Moreover, the EVS only started to collect data on parental education (a key pre-treatment control) during Wave 5.

Third, while the ESS asks 6 immigration questions every wave since 2002, the EVS only starts to consistently ask multiple immigration questions in Wave 4, which furthers limits sample size.

## B.2 Research Design

For the analysis of the EVS, I closely follow the approach in the main text with deviations only when similar questions are not asked. I use data from Waves 4 and 5 (2008-2010 and 2017-2020). I do not use earlier waves because they do not ask the citizenship or birth country of the respondents, which is necessary to ensure the respondent was located in the country during the impressionable years. I include all countries in the restricted sample from the main text that have complete GDPpc data.

To construct the dependent variable, I use three questions asked during Wave 4 and Wave 5 of the EVS. Specifically, the questions ask whether respondents believe immigrants increase crime, whether immigrants are a strain on the welfare system, and whether immigrants take jobs away from country citizens. The response categories range from 1 to 10 with higher values indicating more anti-immigration attitudes. I standardize each variable and then take the average across the three. The dependent variable (*Anti-Immigration Index*) ranges from -1.91 to 1.43 and a standard deviation of 0.84, where higher values indicate more anti-immigration attitudes.

The main independent variable follows the exact approach used in the main analysis. Specifically, *Economic Shock* equals 1 if the respondent experienced a contraction of at least 4 percent for a single year between the ages of 18 and 25. I also create a count variable that measures the number of years between the ages of 18 and 25 where the respondent experienced a contraction of at least 4 percent. Similar to the main analysis, I only include

Table B1: Effect of an economic shock during the impressionable years (18-25) on immigration attitudes when using data from the European Value Study (EVS).

| | **Estimate** | **95% C.I.** | **N** | **Sample** |
|---|---|---|---|---|
| Economic Shock | 0.039 | (0.000, 0.078) | 41674 | Restricted |
| Economic Shock Count | 0.050 | (0.007, 0.092) | 41674 | Restricted |

Standard errors clustered at the country-cohort level. Models include pre-treatment controls, cohort fixed effects, survey wave fixed effects, and country fixed effects.

respondents who are 26 years or older at the time of the survey.

I include several control variables. Specifically, I control for the gender of the respondent, whether the respondent completed secondary education, the age of the respondent, and whether both parents were born in the country. Unfortunately, the EVS does not include consistent questions about the occupation of the parents like the ESS. Further, the EVS only starts to ask about parental education during Wave 5, which would substantially reduce the sample size. Similar to the main analysis, I also include controls for the average level of education and the educational equality of the country when the respondent was between the ages of 10 and 17. Finally, I include cohort fixed effects, country fixed effects, and wave fixed effects.

## B.3 Results

Following the main analysis, I estimate ordinary least squares regressions with survey weights and with standard errors clustered at the country-cohort level. The main results are reported in Table B1. The estimated effects are positive and statistically significant at the $p < 0.05$ level. When using the dichotomous variable (*Economic Shock*), the results suggest that growing up in a recession causes a 0.047 standard deviation change in the *Anti-Immigration Index*, which is about a 1.2 percentage point increase in anti-immigration attitudes. When using the count variable, experiencing three years of poor economic conditions between the ages of 18 and 25 causes about a 0.083 standard deviation change in the index or a 2.1 percentage point increase in anti-immigration attitudes. The results provide evidence that

Table B2: Country-cohort analysis for European Value Study (EVS)

| | **Estimate** | **95% C.I.** | **N** | **Sample** |
|---|---|---|---|---|
| Economic Shock | 0.041 | (0.001, 0.082) | 1239 | Restricted |
| Economic Shock Count | 0.052 | (0.009, 0.095) | 1239 | Restricted |

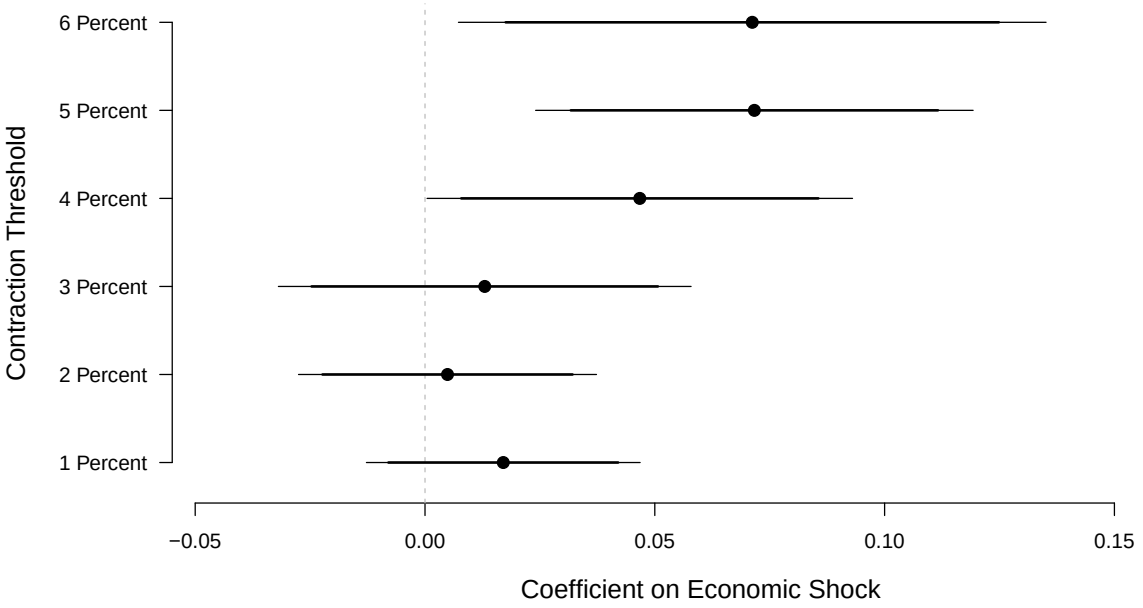
Standard errors clustered at the cohort level. Estimates are from WLS regressions with the country-cohort sample size (appropriately weighted by the sampling weights) as weights. Country fixed effects, average level of education, and educational equality are included in all models.

the frequency of economic shocks increases its effect on anti-immigration attitudes. Broadly, these results are similar to the estimated effects in the main analysis. As a robustness check, I estimate models using weighted least squares (WLS) regression with the within country-cohort sample size as weights (Similar to Table A10 for the ESS). The results are reported in Table B2. The estimated effects are positive and statistically significant ($p < 0.05$).

As a second robustness check, I estimate the effect of an economic shock during the impressionable years using different contraction thresholds. The results are reported in Figure B1 and similar to the main results. That is, the estimated effect of growing up in a recession decreases as the contraction threshold decreases. This aligns with the expectation that the worse the shock, the larger its effect on immigration attitudes.

Finally, I test whether the interaction effect between education and *Economic Shock* is found in the EVS data. Specifically, the main analysis finds that the effect of an economic shock is larger for individuals who completed 12 or more years of education. This result is counterintuitive since educated young adults should have smaller wage decreases during recessions. Figure B2 shows the marginal effects of an economic shock during the impressionable years at different levels of education for the *Anti-Immigration Index* and the three separate immigration questions. The results demonstrate that the interaction effect reported in the main analysis is not an artifact of the ESS data. That is, the effect of growing up in a recession is larger for respondents who completed secondary education in the EVS data. For the index, an economic shock during young adulthood causes a 0.076 standard deviation change in the measure for respondents who completed secondary education, which represents

Figure B1: Effect of an economic shock during the impressionable years (18-25) on immigration attitudes using alternative contraction thresholds for the European Value Study (EVS).




Horizontal lines are 95 and 90 percent confidence intervals with standard errors clustered at the country-cohort level. Model includes pre-treatment covariates, cohort fixed effects, survey wave fixed effects, and country fixed effects.

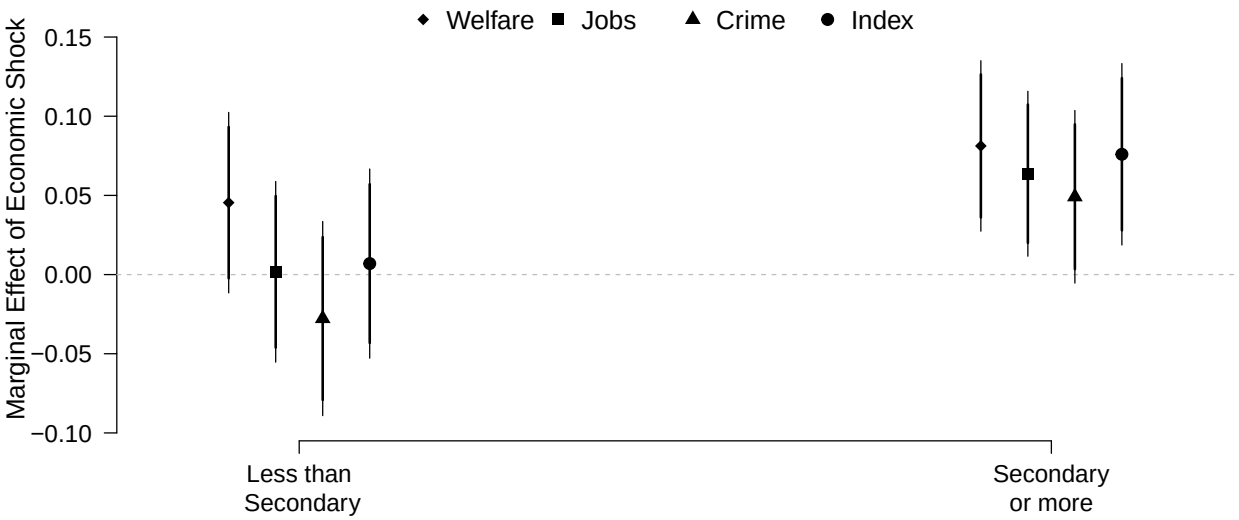
a 1.9 percentage point increase. For individuals who did not complete secondary education, the estimated effect is near zero. These results are consistent across the individual questions, except for the question concerning welfare.

Overall, the results when analyzing the EVS data are consistent with the main analysis. Growing up in a recession causes a significant increase in anti-immigration attitudes. The effect increases as the frequency and severity of shocks increases. Further, I continue to find evidence the estimated effects are larger for educated individuals.

# Appendix C American National Election Study

In Appendix C, I demonstrate the main results are robust when using data from the American National Election Studies (ANES) that leverages economic variation at the subnational level. The section proceeds as follows. First, I discuss the available datasets that provide leverage at a subnational level, their limitations, and why I select the ANES. Second, I provide details

Figure B1: Effect of an economic shock during the impressionable years (18-25) on immigration attitudes using alternative contraction thresholds for the European Value Study (EVS).




Horizontal lines are 95 and 90 percent confidence intervals with standard errors clustered at the country-cohort level. Model includes pre-treatment covariates, cohort fixed effects, survey wave fixed effects, and country fixed effects.

on the research design for the analysis. Finally, I present the estimated effects and several robustness checks. Overall, the results are consistent with the main analysis.

## C.1 Available Datasets and Limitations

To the best of my knowledge, two datasets provide information on the subnational location of respondents around their "impressionable years." The General Social Survey (GSS) asks respondents the census region where they lived when they were 16 years old and the American National Election Studies (ANES) asks respondents the state they lived in when they were 16 years old. Below, I discuss the limitations of each. Specifically, I document the multiple problems that exist in the GSS data that introduce major threats to inference.

### C.1.1 Limitations of the General Social Survey

For the GSS, there are three major limitations. First, the GSS collects data on the census region where the respondent lived when they were 16 years old, which divides the US into 9

regions. Since these regions are very large, the advantages of exploiting subnational variation within a country are limited. Further, at first glance, there is minimal economic variation in the number of economic shocks across these regions. Specifically, Figure C1 shows the years for each region that experienced a contraction of at least 3.5 percent in real regional income per capita.[3] Major recessions at the regional level that are likely to impact immigration attitudes typically occur across most regions at the same time. Further, most regional recessions occur before 1950 and only a handful occur between 1950 and 2000. Figure C1 clearly demonstrates there is minimal variation in economic conditions across regions and time. I show this is primarily driven by census regions masking significant variation between states within each region.

Figure C2 displays the years for each state that experienced a contraction of at least 3.5 percent in real income per capita grouped by census regions. A few points are worth emphasizing. First, at the state level, there is clear variation in economic conditions across time. Recessions are not primarily isolated to the pre-1950 era. They occur at regular intervals across states and across time. Second, there is significant variation within each census region. While some regions are clearly linked at certain times, Figure C2 demonstrates that aggregating economic conditions to the regional level introduces substantial measurement error in identifying recessions. For instance, during the 1960s, the Dakota's experienced recessions while Nebraska, Missouri, Minnesota, Kansas, and Iowa did not. Finally, some census regions are broad groupings of states that clearly do not share similar economic profiles. For instance, Delaware and Florida are assigned to the South Atlantic region, but are over 1,000 miles apart. Montana and New Mexico are both assigned to the Mountain region, but are over 1,200 miles apart. And while Montana and North Dakota are bordering states and share similar economic profiles, they are considered in separate regions. Moreover, some regions are relatively small like the East South Central region that includes Alabama, Tennessee, Kentucky, and Mississippi. This suggests that measurement error will vary across regions.

[3]Giuliano and Spilimbergo (2013) use 3.4 percent. The upshot of the analysis does not change if using 3.4 percent as the threshold.

It will be smaller in the regions that group similar states that are clustered together and larger for regions that stretch across the US and include states with very different economic profiles.

The upshot is clear. The lack of economic variation at the regional level indicates the results are going to be driven by a handful of observations. Further, the substantial amount of measurement error that varies across regions is a clear and significant threat to inference. Unfortunately, statistical modeling cannot solve these issues.

A second limitation is that the GSS only includes a single question across time to elicit immigration attitudes that uses different question wordings and answer categories across waves. As noted in the paper, using a single question to measure latent immigration attitudes is problematic because of the complexity of the issue and measurement error. It is challenging to elicit consistent preferences from a single broad question concerning immigration across time because the context for each respondent is likely very different. As Cavaillé and Marshall (2018) note, there is likely variation in responses to specific questions because the interpretation of non-extreme categories and reference points varies across regions and time. Opposition can be expressed very differently depending on the specific context.

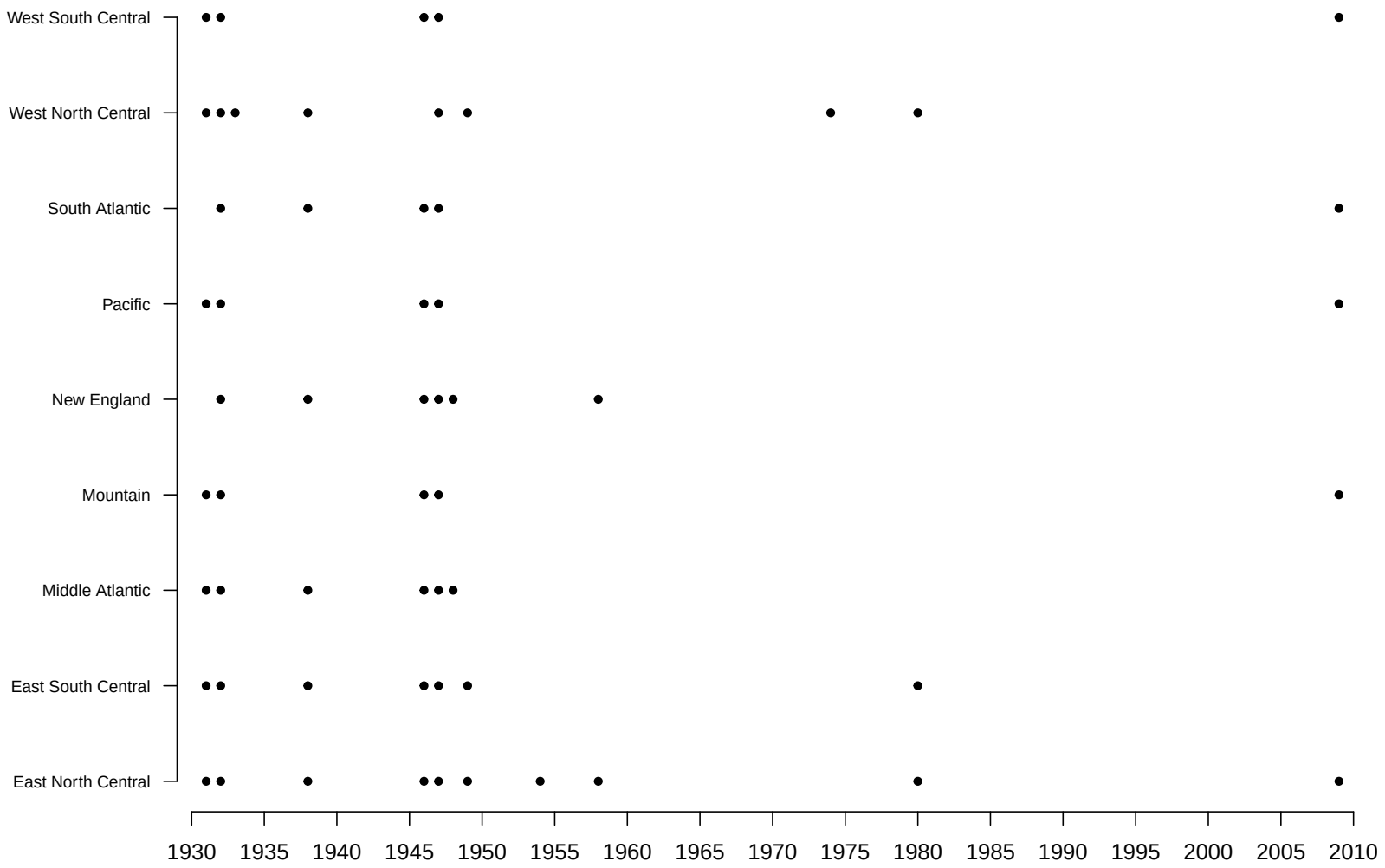
Further, the the GSS used three different question wordings across the waves and different answer categories. Surveys in 1994 and 2004 used one version; surveys in 1996, 2004, and 2014 used another; and surveys in 2004, 2006, 2008, 2010, 2012, 2014, 2016, and 2018 used another. One peculiar component is that in 2004 and 2014, the GSS used two of the question versions. In 2004, respondents only answered one of the questions. However, in 2014, 617 respondents were asked both versions. ***Of these respondents, 174 or 28 percent gave different answers when using the 5 point scale in the same survey!*** This demonstrates that different question versions introduced substantial measurement error.

The lack of multiple questions to measure latent immigration attitudes and the different question wordings suggests that measurement error in the dependent variable will be endemic. Again, there is no modeling decisions that can solve these issues.

A third limitation of the GSS data is the location questions only measure where a respondent lives when they were 16 years old, but does not track the respondent between the ages of 18 and 25 and the other periods of their lives. While, the GSS includes data on whether the respondent is living in the same city and state, it requires the research design to exclude respondents who are living in a different state and/or assume that respondents do not change regions between 16 and 25. Further, estimating the effect of economic conditions for other age ranges would require stronger assumptions about the migration patterns of the respondent.

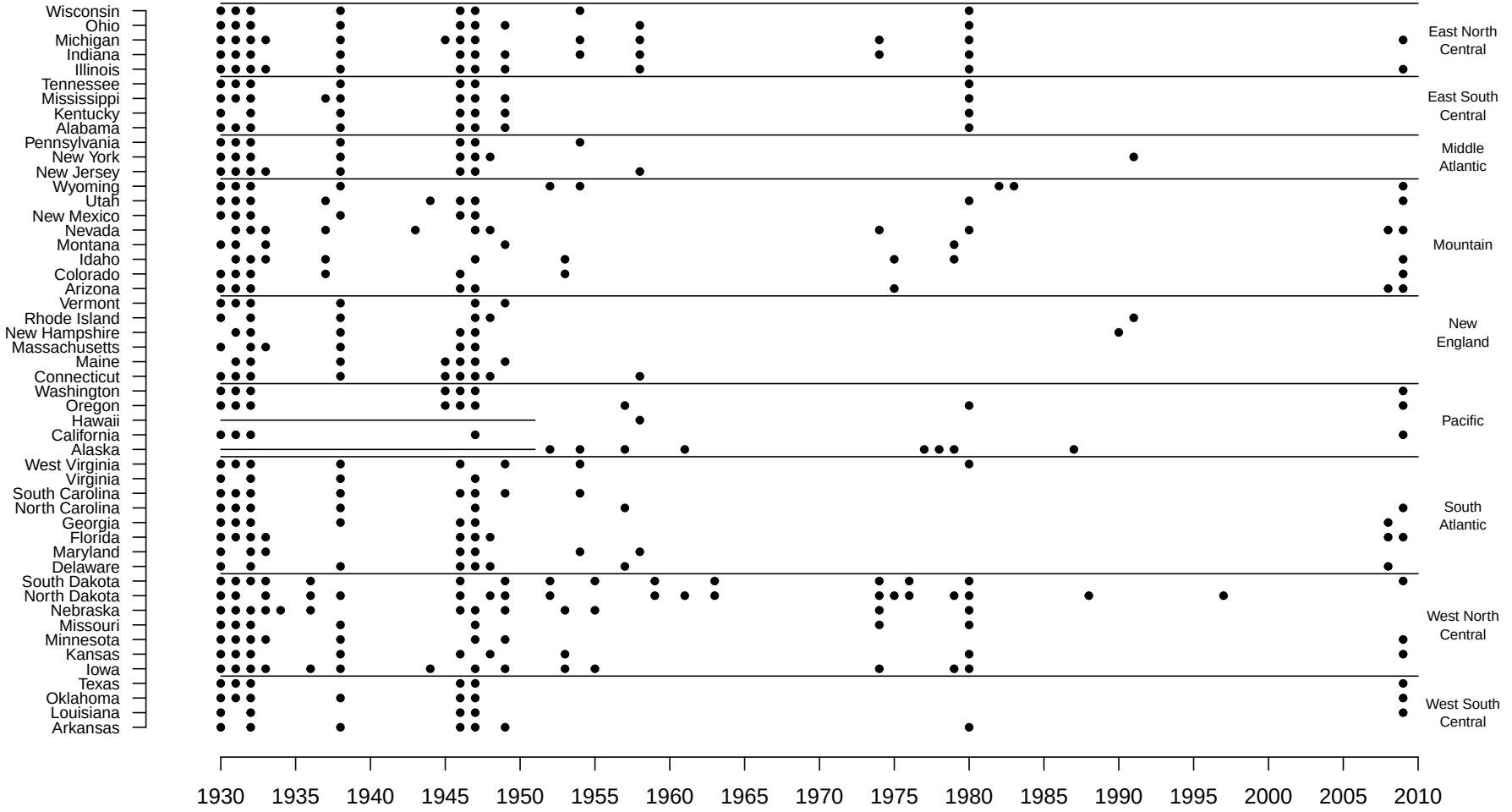
Overall, when taken together, these issues present several serious threats to inference. Given the minimal economic variation across regions, the substantial economic variation across states within regions, and the documented measurement error in the immigration question, it is evident the GSS will introduce more challenges and questions rather than provide additional leverage.

Figure C1: Distribution of shocks across census regions.




The points represent real regional income per capita declines of at least 3.5 percent.

Figure C2: Distribution of shocks across states grouped by census region.




The points represent annual state income per capita declines of at least 3.5 percent. Economic Data for Hawaii and Alaska start in 1951.

### C.1.2 Limitations of the American National Election Studies

For the ANES, there are two limitations. First, the ANES includes a small sample during the 2008 recession. Specifically, for a single "off-wave" of the 2008-2009 panel, the ANES panel collected data on the state where respondents lived when they were 16 years old, which includes about 1,445 respondents. Questions that elicit immigration preferences are from another "off-wave" and reduces the sample size to about 1,000 respondents. Recall, the ESS sample includes over 100,000 respondents across 15 years. The small sample size of the ANES creates challenges when including state (50 levels) and cohort (80 levels) fixed effects, which are critical to eliminating potential confounding explanations.

Second, the ANES collects data on the state where the respondent lived when they were 16 years old. Unlike the GSS, the ANES provides sufficient economic variation at the subnational level (see Figure C2). However, similar to the GSS, it only measures where the respondent lives when they were 16 years old and at the time of the survey. It does not track the respondent between the ages of 18 and 25 and other periods of their lives. Thus, it requires similar assumptions to the GSS about respondents moving locations between 16 and 25.

Overall, while the ANES includes limitations, it also provides several advantages. Specifically, it provides substantial subnational economic variation across cohorts and time and it includes multiple questions that elicit immigration attitudes. While the small sample size and required assumptions about respondent relocation will introduce some challenges, they are not insurmountable unlike the issues in the GSS. Thus, I focus the analysis on the ANES data. The following sections describe the research design and presents the results.

## C.2 Research Design

I use data from Waves 8 and 22 of the 2008-2009 ANES panel. Specifically, in Wave 8, the ANES asked respondents the state they lived in when they were 16 years old. A key assumption is that respondents remained in the state between the ages of 18 and 25..

I construct two dependent variables derived from 9 questions asked during Wave 22 of the panel. The first variable is constructed using the following three questions:

1) "In your opinion, how likely is it that immigration will have a negative impact on the way of life in many American communities? 1-Very likely; 2-Somewhat likely; 3-Somewhat unlikely, 4-Very unlikely"

2) "How strongly do you agree or disagree with each of the following statements: Immigrants today are a burden on our country because they take our jobs. 1-Strongly agree; 2-Agree; 3-Neither agree nor disagree; 4-Disagree; 5-Strongly disagree"

3) "Do you think that American culture is endangered? 1-Strongly agree; 2-Agree; 3-Neither agree nor disagree; 4-Disagree; 5-Strongly disagree"

The first two questions focus directly on immigration and its impact on the American way of life and American jobs. The third question, while not directly mentioning immigration, elicits views about American culture being endangered, which is a strong underlying motivation for anti-immigration attitudes. I standardize each variable and then take the average across the three. *Anti-Immigration Index* ranges from -1.98 to 1.62 and a standard deviation of 0.783. Higher values indicate more anti-immigration attitudes.[4]

The second dependent variable focuses on prejudice toward Latin Americans. Specifically, respondents are asked the following:

"Thinking of immigrants from Latin American countries, which of the following characteristics apply to immigrants from Latin America?
1. Work very hard
2. Often end up on welfare
3. Do very well in school
4. Significantly increase crime
5. Have strong family values
6. Keep to themselves and don't try to fit in."

I code a negative response as equal to one, sum across the 6 variables, and then standardize. *Latin American Prejudice* has a range between 1.92 and 2.40 and a standard deviation of 1, with higher values indicating more anti-immigration attitudes.

[4]I opt just to standardize the variables since the sample only includes a single wave of immigration questions, which suggests that measurement error derived from different reference points is small.

To measure an economic shock, I use data from the Bureau of Economic Analysis on state income per capita starting in 1929 and adjust for inflation. I define an economic shock at various thresholds between a 1 and 5 percent contraction in real state income per capita. Similar to the main analysis, I define the impressionable years as between the ages of 18 and 25. I then construct a binary variable that equals 1 if the respondent experienced an economic shock between the ages of 18 and 25. Additionally, I create a count variable that captures the number of years during the impressionable period that the respondent experienced an economic shock.

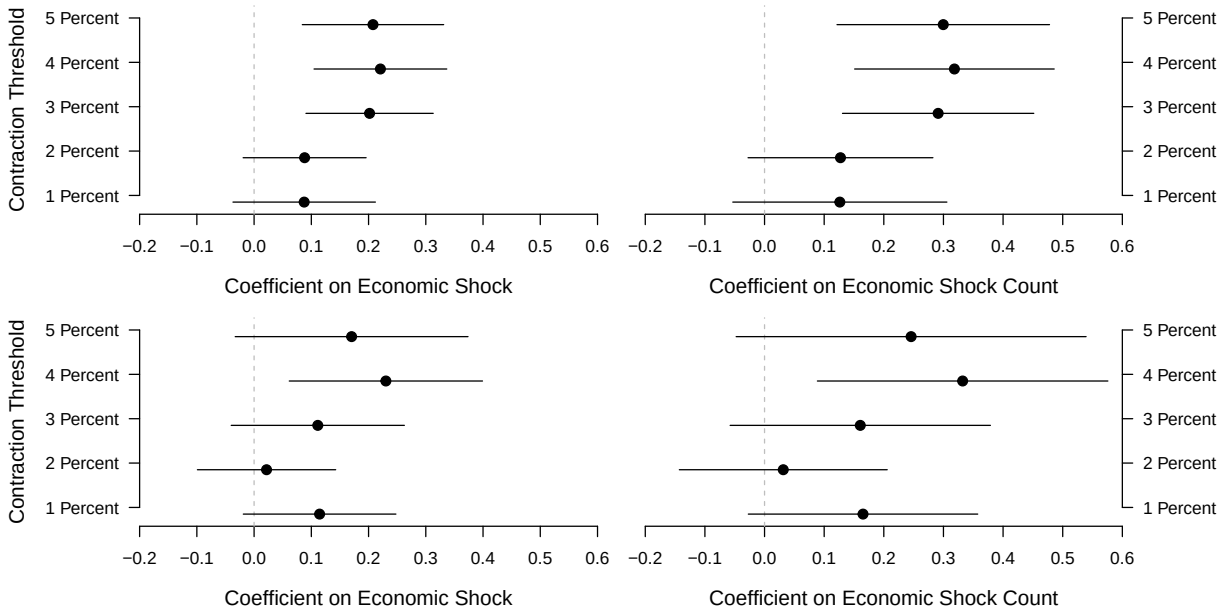
I also include several pre-treatment control variables to account for potential confounding explanations. Again, it is important to exercise caution to not include controls that occur after an economic shock because it may bias the causal effect of growing up in a recession. First, I control for the respondent's age, gender, race, and type of city they lived in when they were 16 years old. I also include a binary variable that equals 1 if the respondent completed any education beyond high school. Second, I include state at 16 fixed effects and cohort fixed effects. Unfortunately, the ANES does not collect information on parental education, occupations, or citizenship.

Similar to the main analysis, I only include respondents who are at least 26 years old at the time of the survey.

## C.3 Results

I estimate ordinary least squares regressions with survey weights and with standard errors clustered at the state at 16-cohort level. Figures C3 (C4) reports the results when using the *Anti-Immigration Index* (*Latin American Prejudice*) as the dependent variable. The top panels report the estimated effects for a simple bivariate regression while the bottom panels report the results when including all controls. Again, it is important to emphasize that the full models are particularly demanding. They include about 80 cohort fixed effects and 50 state fixed effects. The sample for both dependent variables only includes about 1,000

Figure C3: Effect of an economic shock during the impressionable years (18-25) on the *Anti-Immigration Index* for the American National Election Studies (ANES).



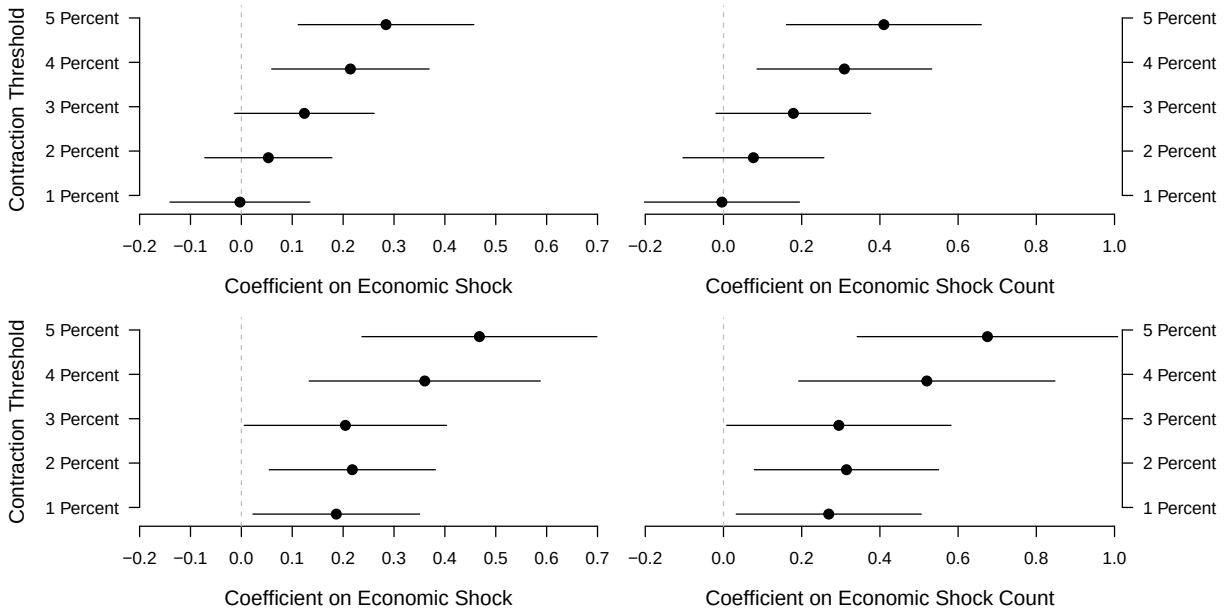

N=999. Horizontal lines are 90 percent confidence intervals with standard errors clustered at the state16-cohort level. The top panels are bivariate regressions. The bottom panels include pre-treatment covariates, cohort fixed effects, and state at 16 fixed effects.

respondents.

Overall, the results are consistent with the main analysis. For the top panel of Figure C3, the estimated effects are in the expected direction. Moreover, consistent with the theoretical argument, as the contraction threshold is increased, the estimated effect of growing up in a recession increases. The effect sizes are also meaningful. Specifically, experiencing an economic shock (4 percent contraction) during young adulthood causes a 0.282 standard deviation change in the *Anti-Immigration Index*, which is about a 6 percent increase in anti-immigration attitudes.

When including the full set of pre-treatment controls, state at 16 fixed effects, and cohort fixed effects, the estimated effects are less precise. However, the effect sizes are similar to the results for the bivariate regressions, which suggests that the large number of fixed effects is adding noise to the estimates. Considering the relative small sample size, the similarity in effect sizes is a promising sign.

Figure C4: Effect of an economic shock during the impressionable years (18-25) on *Latin American Prejudice* for the American National Election Studies (ANES).




N=1010. Horizontal lines are 90 percent confidence intervals with standard errors clustered at the state16-cohort level. The top panels are bivariate regressions. The bottom panels include pre-treatment covariates, cohort fixed effects, and state at 16 fixed effects.

The results are similar for the *Latin American Prejudice* measure (Figure C4). For the simple bivariate regressions, the evidence demonstrates that more severe contractions cause a larger increase in anti-immigration attitudes. For the 4 percent contraction threshold, the estimated effect suggests that growing up in a recession causes about a 0.214 standard deviation change in *Latin American Prejudice*, which is about a 5 percentage point increase. When including the pre-treatment controls, state at 16 fixed effects, and cohort fixed effects, the confidence intervals for the estimated effects increase and the effect sizes slightly increase, but they are relatively similar to the bivariate results. Again, taken together, these results are consistent with the main analysis and suggest that growing up in a recession causes an increase in anti-immigration attitudes.

I also conduct a state16-cohort level analysis using weighted least squares (WLS) with the within-state16-cohort sample size (appropriately weighted by sampling weights) as weights. As Green and Vavreck (2008) show, this approach provides more accurate vari-

Table C1: State16-cohort analysis using 4 percent contraction threshold for American National Election Studies (ANES)

| | Estimate | 90% C.I. | N | Sample |
|---|---|---|---|---|
| **Anti-Immigration Index** | | | | |
| Economic Shock | 0.235 | (0.030, 0.440) | 717 | Full |
| Economic Shock | 0.339 | (0.066, 0.612) | 471 | Same State |
| Economic Shock Count | 0.276 | (0.005, 0.547) | 717 | Full |
| Economic Shock Count | 0.374 | (-0.005, 0.753) | 471 | Same State |
| **Latin American Prejudice** | | | | |
| Economic Shock | 0.365 | (0.077, 0.654) | 720 | Full |
| Economic Shock | 0.521 | (0.141, 0.901) | 475 | Same State |
| Economic Shock Count | 0.503 | (0.133, 0.874) | 720 | Full |
| Economic Shock Count | 0.677 | (0.176, 1.179) | 475 | Same State |

Standard errors clustered at the cohort level. Estimates are from WLS regressions with the state16-cohort sample size (appropriately weighted by the sampling weights) as weights. State at 16 fixed effects and cohort fixed effects are included in all models.

ance estimates when analyzing the effect of a cluster-level variable such as economic shocks at the state16-cohort level. While this approach prevents including individual-level controls, it improves the accuracy of the standard errors, which is important given the relatively small sample size compared to the ESS. Table C1 reports the results when using the 4 percent contraction threshold. Across the models, the estimated effects are positive and 7 of the 8 are significant at the $p < 0.10$ level. The effect sizes are similar to the results reported in Figures C3 and C4. Further, Table C1 reports the estimated effects when limiting the sample to respondents who are located in the same state as when they were 16 years old. The effect sizes increase, which aligns with the expectation there is some measurement error in the independent variable due to the location data on the respondent.

Overall, the results demonstrate the theoretical argument is supported in the ANES data. Growing up in a recession causes an increase in anti-immigration attitudes. While the small sample size decreases the precision of the estimates, the collective evidence consistently points to an economic shock during young adulthood having a meaningful effect on anti-immigration attitudes.

# References


Barro, Robert J and José F Ursúa (2008). Macroeconomic crises since 1870. Technical report, National Bureau of Economic Research.

Cavaillé, Charlotte and John Marshall (2018). Education and anti-immigration attitudes: Evidence from compulsory schooling reforms across western europe. *American Political Science Review, Forthcoming*.

Coppedge, Michael , John Gerring, Carl Henrik Knutsen, Staffan I. Lindberg, Jan Teorell, Nazifa Alizada, David Altman, Michael Bernhard, Agnes Cornell, M. Steven Fish, Lisa Gastaldi, Haakon Gjerlow, Adam Glynn, Allen Hicken, Garry Hindle, Nina Ilchenko, Joshua Krusell, Anna L uhrmann, Seraphine F. Maerz, and Kyle L. Marquardt. (2021). V-dem country-year/country-date dataset v11.

Giuliano, Paola and Antonio Spilimbergo (2013). Growing up in a recession. *Review of Economic Studies 81*(2), 787–817.

Green, Donald P and Lynn Vavreck (2008). Analysis of cluster-randomized experiments: A comparison of alternative estimation approaches. *Political Analysis 16*(2), 138–152.

Halikiopoulou, Daphne and Tim Vlandas (2020). When economic and cultural interests align: the anti-immigration voter coalitions driving far right party success in europe. *European Political Science Review 12*(4), 427–448.

Kustov, Alexander , Dillon Laaker, and Cassidy Reller (2021). The stability of immigration attitudes: Evidence and implications. *The Journal of Politics 83*(4), 1478–1494.