Essays in Empirical Political Economy

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ABSTRACT

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This thesis uses survey data from the United States and Western Europe to examine the determinants of individual political and redistributive preferences. Moreover, the extent to which these translated into policy outcomes is further investigated using public spending data.

Chapter 1 uses survey data from the United States to show that popular support for the Democratic Party declined over the last few decades. I decompose cohort data on political preferences into cohort, life-cycle and period effects in order to distinguish their relative importance in driving the observed trend. I find significant cohort effects in political partisanship, whereby younger cohorts have increasingly reduced support for the Democratic Party. Moreover, life-cycle effects are muted suggesting that individuals tend to retain their preferences over the life-span. These findings suggest that economic and social events that affect an individual's political preferences in his/her youth have lifelong implications.

In Chapter 2, I pursue the hypothesis that the "rightening" of cohorts reflects a decline in the demand for redistribution among younger cohorts by examining whether the development is linked to the rise in high-school education. Successive cohorts across US states were exposed to increasingly stringent compulsory attendance

and child-labor laws. Using these laws as instruments for individual high-school education, I find that those who attended or graduated from high school significantly reduced support for the Democratic Party and for government spending. My estimates indicate that the rise in schooling induced by the laws can account for 10-25 percent of the decline in Democratic support.

Chapter 3 is joint work with Lena Edlund and Rohini Pande. We use survey data for nine West European countries to show that women have become increasingly left-wing compared to men, and that this trend is positively correlated with the decline in marriage in these countries. This pattern is mirrored in German longitudinal data, where transitions out of marriage make women, but not men, significantly more left-leaning. Analysis of public spending data for high-income OECD countries suggests that the political impact of non-marriage extends to the allocation of State resources.

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Acknowledgments

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Chapter 1

Cohort and Life-cycle Effects in Political Preferences

1.1 Introduction

Popular support for the Democratic Party has declined over the last few decades. Survey data indicate that the proportion of 18-64 year olds who favored the Democratic Party declined from 54 percent in 1972 to 45 percent in the year 2000 (Figure 1.1).[1] It is unclear what drove this decline. One possibility is that the elderly tend to favor the Republican Party and as the population has aged, a growing proportion has reduced support for the political left. Another possibility is that new entrants or younger cohorts in the electorate have reduced support for the Democratic Party, hence producing a "rightening" of the electorate. This paper attempts to disentangle cohort, age and year effects in political partisanship in order to determine their relative importance in delivering the observed trend.

The absence of long-running panel data prevents me from tracking individual

[1]These data are from the National Election Studies and the General Social Survey. Respondents are asked their party preference on a seven-point scale ranging from Strong Democrat to Strong Republican. An individual is considered a Democrat if he/she claims to be a Strong, Weak or Independent-leaning Democrat.

political preferences over time. However, a series of cross-sections can be used to follow the behavior of birth cohorts over time. For instance, I can observe political preferences of 18-year-olds from a 1972 survey, 20-year-olds from a 1974 survey, 22-year-olds from a 1976 survey and hence track over time the political orientation of those born in 1954.

Stylized evidence points toward the presence of both cohort and life-cycle effects in political preferences. Figure 1.2 depicts political partisanship for 5 selected cohorts over the period 1972-2000 in the United States. The cohorts are identified by 5-year birth year bands.[2] Each data point represents the proportion of the cohort that supports the Democratic Party in a given year. Each series corresponds to a cohort and gives the profile of the cohort over its life-span.[3] There is a downward shift in the age profiles with each new cohort, suggesting that younger cohorts are less supportive of the political left. A life-cycle effect is also observable – support for the Democratic Party appears to decline with age.

The problem with separating age, cohort and year effects is that there is a linear relationship between the three and hence it is not possible to identify all effects simultaneously. Therefore, I adopt the methodology used by Deaton and Paxson [1994] to decompose cohort data on partisanship into age, cohort and year effects.[4] The decomposition confirms the cohort trend observed in Figure 1.2. I find significant cohort effects in political preferences and that younger cohorts have systematically reduced support for the Democratic Party. The evidence on life-cycle effects is mixed. While there is some indication that individuals become conservative with age, the result is rather weak. More importantly, age effects are muted in comparison to cohort effects suggesting that individuals tend to retain their preferences over the

[2]Variable construction is described in detail in section 1.2.

[3]The youngest and oldest cohorts have shorter line segments because the young enter the sample later and the old depart earlier. This is explained in more detail shortly.

[4]Attanasio [1998] and Jappelli [1999] use a similar decomposition in their analysis of US household savings and Italian household wealth respectively.

life-span. I find that the trend in cohort partisanship is driven by whites and is stronger among males. This rightening was initiated with male cohorts coming of age soon after World War II. My findings suggest that economic and social events that affect an individual's political preferences in his/her youth have lifelong implications. I also use direct evidence on individual demand for redistributive policies and show that younger cohorts have reduced support for redistribution.

The persistence of political preferences over an individual's life-span has received significant attention in the political science literature. Hyman [1959] and Campbell, Converse, Miller, and Stokes [1960] concluded that partisan affiliation is formed early in life and remains quite stable over the lifetime. Several studies emerged to support this claim, Converse [1969], Converse and Markus [1979], Sears [1983], Sears and Funk [1999], Green, Palmquist, and Schickler [2002] to name a few. My findings are consistent with this thesis.[5] There has been debate over the timing of creation of such preferences and evidence suggests that an individual's partisanship, while malleable in early years of adulthood, reaches a stable level between his/her mid-twenties to mid-thirties [Niemi and Jennings 1981], [Jennings and Markus 1984], [Sears 1989], [Alwin, Cohen, and Newcomb 1991], [Alwin and Krosnick 1991].

Political scientists have also discussed the importance of cohort effects in political preferences. It is argued that differences in partisanship are rooted in generational differences as each birth cohort has unique socialization experiences [Campbell, Converse, Miller, and Stokes 1960], [Converse 1976], [Glenn 1976]. For instance, individuals who came of age during the Great Depression exhibit stronger allegiance to the Democratic Party. In contrast, those who came of age during the "optimistic years of the early Reagan administration" lean strongly toward the Republican Party

[5] An alternative to the persistence view, and in line with Downs [1957], was put forth by Fiorina [1981], Franklin [1984], Niemi and Jennings [1991], Markus [1992]. They argued that individuals adjust political partisanship over their adulthood in response to political circumstances, party positions and issue preference. There is considerable agreement, however, that the evidence on stable partisan identification over the life-cycle rules out constant adjustment of such.

[Erikson and Tedin 2001].

While political scientists have documented the increased Republican support among males over time (see Wirls [1986] and Kaufmann and Petrocik [1999]), the increased support among younger cohorts has received limited attention.[6] This appears to be for two reasons. First, the reduction in Democratic support is typically viewed as a feature of Southern politics. Carmines and Stimson [1989] claimed that the Democratic Party's stance on liberal racial policies beginning in 1964 drove southerners to the Republican Party. Green, Palmquist, and Schickler [2002] argued that the passage of the Voting Rights Act of 1965, which brought large proportions of blacks into the voting booths and to the Democratic Party, changed the image of the Republican Party for southerners. Southern cohorts turned Republican gradually as younger cohorts with a new understanding of party images replaced older ones who held on to traditional conceptions. According to these views, changes in party platforms with respect to civil rights underlie the decline in Democratic support. Second, the reduced support is regarded as a fading out of the Depression era.

I present results by geographic region to further investigate the pattern of cohort politics in the South. I find that the cohort trend, while clearest in the South, is not restricted to this region. Moreover, the rightening in the South was initiated with cohorts that came of age around World War II. If the Democratic Party's stance on racial issues is solely responsible for reduced Democratic support, it is unclear why the rightening was initiated already fifteen years before the civil rights movement.

Finally, this paper contributes to the growing economics literature on determinants of redistributive and/or political preferences. The assumption that an individual's political preferences reflects his/her preferences over redistribution and thereby

[6]There are also journalistic accounts of the rise of Republicanism since the 1960s. According to Perlstein [2001], the Republican nomination of conservative Barry Goldwater against Lyndon Johnson in the 1964 presidential election led to a polarization in politics and an improvement in the political machinery of the Republican Party which set the stage for Reagan's victory in 1980. It is unclear in such a story why these developments appealed more strongly to younger cohorts.

his/her economic status is a common assumption in economic models of individual voting behavior (Downs [1957]).[7] Recent studies that have used support for the political left as a proxy for increased demand for redistribution include Edlund and Pande [2002], Alesina and Angeletos [2003] and Edlund, Haider, and Pande [2004b].[8]

The remainder of the paper is organized as follows. Section 2 describes the data and methodology and section 3 outlines the results. Finally, section 4 concludes.

1.2 Data and Methodology

This section outlines the data and identification scheme.

1.2.1 Data

Individual-level data are drawn from the biennial National Elections Studies (NES) and the General Social Survey (GSS) over the period 1972-2000. The samples are combined and restricted to respondents aged 18 to 64.[9] This leaves me with data from 15 survey years and an average of about 3,050 respondents per year.[10] Females comprise 55 percent of the sample. Information on demographics and political preferences is extracted for each respondent. An individual's political orientation is measured by his/her partisan identification. The NES and GSS questionnaires asked respondents to place themselves on a 7-point scale ranging from Strong Democrat to Strong Republican. I collapse these responses into a 0-1 dummy variable, *left* which takes on the value 1 when the respondent claims to be a Strong, Weak or Independent-

[7]Persson and Tabellini [2000] provide a recent overview of the literature.

[8]Other papers that examine determinants of redistributive preferences include Poterba [1997], Ravallion and Lokshin [2000], Alesina, Glaeser, and Sacerdote [2001] and Alesina and La Ferrara [2001].

[9]This avoids sampling issues related to aging and death, an issue I will turn to shortly.

[10]Individual-level records from GSS are appended only for the years when the NES was conducted. This is equivalent to using GSS data from each even-numbered survey year. No GSS data are available in 1992.

Leaning Democrat, and zero otherwise. Table 1.1 provides descriptive statistics, and Appendix A details on variable construction.

1.2.2 Constructing Cohort Data

The absence of long-running panels prevents me from following political preferences of the same individuals over time. I can, however, use series of cross-sections to observe the behavior of cohorts over time, where a cohort is determined by year of birth.[11] In order for cohort data to retain many of the properties of panel data, it is important that cohort membership be considered fixed over time. In other words, the cohort population must be constant over time for successive surveys to generate random samples from the same underlying population [Deaton 1997]. Mortality proves to be problematic in this context as it implies that samples are being drawn from a declining population. The sample is cut off at 64-year-olds to mitigate this problem.

The next issue is to choose the statistic of interest. An average proves sensible in this context. Given that individual partisan preferences are measured by a 0-1 dummy (*left*), the cohort mean translates to the proportion of the cohort population that is left-wing.

A cohort is defined as age in the base year, 1972 in this instance. I choose to construct cohorts from five-year age bands. A narrower band would reduce within-cell heterogeneity but at the expense of a decline in the number of individuals comprising each cell. A broader definition will improve the precision of my cohort means.[12] The earliest birth year in my sample is 1908, i.e. 64 years of age in 1972 and the latest birth year is 1982, or born 10 years after 1972. Each respondent is assigned to a cohort (a five-year age band) based on his/her age in 1972. For ease of reference, a cohort c

[11] The NES and GSS draw fresh samples of respondents in each survey; they provide repeated independent cross-sections through time.

[12] My results are not sensitive to this definition of cohorts. A robustness check with a narrower interval yields qualitatively similar results.

is labelled by the median age of the five-year age band in the base year. There are a total of 15 cohorts. Each individual is assigned to a period t based on his/her year of interview.[13] Next, for each cohort-year combination in the data, I compute averages for *left*. This leaves me with 153 cohort-year cells, where each cell mean refers to the proportion of the cohort that supports the political left in the given year.[14] Finally, the age of each cohort, a is defined as $c + t$.

The cohort definition along with average cell size for each cohort is outlined in Table 1.2. These cohort data are described in further detail in Table 1.3. For alternate cohorts and each survey year, it reports the number of individuals in each cohort sampled.[15] For cohort 7, born between 1948 and 1952, 468 people were sampled in 1972, 342 in 1974 and so on (column (5)). Note that column (2) is for the youngest cohort, born in 1978-82. These individuals enter the sample for only three periods. Similarly, the cut-off age being 64 implies that older cohorts are not sampled in later years. As column (9) indicates, the oldest cohort, born in 1908-12, is also only tracked in the sample for three years. Tables 1.4 and 1.5 provide corresponding information for males and females respectively.

1.2.3 Decomposition

Consider political preferences of a cohort in a given year:

$$left_{ct} = \beta + \alpha_a + \gamma_c + \psi_t + \upsilon_{ct} \tag{1.1}$$

where c, a and t refer to cohort, age and time respectively; their construction is outlined above. $left_{ct}$ is the proportion of cohort c that supports the left in period t.

[13] $t =$ year-1972

[14] The number of cells fall short of $c * t$ because the youngest and oldest cohorts are tracked over fewer years. The youngest ones enter towards the end of the study period and the oldest ones would fall outside the age range were they tracked any longer.

[15] I report numbers for alternate cohorts to preserve space.

α_a, γ_c and ψ_t refer to age, cohort and year effects respectively. In order to capture nonlinear aging effects, I use a quadratic function in age. Cohort and year effects are captured with dummy variables. (1.1) can then be expressed as:

$$left_{ct} = \beta + f(a_{ct}) + C\gamma + Y\psi + v_{ct} \qquad (1.2)$$

where the function f represents the second-order polynomial in age, C the matrix of cohort dummies, and Y the matrix of year dummies. Since there are 15 cohorts, 15 survey years and 153 cohort-year pairs of observations in the data, C and Y each have 153 rows and 15 columns.

The identification problem is that there is a linear relationship between age, cohort and year. The age of a cohort can be inferred from the time period and when the cohort is born. One option is to drop year dummies from the model, equivalent to including year effects as a part of the residuals.[16] This assumption is quite restrictive and implies that cohort political partisanship is not subject to shocks. A less restrictive alternative is to attribute any time trends in the data to either year effects or to a combination of age and cohort effects [Deaton and Paxson 1994]. Since political preferences of cohorts are to be decomposed and there is no *a priori* reason to characterize these with a time-trend in year effects, a sensible approach is to attribute any trends to age and cohort effects and to use year effects to capture shocks which average to zero in the long run. The year effects are simply additive aggregate shocks that surprise all cohorts. Following Deaton and Paxson [1994], the normalization that makes this possible is to constrain the year dummies to add up to zero and be orthogonal to a time-trend.

Deaton [1997] puts forth a simple method to estimate a model as in (1.2) subject to the normalization above. $left_{ct}$ is regressed on the set of cohort dummies (excluding one), the age quadratic and the set of $T-2$ year dummies defined as

[16]Then, by construction, the year effects would be orthogonal to the quadratic in age and the cohort dummies.

follows from $t = 3, ..., T$

$$d*_t = d_t - [(t-1)d_2 - (t-2)d_1] \tag{1.3}$$

where d_t is the year dummy which equals 1 when $year - 1972$ is t and 0 otherwise. This enforces the restriction that year dummies add up to zero and are orthogonal to a linear trend. The coefficients of $d*_t$ give the year coefficients from 3 to T, the first and second can be recovered from the fact that the year effects add up to zero and are orthogonal to a linear trend.

An important structural assumption underlying the identification is that there are no important interaction effects between age, cohort and year. It is assumed, for instance, that different cohorts exhibit similar life-cycle effects – political affiliation varies with age similarly for say, the baby boomers and those who came of age during the Great Depression. To determine the extent to which this assumption is problematic is an area for future investigation.

1.3 Results

Equation 1.2 is estimated with cohort 14 or the 1915 cohort omitted, making this cohort the reference group.[17] The results are reported in Table 3.2, column (1).[18] There are highly significant cohort effects in political partisanship, whereby younger cohorts have increasingly reduced support for the political left. Evidence suggests that this rightening was initiated with the 1935 cohort. On average, relative to the 1915 cohort, the 1940 cohort reduced support for the Democratic Party by 7 percentage points. The spectrum shifted further away from the left with younger cohorts – the 1980 cohort reduced support by 17 percentage points. The trend in

[17]In the remainder of the paper, I refer to a 5-year birth interval using the median birth year, the 1915 cohort refers to individuals born in 1913-17 and so on.

[18]For brevity, the coefficient for every other cohort dummy is reported.

cohort political affiliation is depicted in Figure 1.3.

There is evidence of a non-linear relationship between age and support for the left, though significant only at the 10 percent level. The results depict an inverted U-shape and point estimates imply a turning point at age 23, which falls in the relevant age range. Older respondents reduce support for the Democratic Party. However, this effect is small in magnitude – a 64-year old is 6 percent less likely to support the Democratic Party than a 20-year old.

The year effects, as discussed above, are constrained to capture aggregate shocks which temporarily move all cohorts off their profiles [Deaton 1997]. While the primary interest here is to separate life-cycle and generational components of agents' political preferences, a note on year effects is warranted. The results, depicted in Figure 1.4, show a sharp leftward spike in the beginning of the 1980s. This is followed by a rightward tilt later in the decade and subsequent leftward ones in the early and late 1990s.

1.3.1 By gender and race

In order to determine whether the observed cohort and life-cycle effects are driven by either males or females, the underlying individual-level data are split by gender to create male and female cohort data. This also enables me to identify gender differences in political behavior. As before, there are 153 cohort-year pairs of observations. The reference group remains the 1915 cohort (either male or female). Table 3.2, columns (2) and (3) report the results for males and females respectively. The observed trend in cohort effects is not restricted to any one gender; there are significant cohort effects in male and female political preferences along with evidence of a trend. However, the decline in support for the Democratic Party is much stronger among males. For instance, relative to the 1915 cohort, the 1980 male cohort reduced support for the left by 24 percentage points. The corresponding figure for females was only 13 percentage points. Male and female cohort effects are illustrated in

Figures 1.5 and 1.6 respectively. The observed gender differential is consistent with previous findings that showed that males rather than females increased support for the Republican Party [Wirls 1986], [Kaufmann and Petrocik 1999]. Life-cycle effects are not statistically significant for either subgroup, suggesting that individuals do not alter political partisanship with age.[19]

Next, I split the sample by race and repeat the exercise. The results for whites and blacks are given in columns (4) and (5) respectively. The observed rightening of cohorts appears to be driven by whites. There are significant cohort effects among this group and evidence of a marked trend, whereby younger white cohorts increasingly reduced support for the Democratic Party (Figure 1.7). The rightening among the white sub-sample was initiated with the 1930 cohort. The results for blacks are less clear (Figure 1.8). Relative to the 1915 black cohort, 1920-40 cohorts were significantly more left-leaning. For younger black cohorts, the cohort effects are not statistically significant. However, lack of significance may reflect imprecision since the underlying cell size for the black sub-sample is fairly small.[20] It should be noted that there appears to be a negative trend in support for the Democratic Party among the 1930-70 black cohorts. Also, these cohort effects are jointly significant. As before, I do not find evidence of important life-cycle effects in either the white or black sub-samples.

1.3.2 By region

In order to check whether the observed effects are isolated to certain regions, I split the underlying individual-level data into 4 census regions: Northeast, Midwest, South and West. Each respondent is assigned to a census region based on his/her place of residence at the time of survey. The cohort data for each region are then

[19] Year effects are not shown and are available upon request.

[20] Note that standard errors start blowing up.

decomposed into cohort, age and year effects. Table 1.7 reports the results. The observed trend in cohort partisanship is evident in all regions except the Northeast (Figures 1.9-1.12). Indeed, the reduction in Democratic support is clearest in the South with the Midwest next in line. Age effects are only evident in the Northeast, the inverted U-shape is similar to that observed for the entire sample.[21]

To further examine my findings for the South, I restrict the sample to white southerners. I find that the cohort effects are more pronounced for the white sub-sample (Figure 1.13). Moreover, the rightening was initiated with the 1930 cohort, or with whites coming of age soon after World War II. This was 15 years before the passage of the Civil Rights Act (1964) and the Voting Rights Act (1965). If the Democratic Party's stance on race is solely responsible for lost support, it is unclear why the rightening began with the 1930 cohort and not those born 15 years later. For the story to hold, it must be argued that there is something special about age 35 – the age of the 1930 cohort during the civil rights movement.

Finally, it is possible that a respondent's political affiliation is influenced by where he/she grew up rather than where he/she currently resides. To the extent that respondents migrate across census regions, this would pose a problem for my estimates. To address this issue, I conduct two robustness checks. I repeat the exercise using survey information on where the respondent grew up and obtain qualitatively similar results.[22] The same is true when I restrict the sample to natives, i.e. those who currently reside in the region where they grew up. These checks suggest that my estimates are unlikely to be biased due to migration.[23]

[21] As before, the year effects are not shown and are available upon request. Also, I do not report results by geographical region and gender/race sub-divisions for an important reason: stratification of the underlying sample comes at the expense of reduced precision of the cohort means.

[22] The NES give information on the state where the respondent grew up. If more that one state is mentioned, the state where the respondent spent the most years between the ages 8 and 16 is listed. The GSS gives the census division where the respondent lived at age 16.

[23] Results not reported here.

1.3.3 Interpretation

So far I have shown that younger cohorts have systematically reduced support for the political left. I interpret this as a decline in cohort demand for left-wing policies. The idea is that an individual's preferences are informative of his/her demand for the party's policies. The underlying assumption is that party ideology is exogenous to an individual's decision.

A left-wing political orientation is generally associated with support for redistributive policies. Next, I investigate whether the observed patterns in cohort political partisanship are apparent when I use direct evidence on individual redistributive preferences. To do so, I replace the dependent variable above with a measure of demand for redistribution. Each respondent in GSS is asked whether he/she thinks "the government should reduce income differences between the rich and the poor, perhaps by raising the taxes of wealthy families or by giving income assistance to the poor". The responses range from "should not" to "should" on a 7-pt scale. I collapse them into a 0-1 dummy variable, *redist* which takes on the value 1 when the respondent answers 5-7 and hence favors redistribution, and zero otherwise. These data are taken from the GSS alone as corresponding information is not available in the NES.[24] There are 14 cohorts and 153 pairs of cohort-year observations.[25] As before, the reference group remains the 1915 cohort.

Table 1.8 presents the results. The cohort effects are significant and show declining support for redistribution among younger cohorts (Figure 1.14). Relative to the 1915 cohort, those born around 1950 reduced support for redistributive policies by 16 percentage points and those born around 1980 did so by 26 percentage points. Unlike with political preferences, there is a significant life-cycle effect; I find evidence

[24] There are 15 years of data between 1978 and 2000 in GSS; the question was not asked in the 1982 and 1985 surveys.

[25] The cohort born in median year 1910 is absent here as these data begin six years later, in 1978. The 5-year age bands to construct cohorts remain the same.

of a linear relationship, whereby demand for redistribution declines with age. Due to limited underlying sample size, I do not provide results for population subgroups.

In sum, the observed decline in leftist political orientation among successive cohorts is mirrored in their declining support for redistribution. This indicates that the decline in Democratic support may be stemmed in reduced demand for redistribution. This check also suggests that when individual demand for redistributive policies is not directly observable, support for the political left can provide a viable proxy.

1.4 Discussion

This paper disentangles cohort, age and year effects in political partisanship in order to determine their relative importance in explaining the decline in Democratic support over the last three decades. I use the decomposition suggested by Deaton and Paxson [1994] and document significant cohort effects in political preferences, whereby younger cohorts have systematically reduced support for the Democratic Party. This trend is driven by whites and is stronger among males. Moreover, I find only weak evidence of life-cycle effects, and these are muted in comparison to cohort effects. My results for the South call into question the popular belief that the Democratic Party's stance on racial issues is the key factor driving lost Democratic support. I show that the rightening in the South was initiated with white southerners who came of age soon after World War II, fifteen years before the civil rights movement. Overall, this investigation suggests that individuals tend to retain their preferences over the life-span, and that economic and social events that affect an individual's political affiliation in his/her youth appear to have lifelong implications. Finally, direct evidence on redistributive preferences indicates that the decline in Democratic support may be rooted in a reduction in demand for redistribution.

These results suggest a need to formulate an economic explanation for the observed trend in cohort effects, and for the persistence in individual political partisanship. In Chapter 2, I show that the rise in high-school education across successive cohorts can partially explain the observed rightening.

Table 1.1: Descriptive statistics

Variable	Percent	Variable	Percent
		cohort born	
female	54.7	1908-12	1.1
white	82.4	1913-17	2.4
black	13.6	1918-22	3.5
age	39.1	1923-27	5.4
	(12.6)	1928-32	5.8
left	50.0	1933-37	6.9
redist	47.0	1938-42	8.8
		1943-47	11.9
region		1948-52	13.2
Northeast	19.9	1953-57	13.1
Midwest	26.3	1958-62	11.3
South	34.7	1963-67	7.7
West	19.1	1968-72	5.0
		1973-77	2.7
		1978-82	0.9
N	44,635		

Note: Variable descriptions are provided in Appendix A. There are 17,107 observations for redist.

Table 1.2: Cohort Definition and Average Cell Size

Cohort	Year of Birth	Age in 1972	Median Age in 1972	Average Cell Size
1	1978-82	-10 to -6	-8	142
2	1973-77	-5 to -1	-3	239
3	1968-72	0 to 4	2	281
4	1963-67	5 to 9	7	348
5	1958-62	10 to 14	12	398
6	1953-57	15 to 19	17	399
7	1948-52	20 to 24	22	403
8	1943-47	25 to 29	27	365
9	1938-42	30 to 34	32	269
10	1933-37	35 to 39	37	216
11	1928-32	40 to 44	42	206
12	1923-27	45 to 49	47	246
13	1918-22	50 to 54	52	222
14	1913-17	55 to 59	57	218
15	1908-12	60 to 64	62	164

Table 1.3: Number of individuals in selected cohorts

Year (1)	Cohort 1 (1978-82) (2)	Cohort 3 (1968-72) (3)	Cohort 5 (1958-62) (4)	Cohort 7 (1948-52) (5)	Cohort 9 (1938-42) (6)	Cohort 11 (1928-32) (7)	Cohort 13 (1918-22) (8)	Cohort 15 (1908-12) (9)
1972	0	0	0	468	393	348	341	315
1974	0	0	0	342	256	258	245	121
1976	0	0	40	483	340	248	276	57
1978	0	0	179	492	341	268	283	0
1980	0	0	281	366	239	206	224	0
1982	0	0	313	370	235	212	234	0
1984	0	0	447	453	272	221	144	0
1986	0	17	454	421	247	203	32	0
1988	0	120	426	408	208	207	0	0
1990	0	244	405	331	193	191	0	0
1992	0	189	339	217	171	136	0	0
1994	0	390	651	468	332	142	0	0
1996	11	451	589	446	291	44	0	0
1998	150	387	504	358	242	0	0	0
2000	264	449	552	425	276	0	0	0

Year indicates year of survey. For each cohort, the corresponding birth years are given in parentheses. The figures in the table reflect the number of individuals sampled in each cohort and each survey year.

Table 1.4: Number of males in selected cohorts

Year (1)	Cohort 1 (1978-82) (2)	Cohort 3 (1968-72) (3)	Cohort 5 (1958-62) (4)	Cohort 7 (1948-52) (5)	Cohort 9 (1938-42) (6)	Cohort 11 (1928-32) (7)	Cohort 13 (1918-22) (8)	Cohort 15 (1908-12) (9)
1972	0	0	0	224	179	150	165	128
1974	0	0	0	160	110	116	111	59
1976	0	0	14	215	146	120	111	27
1978	0	0	74	224	149	124	122	0
1980	0	0	116	156	111	89	106	0
1982	0	0	126	168	111	85	87	0
1984	0	0	208	175	118	115	56	0
1986	0	12	212	185	109	90	16	0
1988	0	58	178	199	94	85	0	0
1990	0	110	184	167	88	98	0	0
1992	0	91	164	120	88	69	0	0
1994	0	181	298	233	152	48	0	0
1996	4	198	251	204	140	16	0	0
1998	75	174	236	172	108	0	0	0
2000	110	206	263	194	109	0	0	0

Year indicates year of survey. For each cohort, the corresponding birth years are given in parentheses. The figures in the table reflect the number of males sampled in each cohort and each survey year.

Table 1.5: Number of females in selected cohorts

Year (1)	Cohort 1 (1978-82) (2)	Cohort 3 (1968-72) (3)	Cohort 5 (1958-62) (4)	Cohort 7 (1948-52) (5)	Cohort 9 (1938-42) (6)	Cohort 11 (1928-32) (7)	Cohort 13 (1918-22) (8)	Cohort 15 (1908-12) (9)
1972	0	0	0	244	214	198	176	187
1974	0	0	0	182	146	142	134	62
1976	0	0	26	268	194	128	165	30
1978	0	0	105	268	192	144	161	0
1980	0	0	165	210	128	117	118	0
1982	0	0	187	202	124	127	147	0
1984	0	0	239	278	154	106	88	0
1986	0	5	242	236	138	113	16	0
1988	0	62	248	209	114	122	0	0
1990	0	134	221	164	105	93	0	0
1992	0	98	175	97	83	67	0	0
1994	0	209	353	235	180	94	0	0
1996	7	253	338	242	151	28	0	0
1998	75	213	268	186	134	0	0	0
2000	154	243	289	231	167	0	0	0

Year indicates year of survey. For each cohort, the corresponding birth years are given in parentheses. The figures in the table reflect the number of females sampled in each cohort and each survey year.

Table 1.6: Decomposition of political preferences

	Dependent variable: left				
	Overall	Males	Females	Whites	Blacks
	(1)	(2)	(3)	(4)	(5)
age	0.002	0.002	0.001	0	0.005
	[0.002]	[0.002]	[0.003]	[0.002]	[0.004]
age^2	-0.000*	0	0	0	0
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
1980 cohort	-0.172***	-0.244***	-0.132**	-0.274***	0.006
	[0.030]	[0.057]	[0.055]	[0.035]	[0.130]
1970 cohort	-0.178***	-0.224***	-0.125***	-0.239***	-0.04
	[0.027]	[0.034]	[0.040]	[0.028]	[0.059]
1960 cohort	-0.158***	-0.212***	-0.118***	-0.215***	-0.034
	[0.022]	[0.028]	[0.035]	[0.022]	[0.050]
1950 cohort	-0.068***	-0.113***	-0.03	-0.105***	0.04
	[0.020]	[0.025]	[0.032]	[0.020]	[0.045]
1940 cohort	-0.072***	-0.134***	-0.021	-0.112***	0.067*
	[0.019]	[0.023]	[0.033]	[0.018]	[0.040]
1930 cohort	-0.03	-0.063**	-0.005	-0.058***	0.096**
	[0.020]	[0.027]	[0.031]	[0.020]	[0.043]
1920 cohort	0.042*	0.016	0.063*	0.028	0.089**
	[0.024]	[0.029]	[0.035]	[0.023]	[0.038]
1910 cohort	-0.049	-0.059	-0.039	-0.053	0.104
	[0.038]	[0.064]	[0.032]	[0.039]	[0.072]
Adj. R^2	0.63	0.60	0.39	0.67	0.29
F(14, 123)	12.26	14.32	5.77	21.23	3.03
Prob > F	0.000	0.000	0.000	0.000	0.001

Note: Robust standard errors in brackets. Alternate cohort dummies reported in the table for brevity, the entire vectors of cohort dummies are plotted in Figures 1.3, 1.5-1.8. The F-tests indicate whether the cohort effects are jointly significant. Year effects not reported here. N is 153.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1.7: Decomposition of political preferences, by region

	Dependent variable: left			
	Northeast	Midwest	South	West
	(1)	(2)	(3)	(4)
age	0.008**	-0.001	0	0.001
	[0.004]	[0.004]	[0.003]	[0.004]
age^2	-0.000*	0	0	0
	[0.000]	[0.000]	[0.000]	[0.000]
1980 cohort	-0.189	0.074	-0.354***	0.093
	[0.130]	[0.154]	[0.035]	[0.155]
1970 cohort	-0.049	-0.105**	-0.305***	-0.181***
	[0.064]	[0.041]	[0.036]	[0.054]
1960 cohort	-0.101*	-0.109***	-0.265***	-0.098**
	[0.059]	[0.039]	[0.028]	[0.045]
1950 cohort	-0.019	-0.017	-0.185***	0.015
	[0.057]	[0.039]	[0.027]	[0.038]
1940 cohort	-0.073	-0.056	-0.116***	-0.014
	[0.057]	[0.037]	[0.021]	[0.040]
1930 cohort	-0.097*	0.002	-0.050*	0.039
	[0.053]	[0.030]	[0.026]	[0.039]
1920 cohort	-0.013	0.109*	0.049**	-0.006
	[0.057]	[0.062]	[0.021]	[0.038]
1910 cohort	-0.148***	-0.044	-0.107*	0.113
	[0.055]	[0.046]	[0.059]	[0.074]
Adj. R^2	0.21	0.3	0.61	0.36
F(14, 123)	2.88	4.41	16.31	4.23
Prob > F	0.001	0.000	0.000	0.000

Note: Robust standard errors in brackets. Alternate cohort dummies reported in the table for brevity, the entire vectors of cohort dummies are plotted in Figures 1.9-1.12. The F-tests indicate whether the cohort effects are jointly significant. Year effects not reported here. N is 153.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1.8: Decomposition of redistributive preferences

Dependent variable: redist	
age	-0.011**
	[0.005]
age^2	0
	[0.000]
1980 cohort	-0.260***
	[0.070]
1970 cohort	-0.162***
	[0.055]
1960 cohort	-0.140***
	[0.052]
1950 cohort	-0.160***
	[0.048]
1940 cohort	-0.181***
	[0.044]
1930 cohort	-0.135***
	[0.042]
1920 cohort	-0.078**
	[0.034]
Adj. R^2	0.38
F(13, 124)	3.30
Prob > F	0.000

Note: Robust standard errors in brackets. Alternate cohort dummies reported in the table for brevity, the entire vector of cohort dummies is plotted in Figure 1.14. The F-test indicates whether the cohort effects are jointly significant. Year effects not reported here. N is 153.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 1.1: Proportion of 18-64 year olds supporting the Democratic Party

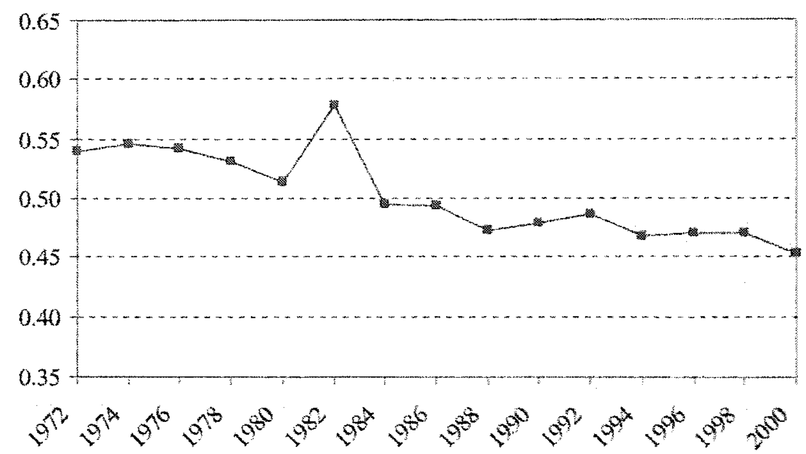


Figure 1.2: Support for the Democratic Party among selected cohorts

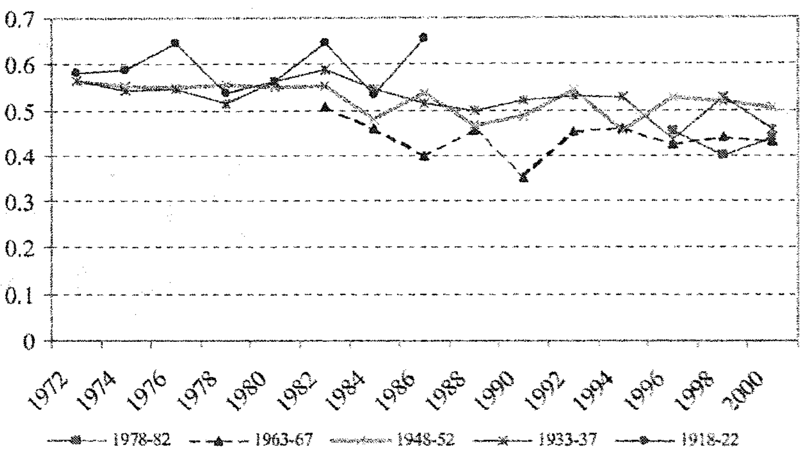


Figure 1.3: Cohort effects in support for the Democratic Party

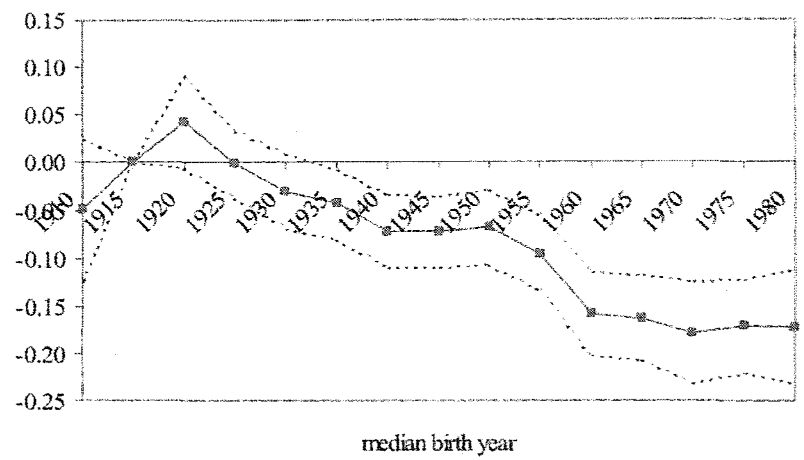


Figure 1.4: Year effects in support for the Democratic Party

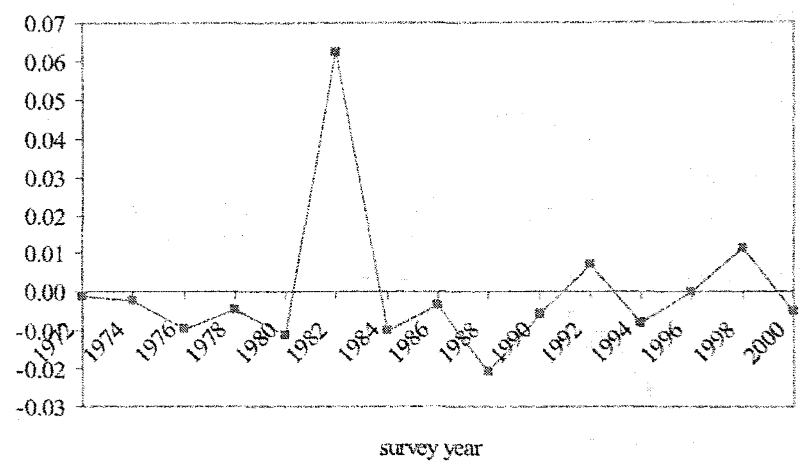


Figure 1.5: Cohort effects in support for the Democratic Party, males

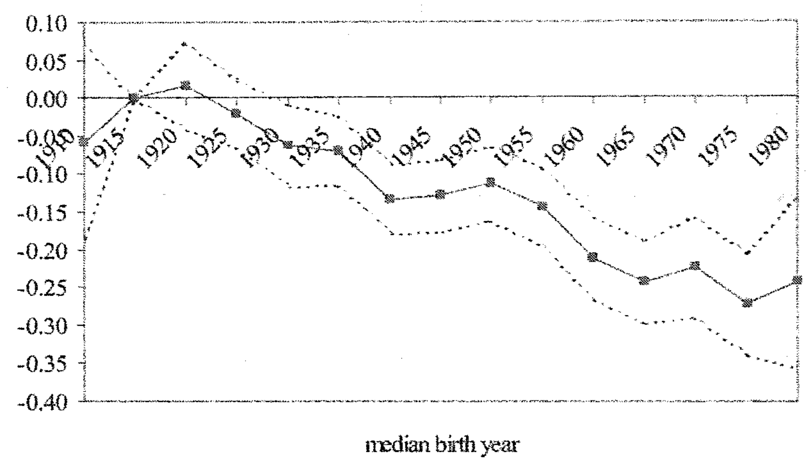


Figure 1.6: Cohort effects in support for the Democratic Party, females

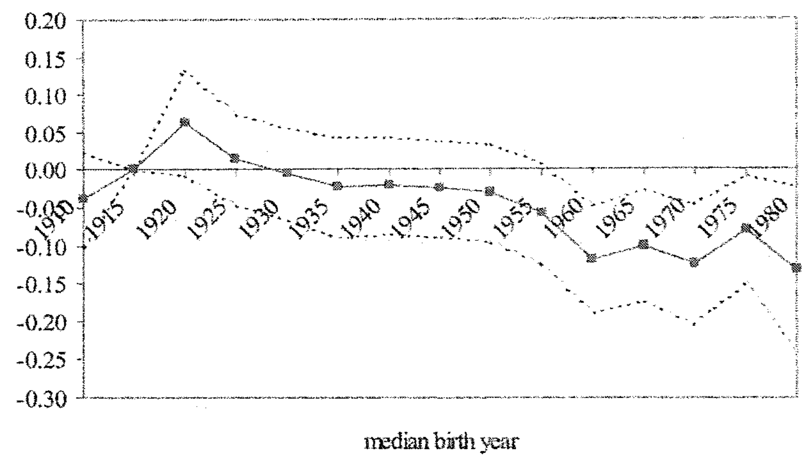


Figure 1.7: Cohort effects in support for the Democratic Party, whites

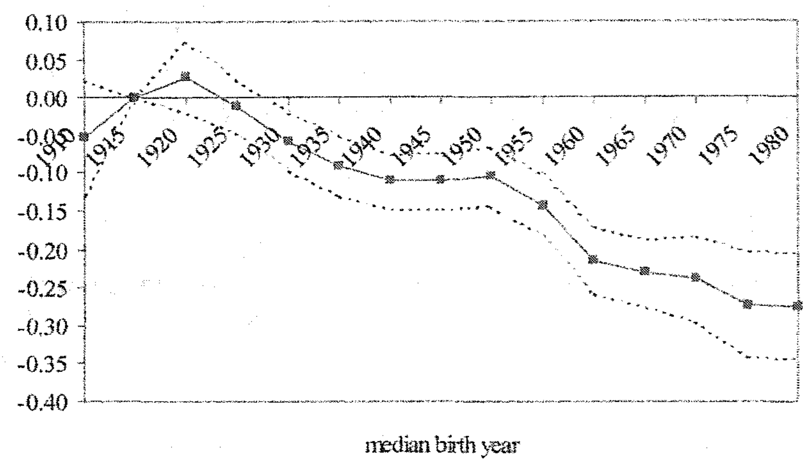


Figure 1.8: Cohort effects in support for the Democratic Party, blacks

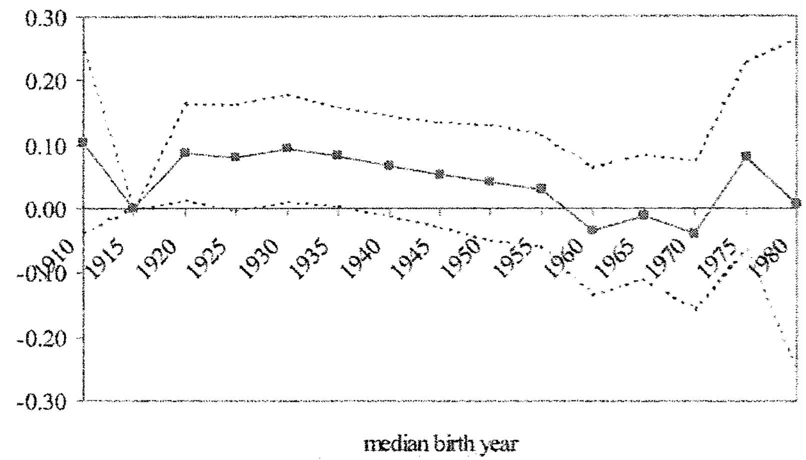


Figure 1.9: Cohort effects in support for the Democratic Party, Northeast

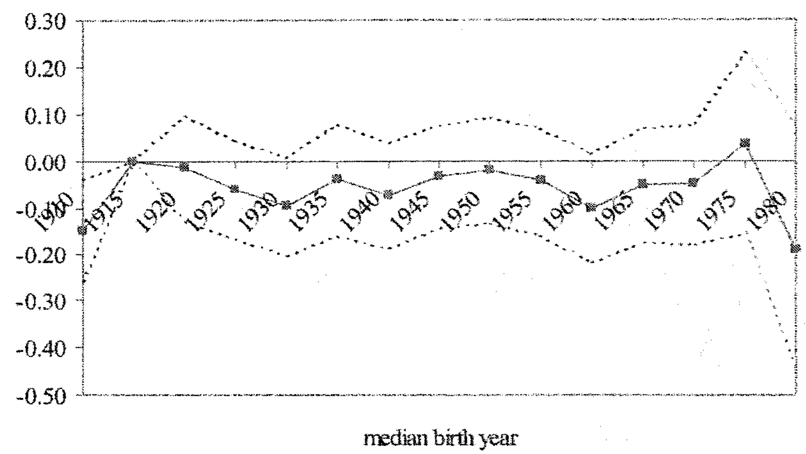


Figure 1.10: Cohort effects in support for the Democratic Party, Midwest

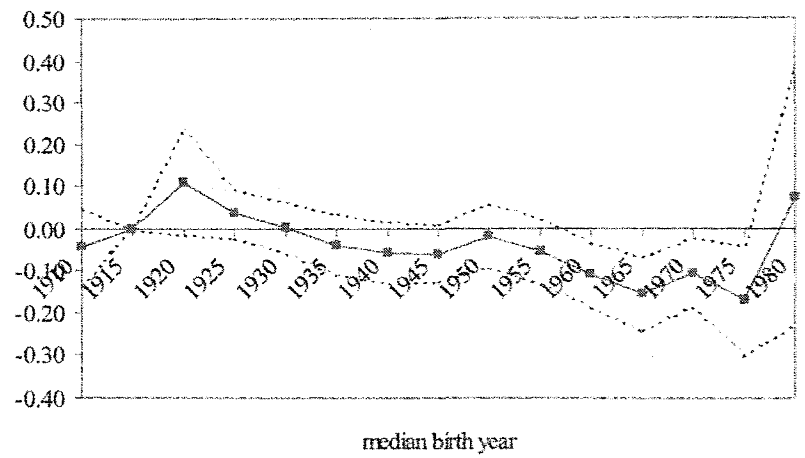


Figure 1.11: Cohort effects in support for the Democratic Party, South

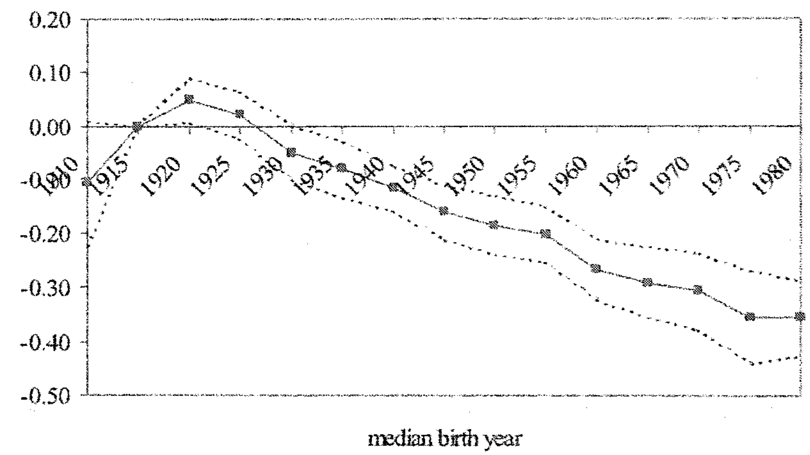


Figure 1.12: Cohort effects in support for the Democratic Party, West

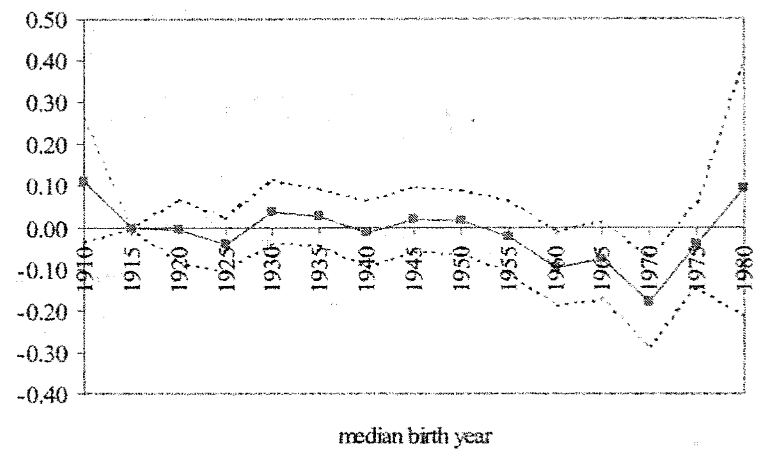


Figure 1.13: Cohort effects in support for the Democratic Party, white southerners

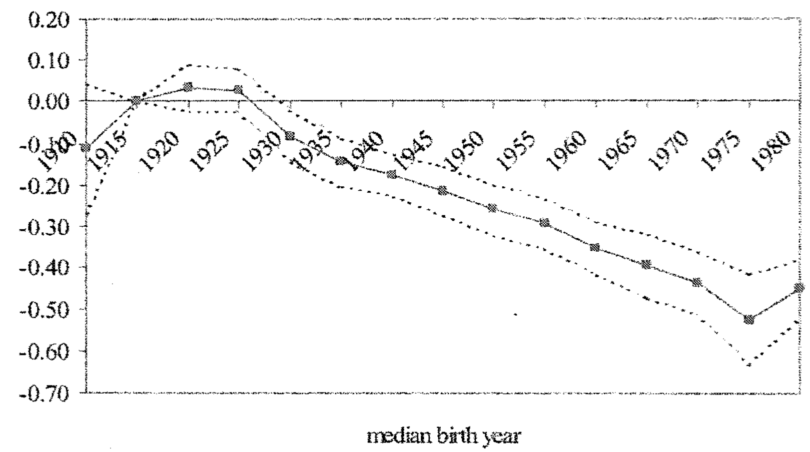
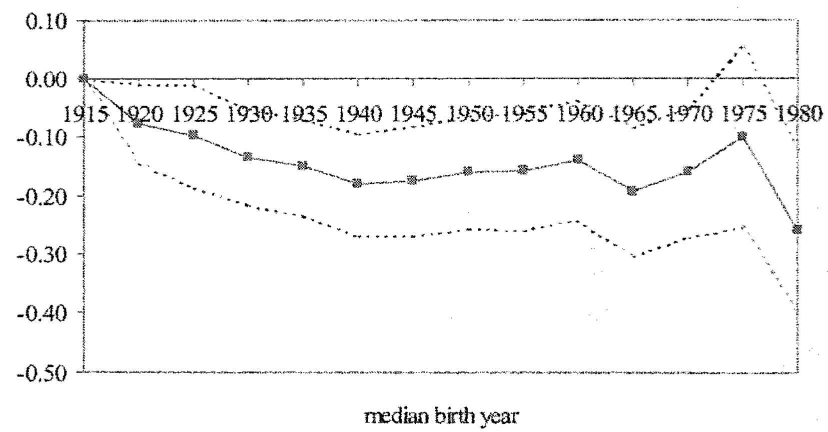


Figure 1.14: Cohort effects in demand for redistribution



Chapter 2

The Impact of High-School Education on Political and Redistributive Preferences

2.1 Introduction

Popular support for the Democratic Party has seen a steady decline over the last half century. According to the National Election Studies, the proportion of 18-64 year olds who favored the Democratic Party declined from 61 percent in 1952 to 49 percent in the year 2000 (Figure 2.1).[1] What drove this decline? Was it the aging of the population? In Chapter 1, I found little evidence of life-cycle or age effects in political preferences. Instead, there were important cohort effects. Younger cohorts have systematically reduced support for the Democratic Party relative to their older counterparts, hence producing a "rightening" of the electorate (Figure 2.2). The reduction in Democratic support is most pronounced for individuals born between

[1]Respondents are asked their party preference on a seven-point scale ranging from Strong Democrat to Strong Republican. An individual is considered a Democrat if he/she claims to be a Strong, Weak or Independent-leaning Democrat.

1908 and 1967. Of individuals born in 1908-12, 56 percent favored the political left. Only 45 percent of individuals born in 1963-67 did so.[2]

Why would younger cohorts reduce support for the Democratic Party? In terms of social issues, younger cohorts tend to be more liberal (Figure 2.3 graphs cohort attitudes toward abortion, women's rights, civil rights and church attendance).[3] These values are at odds with the social conservatism embraced by the Republican Party. This suggests that the decline in Democratic support is not driven by generational differences in social attitudes, which leaves the left-right divide on state redistribution as a plausible candidate.[4] If so, why would younger cohorts be less in favor of redistribution? This paper proposes that the rise in high-school education may be part of the explanation.

The United States experienced a rapid surge in high-school education during the first part of the 20th century. Enrollment rates increased from 10 to 90 percent, and graduation rates from 5 to 65 percent between 1910 and 1960 [Goldin 1998]. Figure 2.4 summarizes the high-school experiences of birth cohorts, as documented in the National Election Studies. Of individuals born between 1908-12, 66 percent attended high school and 46 percent graduated. The corresponding figures for the cohort born in 1963-67 are 98 and 88 percent respectively. In fact, there are negligible or no gains in high-school education for individuals born afterwards.[5] According to Goldin [1998], the "high-school movement" was responsible for the bulk of human capital attainment in the 20th century and the key factor for growth in per capita

[2]Proportions are computed over the period 1952-2000.

[3]Younger cohorts are more likely to be pro-choice, they are also more supportive of women's rights and civil rights. Additionally, they exhibit less religiosity as measured by church attendance.

[4]The assumption that an individual's political preferences reflects his/her redistributive preferences and thereby his/her economic status is a common assumption in economic models of individual voting behavior (Downs [1957]). Persson and Tabellini [2000] provide a recent overview of the literature. Recent studies that have used support for the political left as a proxy for increased demand for redistribution include Edlund and Pande [2002] and Alesina and Angeletos [2003].

[5]High-school graduation rates taper off for the youngest cohorts. Additionally, it is worth noting that there are no gender differences in cohort patterns of high-school attendance and graduation.

incomes over this period. Moreover, individual decisions regarding high-school education are typically realized by early adulthood – this coincides with the time an individual enters the electorate and forms his/her political preferences.

There are (at least) two reasons why cohorts with more high-school education would reduce support for redistribution. First, when such cohorts enter the electorate, they are better educated than existing cohorts, which may lead them to view themselves as upwardly mobile. Thus, younger cohorts may view redistribution less favorably because they stand to lose from higher tax rates. Second, having faced more equal educational opportunities themselves, younger cohorts may find a social safety net less warranted. In other words, greater equality of opportunities may have reinforced the belief that prevailing differences in income status are "fair" and stem from differences in effort and talent, making younger cohorts more tolerant of inequality of outcomes. Alesina and La Ferrara [2001] provided evidence supporting both channels. They showed that the higher the likelihood that an individual will become "rich", the lower his/her support for redistribution.[6] They also showed that those who believe that the United States is a land of "equal opportunities" view redistribution less favorably.[7]

Using the National Election Studies survey data, this paper first documents the presence of significant cohort effects in political preferences. Next, I investigate the impact of high-school education on redistributive preferences as captured by support for the Democratic Party and, more directly, by support for increased government spending. This permits me to examine the role of the high-school movement in driving the observed trend in cohort politics.

An immediate concern in such an investigation is the endogeneity of educational choices. In all likelihood, an individual's schooling and redistributive prefer-

[6]See Benabou and Ok [2000] for a model of the "prospect of upward mobility" hypothesis. The basic relationships between income distribution and redistributive policies are examined in seminal work by Romer [1975] and Meltzer and Richard [1981].

[7]They provided cross-sectional evidence and did not focus on inter-cohort differences.

ences are jointly determined by unobserved factors, family background in particular. If so, then an observed association between the two is not necessarily causal. To address this issue, I instrument high-school education using the passage of compulsory schooling laws across US states.[8] While younger cohorts were subject to more stringent laws, the tightening of these laws varied across states, yielding state and cohort variation in individual exposure to schooling restrictions. I exploit this variation to identify the effect of high-school education.

My findings suggest that individuals who attended or graduated from high school reduced support for redistribution, both as measured by support for the Democratic Party and for government spending. Relative to an individual who did not attend or graduate from high school, a male induced to do so by compulsory schooling laws was 42-51 percent less likely to favor the Democratic Party. The effect is smaller among females; it is in the 24-35 percent range. Back-of-the-envelope calculations indicate that the rise in schooling due to compulsory schooling laws can account for 10-25 percent of the decline in Democratic support.

The reduced allegiance to the Democratic Party is particularly striking given the well-documented rise in income inequality during the last thirty years [Piketty and Saez 2003]. This period witnessed a marked increase in the concentration of incomes. One may expect the rise in inequality to be coupled with an increase in demand for redistribution, but that has not been the case. On the contrary, demand for redistribution, as reflected in the support for the Democratic Party, decreased. Moreover, evidence on taxes is also in line with the reduced demand for redistribution – there was a sharp decline in progressive taxation during the 1980s which has not rebounded.[9] This puzzling development has led people to conclude that social norms must have changed; acceptance of inequality has increased among the electorate (see

[8]State-level compulsory schooling laws have been used in the literature as instruments for high-school education. See Angrist and Krueger [1991]; Acemoglu and Angrist [2000]; Lochner and Moretti [2001]; Lleras-Muney [2002a], and Milligan, Oreopoulos, and Moretti [2003].

[9]See Saez [2003] for data on marginal tax rates for top income groups.

Krugman (New York Times, October 21, 2002) and Piketty and Saez [2003]). My findings suggest that the high-school movement may be the catalyst of this change.

The remainder of the paper is as follows. Section 2.2 discusses the related literature, section 2.3 describes the political survey data and section 2.4 documents the rightening of cohorts and the rise in high-school education in these data. Section 2.5 describes compulsory schooling laws and outlines the identification scheme. Section 2.6 presents the main results for the relationship between high-school education and political/redistributive preferences. Finally, section 2.7 provides robustness checks and section 2.8 concludes.

2.2 Related Literature

The increasing support for the Republican Party among males in the United States has been documented by political scientists (Wirls [1986] and Kaufmann and Petrocik [1999]). They argued that the growing gender gap stems from the changing politics of men rather than women. Edlund and Pande [2002] showed that the growth of the political gender gap is linked to the decline in marriage as measured by divorce incidence. Recently, popular press has highlighted the reduced allegiance to the Democratic Party among young blacks (New York Times, August 8, 2003). The reduction in support for the Democratic Party among southern cohorts has also received significant attention in political science. Carmines and Stimson [1989] claimed that the Democratic Party's stance on liberal racial policies beginning in 1964 drove southerners to the Republican Party. Green, Palmquist, and Schickler [2002] argued that the passage of the Voting Rights Act of 1965, which brought large proportions of blacks into the voting booths and to the Democratic Party, changed the image of the Republican Party for southerners. Southern cohorts turned Republican gradually as younger generations with a new understanding of party images replaced older ones who held on to traditional conceptions. According to these views, changes in party platforms with respect to civil rights underlie the trend in cohort politics.

A large literature in political science also documents the importance of generational or cohort effects in political preferences. The political leanings of certain age groups are shaped by important historical events in their youth. For instance, individuals who came of age during the Great Depression exhibit stronger allegiance to the Democratic Party. In contrast, those who came of age during the "optimistic years of the early Reagan administration" lean strongly toward the Republican Party [Erikson and Tedin 2001]. The decline in Democratic support is often interpreted as the fading of the Depression era. My findings indicate that cohort effects in political preferences do not strictly arise from single events that distinctively stamp a group of individuals. I provide evidence that factors potentially affecting an individual's expected relative position in the income distribution during early adulthood play an important role.

This paper also contributes to the growing economics literature on social and non-market returns to education. Recent studies have examined social returns [Acemoglu and Angrist 2000], crime-reduction [Lochner and Moretti 2001], adult mortality [Lleras-Muney 2002a] and citizenship [Milligan, Oreopoulos, and Moretti 2003]. Finally, it should be noted that political scientists have studied the impact of education on political preferences. They suggest that a college education is correlated with liberal views on social issues [Feldman and Newcomb 1969], [Kesler 1979], [Nie, Junn, and Stehlik-Barry 1998]. This is not true, however, for opinions related to government spending.[10]

2.3 Individual-level data

My analysis uses data from the biennial National Election Studies (NES) which cover the period 1952-2000. These surveys are independent repeated cross-sections of

[10]See Erikson and Tedin [2001] and Page and Shapiro [1992].

individuals and provide information on an individual's political behavior and social attitudes and his/her demographic and economic characteristics.[11] I use 22 survey rounds and restrict my sample to the birth years 1908-1982.[12] This leaves me with approximately 1,400 respondents per survey.[13]

The NES ask respondents for their party preference on a seven-point scale ranging from Strong Democrat to Strong Republican. I collapse the responses into a 0-1 dummy variable, *left* that takes on the value 1 when the individual is a Strong, Weak or Independent-leaning Democrat, and the value 0 otherwise. The average proportion in favor of the Democratic Party in these surveys is 53 percent. I use information on partisan identification rather than actual voting to avoid sample selection issues related with use of the latter. To confirm that an individual's political preferences are aligned with his/her redistributive preferences, an alternate dependent variable on preferences over government spending is also used. A dummy variable, *govspend* is created for a direct measure of redistributive preferences; it takes the value 1 if the individual would like government to provide many more services and increase spending a lot, and zero otherwise.[14] These data are only available since 1982. I also use information on individual preferences over defense spending to qualify my findings.

An individual's high-school education is captured using two dummy variables: (i) high-school attendance which takes on the value 1 if the respondent attended high school at some point in his/her life, and zero otherwise (ii) high-school graduation

[11]Data are collected using telephone and in-person interviews.

[12]There is no survey in 1954. Moreover, the 1962 and 1998 surveys do not provide information on the US state where the respondent grew up.

[13]Respondents below the age of 18 years are excluded.

[14]It is implicit that the individual supports higher taxes. Respondents are asked their opinions on the following statement: "Some people think the government should provide fewer services, even in areas such as health and education, in order to reduce spending. Other people feel that it is important for the government to provide many more services even if it means an increase in spending."

which takes on the value 1 if the respondent graduated from high school, and zero otherwise. In my sample, 89 percent of individuals attended high school and 74 percent graduated from high school.

Finally, I use attitudinal questions on social values and religiosity. These include individual attitudes toward abortion, women's rights, civil rights and church attendance. Table 2.1 provides descriptive statistics, and Appendix B details on variable construction.

2.4 Stylized facts for cohorts

This section documents the rightening of cohorts and illustrates the rise in high-school education among them. To examine cohort effects in political affiliation, I estimate a linear regression of the form

$$left_{icst} = r_s + g_t + \alpha_1 b_c + \alpha_2 X_{icst} + \varepsilon_{icst}, \tag{2.1}$$

where $left_{icst}$ is a dummy variable that takes on the value 1 if individual i of cohort c from state s and year t supports the Democratic Party, and zero otherwise. b_c is a vector of cohort dummies (defined by 5-year birth intervals), r_s is a set of state dummies, g_t a vector of year dummies and X_{icst} a vector of individual-level controls including a quadratic in age, gender and race. The year dummies capture any year-specific factors that affect the electorate's support for the left, including say changes in party platforms. The vector α_1 gives the average cohort effects in political preferences. The linear relationship between a respondent's year of birth, his/her age and the year of survey implies that I cannot include all three in a flexible form. Since my focus is on cohort effects in political partisanship, I present results with either cohort dummies and a set of year dummies or cohort dummies and a quadratic in age (Table 2.2). For brevity, the coefficient for every other cohort dummy is reported.

The results in column (1) indicate that relative to the omitted cohort (1910), younger cohorts have increasingly reduced support for the Democratic Party.[15] The vector α_1 is plotted in Figure 2.5. The cohort effects are negative and significantly different from zero at the 5 percent level beginning with the 1935 cohort.[16] For instance, relative to individuals born around 1910, the 1965 cohort reduced support for the political left by 14 percentage points. Column (2) reports the corresponding results with the inclusion of year dummies instead of a quadratic in age (see Figure 2.6). We observe a similar trend although the cohort effects are smaller in magnitude – the 1965 cohort reduced support for the political left by 11 percentage points.

Next, I examine cohort effects in political preferences by gender and race. The results are displayed in columns (3)-(8). There is evidence of a pronounced rightening among male cohorts but not females. This is consistent with previous findings that showed that males rather than females have reduced support for the Democratic Party [Wirls 1986], [Kaufmann and Petrocik 1999].[17] There appears to be no evidence of a robust trend effect in the case of blacks. Column (7) indicates increased support for the Democratic Party among younger black cohorts. However, we observe the opposite when year dummies are included – column (8) shows that younger black cohorts reduced support for the political left.[18]

In sum, Table 2.2 documents the fact that younger cohorts in the United

[15]Throughout the paper, I refer to a 5-year birth interval using the median birth year, the 1910 cohort refers to individuals born in 1908-12 and so on.

[16]The 1980 cohort is an exception.

[17]A possible reason for the gender gap is that the decline in marriage has increased the demand for redistribution among females relative to males (see Edlund and Pande [2002] and Edlund, Haider, and Pande [2004b]).

[18]Further investigation reveals that the observed leftening in column (7) is not robust to the inclusion of additional individual controls. Magnitudes of the coefficients and significance levels decline for most cohort dummies once we control for the respondent's education level. The rightening of black cohorts reported in column (8), however, remains robust to the inclusion of controls. It should be noted that the remaining results reported in Table 2.2 are robust to the inclusion of additional controls such as marital status, educational attainment, place in income distribution, religious affiliation, etc. Regressions are not reported here and are available upon request.

States have increasingly reduced support for the political left (redistribution).[19] This trend is strongest for white males. The cohort effects in political partisanship are statistically significant and the trend remains unexplained when I include individual-level characteristics.

The presence of significant cohort effects combined with the near absence of age effects indicates a degree of persistence in individual party affiliation. This suggests that an individual's experiences in early adulthood (around age 18 or 19) matter for the formation of his/her political preferences.[20] Individual investment in high-school education is typically realized by early adulthood; this coincides with the time when an individual determines his/her political preferences. Moreover, this level of schooling is important because a high-school graduate can continue educational investment in human capital while a high-school dropout cannot. Goldin [1998] has argued that the high-school movement in the United States is responsible for the bulk of the human capital attainment in the 20th century. For these reasons, it is relevant to examine how the rise in high-school education among successive cohorts impacts redistributive preferences.

To illustrate the inter-cohort rise in high-school education, I plot cohort effects in high-school attendance and graduation in Figure 2.7.[21] The coefficients are significantly different from zero at the 1 percent level for all birth cohorts. For instance, relative to the omitted cohort (1910), individuals born around 1950 are 31 percent more likely to attend high school and 42 percent more likely to graduate.

In the next section I investigate the relationship between individual educational

[19]Since data on *govspend* are only available for a limited time period, I only show cohort effects for political preferences.

[20]Evidence using data on synthetic cohorts provided in Chapter 1 supports this claim. I followed the methodology in Deaton and Paxson [1994] and decomposed political preferences into cohort, age and period effects.

[21]The coefficients plotted here are obtained from a linear regression where individual high-school education is regressed on a vector of cohort dummies (defined by 5 year birth intervals), a set of state dummies and individual-level controls. The two dummies for high-school education are attendance and graduation.

attainment and redistributive preferences. In the absence of explicit randomization in individual schooling choices, a natural fallback is to exploit variation in schooling across individuals generated by policy changes. The passage of compulsory schooling laws across US states provides such an opportunity.

2.5 Institutional background and identification strategy

This section describes the compulsory schooling laws and outlines the empirical framework to study the relationship between schooling and redistributive preferences.

2.5.1 Compulsory schooling laws

Using the variation in high-school education induced by the passage of state-level compulsory schooling laws to study political/redistributive preferences is attractive for multiple reasons. First, states adopted and changed these laws at different points in time generating significant variation across cohorts and regions. Individuals were exposed to compulsory schooling laws that varied in terms of how strict they were based on birth year and state of residence during schooling. Second, changes in compulsory schooling laws in the first part of the 20th century coincided with the surge in high-school education in the United States.[22] Third, while the passage of these laws was possibly determined by the social climate at the time, the laws that affected an individual's schooling are unlikely to be affected by his/her future political/redistributive preferences. In other words, I examine individual political affiliation and preferences over government spending during adulthood, years after

[22]Goldin and Katz [2003] found a positive and statistically significant impact of schooling laws on contemporaneous enrollments and educational attainment. Their estimates indicate that these laws had a modest effect compared to the large increase in high-school enrollment and educational attainment during the period 1910-1940.

their schooling was dictated by the compulsory schooling laws in place.

The impact of state-level compulsory schooling laws on high-school education has been documented in the literature (see Schmidt [1996], Acemoglu and Angrist [2000]; Lochner and Moretti [2001]; Lleras-Muney [2002b] and Goldin and Katz [2003]).[23] Lochner and Moretti [2001] and Lleras-Muney [2002b] confirmed that the laws are exogenous to schooling – more restrictive laws appear to increase education and not vice versa. The list of studies that use these laws as an instrument for high-school education is growing.[24]

I obtained state-level data on compulsory attendance and child-labor laws from Acemoglu and Angrist [2000]. These provide information over the period 1914-1978 on the maximum entrance age (*enter*), the minimum dropout age (*drop*), the minimum schooling required to drop out (*reqsch*), the minimum age to obtain a work permit (*work*) and the minimum schooling required to obtain a work permit (*reqwork*). The difference between *work* and *enter* would give us the years of compulsory schooling to obtain a work permit. This definition would, however, overlook an important constraint that was a part of child-labor laws. In addition to the age requirement, several states mandated a minimum amount of schooling to obtain a work permit. This often exceeded the difference between the work permit age and the school entry age. Following Acemoglu and Angrist [2000], I account for the additional restriction and summarize child-labor laws into a single variable, *Labor*:

$$Labor_{st} = \max\{reqwork_{st}, (work_{st} - enter_{s,t-8})\}$$

Labor in state s and year t is defined as the maximum of the required schooling

[23]For more information on compulsory schooling laws, see Lleras-Muney [2002b] and Goldin and Katz [2003].

[24]See Angrist and Krueger [1991], Acemoglu and Angrist [2000], Lochner and Moretti [2001], Lleras-Muney [2002a] and Milligan, Oreopoulos, and Moretti [2003]. My methodology is similar to that adopted by Milligan, Oreopoulos, and Moretti [2003]. They also use NES data in their study of US voter turnout.

to obtain a work permit and the difference between the work permit age and the entrance age. The maximum of the two yields the effective restriction facing the youth in his/her state. Similarly, compulsory attendance laws are summarized into the variable, *Attend*:

$$Attend_{st} = \max\{reqsch_{st}, (drop_{st} - enter_{s,t-8})\}$$

Attend in state s and year t is the maximum of the required schooling to drop out and the difference between the drop-out age and the entrance age. Following Goldin and Katz [2003], both measures account for the entrance age 8 years prior to when the youth could drop out of school (to work or otherwise).[25]

Figures 2.8-2.10 present state-level data on the laws. There is significant variation over time and across states in the specifications of these laws. Table 2.3 outlines for each census region and time period the number of states that changed laws at least once. This confirms that changes in laws were not confined to the pre-World War II era; several states changed laws in the 1950s, 1960s and 1970s.

The NES provide information on the state where the individual was schooled.[26] I match each respondent with the compulsory attendance and child-labor laws that would have affected him/her at age 14.[27] In other words, each individual is matched with the maximum entrance age observed at age 6 and all other aspects of the laws observed at age 14.[28] Figure 2.11 presents national trends in *Labor* and *Attend*. These indicate that younger cohorts generally experienced stricter laws.

[25]Previous studies inaccurately used the same year for the entrance, drop-out and work permit ages.

[26]More specifically, the NES give information on the state where the respondent grew up. If more that one state is mentioned, the state where the respondent spent the most years between the ages 8 and 16 is listed.

[27]The matching is done by year of birth and state of schooling.

[28]I choose age 14 following the lead of previous researchers. See Schmidt [1996], Acemoglu and Angrist [2000], Lleras-Muney [2002a], Goldin and Katz [2003], Milligan, Oreopoulos, and Moretti [2003] and Oreopoulos [2003].

Data on compulsory schooling laws permit the study of cohorts born in 1908-1964. The distributions of *Labor* and *Attend* are captured using four dummies for each. For *Labor* these are: *Labor6* (0-6 years), *Labor7* (7 years), *Labor8* (8 years) and *Labor9* (9 or more years). Similarly, for *Attend* these are: *Attend8* (0-8 years), *Attend9* (9 years), *Attend10* (10 years) and *Attend11* (11 or more years). The bottom panel of Table 2.1 reports the proportion of individuals in each group.

For compulsory schooling laws to constitute valid instruments, they should induce an increase in schooling and satisfy the exclusion restriction – the laws should only affect political and redistributive preferences through schooling. A number of papers have confirmed the former and I provide multiple checks on the exclusion restriction. Before turning to these checks, it is worth noting some features of my estimation framework that limit the set of omitted variables likely to pose a threat to identification. First, I do not examine political and redistributive preferences of individuals who voted for the passage of these laws. They were too young to vote. Second, identification of the effect of schooling on redistributive choices comes from changes in compulsory schooling requirements within a given state. Violation of the exclusion restriction therefore requires that shifts in political climate within a state influence both the passage of stricter laws and future political/redistributive preferences of individuals. In other words, omitted state-specific and cohort-specific factors are accounted for by fixed effects and are not problematic for identification.

There remains a concern about cohort-specific and state varying variables that may affect both the passage of the laws and redistributive concerns. To investigate this, I examine the role of two such factors in driving the passage of the laws. First, it is plausible that parents who voted for stricter laws also influenced their children's attitudes toward politics and redistribution in a particular direction. For instance, if Republican parents voted for stricter laws and also encouraged their children to support the political right, my estimates for the impact of high school will be biased upward. To examine this, I test whether parental party affiliation is correlated with the passage of stricter laws.[29] Results are reported in Table 2.4, Panel A. I find no significant relationship between parental partisanship and changes in the laws. Second, I investigate this possibility using state rather than parental party affiliation. I use data on governor party identification and test whether Democratic governors are more or less likely to pass stricter laws.[30] Results reported in Table 2.4, Panel B indicate that there is no significant relationship.

2.5.2 Identification strategy

Younger cohorts within a state generally observed more stringent laws relative to older ones. Moreover, cohorts in some states experienced stricter laws than their counterparts in other states. Then, to the extent that more stringent compulsory schooling laws translate to a greater likelihood that an individual attends high school, I can exploit the exogenous variation in schooling generated by these laws and compare political/redistributive preferences across individuals. In other words, I compare political/redistributive choices for individuals who faced stricter compulsion laws and were thereby forced to receive more schooling with those who experienced lenient laws and were not mandated to do the same. The key attraction of this framework is that it circumvents concerns about endogenous schooling choice, a possibility that cannot be ruled out on an a priori basis.

To examine whether (i) high-school education reduced support for redistribution (ii) the rise in high-school education among successive cohorts can explain the observed rightening of cohorts, I introduce information on individual high-school education in equation (3.3.1):

[29]The NES ask respondents for the party affiliation of their parents. The information is available in 11 of the 22 survey rounds I use in the paper. To be clear, I know the compulsory schooling laws that would have affected a respondent at age 14. I test whether party affiliation of the parents is correlated with changes in the laws.

[30]Data on gubernatorial elections are from Wolfers [2002].

$$left_{icst} = r_s + g_t + \alpha_1 b_c + \alpha_2 X_{icst} + \alpha_3 (HS)_{icst} + \alpha_4 (female \times HS)_{icst} + \varepsilon_{icst}, \quad (2.2)$$

A respondent's high-school experience, HS is captured either using a dummy for attendance or a dummy for graduation. To examine gender differential effects of high-school education on redistributive preferences, an interaction between the high school dummy and the female dummy is included. I also estimate a corresponding specification where the dependent variable, *left* is substituted by an alternate measure of redistributive preferences, *govspend*.

For comparison, I first present OLS estimates for equation (3.3.1). Next, I estimate the equation using 2SLS where individual high-school education is instrumented with the compulsory schooling law dummies introduced above. An individual's schooling decision is considered potentially endogenous and I exploit the exogenous variation in schooling generated from compulsory schooling laws. The results are laid out in the following section.

2.6 Results

2.6.1 OLS estimates

OLS estimates of high-school education on political/redistributive preferences are depicted in Table 2.5.[31] Column (1) indicates that males who attended high school reduced support for the Democratic Party by 6 percentage points. Females who attended high school were 8 percent more likely to support the political left than males. Both effects are significant at the 1 percent level. F-tests reveal that the overall impact of high-school attendance on female political leanings is not significant.

[31] To maintain comparability between the estimates presented here and the reduced-form and IV estimates given below, the sample is restricted to birth cohorts 1908-1964. These are the individuals for whom I have data on compulsory schooling laws.

Column (2) reports the effect of high-school graduation – male high-school graduates reduced support for the Democratic Party by almost 10 percentage points. Female graduates increased support relative to male graduates but as before, F-tests indicate that graduation does not have a significant effect on female political affiliation.

Columns (3)-(4) show corresponding results using *govspend* as the dependent variable. Those who attended high school reduced support for increased government spending by 15 percentage points and there is no evidence of a significant gender differential effect, column (3). Male high-school graduates reduced support by 16 percentage points and females by 12 percentage points, column (4). These effects are significant at the 1 percent level.[32]

There are multiple reasons to exercise caution when interpreting these findings. First, schooling choice is potentially endogenous and the observed relationship is not necessarily causal. If unobserved characteristics determine an individual's schooling and shape his/her political/redistributive preferences, OLS estimates will be biased. Moreover, the direction of the bias is unclear. Two important variables of concern are parental influence and geographic causes. Unobserved family characteristics may determine both an individual's schooling and his/her attitudes toward redistribution and politics. Say, rightwing parents encourage their children to attend high school and influence their political choices in the same direction. Then, the OLS estimates would be biased upward. Also, we know that the west and east coasts tend to be more left-wing and more educated than middle America. A possible explanation for the correlation is that the demand for public goods is higher in cities; individuals who live in cities support larger government and are more educated. In this case, not accounting for location would bias the OLS coefficient downward.[33] Second, the

[32]The regressions in Table 2.5 include cohort dummies and year dummies. The results with a quadratic in age instead of year dummies yield similar results and are not reported here. The only difference is that there is no significant gender differential effect of high-school graduation on *govspend*.

[33]The NES provide information on whether the individual resides in a city or elsewhere. When I include this information, I find support for the claim that individuals in cities are more likely

OLS estimate gives the average effect of high-school education across all cohorts and states. The marginal impact of high school on political/redistributive preferences is likely to differ across cohorts – the OLS estimate averages across the marginal effects. It is not surprising then that individual high-school experience leaves the cohort trend in politics almost unaltered and unexplained. Individual-level schooling does not capture the important cohort- and state-varying effect of human capital accumulation on political preferences which I argue are underlying the observed trend in cohort politics.

For these reasons, I now consider IV estimates of high-school education on support for redistribution. The use of compulsory schooling laws across US states generates exogenous variation in schooling permitting me to address concerns about causality and endogenous schooling choice outlined here.

2.6.2 IV estimates

I estimate equation (3.3.1) using 2SLS where individual high-school education is instrumented by compulsory schooling laws. There are two endogenous regressors, *HS* and *female*HS* in the specification and the order condition for identification requires that there be at least two instruments. I use dummies for compulsory attendance and child-labor laws, alone and interacted with the female dummy, as instruments.[34] As before, *HS* is measured using both high-school attendance and graduation. The results are in Tables 2.6 and 2.7.

The first stage gives the relationship between high-school education and com-

to lean to the left. Moreover, it's inclusion does slightly raise the OLS estimates of high-school attendance/graduation. I purposely exclude this information in my main specification – location is potentially endogenous as it may well be that left-wing individuals prefer to live in cities. For the same reason, I do not include other individual-level characteristics such as marital status, labor force participation and income.

[34] More specifically, the set of instruments are the dummies *Labor7, Labor8, Labor9, female*Labor7, female*Labor8, female*Labor9, Attend9, Attend10, Attend11, female*Attend9, female*Attend10* and *female*Attend11*.

pulsory schooling laws (Table 2.6). Let us first examine high-school attendance. Since there are two endogenous regressors, the first stage consists of two regressions. Column (1a) gives the effect of child-labor laws, as captured by *Labor* and *female*Labor* dummies, on *HS* attendance. The omitted category is the least restrictive, *Labor6*. Then, relative to males who were subjected to 0-6 years of compulsory schooling to obtain a work permit, those exposed to 7 years of such were 5 percent more likely to attend high school. Similarly, males exposed to 8 years were 9 percent more likely and those exposed to 9 or more years were 12 percent more likely. The effects of the laws are similar among females, albeit muted relative to males. Females subjected to 7 years of schooling to obtain a work permit were 2 percent more likely to attend high school than those exposed to 0-6 years. Females exposed to 8 years were 4 percent more likely and those exposed to 9 or more years were 7 percent more likely. F-tests confirm that these effects are significant. Column (1b) gives the effect of child-labor laws on *female*HS*. In general, individuals facing more stringent child-labor laws were more likely to attend high school. This is in line with previous findings which show that the laws induced an increase in educational attainment.

The corresponding second-stage results are given in Table 2.7, column (1). Identification comes from changes in the number of years of compulsory schooling experienced by youth in any given state. According to the IV estimates, relative to males who did not attend high school, males induced to attend due to child-labor laws reduced support for the Democratic Party by 51 percentage points. The corresponding effect is weaker for females – they reduced support for the left by 30 percentage points. The effects are statistically significant at the 5 percent (10 percent) level for males (females).

Table 2.7, column (2) reports IV estimates where the instruments are *Attend* and *female*Attend* dummies – males induced to attend high school significantly reduced support for the Democratic Party by 49 percent. The effect is not significant for females. Finally, when I use the entire set of dummies as instruments, my IV estimates are 48 percent for males and 24 percent for females (column (3)).[35]

The results for high-school graduation are given in Table 2.7, column (4)-(6) with corresponding first stage estimates in Table 2.6. The results are similar; IV estimates of high-school graduation are in the 42-49 percent range for males and 24-35 percent range for females.[36] My findings suggest that high-school education did reduce Democratic support – individuals induced to attend or graduate from high school due to the laws significantly reduced support for the political left. Back-of-the-envelope calculations indicate that the rise in high-school education due to compulsory schooling laws can explain 10-25 percent of the decline in Democratic support.

A look at the cohort dummies once high-school education is instrumented by the laws reveals that almost all of them are rendered insignificant and there is no longer an unexplained trend in cohort politics. This evidence suggests that the rise in high-school education among those affected by the laws is an important factor underlying the observed rightening of cohorts.

I also provide corresponding IV estimates of high-school education on support for increased government spending, Table 2.8. The results indicate that individuals induced to attend or graduate from high school were less likely to favor an increase in government expenditure. The estimates are in the 39-71 percent range. The effect of high-school education on this more direct measure of redistributive preferences is aligned with its impact on party choice. There is, however, a striking difference between the findings in Tables 2.7 and 2.8. There is no evidence of a gender differential effect of high-school education on support for increased government spending. This is not the case for political preferences, high-school education has a smaller effect on support for the left among women (compared to men). Clearly, political preferences

[35]The corresponding first stage estimates for column (2) are in Table 2.6, columns (2a) and (2b). Similarly, for column (3) they are in Table 2.6, columns (3a) and (3b).

[36]The inclusion of a quadratic in age instead of year dummies in Table 2.7 yield very similar results.

are driven not only by concerns for redistribution. Shortly, I will examine whether the impact of high-school education on social attitudes can shed some light on the issue.

Hausman specification tests for endogeneity indicate that the IV estimates reported here are statistically different from the corresponding OLS estimates.[37] This suggests that OLS estimates are biased downward. A possible explanation is that unobserved factors that make an individual more likely to attend high school also make him/her more likely to favor the political left. One such example is if liberal parents are more likely to educate their children, and there is intergenerational transmission of political preferences. My results indicate the influence of such unobserved factors, and highlight the importance of an identification strategy which accounts for the endogeneity of schooling choice. However, it is important to recognize the relevance of other reasons for the magnitude of the difference between the OLS and IV estimates.

OLS and IV estimators do not necessarily capture the same parameter of interest. The OLS estimate gives the average marginal effect of high-school education on redistributive preferences (though likely to be biased). The IV estimator provides a consistent estimate of the average marginal effect if we can assume constant treatment effects. Given the underlying heterogeneity in the impact of high-school education on redistributive preferences, this is unlikely to hold. Angrist and Imbens [1994] showed that under an additional assumption (monotonicity of the instrument), the IV estimate yields a *local average treatment effect* that has a useful interpretation. The assumption requires that the instrument should affect all individuals the same way if at all. This is likely given the instrument at hand.[38] Then, the IV estimator captures the average effect of high-school education for individuals influenced by the

[37]Hausman specification tests for Table 2.8 indicate that IV estimates in column (1) are statistically different from corresponding OLS estimates. The estimates in columns (3) and (4) are different at just below the 10 percent level.

[38]The monotonicity assumption can be restated as follows. If individuals who observed stricter compulsory schooling laws are more likely to go to high school, then anyone who would go to high school under less stringent laws must also do so under stricter laws.

laws to attend or graduate from high school.

There are a couple of reasons why the local average treatment effect identified here may be larger than the corresponding OLS estimate. First, individuals most likely to be affected by compulsory schooling legislation are those from poor backgrounds where expectations of educational attainment are low. If extra schooling for these individuals is coupled with a stronger belief that they are upwardly mobile and do not need a social safety net, the IV estimate will be larger than the corresponding OLS estimate. Second, if the schooling decision of older cohorts is more likely to be affected by the laws and the same individuals are more likely to alter party choice, the IV estimate will be larger. This is plausible given that average education is lower amongst older cohorts. Having attended or graduated from high school is more likely to ensure a high income status.

2.7 Robustness

This section outlines a series of robustness checks.

Defense spending I have presented evidence that high-school education reduced individual support for government spending. Next, to qualify my findings I use information on individual attitudes toward defense spending to examine whether the observed effect is driven by concerns over social spending or if the pattern applies to military spending as well.[39] The results are presented in Table 2.9, columns (1) and (2). IV estimates for the impact of high-school attendance and graduation on support for increased defense spending are not significant. This suggests that my findings for government spending are rooted in individual attitudes toward social spending.

[39]Respondents are asked their opinions on the following statement: "Some people believe that we should spend much less money for defense. Others feel that defense spending should be greatly increased."

Social values Throughout the paper, I argue that high-school education affects individual political partisanship through its impact on redistributive preferences. Next, I investigate whether schooling affects political preferences through its effect on social values rather than through concerns over redistribution. To do so, I make use of attitudinal questions in the survey and present IV estimates for the effect of high-school education on social values and religiosity. If a high-school education induces individuals to become more conservative on social issues, it is unclear whether the effect I capture for political preferences stems from individual concerns over redistribution. This scenario seems unlikely given that younger cohorts tend to be more liberal on social issues (as depicted in Figure 2.3). If I find that schooling induces liberal social views, we can be reassured that my estimates capture the economic effect of voting.

Table 2.9, columns (3)-(10) present IV estimates for the effect of high-school attendance and graduation on individual attitudes toward abortion, women's rights, civil rights and church attendance. I find that high-school education induces liberal opinions on abortion and women's rights. Those induced to attend or graduate from high school were significantly more likely to be pro-choice and be supportive of equal roles for men and women.[40] Corresponding estimates for attitudes toward civil rights and church attendance are not significant.[41] In sum, these findings suggest that high-school education resulted in reduced support for the Democratic Party through concerns over redistribution rather than social issues.

The South Next, I examine whether my results are robust to the exclusion of southern states. Since the politics of the region is typically considered a special case,

[40] There is no gender differential effect from schooling on attitudes toward abortion. There is some evidence, however, that the effect on women's rights is stronger among females than males. It emerges when I only use compulsory attendance dummies as instruments for high-school attendance/graduation. This may help us understand why we observe a gender differential effect of high-school education on support for the Democratic Party – it is possible that schooling increases female emancipation and women's support for the left.

[41] I also examine individual support for school prayer and find that high-school education does not have a significant effect; results not reported here.

I examine whether these findings are driven by the south. Table 2.10, columns (1) and (2) show that this is not the case.[42]

Additional covariates I also check whether my results in Table 2.7 are robust to the inclusion of additional covariates. Information on the respondent's current income status, labor force participation, marital status, religious affiliation, union membership and father's occupation is included. Point estimates for high-school education are reported in Table 2.10, columns (3) and (4). The magnitudes are very similar to those in Table 2.7. The individual-level controls affect political affiliation in a manner previously documented in the literature. It should be noted, however, that the controls are potentially endogenous and should not be given a causal interpretation. The exercise serves simply as a robustness check.

Parental Party Affiliation Finally, I test the validity of my instrument using a placebo. To do so, I examine whether a respondent's high-school education affects his or her parent's party affiliation. The results, reported in Table 2.11, confirm that a respondent's high-school attendance/graduation has no impact on either parent's support for the Democratic Party. The IV estimates are statistically insignificant.

2.8 Conclusion

This paper argues that cohort patterns in high-school education underlie the decline in support for redistribution in the United States. I show that younger cohorts are significantly less likely to support the Democratic Party and also more likely to attend and graduate from high school. These trends are particularly pronounced for individuals born between 1908 and 1964. To examine the link between the two devel-

[42]The loss of precision is expected since the South constitutes a third of my sample and provides an important source of variation. Southern cohorts lagged behind the rest in high-school education, but also experienced faster growth in attendance and graduation rates.

opments, I use the passage of compulsory schooling laws across US states. Successive cohorts were exposed to increasingly stringent compulsory attendance and child-labor laws. I use these laws as instruments for individual high-school education and find that those induced to attend/graduate from high school due to the laws significantly reduced support for the Democratic Party. The effect is stronger among males. My estimates indicate that the rise in high-school education due to the laws can account for 10-25 percent of the decline in Democratic support. Throughout the paper, I also use more direct evidence on redistributive preferences, as captured by support for increased government spending, and obtain similar results.

The main contribution of the paper is to establish this set of empirical facts. Possible channels by which more educated cohorts reduced support for redistribution are suggested but the exact mechanism is not uncovered. It is likely that education increased individual economic mobility, and increased mobility lowered the demand for redistribution. Whether schooling affected redistributive preferences only through its effect on individual mobility is a potentially interesting topic for future work.

For birth cohorts under study in this paper, I argue that the relevant level of education that affected an individual's expected relative income status was high school. As noted earlier, there are barely any gains in high-school education for individuals born after the mid-1960s. For younger cohorts who have entered the electorate, college rather than high school is likely to affect individual mobility prospects and demand for redistribution. The use of a valid instrument for college education would permit me to investigate the impact of college in a similar context. This remains an interesting exercise for the future.

Table 2.1: Descriptive statistics

Variable	Percent	Variable	Percent
A. Individual-level NES data			
		cohort born	
female	55.5	1908-12	7.2
black	11.5	1913-17	8.2
age	41.8	1918-22	9.3
	(15.2)	1923-27	9.5
high-school attendance	88.9	1928-32	8.5
high-school graduation	74.3	1933-37	7.2
left	52.5	1938-42	7.8
father Democrat	60.7	1943-47	9.2
mother Democrat	60.1	1948-52	9.2
govspend	37.9	1953-57	8.6
defense	34.2	1958-62	7.1
pro-choice	53.9	1963-67	4.5
equal roles	64.8	1968-72	2.5
civil rights	58.9	1973-77	0.9
church attendance	52.6	1978-82	0.3
N	31899		
B. Compulsory Schooling Laws			
Labor6	12.9	Attend8	24.3
Labor7	22.2	Attend9	41.0
Labor8	39.3	Attend10	8.2
Labor9	25.5	Attend11	26.5
N	29767		

Note: Variable descriptions are provided in Appendix B. There are 31,715 observations for high-school attendance and graduation, 12,584 for govspend, 14,194 for defense, 21,820 for pro-choice, 18,849 for equal roles, 15,452 for civil rights and 31,509 for church attendance.

Table 2.2: Cohort Effects in Political Preferences

	Dependent variable: left							
	All		Males		Females		Blacks	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1915 cohort	0.0144	0.0174	-0.0075	-0.0017	0.0333*	0.0343**	0.0183	-0.0165
	[0.0183]	[0.0174]	[0.0278]	[0.0277]	[0.0184]	[0.0167]	[0.0369]	[0.0326]
1925 cohort	-0.0218	-0.011	-0.0743***	-0.0537**	0.0196	0.0214	0.0821**	-0.0033
	[0.0181]	[0.0154]	[0.0206]	[0.0209]	[0.0220]	[0.0167]	[0.0306]	[0.0297]
1935 cohort	-0.0555**	-0.0388*	-0.1076***	-0.0690***	-0.0139	-0.0165	0.1323***	-0.0174
	[0.0230]	[0.0198]	[0.0277]	[0.0256]	[0.0255]	[0.0213]	[0.0324]	[0.0315]
1945 cohort	-0.0720**	-0.0435*	-0.1603***	-0.0989***	0.0009	0.0012	0.1777***	-0.0361
	[0.0279]	[0.0226]	[0.0340]	[0.0323]	[0.0298]	[0.0230]	[0.0446]	[0.0318]
1955 cohort	-0.0803**	-0.0466**	-0.1710***	-0.0969***	-0.0045	-0.0061	0.1664***	-0.0928***
	[0.0341]	[0.0213]	[0.0406]	[0.0290]	[0.0350]	[0.0217]	[0.0494]	[0.0303]
1965 cohort	-0.1445***	-0.1079***	-0.2849***	-0.2004***	-0.0282	-0.0346	0.1576**	-0.1464***
	[0.0415]	[0.0285]	[0.0440]	[0.0343]	[0.0492]	[0.0304]	[0.0643]	[0.0450]
1975 cohort	-0.1187**	-0.0895**	-0.3148***	-0.2162***	0.0493	0.019	0.2311**	-0.1335*
	[0.0463]	[0.0365]	[0.0568]	[0.0446]	[0.0555]	[0.0542]	[0.0958]	[0.0696]
female	0.0366***	0.0372***					0.0214	0.02
	[0.0056]	[0.0057]					[0.0148]	[0.0148]
black	0.2863***	0.2860***	0.2943***	0.2935***	0.2798***	0.2805***		
	[0.0244]	[0.0247]	[0.0238]	[0.0246]	[0.0268]	[0.0268]		
quadratic in age	yes	no	yes	no	yes	no	yes	no
year dummies	no	yes	no	yes	no	yes	no	yes
N	31899	31899	14200	14200	17699	17699	3678	3678
Adj. R^2	0.06	0.06	0.07	0.07	0.05	0.06	0.04	0.05

Note: The omitted cohort is the 1910 cohort. Alternate cohort dummies reported in the table for the sake of brevity, the entire vector of cohort dummies from columns (1) and (2) are plotted in Figures 2.5 and 2.6 respectively. Robust standard errors in brackets. Standard errors are clustered by state where respondent grew up. Dummies included for state where respondent grew up. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.3: Number of States that Changed Compulsory Schooling Laws at Least Once, by Region and Time Period

	entrance age	drop-out age	work permit age	minimum schooling to drop out	minimum schooling for work permit
Northeast					
1914-23	3	2	2	4	5
1924-33	0	0	1	0	4
1934-43	2	0	1	4	1
1944-53	2	1	4	6	5
1954-63	0	0	3	2	2
1964-73	1	2	0	2	1
1974-78	1	1	3	2	0
Midwest					
1914-23	6	2	2	10	7
1924-33	0	0	0	0	3
1934-43	1	0	0	1	0
1944-53	2	2	3	3	2
1954-63	1	0	0	2	1
1964-73	1	0	0	2	2
1974-78	1	0	1	3	2
South					
1914-23	12	9	12	12	7
1924-33	3	0	0	0	2
1934-43	4	8	3	9	7
1944-53	5	1	11	12	7
1954-63	3	2	2	2	4
1964-73	4	3	0	0	4
1974-78	2	1	1	2	0
West					
1914-23	5	1	6	9	6
1924-33	2	0	0	0	0
1934-43	2	2	3	2	3
1944-53	3	3	2	4	4
1954-63	2	1	2	2	2
1964-73	0	1	2	1	1
1974-78	2	2	3	3	2

Table 2.4: Effect of Party Affiliation on Changes in Compulsory Schooling Laws

	Dependent variable						
	Labor	Attend	entrance age	drop-out age	work permit age	minimum schooling to drop out	minimum schooling for work permit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Parental Party Affiliation							
father Democrat	-0.0178	0.0326	-0.0199	-0.0474	-0.0267	0.2099	-0.0008
	[0.0314]	[0.0442]	[0.0313]	[0.0288]	[0.0511]	[0.1548]	[0.0818]
mother Democrat	-0.039	0.0005	-0.0486	0.0358	-0.044	-0.0924	-0.1367
	[0.0465]	[0.0403]	[0.0381]	[0.0514]	[0.0446]	[0.1667]	[0.1044]
father Democrat	0.0243	-0.0304	0.0507	0.0332	0.0546	-0.1391	0.047
*mother Democrat	[0.0605]	[0.0599]	[0.0583]	[0.0419]	[0.0677]	[0.2177]	[0.1720]
state dummies	yes	yes	yes	yes	yes	yes	yes
cohort dummies	yes	yes	yes	yes	yes	yes	yes
N	11689	11689	11689	11689	11689	11689	11689
Adj. R^2	0.53	0.6	0.35	0.4	0.44	0.55	0.59
Panel B: State Party Affiliation							
Governor Democrat	0.0702	-0.0217	0.0317	-0.0559	0.0264	-0.0539	0.3062
	[0.1048]	[0.1168]	[0.0357]	[0.0711]	[0.1210]	[0.3443]	[0.1944]
state dummies	yes	yes	yes	yes	yes	yes	yes
year dummies	yes	yes	yes	yes	yes	yes	yes
N	3185	3185	3185	3185	3185	3185	3185
Adj. R^2	0.71	0.67	0.4	0.41	0.39	0.51	0.58

Note: Robust standard errors in brackets. Standard errors are clustered by state where respondent grew up.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.5: OLS Estimates of High-School (HS) Education on Redistributive Preferences

	Dependent variable: left		Dependent variable: govspend	
	(1)	(2)	(3)	(4)
HS attendance	-0.0634***		-0.1489***	
	[0.0185]		[0.0293]	
female*HS attendance	0.0772***		-0.0064	
	[0.0196]		[0.0329]	
HS graduation		-0.0967***		-0.1560***
		[0.0127]		[0.0163]
female*HS graduation		0.0828***		0.0396*
		[0.0121]		[0.0230]
female	-0.0338**	-0.0272***	0.0996***	0.0582***
	[0.0167]	[0.0101]	[0.0325]	[0.0213]
black	0.2788***	0.2748***	0.2983***	0.2914***
	[0.0254]	[0.0254]	[0.0148]	[0.0151]
N	29767	29767	11043	11043
R^2	0.06	0.07	0.09	0.09
F-stat	0.41	0.69		55.88
Prob > F	0.525	0.412		0.000

Note: Robust standard errors in brackets. Standard errors are clustered by state where respondent grew up. Cohort dummies, year dummies and state dummies included. The F-test indicates whether HS attendance + female*HS attendance (or HS graduation + female*HS graduation) is significantly different from zero. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.6: First Stage Estimates: Effect of Compulsory Schooling Laws on HS Attendance and Graduation

	Dependent variable											
	hsa	female* hsa	hsa	female* hsa	hsa	female* hsa	hsg	female* hsg	hsg	female* hsg	hsg	female* hsg
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)	(5a)	(5b)	(6a)	(6b)
Labor7	0.049***	-0.034***			0.019	-0.034***	0.058***	-0.063***			0.040**	-0.061***
	[0.011]	[0.008]			[0.012]	[0.009]	[0.014]	[0.011]			[0.016]	[0.013]
Labor8	0.088***	-0.061***			0.056***	-0.061***	0.088***	-0.106***			0.067***	-0.104***
	[0.010]	[0.007]			[0.011]	[0.008]	[0.013]	[0.010]			[0.015]	[0.012]
Labor9	0.118***	-0.068***			0.079***	-0.064***	0.119***	-0.114***			0.094***	-0.108***
	[0.011]	[0.009]			[0.014]	[0.010]	[0.016]	[0.012]			[0.019]	[0.014]
female*Labor7	-0.027**	0.084***			-0.027*	0.054***	-0.049***	0.123***			-0.062***	0.087***
	[0.012]	[0.009]			[0.014]	[0.011]	[0.016]	[0.013]			[0.019]	[0.015]
female*Labor8	-0.044***	0.165***			-0.044***	0.133***	-0.058***	0.231***			-0.073***	0.189***
	[0.011]	[0.008]			[0.013]	[0.009]	[0.015]	[0.012]			[0.017]	[0.013]
female*Labor9	-0.044***	0.207***			-0.046***	0.158***	-0.061***	0.281***			-0.082***	0.221***
	[0.012]	[0.009]			[0.015]	[0.011]	[0.016]	[0.012]			[0.020]	[0.016]
Attend9			0.065***	-0.022***	0.044***	-0.001			0.048***	-0.045***	0.02	-0.008
			[0.008]	[0.006]	[0.010]	[0.007]			[0.011]	[0.009]	[0.013]	[0.010]
Attend10			0.047***	-0.053***	0.029**	-0.031***			0.033**	-0.074***	0.012	-0.038***
			[0.012]	[0.009]	[0.013]	[0.009]			[0.016]	[0.013]	[0.017]	[0.013]
Attend11			0.094***	-0.032***	0.063***	0			0.086***	-0.053***	0.049***	0
			[0.010]	[0.007]	[0.011]	[0.008]			[0.013]	[0.010]	[0.015]	[0.012]
female*Attend9			-0.015*	0.091***	0.001	0.046***			-0.008	0.124***	0.022	0.058***
			[0.009]	[0.007]	[0.011]	[0.008]			[0.012]	[0.009]	[0.014]	[0.011]
female*Attend10			-0.02	0.119***	-0.007	0.072***			0.012	0.166***	0.033*	0.101***
			[0.014]	[0.011]	[0.014]	[0.011]			[0.019]	[0.015]	[0.020]	[0.015]
female*Attend11			-0.019**	0.146***	0.001	0.069***			-0.01	0.193***	0.027	0.086***
			[0.010]	[0.007]	[0.012]	[0.009]			[0.013]	[0.010]	[0.016]	[0.013]

Note: *hsa* refers to high-school attendance and *hsg* to high-school graduation. Cohort dummies, year dummies, state dummies, dummy for black and dummy for female included. N is 29767. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.7: IV Estimates of HS Attendance and Graduation on Support for the Democratic Party (Dependent variable: left)

	Instrument Set					
	Labor (1)	Attend (2)	Labor & Attend (3)	Labor (4)	Attend (5)	Labor & Attend (6)
HS attendance	-0.5081*** [0.1513]	-0.4946*** [0.1475]	-0.4760*** [0.1298]			
female*HS attendance	0.213** [0.0852]	0.2854*** [0.0854]	0.2357*** [0.0755]			
HS graduation				-0.4908*** [0.1704]	-0.4714** [0.1788]	-0.4209*** [0.1416]
female*HS graduation				0.1452** [0.0560]	0.2408*** [0.0604]	0.1789*** [0.0454]
female	-0.1443* [0.0763]	-0.2095*** [0.0755]	-0.1656** [0.0677]	-0.0723* [0.0411]	-0.1429*** [0.0428]	-0.0974*** [0.0334]
black	0.2554*** [0.0319]	0.2585*** [0.0305]	0.2581*** [0.0308]	0.2300*** [0.0395]	0.2401*** [0.0363]	0.2415*** [0.0348]
F-stat	3.46	1.65	3.04	3.21	1.80	2.62
Prob > F	0.069	0.205	0.088	0.080	0.186	0.112

Note: Robust standard errors in brackets. Standard errors are clustered by state where respondent grew up. The instrument set Labor refers to the set of dummies Labor7, Labor8, Labor9, female*Labor7, female*Labor8 and female*Labor9 while the set Attend refers to the dummies Attend9, Attend10, Attend11, female*Attend9, female*Attend10 and female*Attend11. Cohort dummies, year dummies and state dummies included. N is 29767. The F-test indicates whether HS attendance + female*HS attendance (or HS graduation + female*HS graduation) is significantly different from zero. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.8: IV Estimates of HS Attendance and Graduation on Support for Increased Government Spending (Dependent variable: govspend)

	Instrument Set					
	Labor	Attend	Labor & Attend	Labor	Attend	Labor & Attend
	(1)	(2)	(3)	(4)	(5)	(6)
HS attendance	-0.7100**	-0.3775	-0.5246*			
	[0.3348]	[0.2839]	[0.2801]			
female*HS attendance	0.2446	0.2081	0.2718			
	[0.1925]	[0.2635]	[0.1952]			
HS graduation				-0.5974**	-0.3850*	-0.4389**
				[0.2614]	[0.1947]	[0.1829]
female*HS graduation				0.1575	0.1861	0.1954
				[0.1447]	[0.2042]	[0.1278]
female	-0.1289	-0.1001	-0.158	-0.0394	-0.065	-0.0725
	[0.1838]	[0.2502]	[0.1863]	[0.1260]	[0.1738]	[0.1109]
black	0.2784***	0.2922***	0.2869***	0.2539***	0.2772***	0.2723***
	[0.0184]	[0.0179]	[0.0166]	[0.0283]	[0.0208]	[0.0208]

Note: Robust standard errors in brackets. Standard errors are clustered by state where respondent grew up. The instrument set Labor refers to the set of dummies Labor7, Labor8, Labor9, female*Labor7, female*Labor8 and female*Labor9 while the set Attend refers to the dummies Attend9, Attend10, Attend11, female*Attend9, female*Attend10 and female*Attend11. Cohort dummies, year dummies and state dummies included. N is 11043. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.9: IV Estimates of HS Attendance and Graduation on Defense Spending and Social Values

	Dependent variable									
	defense		pro-choice		equal roles		civil rights		church attendance	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
HS attendance	0.0305		0.5540**		0.2928		0.1252		-0.0689	
	[0.2157]		[0.2117]		[0.2067]		[0.1844]		[0.1932]	
female*HS attendance	-0.2199		0.0656		0.2243		-0.1112		-0.1123	
	[0.1950]		[0.1294]		[0.1564]		[0.0937]		[0.1064]	
HS graduation		-0.1078		0.5254***		0.4059**		0.0985		-0.1183
		[0.1635]		[0.1871]		[0.1618]		[0.1758]		[0.1877]
female*HS graduation		-0.156		0.0285		0.1554		-0.0811		-0.0829
		[0.1519]		[0.1007]		[0.1020]		[0.0676]		[0.0853]
N	12,615	12,615	19,825	19,825	17,326	17,326	14,807	14,807	29,544	29,544

Note: Robust standard errors in brackets. Standard errors are clustered by state where respondent grew up. The instrument set is the complete set of dummies in Table 2.7, columns (3) or (6). Cohort dummies, year dummies and state dummies included. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.10: Robustness Checks

	Dependent variable: left			
	Without the South		With Additional Covariates	
	(1)	(2)	(3)	(4)
HS attendance	-0.658***		-0.477***	
	[0.230]		[0.163]	
female*HS attendance	0.262		0.233**	
	[0.179]		[0.102]	
HS graduation		-0.392*		-0.431**
		[0.227]		[0.196]
female*HS graduation		0.164*		0.169**
		[0.089]		[0.075]
female	-0.198	-0.094	-0.146	-0.067
	[0.167]	[0.073]	[0.095]	[0.065]
black	0.366***	0.351***	0.233***	0.228***
	[0.025]	[0.035]	[0.035]	[0.037]
married			-0.059***	-0.061***
			[0.010]	[0.011]
catholic			0.096***	0.095***
			[0.027]	[0.027]
protest			-0.098***	-0.096***
			[0.022]	[0.024]
jewish			0.362***	0.380***
			[0.033]	[0.034]
income 34-95 pctile			0.025	0.053
			[0.030]	[0.053]
income 96-100 pctile			-0.114***	-0.07
			[0.038]	[0.069]
union member			0.120***	0.103***
			[0.012]	[0.020]
labor force			0.036***	0.046**
			[0.013]	[0.022]
father blue collar			0.039**	0.016
			[0.018]	[0.034]
N	19626	19626	21468	21468
F-stat		0.88	1.49	1.18
Prob > F		0.3544	0.2286	0.2818

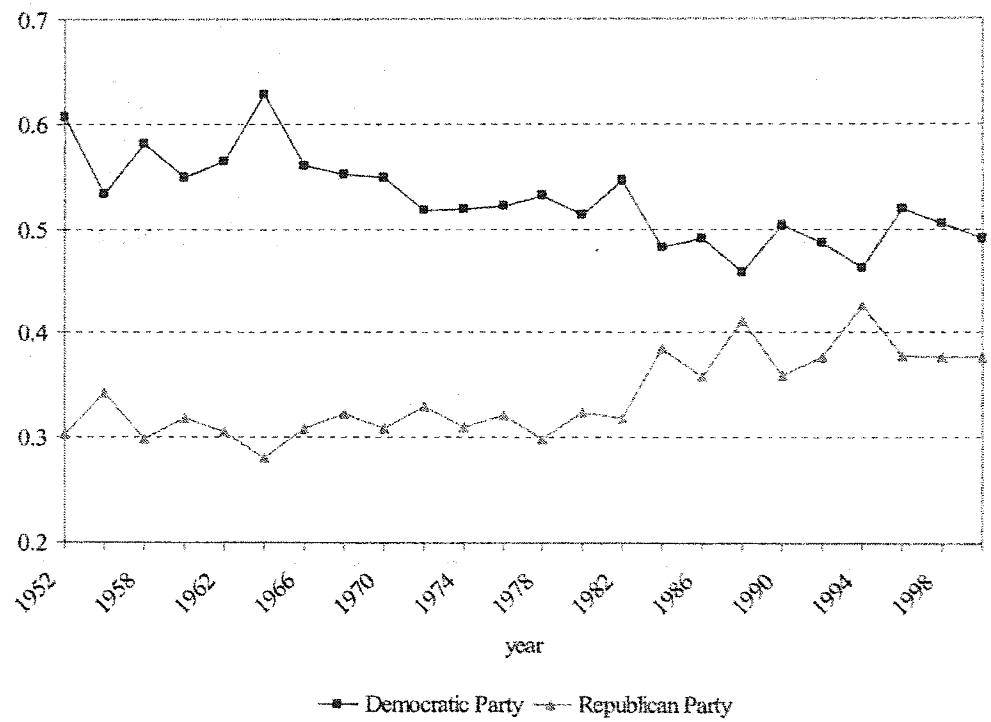
Note: Robust standard errors in brackets. Standard errors are clustered by state where respondent grew up. The instrument set is the complete set of dummies in Table 2.7, columns (3) or (6). Cohort dummies, year dummies and state dummies included. The F-test indicates whether HS attendance + female*HS attendance (or HS graduation + female*HS graduation) is significantly different from zero. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.11: IV Estimates of HS Attendance and Graduation on Parental Party Affiliation

	Dependent variable:			
	father Democrat		mother Democrat	
	(1)	(2)	(3)	(4)
HS attendance	0.05 [0.277]		-0.288 [0.286]	
female*HS attendance	-0.19 [0.148]		-0.23 [0.140]	
HS graduation		0.008 [0.207]		-0.228 [0.224]
female*HS graduation		-0.138 [0.086]		-0.154 [0.098]
N	12713	12713	12458	12458

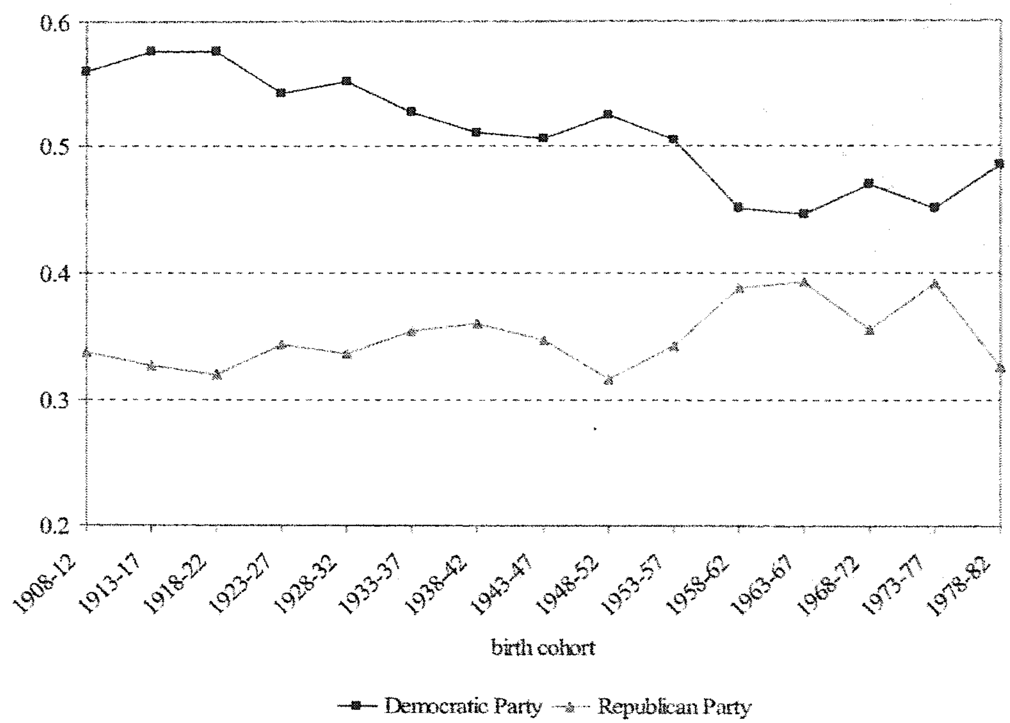
Note: Robust standard errors in brackets. Standard errors are clustered by state where respondent grew up. The instrument set is the complete set of dummies in Table 2.7, columns (3) or (6). Cohort dummies, year dummies and state dummies included. * significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 2.1: Proportion of 18-64 year olds supporting Democratic and Republican Parties



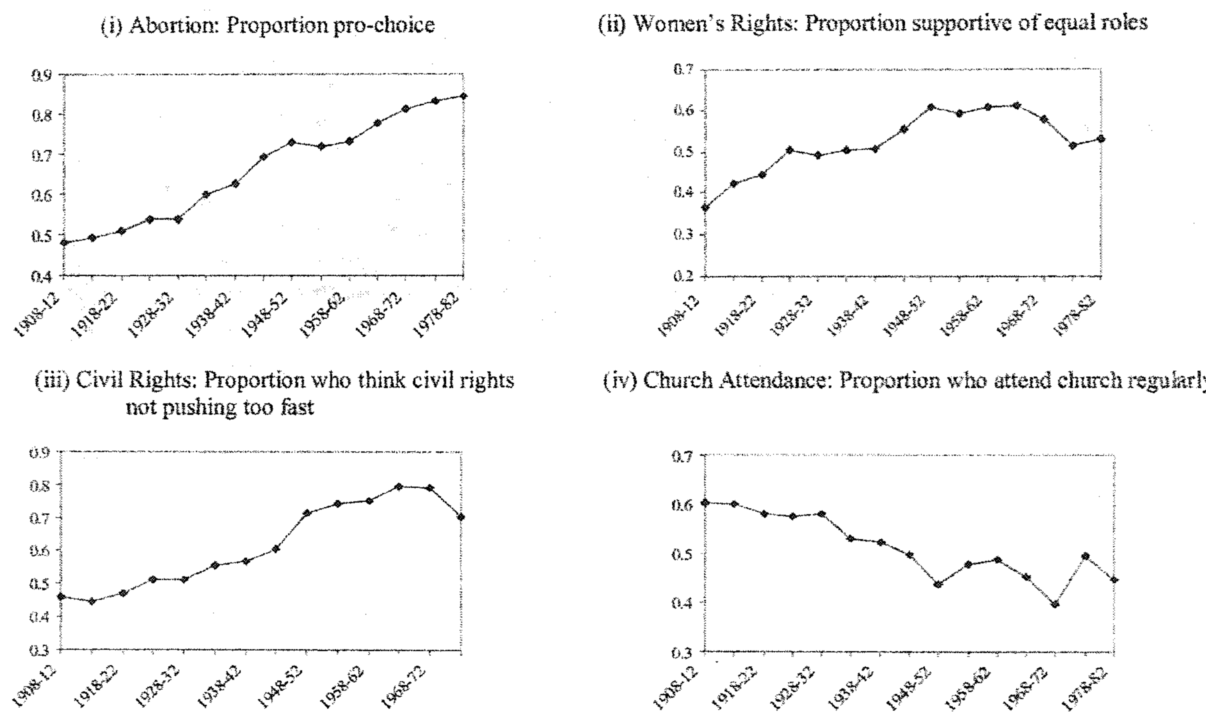
Source: National Election Studies

Figure 2.2: Proportion of cohort supporting Democratic and Republican Parties



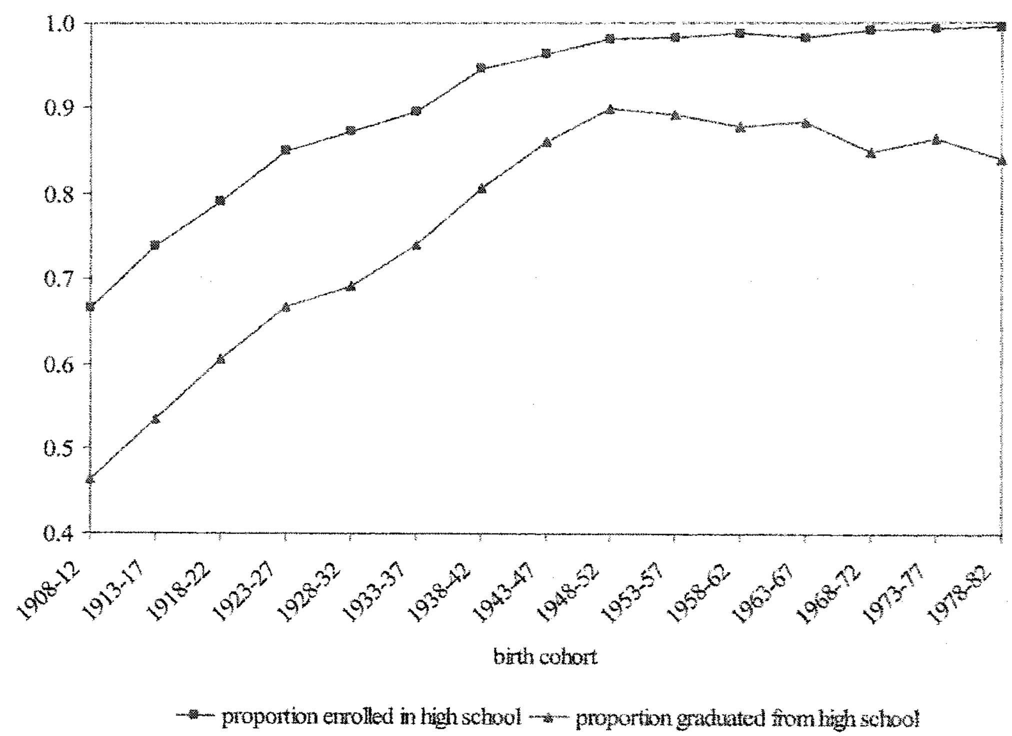
Source: National Election Studies

Figure 2.3: Social attitudes by birth cohort



Source: National Election Studies

Figure 2.4: Secondary schooling by birth cohort



Source: National Election Studies

Figure 2.5: Cohort effects in support for the Democratic Party, with quadratic in age

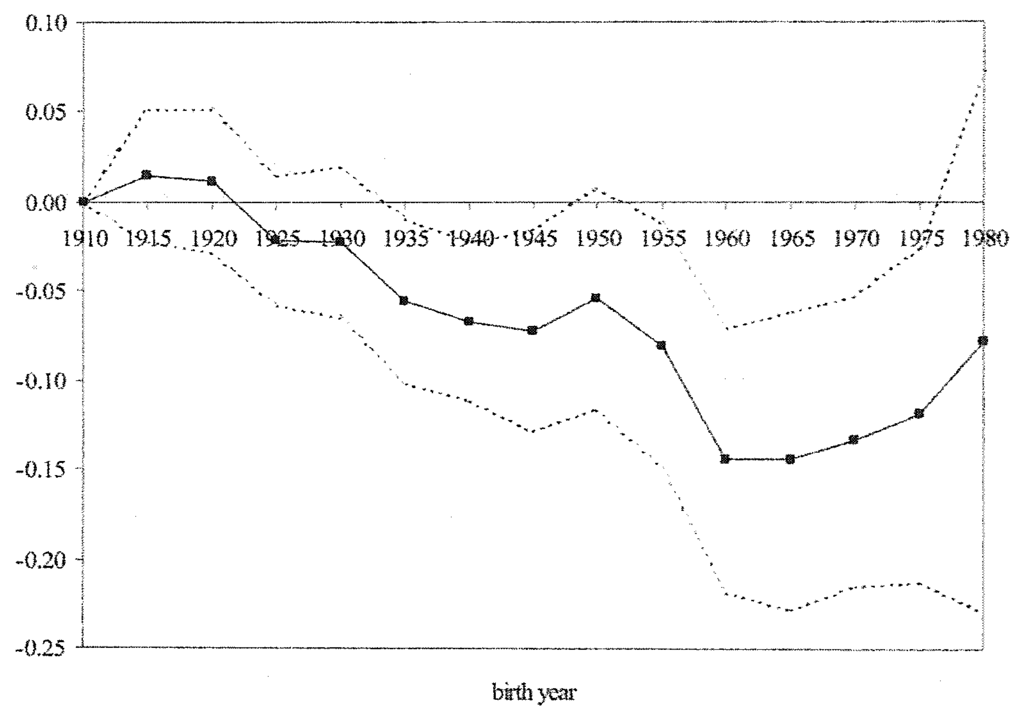


Figure 2.6: Cohort effects in support for the Democratic Party, with year dummies

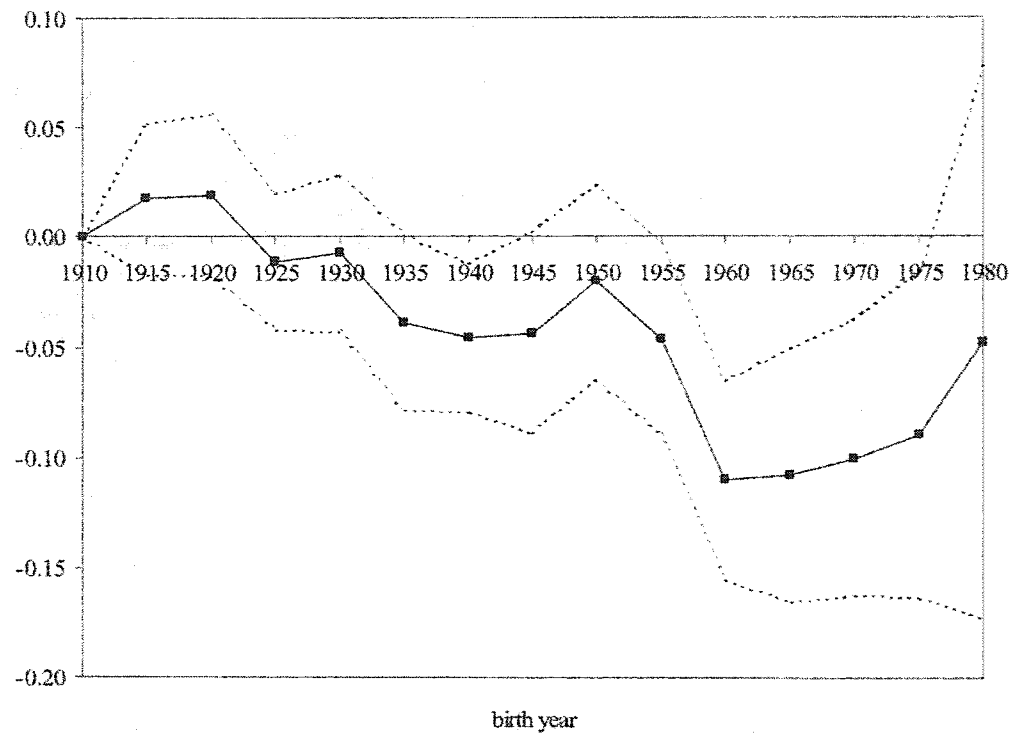


Figure 2.7: Cohort effects in secondary schooling

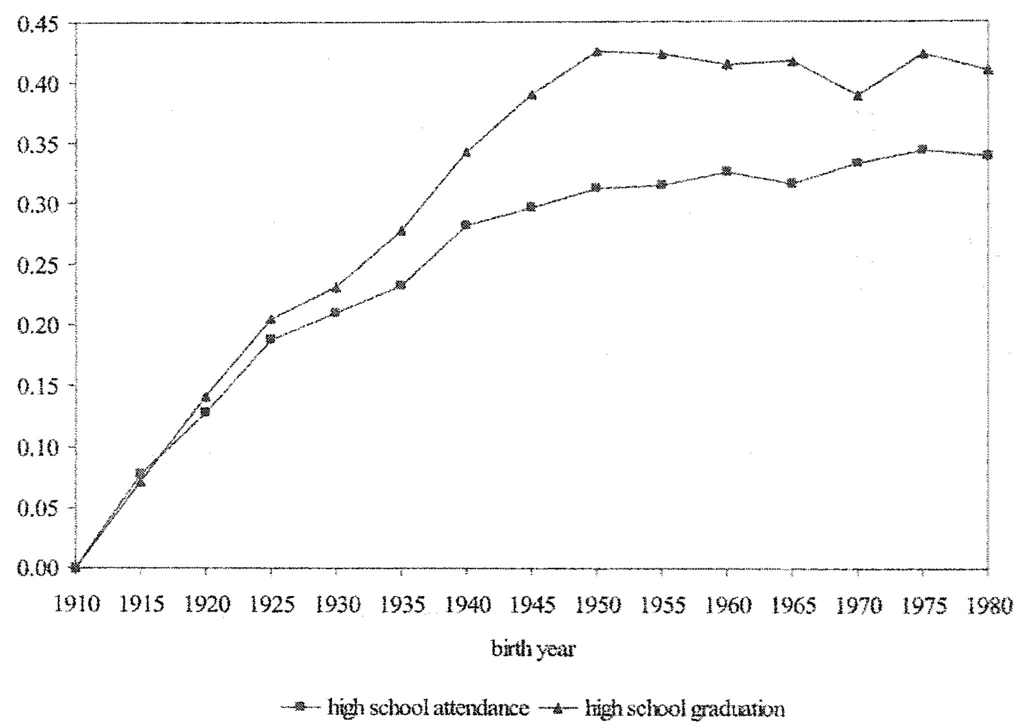


Figure 2.8: Compulsory Schooling Laws, by State

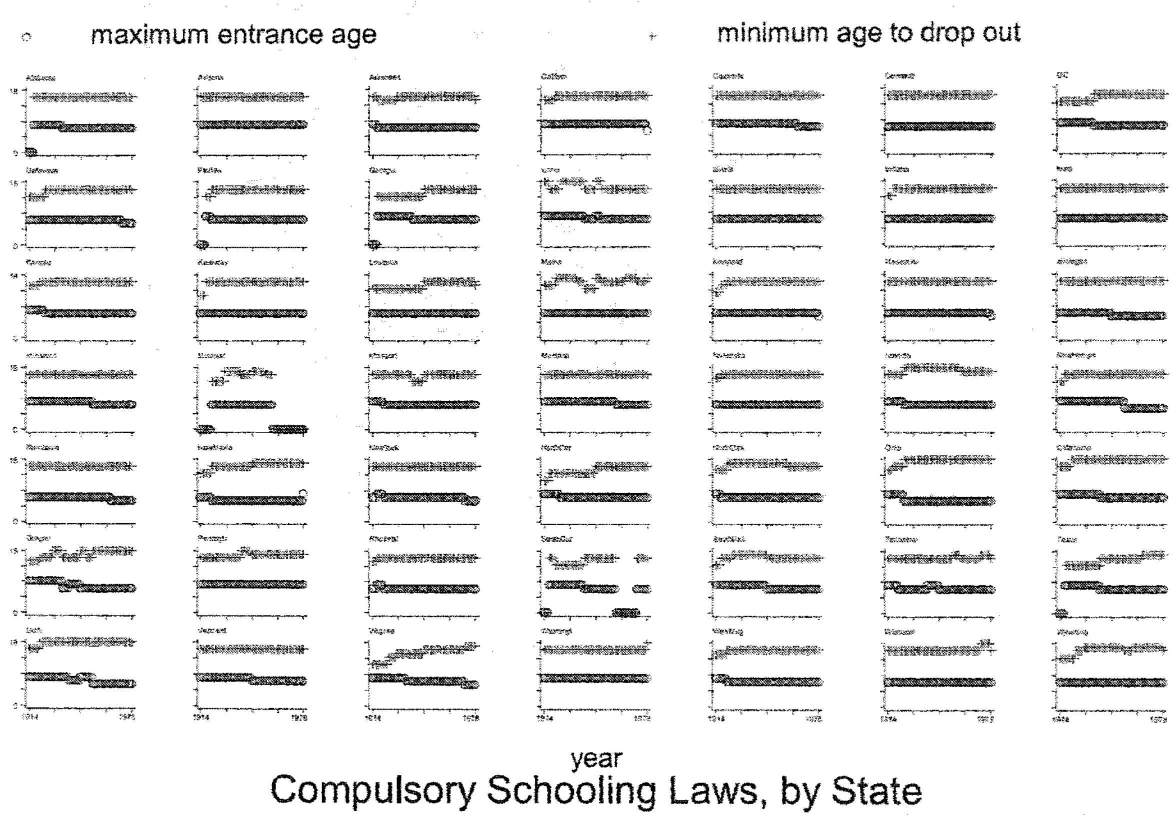


Figure 2.9: Compulsory Schooling Laws, by State (continued)

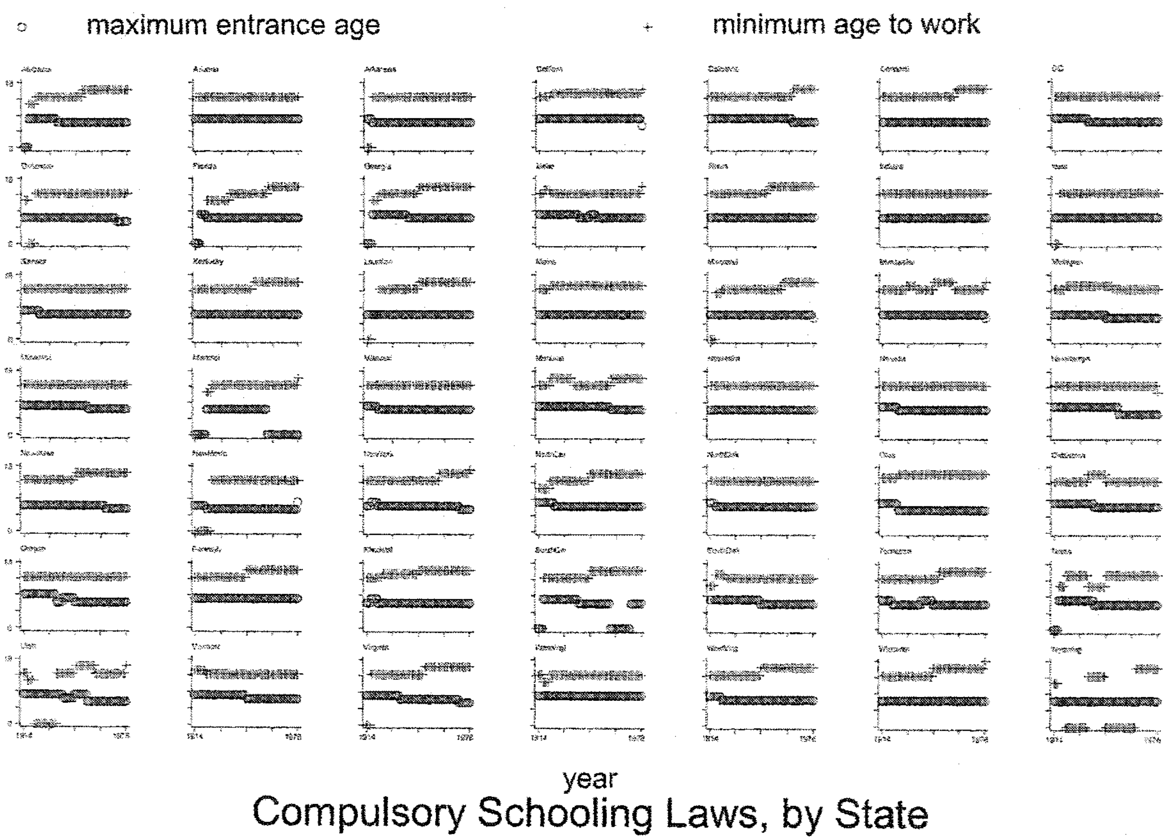


Figure 2.10: Compulsory Schooling Laws, by State (continued)

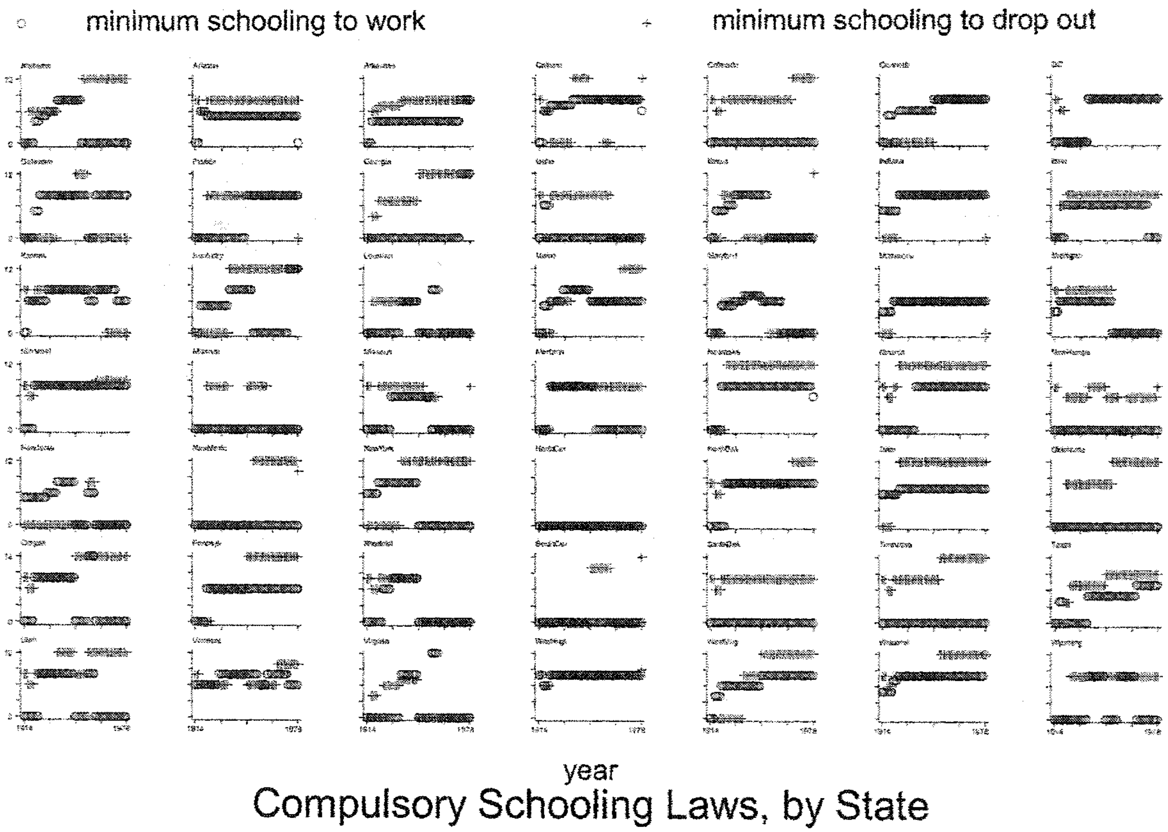
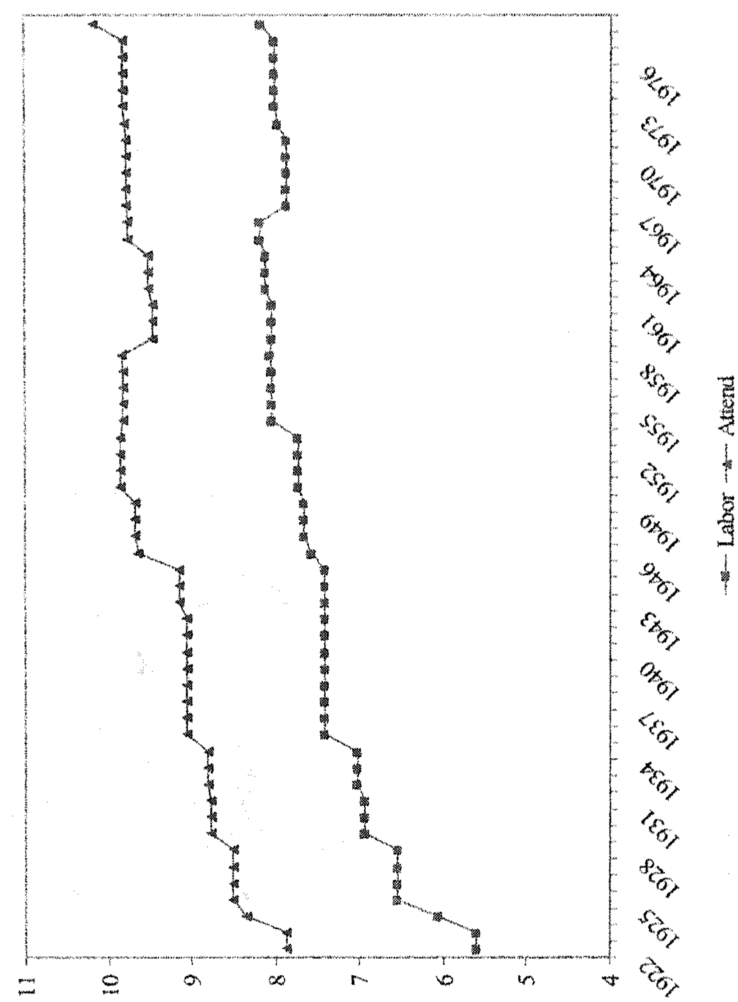


Figure 2.11: National Trends in Labor and Attend



Chapter 3

Unmarried Parenthood and Redistributive Politics

3.1 Introduction

In the last three decades, relative to men, women have become more supportive of the political left. In a number of recent elections in the United States and Europe, the female vote is believed to have swung the election in favor of the political left.[1] The popular press has offered an array of explanations ranging from the left party's stance on social issues to the good looks of its candidates. However, the fact that a near identical political gender gap has emerged in both the United States and Europe (Figure 3.1) suggests that the explanation lies in a left-right divide common to all countries – leaving State redistribution as a likely candidate.

Edlund and Pande [2002], henceforth EP, showed that in the United States, the growth of the political gender gap was linked to the rise in divorce. This, they argued, reduced the private transfers women received from men and caused them therefore to favor State redistribution.

[1]Edlund and Pande [2002] provided a literature review. For Europe see Duverger [1955] and Inglehart and Norris [2000].

In this paper, we use the Eurobarometer surveys (1973-1996) and German longitudinal data (GSOEP, 1984-2001) to show that the same is true for a number of West European countries and for alternative measures of non-marriage. Further, we examine the implications of non-marriage for the allocation of State resources in these and other high-income OECD countries (1980-1998). If non-marriage shifts the economic responsibility for children towards women and individuals vote in line with their economic interests, then we would expect non-marriage to affect popular support for redistribution towards children. In particular, if marriage is positive assortative and the incidence of non-marriage moves up the income distribution over time (while remaining more prevalent among the poor) then we would expect this relationship to be U-shaped. The OECD data bear out this prediction.

Divorce is only one way to not be married, and is preceded by marriage. While the proportion of the adult population that is currently divorced has continued to rise, in many countries marriages have become more stable as fewer people marry.[2] Instead, delayed marriage, unmarried parenthood, and cohabitation are increasingly common. For Europe, it is often contended that the rise in non-marital families simply reflects changing social norms and has not altered resource-sharing within the family (see for instance, "Europeans Opting Against Marriage," *The New York Times*, March 24, 2002). However, there are many reasons why this may not be the case (further discussed in Section 3.2). Ultimately, this remains an empirical question and one we pursue in this paper. We consider three possible measures of non-marriage: divorce incidence, out-of-wedlock fertility and female age at first marriage.[3]

A natural interpretation of the growth of the political gender gap is that it

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[2]This is particularly pronounced in "high divorce" countries such as the United States, Denmark and Sweden (e.g., see the Statistical Abstract of the United States 1998: table 156; and Statistical Yearbook of Sweden 1999: table 49).

[3]Out-of-wedlock fertility which may or may not be accompanied by cohabitation has emerged as an important contributor to non-marriage. Today, out-of-wedlock fertility accounts for more than one-third of births in a number of Western countries, including the United States, Canada, the United Kingdom, France, Denmark, Sweden and Norway.

reflects increasing divergence in the economic well-being of men and women (see EP).[4] This raises the question of whether women's greater demand for redistribution has altered policy outcomes. If absent marriage, men vote right and women left, then the net effect of a decline in marriage on popular support for redistribution is ambiguous. Consider a left-wing couple that divorces. Upon divorce, the woman remains with the left, but the man turns right and the net effect is a decline in support for the left. Conversely, if a right-wing couple divorced, support for the left would increase, since the left now gains the woman. All along, an increase in non-marriage would contribute to a widening of the gender gap, but whether the left gains or loses is unclear. Non-marriage, while more prevalent at the lower end of the income distribution, has increasingly involved higher income groups. This would suggest that the relationship between non-marriage and support for redistribution (the left) will be U-shaped – the initial fall coming from low income men turning right and the subsequent increase from high income women turning left.

To investigate the relationship between non-marriage and policy outcomes, we use public social expenditure data for high-income OECD countries. In line with our theoretical predictions, we find that redistribution towards children first declines and then rises with increasing non-marriage. This finding supports our hypothesis that the decline in marriage has polarized men and women's interests regarding State-led redistribution. Moreover, it suggests that transfers are determined by political salience (rather than, for instance, need) and therefore raises important questions regarding the ability of the State to provide for children. These findings contribute to the empirical public finance literature which examines the relationship between the

[4]It is well-established that non-marital child bearing is linked with single-motherhood and a feminization of poverty. For a literature review, see Akerlof, Yellen, and Katz [1996]. The assumption that individuals vote in line with their economic interests is standard (Downs [1957]; and Persson and Tabellini [2000] for a literature review). Lott and Kenny [1999] showed that between 1870-1940 in the United States, female voter turnout increased the size of government. We focus on a markedly later period, and argue that the growth of the political gender gap reflects a change in the economic realities of men and women, rather than women being inherently more left-wing.

demographic composition of the electorate and the composition of public spending (for instance, Cutler, Elmendorf, and Zeckhauser [1993]; Poterba [1997] and Mulligan and Sala-i-Martin [1999]).[5] Our contribution is to show how non-marriage affects popular support for the composition of public spending.

The remainder of the paper is organized as follows: Section 3.2 provides an overview of the legal framework governing marital and non-marital families. Section 3.3 investigates the relationship between the gender gap and non-marriage and Section 3.4 that between public spending and the gender gap. Section 3.5 concludes.

3.2 Marriage and private transfers

Our analysis is based on two observations. First, men transfer more resources to women within marriage than outside. Second, paternal links to children are weaker outside marriage than within. Although biological asymmetries in reproduction between men and women may provide the ultimate rationale for these stylized facts (for a discussion, see Edlund [2001]), the legal framework delineates these rights and responsibilities. We sketch the main developments pertinent to Western Europe.

Marriage is not the only way for men to obtain parental rights, although until as late as 1969, German law held that "an illegitimate child and its father are not deemed to be related" [Glendon 1996]. The overall trend in the Western world has been towards equalizing the status of children born in and out-of-wedlock and allowing fathers to obtain parental rights without marriage. Still, in no country is it the case that the legal rights and obligation stemming from marriage can be replicated through private contracting, and this is particularly true in the realm of parental and custodial rights. By considering families to consist of adults and their dependent children, the remainder of this section gives a brief overview of the legal differences between marital

[5] Mulligan and Sala-i-Martin [1999] focused on lobbying power as the determinant of public spending on the elderly.

and non-marital families in the following relationships: the rights of children vis-à-vis their parents, parents vis-à-vis children, and partners vis-à-vis each other.

Today, the rights of children born out-of-wedlock, to the extent possible, equal those of children born in-wedlock as outlined in the European Convention on the Legal Rights of Children Born Out of Wedlock. The convention was opened for signature in 1975 and has to date been signed by the following countries in our Eurobarometer sample: France, Italy, Denmark, Ireland, UK, and Sweden.

Turning to the rights of parents vis-à-vis their children, mothers are default custodians of their children irrespective of marital status. If unmarried, they are sole custodians, while if married, they share custodial rights with their husbands (and the child's presumed father). Unmarried mothers and fathers can, if mutually agreed upon, reallocate custodial rights so as to mimic the marital situation (with the exception of West Germany which did not allow unmarried fathers custodial rights until December 1997). Marriage is still the only way in which men obtain default parental rights to a woman's children, with the exception of Iceland, where cohabitation may establish paternity. Private contracting of parental rights is severely restricted since such contracts could amount to the selling of children, which is barred in all countries. Private contracting with respect to the allocation of custodial rights is not likely to be upheld by courts who will consider the interest of the child.

Regarding the rights of unmarried partners vis-à-vis each other, Napoleon famously concluded that "Concubines put themselves outside the law and the law has no interest in them" (quoted in Glendon [1996]). Still today, cohabitation does not imply financial obligations between partners in most countries. For instance, "German Law accepts the proposition that people living together are free not to marry and thereby to avoid the responsibilities and restrictions imposed upon married persons." Graue [1995]:193. In the United States, until recently, private contracts securing maintenance to the financially weaker partner (often the woman) were not upheld in courts on the ground that such contracts amounted to contracts for prostitution

[Folberg 1980]. Contracts need not be entered into explicitly but can be "implied-in-fact." In France, since the mid 20th century, cohabitation may give rise to a joint claim to the lease of the marital home, and a couple who live maritally (certified by two witnesses) can obtain a "certificate of marital life." In Sweden, the "Joint Homes Act" of 1987 established the "matrimonial" home as community property to cohabitants. However, in neither country do the rights arising from cohabitation amount to the rights implied by marriage.

Marriage typically establishes joint ownership of property acquired in marriage and in Europe, unless otherwise specified, of assets brought into the marriage, the legal framework for which has not changed much during the sample period. However, along other dimensions, one can argue that marriage has become more cohabitation-like. The largest change has been in the realm of divorce legislation. No-fault divorce was in place or introduced during the sample period in all countries save Ireland. Since the 1970s, wives are no longer legally subordinated to their husbands, and the obligation to provide for the family no longer rests solely on the husband. Since the 1990s, a wife can deny her husband marital relations.

3.3 Non-marriage and the political gender gap

We examine the relationship between non-marriage and the political gender gap in several ways. First, we combine political survey data for nine West European countries with data on the incidence of non-marriage in these countries to check whether increasing incidence of non-marriage has a differential effect on men and women's political preferences. We also use this data to examine whether the redistributive preferences of men and women differ. Finally, we use German longitudinal data to identify how actual changes in an individual's marital status affect his/her political leaning.

3.3.1 Evidence from Nine West European Countries

Our political survey data are from the Eurobarometer and the Swedish Election Studies (SES) surveys (1973-1996). The Eurobarometer covers member countries of the EU. We exclude countries with less than three years of either political survey data (Austria and Finland) or non-marriage data (Greece, Luxembourg, Spain and Portugal). Our final sample includes Belgium, Denmark, France, Ireland, Italy, Netherlands, Sweden, United Kingdom and West Germany.[6] We restrict attention to respondents aged 18-64. Table 3.1 provides descriptive statistics, and Appendix C describes variable construction.

An individual's political preference is obtained from the question: "If there were a general election tomorrow, which party would you vote for?" We use the Eurobarometer classification of political party ideology to identify whether a respondent favored the left.[7] Figures 3.2 and 3.3 document the country-wise development of the political gender gap, where the gap is defined as the difference between the fraction of women and men who favor the left. In 1973, more women than men favored the political right in all sample countries, save Denmark. However, by 1996, the gender gap had reversed in all but two countries (Belgium and the United Kingdom). Overall, the political gender gap increased by 7 percentage points, from -0.05 to 0.02.

Concurrently, there was a decline in marriage as people postponed or opted out of marriage.[8] Between 1973 and 1996, the mean female age of first marriage rose from 23 to 27 years while the incidence of divorce doubled from 21 to 56 per thousand adults in our sample countries. These statistics, however, underestimate the decline

[6]For SES ("Svenska Valundersökningar 1956-1998"), Bo Särlvik, Olof Petersson, Sören Holmberg were primary researchers ("primärforskare"), and the data were made available by Swedish Social Science Data Service (SSD), Gothenburg University.

[7]The classification of a party as belonging to the political left remains unchanged over the sample period.

[8]In our sample, the fraction respondents married fell from 77 percent in 1973 to 55 percent in 1996.

in marriage – both cohabitation and out-of-wedlock fertility rose. Lack of data on cohabitation restricts our analysis to out-of-wedlock fertility, a statistic which tripled from 9 to 28 percent.

We begin by estimating a linear regression of the form

$$l_{ikt} = c_k + \tau_t + (c_k \times \tilde{t}) + \alpha_1 f_{ikt} + \alpha_2 (f_{ikt} \times \tilde{t}) + \varepsilon_{ikt},$$

where l_{ikt} is a "left" dummy variable that equals 1 if individual i, in country k and year t supports the left, and 0 otherwise. c_k and τ_t are country and year dummies respectively. We also control for a linear country-specific time trend, $c_k \times \tilde{t}$. f_{ikt} is a female dummy ("female" in text). The coefficients α_1 and α_2 measure the unexplained initial level and the trend of the gender gap respectively.

The results are in Table 3.2, column (1). Every year, women, relative to men, become 0.3 percent more likely to favor the political left. The point estimates imply that, between 1973 and 1996, women shifted from being 4.3 percent less likely than men to favor the left to being 2.6 percent more likely.

This period witnessed marked changes in the educational, income and marital profiles of the population. To examine whether these changes can explain the trend in the political gender gap we estimate the following regression

$$l_{ikt} = c_k + \tau_t + (c_k \times \tilde{t}) + \alpha_1 f_{ikt} + \alpha_2 (f_{ikt} \times \tilde{t}) + \alpha_3 X_{ikt} + \alpha_4 \mu_{ikt} + \alpha_5 (f_{ikt} \times \mu_{ikt}) + \varepsilon_{ikt}$$

where μ_{ikt} indicates marital status, and X_{ikt} is a vector of individual demographic and economic controls.

Table 3.2, column (2) reports our findings. Older respondents are less likely to favor the left, while the 1943-1958 cohort is more left-wing. Other individual characteristics predict partisan preferences in a manner consistent with economic models of voting – better educated and/or richer individuals are less likely to favor left-wing parties. Unmarried individuals are more likely to do so, with women more so.[9]

The economic consequences of non-marriage may vary by type of non-marriage. In column (3) we control for type of non-marriage. Single, cohabiting and divorced or separated women are significantly more left-leaning than their male counterparts. This finding is consistent with the claim that, relative to cohabitation or divorce, marriage increases resource-sharing between men and women and therefore aligns their political preferences. However, it is also consistent with a selection-based story. That is, more left-leaning women are less inclined to marry. Still, the trend in the gender gap remains after the inclusion of these controls.

To address the concern of self-selection and to further explore the relationship between non-marriage and political preferences, we add country-level measures of non-marriage as covariates. We consider three measures of non-marriage – proportion of adults currently divorced (*Divorce*), fraction of births to unmarried mothers (*Out-of-Wedlock*) and mean female age of first marriage (*Marriage Age*). Our identifying assumption is that these measures are informative of an individual's marriage market expectations but are exogenous to any single individual's marital decision.

For each non-marriage measure, we report two specifications. First, we include it alone, and then interacted with the female dummy. Increases in *Divorce* make individuals more left-wing but do not affect the trend in the gender gap, Table 3.3 column (1). Column (2) shows that this variable has a significant gender differential effect – a 1 percentage-point rise in *Divorce* is associated with a gender gap of 1.3 percentage points. Moreover, the unexplained trend in the gender gap becomes statistically insignificant. Over this period, *Divorce* increased by 3.6 percentage points and the average gender gap went from -0.05 to 0.02, suggesting that the rise in divorce can account for a gender gap of 4.68 percentage points (3.6×1.3), or 67 percent of the actual gap.

[9]Unfortunately, we lack consistent survey data on number of children per respondent. The survey only asks about children residing at home. Moreover, in response to questions on number of children living at home, the answers "missing" and "none" have been coded together.

In columns (3) and (4) we consider *Out-of-Wedlock*. On average, increases in *Out-of-Wedlock* do not affect an individual's support for the left or the trend in the gender gap, column (3). This, however, masks significant gender differences. Increases in *Out-of-Wedlock* increase the number of women, but not men, who favor the left – a 1 percentage point rise in *Out-of-Wedlock* is associated with a gender gap of 0.26 percentage points, column (4). Thus, the rise in out-of-wedlock fertility can account for a gender gap of 4.9 percentage points (19×0.258), or 70 percent of the actual increase in the gender gap. Controlling for the gender-differential effect of *Out-of-Wedlock* renders the trend in the gender gap insignificant.[10]

Finally, we consider *Marriage Age*. Increases in *Marriage Age* make both men and women more right-wing, column (5). This effect is, however, significantly weaker for women, column (6). We conjecture that this is because increases in *Marriage Age* affect men and women's income in two ways. First, delays in marriage are likely associated with higher human capital investment and greater earnings potential for both genders. Second, delays in marriage reduce the (expected) transfers from men to women, for instance by postponing income pooling.

Edlund, Haider, and Pande [2004a] report various robustness checks, including specifications which control for individual and aggregate labor market participation.

3.3.2 Gender and redistributive preferences

We have argued that women favor the left because of its more generous redistributive policies, rather than its stance on other issues that divide the left and the right (for instance, immigration, abortion, law enforcement or the military). The 1992 Eurobarometer supplement survey asked questions on a respondent's preferences over

[10] In Edlund, Haider, and Pande [2004a] we report country-wise regressions. In every country, except the United Kingdom, the non-marriage variables have a gender differential effect on political preferences. This gender-differential effect is the most pronounced in Italy and West Germany. One interpretation is that countries where social acceptance of non-marriage is low and/or men face fewer legal requirements outside marriage to provide child support, non-marriage has a more divisive effect on the political preferences of men and women.

different types of redistribution, allowing us to investigate this thesis directly. We focus on redistributive preferences over general social protection, child-related benefits, and pensions (details on variable construction are in Appendix C).[11] Unlike the earlier regressions, we can control for the presence of a child under 15 in the household, but not family income.[12] Since some redistribution predominantly benefits the elderly, we include an "old" dummy which equals one if the respondent is aged 55 or above.[13]

Women are no more in favor of government provision of a "broad range of social security benefits" than men, Table 3.4, column (1). However, this is not true for public policies which benefit those with children. Women and respondents with children agree that "more special help should be available to single-parent families who raise their children alone", column (2). This group is also more likely to consider the length of maternity leave too short, column (3). Finally, women are significantly more likely to believe that the fair wage for a woman on maternity leave is her full wage, column (4). In column (5) the dependent variable is support for pensions. Women are not more likely than men to believe that those working should "ensure, through the contribution of taxes they pay, that elderly people have a decent standard of living". Taken together, these findings suggest that those more likely to have child custody – women, and those with a child living at home – favor greater redistribution towards children and child custodians but not other forms of redistribution.

3.3.3 Longitudinal evidence: German Socio-Economic Panel (GSOEP)

We have provided evidence from nine West European countries which suggests that faced with lower marriage expectations, women have increasingly chosen to favor

[11] On average, respondents who supports these redistributive policies are 8-16 percent more likely to favor the political left.

[12] The income information is missing for a quarter of our sample and therefore omitted.

[13] We include all respondents aged 18 and above.

the political left. If changes in an individual's marital status are not fully anticipated then we should see a similar pattern in longitudinal data – that is, transitions from marriage to non-marriage should presage women's, but not men's, switching to the political left.

In this section we use longitudinal data from the German Socio-Economic Panel (GSOEP, waves 1-22) to examine this possibility. Surveys were conducted in face-to-face interviews when possible, and re-interviewed on an annual basis. We restrict attention to West German respondents aged 18-45 in 1984 who have since been interviewed at least twice.[14] Among individuals who entered in 1984, sample attrition is, on average, 6.2% per year.[15]

The survey collects annual information on changes in respondents' marital/cohabiting status (on a monthly basis) during the last year. We use information on the respondent's marital/cohabiting status during the survey month.[16] Respondents are also asked (annually) which political party they support. We follow the Eurobarometer coding of German political parties to determine whether the respondent favored the left.

Table 3.5 provides variable definitions and summary statistics. Between 1984 and 2001, the number of married respondents rose from 60 percent for men and 69 percent for women to 80 percent for both sexes. Cohabitation increased from 6 to 7 percent for men and 6 to 8 percent for women. The proportion of respondents divorced, however, declined.

We start by examining how divorce affects men and women's political preferences. We exclude singles and widowed, and distinguish between respondents on

[14]This corresponds to Sample A and Sample B of the survey. East Germans were included in the survey after 1990. Households were chosen through a multi-stage random sampling process in West Germany.

[15]Attrition ranged from a high of 13.9% (from the 1st to 2nd year) to a low of 4.3%.

[16]We code the marital status of a respondent as missing in a year if he/she does not answer the question in a given year.

the basis of whether they have a child living with them at the point of divorce. We estimate the following equation:

$$l_{it} = \alpha_i + \beta_t + \phi_1 d_{it} + \phi_2(f_i \times d_{it}) + \phi_3(c_{it} \times d_{it}) + \phi_4(f_i \times c_{it} \times d_{it}) + \epsilon_{it}$$

where l_{it} is a dummy variable that equals 1 if individual i in year t favors a left-wing party, and 0 otherwise. α_i is an individual fixed effect, and β_t denotes year dummies. d_{it} is a dummy for whether an individual is divorced, and c_{it} for whether the respondent had a child under 16 living with him/her at the point of divorce. f_i is a female dummy.

Divorce makes a woman 15 percentage points more likely to favor the political left, Table 3.6, column (1). This is robust to child presence and labor force participation, column (2). Working makes the respondent less likely to favor the political left, although muted for women.

We separately consider the impact of cohabitation on the political preferences of individuals who transit from being single to cohabitating, and those who transit from cohabitation to marriage. For each of the two samples, we run regressions of the same form as for divorce.

Marriage makes a female cohabitant 6 percentage points less likely to support the political left, column (3). Once again, the effect does not vary with child presence and is robust to controlling for whether the respondent works, column (4). Working continues to make respondents favor the political right, with a weaker effect for women. In contrast, cohabiting reduces a woman's support for the left by almost 8 percentage points compared to her being single, column (5). The same transition leaves men's political preferences unaffected. Once again, this effect does not vary with child presence, column (6). Finally, for this sample we do not observe a gender differential effect of working.

The patterns in German longitudinal data mirror those found in the Eurobarometer data. Transitions out of marriage make women, relative to men, more left-leaning. The opposite is true of transitions into marriage. The observed changes in men and women's political preferences are consistent with the thesis that the resources women have access to increase as they move from being single to cohabiting and then being married. In contrast, child presence at the time of changes in marital status does not affect political preferences.

3.4 Non-marriage and public social spending

We have provided evidence that the rise of non-marriage in Western Europe has caused men and women's political preferences to diverge (for the US, see EP). We now examine the implications of this divergence for public spending.

A "naive" theory of public spending would assume that public spending is responsive to need and would therefore compensate for reductions in parental spending on children. This, in turn, would predict a mechanical link between single-parenthood and redistribution towards children. However, such a theory fails to consider the political economy of non-marriage. In particular, if what makes women turn to the State for redistribution makes men oppose the same, the net effect on popular support is ambiguous.

In this section we briefly outline why we would expect the relationship between non-marriage and redistribution towards children to be U-shaped and provide corroborative evidence from high-income OECD countries, 1980-1998.

3.4.1 Motivation

Our discussion draws heavily on the theoretical example presented in EP. Consider a three-generation population: children, working-age men and women and elderly. All working-age women have a dependent child. We assume assortative matching in marriage, and that a woman earns less than the man she would be married to. For simplicity, we assume the elderly have no earned income. Each demographic group may receive targeted transfers financed by a proportional income tax. Adult vote in line with their economic interests.

We start by examining popular demand for redistribution towards children, such as subsidized child care or cash allowances for children. Assume that an adult only benefits from such transfers as a custodian. The mother has sole custody unless married, in which case she and her husband share custody. The elderly do not have dependent children and therefore oppose redistribution towards children.

Analogous to the elderly, unmarried working-age men never favor redistribution towards children. Thus non-marriage among low-income parents reduces overall support for redistribution towards children. Conversely, non-marriage among richer parents increases support for redistribution towards children as long as the woman would be a net beneficiary outside, but not within, marriage. Under the assumption that non-marriage first occurred among the poor, and overtime increasingly involved the richer, we would expect non-marriage to first reduce and then increase overall support. That is, we would observe a U-shaped relationship between the incidence of non-marriage and support for redistribution towards children.

We would expect a similar pattern between non-marriage and redistribution towards poor working-age adults. However, the absence of a clear gender-differential component in such transfers (apart from women's being poorer) suggests that this relationship would be weaker than that between non-marriage and redistribution towards children.

The elderly are politically homogenous in that they all favor transfers targeted to them as a group (e.g., pensions or health care). To the extent that the elderly compete with the working-age and their (non-voting) children for State resources, the fractionalizing impact of non-marriage on the working-age population is likely to benefit the elderly. If so, a decline in support for redistribution towards children could be mirrored in an increase in transfers to the elderly.

Thus, we expect a U-shaped relationship between non-marriage and redistribution towards children and, potentially, transfers towards the working-age population. Furthermore, we would expect the opposite pattern to hold for transfers towards the elderly.

3.4.2 Empirical analysis

Our empirical analysis closely follows the above discussion. We analyze public spending data for high-income OECD countries for the period 1980-1998.[17] We group public spending into *Child*, *Working-age* and *Elderly transfers* (in each case normalized by country GDP). *Child-transfers* include parent cash benefits, family allowances for children, maternal and paternal leave, formal day care and other in-kind benefits. *Working-age transfers* include public expenditures on occupational injury, labor market programs, disability benefits, unemployment benefits, and housing. Finally, *Elderly-transfers* include old-age transfers, services for the elderly, and health expenditures.[18] Table 3.7 provides descriptive statistics by country.

For transfer p_{kt} in country k in year t we estimate the following regression:

$$p_{kt} = c_k + \tau_t + \beta_1 \nu_{kt} + \beta_2 \nu_{kt}^2 + \gamma X_{kt} + \varepsilon_{kt},$$

where ν_{kt} denotes the aggregate non-marriage variable. X_{kt} is a vector which includes the proportion of the population between 0-14, between 15-64, and log GDP in US 1995 dollars.

As before, we report results for three non-marriage variables – *Divorce*, *Out-of-Wedlock* and *Marriage Age* (Panels A, B and C of Table 3.8 respectively). For each category of transfers we report results for two samples. First, the countries for which a positive relationship between non-marriage and the gender gap is known to exist,

[17]We follow the World Bank's definition of high-income countries.

[18]We include health in this category as the elderly are important consumers of health care (however, our results are robust to its exclusion).

i.e., our Eurobarometer countries and the United States (EB+US sample). Second, the sample which includes an additional six high-income OECD countries for which consistent data is available (OECD sample).

Columns (1) and (2) of Table 3.8 consider *Child-transfers* as the dependent variable. For all three non-marriage variables, we find evidence of a U-shaped relationship (Panels A, B and C respectively), with the exception of *Out-of-Wedlock* for the OECD sample. For instance, for the OECD sample, the point estimates imply that the turning point is at 6.4 ($22.89/(2\times177.8)$) percent for the fraction of adults divorced (Panel A, column 2), and at 28 ($2.78/(2\times0.05)$) years for *Marriage Age* (Panel C, column 2). While the turning point for *Divorce* lies well within the range of the variable, the turning point for *Marriage Age* implies a negative relationship for nearly the entire range.[19] In contrast, the estimates for *Elderly-spending* imply an *inverted* U-shaped relationship, columns (5) and (6).

We have argued that the pattern for working-age transfers is likely to mimic redistribution towards children. However, the gender-differential impact of working-age transfers is also less clear. In columns (3) and (4) we find mixed evidence. For *Divorce* and *Marriage Age*, non-marriage first reduces and then increases working-aged transfers. The turning points are, however, later than those for redistribution towards children. However, the opposite pattern holds for *Out-of-Wedlock*. A possible explanation may be that higher rates of out-of-wedlock fertility are associated with a greater demand for income support among those of working-age.

These findings lend further support to the hypothesis that the economic implications of non-marriage turn on the provision for children and that this has contributed to the rise of the political gender gap.

[19]This is consistent with our previous finding that increases in *Marriage Age* turn both genders right, albeit women at a lower rate than men.

3.5 Discussion

This paper provides evidence on the political salience of marriage. Our analysis of political survey data makes a strong case for the decline of marriage having turned women left. In addition, evidence on redistributive preferences suggests that the gender gap in redistributive preferences is particularly pronounced in the case of State transfers towards children. Public spending data provide further evidence that provisions for children are an important mechanism linking non-marriage and the political gender gap.

This is the first study to our knowledge which presents evidence suggesting that the political economy of non-marriage may affect the allocation of public spending across different demographic groups. This is important not least because it demonstrates that public provisions for children are not only need-driven, but determined by the willingness of the electorate to internalize these needs.

Our findings strengthen the claim in EP that differences in redistributive preferences, not social attitudes, lie behind the gender divergence in political preferences. Relative to the right, the political left in every country in Western Europe is associated with greater preference for redistribution. However, it is difficult to think of a salient social issue on which the left and right parties consistently diverge across these countries. For instance, abortion rights have been, politically, much less salient and less divisive in Europe than in the United States. They also belie the contention that unmarried parenthood is functionally equivalent to married parenthood, a common perception in Europe where non-marital cohabitation has been more mainstream than in the United States.

We end with two speculations. First, our findings could potentially explain a seeming anomaly: the ability of the European extreme right to attract low-skilled men. Second, it points to a connection between the decline in marriage and the decline in fertility. Total fertility rates are well below replacement level, and falling, in the Western World. On the face of it, the link may appear tenuous. Some of the countries with the highest out-of-wedlock fertility rates also have the highest total fertility rates. However, it may be that in all countries, male private provision for children has fallen, as reflected by lower marriage rates. Still, in some countries public provision for children is high enough to make single-motherhood economically viable, thus creating a positive correlation between out-of-wedlock and total fertility.

Table 3.1: Descriptive statistics: Western Europe

	All	Men	Women
A.			
female	49.7		
left	50.5	50.3	50.7
unmarried	32.4	33.0	31.9
age [years]	39.5	39.7	39.3
	(13.0)	(13.1)	(12.9)
born 1959-78	19.0	19.0	19.1
born 1943-58	38.9	38.5	39.3
born 1921-42	37.2	37.5	36.8
born before 1920	4.9	5.1	4.8
less than high school	36.5	35.7	37.3
high school	39.4	37.2	41.7
more than high school	24.1	27.0	21.1
family income			
<50%-ile	42.7	40.0	45.4
>50%-ile	57.3	60.0	54.6
B.			
single	20.0	23.5	16.6
cohabiting	4.8	5.0	4.6
divorced/separated	4.6	3.4	5.8
widow(er)	3.4	1.4	5.5

Means in % except for age. Standard deviation for age in parentheses. Variable descriptions are provided in Appendix C. The individual data are from the Eurobarometer survey and the Swedish Election Studies. Respondent information by type of marital status (Panel B) is missing for Sweden. The number of observations are 96734 and 95438 for men and women respectively in Panel A and 86311 and 86278 in Panel B. Data span 1973-1996.

Table 3.2: Individual characteristics and the political gender gap

Dependent variable: left

	(1)	(2)	(3)
female	-0.043***	-0.053***	-0.048***
	(0.004)	(0.005)	(0.005)
female×	0.003***	0.003***	0.002***
time-trend	(0.000)	(0.000)	(0.000)
age		-0.003***	-0.003***
		(0.000)	(0.000)
born 1959-78		-0.299	-0.498
		(1.192)	(1.262)
born 1943-58		4.969***	4.205***
		(0.921)	(0.985)
born 1921-42		0.74	0.388
		(0.646)	(0.700)
High school		-9.020***	-8.101***
		(0.282)	(0.298)
More than		-10.452***	-8.826***
high school		(0.330)	(0.350)
family income		-5.500***	-6.078***
>50%-ile		(0.243)	(0.253)
unmarried		0.012***	
		(0.004)	
female×		0.018***	
unmarried		(0.005)	
single			-0.010**
			(0.004)
female×			0.013**
single			(0.006)
cohabit			0.049***
			(0.008)
female×			0.048***
cohabit			(0.011)
div-sep			0.047***
			(0.009)
female×			0.021*
div-sep			(0.012)
widow			0.049***
			(0.014)
female×			-0.050***
widow			(0.016)
N	235,734	192,172	172,589
Adj. R^2	0.03	0.05	0.05

OLS regression results are reported, with robust standard errors in parentheses. All regressions also include (not reported) (i) country and year dummies (ii) a country specific linear trend (iii) income variable interacted with Sweden dummy (as, for Sweden, income refers to individual, not household, income. Age, cohort, education and income variables are divided by 100. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3.3: Aggregate non-marriage and the gender gap – dependent variable: left

	Non-marriage variable (NM):					
	Divorce		Out-of-Wedlock		Marriage Age	
	(1)	(2)	(3)	(4)	(5)	(6)
female	-0.053***	-0.075***	-0.053***	-0.065***	-0.054***	-0.583***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.051)
female×trend	0.003***	0.001	0.003***	0	0.003***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
NM	2.437**	1.748*	0.068	-0.062	-0.029***	-0.041***
	(0.967)	(0.968)	(0.075)	(0.075)	(0.004)	(0.004)
female×NM		1.323***		0.258***		0.024***
		(0.106)		(0.018)		(0.002)
F-stat.		10.07		6.84		17.31
		(0.001)		(0.008)		(0.00)
N	192,172	192,172	192,172	192,172	191,642	191,642
Adj. R^2	0.05	0.05	0.05	0.05	0.05	0.05

OLS regression results are reported, with robust standard errors in parentheses. All regressions include as additional controls the covariates listed in Table 3.2, column 2.

The F-statistic tests the joint significance of NM and female×NM.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3.4: Gender gap in redistributive preferences

	social protection (1)	aid single parents (2)	maternity-leave length (3)	maternity-leave wage (4)	pensions (5)
female	0.012	0.051**	0.077***	0.061**	-0.005
	(0.022)	(0.020)	(0.021)	(0.024)	(0.016)
unmarried	0.003	0.011	-0.029*	-0.031	-0.006
	(0.018)	(0.017)	(0.018)	(0.021)	(0.013)
female×	0.012	0.019	0.043*	0.019	-0.001
unmarried	(0.024)	(0.021)	(0.023)	(0.026)	(0.017)
child	0.058***	0.037**	0.108***	0.032	0.018
	(0.020)	(0.018)	(0.020)	(0.022)	(0.014)
female×	-0.044	-0.029	0.002	0.007	-0.024
child	(0.027)	(0.023)	(0.027)	(0.029)	(0.020)
old	-0.032	0.026	0.060***	-0.028	-0.014
	(0.026)	(0.024)	(0.022)	(0.029)	(0.019)
female×	0.018	-0.016	-0.043*	-0.022	0.013
old	(0.027)	(0.025)	(0.024)	(0.029)	(0.019)
Adj. R^2	0.03	0.07	0.08	0.06	0.02
Mean	0.667	0.787	0.243	0.558	0.867
N	7,284	6,925	6,361	6,924	7,263

OLS regression results are reported, with robust standard errors in parentheses. All regressions include as additional covariates individual age, dummies for whether completed high school or more than high school and country dummies. The dummy old=1 if the respondent is 55 years of age or more, and the dummy child=1 if at least one child under the age of 15 is living in the household. Mean refers to the sample mean for the dependent variable. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3.5: Descriptive statistics, German Socio-Economic Panel (GSOEP)

		1984	2001
female		48.5	45.9
age	Men	32.1	49.1
	Women	32.4	49.0
married	Men	60.0	80.4
	Women	68.8	80.6
divorced	Men	1.3	0.6
	Women	3.5	2.4
cohabiting	Men	6.0	6.6
	Women	6.2	8.2
single	Men	31.5	5.7
	Women	20.1	3.9
child	Men	54.1	37.0
	Women	60.2	40.9
working	Men	84.2	82.8
	Women	56.6	67.6
left	Men	55.6	54.0
	Women	57.3	53.9

Values reported are means (%) for 2,405 respondents in 1984 and 1,058 respondents in 2001. The GSOEP collects information on changes in the respondent's marital status on a monthly basis since the previous survey year. An individual's marital status during the month of survey is used to create 0-1 marital status dummies. The sample excludes singles and widowed. The dummy child=1 if there is a child under the age of 16 living in the household, and the dummy working=1 if the respondent is currently employed. The survey asks respondents which political party they support. We follow the Eurobarometer classification to determine whether the political party is left-wing and create a 0-1 dummy, left.

Table 3.6: Marital status and support for the left: longitudinal evidence (GSOEP)

	Marital status (event):					
	divorce		cohabitation			
Sample:	all		cohabitants and married		singles and cohabitants	
	(1)	(2)	(3)	(4)	(5)	(6)
event	-0.019	-0.020	-0.016	-0.014	0.006	0.002
	(0.023)	(0.024)	(0.018)	(0.018)	(0.021)	(0.022)
female×event	0.150**	0.140***	0.065**	0.061**	-0.076**	-0.082**
	(0.046)	[0.046]	(0.029)	(0.029)	(0.038)	(0.038)
event×child	0.010	0.015	0.075	0.072	0.092	0.079
	(0.056)	(0.056)	(0.05)	(0.051)	(0.135)	(0.136)
female×event×child	-0.227	-0.148	-0.072	-0.079	0.077	0.087
	(0.201)	(0.236)	(0.076)	(0.078)	(0.142)	(0.143)
working		-0.036***		-0.039***		-0.031*
		(0.011)		(0.012)		(0.016)
female×working		0.022*		0.026*		-0.015
		(0.013)		(0.014)		(0.025)
N	25353	24182	23914	22789	6208	5992
Adj. R^2	0.81	0.81	0.81	0.82	0.76	0.77

OLS regression results are reported, with robust standard errors in parentheses. All regressions include individual and year fixed effects. The sample "all" in columns (1) and (2) excludes singles and widowed.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3.7: Country-level descriptives

	Non-marriage			Public social spending		
		Out-of-	Marriage			
	Divorce	Wedlock	Age	Child	Working-Age	Elderly
country	(1)	(2)	(3)	(4)	(5)	(6)
Australia	0.05	0.2	-	1.69	3.73	8.94
Belgium	0.04	0.1	23.89	2.52	9.68	13.18
Canada	0.04	0.26	25.98	0.71	3.57	10.66
Denmark	0.08	0.44	27.26	3.26	9.31	16.46
Finland	0.07	0.24	25.89	3.1	8.53	13.72
France	0.04	0.27	25.17	2.64	6.91	16.56
Germany	0.09	0.12	25.31	1.95	4.33	16.46
Ireland	0	0.15	26.24	1.58	7.18	9.73
Italy	0.01	0.06	25.24	0.95	5.36	16.39
Japan	-	-	25.67	0.43	2.04	9.16
Netherlands	0.05	0.11	25.45	1.79	11.36	13.26
New Zealand	0.04	-	-	2.33	4.83	12.8
Sweden	0.09	0.48	27.69	4.13	9.33	17.49
Switzerland	0.05	0.06	26.54	1.14	4.31	14.11
United Kingdom	0.06	0.25	25.5	2.26	6.31	13.33
United States	0.08	0.26	24.11	0.58	2.76	10.16
All	0.05	0.21	25.71	1.94	6.22	13.27

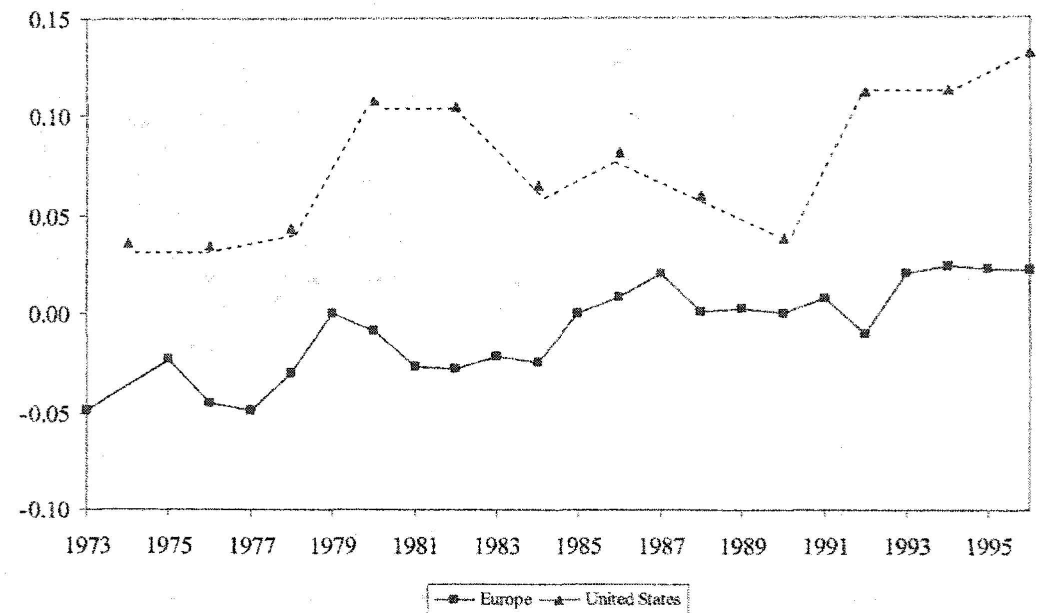
Sample means are reported. See Appendix C for variable construction and sample. The means for $Divorce$, $Out-of-Wedlock$, and $MarriageAge$ for the period 1973-1996 were for France 0.04, 0.21, and 24.41; Belgium 0.03, 0.08, and 23.29; The Netherlands 3 , 0.04, 0.08, and 24.58; Germany: 0.04, 0.09, and 24.82; Italy: 0.01, 0.05, and 24.75; Denmark: 0.07, 0.38, and 26.18; Ireland: 0.00, 0.11, and 25.79; United Kingdom: 0.05, 0.20, and 24.68; Sweden: 0.08, 0.43, and 26.85.

Table 3.8: Aggregate Non-Marriage and Public Social Expenditures, High-Income OECD countries 1980-1998

	Dependent variable:					
	Child-spending		Prime-spending		Elderly-spending	
Sample:	EB+US[a]	OECD[b]	EB+US	OECD	EB+US	OECD
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.	Non-marriage (NM): Divorce					
NM	-47.38***	-22.89**	-99.00***	-65.41**	196.48***	141.24***
	(8.33)	(10.56)	(31.12)	(28.01)	(33.13)	(32.63)
NM^2	233.17***	177.80**	457.40***	397.94***	-820.62***	-706.11***
	(50.42)	(75.07)	(143.87)	(150.01)	(173.19)	(159.42)
Adj. R^2	0.94	0.89	0.92	0.89	0.94	0.88
Panel B.	Non-marriage (NM): Out-of-Wedlock					
NM	-4.28**	0.8	11.86***	16.68***	18.79***	4.68
	(1.70)	(2.00)	(4.47)	(4.88)	(5.06)	(6.12)
NM^2	7.94***	2.05	-14.65*	-24.71***	-22.70***	-24.96**
	(2.56)	(3.08)	(7.51)	(8.19)	(8.14)	(9.76)
Adj. R^2	0.93	0.87	0.92	0.89	0.93	0.89
Panel C.	Non-marriage (NM): Marriage Age					
NM	-2.06***	-2.79***	-0.05	-3.66***	7.15***	1.3
	(0.41)	(0.50)	(1.31)	(1.11)	(1.68)	(1.71)
NM^2	0.04***	0.05***	0	0.06***	-0.13***	-0.03
	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)
Adj. R^2	0.94	0.93	0.92	0.90	0.93	0.89

[a] – 9 Eurobarometer countries and the United States.
[b] – 16 high-income OECD countries for which data were available.
Variable construction and sample are described in Appendix C.
OLS regression results are reported, with robust standard errors in parentheses. Regressions in Panels A, B and C have 180 (272), 182 (239), and 180 (249) observations in odd (even) columns.
All regressions include as additional covariates country dummies, log GDP in US 1995 dollars, the proportion of the population aged 0-14 and the proportion aged 15-64.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 3.1: Political Gender Gap in Europe and the United States



Notes and Sources: The gap is defined as the difference between the proportion of women who favor the left and the proportion of men who favor the left. The data sources for Europe include the Eurobarometer surveys and the Swedish Election Studies. For the United States we use the National Election Studies.

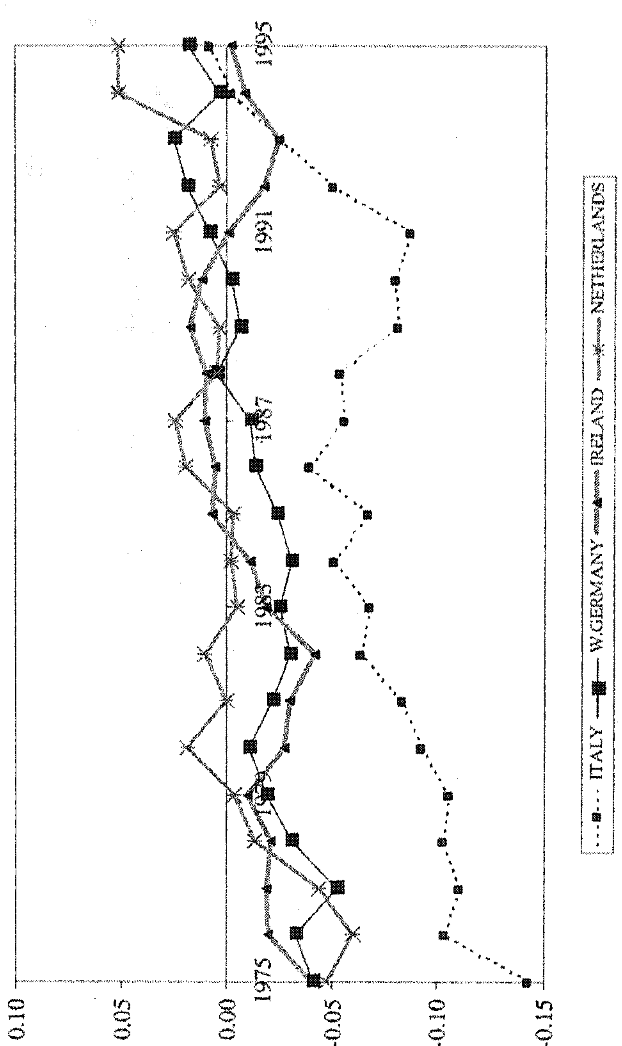
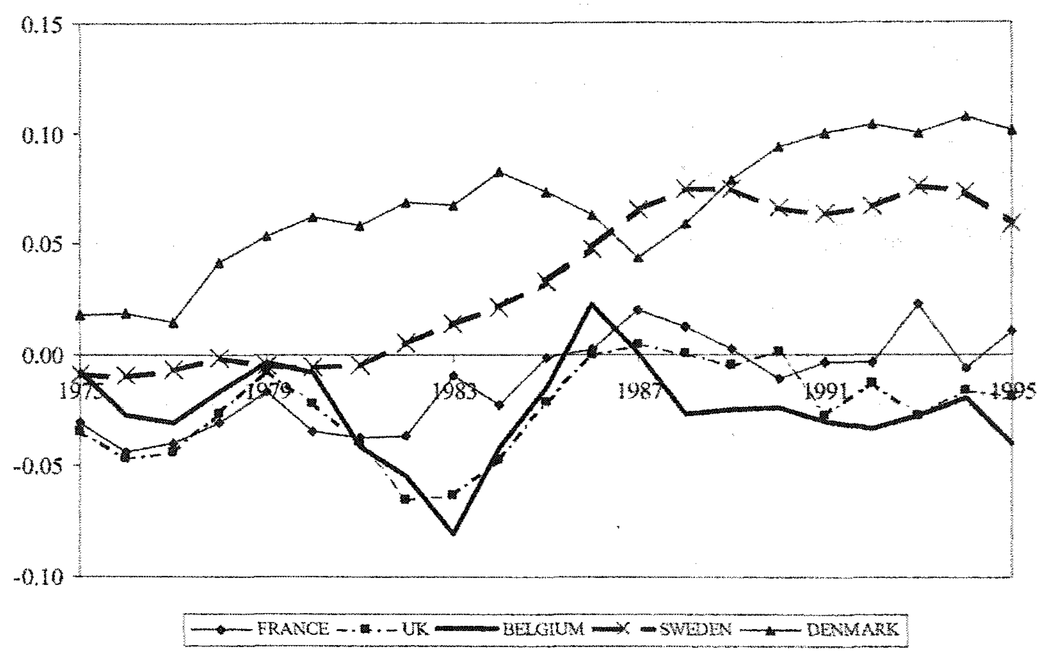


Figure 3.2: Political Gender Gap, by Country

Figure 3.3: Political Gender Gap, by Country



Notes and Sources: The gap is defined as the difference between the proportion of women who favor the left and the proportion of men who favor the left. Three year moving averages reported. The data source is the Eurobarometer surveys for all countries except Sweden. For Sweden we use the Swedish Election Studies.

Appendix A

Data Appendix to Chapter 1

Individual-level data

Data are drawn from the biennial National Election Studies (NES) and the General Social Survey (GSS) over the period 1972-2000. The samples are combined and restricted to respondents aged 18 to 64. Individual-level records from GSS are appended only for the years when the NES was conducted. This is equivalent to using GSS data from each even-numbered survey year.[1] This leaves me with 15 rounds of survey data. The responses "No answer", "do not know" and "not applicable" are coded as missing values.

female Dummy equals 1 if respondent is female.

black Dummy equals 1 if respondent is African-American.

white Dummy equals 1 if respondent is Caucasian.

age Respondent age in years.

left Original question: "Generally speaking, do you think of yourself as a Republican, a Democrat, an Independent or what?" Prompted answers coded as 1

[1]No GSS data are available in 1992 so the NES data stand alone.

= Strong Democrat; 2 = Weak Democrat; 3 = Independent-Democrat; 4 = Independent-Independent; 5 = Independent-Republican; 6 = Weak Republican; 7 = Strong Republican. Dummy equals 1 if respondent answered 1-3 from above classification.

redist Original question (GSS only): "The government should reduce income differences between the rich and the poor, perhaps by raising the taxes of wealthy families or by giving income assistance to the poor." The responses range from 1 = should not to 7 = should on a 7-pt scale. Dummy equals 1 if respondent answered 5-7.

Appendix B

Data Appendix to Chapter 2

Individual-level data

Data are drawn from the biennial National Election Studies (NES) and cover the period 1952-2000. The sample is restricted to birth years 1908-1982 and to respondents aged 18 and above. There is no survey in 1954, and the 1962 and 1998 surveys do not provide information on the US state where the respondent grew up. This leaves me with 22 rounds of NES data. The responses "No answer", "do not know" and "not applicable" are coded as missing values.

female Dummy equals 1 if respondent is female.

black Dummy equals 1 if respondent is African-American.

age Respondent age in years.

high-school attendance Original question: 1952-1972 "How many grades of school did you finish?" 1974-2000 "What is highest grade of school or year of college you have completed?" Dummy equals 1 if the respondent went beyond grade school (0-8 grades).

high-school graduation Dummy equals 1 if the respondent holds a high-school diploma (or a higher degree).

left Original question: "Generally speaking, do you think of yourself as a Republican, a Democrat, an Independent or what?" Prompted answers coded as 1 = Strong Democrat; 2 = Weak Democrat; 3 = Independent-Democrat; 4 = Independent-Independent; 5 = Independent-Republican; 6 = Weak Republican; 7 = Strong Republican. Dummy equals 1 if respondent answered 1-3 from above classification.

father Democrat Original question: "When you were growing up did your father think of himself mostly as a Democrat, as a Republican, or what?" Prompted answers coded as 1 = Democrat; 2 = Independent; 3 = Republican. Dummy equals 1 if respondent answered Democrat.

mother Democrat Original question: "When you were growing up did your mother think of herself mostly as a Democrat, as a Republican, or what?" Prompted answers coded as 1 = Democrat; 2 = Independent; 3 = Republican. Dummy equals 1 if respondent answered Democrat.

govspend Original question: "Some people think the government should provide fewer services, even in areas such as health and education, in order to reduce spending. Other people feel that it is important for the government to provide many more services even if it means an increase in spending. Where would you place yourself on this scale, or haven't you thought much about this?" 7-point scale shown to respondent where 1 = Government should provide many fewer services: reduce spending a lot, and 7 = Government should provide many more services: increase spending a lot. Dummy equals 1 if respondent answered 5-7 on this scale.

defense Original question: "Some people believe that we should spend much less money for defense. Others feel that defense spending should be greatly increased. Where would you place yourself on this scale or haven't you thought much about this?" 7-point scale shown to respondent where 1 = Greatly decrease defense spending, and 7 = Greatly increase defense spending. Dummy equals 1 if respondent answered 5-7 on this scale.

pro-choice Dummy equals 1 if respondent stated that abortion should be permitted if, due to personal reasons, the woman would have difficulty in caring for the child, or that abortion should never be forbidden, since one should not require a woman to have a child she does not want.

equal roles Original question: "Recently there has been a lot of talk about womens rights. Some people feel that women should have an equal role with men in running business, industry and government. Others feel that womens place is in the home. And other people have opinions somewhere in between. Where would you place yourself on this scale?" 7-point scale shown to respondent where 1 = Women and men should have an equal role, and 7 = Women's place is in the home. Dummy equals 1 if respondent answered 1-3 on this scale.

civil rights Original question: "Some say that the civil rights people have been trying to push too fast. Others feel they haven't pushed fast enough. How about you: Do you think that civil rights leaders are trying to push too fast, are going too slowly, or are they moving about the right speed?" Dummy equals 1 if respondent does not think that civil rights leaders are pushing too fast.

church attendance Dummy equals 1 if respondent attends church two or more times a month.

State-level data

compulsory schooling laws State-level data on compulsory attendance and child-labor laws are from Acemoglu and Angrist [2000]. Variable construction is described in the text above. See Appendix B of Acemoglu and Angrist [2000] for data sources.

Governor Democrat Dummy equals 1 if governor of state is a Democrat. Data on gubernatorial elections are from Wolfers [2002]. The original data source is ICPSR, *Candidate and Constituency Statistics of Elections in the United States, 1788-1990.*

Appendix C

Data Appendix to Chapter 3

A. Individual Data

We use 45 Eurobarometer Surveys (EB), twice-yearly 1973-1996, and 11 Swedish Election Studies surveys (SES).[1] "No answer", "do not know" and "not applicable" are coded as missing values.[2] Variables with non self-explanatory names are described below.

left Dummy equals 1 if respondent supports a left party. The respondent was asked "If there were a General Election tomorrow which party would you support?"(EB), and 'Which party do you like best?"(SES). For EB we follow survey classification of parties as left-wing, and for Sweden the left includes the Social Democratic party and all parties to its left.

education (EB) "How old were you when you finished your full-time education?" For "still studying" respondent education was imputed from his/her age. (SES) Respondents stated educational attainment. The education dummies are: (i) less than high school (or 0-15 years old); (ii) high school (or 16-19 years); (iii) more than

[1]SES surveys were conducted during election or referendum years: 1973, 1976, 1979, 1980, 1982, 1985, 1988, 1991, 1994 and 1995.

[2]The SES does not distinguish between married and cohabiting couples. Dummy variables for type of marital status are created for EB, 1975 onwards.

high school (or 20 or older).

income (EB) gives quartile position of respondent's family income in own country's income distribution. (SES) gives respondent's income (1976 and 1979 surveys give family income). When respondent income is reported, we place individuals according to position in own gender income distribution (obtained in sample). We use income dummies for family income in lower and upper half of income distribution.

Redistributive preferences (EB, 1992) Dummy equals 1 in the three cases below if the respondent answered "Yes" to the question.

social protection "The government must continue to provide everyone with a broad range of social security benefits even if this means increasing taxes or contributions."

aid single parents "Do you think that more special help should be available to single-parent families who raise their children alone?"

pension "Those who are now working have a duty to ensure, through the contributions or taxes they pay, that elderly people have a decent standard of living."

maternity-leave length Dummy equals 1 if the respondent answered "Too short" to question "Do you consider maternity leave to be too long, about right or too short?"

maternity-leave wage Dummy equals 1 if the respondent answered "Her full wages or salary" to question "What do you consider to be a fair wage for a young mother on maternity leave?"

Relevant notes on variable construction from German Socio-Economic Panel (GSOEP), 1984-2001, are in Table 3.5.

B. Aggregate Data

Non-marriage Country-wise measures of non-marriage are obtained from World Bank's World Development Indicators (WDI), UN Demographic Yearbook, Eurostat, United Nations Unified Database and country statistical offices.[3] Variable definitions

[3]We linearly interpolated divorce data in the case of Belgium and Italy, for most of the 70s and 80s.

are in main text. Divorce was legalized in Ireland in 1997. Therefore, we code Divorce as 0 for Ireland 1980-1997 and do not include the last year, 1998, since this year is unlikely to be representative. In the social spending regressions, for each non-marriage measure, we exclude countries with less than three years of data. Also in social spending regressions, unlike those using individual data, we (i) use data for unified Germany from 1990 onwards; and (ii) use data for entire United Kingdom (individual regressions use proportion of adults divorced in England and Wales).

Social Spending We use data on three categories of public social expenditure spending, obtained from the OECD Social Expenditure Database. Our sample is high-income OECD countries (WDI definition) for which annual data was available for the years 1980-1998 (see Table 3.7 for list of countries). All spending data enter regressions as a percent of GDP.

Child spending Groups family allowances for children, parental leave, lone parent cash benefits, family support benefits and other family cash benefits, formal day care, personal services, household services, and other in-kind family benefits.

Working-age spending Groups disability benefits, occupational injury and disease, sickness benefits, survivors, labor market programs, unemployment, and housing.

Elderly spending Groups old-age transfers, services for the elderly, and health expenditures.

GDP in 1995 US dollars are from the OECD Social Expenditure Database.

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