Political Socialization in Flux? Linking Family Non-Intactness during Childhood to Adult Civic Engagement

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Abstract

Some sociologists argue that non-intact family structures during childhood have a negative effect on adult children's civic engagement, since they undermine, and in some cases prevent, the processes and activities through which parents shape their children's political attitudes and orientations. In this paper, we evaluate this hypothesis on the basis of longitudinal data from the German Socio-Economic Panel. In a first step, we construct various measures of family structure during childhood, and perform both cross-sectional and sibling difference analyses for different indicators of young adults' civic engagement. Both exercises reveal a significant negative relationship between growing up in a non-intact family and children's political engagement as adults. In a second step, we implement a novel technique—proposed by Oster (2014)—for evaluating robustness of results to omitted variable bias. The distinctive feature of this technique is that it accounts for both coefficient movements *and* movements in R-squared values after the inclusion of controls. Results suggest that our estimates do not suffer from omitted variable bias.

May 11, 2015

1. Introduction

Since the 1970s, economic and social changes in western societies have resulted in families that are more complex in their structure. Family breakdown through separation or divorce has become common, and non-marital birth rates have increased dramatically and appear to be continuing to rise (OECD, 2011). Since most divorces involve children, there is now a substantially higher probability that children will have a lone parent than was once the case. Projections of future changes in family structures indicate that the number of single-parent families is likely to further substantially increase over the next two decades in most OECD countries (OECD, 2012).

An extensive body of research across a range of disciplines has identified childhood family structure as a key determinant of children's later-life socio-economic outcomes, emphasizing that children who grow up in a non-intact family: (i) tend to perform less well in school and to gain lower educational qualifications than children from intact families (Case *et al.*, 2001; Ermisch *et al.*, 2004; Gruber, 2004); (ii) are more likely to leave home when young and to become sexually active or pregnant at an early age (McLanahan and Sandefur, 1994); (iii) tend to report higher levels of smoking (Francesconi *et al.*, 2010b). However, one aspect of childhood family structure has remained largely neglected in the literature: its impact on children's later-life civic engagement. The primary contribution of this paper is to fill that void in the empirical literature.

In discussions on children's socialization into politics and civic affairs, it has long been recognized that the family reproduces interest in the public domain (Hyman, 1959). This idea is based not only on evidence of a transmission of basic civic responsibilities and political orientations from parents to their offspring (Alesina and Giuliano, 2011), but also on findings in the area of partisan commitment and electoral behavior indicating high intergenerational agreement (Jennings *et al.*, 2009). However, the research has also shown that the success of parental socialization of beliefs and values may differ systematically with family structure. In particular, it has been suggested that growing up in a non-intact family frequently deprives children of important parental and community resources, in turn leaving them with a lack of knowledge and skills to operate effectively in society. One hypothesis, therefore, is that non-intact family structures during childhood have a negative causal effect on adult children's civic engagement, since they undermine, and in some cases prevent, the processes and activities through which parents shape their children's political orientations (*causation hypothesis*). The opposite hypothesis is that factors which increase the risk of family breakdown are also linked to children's civic and political engagement, implying that at least part of the relationship has to be seen as selective (*selection hypothesis*).

To gain a deeper understanding of these issues, we use data from the German Socio-Economic Panel on about 6,000 individuals with matched parental marital histories and family characteristics. The data allows us to derive measures of family non-intactness during childhood by exploiting parental divorces and extramarital births. As will become apparent later, our definition of extramarital births encompasses all births to nonmarried mothers, even if they are in a stable relationship. In a first step, we perform both cross-sectional and sibling difference analyses for different indicators of civic engagement. Sibling difference estimations rest on less restrictive assumptions than cross-sectional estimations as they allows us to compare adult children to their own siblings and so to control for time-invariant fixed effects of mother and neighborhood characteristics that may influence both childhood family structure and young adults' political behavior. Both exercises reveal a significant negative relationship between growing up in a non-intact family and children's political engagement as adults. For example, sibling difference estimates suggest that adult children who lived in a non-intact family during childhood have a 9 percentage points lower likelihood of being interested in politics. This is a sizable effect, given that 24 percent of adult children report being interested in politics.

In a second step, we acknowledge that concerns about bias from unobserved controls may remain even after netting out time-invariant unobservable mother and neighborhood characteristics using sibling-difference estimators. To address these concerns, we follow Oster (2014) and implement a novel procedure for evaluating robustness of results to omitted variable bias. The distinctive feature of this procedure is that it allows for a "full adjustment" by not only exploiting information on coefficient movements after the inclusion of controls, but also information on movements in R-squared values. To the best of our knowledge, the full adjustment derived in Oster (2014) has previously not been used in the sibling fixed-effects literature or in the literature studying intergenerational effects. We show that all results in the paper survive a fully adjusted bias assessment. Thus, our findings are consistent with causation rather than selection as the explanation for the negative relationship between growing up in a non-intact family and children's civic engagement as adults.

2. Civic Engagement: Trends, Relevance and Theories

In the last few decades, the developed world has seen not only radical changes in family structures, but also another significant social development: the increasing disengagement of citizens from public affairs. Established democracies are managing to motivate an ever smaller proportion of the electorate to exercise the right to vote. The broad pattern of reduced civic engagement is confirmed by evidence in other domains as well. Indeed, Putnam (2002) argues that participation in political parties, unions and churches is almost universally waning. While the decrease in civic engagement is prevalent throughout the population, additional evidence shows that declines in voting, political interest and association membership are much more pronounced among younger cohorts than among older cohorts (Putnam, 2000).

These trends are a matter of concern for at least two important reasons. First, it is well understood that civic engagement strengthens civic values among the population and enhances the responsiveness of government and political elites to citizens' concerns (Fukuyama, 1995; Uslaner, 2002). Second, higher levels of civic engagement tend to go hand in hand with higher levels of social capital. This not only enhances democratic representation by facilitating interest aggregation and articulation (Putnam, 1993), but also promotes collective and collaborative action (Arrow, 1974). Moreover, a growing body of research suggests that social capital also has a positive impact on a wide range of macroeconomic and microeconomic outcomes. For example, Knack and Keefer (1997) provide strong evidence that norms of civic cooperation have significant impacts on aggregate economic activity.

The importance of civic engagement has made it a major focus of analysis and commentary in the social sciences. Explanations for the contemporary phenomenon of growing civic disengagement can be broken down into three main theories (for a detailed discussion see Hay, 2007). The "social capital" thesis (Putnam, 2000) associates declining civic engagement with an accelerated tendency towards individualism, leading to a disintegration in the community bonds that once held society together. The "critical citizens" thesis (Norris, 2002) suggests that younger cohorts of voters are more difficult to please than their parents' or grandparents' generations, and that they tend more to express dissatisfaction through abstention from political affairs. Finally, the "voting age" thesis (Franklin, 2004) argues that the lowering of the voting age in most advanced democracies can account almost entirely for recent declines in voter turnout. Of these explanations, the social capital hypothesis is the most pertinent to the study at hand. For Putnam (2000), lack of civic engagement and declining respect for the obligations of citizens in democracies are the result of the pervasive individualism that accompanies the disintegration of communities. Our findings add a new dimension to this view by showing that the *family disintegration* may also be a root cause of civic disengagement.

3. Why Does Family Structure Matter?

As we have seen above, the decline in civic engagement over recent decades coincided with a breakdown in traditional family structures. Despite this, Putnam (2000) dismisses the decline in the traditional family as a possible explanatory factor for the erosion of civic engagement:

> "... apart from youth- and church-related engagement, *none* of the major declines in social capital and civic engagement that we need to explain can be accounted for by the decline in the traditional family structure. In my view, there are important reasons for concern about the erosion of traditional family values, but I can find no evidence that civic disengagement is among them." Putnam (2000, p.279)

An alternative view suggests that parents can have an enormous influence on their children's political learning in pre-adulthood (Jennings *et al.*, 2009). On the one hand, this view coincides with childhood socialization theory, which emphasizes the role of the family in maintaining continuity in social ideologies over time (Glass *et al.*, 1986, and references therein). More generally, however, it is also compatible with the idea that social capital within the family is of key importance for a child's intellectual development. In this regard, Coleman (1988, 109–113) argues that: (i) social capital within the family depends on the physical presence of adults and the time and effort spent by the adults with a child on intellectual matters; (ii) the physical absence of adults can be described as a structural deficiency in family social capital; and (iii) the most prominent element of structural deficiency in modern families is the single-parent family. Given these arguments, one might expect that the decision of parents to live separately—e.g., as a result of a divorce—damages the social capital that might have been available to the child had it been raised jointly by both parents. We therefore hypothesize that young adults who experience family non-intactness as children are less likely to be civically engaged than those from intact families. We now turn to empirically examining this hypothesis.

4. Data

Our data source is the German Socio-Economic Panel (SOEP distribution v26), a representative longitudinal survey of private households in Germany. The SOEP is the second longest running longitudinal household survey in the world and similar to the Panel Study of Income Dynamics (PSID) in the United States and the British Household Panel Survey (BHPS) in the United Kingdom. We combine information from the first 26 annual interview waves (1984-2009). In the SOEP, individuals are re-interviewed each successive year. If children move out of their parents' home to form a new household, they are followed and all adults living in the new household are invited to become SOEP respondents as well. Moreover, children living in SOEP households become adult respondents in their own right the year they turn 17. The SOEP also collects retrospective lifetime marital, fertility and employment histories, which for many respondents span the pre-panel period, i.e., years before 1984. We combine retrospective information provided by mothers with their annual interview data. By using mother-child identifiers, we match all maternal characteristics to adult children. We focus on mothers as there are only very few families headed by single fathers in the sample. We then reconstruct a respondent's childhood family structure by combining his or her birth date and the mother's marital history. The data is therefore well suited for the analysis of childhood family structure and its effects on young adults' outcomes. In particular, the long panel structure enables us to difference out unobserved time invariant family specific-effects by estimating sibling differences models. Second, since parents' family structure is self-reported, the estimates are likely to be less affected by measurement error than if it were reported by adult children retrospectively.

For the pooled cross-sectional analysis (individual sample) in which we study repeated observations of civic outcomes in relation to childhood family structure, we select individuals who: (i) were 18 or younger in their first year as SOEP respondents; (ii) were living with their biological mother for at least one year during the panel; and (iii) have complete information on their mother's family history. We impose condition (i) to ensure an age structure that captures young adults and minimizes the risk of oversampling adult children who leave their parents' house at a relatively late age. Condition (ii) is necessary to match mothers' characteristics to young adults, as both generations had to be interviewed as adults. Condition (iii) ensures that childhood family structure can be consistently reconstructed for all young adults. Note that we use repeated observations of civic outcomes to improve the estimators precision.

In a second step, we estimate sibling difference models (sibling sample), which requires us to impose further conditions: (iv) an individual must have at least one sibling; (v) civic outcome measures of the siblings must be observed in the same year. Conditions (i) through

(v) are used to construct the sibling sample. For similar sample selection approaches and discussions see Ermisch *et al.* (2004), Francesconi *et al.* (2010a,b) and Siedler (2011).

4.1. Civic Engagement

As the main dependent variable in our empirical analysis, we use an index of civic engagement that averages together four component measures of civic engagement: (i) *political interest*; (ii) *party identification*; (iii) *organizational involvement*; and (iv) *individual voluntarism.* In order to come to as broad a conclusion as possible, we not only present findings for the summary index, but also report results for its components. Our information about *political interest* and *party identification* is derived from questions asked in the SOEP survey in the years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey in 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. We now provide a detailed description of our outcome variables.

Political Interest. In line with recent research suggesting that citizens with a greater interest in politics are more likely to be involved in public affairs (Bekkers, 2005), we view political interest as one precondition for civic engagement. In the empirical work, we make use of a survey question which reads: "Generally speaking, how much are you interested in politics?". We create an indicator variable which equals one if an individual reports being interested in politics ("very much" or "much"), and is zero for those who declare that they are not interested ("not so much" or "not at all").

Party Identification. In the past few decades, the concept of party identification has reached an important position in electoral research because public attachment to political parties is seen as a key determinant of many different aspects of political behavior (Dalton, 2002). For example, partisan ties motivate people to participate in parties, elections, and the processes of representative government. One survey question asks: "Many people in Germany lean towards one party in the long term, even if they occasionally vote for another party. Do you lean towards a particular party?". We construct an indicator variable that equals one if a respondent reports a long-term identification with a democratic party, and zero otherwise. In this definition, the right-wing extremist parties "NPD", "Republikaner", and "DVU" are not considered democratic parties.

Organizational Involvement. While official membership in formal organizations is only one aspect of civic engagement, it is regarded as a useful indicator of community involvement (Putnam, 2000). To construct a measure of organizational involvement, we use a question that reads: "Which of the following activities do you take part in during your free time? Please check off how often you do each activity: at least once a week, at least once a month, less often, never." We construct a dummy variable that equals one for individuals who report some kind of "involvement in a citizens' group, political party, or local government", and is zero for respondents who report no involvement at all.

Individual Voluntarism. For Putnam (2000), volunteering—i.e., the readiness to help others—is an important aspect of good citizenship and political involvement. He argues, for example, that volunteers are more interested in politics and less cynical about political leaders than non-volunteers are. To quantify individual voluntarism, we create an indicator variable that equals one for respondents who report doing "voluntary work in clubs or social services" ("at least once a week", "at least once a month", "less often"), and is zero for individuals who report doing no volunteer work ("never").

Index of Civic Engagement. Our main variable of interest is an index of civic engagement that aggregates the four component measures described above. We derive our index of civic engagement from all survey years in which each of the four component measures is collected. The aggregation improves the statistical power by identifying effects that point in the same direction for different outcome measures with identical domains. As suggested by Kling *et al.* (2007), the summary index of civic engagement is an equally weighted average of its components' z-scores. The component measures used are such that higher scores reflect higher civic engagement. To compute the z-scores of each component, we subtract the mean in the estimation sample, in which none of the other components and none of the control variables has missing information, and divide it by the standard deviation. Thus, each standardized component has a mean of zero and a standard deviation of one. The index then aggregates the components with equal weights. Estimated coefficients on dichotomous explanatory variables can therefore be interpreted as percentage changes in standard deviations.

4.2. Childhood Family Structure

Our measure of childhood family structure, *ever lived in a non-intact family*, is derived using the self-reported marital histories of biological mothers. The sample is restricted to individuals whose mothers have complete marital histories spanning the individual's entire childhood. An individual is defined as having experienced family non-intactness during childhood if the child's mother was ever unmarried before he or she reached the age of 16, either because the parents ended their marriage (*divorced parents*), or because the mother was unmarried when she gave birth and did not marry within the next year (*born outside marriage*).

The variable *divorced parents* captures individuals who lived with divorced parents for at least one year of their childhood (ages 0-16). In light of Coleman (1988), we would expect children of divorced parents to suffer from a structural deficiency in family social capital, and therefore be civically less engaged than children from intact families. The variable *born outside marriage* captures individuals either born to lone mothers or to cohabiting parents. It is important to note that children born outside marriage have not necessarily experienced family non-intactness, since cohabiting parents may have provided an intact family environment throughout the entire childhood period. However, it is well understood that mothers who were not in a relationship at the birth of their child are more likely to continue living as a lone parent than those in a married relationship (Kiernan and Mensah, 2010). Moreover, a greater fragility of cohabiting unions compared with marital ones has been observed in most developed nations (Bumpass and Hu, 2000; Andersson, 2002). Since our data do not allow us to identify cohabitation history, we use the variable *born outside marriage* as a proxy for the family disruptions that affect children born into cohabitation or other unions than marriage. Because the variable *born outside marriage* captures not only children who experience family non-intactness at some time during their childhood, but also those who experience an intact family environment with long-term cohabiting parents, the estimates for it represent a conservative lower bound for the effect of family non-intactness and born outside marriage on civic engagement.

4.3. Control Variables

Cross-sectional relationships between childhood family structure and adult civic engagement could be driven solely by selection, i.e., factors that increase the risk of family non-intactness may also be linked to children's civic engagement. We therefore choose potential observable confounders as control variables. Bedard and Deschenes (2005) show that the gender of the first child is related to parents' propensity to divorce. Gender is also likely to affect the political participation and civic engagement of adult children. Therefore, gender is among our control variables in the cross-sectional estimation.

Maternal education is also likely to affect both the probability of family non-intactness and the political behavior of children. While the classical Beckerian prediction for the effect of education on divorce is ambiguous (Becker *et al.*, 1977), we would expect parental education to go hand in hand with political and civic participation. We control for mothers' education by including three mutually exclusive indicator variables that equal one if they have completed intermediate secondary school (Realschule), advanced secondary school (Abitur), or a university degree, and zero otherwise. Fathers' education is not included because we rarely observe fathers' educational attainment after a divorce or in extramarital birth circumstances.

Closely related to the educational attainment of mothers is their labor market attachment. To control for a correlation between labor market participation and non-intactness and possible adverse effects on children's development (Ruhm, 2004), we add the number of years a mother worked part-time and the number of years she worked full-time during the individual's childhood. As before, fathers' labor market history is missing too often in the case of non-intact families to ensure a non-selective sample when included.

To complete our set of control variables we add usual socio-demographic measures, which may cause selection bias if not adjusted for. Among these are the mother's age at birth, a maximum set of adult children's age dummy variables, federal state dummy variables ("Bundesländer"), year of interview dummy variables, an only child indicator, and dummy variables for children's birth order. We also control for the SOEP respondent samples to account for peculiarities in survey-specific selection.

Finally, we present specification with and without controlling for household income during childhood. As stated in Adda *et al.* (2011), reduced income is one channel through which family dissolution affects life outcomes, suggesting that household income would be a bad control. Household income during childhood after departure of the father is likely to consist mainly of mother's income and is probably not a good measure of financial constraints affecting children if the children also benefit from an absent father's income. Moreover,

Table 1: Summary statistics, by sample

	Basic sample		Sibling sample	
	Means	Standard deviation	Means	Standard deviation
Dependent variables				
Political interest	0.24	0.43	0.24	0.43
Party identification	0.30	0.46	0.30	0.46
Organizational involvement	0.07	0.26	0.07	0.26
Individual voluntarism	0.33	0.47	0.35	0.48
Explanatory variables				
Ever lived in a non-intact family	0.21	0.41	0.18	0.38
Parents divorced	0.15	0.35	0.12	0.32
Born outside marriage	0.09	0.28	0.06	0.22
Control variables				
Age	24.19	6.33	24.61	5.76
Female	0.50	0.50	0.51	0.50
Mother's age at birth	26.65	5.09	26.36	4.78
Only child	0.13	0.33		
Firstborn child[a]	0.40	0.49	0.38	0.48
Second born child[a]	0.40	0.49	0.42	0.49
Third born child or higher birth order[a]	0.20	0.40	0.20	0.40
Mother's highest educational attainment				
Secondary school certificate or less	0.46	0.50	0.45	0.50
Intermediate school qualification	0.34	0.47	0.35	0.48
High school degree	0.04	0.19	0.04	0.20
Technical college or university degree	0.16	0.37	0.16	0.37
Mother's employment during childhood				
Number of years part-time employed	4.76	5.31	4.83	5.23
Number of years full-time employed	5.90	6.35	5.23	6.10
Number of individuals	*5828*		*3325*	

Notes: Figures shown are sample means computed in the last survey year for which individuals are observed. The sibling sample is constrained to the observations used in the sibling-fixed effects approach, i.e. two siblings must be observed in the same year. [a]Computed for children with siblings only.

household income is almost inevitably lower for lone mothers than for couples, such that controlling for income may lead to strong multicollinearity which might pick up most variation in non-intactness.

On the other hand, by not controlling for household income, one might risk to overstate the impact of growing up in a non-intact family on civic engagement later in life, since the estimated relationship might partly be driven by changes in family income during childhood. Further, Weiss and Willis (1985) argue that if monetary resources available to parents change upon divorce, omitting family income might lead to an overestimation of the coordination problem.

4.4. Summary Statistics

We present means and standard deviations for the individual sample and the sibling sample in Table 1, measured in the last survey year adult children are observed in the sample. It is evident that a relatively large proportion of young adults are interested in politics, identify with a democratic party, and do volunteer work in clubs or social organizations. Organizational involvement is less common, with only 7 percent of young adults reporting volunteer work in a citizen's group, political party, or local government.

21 percent of our respondents (18 percent of the sibling sample) lived in a non-intact family at some point during childhood; 15 (12) percent experienced the divorce of their parents and 9 (6) percent were born outside marriage. The dummy variables *parents divorced* and *born outside marriage* are not mutually exclusive. As a consequence, 3 (0) percent of respondents were born outside marriage and experienced the subsequent marriage and divorce of their mothers. The average age of adult children is 24 (25) years and both samples are balanced with respect to gender. 46 (45) percent of mothers have completed a lower secondary school, 34 (35) percent have an intermediate secondary school degree, 4 (4) percent have a high school degree and 16 (16) percent have completed a technical college or university. Mothers were on average 27 (26) years old when they gave birth to their child. Maternal employment during childhood averages 4.76 (4.83) years in part-time and 5.90 (5.23) years in full-time work.

5. The Effect of Family Non-Intactness during Childhood on Adult Civic Engagement

We start our empirical analysis by estimating cross-sectional models to understand the overall relationship between family non-intactness during childhood and children's civic engagement later in life. Thereafter, we present sibling difference regressions which rest on weaker identifying assumptions for estimating the effect of family non-intactness on adult civic engagement. These models are meant to eliminate family-specific characteristics (e.g., parenting style, parents' political and social values, neighborhood environment) that are assumed to be the same across siblings and to be constant over time. In a third step, we implement a novel procedure to evaluate robustness of results to omitted variable bias. Finally, we provide additional robustness checks and examine effect heterogeneity by gender, residential area during childhood, and mother's education.

5.1. Cross-sectional analysis: selection on observables

Empirical model. We start our empirical investigation by estimating pooled cross-sectional regressions of the form

$$Y_{ijt} = \alpha + \beta_{CS} NIT_{ij} + \gamma x_{ijt} + e_{ijt}, \tag{1}$$

where Y_{ijt} is one of our five dependent variables described above for adult child i from mother j at time t. For the binary outcome variables, we estimate non-linear probit models. For the continuous index variable, we estimate ordinary least-squares regressions. The key

Table 2: Childhood family structure and civic engagement
(Cross-sectional estimates)

Dependent variable:	(1) Index of civic engagement	(2) Political interest	(3) Party identification	(4) Organizational involvement	(5) Individual voluntarism
Panel A					
Ever lived in a non-intact family	-0.104**	-0.013	-0.041**	-0.027**	-0.088**
	(0.019)	(0.013)	(0.014)	(0.005)	(0.013)
Panel B					
Parents divorced	-0.117**	-0.019	-0.031+	-0.030**	-0.102**
	(0.020)	(0.015)	(0.016)	(0.005)	(0.013)
Born outside marriage	-0.062*	-0.005	-0.049*	-0.012	-0.041*
	(0.030)	(0.019)	(0.021)	(0.009)	(0.016)
Equality of coefficients (p-value)[1)]	*0.14*	*0.56*	*0.51*	*0.07*	*0.01*
Person-year observations	*18503*	*42913*	*40947*	*19738*	*19754*

Notes: Each column in each panel reports the results of a regression for the outcome listed in that column. Figures are estimated coefficients from OLS regressions [(1)] or marginal effects from probit models [(2),(3),(4),(5)]. Probit effects are evaluated at the mean of all covariates. Standard errors are clustered on individuals' identification numbers, as there are multiple observations per person over time. Other explanatory variables are age dummy variables, sex, mother's highest educational attainment, mother's age at the child's birth, whether the respondent is an only child, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, regional dummy variables, survey years dummy variables, a dummy variable for East Germany, indicators of SOEP-samples, and a constant. The information about *political interest* and *party identification* is derived from questions asked in the survey years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey years 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.
[1)]Test for equality of coefficients tests whether the coefficients for *parents divorced* and *born outside marriage* from the same regression can be distinguished statistically in a Chow test. We report p-values for the null hypothesis of equal coefficients.

variable NIT_{ij} denotes the various childhood family structure measures for adult child i from mother j. The vector x_{ijt} includes all other control variables. Consistent and unbiased estimation of our key coefficient β_{CS} requires that all explanatory variables are uncorrelated with the error term e_{ijt}, obviously a very strong assumption which is unlikely to hold in the present context. Throughout the analysis, we compute standard errors that are robust to arbitrary forms of heteroscedasticity, as there are multiple observations of civic outcomes per individual over time. In the robustness section below, we also present estimates when we use one observation per individual only, by measuring individuals' civic engagement in the last year they are observed in the panel.

Baseline results. Table 2 reports the results from the pooled cross-sectional regressions. For convenience, we only report the estimates of our key explanatory variables (for the full set of results see Table **??** in the Appendix). Panel A reports the estimates for the dichotomous explanatory variable whether adult children ever lived in a non-intact family during childhood. Panel B reports the estimated effects for the explanatory variables *parents divorced* and *born outside marriage*. The results in Table 2, Panel A, point to a negative and statistically significant relationship between growing up in a non-intact family and the majority of civic engagement outcomes. The point estimate in Panel A, column 1 suggests that young adults who have lived in a non-intact family show a 10.4 percent of a standard deviation lower civic engagement (significant at the 1 percent level). This is a sizable effect, as it implies a 10.4 percent of a standard deviation decline in each of the index components, on average. Moreover, respondents who have ever lived in a non-intact family during childhood are 4 percentage points less likely to identify with a democratic party; 3 percentage points less likely to participate in a citizen's group, political parties or the local government; and 9 percentage points less likely to engage in volunteer work. These marginal effects are all precisely estimated and are statistically significant at the 1 percent level. Moreover, these are sizable effects given that 30 percent of young adults report attachment to a democratic party, 33 percent volunteer in clubs or social services, and 7 percent are active in citizen's groups or political parties. The only point estimate that is not precisely estimated in Panel A is the one for the outcome political interest.

Disentangling the two sources of non-intactness in Panel B reveals that a considerable proportion of the negative associations between family non-intactness and the majority of outcomes are driven by children of divorced parents. The civic engagement index is negatively associated with both growing up with divorced parents and being born outside marriage. We see a decline of 11.7 percent of a standard deviation for adults with divorced parents (1 percent significance) and a decline of 6.2 percent of a standard deviation for respondents born outside marriage (5 percent significance). However, the estimates cannot be distinguished statistically from one another, as indicated by the p-value of a Chow test for equality of coefficients at the bottom of the table. The marginal effects in columns 2-5 all point to a negative relationship between having experienced parental divorce or being born outside marriage and the civic engagement measures. Note, however, that only half of them are statistically significant. The results of the Chow tests for equality of coefficients suggest that the estimated coefficients for the two different non-intactness measures are only statistically different from each other for organizational involvement (p-value: 0.07) and individual voluntarism (p-value: 0.01). The test statistics reveal that the relationship for these two outcome measures with growing up with divorced parents is larger in magnitude (more negative) than with being born outside marriage. In unreported regressions, we clustered standard errors at the family level. We found that all estimates in Table 2 to be robust to family level clustering.

Table **??** in the Appendix presents similar cross-sectional estimates than in Table 2, also controlling for average household income during childhood years. To be more precise, we use household income after taxes and transfers adjusted by weights from the OECD-modified equivalence scale (Hagenaars *et al.*, 1996) to obtain equivalent household income, which is then averaged over all years for which income information is available between the child's ages of 0 and 16. The marginal effects in Table **??** are quite similar in magnitude and statistical significance to those in Table 2. The results indicate that the negative relationship between family non-intactness and civic engagement later in life is unlikely to be mainly driven by household income during formative years.

5.2. Sibling difference analysis: selection on unobservables

Empirical model. Our cross-sectional results cannot be readily interpreted as causal. The major threat to causal identification in our setting is omitted variable bias or selection bias. Reverse causality is arguably not the main concern for identification in the present context, as we measure civic engagement many years after measuring family non-intactness during childhood. Our outcomes are behavioral measures of adults, and these are unlikely to cause family dissolution during childhood. Furthermore, we control for selection on observable characteristics in our pooled cross-sectional estimations to exclude the main sources of selection bias. However, unobservable factors may still confound the estimates. For example, parents who are likely to divorce may have different unobserved preferences, values and abilities than parents who are unlikely to divorce. These unobserved characteristics may in turn affect civic engagement of their offspring later in life, confounding the negative correlation in our cross-sectional estimates. We address this issue with sibling differences estimation (mother fixed-effects) of the form

$$Y_{ijt} = \alpha + \beta_{SD} NIT_{ij} + \gamma x_{ijt} + u_j + e_{ijt}, \tag{2}$$

where u_j, a time-invariant unobserved effect for siblings, is added to equation (1) in order to eliminate time-constant unobserved background characteristics from mother j. Recent studies using a similar estimation method are Ermisch and Francesconi (2012), Siedler (2011), Francesconi *et al.* (2010b), Currie *et al.* (2010) and Anderson *et al.* (2003). Ermisch and Francesconi (2001) provide a detailed discussion of the advantages and disadvantages of the sibling difference approach. In essence, sibling difference estimation eliminates all observed and unobserved time-constant mother-specific factors which are assumed to be the same for siblings and which might be associated with both family non-intactness and children's civic engagement later in life. Hence, the unobserved mother-specific error term u_j cancels out from our sibling difference regressions. We estimate sibling differences in the same observational year t, and use mother level clustering for estimating standard errors to account for correlations across siblings.

The identifying assumption for unbiased sibling difference estimators is uncorrelatedness of sibling differences in family non-intactness with sibling differences in unobserved individual characteristics. This is a weaker identifying assumption than imposed by the cross-sectional estimator which needs family non-intactness and unobserved mother-specific factors to be uncorrelated. Arguably, much of the correlated variation in those measures is absorbed by cancelling out family specific unobserved factors. However, it is important to note that the sibling difference does not eliminate potential bias from individual unobserved heterogeneity between siblings which is correlated with both family non-intactness and civic engagement. Such heterogeneity between siblings may either be caused by unobserved individual characteristics or by time-varying family-level factors. Thus, it is imperative to subject the sibling difference estimates to a thorough test of omitted variable bias.

Other concerns for causal identification in sibling difference estimations are inevitable age and birth order differences between siblings. Among children born outside marriage, the affected child might be systematically older and of lower birth order than the compared and unaffected child. When estimating the effects of growing up with divorced parents, the affected sibling might be systematically younger, and of higher birth order than the unaffected child. Therefore, we add age and birth order fixed effects to the sibling difference estimation that are likely to eliminate confounding variations from these sources.

It is important to keep in mind that sibling difference estimates are identified through adult siblings with differing family non-intactness during childhood years. Hence, the sibling difference estimates may deviate from cross-sectional estimates and require a different interpretation. First, sibling difference estimates are based on a sibling sample, and singletons are excluded from the sample. Second, sibling differences occur only in particular situations. The individual affected by family non-intactness during childhood is compared to a sibling who did not grow up in a non-intact family. As they have the same mother, both siblings experience the event of a non-intact family, but at different times in their lives. For instance, the younger sibling might experience the divorce of his parents during childhood, at a point in time when the older sibling was already an adult and had already moved out of her parents' home. If we assume that non-intactness has a negative effect on civic engagement for both children, even if it occurs when children are adults and no longer live at home, our estimates can be regarded as lower bounds.

Sample description. Identification of sibling difference estimation hinges on within-sibling variation in family non-intactness during childhood and within-sibling variation in civic engagement. Table **??** in the Appendix summarizes the number of sibling pairs per year with differences in both family non-intactness and civic outcome measures. We can draw on over 1,000 sibling differences in family non-intactness when analyzing political interest and party identification. This number drops just below 500 for the outcomes organizational involvement and individual voluntarism. Within-sibling differences in outcomes generally occur more often than differences in family non-intactness. Table **??** also reports the number of observations when splitting the sample by (i) gender; (ii) mother's education and (iii) residential area, as we will study potential heterogenous effects for these groups in section 5.4. Note that, for the majority of subsamples, we still have a comfortable number of differences in family non-intactness, but in some cases the differences used for identification fall below 200. Interpretation of results based on relatively small samples will be made with caution.

Baseline results. Sibling difference estimates of the effect of family non-intactness on civic engagement are presented in Table 3. In the first column of Panel A, effects on the civic engagement index are negative and statistically significant at the 1 percent level. Growing up in a non-intact family reduces civic engagement by 15.7 percent in standard deviations. For the component outcome measures in the sibling difference analysis, family non-intactness during childhood decreases the occurrence of political interest by 9.4 percentage points, decreases identification with a democratic party and individual voluntarism by around 8 percentage points, respectively. These marginal effects are precisely estimated and statistically significant at the 5 percent level. We also find a negative effect in the magnitude of 2.8 percentage points on organizational involvement, but we cannot statistically distinguish it from zero. In sum, the results from our sibling difference estimates point to a negative effect of growing up in a non-intact family on children's civic outcomes later in life. Table **??** in the Appendix reports pooled cross-sectional estimates based on our sibling sample (e.g. excluding only

Table 3: Family non-intactness and civic engagement
(Sibling difference estimates)

Dependent variable:	(1) Index of civic engagement	(2) Political interest	(3) Party identification	(4) Organizational involvement	(5) Individual voluntarism
Panel A					
Ever lived in a non-intact family	-0.157**	-0.094**	-0.077*	-0.028	-0.081*
	(0.052)	(0.036)	(0.036)	(0.018)	(0.040)
Panel B					
Parents divorced	-0.171*	-0.048	-0.069	-0.040+	-0.101+
	(0.082)	(0.051)	(0.052)	(0.022)	(0.058)
Born outside marriage	-0.133*	-0.126**	-0.066	-0.018	-0.037
	(0.056)	(0.043)	(0.042)	(0.021)	(0.047)
Equality of coefficients (p-value)[1)]	*0.70*	*0.25*	*0.96*	*0.48*	*0.40*
Person-year observations	*8892*	*20613*	*19679*	*9445*	*9448*
Number of sibling-year pairs	*4423*	*9751*	*9663*	*4478*	*4479*
Birth order FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

Notes: Each column in each panel reports the results of a regression for the outcome listed in that column. Results are sibling-difference estimates at same survey time. Estimates from linear fixed-effects models. Standard errors are clustered on mother's identification number, as there are multiple observations for sibling pairs. Other explanatory variables are age dummy variables, sex, mother's age at the child's birth, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, and a constant. The information about *political interest* and *party identification* is derived from questions asked in the survey years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey years 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.
[1)]Test for equality of coefficients tests whether the coefficients for *parents divorced* and *born outside marriage* from the same regression can be distinguished statistically in a Chow test. We report p-values for the null hypothesis of equal coefficients.

children and children whose siblings did not participate in the survey). One can see that the cross-sectional point estimates are relatively similar in magnitude to the corresponding sibling difference estimates. We also conducted local Hausman tests for models controlling (i) only for the key explanatory variable "ever lived in a non-intact family, and (ii) for the full models as in Table 3. The null hypothesis that the FE and RE estimator are the same was rejected in three out of twenty Hausman tests at the 5 percent significant level. Since the former estimation method is based on less restrictive assumptions, we decided to report the FE results, even though the RE estimates are more efficient.

Turning to the separate effects of the two sources of family non-intactness in Table 3, *divorced parents* and *born outside marriage*, reveals a similar picture. The effects of growing up with divorced parents on the civic engagement index are minus 17.1 percent in standard deviations (5 percent significance) and minus 13.3 percent in standard deviations for children born outside marriage (5 percent significance). However, note that these two point estimates are not different from each other at conventional significance levels. The estimated effects on the component outcome measures are all negative, but only three point estimates are precisely estimated. In the robustness section below, we present additional evidence from an alternative estimation strategy to examine whether the sibling difference estimates are likely to be biased by unobserved individual heterogeneity.

5.3. Bounding values and omitted variable bias

The cross-sectional and sibling difference estimates in the previous sections consistently point to negative effects of growing up in a non-intact family on adult civic engagement. We argue that the sibling difference estimations rest on less restrictive assumptions compared to the cross-sectional estimations, as unobservable time-invariant mother- and neighborhood-specific characteristics are controlled for. Yet, potential bias from unobserved individual heterogeneity between siblings could still be a threat to this estimation method. This could originate from two sources: (i) unobserved individual factors—e.g., certain personality traits—which lead to family breakup and lower civic engagement, and (ii) time-varying family-level factors (e.g., health shocks) or environmental factors (e.g., macroeconomic shocks) that have an effect on family non-intactness as well as affecting children's political socialization differentially by hitting them at different points in their lives.

In this section, we therefore investigate the robustness of our results to omitted variable bias. In so doing, we follow Oster (2014) who, building on Altonji *et al.* (2005), recently developed a novel method for assessing bias from unobservable factors. The seminal work by Altonji *et al.* (2005) assesses omitted variable bias in the analysis of the effect of Catholic school attendance on educational outcomes. They propose an estimate of a lower bound and an estimate of the degree of selection on unobservables that would confound the effect. The basic idea of observable covariates being a random subset of all relevant covariates yields the central assumption that selection on observable covariates is the same as selection on unobservable covariates. Thus, the coefficient movement caused by the introduction of observable covariates can be used to find a lower bound estimate. Furthermore, there exists a degree of selection on unobservables that fully confounds the estimate, which is used by Altonji *et al.* (2005) to argue whether a causal effect is likely or not. The novelty of Oster's (2014) approach lies in implementing a *full adjustment* after including additional controls, i.e., it exploits information on both coefficient movements and movements in R-squared values in order to compute bounding values for the treatment effect. This method is new to the sibling fixed-effects literature and to the literature studying intergenerational effects.

We start by summarizing the key insights of Oster's (2014) approach. Suppose that observed control variables are captured by the index W_1 which is a linear combination of observed control variables multiplied by their coefficients. Unobservable confounding factors are summarized by the index W_2, which is correlated with civic engagement Y and the treatment (family non-intactness) NIT. Some of the components in W_2 might be orthogonal to the treatment NIT, for example, because of measurement error in the outcome variable. The baseline model is (for notational convenience, we suppress indices):

$$Y = \beta NIT + W_1 + W_2. \tag{3}$$

W_1 (W_2) are linear combinations of observed (unobserved) control variables multiplied with their coefficients (i.e., $W_1 = \sum_{j=1}^{J_o} w_j^o \gamma_j^o$, and $W_2 = \sum_{j=1}^{J_u} w_j^u \gamma_j^u$, with the subscript o (u) indicating observed (unobserved)). Further, it is assumed that W_1 and W_2 are orthogonal such that $Cov(W_1, W_2) = 0$ and $Var(NIT) = 1$. One way to think about the orthogonality assumption is that W_2 contains variables after they are residualized with respect to W_1.

As β is not identified in case of omitted variables, Oster (2014) suggests to report an identified set of parameters on the treatment effect. The identified set depends on estimated parameters ($\tilde{\beta}, \dot{\beta}, \tilde{R}, \dot{R}$) and chosen values for δ, the coefficient of proportionality, and R_{max}, the unknown overall R-squared of a model which controls for observables, unobservables and the treatment variable. The parameter δ captures how strongly unobservables are correlated with the key explanatory variable relative to observables. Estimates of the parameter β and R-squared values result from (1) regressing Y on NIT, and (2) regressing Y on NIT and W_1. $\dot{\beta}$ is the point estimate from the first OLS regression without additional explanatory variables (i.e., only controlling for a measure of non-intactness), and $\tilde{\beta}$ comes from a regression with control variables. $\dot{R}$ and $\tilde{R}$ denote the R-squared from the estimated regressions, respectively.

This yields the proportional selection assumption such that

$$\frac{Cov(NIT, W_2)}{Var(W_2)} = \delta \frac{Cov(NIT, W_1)}{Var(W_1)}. \tag{4}$$

A degree of proportionality with $\delta = 1$ would imply equal importance of observed and unobserved factors, and $\delta > 1$ ($\delta < 1$) implies a larger (smaller) impact of unobservables than observables on the outcome variable. Oster (2014) assumes that $\delta > 0$.

The index W_2 contains all residual variation in the outcome civic engagement that cannot be explained by NIT and W_1, and part of this variation might be idiosyncratic. Oster (2014) therefore defines $W_2 = \tilde{W}_2 + \epsilon$, with $Cov(NIT, \epsilon) = 0$, $Cov(W_1, \epsilon) = 0$ and $Cov(\tilde{W}_2, \epsilon) = 0$. The full model has the form:

$$Y = \beta NIT + W_1 + \tilde{W}_2 + \epsilon. \tag{5}$$

This allows separating the coefficient of proportionality δ into two components, $\tilde{\delta}$ and R_{max}. First, $\tilde{\delta}$ is defined as the proportionality value relating W_1 and $\tilde{W}_2$ such that:

$$\frac{Cov(NIT, \tilde{W}_2)}{Var(\tilde{W}_2)} = \tilde{\delta} \frac{Cov(NIT, W_1)}{Var(W_1)}. \tag{6}$$

The coefficient of proportionality $\tilde{\delta}$ measures how much of NIT is explained by observables versus unobservables, but only those unobserved variables that are correlated with NIT and are proxied by W_1.

Second, Oster (2014) defines R_{max}, the overall R-squared of the model. This measure indicates how much of the variation in the outcome variable can be explained by controlling for observables (NIT, W_1) and unobservables ($\tilde{W}_2$). Note that, because R_{max} depends on unobservables, it cannot be estimated. However, $R_{max} < 1$ if $\epsilon \neq 0$, and the difference between $1 - R_{max}$ therefore gives an indication about the idiosyncratic variation in the outcome variable.

Oster (2014) then defines $\beta^{\star\prime}$, a bias-adjusted coefficient equal to

$$\beta^{\star\prime} = \tilde{\beta} - \tilde{\delta} \frac{(\dot{\beta} - \tilde{\beta})(R_{max} - \tilde{R})}{(\tilde{R} - \dot{R})}, \tag{7}$$

if $\tilde{\delta} = 1$. For the estimation of $\beta^{\star\prime}$ with $\tilde{\delta} \neq 1$, see Oster (2014) for details. To identify $\beta^{\star\prime}$, one needs assumptions for $\tilde{\delta}$ and R_{max}. Oster (2014) argues that $\tilde{\delta} \in [0, 1]$ is a useful bound, because observed control variables are deliberately chosen as determinants of the outcome. Hence, it is unlikely that unobservables have a stronger impact on the outcome variable than the control variables, which would be the case by assuming a value for $\tilde{\delta}$ greater than one. In the main specification, we use a conservative upper bound value of unity for $\tilde{\delta}$, assuming that unobserved components are as important as observed control variables. In order to test for robustness with respect to different proportionality assumptions, we also report the bias-adjusted coefficient for $\tilde{\delta} = 2$, assuming that the impact of unobserved variables on the outcome is twice as large as those of observed variables.

Similarly, R_{max} is not identified, but there are useful bounds to it. It is plausible to assume that $R_{max} < 1$, as some idiosyncratic component in the variation of Y is likely, which cannot be explained entirely by the (observed and unobserved) explanatory variables. She argues that a useful bound is given by $R_{max} = min\{2.2\tilde{R}, 1\}$.

Consequently, we report the identified set for the treatment effect of growing up in a non-intact family $[\tilde{\beta}, \beta^{\star\prime}(min\{2.2\tilde{R}, 1\}, 1)]$. If this set excludes zero, the results from the controlled regressions can be considered robust to omitted variable bias. This implies that the bias-adjusted coefficient $\beta^{\star\prime}$ with the chosen upper bounds on $\tilde{\delta}$ and R_{max} does not change sign considerably relative to $\tilde{\beta}$.

Oster (2014) also suggests evaluating whether the bounds of the identified set are within the confidence interval of $\tilde{\beta}$. This is particularly informative if the estimated coefficient does not move towards zero when controlling for additional explanatory variables. It also tells us whether the magnitude conclusions are robust to omitted variable bias. Finally, in the spirit of Altonji *et al.* (2005), we calculate the ratio of the impact of unobserved variables relative to the observed explanatory variables that would be needed to fully explain away our treatment effect of growing up in a non-intact family on the civic outcome measures (we denote this ratio as δ^0). Altonji *et al.* (2005) study the impact of attending a Catholic high school on educational attainment and test scores in the United States. For the outcome variable high school graduation, for example, they estimate a ratio (δ^0) of 3.55 that would be required in order to attribute their treatment effect of Catholic high school attendance entirely to the influence of unobservables. The authors argue that this is very unlikely.

The results from the Oster (2014) bounding method and the Altonji *et al.* (2005) approach are shown in Table 4. The estimates from the bounding method are computed using the Stata module *psacalc* provided by Oster (2014). The upper panel shows the results for the index of civic engagement, followed by the outcomes political interest, party identification, organizational involvement, and individual voluntarism. For all outcome measures, we report three different treatment effects (e.g., ever lived in a non-intact family, parents divorced, born outside marriage). Column (1) shows the estimated treatment effects for the baseline model (together with standard errors (in parentheses) and the R-squared $\dot{R}$ (in brackets)). Column (2) presents the point estimates for the model with all explanatory variables. We include all control variables and sibling-year fixed effects. Control variables are a maximum set of age dummy variables, sex, mother's age at the child's birth, birth order dummy variables, the number of years of maternal part-time and full-time employment

Table 4: Robustness to omitted variable bias

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Depvar / **Indepvar**	**Baseline Effect** $\dot{\beta}$, **(S.E.)**, $[\dot{R}]$	**Controlled Effect** $\tilde{\beta}$, **(S.E.)**, $[\tilde{R}]$	**Identified Set** $[\tilde{\beta}, \beta^{\star\prime}(min\{2.2\tilde{R}, 1\}, 1)]$	**Exclude Zero?**	**Within Conf. Interval?**	**Bias-adjusted** $\beta^{\star\prime}$ **with** $\tilde{\delta} = 2$	δ^0 **for** $\beta = 0$
Index							
Non-intact	-0.201**	-0.157**	**[-0.157,-0.136]**	Yes	Yes	-0.115	6.517
	(0.032)[0.01338]	(0.052)[0.68666]					
Divorced	-0.194**	-0.171*	**[-0.171,-0.160]**	Yes	Yes	-0.149	12.849
	(0.037)[0.00911]	(0.082)[0.68673]					
Outside mar	-0.187**	-0.133*	**[-0.133,-0.109]**	Yes	Yes	-0.084	4.903
	(0.046)[0.00461]	(0.056)[0.68673]					
Pol. Interest							
Non-intact	-0.057**	-0.094**	**[-0.094,-0.120]**	Yes	Yes	-0.147	-
	(0.020)[0.00225]	(0.036)[0.59167]					
Divorced	-0.053*	-0.048	**[-0.048,-0.046]**	Yes	Yes	-0.043	16.164
	(0.025)[0.00141]	(0.051)[0.59204]					
Outside mar	-0.062*	-0.126**	**[-0.126,-0.170]**	Yes	Yes	-0.219	-
	(0.027)[0.00109]	(0.043)[0.59204]					
Party ident.							
Non-intact	-0.083**	-0.077*	**[-0.077,-0.075]**	Yes	Yes	-0.072	25.329
	(0.026)[0.00397]	(0.036)[0.66642]					
Divorced	-0.057+	-0.069	**[-0.069,-0.076]**	Yes	Yes	-0.082	-
	(0.030)[0.00134]	(0.052)[0.66636]					
Outside mar	-0.118**	-0.066	**[-0.066,-0.040]**	Yes	Yes	-0.013	2.468
	(0.033)[0.00327]	(0.042)[0.66636]					
Organiz. Involv.							
Non-intact	-0.039**	-0.028	**[-0.028,-0.019]**	Yes	Yes	-0.010	3.083
	(0.028)[0.00278]	(0.018)[0.55842]					
Divorced	-0.046**	-0.040+	**[-0.040,-0.034]**	Yes	Yes	-0.029	7.302
	(0.008)[0.00290]	(0.022)[0.55847]					
Outside mar	-0.018	-0.018	**[-0.018,-0.018]**	Yes	Yes	-690.435	-
	(0.029)[0.00023]	(0.021)[0.55847]					
Individ. Volunt.							
Non-intact	-0.134**	-0.081*	**[-0.081,-0.047]**	Yes	Yes	-0.011	2.273
	(0.020)[0.01066]	(0.040)[0.61230]					
Divorced	-0.141**	-0.101+	**[-0.101,-0.075]**	Yes	Yes	-0.049	3.613
	(0.022)[0.00863]	(0.058)[0.61220]					
Outside mar	-0.097**	-0.037	**[-0.037,0.002]**	No	Yes	0.043	0.954
	(0.030)[0.00220]	(0.047)[0.61220]					

Notes: See text for discussion of table. Results of the uncontrolled model are from OLS regressions. Results of the controlled model are from the sibling-difference regressions. R^2 values for column 2 are obtained from OLS regressions using sibling-time-fixed-effects. Results in columns 6 and 7 are computed using Stata code psacalc provided by Oster (2014). Robust standard errors are clustered on mothers. Control variables are age dummy variables, sex, mother's age at the child's birth, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, and a constant. The information about *political interest* and *party identification* is derived from questions asked in the survey years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey years 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.

during the respondent's childhood, and a constant. Column (3) reports the identified set $[\tilde{\beta}, \beta^{\star\prime}(min\{2.2\tilde{R}, 1\}, 1)]$, column (4) shows whether the identified set excludes zero, and column (5) reports whether the estimated biased-adjusted coefficient is within the confidence interval of the estimated controlled effect.

The estimated baseline treatment effect of growing up in a non-intact family on the civic engagement index $\dot{\beta}$ in the first row in Table 4 is -0.201, with an R-squared value ($\dot{R}$) of 0.013. The corresponding estimate in the controlled model $\tilde{\beta}$ is -0.157, with an R-squared ($\tilde{R}$) of 0.687. These findings point to a relative small movement in coefficients along with a large movement in the R-squared values. Importantly, the identified set for the treatment of growing up in a non-intact family on civic engagement does not include zero [-0.157,-0.136]. The bias-adjusted coefficient (-0.136) is only slightly smaller in magnitude than the controlled effect (-0.157). According to Oster (2014), this estimate can be considered as robust against omitted variable bias. To further test robustness, we increase the proportional selection assumption to $\tilde{\delta} = 2$ and report the corresponding treatment effect in column (6). The result is quite stable with an estimate of -0.115. Hence, even if the impact of unobservables on the outcome is twice as large as the influence of the observables, the identified set would still not include zero. In fact, the hypothetical δ^0 suggests a treatment effect of $\beta = 0$ only if omitted variables are six times as important for the outcome than the included control variables.

The bias-adjusted coefficient of the treatment effect of experiencing parental divorce during childhood on the civic engagement index is -0.160, compared to -0.171 from the controlled regression. Thus, the identified set excludes zero, which is indicative of robustness against omitted variable bias. Increasing $\tilde{\delta}$ to two has only little effect on the bias-adjusted coefficient which turns out to be -0.149. The corresponding δ^0 is larger than 12, indicating that it is very unlikely that unobservables explain the whole treatment effect. Being born outside marriage yields an identified set of [-0.133,-0.109] which also excludes zero, and the bias-adjusted coefficient with $\tilde{\delta} = 2$ is far away from zero (-0.084). With an estimated ratio (δ^0) of 4.9 the findings can also be considered as robust against omitted variable bias.

When looking at the results for the other outcome measures in Table 4, one can see that virtually all identified sets exclude zero. The only exception is the identified set for the effect of being born outside marriage on the outcome individual voluntarism at the bottom of the table. In four of the estimations the treatment effects move away from zero rather than towards zero when including control variables. This is indicative of robust treatment effects as similarly correlated unobserved factors do not imply confounding variation. In these cases, we do not report the negative multipliers in column (7). Negative multipliers imply that the unobservables would have to be δ^0 times stronger correlated than observables but with reversed sign. Further, the identified sets from all regressions are within the confidence intervals of the controlled effect and omitted variables are therefore unlikely to drive the results. It is also important to point out that the calculated $R_{max} = min\{2.2\tilde{R}, 1\}$ is always one. This is because $\tilde{R}$ is relatively large, varying between 0.56 and 0.69. These large R^2 values are not very surprising since we control for family (mother) fixed effects, and therefore many unobservable factors at the family level are controlled for. Using a $R_{max} = 1$ is a strict criteria and a lower value might be even more appropriate.

In summary, the findings in Table 4 suggest that it is very unlikely that the sibling difference estimations are severely biased by unobserved time-varying confounders. Because the overwhelming majority of identified sets exclude zero, the treatment effects can be interpreted as being as robust as if treatment were randomized.

5.4. Sensitivity Analysis

As discussed earlier, there are plausible reasons to also control for household income. Table **??** in the Appendix presents sibling difference estimates, conditional on household income during childhood years. Consistent with the estimates in Table **??**, we find that growing up in a non-intact family leads to a decrease in civic engagement as adults. The point estimates in Panel A, Table **??** are precisely estimated for the index of civic engagement, political interest, party identification and individual voluntarism. Overall, the sibling difference estimates are robust to controlling for average household income during childhood years. The results in Table **??** therefore provide further support to the validity of our sibling differences estimates.

In order to assess our measure of family non-intactness we compare the results with estimations using a different specification of the explanatory variable. A smaller sample of new respondents from 2000 to 2009 is asked how many years they have lived with both their biological parents during ages 0 through 15. We construct a measure of non-intactness during childhood by setting it equal to zero if respondents lived with both parents until the age of 15, and one if they lived less than 15 years with both parents. Results from sibling difference in Table **??** show comparable results for the index, political interest and organizational involvement in terms of sign and magnitude. However, standard errors increase and only one of the estimates is statistically significant. Estimates for party identification and individual voluntarism are virtually zero. Note that the sample we can use is considerably smaller than in the previous sections. The similarity in results points towards the conclusion that our measure of family non-intactness is primarily mirroring the situation of not growing up with both parents.

Robustness—Age difference. Comparison of siblings who both experienced a very similar family background during childhood may raise some concerns. On the one hand, it is a prerequisite for our identification strategy to have siblings with very similar background characteristics. This is essential to be able to exclude family omitted variable bias in the sibling difference regressions. On the other hand, if siblings are born within a very small time span, small differences in age between the siblings will likely result in an attenuation bias, because both siblings will be affected almost identically. This might result in underestimating the negative effect of family non-intactness on adult children's civic outcomes. As additional evidence, we estimate the intergenerational effects for a smaller sample of siblings who are born more than two years apart. In fact, the estimated coefficients on the variable family non-intactness from this new sample are more negative and more precisely estimated than the corresponding sibling difference estimates in Table 3. As depicted in Table 5, growing up in a non-intact family leads to a decline in civic engagement of 20.0 percent in standard deviations (1 percent significance) compared to 15.7 percent in the baseline sibling

Table 5: Robustness—Siblings more than two years apart (Sibling difference estimates)

Dependent variable:	(1) Index of civic engagement	(2) Political interest	(3) Party identification	(4) Organizational involvement	(5) Individual voluntarism
Panel A					
Ever lived in a non-intact family	-0.200**	-0.092*	-0.131**	-0.033+	-0.102*
	(0.060)	(0.042)	(0.034)	(0.020)	(0.044)
Panel B					
Parents divorced	-0.236**	-0.054	-0.123*	-0.058*	-0.140*
	(0.086)	(0.060)	(0.053)	(0.026)	(0.057)
Born outside marriage	-0.153*	-0.138**	-0.113**	-0.010	-0.028
	(0.070)	(0.049)	(0.037)	(0.023)	(0.057)
Equality of coefficients (p-value)[1)]	*0.45*	*0.29*	*0.88*	*0.18*	*0.17*
Person-year observations	*5708*	*13233*	*12676*	*6039*	*6042*
Number of sibling-year pairs	*2702*	*5984*	*5941*	*2729*	*2729*
Birth order FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

Notes: Each column in each panel reports the results of a regression for the outcome listed in that column. Results are sibling difference estimates at same survey time. Estimates from linear fixed-effects models. Standard errors are clustered on mother's identification number, as there are multiple observations for sibling pairs. Other explanatory variables are age dummy variables, sex, mother's age at the child's birth, birth order dummy variables, the number of years of maternal part-time and full-time employment during the respondent's childhood, and a constant. The information about *political interest* and *party identification* is derived from questions asked in the survey years 1985-2009 and 1984-2009, respectively. For *organizational involvement* and *individual voluntarism*, we use information derived from questions asked in the survey years 1985, 1986, 1988, 1992, 1994, 1996, 1997, 1999, 2001, 2005, 2007 and 2009. + significant at 10%; * significant at 5%; ** significant at 1% level.
[1)]Test for equality of coefficients tests whether the coefficients for *parents divorced* and *born outside marriage* from the same regression can be distinguished statistically in a Chow test. We report p-values for the null hypothesis of equal coefficients.

difference estimation. Furthermore, the estimates indicate that growing up in a non-intact family decreases the likelihood to feel close to a democratic party by 13 percentage points (1 percent significance), and the likelihood to engage in volunteer work by 10.2 percentage points (5 percent significance). Also, estimates for having experienced a divorce during childhood are more precisely estimated. For all outcome variables, with the exception of political interest, we find negative effects that are statistically significant at least at the 5 percent level. These results support our main finding that growing up in a non-intact family seems to have negative and long-lasting influences on children's civic engagement.

Robustness–Long-Term Effects. Throughout the study, we present cross-sectional and sibling difference estimates using multiple observations per individual. Since the political outcomes are elicited in the SOEP in most panel years, this 'pooling' approach leads to relatively large sample sizes and precise estimates in the majority of regressions. A potential problem might be that individuals with a lower risk of dropping out of the panel get a larger weight in the regressions, as we observe their civic engagement over more years compared to those with higher attrition rates. If the likelihood to drop out is related to respondents' childhood experiences and their civic behavior, or varies across siblings due to unobserved time-varying factors, the estimates might suffer from attrition bias. To probe the robustness of our results, Tables **??** and **??** in the Appendix present cross-sectional and sibling difference estimates of childhood family structure and civic engagement using one observation per individual only. Since we are interested in the long-term effects, we measure adult children's political behavior in the last year they are observed in the panel. The estimates in both tables are consistent with the results in Tables 2 and 3. Note, however, that some of the marginal effects are slightly lower in magnitude (less negative) and less precisely estimated.

5.5. Effect heterogeneity

We have seen substantial effects of family non-intactness on civic engagement later in life. These effects may not be universally relevant for different groups of the population, and effect heterogeneity may arise for a variety of reasons. For instance, when thinking about the effect of family non-intactness during childhood on civic engagement, we have in mind certain modes of nurturing that might have adverse consequences for children's development. In the majority of families, non-intactness implies an absent father, and daughters and sons might therefore be affected differently by growing up in a non-intact family. Note that we cannot actually distinguish effects of missing fathers and missing mothers as there are too few observed lone fathers in the data set. We did not find clear heterogeneous effects of non-intactness by gender. However, in the sibling difference estimations, we found negative and statistically significant effects for men only, and the magnitude of the effects of growing up in a non-intact family was larger for men than for women in most regressions.

Nurturing capacity or the effectiveness of parenting may as well produce heterogeneous effects of family non-intactness. In the case of missing fathers, mothers with higher capacity should be more able to compensate for the potential loss of paternal involvement in their children's development. On the other hand, highly educated women are likely to be married to highly educated men, and the absence of highly educated fathers might be more harmful for children. We therefore distinguish between adult children with highly educated mothers (completed upper secondary school or have an university degree), and those with lower educated mothers. Overall, the results did not point to substantial differences in the effects of family non-intactness by mother's education.

Putnam (2000) notes that social connectedness among inhabitants of densely populated urban areas is weaker than in smaller communities. Indeed, considerable differences in the levels of civic engagement and organizational involvement exist between individuals who grew up in rural or urban areas (e.g., 26 percent of respondents who lived in a city report being interested in politics, compared to 18 percent who come from a rural area), and these differences may also produce heterogeneous effects of family non-intactness. However, distinguishing between individuals who grew up in urban and rural areas did not point to heterogeneous effects. We also examined another potential source of heterogeneity: the age of the child at the time of parents' divorce. Again, our results did not point to important heterogeneous effects.

In sum, the effect of family non-intactness on civic engagement later in life is not restricted to either males or females, children of low or highly educated mothers, or rural or urban residential areas. We find some indication of more severe effects for boys and slightly weaker effects for girls. However, due to the low precision of some estimates, there remains some uncertainty about the nature of heterogeneity. The overall phenomenon of adverse effects of non-intact families during childhood on civic engagement seems to be strikingly universal. The results of the heterogeneity analyses can be found in Appendix A in Tables **??**, **??** and **??**.

6. Final remarks

Well functioning democracies depend on active and informed citizens. Understanding the forces that shape civic engagement is therefore a significant challenge to the social sciences. This paper has focused on one very specific aspect of this challenge, examining empirically the effects of childhood family structure on adult civic engagement. The line of thinking behind our analysis comes from sociologists arguing that family non-intactness during childhood disrupts the production process through which social capital and civic engagement is created within the family. The empirical results from our cross-sectional and sibling difference estimates do lend support to this idea. In particular, we find a significant negative relationship between growing up in a non-intact family and various measures of civic engagement, from individual voluntarism to organizational involvement to partisan commitment. The core methodological contribution of this paper is to evaluate the robustness of results to omitted variable bias by accounting for both coefficient movements *and* movements in R-squared values after inclusion of controls. To the best of our knowledge, this full adjustment—based on recent work by Oster (2014)—has previously not been implemented in the literature on the effects of childhood family structure on children's later-life socio-economic outcomes. The results from this exercise suggest that our results are not severely biased by unobserved confounders.

As we have emphasized throughout, our results complement others in the literature, in particular those which have singled out family structure as of special importance for the intellectual development of children, and have conjectured that family non-intactness is a phenomenon which has broad implications for social capital and civic engagement (see, for example, Coleman's (1988) extensive discussion of social capital in the family). Despite this, there have been few previous attempts and little evidence to show that family non-intactness during childhood actually causes adult civic disengagement. From a practical standpoint, our findings suggest that schools or community organizations, which reach children across socioeconomic strata, might need to offer more opportunities for civic and political learning to counteract some of the negative effects on civic engagement stemming from the break-up of families.

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