The Long-Run Effects of Unilateral Divorce Laws on Offspring's Smoking

Preliminary Draft

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Abstract

This paper examines the causal effect of parental divorce on smoking behavior by exploiting state-cohort variation in the exposure to unilateral divorce laws by age 25. Using the National Survey of Midlife Development in the United States, a nationally representative survey with rich information on the tobacco use history of fifty cohorts, we assess the long-run impact of divorce laws on affected children. We employ the semi-parametric difference-in-differences identification strategy developed by Callaway and Sant'Anna (2021) that accounts for the variation in treatment timing across the states and, therefore, produces unbiased estimates. The results provide strong empirical support for the adverse effect of divorce reform on affected women and men, indicating that the exposed cohorts have a higher probability of regularly using tobacco in adulthood. The underlying mechanisms differ by gender: Women's greater propensity to smoke is related to not growing up with both parents as a result of the reform, while men's greater propensity is related to growing up poor and changes in parental behavior that are not proxied by education, poverty, or parental affection and discipline.

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1. Introduction

Smoking is costly for the individual and for society. In the US, smokers die on average 10 years earlier than non-smokers (Centers for Disease Control and Prevention (CDC), 2020) and smoking costs an estimated $300 billion in medical care and lost productivity each year (CDC, 2022a). Understanding why people start smoking is crucial for the development of anti-smoking policies. Moreover, a better understanding of the factors that increase the likelihood of risky health behaviors can contribute meaningfully to our understanding of socioeconomic inequalities and the transmission of disadvantage across generations; people of low socioeconomic status are more likely to start smoking and less likely to quit, and are thus disproportionally affected (Balin and Jones, 2008; Cawley and Ruhm, 2012; Siegel, 2014).

Since most smokers start as teenagers (CDC, 2022c) and family environment and socioeconomic status are correlated, the family environment likely affects an individual's decision to start smoking. However, it is also likely that there are factors not observed by researchers that affect both the probability of offspring's smoking and the type of family environment. There may also be reverse causality in the sense that the family environment changes in response to adolescents' behavior (see, e.g., Kabátek and Ribar, 2020). Because of a lack of plausible exogenous variation in the family environment, most existing studies are descriptive or use selective samples, making it difficult to assess whether the results are causal or apply more generally.

We overcome this limitation by exploiting the exogenous variation in the family environment caused by the staggered adoption of (no-fault) unilateral divorce laws by U. S. states. These laws made it easier to divorce, which led to an initial increase in divorce rates (Wolfers, 2006) and, perhaps more importantly, a change in family dynamics (Gruber, 2004; Reinhold et al., 2013; Stevenson, 2007). The effects of the introduction of unilateral divorce laws have been widely studied, though with few exceptions (e.g., Hayduk and Kleinjans, 2023) with standard difference-in-differences methods that do not take into account the staggered timing of the law introduction. Most studies have found negative effects on those who were children when these laws were introduced: they at least initially increased divorce

rates (Wolfers 2006; though see Lee and Solon 2011), lowered marriage-specific investments (Genadek, Stock, and Stoddard, 2007; Stevenson 2007), and reduced education, family income, and the noncognitive skill of conscientiousness of children who grew up while the law was in effect (e.g., Gruber, 2004; Hayduk and Kleinjans,2023). On the other hand, the law introduction also reduced domestic violence (Stevenson and Wolfers 2006), increased family savings, and increased mothers' labor force participation at least in states in which property is not equally divided after a divorce (Angelini et al. 2019, Genadek, Stock, and Stoddard 2007; Stevenson 2007; Voena 2015). While increased female labor force participation may have increased maternal stress and reduced the quantity and quality of the time spent with their children, it could also have had the opposite effect by increasing maternal life satisfaction and family income (Goldin, 2006). A working or more career-oriented mother could have also had positive role model effects for girls, increasing their educational attainment and income in adulthood (Bertrand, 2020).

The change in family environment caused by the introduction of unilateral divorce is also likely to affect risky health behaviors, including smoking, but there is little causal and general evidence for this. To investigate this question, we use the semi-parametric difference-in-differences approach developed by Callaway and Sant'Anna (2021) to assess whether those who grew up at the time of the law adoption are more likely to smoke in adolescence and adulthood (see also Roth and Sant'Anna 2023 and Roth et al. 2023). This new empirical approach uses only those not-yet-treated and those never treated as comparison groups, and as such avoids the recently discovered bias introduced by using the standard difference-in-differences approach in the presence of variation in treatment timing.

Specifically, we investigate whether there is a relationship between the introduction of unilateral divorce laws and the smoking behavior of children in adolescence and adulthood. We use data from the 1995 National Survey of Midlife Development in the U. S. (MIDUS), which allows us to construct the detailed smoking history of respondents and includes information on the family environment in childhood and adolescence. We further explore potential mechanisms by analyzing changes in financial resources and parenting as a result of the changes in divorce laws and their effects on smoking.

We find that both women and men exposed to the divorce laws as children were about 40% more likely to smoke in adolescence and adulthood than those who were not exposed, but that the underlying mechanisms differ by gender, pointing to differential effects of family formation and dissolution and family dynamics for girls and boys. Women are more likely to smoke if they grew up without both parents because of the law change. Men, on the other hand, are more likely to smoke if they did not grow up with both parents only if they also grew up poor (likely as a result of it). Moreover, men who did grow up with both parents are more likely to smoke for reasons that are unrelated to parental education, poverty status in childhood, and parenting.

Our paper contributes to our understanding of the link between risky health behaviors and adolescence and adulthood by providing causal evidence of the effect of the introduction of unilateral divorce laws on those who were affected as children. Moreover, our results shed light on the origins of the intergenerational transmission of inequality because of the causal links between SES, family formation and parenting, and smoking behavior. The SES gradient in smoking, in turn, contributes to the SES gap in mortality and morbidity, further transmitting disadvantage to children who already grow up in more difficult circumstances than children from more affluent families (Case and Deaton, 2020; Marteau et al., 2021). We also add to the literature on the differential effects of parental environment on boys and girls by showing the differential effects of growing up without both parents and poverty, contributing to our understanding of the gender gap in behavioral outcomes (see also Autor et al. 2019).

2. Smoking, family environment, and socioeconomic status

Part of the mortality gradient by SES is related to health behaviors, and especially the greater likelihood of people from lower socioeconomic background to smoke. Since most smokers start smoking in adolescence, we hypothesize that part of this association is the result of a detrimental family environment when growing up, which happens more often for children from disadvantaged backgrounds.

Over 90% of daily smokers in the U.S. smoked their first cigarette by age 18, and 99% by age 26 (CDC 2022c). Since most smoking initiation occurs in adolescence, well-documented

differences in family environment by SES could plausibly explain part of the SES gradient in smoking by SES. Children from disadvantaged background have parents who spend less quality time with them and provide less warmth and consistency (Kalil and Ryan, 2020). At the same time, even though Americans with low levels of education are less likely to marry, they are more likely to divorce (e.g., Aughinbaugh et al. 2013). Children from lower socioeconomic backgrounds are also more likely to grow up with a single parent, resulting in disadvantage compounded by the combined effects of low resource access and detrimental family structure (see Kearney 2023 for an overview).

In 1995, the time when the retrospective data that we use in this study was collected, 25% of adults in the U. S. smoked (CDC, 1997). Estimates suggest that in the year 2000, almost one out of every two deaths in the U.S. was the result of health behaviors, with smoking as the leading cause of death (Mokdad et al., 2004). In the U.S., there is a strong smoking gradient in education and income. Those without a high school degree are almost four times as likely to smoke as those with an undergraduate degree, and those with household incomes below $35,000 are over three times as likely to smoke as those with a household income of $100,000 and higher (CDC, 2022b). Smoking rates are also higher for those with low levels of education above and beyond its effect on income (Cutler and Lleras-Muney, 2010). This gradient starts early - people from low socioeconomic backgrounds are more likely to start smoking when young, a time when people are more likely to become addicted (CDC, 2022c; Lillard, 2020; Siegel, 2014). But they are also less likely to quit, widening the gradient with increasing age (Siegel, 2014).

Smoking and SES are further linked because smoking in adolescence is positively correlated with parental smoking (see, e.g., Göhlmann et al., 2010; Loureiro et al., 2010). Parental smoking, in turn, is higher for low SES individuals. Smoking rates are also higher for those who face financial strain (a common consequence of divorce) and those separated from a partner (Grafova 2011; Hillebrandt 2022) – which again are more likely to be the case for people of low SES. This is reflected even in basic smoking statistics - people who are divorced, separated, or widowed have higher smoking rates (17%) than those who are single/ never married or not living with a partner (13%) and those who are married or living with a partner (11%) (CDC 2022b).

The introduction of unilateral "no-fault" divorce laws can affect smoking of those who were exposed to those laws as children through three channels: Increased parental divorce; lower marriage rates; and changed behavior within marriages.

First, unilateral divorce laws might have increased the probability of parental divorce, which has been linked extensively to negative child outcomes (see, Kearney, 2023 for an overview of the ample literature). Wolfers (2006) provides evidence that divorce rates increased after unilateral divorce laws were introduced, at least in the initial years following the introduction (though see also Lee and Solon, 2011). We also find an increased probability of parental divorce in our data. Children with divorced parents are much more likely to grow up poor (e.g., Ananat and Michaels, 2008). Growing up with a single parent reduces educational attainment, family income in adulthood, and decreases the probability of being married in adulthood (e.g., Lopoo and DeLeire, 2014). But parental divorce not only reduces children's access to resources in terms of finances but also of parental attention. Divorced mothers, but especially divorced fathers, spend less overall time and less quality time with their children (e.g., Amato, 2005; Hamermesh, 2021; McLanahan et al., 2013). Children of divorced parents also receive less socio-emotional support, which may be because of the greater stress experienced by single mothers (Hamermesh, 2021). These differences in parental attention and affection might increase the probability of smoking, for example, because of decreased socialization against smoking, and of increased parental stress and associated parental smoking (see, e.g., Christopoulou et al., 2013; Grafova 2011; Kleinjans and Gill, 2022). Children might also take up risky health behaviors to better cope with the stress, disruption, and uncertainty that they experience as a result of parental divorce (van der Kolk, 2015; Lillard, 2020).

This relationship between divorced parents and risky behavior of adolescents is supported by an extensive literature of descriptive studies (see, e.g., Brown and Rinelli, 2010, and Thomas and Högnäs, 2015). For example, Kirby (2002) finds that adolescents are more likely to start smoking when their parents separate. This effect is likely higher for those from low SES families because of the compounding of disadvantage experienced in childhood, for example, because parents are less likely to be able to make up for the

economic disadvantage caused by divorce and have worse preexisting relationships with their children (see, e.g., Kleinjans and Gill, 2022; Kearney, 2023; Thomas and Högnäs, 2015).

Second, marriage rates might decline in response to the decrease in marriage security caused by unilateral divorce laws. Children might thus be more likely to have parents who are not married. In the U. S., cohabiting parents are much more likely to separate than married ones, which means that their children grow up in less stable family structures even if parents are initially living together. Unstable family environments are associated with worse child outcomes, including increased externalizing behavior (such as fighting and acting impulsively) of boys (Bertrand and Pan, 2013; Brown, 2006; Manning, 2015). Kearney and Levine (2017) find that the parental marriage premium, high-school achievement and avoiding poverty at age 25 have an inverted U shape in mother's education and age, so it might be that those children are more likely to be negatively affected by a decrease in marriage rates. However, it is not clear that this relationship also exists for risk-taking behaviors, and there is some evidence that especially a relationship with a father is even more important for boys who come from disadvantaged backgrounds (Bertrand and Pan, 2013). Cohabitation rates and single motherhood show a strong negative socioeconomic gradient in the U.S. (Kearney, 2023; Kearney and Levine, 2017), so it is likely that disadvantaged children are more likely to be affected, and if they are, the impact is greater.

And, lastly, behavior in a marriage might change upon the introduction of unilateral divorce laws, for example, because of a shift in bargaining power (Stevenson and Wolfers, 2006) or because specialization becomes more costly if marriages can be dissolved more easily. In particular, changes in maternal labor force participation as well as maternal and paternal life satisfaction might affect the quantity and quality of time spent with children, including jointly as a couple. Theoretically, the net effects are ambiguous.

There is ample evidence in support of a compounding effect of childhood disadvantage and family structure on health behaviors, though most of the results are descriptive. An exception is Kleinjans and Gill (2022), who used difference-in-differences approach and found that the effect of the economic and social upheaval experienced by East German families in the aftermath of the fall of the Berlin Wall in 1989 increased smoking of boys

born during this time - but only if they grew up with only one parent for at least some time during their childhood. Fletcher and Sindelar (2012) found that family stressors (which include divorce) are positively correlated with the probability of teenagers to start smoking, using a hazard model with school fixed-effects. Similarly, Slade et al. (2017) find that family structure predicts smoking initiation and quitting, with stronger effect for girls than for boys. Wolfinger (1998) and Martindale and Lacey (2017) find that parental separation predicts tobacco consumption in adulthood but are also not able to causally identify this effect because of a lack of exogenous variation in the empirical setup. Gustavsen et al. (2016) find that parental divorce increases teenagers' probability of smoking, using propensity score matching. This method reduces but does not eliminate the selection bias arising from non-random selection into divorce. Hussey et al. (2016), also using propensity score matching, investigate whether living with one parent instead of with two affects outcomes of affected adolescents in the short, medium, and long run. They find that in the medium and long run, the probability of smoking increases. Antecol and Bedard (2005) find that teenagers who are raised by single parents are more likely to smoke, using the timing of the family breakdown as exogenous variation. While this identification reduces selection bias it does not take into account the potential endogeneity of divorce and the gender of a child (Dahl and Moretti 2008; Kabátek and Ribar, 2020), making a causal interpretation of these earlier findings somewhat less convincing. Reinhold et al. (2013) conduct standard difference-in-differences analysis using European SHARE data, relying on two countries in the sample that changed divorce laws during the sample period and cross-country variation in divorce laws for their identification. They find that individuals from countries that had unilateral divorce laws when they were 18 years old are less likely to have never smoked. Closest to our study is Francesconi et al. (2010) who use the German Socioeconomic Panel (SOEP) to investigate whether lone parenthood affects the probability of smoking before age 21. While they find a strong effect for children of divorced mothers using logit regression and propensity score matching, they find less clear results when using mother fixed-effects, though argue that the latter could be related to small sample sizes.

In contrast to the studies above, we use the exogenous variation introduced by the differential timing of the introduction of unilateral divorce laws in different U.S. states to

causally identify its effect on smoking in adolescence and young adulthood. We further explore potential mechanisms by analyzing the effect of unilateral divorce law introduction on poverty in childhood and parental inputs, providing insights into the mechanisms by which smoking rates are affected (or not). We also consider the heterogeneity of the effect by gender. This is important because the child's gender affects the probability of divorce (Ananat and Michaels, 2008), the quantity, quality, and importance of parental inputs for socio-emotive development and risky health behaviors (Kleinjans and Gill, 2022; McLanahan, 2013), as well as how parental inputs change in response to changed divorce laws (Hayduk and Kleinjans, 2023).

3. Empirical approach

To assess the long-run impact of the introduction of unilateral divorce laws on smoking behavior, we use the staggered difference-in-differences design developed by Sant'Anna and coauthors (Callaway and Sant'Anna 2021; Sant'Anna and Zhao 2020), which compares treated and untreated groups at different time intervals from the time right before treatment, and then aggregates those individual effects for the aggregate treatment effect.[1] We use inverse probability weights. This method uses only observations from not treated or not yet treated states as control group, and thus avoids the bias introduced by also using the "forbidden" comparisons between already treated states as is done in conventional difference-in-difference methodology (Roth et al. 2023).

We exploit cross-state differences in the timing of the law adoption and the differential exposure to them across cohorts by estimating the following model linking the smoking status *Smoking* of individual *i* from the group of states g[2] in year *t* to the individual's state of residence at the adoption of unilateral divorce laws $Smoking_{igt} = \alpha + \mu_t + \pi_g + \beta[Exposed\ to\ law\ by\ age\ 25]_g + \theta X_{ig} + \text{Age}_t + \varepsilon_{igt}$ (1)

where [*Exposed to law by age 25*]$_g$ identifies the treated states and equals the year of the law adoption if a resident of the treated state was less than 26 years old (an age by which

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[1] See also Roth and Sant'Anna 2023. We use the Stata csdid command in our estimation (Rios-Avila et al. 2023).
[2] Each group includes a set of states that adopted a unilateral divorce law in the same year.

virtually all smokers have started smoking, see CDC 2022c) at the time of the law adoption and zero otherwise. The group fixed effects, π_g, control for all time-invariant unobserved determinants of smoking that vary acrossgroups, and the year fixed effects, μ_t, capture the aggregate unobserved influences on outcomes that vary over time. θX_{igt} is a vector of covariate and cohort-specific time trends. Age_t is a vector of predetermined demographic characteristics that consists of age dummies. We construct these dummies by utilizing the information about the respondents' age in each calendar year during the study period, between 1955 and 1990. Since smoking behavior and upbringing have differential effects by gender, we conduct our analyses separately for women and men. To explore potential mechanisms by which the introduction of unilateral divorce laws might have affected smoking behavior, we also consider only those who grew up with both parents until age 16, and consider factors potentially affected by the law (parental education, parenting, and poverty when growing up). We also explore racial differences.

Using exposure to the divorce reform, rather than an indicator for parental divorce, eliminates any potential concerns for the endogeneity bias arising from unobserved differences between children of divorced parents and those from intact families. Our policy variable is plausibly exogenous, uncorrelated with the child and family characteristics, and, therefore, grants the opportunity to obtain an unbiased estimate of β.

There are two potential threats to our identification. First, staggered difference-in-differences assumes that smoking rates in treated and non-treated states would have developed in parallel in the absence of the legislation. While this is a less strong assumption than parallel trends prior to the introduction of unilateral divorce, it is not directly testable. We provide some evidence on parallel pre-trends after the discussion of the results.

Second, we assume that there is no other policy that is correlated with the timing of the introduction of unilateral divorce laws that affected smoking behavior. The most obvious policies here are those that were designed to reduce smoking: Smoking restrictions, minimum legal purchasing ages, and taxes on cigarettes. Some studies have linked smoking bans to a decline in teenage smoking (see Josten and Lordan, 2020 for an overview), but since the first comprehensive statewide ban was not imposed until 1995 (ARB, n.d.) – the

year our data with retrospective questions on smoking behavior was collected - this does not affect our results. There were not many minimum legal purchasing age changes in the 1970s, the most common decade when states legalized unilateral divorce, and if any these were increases in the legal ages (see Appolonio and Glantz, 2016). Lastly, cigarette prices and taxes have been linked to smoking initiation (Lillard et al., 2013). However, most tax changes implemented before the 1990s were small (Friedson et al. 2023). In the 1990s, the majority of our sample was well past the typical time of smoking initiation[3] However, real taxes slightly decreased during the 1970s, which could have affected the smoking behavior of cohorts also affected by the change in divorce law.

Third, we are only able to identify the state of residence in 1995, which may have differed from the state in which the individuals grew up. To assess this, we use information on past moves, and conduct sensitivity analyses in which we restrict the sample to those who either lived in their current state of residence at the time of the reform or who moved to it within three years after the reform. While this reduces our sample size and increases standard errors, the results are qualitatively similar (results not shown).

4. Data

Our main analysis is based on the restricted version of the National Survey of Midlife Development in the U.S. (MIDUS I), a nationally representative survey of the 25-75 years old adult population that was administered by the National Institute of Aging in 1995. The primary advantage of MIDUS is the detailed information on smoking behavior, which allows us to uncover the entire smoking history of the respondents by assessing their smoking status in each year of their lives. In addition to the smoking data, the survey also captures important information on the respondents' family environment in childhood and

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[3] The cigarette price wars likewise did not happen until the early 1990s. The associated drop in prices has been linked to an increase in smoking among older teenagers, which was especially strong among disadvantaged groups (Gruber, 2001). The effect may depend on gender (Palali and van Ours, 2019).

adolescence, which we used to shed light on underlying mechanisms of the observed effects.

To ensure that in each pre- and post-reform year we compare treated and control respondents that are of the same age, we reshape the dataset by imposing the following changes to its structure. For each year between 1955 and 1990, we calculate the respondents' age and keep only those individuals who are between 0 and 25 years at that year. All years are then stacked to create a dataset that follows each individual from the year 1955, or the birth for those born after 1955, to 1990. Our study period ends in 1990 because in later years the sample size decreases substantially, and the sample comprises of less than five cohorts because the youngest cohort was born in 1970. We restrict the sample to those who are less than 26 years old because the overwhelming majority of smokers start using tobacco by age 25. This also reduces differential sample attrition, which may be correlated with bad health outcomes as a result of smoking (Daden and Kaestner, 2022). We also remove individuals with missing data on dependent and/or independent variables (about 10% of the sample). The resulting pooled sample includes 21,428 repeated, person-year, observations of men and 20,469 repeated observations of women, with 1,346 unique observations of men and 1,263 of women.

The dependent variable in the analysis is a binary time-varying variable that reflects the respondent's smoking status in year *t*. The participants were asked to report whether they had ever smoked, and if so, the age at which they started to smoke regularly and the age at which they stopped smoking regularly. Based on the answers to these questions, we classify a respondent as a smoker. The mean of the dependent variable is 0.165 for men and 0.203 for women.[4]

As noted above, we rely on the staggered difference-in-differences methodology developed by Callaway and Sant'Anna (2021) because the timing of the law adoption differs by state. Therefore, our variable of interest equals the year of the law adoption for 0-25 year old respondents from adopting states and zero otherwise. In our repeated sample,

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[4] In the raw MIDUS I data, the smoking rate for the combined sample of men and women is 25%, similar to the rate reported by the CDC for the year 1997 (CDC, 1997).

19.85% of men and 21.11% of women were exposed to unilateral divorce laws before reachingthe age of 25 years old.

We construct the dichotomous time-variant variable for *parental divorce* using information on the respondent's age at the time of parental divorce or separation, which we know up to age 16. It is equal to one for any age equal to or greater than the one given for the age at parental separation, and equal to zero for those whose parents never divorced or divorced when a respondent was over age 16. We use this variable to assess whether, in our data, the introduction of unilateral divorce laws increased the probability of parental divorce. We also define a dummy variable of whether a respondent grew up with both parents (at least) up to age 16 to estimate whether for these respondents unilateral divorce laws affected smoking behavior, thus separating the effects of changes in family formation and dissolution from changes in parental behavior in their relationship and family. Our sample size does not allow us to separately estimate this for those who did not grow up with both parents.

To assess potential channels mediating the effect of divorce laws on smoking, we use five variables describing parental education, poverty status, and parenting in childhood – all variables that might have been affected by the change in divorce laws. For *Parental Education*, we define a dummy each if any parent has a college education and if any parental information is missing. *Poverty in childhood* indicates whether a respondent grew up in a household with financial well-being well below average and/or a household that received welfare benefits. *Maternal (paternal) discipline and affection* are two variables for each parent that were constructed using factor analysis combining answers to questions about the family environment when the respondents grew up. These questions were designed to measure parental warmth and parenting strategies (see Appendix Table A2 for more information). As shown in Table 1, women report higher levels of maternal and paternal affection than men, higher levels of maternal discipline and lower levels of paternal discipline. Women were less likely to report having grown up in poverty than men.

Table 1: Summary Statistics

	(1) Women	(2) Men
Age	14.3	14.4
	(7.09)	(7.13)
Year of birth	1954.4	1954.6
	(8.82)	(9.00)
Smoking status at time t	20.3%	16.5%
Raised in poor family	10.6%	14.2%
Maternal affection	0.15	-0.13
	(0.9)	(1.1)
Maternal discipline	-0.06	0.06
	(0.9)	(1.1)
Paternal affection	0.05	-0.04
	(0.9)	(1.0)
Paternal discipline	0.08	-0.07
	(0.9)	(1.1)
At least one parent has college degree	31.7%	32.3%
Missing parental education	10.8%	14.2%
Parents were separated/ divorced at time t in childhood	9.9%	12.4%
Grew up with both parents until at least age 16	79.4%	72.6%
Respondent is white	95.2%	91.1%
N	20,469	21,428

Note: Standard deviations for continuous variables are shown in parentheses.

5. Results

In what follows, we first report the effect of the introduction of unilateral divorce laws on the probability of growing up with divorced parents, followed by the main results and an

exploration of potential mechanisms. We end this section with an assessment of the validity of our identification, a brief discussion of performed robustness checks, and a discussion.

Effect of Unilateral Divorce Laws on Divorce

As discussed above, the introduction of unilateral divorce laws might affect smoking behavior of offspring through three different channels: The effect of increased divorce rates and the associated changes in available resources and quality and quantity of parental attention; changes in selection into marriage; and changes in parental behavior resulting from the increased ease of divorcing, for example, through changes in intrafamily bargaining.

It is, therefore, informative to evaluate whether in our sample the introduction of unilateral divorce led to an increased likelihood of growing up with divorced or separated parents. The findings in the literature differ on the effect but point to an at least initial increase (e.g., Wolfers, 2006).

Table 2 shows the effects of the introduction of unilateral divorce laws on growing up with divorced or separated parents, using the same methodology as in our main estimations. We find that both women and men who were exposed to unilateral divorce laws by the age 25 have an about 4%-points higher probability of having divorced parents, which corresponds to an about 40% increase compared to the overall sample average. While only borderline significant at the 10% level, given the small number of individuals with divorced parents, we interpret this as evidence for an increased likelihood of parental divorce. Note, however, that in our sample, contrary to previous findings (e.g., Kábatek and Ribar, 2020), men are more likely to report that their parents divorced (12.4% versus 9.9%).

Table 2: Effect of Unilateral Divorce Laws on Parental Divorce

	(1) Women	(2) Men
Exposed to law by age 25	0.041*	0.043*
	(0.023)	(0.024)
Age dummies	√	√
Parental education	√	√
Group fixed effects	√	√
Year fixed effects	√	√
Covariate and cohort-specific time trends	√	√
Mean of dependent variable	9.9%	12.4%
Observations	20,469	21,427

Clustered standard errors are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Unilateral divorce laws and smoking

Tables 3a and 3b show in column 1 our baseline results for women and men with age dummies as the only control variables. Columns (2) to (4) show the results when adding additional control variables that, while potentially endogenous to the change in divorce laws, nevertheless help us better understand the underlying reasons for its effect on offspring's smoking.

In our baseline results, we find that women are 8%-points and men 6.5%-points more likely to smoke in adolescence and adulthood, which for both genders constitute an astonishing 40% increase compared to the overall averages in the sample.

For women, as shown in Table 3a, controlling for parental education reduces the effect to a statistically not significantly different 6.7%, pointing to a potential differential effect of unilateral divorce laws for individuals from different socioeconomic backgrounds. Note, though, that parental education may be endogenous for some parents, since as Stevenson (2007) has shown using conventional difference-in-differences methods, the introduction of unilateral divorce laws has reduced marriage-specific investment. Further controlling for poverty in childhood (also an outcome of parental divorce) does not change the effect in any significant way, and neither does adding parenting variables.

Table 3a: Effect of Unilateral Divorce Laws on Smoking: Women

	(1)	(2)	(3)	(4)
		+	+	+
		parental	poverty	parenting
	Main	education		
Exposed to law by age 25	0.080***	0.067*	0.070*	0.072*
	(0.030)	(0.038)	(0.040)	(0.042)
Age dummies	√	√	√	√
Parental education		√	√	√
Poverty in childhood			√	√
Parenting				√
Group fixed effects	√	√	√	√
Year fixed effects	√	√	√	√
Covariate and cohort-specific time trends	√	√	√	√
Mean of dependent variable	20.3%	20.3%	20.3%	20.3%
Observations	20,469	20,469	20,469	20,469

*Clustered standard errors are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Table 3b: Effect of Unilateral Divorce Laws on Offspring's Smoking - Men

	(1)	(2)	(3)	(4)
		+	+	+ parenting
		parental	poverty	
	Main	education		
Exposed to law by age 25	0.065**	0.061*	0.027	-0.002
	(0.031)	(0.030)	(0.029)	(0.026)
Age dummies	√	√	√	√
Parental education		√	√	√
Poverty in childhood			√	√
Parenting				√
Group fixed effects	√	√	√	√
Year fixed effects	√	√	√	√
Covariate and cohort-specific time trends	√	√	√	√
Mean of dependent variable	16.5%	16.5%	16.5%	16.5%
Observations	21,428	21,428	21,428	21,428

*Clustered standard errors are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

The picture for men is different. As can be seen in Table 3b, while there is also little change between the baseline specification and the one with added parental education, adding a dummy for growing up poor reduces the coefficient of interest by 60% and renders it statistically insignificant. Further adding parenting variables has no additional effect. We conclude from these results that for men it is the increased likelihood of poverty that causes the association with the greater likelihood of smoking. We will explore its relation to whether an individual grew up with both parents in the next section.

Response Heterogeneity

A different way to explore potential mechanisms is not to control for relevant (but endogenous) variables but to split the sample. In what follows, we report results from samples restricted to only those who grew up with both parents and for white respondents only, followed by a discussion of potential reasons for the observed gender differences. Due to small sample sizes, we are unable to look at those who did not grow up with both parents and those from minorities separately.

Starting with those who grew up with both parents, the results are shown in Tables 4a and 4b. For women, limiting the sample reduces all coefficients and renders all coefficients statistically insignificant. This suggests that the effects for women are driven by those who, as a result of the introduction of unilateral divorce, grew up either with separated or divorced parents or with parents who never cohabited in the first place. This would be in line with previous literature that found negative effects of divorce and single motherhood for children, as discussed earlier.

For men, the pattern is different. Reducing the sample to those who grew up with both parents (see Table 4b) yields results that in the case of the baseline specification are similar to the complete sample, but in all other cases the results are stronger and statistically significant. While the difference in the coefficients across specifications is only suggestive, one possible interpretation is as follows: Men who did not grow up with both parents are much more likely to start smoking if they grew up in poverty. For those whose parents did not separate, the increase in smoking may be stronger for those born to lower SES parents who may be working more hours (recall that our control is for college education of a parent

or missing information on one parents' college education), and unrelated to growing up poor or parenting styles.

Table 4a: Effect of Unilateral Divorce Laws on Offspring's Smoking – Women: Excluding those who did not grow up with both parents

	(1) Main	(2) + parental education	(3) + poverty	(4) + parenting
Exposed to law by age 25	0.018	0.008	0.009	-0.005
	(0.036)	(0.034)	(0.037)	(0.068)
Age dummies	√	√	√	√
Parental education		√	√	√
Poverty in childhood			√	√
Parenting				√
Group fixed effects	√	√	√	√
Year fixed effects	√	√	√	√
Covariate and cohort-specific time trends	√	√	√	√
Mean of dependent variable (those who grew up with both parents)	19.1%	19.1%	19.1%	19.1%
Observations	16,283	16,283	16,283	16,283

*Clustered standard errors are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Table 4b: Effect of Unilateral Divorce Laws on Offspring's Smoking – Men: Excluding those who did not grow up with both parents

	(1) Main	(2) + parental education	(3) + poverty	(4) + parenting
Exposed to law by age 25	0.068**	0.094***	0.083**	0.047
	(0.033)	(0.034)	(0.035)	(0.031)
Age dummies	√	√	√	√
Parental education		√	√	√
Poverty in childhood			√	√
Parenting				√
Group fixed effects	√	√	√	√
Year fixed effects	√	√	√	√
Covariate and cohort-specific time trends	√	√	√	√
Mean of dependent variable (those who grew up with both parents)	15.5%	15.5%	15.5%	15.5%
Observations	15,729	15,729	15,729	15,729

*Clustered standard errors are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Table 5: Effect of Unilateral Divorce Laws on Offspring's Smoking – Whites only

	(1) Women Main	(2) Women + parental education	(3) Men Main	(4) Men + parental education
Exposed to law by age 25	0.048	0.049	0.067**	0.070**
	(0.035)	(0.044)	(0.03)	(0.031)
Age dummies	√	√	√	√
Parental education		√		√
Group fixed effects	√	√	√	√
Year fixed effects	√	√	√	√
Covariate and cohort-specific time trends	√	√	√	√
Mean of dependent variable (excluding those with divorced parents)	20.4%	20.4%	17.1%	17.1%
Observations	19,418	19,418	19,444	19,444

*Clustered standard errors are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Since smoking rates, family formation, and socioeconomic status differ by race, we also conducted our estimations on samples that only include whites. Unfortunately, we are not able to separately conduct those for minorities because sample sizes become too small. The results are shown in Table 5. While we find not much difference for men, we find once again that women no longer have an increased propensity to smoke. It seems thus that the results for women were driven by changes in the family formation of non-whites.

The findings above show clear gender differences. They suggest that unilateral divorce laws increase men's probability of smoking through a) the channel of increased poverty for those who as a result grow up without both parents and b) for those who grew up with both parents, through changed parental behavior that is not captured by parental college education, poverty status, and the parenting variables available to us. Women, on the other hand, are more likely to smoke only if they did not grow up with both parents – independently of poverty status.

This finding is in line with previous findings that boys are more vulnerable to growing up in disadvantage, for example, low school quality has been linked to worse effects for boys' behavior than for girls' (Autor et al., 2019), but not linked to worse long-term outcomes in terms of education and income (Lei and Lundberg, 2020). While we are not able to investigate this further, it may be that the increase in labor force participation by mothers documented by Stevenson (2007) negatively affects the quantity and quality of parental time with children and especially of those mothers without a college degree (see also Bertrand and Pan, 2013).

For women, the absence of an effect of poverty on smoking suggests that the channel might be through the absence of a father or maybe the increased stress of their mother and associated increased maternal smoking. Parental smoking is, unsurprisingly, correlated with their offspring's smoking (e.g., Göhlmann et al., 2010), and increased maternal smoking after divorce has also been documented in the literature (e.g., Hillebrandt 2022).

Threats to Identification: The parallel trends assumption

Our employed staggered difference-in-differences estimation relies on the assumption that in the absence of treatment the treated and the control groups would have evolved similarly. By definition, this assumption is not testable, but pre-trends might be informative. Figures 1 and 2 show the results of estimating our difference-in-differences model by gender for the periods to and from treatment (that is, years before and after the introduction of unilateral divorce laws), using our main specification with age dummies.

As can be seen in the figures, while confidence bands are wide because of our sample sizes, there is no difference in smoking trends for the periods before the introduction of unilateral divorce. Trends diverge a few years afterwards, when couples may have started to divorce (or not get married in the first place) or to change their intra-household behavior in response.
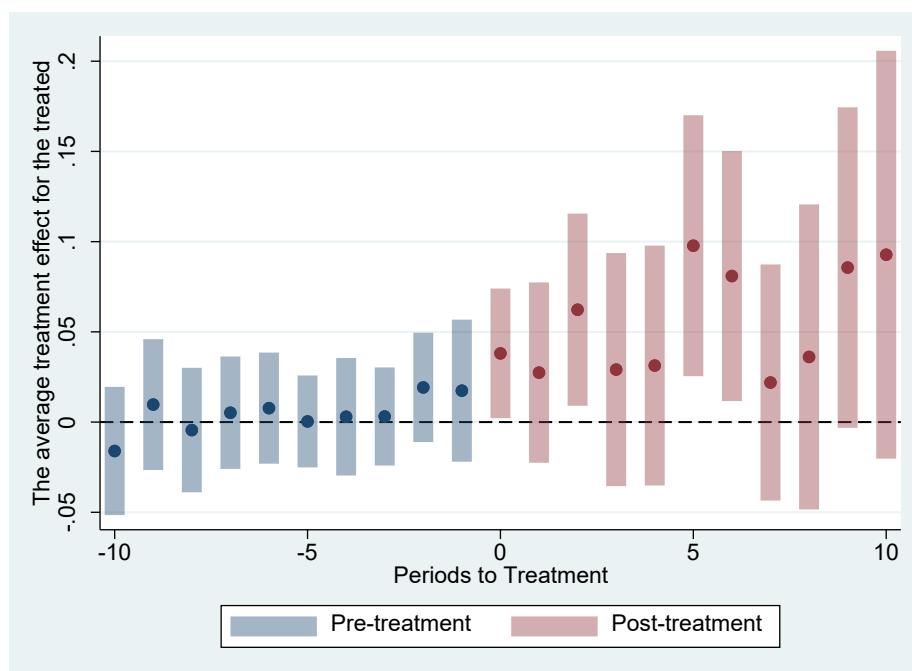

Figure 1: Parallel Trends of Smoking – Women

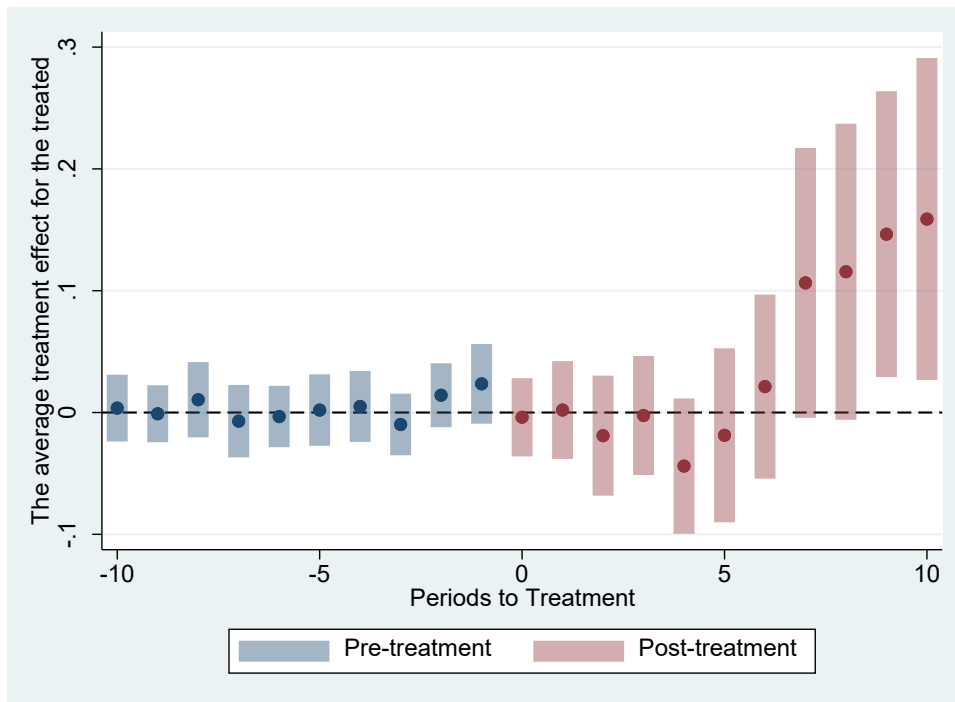*Specification (1), see Table 3a. Confidence intervals shown are 95%.*

Figure 2: Parallel Trends of Smoking – Men

Specification (1), see Table 3b. Confidence intervals shown are 95%.

Sensitivity Analysis and Conventional Difference-in-Differences

To support our conclusions, we conduct robustness checks and employ the conventional difference-in-differences methodology. First, we examine the sensitivity of our results to quitting by excluding those who quit smoking within five years after the end of the observation period. As shown in Panel A of Appendix Table A3, our conclusions hold even if we account for quitting. Next, to address the possibility of a recall bias, we use the second wave of MIDUS administered ten years later. Despite sample attrition, the point estimates in Panel B do not differ qualitatively from our baseline findings. Finally, for comparison, and to establish the relevance of using an estimation method that takes into account the staggered introduction of unilateral divorce laws, we show in Panel C our main results using conventional difference-in-differences estimation. In these results, there is neither an effect for women nor for men, which to a large extent is expected because these coefficients are biased downward.

6. Conclusions

In this paper, we assess the effect of a change in unilateral divorce law on the smoking behavior of offspring born before or during law adoption, using the econometric identification method developed by Callaway and Sant'Anna (2021) that eliminates the bias introduced by using conventional difference-in-differences methods in a setting where treatment is staggered. We find that exposure to these laws by age 25 significantly increases the probability of using tobacco products in youth and young adulthood. The effect is strong for both women and men but related to different mechanisms. For women, the driving factor is not growing up with both parents, while for men it is growing up poor and not with both parents. Men are also more likely to smoke if they grew up with both parents, for reasons other than the mechanisms we can control for with our data – parental education, poverty, and parenting.

There are a few caveats to keep in mind when considering our findings. First, selection into marriage and divorce in the U. S. has changed quite dramatically since the time period we are using for our identification. Most importantly, children of all but the highest educated parents are much more likely to grow up without both parents (Kearney, 2023). Taking our results at face value, they predict higher smoking rates as a result for most women and men, because families with one parent are much more likely to be poor. While smoking rates have declined significantly since 1995, the year our retrospective data was collected (CDC, 2022b), the mechanisms that made our treated cohorts more likely to smoke have not disappeared and can probably be expected to result in different types of risky behaviors.

hosting her during the 2023 Michaelmas term during which part of this research was conducted.

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APPENDIX

Table A1: Introduction of Unilateral Divorce Laws in the United States

State	Year	State	Year
Alabama	1971	Montana	1973
Alaska	1935	Nebraska	1972
Arizona	1973	Nevada	1967
Arkansas		New Hampshire	1971
California	1970	New Jersey	
Colorado	1972	New Mexico	1933
Connecticut	1973	New York	
Delaware	1968	North Carolina	
Florida	1971	North Dakota	1971
Georgia	1973	Ohio	
Hawaii	1972	Oklahoma	1953
Idaho	1971	Oregon	1971
Illinois		Pennsylvania	
Indiana	1973	Rhode Island	1975
Iowa	1970	South Carolina	
Kansas	1969	South Dakota	1985
Kentucky	1972	Tennessee	
Louisiana		Texas	1970
Maine	1973	Utah	1987
Maryland		Vermont	
Massachusetts	1975	Virginia	
Michigan	1972	Washington	1973
Minnesota	1974	West Virginia	
Mississippi		Wisconsin	1978
Missouri		Wyoming	1977

Source: Gruber (2004).

Table A2: Parenting – questions used and factor loadings

	Factor 1	
Panel A: Affection		
	Paternal affection	Maternal affection
How would you rate your relationship with your father/mother during the years you were growing up?	0.8528	0.7933
How much did he/she understand your problems and worries?	0.8637	0.8279
How much could you confide in him/her about things that were bothering you?	0.8142	0.7913
How much love and affection did he/she give you?	0.8454	0.8305
How much time and attention did he/she give you when you needed it?	0.8963	0.8575
How much effort did he/she put into watching over you and making sure you had a good upbringing?	0.7806	0.6965
How much did he/she teach you about life?	0.7759	0.6865
Panel B: Discipline		
	Paternal discipline	Maternal discipline
How strict was he/she with his rules for you?	0.9619	0.9399
How consistent was he/she about the rules?	0.8116	0.7033
How harsh was he/she when he punished you?	0.7177	0.5566
How much did he/she stop you from doing things that other kids your age were allowed to do?	0.6645	0.5410

Notes: We follow the survey methodology to construct these variables, retaining for each factor 1, which have eigenvalues greater than one.

Table A3: Robustness Checks and Conventional Difference-in-Differences Estimates

	Women	Men
	Main	Main
Panel A: Excluding those who quit smoking within five years after the end of the observation period		
Exposed to law by age 25	0.071**	0.060*
	(0.033)	(0.034)
Age dummies	√	√
Year fixed effects	√	√
Group fixed effects	√	√
Covariate- and cohort-specific time trends	√	√
Mean of dependent variable	18.9%	15.4%
Observations	19,277	20,122
Panel B: Using MIDUS II sample		
Exposed to law by age 25	0.103***	0.046
	(0.030)	(0.030)
Age dummies	√	√
Year fixed effects	√	√
Group fixed effects	√	√
Covariate- and cohort-specific time trends	√	√
Mean of dependent variable	19.2%	15.7%
Observations	13,882	15,452
Panel C: Conventional difference-in-differences		
	0.012	0.010
	(0.026)	(0.014)
Age dummies	√	√
Year fixed effects	√	√
Group fixed effects	√	√
Group-specific time trends	√	√
Mean of dependent variable	20.3%	16.5%
Observations	20,469	21,428