The Parenthood Penalty in Mental Health

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Abstract

Using Austrian and Danish administrative data, we examine the impacts of parenthood on mental health equality. Parenthood imposes a greater long-run mental health burden on mothers than on fathers: the percentage increase in mothers' antidepressant prescriptions due to parenthood exceeds that of fathers by about 98.6 percentage points (Austria) and 34.8 points (Denmark). These parenthood penalties in mental health mainly reflect the demands of having and raising children rather than biology, relationship breakdown, or non-employment. Supporting this interpretation, we show that adoptive mothers encounter substantial penalties and also that employed, cohabiting mothers account for most of the increase in prescriptions. Given our results, one may call for interventions such as longer parental leave. However, additional quasi-experimental evidence demonstrates that such extensions increase rather than reduce the prevalence of mental health problems among mothers and thereby widen inequality.

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1 Introduction

The costs of parenthood are unequally distributed between men and women. Childbirth has strong and persistent effects on women's labor market outcomes but not on men's (Angelov *et al.*, 2016; Kleven *et al.*, 2019a,b, 2021, 2024, 2025; Andresen and Nix, 2022; Kleven, 2023). Mothers also spend more time on child-related non-market activities, such as childcare (Guryan *et al.*, 2008) and home production (Borra *et al.*, 2021). Whether these unequal costs also cause a disproportionately greater mental burden for mothers than fathers is much less clear:[1] measurement is difficult, and credible identifying variation is scarce. As a result, the impacts of parenthood on men's and women's mental health remain poorly understood.[2]

This paper uses quasi-experimental research designs and administrative data from Austria and Denmark to show that parenthood creates enduring inequality in mental health. The adverse long-run effects are present for both sexes, but are much more pronounced in mothers. We term the resulting decline in mothers' relative to fathers' mental well-being the "parenthood penalty in mental health." The penalties arise in both Austria and Denmark, demonstrating robustness across two contrasting settings with different family policies and gender norms. These insights matter because mental health is a core dimension of well-being and has profound effects on key economic outcomes (Holden, 2000; Currie and Stabile, 2006; Fletcher and Wolfe, 2008; Currie, 2009; Friedrich, 2017; Cuddy and Currie, 2020; Biasi *et al.*, 2021; Edin *et al.*, 2022; Bhalotra *et al.*, 2025).

We can derive these insights because our setting allows us to tackle the two empirical challenges noted earlier. First, we tackle the "measurement challenge" by drawing on administrative health data. Particularly, following the literature, we use antidepressant prescriptions as our primary measure for mental health (Persson and Rossin-Slater, 2018; Ahammer and Packham, 2023). This outcome is valuable as it clearly reveals detected mental health problems, we can measure it consistently across countries, it does not suffer from reporting bias, and we can observe it for the entire population. Second, we respond to the "identification challenge" and estimate the impacts of parenthood for women relative to men using event studies around the birth of the first child (Kleven *et al.*, 2019a). The approach identifies the impact of parenthood on mothers, repeats the same estimation for fathers, and then compares the two resulting impacts. This final comparison yields our estimate

[1] Several mechanisms may explain larger adverse effects for mothers. The first relates to the biological effects of giving birth: women undergo the physical and hormonal changes associated with pregnancy, childbirth, and breastfeeding. These changes can have short- and long-term consequences for mental health. The second concerns the effects of having and raising children. Mothers carry most child-rearing responsibilities (Guryan *et al.*, 2008; Borra *et al.*, 2021) and face substantial cognitive load and stress. They then often choose between reducing labor supply (lower income and job satisfaction) or shouldering the double burden of paid work and childcare (which can be exhausting). Together, these burdens make mothers especially vulnerable.

[2] Some correlational studies examine the association between parenthood and self-reported well-being (Clark *et al.*, 2008; Dolan *et al.*, 2008; Blanchflower, 2009; Ferrer-i Carbonell, 2013). The studies are usually not design-based, and gender equality is not their focus. Baetschmann *et al.* (2016) also criticize the used methods and question the results.

of the parenthood penalty. We complement this estimation strategy with a difference-in-differences approach that uses childless individuals as a comparison group. A strength of these two estimation strategies is that they provide average effects for the population rather than identifying effects for specific compliers or selected samples with poor mental health.[3]

The details of our results are as follows. First, women in both countries face substantial mental health penalties. Before the first birth, fathers' and mothers' prescription trends evolve in parallel; afterwards, parents' prescriptions rise sharply due to childbirth, but this increase is much larger for mothers than for fathers. Several pieces of evidence suggest that these gender gaps do not simply reflect differences in help-seeking between the sexes.[4] Second, the parenthood penalty is larger in Austria: Nine years after birth, Austrian mothers' prescription probability is almost 5.0 percentage points (165.9%) higher than in the counterfactual without children, compared to 2.1 points (68.3%) for fathers. The resulting penalty, defined as the difference between mothers' and fathers' impacts, is 2.9 percentage points in levels (5.0-2.1) and 98.6 percentage points in relative terms (166.9-68.3). By contrast, in Denmark, the level penalty is 1.9 points and the relative penalty 34.8 points, with mothers' prescriptions rising by 2.7 points (62.4%) compared to 0.8 points (27.6%) for fathers. Third, regarding channels, the evidence indicates that the penalties do not result from biology: adoptive mothers (who have no biological connection) face similar penalties. Decomposition analyses further show that while relationship breakdown and non-employment contribute, the bigger part of the average increase in prescriptions is borne by cohabiting, employed mothers. Taken together, the evidence is consistent with the penalties stemming from the broader pressures and demands of having and raising children rather than from biology, relationship breakdown, or non-employment.

In sum, our paper shows that women bear the higher mental cost of parenthood. Prior work has established that women continue to face child penalties in earnings. We demonstrate that this type of inequality extends beyond the labor market: alongside lost earnings, many mothers suffer mental health penalties. The fact that both Austrian and Danish mothers face penalties suggests that this phenomenon can emerge in vastly different institutional contexts. Subsequent work strengthens this conclusion by confirming the lasting parenthood-induced increases in antidepressant prescriptions among German (Barschkett and Bosque-Mercader, 2024; Dehos *et al.*, 2024) and Swiss mothers (Bearth, 2024). However, these studies cannot examine gender inequality as they lack information on fathers.

Beyond our main result, we offer two additional insights. The first concerns the gender

[3] Economists have traditionally addressed endogeneity in fertility using instrumental variables (IVs). For first births, however, valid instruments are scarce. One exception is variation in the success of in vitro fertilization (IVF): Lundborg *et al.* (2017) find large and lasting negative effects on women's earnings at the extensive margin. They also test if failed IVF leads to greater antidepressant use and find no evidence of such an effect. For higher-order births, the literature exploits twin births or same-sex sibling pairs (Angrist and Evans, 1998).

[4] We, for example, find no gender gap in stated treatment-seeking and no increase in mothers' visits to prescribing doctors. Furthermore, additional robustness checks (including analysis of adoptive mothers, screening at birth, psychiatrist visits, and placebo outcomes) point in the same direction.

gap in mental health. A consistent finding in social epidemiology is that women are much more likely than men to receive treatment (see, e.g., Piccinelli and Wilkinson, 2000; Nolen-Hoeksema, 2001; Van de Velde *et al.*, 2010; WHO, 2017; Churchill *et al.*, 2020). Austrian and Danish parents fit this pattern: mothers in Austria are about 81.5% more likely than fathers to obtain antidepressants; in Denmark, the gap is 82.4%. Given the large and persistent parenthood penalties, a natural question is to what extent parenthood explains these gender gaps in prescriptions. Combining a simple decomposition framework with our event study approach, we find that parenthood indeed accounts for a substantial share of the overall gaps: 37.8% in Austria and 25.4% in Denmark. This result shows that children play a key part in why women are more likely to receive mental health treatment. Policymakers concerned with mental health equity might, therefore, be able to level the playing field with reforms that tackle the parenthood-related part of the gender gap in mental health.

The second complementary insight follows from examining how one such intervention — namely, reforms that vary paid maternity leave — impacts parents' mental health. Longer leave could, in principle, improve mothers' mental health by easing the transition to parenthood, reducing the short-run double burden of work and childcare, and offering more time to bond with children. Yet expansions may also backfire: they can prolong exclusive caregiving, reinforce traditional gender roles, amplify mothers' childcare contributions in the long run, and risk lowering mothers' income and job satisfaction. In short, the expected effects are ambiguous. We, therefore, evaluate the impacts of such reforms using regression discontinuity designs that exploit sharp eligibility cutoffs in Austrian and Danish reforms (Schönberg and Ludsteck, 2014; Danzer *et al.*, 2022). Our main result is that parental leave reforms exacerbate the mental health penalties when they substantially extend mothers' leave in already long-leave environments. Mothers who spend markedly more time with their children face higher risks of mental health problems, while fathers are unaffected. The first adverse effects appear during the extended leave period, they strengthen when women re-enter the workforce, and persist for more than a decade. These findings suggest that maternity leave extensions reinforce mothers' roles as primary caregivers or act as a one-time trigger for lasting mental health challenges. By contrast, the effects are unlikely to operate through labor supply or earnings. The same reforms did not alter mothers' long-run labor market outcomes (Kleven *et al.*, 2024). Overall, prolonging exclusive caregiving seems to place an additional burden on mothers and amplifies the penalties they face.[5]

Our paper contributes to several strands of the literature. First, we build on and extend a primarily medical literature highlighting the role of postpartum depression (e.g., Evenson and Simon, 2005; Shorey *et al.*, 2018). While most existing studies rely on self-reported data, we use administrative data and a design-based approach to show that the detrimental mental health effects persist well beyond the short run and are unlikely to be purely

[5] Glogowsky *et al.* (2023) provide supporting evidence from Austrian data. They show that fathers of sons invest more time in childcare and also experience poorer mental health.

biological. We contribute further by documenting similar long-run effects across two settings with very different institutions and gender norms, analyzing both mothers and fathers to study inequality, and examining how family policy shapes these outcomes. Second, we contribute to research on gender inequality (Bertrand, 2011; Azmat and Petrongolo, 2014; Blau and Kahn, 2017; Bertrand, 2020), particularly to research on the impact of parenthood on wages, labor supply, and job absences (Bertrand *et al.*, 2010; Angelov *et al.*, 2016; Kuziemko *et al.*, 2018; Kleven *et al.*, 2019a,b; Andresen and Nix, 2022; Cortés and Pan, 2023; Kleven, 2023; Rosenbaum, 2023; Kleven *et al.*, 2025). Our distinctive contribution is to show that parenthood creates gender inequality in mental health. We also advance the literature by decomposing the overall gender gap into parenthood-related and parenthood-unrelated components, and examining mechanisms (such as biology versus child-rearing demands). Third, we extend work on the impacts of family policy (Olivetti and Petrongolo, 2016) by analyzing how parental leave reforms affect mental health equality. In doing so, we complement Chuard (2023), who examines the short-run effects of Austrian reforms on maternal health utilization and (in a sub-analysis) maternal mental health. We extend this work to include long-run effects, fathers, parenthood penalties, and evidence from Denmark.[6]

2 Institutional background

One goal of our analysis is to assess whether mental health penalties appear in different societal contexts. We, therefore, study two countries that allow consistent measurement of outcomes but differ markedly in their policies and gender norms: Austria and Denmark. This section highlights the most relevant similarities and differences. Appendix A provides more details.

Both Austria and Denmark are high-income welfare states with comprehensive social security systems and universal access to healthcare. Both countries devote about 10% of their GDP to healthcare, and their aggregate health indicators (such as infant mortality and life expectancy) are very similar (Appendix A). Several similarities are particularly important for our joint analysis. First, both countries' healthcare systems provide equivalent coverage, including all costs associated with illness and maternity in inpatient and outpatient care. Second, they implement the WHO Anatomical Therapeutic Chemical (ATC) classification system, which allows prescription outcomes to be coded uniformly and compared directly. Third, mental health status and service use are broadly comparable. For example, survey evidence shows that self-reported average mental health differs only slightly between the two countries. Moreover, about 15–17% of adults report seeking professional help for psycho-

[6] Chuard (2023) reports adverse short-run effects of Austrian reforms on maternal mental health. Instead, Bütikofer *et al.* (2021) find positive health effects from parental leave extensions in Norway.

logical problems, and suicide rates among women are almost identical.[7] Our administrative data confirm comparability in treatment patterns: the probability of taking antidepressants one year before the first birth is similar in both countries (Panel B in Table 1). Fourth, the organization of mental healthcare is similar. In both countries, general practitioners are the first point of contact; they prescribe the majority of antidepressants, and the distribution of treatments is comparable: most patients rely on medication, smaller shares receive psychotherapy, and very few are hospitalized (Appendix A).

Despite these commonalities, Austria and Denmark differ strongly in the policies governing the transition to parenthood. Austria provides substantially longer leave than for Denmark. For example, in 2021, Austrian parents could take up to 35 months of paid leave if shared, or 28 months if taken by one parent alone. By contrast, Denmark offered only 32 weeks of fully paid shared leave (after 14 weeks of maternity leave). Childcare provision and use show the opposite pattern. In Austria, kindergartens for ages three to six are common. However, nurseries for children under three are more scarce and often constrained by short hours and long holidays. Reflecting this pattern, enrollment among the youngest children is low: in 2018, only about 27% of Austrian children under three attended formal childcare (UNECE, 2025). In Denmark, children are entitled to a slot from six months, and in 2018 the enrollment rate under three was more than twice as high at 68% (UNECE, 2025).

Mothers' labor market behavior after childbirth shows patterns consistent with these institutional differences. Their participation rates in the core working ages (25–54) are lower in Austria (76.4%) than in Denmark (81.8%). The differences in working time among mothers are even larger. In 2021, 41% of employed mothers in Austria worked part-time, compared to only 9% in Denmark (Appendix Table A.2). Reflecting these patterns, the child penalty in earnings is much larger in Austria. The arrival of children leads to a long-run gender gap in earnings of about 50% in Austria compared to 20% in Denmark (Kleven *et al.*, 2019a). Also, the breastfeeding patterns mirror these institutional contrasts: while initiation is nearly universal in both countries (about 97% of mothers), continuation is substantially higher in Austria than in Denmark (Appendix A).

These institutional and behavioral differences coincide with equally distinct social norms. Austrians hold more conservative views on family and gender roles than Danes. This insight, for example, follows from the 2002/2012 ISSP survey data. One dimension in which differences emerge is individuals' views on the compatibility between maternal employment and family well-being. Far more women in Austria strongly agree that preschool children suffer if the mother works (25% Austrians vs. 6% of Danes), and fewer believe that working mothers can maintain a warm relationship with their children (47% vs. 61%). Another dimension concerns gender identity and preferences. Austrian women are slightly more likely

[7] There is a significant difference in the suicide rates for men. It is higher for Austria (24.1 cases per 100,000 population) than Denmark (19.6).

to agree that what most women really want is a home and children (9% vs. 7%). Appendix A provides more results, separately for men and women.

Beyond these attitudinal differences, the two countries also differ in key demographic patterns. During our sample period, Denmark's fertility was consistently higher than Austria's, and Danish women had their first child at a later age on average (Appendix Table A.4).

3 Data sources, outcomes, samples, and descriptives

We next outline the data sources, define the outcome variables and samples, and present descriptive statistics.

3.1 Data sources

For Austria, we combine two administrative datasets. The first is the "Austrian Social Security Database" (ASSD). These administrative records span the period 1972–2021 and allow us to observe employment and childbirth histories for the universe of Austrian women. The second dataset is provided by the "Austrian Health Insurance Fund" ("Österreichische Gesundheitskasse", hereafter ÖGK). The ÖGK insures the vast majority of employed people, their dependents, and all non-employed residents, covering about 82% of the population. We have access to the ÖGK data for the entire population of Upper Austria.[8] These data cover roughly one million individuals and include all prescribed drugs, physician visits, and hospital stays between 1998 and 2016. The coding of therapeutics follows the ATC classification system, which ensures comparability of outcomes between Denmark and Austria.

The Danish data also come from two sources. First, we use register data from "Statistics Denmark," covering the entire population of persons registered in Denmark between 1986 and 2022. Hence, our primary data source is the population register, which includes the exact birthdates, a unique personal identifier, and links to the legal mother and father. These features allow us to identify the family links necessary for estimation. We supplement these data with further information on the place of residence, earnings, labor market attachment, civil status, contacts with the health system, and diagnoses from general and psychiatric hospitals. Second, we use ATC-coded prescription drug data from the "Danish Health Data Authority."

3.2 Main outcome

Using these data, we construct our primary outcome variable as a dummy indicating whether an individual received an antidepressant prescription in a given year (ATC code N06A). Our

[8] Upper Austria is one of nine federal states in Austria and accounts for about one-sixth of the Austrian population and workforce. Until 2020, each state had its own regional health insurance fund. That year, the government merged the nine regional funds into the ÖGK.

paper focuses on this outcome for several reasons. First, antidepressant prescriptions are a clear indicator of mental health problems. They require a physician's assessment and cannot be obtained without a documented indication. Moreover, unlike survey-based measures, prescription data are not subject to self-reporting or recall bias. Second, antidepressants are the first-line treatment for depression and anxiety.[9] These conditions are sensitive to social and economic stressors that can emerge during the transition to parenthood (such as sleep deprivation, financial strain, and work-family conflict). Third, depression and anxiety are by far the most prevalent and, therefore, the most policy-relevant mental health problems among working-age adults (World Health Organization, 2025). Fourth, antidepressants are readily accessible in routine primary care in both Austria and Denmark. As a result, prescription data capture a broad population of people with mental health problems. Hospital diagnoses, often used as a measure when prescription data are unavailable, only reflect more acute cases and, therefore, miss most mental health patients. Diagnosis data are also less comparable across settings. Fifth, prescription records are universally available for all individuals in our study population and, through harmonized ATC codes, are directly comparable across Austria and Denmark. Sixth, neither country uses automatic refills, and prescriptions specify package sizes rather than intended duration.[10] This feature allows us to study the persistence of mental health problems over time.

3.3 Samples

A key step in our analysis is constructing similar estimation samples for both countries to facilitate comparability. Our approach is to (a) construct an estimation sample for Austria and (b) adapt the Danish sample to the Austrian one. The Austrian data serve as a starting point because they are more limited in sample length and detail.

We construct the Austrian sample in a step-by-step process. First, we start with a dataset containing all parents with a first-born child between 2002 and 2007. We focus on this period because the health data cover only 1998 to 2016, and we aim to analyze behavior in four years before birth and nine years after birth. Second, we merge the health data with the dataset from step one. Third, following Kleven *et al.* (2019a), we construct a fully-balanced panel. Consequently, our final estimation sample includes parents insured with the ÖGK throughout the entire 14-year period. Fourth, we drop all parents who were younger than 18 or older than 55 when they had their first child. We focus on this age group because

[9] We focus on antidepressants rather than all psychotropics because the broader category bundles medications for rare and/or acute conditions (such as schizophrenia or bipolar disorder). Many of these disorders have a stronger biological and genetic component and, thus, are likely to respond less to the stressors of parenthood. Including these heterogeneous drug classes would blur interpretation rather than sharpen it.

[10] In Austria, patients pay a prescription fee for medicines, amounting to €6.85 (2023) per item. If the market price of a prescribed medicine is lower than the prescription fee, the patient only pays the market price. In these rare cases, our data do not record the medicines dispensed. Low-income patients are exempt from the prescription fee. We observe all dispensed medicines for this group, regardless of their market price. In the Danish data, we observe all prescription medicines dispensed at pharmacies.

Table 1: Summary statistics

	Austria		Denmark	
	Women	Men	Women	Men
A. Socioeconomic variables				
Age at birth	28.16	30.86	28.83	30.94
Birth year	2,004.59	2,004.42	2,004.52	2,004.50
Married at birth	0.41	0.65	0.42	0.43
Annual wage (1,000 EUR) two years before birth	19.46	25.97	23.11	31.58
Employed two years before birth	0.97	0.99	0.93	0.92
B. Primary outcome				
Any antidepressant prescription (%) two years before birth	2.97	1.61	3.23	1.65
C. Outpatient physician visits (fractions)				
GP visit two years before birth	0.82	0.74	0.95	0.75
Neurologist or psychiatrist visit two years before birth	0.03	0.02	0.02	0.01
Gynecologist and urologist visit two years before birth[a]	0.69	0.06	0.10	
Other specialist visit two years before birth	0.71	0.56	0.79	0.68

Notes: This table shows descriptive statistics for our Austrian and Danish estimation samples. It considers men and women separately. Moreover, it presents arithmetic means for (a) a selection of socioeconomic variables, (b) our main outcome variable, and (c) the probabilities of different outpatient physician visits. All birth-related variables refer to the first child's birth. [a]In Denmark, we cannot observe urologists in outpatient settings.

we are interested in the impact of children on the working-age population. A limitation of the Austrian data is that we observe fathers only if their children were born in wedlock or if we can identify them through other means (e.g., if they claim certain tax deductions or co-insure their children). To verify that this restriction is not driving differences in results between Austria and Denmark, we also provide estimates for married parents alone.

Following the construction of the Austrian sample, we align the Danish sample with the Austrian one in terms of years of observation, years of births, and age at first birth. Therefore, we merge all birthdates to mothers and fathers aged 18 to 55 years at birth and keep the first births from 2002 to 2007. Again, we construct a balanced panel of four years before and nine years after the first birth, yielding a second panel spanning from 1998 to 2016. In Denmark, we can match children to all fathers.

3.4 Descriptive statistics

Table 1 summarizes the descriptive statistics of our estimation samples. Both countries have similar average ages at birth and comparable employment statuses two years before birth (Panel A). Women in both countries are also more likely than men to visit outpatient physicians (Panel C). We observe this pattern for general practitioners, mental health specialists, gynecologists, and other specialists.[11] The table also demonstrates institutional and demographic differences. First, Austrian men seem to be more likely to be married at birth than the Danes. However, this finding is due to the limitation that our data does not include all unmarried fathers in Austria. For women, we do not find significant differences in mari-

[11]Austrian women routinely visit gynecologists, while Danish women see their GPs for standard gynecological screenings. We cannot identify outpatient urologists in the Danish data. Telemedicine is much more widely available in Denmark than in Austria.

tal status between the two countries. Second, Austrian parents earn less than their Danish counterparts, but wages are hardly comparable across the two countries due to differences in economic contexts (such as living costs). Third, consistent with the Eurobarometer survey evidence, we find that the probability of taking medication (here: antidepressants) is comparable in Austria and Denmark (Panel B).

4 Impact of parenthood on mental health

This section presents our estimation strategy and reports the baseline results. We then examine robustness, mechanisms, and heterogeneity.

4.1 Event study methodology

To measure the impact of parenthood on mental health, we would ideally like to parenthood randomly. As such experiments are not available, we apply the standard approach of Kleven *et al.* (2019a,b) to our setting. This method is suited to trace out the overall dynamic impacts of parenthood on mental health and gender inequality.[12] The key idea of this approach is that, although fertility is not exogenous, childbirth should generate sharp changes in the outcome that are arguably orthogonal to the unobserved determinants of the outcome. Following this rationale, Kleven *et al.* (2019a,b) propose an event study approach that exploits changes around the birth of the first child for identification.

The estimation strategy proceeds in three steps. In the first step, we assess the impact of parenthood on the probability that mothers m get antidepressant prescriptions. For that purpose, we define Y^m_{ist} as a binary variable indicating whether or not mother i of age a_{is} gets such a prescription in calendar year s at event time t (measuring the time relative to the year of the first child's birth). Using this notation, we estimate the following regression for the balanced sample of women with children:

$$\begin{aligned} Y^m_{ist} = \sum_{event \neq -2} \alpha^m_{event} \cdot \mathbb{1}[event = t] + \sum_{year} \beta^m_{year} \cdot \mathbb{1}[year = s] \\ + \sum_{age} \gamma^m_{age} \cdot \mathbb{1}[age = a_{is}] + u^m_{ist}. \end{aligned} \tag{1}$$

The first term on the right-hand side of equation (1) captures event dummies, the second term reflects year dummies (to control flexibly for time trends in prescriptions), and the third term denotes age dummies (to factor out life-cycle effects). The event time $t = -2$

[12] Alternatively, one could use in vitro fertilization as an instrument for becoming a parent (Footnote 3). However, as this approach relies on a specific population and specific compliers, it is not appropriate to examine the impacts of parenthood on overall gender inequality. Other standard instruments, such as twin births or sibling sex, also cannot identify the overall impact of parenthood on mental health or gender inequality (Kleven *et al.*, 2019a,b). Instead, they identify the marginal effects of the second or third child on various outcomes.

serves as the reference period.[13] If the non-child prescription path is smooth conditional on controls, the estimates of the parameters α_t^m identify the total impacts of parenthood on Y_{ist}^m at event time t.

In the second step, we estimate model (1) on the sample of fathers f, obtaining the corresponding coefficient estimates $\hat{\alpha}_t^f$, $\hat{\beta}_s^f$, and $\hat{\gamma}_{a_{is}}^f$. Note that while the approach is anchored in the event of the first childbirth, the longer-run effects incorporate the impacts of subsequent children. We present the step-one and step-two estimates in two forms: First, we plot the coefficients $\hat{\alpha}_t^m$ and $\hat{\alpha}_t^f$ across t in levels, illustrating the impacts on the share of parents with antidepressant prescriptions (in percentage points). Second, we express these level effects as percentage deviations from parents' counterfactual prescription probability in the absence of children (in percent). Specifically, we construct parent i of gender $g \in \{m, f\}$'s counterfactual as the predicted value of the outcome when setting the event-dummy effects to zero: $\tilde{Y}_{ist}^g = \sum_{year} \hat{\beta}_{year}^g \cdot \mathbb{1}[year = s] + \sum_{age} \hat{\gamma}_{age}^g \cdot \mathbb{1}[age = a_{is}]$. The resulting average percentage effect at event time t is: $\hat{\alpha}_t^g / E[\tilde{Y}_{ist}^g | t]$. The absolute effects measure changes in the prevalence of mental health problems, indicating the additional public health burden associated with parenthood. The relative effects instead scale these changes by counterfactual risk. This scaling allows comparisons across groups with different counterfactual prescription levels.

In the third step, we estimate the parenthood penalty in antidepressant prescription at event time t. In levels, the definition of the penalty is:

$$P_t^l = \hat{\alpha}_t^m - \hat{\alpha}_t^f. \tag{2}$$

Intuitively, P_t^l reflects the extent to which the impact of parenthood on the probability of prescription for mothers exceeds that for fathers (in percentage points). Moreover, we define the relative penalty as follows:

$$P_t = \frac{\hat{\alpha}_t^m}{E[\tilde{Y}_{ist}^m | t]} - \frac{\hat{\alpha}_t^f}{E[\tilde{Y}_{ist}^f | t]}. \tag{3}$$

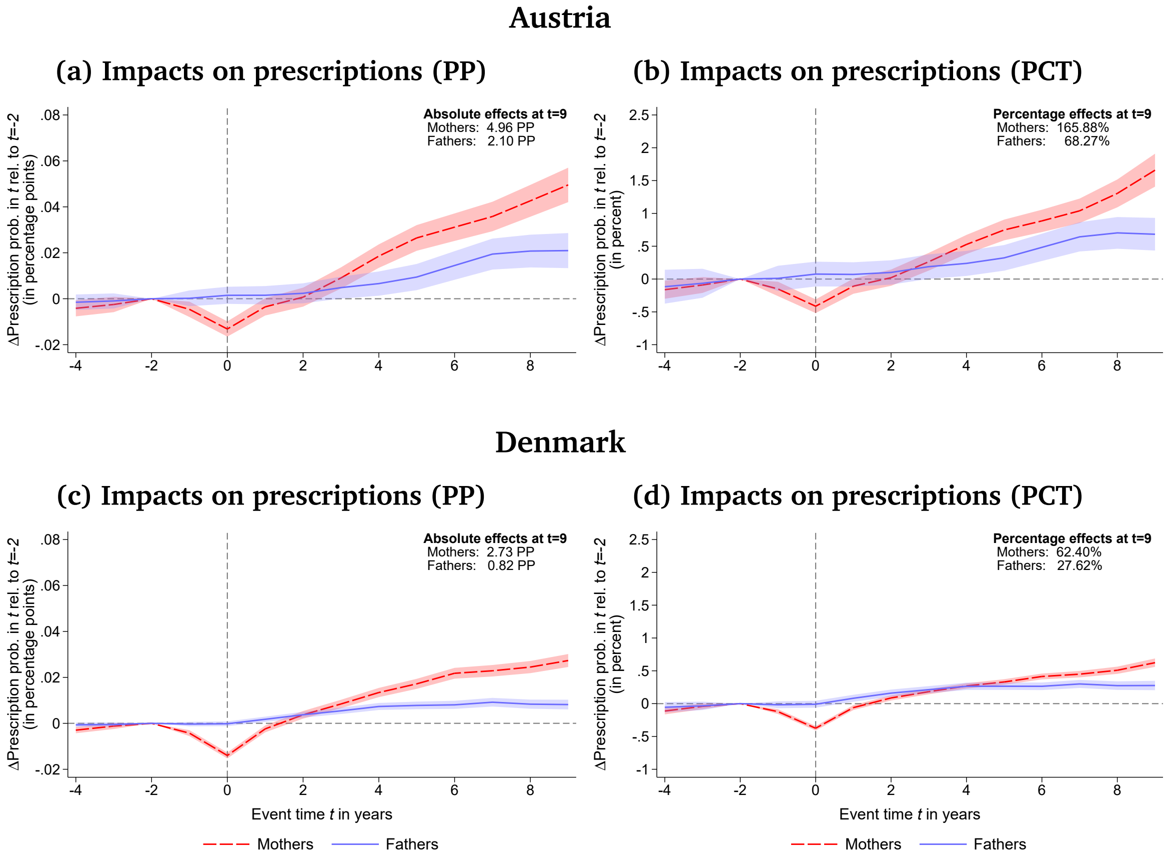
The relative penalty shows the proportional increase in prescriptions for mothers compared to fathers (in percentage points).

The penalties are identified if, conditional on age and year fixed effects, prescriptions for mothers and fathers would have followed parallel trends in the absence of childbirth.[14] We

[13]There are potential risks of taking certain antidepressants during pregnancy. Therefore, we expect to observe a drop in the prescription probability in the pre-birth year, and we use $t = -2$ as our reference period.

[14]Formally, write $\hat{\alpha}_t^g = \theta_t^g + \delta_t^g$, where θ_t^g denotes the causal effect of parenthood for gender $g \in \{m, f\}$ at event time t, and δ_t^g captures any remaining non-child-related change at t. Moreover, let $\mu_t^g = E[\tilde{Y}_{ist}^g \mid t]$ denote the true counterfactual prescription probability (with $\mu_t^g > 0$). P_t^l is identified when $\delta_t^m = \delta_t^f$ (parallel trends in levels), and P_t is identified when $\delta_t^m / \mu_t^m = \delta_t^f / \mu_t^f$ (parallel trends in proportional terms). In addition, under the within-gender smoothness assumption, $\delta_t^m = \delta_t^f = 0$, both P_t^l and P_t have a causal interpretation.

Figure 1: Impacts of parenthood on antidepressant prescriptions



Notes: This figure shows the estimated impacts of parenthood on antidepressant prescriptions before and after the birth of the first child. Figures 1a and 1b show results for Austria, and Figures 1c and 1d for Denmark. Moreover, Figures 1a and 1c report effects in percentage points ($\hat{\alpha}_t^j$), while Figures 1b and 1d report relative effects in percent ($\hat{\alpha}_t^j / E[\tilde{Y}_{ist}^j | t]$). The dashed lines refer to mothers ($j = m$) and the solid lines to fathers ($j = f$). We obtain the event time coefficients by estimating regression (1) on a balanced sample of parents who have their first child between 2002 and 2007. The shaded areas represent 95% confidence intervals based on robust standard errors. The top-right corner of each figure also shows effects at event time $t = 9$.

provide robustness checks to guard against the possibility that the age and year dummies do not absorb all factors driving differences in trends between men and women. For example, we use individuals without children as a control group to account for potential non-child-related trends in our mental health outcomes.

4.2 Baseline estimates of parenthood penalties

Figure 1 depicts the impacts of parenthood on mothers' (dashed lines) and fathers' (solid lines) antidepressant prescription probability for Austria (top figures) and Denmark (bottom figures). Figures 1a and 1c report the event-time-specific coefficients $\hat{\alpha}_t^m$ and $\hat{\alpha}_t^f$ as percentage-point effects. By contrast, Figures 1b and 1d show the same impacts relative to the counterfactual prescriptions without children: $\hat{\alpha}_t^m / E[\tilde{Y}_{ist}^m | t]$ and $\hat{\alpha}_t^f / E[\tilde{Y}_{ist}^f | t]$.

The patterns showcase parenthood penalties in antidepressant prescriptions in Austria and Denmark. In both countries, prescriptions for mothers and fathers evolve in parallel before the first birth (after netting out life-cycle and year effects). At the time of birth, mothers' prescription probability drops significantly by 1.3 percentage points (Austria) and 1.4 percentage points (Denmark) relative to the counterfactual.[15] This decline likely occurs because taking certain antidepressants during pregnancy and breastfeeding carries potential risks or is not always recommended. Fathers do not exhibit a corresponding dip. The first two post-birth years for mothers mainly reflect a rebound from the temporary reduction in medication around delivery. During this period, mothers' prescriptions increase more steeply than fathers' but largely remain below the implied counterfactual without children (zero line). Fathers, by contrast, start from their baseline and follow a gentle upward trend. From about the third year onward, however, the point estimates for mothers exceed those for fathers, giving rise to parenthood penalties in prescriptions that then grow steadily over time.[16] The 95% confidence intervals for the percentage-point estimates of mothers and fathers no longer overlap after roughly three to four years after birth, and the intervals for the percentage effects diverge a bit later. This pattern suggests that the absolute effects materialize slightly earlier than the relative ones. In conclusion, aside from minor differences in timing, the overall patterns are strikingly similar in both Austria and Denmark.

In terms of magnitudes, the effects are substantial in size: In Austria, nine years after the first birth, mothers' antidepressant prescriptions are almost 5.0 percentage points (or 165.9%) higher than the implied counterfactual without children.[17] For men, the corresponding increase is about 2.1 percentage points (or 68.3%). These differential impacts of parenthood translate into substantial parenthood penalties in antidepressant prescriptions. The parenthood penalty in the ninth year after birth, defined as the difference between mothers' and fathers' impacts, is $P_9^l = 2.9$ percentage points in levels or $P_9 = 98.6$ percentage points in relative terms. Due to children, Austrian mothers are 2.9 percentage points more likely to get antidepressants than Austrian fathers. Put differently, the increase in mothers' prescriptions compared to their childless counterfactual is 98.6 percentage points larger than the corresponding increase for fathers.

Quantitatively, the impacts of parenthood on both mothers and fathers appear somewhat weaker in Denmark than in Austria. Nine years after birth, Danish mothers only experience a 2.7 percentage point (or 62%) increase in the probability of receiving antidepressant prescriptions. By contrast, Danish fathers' probabilities rise by only 0.8 percentage points (or

[15]The dip in prescriptions in the year of birth most likely masks an increase in postpartum depression. The reason is that breastfeeding mothers may refrain from taking antidepressants.

[16]Note that in one case, mothers' effects surpass those of fathers from the fourth rather than the third year after childbirth: the percentage specification for Denmark. The difference between the percentage-point and percentage effects is due to the relatively larger counterfactual prescription propensity for women.

[17]As described in Section 4.1, we compute percentage effects relative to the implied counterfactual prescription probability in the absence of children (rather than relative to the observed mean at $t = -2$). This approach accounts for life-cycle trends and secular trends in prescribing, ensuring consistent interpretation across groups and event times.

28%). Consequently, the implied penalties are $P_9^l = 1.9$ and $P_9^l = 34.8$ percentage points. We conclude that although the penalties are smaller in Denmark, the differences between Austria and Denmark do not reduce the burden on mothers enough to close the gender disparities entirely (even Danish mothers face penalties). The parenthood penalty in antidepressant prescriptions, thus, seems to be a resilient phenomenon that persists across two different societal contexts.

Given the observed effect dynamics, two questions arise naturally. The first question is why the effects begin to increase after birth and continue to grow over time. Our intuition is that a multitude of factors contribute to this pattern. The development of depression is not an instantaneous process, and there is some delay after the onset of the disorder until antidepressants are prescribed. Moreover, the treatment of being a parent likely unfolds and intensifies in the years after birth. At the same time, counteracting honeymoon effects may fade. Also, the probability of a health shock in children increases over time. Finally, but perhaps most importantly, subsequent fertility could also play a critical role. The demands and challenges of parenthood likely intensify with the number of children, and we indeed find suggestive evidence in line with this hypothesis. Appendix Figure G.1 separates the Danish sample by the total number of children, information available only in Denmark. The figure suggests that subsequent fertility drives the continuous increase in antidepressant prescriptions four or more years after the birth of the first child. This mechanism, however, cannot explain the larger penalties in Austria. Austrian women had lower fertility than Danish women during our sample period (Table A.4). Hence, despite having fewer children, Austrian women experience more substantial increases in antidepressant prescriptions.

The second question is whether the penalties persist beyond the ninth year after birth. This topic is pertinent because economists typically view children as long-term investments: while the costs of parenting may concentrate in the early years, mental health benefits may emerge later. For Denmark, we follow parents for up to 25 years after birth. Exploiting this feature, Appendix B provides suggestive evidence that the adverse effects on mothers' prescriptions prevail for decades after the first birth. Instead, fathers exhibit only small and stable increases in their prescription rates. This pattern points to lasting gender differences in the effects of parenthood.

4.3 Specification checks

Our first robustness check explores whether the patterns observed in Figure 1 mirror general trends unrelated to parenthood that our age and year controls fail to capture. To that end, we assign placebo birth events to childless individuals to match them with parents, and then examine whether similar patterns emerge around these hypothetical events. If such patterns do appear, we likely cannot ascribe the estimated baseline impacts solely to parenthood. Appendix C discusses the empirical approach and presents the results. In contrast to our baseline estimates, we find no changes in women's and men's antidepressant prescriptions around the placebo event (Appendix Figure C.1). As an extension, we use childless individuals with assigned placebo births as a control group in a difference-in-differences event study design (Kleven *et al.*, 2019b). This estimator compares changes over time for those with children (treatment group) to changes for childless individuals with assigned placebo births (control group). Reassuringly, for both countries, we find point estimates that are very comparable to the baseline results (Appendix Figure C.2).[18] The evidence indicates that non-child trends do not confound our baseline estimates.

A second robustness check addresses the fact that we cannot link all Austrian fathers to their children. To assess whether this limitation drives our findings, we re-estimate our results in a sub-sample where linkage is complete: married parents. All results are robust (Appendix Figure G.2). Most importantly, in both countries, mothers face parenthood penalties in prescriptions. Note that this exercise does not aim to ensure full cross-country comparability, as selection into marriage may differ across contexts. Instead, it verifies that our main conclusion holds in a subsample without linkage limitations.

To further establish the credibility of our results, we plot the raw data as a third plausibility check (Appendix Figure G.3). The figure reveals parenthood penalties in both countries, showing that our main conclusion is even evident in the raw data.[19]

4.4 Parenthood penalties: Actual differences in mental health?

A key question is whether the parenthood penalties in prescriptions capture genuine differences in mental health. We, therefore, we discuss several alternative explanations. Importantly, receiving an antidepressant prescription requires that an individual first experiences mental distress, then seeks medical care, and ultimately obtains treatment. The penalty we observe would not indicate a true decline in mental health if (a) fathers are equally distressed but substantially less likely to seek help and obtain treatment, (b) childbirth leads to a sharp increase in detection among mothers but not fathers, or (c) antidepressant prescription is a poor proxy for mental distress. We discuss each of these alternative explanations in turn.

The first alternative explanation (a) is that the penalty reflects general gender differences in help-seeking or treatment (e.g., due to stigma). A simple bounding exercise, however, shows that explaining the entire gap through this channel would require Austrian (Danish) mothers to be about 136% (238%) more likely to seek help and receive treatment than equally distressed fathers.[20] Survey evidence from the Special Eurobarometer 246 (Euro-

[18]Because we estimate additional parameters and interaction terms, the confidence bands are wider. However, the parenthood penalty is still significantly positive in both countries.

[19]The raw event-time profiles for men and women are of similar magnitude in Austria and Denmark. However, they mix the effects of parenthood with life-cycle patterns and secular changes in prescriptions. Because age and year drifts differ across genders, we cannot interpret the raw estimates as parenthood penalties.

[20]The calculation assumes that mothers and fathers experience the same underlying deterioration in mental

Table 2: Survey-based evidence on help-seeking behavior among women and men

	Pooled sample		Austria		Denmark	
Respondent is male	−0.018	−0.000	−0.012	0.020	−0.014	−0.009
	(0.051)	(0.050)	(0.071)	(0.071)	(0.073)	(0.071)
MHI-5 score		−0.012***		−0.014***		−0.010***
		(0.002)		(0.004)		(0.004)
No. of observations	345	345	193	193	152	152
Mean of dep. variable	0.304	0.304	0.332	0.332	0.270	0.270
Adjusted R^2	-0.003	0.058	-0.005	0.065	-0.006	0.041

Notes: This table summarizes estimation results from OLS regressions. The estimation sample includes Eurobarometer respondents from Austria and Denmark with an MHI-5 score below 52 points. The dependent variable is a binary variable equal to one if the respondent answers "*yes*" to the following question "*In the last 12 months, did you seek help from somebody in respect of a mental health problem?*" and zero otherwise. The main explanatory variable is a dummy indicating that the respondent was a men. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

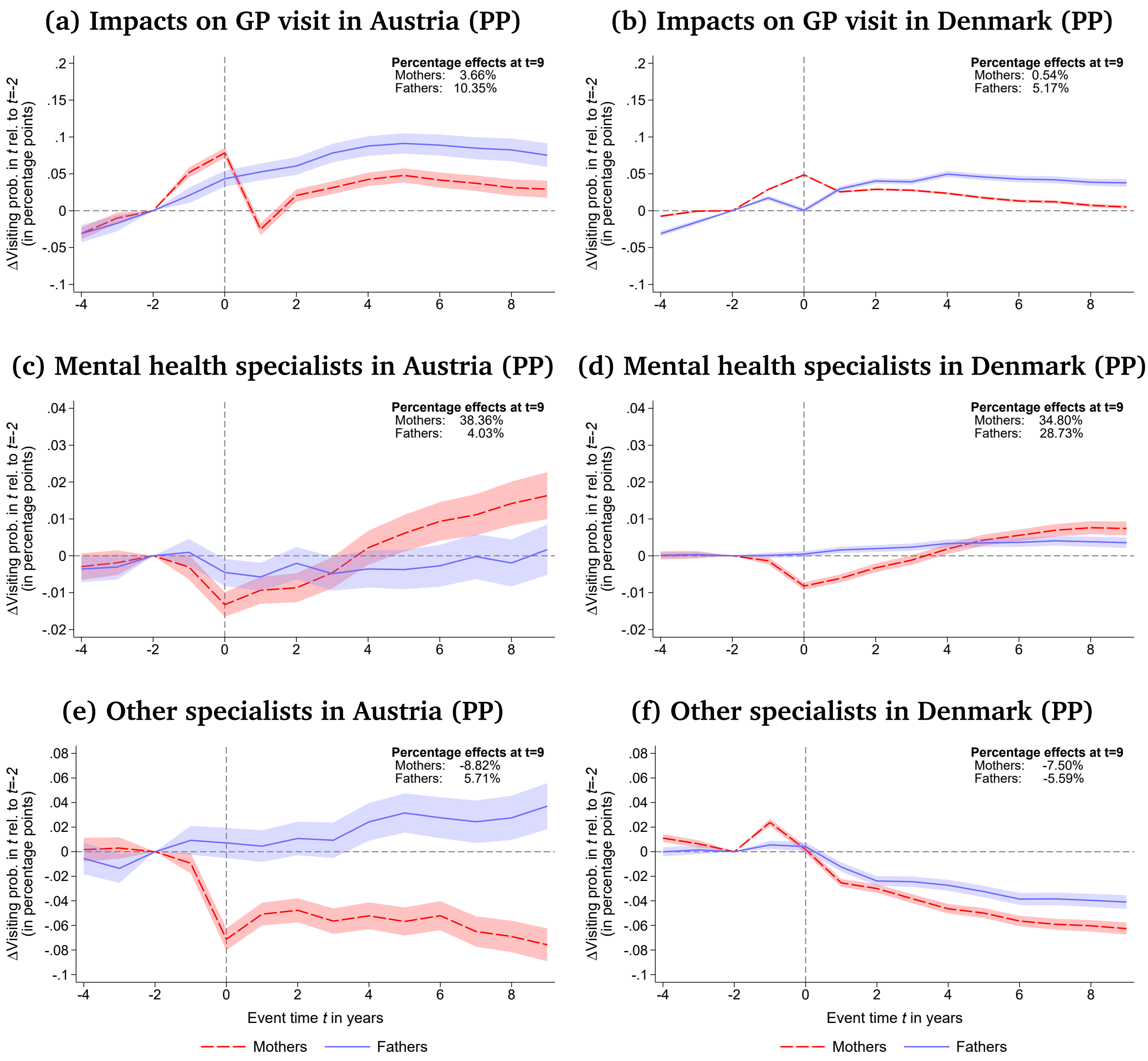
barometer, 2006), which jointly records mental health (MHI-5) and help-seeking, suggests that such enormous gender differences in help-seeking are implausible: Among individuals with poor self-reported mental health (MHI-5 score below 52), women are no more likely than men to report seeking professional help (Table 2; Appendix Figure G.4). There is also no evidence that men substitute professional care with self-medication: measures of substance use-related disorders and prescriptions for drugs used in addictive disorders do not show gender differences (Appendix Figure G.5). Moreover, the fact that the mental health penalties increase with additional children (Figure G.1) also speaks against a help-seeking explanation. To explain such a pattern with help-seeking, mothers would have to become progressively more likely to seek treatment with each birth (conditional on distress). Such parity-specific shifts in help-seeking are, however, difficult to reconcile with plausible mechanisms. Instead, stress and caregiving burdens naturally intensify with parity. Together, these findings suggest that differential help-seeking cannot explain the parenthood penalty.

The second alternative explanation (b) is that the penalties occur because childbirth increases detection among mothers (e.g., by leading to more help-seeking or greater contact with the healthcare system). The evidence also does not support this interpretation. First, after childbirth, mothers' probability of visiting a general practitioner (GP) increases by less than fathers' (Figures 2a and 2b).[21] Thus, the penalties are unlikely to result from mothers

health after childbirth. Then, any observed differences in the effects must come entirely from differences in treatment probabilities. Formally, changes in prescriptions satisfy $\alpha_t^g \approx \kappa_t^g \Delta p_M^g$, where α_t^g is the observed event-study effect for gender $g \in \{m, f\}$, Δp_M^g is the (unobserved) change in mental health, and κ_t^g is the treatment probability. If $\Delta p_M^m = \Delta p_M^f$, then $\kappa_t^m / \kappa_t^f = \alpha_t^m / \alpha_t^f$. Using the observed nine-year effects, this ratio equals $5.0/2.1 \approx 2.36$ in Austria (136% more likely) and $2.7/0.8 \approx 3.38$ in Denmark (238% more likely).

[21]Note that GP visit data from Austria and Denmark are not directly comparable: GPs in Denmark conduct various gynecological examinations, whereas specialists in Austria undertake these procedures. This distinction, with GPs in Denmark performing such examinations, may also explain why the pre-trends differ somewhat for Danish men and women.

Figure 2: Impacts of parenthood on visits of certain physicians



Notes: This figure shows the estimated impacts of parenthood on the probability of a certain outpatient physician visit before and after the birth of the first child (in percentage points). Figures 2a, 2c, and 2e focus on Austria; Figures 2b, 2d, and 2f on Denmark. The dashed lines refer to mothers ($j = m$) and the solid lines to fathers ($j = f$). Figures 2a and Figures 2b show the effects for GPs, Figures 2c and 2d for mental health professionals, and Figures 2e and 2f for other specialists. We obtain the event time coefficients from estimating regression (1) on a balanced sample of parents who have their first child between 2002 and 2007. The shaded areas represent 95% confidence intervals based on robust standard errors. The top-right corner of each figure also shows percentage effects at event time $t = 9$.

increasing their help-seeking with GPs more.[22] More generally, because GPs are the primary prescribers (AT: 84%; DK: 88%), the evidence also speaks against broader increases in help-seeking. Second, postpartum screening cannot explain our results. Denmark offers a nurse home visit program and Austria a well-child program, both of which increase contact with the healthcare sector in the first year after birth.[23] If these programs were driving our find-

[22] Thus, conditional on seeing a GP, mothers' probability of receiving antidepressants is higher post-birth.

[23] In Denmark, the nurse home visit program is available within the first year after childbirth. The first visit

ings, we should see an immediate spike in prescriptions in the first post-birth year. Instead, there is no such immediate spike (Figure 1). Moreover, prescriptions continue to rise even several years after childbirth, when postpartum screening no longer plays a role. Because any antidepressant prescription requires a doctor visit, such programs would also show up as a more substantial rise in mothers' GP visits than in fathers' (Figures 2a and 2b). Third, we find similar penalties for Danish adoptive mothers (Subsection 4.5). Adoptive parents have fewer routine contacts with the healthcare system: they do not experience medical visits linked to the physical consequences of childbirth, and they are not automatically enrolled in the nurse home visit program. Instead, they must actively contact the municipality to initiate support. Consequently, they are also not covered by routine postpartum depression screening. Finding similar penalties for adoptive mothers, thus, strengthens the case that our results do not simply reflect increased detection.

The third alternative explanation (c) is that antidepressant prescriptions are a poor proxy for mental health. To address this, we first examine other outcomes that directly reflect mental health problems. For example, parenthood penalties also appear in the probability of consulting a psychiatrist or neurologist. From about the fourth year after childbirth, this probability is higher for mothers than for fathers (Figures 2c and 2d). Second, we demonstrate that there are no penalties for placebo outcomes. For instance, we find no evidence that mothers' visits to other specialists increase more than fathers' (Figures 2e and 2f). Likewise, parenthood has little effect on prescriptions of other nervous system drugs that are less directly related to mental health problems (Appendix Figures G.6 and G.7). These observations suggest that the penalties are specific to mental health. The only significant penalties occur for psychoanaleptics and analgesics. These are, however, categories that include drugs for depression, anxiety, or bipolar disorder, consistent with mental health effects.[24] In sum, the results indicate that the prescription penalties reflect genuine differences in mental health.

4.5 Parenthood penalties: Effects of giving birth or of having a child?

This section examines two potential explanations for the emergence of parenthood penalties in prescriptions. The first relates to the psychological demands of having and raising a child.

typically takes place four to five days after birth, and the nurse conducts postpartum depression screening during the second month. If the screening indicates an elevated risk, nurses advise mothers to follow up with their general practitioner for further evaluation and support. Austria does not have a comparable structured screening for maternal mental health. Instead, a well-child program (the "Mutter-Kind-Pass") requires pediatric check-ups through the child's fourth year. However, the program's focus is on children, and the program does not include a formal mandatory screening for postpartum depression (Zechmeister-Koss *et al.*, 2025).

[24] One might wonder why we do not observe parenthood penalties in psycholeptics, which are partly used to treat mental health problems. The likely reason is that these drugs are mainly used for acute, short-term symptoms (e.g., insomnia, acute anxiety) and are generally discouraged for long-term treatment because of dependence risks (particularly among women of childbearing age). By contrast, antidepressants are the first-line treatment for persistent mood and anxiety disorders. Our estimates capture sustained changes in mental health after childbirth. Hence, it seems plausible that effects appear in antidepressants but not in psycholeptics.

Parenthood imposes additional childcare responsibilities on mothers (Guryan *et al.*, 2008; Borra *et al.*, 2021), disproportionately disrupts their routines and labor supply (Angelov *et al.*, 2016; Kleven *et al.*, 2019a,b), and may sometimes create a double burden of paid work and childcare. It can, thus, increase mothers' cognitive load and stress (Orchard *et al.*, 2023), strain their relationships, and lower their job satisfaction and income. The second explanation concerns the potential biological effects of giving birth. Only women experience the physical and hormonal changes associated with pregnancy, delivery, and breastfeeding. All these factors likely have immediate short-term effects but may also persistently affect mothers' mental health.[25]

Perhaps the most prominent biological driver that could create adverse effects on mental health is postpartum depression. By definition, this type of depression occurs within four weeks after childbirth but can, in principle, lead to chronic depression (Vliegen *et al.*, 2014; Slomian *et al.*, 2019; Mughal *et al.*, 2022). However, the patterns in Figure 1 are unlikely the result of postpartum depression alone. If only postpartum depression explained the decline in mothers' mental health, the impacts on the prescription probability should spike in the first year after childbirth, with no further increase thereafter.[26] Instead, the estimated adverse effects only emerge after the first child reaches the age of three and increase over time. Our key outcome (antidepressant prescriptions) is arguably also not the most powerful measure of postpartum depression. The reason is that this form of depression occurs shortly after childbirth, when many mothers should not take antidepressants for medical reasons.

Despite these considerations, we conduct two tests to explore whether postpartum depression drives the effects on antidepressant prescriptions. The first check excludes mothers diagnosed with postpartum depression from our analysis. The prescription dynamics and the sizes of the parenthood penalties are unchanged (Appendix Figure G.8). The second test uses a binary indicator for whether person i receives the very first prescription in year s and event time t as an outcome in model (1). This approach allows us to accurately identify the onset of antidepressant therapy, offering a clear distinction between immediate postpartum depressive episodes and mental health issues that emerge or require medication later. Contrary to the hypothesis that postpartum depression is driving our effects, parenthood affects first prescriptions well beyond the first year after birth (Appendix Figure G.9).[27] This result also speaks against the hypotheses that our effects stem solely from postpartum screenings or from birth-related mental-health shocks that lead to drug dependence. In both cases, we

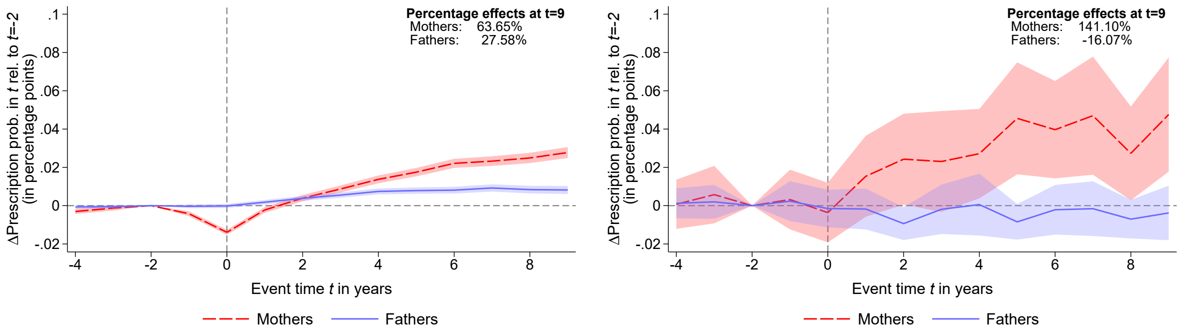
[25]For example, long-lasting postpartum health complications or changes in brain structures and hormonal levels could affect mental health. Indeed, pregnancy and childbirth induce changes in hormones and gray matter linked to maternal attachment (Numan and Insel, 2003; Feldman *et al.*, 2007; Hoekzema *et al.*, 2017).

[26]Increasing effects in later years may either reflect (a) that mothers begin treatment after the typical postpartum depression period or (b) that they experience postpartum depression after having additional children. To exclude the latter possibility, Appendix Figure G.1 focuses on parents with one child. In support of (a), the adverse effect of parenthood in one-child families emerges after two years and grows over time.

[27]We cannot conduct a similar analysis for Austria. The reason is that our health data starts in 1998. Thus, the prescription history is incomplete, and we cannot identify first prescriptions consistently across cohorts.

Figure 3: Impacts of parenthood by adoptive status

(a) Impacts for biological parents (PP) **(b) Impacts for adoptive parents (PP)**



Notes: This figure focuses on Denmark and shows the estimated impacts of parenthood on antidepressant prescriptions by the adoption status (in percentage points). The dashed lines refer to mothers ($j = m$) and the solid lines to fathers ($j = f$). We obtain the event time coefficients from estimating Appendix regression (D.1) on a balanced sample of parents who have their first (adopted) child between 2002 and 2007. We match the sample of adoptive parents to the sample of biological ones (Appendix Section D.1). Moreover, the model interacts the event dummies with an indicator for adopted families and identifies the year and age dummies from the pooled sample. The shaded areas represent 95% confidence intervals based on robust standard errors. The top-right corner of each figure also shows percentage effects at event time $t = 9$.

would expect first prescriptions to peak after birth and slowly fade over time. Instead, the figure demonstrates that most of the new parenthood-induced prescriptions among mothers arise several years after birth. In addition, returning to the argument that prescriptions are unlikely to capture postpartum depression for medical reasons, note that Figures 2c and 2d consider an outcome that does not have this property: the decision to visit mental health professionals. Parenthood also affects mothers' probability of visiting such doctors in the long and not the short run, suggesting that this outcome also captures effects beyond postpartum depression.

The last and most general step of our analysis more systematically seeks to separate the psychological effects of having and raising a child from the biological effects of childbirth. One approach to achieve this goal is to examine parenthood penalties within adoptive families (Kleven *et al.*, 2021). By focusing on adoptions, we break all biological links between mothers and children, ensuring that any remaining penalties are not the result of biology. Although intuitively compelling, this type of analysis faces a key identification challenge. Families with adopted children represent a specific subset of the population. Any potential difference in the parenthood penalty between adoptive and biological parents could then reflect selection rather than biology. Similarly to Kleven *et al.* (2021), we address this challenge with a matching approach that matches the sample of adoptive families to that of biological ones based on a rich set of observables (see Appendix D for details). Another challenge is the sample size. Austrian families rarely adopt children (e.g., 265 cases in 2012), and we cannot identify all of them in the data. Thus, our analysis focuses on Denmark. Using the matching approach, Figure 3 demonstrates that mothers of adopted children face similarly sized parenthood penalties as biological parents. Even if we cut all biological ties, the parenthood penalties arise. This finding suggests that biological effects (such as postpartum depression) are not the crucial driver of the gaps. Similarly, by definition, we can rule out that features of the healthcare system that only target biological mothers (such as postpartum check-ups) or medical concerns about taking antidepressants before or during pregnancy drive the prescription patterns for mothers of adopted children.[28] In sum, as the evidence indicates a limited role for biology, challenges associated with having and raising a child most plausibly drive the penalties.

4.6 Parenthood penalties: Separations and non-employment?

Returning to the full sample of parents, we now examine two of these challenges associated with having and raising a child. The first is relationship strain and separations after childbirth, likely reflecting the stress and role adjustments following birth. The second relates to episodes of non-employment or withdrawal from paid work. Beyond financial losses, being out of work can reduce social interaction, purpose, and sense of achievement. To examine the role of these two channels, we separately quantify the portion of the overall mental health effect attributable to non-cohabiting parents (capturing relationship breakdowns) and non-employed parents (capturing employment interruptions).

To do so, Appendix E introduces a descriptive decomposition framework. For each event time, we estimate the share of the overall effect explained by a given subgroup (e.g., non-cohabiting parents), along with a residual share capturing other factors. A subgroup contributes more when it is larger or when parenthood has stronger effects on its members' outcomes. Importantly, subgroup size can itself change over time (e.g., if parenthood leads to more separations). The subgroup contribution, therefore, reflects not only the effect's intensity within the group but also its evolving share in the population. The residual, instead, represents everything not explained by the subgroup of interest.

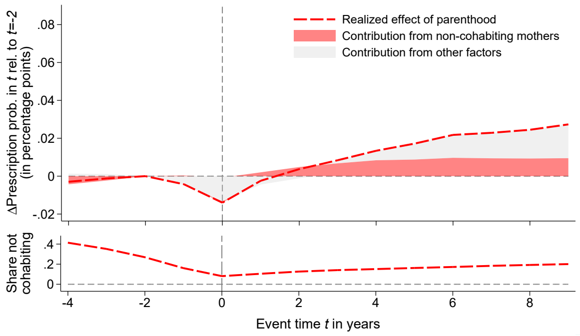
Figure 4 presents the decomposition results. It shows the portion of the overall effects attributed to non-cohabitation (DK: Figures 4a-4b) and non-employment (DK: 4c-4d; AT: 4e-4f). The Austrian data do not contain information on cohabitation status. Each of the Figures 4a-4f consists of two panels: the lower panel displays the subgroup's share in the population over event time, while the upper panel shows the decomposition of the parenthood effects. More specifically, in the upper panel, the color-shaded area represents the contribution of the subgroup of interest (non-cohabiting parents or parents without income). By contrast, the gray-shaded area captures the residual. The bold line additionally shows the

[28] A possible worry is that mothers of biological children avoid antidepressants when attempting to conceive due to medical concerns and return to their usual dosage levels post-delivery. This issue, however, is not relevant for adoptive parents. Moreover, our main effects for biological parents are also unlikely to result from such considerations. First, if true, such a concern should lead to pre-trends in our event studies. Second, our DiD approach suggests that future parents and those without children do not exhibit different pre-event trends. Third, we find similar parenthood penalties for visits to mental health professionals.

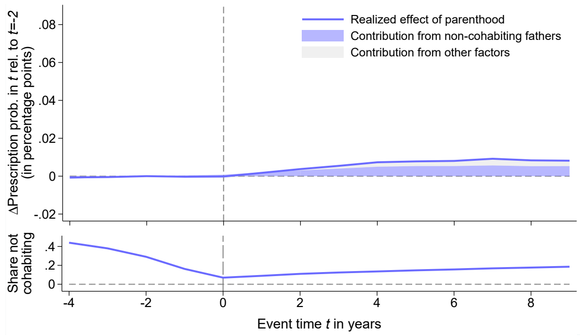
Figure 4: Decomposition of the impacts of parenthood on prescriptions

Impacts for Denmark

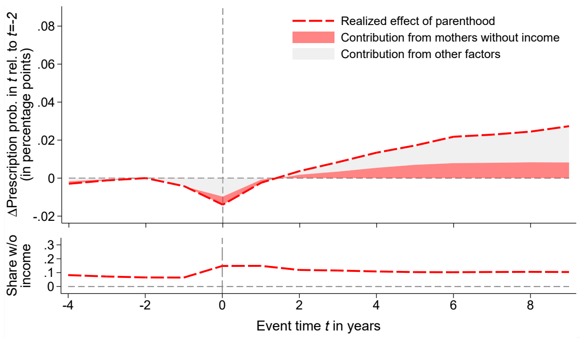
(a) Non-cohabiting mothers (PP)



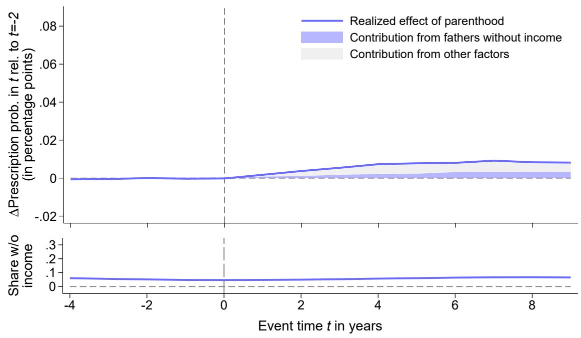
(b) Non-cohabiting fathers (PP)



(c) Mothers without income (PP)

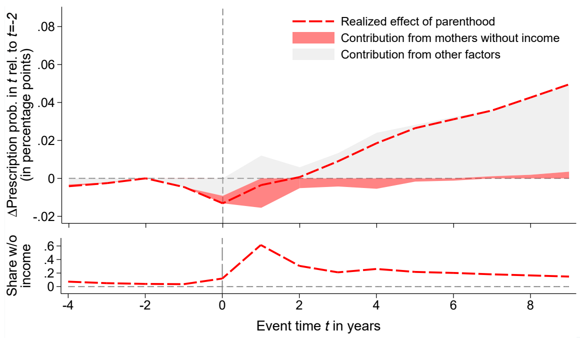


(d) Fathers without income (PP)

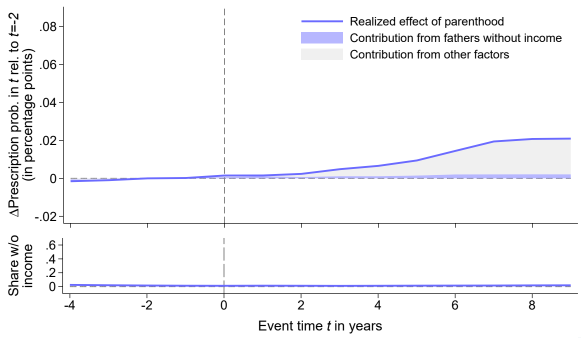


Impacts for Austria

(e) Mothers without income (PP)



(f) Fathers without income (PP)



Notes: This figure decomposes the estimated impacts of parenthood on antidepressant prescriptions (in percentage points). Figures 4a and 4b focus on Denmark and show the results by cohabitation status (non-cohabiting vs. residual), while Figures 4c and 4d focus on a decomposition by income status (without income vs. residual). Figures 4e and 4f present the decomposition for Austria by income. In each figure, the color-shaded area displays the contribution of the indicated (non-cohabiting parents or parents without income), while the gray-shaded area captures the residual components (including contributions of the complementary groups, baseline differences, and cross-period effects). A subgroup's contribution at event time t reflects both the change in the subgroup's antidepressant prescription rate (the within-group effect of parenthood) and the group's share in the population at t. The bold lines show the total effect of parenthood at event time t, which equals the sum of the subgroup contribution and the residual. We can calculate the total impacts from regression (1) on a balanced sample of parents whose first child is born between 2002 and 2007.

overall effect of parenthood at event time t, our baseline estimate, which equals the sum of the subgroup contribution and the residual.

The figure shows that although non-cohabiting and non-employed mothers account for a noticeable share of the overall rise in prescriptions after birth, most of the increase occurs among employed, cohabiting mothers. In Denmark, for example, non-cohabiting mothers are responsible for approximately 35% of the total increase nine years after birth (Figure 4a). The corresponding share among fathers even reaches around 65% (Figure 4b). These shares are notable given that most parents live with a partner in a cohabiting relationship (lower panels). Therefore, non-cohabiting parents appear to experience particularly large increases in prescriptions. The analysis by employment status shows a similar pattern. In Denmark, mothers and fathers without income account for around 30% and 38% of the increase (Figures 4c and 4d). In Austria, the corresponding shares are about 7% and 10% (Figures 4e and 4f).

Appendix E provides complementary analyses that unpack and validate the decomposition results. First, we replicate the decomposition for the percentage effects. Second, we extend the decomposition framework by further splitting the residual into three components: contributions from the complementary group, baseline composition differences, and cross-period spillovers from earlier to later periods. Third, as an alternative to the decomposition approach, we use a simple mediation exercise that controls for potential mediators to assess the extent to which they explain the effect. Specifically, we include dummy variables for having no income and not cohabiting.[29] Across all approaches, the patterns remain consistent: both separations and non-employment account for meaningful parts of the overall rise in prescriptions, but neither can explain it in full.

Taken together, all the results suggest that the broader mental health costs of parenthood extend beyond separations and temporary withdrawal from paid work. These events seem to matter to some extent, but they are not widespread enough to fully explain our effects. Instead, the remaining rise in prescriptions rather reflects broader strains associated with the transition to parenthood or the challenge of combining work and family life.

4.7 Heterogeneity in the effects of parenthood

After analyzing the underlying channels, we now examine which groups of parents are most vulnerable to the mental health consequences of parenthood. To that end, we conduct a heterogeneity analysis by parents' background characteristics. The effects are widespread

[29]The two approaches answer different questions. Our baseline decomposition approach quantifies the contributions of specific subgroups to the observed total effect, using their actual shares and outcome changes. The mediation approach, instead, asks how large the total effect would be if specific mediating channels were fixed. This counterfactual interpretation relies on strong identifying assumptions, in particular that the mediator is conditionally exogenous (i.e., no unobserved factors jointly affect the mediator and the outcome). As this assumption is difficult to justify, and our primary interest lies in describing the composition of the observed effect, we use the decomposition framework as our baseline.

across subgroups but clearly patterned by socioeconomic background. In both Austria and Denmark, parents without tertiary education (ISCED 1 to 4) and those younger than the median age at first birth show markedly larger increases in antidepressant prescriptions than highly educated and older parents (Appendix Figures G.10-G.11). By contrast, child health seems to play little role. Using low birth weight as an indicator for infant health complications (Currie, 2011; Hoynes *et al.*, 2015), we document similar effects among parents of low birth weight children and other parents (Appendix Figure G.12). Finally, we find limited differences by cultural background, using religious denomination as a rough proxy. Specifically, Muslim and non-Muslim parents show comparable trends in percentage estimates (Appendix Figure G.13). However, among Austrians, similar percentage-point effects translate into larger percentage effects because of lower counterfactual prescription rates.

5 Decomposition of the overall mental health gap

A well-established finding in medicine is the existence of a gender gap in mental health problems (see, e.g., Piccinelli and Wilkinson, 2000; Nolen-Hoeksema, 2001; Van de Velde *et al.*, 2010; WHO, 2017; Churchill *et al.*, 2020). For example, depression is roughly twice as common among women as among men in most countries. This section explores the extent to which parenthood explains this overall gender gap in antidepressant prescriptions. Specifically, we decompose the overall gap among parents into a part attributable to parenthood (parenthood-related gap) and a part unrelated to it (residual gap). If the parenthood penalties are well identified, the decomposition has a causal interpretation.

5.1 Decomposition framework

To decompose the overall gap, we follow a three-step procedure (Kleven *et al.*, 2019b).[30] The first step is to calculate the overall gender gap in antidepressant prescriptions for all parents in our dataset (which starts in 1998) as:

$$\Delta = \frac{E[Y_{is}^m] - E[Y_{is}^f]}{E[Y_{is}^f]} \cdot 100, \tag{4}$$

where Y_{is}^j is a dummy variable indicating whether a mother ($j = m$) or a father ($j = f$) received an antidepressant prescription in year s. Notably, we calculate Δ for the entire population of mothers and fathers aged 18 to 55 at birth. The gap Δ consequently measures the percentage by which the average probability of mothers receiving antidepressants exceeds that of fathers.

[30] We deviate from Kleven *et al.* (2019a) in two dimensions. First, because we observe a shorter panel, we do not examine how the decomposition of the overall gap varies over time. Second, given our results for fathers, our decomposition does not impose the assumption of no effect on fathers' mental health outcomes.

In the second step, we estimate the overall gender gap we would have observed among parents if mothers and fathers had never become parents Δ^r (i.e., the residual gender gap). This step requires estimating a counterfactual scenario without children. Our event study models approximate this world: We estimate model (1) separately for mothers m and fathers f, set the event-dummy effects to zero as in Subsection 4.1, calculate the corresponding predicted outcomes $\tilde{Y}_{is}^m$ and $\tilde{Y}_{is}^f$, and compute the residual gender gap unrelated to parenthood as:

$$\Delta^r = \frac{E[\tilde{Y}_{is}^m] - E[\tilde{Y}_{is}^f]}{E[\tilde{Y}_{is}^f]} \cdot 100. \tag{5}$$

The intuition is that, under our identifying assumptions, the predictions $\tilde{Y}_{is}^f$ and $\tilde{Y}_{is}^m$ reflect the mental health outcomes fathers and mothers would have had without children. Consequently, equation (5) approximates the gap among parents in a world without parenthood.

The last step is determining the parenthood-related gender gap among parents Δ^c as:

$$\Delta^c = \Delta - \Delta^r. \tag{6}$$

Thus, we obtain the gap related to parenthood by correcting the overall gap among parents Δ with the gap among parents we would have observed without children Δ^r. If Δ and Δ^r are well identified, the remainder of the overall gap results from parenthood.

Two last points are essential to note. First, we define the overall gap (4) for all parents with firstborn children through 2007, rather than considering our balanced sample of parents with births between 2002 and 2007. We do this to capture the broadest possible measure of gender inequality in prescriptions. However, to accurately decompose this gap, we need counterfactual predictions for all parents, including those with first children born before 2002. Otherwise, we cannot consistently compute (5) and (6). For these predictions, we follow Kleven *et al.* (2019a) and estimate model (1) on an unbalanced sample of all parents that, in our case, encompasses all parents with firstborn children before 2007. Second, as we use this alternative sample definition for our decomposition analysis, the reported levels of the gaps in Table 3 differ from those shown before (e.g., in Table 1). Moreover, we previously reported average values for our baseline period ($t = -2$); we now consider all event periods.

5.2 Decomposition results

Table 3 presents all the necessary ingredients for the decomposition analysis. Panel A focuses on Austria, and Panel B on Denmark. Several features stand out. First, in both countries, mothers are, on average, much more likely to receive antidepressants than fathers. Specifically, in Austria, about 6.4% of mothers and about 3.5% of fathers receive antidepressants.

Table 3: Decomposition of the overall gender gap in antidepressant prescriptions

	Actual share of individuals who receive antidepressants		Predicted share of individuals who receive antidepressants when having no children
A. Austria			
Mothers	6.36%		4.98%
Fathers	3.50%		3.32%
Overall gender gap		81.50%	
Residual gender gap		50.72%	
Parenthood-related gender gap		30.78%	
B. Denmark			
Mothers	4.45%		3.06%
Fathers	2.44%		1.89%
Overall gender gap		82.40%	
Residual gender gap		61.50%	
Parenthood-related gender gap		20.90%	

Notes: This table presents all the ingredients for the decomposition analysis. Panel A focuses on Austria, and Panel B on Denmark. The table shows the average shares of mothers and fathers who receive antidepressants, the predicted shares we would have observed if mothers and fathers had never become parents, the overall gender gaps in antidepressant prescriptions among parents that follow from equation (4), the predicted residual gender gaps among parents that we calculate using equation (5), and the parenthood-related gender gaps we obtain from equation (6). We calculate all values for an unbalanced sample of all parents with firstborns before 2007.

In Denmark, the figures are 4.5% and 2.4%, respectively. Second, the resulting overall gender differences in antidepressants are large. The probability of antidepressant prescription among Austrian mothers is about 81.5% higher than among Austrian fathers. The corresponding gap in Denmark is 82.4%. Third, we predict that in a hypothetical scenario where mothers were not parents, only about 5.0% (3.1%) of all Austrian (Danish) mothers would receive antidepressants. The figures for Austrian (Danish) fathers are 3.3% (1.9%). Fourth, we estimate that the resulting residual gender gap in a world without children amounts to about 51% in Austria and 62% in Denmark. Fifth, the corresponding parenthood-related gender gap is roughly 31% (Austria) and 21% (Denmark). These numbers imply that parenthood boosts the gender gap in antidepressant prescriptions among parents substantially. Policymakers concerned about mental health equity should think carefully about the role of children. They might even design reforms to address the parenthood-related part of the gender gap in mental health.

6 The role of parental leave

Parental leave policies are one potential measure to reduce parental stress and, consequently, promote gender equality. The rationale is that longer leave may ease the burden of parenthood, particularly for mothers (e.g., by allowing them a smoother transition into parental responsibilities). This section, however, shows that parental leave expansions, resulting in prolonged maternity leave, backfire: they further deteriorate mothers' mental health and boost the parenthood penalties.

6.1 The Austrian 2000 parental leave reform

Austria enacted several reforms to its parental leave policy. We next focus on the 2000 parental leave extension and discuss other Austrian and Danish reforms later in the paper. Several features make the 2000 reform particularly well-suited for identifying whether longer maternal leave causally affects mothers' mental health and parenthood penalties. First, in the 2000s, almost exclusively mothers took parental leave. Second, the reform expanded the maximum paid maternity leave duration from 18 to 30 months (Figure 5). Almost all first-time mothers qualified for the program, and the take-up among eligible mothers was nearly 100% (Subsection 6.3). Third, all the other aspects of the parental leave system, such as job protection or parental benefits, remained unchanged.[31] The reform, therefore, allows us to estimate the pure effect of changing the leave duration. Fourth, fathers did not change their leave-taking behavior in response to the reform.[32] Fifth, and crucially for our empirical design, the reform implemented a strict birthdate cutoff, determining eligibility for extended parental leave without any transition rule. In particular, mothers who delivered their child on or before June 30 qualified for 18 months of leave, while those who gave birth on or after July 1 could take 30 months. This feature enables us to use regression discontinuity designs (RDDs) for identification, relying on the first child's birthdate as the assignment variable. Sixth, the government announced and implemented the reforms on short notice. Selection into treatment is, therefore, not an issue.

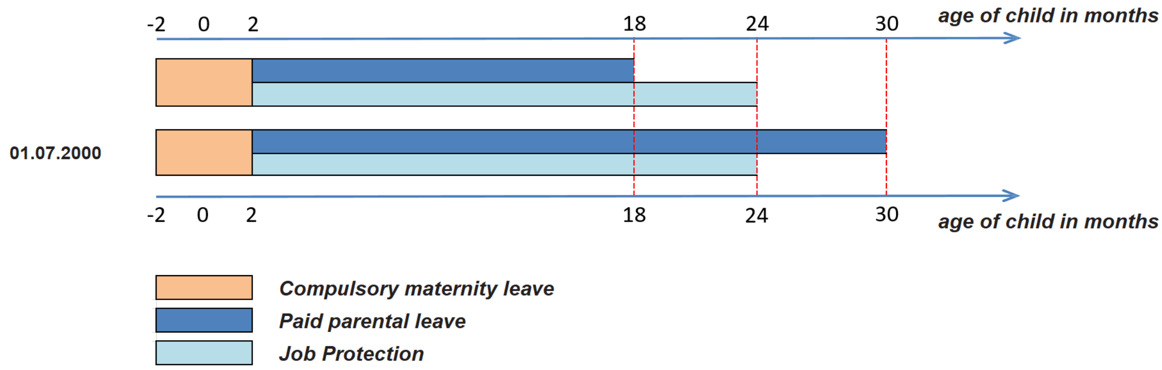
6.2 Regression discontinuity approaches

Our regression discontinuity design exploits the fact that, in a narrow window around the reforms' implementation dates, the first child's birthday exogenously assigns mothers to different parental leave regimes. The birthday, therefore, serves as a local treatment assignment variable with strong (but not perfect) predictive power for the actual paid maternity

[31] Women received a flat payment (adjusted for wage inflation) over the entire leave period. Until 2008, these payments were unrelated to prior earnings or the length of the leave.

[32] In principle, fathers might also have responded. The reason is that the reform expanded the total maximum leave period taken by both parents from 24 to 36 months. One parent could not take more than 30 months. However, only 3% of all fathers took parental leave, and the reforms did not affect this decision.

Figure 5: The Austrian 2000 parental leave reform



Notes: This figure summarizes the key aspects of the Austrian parental leave reform. Since 1990, parents could share parental leave. In practice, the take-up of fathers was virtually zero. Before 2000, parental leave was 24 months. However, one parent could not take more than 18 months, bounding maternity leave at 18 months. The 2000 reform increased the maximum duration of parental leave to 36 months. Now, one parent could not take more than 30 months. Thus, the reform effectively increased maternity leave from 18 to 30 months. The reforms also introduced a strict birthdate cutoff (July 1), determining regime eligibility.

leave duration. We use two approaches to quantify the effects of this quasi-experimental variation in maternity leave on mental health.

First approach, reduced-form plots: As a first approach, we visualize the relationship between several mental health measures and the assignment variable in reduced-form plots to detect potential discontinuities at the cutoff. Our estimation strategy closely follows Danzer *et al.* (2022) and accounts for unobserved outcome characteristics that follow a time-invariant seasonal birth pattern over a year (seasonality). To that end, we include mothers with births in the same calendar months in the pre-reform year as control cohorts in our analysis. Formally, our reduced-form plots visualize the treatment effects by depicting the fitted regression lines of triangular-weighted ordinary-least-squares regressions of the form:

$$\begin{aligned} Y_i \;=\; & \alpha_0 + \alpha_1 \cdot \mathbb{1}[After_i] \;+\; \alpha_2 \cdot Run_i \;+\; \alpha_3 \cdot Run_i \times \mathbb{1}[After_i = 1] \\ & + \alpha_4 \cdot \mathbb{1}[Treat_i = 1] \;+\; \alpha_5 \cdot \mathbb{1}[Treat_i = 1] \times \mathbb{1}[After_i = 1] \;+\; \mathbf{X_{i,t=0}}\delta' \;+\; u_i, \end{aligned} \tag{7}$$

where Y_i is a measure for mother i's mental health in the post-birth period, $\mathbb{1}[After_i = 1]$ denotes if the first child was born after the reform's birthdate cutoff (June 30), and $\mathbb{1}[Treat_i = 1]$ indicates if a mother belongs to the treatment cohort (birth in 2000) or the control cohort (birth in 1999). The first child's birthdate (measured in days) is the running variable Run_i (centered at the cutoff). The regression also includes a potential vector of controls, $\mathbf{X_{i,t=0}}$.[33] Intuitively, α_3 measures the jump in the outcome at the cutoff in the

[33]In the full specification, the vector comprises age dummies that measure the mother's age in the first available post-birth year, the child's legitimacy status, a dummy controlling for the child's sex, maternal education

pre-reform year, and α_5 identifies the additional jump in the reform year. The estimate $\hat{\alpha}_5$ causally identifies the effect of being assigned to the new regime when there is no sorting into treatment, and a typical parallel-trends assumption holds.

Second approach, fuzzy regression discontinuity designs: Our second approach is to estimate a fuzzy regression discontinuity design via two-stage least squares (2SLS) to obtain local average treatment effects (LATEs). This estimation strategy instruments the actual maternity leave duration (i.e., our endogenous treatment variable) with treatment assignment. The 2SLS model again uses a control cohort (Danzer *et al.*, 2022). Thus, its first stage reads:

$$\begin{aligned} ML_i \;=\; & \beta_0 + \beta_1 \cdot \mathbb{1}[After_i] \;+\; \beta_2 \cdot Run_i \;+\; \beta_3 \cdot Run_i \times \mathbb{1}[After_i = 1] \\ & + \beta_4 \cdot \mathbb{1}[Treat_i = 1] \;+\; \beta_5 \cdot \mathbb{1}[Treat_i = 1] \times \mathbb{1}[After_i = 1] \;+\; \mathbf{X_{i,t=0}}\delta' \;+\; u_i, \end{aligned} \tag{8}$$

and the second stage is:

$$\begin{aligned} Y_i \;=\; & \gamma_0 + \gamma_1 \cdot \mathbb{1}[After_i] \;+\; \gamma_2 \cdot Run_i \;+\; \gamma_3 \cdot Run_i \times \mathbb{1}[After_i = 1] \\ & + \gamma_4 \cdot \mathbb{1}[Treat_i = 1] \;+\; \gamma_5 \cdot \widehat{ML}_i \;+\; \mathbf{X_{i,t=0}}\delta' \;+\; u_i. \end{aligned} \tag{9}$$

The dependent variable in the first-stage equation, ML_i, reflects the actual maternity leave duration measured in years. The second stage regresses a measure of mother i's mental health (Y_i) on the predicted maternity leave duration from the first stage $\widehat{ML}_i$. The estimated coefficient of interest is $\hat{\gamma}_5$. Under our identifying assumptions, it identifies the causal effect of an additional year of maternity leave by being assigned to the new regulations. We provide this coefficient for different covariate specifications (no controls versus the complete set of controls) and different weighting schemes (unweighted versus triangular weights).

6.3 Effects of the Austrian 2000 reform on mental health

Our analysis proceeds in several steps. We first study whether the Austrian 2000 reform affected mothers' leave taking behavior and subsequently discuss the effects on mental health. We continue with studying the timing of the reform effects and examining spillovers on fathers. The subsection concludes by discussing the effects of other parental leave reforms.

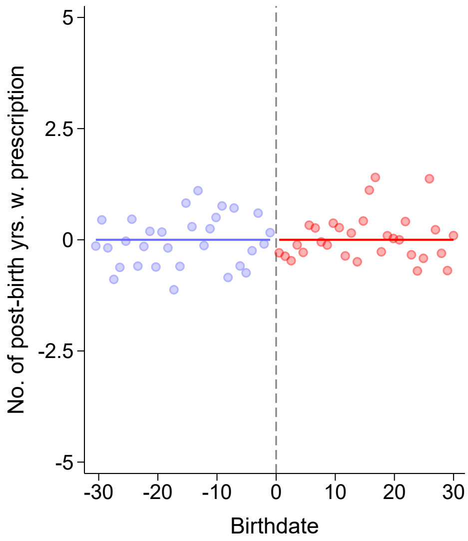
Effects on mothers' leave-taking behavior: To lay the groundwork for further analyses, we first show that the reform strongly affected mothers' actual leave taking behavior. As evidenced by a sharp discontinuity in leave length at the cutoff (Appendix Table G.1 and Appendix Figure G.14), eligible mothers take, on average, about nine more months of leave due to the reform. Given that the reform increased the maximum available leave by 12 months, it triggered a substantial behavioral response.

dummies, a dummy indicating whether the child is born preterm, and a dummy indicating whether the mother was born abroad.

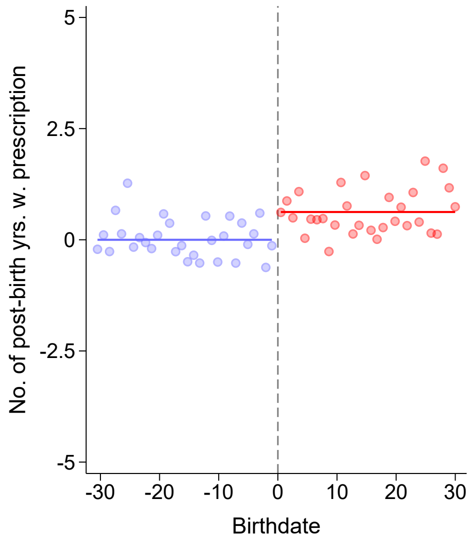
Figure 6: Reduced-form impacts of the Austrian 2000 reform on maternal mental health

Impact on the number of post-birth years with a prescription

(a) Pre-reform year 1999

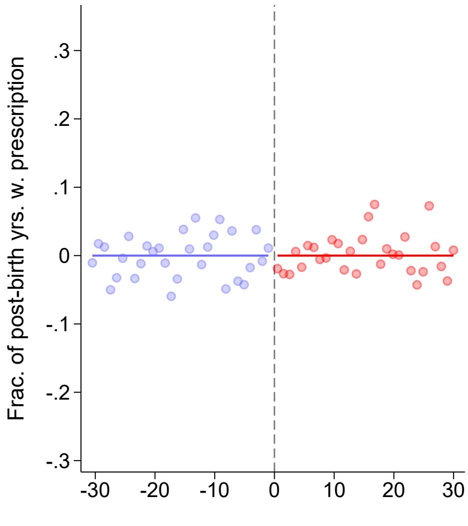


(b) Reform year 2000

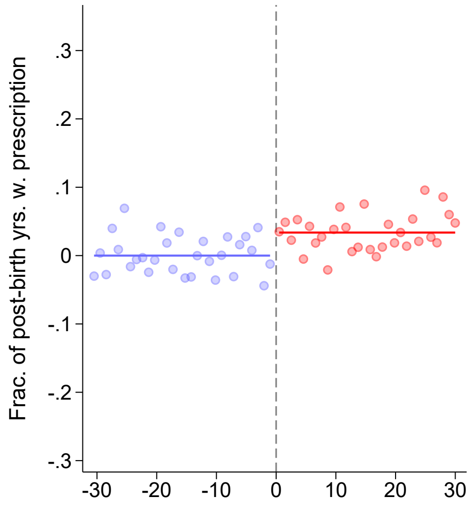


Impact on the fraction of post-birth years with a prescription

(c) Pre-reform year 1999



(d) Reform year 2000



Notes: This figure shows the reduced-form impacts of the 2000 reform on the number of post-birth years with antidepressant prescriptions (Figures 6a and 6b) and the fraction of post-birth years with antidepressant prescriptions (Figures 6c and 6d). For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side). Each circle represents an average for a particular day. The vertical line refers to the cutoff (July 1). The figures are covariate-adjusted. We adjust for covariates by (a) estimating the model (7), (b) setting α_5 to zero, (c) predicting the outcome $\hat{Y}_i$ for $\alpha_5 = 0$, (d) calculating the residual as $Y_i - \hat{Y}_i$, and (e) plotting the residuals. This procedure factors out trends along the running variable and pre-reform jumps at the cutoff. The post-birth sample runs from 2000 to 2016. The estimates rely on triangular weights and include the following covariates: The mother's age, the child's sex, the child's legitimacy status, maternal education dummies, a dummy indicating whether the child is born preterm, a dummy indicating whether the mother was born abroad.

Effects on mothers' mental health: Next, we examine how this change in leave-taking affects mothers' post-birth mental health. To reduce the complexity of our analysis, we start by studying the effects on post-birth mental health over the entire post-reform period (ignoring the timing of the reform effects for now). Specifically, we create two individual-level outcome variables that aggregate all the available information on antidepressant prescriptions issued to mothers after childbirth and throughout the years following the reform. In effect, this means that our outcomes rely on post-birth prescription data from 2000 (when the reform took place) to 2016 (when our sample ends). The first of the two outcome variables counts the number of these post-birth years in which a given mother receives prescriptions. The second outcome instead calculates the fraction of these years with prescriptions. As a robustness check and for ease of exposition, we also report average effects on prescription probabilities (which requires a more complicated panel estimation approach). Importantly, the analyses based on these outcomes capture impacts on mothers' post-birth mental health across the entire post-reform period (including short-, medium-, and long-run effects).

Figure 6 then uses these two variables as an outcome of regression (7) to derive covariate-adjusted reduced-form RDD plots (first row: counts; second row: fraction).[34] For intuition, we also provide placebo plots for the pre-reform year (Figures 6a and 6c) alongside the main plots for the reform year (Figures 6b and 6d). Appendix Table G.2 reports the corresponding reduced-form estimates, and Table 4 summarizes the LATE estimates for three specifications. One uses triangular weights and omits covariates, the second omits weights and covariates, and the last combines triangular weights with covariates.

Across all estimation methods and outcomes, a consistent pattern emerges: Mothers who are quasi-experimentally assigned to take longer leaves are significantly more likely to receive antidepressant prescriptions after the first child's birth. Specifically, these mothers obtained antidepressants for an average of seven additional post-birth months between 2000 and 2016 (Figure 6b and Appendix Table G.2). In other words, an additional year of leave causes between 0.63 and 0.82 more years with prescriptions (Table 4). This value corresponds to an increase of at least 92%. The estimated impacts of one more year of leave on the fraction of years with prescriptions are also substantial. They range from 3.5 to 4.5 percentage points (Table 4). The Appendix also discusses how the reform affects the annual probability of receiving an antidepressant prescription after birth. To that end, we expand our analysis to a panel framework. Here, we use an indicator for whether mother i receives a prescription in post-birth year t as the outcome. We find that, in each post-birth year, affected mothers are, on average, 3.1 to 3.9 percentage points more likely to receive antidepressants (Appendix Table G.3 and Appendix Figure G.15). This number corresponds to a considerable increase of about 77%. Appendix Table F.3 presents the corresponding LATEs.

[34] We adjust for covariates by estimating the model (7), setting α_5 to zero, predicting the outcome $\hat{Y}_i$ for $\alpha_5 = 0$, calculating the residual as $Y_i - \hat{Y}_i$, and plotting the residuals. This procedure factors out trends along the running variable, and pre-reform jumps at the cutoff.

Table 4: Impact of maternity leave on mental health (LATEs)

	Triangular	Unweighted	Covariates
A. Impact on the number of post-birth years with a prescription			
Years of maternity leave	0.7736**	0.6271**	0.8245***
	(2.4578)	(2.2430)	(2.6354)
Mean of outcome		0.6819	
B. Impact on the fraction of post-birth years with a prescription			
Years of maternity leave	0.0410**	0.0350**	0.0445***
	(2.5128)	(2.1186)	(2.7735)
Mean of outcome		0.0455	
Observations		1,901	

Notes: This table provides LATE estimates of the 2000 parental leave reform in Austria. The outcome is the number of post-birth years with an antidepressant prescription (Panel A) or the fraction of post-birth years with an antidepressant prescription (Panel B). The estimates rely on a bandwidth of 30 days. The first column uses triangular weights, the second column does not use any weighting, and the third column combines triangular weights with covariates. We use the following covariates: Mother's age, the child's sex, the child's legitimacy status, maternal education dummies, a dummy indicating whether the child is born preterm, and a dummy indicating whether the mother was born abroad. t statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

The estimates are notable because they reflect outcomes across the full span of post-birth years, including those long after the reform.

Timing of the reform effects: The Appendix also provides supplementary analyses documenting the timing of the reform effects. Our empirical approach relies on segmenting the postnatal years into several periods and estimating separate period-specific models. We use quarterly data for that purpose to get a more detailed picture.[35] Appendix Table G.4 summarizes our results. It focuses on the impacts of the reform on the fraction of post-birth quarters with antidepressant prescriptions.[36] Three findings emerge: First, for the first 18 postnatal months (6 quarters), we find insignificant reform effects very close to zero (Panel A). Because the 2000 reform neither changed the right to take leave during the first 18 postnatal months nor the associated parental benefits, this finding is intuitive. Second, in line with the hypothesis that parenting is stressful, the table provides suggestive evidence that the first adverse effects on mental health already emerge during the expanded leave

[35] Annual data would only allow us to conduct a coarser analysis. Such data, for example, prohibit analyzing whether mothers already develop depression during the expanded leave period (the extended leave period sometimes spans multiple years).

[36] Appendix Table G.5 reports the impacts of the reform on the number of quarters with prescriptions. The results are identical. The drawback of this outcome is that the estimates are not directly comparable across periods of different lengths. Longer periods naturally affect the number of prescriptions and the effects' size.

period (when affected mothers are still on leave). The point estimates for the extended leave period (months 19 to 30 after birth) are much higher than those measured for the first 18 months. However, they are statistically significant only in specifications with covariates. Third, the adverse effects of the reform on mental health manifest over time. We find significant and somewhat larger point estimates for the immediate post-maternity leave period (Panel C). The adverse effects even persist for 7.5 to 16 years after birth (Panel D). In sum, affected mothers even experience worse mental health after the expanded leave period ends (when many of them work again).

Reform evidence and effect decomposition: At first glance, the decomposition in Figure 4 might appear inconsistent with the reform evidence. The decomposition shows that non-employed mothers contribute only a small share to the overall rise in antidepressant prescriptions in Austria. In contrast, the reform analysis finds that longer periods out of work worsen mothers' mental health. The two results, however, describe different perspectives. The decomposition provides a descriptive breakdown of the total increase in prescriptions after childbirth. The reform analysis, in contrast, causally identifies what happens when mothers spend more time outside of paid work immediately after childbirth. Most of the affected mothers later return to employment, and their mental health problems persist and fully manifest after re-entry. As a result, the decompositions capture the reform-induced rise in prescriptions mainly among employed women. Taken together, the results imply that while withdrawing from work can be individually harmful, it is not prevalent enough to account for the overall adverse effect on mothers' mental health.

Effects on long-term earnings and labor supply: When interpreting the results, another crucial question is whether the reform also affected long-term earnings and long-term labor supply. If it did, the reform effects on mental health could work through these channels. Previous papers have documented the impacts of the Austrian reforms on maternal labor supply, fertility, and child outcomes (Lalive and Zweimüller, 2009; Lalive *et al.*, 2014; Danzer *et al.*, 2022). Most notably, Kleven *et al.* (2024) demonstrated that the 2000 reform affected mothers' earnings and labor supply only during the leave period, with no long-term effects after the leave. Consequently, the potential long-term effects on mental health are unlikely to operate through changes in earnings and labor supply.

Effects on fathers' mental health: Although the reform did not directly affect fathers' leave-taking behavior, it may indirectly influence them through changes in their partners' choices. For example, extended maternity leave may change household dynamics, parenting responsibilities, or stress levels for both mothers and fathers. All of these changes could, in turn, impact fathers' mental health. We need to explore these spillover effects to gain a complete perspective on the implications of parental leave policies for gender equality in mental health. We, therefore, re-run our analysis for fathers. The key conclusion is that the 2000 reform did not affect fathers' mental health. The reduced-form figures do not reveal any discontinuity at the cutoff across outcomes (Appendix Figures G.16 to G.18). Moreover, the underlying reduced-form regression analysis provides economically and statistically insignificant estimates (Appendix Tables G.3 and G.6). These results show that the reform did not significantly impact fathers despite the potential for spillovers from changes in mothers' behavior. The lack of impact on fathers highlights the gendered nature of the effects of parental leave extensions.

Discussion: In sum, the analysis offers several insights. First, it demonstrates that maternity leave extensions do not reduce but increase the mental burden for mothers. Second, given the lack of impact on fathers and the adverse effects on mothers, the parenthood penalty in mental health even increases with the length of maternity leave. Extended maternity leave periods can, therefore, not serve as a silver bullet to promote mental health equality. Instead, such reforms can backfire and reinforce gender inequality. The reform increases mothers' average yearly prescription probability by 3.1 to 3.9 percentage points, while leaving fathers' mental health unaffected. Third, the evidence is consistent with the idea that childcare investments and periods of pure parenting are psychologically taxing. We find that mothers who invest more exclusive time in childcare in a quasi-experimental setting face more severe mental health problems. Moreover, the reform did not affect other aspects of the leave schemes, fathers' mental health, and mothers' long-run labor supply and earnings. Hence, its effects cannot work through these alternative channels. Fourth, the adverse effects on mothers persist even in the long run. Such adverse long-run effects could result from a combination of factors. They, for example, resonate with the notion that leave extensions act as a trigger for prolonged mental health challenges, for example, by lengthening the stressful period of exclusive caregiving. Indeed, one-time events and periods of stress can permanently impact mental well-being (see, e.g., Brown and Harris, 1978; Monroe and Harkness, 2005; Stroud *et al.*, 2011). On top of that, an extended maternity leave may reinforce mothers' roles as primary caregivers. Such a change likely increases women's long-term workload and mental burden, particularly for working mothers who have to juggle the dual responsibilities of their careers and childcare. The reported effect dynamics suggestively align with this idea (Appendix Table G.4): The effects increase in the immediate post-maternity leave period, coinciding with the time many mothers re-enter the workforce. The decomposition analysis is also consistent with a double-burden interpretation. Most of the increase in prescriptions occurs among employed mothers who combine paid work with childcare responsibilities (Figure 4).

6.4 Effects of other parental leave reforms

The Austrian and Danish governments implemented additional parental leave reforms in 1990, 1996, and 2002 (described in Appendix F). Due to the institutional and data-related challenges discussed in Appendix F, these reforms offer less clear-cut experiments for assessing their impacts on mental health. We, nevertheless, study these reforms' effects using regression discontinuity designs and reach a general conclusion: All the reforms that considerably shifted mothers' actual leave-taking behavior (by eight or ten months) had substantial adverse impacts on their long-term mental health but no effects on fathers (including the Austrian 2000 reform). Note that policymakers implemented these reforms in contexts where statutory leave periods were already long before the reform. Instead, moderate reforms that shifted actual maternity leave by a shorter period (one or four months) or expanded leave starting from a shorter baseline period did not cause such adverse effects (see Appendix F.3 for detailed results). Staying a few months longer on leave does not matter much, while greater behavior shifts heavily influence mothers. These results solidify our previous conclusion that higher childcare investments in the form of substantially longer maternity leaves worsen mothers' mental health.

7 Conclusion

This paper examines the impact of parenthood on gender gaps in mental health in Austria and Denmark. Using administrative data and event study designs, we find that parenthood imposes a substantial and persistent mental health burden on mothers relative to fathers. Specifically, the percentage increase in antidepressant prescriptions due to parenthood in Austria (Denmark) is about 98.6 (34.8) percentage points higher for mothers than for fathers. These gaps are unlikely to reflect biology, relationship breakdown, or non-employment. Instead, they are more likely related to the demands of having and raising children. In particular, we find that adoptive mothers experience similar penalties, and that cohabiting, employed mothers account for most of the average increase in prescriptions.

Our work also demonstrates that the mental health penalty is rather a resilient phenomenon that extends beyond a single country: the parenthood penalties in both Austria and Denmark are substantial and account for a large share of the overall gender gap in mental health. We conclude that mothers bear a disproportionate part of the mental health costs of raising children. While prior work has documented hefty parenthood penalties in earnings (e.g., Kleven *et al.*, 2019b), our results show that these penalties extend beyond the labor market to the mental health domain.

Given these results, policymakers may call for interventions such as longer parental leave. Yet additional quasi-experimental evidence reveals that such extensions intensify rather than alleviate mothers' mental health problems. One reason may be that they reinforce caregiving responsibilities well beyond the leave period, increasing mothers' overall load once they return to work. Improving equity, therefore, requires alternative approaches. Promising candidates include earmarked paternity leave or shared leave in the immediate postpartum period (Persson and Rossin-Slater, 2024). These policies could help redistribute caregiving responsibilities more evenly between parents and thereby may ease mothers' disproportionate mental health burden.

Finally, note that by focusing on outcomes such as antidepressant prescriptions, our paper deliberately studies the adverse mental health effects of parenthood. These effects are economically important and widespread. However, not all parents experience depression, and for some, parenthood certainly brings psychological benefits and purpose. Therefore, future research should consider both the adverse and the potentially positive effects of parenthood, as well as their heterogeneity across parents and contexts. Our paper is also silent on the extent to which mothers' mental health penalties contribute causally to the earnings penalties associated with parenthood, and on how policies beyond parental leave affect mental health equality. Therefore, much research remains to be done, and we hope our study contributes to a broader agenda on the multifaceted effects of parenthood.

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Web Appendix

This Web Appendix provides additional material discussed in the unpublished manuscript “The Parenthood Penalty in Mental Health” by Alexander Ahammer, Ulrich Glogowsky, Martin Halla, and Timo Hener.

A Institutional background: Additional details

This appendix section provides additional details on the institutional context introduced in Section 2 of the paper. In particular, we expand on similarities that make results across Austria and Denmark comparable, as well as differences that allow us to test whether parenthood penalties are universal. We document these aspects in more depth by discussing fertility patterns, healthcare systems, mental healthcare and utilization, labor markets, family policies, and gender identity norms.

A.1 Healthcare systems

Austria and Denmark both provide universal access to high-quality healthcare, yet they do so through different institutional models. Austria follows a social health insurance approach (a *Bismarck* model), with financing based primarily on compulsory contributions linked to employment and occupational status. The *Austrian Health Insurance Fund* (*Österreichische Gesundheitskasse*, ÖGK) covers about 82% of the population, including most employees, their dependents, and non-employed residents.[1]

Denmark, by contrast, operates a tax-financed system (a *Beveridge* model). Funding comes largely from national and municipal tax revenues, which are pooled and allocated to regions and municipalities that provide health services. All residents are automatically covered under this system.

Organizational differences also show up in hospital care: Austria maintains significantly more hospital capacity (around 8 beds per 1,000 residents) than Denmark (3.4).

Despite these structural differences, both systems provide comprehensive coverage of services, including sickness and maternity care, ensuring that our measures of (mental) health outcomes are comparable. Both have adopted the WHO Anatomical Therapeutic Chemical (ATC) classification system, which guarantees consistency in prescription coding across countries. As Table A.1 shows, spending levels and major health outcomes (such as infant mortality and life expectancy) are also very similar, providing a solid basis for cross-country comparison.

A.2 Mental health and mental healthcare utilization

We next consider mental health status and service use. Data from the *Special Eurobarometer* 246 (2005/06) provide comparable measures across the two countries.

First, mental health scores based on the five-item *Mental Health Inventory* (MHI-5) are close: Austrian respondents average 67.5, compared to 71.3 in Denmark, a difference of about one-fifth of a standard deviation. Second, help-seeking rates are similar: 15% of Austrians and 17% of Danes reported consulting a professional about psychological or emotional health problems in the past year. Third, suicide rates among women are nearly identical at around 7 per 100,000 population, though male suicide is more frequent in Austria (24 vs. 20).

The survey also sheds light on treatment pathways. About 80% of respondents who sought help did so from a general practitioner, underscoring the GP's role as the primary

[1]The remaining 18% are covered by other statutory providers (civil servants, miners, federal railway employees, self-employed, freelance professionals, and farmers). Most public sector employees are not civil servants and, thus, insured with the ÖGK.

Table A.1: A comparison of the Austrian and Danish health care systems

	Austria	**Denmark**
Health expenditures		
Total expenditures as % of GDP†	9.9	9.6
Out-of-pocket expenditures as % of total†	18.9	14.3
Doctors and hospital beds (per 1,000 population)		
All physicians†	4.6	3.6
GP‡	1.6	0.8
Psychiatrists‡	0.17	0.19
Hospital beds†	7.6	3.4
Mortality & life expectancy†		
Infant mortality (per 1,000 live births)	3.6	3.8
Life expectancy at birth (in years)	80.4	79.2
Suicide rates (per 100,000 population)§		
Men	24.09	19.59
Women	7.04	7.55

Notes: † Average over 2000–2019/20, World Bank. ‡ Average over 2000–2019, OECD. § Average over 2000–2017, age standardized, from Ritchie *et al.* (2022).

entry point for mental healthcare in both systems. GPs are authorized to prescribe antidepressants and often manage mild to moderate conditions without referral. Treatment distribution is also broadly similar: around 10% of Austrians and 7% of Danes reported medication use, 3–4% psychotherapy, and 1–2% hospitalization due to psychological problems.

Together, these data confirm that both the prevalence of mental health issues and the structure of treatment are comparable across the two countries, ensuring that observed differences in our analysis are not driven by divergent diagnostic or coding practices.

A.3 Labor markets

Turning to labor markets, Austria and Denmark have long had similar male participation rates, but women's attachment has differed markedly. By 2021, women aged 25–54 were slightly more likely to participate in Austria (85.1%) than in Denmark (82.9%). However, Austrian women's greater participation is driven largely by part-time work: one in three Austrian women (33.1%) worked part-time versus one in four in Denmark (23.1%).

This difference is particularly pronounced after childbirth. While Danish mothers typically maintain full-time hours, Austrian mothers more often re-enter employment part-time, usually as secondary earners in the household. In Austria, among women with at least one child aged 0–14, the employment rate in 2021 is 76.4%, and 41% of employed mothers work part-time, whereas in Denmark maternal employment is 81.8% with only 9% part-time (see Table A.2). The consequences for long-run earnings are substantial: Kleven *et al.* (2019a) document a persistent child penalty of about 50% in Austria compared to about 20% in Denmark.

Table A.2: Maternal employment by part-time/full-time status (women 25–54, 2021)

	Austria	Denmark	AT – DK
No children aged 0–14	83.7	81.8	1.9
At least one child aged 0–14	76.4	81.8	-5.4
Part-time	40.9	9.1	31.8
Full-time	35.5	72.8	-37.3
No information on hours	0.0	0.0	0.0

Notes: Employment rates (%) for women aged 25–54, disaggregated by the presence of at least one child (aged 0–14) and by part-time/full-time status. Source: OECD Family Database (2021).

A.4 Family policies

Family policies also diverge sharply. Austria supports mothers as primary caregivers, whereas Denmark emphasizes support for working mothers.

Consider paid leave in 2021: Austria mandated 16 weeks of maternity leave (8 before and 8 after birth), followed by parental leave of up to 35 months if shared (28 months if taken by one parent, typically the mother). In Denmark, mothers were entitled to 18 weeks of maternity leave, and parents together could share 32 weeks of fully paid parental leave, with extensions at lower replacement rates.

Childcare provision mirrors these differences. In Austria, kindergartens for ages 3–6 are widely available, but nurseries for children under 3 remain scarce, often with restricted hours and long holiday closures. Enrollment among children under 3 was only 27% in 2018. Denmark guarantees children a nursery slot from 26 weeks of age, and nearly 40% of children under 3 are enrolled, while enrollment among 3–6 year-olds approaches universality. These institutional contrasts explain why Austrian mothers spend more time as primary caregivers, while Danish mothers typically resume employment much sooner.

A.5 Breastfeeding

Breastfeeding patterns follow a similar line: while initiation is nearly universal in both countries (about 97% of mothers), continuation is markedly higher in Austria. In 2021, 77% of Austrian children were still breastfed at four months, 64% at six months, and 41% at twelve months, compared to only 14% of Danish children exclusively breastfed for six months and about 3% breastfed at one year.

A.6 Gender identity norms

Finally, cultural norms further distinguish the two countries. Austria is more conservative in attitudes toward maternal employment, consistent with the male breadwinner model. Evidence from the *International Social Survey Programme* (2002 and 2012) illustrates these gaps (Table A.3) The contrast is stark: only 6% of Danes but 25% of Austrians strongly believe that preschool children suffer if mothers work. Conversely, a majority of Danes (61%) but fewer Austrians (47%) strongly agree that working mothers can have a warm relationship with their children. These differences underline that Austrian social norms remain more traditional, reinforcing the institutional structures documented above.

Overall, the evidence shows that Austria and Denmark differ in healthcare organiza-

Table A.3: Attitudes towards family and gender roles

	Share of respondents strongly agreeing			
	Austria		Denmark	
	Women	Men	Women	Men
(a) Working mother can have warm relation with child	0.47	0.35	0.61	0.61
(b) Preschool child suffers through working mother	0.25	0.27	0.06	0.06
(c) Family life suffers through working mother	0.26	0.24	0.08	0.08
(d) Women's preference: home and children	0.09	0.10	0.07	0.07
(e) Being housewife is satisfying	0.16	0.15	0.17	0.17

Notes: Data from the *International Social Survey Programme* (2002, 2012). Question wording: "To what extent do you agree or disagree?" Response categories: strongly agree, agree, neither, disagree, strongly disagree. The table shows the share of respondents who strongly agree.

tion, family policies, labor markets, and gender norms, despite broadly similar resources and health outcomes. These contrasts provide a strong setting in which to test whether parenthood penalties are universal or context-dependent.

A.7 Fertility patterns

Austria and Denmark differ in fertility patterns. During our sample period (2002–2007), Denmark's total fertility rate (TFR) was consistently higher than Austria's. The same holds when using the tempo-adjusted TFR, which accounts for changes in the timing of births. Danish women also had their first child later on average (around 28 years) compared to Austrian women (around 27 years) (Table A.4).

Table A.4: Period fertility indicators, Austria and Denmark (2002–2007)

	Total fertility rate (TFR)		Tempo-adjusted TFR		Mean age at first birth	
Year	AT	DK	AT	DK	AT	DK
2002	1.394	1.720	1.681	2.071	26.75	28.00
2003	1.376	1.755	1.587	1.999	26.94	28.16
2004	1.419	1.781	1.638	1.949	27.04	28.29
2005	1.408	1.798	1.728	1.959	27.26	28.32
2006	1.409	1.846	1.687	2.067	27.48	28.40
2007	1.385	1.843	1.613	1.980	27.65	28.47

Notes: Total Fertility Rate (TFR), defined as the average number of children who would be born alive to a woman if age-specific fertility rates of a given year remained constant. The tempo-adjusted TFR corrects for changes in the timing of births. Mean age at first birth is the average age of women at first childbirth. *Source:* Human Fertility Database (downloaded September 2025).

Long-run cohort data confirm these differences. Table A.5 shows parity distributions for the 1970–1979 birth cohorts, which roughly correspond to mothers in our study period. Austrian women are more likely to remain childless or stop at one child, while Danish women more often progress to two or more children. Together, these differences explain why Denmark's fertility is higher despite later average childbearing.

Table A.5: Cohort parity distribution, Austria and Denmark (1970–1979 birth cohorts, in %)

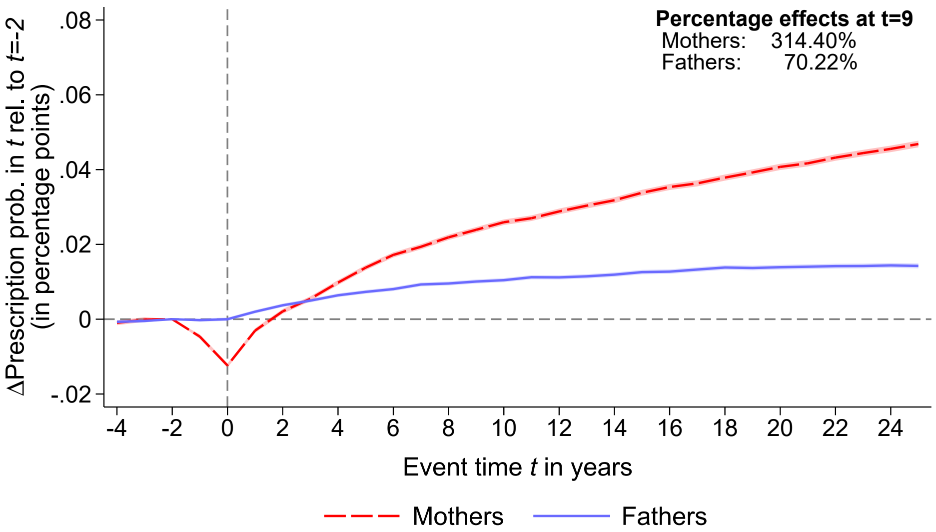
	Parity 0		Parity 1		Parity 2		Parity 3+	
Cohort	AT	DK	AT	DK	AT	DK	AT	DK
1970	20.5	13.3	22.5	14.1	38.3	44.9	18.7	27.7
1971	20.3	13.8	22.3	14.0	38.5	45.1	18.9	27.1
1972	20.2	13.3	22.1	14.3	38.3	45.1	19.4	27.3
1973	19.8	13.1	21.9	14.0	38.5	45.8	19.8	27.1
1974	19.7	13.1	22.0	14.3	38.7	45.5	19.6	27.1
1975	19.8	13.1	22.4	14.2	38.5	45.4	19.3	27.3
1976	20.2	12.7	22.5	14.3	38.1	46.0	19.2	27.0
1977	21.0	12.6	21.7	14.8	38.1	46.1	19.2	26.5
1978	20.5	13.0	21.9	14.8	38.5	46.2	19.1	26.0
1979	20.8	13.3	22.1	15.2	38.5	46.0	18.6	25.5

Notes: The table reports the distribution of completed parity: Parity 0 = childless; Parity 1 = exactly one child; Parity 2 = exactly two children; Parity 3+ = three or more children. *Source:* Human Fertility Database (downloaded September 2025).

B Long-run impacts of parenthood: Detailed results

This appendix section provides additional detail on the persistence of parenthood penalties briefly mentioned in Section 4 of the paper. In particular, we explore whether the mental health effects extend beyond the first decade after childbirth. For Denmark, our data allow us to study parents over a period of up to 25 post-birth years. To this end, we expand our balanced panel into an unbalanced sample that (a) covers the years 1995–2019 and (b) includes parents who gave birth until 2021. Figure B.1 shows that penalties persist: men

Figure B.1: Long-run impacts of parenthood on prescriptions in Denmark (PP)



Notes: This figure shows the estimated impacts of parenthood on antidepressant prescriptions before and after having the first child (in percentage points). The dashed lines refer to mothers ($j = m$) and the solid lines to fathers ($j = f$). We obtain the event time coefficients from estimating regression (1) on an unbalanced sample of parents with their first child born before 2022. The shaded areas represent 95% confidence intervals based on robust standard errors. The top-right corner of each figure also shows percentage effects at event time $t = 25$.

remain on an elevated but flat trajectory after the seventh post-birth year, whereas women's prescription probability continues to increase. Because of the long delay between birth and outcome measurement, these estimates rely on stronger identifying assumptions. We, therefore, interpret them as suggestive evidence that the parenthood penalties in mental health are a persistent phenomenon.

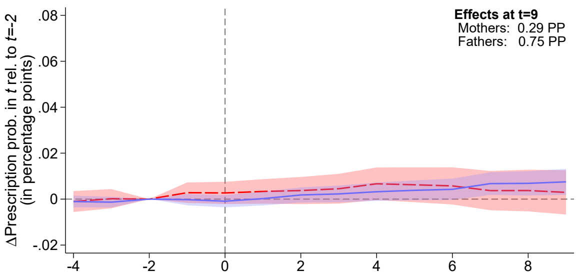
C Comparison group: Childless parents

One concern is that our baseline estimates may capture general trends not fully absorbed by age and year controls rather than genuine parenthood effects. To address this, we construct a matched comparison group of childless individuals. Our strategy assigns placebo birth events to childless individuals in a way that reproduces the empirical age-at-first-birth distribution of parents. We then evaluate whether patterns similar to those observed for parents emerge in the absence of a childbirth and also use childless individuals with assigned placebo births as a comparison group in a difference-in-differences framework.

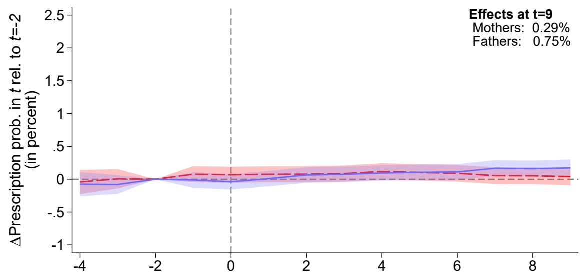
Figure C.1: Impacts of placebo births on antidepressant prescriptions

Austria

(a) Impacts on prescriptions (PP)

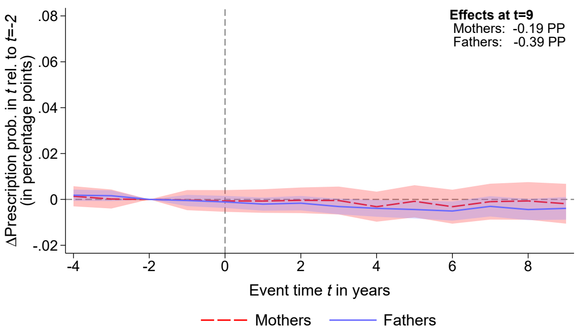


(b) Impacts on prescriptions (PCT)

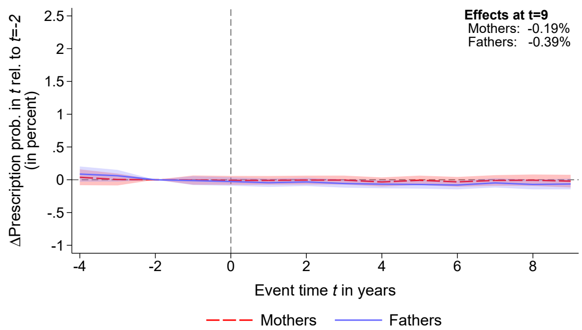


Denmark

(c) Impacts on prescriptions (PP)



(d) Impacts on prescriptions (PCT)



Notes: This figure shows the estimated impacts of the placebo birth on antidepressant prescriptions before and after the placebo birth. Figures C.1a and C.1b show results for Austria, and Figures C.1c and C.1d for Denmark. Moreover, Figures C.1a and C.1c report effects in percentage points ($\hat{\alpha}_t^j$), while Figures C.1b and C.1d report relative effects in percent ($\hat{\alpha}_t^j / E[\tilde{Y}_{ist}^j | t]$). The dashed lines refer to childless women ($j = m$) and the solid lines to childless men ($j = f$). We obtain the event time coefficients by estimating regression (1) on a balanced sample of individuals with allocated placebo births between 2002 and 2007. The shaded areas represent 95% confidence intervals based on robust standard errors.

C.1 Empirical strategy: Placebo births

Assigning placebo birth events: The construction of placebo events parallels Kleven *et al.* (2019b). Specifically, we approximate the empirical distribution of age at first birth by a log-normal distribution:

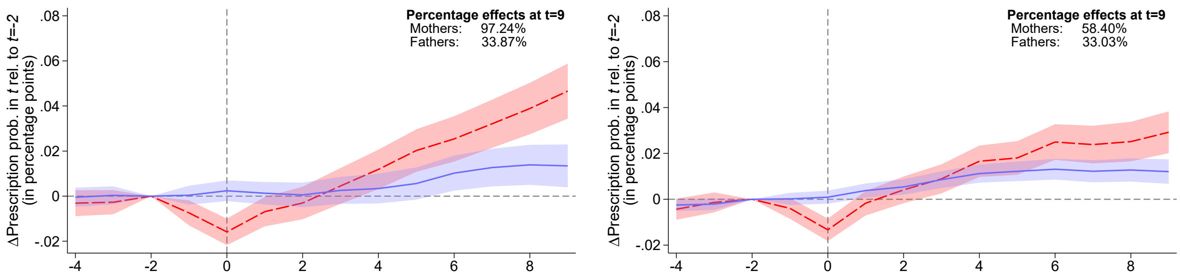
$$age\ at\ first\ birth_{jc} \sim \mathscr{L}\mathscr{N}(\mu_{jc}, \sigma^2_{jc}),$$

within cells defined by sex j and birth cohort c. We obtain the parameters μ_{jc} and σ^2_{jc} from the actual ages observed at the first birth of parents in the respective cell. For each childless individual, we then draw a hypothetical age at the first birth from this fitted distribution and compute the corresponding placebo event date.[1]

Estimation: We use placebo events in two complementary estimation strategies. First, we estimate event studies for childless individuals around the assigned placebo birth events. This exercise reveals whether the outcome trajectories show discontinuities at ages when parents typically experience childbirth. Second, we use childless individuals with assigned placebo births as a comparison group in a difference-in-differences event-study framework. In this specification, parents constitute the treatment group, and childless individuals with placebo births form the comparison group. This design compares the relative changes in outcomes around childbirth versus placebo events.

Figure C.2: Impact of parenthood on antidepressant prescriptions (DiD estimator)

(a) Impacts on prescriptions in Austria (PP) **(b) Impacts on prescriptions in Denmark (PP)**



Notes: This figure shows the estimated impacts of parenthood on antidepressant prescriptions (in percentage points). Figure C.2a presents results for Austria, and Figure C.2b for Denmark. The dashed lines refer to mothers ($j = m$) and the solid lines to fathers ($j = f$). The estimates rely on a difference-in-differences event-study design that uses childless parents as a control group. The design assigns placebo births for childless parents based on the factual age-at-first-birth distribution. Moreover, the estimates are based on a balanced sample of parents who have their first child between 2002 and 2007. The shaded areas represent 95% confidence intervals based on robust standard errors. The top-right corner of each figure also shows percentage effects at event time $t = 9$.

C.2 Results

Placebo events: Figure C.1 shows the estimated impacts of placebo births on antidepressant prescriptions for childless women and men in Austria and Denmark in percentage points (Figures C.1a and C.1c) and percent (Figures C.1b and C.1d). In both countries, we observe no sharp changes in prescription probabilities around the assigned placebo event. The trajectories remain flat, and the 95% confidence intervals for women and men overlap throughout the event window. This finding suggests that there are no general non-child trends that resemble the patterns of our baseline effects.

[1] As an extension, we also conduct robustness checks that additionally stratify by other covariates (e.g., education). The results remain unchanged.

Placebo-based DiD: Figure C.2 reports the estimates from the difference-in-differences event-study design that uses childless individuals with assigned placebo births as the control group. The results are similar to ou baseline estimates. Mothers in both countries show pronounced increases in antidepressant prescriptions after childbirth, whereas the effects for fathers are smaller. While confidence intervals are wider due to the more demanding specification, the parenthood penalty remains statistically significant.

D Effects for adoptive families

D.1 Empirical strategy: Matching approach

Data: The Danish data allows us to (a) identify adopted children and, thus, (b) estimate the impact of parenthood if children are not biological. Our main sample includes adopted and biological (first) children. In the following, we use the same sample of children but split it into biological and adopted ones. We define the first child of a parent as being adopted if the child-parent pair appears in the adoption register (ADOP). Using this strategy, we label each parent in the main sample as having a first biological or first adopted child.

Estimation strategy: As discussed in the main text, we study adoptions to separate the psychological effects of having and raising a child from the biological effects of childbirth. This type of analysis faces a key identification challenge. Families with adopted children represent a specific subset of the population. Any potential difference in the parenthood penalty between adoptive and biological parents could reflect selection rather than biology. Indeed, adoptive parents tend to be older and more educated than biological parents.

Our empirical approach deals with such selection issues by using weighted (instead of unweighted) regressions (Kleven *et al.*, 2021). Specifically, our regressions reweigh the adoptive parents' characteristics to mimic the biological parents' ones. We construct those weights based on a combination of the highest obtained ISCED level and the age at first birth in years. We then adjust the weights for adoptive parents within each education-age-at-birth cell as the ratio of the relative cell size in the biological and adoptive parents' populations. By contrast, we set weights for biological parents to one. We compute the weights for mothers and fathers separately.

We then estimate the following weighted models. First, comparably to our main analysis, we estimate the impact of adopted children on mothers' prescription probability. Besides the weights, a second difference to model (1) is that we now estimate an interacted model:

$$\begin{aligned} Y^m_{ist} =& \phi^{m,adop}_{ist} + \sum_{event \neq -2} \alpha^{m,adop}_{event} \cdot \mathbb{1}[event = t] \\ &+ \sum_{event \neq -2} \alpha^{m,bio}_{event} \cdot \mathbb{1}[event = t] + \sum_{year} \beta^m_{year} \cdot \mathbb{1}[year = s] \\ &+ \sum_{age} \gamma^m_{age} \cdot \mathbb{1}[age = a_{is}] + u^m_{ist}, \end{aligned} \tag{D.1}$$

where the event coefficients $\alpha^{m,adop}_{event}$ and $\alpha^{m,bio}_{event}$ separately identify the impact on antidepressants for adoptive and biological mothers, and $\phi^{m,adop}_{ist}$ represents the intercept for adoptive mothers. The main benefit of using an interacted model on the full sample of adoptive and non-adoptive parents is that this strategy increases precision. The reason is that the model identifies the coefficients of the year and age dummies as common effects in the pooled sample. Second, we repeat the same analysis for fathers.

E Effect decomposition

E.1 Empirical strategy: Decomposition approach

This appendix explains how we can decompose the event-study coefficients into transparent building blocks within a given sex (mothers or fathers). The goal is to understand how much of the pooled effect at event time t comes from the highlighted subgroup (e.g., non-cohabiting mothers or mothers without income), how much comes from the complementary subgroup, and how much reflects composition forces and cross-period interactions.

The discussion proceeds in three steps. First, we introduce the pooled regression that delivers the "realized effect of parenthood" at each t. Second, we discuss a decomposition that follows from a simplified regression without controls to build intuition. In that case, the pooled effect is literally a weighted average of the subgroup effects (with weights representing raw shares). Third, we add controls and derive the exact decomposition used in the figures. Controls matter because they force us to use conditional (control-adjusted) shares rather than raw shares.

E.1.1 Baseline specification

Pooled model: Let Y^g_{ist} be an indicator for whether individual i of sex g receives an antidepressant prescription in calendar year s at event time t, where t measures years relative to the first child's birth and $t = b$ is the baseline period (in the main text $b = -2$). Moreover, $g = m$ refers to the mothers' sample and $g = f$ to the fathers' sample. Throughout, we fix a sex $g \in \{m, f\}$ and decompose our estimates within that sex. We control for year fixed effects, $\mathbf{1}[year = s]$, and age fixed effects, $\mathbf{1}[age = a_{is}]$. The pooled event study for sex g is:

$$\begin{aligned} Y^g_{ist} = \sum_{event \neq -2} \alpha^g_{event} \cdot \mathbb{1}[event = t] + \sum_{year} \beta^g_{year} \cdot \mathbb{1}[year = s] \\ + \sum_{age} \gamma^g_{age} \cdot \mathbb{1}[age = a_{is}] + u^g_{ist}. \end{aligned} \tag{E.1}$$

The coefficients α^g_t are the "realized effects of parenthood" for sex g. They measure the change in antidepressant prescriptions at event time t relative to the implied counterfactual.

Interacted model: Let S denote the subgroup indicator ($S = 1$ for the highlighted subgroup, $S = 0$ for its complement). For example, in the "non-cohabiting vs. cohabiting" figures, $S = 1$ picks out non-cohabiting people; in the "without income vs. with income" figures, $S = 1$ picks out those without income. To connect the pooled effect α^g_t to subgroup objects, we estimate an interacted model:

$$\begin{aligned} Y^g_{ist} = \sum_{event \neq -2} \alpha^{g,0}_{event} \cdot \mathbb{1}[event = t] + \sum_{event \neq -2} \alpha^{g,\Delta}_{event} \cdot \mathbb{1}[event = t] \times S \\ + \lambda^g \cdot S + \sum_{year} \beta^g_{year} \cdot \mathbb{1}[year = s] \\ + \sum_{age} \gamma^g_{age} \cdot \mathbb{1}[age = a_{is}] + u^g_{ist}. \end{aligned} \tag{E.2}$$

Here, $\alpha^{g,0}_{event}$ is the event-time effect for the complement $S = 0$. Similarly, $\alpha^{g,0}_{event} + \alpha^{g,\Delta}_{event}$ is the event-time effect for the subgroup $S = 1$, and λ^g is the baseline gap between the two groups in period b. The pooled model (E.1) is nested in (E.2). The decomposition tells us how to express α_t^g using the coefficients from (E.2) plus control-adjusted shares.

E.1.2 Warm-up: Decomposition without controls

Decomposition: To build intuition, suppose we temporarily omit the age and year controls from model (E.1). Let $\pi_t^g = \Pr(S = 1 \mid event = t)$ denote the raw share of subgroup $S = 1$ at event time t, and let π_b^g be the corresponding share in the baseline period. We can then verify that:[1]

$$\alpha_t^g = (1 - \pi_t^g)\,\alpha_t^{g,0} + \pi_t^g\left(\alpha_t^{g,0} + \alpha_t^{g,\Delta}\right) + (\pi_t^g - \pi_b^g)\,\lambda^g. \tag{A3}$$

In words, the pooled effect α_t^g is a raw-share-weighted average of the current-period subgroup effects: it averages over $\alpha_t^{g,0}$ for the complement $S = 0$ and $\alpha_t^{g,0} + \alpha_t^{g,\Delta}$ for the subgroup $S = 1$. In addition, there is a composition term, $(\pi_t^g - \pi_b^g)\lambda^g$, that shifts the pooled effect whenever the prevalence of $S = 1$ differs between period t and the baseline period b.

Intuition: Without controls, the data at t are literally a mixture of $S = 0$ and $S = 1$ units in proportions π_t^g and $1 - \pi_t^g$. The pooled mean, therefore, averages their effects at t. Additionally, the pooled effect reacts to compositional shifts by inheriting parts of the baseline difference λ^g if the subgroup's share changes from baseline period b to t.

E.1.3 Why controls change the arithmetic

Complications: Once we add fixed effects for age and year, the simple raw-share decomposition from the no-control case no longer applies. Two complications arise. First, when we control for age and year effects, the prevalence of the relevant subgroup is no longer given by the raw shares π_t^g. OLS now, intuitively, compares individuals of the same age in the same calendar year, and the subgroup prevalence in these comparisons can differ from the raw prevalence. The appropriate weighting objects are, hence, control-adjusted, shares.

Second, including controls can create cross-period spillovers. In the no-control case, the event-time indicators are mutually orthogonal (and orthogonal to the subgroup interactions for $k \neq t$ in the interacted specification). This orthogonality guarantees that the coefficient at t only reflects subgroup behavior in that period. Once we control for age and year, this orthogonality is lost. To understand this, recall the Frisch-Waugh-Lovell theorem that allows us to obtain the coefficients in the interacted model (E.2) by regressing the residualized outcome on the residualized event-time indicators and their residualized interactions with S. After this residualization, the indicator for event period t can overlap with residualized regressors for other periods $k \neq t$. As a result, the OLS coefficient on t also potentially loads on subgroup deviations from other periods.

Implications: These two complications imply that the simple raw-share formula no longer holds once we control for age and year. Instead, the pooled coefficient at t must be expressed through a decomposition that accounts for control-adjusted terms and allows for spillovers

[1] To derive this equation, recall that the effect for subgroup $S = 0$ at event time t is $\alpha_t^{g,0}$ and the effect for subgroup $S = 1$ is $\alpha_t^{g,0} + \alpha_t^{g,\Delta}$. Moreover, the baseline gap between the two groups is λ^g. The pooled effect is, therefore, the weighted average: $(1-\pi_t^g)\,\alpha_t^{g,0} + \pi_t^g\,(\alpha_t^{g,0} + \alpha_t^{g,\Delta} + \lambda^g)$, where π_t^g is the share of subgroup $S = 1$ at time t. Subtracting the baseline component $\pi_b^g\lambda^g$ yields: $\alpha_t^g = (1-\pi_t^g)\alpha_t^{g,0} + \pi_t^g(\alpha_t^{g,0} + \alpha_t^{g,\Delta}) + (\pi_t^g - \pi_b^g)\lambda^g$.

across periods. To construct this decomposition, we need the subgroup coefficients from the interacted model (E.2) together with three types of control-adjusted weights:

1. The own-period weights $\phi^g_{t,t}$ play the role of conditional subgroup shares at event time t. They determine how strongly the pooled coefficient at t reflects the subgroup's own effect at that period.

2. The cross-period weights $\phi^g_{t,k}$ for $k \neq t$ determine the size of spillovers from subgroup deviations at other event times k into the coefficient at t.

3. The baseline-gap weights κ^g_t measure the change in subgroup prevalence between t and the baseline b. They determine how much of the baseline difference λ^g is transmitted into the pooled effect.

These weights are the control-adjusted analogues of the raw subgroup shares from the no–control case, and they are the building blocks of the exact decomposition derived in the next section.

E.1.4 Construction of the control-adjusted weights

To construct the weights, we estimate two sets of auxiliary regressions. First, we obtain the baseline-gap weights, κ^g_t, from:

$$S_{ist} = \sum_{t\neq b} \kappa^g_t \cdot \mathbb{1}[event = t] + \sum_s \eta_s \cdot \mathbb{1}[year = s] + \sum_a \xi_a \cdot \mathbb{1}[age = a_{is}] + \nu_{ist}. \tag{E.3}$$

By construction, in the absence of controls, we recover $\kappa^g_t = \pi^g_t - \pi^g_b$. Second, we obtain the own-period weights, $\phi^g_{t,t}$, and the cross-period weights, $\phi^g_{t,k}$, by estimating the following model separately for each $k \neq b$:

$$\begin{aligned} \mathbb{1}[event = k] \times S_{ist} = \sum_{t\neq b} \phi^g_{t,k} := \psi_{t,k}\, \mathbb{1}[event = t] + \sum_s \eta_{s,k}\, \mathbb{1}[year = s] \\ + \sum_a \xi_{a,k}\, \mathbb{1}[age = a_{is}] \;+\; w_{ist,k}. \end{aligned} \tag{E.4}$$

Intuitively, $\phi^g_{t,k}$ measures the conditional overlap, after removing age and year effects, between being at event time t and belonging to subgroup $S = 1$ in period k. When $k = t$, this coefficient is the control-adjusted weight of the own period $\phi^g_{t,t}$. In the absence of controls, $\phi^g_{t,t}$ reduces to the raw subgroup share π^g_t. Instead, with controls, it represents the conditional prevalence of $S = 1$ at t holding age and year fixed. When $k \neq t$, the coefficients $\phi^g_{t,k}$ capture cross-period spillovers: they quantify the extent to which subgroup deviations in period k also load onto the pooled coefficient at t once the controls induce correlations across residualized event-time indicators.

E.1.5 Decomposition with controls

Decomposition: Using the Frisch-Waugh-Lovell theorem, we can derive the following decomposition identity:[2]

$$\alpha_t^g \;=\; \alpha_t^{g,0} \;+\; \kappa_t^g \lambda^g \;+\; \sum_{k \neq b} \phi_{t,k}^g \, \alpha_k^{g,\Delta}. \tag{E.5}$$

This expression generalizes the raw-share decomposition from the no-control case.

Intuition: The decomposition has three components. The first term, $\alpha_t^{g,0}$, is the effect for the complement subgroup $S = 0$ at time t after adjusting for age and year. Intuitively, the pooled coefficient would equal this effect if the sample only contained $S = 0$ units. The second term, $\kappa_t^g \lambda^g$, reflects the fact that subgroup $S = 1$ differs from $S = 0$ already in the baseline period by λ^g. If the prevalence of $S = 1$ at time t differs from its prevalence at the baseline, this baseline gap mechanically shifts the pooled effect. The magnitude of this shift depends on the control-adjusted change in prevalence κ_t^g. The third term, $\sum_{k \neq b} \phi_{t,k}^g \, \alpha_k^{g,\Delta}$, captures dynamic deviations of subgroup $S = 1$ from subgroup $S = 0$ across all periods k. As explained, because residualization on age and year destroys orthogonality across event dummies, the pooled coefficient at t also picks up deviations at other periods $k \neq t$ (each weighted by the conditional overlap $\phi_{t,k}^g$).

Raw-share analogue: Rearranging equation (E.5) clarifies that the structure of the decomposition mirrors the raw-share formula from the no-control case: the pooled effect at t is a weighted average of period-t-specific subgroup effects, with an additional remainder once controls are included. To show this, we take equation (E.5) and separate out the $k = t$ term in the summation:

$$\alpha_t^g = \alpha_t^{g,0} + \phi_{t,t}^g \alpha_t^{g,\Delta} + \kappa_t^g \lambda^g + \sum_{k \neq t} \phi_{t,k}^g \alpha_k^{g,\Delta}. \tag{E.6}$$

We can equivalently rewrite the equation as:

$$\alpha_t^g = (1 - \phi_{t,t}^g) \alpha_t^{g,0} \;+\; \phi_{t,t}^g (\alpha_t^{g,0} + \alpha_t^{g,\Delta}) \;+\; \kappa_t^g \lambda^g \;+\; \sum_{k \neq t} \phi_{t,k}^g \alpha_k^{g,\Delta}. \tag{E.7}$$

The first two terms represent the current-period subgroup effects, weighted by the control-adjusted subgroup shares $(1 - \phi_{t,t}^g)$ and $\phi_{t,t}^g$. The third term reflects compositional effects: it captures how changes in the prevalence of $S = 1$ relative to the baseline transmit the baseline gap λ^g into the pooled coefficient. Together, the first three terms mirror the raw-share decomposition from the no-control case. The last term represents the cross-period spillovers that arise once we include controls. In the absence of controls, the cross-period weights vanish and the baseline term simplifies: $\phi_{t,t}^g = \pi_t^g$, $\phi_{t,k}^g = 0$ for $k \neq t$, and $\kappa_t^g = \pi_t^g - \pi_b^g$. In that case, the equations collapses to the raw-share formula.

[2] Formally, let T be the matrix of event-time indicators, let Z be the matrix of age and year dummies, and let W be the matrix of interactions. The pooled model is $Y = T\alpha^g + Z\vartheta + u$, and the interacted model is $Y = T\alpha^{g,0} + \lambda^g S + W\alpha^{g,\Delta} + Z\theta + \varepsilon$. By the Frisch-Waugh-Lovell theorem, $\alpha^g = (T' M_Z T)^{-1} T' M_Z Y$ with $M_Z = I - Z(Z'Z)^{-1} Z'$. Next, we replace Y in this expression by its representation from the the interacted model, and we define $\kappa^g = (T' M_Z T)^{-1} T' M_Z S$ and $\phi_{\cdot,k}^g = (T' M_Z T)^{-1} T' M_Z W_k$. These steps yield $\alpha_t^g = \alpha_t^{g,0} + \kappa_t^g \lambda^g + \sum_{k \neq b} \phi_{t,k}^g \, \alpha_k^{g,\Delta}$, which is equation (E.5).

Graphical representation: In our figures, we present these decompositions in two complementary ways. In the main text, we focus on the subgroup of interest by plotting

$$\phi^g_{t,t}(\alpha^{g,0}_t + \alpha^{g,\Delta}_t) \tag{E.8}$$

as the color-shaded area and grouping all other components together as "other factors." This decomposition emphasizes how much of the effect of parenthood in t is driven by the subgroup's own current period effect (e.g., non-cohabiting or parents without income). The contribution of the subgroup can vary either because its conditional share $\phi^g_{t,t}$ changes over time (e.g., due to parenthood) or because the subgroup's event-time effect ($\alpha^{g,0}_t + \alpha^{g,\Delta}_t$) changes. In the Appendix, we instead display the full decomposition with all four components: the contribution of subgroup $S = 0$ to the pooled effect, the contribution of subgroup $S = 1$, the baseline composition term, and the cross-period spillovers.

Estimation: We proceed in four steps. First, estimate the interacted model for sex g to obtain the coefficients $\alpha^{g,0}_t$ for subgroup $S = 0$, the deviations $\alpha^{g,\Delta}_t$ for subgroup $S = 1$, and the baseline gap λ^g. Second, we estimate the auxiliary regressions for the same sex to obtain the control-adjusted shares κ^g_t and $\phi^g_{t,k}$. Third, we compute the pooled coefficients using the decomposition identity (E.5). Fourth, we construct the four components of equation (E.7).

E.2 Additional decomposition results

Main decomposition in percent: Figure E.1 replicates the decomposition in percentage terms (PCT) rather than percentage points (PP). The qualitative patterns mirror those in the main text. In Denmark, non-cohabiting parents and parents without income account for a notable share of the post-birth increase in antidepressant use for both mothers and fathers. The corresponding income-based contributions are smaller in Austria. At the same time, we find that the residual component is larger than the explained subgroup contribution. This indicates that complementary groups, baseline composition, and cross-period spillovers account for a substantial share of the total percentage effect.

Extended decomposition: Figures E.2 (in PP) and E.3 (in PCT) present the results from the extended decomposition approach. The darker color-shaded areas show the contributions of the subgroup of interest (non-cohabiting parents or parents without income), while the lighter color-shaded areas depict the contributions of the complementary groups (cohabiting parents or parents with income). The light gray-shaded areas capture composition effects. They reflect how shifts in group shares over time, together with baseline differences in prescription rates between groups, affect the total effect. Intuitively, when a subgroup with higher baseline prescriptions becomes more prevalent after childbirth, the aggregate impact increases even if within-group effects remain unchanged. The dark gray-shaded areas represent cross-period spillovers. Finally, the bold line in each panel indicates the total effect at a given event time, obtained from the sum of all components.

Both figures confirm our main findings. They show that although non-cohabiting and non-employed mothers account for a noticeable share of the overall rise in prescriptions after birth, most of the increase occurs among employed, cohabiting mothers. Similarly, non-cohabiting and non-employed fathers also contribute to the rise in antidepressant use among fathers. A novel aspect of the detailed decomposition is that it highlights the contribution of composition and spillover effects. Changes in group shares over time and the cross-period effects explain part of the total effect. However, the contributions of these residual components remain small relative to the direct current-period contributions of the subgroups themselves. Note that expressing the results in percentages rather than percentage points leaves the qualitative conclusions unchanged.

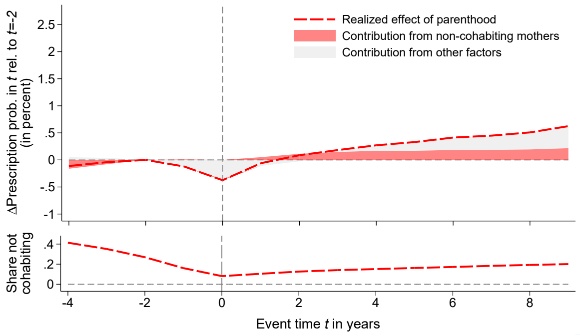
Controlling for mediators: Figures E.4 and E.5 show the estimated impacts of parenthood on antidepressant prescriptions before and after controlling for potential mediators. Figure E.4 expresses the effects in percentage points (PP); Figure E.5 reports the corresponding percentage effects (PCT). Both figures compare the baseline specification (long dashed line) with models that progressively add controls for mediating factors. Specifically, one specification adds dummy variables indicating whether a parent has no income at event time t (solid line). For Denmark, we estimate a second specification that additionally controls for a dummy indicating whether a parent is non-cohabiting at time t (short-dashed line). The shaded areas represent 95% confidence intervals, and the top-right corner shows the percentage effects at $t = 9$ under each specification.

The findings are as follows. First, the figures show that controlling for the no-income dummy substantially reduces the estimated effects for mothers in both countries, and to a lesser extent for fathers. Adding the non-cohabitation control in Denmark does not dramatically change the point estimates. Together, the results suggest that differences in income and relationship status account for part of the observed rise in antidepressant use. However, a substantial share of the effect persists even after controlling for these channels. Expressing the results in percentage rather than absolute terms yields the same qualitative conclusions. Taken at face value, the analysis indicates that non-cohabitation and non-employment mediate some, but not all, of the mental-health costs of parenthood.

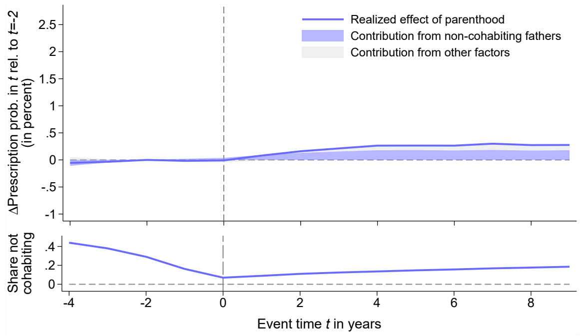
Figure E.1: Decomposition of the impacts of parenthood on prescriptions (PCT)

Impacts for Denmark

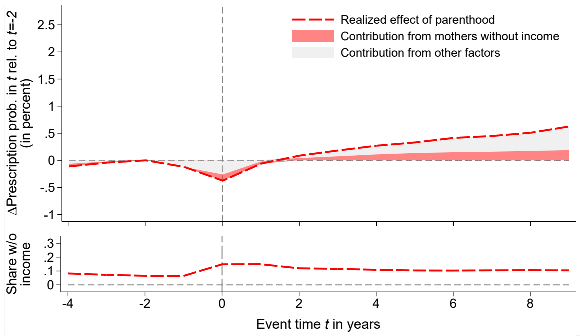
(a) Non-cohabiting mothers (PCT)



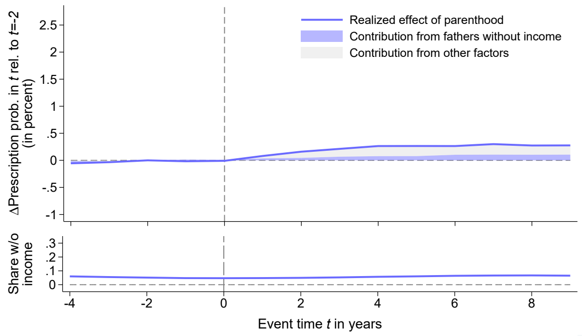
(b) Non-cohabiting fathers (PCT)



(c) Mothers without income (PCT)

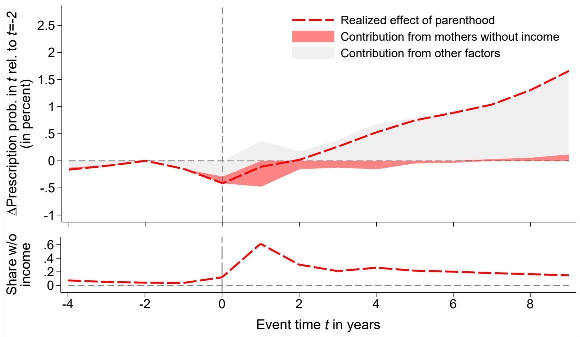


(d) Fathers without income (PCT)

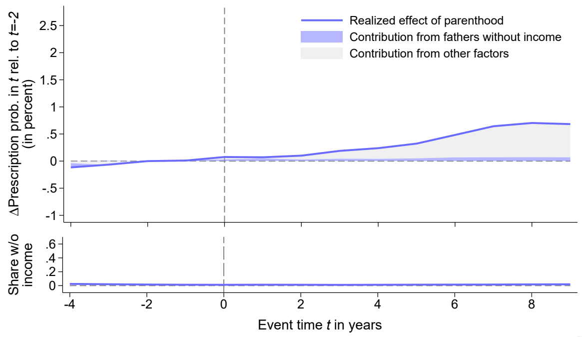


Impacts for Austria

(e) Mothers without income (PCT)



(f) Fathers without income (PCT)

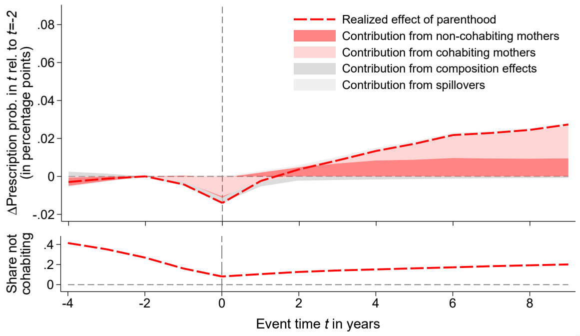


Notes: This figure decomposes the estimated impacts of parenthood on antidepressant prescriptions (in percent). Figures E.1a and E.1b focus on Denmark and show the results by cohabitation status (non-cohabiting vs. residual), while Figures E.1c and E.1d focus on a decomposition by income status (without income vs. residual). Figures E.1e and E.1f present the decomposition for Austria by income. In each figure, the color-shaded area displays the contribution of the indicated (non-cohabiting parents or parents without income), while the gray-shaded area captures the residual components (including contributions of the complementary groups, baseline differences, and cross-period effects). A subgroup's contribution at event time t reflects both the change in the subgroup's antidepressant prescription rate (the within-group effect of parenthood) and the group's share in the population at t. The bold lines show the total effect of parenthood at event time t, which equals the sum of the subgroup contribution and the residual. We can calculate the total impacts from regression (1) on a balanced sample of parents whose first child is born between 2002 and 2007.

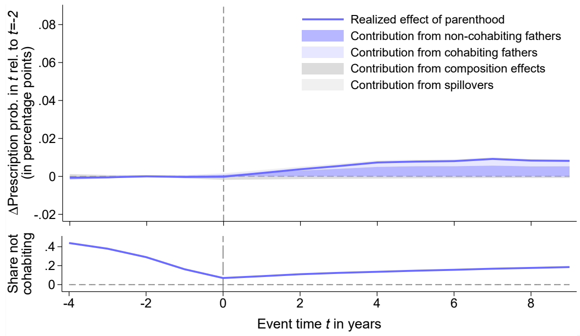
Figure E.2: Detailed decomposition of the impacts of parenthood on prescriptions

Impacts for Denmark

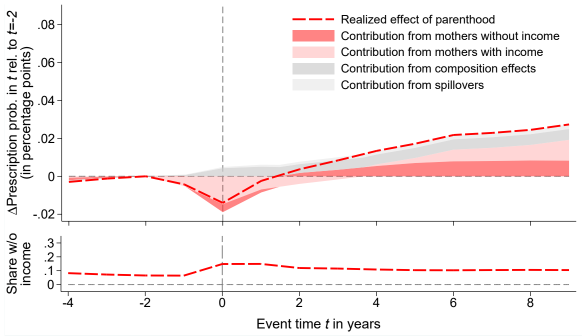
(a) Cohabiting status: Mothers (PP)



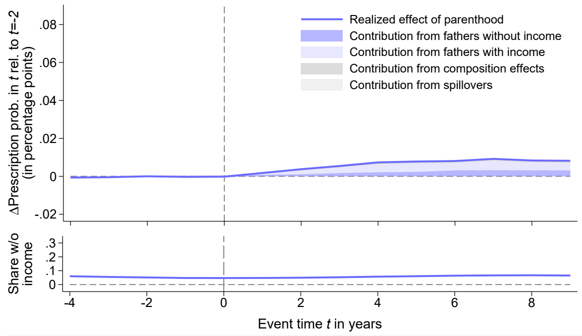
(b) Cohabiting status: Fathers (PP)



(c) Income status: Mothers (PP)

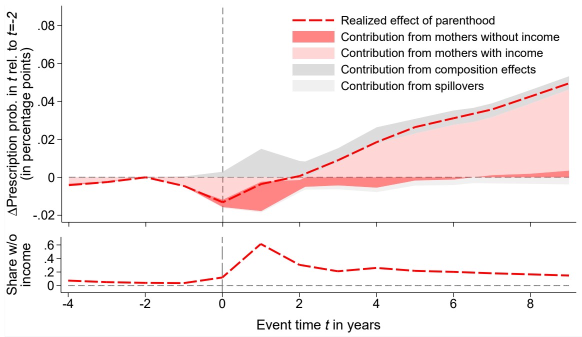


(d) Income status: Fathers (PP)

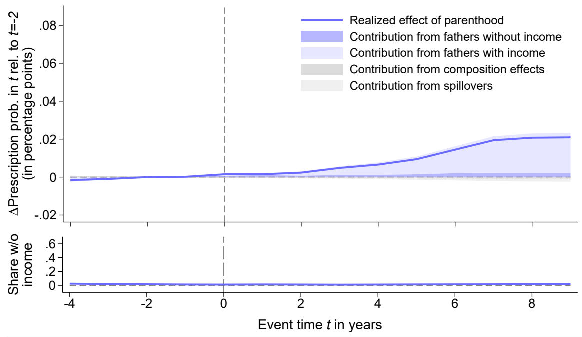


Impacts for Austria

(e) Income status: Mothers (PP)



(f) Income status: Fathers (PP)

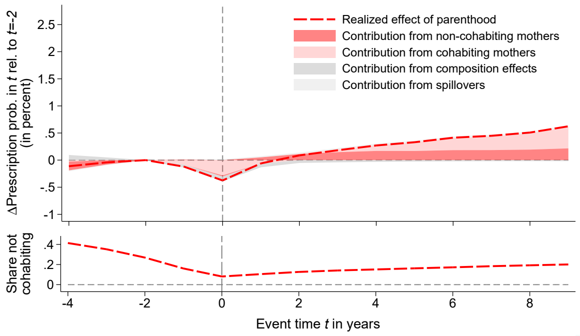


Notes: This figure decomposes the estimated impacts of parenthood on antidepressant prescriptions (in percentage points) into all underlying components. Figures E.2a and E.2b focus on Denmark and show the results by cohabitation status, while Figures E.2c and E.2d focus on a decomposition by income status. Figures E.2e and E.2f present the decomposition for Austria by income. In each figure, the color-shaded areas display the contributions of the subgroup-specific effects of parenthood. A subgroup's contribution at event time t reflects both the change in the subgroup's antidepressant prescription rate (the within-group effect of parenthood) and the group's share in the population at t. The gray-shaded areas either show composition effects (light gray) or cross-period spillovers (dark gray). Together, these components sum to the bold line, which represents the total effect of parenthood at event time t. We can calculate the total impacts from regression (1) on a balanced sample of parents whose first child is born between 2002 and 2007.

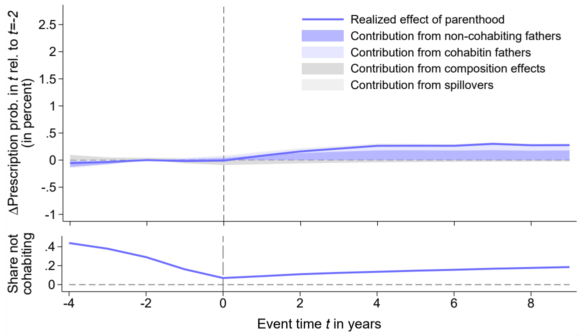
Figure E.3: Detailed decomposition of the impacts of parenthood on prescriptions (PCT)

Impacts for Denmark

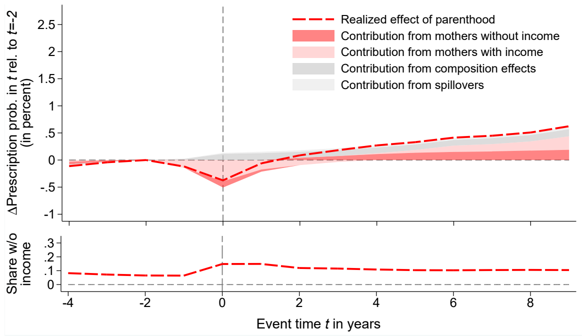
(a) Cohabiting status: Mothers (PCT)



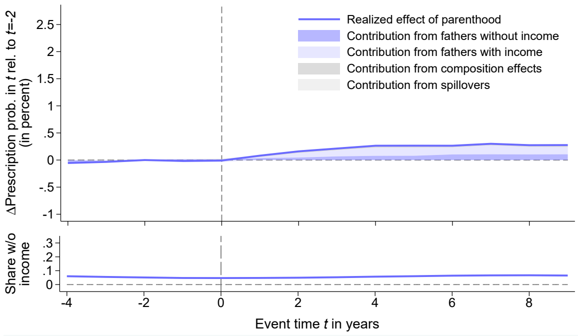
(b) Cohabiting status: Fathers (PCT)



(c) Income status: Mothers (PCT)

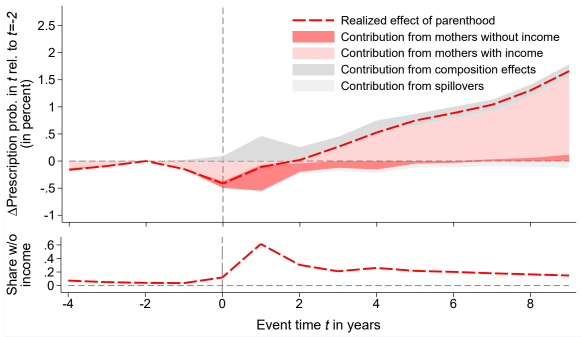


(d) Income status: Fathers (PCT)

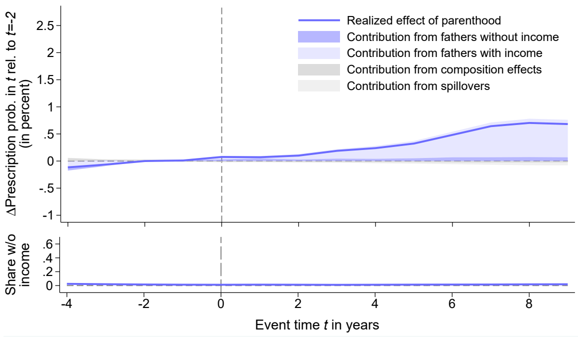


Impacts for Austria

(e) Income status: Mothers (PCT)



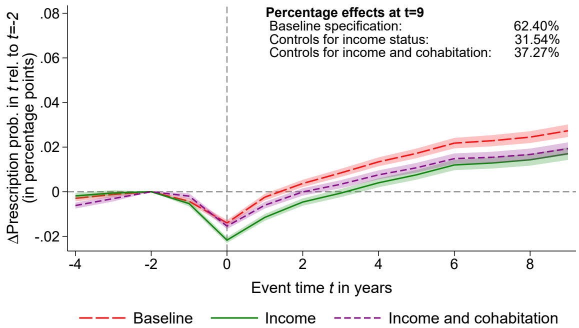
(f) Income status: Fathers (PCT)



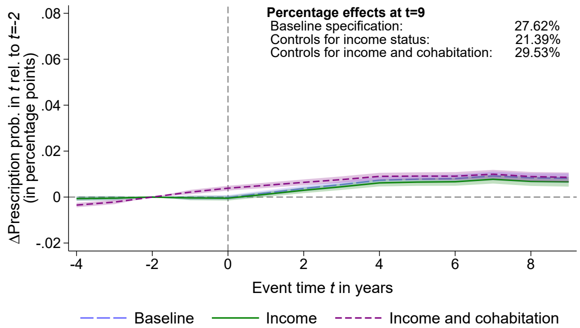
Notes: This figure decomposes the estimated impacts of parenthood on antidepressant prescriptions (in percent) into all underlying components. Figures E.3a and E.3b focus on Denmark and show the results by cohabitation status, while Figures E.3c and E.3d focus on a decomposition by income status. Figures E.3e and E.3f present the decomposition for Austria by income. In each figure, the color-shaded areas display the contributions of the subgroup-specific effects of parenthood. A subgroup's contribution at event time t reflects both the change in the subgroup's antidepressant prescription rate (the within-group effect of parenthood) and the group's share in the population at t. The gray-shaded areas either show composition effects (light gray) or cross-period spillovers (dark gray). Together, these components sum to the bold line, which represents the total effect of parenthood at event time t. We can calculate the total impacts from regression (1) on a balanced sample of parents whose first child is born between 2002 and 2007.

Figure E.4: Impacts of parenthood after controlling for potential mediators

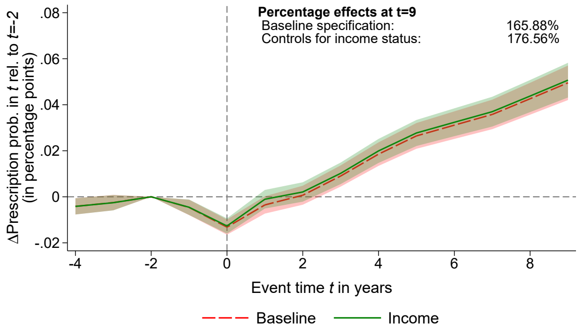
(a) Impacts for mothers in Denmark (PP)



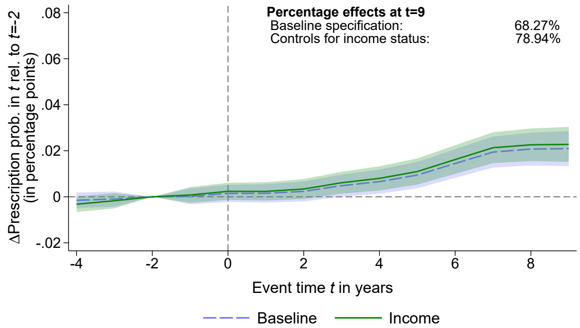
(b) Impacts for fathers in Denmark (PP)



(c) Impacts for mothers in Austria (PP)

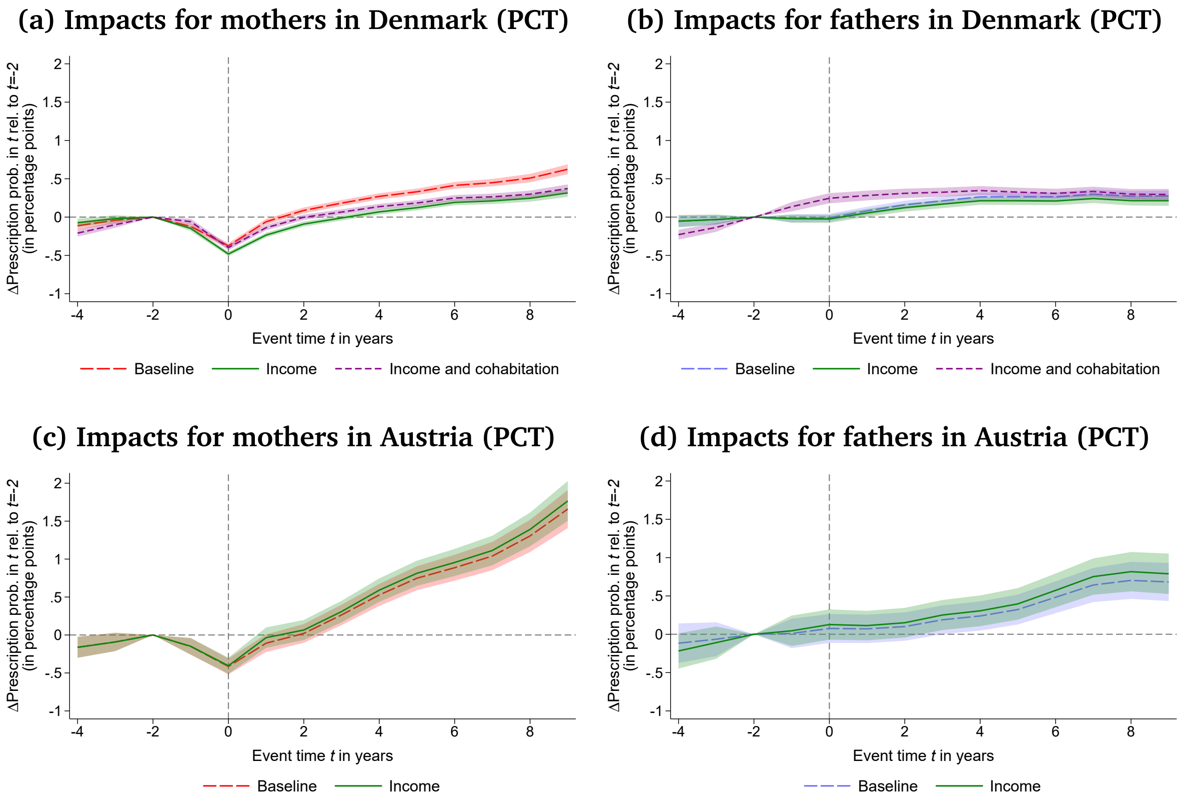


(d) Impacts for fathers in Austria (PP)



Notes: This figure shows the impacts of parenthood on antidepressant prescriptions after controlling for potential mediators (in percentage points). Specifically, it compares (a) the baseline specification (long dashed lines) with (b) one that controls for a dummy indicating whether a person earned income at event time t (solid line) and (b) with another that additionally controls for a dummy indicating whether a person is non-cohabiting or cohabiting at t (short dashed lines). We only observe the non-cohabiting vs. cohabiting status in Denmark. The figure focuses on mothers in Denmark (Figure E.4a), fathers in Denmark (Figure E.4b), mothers in Austria (Figure E.4c), and fathers in Austria (Figure E.4d). We estimate separate regressions for all specifications. Moreover, we obtain the event time coefficients from estimating regression (1) on a balanced sample of parents who have their first child between 2002 and 2007. The shaded areas represent 95% confidence intervals based on robust standard errors. The top-right corner of each figure also shows percentage effects at event time $t = 9$.

Figure E.5: Impacts of parenthood after controlling for potential mediators (PCT)



(a) Impacts for mothers in Denmark (PCT)

(b) Impacts for fathers in Denmark (PCT)

(c) Impacts for mothers in Austria (PCT)

(d) Impacts for fathers in Austria (PCT)

Notes: This figure shows the impacts of parenthood on antidepressant prescriptions after controlling for potential mediators (in percent). Specifically, it compares (a) the baseline specification (long dashed lines) with (b) one that controls for a dummy indicating whether a person earned income at event time t (solid line) and (b) with another that additionally controls for a dummy indicating whether a person is non-cohabiting or cohabiting at t (short dashed lines). We only observe the non-cohabiting vs. cohabiting status in Denmark. The figure focuses on mothers in Denmark (Figure E.5a), fathers in Denmark (Figure E.5b), mothers in Austria (Figure E.5c), and fathers in Austria (Figure E.5d). We estimate separate regressions for all specifications. Moreover, we obtain the event time coefficients from estimating regression (1) on a balanced sample of parents who have their first child between 2002 and 2007. The shaded areas represent 95% confidence intervals based on robust standard errors. The top-right corner of each figure also shows percentage effects at event time $t = 9$.

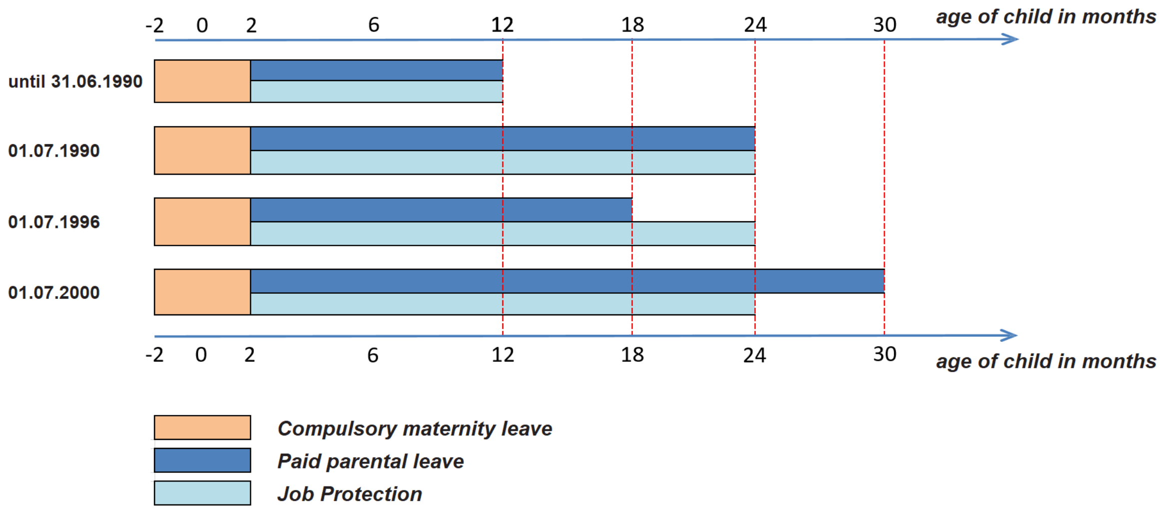
F Effects of other parental leave reforms: Additional details

F.1 Design of the other parental leave reforms

F.1.1 The Austrian parental leave reforms:

Austria implemented parental leave policy reforms in 1990, 1996, and 2000, primarily altering the length of parental leave. In response, mothers significantly adjusted their leave-taking behavior. By contrast, almost all fathers decided not to take parental leave, and the reforms did not affect this decision.[1] Because only mothers changed their behavior, we can exploit the reforms to assess the effects of extended maternity leave on parents' mental health. Our main analysis, nevertheless, focuses on the 2000 reform for reasons discussed below (Section F.2).

Figure F.1: Parental leave reforms in Austria



Notes: This figure summarizes the key aspects of the Austrian parental leave reforms. The 1990 reform introduced the possibility that parents could share parental leave. Before 1990, parental leave benefits were only available to mothers. In practice, the take-up of fathers was virtually nonexistent. Therefore, the reform can be interpreted as an increase in maternity leave from 12 to 24 months. The 1996 reform kept the total duration of parental leave constant at 24 months but introduced a new rule that one parent could not take more than 18 months. Thus, this reform decreased maternity leave from 24 to 18 months. The 2000 reform increased the maximum duration of parental leave to 36 months. However, one parent could not take more than 30 months. Thus, this reform increased maternity leave from 18 to 30 months. The reforms introduced strict birthday cutoffs that determined eligibility for the pre- or post-reform schemes.

In principle, all the reforms share features that allow us to examine them using the RD approach described in Section 6. First, each reform effectively changed the maximum paid *maternal* leave duration (Figure F.1). The 1990 reform increased it from 12 to 24 months, the 1996 reform decreased it from 24 to 18 months, and the 2000 reform increased it from 18 to 30 months. Second, the other aspects of the parental leave system, such as job protection or parental benefits, remained unchanged after 1990.[2] The reforms in 1996 and 2000,

[1]Around the 1990 reform, only 0.5 percent of fathers took any parental leave, around the 1996 and 2000 reforms these shares were 1.7 percent and 3 percent, respectively.

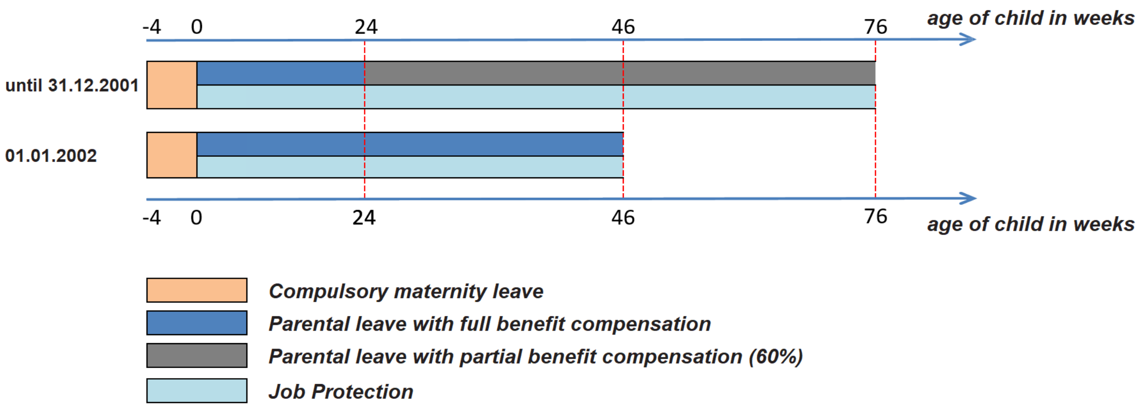
[2]The reform in 1990 changed the job protection period. Before this reform, mothers had the right to

therefore, allow us to estimate the pure effect of changing the leave duration. Instead, the 1990 reform jointly altered the leave and job protection period (Figure F.1), permitting us to gauge a combined effect. Third, all the reforms were implemented according to strict birth-date cutoffs (Figure F.1), determining eligibility for extended (or reduced) parental leave without any transition rule. This feature enables us to use RD designs. Fourth, the government announced and implemented the reforms at relatively short notice and the reforms affected the majority of mothers.

F.1.2 The Danish parental leave reform:

The Danish government also implemented a paid parental leave reform in 2002 compatible with our RD approach. As in Austria, policymakers embedded the reform in a parental leave system offering employment protection and wage compensation.

Figure F.2: Parental leave reform in Denmark



Notes: This figure summarizes the key aspects of the Danish parental leave reform. Before 2002, mothers could take 24 weeks of parental leave with full benefit compensation and 52 weeks at a reduced compensation rate of 60%. The 2002 reform (a) abolished the period with partial compensation and (b) expanded the period with full benefit compensation to 46 weeks. Before and after the reform, mothers benefited from job protection over the entire leave period. The reform introduced a strict birthday cutoff that determined eligibility for the pre- or post-reform scheme.

Figure F.2 provides a rough overview of the most important policy changes. As is apparent, the Danish reform altered the job-protected paid leave period. Before the reform, mothers could take a maximum of 24 weeks of fully paid parental leave.[3] During this period, they received "full benefit compensation."[4] Additionally, the regime allowed mothers to take up to 52 weeks of leave at a reduced benefit compensation (60%). The 2002 reform (a) abolished the period with partial compensation and (b) expanded the period with full benefit

return to their previous workers within 12 months. After the reform, the job-protection period was 24 months. Moreover, all the reforms did not change parental-benefit payments. Women received a flat payment (adjusted for wage inflation) over the entire leave period. Until 2008, these payments were unrelated to previous earnings or the leave duration.

[3]This period included 14 weeks of exclusive maternity leave and ten weeks of leave mothers and fathers had to share. Leave-taking of fathers reduced the mother's potential leave period.

[4]Full benefit compensation implies full wage payment during a period specified in the collective bargaining agreements in the employment sector and 90% wage compensation by the state for the remaining weeks with a cap on the total amount.

compensation to 46 weeks.[5] Crucially for our quasi-experimental design, the regime eligibility again depended on the child's date of birth. Mothers with births between January 1, 2002, and March 26, 2002, had the flexibility to choose their preferred regime, either the pre-reform or the post-reform one. Thus, mothers who gave birth between January 1 and March 26 could always choose the most beneficial scheme, and many of them faced incentives to increase the duration of leave with full compensation (Beuchert *et al.*, 2016). Mothers who gave birth after March 26 faced the new regime automatically.

F.2 Discussion of the other reforms

Given our data and the policy changes implemented, the Austrian 2000 reform is more useful for studying the impact of maternity leave than the other reforms. First, because the Austrian data commences in 1998, we do not observe all postnatal periods for the other Austrian reforms in 1990 and 1996. Thus, we can only analyze the longer-run and not the short-run effects of these other two reforms. Second, as discussed, the Austrian 2000 reform solely adjusted the length of leave, enabling us to estimate the clean impacts of extended leave-taking periods. The Danish reform, instead, varied a bundle of features, including the benefit schedule. Such changes to the budget set could directly influence mental health, potentially obscuring the effect of leave-taking. Third, the Austrian 2000 reform offers a larger first stage than most other reforms. For example, Danish mothers affected by the 2002 reform increased leave-taking by only 32 days (Beuchert *et al.*, 2016), and fathers did not change their behavior at all. Potential reasons for this small first stage are that the Danish reform introduced transition rules and imposed much more complicated monetary incentives than the Austrian 2000 reform. Therefore, we do not anticipate large impacts on mental health and parenthood penalties. The only other reform with a sizeable first stage is the Austrian 1990 reform. Given these considerations, our primary analysis centers on the Austrian 2000 reform. Nevertheless, we subsequently summarize the impacts of the Danish reform and the other two Austrian reforms.

F.3 Estimated effects of the other reforms

An analysis of the other parental leave reforms reinforces our central conclusion: Parental leave reforms that policymakers implemented in contexts with already long leave periods before the reform harm mothers' mental health. However, these adverse effects only manifest if the reforms cause substantial changes in mothers' leave-taking behavior. These results follow from the reform-specific first-stage estimates, the corresponding reduced-form estimates, and the resulting 2SLS estimates (reported in the Appendix).

Two of the four reforms substantially affect mothers' leave-taking behavior and their mental health. The first is the already discussed Austrian 2000 reform, and the other is the 1990 reform. As discussed, the 1990 reform expanded maternity leave by 12 months and mothers actually remained between ten and eleven additional months on leave (Figure F.3). This change in leave-taking behavior significantly affects mothers' mental health: The 2SLS estimates imply that an additional year of maternity leave increases the number of post-birth years with an antidepressant prescription by about 0.9 years or 60% (Table F.1). The reduced-form estimates (Figures F.4 to F.6) and the 2SLS estimates for the other two outcomes (Tables F.2 and F.3) confirm this finding. By definition, the estimates for this reform reflect long-run estimates. The reason is that while the reform took place in 1990, we

[5] The extended period included 14 weeks of maternity leave and 32 weeks of shared leave.

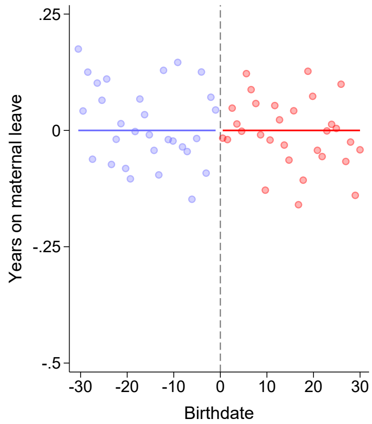
measure the outcome from 1998 to 2016. Moreover, the reform also did not affect fathers' mental health (Figures F.7 to F.9). In conclusion, the analysis of the Austrian 1990 reform leads to identical conclusions as our analysis of the 2000 reform.

The other two reforms induce smaller changes in mothers' leave-taking behavior and do not significantly affect their mental health. The Danish 2002 reform, for example, increases mothers' actual leave-taking by only one month. On top of that, the baseline leave period was short. Given the small first stage, the absence of significant effects on mothers' (Figure F.10 and Table F.4) and fathers' (Figure F.11 and Table F.5) mental health is no surprise. Also, examining the Austrian 1996 reform leads to similar conclusions. It decreases the actual leave period by about four months (Figure F.3), and it does not significantly affect all mental health outcomes (Figures F.4 to F.6 and Tables F.1 to F.3). However, consistent with the hypothesis that shorter leaves lead to better mental health, the reduced-form estimates have a positive sign. Also, this reform did change fathers' mental health (Figures F.7 to F.9).

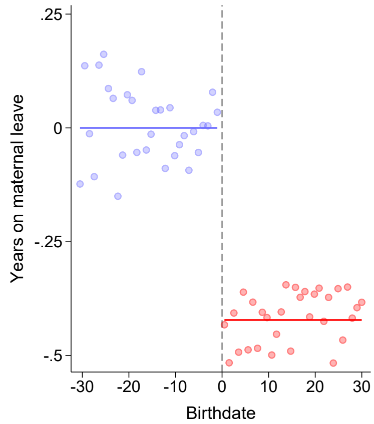
Figure F.3: Effects of other Austrian reforms on years of maternity leave (first stage)

1996 reform

(a) Pre-reform year 1995

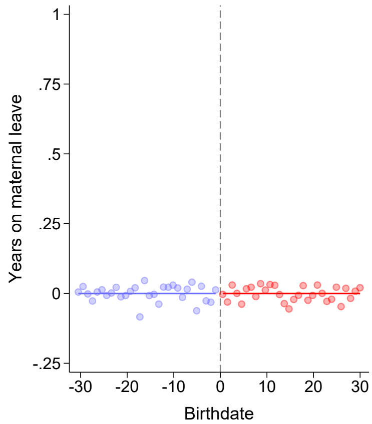


(b) Reform year 1996

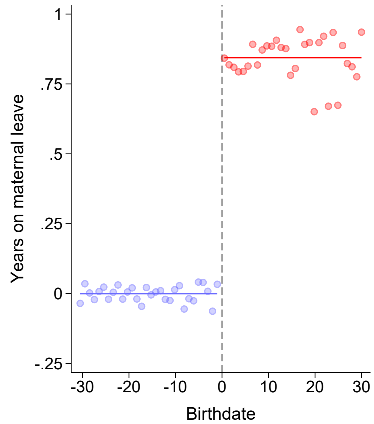


1990 reform

(c) Pre-reform year 1989

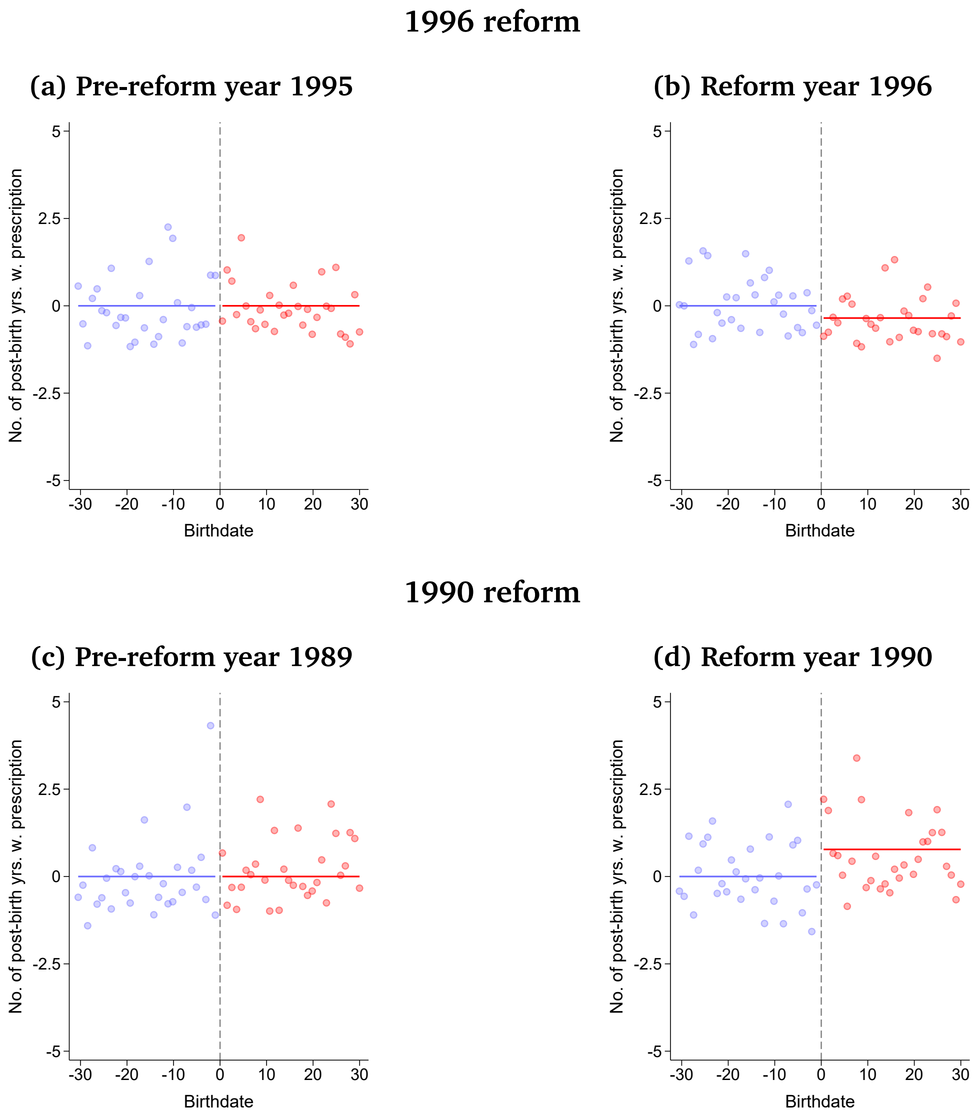


(d) Reform year 1990



Notes: This figure shows the effects of the other Austrian reforms on the number of taken maternity leave years (first stage). The figures are covariate adjusted. For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side).

Figure F.4: Reduced-form effects of other Austrian reforms on no. of years with prescriptions

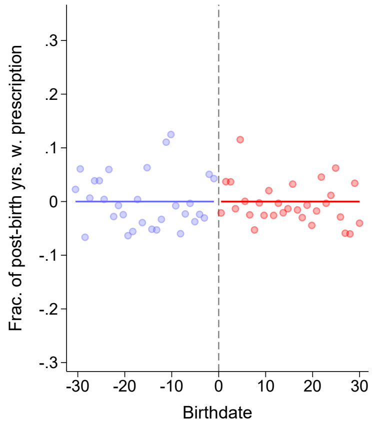


Notes: This figure shows the reduced-form effects of the other Austrian reforms on the number of post-birth years with antidepressant prescriptions for mothers. The figures are covariate adjusted. For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side).

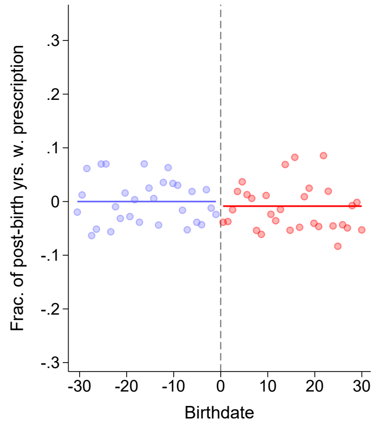
Figure F.5: Reduced-form effects of other Austrian reforms on frac. of years with prescriptions

1996 reform

(a) Pre-reform year 1995

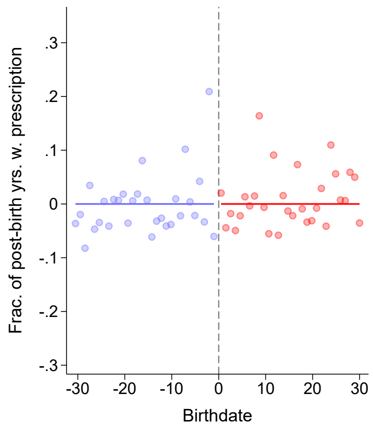


(b) Reform year 1996

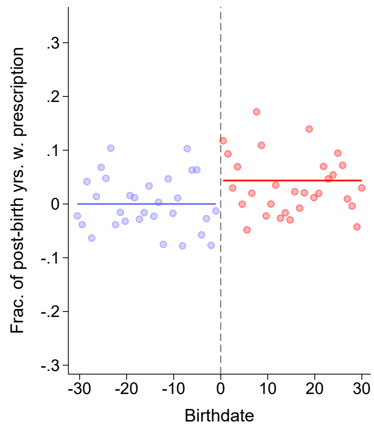


1990 reform

(c) Pre-reform year 1989

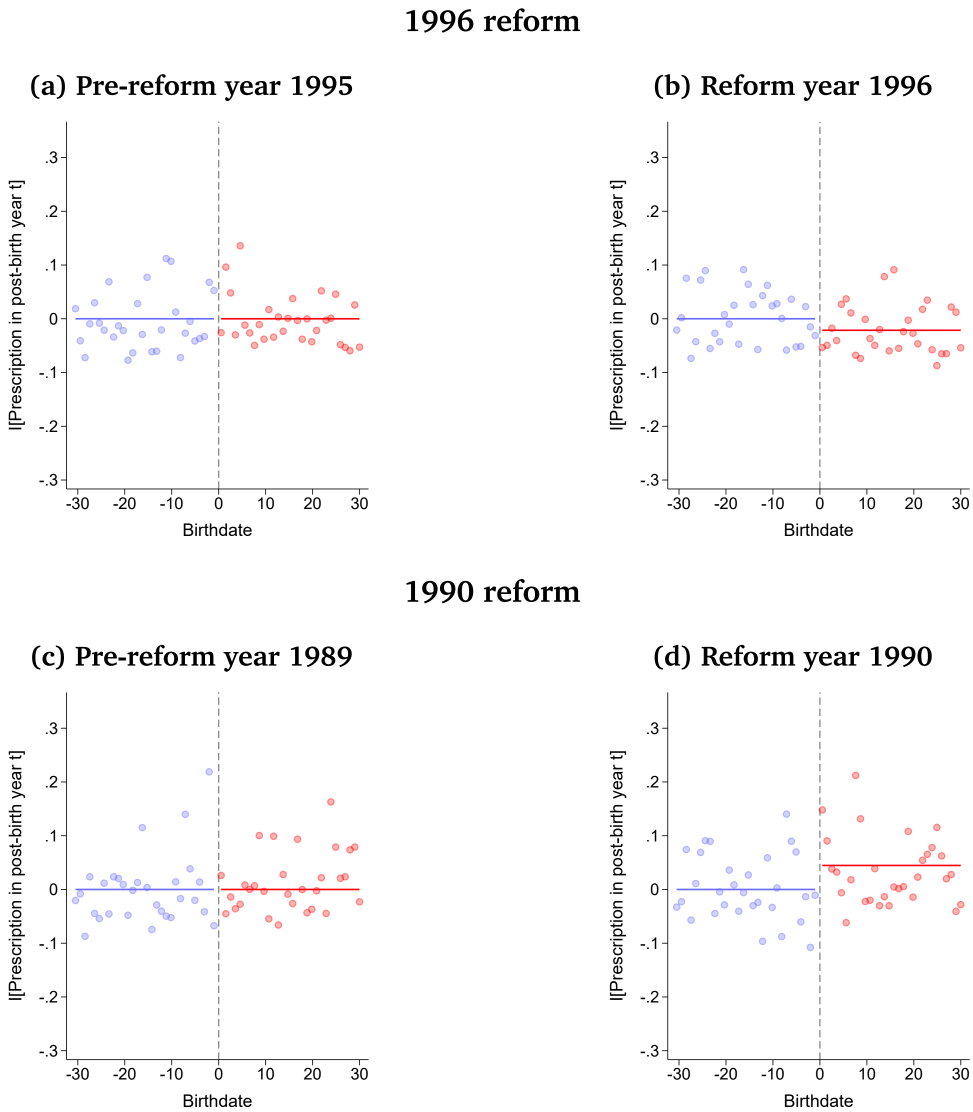


(d) Reform year 1990



Notes: This figure shows the reduced-form effects of the other Austrian reforms on the fraction of post-birth years with antidepressant prescriptions for mothers. The figures are covariate adjusted. For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side).

Figure F.6: Reduced-form effects of other Austrian reforms on yearly prescription probability

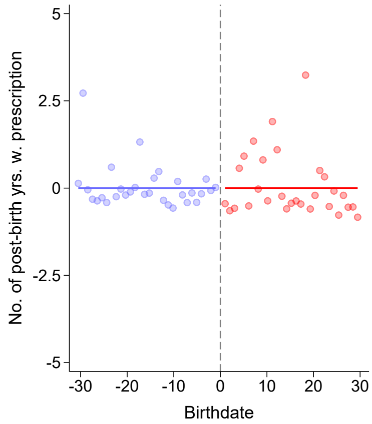


Notes: This figure shows the reduced-form effects of the other Austrian reforms on the yearly probability of receiving an antidepressant prescription in a given year for mothers. The figures are covariate adjusted. For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side). The underlying dataset has a panel structure and a binary variable that indicates years with a prescription serves as the outcome.

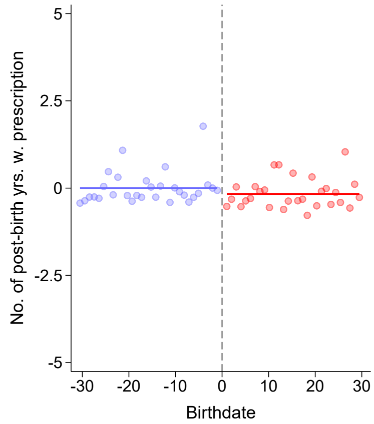
Figure F.7: Reduced-form effects of other Austrian reforms on no. of years with prescriptions (fathers)

1996 reform

(a) Pre-reform year 1995

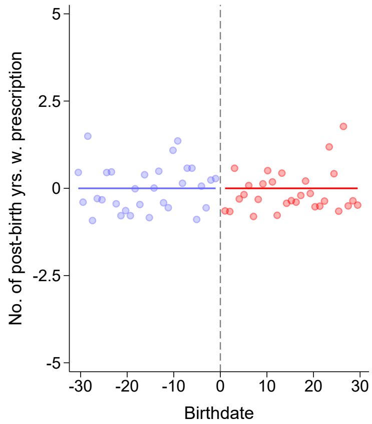


(b) Reform year 1996

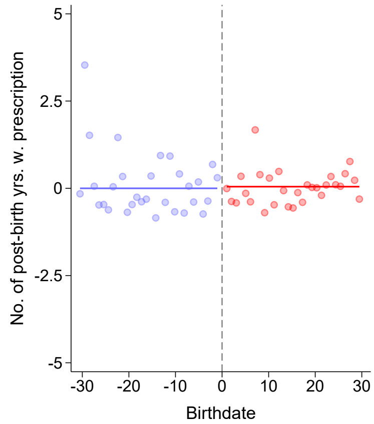


1990 reform

(c) Pre-reform year 1989



(d) Reform year 1990

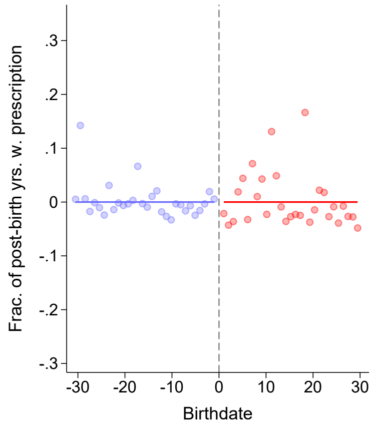


Notes: This figure shows the reduced-form effects of the other Austrian reforms on the number of post-birth years with antidepressant prescriptions for fathers. The figures are covariate adjusted. For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side).

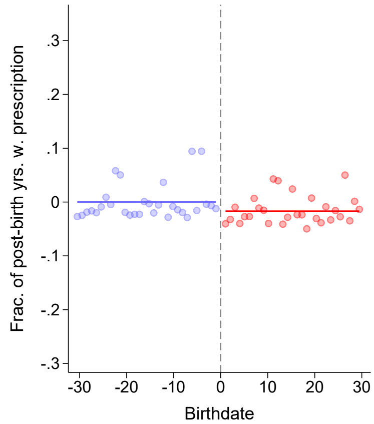
Figure F.8: Reduced-form effects of other Austrian reforms on frac. of years with prescriptions
(fathers)

1996 reform

(a) Pre-reform year 1995

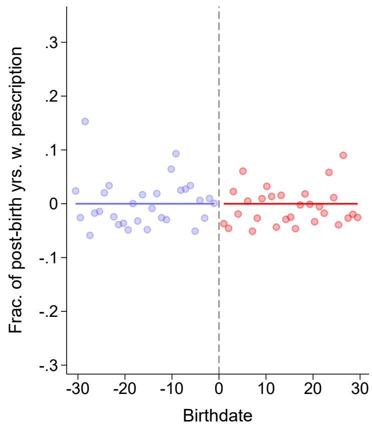


(b) Reform year 1996

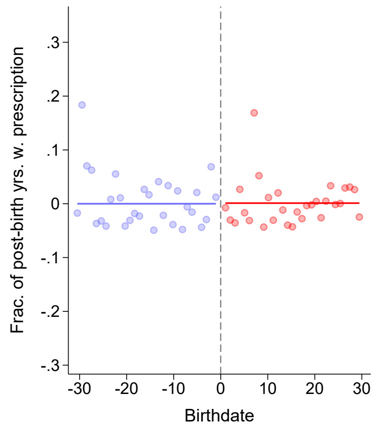


1990 reform

(c) Pre-reform year 1989



(d) Reform year 1990

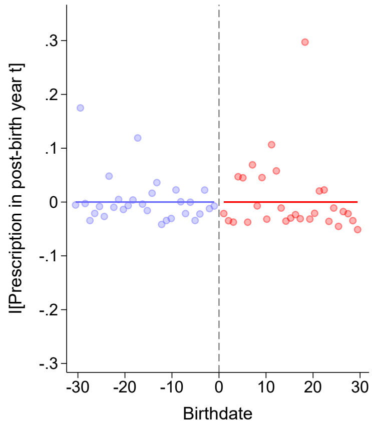


Notes: This figure shows the reduced-form effects of the other Austrian reforms on the fraction of post-birth years with antidepressant prescriptions for fathers. The figures are covariate adjusted. For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side).

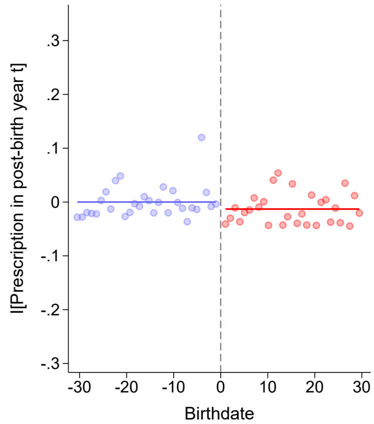
Figure F.9: Reduced-form effects of other Austrian reforms on yearly prescription probability
(fathers)

1996 reform

(a) Pre-reform year 1995

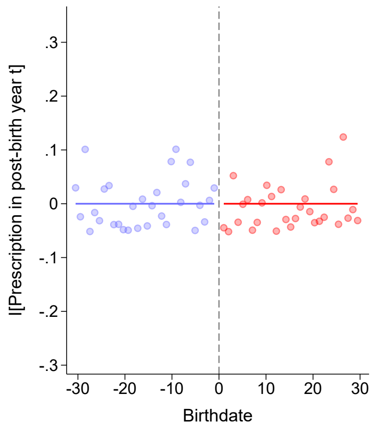


(b) Reform year 1996

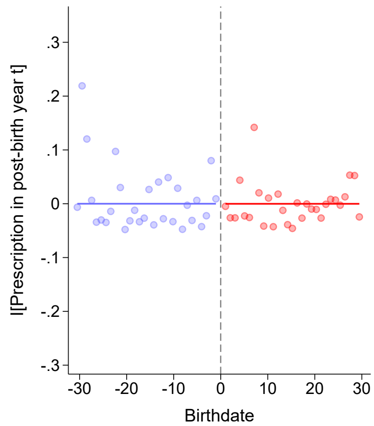


1990 reform

(c) Pre-reform year 1989



(d) Reform year 1990

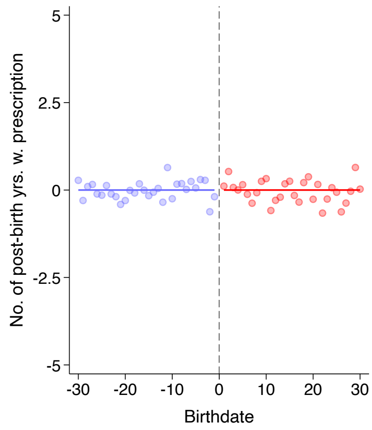


Notes: This figure shows the reduced-form effects of the other Austrian reforms on the yearly probability of receiving an antidepressant prescription in a given year for fathers. The figures are covariate adjusted. For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side). The underlying dataset has a panel structure and a binary variable that indicates years with a prescription serves as the outcome.

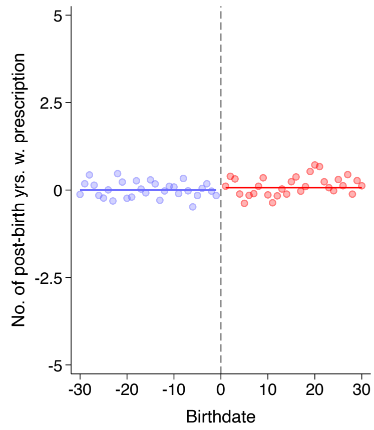
Figure F.10: Reduced-form impacts of the Danish 2002 reform on mother's mental health

Impact on the number of post-birth years with a prescription

(a) Pre-reform year 2000/01

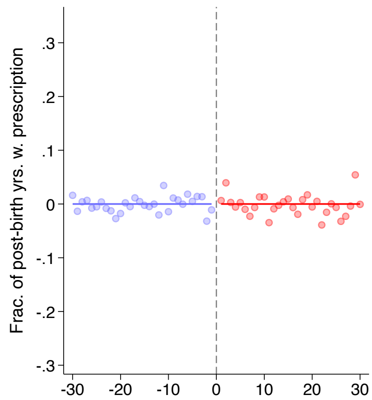


(b) Reform year 2001/02

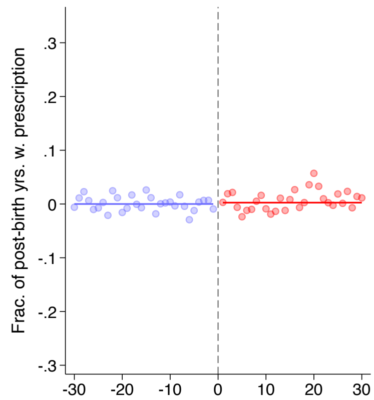


Impact on the fraction of post-birth years with a prescription

(c) Pre-reform year 2000/01



(d) Reform year 2001/02

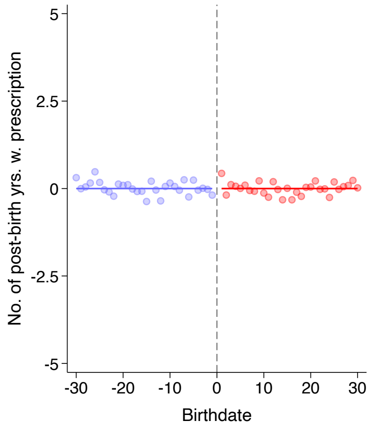


Notes: This figure focuses on mothers and shows the reduced-form impacts of the Danish 2002 reform on the number of post-birth years with antidepressant prescriptions (Figures F.10a and F.10b) and the fraction of post-birth years with antidepressant prescriptions (Figures F.10c and F.10d). For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side). Each circle represents an average for a particular day. The vertical line refers to the cutoff (January 1). The figures are covariate-adjusted. We adjust for covariates by (a) estimating the model (7), (b) setting α_5 to zero, (c) predicting the outcome $\hat{Y}_i$ for $\alpha_5 = 0$, (d) calculating the residual as $Y_i - \hat{Y}_i$, and (e) plotting the residuals. This procedure factors out trends along the running variable and pre-reform jumps at the cutoff. The post-birth sample runs from 2001 to 2016. The estimates rely on triangular weights and include the following covariates: The mother's age, the child's sex, family form at birth, high education dummies, and a dummy indicating the mother's immigrant status.

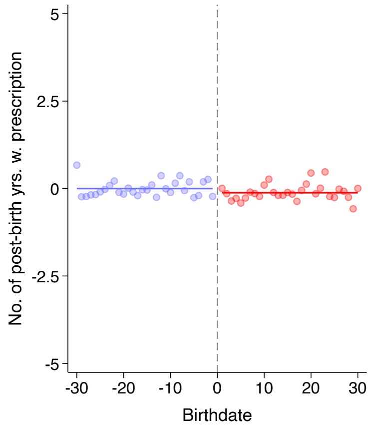
Figure F.11: Reduced-form impacts of the Danish 2002 reform on fathers' mental health

Impact on the number of post-birth years with a prescription

(a) Pre-reform year 2000/01

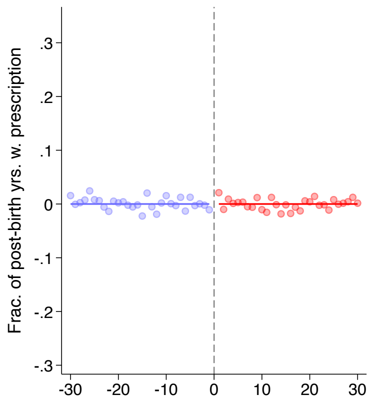


(b) Reform year 2001/02

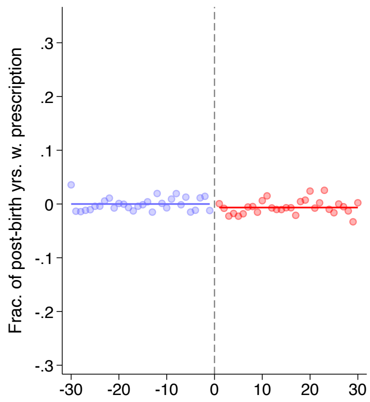


Impact on the fraction of post-birth years with a prescription

(c) Pre-reform year 2000/01



(d) Reform year 2001/02



Notes: This figure focuses on fathers and shows the reduced-form impacts of the Danish 2002 reform on the number of post-birth years with antidepressant prescriptions (Figures F.11a and F.11b) and the fraction of post-birth years with antidepressant prescriptions (Figures F.11c and F.11d). For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side). Each circle represents an average for a particular day. The vertical line refers to the cutoff (January 1). The figures are covariate-adjusted. We adjust for covariates by (a) estimating the model (7), (b) setting α_5 to zero, (c) predicting the outcome $\hat{Y}_i$ for $\alpha_5 = 0$, (d) calculating the residual as $Y_i - \hat{Y}_i$, and (e) plotting the residuals. This procedure factors out trends along the running variable and pre-reform jumps at the cutoff. The post-birth sample runs from 2001 to 2016. The estimates rely on triangular weights and include the following covariates: The father's age, the child's sex, family form at birth, high education dummies, and a dummy indicating the father's immigrant status.

Table F.1: Impact of ML duration on the number of post-birth years with AD prescriptions (LATEs)

	(1)	(2)	(3)	(4)
	30 day bandwidth		60 day bandwidth	
	Triangular	Covariates	Triangular	Covariates
1990 reform				
Years of maternity leave	0.9053**	0.9186**	0.4373	0.4495
	(2.0646)	(2.0786)	(1.4330)	(1.4798)
Observations	2,083	2,083	4,182	4,182
1996 reform				
Years of maternity leave	0.7388	0.8309	0.3391	0.3837
	(1.0480)	(1.2033)	(0.6502)	(0.7429)
Observations	2,171	2,171	4,215	4,215
2000 reform				
Years of maternity leave	0.7736**	0.8245***	0.6693***	0.6946***
	(2.4578)	(2.6354)	(3.0180)	(3.1459)
Observations	1,901	1,901	3,842	3,842

Notes: This table provides LATE estimates of an additional year of maternity leave on the number of post-birth years with antidepressant prescriptions. It focuses on all three reforms in 1990, 1992, and 2000. Columns (1) and (2) use a bandwidth of 30 days. Columns (4) and (5) rely on a bandwidth of 61 days. Column (1) and (3) employ triangular weights, and Column (2) and (4) combine triangular weights with covariates. They control for the mother's age, the child's sex, the child's legitimacy status, maternal education dummies, a dummy indicating whether the child is born preterm, and a dummy indicating whether the mother was born abroad. t statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table F.2: Impact of ML duration on the fraction of post-birth years with AD prescriptions (LATEs)

	(1)	(2)	(3)	(4)
	30 day bandwidth		60 day bandwidth	
	Triangular	Covariates	Triangular	Covariates
1990 reform				
Years of maternity leave	0.0488**	0.0517**	0.0278*	0.0297*
	(2.0457)	(2.1495)	(1.6630)	(1.7860)
Observations	2,083	2,083	4,182	4,182
1996 reform				
Years of maternity leave	0.0138	0.0204	-0.0063	-0.0039
	(0.3504)	(0.5356)	(-0.2159)	(-0.1343)
Observations	2,171	2,171	4,215	4,215
2000 reform				
Years of maternity leave	0.0410**	0.0445***	0.0380***	0.0404***
	(2.5128)	(2.7735)	(3.2589)	(3.4937)
Observations	1,901	1,901	3,842	3,842

Notes: This table provides LATE estimates of an additional year of maternity leave on the fraction of post-birth years with antidepressant prescriptions. It focuses on all three reforms in 1990, 1992, and 2000. Columns (1) and (2) use a bandwidth of 30 days. Columns (4) and (5) rely on a bandwidth of 61 days. Column (1) and (3) employ triangular weights, and Column (2) and (4) combine triangular weights with covariates. They control for the mother's age, the child's sex, the child's legitimacy status, maternal education dummies, a dummy indicating whether the child is born preterm, and a dummy indicating whether the mother was born abroad. t statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table F.3: Impact of ML duration on the prescription probability (LATEs)

	(1)	(2)	(3)	(4)
	30 day bandwidth		60 day bandwidth	
	Triangular	Covariates	Triangular	Covariates
1990 reform				
Years of maternity leave	0.0528**	0.0529**	0.0265	0.0266
	(1.9694)	(1.9894)	(1.4160)	(1.4316)
Observations	33,418	33,418	66,913	66,913
1996 reform				
Years of maternity leave	0.0415	0.0498	0.0189	0.0260
	(0.9630)	(1.1696)	(0.5880)	(0.8143)
Observations	34,137	34,137	66,765	66,765
2000 reform				
Years of maternity leave	0.0490**	0.0518**	0.0437***	0.0468***
	(2.3670)	(2.5264)	(2.9936)	(3.2168)
Observations	28,129	28,129	56,699	56,699

Notes: This table provides LATE estimates of an additional year of maternity leave on the probability of receiving an antidepressant prescription in a given year. The underlying dataset has a panel structure and a binary variable that indicates years with an antidepressant prescription serves as an outcome. The table focuses on all three reforms in 1990, 1992, and 2000. Columns (1) and (2) use a bandwidth of 30 days. Columns (4) and (5) rely on a bandwidth of 61 days. Column (1) and (3) employ triangular weights, and Column (2) and (4) combine triangular weights with covariates. They control for the mother's age, the child's sex, the child's legitimacy status, maternal education dummies, a dummy indicating whether the child is born preterm, and a dummy indicating whether the mother was born abroad. We cluster the standard errors at the individual level. t statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table F.4: Reduced-form impact of the Danish 2002 reform on mothers' mental health

	(1) Triangular	(2) Unweighted	(3) Covariates
A. Impact on the fraction of post-birth years with AD prescriptions			
Reform effect	0.0007	0.0032	0.0025
	(0.1112)	(0.5612)	(0.3913)
Mean of outcome		0.0596	
B. Impact on the number of post-birth years with AD prescriptions			
Reform effect	0.0281	0.0601	0.0696
	(0.2493)	(0.5944)	(0.6232)
Mean of outcome		1.0295	
Observations		10,128	

Notes: This table provides reduced-form estimates for the impact of the Danish 2002 reform on women's fraction of post-birth years with antidepressant prescriptions (Panel A) and the number of post-birth years with antidepressant prescriptions (Panel B). The estimates rely on a bandwidth of 30 days. Column (1) uses triangular weights, Column (2) does not use any weighting, and Column (3) combines triangular weights with covariates. It controls for the mother's age, the child's sex, family form at birth, high education dummies, and a dummy indicating the mother's immigrant status. t statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table F.5: Reduced-form impact of the Danish 2002 reform on fathers' mental health

	(1) Triangular	(2) Unweighted	(3) Covariates
A. Impact on the fraction of post-birth years with AD prescriptions			
Reform effect	-0.0062	-0.0029	-0.0066
	(-1.2296)	(-0.6612)	(-1.3058)
Mean of outcome		0.0333	
B. Impact on the number of post-birth years with AD prescriptions			
Reform effect	-0.1171	-0.0646	-0.1199
	(-1.3452)	(-0.8417)	(-1.3802)
Mean of outcome		0.5748	
Observations		9,683	

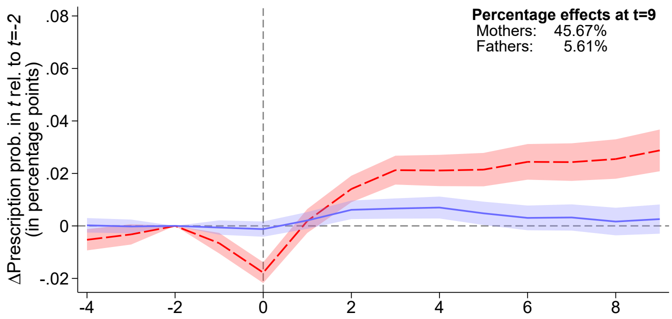
Notes: This table provides reduced-form estimates for the impact of the Danish 2002 reform on men's fraction of post-birth years with antidepressant prescriptions (Panel A) and the number of post-birth years with antidepressant prescriptions (Panel B). The estimates rely on a bandwidth of 30 days. Column (1) uses triangular weights, Column (2) does not use any weighting, and Column (3) combines triangular weights with covariates. It controls for the father's age, the child's sex, family form at birth, high education dummies, and a dummy indicating the father's immigrant status. t statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

G Additional estimation output

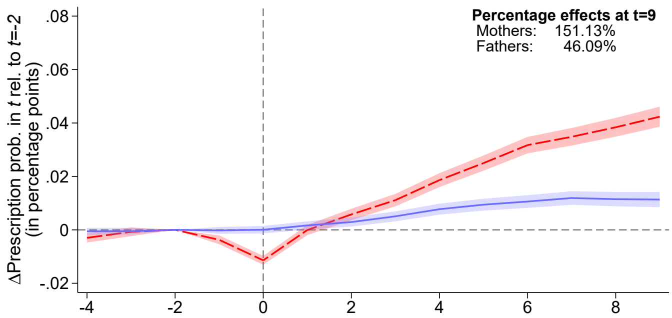
G.1 Additional figures

Figure G.1: Impacts of parenthood by number of children in Denmark

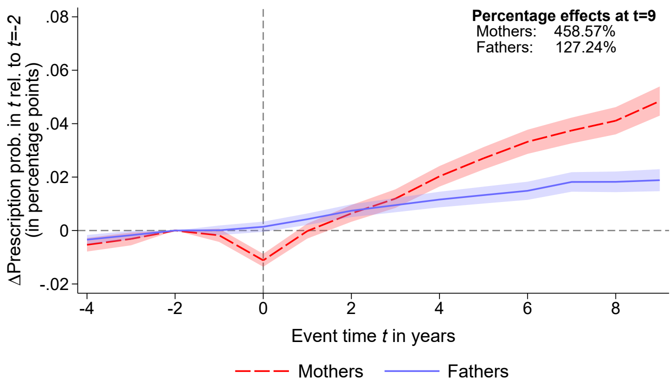
(a) Impacts for sub-sample with one child (PP)



(b) Impacts for sub-sample with two children (PP)



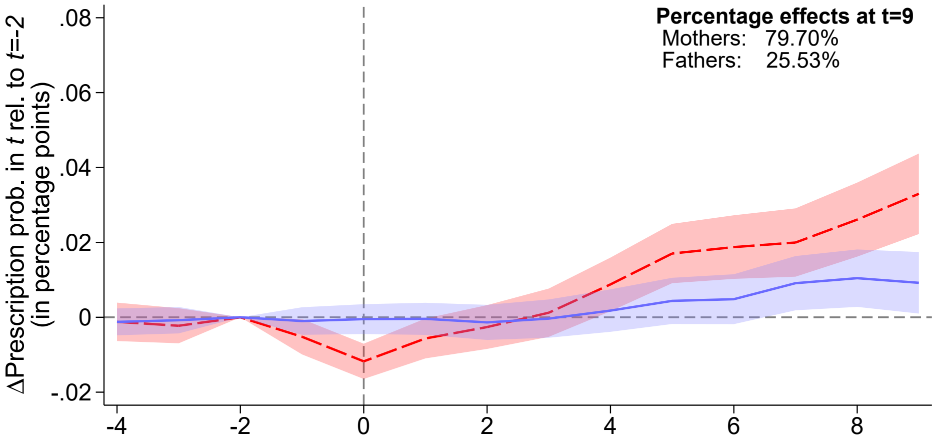
(c) Impacts for sub-sample with three or more children (PP)



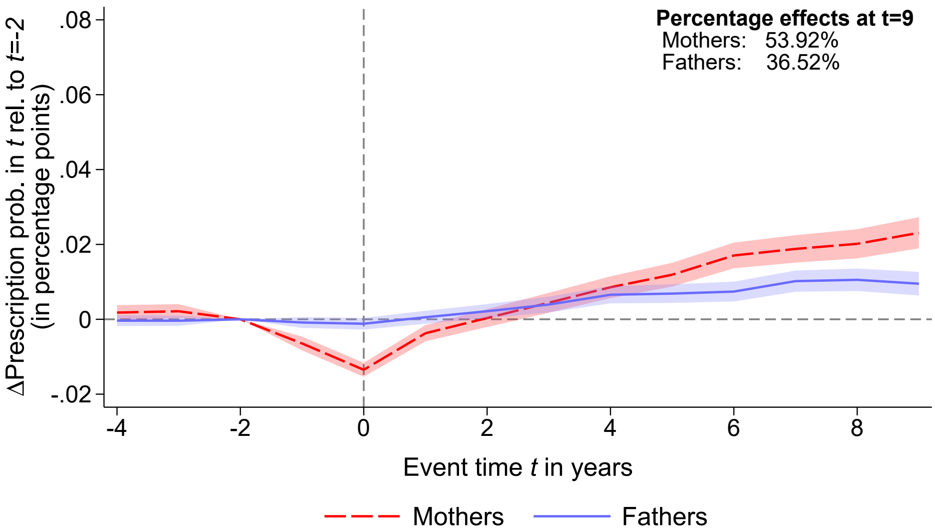
Notes: This figure focuses on Denmark and shows the estimated impacts of parenthood on antidepressant prescriptions by the number of children (in percentage points). The dashed lines refer to mothers ($j = m$) and the solid lines to fathers ($j = f$). We estimate separate regressions around the birth of the first child for parents with one child (Figure G.1a), two children (Figure G.1b), and three or more children (Figure G.1c). Moreover, we obtain the event time coefficients by estimating regression (1) on a balanced sample of parents who have their first child between 2002 and 2007. The shaded areas represent 95% confidence intervals based on robust standard errors. The top-right corner of each figure also shows percentage effects at event time $t = 9$.

Figure G.2: Impacts of parenthood on antidepressant prescriptions (married parents)

(a) Impacts on prescriptions in Austria (PP)

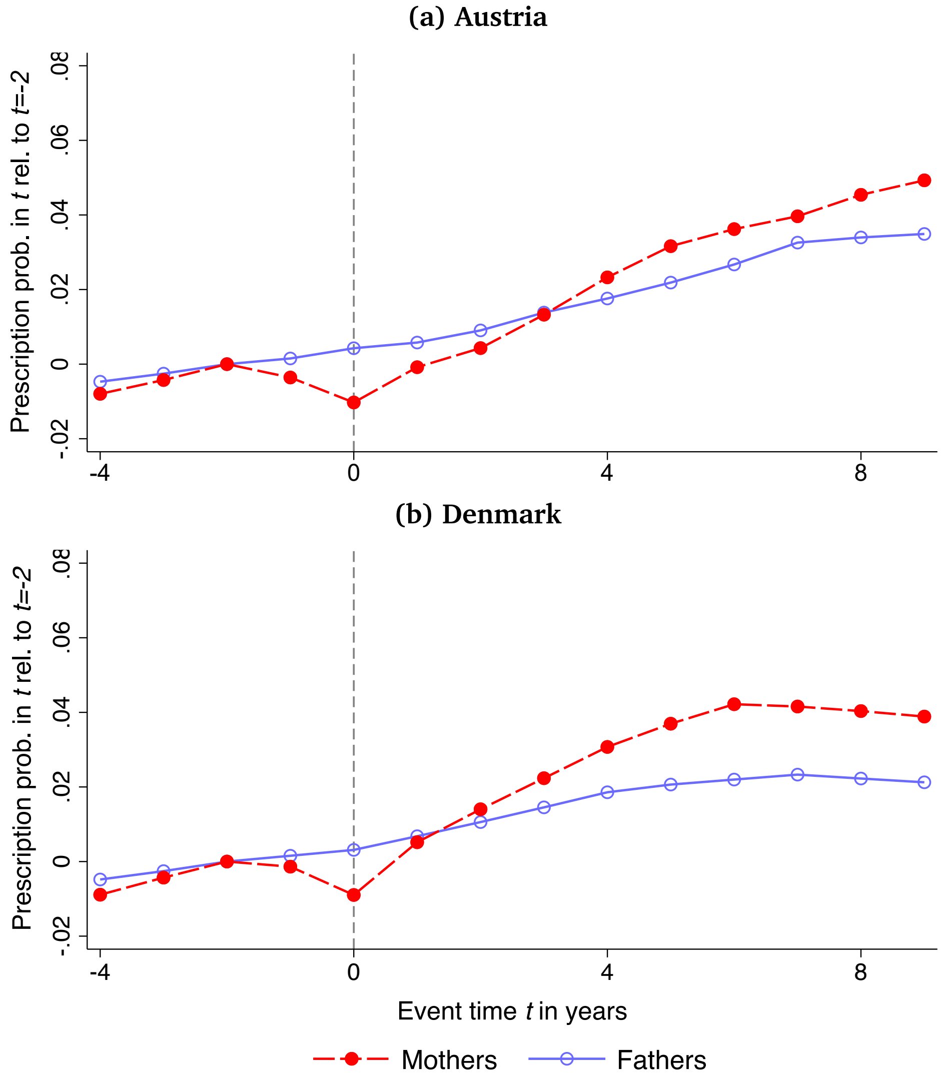


(b) Impacts on prescriptions in Denmark (PP)



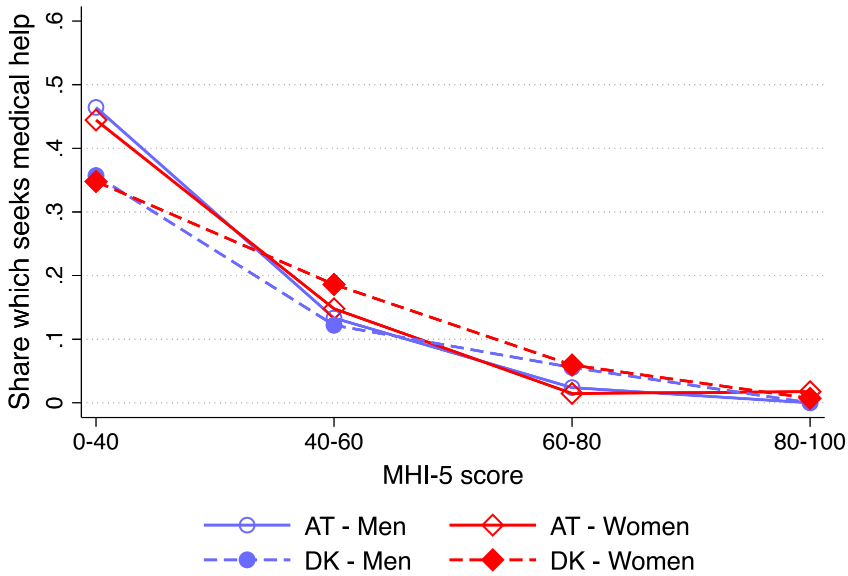
Notes: This figure focuses on married parents and shows the estimated impacts of parenthood on antidepressant prescriptions before and after having the first child (in percentage points). Figure G.2a presents results for Austria, and Figure G.2b for Denmark. The dashed lines refer to mothers ($j = m$) and the solid lines to fathers ($j = f$). We obtain the event time coefficients from estimating regression (1) on a balanced sample of parents who have their first child between 2002 and 2007. The shaded areas represent 95% confidence intervals based on robust standard errors. The top-right corner of each figure also shows percentage effects at event time $t = 9$.

Figure G.3: Impacts of parenthood on antidepressant prescriptions (raw data)



Notes: This figure focuses plots the raw data around childbirth. It focuses on mothers ($j = m$, dashed lines) and fathers ($j = f$, solid lines). Figure G.3a is for Austria and Figure G.3b for Denmark. Each dot represents an event-time-specific average.

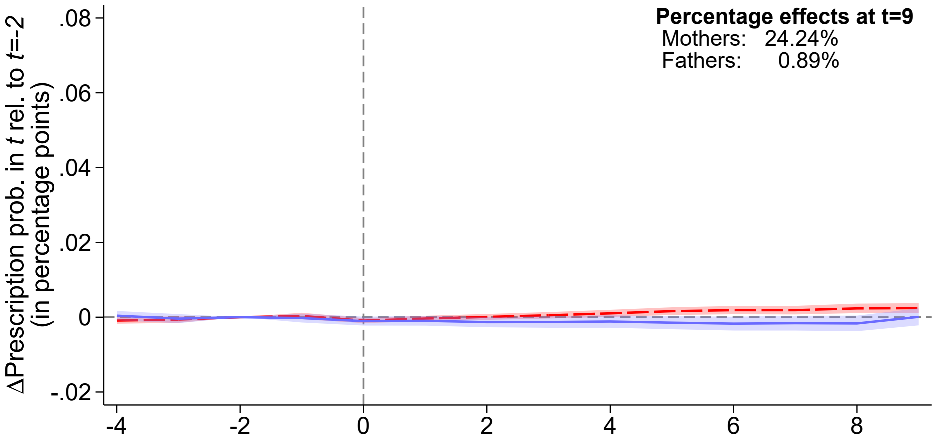
Figure G.4: Help-seeking behavior by MHI-5 score among women and men in Austria and Denmark



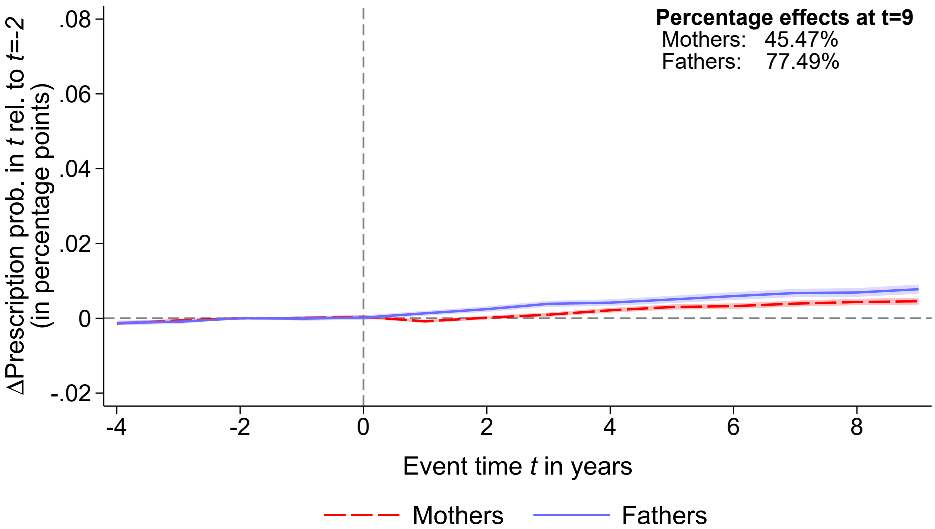
Notes: This graph correlates two variables from the *Special Eurobarometer* 246 in 2005/06 for men and women in Austria and Denmark respectively. The horizontal axis shows the MHI-5 score. This is a survey instrument for assessing the mental health of adults, with a scale ranging from 0 (indicating a high level of mental disorders) to 100 (representing optimal mental health). The vertical axis shows the proportion of the population who have sought professional help for mental health problems, based on the following question: "*In the last 12 months, did you seek help from a professional regarding a psychological or emotional health problem?*".

Figure G.5: Impacts of parenthood on the use of psychoactive substances

(a) Impacts on psychoactive substances in Austria (PP)



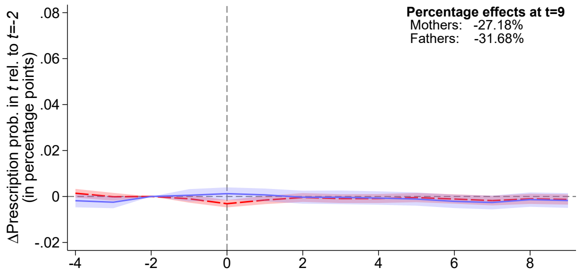
(b) Impacts on psychoactive substances in Denmark (PP)



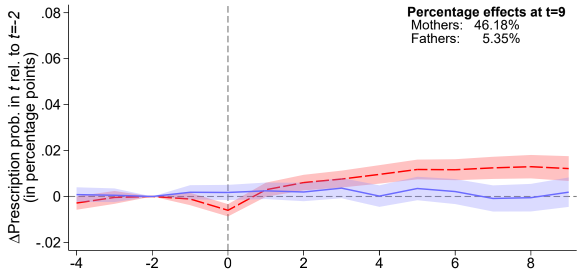
Notes: This figure shows the estimated impacts of parenthood on a dummy variable measuring the use of psychoactive substances before and after the birth of the first child (in percentage points). We construct this variable to indicate whether (a) an individual received an in-patient diagnosis of a mental or behavioral disorder due to the use of psychoactive substances or (b) obtained drugs used in addictive disorders. Figures G.5a shows results for Austria, and Figures G.5b for Denmark. The dashed lines refer to mothers ($j = m$) and the solid lines to fathers ($j = f$). We obtain the event time coefficients from estimating regression (1) on a balanced sample of parents who have their first child between 2002 and 2007. The shaded areas represent 95% confidence intervals based on robust standard errors. We bound counterfactual predictions at 0.01 to avoid implausible negative values. The top-right corner of each figure also shows percentage effects at event time $t = 9$.

Figure G.6: Impacts of parenthood on the use of certain medications in Austria

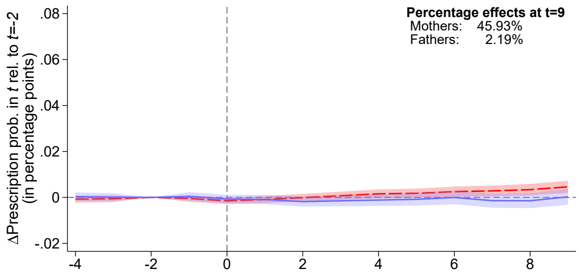
(a) Impacts on Anesthetics (PP)



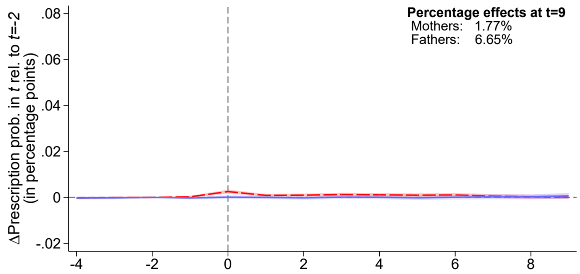
(b) Impacts on Analgesics (PP)



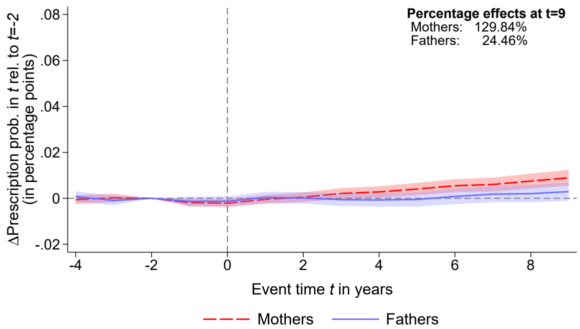
(c) Impacts on Antiepileptics (PP)



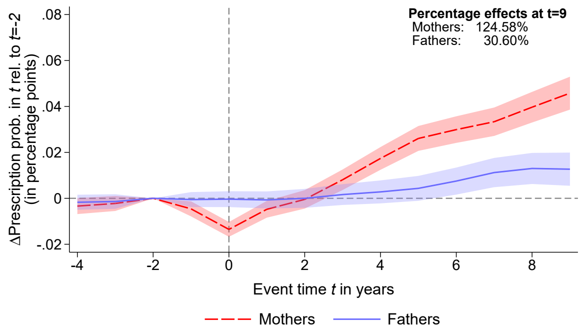
(d) Impacts on Anti-Parkinson drugs (PP)



(e) Impacts on Psycholeptics (PP)



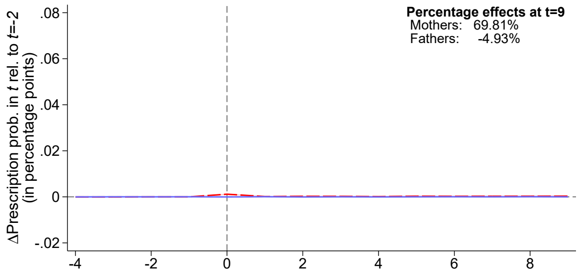
(f) Impacts on Psychoanaleptics (PP)



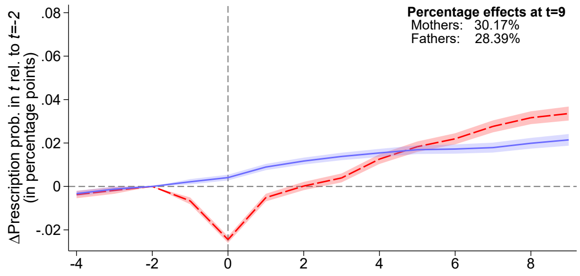
Notes: This figure shows the estimated impacts of parenthood on the use of certain medications in Austria before and after having the first child (in percentage points). The dashed lines refer to mothers ($j = m$) and the solid lines to fathers ($j = f$). Figure G.6a focuses on anesthetics (mainly drugs used to induce anesthesia), Figure G.6b on analgesics (mainly drugs used to relieve pain), Figure G.6c on antiepileptics (mainly drugs used in the treatment of epileptic seizures), Figure G.6d on anti-Parkinson drugs (mainly drugs used in the treatment of Parkinson's disease), Figure G.6e on psycholeptics (mainly drugs used to produce calming effects upon a person), and Figure G.6f on psychoanaleptics (central nervous system stimulants). We obtain the event time coefficients from estimating regression (1) on a balanced sample of parents who have their first child between 2002 and 2007. The shaded areas represent 95% confidence intervals based on robust standard errors. For drugs that are prescribed very rarely (i.e., Figure G.6c and Figure G.6d), we bound counterfactual predictions at 0.01 to avoid implausible negative values. The top-right corner of each figure also shows percentage effects at event time $t = 9$.

Figure G.7: Impacts of parenthood on the use of certain medications in Denmark

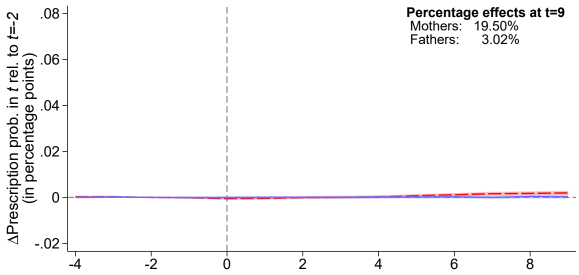
(a) Impacts on Anesthetics (PP)



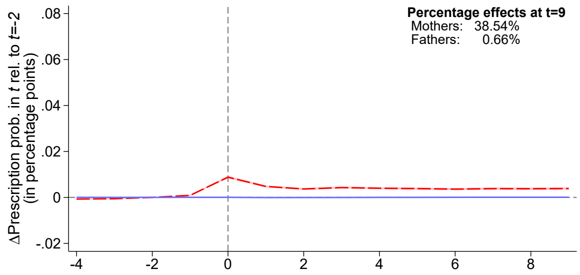
(b) Impacts on Analgesics (PP)



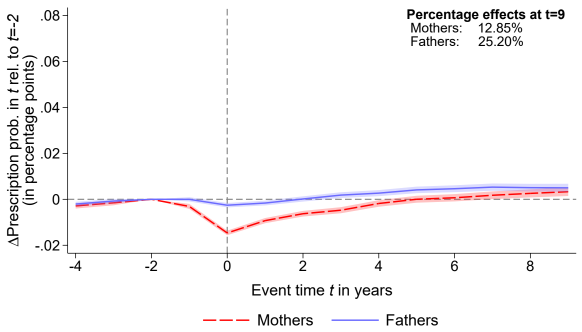
(c) Impacts on Antiepileptics (PP)



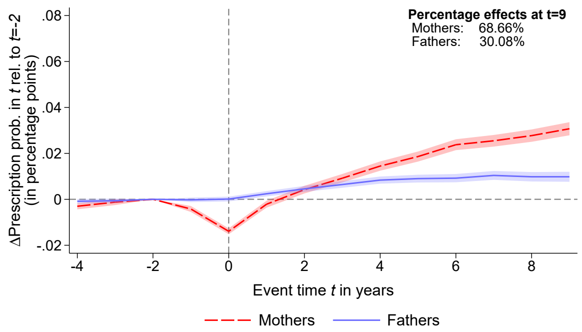
(d) Impacts on Anti-Parkinson drugs (PP)



(e) Impacts on Psycholeptics (PP)

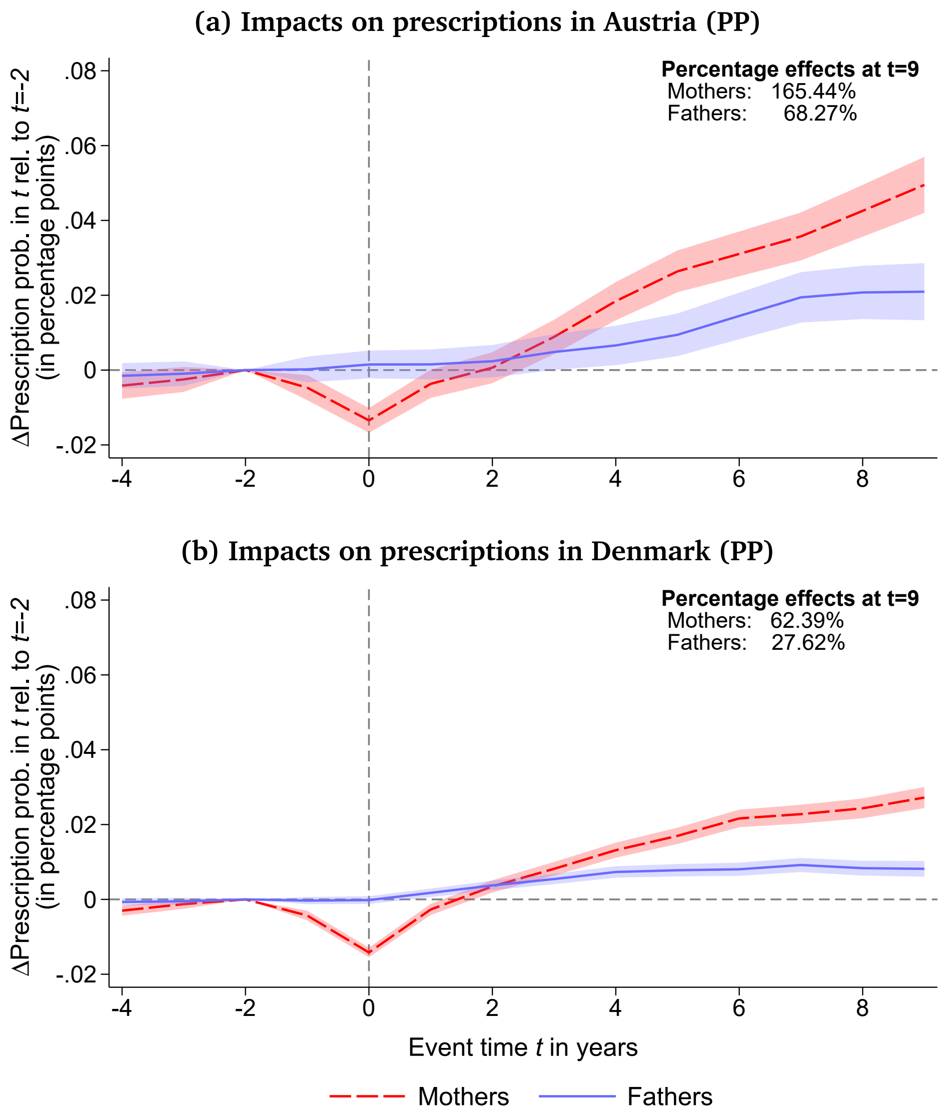


(f) Impacts on Psychoanaleptics (PP)



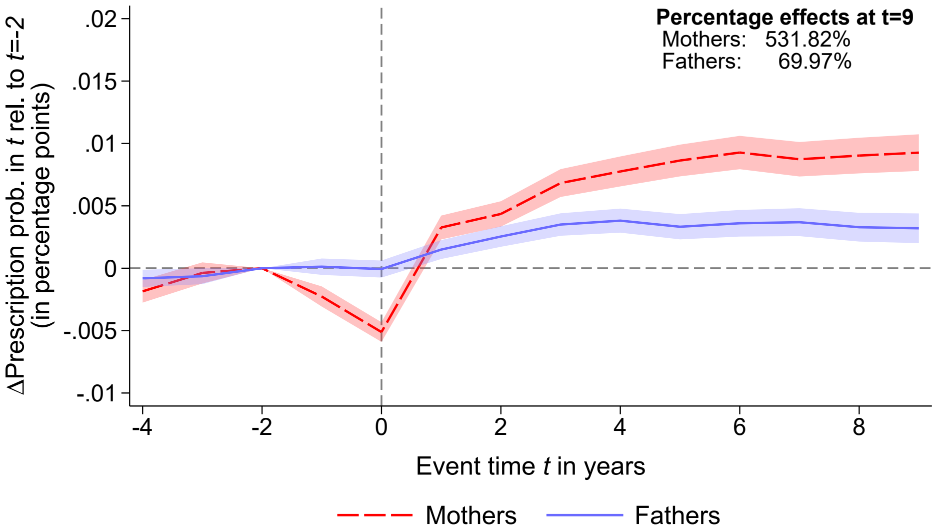
Notes: This figure shows the estimated impacts of parenthood on the use of certain medications in Denmark before and after having the first child (in percentage points). The dashed lines refer to mothers ($j = m$) and the solid lines to fathers ($j = f$). Figure G.7a focuses on anesthetics (mainly drugs used to induce anesthesia), Figure G.7b on analgesics (mainly drugs used to relieve pain), Figure G.7c on antiepileptics (mainly drugs used in the treatment of epileptic seizures), Figure G.7d on anti-Parkinson drugs (mainly drugs used in the treatment of Parkinson's disease), Figure G.7e on psycholeptics (mainly drugs used to produce calming effects upon a person), and Figure G.7f on psychoanaleptics (central nervous system stimulants). We obtain the event time coefficients from estimating regression (1) on a balanced sample of parents who have their first child between 2002 and 2007. The shaded areas represent 95% confidence intervals based on robust standard errors. For drugs that are prescribed very rarely (i.e., Figure G.7c and Figure G.7d), we bound counterfactual predictions at 0.01 to avoid implausible negative values. The top-right corner of each figure also shows percentage effects at event time $t = 9$.

Figure G.8: Impacts for mothers who have not been diagnosed with postpartum depression



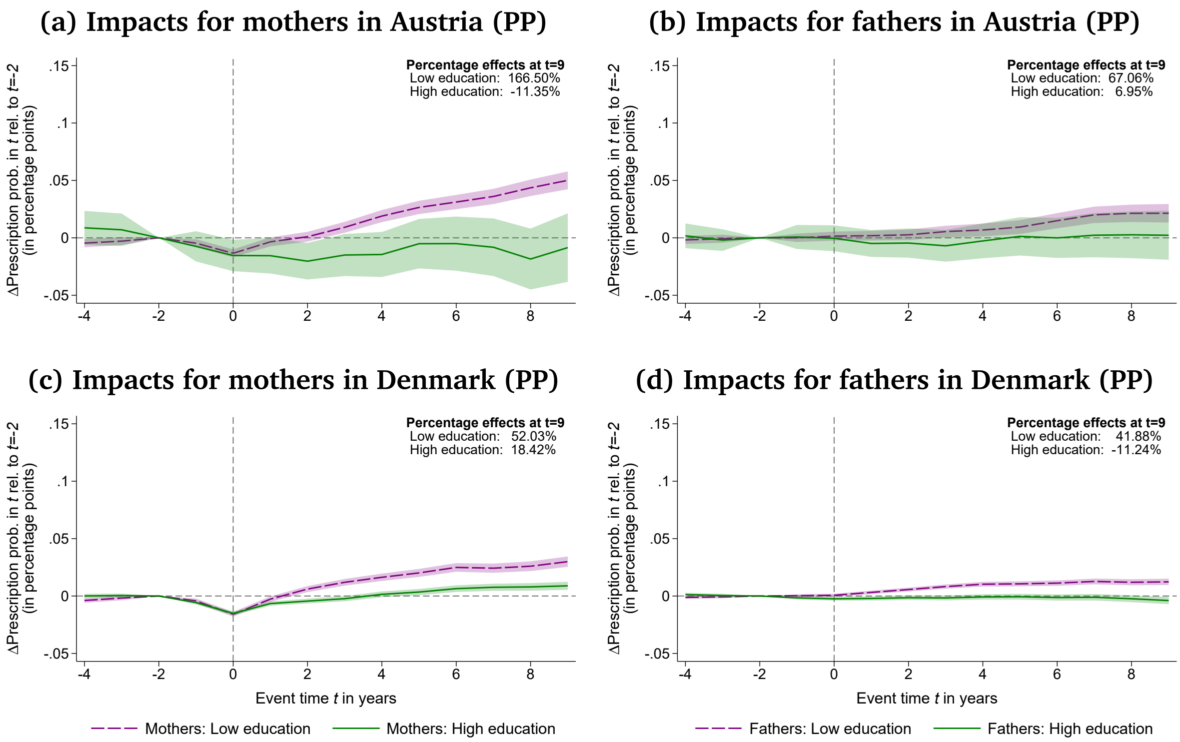
Notes: This figure shows the estimated impacts of parenthood on antidepressant prescriptions before and after having the first child (in percentage points). The sample consists of mothers who have not been diagnosed with postpartum depressions (in the inpatient sector). Figures G.8a shows results for Austria, and Figures G.8b for Denmark. The dashed lines refer to mothers ($j = m$) and the solid lines to fathers ($j = f$). We obtain the event time coefficients from estimating regression (1) on a balanced sample of parents who have their first child between 2002 and 2007. The shaded areas represent 95% confidence intervals based on robust standard errors. The top-right corner of each figure also shows percentage effects at event time $t = 9$.

Figure G.9: Impacts of parenthood on first antidepressant prescriptions in Denmark (PP)



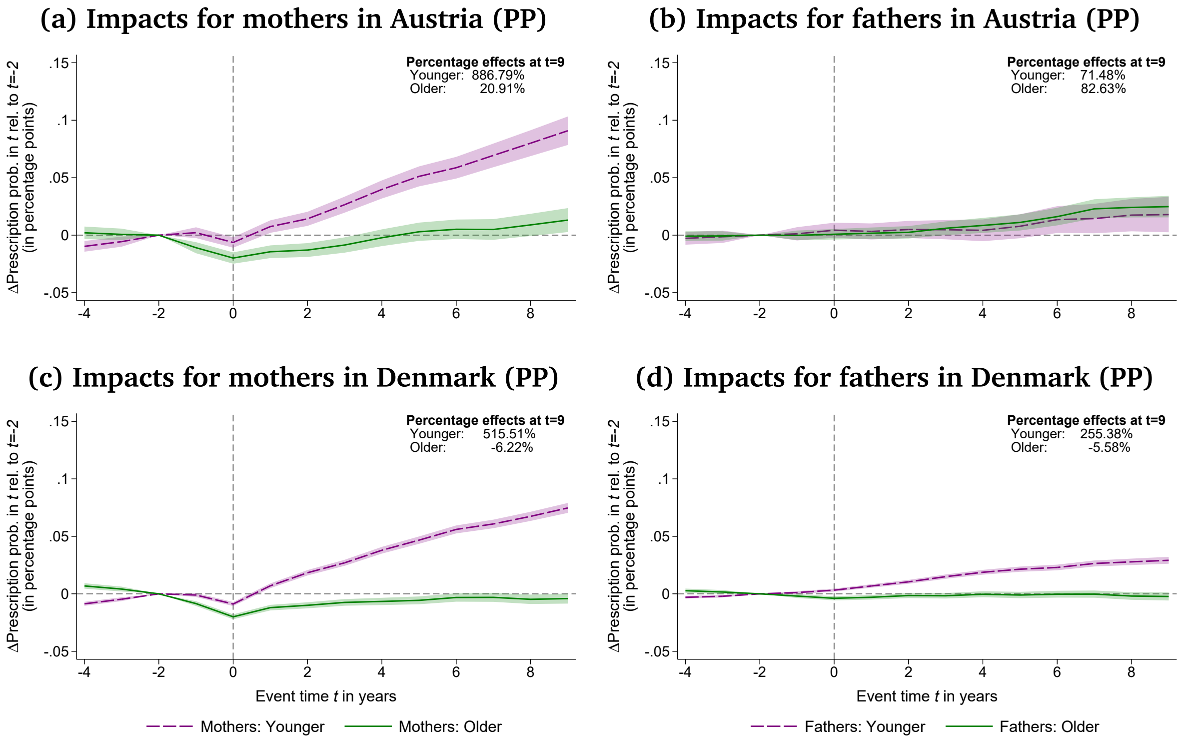
Notes: This figure shows the estimated impacts of parenthood on first antidepressant prescriptions before and after having the first child (in percentage points). The dashed lines refer to Danish mothers ($j = m$) and the solid lines to Danish fathers ($j = f$). We obtain the event time coefficients from estimating regression (1) on a balanced sample of parents who have their first child between 2002 and 2007. The shaded areas represent 95% confidence intervals based on robust standard errors. The top-right corner of each figure also shows percentage effects at event time $t = 9$.

Figure G.10: Impacts of parenthood on prescriptions by educational attainment



Notes: This figure shows the impacts of parenthood on antidepressant prescriptions by educational attainment (in percentage points). It focuses on mothers in Austria (Figure G.10a), fathers in Austria (Figure G.10b), mothers in Denmark (Figure G.10c), and fathers in Denmark (Figure G.10d). We estimate separate regressions for parents with low educational attainment (ISCED levels 1 through 4) and high educational attainment (ISCED levels 5 or 6). Moreover, we obtain the event time coefficients from estimating regression (1) on a balanced sample of parents who have their first child between 2002 and 2007. The shaded areas represent 95% confidence intervals based on robust standard errors. The top-right corner of each figure also shows percentage effects at event time $t = 9$.

Figure G.11: Impacts of parenthood on prescriptions by parents' age



(a) Impacts for mothers in Austria (PP)

(b) Impacts for fathers in Austria (PP)

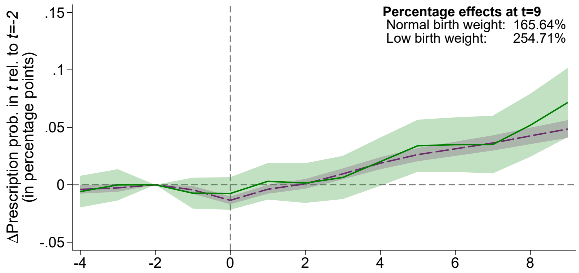
(c) Impacts for mothers in Denmark (PP)

(d) Impacts for fathers in Denmark (PP)

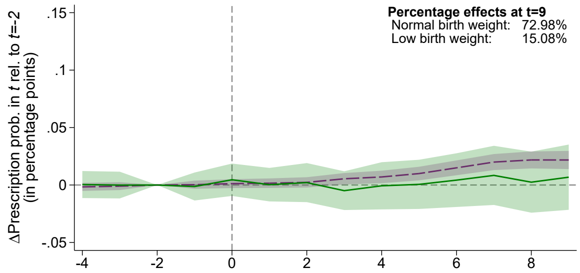
Notes: This figure shows the impacts of parenthood on antidepressant prescriptions by parents' age (in percentage points). It focuses on mothers in Austria (Figure G.11a), fathers in Austria (Figure G.11b), mothers in Denmark (Figure G.11c), and fathers in Denmark (Figure G.11d). We estimate separate regressions for parents below and above the group-specific median age. Moreover, we obtain the event time coefficients from estimating regression (1) on a balanced sample of parents who have their first child between 2002 and 2007. The shaded areas represent 95% confidence intervals based on robust standard errors. We bound counterfactual predictions at 0.01 to avoid implausible negative values. The top-right corner of each figure also shows percentage effects at event time $t = 9$.

Figure G.12: Impacts of parenthood on prescriptions by child's birth weight

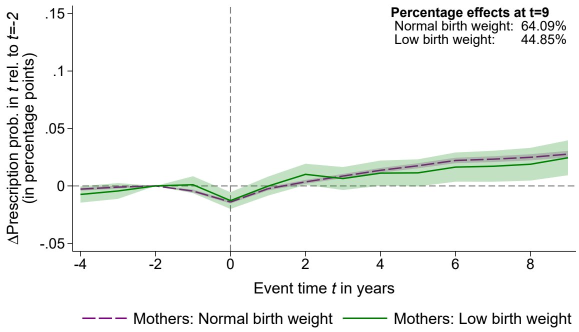
(a) Impacts for mothers in Austria (PP)



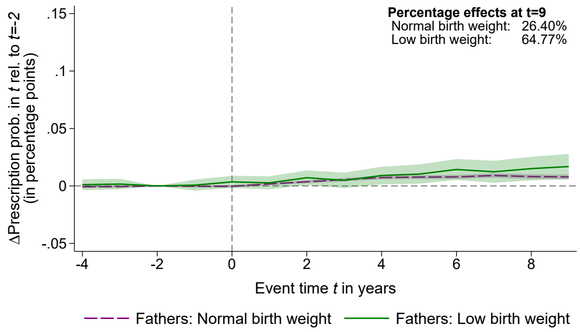
(b) Impacts for fathers in Austria (PP)



(c) Impacts for mothers in Denmark (PP)

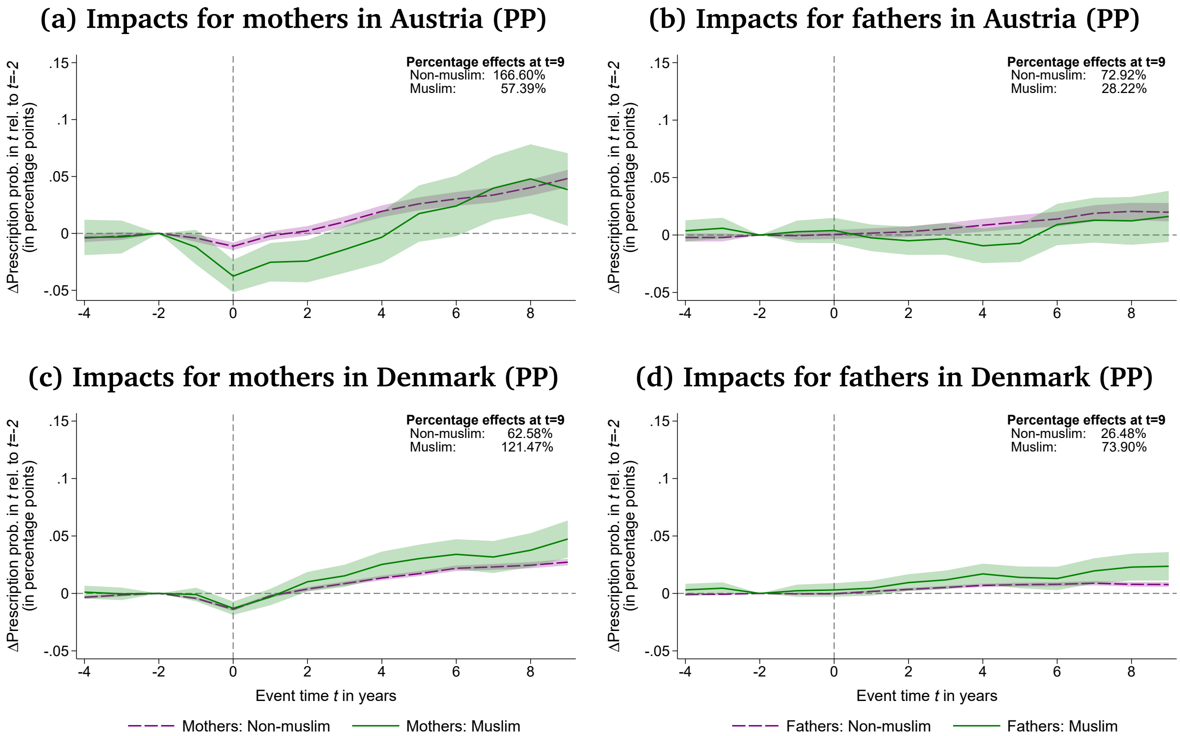


(d) Impacts for fathers in Denmark (PP)



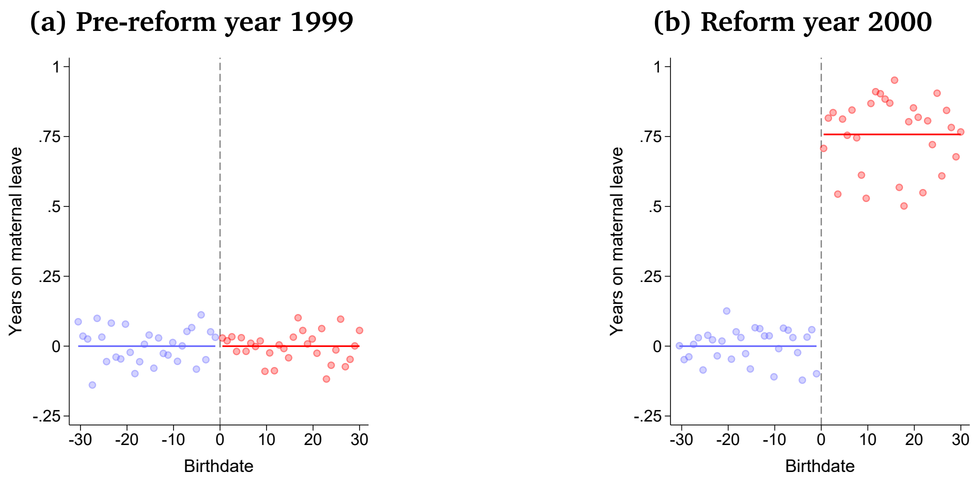
Notes: This figure shows the impacts of parenthood on antidepressant prescriptions by the first child's birth weight (in percentage points). It focuses on mothers in Austria (Figure G.12a), fathers in Austria (Figure G.12b), mothers in Denmark (Figure G.12c), and fathers in Denmark (Figure G.12d). We estimate separate regressions for parents with children with a normal birth weight (more than 2,500 grams) and a low birth weight (less than 2,500 grams). Moreover, we obtain the event time coefficients from estimating regression (1) on a balanced sample of parents who have their first child between 2002 and 2007. The shaded areas represent 95% confidence intervals based on robust standard errors. The top-right corner of each figure also shows percentage effects at event time $t = 9$.

Figure G.13: Impacts of parenthood on prescriptions by parents' migration status



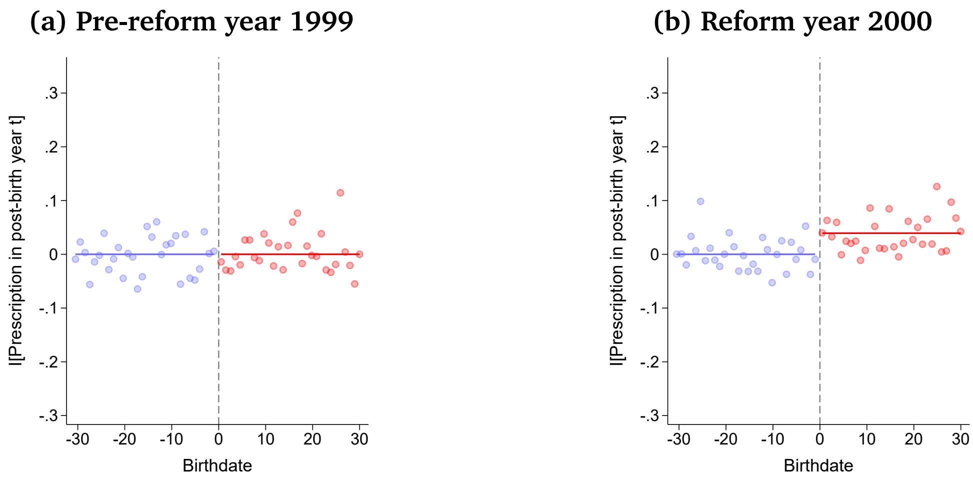
Notes: This figure shows the impacts of parenthood on antidepressant prescriptions by the cultural background of the parents (in percentage points). In the Austrian data, we use information on religious denomination (available in administrative data) and distinguish between Muslims and non-Muslims. In the Danish data, we use information on country of birth and distinguish between migrants from predominantly Muslim countries and natives plus other migrants. According to the *CIA World Factbook* the following countries are predominantly Muslim: Sudan, Somalia, Turkey, Afghanistan, Iran, Niger, Yemen, Algeria, Morocco, Saudi Arabia, Tunisia, Tajikistan, Comoros, Jordan, Azerbaijan, Libya, Pakistan, Iraq, Senegal, Gambia, Djibouti, Mali, Turkmenistan, Egypt, Kyrgyzstan, Bangladesh, Guinea, Uzbekistan, Indonesia, Syria, Sierra Leone, United Arab Emirates, Kazakhstan, Burkina Faso, Malaysia, Chad, Mauritania, Oman, Kuwait, Bahrain, Qatar, Lebanon, Albania, Bosnia And Herzegovina, Maldives, Brunei. The figure focuses on mothers in Austria (Figure G.13a), fathers in Austria (Figure G.13b), mothers in Denmark (Figure G.13c), and fathers in Denmark (Figure G.13d). We estimate separate regressions for Muslims and non-Muslims. Moreover, we obtain the event time coefficients from estimating regression (1) on a balanced sample of parents who have their first child between 2002 and 2007. The shaded areas represent 95% confidence intervals based on robust standard errors. The top-right corner of each figure also shows percentage effects at event time $t = 9$.

Figure G.14: Effects of the 2000 reform on years of maternity leave (first stage)



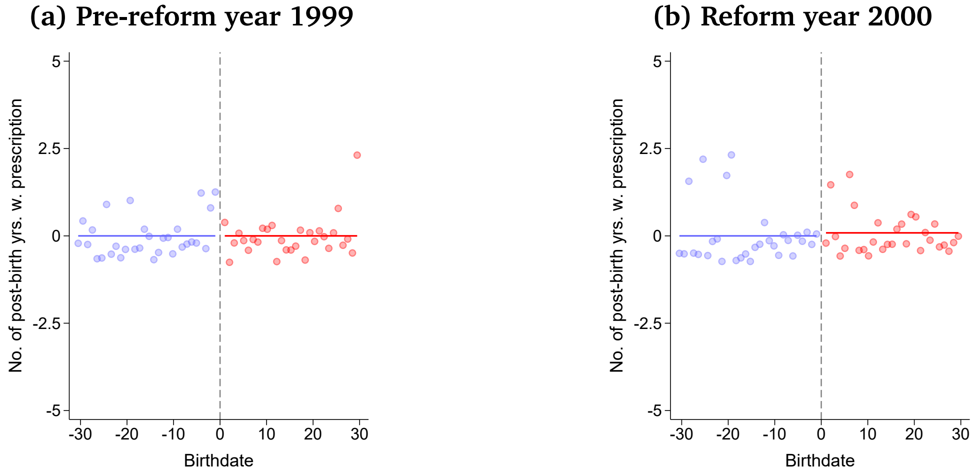
Notes: This figure shows the effects of the Austrian 2000 reform on the number of taken maternity leave years (first stage). The figures are covariate adjusted. For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side).

Figure G.15: Reduced-form effects of the Austrian 2000 reform on yearly AD prescription probability



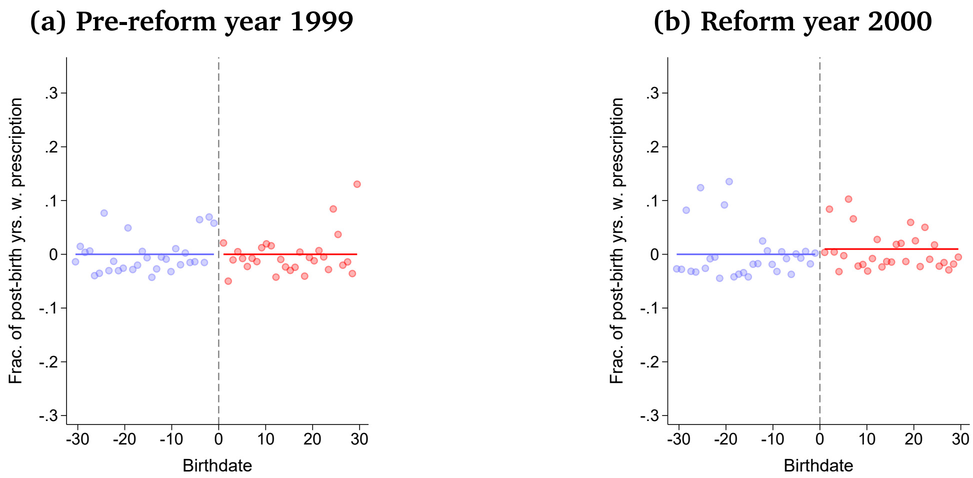
Notes: This figure shows the reduced-form effects of the Austrian 2000 reform on the yearly probability of receiving an antidepressant prescription in a given year for mothers. The figures are covariate adjusted. For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side). The underlying dataset has a panel structure and a binary variable that indicates years with a prescription serves as the outcome.

Figure G.16: Reduced-form effects of the Austrian 2000 reform on no. of years with AD prescriptions (fathers)



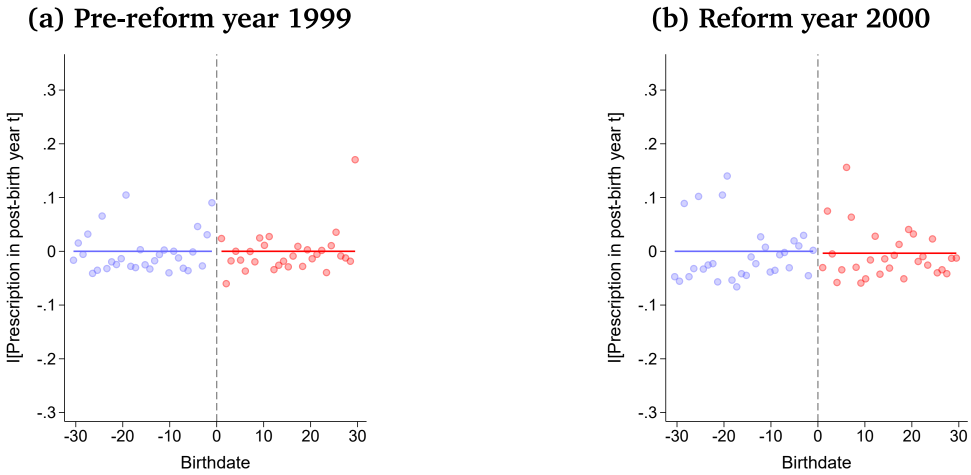
Notes: This figure shows the reduced-form effects of the Austrian 2000 reform on the number of post-birth years with antidepressant prescriptions for fathers. The figures are covariate adjusted. For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side).

Figure G.17: Reduced-form effects of the Austrian 2000 reform on frac. of years with prescriptions (fathers)



Notes: This figure shows the reduced-form effects of the Austrian 2000 reform on the fraction of post-birth years with antidepressant prescriptions for fathers. The figures are covariate adjusted. For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side).

Figure G.18: Reduced-form effects of the Austrian 2000 reform on yearly AD prescription probability (fathers)



Notes: This figure shows the reduced-form effects of the Austrian 2000 reform on the yearly probability of receiving an antidepressant prescription in a given year for fathers. The figures are covariate adjusted. For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side). The underlying dataset has a panel structure and a binary variable that indicates years with a prescription serves as the outcome.

G.2 Additional tables

Table G.1: Impact of the Austrian 2000 reform on years of maternity leave (first stage)

	(1) Triangular	(2) Unweighted	(3) Covariates
Impact on the years of maternity leave			
Reform effect	0.7500***	0.7521***	0.7575***
	(24.3828)	(24.1866)	(24.4960)
Mean of outcome		1.4549	
Observations		1,901	

Notes: This table provides estimates for the impact of the Austrian 2000 reform on the number of taken maternity leave years. The estimates rely on a bandwidth of 30 days. Column (1) uses triangular weights, Column (2) does not use any weighting, and Column (3) combines triangular weights with covariates. It controls for the mother's age, the child's sex, the child's legitimacy status, maternal education dummies, a dummy indicating whether the child is born preterm, and a dummy indicating whether the mother was born abroad. t statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table G.2: Reduced-form impact of the Austrian 2000 reform on mental health

	(1) Triangular	(2) Unweighted	(3) Covariates
A. Impact on the fraction of post-birth years with AD prescriptions			
Reform effect	0.0307**	0.0263**	0.0337***
	(2.5140)	(2.1168)	(2.7465)
Mean of outcome		0.0455	
B. Impact on the number of post-birth years with AD prescriptions			
Reform effect	0.5802***	0.4716**	0.6246***
	(2.8399)	(2.2525)	(3.0400)
Mean of outcome		0.6819	
Observations		1,901	

Notes: This table provides reduced-form estimates for the impact of the Austrian 2000 reform on the fraction of post-birth years with antidepressant prescriptions (Panel A) and the number of post-birth years with antidepressant prescriptions (Panel B). The estimates rely on a bandwidth of 30 days. Column (1) uses triangular weights, Column (2) does not use any weighting, and Column (3) combines triangular weights with covariates. It controls for the mother's age, the child's sex, the child's legitimacy status, maternal education dummies, a dummy indicating whether the child is born preterm, and a dummy indicating whether the mother was born abroad. t statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table G.3: Reduced-form impact of the Austrian 2000 reform on the prescription probability

	(1) Triangular	(2) Unweighted	(3) Covariates
A. Impact on women's prescription probability			
Reform effect	0.0371***	0.0306***	0.0393***
	(6.9050)	(5.5916)	(7.3576)
Observations		28,129	
B. Impact on men's prescription probability			
Reform effect	-0.0013	-0.0080	-0.0036
	(-0.2069)	(-1.2476)	(-0.5586)
Observations		13,897	

Notes: This table provides reduced-form estimates for the impact of the Austrian 2000 reform on the probability of receiving an antidepressant prescription in a given year for women (Panel A) and men (Panel B). The underlying dataset has a panel structure and a binary variable that indicates years with an antidepressant prescription serves as an outcome. The estimates rely on a bandwidth of 30 days. Column (1) uses triangular weights, Column (2) does not use any weighting, and Column (3) combines triangular weights with covariates. It controls for the mother's age, the child's sex, the child's legitimacy status, maternal education dummies, a dummy indicating whether the child is born preterm, and a dummy indicating whether the mother was born abroad. t statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table G.4: Timing of reduced-form impacts on fraction of post-birth quarters with prescriptions

	(1) Triangular	(2) Unweighted	(3) Covariates
A. First 18 post-birth months: Pre-reform leave period			
Reform effect	0.0044	0.0013	0.0056
	(0.6889)	(0.2035)	(0.8660)
Observations	1709	1709	1709
B. Post-birth months 19 to 30: Extended leave period			
Reform effect	0.0135	0.0119	0.0161^{*}
	(1.5902)	(1.3086)	(1.8668)
Observations	1646	1646	1646
C. First five years after end of extended leave			
Reform effect	0.0240^{***}	0.0166^{**}	0.0269^{***}
	(2.8439)	(1.9625)	(3.1855)
Observations	1748	1748	1748
D. More than five years after end of extended leave			
Reform effect	0.0353^{**}	0.0271^{*}	0.0352^{**}
	(2.3555)	(1.7860)	(2.3232)
Observations	1715	1715	1715

Notes: This table visualizes the timing of the reduced-form estimates. It considers the Austrian 2000 reform and uses the fraction of post-birth quarters with antidepressant prescriptions as the outcome variable. The estimates rely on quarterly data and a bandwidth of 30 days. The reform affected mothers who gave birth after 30.6.2000 (i.e., after 2000q2). Panel A considers the pre-reform leave period (i.e., 18 months or six quarters after birth). Specifically, it shows reduced-form effects of the reform on the fraction of those six quarters in which individuals receive antidepressants. Panel B considers the expanded leave period. The 2000 reform increased the maximum leave period from 18 to 30 months (i.e., by four quarters). Correspondingly, the estimates in Panel B show how the reform affects the fraction of those four quarters in which mothers receive antidepressants. Panel C examines the impacts on mothers' mental health in the first five years after the end of the extended leave period (i.e., 2.5 to 7.5 years after birth). Panel D shows the impacts mothers experience more than five years after the end of extended leave (i.e., 7.5 to 16 years after birth). The table contains several specifications. Column (1) uses triangular weights, Column (2) does not use any weighting, and Column (3) combines triangular weights with covariates. It controls for the mother's age, the child's sex, the child's legitimacy status, maternal education dummies, a dummy indicating whether the child is born preterm, and a dummy indicating whether the mother was born abroad. t statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table G.5: Timing of reduced-form impacts on no. of post-birth quarters with prescriptions

	(1) Triangular	(2) Unweighted	(3) Covariates
A. First 18 post-birth months: Pre-reform leave period			
Reform effect	0.0238	0.0067	0.0301
	(0.6370)	(0.1779)	(0.8074)
Observations	1709	1709	1709
B. Post-birth months 19 to 30: Extended leave period			
Reform effect	0.0514	0.0437	0.0653**
	(1.6062)	(1.2820)	(2.0303)
Observations	1646	1646	1646
C. First five years after end of extended leave			
Reform effect	0.4760***	0.3303**	0.4824***
	(3.0179)	(2.0641)	(3.0324)
Observations	1748	1748	1748
D. More than five years after end of extended leave			
Reform effect	1.3869***	1.1197**	1.5133***
	(2.6092)	(2.0823)	(2.8352)
Observations	1715	1715	1715

Notes: This table visualizes the timing of the reduced-form estimates. It focuses on the Austrian 2000 reform and uses the number of post-birth quarters with antidepressant prescriptions as the outcome variable. The estimates rely on quarterly data and a bandwidth of 30 days. The reform affected mothers who gave birth after 30.6.2000 (i.e., after 2000q2). Panel A considers the pre-reform leave period (i.e., 18 months or six quarters after birth). Specifically, it shows reduced-form effects of the reform on the number of those six quarters in which individuals receive antidepressants. Panel B considers the expanded leave period. The 2000 reform increased the maximum leave period from 18 to 30 months (i.e., by four quarters). Correspondingly, the estimates in Panel B show how the reform affects the number of those four quarters in which mothers receive antidepressants. Panel C examines the impacts on mothers' mental health in the first five years after the end of the extended leave period (i.e., 2.5 to 7.5 years after birth). Panel D shows the impacts mothers experience more than five years after the end of extended leave (i.e., 7.5 to 16 years after birth). The table contains several specifications. Column (1) uses triangular weights, Column (2) does not use any weighting, and Column (3) combines triangular weights with covariates. It controls for the mother's age, the child's sex, the child's legitimacy status, maternal education dummies, a dummy indicating whether the child is born preterm, and a dummy indicating whether the mother was born abroad. t statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table G.6: Reduced-form impact of the Austrian 2000 reform on men's mental health

	(1) Triangular	(2) Unweighted	(3) Covariates
A. Impact on the fraction of post-birth years with AD prescriptions			
Reform effect	0.0049	-0.0018	0.0098
	(0.3497)	(-0.1260)	(0.6813)
Mean of outcome		0.0378	
B. Impact on the number of post-birth years with AD prescriptions			
Reform effect	0.0376	-0.0728	0.0859
	(0.1621)	(-0.3080)	(0.3655)
Mean of outcome		0.6375	
Observations		1,025	

Notes: This table provides reduced-form estimates for the impact of the Austrian 2000 reform on men's fraction of post-birth years with antidepressant prescriptions (Panel A) and men's number of post-birth years with antidepressant prescriptions (Panel B). The estimates rely on a bandwidth of 30 days. Column (1) uses triangular weights, Column (2) does not use any weighting, and Column (3) combines triangular weights with covariates. It controls for the mother's age, the child's sex, the child's legitimacy status, maternal education dummies, a dummy indicating whether the child is born preterm, and a dummy indicating whether the mother was born abroad. t statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.