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The Parenthood Penalty in Mental Health: Evidence from Austria and Denmark

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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 mental health burden on mothers than on fathers. It creates a long-run gender gap in antidepressant prescriptions of about 93.2% (Austria) and 64.8% (Denmark). Further evidence suggests that these parenthood penalties in mental health are unlikely to reflect differential help-seeking behavior across the sexes or the biological effects of giving birth to a child. Instead, they seem to mirror the psychological effects of having, raising, and investing in children. Supporting this interpretation, matched adoptive mothers (who do not experience the biological impacts of childbirth) also encounter substantial parenthood penalties. Moreover, mothers who invest more in childcare (by taking extended maternity leave in quasi-experimental settings) are more likely to face mental health problems.

JEL Classification: D63, J13, I10, J16, J22.
Keywords: Gender equality, fertility, parenthood, motherhood, mental health, parental leave.

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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 those of men (Angelov *et al.*, 2016; Kleven *et al.*, 2019a,b, 2020, 2021, 2022; Andresen and Nix, 2022; Kleven, 2023).[1] In line with this finding, descriptive work suggests that mothers spend much more time on child-related non-market activities than fathers. Examples include the time invested in childcare (Guryan *et al.*, 2008) or other home-production activities (Borra *et al.*, 2021). These imbalances in the parents' roles and responsibilities likely cause a disproportionately higher psychological and mental burden for mothers than fathers. However, uncovering such unequal effects of parenthood on *mental health* is challenging. Not only is mental health difficult to measure, but suitable identification approaches are also hard to find. As a result, the differential impacts of parenthood on mental health between sexes are not fully understood.[2]

In this paper, we combine quasi-experimental research designs with rich administrative data from Austria and Denmark to compare the impact of parenthood on mothers' and fathers' mental health. The overarching insight of our paper is that, in line with higher parenting costs, mothers pay a much higher psychological toll of parenthood in the long run than fathers. Although parentalhood also negatively affects men's mental health, the adverse effects for women are much larger. We label the decline in women's relative to men's mental well-being the "*parenthood penalty in mental health*." Supplementary evidence suggests that the parenthood penalties are unlikely to reflect differential help-seeking behavior across the sexes or the biological effects of giving birth to a child, such as postpartum depression.[3] Instead, they (a) seem to mirror the psychological effects of having and raising a child (mothers who adopt also face penalties) and (b) are related to mothers' higher investments in childcare (maternity leave extensions heighten mothers' risk of mental health issues).

We can derive these general insights because our setting allows us to tackle the two empirical challenges we mentioned before. First, we overcome the "measurement challenge" with administrative health data. Following the literature, we use antidepressant prescrip-

[1]The recent literature highlights that the associated relative reduction in women's earnings relative to men's, labeled *child penalties*, accounts for most of the remaining gender inequality in the labor market. By contrast, the earlier studies focused on specific groups of highly educated professionals and found negative effects of children for women but not for men (see, e.g., Bertrand *et al.*, 2010; Azmat and Ferrer, 2017).

[2]A literature strand summarized by Blanchflower (2009), Clark *et al.* (2008), Dolan *et al.* (2008), and Ferrer-i Carbonell (2013) studies the association between parenthood and self-reported well-being. The studies are usually not design-based, and gender inequality is not their focus. Moreover, self-reported data measures the mental health state after treatment (e.g., after antidepressant intake). It hereby obscures the true occurrence and severity of the initial symptoms before potential interventions. This literature suggests that parenthood negatively affects subjective happiness; the effects are similar for mothers and fathers. Baetschmann *et al.* (2016) criticize the methods used in this literature and question the results.

[3]Postpartum depression occurs within four weeks of giving birth, and it can last up to 6 months (Miller, 2002). They affect around one in six healthy mothers (Shorey *et al.*, 2018).

tions as our primary measure for mental health (Persson and Rossin-Slater, 2018; Ahammer and Packham, 2023). This outcome is valuable as it clearly reveals diagnosed 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.[4] Second, we respond to the "identification challenge" and estimate the impacts of parenthood on women relative to men with event studies around the birth of the first child (Kleven *et al.*, 2019a). The approach (a) estimates the impact of parenthood on mothers with event studies around the birth of the first child, (b) repeats the same analysis for fathers, and (c) compares the impacts across the sexes. The last step (c) identifies the parenthood penalty. We also complement this estimator with a difference-in-difference approach that uses childless people as a comparison group. A strength of our approaches is that they provide average effects for the population rather than identifying effects for certain compliers or selected samples with poor mental health.[5]

Conceptually, there are many potential causes for parenthood penalties in mental health. Parenthood is a life-changing event that forces families to undergo significant changes in a relatively short period. Depending on how parents cope with the physiological, psychological, and social adjustments, the transition to parenthood may not only have positive but also negative effects on their mental health. Notably, the adverse effects might even be more severe for women. Women bear the physical burden of childbirth and, in most cases, the majority of child-rearing (Guryan *et al.*, 2008; Borra *et al.*, 2021). They may then feel forced to choose between adjusting their labor supply (which reduces their income and, perhaps, their job satisfaction) or carrying the double burden of work and child-rearing (which can be overwhelming). Childcare investments are also associated with work, cognitive load, and mental stress. Put differently, the act of parenting is mentally challenging in itself. These higher overall childcare costs borne by women may ultimately lead to an increased vulnerability to mental health disorders and, thus, to parenthood penalties in mental health.

Economists should care about the existence of such penalties for several reasons. First, mental health is a pivotal component of individual well-being, and mental health problems indicate detrimental losses in this domain. An unambiguous mapping to well-being like that often does not exist for other standard outcomes. Child-related income losses, for example, might not have a clear interpretation. They potentially come with utility losses (due to lower income) and utility increases (due to increased leisure or quality time with

[4]One might be concerned that (a) the penalties are an artifact in prescriptions or (b) reflect differential help-seeking behavior across the sexes. We study other mental health indicators (such as psychiatric visits) and present many robustness analyses to protect us against these possibilities.

[5]Traditionally, economists (studying other outcomes) tried to tackle the endogeneity of fertility with instrumental variable (IV) approaches. Unfortunately, there are hardly any good IVs for first births. One notable exception is variation in the success of in vitro fertilization. Lundborg *et al.* (2017) find negative, large, and long-lasting fertility effects at the extensive margin on earnings of women. To probe an alternative mechanism, they test whether failed in vitro fertilizations lead to the use of depression medication and find no support for this hypothesis. To identify the effects of higher-order births, the literature uses IVs such as twin births or same-sex sibling pairs (Angrist and Evans, 1998).

kids). Against this background, we are naturally interested in whether mothers bear the greater mental burden of parenthood. Second, mental health problems affect key economic outcomes. They are the leading cause of disability (Holden, 2000; Friedrich, 2017), shape labor market participation (Cuddy and Currie, 2020; Biasi *et al.*, 2021), and human capital formation (Currie and Stabile, 2006; Fletcher and Wolfe, 2008; Currie, 2009). Mental health problems may also affect fertility rates or undermine non-cognitive skills, such as focus or motivation, which impact productivity and wages (Edin *et al.*, 2022). Operating through the effects on wages and labor supply, parenthood penalties in mental health may even partly explain the gender gap in income. Third, maximizing and equalizing mental health across the sexes is a ubiquitous policy goal.[6] Along these lines, it is essential to trace out the determinants of inequality in mental health (such as parenthood). Fourth, by focusing on mental health issues, we study serious illnesses. If there are, for example, parenthood penalties in depression rates, the additional burden for women must be of non-negligible magnitude.

An essential contribution of our paper is its focus on two countries. A key advantage of such a two-pronged approach is that it allows us to explore whether mental health penalties are a general phenomenon that exists across settings. With this in mind, we deliberately chose two countries that are vastly different in dimensions that could shape the size of mental health penalties: Austria and Denmark. Factors such as family policies, societal support for mothers, sexism, gender norms, and gender roles vary considerably between those two countries. For example, unlike Denmark, Austria does not provide universal childcare. The Austrians are also more sexist (Bertrand *et al.*, 2021),[7] more gender conservative (Kleven *et al.*, 2019a), and expect mothers to be the primary caregivers, which they typically are (Goldstein *et al.*, 2022). The child-related income inequality in Austria is also much higher than in Denmark (Kleven *et al.*, 2019a). Our analysis reveals whether the combination of all these differences neutralizes the parenthood penalties, or whether the penalties transcend cultural and policy environments. Another benefit of the dual focus is that both countries are similar enough to be able to compare the country-specific results in a meaningful way. They are both rich and have comprehensive social security systems. Most importantly, the countries provide similar mental healthcare services, which ensures a high comparability of the health outcomes between the settings.

This comparability of the two settings is the basis of our key result: As evidenced by consistent patterns in the data, women in both countries face substantial mental health penalties. The antidepressant prescription probabilities for men and women consistently evolve in parallel before the birth of the first child; after birth, prescriptions increase sharply, espe-

[6]Promoting well-being and mental health is, for example, one of the Sustainable Development Goals established by the United Nations General Assembly in 2015.

[7]This categorization builds on a continuous sexism index derived from survey responses in the *European Value Survey* (EVS), the *World Value Survey* (WVS), and the *International Social Survey Programme* (ISSP) in a sample of up to 26 countries over the period between 1990 and 2012 (Bertrand *et al.*, 2021).

cially for women. The resulting parenthood penalty is particularly large in Austria. Here, the first child's birth increases women's prescription probability by about 5.0 percentage points in the ninth year after birth. For men, the effect is 2.1 percentage points. The resulting parenthood penalty, defined as the increase in the probability of prescription for mothers induced by parenthood compared to the corresponding increase for fathers, is 2.9 percentage points. In other words, due to parenthood, women's antidepressant prescription probability exceeds that of men by 93.2%. This penalty reflects the total penalty, including the cost of children born after the first one. For Denmark, we document a significant and somewhat smaller parenthood penalty in mental health. Mothers show an increase in antidepressant prescriptions of 2.7 percentage points in the ninth year after birth, while the corresponding increase for fathers is 0.8 percentage points. The resulting parenthood penalty for mothers amounts to a 1.9 percentage point increase in the probability of antidepressant prescription (or 64.8%). We present several pieces of evidence suggesting that these penalties (a) do not reflect the biological effects of giving birth to a child but (b) rather represent the psychological effects of having and raising a child. One compelling finding in this context follows from the comparison of biological and adoptive families (Kleven *et al.*, 2021; Andresen and Nix, 2022): Even matched adoptive mothers (who do not experience the biological aspects of childbirth) encounter substantial penalties.

In a nutshell, these results support the hypothesis that women bear a higher mental cost of parenthood. While the previous literature has established that — despite considerable gender convergence over time — women still experience a parenthood penalty in earnings, our results demonstrate that gender inequality due to parenthood extends beyond the labor market. In particular, many mothers face more severe mental health problems in addition to lost earnings. Interestingly, despite substantial differences in institutions and sociocultural factors, such as norms or gender roles, the unequal impact of parenthood on the mental health of mothers and fathers occurs in Austria *and* Denmark. This finding does not imply that institutional and sociocultural factors do not matter at all for gender inequality in mental health. The penalty is indeed 1.0 percentage points higher in Austria, the more gender-conservative environment. However, all the differences between Austria and Denmark do not reduce the burden on mothers enough to close the existing gender disparities entirely (even Danish mothers face penalties). Our conclusion from this evidence is that the parenthood penalty in mental health is a general and resilient phenomenon.

Having established our main results, we provide two additional contributions to the literature. The first contribution concerns the so-called gender gap in mental health. A consistent finding in the social epidemiology of mental health is that women are generally much more likely to receive mental health treatment than men (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 are no exception: Austrian mothers are about 81.5% more likely to receive antidepressants than fathers. Similarly, in Denmark, the average probability of mothers receiving antidepressants exceeds that of fathers by 82.4%. Given the large and persistent parenthood penalties in mental health, it is natural to ask what part of these overall prescription gaps between mothers and fathers is caused by parenthood. To examine this topic, we combine a simple decomposition technique with our event-study estimates. Again, the results are clear and qualitatively similar across countries: Parenthood causes a substantial fraction of the overall gender gap in antidepressant intake (30.8% in Austria and 20.9% in Denmark). This finding demonstrates that the parenthood penalties explain a crucial part of the puzzle of why women are more likely to receive mental health treatments than men. Policymakers concerned about mental health equity should carefully consider the role of children. They may even have the opportunity to level the playing field with reforms that tackle the parenthood-related part of the gender gap in mental health.

One intervention that policymakers have repeatedly proposed to promote gender equality is parental leave schemes. Our second additional contribution is, therefore, to explore how reforms that vary the length of paid maternity leave affect parents' mental health post-birth. On the one hand, parental leave expansions could improve mothers' mental well-being and reduce the parenthood penalty. Affected mothers receive more time to adjust to the demands of parenthood, paving the way for a smoother transition back to work and potentially promoting better long-term mental health outcomes. Positive effects on mothers' mental health may also arise because longer leaves reduce the double burden in the short run or offer mothers more time to bond with their children. On the other hand, maternity leave expansions likely increase mothers' parenting costs and could impair their mental health. For example, they prolong the period of exclusive caregiving and could also amplify mothers' childcare contributions in the long run (by reinforcing their roles as primary caregiver). Because parenting can be fraught with stress, dissatisfaction, and a high cognitive load, longer leave could affect mothers' mental health. Maternity leave expansions could also push mothers out of the workforce, lowering their income and job satisfaction. In sum, the sign of reform-induced parental leave extensions *a priori* unclear. We, therefore, evaluate their impacts by applying the regression discontinuity design (RDD) proposed by Schönberg and Ludsteck (2014) and Danzer *et al.* (2022) to Austrian and Danish data.[8]

We find that parental leave reforms exacerbate the mental health penalties when they possess two characteristics: they markedly prolong mothers' leave-taking and have been implemented in environments where the leave period was already long prior to the reform. Specifically, we demonstrate that mothers who substantially share more time with their kids after birth more frequently struggle with mental health issues, with no spillovers on fathers. The first adverse effects seem to emerge during the expanded leave period (weakly significant short-run effects). The effects then grow stronger right after the extended leave

[8]The approach exploits that the government conditioned the reform for extended parental leave on a sharp birthday cutoff date. Thus, we can compare families with children born shortly before and after the cutoff date to identify the reforms' effects.

period (when many women re-enter the workforce) and persist for over a decade (large and significant long-run effects). Such negative long-run impacts resonate with the notion that leave extensions reinforce mothers' roles as primary caregivers and/or act as a trigger for prolonged mental health challenges. By contrast, the adverse effects are unlikely to operate through labor-supply and earnings channels: The same reforms did not impact mothers' long-run labor supply and earnings (Kleven, 2023). Overall, the evidence demonstrates that prolonging mothers' exclusive caregiving period places an extra mental burden on them and boosts the penalties they face, likely because childcare investments and parenting are mentally taxing.[9]

Our paper adds to several strands of literature. First, we contribute to the work on gender inequality (Bertrand, 2011; Azmat and Petrongolo, 2014; Blau and Kahn, 2017; Bertrand, 2020), particularly to recent papers studying the impact of parenthood on wages and labor supply (Angelov *et al.*, 2016; Cortés and Pan, 2022; Kuziemko *et al.*, 2018; Bertrand *et al.*, 2010; Kleven *et al.*, 2019b,a; Kleven, 2023; Kleven *et al.*, 2022; Andresen and Nix, 2022), job absences (Rosenbaum, 2023), and women's health (Dehos *et al.*, 2023). Relative to this previous research, our paper has a different and unique focus: It demonstrates that parenthood creates gender inequality in mental health. Particularly, on top of estimating mental health penalties for two countries, we further enhance the literature by (a) formally decomposing the overall gender gap into components related and unrelated to parenthood and (b) providing evidence on mechanisms (biological effect vs. effect of having a child). Second, we add to the (primarily medical) literature studying the effects of parenthood on mothers' mental health. This literature mainly uses self-reported data and focuses on (postpartum) depression during and immediately after pregnancy (Shorey *et al.*, 2018).[10] We, instead, use administrative data and establish that the adverse effects of parenthood on mothers' mental health last for many years and are unlikely to reflect the well-known biological effects of having a child. Third, we supplement the literature on the effects of family policies (Olivetti and Petrongolo, 2016) and the effects of parental leave schemes on parents (e.g., Lalive and Zweimüller, 2009; Schönberg and Ludsteck, 2014; Dahl *et al.*, 2016; Rossin-Slater *et al.*, 2013; Ruhm, 1998; Andresen and Nix, 2023). Our contribution is to examine the impacts of parental leave reforms on mental health and mental health inequality. Specifically, we complement the paper of Chuard (2023) by studying the impacts on mental health equality instead of focusing solely on the effects on mothers. We also expand

[9] Glogowsky *et al.* (2023) present evidence from Austrian administrative and time-use data in line with this interpretation. They demonstrate that, despite having a preference for sons, fathers of sons have poorer mental health than those of daughters. The reason seems to be that they invest significantly more in childcare.

[10] This literature finds that, in the short run, parenthood is negatively associated with mental health (e.g., Evenson and Simon, 2005). Only very few descriptive papers study the correlation between parenthood and medium- to long-run mental health with prescription data. One example is Kravdal *et al.* (2017), who find that more children are associated with fewer antidepressant prescriptions for Norwegian men and women. By contrast, women with only one child are significantly more likely to require antidepressants than childless women.

the analysis to longer-run effects.[11] Finally, our paper relates to studies on the influence of social norms and culture on maternal labor supply (Fernández *et al.*, 2004; Boelmann *et al.*, 2021).

Our paper unfolds as follows: We begin in Section 2 with an overview of the pertinent institutional background in Austria and Denmark. Section 3 details our data sources, analysis samples, and descriptive statistics. In Section 4, we lay out our estimation strategy, clarify the identification assumptions, present our main estimates of parenthood penalties in mental health, and test the robustness of our results. This section also discusses the degree to which differential healthcare utilization behaviors across the sexes represent health-status effects, examines the role of biology, and explores effect heterogeneity. Section 5 decomposes the total gender gap in mental health into a child-related and a child-unrelated part. In Section 6, we examine the impacts of parental leave policies, and we conclude the paper in Section 7.

2 Institutional background

This section compares the institutional landscapes in Austria and Denmark. The goal is to point out (a) similarities that permit comparisons of results across countries and (b) differences that enable us to test whether parenthood penalties are universal. To highlight the key similarities and differences, we contrast the countries' healthcare systems, mental healthcare utilization levels, labor market institutions, and family policies. We also present survey evidence on the prevailing gender identity norms, particularly on the division of labor and childcare.

2.1 Healthcare systems

Austria and Denmark provide universal access to high-quality healthcare, albeit with two central organizational differences. The first difference relates to financing. Austria relies on a social health insurance system (i.e., a *Bismarck Model*). The system's core funding comes from compulsory social security contributions tied to employment and occupational status. The *Austrian Health Insurance Fund* (*Österreichische Gesundheitskasse*, hereafter ÖGK) insures about 82% of the population. The ÖGK is responsible for the vast majority of all employees, their dependents, and all non-employed residents.[12] By contrast, Denmark primarily relies on a tax-financed health insurance system (i.e., a *Beveridge Model*). The primary

[11] Chuard (2023) examines the impacts of the same Austrian reforms on maternal healthcare utilization and reports negative impacts of these reforms on mothers' mental health. We extend her work by (a) studying long-run effects, (b) estimating LATEs, and (c) investigating impacts on men and parenthood penalties.

[12] The remaining 18% are certain occupational groups (civil servants, miners, and federal railway employees), self-employed, freelance professionals, and farmers. These people receive insurance from other statutory health insurance providers. Notably, most public sector employees are not civil servants and, therefore, insured with the ÖGK.

funding sources are state-level tax revenues supplemented to a lesser degree by municipal income tax revenues. The national government administers the system, allocating block grants to the regions and municipalities that deliver health services. All Danish residents automatically receive coverage from the national health system. The second organizational difference concerns the role of hospitals. Austria spends considerably more on inpatient hospital care than Denmark. The number of hospital beds reflects this difference. Austria has about 8 hospital beds per 1,000 population, while Denmark has only 3.4.[13] Austria also has slightly more doctors, with 4.6 physicians per 1,000 population, compared to Denmark's 3.5.

Despite these differences, both systems share many similarities that are significant for our analyses. Most importantly, the universal healthcare systems in both countries cover comparable services, including all expenses tied to sickness and maternity in the inpatient and outpatient sectors. This feature ensures the complete measurement of (mental) health outcomes in both settings. Both countries have also adopted the WHO Anatomical Therapeutic Chemical (ATC) classification system, allowing us to quantify health outcomes identically across countries. Table A.1 in the Appendix highlights further dimensions in which the healthcare systems are similar. In line with comparable service provision, both countries invest about 10% of their gross domestic product in healthcare. Moreover, many aggregate health outcomes, such as infant mortality rates and life expectancy, are very similar. In conclusion, the similarities between the two healthcare systems provide a solid foundation for our comparative analysis of mental health outcomes.

2.2 Mental health and mental healthcare utilization

This subsection demonstrates that the mental health status and mental healthcare utilization are comparable in Denmark and Austria. To that end, we mainly use data from the *Special Eurobarometer* 246 in 2005/06, a uniform survey framework focusing on mental well-being. We concentrate on survey data as they provide insights into aspects (such as help-seeking behavior) not captured in administrative data. However, further in the paper, we provide comparable healthcare utilization statistics from our administrative data, where available.

The two key messages are as follows. First, as indicated by responses to the five-item version of the *Mental Health Inventory* (MHI-5) survey of Veit and Ware (1983), respondents in both countries have largely comparable mental health. The MHI-5 is a brief and reliable instrument for assessing mental health in adults (Berwick *et al.*, 1991); its scale ranges from 0 (indicating a high level of mental disorders) to 100 (representing optimal mental health).[14]

[13] In line with these figures, inpatient care accounts for about 33% of total health spending in Austria and only 25% in Denmark (OECD/European Observatory on Health Systems and Policies, 2021a,b).

[14] Researchers developed the MHI as part of the National Health Insurance Study, and they used it in various populations. The survey asks respondents how often they felt (a) "*nervous*," (b) "*calm and peaceful*," (c) "*downhearted and miserable*," (d) "*happy*," and (e) "*so down in the dumps that nothing could cheer you up*" in the last month. For each item, answers are provided on a six-point scale, with the following categories: "*all*

The score of Austrian respondents is, on average, quite comparable (67.5) to that of their Danish counterparts (71.3). This difference in scores is only about one-fifth of the standard deviation, but it is statistically significant. Second, the share of the population seeking professional help for mental health problems is broadly comparable in Austria and Denmark. When asked, "*In the last 12 months, did you seek help from a professional regarding a psychological or emotional health problem?*" 15% of Austrian and 17% of Danish respondents report seeking such help. Third, the suicide rates among women are very comparable (on average, about 7 cases per 100,000 population between 1990 and 2017). There is, however, a significant difference in the rates for men. The latter is higher for Austria (24.1) than for Denmark (19.6).

The Eurobarometer also allows us to study the types of healthcare providers the Austrians and Danes consult when encountering mental health problems and the treatments they receive. Notably, general practitioners are the first point of contact for mental healthcare in both countries. The survey includes a multiple-response question that supports this observation. It asked respondents who reported seeking mental health aid to identify their care providers. In both countries, general practitioners predominantly treat mental health problems in primary care (about 80%). This result seems reasonable: GPs in either country can prescribe medications for symptoms of depression or other mental illnesses. The distribution of treatments is also comparable across countries. Specifically, the shares of respondents who have taken medication (AT: 10%, DK: 7%), received psychotherapy (AT: 3%, DK: 4%), or have been admitted to a hospital due to psychological problems (AT: 2%, DK: 1%) are broadly similar in Austria and Denmark.

2.3 Labor markets

As our paper focuses on gender inequality, we next compare the Austrian and Danish labor markets from this perspective. While Austrian and Danish men have always had comparable labor force participation rates, Austrian women have traditionally been less attached to the labor market than Danish women. Over time, however, the labor force participation rate of women in Austria has increased; in the age group 25 to 54, it is now even slightly higher than in Denmark (AT: 85.1%, DK: 82.9%). Changes in female part-time employment mainly explain this development. Today, the share of female part-time employment in Austria (33.1%) is higher than in Denmark (23.1%). This difference in part-time employment arises mainly after childbirth: While most Danish mothers continue to work full-time, the typical Austrian family with children follows the characterization of the male breadwinner model. Mothers mostly re-enter the labor market as secondary earners in part-time jobs. In line with this observation, the child penalty in earnings in Austria is much higher than Denmark's (Kleven *et al.*, 2019a). The arrival of children leads to a long-run gender gap in

the time," "*very often*," "*often*," "*rarely*," "*very rarely*," and "*never*."

earnings in Denmark of around 20%. For Austria, the gap is about 50%.

2.4 Family policies

Mothers' mental health likely depends on how strongly family policies support them. Although both countries provide generous family policies, they differ in their approach. Austrian policy leans towards supporting mothers as primary caregivers, while Danish policy actively seeks to help working mothers.

Consider, for example, the paid maternity and parental leave schemes in 2021. Austrian law mandates a compulsory paid maternity leave period of 16 weeks for mothers (eight weeks before and eight weeks after childbirth). In Denmark, the maternity leave period extends to 18 weeks (four weeks before and 14 weeks after birth). The subsequent parental leave system is much more generous in Austria. Here, parents can take up to 35 *months* of paid parental leave if mothers and fathers share that time. If only one parent uses parental leave, the duration decreases to around 28 months. In the vast majority of cases, it is the mother who takes the leave. In Denmark, the fully paid leave duration partners can share is only 32 *weeks*. The leave periods can be extended with correspondingly lower compensation. As already discussed, such differences in the leave period may affect the parents' mental burdens.

There is another marked difference between the two countries' policies: In line with mothers being the primary caregivers, enrollment rates in formal childcare among young Austrian children are much lower than those of their Danish counterparts. The formal childcare system in both countries distinguishes between facilities for children below the age of three (nurseries) and those aged three to six (kindergarten). While most Austrian communities have offered a kindergarten since the 1980s, the local availability of nurseries has traditionally been much lower. In 1995, for example, only about 3% of the communities had nurseries. These nurseries were mainly located in more densely populated areas and covered about 35% of the population. However, nurseries and kindergartens had to cope with over-subscriptions, short opening hours (up to noon), and long holidays. By contrast, public institutions admit Danish children as young as six months and guarantee them a slot by the time they are 26 weeks old. Nearly 40% of children aged zero to two are enrolled in public nurseries, while almost all children aged three to six attend kindergartens.

2.5 Gender identity norms

Compared to Denmark, Austria is a conservative society with strong gender identity norms (Akerlof and Kranton, 2000). The divergent norms are not only reflected in different patterns of maternal labor supply but are also visible in survey responses on attitudes toward family and gender roles.

Table 1: Attitudes towards family and gender roles

	Share or respondents who strongly agrees with respective statement			
	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: The exact survey question in the *International Social Survey Programme* read as follows: "*To begin, we have some questions about women. To what extent do you agree or disagree?*" (a) "*A working mother can establish just as warm and secure a relationship with her children as a mother who does not work.*" (b) "*A preschool child is likely to suffer if his or her mother works.*" (c) "*All in all, family life suffers when the woman has a full-time job.*" (d) "*A job is all right, but what most women really want is a home and children.*" (e) "*Being a housewife is just as fulfilling as working for pay.*" In each case, survey respondents must select one of the following response alternatives: "*strongly agree, agree, neither agree nor disagree, disagree, strongly disagree*." The values indicate the share of respondents who "*strongly agree*."

Table 1 substantiates this observation based on norm-related data from the *International Social Survey Programme* for the years 2002 and 2012. The columns represent the average proportion of respondents by sex who strongly agree with various statements about gender identity norms. Danish respondents, regardless of their sex, have a substantially more positive view of working mothers than their Austrian counterparts. For example, very few Danes strongly agree that "a preschool child likely suffers if his or her mother works," and over half strongly affirm that "a working mother can have a warm relationship with her child." By contrast, about a quarter of Austrians strongly believe that working mothers have a potentially negative impact on preschool children, and less than half are convinced that a working mother can maintain a warm relationship with her children.

Collectively, the evidence in Section 2 shows that Austria is a more gender-conservative environment than Denmark. Austrians are also more likely to follow a male breadwinner model and implement policies supporting women as primary caregivers. Our analysis of parenthood penalties, therefore, reveals whether child-related inequalities persist across two countries with vast differences in dimensions that may shape the penalties' size.

3 Data sources, samples, and descriptive statistics

One major strength of our paper is the data, as they facilitate a comprehensive comparison between the two countries. In this section, we present the specifics of our datasets. We outline our data sources, clarify the estimation samples, and provide descriptive statistics.

3.1 Data sources

For Austria, we combine two administrative data sets. The first is the *Austrian Social Security Database* (ASSD). These administrative records span from 1972 to 2021 and allow us to observe the employment and childbirth for the universe of Austrian women. The second data source is administrative data from the ÖGK. We can access the ÖGK data for the entire Upper Austria's population.[15] These data comprise around one million people, and cover all prescription drugs, physician visits, and hospital stays between 1998 and 2016. The coding of therapeutics follows the ATC classification system, allowing us to construct comparable outcomes for 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. Our primary data source is the population register, which includes the exact birthdates of all persons, a unique personal identifier, and a link 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*.

We then construct our primary outcome variable as a dummy measuring whether an individual received an antidepressant prescription (ATC code N06A) in a particular year. The key benefit of this variable is that it is comparable between both countries. Another critical aspect of both countries' data is that there are no automatic drug refills, and prescriptions always specify package sizes instead of indicating the duration for which the medication should last.[16]

3.2 Analysis 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

[15] Upper Austria is one of nine federal states in Austria and comprises about one-sixth of the Austrian population and workforce. Traditionally, each state had its own regional health insurance fund. In 2020, the government merged the nine regional health insurance funds into the ÖGK. We have a research cooperation with the Upper Austrian branch.

[16] In Austria, patients pay a co-payment (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 exempted from the prescription fee. We observe all dispensed medicines for this group, regardless of their market price. In the Danish data, we observe all prescribed medicines.

that contains all parents with first-born children between 2002 and 2007. We focus on this period because the health data only cover the period 1998 to 2016, and our aim is to analyze behavior in four years prior to birth and nine years after birth. Second, we merge the health data with this dataset. Third, following Kleven *et al.* (2019a), we construct a fully-balanced panel. Consequently, our final estimation sample only considers parents insured with the ÖGK during the entire 14-year period. Fourth, we drop all parents younger than 18 or older than 55 when they had their first child. We focus on this age group because 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 coinsure their children). To verify that this restriction is not driving differences in the results between Austria and Denmark, we also provide estimates for married parents.

Following the construction of the Austrian sample, we align the Danish sample with the Austrian 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 prior to 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.

3.3 Descriptive statistics

Table 2 summarizes the descriptive statistics of our estimation samples, highlighting similarities and differences between Denmark and Austria. Both countries have similar average ages at the time of birth and comparable employment statuses two years prior to birth (see Panel A). Women in both countries are also more likely to visit outpatient physicians than men (see Panel C). We observe this pattern for general practitioners, mental health specialists, gynecologists, and other specialists.[17] 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 marital status across both countries. Second, Austrian parents earn less than their Danish counterparts, but wages are hardly comparable between the two countries due to differences in economic context (such as different living costs). Third, consistent with the Eurobarometer survey evidence, we find that the probability of taking antidepressants is somewhat higher in Austria than in Denmark (see Panel B).

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

Table 2: Summary statistics

	Austria		Denmark	
	Women	Men	Women	Men
A. Socioeconomic variables				
Age at birth	28.16	30.86	28.83	30.94
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	6.62	5.31	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 the birth-related variables refer to the birth of the first child.

[a] In Denmark, we cannot observe outpatient urologists.

4 Impact of parenthood on mental health

In this section, we estimate the impact of parenthood on mental health and determine the corresponding parenthood penalties. Subsection 4.1 introduces our estimation strategy. We continue presenting our baseline estimates in Subsection 4.2 and conduct specification tests in Subsection 4.3. Subsection 4.4 explores if the penalties reflect differential help-seeking behavior across the sexes. Finally, we explore the nature of the penalties in Subsection 4.5 (biology versus having/raising a child) and examine effect heterogeneity in Subsection 4.6.

4.1 Event study methodology

To measure the impact of parenthood on mental health, we ideally would like to assign parenthood randomly. As such experiments are not available, we employ 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.[18] 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

[18] Alternatively, one could use in vitro fertilization as an instrument (see Footnote 5). 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. Also, other standard instruments (such as twin birth and sibling sex) can neither identify the overall impact of parenthood on mental health nor on gender inequality (Kleven *et al.*, 2019a,b). Instead, they identify the marginal effects of the second or third child on various outcomes.

of the outcome.[19] Following this rationale, Kleven *et al.* (2019a,b) propose an event study approach exploiting changes around the birth of the first child for identification.

The estimation strategy proceeds in three steps. The first step is assessing 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 $t = -2$ serves as the reference period.[20] If the non-child prescription path is smooth conditional on controls, the estimates of the coefficients α^m_t will identify the total impacts of parenthood on Y^m_{ist} at event time t.

For interpretability, we sometimes translate these estimated level effects into percentage effects, measuring deviations from mothers' counterfactual prescription probability without children (in percent). Specifically, we uncover the mother i's non-child counterfactual as the predicted value of the outcome when setting the event-dummy effects to zero: $\tilde{Y}^m_{ist} = \sum_{year} \hat{\beta}^m_{year} \cdot \mathbb{1}[year = s] + \sum_{age} \hat{\gamma}^m_{age} \cdot \mathbb{1}[age = a_{is}]$. The resulting average percentage effect at event time t is: $\hat{\alpha}^m_t / E[\tilde{Y}^m_{ist} | t]$.

In the second step, we estimate the model (1) for the sample of fathers f and obtain the corresponding parameter estimates $\hat{\alpha}^f_t$, $\hat{\beta}^f_s$, and $\hat{\gamma}^f_{a_{is}}$. Again, we can convert the estimated effects of parenthood into percentages: $\hat{\alpha}^f_t / E[\tilde{Y}^f_{ist} | t]$.

The third step is estimating the parenthood penalty in antidepressant prescription at event time t in levels as:

$$P^l_t = \hat{\alpha}^m_t - \hat{\alpha}^f_t. \tag{2}$$

Intuitively, P^l_t reflects by how many percentage points the impact of parenthood on mothers' prescription probability exceeds that of fathers. Bear in mind that although the approach relies on the event of having the first child, the longer-run penalties incorporate the effects of subsequent children. Moreover, we can relate P^l_t to the fathers' counterfactual outcome:

[19] Kleven *et al.* (2019a) argue that unobserved outcome determinants should evolve smoothly over time.

[20] 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.

$P_t = \frac{\hat{\alpha}_t^m - \hat{\alpha}_t^f}{E[\tilde{Y}_{ist}^f|t]}$. The resulting percentage penalty P_t identifies the percentage by which women's prescription probability at t exceeds that of men due to children.

Kleven *et al.* (2019b) discuss the identifying assumptions ensuring a causal interpretation of the parenthood penalties P_t^l and P_t. In essence, identification requires parallel non-child outcome trends for men and women over the event time, conditional on life-cycle effects and year effects. We provide robustness checks to insure against the possibility that life-cycle and year effects do not fully capture all factors that lead to different 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 (see Figure 1a) and Denmark (see Figure 1b). The event-time-specific effects represent the estimated coefficients $\hat{\alpha}_t^m$ and $\hat{\alpha}_t^f$ and denote changes in percentage points.[21]

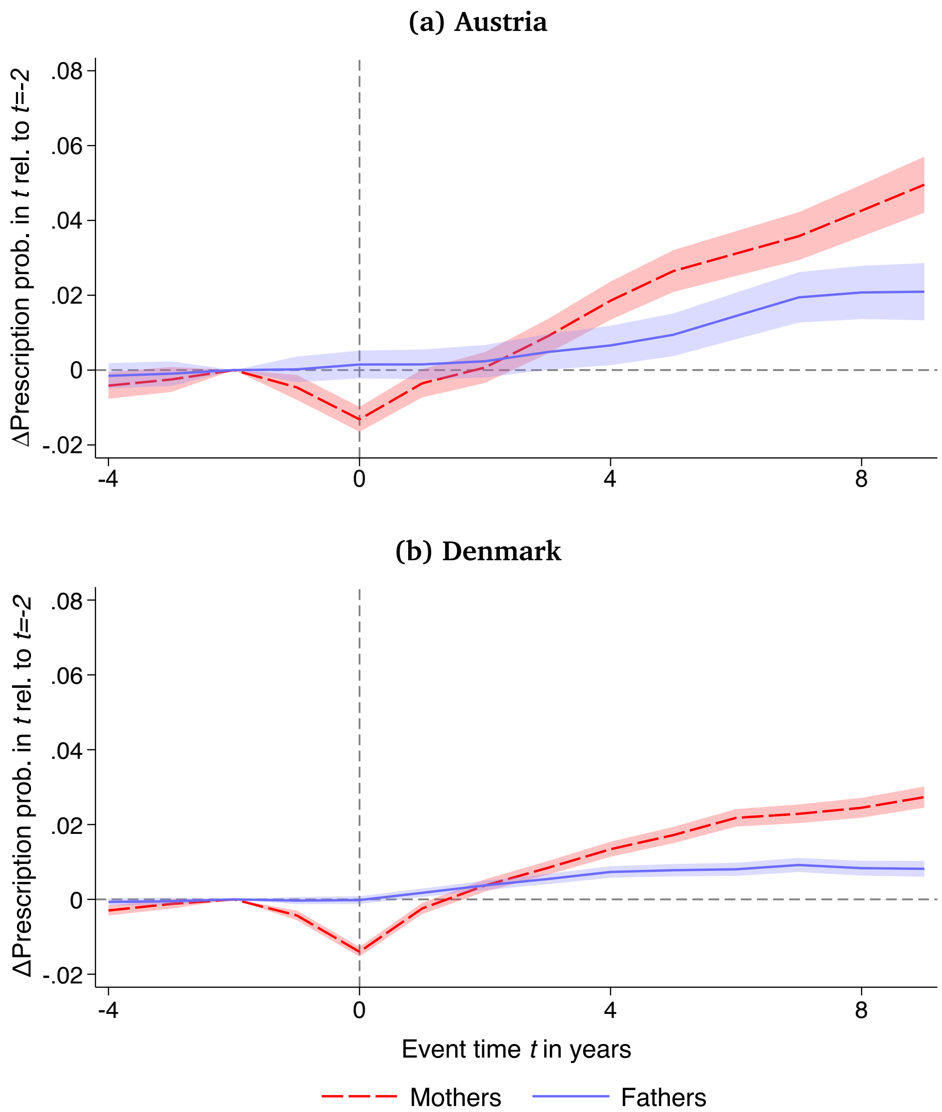
The patterns in Figure 1 showcase parenthood penalties in antidepressant prescriptions in both countries. After partialling out life-cycle and year effects, prescriptions for men and women in both countries evolve in parallel before the first birth. In the year of birth, women's prescription probability drops significantly by 1.3 (Austria) and 1.4 (Denmark) percentage points.[22] This decline occurs because taking certain antidepressants during pregnancy and breastfeeding carries potential risks. In Austria, the parenthood penalties then quickly emerge after birth. Women's likelihood of taking antidepressants increases sharply after the first child's birth. The impacts almost follow a linear upward trend throughout the observation window. By contrast, we observe much smaller increases in the prescription probability for Austrian men. Their prescriptions change hardly until the child's third birthday. Thereafter, men also are more likely to receive antidepressants, but their effects stays statistically significantly below that of women.[23] In terms of magnitudes, nine years after the birth of the first child, women's antidepressant prescriptions are 5.0 percentage points higher compared to the implied counterfactual without children. For men, we observe an increase of about 2.1 percentage points. These differential impacts of parenthood translate into substantial parenthood penalties in antidepressant prescriptions. The parenthood penalty in the ninth year after birth amounts to $P_9^l = 2.9$ percentage points or $P_9 = 93.2\%$. In other words, due to children, Austrian mothers are 2.9 percentage points and 93.2%

[21] Appendix Figure A.1 shows a version of this figure that measures the effects in percent (instead of percentage points). Specifically, it plots $\hat{\alpha}_t^m / E[\tilde{Y}_{ist}^m|t] \cdot 100$ and $\hat{\alpha}_t^f / E[\tilde{Y}_{ist}^f|t] \cdot 100$.

[22] 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.

[23] Glogowsky *et al.* (2023) show in a follow-up paper that only fathers of boys develop depression, likely because they invest more time in childcare.

Figure 1: Impacts of parenthood on antidepressant prescriptions

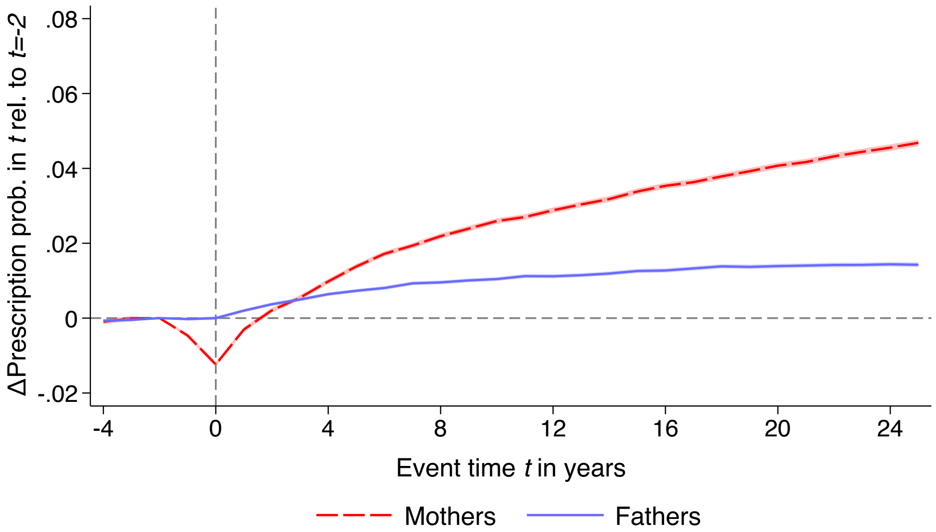


Notes: This figure shows the estimated impacts of parenthood $\hat{\alpha}_t^j$ on antidepressant prescriptions before and after having the first child (in percentage points). It focuses on mothers ($j = m$, dashed lines) and fathers ($j = f$, solid lines). Figure 1a is for Austria and Figure 1b for Denmark. 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.

more likely to get antidepressants than Austrian fathers.

In Denmark, the post-birth antidepressant prescription patterns for mothers and fathers are qualitatively similar to those in Austria. Quantitatively, the impacts of parenthood on both parents are, however, somewhat weaker ($\hat{\alpha}_9^f \approx 2.7$ and $\hat{\alpha}_9^m \approx 0.8$). Also, the resulting parenthood penalties are around 1.0 percentage points lower. Nine years after giving birth, the impact of parenthood on mothers' prescription probability is about 1.9 percentage points larger than that for fathers. Put differently, due to children, Danish mothers are $P_9^l = 1.9$ percentage points or $P_9 = 64.8\%$ more likely to get antidepressants than fathers.

Figure 2: Long-run impacts of parenthood on prescriptions in Denmark



Notes: This figure shows the estimated impacts of parenthood $\hat{\alpha}_t^j$ on antidepressant prescriptions before and after having the first child (in percentage points). It focuses on mothers ($j = m$, dashed lines) and fathers ($j = f$, solid lines) in Denmark. 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.

Given the observed effect dynamics, two questions arise naturally. The first question is why the effects and penalties grow over time. Our intuition is that a multitude of factors contribute to these patterns. The development of depression is not an instantaneous process, and the probability of a health shock in children increases over time. 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.[24] As a side note, the evidence from one-child families reported in Appendix Figure A.2 suggests that the initial years of parenthood are the most mentally taxing (the increase in antidepressant prescriptions is the strongest for the first year). The second question that follows naturally concerns the persistence of the penalties. Do they continue beyond the ninth year after birth? This topic is particularly pertinent given that economists usually consider children as long-term investments. Along these lines, while the costs of parenting may be incurred primarily in the first years after birth, the mental benefits may unfold in the long term. For Denmark, we have enough data to study parents in 25 post-birth years. Specifically, we can expand our balanced panel to an unbalanced one that (a) runs from 1995 to 2019 and (b) includes parents who gave birth until 2021. Figure 2 studies this sample and shows that the parenthood penalties persist over time.

[24]For Denmark, we can explore how the effect sizes vary across total fertility. Given that the subsequent fertility is (potentially) endogenous, we do not derive firm conclusions from this analysis. Nevertheless, we estimate separate event study models (around the first child's birth) for those with one, two, or three or more children. Appendix Figure A.2 shows that the effects of parenthood on mothers and fathers increase in parity. In line with the notion that men bear more costs of higher-order births, the increases are stronger for fathers.

While men remain on an elevated but flat trend after the seventh year of their child's birth, women's prescription probability increases continuously. Due to the long delay between birth and outcome measurement, these estimates rely on stronger identifying assumptions. Thus, we interpret Figure 2 as suggestive evidence that the parenthood penalty in mental health is a persistent phenomenon.

4.3 Specification checks

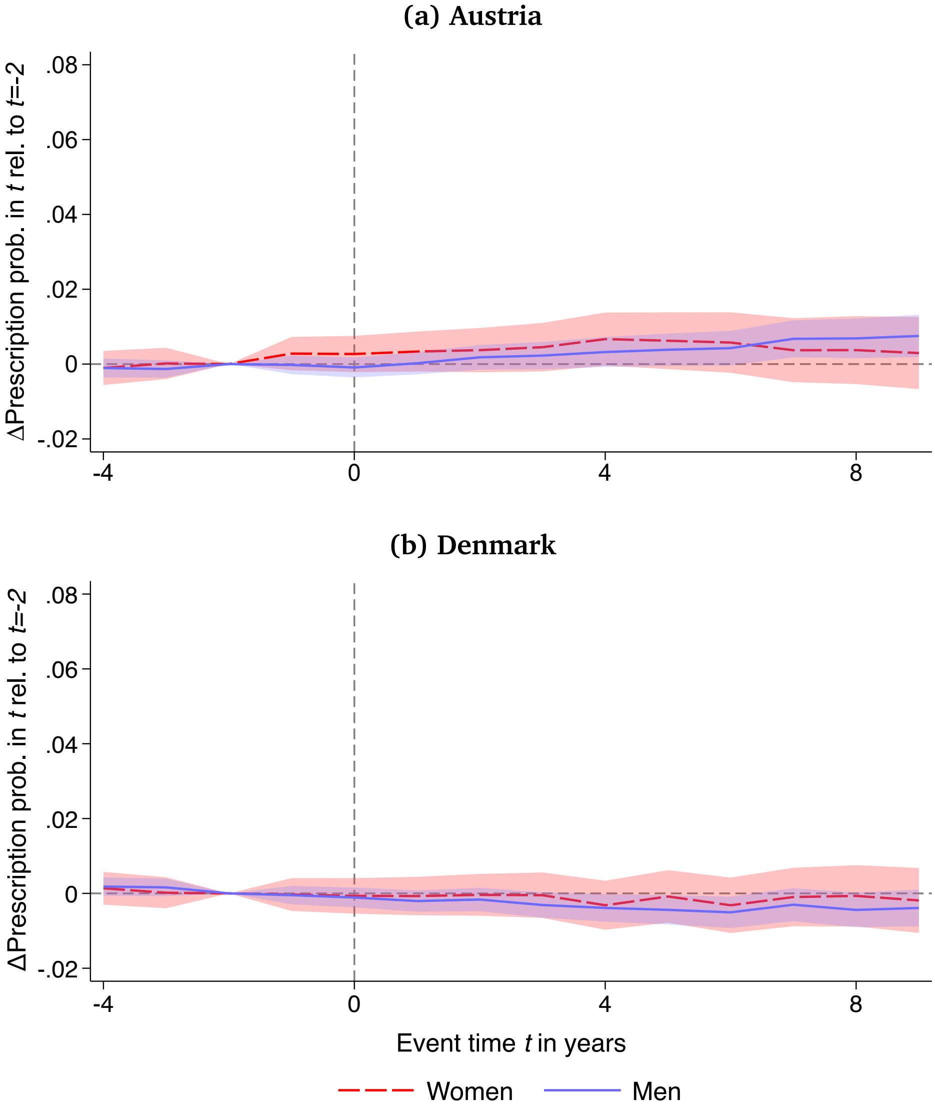
We probe the robustness of our results with several specification checks. The first check explores whether the patterns observed in Figure 1 mirror general trends unrelated to parenthood that our age and year controls fail to capture. One may view this test as particularly crucial, given that the effects of parenthood increase gradually over time. Our approach to examining general trends lies in studying childless individuals. The idea is to (a) assign placebo birth events to childless individuals and (b) study whether similar patterns appear around this hypothetical event.[25] If such patterns do appear, we unlikely can ascribe the estimated impacts solely to the impact of parenthood. Figure 3 depicts the results for Austria (see Figure 3b) and Denmark (see Figure 3b). In contrast to our baseline estimates, we see no sharp changes in women's and men's antidepressant prescriptions around the placebo event. Importantly, the 95% confidence intervals for men and women overlap in both countries. As an extension, we use childless individuals with assigned placebo births as a control group in a difference-in-difference 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 very comparable point estimates to the baseline results (see Appendix Figure A.3).[26] The evidence indicates that non-child trends do not confound our baseline estimates.

The second check explores if our comparison between Austria and Denmark is confounded because we cannot link all unmarried Austrian fathers to children. Our approach to harmonize the analysis across countries is to apply it to the well-defined sub-samples of married parents. We can confirm all of our basic conclusions for this comparable sub-sample (see Appendix Figure A.4). In both countries, married mothers and fathers experience an increase in the prescription probability after the birth of their first child. The increase is more pronounced for women than men, and the resulting parenthood penalties are larger

[25] The details of our approach are as follows: To assign placebo birth events, we approximate the factual distribution of age at first birth by a log-normal distribution $\mathscr{L}\mathscr{N}(\mu_{jc}, \sigma^2_{jc})$ within cells of parents' birth cohorts c and their sex j. The mean μ_{jc} and variance σ^2_{jc} follow from the actual age at the first birth in the cell jc. We then draw an hypothetical age at birth from this distribution for each childless individual and repeat the event-study analysis based on the placebo events.

[26] Because we estimate additional parameters and an interacted treatment effect, the confidence bands are wider. However, the parenthood penalty is still significantly positive in both countries.

Figure 3: Impact of placebo birth on antidepressant prescriptions



Notes: This figure shows the estimated impacts of the placebo birth $\hat{\alpha}_t^j$ on antidepressant prescriptions before and after the placebo birth (in percentage points). It focuses on childless women (dashed lines) and childless men (solid lines). Figure 3a is for Austria and Figure 3b for Denmark. We obtain the event time coefficients from 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.

in Austria.[27] Our interpretation is that the stronger penalties observed in Austria are unlikely to result from the discussed data limitation.

To add further credibility to our results, we plot the raw data as a third plausibility check (see Appendix Figure A.5). The figures show substantial parenthood penalties in both countries. Thus, our main conclusions remain even unchanged if we consider the raw

[27]Notably, compared to the overall sample, the increase in prescriptions for married Austrian men is lower and only statistically significant after the sixth post-birth year. With this consistent, the effects for unmarried fathers are higher. A potential explanation is that unmarried fathers are more involved in childcare. Although this holds for single fathers almost by definition, cohabiting fathers may also be more involved (they could be more gender progressive). Cohabiting mothers may also be less willing to make marriage-related investments.

Table 3: 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 tables 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.

data instead of employing the event-study machinery with flexible age and year controls.

4.4 Parenthood penalties: Actual differences in mental health?

A key question in interpreting parenthood penalties is whether they reflect differences in actual mental health between the sexes or, for example, differences in help-seeking behavior. In this section, we present evidence suggesting that parenthood indeed imposes a greater psychological burden on mothers than fathers. We organize the discussion around potential concerns that challenge this interpretation and explore the validity of these concerns.

The first and probably most important concern is that the help-seeking behavior of women and men with mental health problems may differ. Specifically, it is possible that both parents experience declining mental health, but women are more likely to seek help. We present two lines of evidence that speak against this interpretation.

First, we use survey data to examine whether women with mental health problems are generally more likely than men to seek medical help. The *Special Eurobarometer 246* (Eurobarometer, 2006) is well suited for this purpose because it includes information on self-reported mental health and help-seeking behavior. Based on these data, Table 3 tests whether women with self-reported mental disorders (i.e., with an MHI-5 score below 52) are more likely than men to express that they have sought help "*from a professional in respect of a psychological or emotional health problem.*" We employ country-specific and pooled regressions for that test. Whether or not we control for respondents' MHI-5 score, we find no statistically significant difference between the self-reported help-seeking behavior of women and men who face mental health problems.[28] Moreover, the coefficients are small, particularly if we condition on the mental health score. This self-reported gender equality in the propensity to seek help applies to the entire distribution of MHI-5 scores (see Appendix Fig-

[28]The statistically significant negative coefficient for the MHI-5 score indicates that respondents with better mental health are less likely to seek professional help.

ure A.6). The takeaway message from this analysis is that women with self-reported mental health issues do not report seeking help more often than corresponding men.

Second, we also examine traces of differential help-seeking between the sexes in our administrative data. Our test aims to explore whether men and women adjust their help-seeking behavior differently post-birth. One possible explanation for such differential adjustments could be that, due to the nature of maternity leave, mothers may have more opportunities or time to consult doctors than fathers. Naturally, administrative data do not contain explicit information on help-seeking behavior. We, however, observe consultations with those doctors who provide medication for mental health problems. In both countries, it is the GPs who prescribe almost all antidepressants.[29] Given that feature, any sex-specific adjustments in postnatal help-seeking behavior that result in differences in antidepressant prescriptions would become apparent in the GP visit patterns. Specifically, if women adjust their help-seeking behavior more than men, we should observe a greater rise in their probability of a GP visit than in that of men.

The patterns in our data challenge this notion. Consider the Austrian case depicted in Figure 4a. After a period of comparable prenatal trends, the GP visits of Austrian mothers and fathers diverge. Intuitively, pregnant mothers are more likely to visit a GP. Post-birth, the pattern for Austrian mothers flips, and their visiting probability even drops below the level in the pre-birth year. This finding suggests that new mothers have less time and opportunity for self-care and help-seeking with GPs, not more. However, most importantly, although mothers' visiting probability gradually begins to rise after birth, it constantly remains below the fathers' trend. Both parents' probabilities even develop in parallel around the time and after the mental health penalty materializes. We, therefore, do not observe the expected postnatal rise in mothers' GP visiting probability (relative to men). This finding suggests that the penalties are unlikely to arise from mothers adjusting their help-seeking behavior with GPs more than fathers. It also implies that, conditional on seeing a doctor, mothers' probability of receiving antidepressants is much higher post-birth. The results for Denmark presented in Figure 4b are similar. Again, the postnatal trends for women always stay below those for men.[30]

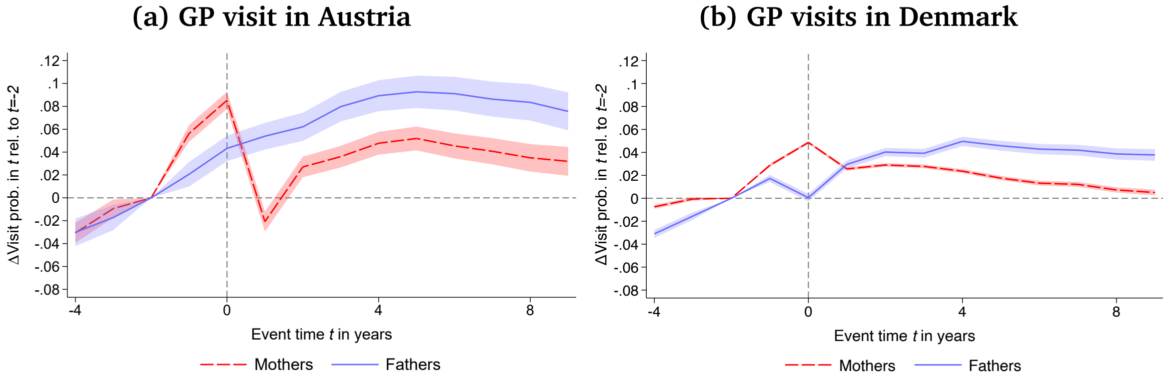
A related concern is that due to the features of the health system, women may be more closely screened for mental health-related issues after birth. Denmark, for example, offers a free nurse home visiting program, and Austria has a well-child visiting program.[31]

[29] They, for example, regularly prescribe over 84% of all antidepressants in Austria.

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

[31] The Danish program is available within the first year after childbirth. Typically, the first visit takes place four to five days after birth. As part of the program, the nurse participates in postpartum depression screening in the second month after delivery. In Austria, a similar structured screening for mental health issues does not exist. However, a well-child visiting program exists in Austria (called the *Mutter-Kind-Pass*). This program includes developmental screenings at the pediatrician's office throughout the child's fourth year. Consequently,

Figure 4: Impacts of parenthood on doctor visits



Notes: This figure shows the estimated impacts of parenthood on the probability of a GP visit in Austria (Figure 4a) and Denmark (Figure 4b) before and after having the first child for mothers ($j = m$, dashed lines) and fathers ($j = f$, solid lines). 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.

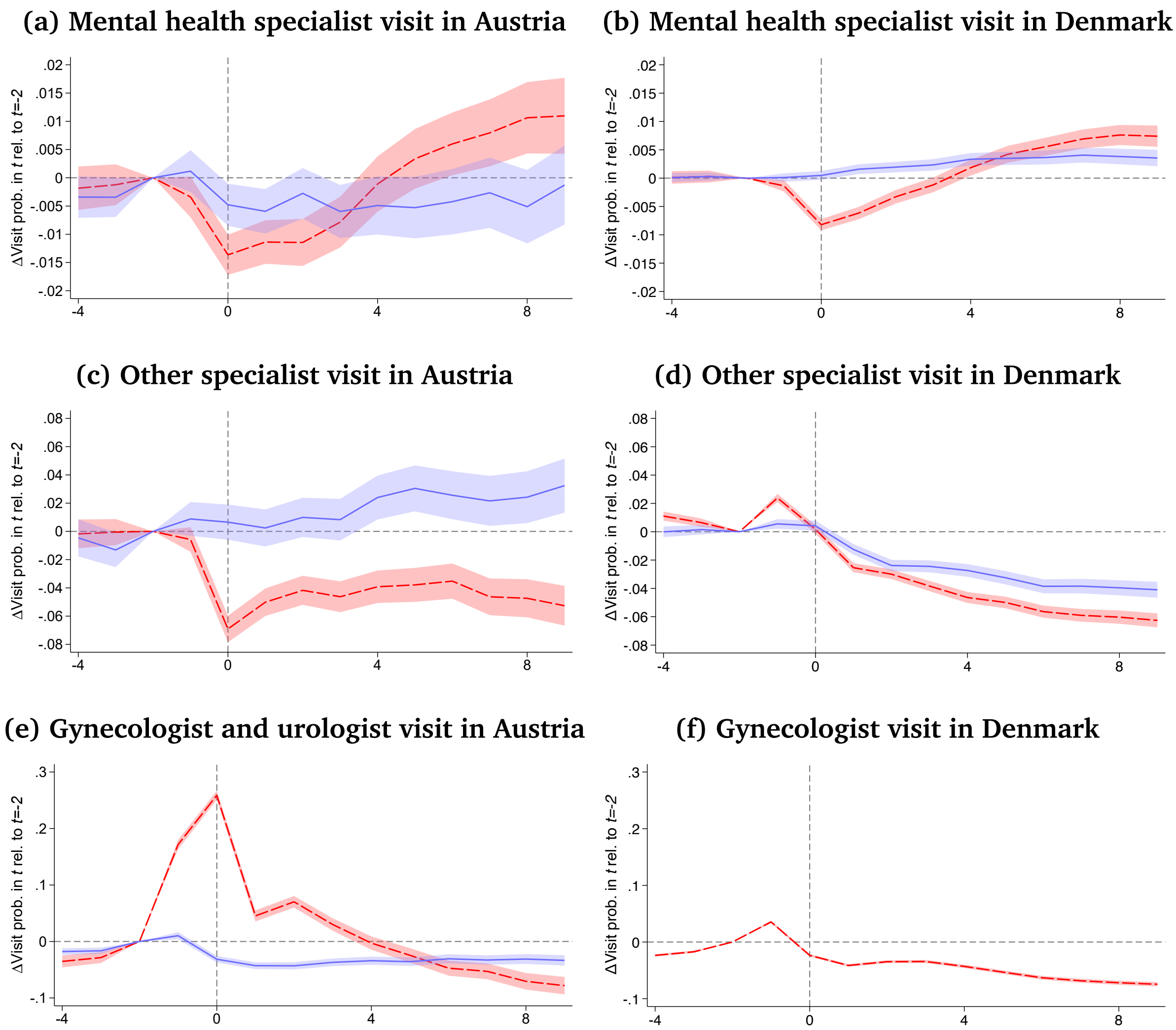
However, it seems unlikely that the parenthood penalties result from these features of the healthcare systems. First, if the observed effects on mothers' mental health were a result of such programs, we should see almost immediate impacts following childbirth (this is when the programs take place). Instead, the adverse effects manifest after the first child reaches the age of three and intensify after these programs end. Second, any prescription of antidepressants (including refills) requires a consultation with a GP (or a specialist). Thus, any gender difference due to these home or well-child visiting programs would lead to a higher increase in mothers' than in fathers' GP visiting probability (see Figure 4). Third, we also document parenthood penalties in the case of adoptions (see Subsection 4.5). These penalties arise despite the fact that adoptive parents naturally have fewer interactions with the nurses in the program.[32]

Another potential concern is that the parenthood penalties could be mere artifacts reflected in antidepressant prescriptions. We respond to this worry by investigating whether parenthood penalties (a) are evident in other mental health-related outcomes and (b) are absent in placebo outcomes unrelated to mental health issues. Figures 5a and 5b examine an outcome that should indicate mental health problems: the probability of consulting a psychiatrist or neurologist. For both countries, women are more likely to consult one of these specialists from about the fourth year after childbirth. These patterns are in line with those we observe for antidepressant prescriptions. On the contrary, there are no penalties for visits to other specialists who do not treat mental health problems (see Figures 5c and 5d). This piece of evidence also speaks against the hypothesis that there are differences in the

Austrian mothers see a pediatrician regularly who could then identify mental health problems.

[32]Adoptive parents do not experience the physical consequences of childbirth that usually necessitate these visits. Furthermore, the program schedules fewer visits for adoptive families, given that adoption cannot occur until the child is at least three months old. These factors together reduce the engagement opportunities for adoptive parents.

Figure 5: Impacts of parenthood on visits of outpatient specialists



Notes: This figure shows the estimated impacts of parenthood on the likelihood of a certain outpatient physician visit in Austria and Denmark before and after having the first child for mothers ($j = m$, dashed lines) and fathers ($j = f$, solid lines). Figures 5a and 5b focus on mental health professionals, Figures 5e and 5f on gynecologists and urologists (Austria) and gynecologists (Denmark), and Figures 5c and 5c on other specialist visits. 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.

general help-seeking propensity across the sexes that explain the gender disparities in our baseline effects. As a sanity check, we examine the visits of gynecologists and urologists in Austria (see Figure 5e). Unsurprisingly, we see a large uptick of visits around childbirth for women but not for men. In Denmark, we observe a similar pattern for gynecologists (see Figure 5f), but we cannot identify urologists in the data. Appendix Figures A.7 (for Austria) and A.8 (for Denmark) complete the picture by relying on other nervous system drugs as outcomes. Reassuringly, we do not find significant parenthood penalties for drugs unrelated to mental health issues.[33] Overall, the evidence indicates that our results do not just reflect

[33]The only two categories with significant and economically meaningful penalties are psychoanaleptics and

artifacts measured in antidepressants but, more generally, penalties in mental health.

The last concern is that men could more often turn to self-medication than women (e.g., by taking narcotics) instead of seeking professional help. To probe this hypothesis, we investigate the impact of parenthood on a dummy variable representing the use of psychoactive substances (see Appendix Figure A.9). We construct this variable to indicate whether (a) an individual received an inpatient diagnosis of a mental or behavioral disorder due to the use of psychoactive substances (ICD-10 code: F1) or (b) obtained drugs used in addictive disorders (ATC code: N07B). For both countries, we find virtually no differences in the impacts of parenthood across the sexes. The evidence clearly speaks against the self-medication theory. In sum, our analysis of the various concerns strongly suggests that the observed parenthood penalties reflect genuine differences in mental health between men and women following childbirth.

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

What explains the parenthood penalties in prescriptions, and why do they persist? This section explores two sets of explanations. The first pertains to the psychological impact of *having and raising* a child. For example, parenthood disproportionately disrupts mothers' routines or labor supply (Angelov *et al.*, 2016; Kleven *et al.*, 2019a,b); it also imposes additional childcare responsibilities on them (Borra *et al.*, 2021; Guryan *et al.*, 2008) and increases their cognitive load (Orchard *et al.*, 2023). The second explanation concerns potential *biological effects of giving birth* to a child. Only women undergo the physical and hormonal changes associated with pregnancy, childbirth, and breastfeeding. These factors likely have immediate short-term effects but may also long-lastingly affect mothers' mental health.[34]

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 (Mughal *et al.*, 2022; Slomian *et al.*, 2019; Vliegen *et al.*, 2014). The patterns in Figure 1 are, however, unlikely the result of postpartum depression alone. First, 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.[35] Instead, the

analgesics. Both types of nervous system drugs contain mental health drugs. Psycholeptics include antidepressants, and analgesics contain drugs for the treatment of anxiety disorders or bipolar disorders. Thus, this result also aligns with actual penalties in mental health.

[34]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 (Feldman *et al.*, 2007; Hoekzema *et al.*, 2017; Numan and Insel, 2003).

[35]Increasing effects in later years may (a) suggest additional mothers receive prescriptions after the period for postnatal depression or (b) indicate mothers experience postnatal or other depressions after having more children. To exclude the latter possibility, Appendix Figure A.2 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.

estimated adverse effects only emerge after the first child reaches the age of three and increase over time. Second, our key outcome, antidepressant prescriptions, is arguably 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 certain antidepressants for medical reasons.

Despite these considerations, we nevertheless conduct two test to explore if 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 (see Appendix Figure A.10). The second test uses a binary indicator for whether or not person i receives the very first prescription in year s and event time t as an outcome of model (1). This approach allows us to accurately identify when medication begins, offering a clear distinction between immediate postpartum depressive episodes and mental health issues that emerge or receive medication later in the motherhood journey. Contradicting the hypothesis that postpartum depression dominates, the impacts on the first prescriptions occur when the kids are two years or older (see Appendix Figure A.11).[36] Again, the effects grow over time. This result also counters the hypothesis that our results arise only due to mental health shocks at birth that lead to drug dependence. In that case, we 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 5a and 5b consider an outcome that does not have this property: the decision to visit mental health professionals. Parenthood also affects mothers' probability to visit such doctors in the long and not the short run, suggesting that also this outcome 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 examining 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 could then reflect selection rather than biology. Similarly to Kleven *et al.* (2021), we respond to 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 C for details). Another challenge is the sample size. Austrian families rarely adopt children (e.g., 265 cases on 2012) and we

[36] 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.

cannot identify all of them in the data. Thus, our analysis focuses on Denmark. Applying the matching approach, we find that mothers of adopted children face similarly sized parenthood penalties as biological parents (see Appendix Figure C.1). 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 (a) features of the healthcare system that target biological mothers (such as postpartum check-ups) or (b) medical concerns about taking antidepressants before or during pregnancy drive the prescription patterns for mothers of adopted children.[37] Instead, the challenges of having and raising a child seem to play a crucial role.

4.6 Heterogeneity in the effects of parenthood

The previous analysis reported average effects of parenthood on mental health, which may mask potentially important heterogeneity. Understanding effect heterogeneity is, however, crucial for crafting targeted policies. Thus, our next step is to study how the effects vary across subgroups.

We consider child health as the first dimension of interest. Intuitively, the child's health status could significantly affect the parents' mental health, especially because parents of sick children may be constantly worried or have to provide additional care. Low infant birth weight (less than 2,500 grams) is the standard measure of potential health complications in children. This is because it is not only a good predictor of short- and long-term health but also a major risk factor for infant mortality (Currie, 2011; Hoynes *et al.*, 2015). Along these lines, we study whether there is heterogeneity in the impacts of parenthood depending on whether children have low birth weight or not (see Appendix Figure A.12). The evidence rejects the hypothesis that parents of children with low birth weight are more likely to develop mental health problems after childbirth. The adverse mental health effects of parenthood seem to be a more general phenomenon that affects not only parents of vulnerable children.

Next, we examine if education plays a role in the effects of parenthood (see Appendix Figures A.13). Education might matter as it can protect against mental health issues (Chevalier and Feinstein, 2006) and likely shapes gender roles and family values (Thornton *et al.*, 1983; Du *et al.*, 2021). Indeed, our findings underscore the importance of education in these contexts. In both countries, the increase in antidepressant prescriptions after birth is much more pronounced among low-educated mothers than high-educated mothers. For mothers with low education, the impact of parenthood on the prescription probability in

[37] A possible worry is, for example, 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 of mental health professionals.

the ninth year after birth is, for example, approximately 5.0 percentage points (in Austria) and 3.0 percentage points (in Denmark). The corresponding estimate for highly educated Austrian mothers is not statistically significant, possibly due to a smaller sample size resulting in wide confidence bands. By contrast, for Denmark, the impact of parenthood for the same group amounts to only 0.9 percentage points. We observe similar patterns for men, with higher effects for low-educated fathers but, again, on a lower level than for mothers.

Our next heterogeneity analysis splits the sample by parents' age at first birth. On the one hand, younger parents tend to be healthier and may generally be more resilient to stress. On the other hand, younger parents may face greater challenges related to parenthood. They are in the process of establishing their careers and may still need to achieve financial stability. Such circumstances could, in turn, add to the stress of parenthood. We find more support for the second hypothesis: The impacts of parenthood are much larger for younger parents (i.e., those below the median age) and close to zero for older parents (see Appendix Figure A.14). The findings on education and age suggest that policy measures aimed at supporting (a) less educated and (b) younger parents may be particularly beneficial in reducing the mental burden of parenthood.

Finally, we study whether cultural background shapes the parenthood penalty in mental health. Culture is related to gender norms and parenting styles (Doepke *et al.*, 2019). If parents from different backgrounds experience similar penalties, this would support the notion that universal factors and challenges associated with parenthood rather than cultural norms are more likely driving the observed effects. As a very rough proxy for culture, we use religious denomination, particularly whether parents are Muslim or not. We focus on these groups because, although religious beliefs are diverse and multifaceted, previous research suggests that gender conservatism may vary along this dimension (Diehl *et al.*, 2009; Norris and Inglehart, 2012; Kalmijn and Kraaykamp, 2018).[38] In Austria and Denmark, we find that Muslim parents experience the same penalty in mental health as the rest of the population (see Appendix Figure A.15). While the confidence intervals for Muslims are larger due to smaller sample sizes, the patterns in antidepressant prescriptions are similar. This finding suggests that the parenthood penalty in mental health arises independently of cultural background.

5 Decomposition of the overall mental health gap

A well-established finding in the field of social epidemiology of mental health is the existence of a gender gap in mental health problems (see, e.g., Piccinelli and Wilkinson, 2000;

[38]The survey evidence might not represent the views of all individuals within a religious community. Muslims in our data are primarily first- or second-generation migrants from Turkey and former Yugoslavia. In Denmark, we do not observe religious denominations. We focus on migrants from predominantly Muslim countries.

Nolen-Hoeksema, 2001; Van de Velde *et al.*, 2010; WHO, 2017; Churchill *et al.*, 2020). For example, depression is about twice as common among women than among men in most countries. In light of this finding, and given the significant magnitude of the estimated parenthood penalty, this section explores the extent to which parenthood explains the overall gender gap in antidepressant prescriptions *among parents*. Specifically, we decompose the overall gender gap in antidepressant prescriptions among parents into (a) a part attributable to parenthood (the *parenthood-related gender gap*) and (b) a part unrelated to parenthood (the *residual gender gap*). If the parenthood penalties are correctly identified, the decomposition also has a causal interpretation.

5.1 Decomposition framework

We tailor the decomposition framework of Kleven *et al.* (2019b) to our context:[39] The approach proceeds in three steps. The first step is to calculate the overall gender gap in antidepressant prescriptions for all parents in our dataset (that starts in 1998) as:

$$\Delta = \frac{E[Y_{is}^m] - E[Y_{is}^f]}{E[Y_{is}^f]} \cdot 100, \tag{3}$$

where Y_{is}^j refers to a dummy variable measuring if 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.

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 the counterfactual scenario without children. Our event study models allow us to approximate this world: We estimate model (1) separately for mothers m and fathers f, set the event-dummy effects to zero as explained in Subsection 4.1, calculate the corresponding predicted outcomes $\tilde{Y}_{is}^m$ and $\tilde{Y}_{is}^f$, and compute the residual gender gap that is unrelated to parenthood as:

$$\Delta^r = \frac{E[\tilde{Y}_{is}^m] - E[\tilde{Y}_{is}^f]}{E[\tilde{Y}_{is}^f]} \cdot 100. \tag{4}$$

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 (4) approximates the gap among parents in a world without parenthood.

[39] 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.

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

$$\Delta^c = \Delta - \Delta^r. \tag{5}$$

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.

One last point is essential to note. We define the overall gap (3) for *all* parents with firstborns until 2007 instead of 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 decompose this gap accurately, we need counterfactual predictions for all parents, including those with first children born before 2002. Otherwise, we cannot consistently compute (4) and (5). 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 firstborns before 2007.[40]

5.2 Decomposition results

Table 4 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. 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.

[40] As we use this alternative sample definition for our decomposition analysis, the reported levels of the gaps in Table 4 are different from those implied before (e.g., in Table 2).

Table 4: 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 (3), the predicted residual gender gaps among parents that we calculate using equation (4), and the parenthood-related gender gaps we obtain from equation (5). We calculate all values for an unbalanced sample of all parents with firstborns before 2007.

6 The role of parental leave

Parental leave policies are one of the often proposed measures to (a) reduce parental stress and, consequently, (b) 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, further deteriorate mothers' mental health, and boost the child penalties. There are two reasons why this is an important insight. One is that understanding the effects of such reforms on mental health and mental health inequality is important in itself, especially as the direction of the effects is unclear (see the discussion in the introduction). The second reason is that such an analysis sheds new light on the channel through which parenthood influences mothers' mental health. In particular, our evidence is consistent with the idea that childcare investments and the act of parenting can be psychologically taxing. This finding represents our second indication that the experience of having and raising children could be mentally taxing.

6.1 The Austrian 2000 parental leave reform

Austria enacted several parental leave policy reforms. Due to the following features, the 2000 reform is especially well-suited to study if parental leave extensions taken by mothers causally affect (a) mothers' mental health and (b) child penalties. First, in the past, almost exclusively mothers took parental leave. Second, the reform expanded the maximum paid *maternity* leave duration from 18 to 30 months (see Figure 6). Almost all first-time mothers qualified for the program, and the take-up among eligible mothers was nearly 100 percent (see Subsection 6.3). Third, all the other aspects of the parental leave system, such as job protection or parental benefits, remained unchanged.[41] The reform, therefore, allows us to estimate the pure effect of changing the leave duration (i.e., the pure effect of changing investments in childcare). Fourth, fathers did not change their leave-taking behavior in response to the reform.[42] Because only mothers changed their leave time, we can assess the effects of a pure maternity leave extension on parents' mental health. Fifth, and crucially for our empirical design, the reform implemented a strict birthdate cutoff, determining eligibility for extended (or reduced) parental leave without any transition rule. Specifically, 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 36 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 relatively short notice. Selection into treatment is consequently not an issue.

6.2 Regression discontinuity approaches

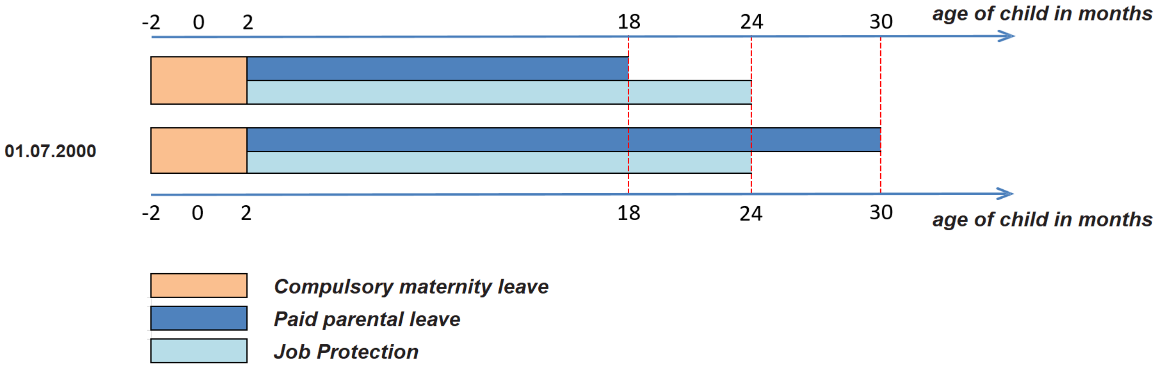
Our regression discontinuity design exploits the fact that, in a narrow window around the reforms' implementation dates, the first child's birthday exogenously assigns mothers to different parental leave regimes. The birthday, therefore, serves as a local treatment assignment variable with strong (but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

[41]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.

[42]In principle, fathers might also have responded to the reform. 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 decided to take parental leave, and the reforms did not affect this decision.

Figure 6: 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.

invariant seasonal birth pattern over a year (seasonality).[43] 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:

$$\begin{aligned} Y_i \;=\; & \alpha_0 + \alpha_1 \cdot \mathbb{1}[After_i] \,+\, \alpha_2 \cdot Run_i \,+\, \alpha_3 \cdot Run_i \times \mathbb{1}[After_i = 1] \\ & + \alpha_4 \cdot \mathbb{1}[Treat_i = 1] \,+\, \alpha_5 \cdot \mathbb{1}[Treat_i = 1] \times \mathbb{1}[After_i = 1] \,+\, \mathbf{X_{i,t=0}}\delta' \,+\, u_i, \end{aligned} \tag{6}$$

where Y_i is a measure for mother i's mental health state 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 $\mathbf{X_{i,t=0}}$.[44] Intuitively, α_3 measures the jump in the outcome at the cutoff in the 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 if (a) there is no sorting into treatment and (b) a typical parallel trend assumption holds.

[43] There is some evidence from the US that children born at different times of the year are born to mothers with significantly different characteristics (Buckles and Hungerman, 2013).

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

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:

$$\begin{aligned} ML_i \;=\; & \beta_0 + \beta_1 \cdot \mathbb{1}[After_i] \;+\; \beta_2 \cdot Run_i \;+\; \beta_3 \cdot Run_i \times \mathbb{1}[After_i = 1] \\ & + \beta_4 \cdot \mathbb{1}[Treat_i = 1] \;+\; \beta_5 \cdot \mathbb{1}[Treat_i = 1] \times \mathbb{1}[After_i = 1] \;+\; \mathbf{X_{i,t=0}}\delta' \;+\; u_i, \end{aligned} \tag{7}$$

and the second stage is:

$$\begin{aligned} Y_i \;=\; & \gamma_0 + \gamma_1 \cdot \mathbb{1}[After_i] \;+\; \gamma_2 \cdot Run_i \;+\; \gamma_3 \cdot Run_i \times \mathbb{1}[After_i = 1] \\ & + \gamma_4 \cdot \mathbb{1}[Treat_i = 1] \;+\; \gamma_5 \cdot \widehat{ML}_i \;+\; \mathbf{X_{i,t=0}}\delta' \;+\; u_i. \end{aligned} \tag{8}$$

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 parameter 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 if 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 with discussing the effects of other parental leave reforms.

Effects on mothers' leave-taking behavior: Building the basis for further analyses, we first note that the reform strongly affected the actual leave-taking behavior of mothers. Specifically, as evidenced by a sharp discontinuity in the length of leave at the cutoff (see Appendix Table A.2 and Appendix Figure A.16), eligible mothers take, on average, about nine more months of leave due to the reform. Thus, the reform substantially altered mothers' behavior.

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

Table 5: 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 *** $p < 0.01$.

(ignoring the timing of the reform effects for the moment). 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 effects on prescription probabilities (which requires a more complicated panel estimation approach).

Figure 7 then uses these two variables as an outcome of regression (6) to derive covariate-adjusted reduced-form RDD plots (first row: counts; second row: fraction).[45] As a robustness check, we provide placebo plots for the pre-reform year (see Figures 7a and 7c) besides the main plots for the reform year (see Figures 7b and 7d). Appendix Table A.3 reports the corresponding reduced-form estimates, and Table 5 summarizes the LATE estimates for three specifications. One uses triangular weights and omits covariates, the second omits weights

[45]We adjust for covariates by (a) estimating the model (6), (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.

and covariates, and the last combines triangular weights with covariates.

Across all estimation methods and outcomes, a consistent core finding 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 obtain antidepressants for an average of seven additional post-birth months between 2000 and 2016 (see Figure 7b and Appendix Table A.3). In other words, an additional year of leave causes between 0.63 and 0.82 more years with prescriptions (see Table 5). 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, ranging from 3.5 to 4.5 percentage points (see Table 5). In the Appendix, for ease of exposition, we discuss how the reform affects mental health in terms of probabilities. Specifically, we demonstrate how the reform affected the annual probability of receiving an antidepressant prescription after birth. To that end, we expand our analysis to a panel framework. This framework uses an indicator for whether or not mother i receives a prescription in post-birth year t as the outcome. We find that the reform increases the average annual post-birth prescription probability by between 3.1 and 3.9 percentage points (see Appendix Table A.4 and Appendix Figure A.17). This number corresponds to a considerable increase of about 77%. Appendix Table B.3 presents the corresponding LATEs.

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.[46] Appendix Table A.5 summarizes our results. It focuses on the impacts of the reform on the fraction of post-birth *quarters* with antidepressant prescriptions.[47] Three key 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 makes intuitive sense. 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 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 only statistically significant in specifications with covariates. Third, the adverse effects of the reform on mental health manifest over time. We find signif-

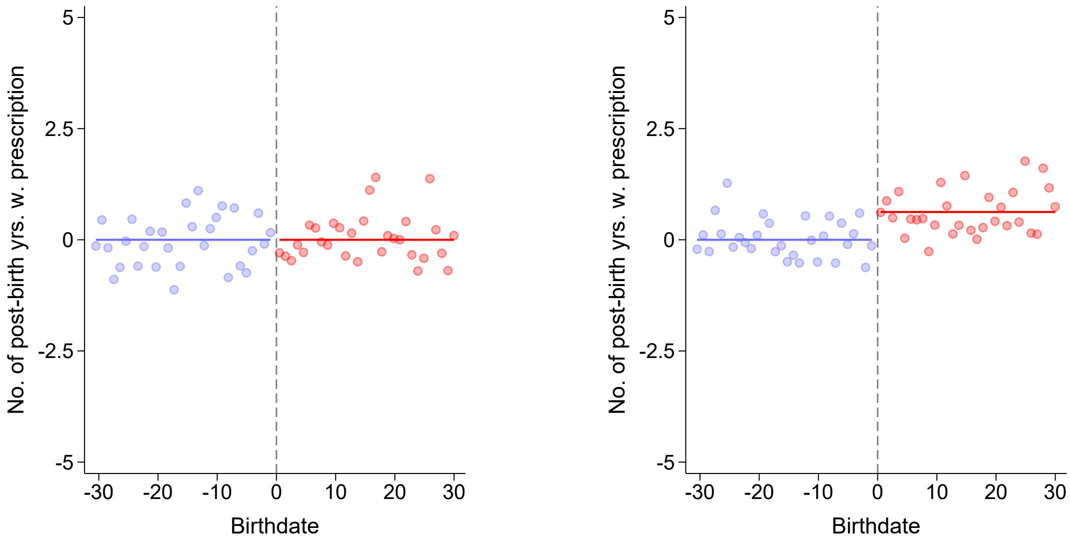
[46] Annual data would only allow us to conduct a coarser analysis. Such data, for example, prohibit analyzing whether mothers already develop depression during the expanded leave period (as the extended leave period sometimes spans over multiple years). For example, we could then not cleanly analyze if the reform has already affected behavior during the expanded leave period.

[47] Appendix Table A.6 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 length. Longer periods naturally affect the number of prescriptions and the effects' size.

Figure 7: Reduced-form impacts of the Austrian 2000 reform on maternal mental health

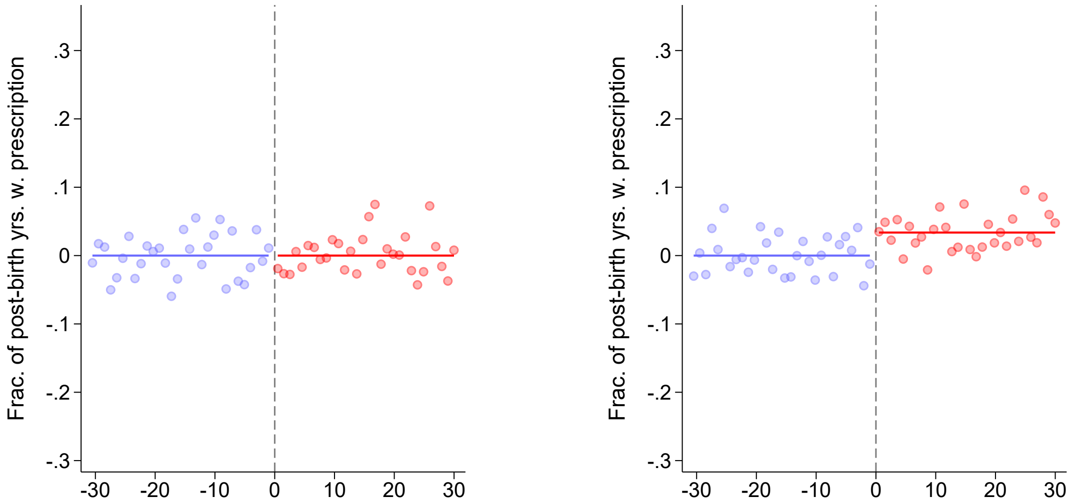
Impact on the number of post-birth years with a prescription

(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 7a and 7b) and the fraction of post-birth years with antidepressant prescriptions (Figures 7c and 7d). 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 (6), (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.

icant 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).

Effects on long-term earnings and labor supply: When interpreting the results, one 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 examined this question and documented the Austrian reforms' impacts 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.* (2020) demonstrated that the 2000 reform only affected mothers' earnings and labor supply during the leave period, with no long-term effects after 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 overarching 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 (see Appendix Figures A.18 to A.20). Moreover, the underlying reduced-form regression analysis provides economically and statistically insignificant estimates (see Appendix Tables A.4 and A.7). 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, hence, cannot serve as a silver bullet to promote mental health equality. Instead, they backfire and boost 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 the act of 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. Specifically, the reform did not affect (a) other aspects of the leave schemes, (b) fathers' mental health, and (c) mothers' long-run labor supply and earnings. Hence, its effects cannot work through these alternative channels. Fourth, the adverse effects for mothers even persist in the long run. Such negative long-run effects could be the result of 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, maternity leave extension may manifest 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 (see Appendix Table A.5): The effects increase in the immediate post-maternity leave period, coinciding with the time many mothers re-enter the workforce.

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 B). Due to institutional and data-related challenges discussed in the Appendix B, these reforms offer somewhat less clear experiments for assessing impacts on mental health. If we, nevertheless, estimate these reforms' effects with regression discontinuity designs, we 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 mildly starting from a shorter baseline period did not cause such negative effects (see Appendix B.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 Conclusions

To address gender inequality, it is essential to understand how modern societies allocate the costs of fertility between the sexes. By considering a key component of well-being, mental health, this paper highlights an unexplored dimension of child-related inequality: In Austria and Denmark, parenthood imposes a greater mental health burden on mothers than on fathers. The resulting parenthood penalty in the ninth year after birth, defined as the relative increase in the probability that mothers experience a mental health problem compared to fathers, is 2.9 percentage points in Austria and 1.9 percentage points in Denmark. In other words, due to parenthood, women's antidepressant prescription probability exceeds that of men by 93.2% (Austria) or 64.8% (Denmark). The disparities are unlikely to reflect differences in help-seeking or the biological effects of giving birth to a child. Instead, they (a) seem to mirror the psychological effects of having and raising a child and (b) are related to mothers' greater investment in childcare.

The substantial parenthood penalties show that women bear a disproportionate share of the mental costs of raising a child. While previous literature has documented substantial parenthood penalties in earnings (e.g., Kleven *et al.*, 2019b), our results show that the penalties extend beyond the labor market. Thus, many mothers also face more severe mental health problems in addition to lost earnings.

One contribution of our paper is its focus on two countries. A key advantage of such a two-pronged approach is that it allows us to test whether mental health penalties exist in different settings. We have deliberately chosen two countries, Austria and Denmark, that are very different along dimensions that could critically shape the magnitude of mental health penalties. Despite substantial differences in factors such as norms, institutions, or gender roles, the unequal impact of parenthood on mothers' and fathers' mental health exists in Austria *and* Denmark. This finding does not necessarily imply that these factors are unimportant for gender inequality in mental health. However, all the differences between Austria and Denmark do not reduce the burden on mothers enough to close the existing gender disparities entirely (even Danish mothers face penalties). Our conclusion from this evidence is that the parenthood penalty in mental health is a general and resilient phenomenon.

As an additional contribution, we show in a decomposition analysis that parenthood explains an integral part of the overall gender gap in mental health problems among parents. Policymakers concerned with mental health equity should, therefore, consider the role of parenthood. The decomposition finding also implies that there is scope to close the mental health gap through interventions tackling the parenthood-related part of gender inequality. One intervention that policymakers often propose to level the playing field is parental leave schemes. 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). However, we find in a quasi-experimental setting that longer maternity leaves (a) actually worsen women's mental health in the long run and (b) boost the parenthood penalties. Such negative long-run effects resonate with the notion that leave extensions act as a trigger for prolonged mental health challenges. On top of that, longer leaves may reinforce mothers' roles as primary caregivers, increasing their workload and mental burden. Along these lines, advancing mental health equity may necessitate policies that ease the childcare demands on mothers (such as earmarked paternity leave).

Finally, we highlight that our paper deliberately focuses on the adverse mental health effects of parenthood. These negative effects are economically important (see the introduction) and not niche phenomena (see the magnitude of our estimated effects). However, it is crucial to emphasize that not all parents develop depression after childbirth, and for some, parenthood can bring profound psychological benefits and a heightened sense of purpose. As with all multifaceted phenomena, full understanding requires a broader perspective. Thus, we look forward to future research elucidating the complete range of mental health effects of parenthood. Our paper also does not explore the extent to which the parenthood penalties in mental health tie back to child-related earnings inequalities. Such an analysis would necessitate a formal mediation analysis that leverages additional sources of exogenous variation in mental well-being, an exercise beyond the scope of this paper.[48] In this sense, much research remains to be done, and we hope our paper will be part of a broad research agenda.

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[48] A related question is to what degree the decline in mothers' mental health stems from their disproportionately greater losses in earnings. Although we do not formally decompose the effect on mental health along this dimension, our analysis of parental leave reforms suggests that mental health penalties may arise through more than just financial channels. The analysis suggests that reforms that (a) increase childcare investments but (b) leave long-run earnings unaffected worsen mental health.

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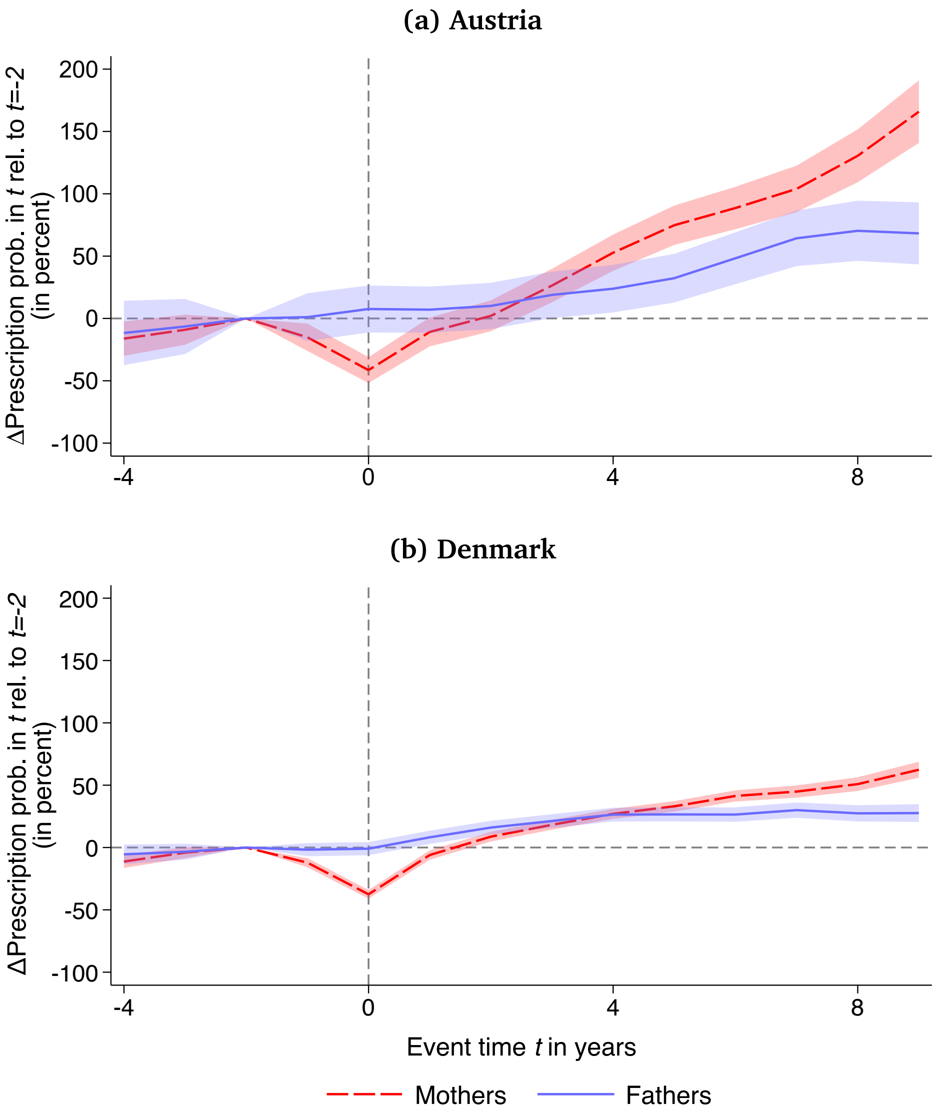
Web Appendix

This Web Appendix provides additional material discussed in the unpublished manuscript "The Parenthood Penalty in Mental Health: Evidence from Austria and Denmark" by Alexander Ahammer, Ulrich Glogowsky, Martin Halla, and Timo Hener.

A Additional estimation output

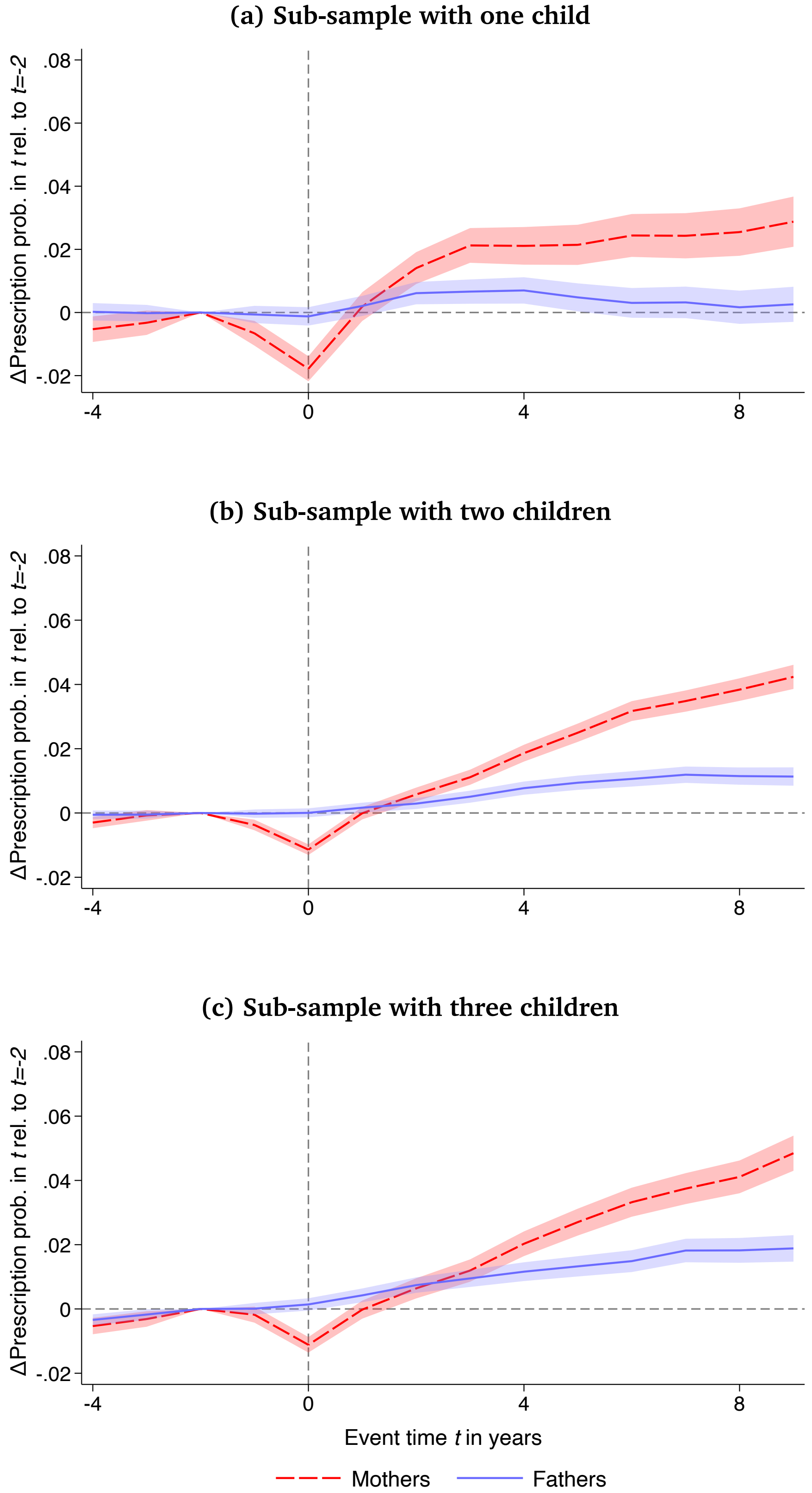
A.1 Additional figures

Figure A.1: Impacts of parenthood on antidepressant prescriptions in percent



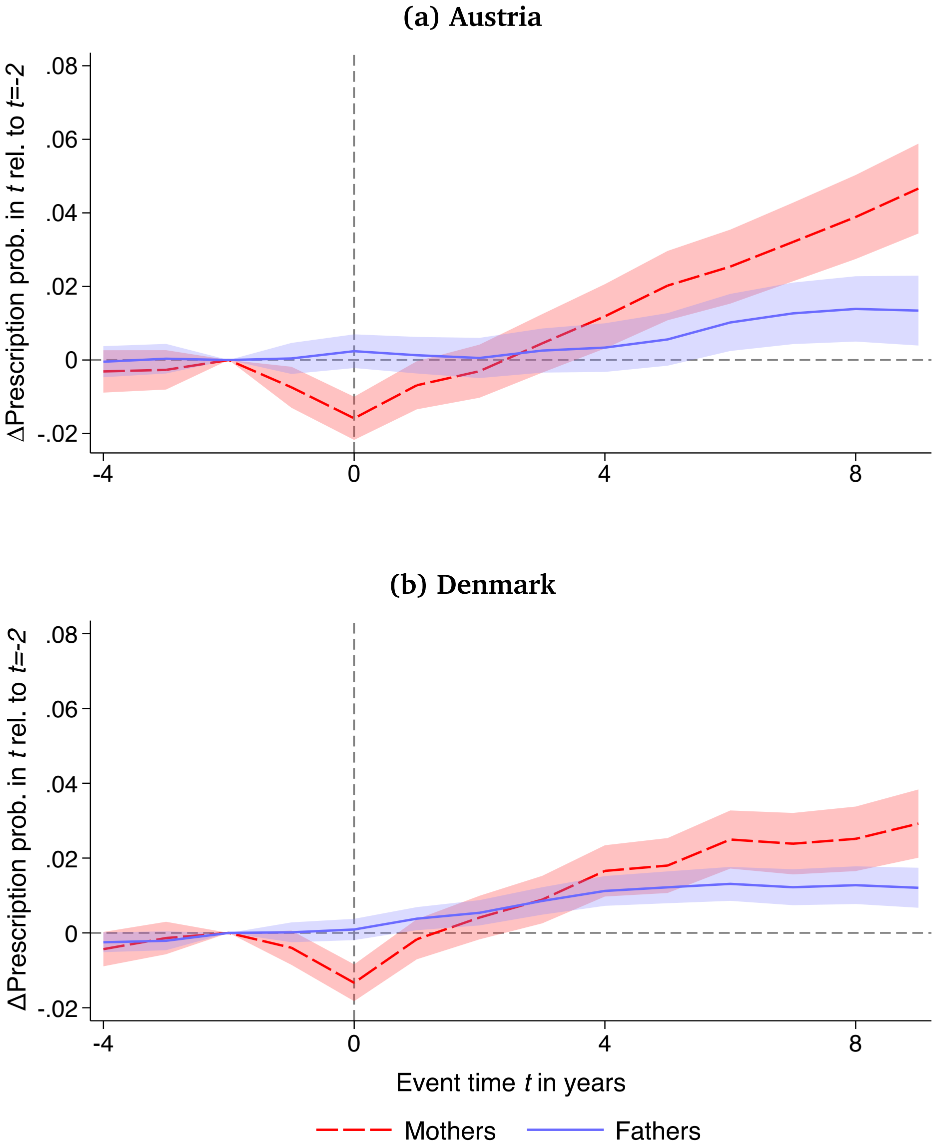
Notes: This figure shows the estimated percentage impacts of parenthood $\hat{\alpha}_t^j / E[\tilde{Y}_{ist}^j | t] \cdot 100$ on antidepressant prescriptions for mothers ($j = m$, dashed lines) and fathers ($j = f$, solid lines). Figure A.1a is for Austria and Figure A.1b for Denmark. We obtain the percentage effects 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.

Figure A.2: Impacts of parenthood by number of children in Denmark



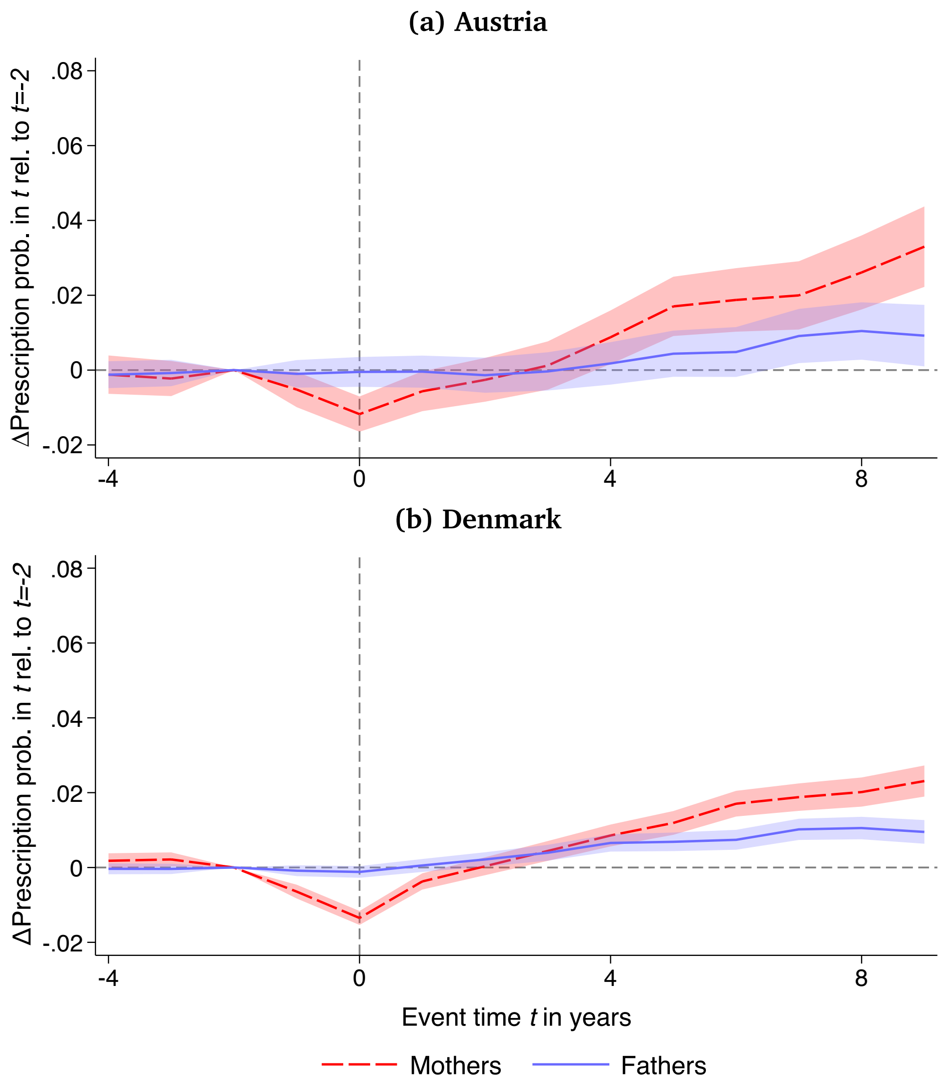
Notes: This figure focuses on Denmark and shows the estimated impacts of parenthood $\hat{\alpha}_t^j$ on antidepressant prescriptions (in percentage points) by the number of children, It focuses on mothers ($j = m$, dashed lines) and fathers ($j = f$, solid lines). We estimate separate regressions around the birth of the first child for parents with one child (Figure A.2a), two children (Figure A.2b), and three children (Figure A.2c). 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.

Figure A.3: Impact of parenthood on antidepressant prescriptions (DiD estimator)



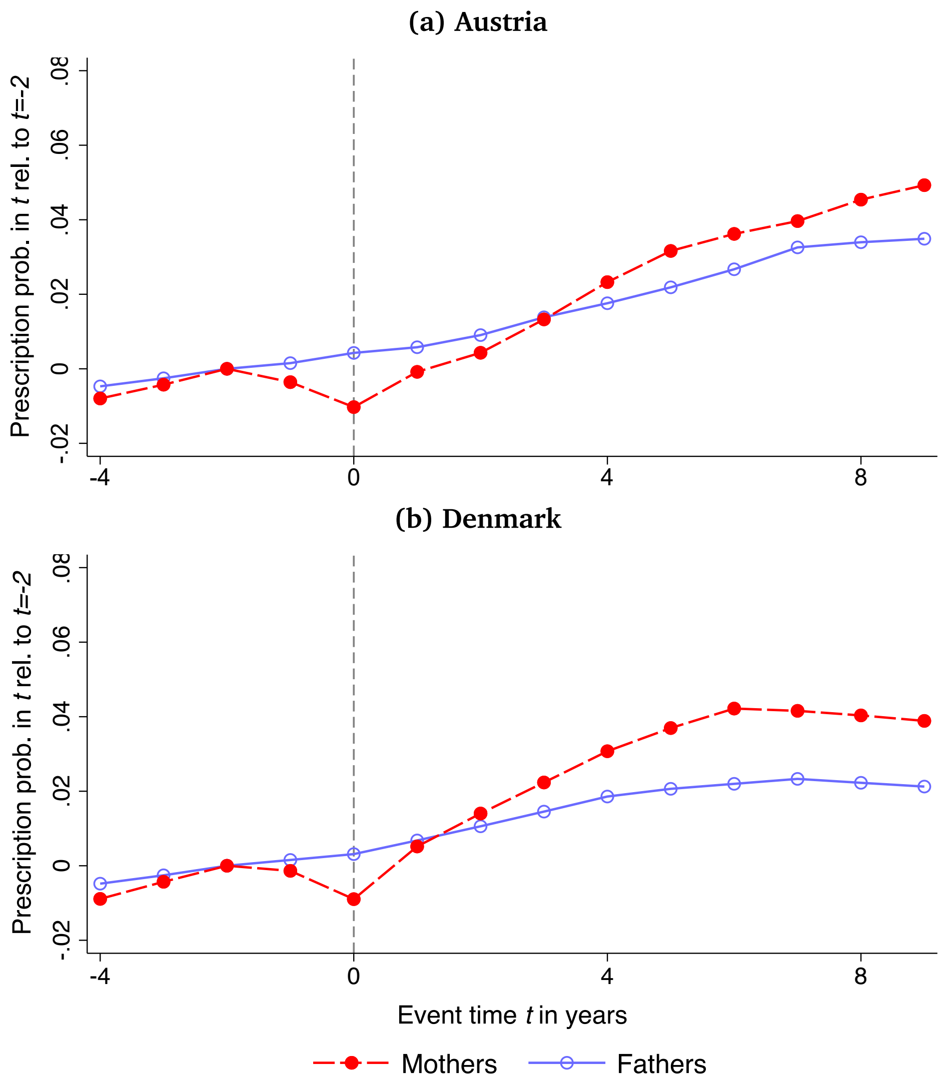
Notes: This figure shows the estimated impacts of parenthood on antidepressant prescriptions (in percentage points). It focuses on mothers ($j = m$, dashed lines) and fathers ($j = f$, solid lines). Figure A.3a is for Austria and Figure A.3b for Denmark. The estimates rely on a Difference-in-Difference-Event-Study 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.

Figure A.4: Impacts of parenthood on antidepressant prescriptions (married parents)



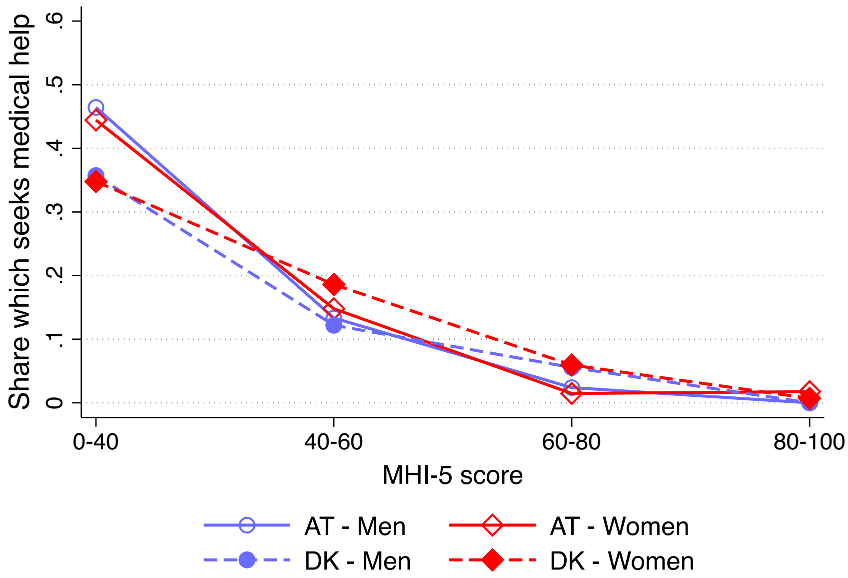
Notes: This figure focuses on married parents and shows the estimated impacts of parenthood $\hat{\alpha}_t^j$ on antidepressant prescriptions before and after having the first child (in percentage points). It focuses on mothers ($j = m$, dashed lines) and fathers ($j = f$, solid lines). Figure A.4a is for Austria and Figure A.11a for Denmark. 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.

Figure A.5: Impacts of parenthood on antidepressant prescriptions (raw data)



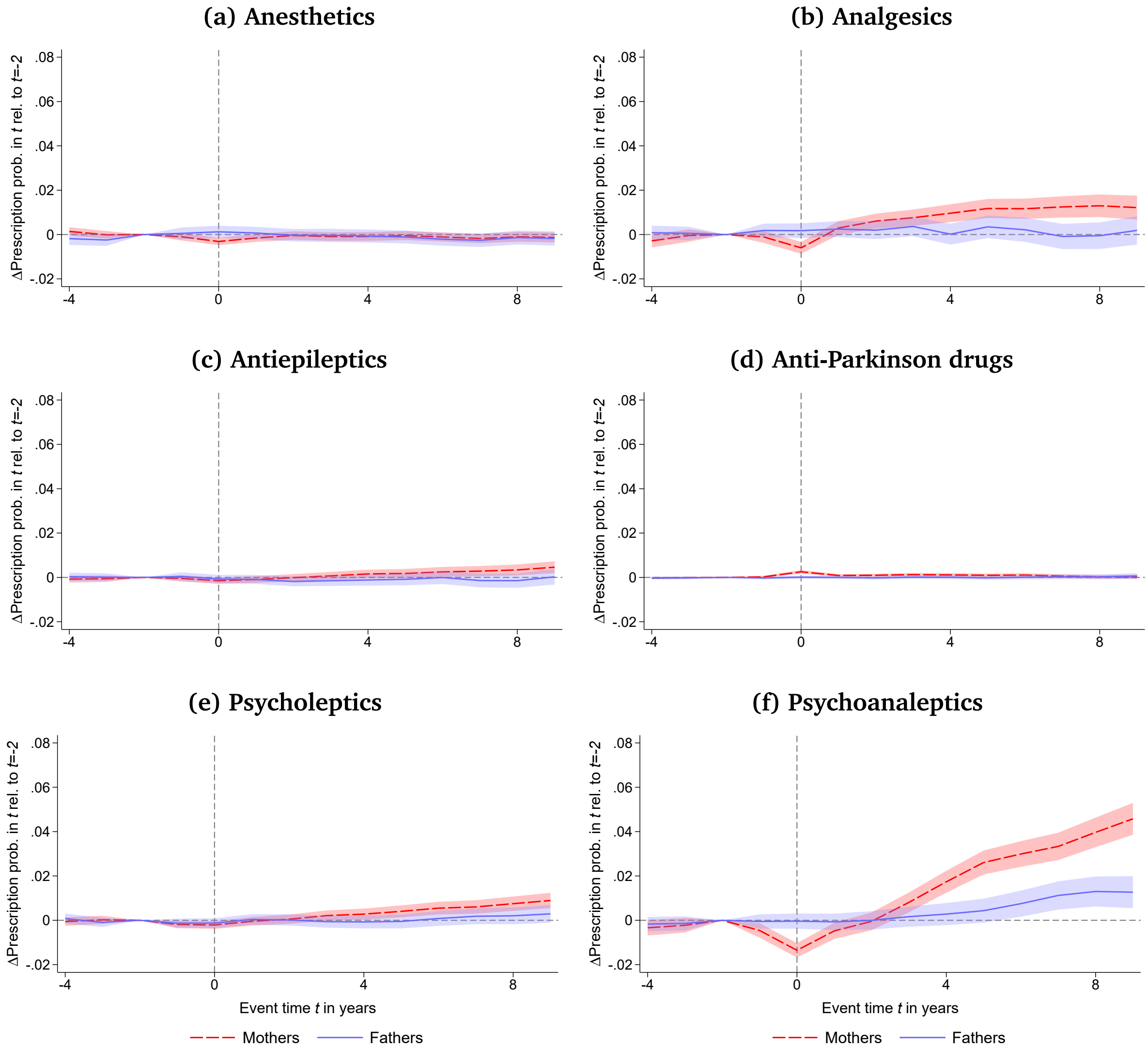
Notes: This figure focuses plots the raw data around childbirth. It focuses on mothers ($j = m$, dashed lines) and fathers ($j = f$, solid lines). Figure A.5a is for Austria and Figure A.5b for Denmark. Each dot represents an event-time-specific average.

Figure A.6: Help-seeking behavior by MHI-5 score among women and men in Austria and Denmark



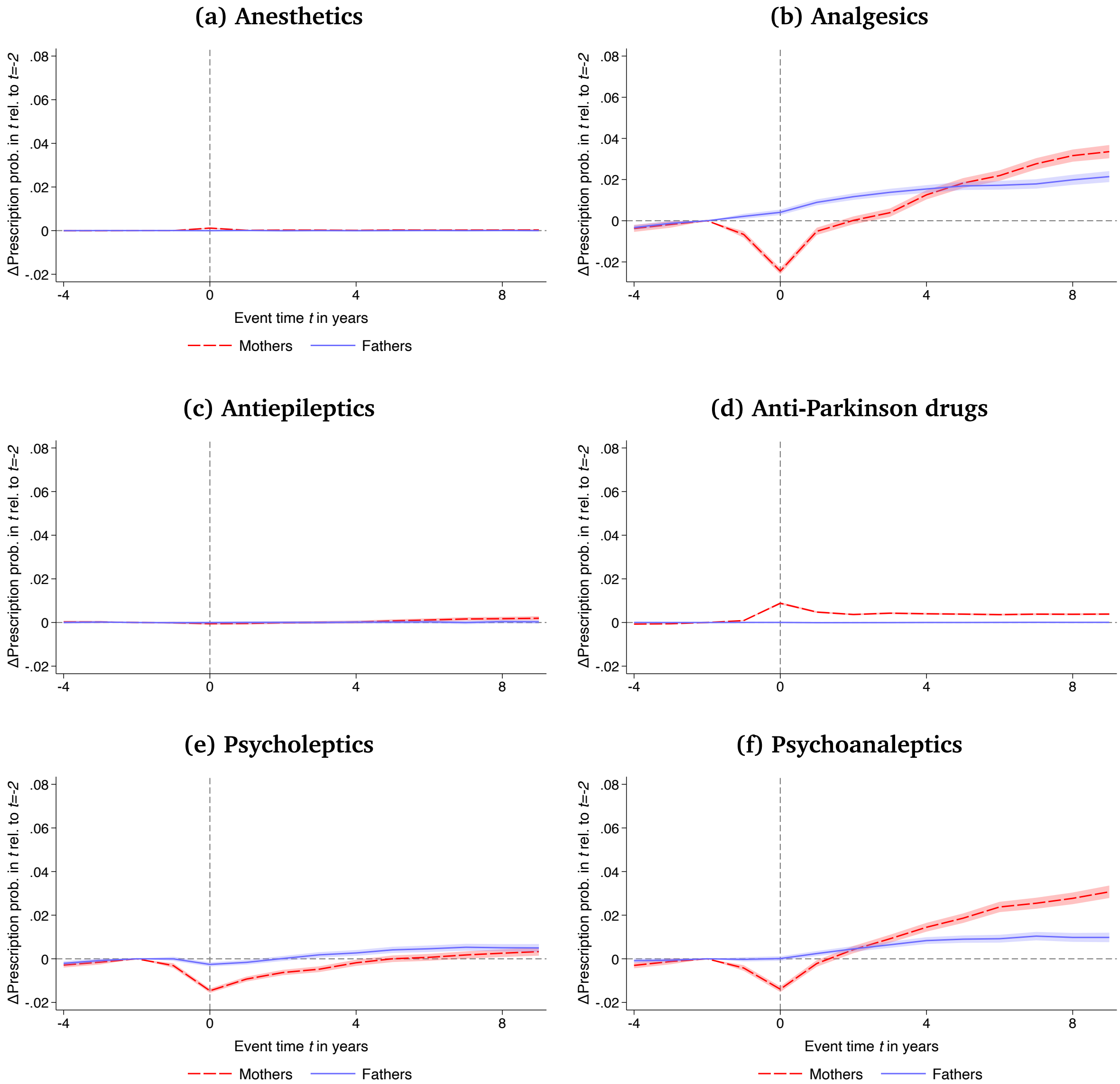
Notes: This graph correlates two variables from the *Special Eurobarometer* 246 in 2005/06 for men and women in Austria and Denmark respectively. The horizontal axis shows the MHI-5 score. This is a survey instrument for assessing the mental health of adults, with a scale ranging from 0 (indicating a 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 A.7: Impacts of parenthood on the use of certain medications in Austria



Notes: This figure shows the estimated impacts of parenthood on the use of certain medications in Austria before and after having the first child for mothers ($j = m$, dashed lines) and fathers ($j = f$, solid lines). Figure A.7a focuses on anesthetics (mainly drugs used to induce anesthesia), Figure A.7b on analgesics (mainly drugs used to relieve pain), Figure A.7c on antiepileptics (mainly drugs used in the treatment of epileptic seizures), Figure A.7d on anti-Parkinson drugs (mainly drugs used in the treatment of Parkinson's disease), Figure A.7e on psycholeptics (mainly drugs used to produce calming effects upon a person), and Figure A.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.

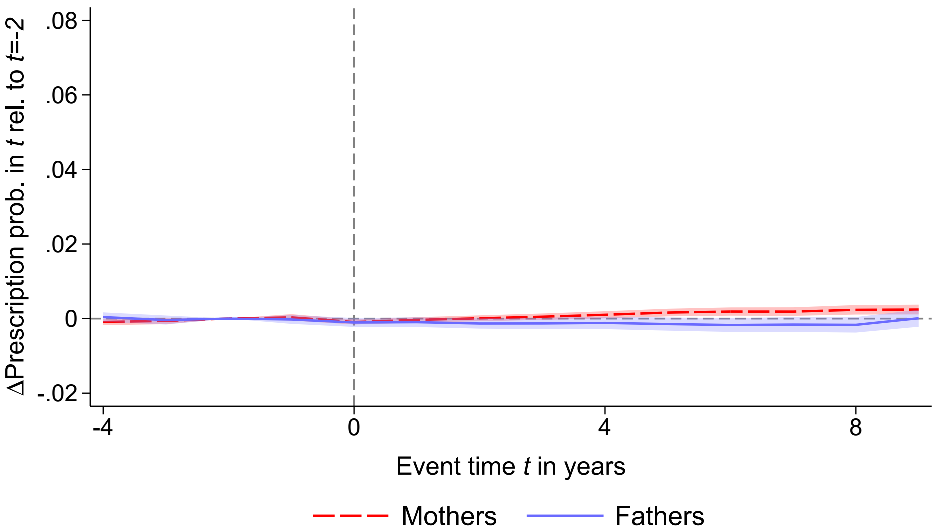
Figure A.8: Impacts of parenthood on the use of certain medications in Denmark



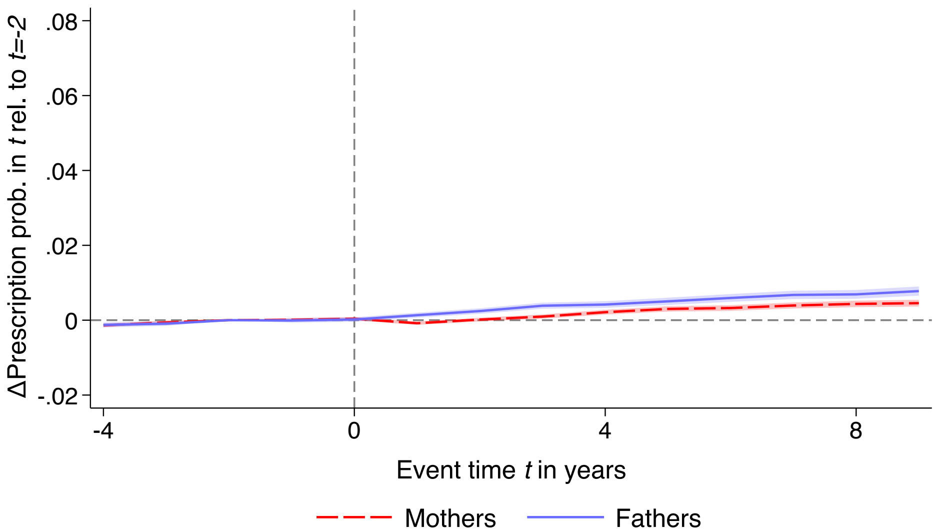
Notes: This figure shows the estimated impacts of parenthood on the use of certain medications in Denmark before and after having the first child for mothers ($j = m$, dashed lines) and fathers ($j = f$, solid lines). Figure A.8a focuses on anesthetics, Figure A.8b on analgesics, Figure A.8c on antiepileptics, Figure A.8d on anti-Parkinson drugs, Figure A.8e on psycholeptics, and Figure A.8f on psychoanaleptics. 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.

Figure A.9: Impacts of parenthood on the use of psychoactive substances

(a) Austria

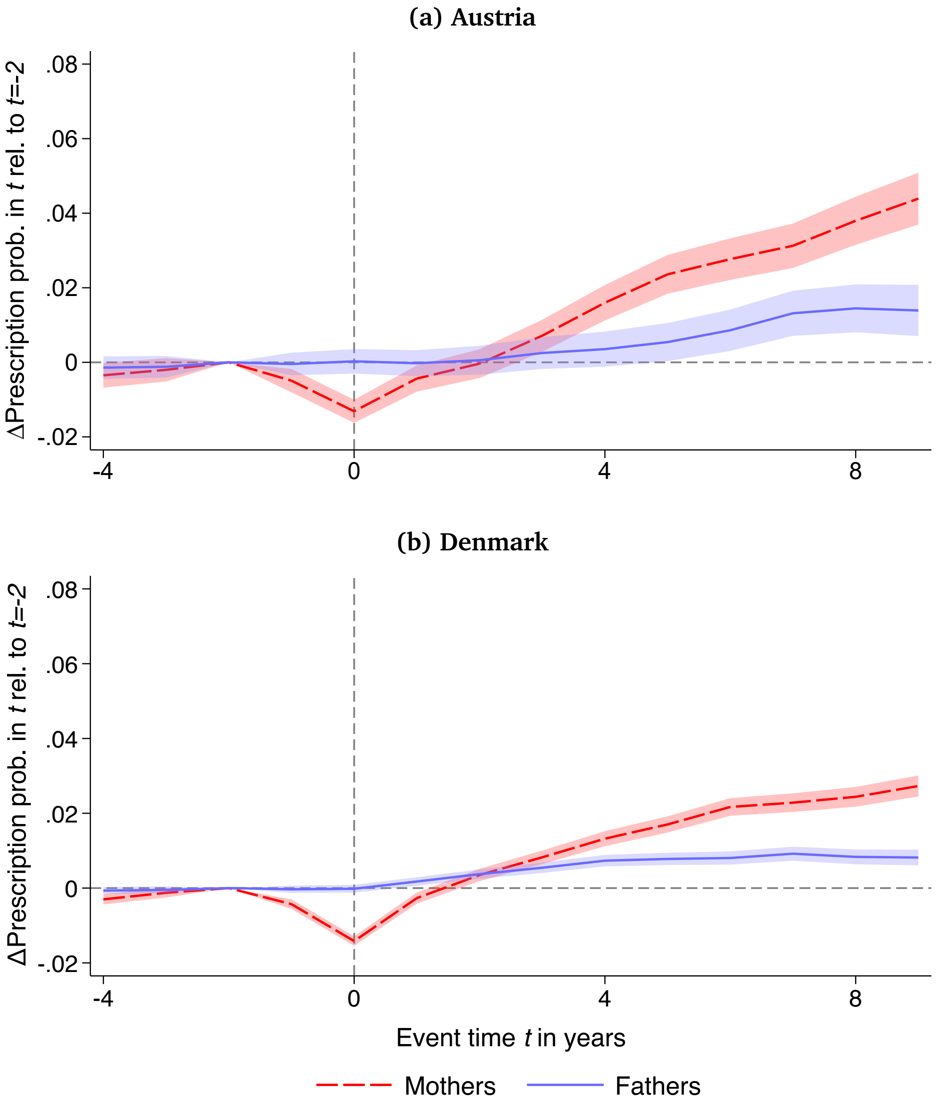


(b) Denmark



Notes: This figure shows the estimated impacts of parenthood on a dummy variable, proxying for the use of psychoactive substances for mothers ($j = m$, dashed lines) and fathers ($j = f$, solid lines). 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. Figure A.9a focuses on Austria and Figure A.9b on Denmark. 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.

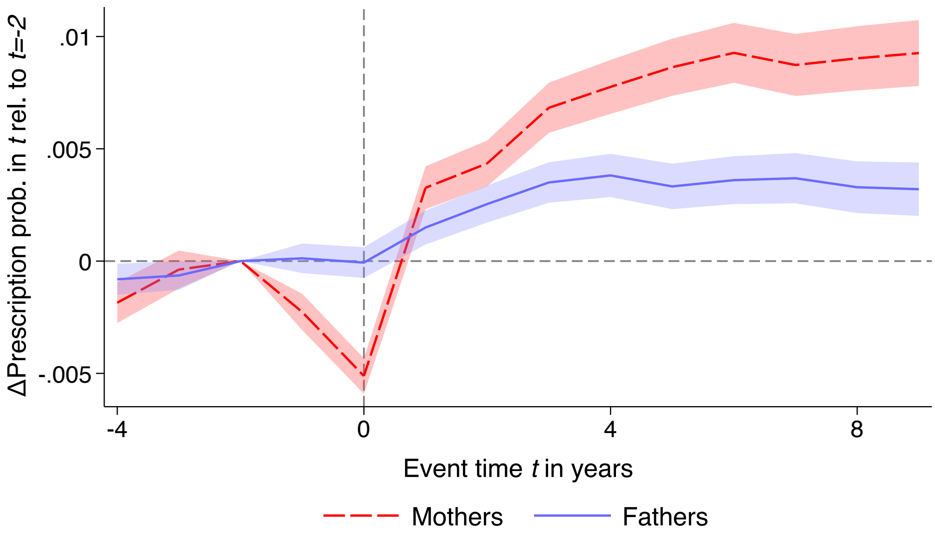
Figure A.10: Impacts for mothers who have not been diagnosed with postpartum depression



Notes: This figure shows the estimated impacts of parenthood $\hat{\alpha}_t^j$ 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). The figure focuses on mothers ($j = m$, dashed lines) and fathers ($j = f$, solid lines). Figure A.10a is for Austria and Figure A.10b for Denmark. 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.

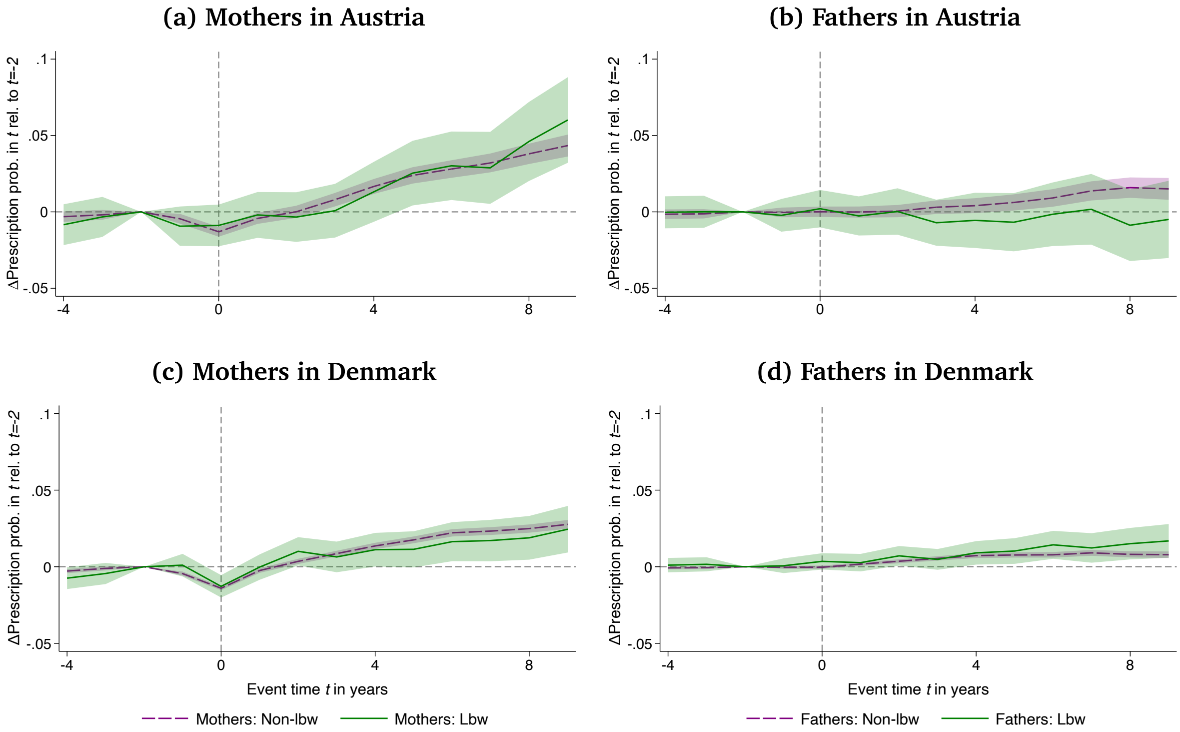
Figure A.11: Impacts of parenthood on first antidepressant prescriptions in Denmark

(a) Denmark



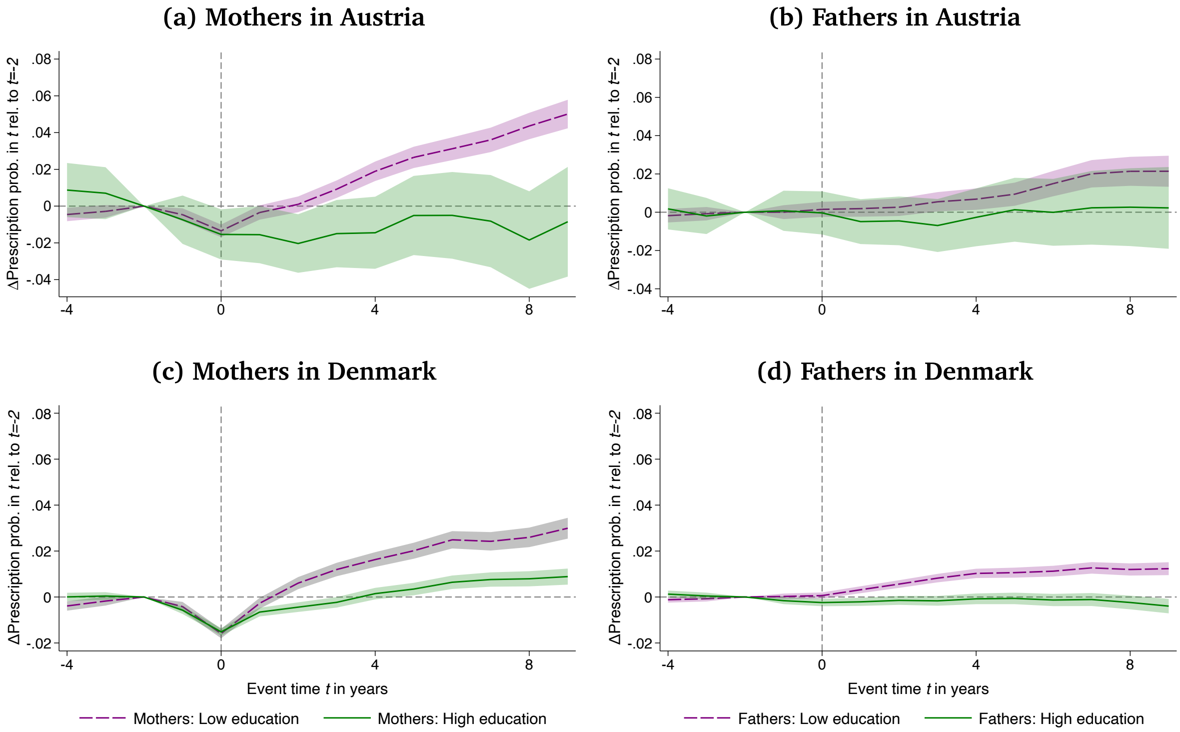
Notes: This figure shows the estimated impacts of parenthood $\hat{\alpha}_t^j$ on first antidepressant prescriptions before and after having the first child (in percentage points). It focuses on Danish mothers ($j = m$, dashed lines) and Danish fathers ($j = f$, solid lines). We obtain the event time 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.

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



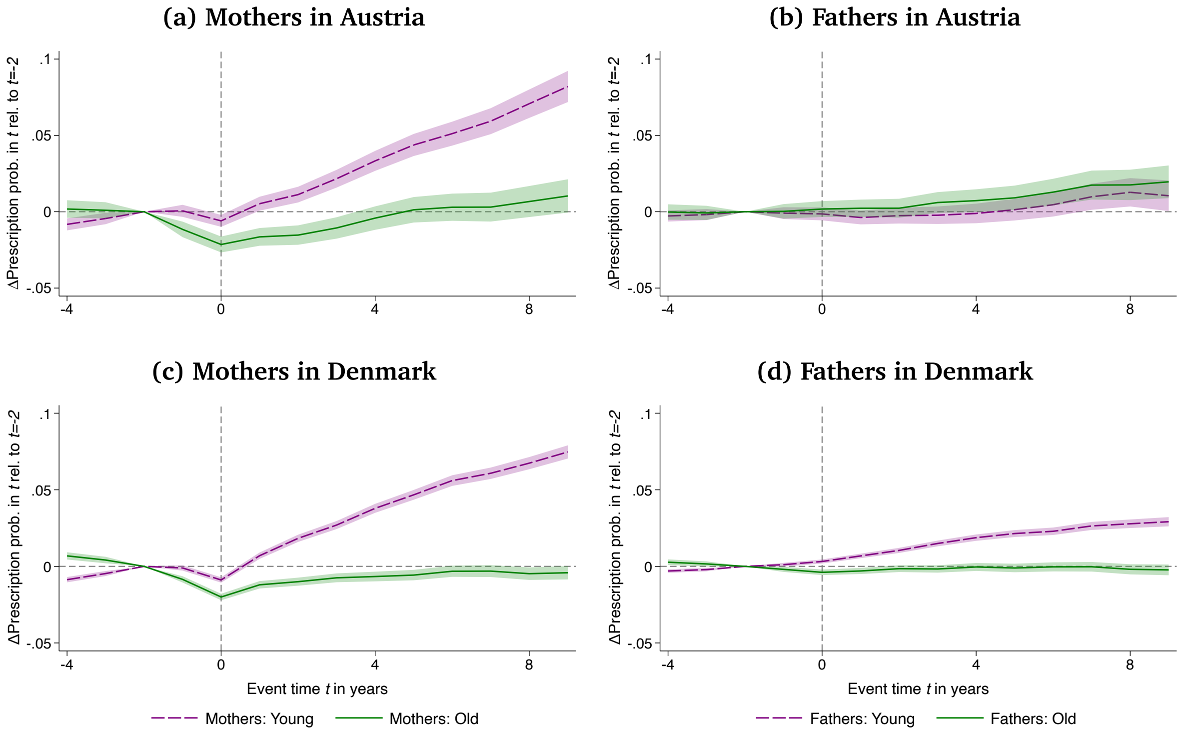
Notes: This figure shows the impacts of parenthood on antidepressant prescriptions (in percentage points) by child's birthweight. It focuses on mothers in Austria (Figure A.12a), fathers in Austria (Figure A.12b), mothers in Denmark (Figure A.12c), and fathers in Denmark (Figure A.12d). We estimate separate regressions for parents with children with a regular 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.

Figure A.13: Impacts of parenthood on prescriptions by educational attainment



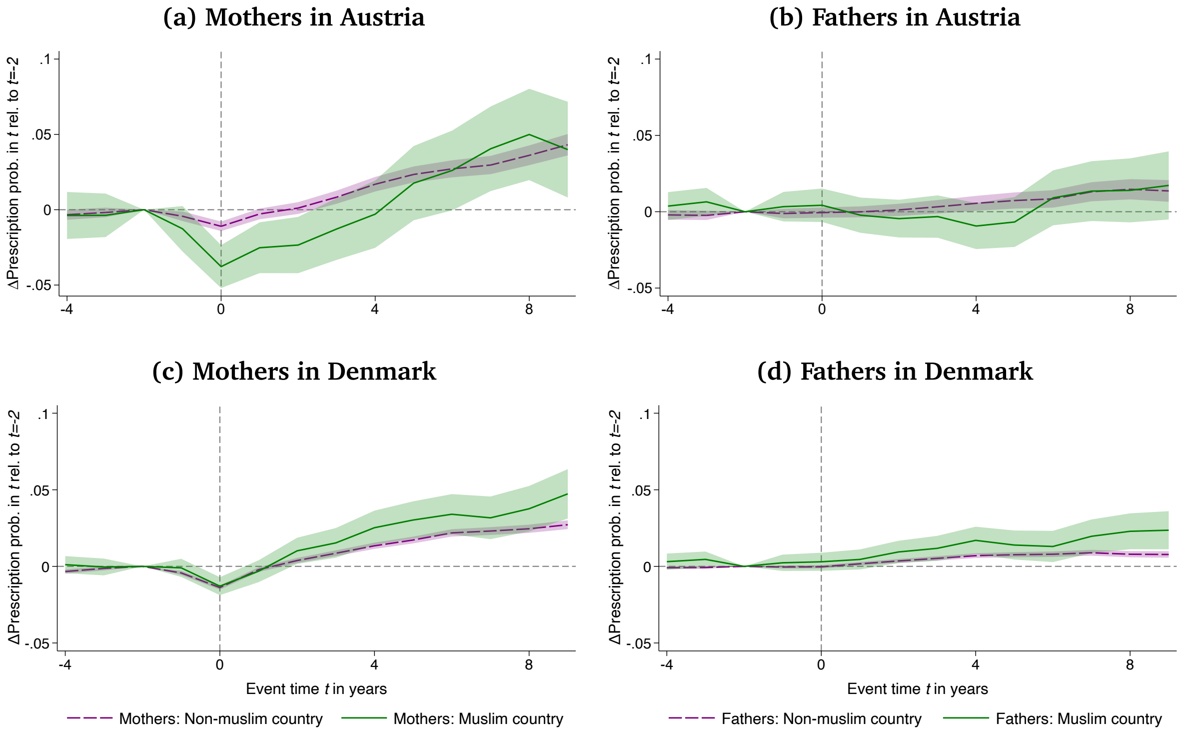
Notes: This figure shows the impacts of parenthood on antidepressant prescriptions (in percentage points) by educational attainment. It focuses on mothers in Austria (Figure A.13a), fathers in Austria (Figure A.13b), mothers in Denmark (Figure A.13c), and fathers in Denmark (Figure A.13d). We estimate 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.

Figure A.14: Impacts of parenthood on prescriptions by parents' age



Notes: This figure shows the impacts of parenthood on antidepressant prescriptions (in percentage points) by parents' age. It focuses on mothers in Austria (Figure A.14a), fathers in Austria (Figure A.14b), mothers in Denmark (Figure A.14c), and fathers in Denmark (Figure A.14d). 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.

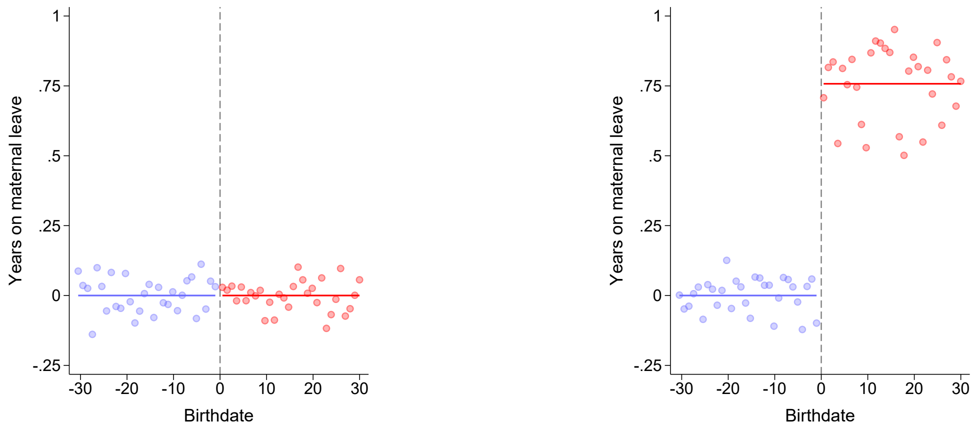
Figure A.15: Impacts of parenthood on prescriptions by parents' migration status



Notes: This figure shows the impacts of parenthood on antidepressant prescriptions (in percentage points) by the cultural background of the parents. 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. We focus on mothers in Austria (Figure A.15a), fathers in Austria (Figure A.15b), mothers in Denmark (Figure A.15c), and fathers in Denmark (Figure A.15d). 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.

Figure A.16: Effects of the 2000 reform on years of maternity leave (first stage)

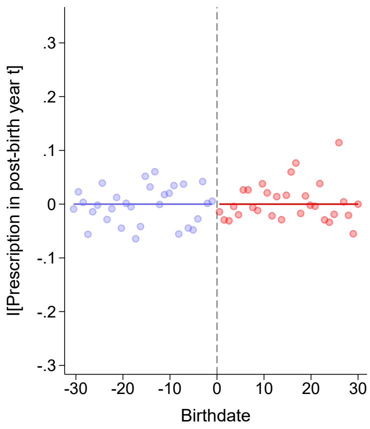
(a) Pre-reform year 1999 **(b) Reform year 2000**



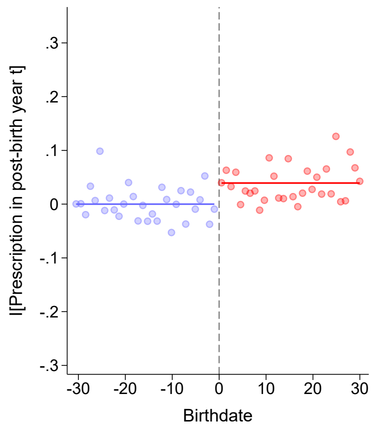
Notes: This figure shows the effects of the Austrian 2000 reform on the number of taken maternity leave years (first stage). The figures are covariate adjusted. For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side).

Figure A.17: Reduced-form effects of the Austrian 2000 reform on yearly AD prescription probability

(a) Pre-reform year 1999

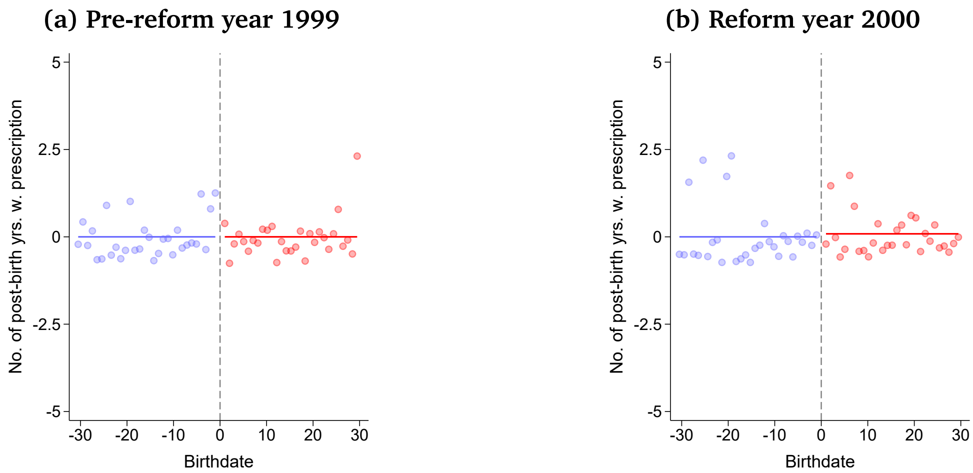


(b) Reform year 2000



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 data set has a panel structure and a binary variable that indicates years with a prescription serves as the outcome.

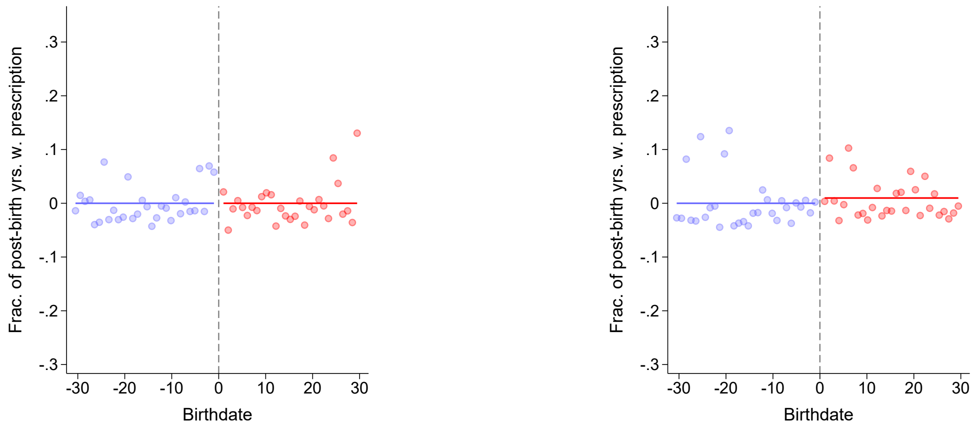
Figure A.18: Reduced-form effects of the Austrian 2000 reform on no. of years with AD prescriptions (fathers)



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 A.19: Reduced-form effects of the Austrian 2000 reform on frac. of years with prescriptions (fathers)

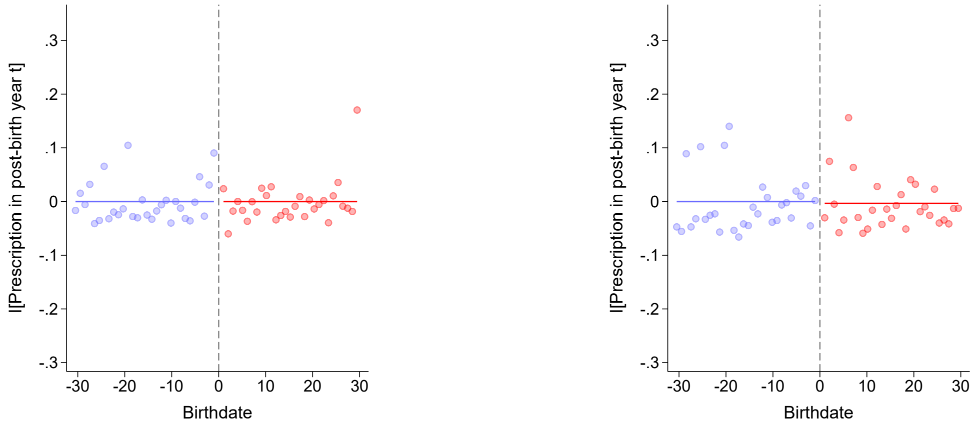
(a) Pre-reform year 1999 **(b) Reform year 2000**



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 A.20: Reduced-form effects of the Austrian 2000 reform on yearly AD prescription probability (fathers)

(a) Pre-reform year 1999 **(b) Reform year 2000**



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 data set has a panel structure and a binary variable that indicates years with a prescription serves as the outcome.

A.2 Additional tables

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 100,000 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 1000 live births)	3.6	3.8
Life expectancy at birth (in years)	80.4	79.2
Suicide rates (cases per 100,000 population)§		
Men	24.09	19.59
Women	7.04	7.55

Notes: † Average over the period from 2000 to 2019/20. Data is retrieved from the Database of the Worldbank. ‡ Average over the period from 2000 to 2019. Data is retrieved from the Database of the OCED. § Average over the period from 2000 to 2017. Rates are age standardized. Data is from Ritchie *et al.* (2022).

Table A.2: 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 A.3: 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 A.4: 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 data set 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 A.5: 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 A.6: 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 A.7: 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 *** $p < 0.01$.

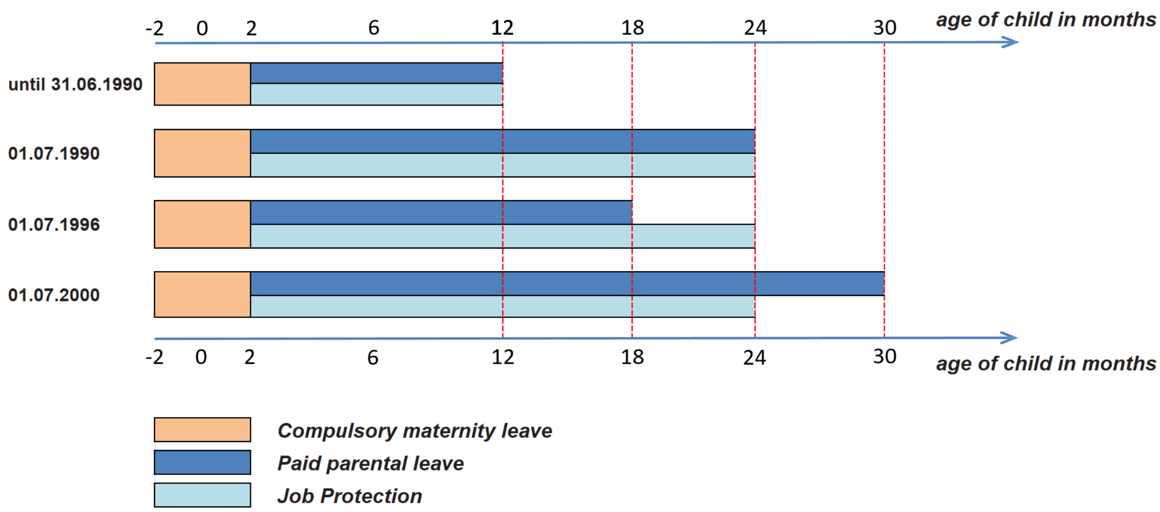
B Effects of other parental leave reforms

B.1 Design of the other parental leave reforms

B.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 (see Section B.2).

Figure B.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 (see Figure B.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

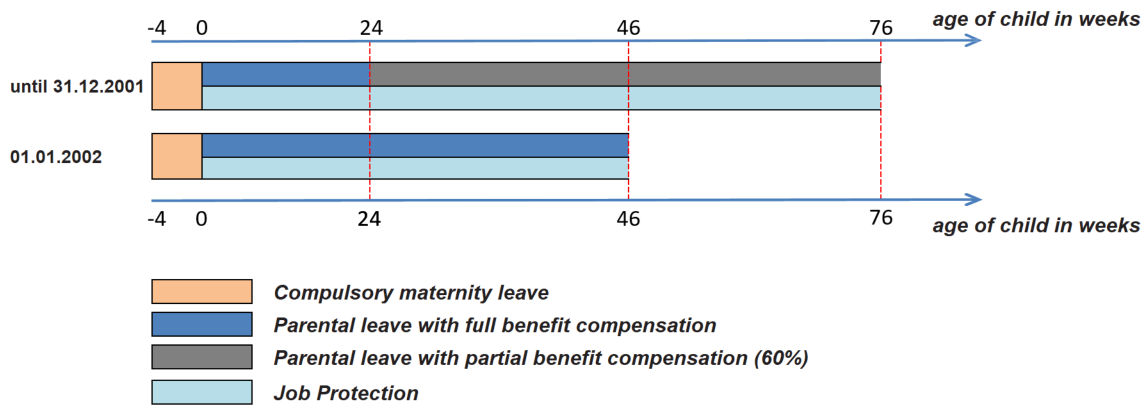
[1] Around the 1990 reform, only 0.5 percent of fathers took any parental leave, around the 1996 and 2000 reforms these shares were 1.7 percent and 3 percent, respectively.

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, 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 (see Figure B.1), permitting us to gauge a combined effect. Third, all the reforms were implemented according to strict birthdate cutoffs (see Figure B.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.

B.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 B.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 B.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

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

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 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.

B.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.

B.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

[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.

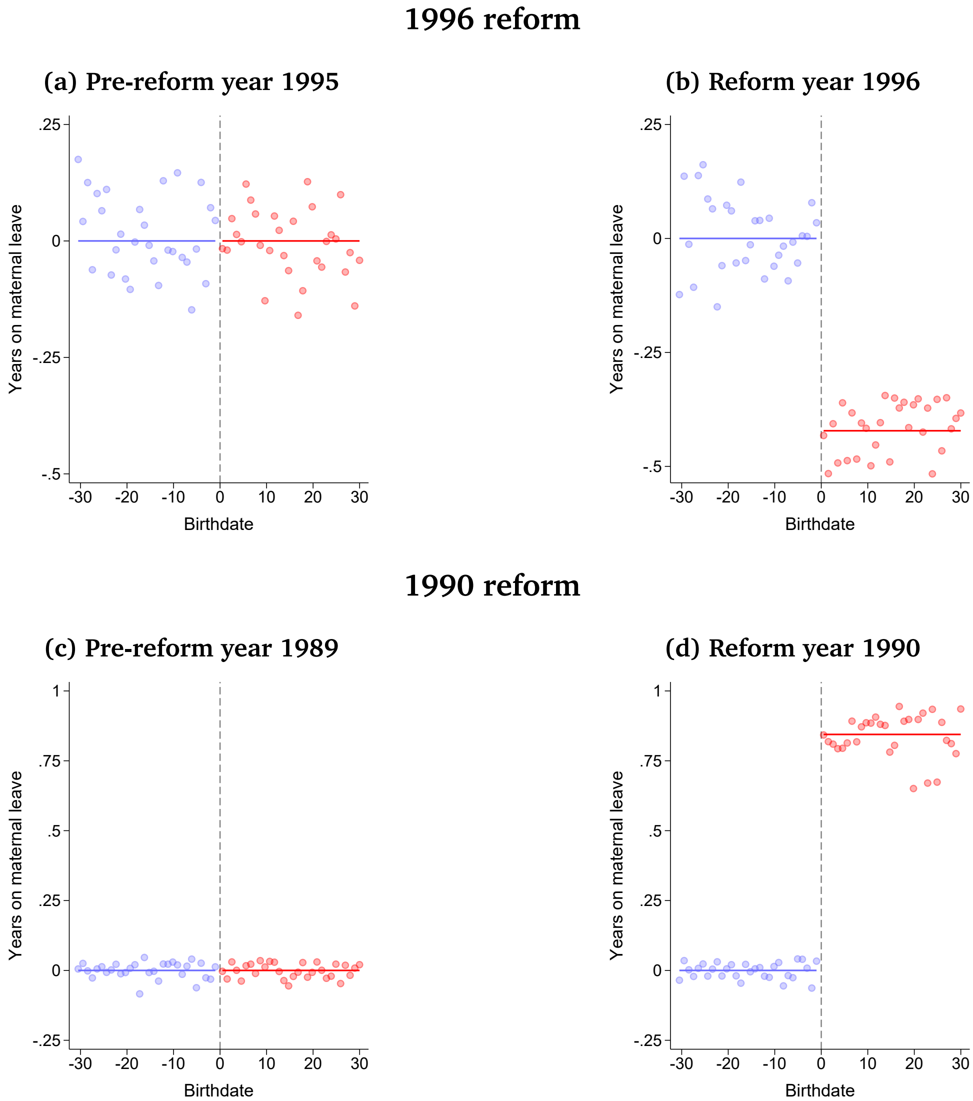
[5]The extended period included 14 weeks of maternity leave and 32 weeks of shared leave.

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 (see Figure B.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% (see Table B.1). The reduced-form estimates (see Figures B.4 to B.6) and the 2SLS estimates for the other two outcomes (see Tables B.2 and B.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 measure the outcome from 1998 to 2016. Moreover, the reform also did not affect fathers' mental health (see Figures B.7 to B.9). In conclusion, the analysis of the Austrian 1990 reform leads to identical conclusions as our analysis of the 2000 reform.

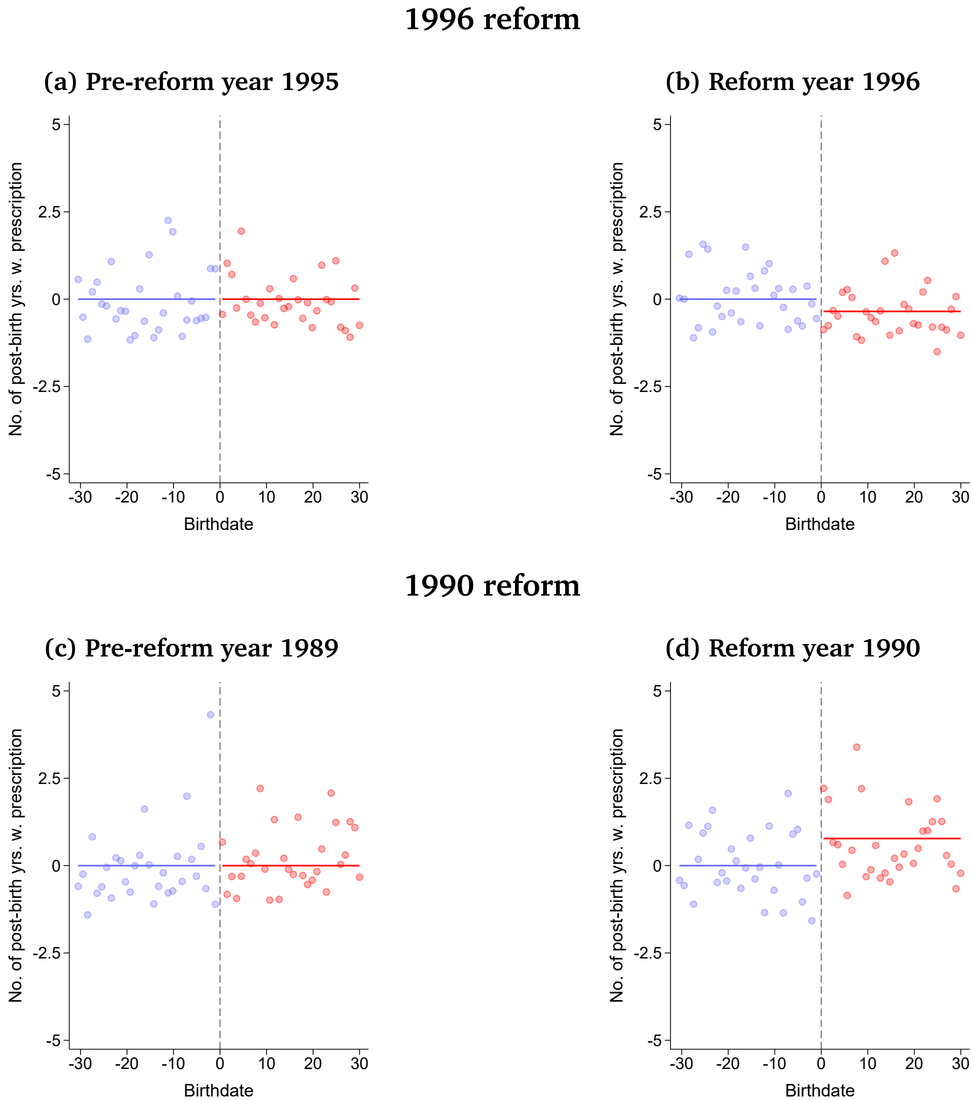
The other two reforms induce smaller changes in mothers' leave-taking behavior and do not significantly affect their mental health. The Danish 2002 reform, for example, increases mothers' actual leave-taking by only one month. On top of that, the baseline leave period was short. Given the small first stage, the absence of significant effects on mothers' (see Figure B.10 and Table B.4) and fathers' (see Figure B.11 and Table B.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 (see Figure B.3), and it does not significantly affect all mental health outcomes (see Figures B.4 to B.6 and Tables B.1 to B.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 (see Figures B.7 to B.9).

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



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

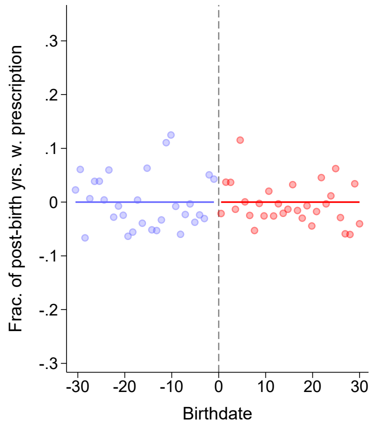


Notes: This figure shows the reduced-form effects of the other Austrian reforms on the number of post-birth years with antidepressant prescriptions for mothers. The figures are covariate adjusted. For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side).

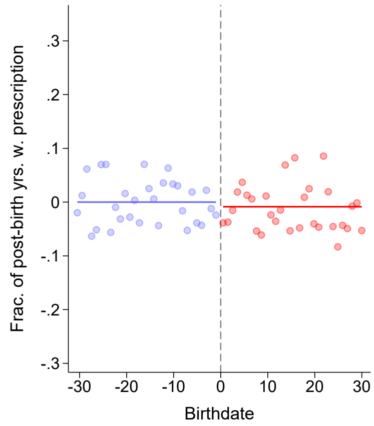
Figure B.5: Reduced-form effects of other Austrian reforms on frac. of years with prescriptions

1996 reform

(a) Pre-reform year 1995

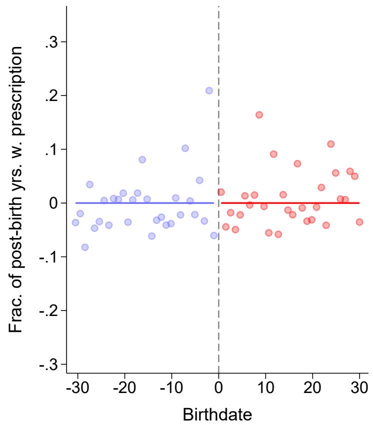


(b) Reform year 1996

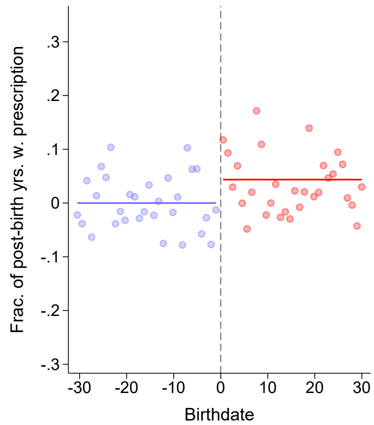


1990 reform

(c) Pre-reform year 1989

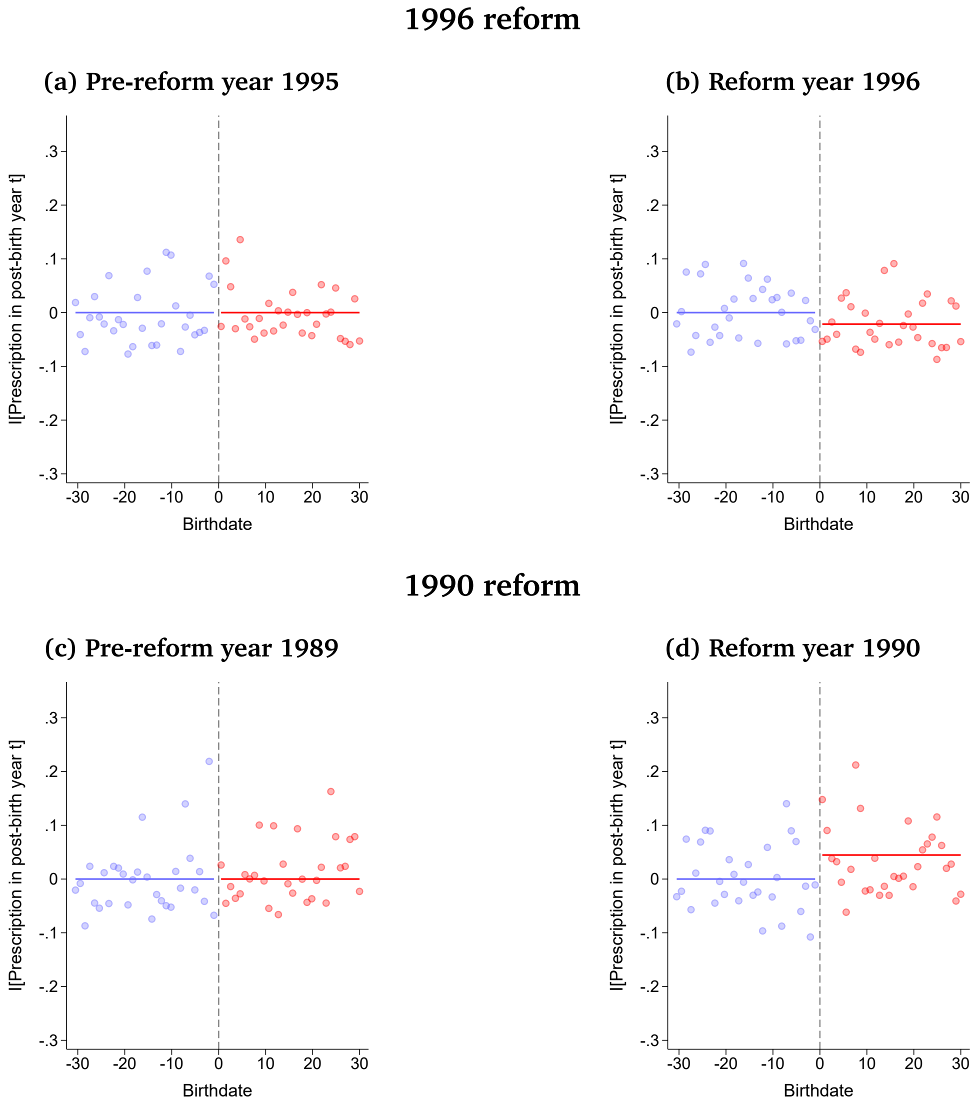


(d) Reform year 1990



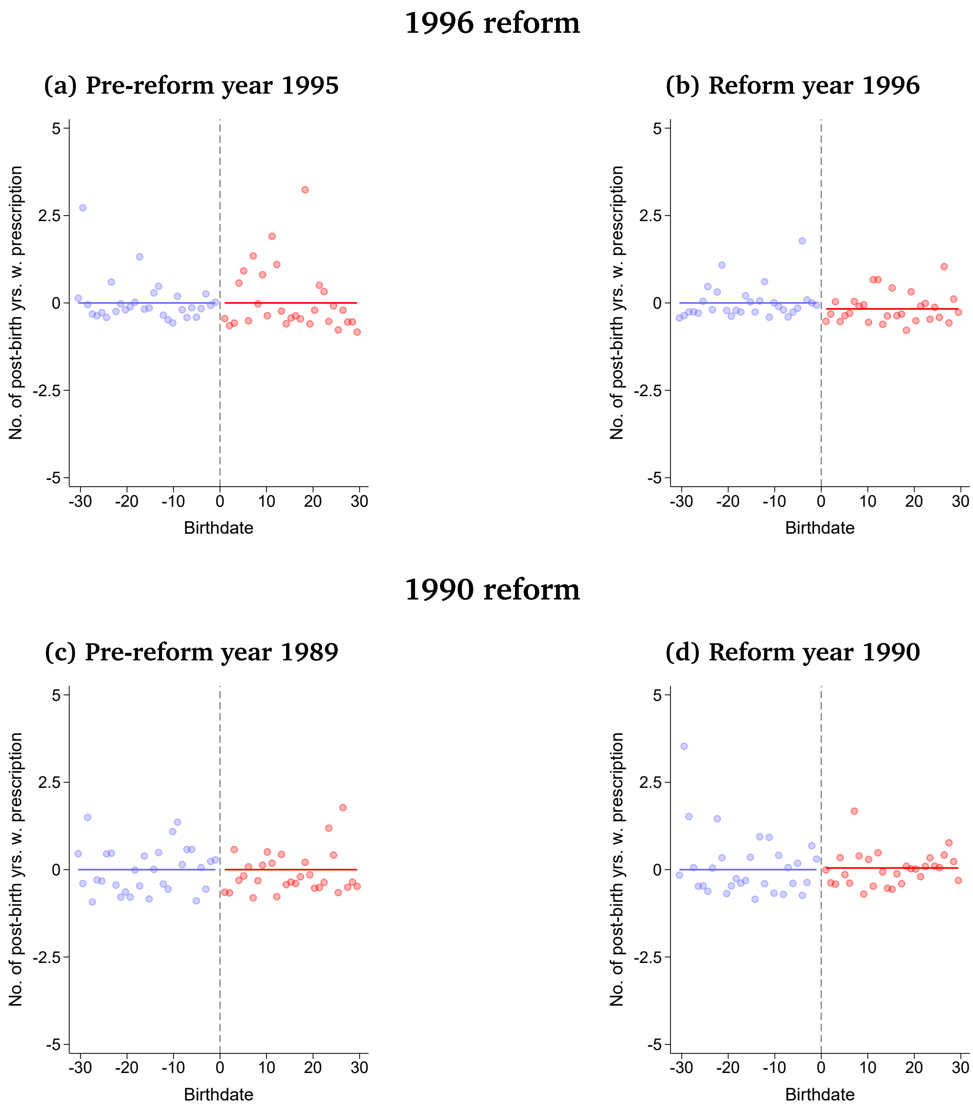
Notes: This figure shows the reduced-form effects of the other Austrian reforms on the fraction of post-birth years with antidepressant prescriptions for mothers. The figures are covariate adjusted. For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side).

Figure B.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 data set has a panel structure and a binary variable that indicates years with a prescription serves as the outcome.

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

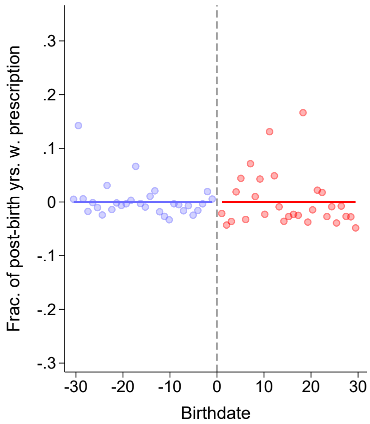


Notes: This figure shows the reduced-form effects of the other Austrian reforms on the number of post-birth years with antidepressant prescriptions for fathers. The figures are covariate adjusted. For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand side).

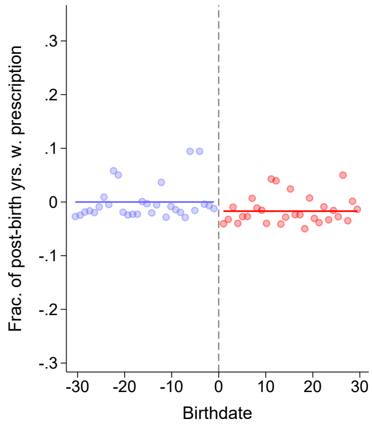
Figure B.8: Reduced-form effects of other Austrian reforms on frac. of years with prescriptions (fathers)

1996 reform

(a) Pre-reform year 1995

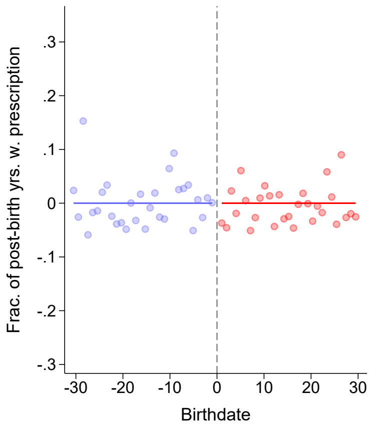


(b) Reform year 1996

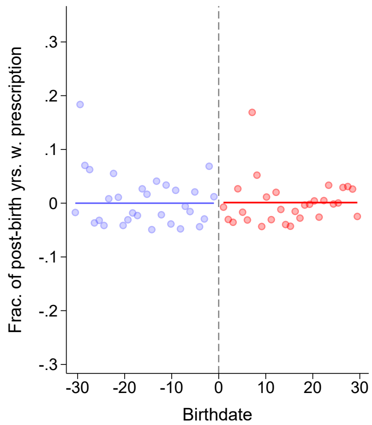


1990 reform

(c) Pre-reform year 1989

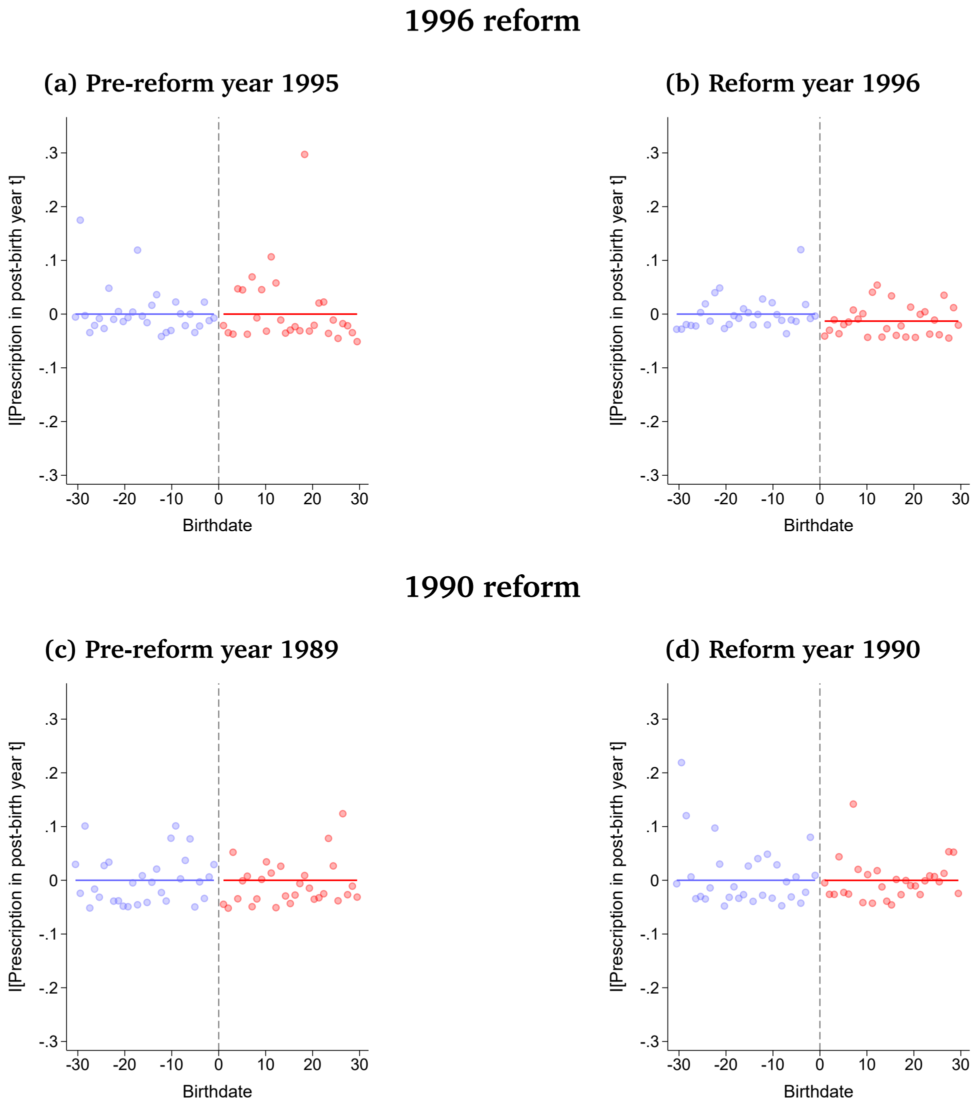


(d) Reform year 1990



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

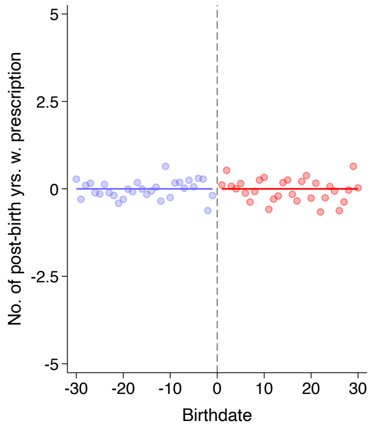


Notes: This figure shows the reduced-form effects of the other Austrian reforms on the yearly probability of receiving an antidepressant prescription in a given year for fathers. The figures are covariate adjusted. For comparison, we plot the pre-reform year (left-hand side) and the reform year (right-hand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

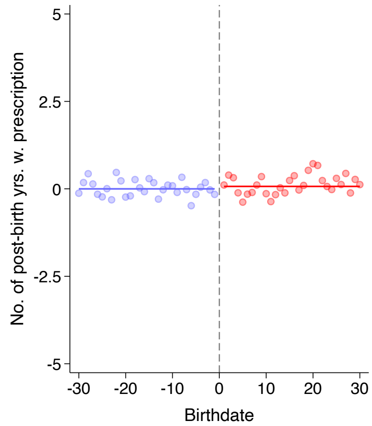
Figure B.10: Reduced-form impacts of the Danish 2002 reform on mother's mental health

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

(a) Pre-reform year 2000/01

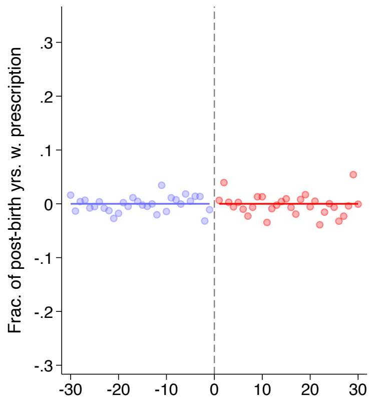


(b) Reform year 2001/02

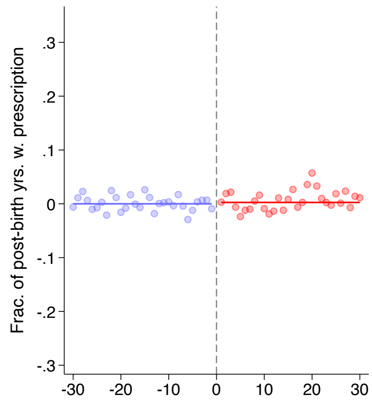


Impact on the fraction of post-birth years with a prescription

(c) Pre-reform year 2000/01



(d) Reform year 2001/02

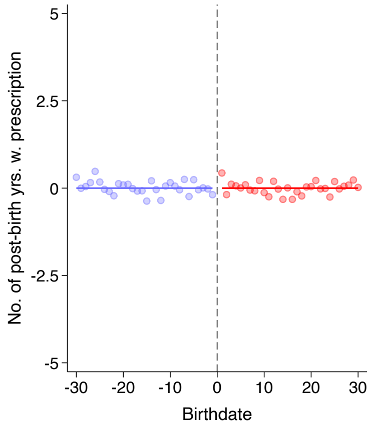


Notes: This figure focuses on mothers and shows the reduced-form impacts of the Danish 2002 reform on the number of post-birth years with antidepressant prescriptions (Figures B.10a and B.10b) and the fraction of post-birth years with antidepressant prescriptions (Figures B.10c and B.10d). For comparison, 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 (6), (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.

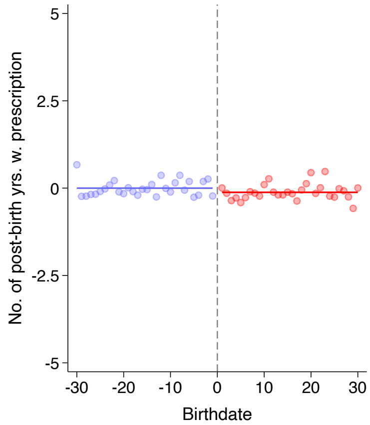
Figure B.11: Reduced-form impacts of the Danish 2002 reform on fathers' mental health

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

(a) Pre-reform year 2000/01

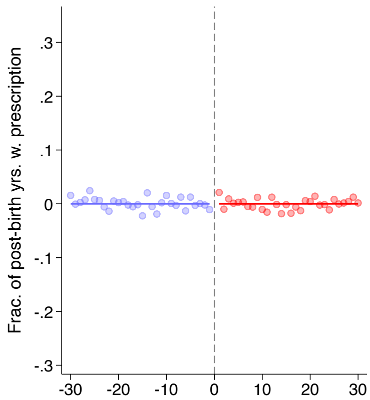


(b) Reform year 2001/02

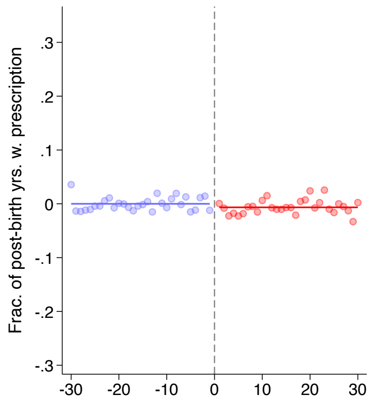


Impact on the fraction of post-birth years with a prescription

(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 B.11a and B.11b) and the fraction of post-birth years with antidepressant prescriptions (Figures B.11c and B.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 (6), (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 B.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 B.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 B.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 data set 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 B.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 B.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 *** $p < 0.01$.

C Effects for adoptive families

C.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.

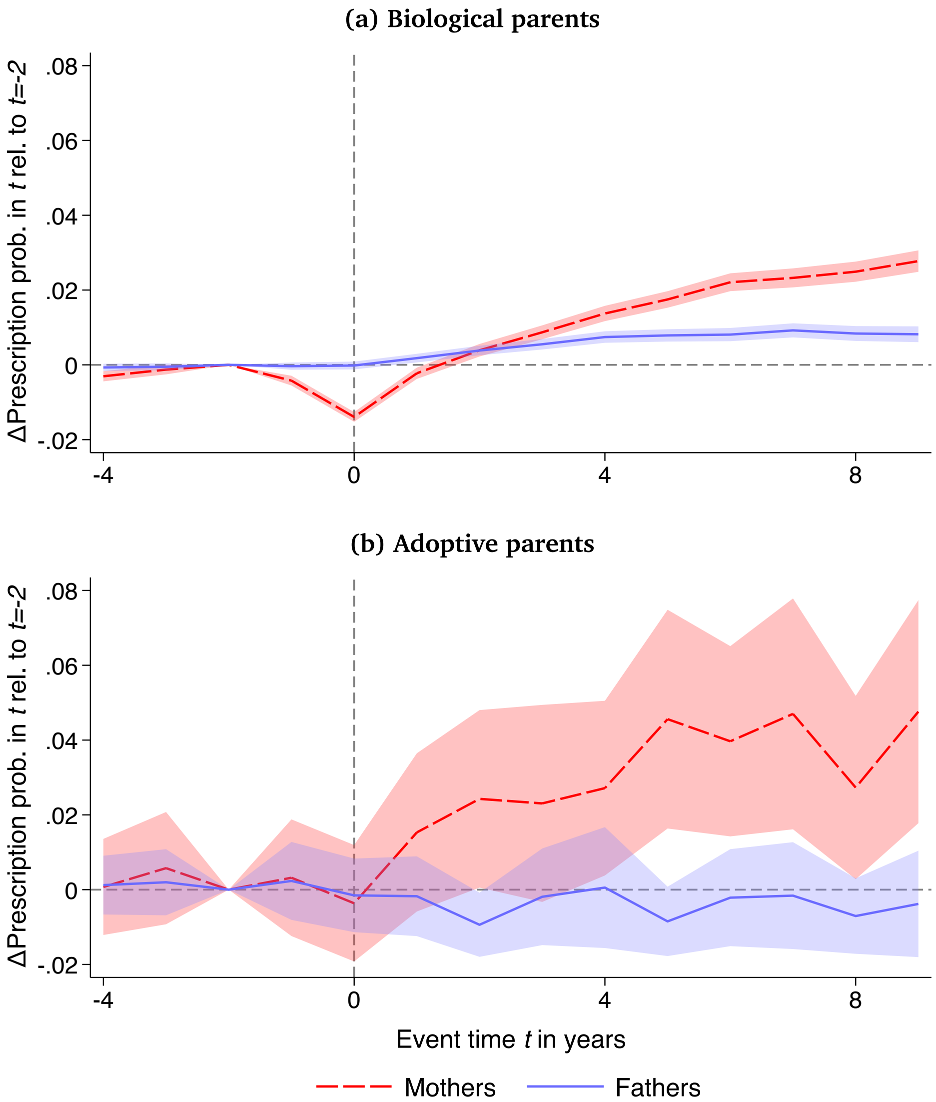
We then estimate the following weighted models. First, comparably to our main analysis, we estimate the impact of adopted children on mothers' prescription probability. Besides the weights, a second difference to model (1) is that we now estimate an interacted model:

$$\begin{aligned} Y^m_{ist} =& \phi^{m,adop}_{ist} + \sum_{event \neq -2} \alpha^{m,adop}_{event} \cdot \mathbb{1}[event = t] \\ &+ \sum_{event \neq -2} \alpha^{m,bio}_{event} \cdot \mathbb{1}[event = t] + \sum_{year} \beta^m_{year} \cdot \mathbb{1}[year = s] \\ &+ \sum_{age} \gamma^m_{age} \cdot \mathbb{1}[age = a_{is}] + u^m_{ist}, \end{aligned} \tag{9}$$

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.

C.2 Results for adoptive parents

Figure C.1: Impacts of parenthood on antidepressant prescriptions for adoptive parents



Notes: This figure shows the estimated impacts of parenthood on antidepressant prescriptions before and after having the first child for biological parents ($\hat{\alpha}_t^{j,bio}$) in Figure C.1a and for adoptive parents ($\hat{\alpha}_t^{j,adop}$) in Figure C.1b. It focuses on mothers ($j = m$, dashed lines) and fathers ($j = f$, solid lines). We obtain the event time coefficients from estimating regression (9) 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 (see Section C.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.