Traditional Views, Egalitarian Views, and the Child Penalty: Insights from Immigrant Populations in France

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Traditional Views, Egalitarian Views, and the Child Penalty: Insights from Immigrant Populations in France*

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Abstract

This study uses French survey data on immigrants to explore whether the child penalty is driven by traditional gender attitudes. The dataset includes individual perceptions of gender inequality and women's bodily autonomy, alongside fertility histories and labor market trajectories for immigrants living in France during 2019–2020. While women holding more traditional views are less likely to participate in the labor force overall, the child penalty does not appear to be larger for this group. Interestingly, the child penalty accounts for a significantly greater share of the gender gap in labor force participation among those with more egalitarian views. Comparative analyses across immigrants' upbringing environments and countries of origin further support a causal interpretation of the absence of a relationship between traditional gender attitudes and the child penalty.

Keywords: Gender, Child penalty, Immigrants, Attitudes, Values

JEL Classification: J15, J16, J24, D63

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1 Introduction

In developed countries, the child penalty – the significant decline in women's labor outcomes following the birth of children – remains the leading driver of gender inequality in the labor market (Angelov, Johansson, and Lindahl, 2016; Kleven, Landais, and Søgaard, 2019; Cortés and Pan, 2023). Numerous studies have examined potential explanations for this phenomenon. These investigations have shown that the child penalty is not linked to biological differences in reproductive contributions or productivity disparities in the labor market (Kleven, Landais, and Søgaard, 2021). Evidence from policy reforms suggests that family policies, such as affordable childcare or parental leave, have limited long-term effects on the child penalty (Rabaté and Rellstab, 2022; Kleven et al., 2024a). As a result, the role of institutions is largely ruled out, leaving norms and preferences as the most plausible explanation. This hypothesis is further supported by various spatial comparisons, both across countries and regions (Kleven et al., 2019; Rabaté and Rellstab, 2022; Casarico and Lattanzio, 2023; Kleven, Landais, and Leite-Mariante, 2024; Kleven, 2022), as well as more recent studies on migration, encompassing both internal and international migrants (Boelmann, Raute, and Schönberg, 2020; Rabaté and Rellstab, 2022; Kleven, 2022).

While there is now substantial and growing evidence that the child penalty is shaped by norms and preferences, the literature largely remains silent on the exact nature of the underlying beliefs and preferences. This question is significant both theoretically and practically. From a theoretical point of view, it can help explain why the child penalty persists, despite significant convergence in men's and women's outcomes, and the decreasing prevalence of negative attitudes towards working women. Practically, this issue has important policy implications, as the design of effective interventions is likely to require a deep understanding of the beliefs that such policies are intended to address.

This paper explores the hypothesis that the child penalty is influenced by traditional gender-related attitudes. We define these attitudes as negative views on female employment and women's autonomy. Such attitudes are known to be strongly correlated with gender gaps in labor force participation, both across countries (Fortin, 2005) and over time (Fortin, 2015), and to decrease maternal labor supply both at the individual level and through peer effects (Cavapozzi, Francesconi, and Nicoletti, 2021). Given the prominence of the child penalty as a key driver of gender gaps, this investigation is therefore highly relevant.

To investigate this, we draw on individual-level survey data about opinions and attitudes, which have rarely been utilized in the child penalty literature. Most studies in this field rely on high-quality administrative data, which allows a detailed analysis of labor market histories. However, administrative data are typically not well suited for exploring beliefs and opinions, as they are not collected for this purpose. In contrast, the survey data we use enable us to link individual beliefs and attitudes about female employment and women's autonomy to labor supply decisions that contribute to the child penalty. This allows us to quantify the role of traditional gender-related beliefs and attitudes in the child penalty.

Specifically, we rely on recent French survey data on immigrants (both first- and second-generation) living in France between 2019 and 2020. The key advantage of these data is that they provide individual-level information on gender-related beliefs collected at the time of the survey, alongside retrospective labor market histories and comprehensive fertility decisions, both of which are crucial for estimating the child penalty.

This allows us to make three distinct comparisons that form the core of the paper. First, at the individual level, we compare immigrants with more traditional gender views to those with more egalitarian perspectives. Second, building on evidence that immigrants raised in more traditional family environments tend to hold more traditional gender views as adults, we compare immigrants based on their family environment before the age of 18. Lastly, drawing on the correlation between countries of origin and beliefs about traditional gender roles, we compare second-generation immigrants – who all

grew up in France – based on whether their parents were born in countries from which first-generation immigrants are more likely to hold traditional gender views.

Empirically, we find no variation in the child penalty along these dimensions, which appears to contradict the idea that traditional gender beliefs are a major driver of the child penalty. We conclude that this hypothesis is not supported by the data. However, this finding does not imply that norms and preferences are not factors contributing to the child penalty, as norms and preferences have many dimensions beyond the one we focus on. However, it does suggest that the child penalty is largely unrelated to negative views regarding female employment or women's autonomy.

We provide further evidence that this finding does not undermine the notion that such beliefs do influence gender differences in labor supply. Ultimately, it remains true that (i) traditional gender-related attitudes contribute to gender gaps in the labor market, and (ii) the larger share of gender differences in labor outcomes arises from the child penalty, but these two facts appear to be independent of one another. The validity of both statements suggests, in particular, that the significance of the child penalty as a key driver of gender gaps in labor outcomes is greater when attitudes towards female employment and women's autonomy are more positive.

This investigation faces three main empirical challenges. The first concerns the measurement of beliefs and attitudes. We rely on several opinion items from the TeO2 survey, which specifically address gender inequality and women's bodily autonomy, as well as religion and political orientation, to construct a one-dimensional index of gender-related beliefs and attitudes. The second challenge involves the measurement of the child penalty. We build upon the now-standard event-study approach developed by Angelov, Johansson, and Lindahl (2016) and Kleven, Landais, and Søgaard (2019). The third challenge pertains to identifying the effect of beliefs and attitudes, given that we use opinions reported at the time of the survey to explain labor supply decisions made several years prior. To address this issue, we exploit variation that predates these decisions, both in terms of timing and causality, such as differences in immigrants' family environments during childhood and comparisons of second-generation immigrants based on their parents' countries of origin.

This paper contributes to the extensive literature on gender inequality, which has been notably highlighted by the recent Nobel Prize awarded to Claudia Goldin. Empirical research on this topic has grown significantly in recent years, with two main trends. The first is a shift away from analyses that focus on labor demand towards those that emphasize the labor supply side. Empirically, this shift corresponds to studies that quantify gender inequalities in terms of annual labor earnings — that is, the total salaried earnings over a year, without conditioning on salaried employment — rather than hourly wages, allowing for the inclusion of interruptions in working life.

This shift has been partly driven by a second trend, which emphasizes the role of family life, and particularly parenthood, in generating gender gaps in the labor market (Juhn and McCue, 2017). The spread of contraceptives, by allowing young women to control their childbearing schedules, has led to a change in women's attitudes toward the labor market (Goldin and Katz, 2002; Goldin, 2006), contributing to the narrowing of gender gaps in OECD countries since the 1970s. However, family responsibilities continue to fall disproportionately on women. As organizing daily life often requires balancing family and work, the two spheres compete (Goldin, 2021), generating not only mechanical gender differences in labor market participation and working hours, but also gender pay gaps — either due to higher anticipated separation rates for women (Lazear and Rosen, 1990) or because highly-skilled occupations disproportionately reward time availability and long hours (Goldin, 2014).

A particularly salient strand of the growing literature focuses on the child penalty, which refers to the significant decline in mothers' labor outcomes following the arrival of children (Angelov, Johansson, and Lindahl, 2016; Kleven, Landais, and Søgaard, 2019). This effect has now been consistently quantified across a variety of countries (see e.g. Kleven et al., 2019; Meurs and Pora, 2019; Sieppi and Pehkonen, 2019;

Quinto, Hospido, and Sanz, 2021; Rabaté and Rellstab, 2022; Casarico and Lattanzio, 2023; Lebedinski, Perugini, and Vladisavljević, 2023), and more recently, globally (Kleven, Landais, and Leite-Mariante, 2024). The evidence confirms that, in highly developed countries, gender inequality in the labor market is primarily driven by the child penalty.

Several potential causes of the child penalty have been explored in the literature, though the debate is far from settled. Overall, the child penalty appears to be unrelated to biological reproduction-related factors or within-couple comparative advantage (Kleven, Landais, and Søgaard, 2021; Cortés and Pan, 2023). Evidence from policy reforms suggests that financial incentives, such as those provided by family policies, do not play a major role in the child penalty (Rabaté and Rellstab, 2022; Kleven et al., 2024b). While specific evidence on the contribution of family policies to the child penalty is lacking in the French case studied in this paper, evidence showing that increasing the availability of affordable daycare slots does not improve maternal labor outcomes (Pora, 2020), or that earmarking parental leave does not shift the burden to fathers (Périvier and Verdugo, 2024), is consistent with this notion.

These negative findings have prompted interest in explanations linked to beliefs and identity, in the spirit of Akerlof and Kranton (2000), thereby connecting the child penalty literature to a body of work that has examined the role of beliefs and attitudes regarding gender in shaping labor supply decisions. While some studies have identified gender norms through anomalies in the data (see e.g. Bertrand, Kamenica, and Pan, 2015), a significant portion of this literature has drawn on self-reported survey data to elicit beliefs and attitudes regarding gender. This approach has enabled researchers to demonstrate that beliefs prioritizing traditional gender roles are associated with lower female labor force participation, both across countries (Fortin, 2005) and over time (Fortin, 2015).

In the specific context of the child penalty, several authors have attempted to estimate the contribution of beliefs and attitudes by using a variety of spatial comparisons: across countries, based on beliefs measured in the ISSP data (Kleven et al., 2019); across US states, using State-level average beliefs and attitudes regarding gender derived from the GSS data (Kleven, 2022); across Italian regions, based on sensitivity to traditional gender norms captured in the European Value Study (Casarico and Lattanzio, 2023); and across Dutch municipalities, relying on average religious attendance (Rabaté and Rellstab, 2022). Other studies have focused on same-sex couples (Andresen and Nix, 2022). The findings osf these studies can vary, with some reporting strong correlations, while others observe limited differences. Additionally, in the former case, even in the most egalitarian areas in terms of gender-related attitudes, the child penalty remains substantial. Individual-level evidence based on elicited beliefs and attitudes is still rare, with the notable exception of Lebedinski, Perugini, and Vladisavljević (2023), who use comparisons based on levels of self-reported religiosity in Russia. In contrast, our paper primarily draws insights from individual-level comparisons enabled by the rich survey data we use, which include directly elicited beliefs about gender, without relying on proxies like religion.

However, even properly measured individual-level beliefs may not provide convincing evidence, as these beliefs may be endogenous outcomes of labor supply decisions. This is why a significant portion of the literature on the impact of culture on labor supply has focused on immigrants, especially second-generation immigrants. This approach allows researchers to apply what is known as the "epidemiological approach" to culture (Fernández, 2007; Fernández and Fogli, 2009; Fernández, 2011). The intuition behind this approach is that second-generation immigrants offer a valuable case for distinguishing between culture, on the one hand, and institutions and policies, on the other, since they were born and raised within similar institutions and markets, while their beliefs and attitudes are often strongly correlated with those of their parents' countries of origin (Alesina, Giuliano, and Nunn, 2013). Thus, if second-generation immigrants' labor supply decisions are uncorrelated with their parents' origins, it would provide a strong argument against the role of culture and beliefs.

The epidemiological approach has recently gained traction in the child penalty literature. Kleven

(2022) finds no correlation between child penalties for second-generation immigrants in the US and those in their parents' countries of birth. Similarly, Rabaté and Rellstab (2022) examines immigrants in the Netherlands, though without distinguishing generations, complicating interpretation. Other studies complement this perspective: Ichino et al. (2024) combine the epidemiological approach with exogenous tax reforms in Sweden, showing that couples from conservative cultures reinforce traditional childcare roles when taxes are adjusted. Meanwhile, Boelmann, Raute, and Schönberg (2020) reveal that East German mothers retain egalitarian employment attitudes despite migrating to West Germany, while West German mothers partially align with East German norms, suggesting a greater persistence of egalitarian attitudes.

In line with this trend, our paper focuses on immigrants (both first- and second-generation), primarily due to data limitations. We also conduct an empirical investigation similar to the epidemiological approach, though it constitutes only one piece of the evidence we present. Consistent with the other comparisons we examine, we find no correlation between the child penalty and gender-related attitudes inferred from the countries of birth of second-generation immigrants' parents.

The paper proceeds as follows. Section 2 introduces the database used for our first two comparisons and outlines the construction of the variables of interest, including maternity and paternity dates, labor market histories, and the opinion and attitude variable derived from questions in various TeO2 modules. Section 3 describes our empirical framework. The results from our first two comparisons, namely those based on current attitudes and past family environments, are presented in Section 4. Section 5 details our third empirical test, an adaptation of the epidemiological approach focusing on the countries of birth of second-generation immigrants' parents. The paper concludes with a summary of the findings. Appendices include robustness checks and alternative specifications.

2 Data

This paper relies on the *Trajectoires et Origines 2* (TeO2) survey, conducted in France between 2019 and 2020 by Insee (the French statistical office) and Ined (the French institute for demographic studies). This survey focuses on the lived experiences of immigrants and provides rich data on values and attitudes, including opinions on gender inequality and women's bodily autonomy. It also includes a detailed retrospective labor market calendar and comprehensive information about respondents' children.

This section explains how we combine these data elements and presents summary statistics for our final sample. Additionally, we validate the survey data and our methodological choices by comparing them with administrative records previously used to estimate the child penalty. A detailed discussion is available in Appendix C.

2.1 *Trajectoires et Origines 2* survey : general overview

The *Trajectoires et Origines 2* (TeO2) survey is a large-scale statistical study conducted by Insee and Ined between July 2019 and November 2020. It includes responses from over 27,000 individuals aged 18 to 59 living in ordinary dwellings in mainland France. This second edition of the TeO survey, initially conducted in 2008–2009, aims to provide national statistics on population diversity and to explore the influence of migratory origins on individual trajectories. Given its focus, the sample intentionally over-represents first- and second-generation immigrants. Further methodological details are provided in Thao Khamsing et al. (2022).

The survey covers a wide array of topics, from migration histories to housing, experiences of discrimination, and cultural practices. For the purposes of this paper, three specific features of the survey are particularly relevant:

- It includes questions on attitudes and opinions, which we use to infer gender-related attitudes;

- It collects detailed information on all children born to immigrant parents, regardless of their current living arrangements;
- It provides a retrospective calendar of labor market outcomes, enabling the reconstruction of entire career trajectories rather than snapshots of current employment.

These three features form the foundation of our analysis, allowing us to explore the connection between child penalties and gender-related attitudes.

2.2 Eliciting gender-related attitudes

A central aspect of this study is the identification of gender-related attitudes from the responses provided in the TeO2 survey. The following sections outline the methodology used to extract and interpret these attitudes.

2.2.1 Relevant survey items

The TeO2 survey provides a comprehensive perspective on the lived experiences of immigrants in France. In addition to standard demographic and occupational data, it includes questions on values, attitudes, religion, and social life. This study relies on six specific survey items to assess the degree of traditional gender views among respondents.

The first three items, extracted from the "Attitudes and Opinions" module, address key aspects of gender-related beliefs:

I_GENRE When there are not many jobs, men are more entitled to work than women. 1. Totally agree; 2. Agree; 3. Disagree; 4. Totally disagree.

I_AVORT A woman can have an abortion for non-medical reasons. 1. Totally agree; 2. Agree; 3. Disagree; 4. Totally disagree.

I_HOMO Same-sex couples should have the same rights as different-sex couples. 1. Totally agree; 2. Agree; 3. Disagree; 4. Totally disagree.

The first item, highlighted by Fortin (2005), is strongly correlated with international gender disparities in labor force participation. The second item captures attitudes toward women's autonomy and their societal role as mothers. The third item is relevant because it touches on adoption and parenthood rights for same-sex couples, linking gender and family structures. Additionally, lower child penalties observed among same-sex couples suggest the influence of gender identity on mothers' labor supply (Andresen and Nix, 2022).

We also include three additional items from other modules of the survey. These items capture complementary dimensions of gender-related attitudes:

A_RHOM and A_RFEM [For respondents who met friends within the two weeks prior to the survey] Among these friends, how many are of the same gender as the respondent? 1. Almost all; 2. More than half; 3. Half; 4. Less than half; 5. Almost none or none.

R_IMPVIE [For respondents who currently practice a religion] How important is religion in your life today? 1. Not at all important; 2. Moderately important; 3. Important; 4. Very important.

I_OPIPOL Would you say that you are...? 1. On the far left; 2. On the left; 3. Centrist; 4. On the right; 5. On the far right; 6. Neither right nor left.

The first item reflects gender segregation within friendship networks, providing a behavioral perspective on gender identity. The second captures religiosity, which has been linked to more traditional gender attitudes (Seguino, 2011). The third highlights political orientation, a strong predictor of gender equality views (Sevincer et al., 2023).

Both the first and second items apply only to respondents who met specific conditions—having interacted with friends recently or practicing a religion. We account for this by including additional levels in the categorical variables: "no recent social interactions" for the first item and "no religion" for the second.

We acknowledge that combining these diverse dimensions into a single measure is inherently complex. Each item captures a different aspect of gender-related attitudes, and integrating them requires careful consideration. To address potential concerns, we perform a robustness check by focusing exclusively on the **I_GENRE** item, an approach similar to Fortin (2005). Specifically, we divide the sample based on whether respondents totally disagreed with the statement or not. This simplified approach produces results that are largely consistent with our main findings (see Appendix H.1).

2.2.2 Using PCA to infer gender-related attitudes

To explore the relationship between child penalties and gender-related attitudes, this paper requires a low-dimensional—preferably one-dimensional—measure of such attitudes. However, deriving this measure from six multinomial variables is not straightforward. We address this challenge by adopting a Principal Component Analysis (PCA) approach. Specifically, we focus on a restricted sample comprising immigrant parents who responded to all relevant survey items,[1] and estimate the first principal component of the dataset. This synthetic continuous variable is then interpreted as capturing the degree of traditionality in gender-related attitudes.

For ease of comparison, we discretize the first principal component by categorizing individuals as either above or below the estimated population median. Respondents above the median are interpreted as holding more traditional views regarding gender inequality and women's bodily autonomy compared to those below the median.[2]

Appendix A.1 further supports this interpretation by showing how these two groups differ across survey items. Immigrants above the median are more likely to favor male employment over female employment, oppose non-medical abortion and equal rights for same-sex couples, maintain more gender-segregated friendships, display higher levels of religiosity, and identify more frequently as politically right-leaning or apolitical.

2.2.3 Interpretation of the PCA analysis

Because the TeO2 survey is not specifically focused on gender and family dynamics, one might question the inclusion of the specific items on values and attitudes used in this analysis. However, we demonstrate that such concerns do not undermine our results. Firstly, as shown in Appendix A.1, immigrants' responses to these items are strongly correlated, indicating that they capture a low-dimensional aspect of values and attitudes. Secondly, summary statistics reveal that these inferred gender-related attitudes are significantly associated with gender gaps in labor market outcomes (see 2.5) and unequal distributions of household chores (see Appendix A.2).

Finally, our results remain robust even when excluding certain survey items. Dropping any particular item does not notably change how respondents are categorized or the overall conclusions regarding child

[1] This includes individuals who reported having no friends of the same gender or no religious affiliation, as these categories are accounted for in the categorical variables.

[2] Baseline estimates compare immigrant parents above and below the median. Appendix H.3 demonstrates that similar conclusions are drawn when comparing respondents in the top tertile with those in the bottom tertile.

penalties (see Appendix H.2.1 and H.2.2).

2.3 Fertility measurement

The TeO2 survey includes questions about respondents' children, specifically gathering the total number of children they have had over their lifetime, regardless of whether: (i) these children still live with them; (ii) these children were born in France; or (iii) these children currently reside in France. Additional details about each child are also available in the survey data. Of particular importance to this paper is the inclusion of the children's birth dates. Our identification of the child penalty is based on the timing of the birth of the immigrant's first child. In this context, the first child is defined broadly, irrespective of whether: (i) the child was born in France; (ii) the child is still alive; or (iii) the child currently lives in France.

2.4 Labor market outcomes

The TeO2 survey gathered data on immigrants' professional careers. This feature is essential to our analysis as we aim to examine the child penalty over the entire career lifecycle, rather than focusing solely on cross-sectional labor market disparities. Specifically, the survey recorded up to 15 periods of career-related data, beginning either after formal education or at age 14. Each period was required to last at least one year, with the following information provided for each period: (i) the year it began; (ii) the year it ended; and (iii) the respondent's status during that time, including whether they were employed as a salaried worker, unemployed, in education, out of the labor force, or in another status.

We rely on this feature to reconstruct immigrants' labor market participation and employment trajectories over the lifecycle on a yearly basis. Because this paper focuses on the French context, and to ensure comparability with our administrative registers that do not include information on labor market outcomes out of France, for first-generation immigrants we restrict ourselves to their labor market trajectories from the first year they began to live in mainland France.

2.5 Sample construction and summary statistics

Our baseline sample is drawn from the TeO2 dataset. Specifically, we focus on first- and second-generation immigrants who have lived at least once in a cohabiting relationship that lasted at least 6 months, with at least one child, regardless of whether the child still resides with the parent. Individuals who reported having children before the age of 15 are excluded. To ensure sufficient labor market history for identifying child penalties, we limit our sample to those born before 1995. All combined, these restrictions reduce the sample size from 27,200 to 11,900 respondents. Further, our empirical strategy relies on information about immigrant parents' attitudes, which leads to a reduction in the sample size to 7,000 individuals due to non-responses to relevant survey items. Finally, we also incorporate information on the environments in which immigrants were raised. Non-responses to these specific questions leave us with 6,900 respondents.

Table 1 presents summary statistics for our sample. After appropriate weighting, the sample represents a total population of 2.9 million first- and second-generation immigrant parents living in France. This population includes individuals born between the 1960s and 1990s, with an average year of birth in the 1970s.

We divide the data based on whether respondents hold more or less traditional views regarding gender. A key finding from this exercise is that individuals with less traditional views tend to be slightly older and more educated than those with more traditional gender-related attitudes. These individuals are also more likely to be second-generation immigrants.[3] Finally, those with less traditional views generally had

[3] Appendix A.3 presents a breakdown of attitudes by the countries of birth of first-generation immigrants and the countries of birth of second-generation immigrants' parents. The takeaway is that more traditional attitudes are less common among

their first child at an older age, particularly among women, and have fewer children overall than those with more traditional views.

Figure 1 shows the lifecycle profiles of labor force participation, segmented by gender and attitudes. While differences in labor market participation between attitude groups are minor for fathers, they are substantial for mothers. This suggests that the gender gap in labor force participation is more pronounced among first and second-generation immigrant parents who hold more traditional views about gender compared to those who hold less traditional views. Specifically, at age 45, the labor force participation gap is 16 percentage points for those with more traditional views, compared to 7 percentage points for those with less traditional views. This supports our interpretation that the first component of the PCA analysis captures attitudes related to gender.

The divergence in labor force participation rate across attitudes groups begins at age 20, which coincides with the start of childbearing ages for our population of interest. The remainder of this paper aims at clarifying whether these fertility events can actually explain this divergence.

3 Empirical analysis

The empirical analysis of this paper aims to quantify the effect of gender-related attitudes on the child penalty – specifically, how these attitudes influence the impact of children on the labor market outcomes of parents, particularly mothers. An ideal, though infeasible, solution would be to conduct an experiment in which both children and gender-related beliefs are randomly assigned to potential parents. Since we cannot rely on this infeasible solution, our analysis must address three key issues.

Measurement issue In principle, assigning beliefs would have the beneficial side effect of ensuring that beliefs are known to the experimenter. However, this is not the case in practice, which is why we must infer gender-related attitudes from individual responses to our survey data. Our approach is detailed in 2.2.

Endogenous fertility decisions Children are not randomly assigned; rather, they result from fertility decisions that partly depend on parents' expectations regarding labor market outcomes. Consequently, the comparison between parents and non-parents may not yield a causal interpretation. We address this issue by employing an approach that improves on the now standard event-study approach to identify the child penalty (Angelov, Johansson, and Lindahl, 2016; Kleven, Landais, and Søgaard, 2019). Subsection 3.1 provides further details on our approach.

Endogenous gender-related attitudes Even in the best-case scenario, where gender-related attitudes could be observed directly, we would still face the issue of their endogeneity. This issue is particularly salient in this context because we rely on current self-reported beliefs to infer gender-related attitudes, while we measure the effect of children based on past fertility decisions and labor outcomes. As a result, gender-related attitudes may be the product of an *ex-post* rationalization of past fertility and labor supply choices. This would imply that comparisons of child penalties across different gender-related attitudes cannot be interpreted causally. Our solution to this issue is twofold: (i) we ensure that the immigrants we compare across varying gender-related attitudes are as similar as possible in terms of observable characteristics relevant to the child penalty; (ii) we compare immigrants raised in different environments, with respect to dimensions strongly correlated with current gender beliefs, which are causally anterior to the fertility and labor supply decisions that lead to the child penalty. Subsection 3.2 provides further details on these solutions.

immigrants from Western Europe compared to those from African or Middle Eastern countries.

3.1 Child penalty estimation

Our approach to estimating the child penalty builds on the widely used event-study method in this context (Angelov, Johansson, and Lindahl, 2016; Kleven, Landais, and Søgaard, 2019). This method relies on a difference-in-differences framework that compares parents from the same generation based on their age at first child. If the impact of children on their parents' labor outcomes is negligible before birth, and if age at first child is not correlated with a more pronounced upward trend in labor market outcomes, then the labor market trajectories of parents with a later-born first child serve as a valid counterfactual for those with an earlier-born first child. The remainder of this subsection formalizes this intuition, details its implementation, and discusses the relevance of this approach in the context of this paper.

3.1.1 Model and identification

Our analysis builds on the event-study approach proposed by Kleven, Landais, and Søgaard (2019), with a slight improvement by incorporating insights from recent difference-in-differences literature (see Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). Specifically, our approach aims to address identification issues related to the use of two-way fixed effects in settings where treatment effects are likely to be heterogeneous.[4] The presentation of our empirical framework largely follows Callaway and Sant'Anna (2021) and Sun and Abraham (2021).

Our approach relies on within-gender, within-generation (year of birth)[5] across-age-at-first-child comparisons. Specifically, let $Y_{i,t}$ denote the relevant labor outcome – e.g., labor earnings or labor market participation – of individual i at age t. Let $G_i \in \{f, m\}$ denote individual i's gender and B_i her generation (year of birth).[6] Finally, let C_i denote her age when her first child was born ($C_i = \infty$ if she is childless).

Let $Y_{i,t}(c)$ denote the potential labor supply of individual i at time t had she given birth to her first child at age c. Consistently, $Y_{i,t}(\infty)$ represents her labor supply at time t had she chosen to remain childless. By construction:

$$Y_{i,t} = Y_{i,t}(\infty) + \sum_c (Y_{i,t}(c) - Y_{i,t}(\infty)) \mathbb{1}\{C_i = c\} \tag{1}$$

Our analysis focuses on the causal effect of parenthood on labor supply. In other words, we are interested in (functionals of) the distribution of random variables $Y_{i,t}(c) - Y_{i,t}(\infty)$, with $c < \infty$. Specifically, we define the gender-generation-cohort-specific average treatment effect on the treated:

$$CATT_{g,b,c,t} = \mathbb{E}[Y_{i,t}(c) - Y_{i,t}(\infty) \mid G_i = g,\, B_i = b,\, C_i = c] \tag{2}$$

This quantity corresponds to the effect of being $t-c$ years away from the birth of one's first child, for those individuals of gender g, born in b, who gave birth to their first child at age c. These average treatment effects are not conditional on possible subsequent childbirths. As a result, they incorporate both the causal effect of motherhood at the extensive margin, i.e., choosing to be a parent or not, and at the intensive margin, i.e., choosing to give birth to an additional child for those who are already parents. In other words, the causal effect of parenthood combines that of the first child and all subsequent children, with weights depending on the difference between the time-period t and the timing of the first child's birth c: short-run effects ($t = c$) relate almost exclusively to the extensive margin of fertility, whereas longer-run effects ($t > c$) will integrate a larger share of the consequences of the intensive margin. This

[4] Appendix H.4 presents results where the child penalty estimation strictly follows the method developed by Kleven, Landais, and Søgaard (2019). The key takeaway is that, while their approach leads to different results regarding post-birth dynamics, it does not alter our conclusion that the child penalty is the same for both traditional and egalitarian groups.

[5] In practice, due to limited sample size, we group immigrant parents by decennial generation, i.e., those born in the 1960s, 1970s, and so on.

[6] We further discuss the consequences of comparing individuals within their own generation at 3.1.3.

is particularly true in a context where most parents choose to have more than one child, as indicated by Table 1. We discuss this concern further in 3.2.1.

To identify these quantities from the data, we make two key assumptions: (i) a parallel trends assumption and (ii) a limited anticipation assumption.

Assumption 1 (Parallel trends in baseline outcomes) *For all g, b, (t, t'), and (c, c'), where $c, c' > 1$ and $c, c' < \infty$, we assume:*

$$\mathbb{E}[Y_{i,t}(\infty) - Y_{i,t'}(\infty) \mid G_i = g, , B_i = b, , C_i = c] = \mathbb{E}[Y_{i,t}(\infty) - Y_{i,t'}(\infty) \mid G_i = g, , B_i = b, , C_i = c'] \quad (3)$$

Assumption 2 (Limited anticipation) *For all t, g, b, and c, if $t < c - 1$, then:*

$$\mathbb{E}[Y_{i,t}(c) - Y_{i,t}(\infty) \mid G_i = g, B_i = b, C_i = c] = 0 \quad (4)$$

Assumption 1 implies that, in the absence of children, the average labor outcomes for parents of the same gender and birth cohort evolve in parallel over time. Assumption 2 assumes that the effect of children on their parents' labor outcomes is zero up to two years before the child's birth. This differs from a full no-anticipation assumption because (i) fertility decisions typically occur in the year prior to childbirth, and (ii) maternity leave often starts in the final year before childbirth, especially if the birth occurs early in the civil year, thus affecting the mother's labor supply.

Under these assumptions, if there is sufficient variation in the timing of childbirth within each gender and cohort group, cohort-specific average treatment effects on the treated (ATTs) can be identified from the data.

Proposition 1 (Difference-in-differences estimand) *For all (g, c, t), with $1 < c < \infty$, the cohort-specific treatment effect is given by:*

$$\begin{aligned} CATT_{g,b,c,t} \quad = \quad & \mathbb{E}[Y_{i,t} \mid G_i = g, B_i = b, C_i = c] \\ & -\mathbb{E}[Y_{i,c-2} \mid G_i = g, B_i = b, C_i = c] \\ & -\mathbb{E}[Y_{i,t} \mid G_i = g, B_i = b, \max(1, c-2, t+1) < C_i < \infty] \\ & +\mathbb{E}[Y_{i,c-2} \mid G_i = g, B_i = b, \max(1, c-2, t+1) < C_i < \infty] \end{aligned} \quad (5)$$

Additionally:

$$\begin{aligned} \mathbb{E}[Y_{i,t}(\infty) \mid G_i = g, C_i = c] \quad = \quad & \mathbb{E}[Y_{i,c-2} \mid G_i = g, B_i = b, C_i = c] \\ & +\mathbb{E}[Y_{i,t} \mid G_i = g, B_i = b, \max(1, c-2, t+1) < C_i < \infty] \\ & -\mathbb{E}[Y_{i,c-2} \mid G_i = g, B_i = b, \max(1, c-2, t+1) < C_i < \infty] \end{aligned} \quad (6)$$

Proposition 1 suggests that, within each gender × generation group, if we can observe individuals at least two years before the birth of their first child, it is possible to impute the counterfactual labor supply profiles of parents whose first child is already born, allowing for the identification of cohort-specific ATTs.[7]

Specifically, let $\underline{T(g,b)}, , \underline{T(g,b)} + 1, , ..., , \overline{T(g,b)} - 1, , \overline{T(g,b)}$ denote the time periods that can be observed for individuals in group (g, b). For all c, $CATT(g, b, c, t)$ is identified from the data if:

[7] In practice, when dealing with bounded outcomes, such as binary outcomes like labor market participation, we cap counterfactual labor supply profiles to ensure they stay within admissible ranges. For example, for labor market participation, we write:

$$\begin{aligned} \mathbb{E}[Y_{i,t}(\infty) \mid G_i = g, C_i = c] \quad = \quad & \min(1, \max(0, \mathbb{E}[Y_{i,c-2} \mid G_i = g, B_i = b, C_i = c] \\ & +\mathbb{E}[Y_{i,t} \mid G_i = g, B_i = b, \max(1, c-2, t+1) < C_i < \infty] \\ & -\mathbb{E}[Y_{i,c-2} \mid G_i = g, B_i = b, \max(1, c-2, t+1) < C_i < \infty])) \end{aligned}$$

(i) $\underline{T(g,b)} \leq c-2 \leq \overline{T(g,b)}$;

(ii) $\underline{T(g,b)} \leq t \leq \overline{T(g,b)}$;

(iii) $\mathbb{P}(\max(1,,c-2,,t+1) < C_i < \infty,|,G_i = g,,B_i = b) > 0$.

This final condition implies that very long-run effects are generally unidentifiable under these assumptions, as no counterfactual can be inferred from the data after the last mother is about to give birth to her first child. However, as long as we focus on impacts of parenthood within the first 10 years following the birth of the first child, this is not an issue (see Appendix B).

3.1.2 Aggregation and estimation

Aggregation The quantities of interest represent the causal effect of parenthood for a given gender and a specified time period. These effects are recovered by aggregating gender-generation-cohort-specific ATTs, weighted proportionally to population size. To enhance comparability with existing literature and across labor outcomes, we measure these effects relative to the counterfactual level.[8] Specifically, let Ω denote the subset of gender-generation-cohort-time-period combinations for which all three conditions hold, as well as $C_i > 1$, ensuring that $CATT(g,b,c,t)$ is identified from the data. We define:

$$\tau(g,s) = \frac{\mathbb{E}[Y_{i,C_i+s}(C_i) - Y_{i,C_i+s}(\infty) \mid (g,B_i,C_i,C_i+s) \in \Omega]}{\mathbb{E}[Y_{i,C_i+s}(\infty) \mid (g,B_i,C_i,C_i+s) \in \Omega]} \tag{7}$$

This expression captures the relative average treatment effect of being s years past the birth of one's first child, for a specific subset of individuals that varies with s. By Proposition 1, $\tau(g,s)$ can be expressed as a function of quantities fully identified from the data.

Finally, to quantify how parenthood amplifies gender gaps in labor market outcomes, we introduce the child penalty $\xi(S)$. This measure represents the average difference in the relative effects of parenthood between mothers and fathers over a specified duration::

$$\xi(S) = \frac{1}{S}\sum_{s=0}^{S}\{\tau(f,s) - \tau(m,s)\} \tag{8}$$

Estimation The above discussion motivates a straightforward plug-in estimator, where population probabilities and expectations are replaced by their empirical counterparts. The same approach applies to the estimation of $\xi(S)$.

Under standard integrability assumptions, these estimators are asymptotically normal (Callaway and Sant'Anna, 2021). For inference, we rely on a reweighted bootstrap approach (Shao and Tu, 1995), clustered at the individual level. This method is particularly advantageous as it mitigates the risk of empty comparison groups in bootstrap replications—a non-negligible concern given the narrow groups and finite sample in our analysis. Clustering at the individual level is justified by the study design, as the treatment, i.e., parenthood, is assigned at the individual level (Abadie et al., 2022).

3.1.3 Validation exercises

Credibility of the parallel trends assumption Our entire approach relies on the assumption that, in the absence of children, the average labor outcomes of parents of the same gender and generation who had their first child at different ages would have evolved at the same rate. This is the key assumption underpinning the event-study methodology developed by Kleven, Landais, and Søgaard (2019) to study the child penalty. However, this assumption is subject to debate, as the age at first childbirth is strongly linked to the age of entry into stable employment (Landaud, 2021), which, in turn, is closely correlated

[8] In Appendix G we compare our baseline results with those obtained when the child penalty is measured in absolute value. We find that this does not alter our conclusions.

with educational attainment. Consequently, comparing parents who had their first child later with those who had their first child earlier effectively compares individuals with differing levels of education, who are likely to experience differences in labor market opportunities not only in levels but also in trends.

One potential solution to this issue is to restrict the comparison groups to parents with similar levels of educational attainment. However, this approach is impractical in our context due to the limited sample size in the survey data; further restricting the groups results in highly imprecise estimates. In Appendix C.2.1, we address this limitation by utilizing large administrative datasets where an educational proxy is available. We demonstrate that narrowing comparison groups by educational levels in these datasets does not alter our findings regarding the aggregate child penalty—the average widening of gender gaps in labor market outcomes caused by parenthood over time.

Comparison with administrative registers We validate our survey-based results by comparing them with analogous estimates derived from administrative data, as detailed in Appendix C.2.2. This comparison reveals that for labor outcomes observed in both datasets, the estimated child penalty for immigrant parents is remarkably consistent across data sources.

Couple penalties vs. child penalties As discussed by Kleven, Landais, and Leite-Mariante (2024), if living as a couple is associated with anticipated parenthood, part of the effect of parenthood on parents' labor supply may manifest before the arrival of children and materialize as soon as men and women enter a stable relationship. This raises two important questions: (i) if children arrive shortly after the start of a stable marital life, our approach might capture the gender-biased impact of stable relationships rather than the actual effect of parenthood on labor supply; and (ii) the anticipated effect of motherhood on women's labor supply might be larger for those with more traditional views, even if the direct impact of motherhood is the same.

Appendix D shows that minor modifications to our model allow us to jointly identify the couple penalty and the child penalty, provided there is sufficient variation in the time lapse between the beginning of a relationship and the arrival of children. It also provides empirical estimates of both penalties based on this approach. The key finding is that our estimate of the child penalty is not substantially different from the one derived using this alternative approach. Finally, the appendix demonstrates that both the couple penalty and the child penalty are similar across attitude groups.

3.1.4 Choice of labor outcome

The survey data underlying this study tracks labor market trajectories along only a few dimensions. Specifically, earnings, wages, and hours worked are not included in this dataset, which focuses solely on labor market participation and employment. While analyzing salaried employment is appealing for comparability with administrative registers, we choose to focus on labor market participation. This choice is motivated by the fact that labor market participation, by accounting for the possibility of involuntary unemployment, aligns more closely with the concept of labor supply than realized employment does, because the former is not an equilibrium outcome as the latter is.

Figure 2 presents our estimates of the average impact of parenthood on labor market participation among first- and second-generation immigrants in France. The results show that the decline in labor market participation due to parenthood is 15 percentage points greater for mothers than for fathers.

Although we focus on labor market participation, our results are not sensitive to this specific choice. Appendix H.10 provides estimates similar to our baseline findings, but based on employment-to-population ratios rather than labor market participation rates.

3.2 Does the comparison of immigrants with different gender-related attitudes have a causal interpretation?

To support the causal interpretation of our comparison of immigrants with different gender-related attitudes, we consider two distinct approaches. The first, which involves a reweighting procedure based on the inverse propensity score, aims to make immigrants as similar as possible across comparison groups in terms of the observable characteristics most relevant to the child penalty. While the implementation is straightforward, it raises concerns about which characteristics should be included. We specifically discuss whether variables describing fertility decisions should be part of the covariate set.

The second approach ensures that our comparisons are immune to the concern that current self-reported beliefs may themselves result from past life events directly relevant to the child penalty. This would be the case, for instance, if these beliefs are an *ex-post* rationalization of past fertility and labor supply decisions that contribute to the child penalty. To address this, we build on an approach similar to an instrumental variable strategy. Specifically, we first compare immigrants across the environments in which they were raised. We show that immigrants raised in larger families, where parents' allocation of household chores was more gender-unequal, and who had a more religious upbringing, hold more traditional views regarding gender today. Because, in terms of timing, this environment precedes the fertility and labor supply decisions that give rise to the child penalty, it should also be a causal antecedent of these decisions. Assuming that this correlation was equally strong when the relevant fertility and labor supply decisions were made, comparing immigrants across these dimensions is therefore informative regarding the impact of gender-related attitudes on the child penalty.

Even though this idea closely resembles an instrumental variable strategy, we do not go as far as to move from this reduced-form comparison to a Wald estimand. The reason is that for this Wald estimand to have a straightforward causal interpretation, a restriction exclusion assumption must hold. In the context of this paper, the event-study approach to the child penalty identifies the impact of parenthood on parents. The issue is that these quasi-instruments we consider may lead immigrants to change their fertility decisions in addition to altering their gender-related attitudes. For instance, let us assume that a more traditional upbringing makes immigrants more prone to having children, as seems plausible. In this case, the set of parents raised in more traditional families includes: (i) parents who would have become parents even if they had been raised in less traditional families, for whom the child penalty for parents raised in less traditional families serves as a good counterfactual, and (ii) parents who would not have become parents had they been raised in less traditional families, for whom the counterfactual child penalty cannot be identified from the data on immigrants raised in less traditional families. For this reason, we stick to a reduced-form estimate, acknowledging that selection into parenthood may still be at play.

In addition to this comparison across upbringing environments, we develop a related framework that compares second-generation immigrant parents across their parents' countries of birth, based on the observation that (i) first-generation immigrants with similar migration backgrounds tend to hold the same views; and (ii) second-generation immigrants' attitudes correlate with those of first-generation immigrants from the same country as their parents (see Tables A.1 to A.4). Section 5 details this framework and performs this comparison using both survey and administrative data.

3.2.1 Comparing immigrants with similar observable characteristics

Interpreting the comparison of child penalties across groups defined by their adherence to more or less traditional views regarding gender and bodily autonomy may prove tricky if these groups differ in characteristics that influence both their views and labor supply decisions. Indeed, as Table 1 suggests, immigrants with more traditional gender attitudes tend to be less educated, more likely to be first-generation

immigrants, and usually have more children. Addressing this issue requires techniques aimed at making the groups more similar in terms of observable characteristics.

In the context of this paper, it is helpful to distinguish between two sets of observable characteristics. The first set includes characteristics that are causal antecedents to both attitudes and labor supply decisions. An example here is first-generation status, as country of birth is determined prior to the formation of attitudes. Clearly, we should compare immigrants who are similar with respect to such observable characteristics (Pearl, 2009).

The second set consists of characteristics that are likely to be consequences, rather than causes, of attitudes and that are likely to influence labor supply decisions. A relevant example here is fertility decisions. Whether we should make immigrants similar with respect to these characteristics depends on the question at hand. Essentially, this is a matter of mediation analysis, rather than an issue of whether our causal estimates are biased.

Dealing with differences in fertility The question, therefore, is whether we should compare immigrants with similar fertility decisions. Ultimately, we choose to make attitude groups similar in terms of their fertility decisions. The reason for this choice is that, while we aim to assess the consequences of gender-related attitudes, of which fertility decisions certainly form a part, our outcome of interest is not an observable outcome *per se*, but the consequences of fertility decisions on labor supply. Particularly relevant in this context is the fact that, while our approach is based on the timing of *ego*'s first child's birth, the effect of parenthood incorporates not only the consequences of this first child on *ego*'s labor force participation but also those of all her potential subsequent children. As such, it would be problematic to compare the effects of fertility across groups with differing fertility outcomes.

While conditioning on fertility is our baseline approach, we also present estimates based on two different approaches: (i) comparisons based on raw data, and (ii) comparisons based on data in which we condition on the first set of observable characteristics, such as generation, migration status, or educational attainment, but not on the second set, which includes fertility decisions. These estimates are available in Appendix F.

Supply or demand effects? Given that gender-related attitudes are strongly correlated with migration status and countries of origin (see Appendix A.3), a natural question that may arise is whether our framework captures labor supply or labor demand effects. Indeed, the demand for immigrant labor may depend on their migration background, for instance, if discrimination against immigrants from certain countries is pervasive in the labor market.

Our answer to this question is threefold. Firstly, our outcome of interest is labor force participation, as opposed to actual employment: it includes the possibility of involuntary unemployment that may result from employers discriminating against a particular group of immigrants at the hiring stage (see 3.1.4). Secondly, what we are comparing across attitude groups are not labor force participation rates *per se*, but triple differences in labor force participation rates (across gender, over time, and between parents who have their first child earlier or later in their lifecycle). As a result, for discrimination or, more generally, labor demand to drive our results, it would require employers to discriminate against actual mothers of a particular migration background, as opposed to immigrants from a particular set of countries. Thirdly, we include in our covariate set a synthetic variable built from the TeO2 data that corresponds to (i) self-reported experiences with discrimination and unequal treatment in the labor market, either at the hiring stage or at the workplace, due to either their origins or the color of their skin; and (ii) whether survey respondents believed that they belong to a group subject to racial discrimination in France.[9] As a result, after this reweighting, both attitude groups should be considered as having the same experience with

[9] In practice, experience with discrimination only deals with experiences within 5 years before the survey.

labor market discrimination.

In practice, our approach to the conditioning that allows us to make immigrants more similar across attitude groups is based on an inverse propensity score reweighting technique. Appendix F discusses its practical implementation.

3.2.2 Leveraging the environment in which immigrants grew up

Even when comparing immigrants who are very similar in terms of their observable characteristics, a legitimate concern could be that current gender-related attitudes stem from an *ex-post* rationalization of their past decisions which contribute to the child penalty. To address this issue, we propose moving from current gender-related attitudes to variables assigned prior to these attitudes, in an approach similar to an instrumental variable strategy.

To do so, we begin by considering variables that describe the family environment in which immigrants were raised. Specifically, we examine the prominence of religion during immigrants' childhood, the gender imbalance in household chores between their parents, and the number of children born to their mothers. The premise is that these factors shape attitudes in adulthood, and that we should attribute potential differences in child penalties across immigrants raised in different environments along these dimensions to their gender-related attitudes in adulthood.

We begin by regressing binary gender-related attitudes on these variables using a linear probability model estimated via ordinary least squares. We then split our sample of immigrant parents into two groups, based on whether individuals fall above or below the gender-specific estimated population median in predicted attitudes from this regression. The difference in the share of immigrants whose attitudes we have characterized as more or less traditional between these two environmental groups exceeds 30 percentage points, regardless of gender.[10] This suggests that upbringing environments have a strong effect on later attitudes regarding gender and bodily autonomy.

The final step of our approach is to compare child penalties across these upbringing environment groups, after reweighting the data to make these groups more similar in terms of the relevant observable characteristics (see 3.2.1). Inference for these results is based on a reweighted bootstrap, similar to the method discussed in Subsection 3.1. Subsection 4.2 presents our results. Additional results, considering specific subsets of the variables that describe upbringing environments, are available in Appendix H.5.

4 Results

4.1 Comparison across self-reported gender-related attitudes

4.1.1 Child penalty

We now present our baseline results, comparing child penalties across groups of immigrant parents based on their self-reported gender-related attitudes. Figure 3 displays the estimates, obtained after reweighting the data to ensure similarity across groups in terms of pre-childbirth observable characteristics (e.g., migration status, generation, educational attainment, marital history, experience with discrimination), as well as fertility and post-childbirth decisions (e.g., age at first childbirth, total number of children).[11]

The differences in child penalties across attitude groups are minimal. In fact, the penalty appears slightly larger for parents with less traditional attitudes, though our estimates lack the precision to draw

[10] After reweighting the data to make the groups comparable in terms of relevant observable characteristics.

[11] Similar results, obtained without reweighting or using only pre-birth characteristics in the covariate set, are provided in Appendix F.4. These results are consistent with our baseline estimates.

firm conclusions.[12] Ultimately, the key takeaway is that the child penalty in labor market participation remains consistent regardless of whether immigrant parents hold traditional or progressive views on gender and bodily autonomy.

This result withstands several methodological concerns. First, it is not driven by our use of a composite index for gender-related attitudes; similar results are obtained when comparing groups based on responses to a single survey item about gender inequality in the labor market (**I_GENRE**, see Appendix H.1). Second, the result holds irrespective of the inclusion or exclusion of specific survey items in defining gender-related attitudes (Appendix H.2.2). Third, it remains robust to variations in the threshold used to split groups (Appendix H.3) or in the child penalty specification (Appendix H.4). Fourth, the result is also robust to accounting for the effects of couple life, as shown in Appendix D. This is particularly relevant since selection into parenthood is closely tied to selection into stable relationships, which could have otherwise biased our findings. Finally, the finding applies equally to the child penalty in employment (Appendix H.10), suggesting that: (i) the choice of labor market participation as the baseline outcome does not drive the result, and (ii) for immigrant mothers, motherhood generally does not lead to unemployment, as the magnitude of child penalties is similar across these outcomes.

Additionally, Appendix G demonstrates that this finding persists when the child penalty is measured in absolute terms (percentage points) instead of relative to the counterfactual labor market participation rate (percentage).

4.1.2 Labor market participation

We further explore this topic and show that, among women, both realized and counterfactual labor market participation rates differ significantly across attitude groups. In contrast, these differences are negligible among men. Consequently, the absence of a correlation between child penalties and gender-related attitudes does not contradict the observation that gender disparities in labor market participation are influenced by these attitudes.

To better understand these implications, Table 2 breaks down the contribution of different groups of mothers – categorized by how their child-related decisions change based on their gender-related attitudes – to variations in the child penalty. This analysis relies on a causal interpretation of differences between attitude groups and is based on two simplifying assumptions: (i) that having children always reduces female labor force participation, and (ii) that holding more traditional gender views consistently lowers female labor supply.

Under these assumptions, only two groups of mothers contribute to attitude-related differences in the child penalty. The first group consists of mothers who, under nontraditional views, would always work regardless of having children but, under traditional views, would leave the labor market because of children; they increase the child penalty. The second group includes mothers who, under nontraditional views, leave the labor market because of children but, under traditional views, would not participate in the labor market even without children; they reduce the child penalty. Furthermore, the first group creates a gap in realized labor supply across attitudes (but not in counterfactual labor supply), while the second group creates a gap in counterfactual labor supply (but not in realized labor supply). Ultimately, our findings suggest that these two groups are approximately equal in size.

We conclude this analysis by comparing the contribution of the child penalty to the average gender gaps in labor force participation across attitude groups. To do so, we consider all individual-age observa-

[12] The standard error of the child penalty estimate for each attitude group is approximately 0.03, allowing us to detect differences greater than 8 percentage points. To put this into perspective, in the child penalty data published by Kleven et al. (2024a), this magnitude corresponds roughly to the difference in the employment child penalty between France and countries such as Italy or the UK. Conversely, drawing firm conclusions on the typical differences observed in our estimates – about 3 to 4 percentage points – would require standard errors that are three times smaller. In practical terms, this would necessitate a 10-fold increase in sample size.

tions in our sample of immigrant parents. We impute a counterfactual labor force participation value by adding the corresponding estimated average impact of parenthood to the observed value for individuals with children. For each age and attitude group, we compute both the realized and counterfactual gender gaps in labor force participation, and then average these values over all ages between 20 and 50. The counterfactual value represents the residual gap, not explained by the child penalty, while the difference between the realized and counterfactual values quantifies the contribution of the child penalty to the overall labor force participation gap.

Figure 4 presents our estimates. The realized gender gap in labor force participation for more traditional immigrants is 20.4 percentage points, nearly twice as large as for their more egalitarian counterparts (10.9 p.p.). While the child penalty is very similar across groups, more traditional mothers tend to have their first child at a younger age (see Table 1), which slightly increases the contribution of the child penalty to the gender gap for immigrants with more traditional views: 9.6 p.p. compared to 8.1 p.p. for their less traditional counterparts. Put differently, the child penalty accounts for 47% of the gender gap in labor force participation in the more traditional group, compared to 86% in the less traditional one. Lastly, the residual gap is sizable for more traditional immigrants (9.5 p.p.) but much smaller for the less traditional group (1.4 p.p.). Ultimately, the wider gender gap in the more traditional group (20.4 - 10.9 = 9.5 p.p.) is more attributable to differences unrelated to the child penalty (9.5 - 1.4 = 8.1 p.p.) than to differences in the child penalty's contribution (9.6 - 8.1 = 1.5 p.p.).

This finding aligns closely with the cross-country comparisons presented by Kleven, Landais, and Leite-Mariante (2024). Their research shows that the proportion of the gender gap attributable to child penalties is higher – often exceeding 100% – in countries such as Denmark, Sweden, the Netherlands, and Australia. They support this conclusion with historical data, framing it within a discussion of the child penalty's relevance at various stages of economic development and structural transformation. In contrast, the immigrants in our study all reside in France, where economic development is consistent across the population. Our evidence therefore suggests that the importance of the child penalty in explaining gender gaps in labor outcomes depends on gender-related attitudes. Put differently, the data indicate that the child penalty becomes a dominant driver of gender disparities in the labor market only when gender attitudes shift toward more positive views on female employment and women's autonomy.

4.2 Comparison across environments in which immigrants grew up

Since current self-reported attitudes may be influenced by past fertility and labor supply decisions, the above comparison could provide a biased assessment of the role of gender-related attitudes in the child penalty. To address this issue, we compare child penalties across groups of immigrant parents defined by the environment in which they were raised. This approach relies on two key arguments: (i) these environments are not outcomes of fertility and labor supply decisions made later in life, and (ii) growing up in a more traditional family -— characterized by higher religiosity during childhood, unequal distribution of household chores among *ego*'s parents, or a larger number of siblings -— increases the likelihood of holding more traditional gender views by over 30 percentage points (see 3.2.2).

Figure 5 presents our findings. The key takeaway is that the child penalty in labor market participation is remarkably similar across different upbringing environments. In other words, having children is no more detrimental to the labor market participation of mothers raised in traditional families than to those raised in less traditional families. Since the causal pathway from upbringing environments to the child penalty operates through gender-related attitudes held in adulthood, this finding suggests that such attitudes do not account for the child penalty, reinforcing the causal interpretation of the previous comparison.

This result is not driven by our decision to reweight the data to make the comparison groups comparable in terms of pre-childbirth observable characteristics and fertility decisions (see Appendix H.6). It is also robust to using more restrictive measurements of the upbringing environment (see Appendix H.5),

or defining our attitudes based solely on views regarding gender inequality in the labor market (see Appendix H.1).

In addition to these robustness checks, Appendix G shows that this result remains unchanged when the child penalty is measured in absolute terms rather than relative to the counterfactual labor market participation rate. Furthermore, for women, both realized and counterfactual participation rates exhibit significant differences across upbringing environments, while no such differences are observed for men. Since comparisons across upbringing environments are less likely to be affected by reverse causality bias than those based on current attitudes (as reported in 2019-2020), this reinforces the conclusion that the child penalty is likely not causally related to gender-related attitudes, while still supporting the idea that these attitudes are a key driver of gender gaps in labor market participation.

5 Additional evidence from second-generation immigrants

In this section, we use administrative data on second-generation immigrant parents to compare the child penalty across groups defined by the prevalence of traditional gender-related attitudes among first-generation immigrant parents from the same country. This approach is similar to the epidemiological method used to study the impact of culture on economic outcomes (Fernández, 2011), and closely follows that of Alesina, Giuliano, and Nunn (2013). The rationale for this comparison is similar to that behind the comparison across upbringing environments: namely, that countries of origin are assigned before gender-related attitudes are formed. Restricting the comparison to second-generation immigrants has the additional advantage of ensuring that all individuals are born and raised in France, thus growing up within similar institutions.

This comparison has the advantage of requiring only knowledge of immigrants' countries of origin and aggregate information on gender-related attitudes by country, without needing additional individual data on second-generation immigrants, whose fertility and labor supply decisions we track. This enables us to conduct these comparisons using administrative registers rather than survey data. Moving from survey data to administrative registers offers two main advantages: (i) the sample size is much larger, improving the precision of the comparison; and (ii) the data on labor market outcomes are more comprehensive, allowing us to examine the impact of gender-related attitudes on dimensions beyond labor market participation, and making our results more comparable to the standard child penalty literature.

5.1 EDP data: general overview

The Permanent Demographic Sample (*'Echantillon Démographique Permanent*, EDP) collects administrative data from birth registers, tax returns, firm records, and welfare benefits recipients' files, as well as information from comprehensive censuses and census surveys. These data pertain to a representative sample of the French population, including all individuals born on one of the first four days of April, July, and October, or between January 2nd and 5th. The EDP tracks the professional, financial, residential, and family situations of these "EDP individuals" annually.

In the context of this paper, the EDP data have three key advantages:

- Combining information from birth certificates with census data allows us to identify both first- and second-generation immigrants, including their country of birth and, for second-generation immigrants, their parents' country of birth;
- Information from birth certificates regarding EDP individuals' children allows us to track their fertility decisions;
- Lastly, payroll tax data enable us to track entire labor market histories.

Taken together, these three features make it possible to estimate the child penalty by country of origin for second-generation immigrants, helping us assess the causal interpretation of our results regarding the link between gender-related attitudes and the child penalty.[13]

Although the data are of high quality, using them requires combining information from multiple sources – census data, birth certificates, tax returns, and payroll tax forms. Therefore, decisions must be made regarding how to handle these various pieces of information, which may raise some issues in terms of methodology. Appendix C.1 details how we address these concerns, particularly regarding the identification of immigrants and migration outflows, the measurement of fertility decisions, and the definition of relevant labor market outcomes.

5.2 Method

Our approach here is based on ideas similar to those outlined in Subsection 3.2, except that instead of considering upbringing environments measured at the individual level, it relies on a more aggregated variation. The intuition is that, while it is possible that one's self-reported beliefs may depend on past decisions, this should not be the case when it comes to the beliefs of their peers. To take advantage of this fact, we first document that first-generation immigrants' friends often come from the same country as they do. We then show that the beliefs of first-generation immigrants are strongly correlated with those of other first-generation immigrants born in the same country. Additionally, we provide evidence that second-generation immigrants whose parents were born in countries where first-generation immigrants are more likely to report traditional gender-related attitudes are also more likely to hold traditional views on gender. This, in turn, motivates the comparison of second-generation immigrants in terms of the child penalty, based on how frequently traditional gender beliefs are held among first-generation immigrants from their parents' countries of origin, which closely mirrors Alesina, Giuliano, and Nunn (2013)'s approach to identifying the effect of cultural norms on economic outcomes.

Lateral transmission We begin by considering survey items related to friendship networks. The proportion of immigrant parents who report having met with at least one friend with the same migration background as themselves in the last two weeks is above 50% for first-generation immigrant mothers and over 60% for first-generation immigrant fathers. This suggests that, in most cases, first-generation immigrants tend to stay close to their peers, which creates an opportunity for the lateral transmission of attitudes.

We then turn to the attitudes themselves. To do so, we compare first-generation immigrant parents' attitudes to those of their peers using a leave-one-out approach. Specifically, we regress *ego*'s gender-related attitudes on the average attitudes of first-generation immigrant parents born in the same country as her, excluding *ego* from the computation of these average attitudes. This regression is based on a linear probability model estimated using ordinary least squares. Table 3 presents our results. These results show that first-generation immigrant parents' attitudes regarding gender are indeed positively correlated within groups defined by their country of birth, which we interpret as evidence of lateral transmission.

Vertical transmission We then turn to second-generation immigrants. Specifically, we regress second-generation immigrant parents' attitudes regarding gender on the average attitudes of first-generation immigrant parents born in the same country as their parents[14], using a linear probability model estimated

[13] This dataset also allows us to estimate the child penalty for both first- and second-generation immigrants, which helps validate the quality of the survey data by comparing them with a dataset that has already provided published estimates of the child penalty in France (see C.2).

[14] If one of the parents is born in France, we use the average attitudes of French parents of the same gender who are neither first- nor second-generation immigrants.

with ordinary least squares. Table 4 presents our estimates. These results show that second-generation immigrant parents' attitudes closely track those of their parents' peers.

We use the estimated coefficients to predict gender-related attitudes for second-generation immigrant parents in the administrative registers, where the attitudes of first-generation parents' peers were imputed based on the survey results.

Consistent with the rationale, we find that second-generation immigrant parents whose parents were born in countries from which immigrants in France typically hold more traditional views on gender also tend to hold more traditional views on gender. The difference in actual traditional views on gender between those above the median predicted probability of holding traditional views is about 15 percentage points for fathers and up to 20 percentage points for mothers.

The final step of our approach is to compare child penalties across these origin groups, after reweighting the data to make the groups more similar in terms of relevant observable characteristics (see 3.2.1). Inference regarding these results is based on a reweighted bootstrap procedure similar to the one discussed in Subsection 3.1, with the main difference being that we now need to generate replicates for both the survey data (to account for the uncertainty in the estimated coefficients displayed in 4, upon which the grouping is based) and the administrative registers (to account for the uncertainty in the estimation of the child penalty)

5.3 Results

Figure 6 displays the child penalties in labor earnings for each group defined by the attitudes of second-generation immigrant parents' parents' peers. We find that the aggregate child penalty—the difference in the impact of parenthood on mothers and fathers, averaged over time—is not larger for second-generation immigrants who, based on their parents' countries of birth, are predicted to be more likely to hold traditional views. Specifically, this quantity remains around 30 percentage points, regardless of the group considered. This finding supports our interpretation that the child penalty is not easily explained by traditional gender-related attitudes.

Additional results that separately consider transmission through fathers and mothers are available in Appendix H.7. These results do not alter our conclusion. We also demonstrate that this finding is not dependent on our decision to consider the child penalty in relative rather than absolute terms (see Appendix G), the reweighting of the data to make origin groups more comparable (see Appendix H.8), or the restriction of the comparison set to parents who entered the labor market at the same age (see Appendix H.9).

6 Concluding remarks

This study leverages a rich dataset on first- and second-generation immigrants in France to examine whether traditional gender-related attitudes are a primary driver of the child penalty in labor outcomes. Across three empirical tests, we find that the child penalty remains remarkably consistent across groups, regardless of variations in self-reported beliefs, childhood environments, or country of origin. This uniformity challenges the idea that traditional gender attitudes, as captured in our data, directly explain differences in the child penalty.

Our results confirm that the child penalty is substantial and borne almost entirely by mothers, leading to a 15% reduction in their labor force participation compared to a counterfactual scenario without children, while fathers' labor market outcomes remain unaffected. Notably, we observe significant differences in female labor force participation across groups with varying gender attitudes, but these differences are present both with and without children. This suggests that the child penalty itself operates indepen-

dently of the attitudinal dimensions captured by our measures, even if cultural norms influence overall participation levels.

While our findings may seem at odds with previous research that links gender attitudes to labor market outcomes, they underline a critical distinction: gender-related attitudes may shape the baseline levels of participation or other dimensions of inequality, but they do not appear to influence the additional labor market disadvantage triggered specifically by motherhood. This raises important questions about the cultural and institutional determinants of the child penalty.

Our work is motivated by earlier literature showing that family policies often fail at mitigating the child penalty (Rabaté and Rellstab, 2022; Kleven et al., 2024a). These findings have prompted a shift in focus toward cultural determinants of gender inequality, which this study aims to explore. However, our results do not directly assess the role of public policies, nor do they imply that such policies are ineffective. Instead, they suggest that even as cultural and institutional shifts reduce overall gender gaps, the child penalty may persist as a structural barrier disproportionately affecting mothers.

Future research should aim to refine the measurement of cultural norms and attitudes to better capture the subtle and multidimensional nature of their influence on the child penalty. Beliefs about motherhood, family responsibilities, and social expectations may play a significant role but remain difficult to quantify with current tools. Cross-national comparisons and longitudinal approaches could also help identify how institutional and cultural contexts interact to shape labor market outcomes for mothers.

These findings suggest a paradoxical outcome: as societies move toward more egalitarian attitudes and reduce overt gender discrimination, the child penalty may become the dominant driver of labor force gender gaps, as appears to be the case in many developed countries (Kleven, Landais, and Leite-Mariante, 2024). Understanding and addressing the specific norms, practices, and structural barriers that sustain the child penalty will be critical for achieving gender equality in labor markets.

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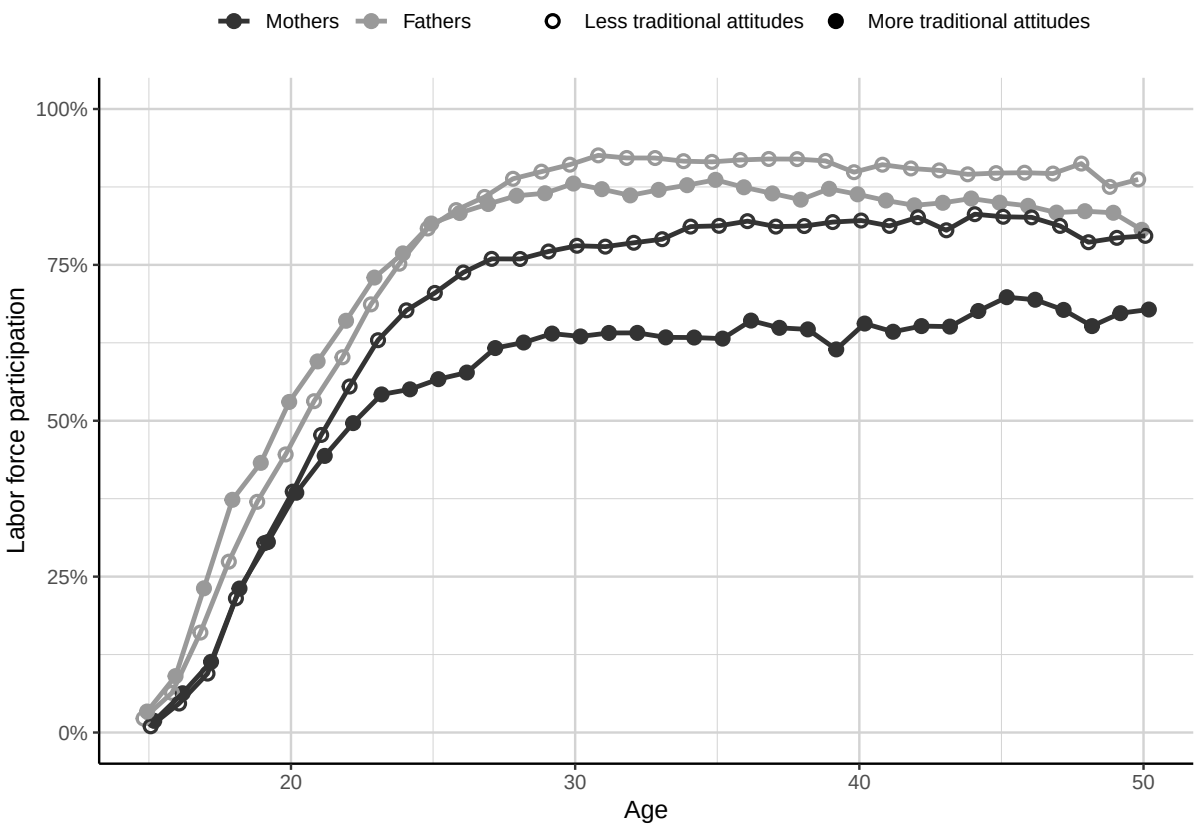
Figures

Figure 1. Lifecycle profiles of labor force participation: by gender and gender-related attitudes

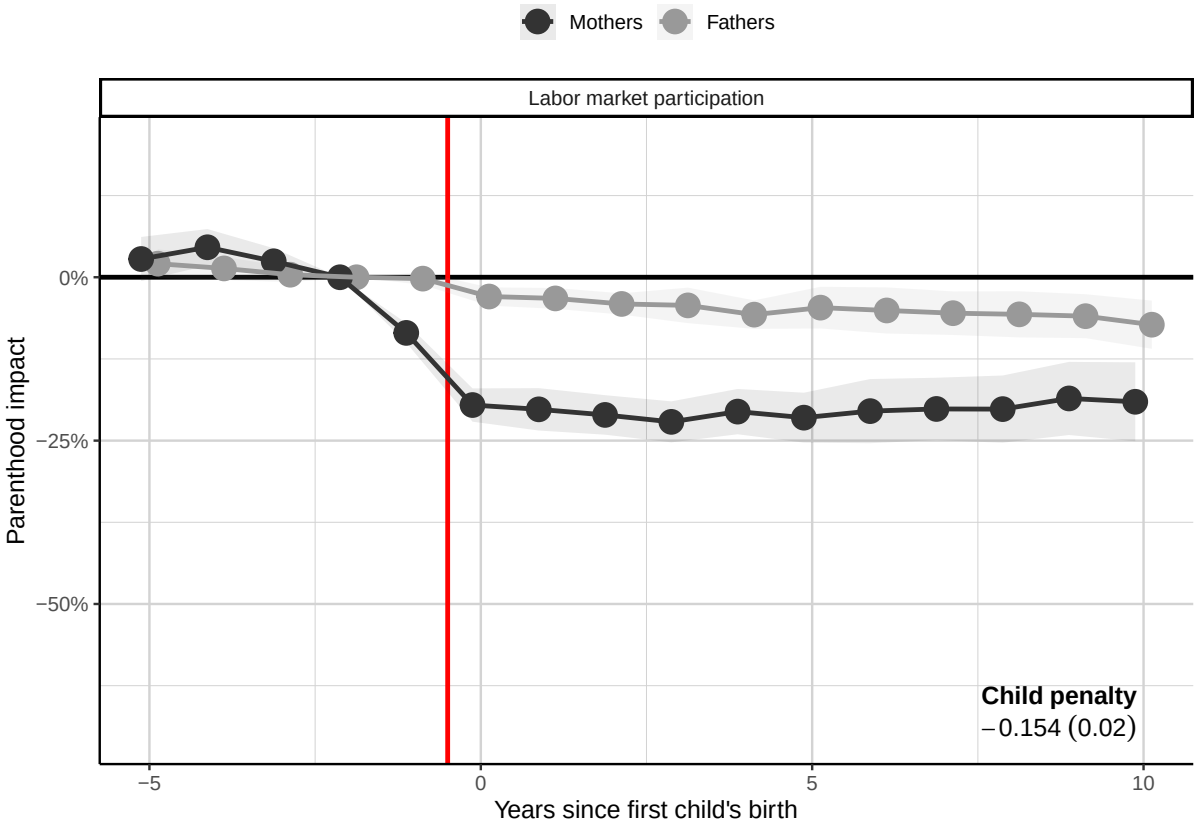


Labor force participation rate for immigrant parents, by age, gender and gender-related attitudes. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure 2. Child penalty in labor market participation for immigrant parents

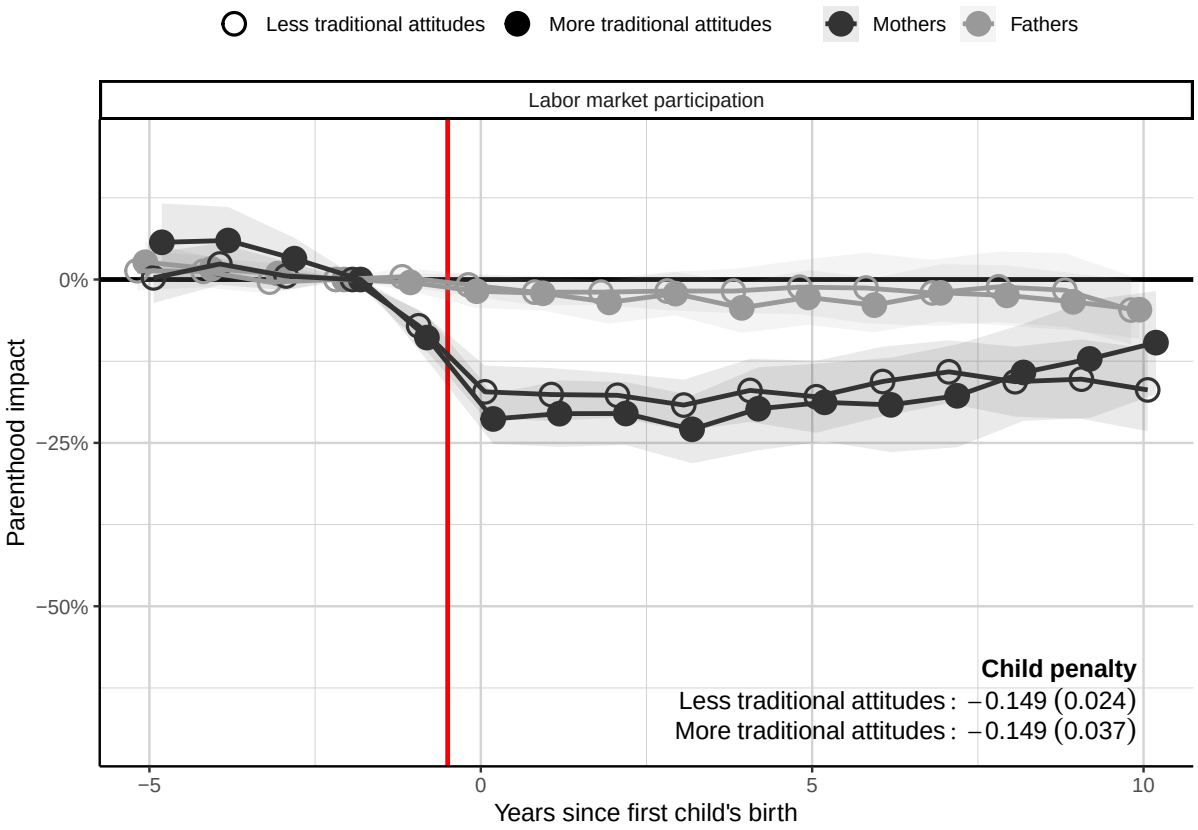


Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure 3. Child penalty in labor market participation for immigrant parents: by self-reported attitudes, balanced on pre-birth characteristics and fertility decisions

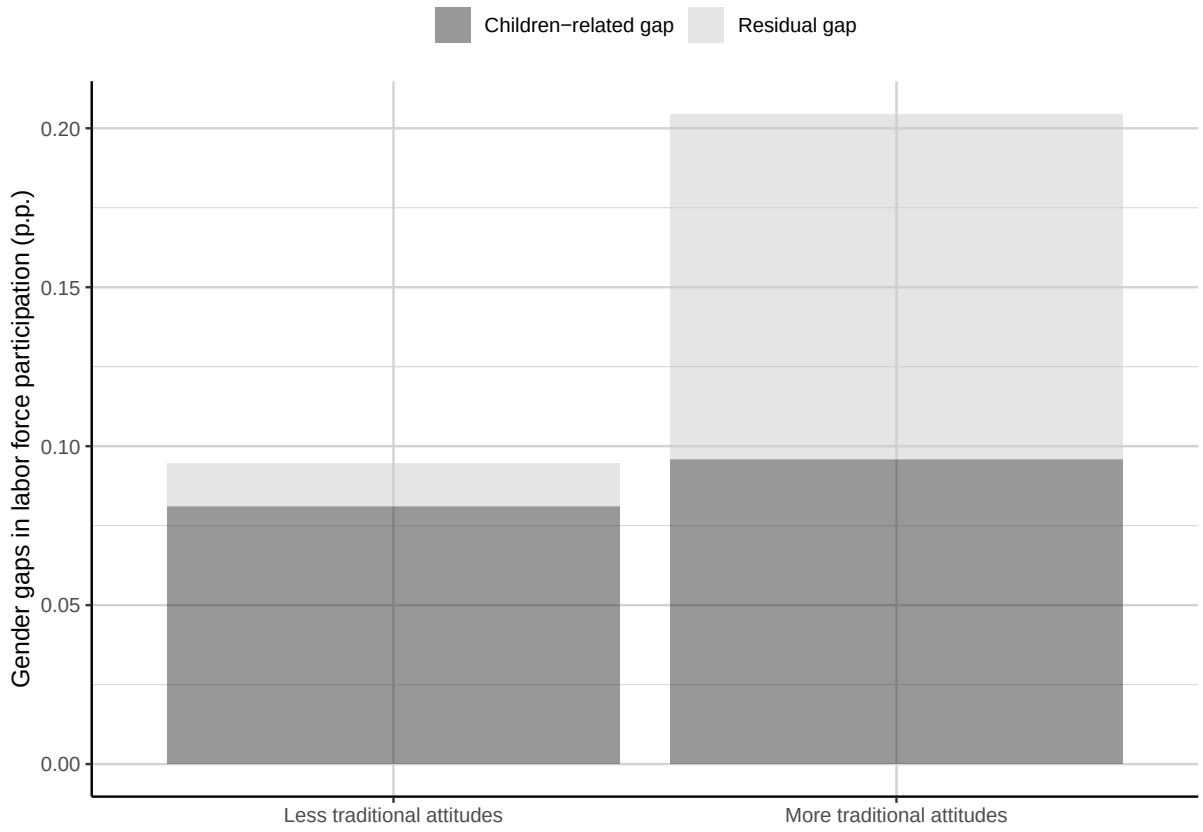


Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make attitudes groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure 4. Gender gaps in labor force participation: contribution of the child penalty, by gender-related attitudes

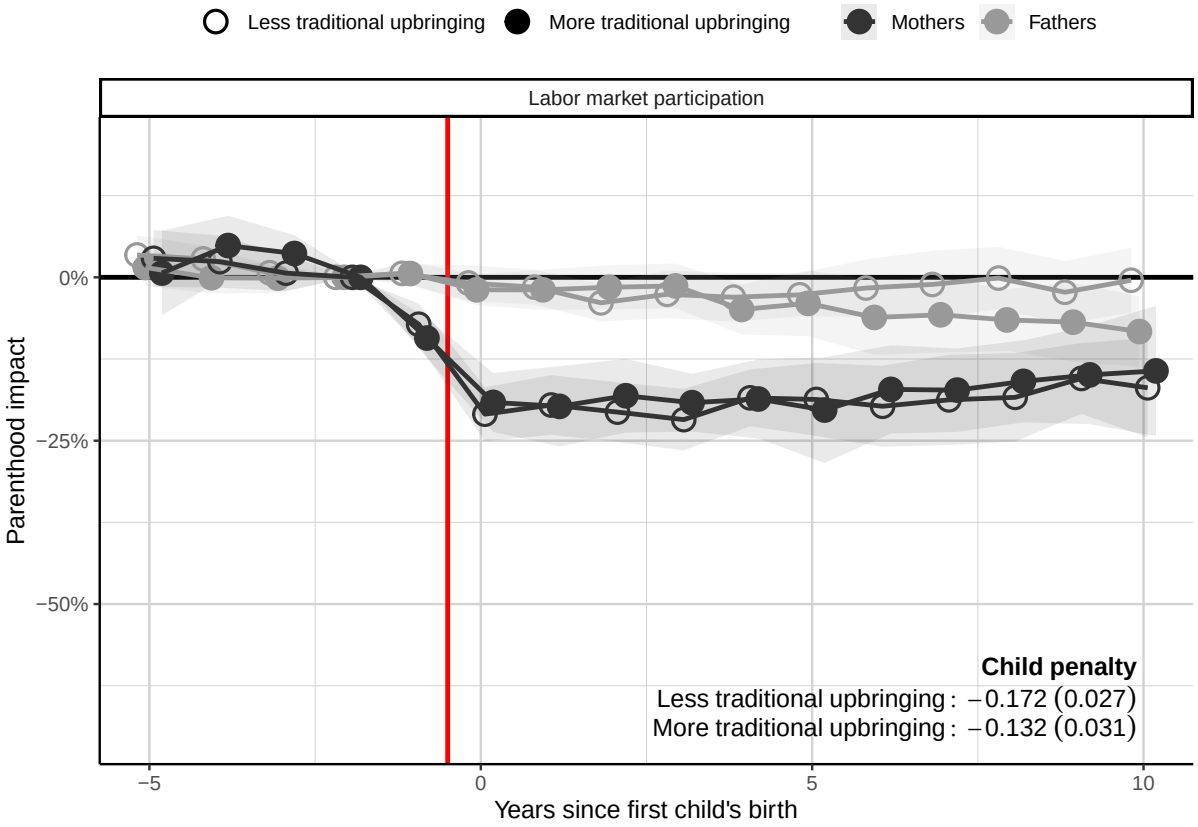


Average labor force participation gaps from age 20 to age 50 for immigrant parents, by gender-related attitudes. This figure displays the contribution of the child penalty to the gender gap, that is the magnitude of the child penalty in labor force participation multiplied by the prevalence of parenthood, and the residual gap, that is the difference between the realized rates and this contribution. Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure 5. Child penalty in labor market participation for immigrant parents: by upbringing environments, balanced on pre-birth characteristics and fertility decisions

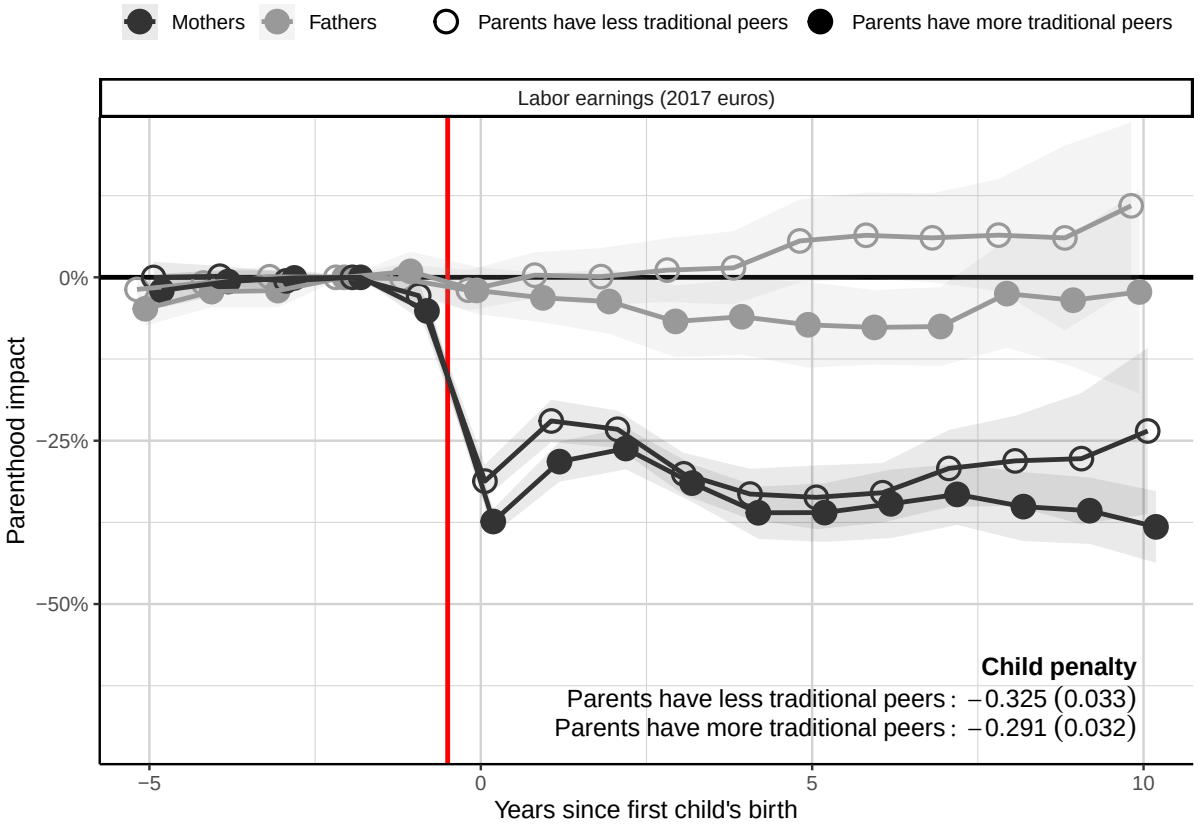


Upbringing environment is measured by *ego*'s father's and mother's having a religion, prominence given to religion in *ego*'s education before the age of 18, gender imbalance between *ego*'s parents' allocation of household chores (daily meals, grocery shopping and clothes washing) before the age of 18 and the number of children born to *ego*'s mother. Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make upbringing environments groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure 6. Child penalty in labor earnings for second-generation immigrant parents: by second-generation immigrant parents' parents' peers' attitudes, balanced on pre-birth characteristics and fertility decisions



Parents' peers' attitudes are measured as the share of first-generation female (resp. male) immigrants from *ego*'s mother's (resp. fathers') country of birth with above-median traditional gender-related attitudes. Parenthood impact is identified from comparisons between parents of the same cohort (year of birth) who got their first salaried job at the same time, but had their first child at different ages (at least two years after their first salaried job), and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make origin groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level.

Population. Second-generation immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

Tables

Table 1. Immigrant parents average outcomes: by gender-related attitudes

	Fathers		Mothers	
	Less traditional	More traditional	Less traditional	More traditional
Sample size	1,591	1,693	1,847	1,725
Population size	690,000	720,000	762,000	691,000
Year of birth	1974	1975	1975	1976
Age at first relationship	23.6	25.0	21.9	22.1
Age at first cohabitation	25.8	27.1	24.0	24.1
Age at first child's birth	31.8	31.1	29.3	27.8
Total number of children	2.0	2.4	2.0	2.4
Age at the end of education	20.8	19.6	21.0	19.8
Self-reported discrimination	0.22	0.29	0.22	0.24
First-generation immigrant	0.42	0.69	0.45	0.66

Traditional gender-related attitudes are defined by scoring above median on the first component of the PCA of the relevant survey items. *Population.* Immigrant parents living in mainland France in 2019-2020 *Source.* Ined and Insee, Trajectoires et Origines 2 survey (2019-2020).

Table 2. Impact of traditional gender-related attitudes on the child penalty, by potential children-related labor supply decisions

Under nontrad. views \ Under trad. views	$(1 \to 1)$	$(1 \to 0)$	$(0 \to 0)$
$(1 \to 1)$	=	−	=
$(1 \to 0)$	∅	=	+
$(0 \to 0)$	∅	∅	=

Under the assumptions that children always decrease mothers' labor supply, the only possible potential children-related labor supply decisions are (i) always to participate in the labor market $(1 \to 1)$, (ii) to leave the labor force $(1 \to 0)$ and (iii) never to participate in the labor market $(0 \to 0)$. Under the additional assumption that holding traditional views always decrease mothers' labor supply, three combinations of potential labor supply decisions are forbidden. The only two combinations that change the child penalty, that is the difference between what women do with children and what they would do without children, depending on the views they hold are $((1 \to 1) \to (1 \to 0))$, that is mothers who would always work regardless of children under nontraditional views, but would leave the labor market due to children under traditional views, who make the child penalty even more negative, and $((1 \to 0) \to (0 \to 0))$, that is mothers who leave the labor market due to children under nontraditional views, and would never participate in the labor market even without children under traditional views, who make the child penalty less negative.

Table 3. Regression of first-generation immigrant parents' attitudes on their peers' attitudes

	Traditional gender-related attitudes	
	Fathers	Mothers
hline Peers' attitudes	0.57	0.57
	(0.05)	(0.04)
Constant	0.27	0.24
	(0.03)	(0.03)
N	1,909	2,047
R^2	0.07	0.08
F-Statistic	143.03	167.50

First-generation immigrant parents' peers are defined as first-generation immigrant parents of the same gender as them who were born in the same country as them. This variable is computed using a leave-one-out approach. *Population.* First-generation immigrant parents living in mainland France in 2019-2020 *Source.* Ined and Insee, Trajectoires et Origines 2 survey (2019-2020).

Table 4. Regression of second-generation immigrant parents' attitudes on their parents' peers' attitudes

	Traditional gender-related attitudes	
	Fathers	Mothers
Ego's father's peers' attitudes	0.32	0.17
	(0.08)	(0.07)
Ego's mother's peers' attitudes	0.26	0.42
	(0.07)	(0.06)
Constant	0.08	0.09
	(0.04)	(0.04)
N	1,324	1,475
R^2	0.05	0.06
F-Statistic	33.18	47.20

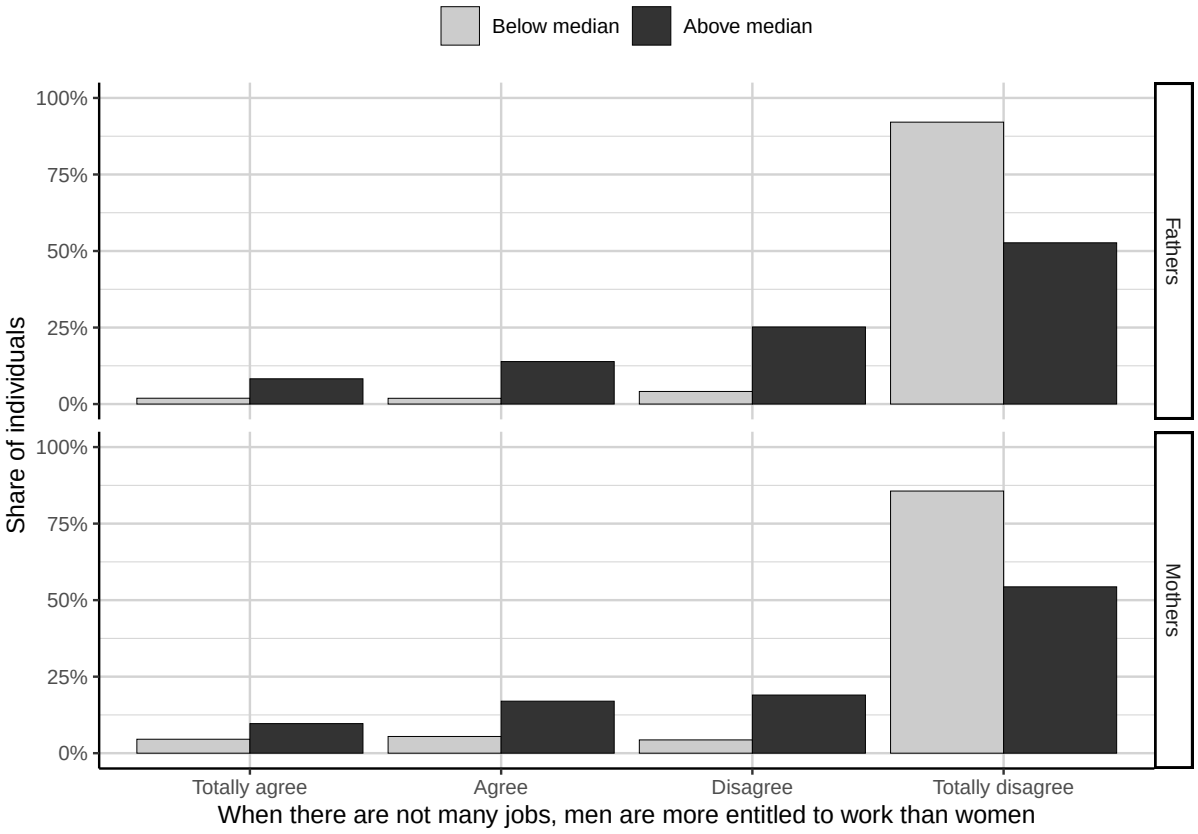
First-generation immigrant parents' peers are defined as first-generation immigrant parents of the same gender as them who were born in the same country as them. *Population.* Second-generation immigrant parents living in mainland France in 2019-2020 *Source.* Ined and Insee, Trajectoires et Origines 2 survey (2019-2020).

A Construction and interpretation of gender-related attitudes

For each survey item included in the PCA step of our analysis, Figures A.1 to A.6 present the distribution of immigrant parents across the possible responses. These figures reveal clear differences in the views expressed by immigrant parents on gender inequality, women's bodily autonomy, religion, political orientation, and the gender of their friends. Specifically, they show that immigrant parents scoring above the median of the first component are more likely to prioritize male employment, disapprove of non-medical abortion and same-sex couples, report strong religious feelings, identify as right-leaning or politically neutral, and have friends of the same gender.

A.1 Contribution of each survey item

Figure A.1. Distribution of immigrant parents' views regarding male and female employment



Immigrant parents' distribution along the different levels of survey item **I_GENRE**. The median is that of the first component of the PCA estimated over the six relevant survey items (see 2.2).

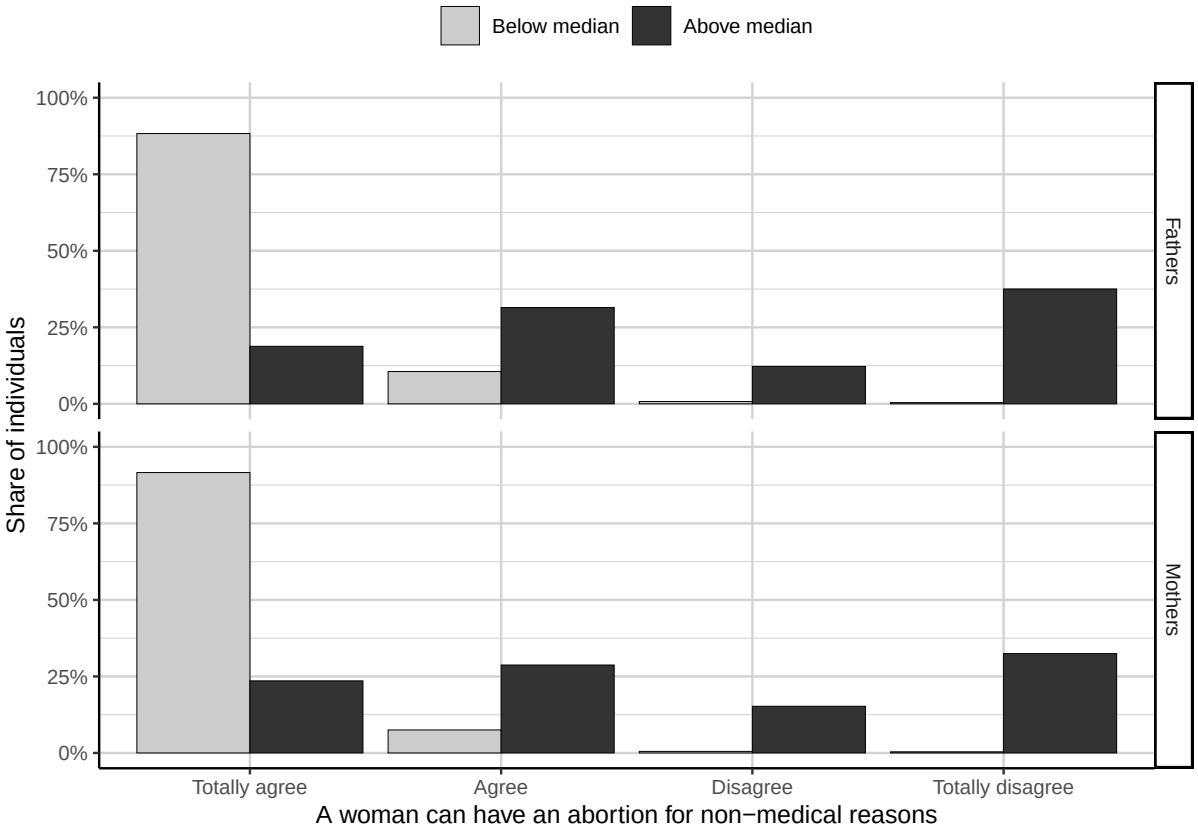
Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

A.2 Gender-related attitudes and gender imbalance in household chores

Figures A.7 and A.8 show the distribution of immigrant parents currently living with a partner, based on their self-reported allocation of household chores, disaggregated by gender and gender-related attitudes. Both attitude groups display clear gender imbalances in the division of household tasks: women are much more likely to report being primarily responsible for daily meals and laundry, while men are more likely to

Figure A.2. Distribution of immigrant parents' views regarding non-medical abortion

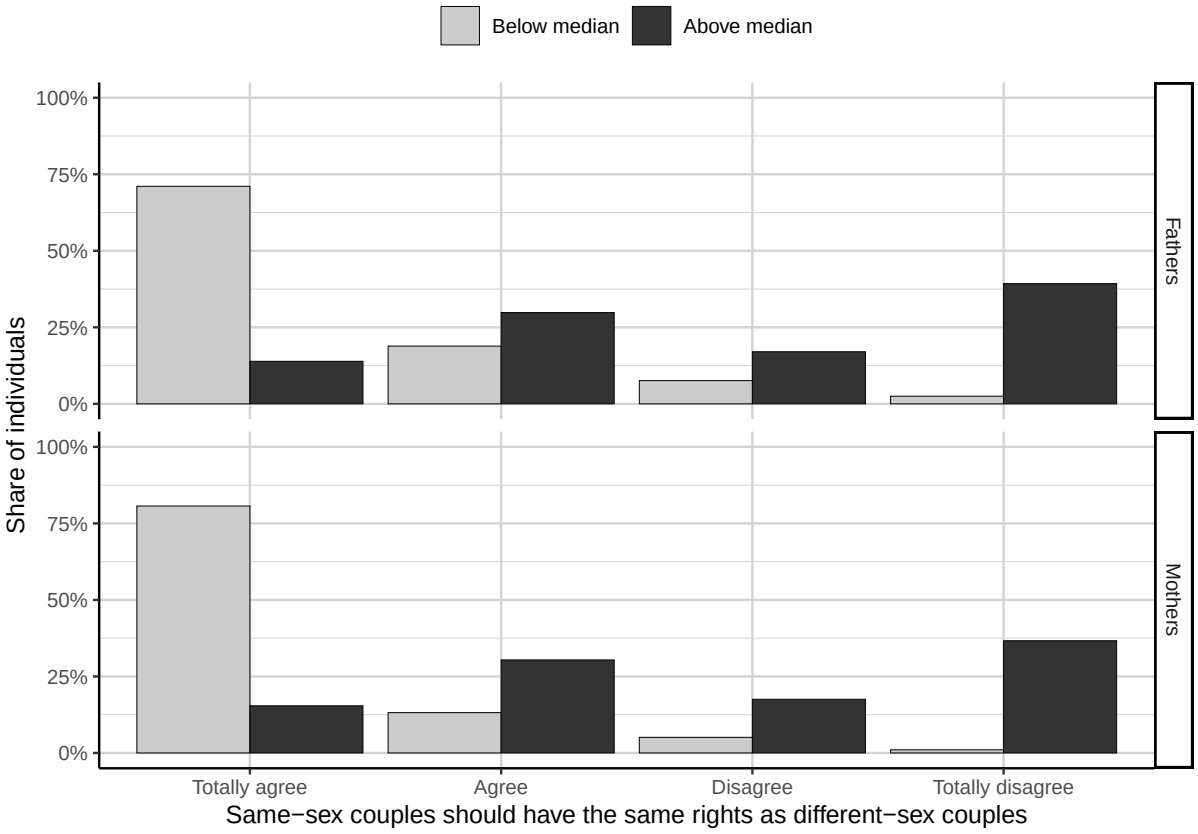


Immigrant parents' distribution along the different levels of survey item **I_AVORT**. The median is that of the first component of the PCA estimated over the six relevant survey items (see 2.2).

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure A.3. Distribution of immigrant parents' views regarding equal rights for same-sex couples

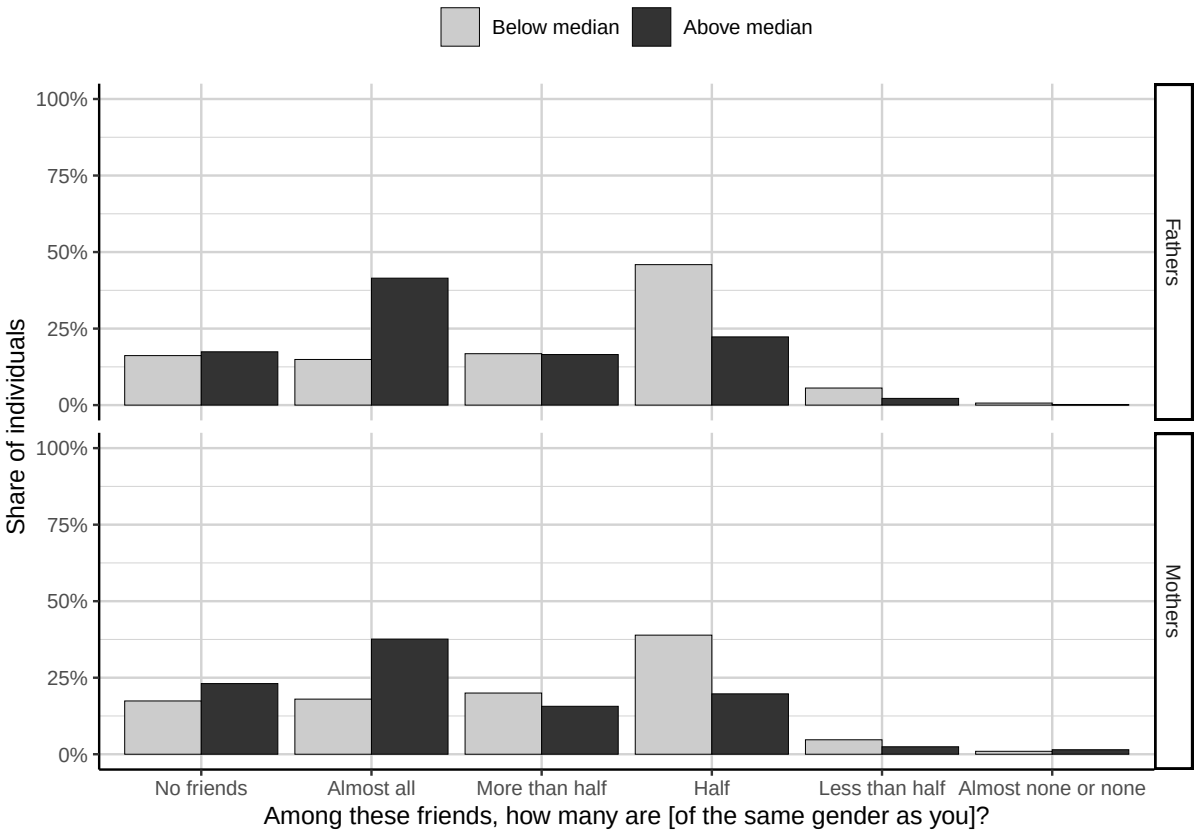


Immigrant parents' distribution along the different levels of survey item **I_HOMO**. The median is that of the first component of the PCA estimated over the six relevant survey items (see 2.2).

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure A.4. Distribution of immigrant parents' depending on how many of their friend are of the same gender as them

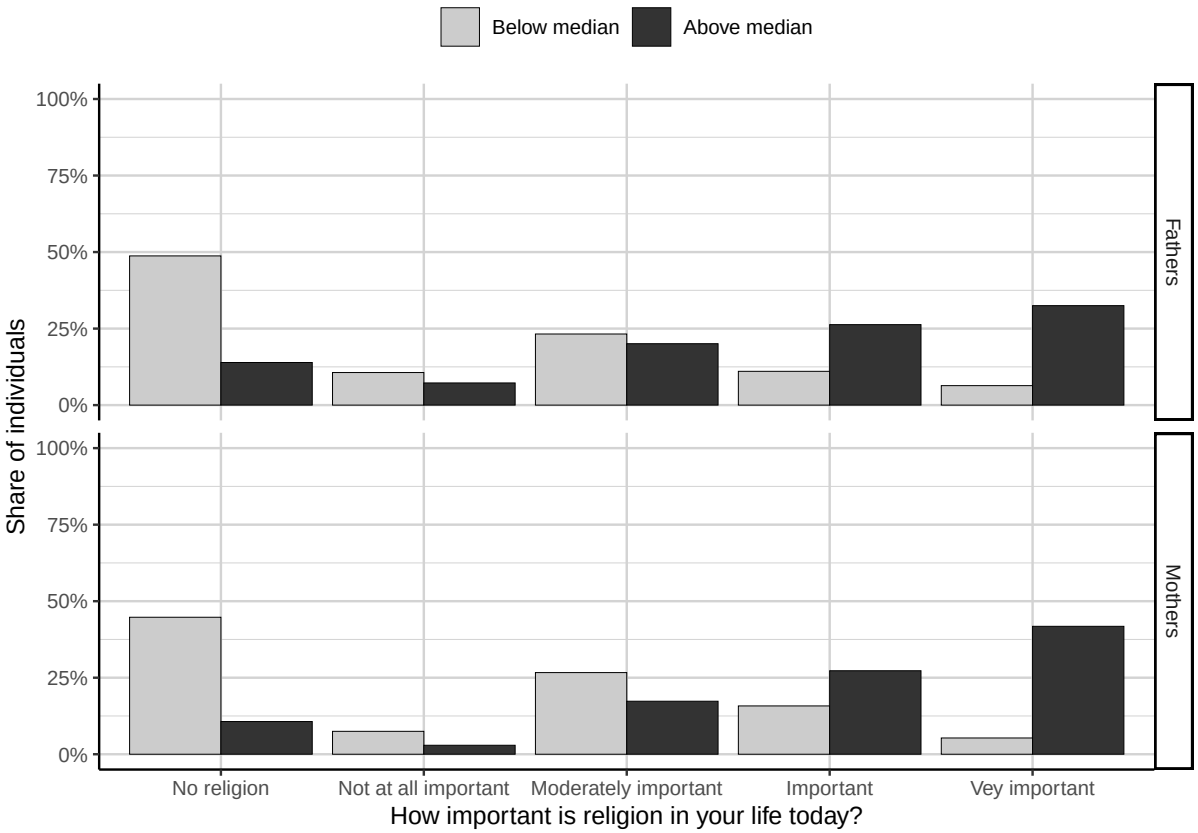


Immigrant parents' distribution along the different levels of survey item **A_RHOM** and **A_RFEM** (depending on their own gender). Immigrants who have not met with any friend over the last two weeks before the survey interrogation are included as a specific category. The median is that of the first component of the PCA estimated over the six relevant survey items (see 2.2).

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure A.5. Distribution of immigrant parents' prominence given to religion

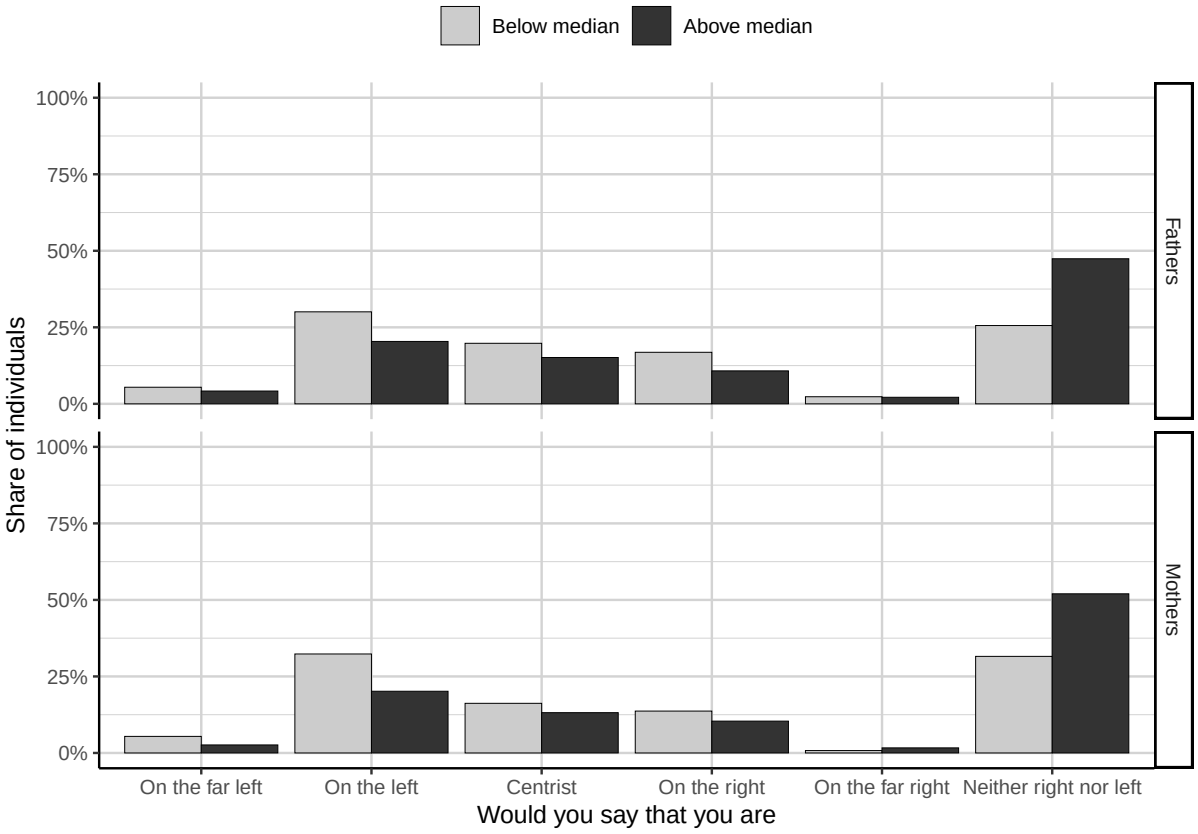


Immigrant parents' distribution along the different levels of survey item **R_IMPVIE**. Immigrants currently without a religion are included as a specific category. The median is that of the first component of the PCA estimated over the six relevant survey items (see 2.2).

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure A.6. Distribution of immigrant parents' self-reported political orientation



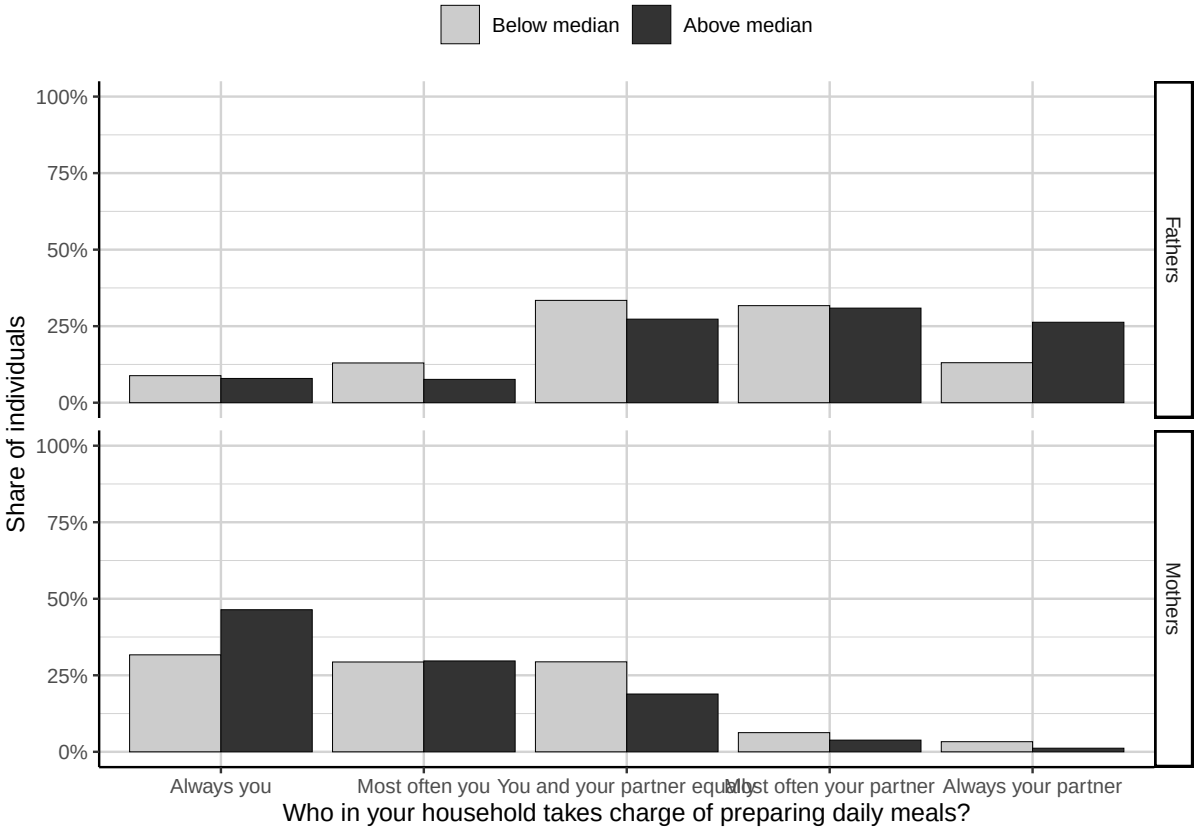
Immigrant parents' distribution along the different levels of survey item **I_OPIPOL**. The median is that of the first component of the PCA estimated over the six relevant survey items (see 2.2).

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

state that these tasks are managed by their partners. This imbalance is, however, more pronounced among immigrant parents characterized as holding more traditional gender views. Specifically, the complete delegation of these tasks to women appears relatively more common in this group than among those with less traditional attitudes.

Figure A.7. Distribution of immigrant parents' self-reported allocation of household chores: daily meals



Immigrant parents' distribution along the different levels of survey item **C_REPAS**. The median is that of the first component of the PCA estimated over the six relevant survey items (see 2.2).

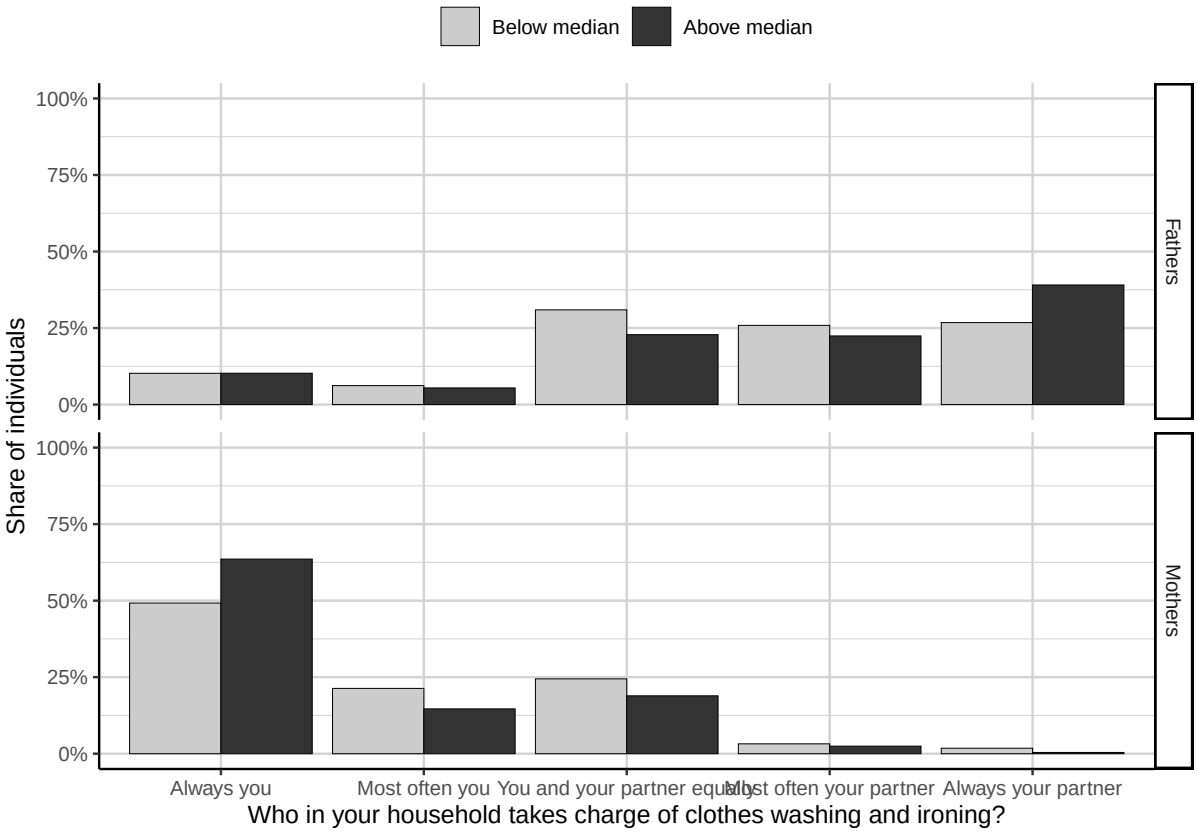
Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

A.3 Gender-related attitudes by countries

Tables A.1 to A.4 present the proportion of immigrant parents identified as holding more traditional gender views, disaggregated by gender and migration background. For this analysis, we limit ourselves to cells with at least 20 individuals in the sample, representing over 7,500 individuals in the population. The ranking of countries of origin by the share of immigrant parents with traditional attitudes is broadly consistent across genders and between first- and second-generation immigrants. The main finding is that immigrants from Western Europe are typically the least likely to hold traditional gender views, whereas these attitudes are more prevalent among immigrants from African countries, both North African and Sub-Saharan.

Figure A.8. Distribution of immigrant parents' self-reported allocation of household chores: clothes washing and ironing



Immigrant parents' distribution along the different levels of survey item **C_LINGE**. The median is that of the first component of the PCA estimated over the six relevant survey items (see 2.2).

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Table A.1. First-generation immigrant parents with traditional gender-related attitudes: by mothers' country of birth

Country of birth	Sample size	Population size	Share with traditional attitudes
Spain	48	17,600	0.14
Germany	35	14,100	0.17
United Kingdom	37	15,000	0.18
Italy	37	15,200	0.20
Belgium	31	14,800	0.27
Poland	31	13,800	0.42
China	117	14,000	0.44
Vietnam	88	7,900	0.47
Romania	43	25,300	0.49
Portugal	189	76,200	0.51
Russia	45	14,100	0.64
Algeria	218	94,000	0.68
Tunisia	33	30,100	0.69
Morocco	202	101,100	0.73
Cameroon	39	11,000	0.74
Senegal	58	15,400	0.77
Turkey	167	32,100	0.79
Côte d'Ivoire	40	12,900	0.80
Congo - Brazzaville	34	13,100	0.89
Congo - Kinhasa	42	15,400	0.90

Traditional gender-related attitudes are defined by scoring above median on the first component of the PCA of the relevant survey items. *Population.* First-generation immigrant mothers living in mainland France in 2019-2020 *Source.* Ined and Insee, Trajectoires et Origines 2 survey (2019-2020).

Table A.2. First-generation immigrant parents with traditional gender-related attitudes: by fathers' country of birth

Country of birth	Sample size	Population size	Share with traditional attitudes
United Kingdom	30	11,900	0.07
Germany	23	11,700	0.18
Belgium	29	10,700	0.19
Spain	44	19,100	0.43
Mali	31	7,900	0.49
Italy	33	15,000	0.51
Portugal	176	77,200	0.51
Romania	25	14,100	0.62
Senegal	74	21,500	0.64
Cameroon	25	7,700	0.66
China	61	8,300	0.67
Congo - Kinhasa	32	9,700	0.67
Tunisia	61	51,300	0.71
Algeria	247	114,900	0.72
Congo - Brazzaville	31	10,700	0.76
Côte d'Ivoire	45	11,700	0.76
Turkey	212	39,000	0.78
Morocco	254	121,800	0.78

Traditional gender-related attitudes are defined by scoring above median on the first component of the PCA of the relevant survey items. *Population.* First-generation immigrant fathers living in mainland France in 2019-2020 *Source.* Ined and Insee, Trajectoires et Origines 2 survey (2019-2020).

Table A.3. Second-generation immigrant parents with traditional gender-related attitudes: by mothers' parents' countries of birth

Father	Mother	Sample size	Population size	Share with traditional attitudes
France	Italy	29	15,000	0.13
Italy	France	64	43,700	0.21
Italy	Italy	30	19,200	0.23
France	Spain	32	18,300	0.25
Portugal	France	36	22,300	0.26
Spain	France	29	22,200	0.27
Algeria	France	65	31,100	0.27
Spain	Spain	40	29,500	0.32
France	Germany	25	10,000	0.35
Portugal	Portugal	95	58,400	0.39
Tunisia	Tunisia	56	27,600	0.41
Algeria	Algeria	204	90,500	0.50
Morocco	Morocco	104	53,200	0.57
Turkey	Turkey	64	9,400	0.80

Traditional gender-related attitudes are defined by scoring above median on the first component of the PCA of the relevant survey items. *Population.* Second-generation immigrant mothers living in mainland France in 2019-2020 *Source.* Ined and Insee, Trajectoires et Origines 2 survey (2019-2020).

Table A.4. Second-generation immigrant parents with traditional gender-related attitudes: by fathers' parents' countries of birth

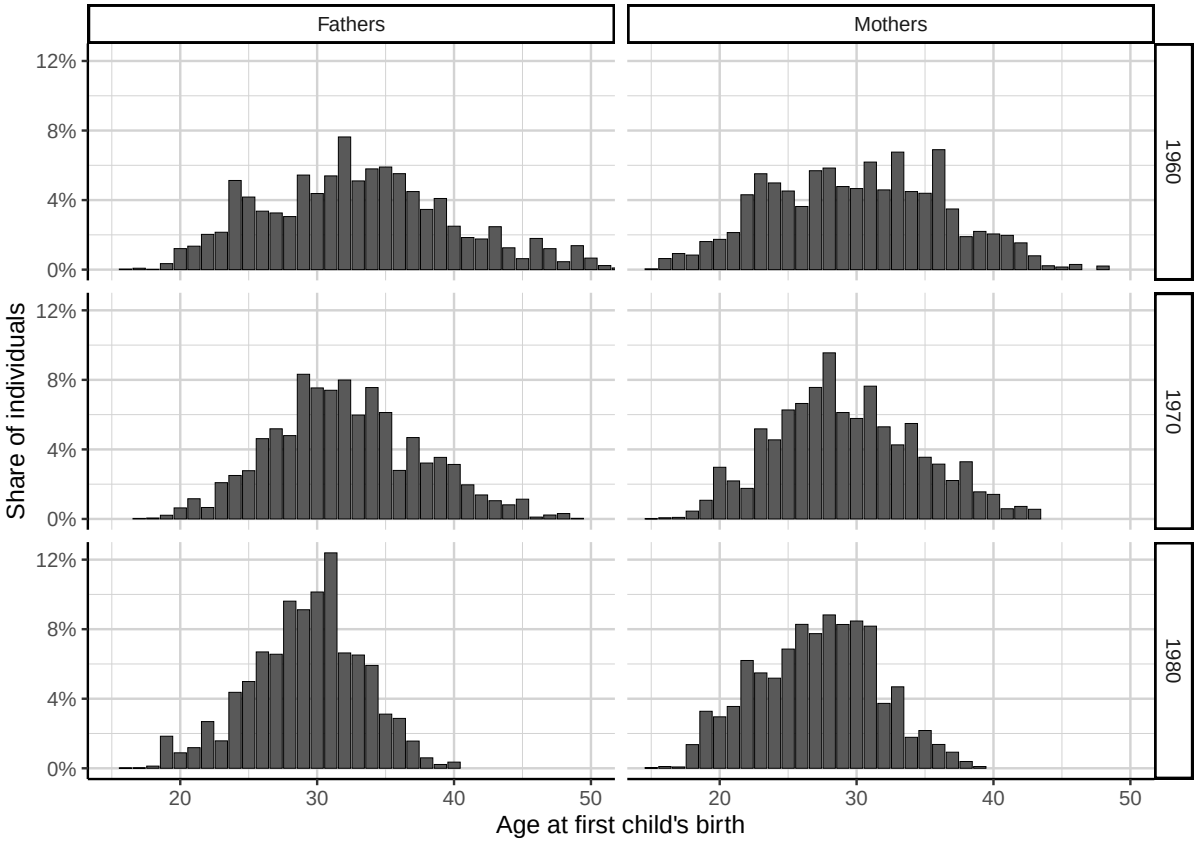
Father	Mother	Sample size	Population size	Share with traditional attitudes
Spain	Spain	33	27,700	0.15
France	Spain	35	23,100	0.22
Spain	France	33	22,600	0.24
Portugal	Portugal	101	61,700	0.26
Italy	Italy	43	34,800	0.32
France	Italy	36	24,100	0.33
Italy	France	55	45,400	0.33
Algeria	France	44	21,400	0.36
Tunisia	Tunisia	37	19,500	0.45
Algeria	Algeria	190	91,800	0.48
Portugal	France	35	23,100	0.48
Morocco	Morocco	69	36,000	0.63
Turkey	Turkey	68	9,000	0.80

Traditional gender-related attitudes are defined by scoring above median on the first component of the PCA of the relevant survey items. *Population.* Second-generation immigrant fathers living in mainland France in 2019-2020 *Source.* Ined and Insee, Trajectoires et Origines 2 survey (2019-2020).

B Age at first child's birth

Figure B.1 shows the distribution of age at first childbirth in our sample, disaggregated by gender and generation. This demonstrates that, as long as we limit our focus to the consequences of parenthood within the first 10 years after becoming a parent, there is sufficient variation in age at first childbirth to identify relevant average treatment effects.

Figure B.1. Distribution of immigrant parents' age at first child's birth: by decennial generation



Immigrant parents' distribution along age at first child's birth, by decennial generation.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

C Additional evidence from administrative data

C.1 Data

C.1.1 Identifying immigrants

By definition, the empirical analysis in this paper relies on our ability to distinguish between immigrants and natives within the French population. While the TeO2 survey focuses on immigrants' lived experiences and inherently incorporates this distinction, the same is not true for the EDP data. Specifically, our empirical strategy requires two key elements: (i) the ability to differentiate between immigrants and natives; and (ii) knowledge of immigrants' countries of origin. Since this paper examines both first- and second-generation immigrants, the challenge is twofold: collecting data on the place of birth of EDP individuals and their parents.

For EDP individuals, the data already include synthetic information on their place of birth, making it straightforward to distinguish first-generation immigrants from the rest of the French population. The situation is more complex, however, when considering the place of birth of EDP individuals' parents. Specifically, the most direct source of this information would be birth registers. Yet, birth registers are missing for a significant portion of the sample, and even when collected, a non-negligible share of the certificates lacks this information. To address this, we combine birth certificate data with additional information from exhaustive censuses. Ultimately, this procedure allows us to recover information on the parents' place of birth for over 90% of the sample, compared to a baseline recovery rate of 50% for the most affected cohorts born in the 1980s and 1990s. Appendix E.1 provides further details on this issue and our solution.

C.1.2 Identifying migration outflows

As detailed in C.1.4, information on labor market outcomes is derived from payroll tax data, which cover the universe of salaried employees. Consequently, the absence of an EDP individual from the payroll tax records at a given point in time is interpreted as evidence of nonemployment. This approach, however, is subject to two main limitations. First, it does not account for non-salaried employment. Second, the fundamental design of the EDP data is not to assess the characteristics of EDP individuals annually but to track information for individuals who, at some point, were identified as part of the sample. In other words, the EDP data do not aim to assess employment status on a yearly basis for a fixed set of individuals known to reside in France but rather to search payroll tax data for individuals previously included in the sample, regardless of whether they remain part of the French population.

As a result, our approach may misclassify as nonemployed individuals who have actually left France. This issue is particularly relevant for this study, which focuses on immigrants, as they are more likely than natives to leave France later in life. To address this problem, we rely on recent methods that combine exhaustive census data with comprehensive income tax records to identify migration outflows (Caron and Reeve, 2018; Solignac and Dutreuilh, 2018). An additional benefit of this approach is that it allows us to estimate the approximate date when first-generation immigrants initially arrived in France. Appendix E.2 provides a detailed explanation of this solution.

C.1.3 Fertility measurement

By construction, the EDP data include birth certificates for the children of EDP individuals, which provides a straightforward way to track fertility events. However, many of these birth registers are missing for children born in the 1980s and 1990s. Additionally, the birth certificates only record children born in France, which poses a challenge for first-generation immigrants who may have had children prior to

their arrival in the country. To address this issue, we supplement the birth register data with information from: (i) children reported as part of families where EDP individuals were identified as parents in the 1990 and 1999 exhaustive censuses; and (ii) children reported as living in the same household as EDP individuals in income tax records between 2011 and 2018. For the latter, we focus on children living with first-generation immigrants, as the birth certificate data are complete for the youngest cohorts. Appendix E.3 details our approach and demonstrates that we successfully filled gaps for the most affected EDP individuals.

C.1.4 Labor market outcomes

In the EDP data, labor market histories are derived from payroll tax registers known as *Déclarations Annuelles de Données Sociales* (DADS). By law,[15] French employers are required to complete a DADS form for every employee subject to payroll taxes. These forms contain detailed information on days paid, hours paid, occupation, industry, gross and net wages, other job characteristics (start, duration, and end of employment periods, as well as part-time employment), employer characteristics (size and location), and individual characteristics (age, gender, and municipality of residence).

In principle, the data are available from 1967 onwards, but their scope has not remained constant over time. Specifically, the data do not include public sector workers before 1988, agricultural workers or workers in overseas territories before 2002, or salaried employees paid directly by households before 2009. We have chosen to exclude data prior to 1988 and omit information on agricultural workers, overseas territories, and household employees, in order to rely on consistent measures of labor earnings and labor supply.

In the context of this paper, our main variables of interest are: (i) net real annual labor earnings, defined as the sum of all salaried earnings across all employers; (ii) time worked, measured as both the number of paid hours and the number of days worked; and (iii) hourly wages, defined as the ratio of annual earnings to time worked. Further details on the measurement of earnings and time worked are provided in Appendix E.4.1. The key points are that, with a few exceptions: (i) maternity leave allowances paid by social security are not included in our measure of earnings; (ii) the duration of maternity leave, in days, is counted as a positive number of days worked; (iii) the number of hours worked during maternity leave is zero; and (iv) the number of hours worked (hourly wages) is overestimated (underestimated) for workers not paid by the hour during years when they take maternity leave.

Information regarding working time is not fully available before 1995. In Appendix E.4.2 we detail how we deal with this issue, as well as few other technicalities.

C.1.5 Sample construction and summary statistics

Our sample is a subset of the EDP sample. Specifically, our empirical analysis require that we focus on individuals of whom countries of birth and parents' countries of birth (for those born in France) are observed, as well as complete labor market histories. These requirements lead us to focus on individuals born on October, 1^{st} to 4^{th} on even-numbered years after 1967. The estimation of child penalties is based on differences among parents and requires sufficiently long labor market histories. For this reason, we restrict ourselves to individuals with at least on child and we drop all individuals born in 1990 or later. To be consistent with the scope of the survey data, we discard individuals who did not fill an income tax form in 2019, so as to limit ourselves to immigrant parents living in France in 2019. This leaves us with 25,800 individuals who represent 4.7 millions first and second-generation immigant parents living in France.

[15] Failure to submit DADS forms or providing incorrect or missing information is subject to fines.

Information on working time is not fully available prior to 1995. In Appendix E.4.2, we detail how we address this issue, along with a few other technicalities.

Table C.1. Immigrant parents average outcomes: by gender

	Fathers	Mothers
Sample size	12,028	13,749
Population size	2,187,000	2,500,000
Year of birth	1977	1978
Age at first child's birth	30.0	27.6
Total number of chidren	2.3	2.4
Age at first job	23.0	19.3
First-generation immigrant	0.55	0.55
Age when first arrived in France (for first-generation immigrants)	29.5	29.2

Age at first job is only based on salaried jobs held in France. *Population.* Immigrant parents living in mainland France in 2019-2020 *Source.* CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

C.2 Validating the child penalty estimated from survey data

C.2.1 Parallel trends assumption

Although educational attainment is not directly observed in the administrative registers, it is still possible to assess the amount of bias resulting from omitting educational attainment in the grouping of individuals. Indeed, since entire labor market trajectories are collected in the data, we can group individuals by gender × year-of-birth × age at first job, which is feasible given the large sample size, and compare the results with those obtained using gender × decennial generation groupings, which are our baseline estimates.

Figures C.1 and C.2 display the results of these two analyses based on the administrative data. The main takeaway from this comparison is that, as long as we are interested in the aggregate child penalty — that is, the difference between mothers and fathers in the consequences of parenthood, averaged over time — not accounting for this potential threat to the credibility of the parallel trends assumption does not change the results.

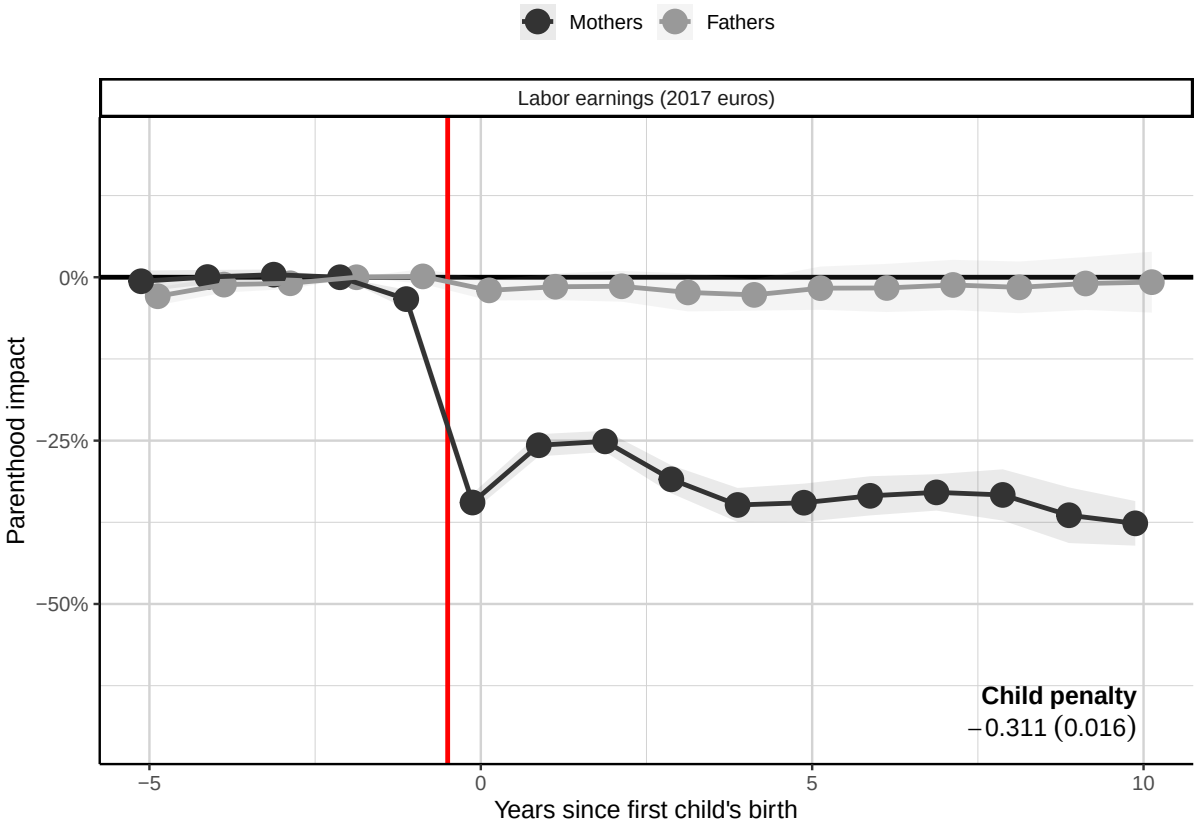
Based on this result, for the remainder of this paper, we will present estimates from administrative data based on the most restrictive grouping, in order to provide an accurate picture of the dynamics of the effects. When it comes to estimates from survey data, we will maintain our baseline grouping, emphasizing that what truly matters is the aggregate child penalty, rather than the year-to-year dynamics of the effects.

C.2.2 Comparisons across datasets

To assess whether survey data provide reliable estimates of the child penalty, we focus on the only labor outcome common to both the TeO2 and EDP data, namely salaried employment. Figures C.3 and C.4 present the results of this analysis. These confirm that we obtain very similar results for our population of first- and second-generation immigrants from both datasets.

An additional validation exercise for our approach to the child penalty is to compare our results with those of the existing literature. Specifically, our closest match is the child penalty estimation exercise by Meurs and Pora (2019), which relies on the same administrative data and comparisons as we do, although they consider the entire French population, whereas we focus on first- and second-generation immigrants.

Figure C.1. Child penalty in labor earnings for immigrant parents: baseline estimate

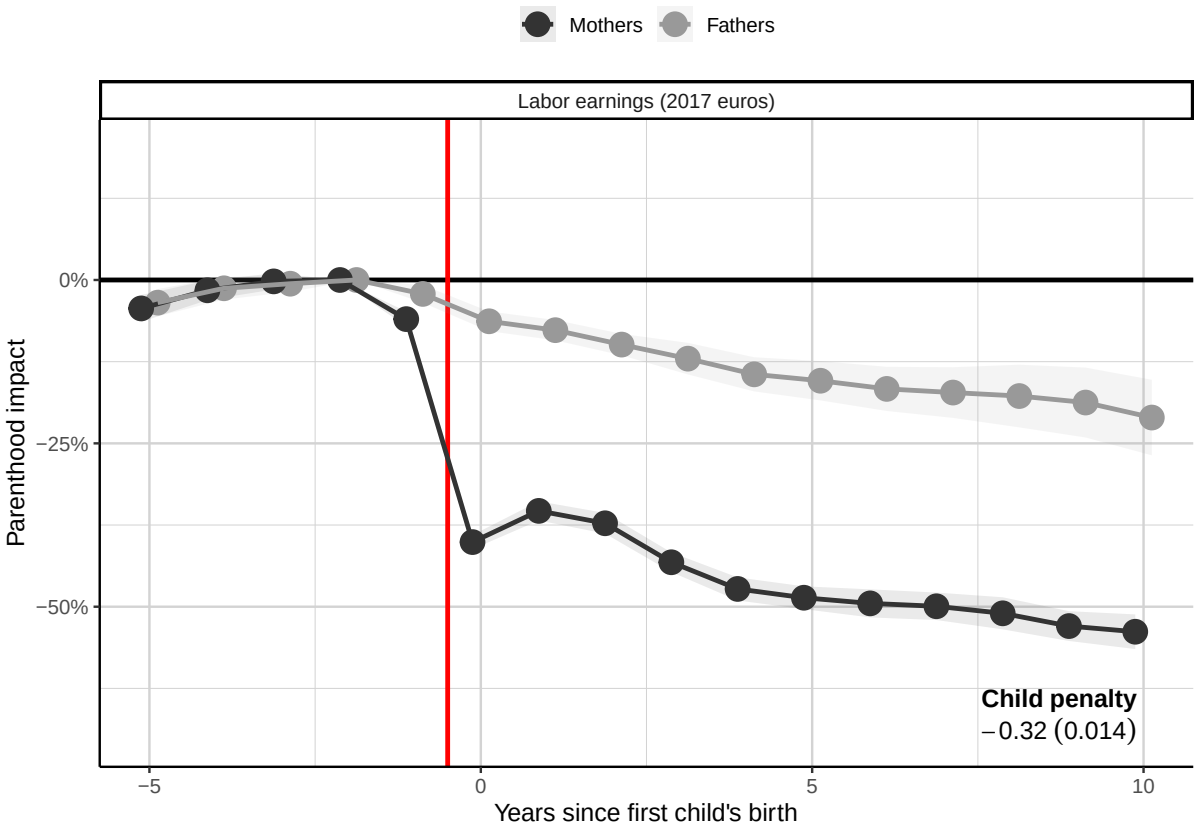


Parenthood impact is identified from comparisons between parents of the same cohort (year of birth) who got their first salaried job at the same time, but had their first child at different ages (at least two years after their first salaried job), and displayed relative to the counterfactual earnings level. The child penalty is the difference in relative parenthood impact between men and women, averaged over the first 10 years after their first child is born. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

Figure C.2. Child penalty in labor earnings for immigrant parents: comparisons *à la* Kleven, Landais, and Søgaard (2019)



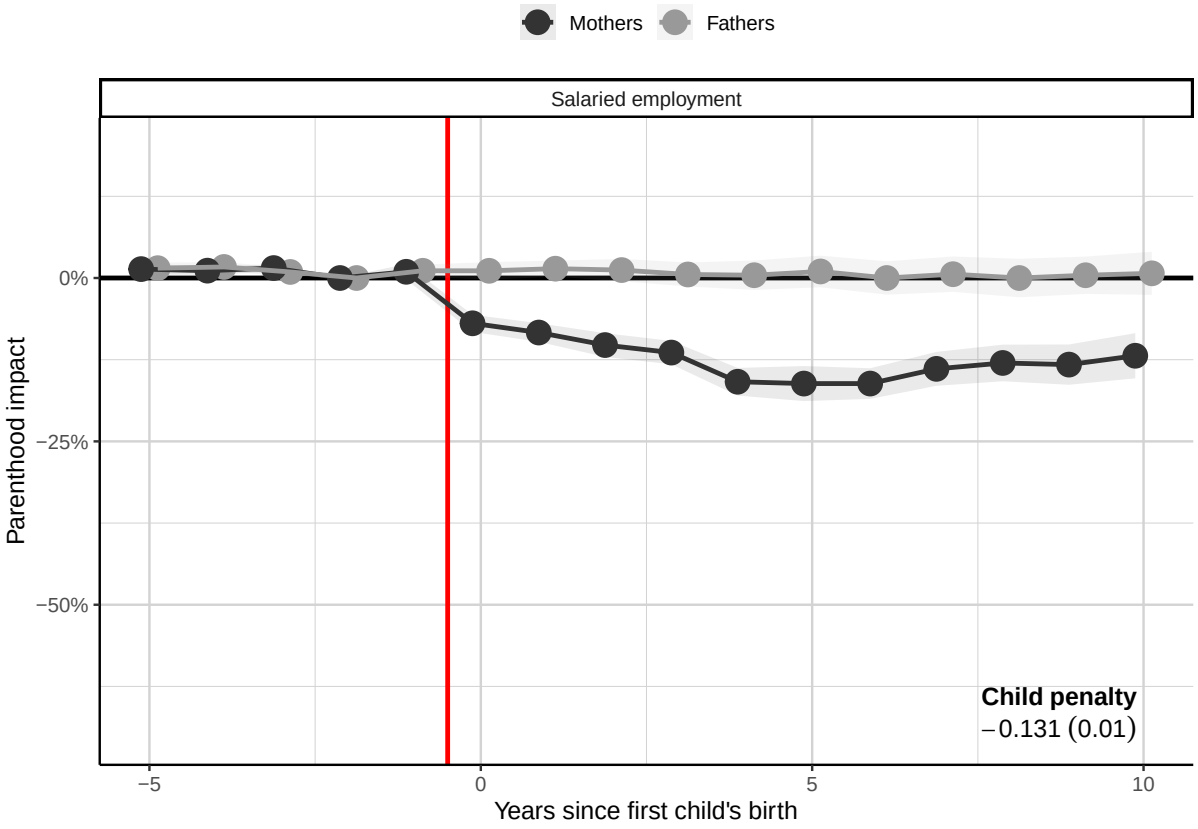
Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual earnings level. The child penalty is the difference in relative parenthood impact between men and women, averaged over the first 10 years after their first child is born. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

We find that our estimates are remarkably similar to theirs, with a roughly 30% decline in earnings and a 15% decline in salaried employment due to motherhood.

Figure C.3. Child penalty in salaried employment for immigrant parents: estimates from administrative registers

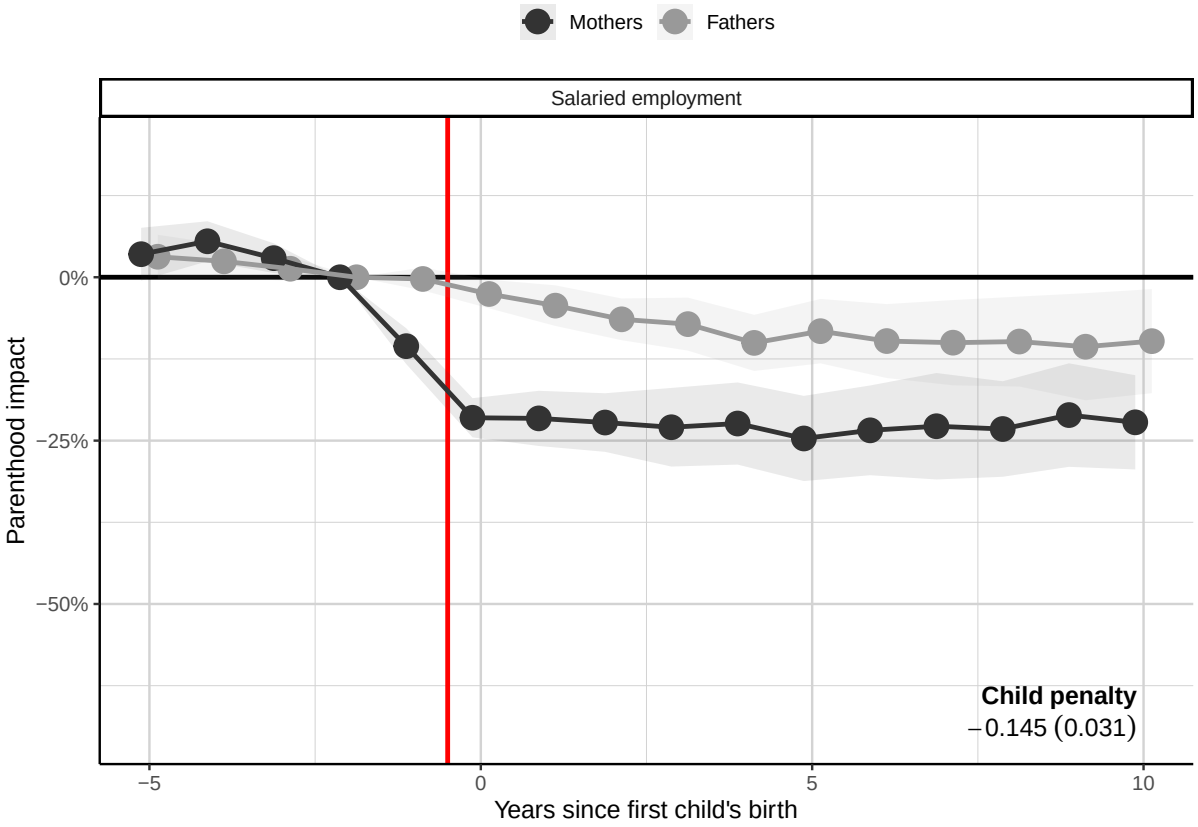


Parenthood impact is identified from comparisons between parents of the same cohort (year of birth) who got their first salaried job at the same time, but had their first child at different ages (at least two years after their first salaried job), and displayed relative to the counterfactual salaried employment rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

Figure C.4. Child penalty in salaried employment for immigrant parents: estimates from survey data



Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual salaried employment rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

D Couple penalties vs. child penalties

D.1 Method

Our approach extends the staggered event-study design to accommodate cases where individuals undergo two treatments. We build on the same foundational elements used in our main empirical analysis.

D.1.1 Model

We keep the same notations as in the core of the paper. Let $Y_{i,t}$ denote the relevant labor outcome – e.g., labor earnings or labor market participation – of individual i at age t. Let $G_i \in \{f, m\}$ denote individual i's gender and B_i her generation (year of birth).[16] Finally, let R_i (respectively C_i) denote her age when she began her first stable relationship (respectively when her first child was born), with $R_i = \infty$ if she never had a stable relationship.

Let $Y_{i,t}(r, c)$ denote the potential labor supply of individual i at time t had she (i) started her first stable relationship at age r; and (ii) given birth to her first child at age c. Consistently, $Y_{i,t}(\infty, \infty)$ represents her labor supply at time t had she chosen (i) never to enter a relationship; and (ii) to remain childless. By construction:

$$Y_{i,t} = Y_{i,t}(\infty, \infty) + \sum_{r,c} (Y_{i,t}(r, c) - Y_{i,t}(\infty, \infty)) \mathbb{1}\{R_i = r, C_i = c\} \tag{9}$$

Because there are two treatments here, the definition of treatment effects, and thus that of the quantities of interest, is not as straightforward as when only parenthood is considered. Specifically, for any individual i and time t, $Y_{i,t}(r, c) - Y_{i,t}(\infty, c)$ represents the partial effect of living as a couple for $t - r$ years, while holding parenthood status constant, whereas $Y_{i,t}(r, c) - Y_{i,t}(r, \infty)$ corresponds to the causal effect of having been a parent for $t - c$ years, while holding relationship status constant. In practice, we define two sets of ATTs:

$$CATT^{R}_{g,b,r,c,t} = \mathbb{E}[Y_{i,t}(r, c) - Y_{i,t}(\infty, c) \mid G_i = g, B_i = b, R_i = r, C_i = c] \tag{10}$$

$$CATT^{C}_{g,b,r,c,t} = \mathbb{E}[Y_{i,t}(r, c) - Y_{i,t}(r, \infty) \mid G_i = g, B_i = b, R_i = r, C_i = c] \tag{11}$$

Here, $CATT^{R}_{g,b,r,c,t}$ denotes the average impact of relationships for a group of individuals defined by the ages at which they began their first relationship and had their first child, while $CATT^{C}_{g,b,r,c,t}$ denotes the average impact of parenthood for the same group.

Our analysis relies on two key assumptions: (i) a parallel trends assumption and (ii) a limited anticipation assumption.

Assumption 3 (Parallel trends in baseline outcomes: relationships) *For all g, b, (t, t'), c, and (r, r'), where $c, c' > 1$ and $c, c' < \infty$, we assume:*

$$\begin{aligned} & \mathbb{E}[Y_{i,t}(\infty, c) - Y_{i,t'}(\infty, c) \mid G_i = g, B_i = b, R_i = r, C_i = c] \\ = \; & \mathbb{E}[Y_{i,t}(\infty, c) - Y_{i,t'}(\infty, c) \mid G_i = g, B_i = b, R_i = r', C_i = c] \end{aligned} \tag{12}$$

Assumption 4 (Parallel trends in baseline outcomes: children) *For all g, b, (t, t'), r, and (c, c'), where $c, c' > 1$ and $c, c' < \infty$, we assume:*

$$\begin{aligned} & \mathbb{E}[Y_{i,t}(r, \infty) - Y_{i,t'}(r, \infty) \mid G_i = g, B_i = b, R_i = r, C_i = c] \\ = \; & \mathbb{E}[Y_{i,t}(r, \infty) - Y_{i,t'}(r, \infty) \mid G_i = g, B_i = b, R_i = r, C_i = c'] \end{aligned} \tag{13}$$

[16] We further discuss the consequences of comparing individuals within their own generation in Section 3.1.3.

Assumption 5 (Limited anticipation: relationships) *For all t, g, b, r, and c, if $t < r - 1$, then:*

$$\mathbb{E}[Y_{i,t}(r,c) - Y_{i,t}(\infty,c) \mid G_i = g,\, B_i = b,\, R_i = r,\, C_i = c] = 0 \tag{14}$$

Assumption 6 (Limited anticipation: children) *For all t, g, b, r, and c, if $t < r - 1$, then:*

$$\mathbb{E}[Y_{i,t}(r,c) - Y_{i,t}(r,\infty) \mid G_i = g,\, B_i = b,\, R_i = r,\, C_i = c] = 0 \tag{15}$$

Assumption 3 implies that, in the absence of stable relationships, the average labor outcomes for individuals of the same gender and birth cohort, and who have children at the same age, would evolve in parallel over time. Assumption 4 states that, in the absence of children, the average labor outcomes for individuals of the same gender and birth cohort, and who began their first relationship at the same age, would evolve in parallel over time. Assumption 5 (resp. 6) assumes that the effect of stable relationships (resp. parenthood) on labor outcomes is zero up to two years before the beginning of the first stable relationship (resp. the birth of the first child).

Taken together, these assumptions imply testable restrictions on the data. First, they suggest that before both couple formation and parenthood, average labor outcomes for all (r,c)-cohorts should evolve in parallel. This restriction is similar to the usual "no pre-trend" assumption in difference-in-differences approaches with only one treatment. Secondly, they imply that, before the first child is born, the average impact of relationships should be the same across future parents who began their first relationship at the same age but are expected to have their first child at different ages. They may, however, have different baseline levels.

If the first stable relationship closely follows the first stable job (Landaud, 2021), then Assumption 3 becomes implausible because the sorting of individuals based on the timing of their first relationship is strongly correlated with the trajectory of their potential labor outcomes. In this case, our estimates of the impact of relationships are likely to be biased upwards. One possibility is to consider only the differences in impact between men and women, under the additional assumption that the bias is the same for both genders. However, this approach may fail if the dynamic sorting is, for example, stronger for men than for women, as suggested by Landaud (2021). In that case, our estimates based on gender differences would overestimate the detrimental impact of relationships on women's labor supply.

D.1.2 Identification

Under these assumptions, if there is sufficient variation in the timing of the first stable relationship within each gender × cohort (year of birth) × entry into parenthood group, the group-specific average effects of stable relationships can be inferred from the data:

Proposition 2 (Difference-in-differences estimand: relationships) *For all (g,r,c,t), with $1 < r < \infty$ and $1 < c < \infty$, the average treatment effect is given by:*

$$\begin{aligned} CATT^{R}_{g,b,r,c,t} \;=\; & \mathbb{E}[Y_{i,t} \mid G_i = g,\, B_i = b,\, R_i = r,\, C_i = c] \\ & -\mathbb{E}[Y_{i,r-2} \mid G_i = g,\, B_i = b,\, R_i = r,\, C_i = c] \\ & -\mathbb{E}[Y_{i,t} \mid G_i = g,\, B_i = b,\, \max(1,\, r-2\,, t+1) < R_i < \infty,\, C_i = c] \\ & +\mathbb{E}[Y_{i,r-2} \mid G_i = g,\, B_i = b,\, \max(1,\, r-2,\, t+1) < R_i < \infty,\, C_i = c] \end{aligned} \tag{16}$$

Conversely, if there is sufficient variation in the arrival of children conditional on when the first relationship begins, group-specific average effects of parenthood can be inferred from the data:

Proposition 3 (Difference-in-differences estimand: children) *For all* (g, r, c, t)*, with* $1 < r < \infty$ *and* $1 < c < \infty$*, the average treatment effect is given by:*

$$\begin{aligned} CATT^{C}_{g,b,r,c,t} \quad = \quad & \mathbb{E}[Y_{i,t} \mid G_i = g,\, B_i = b,\, R_i = r,\, C_i = c] \\ & -\mathbb{E}[Y_{i,c-2} \mid G_i = g,\, B_i = b,\, R_i = r,\, C_i = c] \\ & -\mathbb{E}[Y_{i,t} \mid G_i = g,\, B_i = b,\, R_i = r,\, \max(1,\, c-2\,, t+1) < C_i < \infty] \\ & +\mathbb{E}[Y_{i,c-2} \mid G_i = g,\, B_i = b,\, R_i = r,\, \max(1,\, c-2\,, t+1) < C_i < \infty] \end{aligned} \tag{17}$$

D.1.3 Estimation

These identification results suggest a simple plug-in estimator, where expectations are replaced with their empirical counterparts. In practice, we aggregate the impact over time relative to events in the same way as in our baseline approach. Standard errors are computed using the same reweighted bootstrap method as in our baseline estimates, with clusters defined at the individual level.

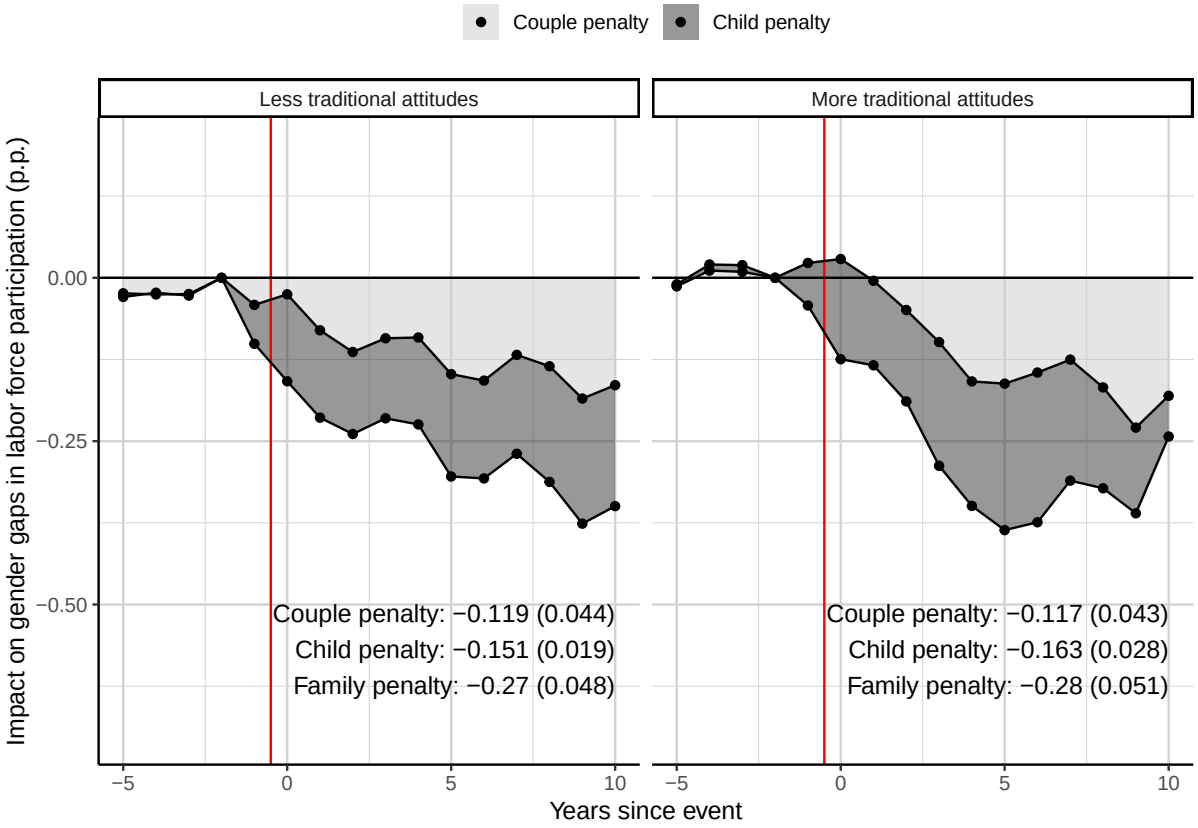
We operationalize the age at which individuals began their first relationship by considering when the relationship started, rather than (i) marriage or (ii) the moment they began living with their first partner. The reason for this choice is that (i) in France, most children are now born outside of marriage; and (ii) cohabitation is more likely to be tied to future plans regarding parenthood than the beginning of a relationship.

D.2 Results

Figure D.1 presents our results on couple and child penalties, jointly estimated by gender-related attitudes. It displays estimates based on comparisons between men and women, with impacts measured in absolute values (percentage points). While the dynamics of the effects differ slightly across attitude groups, the aggregate impacts of both relationships and children on gender gaps in labor force participation do not appear to depend on views regarding gender inequality and women's bodily autonomy. The couple penalty is notably large, especially compared to available estimates of marriage penalties at advanced stages of development, as presented by Kleven et al. (2024a). This suggests that the bias resulting from the stronger causal impact of stable jobs on stable relationships for men compared to women may lead to an overestimation of the couple penalty.

Figure D.2 builds on these results to decompose the average gender gap in labor force participation between ages 20 and 50, into (i) one component stemming from the impact of relationships, (ii) one component resulting from the child penalty, and (iii) a residual gap unrelated to family events. For both attitude groups, family events explain more than 100% of the gender gap in labor force participation, implying that the residual gap is negative. In other words, in the absence of relationships and children, our estimates suggest that women would actually participate more in the labor market than men. The contributions of family events to the gender gap in labor force participation are quite similar across attitude groups, although the contribution of the child penalty is slightly smaller for less traditional immigrants, who are more likely to have children later in their lives. Consistent with our baseline estimates, we find that the larger gender gap in labor force participation for immigrant parents with more traditional views regarding gender is mainly due to the residual gap, which is strongly correlated with gender-related attitudes and unrelated to both relationships and parenthood.

Figure D.1. Child penalty in labor market participation for immigrant parents: by self-reported attitudes, balanced on pre-birth characteristics and fertility decisions

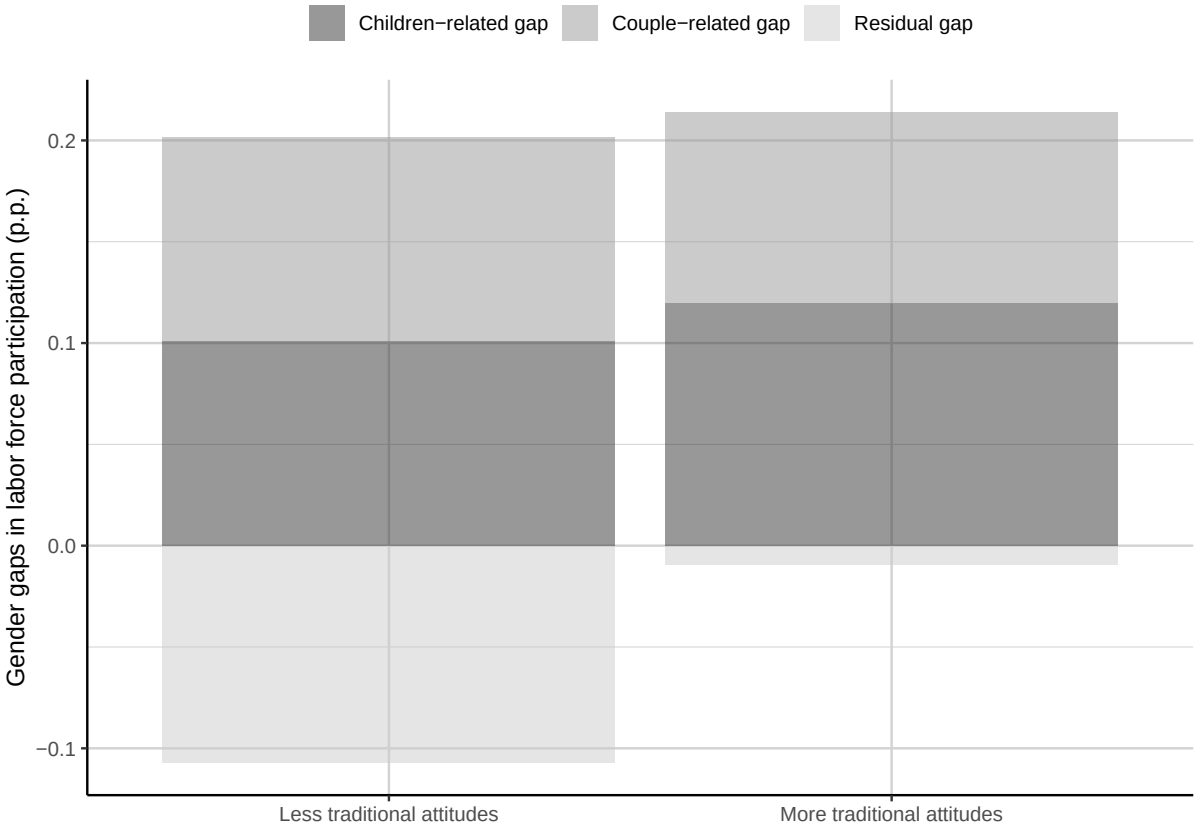


Couple (parenthood) impact is identified from comparisons between parents of the same decennial cohort and who had their first child at the same age (began their first relationship at the same age), but began their first relationship (had their first child) at different ages, and displayed in its absolute value. The couple (child) penalty is the difference in couple impact between men and women, averaged over the first 10 years after their first child is born. The family penalty is the sum of both these penalties. The data are reweighted based on an inverse propensity score approach so as to make attitudes groups similar in terms of pre-birth observables characteristics and fertility decisions. Standard errors are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure D.2. Gender gaps in labor force participation: contribution of the couple and the child penalties, by gender-related attitudes



Average labor force participation gaps from age 20 to age 50 for immigrant parents, by gender-related attitudes. This figure displays the contribution of the couple penalty and the child penalty to the gender gap. The contribution of the couple (child) penalty is the magnitude of the couple (child) penalty in labor force participation multiplied by the prevalence of stable relationship (parenthood). The residual gap is the difference between the realized gaps the sum of both these contributions. Couple (parenthood) impact is identified from comparisons between parents of the same decennial cohort and who had their first child at the same age (began their first relationship at the same age), but began their first relationship (had their first child) at different ages. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

E Synthesizing the information from the EDP registers

E.1 Countries of origin

A synthetic variable representing the country of birth is available for every EDP individual. This allows us to identify first-generation immigrants. Identifying second-generation immigrants is more challenging, as no synthetic variable is available for the entire sample regarding *ego*'s parents' countries of birth. A natural solution would be to rely on birth certificate data. However, this solution is not straightforward because (i) no birth certificate data are available for individuals born before 1967 (see Figure E.1); (ii) no birth certificate data are available for individuals born in January, April, or July before 2004 (see Figure E.2); (iii) a large portion of the birth certificate data is missing for those born in October during the 1980s and 1990s (see Figure E.3); and (iv) even when birth certificate data are available, the information regarding parents' country of birth is missing for many individuals born in the 1980s and 1990s (see panel a of Figure E.4).

When birth certificate information is unavailable, we develop a solution based on census data. Specifically, we rely on data from the 1990 and 1999 comprehensive censuses. This data are only available for individuals born in October, so we discard data for those born in January, April, and July. A key limitation is that the census form did not collect information on parents' countries of birth. Instead, we rely on the family delineation performed by Insee after the census collection. This delineation is based on whether people live in the same dwelling, using information collected about family links between cohabitants. As a result, we determine *ego*'s parents' countries of birth based on the assumption that *ego* was observed living with her parents as a child. An additional limitation is that the census did not record all possible family links within a dwelling. Specifically, the family reconstruction assumes that when an adult self-reported as a parent of a child living with her, her partner was considered the other parent. Although this assumption is imperfect, we proceed with it. Ultimately, our approach relies on:

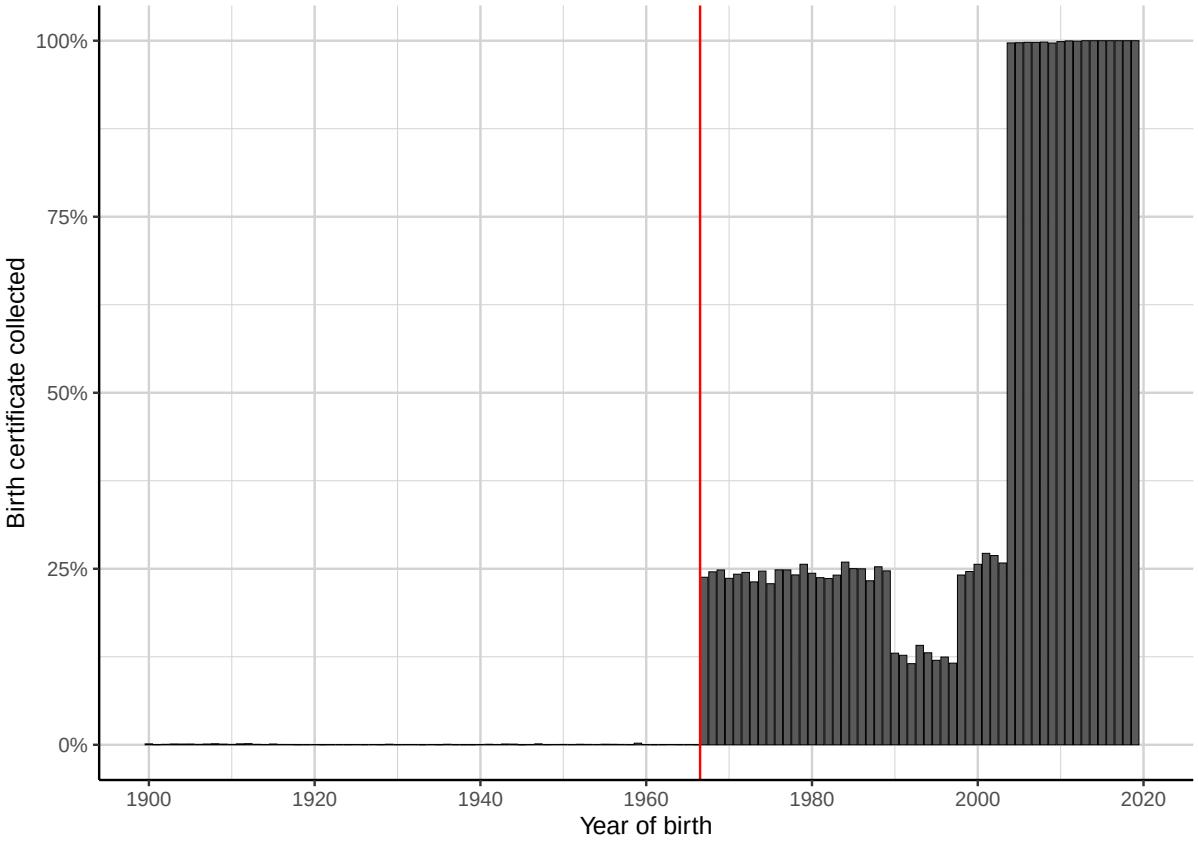
1. *ego*'s parents' countries of birth from the birth certificate data, when available;
2. self-reported countries of birth of adults identified as parents in the 1990 comprehensive census, based on *ego* being determined a child in the family;
3. self-reported countries of birth of adults identified as parents in the 1999 comprehensive census, based on *ego* being determined a child in the family;

The rationale for this choice is that the older the observation date in the census data, the higher the likelihood that the adults identified as *ego*'s parents are indeed her biological parents. This approach enables us to recover a large portion of the missing data: for the most affected cohorts, the missing data rate decreases from over 50% to approximately 10% (see Figure E.4). We further validate the quality of this information by cross-checking it with Insee's countries database, confirming that in the vast majority of cases, recovered information corresponds to an actual country (see Figure E.5).

E.2 Migration outflows

Accurately computing the employment-to-population ratio to identify the child penalty requires an accurate denominator, namely knowing whether an individual currently resides in France. The reason for this is that in the labor market data, only employed individuals are observed. To address this issue, we rely on an approach proposed by Caron and Reeve (2018) and Solignac and Dutreuilh (2018), which combines two data sources that are independent of employment status and form part of the EDP data: census data and income tax returns data. Specifically, we compute for each individual in the sample the first and last times they were observed in either the census data or income tax returns. Because yearly

Figure E.1. Missing birth certificates in the EDP data: by year of birth

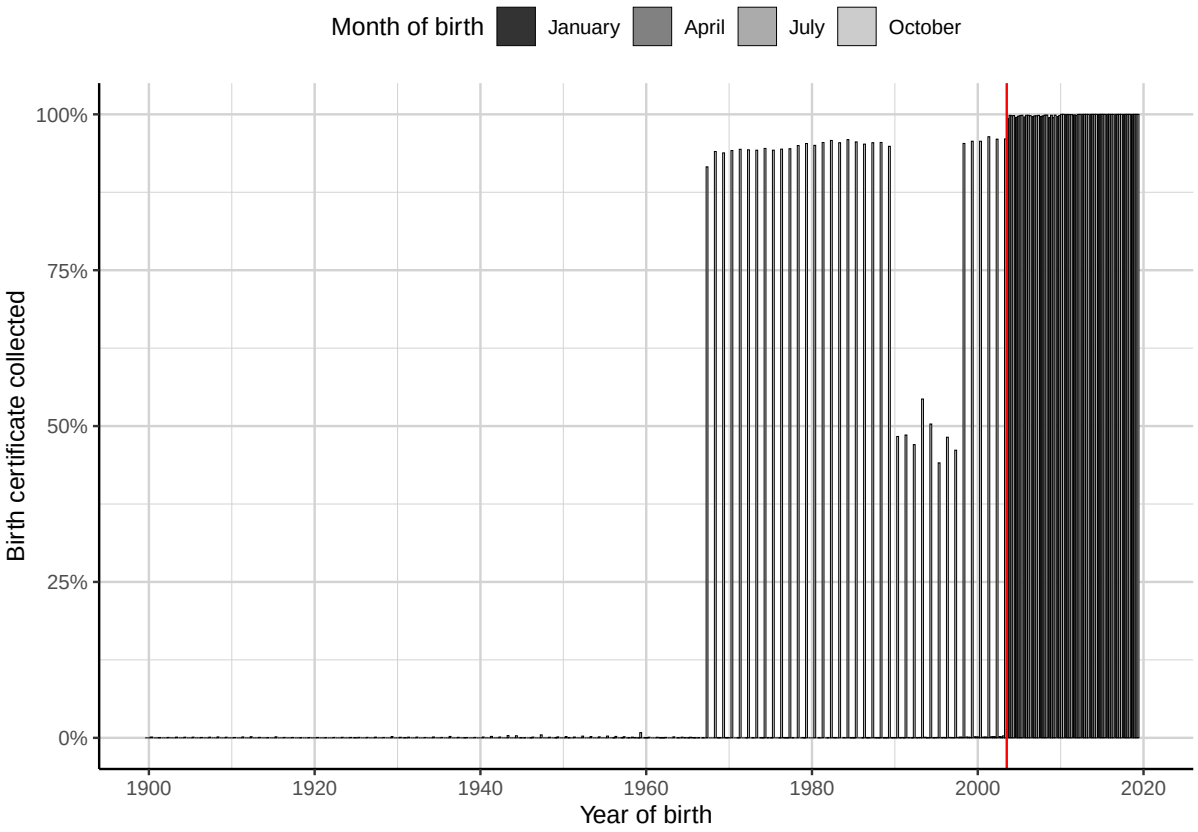


Share of EDP individuals who were born in France of whom the birth certificate is available in the data, by year of birth.

Population. Individuals who were born in France and sampled in the EDP data.

Source. CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

Figure E.2. Missing birth certificates in the EDP data: by month of birth

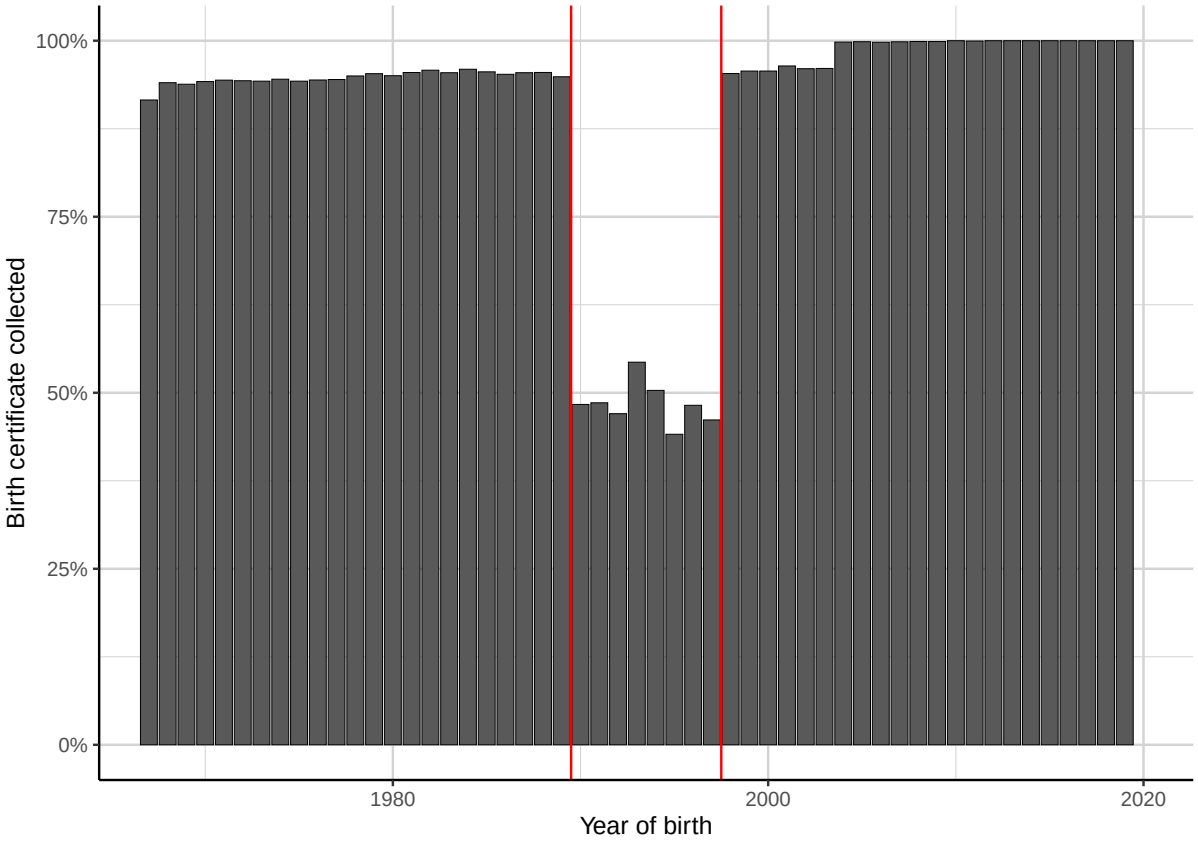


Share of EDP individuals who were born in France of whom the birth certificate is available in the data, by month of birth.

Population. Individuals sampled in the EDP data.

Source. CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

Figure E.3. Missing birth certificates in the EDP data: individuals born Oct, 1st to 4th



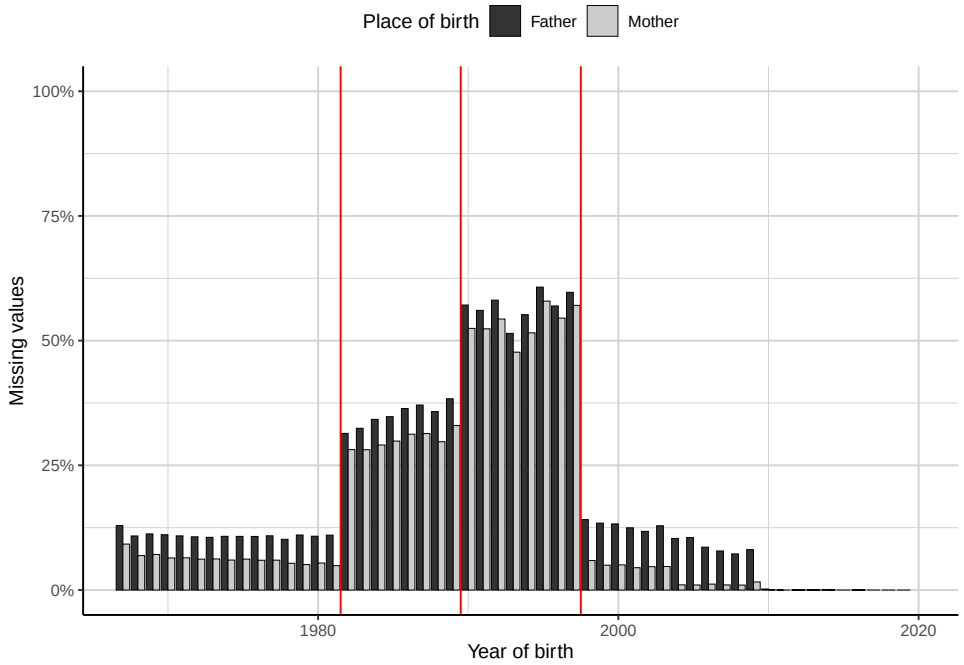
Share of EDP individuals who were born in France of whom the birth certificate is available in the data, by year of birth.

Population. Individuals born on October 1st to 4th and sampled in the EDP data.

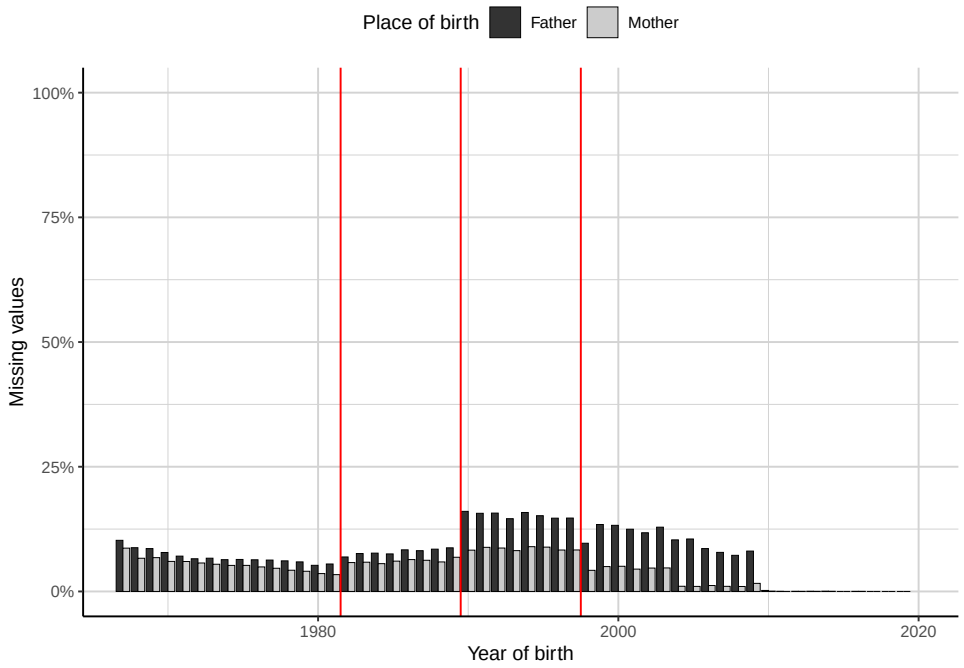
Source. CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

Figure E.4. Recollection of the information on parents' country of birth in the EDP data

(a) Before recollection



(b) After recollection

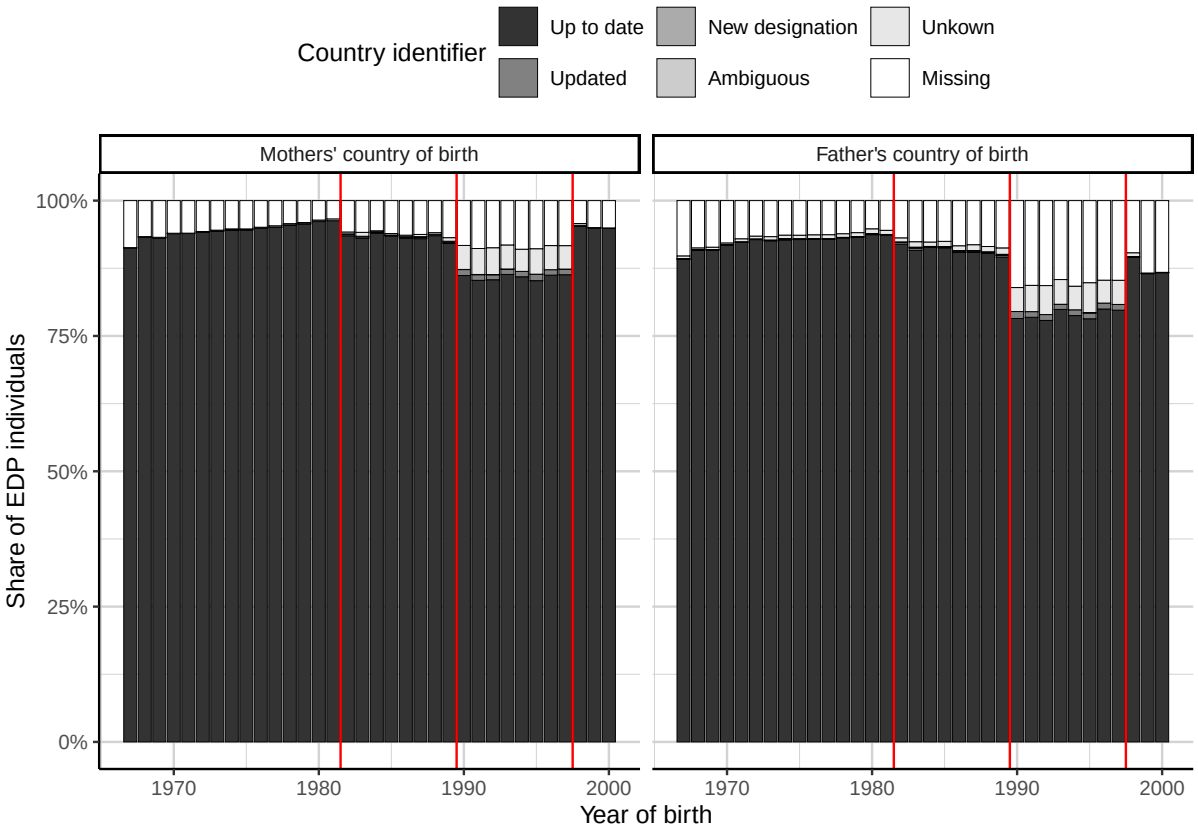


Share of EDP individuals who were born in France of whom parents' countries of birth is known, before and after recollection from comprehensive census data.

Population. Individuals born on October 1st to 4th and sampled in the EDP data.

Source. CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

Figure E.5. Quality of the recollected information on parents' country of birth in the EDP data



Assessment of the information regarding EDP individuals' parents' country of birth against Insee countries database.

Population. Individuals born on October 1st to 4th and sampled in the EDP data.

Source. CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

income tax data were not collected in the EDP before 2011, this method does not provide year-by-year indicators of French residence. For this reason, we exclude issues related to temporary emigration.

The share of individuals sampled in the EDP who were never observed in either the census data or income tax returns is small for those born in France: it is below 1% for individuals whose both parents were also born in France, and below 2% for those with at least one foreign-born parent. It is larger for individuals born outside France, amounting to 12.6%.

For individuals born in France, the first observation is usually the first census after their birth (see Figure E.6), given that we focus on people born between 1967 and 1990. For those born outside France, the first observation occurs later. The last observation is usually the last available year for income tax returns in the EDP data (2019). For individuals born outside France, this observation occurs earlier, in line with the literature, which has found that immigrants are more likely to leave France than natives.

E.3 Fertility decisions

The most straightforward way to recover information about fertility events in the EDP data would be to use the birth certificate data. However, as the previous discussion highlights, this solution is not simple, as the birth certificate data are incomplete for part of the sample, particularly for children born in the 1980s and 1990s. Since we focus on potential parents born between 1967 and 1990, our analysis may underestimate the fertility of these individuals. Additionally, first-generation immigrants may have children born outside France, who are not covered by the birth certificate data.

We address this issue by relying on census data and income tax returns. Specifically, we add children already observed in the birth certificate data with:

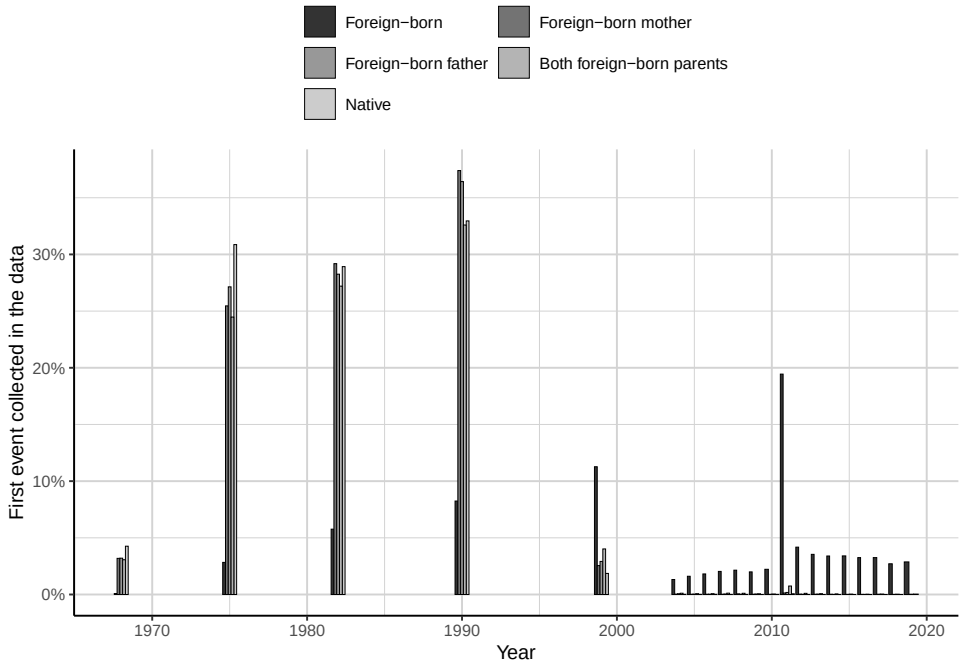
- children living in families (as defined in the census data, see E.1) where EDP individuals were identified as parents in the 1990 comprehensive census;
- children living in families (as defined in the census data, see E.1) where EDP individuals were identified as parents in the 1999 comprehensive census;
- dependent children listed in EDP individuals' income tax returns.

To ensure that we do not count the same children twice and to avoid categorizing as *ego*'s children those who are merely living with *ego*, we exclude from the count:

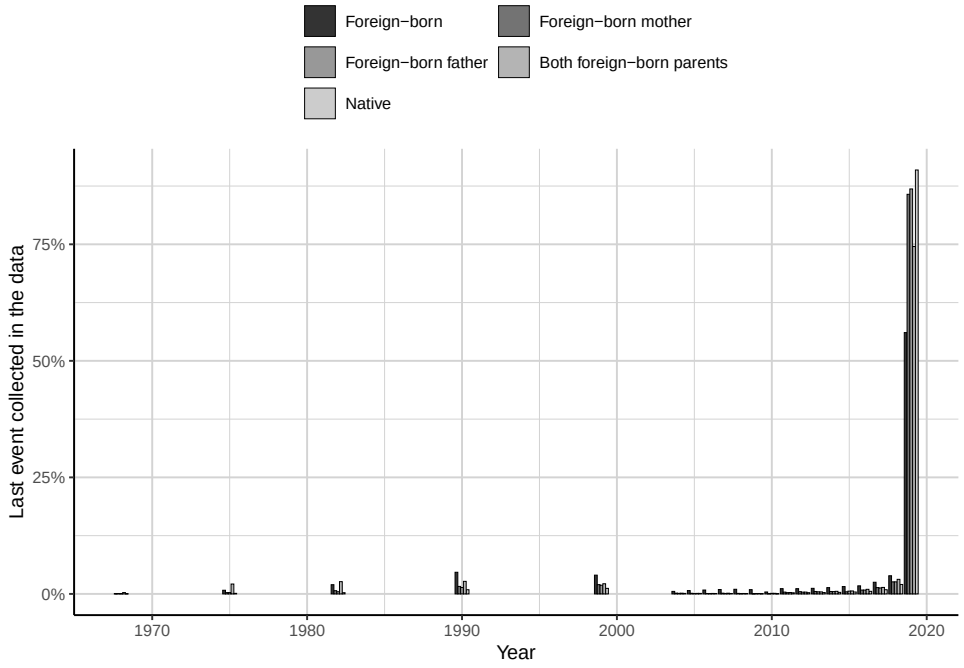
- children who only appear in the census data or income tax returns for individuals born on Oct 1^{st} or 4^{th} in France, since, assuming they did not leave France, the birth certificate data should be comprehensive for their children;
- children who only appear in the census data or income tax returns and were born either before 1982 or after 1997 for individuals born on Oct 2^{nd} or 3^{rd} in France, since, assuming they did not leave France, the birth certificate data should be comprehensive for these children;
- children who only appear in the census data or income tax returns and were born after their parents' arrival in France for individuals born on Oct 1^{st} or 4^{th} outside France, since, assuming they did not leave France, the birth certificate data should be comprehensive for these children;
- children who only appear in the census data or income tax returns and were born after their parents' arrival in France and outside the 1982-1997 time period for individuals born on Oct 1^{st} or 4^{th} outside France, since, assuming they did not leave France, the birth certificate data should be comprehensive for these children;
- children born less than 15 years after their potential parent.

Figure E.6. Inferring migration flows from the EDP data

(a) Inflows



(b) Outflows



Year of first and last appearance in EDP data, by migration status.

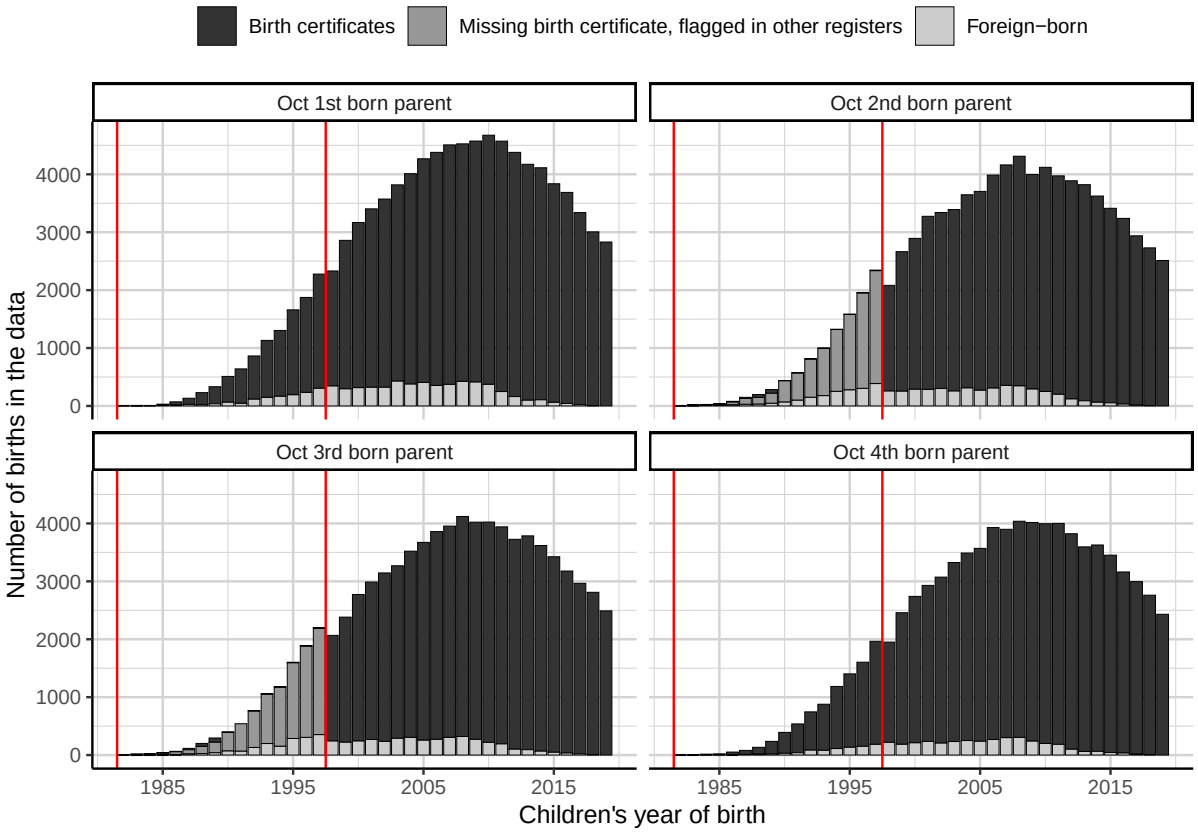
Population. Individuals born on October 1st to 4th between 1967 and 1990 and sampled in the EDP data.

Source. CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

To handle multiple births while avoiding double-counting the same child, we assume that two children born in the same year to the same EDP individual are the same child, unless both are observed in the same data source.

Ultimately, this approach enables us to fill the gap caused by the missing birth certificate data and allows us to identify the children of first-generation immigrants who were born before their arrival in France (see Figure E.7). When comparing individuals born on October 2nd or 3rd, for whom the birth certificate data are not corrupted, with their counterparts born on October 1st to 4th, for whom the birth certificate data are corrupted, we obtain very similar estimates regarding fertility decisions. There is a possibility that we slightly overestimate fertility before 1997 in the former group.

Figure E.7. Recollection of missing children in the EDP data



Number of childbirths in the EDP data, by year of birth of the child and day of birth of her EDP parent.

Population. Individuals born on October 1st to 4th between 1967 and 1990 and sampled in the EDP data.

Source. CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

E.4 Labor market outcomes

E.4.1 Earnings and working time measures

Earnings Our measure of labor earnings is based on net annual earnings. This measure aggregates all wages paid to an individual, including performance pay, bonuses, paid vacations, in-kind benefits, severance payments exceeding the legal minimum, and early retirement benefits (to the extent that these benefits exceed an amount approximately equal to the minimum wage), but excludes stock options. Social security contributions, public pension schemes, unemployment benefits, and other contributions, including two flat-rate taxes on earned income (CSG and CRDS), are subtracted from this amount to

compute our measure of net annual earnings. In this sense, we measure earnings before income taxes, but after some transfers.

Maternity leave allowances are paid by the Social Security administration and are therefore not part of our measure of earnings. However, they may be paid through the employer (*subrogation*): in this case, the employer pays the employee the equivalent of maternity leave allowances during her maternity leave and is later reimbursed by the Social Security administration. The employer then subtracts the maternity leave allowances that were advanced from the measure of earnings. Because the reimbursement occurs after the maternity leave, the decline in earnings may occur a few weeks later. Since we consider annual earnings, this issue only affects childbirths that occur at the end of the calendar year.

Lastly, in some firms the employer may be bound by collective agreement to complement earnings during maternity or sick leaves in addition to Social Security-provided allowances. This complement is part of labor earnings as measured by the DADS.

Days In the DADS dataset, days worked refer to the duration during which an employee is part of a firm's workforce within a given year. As a result, maternity and sick leaves, as well as paid vacations, are included in this measure of days, while periods of unemployment between two distinct employment spells are not. Additionally, this measure of days is capped at 360.

Hours In the DADS dataset, hours worked refers to the hours for which the worker is paid under their labor contract. The data on hours are reported by employers when they complete payroll tax forms. Before making the data available, Insee performs three checks:

- The total number of hours for a given individual × employer × year observation should not exceed an industry-specific threshold of 2,500 hours per year in a small subset of industries (mostly manufacturing, transportation, and hotels and restaurants), and 2,200 hours per year elsewhere;
- The implied hourly wages should exceed 80% of the minimum wage;
- The total number of hours should be positive, with the exception of a narrow subset of occupations (mostly journalists and salespersons) working on a fixed-price or commission basis.

If one of these conditions is not met, Insee assigns hours to the observation to make the hourly wage consistent within narrow cells defined by 4-digit occupation, full-time or part-time status, age, and gender.

For workers whose pay does not depend on the time worked but who do not belong to any of the above-mentioned occupations (i.e., typically highly qualified personnel working on a "day rate" (*"forfait-jour"*)), employers provide the number of days only. A number of hours is then ascribed to these observations based on the legal working hours of full-time workers, the number of workdays, and the implied hourly wages.

During maternity leave, since an employee is not paid for any hours by her employer but is instead paid by Social Security (and may receive a top-up payment from her employer), hours worked are equal to 0. Workers not paid by the hour are an exception to this rule because their hours are imputed based on days paid, which do not vary during maternity leave. As a result, the DADS dataset overestimates hours paid—and underestimates hourly wages—for such workers during years when they give birth to children. In general, these workers belong to the "Manager and Professionals" occupation group, so this is not a concern for this particular paper.

E.4.2 Measurement issues

Full-time units computation Hours worked were not collected before 1993, and data quality is poor before 1995. Additionally, for central State civil servants, these data were not collected before 2009.

However, for these workers, a measure of working time expressed in full-time units (FTU) is available between 1995 and 2009. Furthermore, a qualitative variable (full-time vs. part-time worker) is available from the beginning of our time period of interest (1988).

We reconcile all this information and address missing data through the computation of an FTU measure:

- This measure equals 1 for individuals working full-time full years;
- This measure equals the number of days worked divided by 360 for full-time part-year workers;
- For part-time workers:
  - As of 1995, this measure equals either their FTU working-time for central State employees or their total hours worked for other employers, divided by the median hours worked by full-time full-year workers the same year (2,028 hours before 1999, 1,820 after 2002, with an intermediary period corresponding to changes in the legal working time);
  - Before 1995, it is equal to days worked, divided by 360, multiplied by the median normalized hours-to-days ratio (between 0 and 1) for part-time workers in 1995;
  - We also use this imputation for rare observations with missing hours data after 1995.

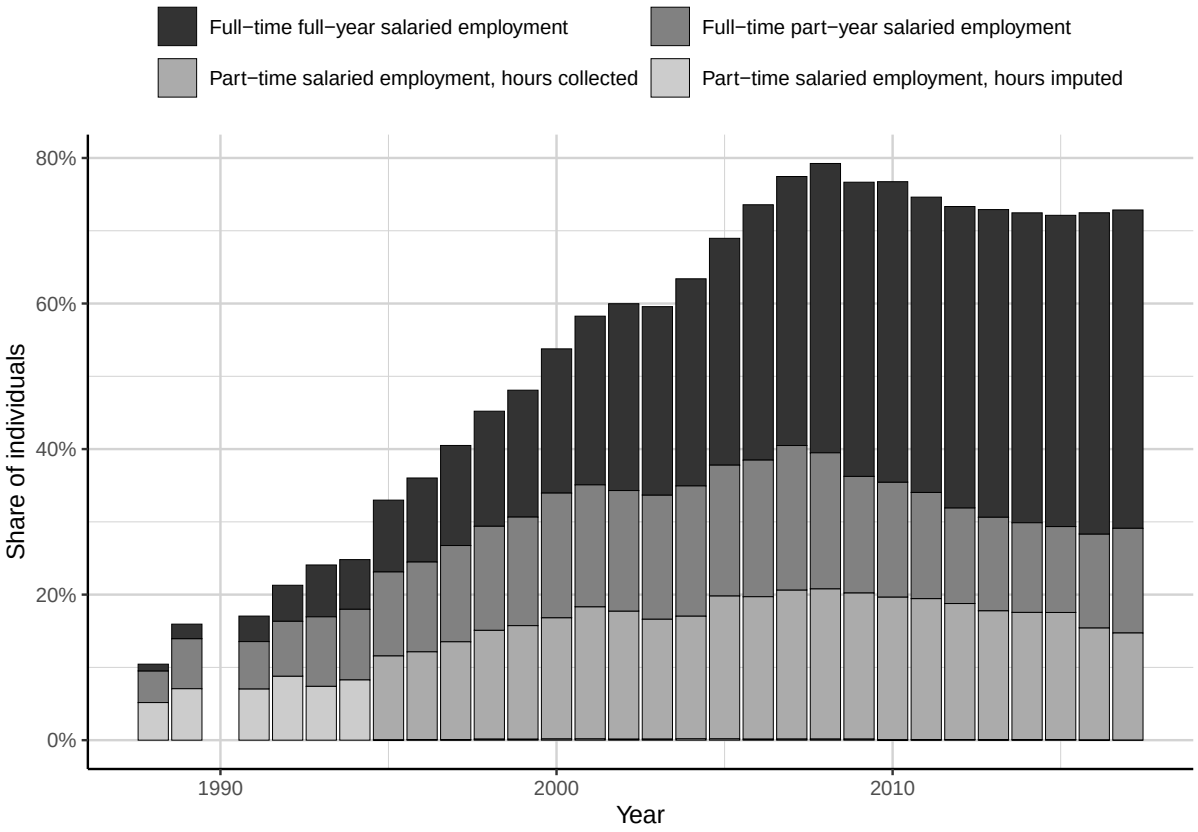
In practice, given that our sample consists of individuals born between 1967 and 1990, the imputation procedure used before 1995 is only relevant for a small portion of their labor market histories (see Figure E.8). Based on this measure, we now employ an accounting decomposition of labor earnings:

$$Y_{it} = D_{it} X_{it} H_{it} W_{it} \tag{18}$$

where Y_{it} represents overall yearly labor earnings (including 0 for individuals not in salaried employment), D_{it} is a dummy variable indicating whether the individual held salaried employment during year t, X_{it} represents days worked divided by 360 (which approximates average weekly employment over the year), H_{it} is the normalized hours-to-day ratio (relative to a full-time worker), and W_{it} is the FTU wage. Our FTU measure is the product of the first three components. Figure E.9 shows the corresponding labor supply time series. Consistent with the fact that the oldest individuals in our sample were 21 in 1988, their labor supply increases over time, primarily because they are more likely to hold salaried jobs.

Equipped with this FTU measure, we can now consider FTU wages. Figure E.10 displays the corresponding time series. The raw time series is noisy, resulting from observations related to jobs with very low paid hours. Winsorizing the FTU wage at the 99th percentile level seems an appropriate way to address this issue. In the end, we use the winsorized hourly wages to compute labor earnings based on 18. This changes the corresponding time series profile only marginally (see Figure E.11). This is our final measure of individual earnings, upon which our estimates of the child penalty are based.

Figure E.8. Working time status in the EDP data

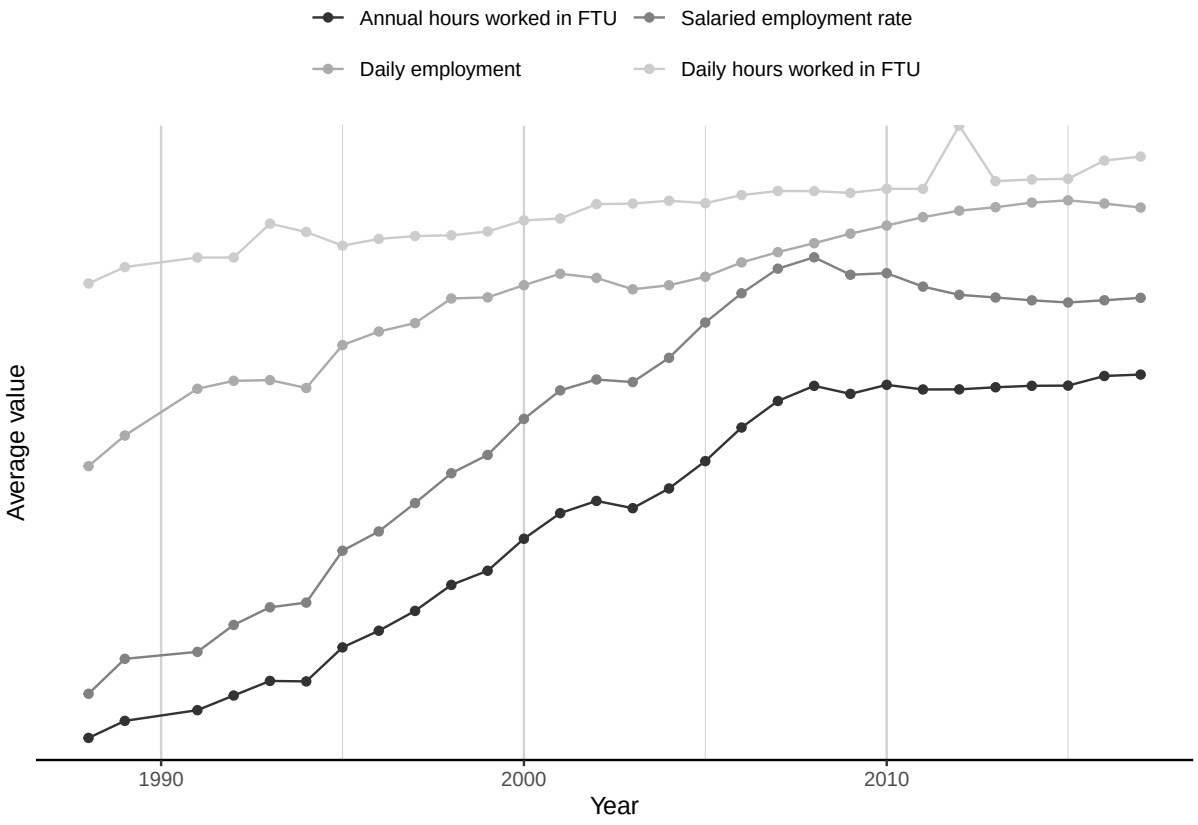


Working time status and imputation of FTU hours for EDP individuals, by year of salaried employment.

Population. Individuals born on October 1st to 4th between 1967 and 1990 and sampled in the EDP data.

Source. CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

Figure E.9. Labor supply measures in the EDP data

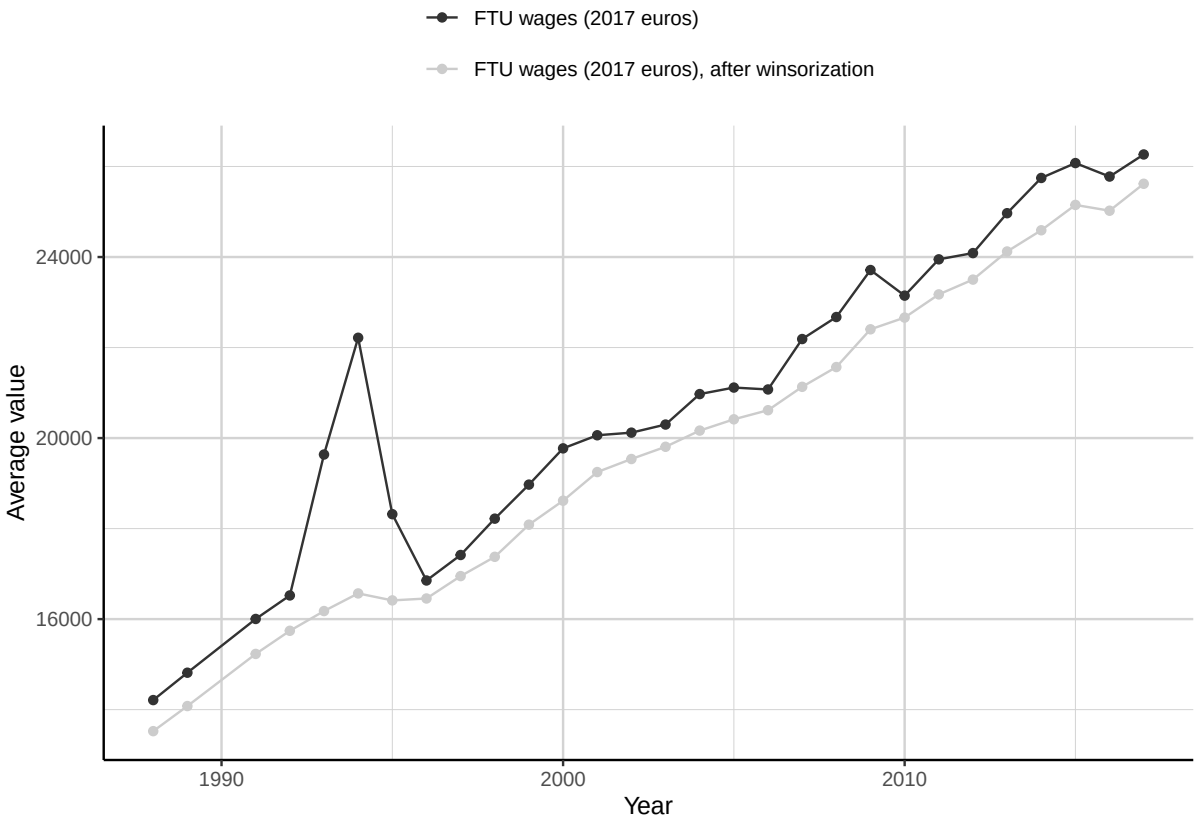


Decomposition of annual salaried FTU hours into three margins, by year.

Population. Individuals born on October 1st to 4th between 1967 and 1990 and sampled in the EDP data.

Source. CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

Figure E.10. Hourly wages in the EDP data

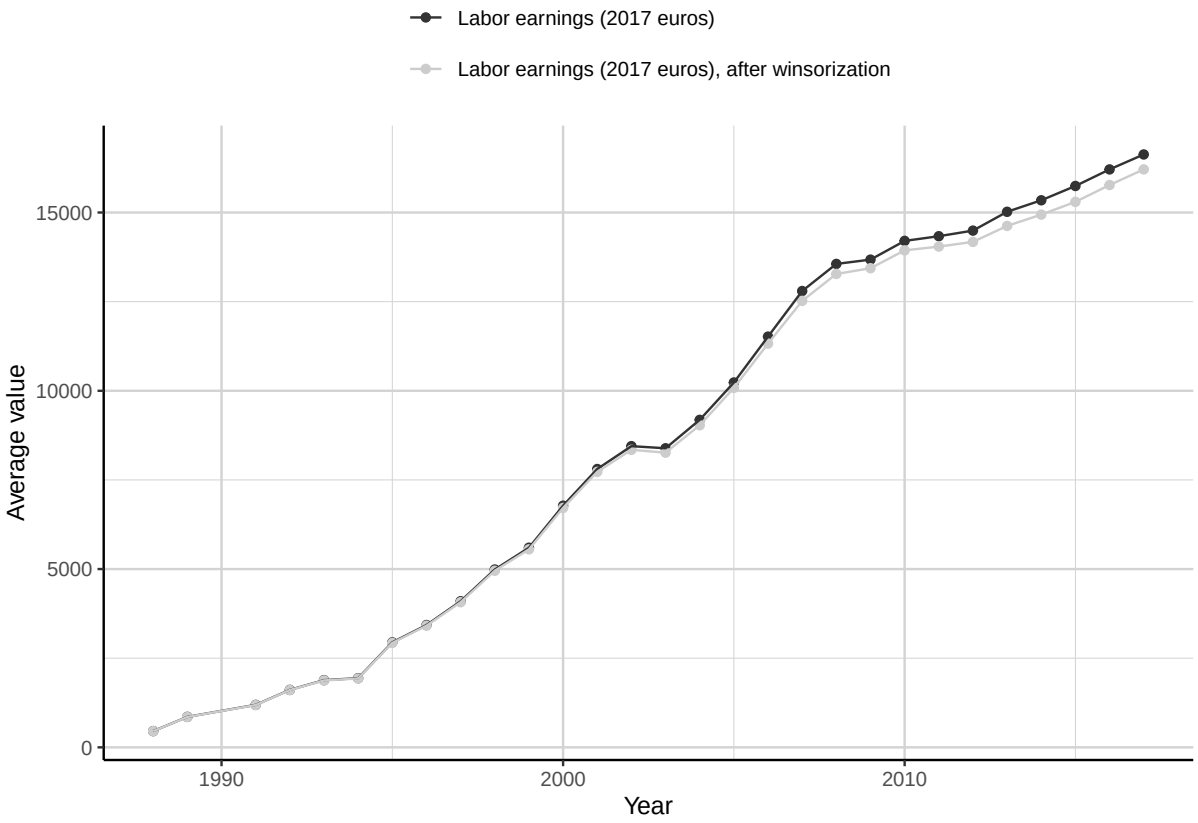


FTU wage before and after winsorization at the 99th centile, by year of salaried employment.

Population. Salaried individuals born on October 1st to 4th between 1967 and 1990 and sampled in the EDP data.

Source. CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

Figure E.11. Labor earnings in the EDP data



Labor earnings before and after winsorization of the FTU wage at the 99th centile, by year.

Population. Individuals born on October 1st to 4th between 1967 and 1990 and sampled in the EDP data.

Source. CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

F Reweighting procedure

F.1 Implementation

Our reweighting procedure is based on an inverse propensity score approach. The basic intuition behind this approach is that reweighting individuals using the inverse of the probability of belonging to their observed attitude groups makes the groups similar to the entire population in terms of the distribution of covariates. In more formal terms, if X_i represents a vector of individual covariates and D_i is a binary variable that equals 1 if individual i holds more traditional views on gender and 0 if she holds less traditional views, then, under a common support assumption, for any $d \in \{0,1\}$ and any measurable function f:

$$\mathbb{E}\left[\frac{f(X_i)\mathbb{P}(D_i = d)}{\mathbb{P}(D_i = d \mid X_i)} \mid D_i = d\right] = \mathbb{E}\left[f(X_i)\right] \tag{19}$$

The main challenge is that the true propensity score function $p(x) := \mathbb{P}(D_i = 1 \mid X_i = x)$ is not known to the econometrician. Therefore, it must be estimated, which requires additional assumptions. In the context of this paper, we address this issue with a parametric model. Specifically, we use a linear probability model estimated by ordinary least squares, with the following covariates:

- migration status (first or second generation);
- decennial cohort interacted with diploma (7 levels);
- age at the beginning of the first stable relationship;
- age at the beginning of the first cohabiting relationship;
- experience of discrimination in the labor market;
- age at first child;
- total number of children.

The last two dimensions describe individuals during or after the birth of their first child, particularly in terms of their fertility (see 3.2.1). For this reason, we also consider alternate specifications where these variables are excluded from the selection model.

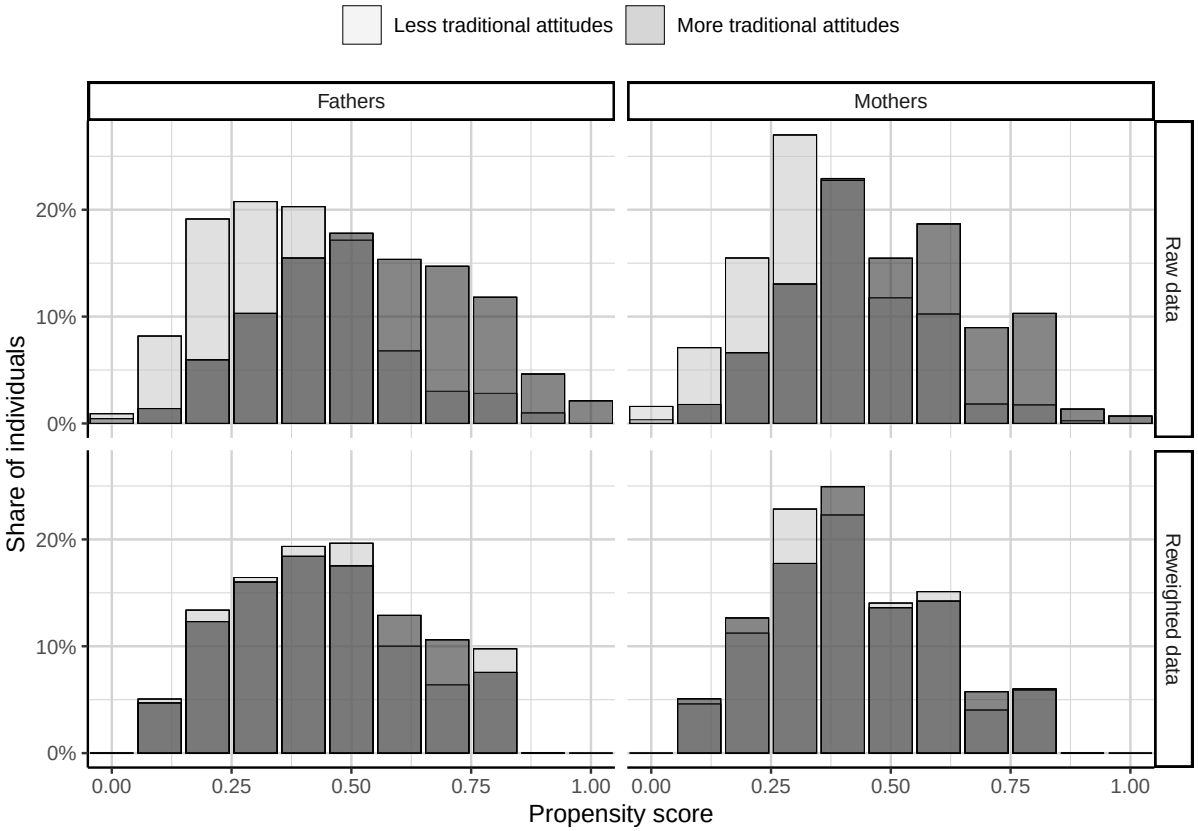
Finally, the estimated propensity score is simply the predicted probability of holding more traditional views according to the estimated model. Following Crump et al. (2009); Imbens (2015), we discard individuals whose estimated propensity score is less than 0.1 or greater than 0.9, in order to make the reweighted estimates more stable and ensure sufficient overlap in the sample.

F.2 Common support assumption

As recommended by Imbens (2015), we validate this model using assessments that are not dependent on our outcome of interest (the child penalty). Specifically, we evaluate the credibility of the common support assumption by examining the distributions of the estimated propensity score and displaying balancing plots.

Figures F.1 and F.2 show the distributions of the estimated propensity score, before and after reweighting the data, for both the model in which fertility decisions are omitted from the selection equation and the model in which they are included. The key findings are that (i) there is reasonable overlap in the distributions across attitude groups, making our common support assumption plausible; and (ii) after trimming and reweighting the data, the distributions are very similar across groups, suggesting that the reweighting improves balance across groups in terms of the propensity score.

Figure F.1. Distribution of the propensity score: balancing on pre-birth characteristics

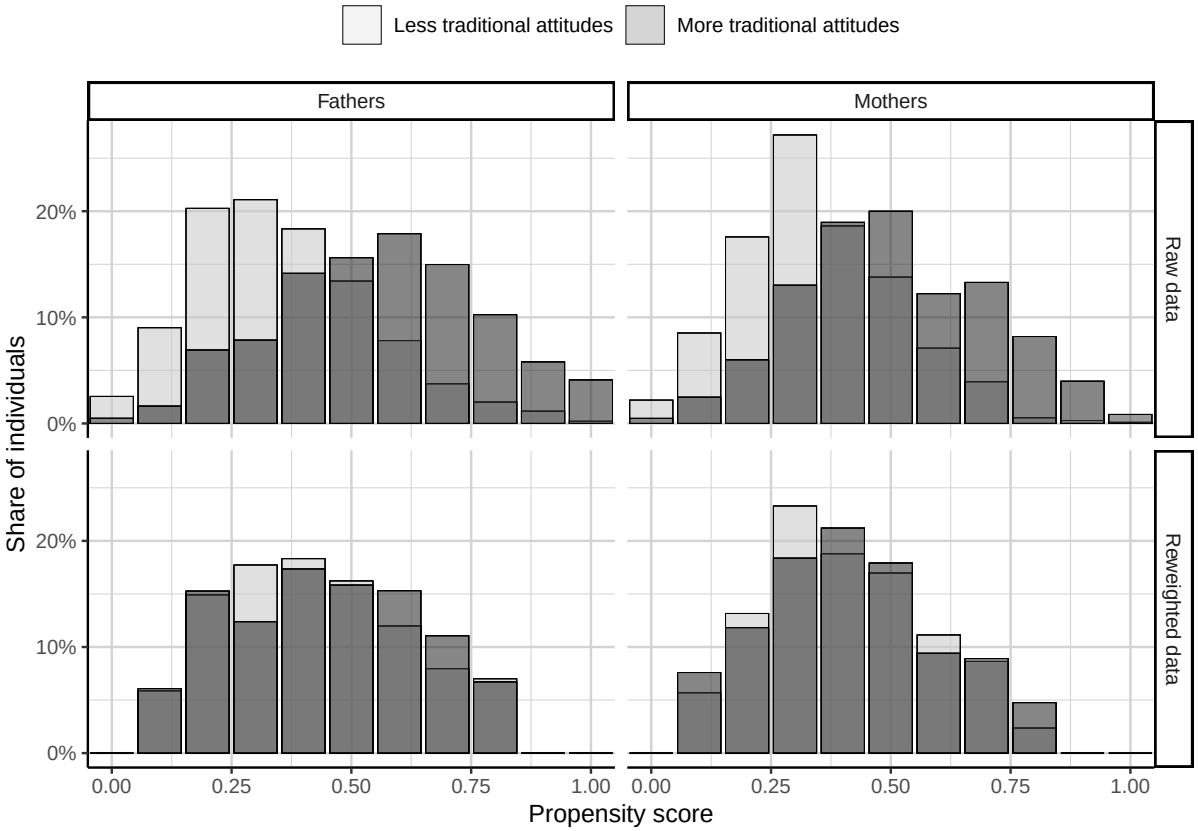


Distribution of the estimated propensity score, before and after reweighting. Reweighting variables include migration status (first or second generation), decennial cohort interacted with diploma (7 levels), experience with marital life and discrimination experience on the labor market.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure F.2. Distribution of the propensity score: balancing on pre-birth characteristics and fertility decisions



Distribution of the estimated propensity score, before and after reweighting. Reweighting variables include migration status (first or second generation), decennial cohort interacted with diploma (7 levels), experience with marital life and discrimination experience on the labor market, as well as decennial cohort interacted with quinquennial age at first child, total number of children, and current life with a partner.

Population. Immigrant parents living in mainland France in 2019-2020.

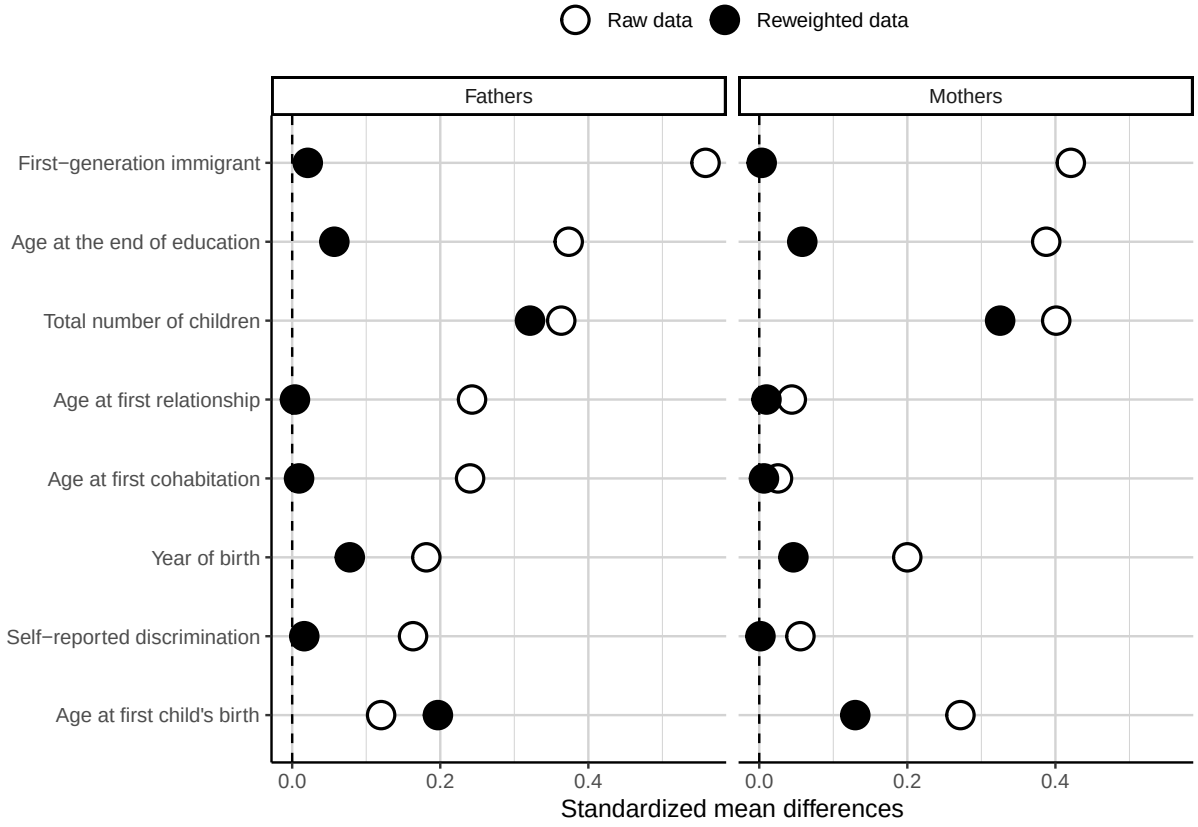
Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

F.3 Balancing property

Figures F.3 and F.4 take the analysis a step further by verifying that, after reweighting, attitude groups are more balanced in terms of each covariate. Specifically, they show the standardized mean differences across groups for each covariate, both before and after reweighting the data. We include the fertility covariate in the assessment of both models, even when fertility variables are not part of the propensity score estimation, to provide a complete understanding of the differences across groups in each case.

The key finding is that after reweighting based on the inverse of the estimated propensity score, attitude groups are much more similar in terms of their observable characteristics than before. This is, of course, only true for characteristics included in the propensity score estimation, as shown by the fact that when fertility decisions are omitted from the model, groups remain imbalanced in terms of their fertility decisions.

Figure F.3. Balancing properties of the estimated propensity score: balancing on pre-birth characteristics

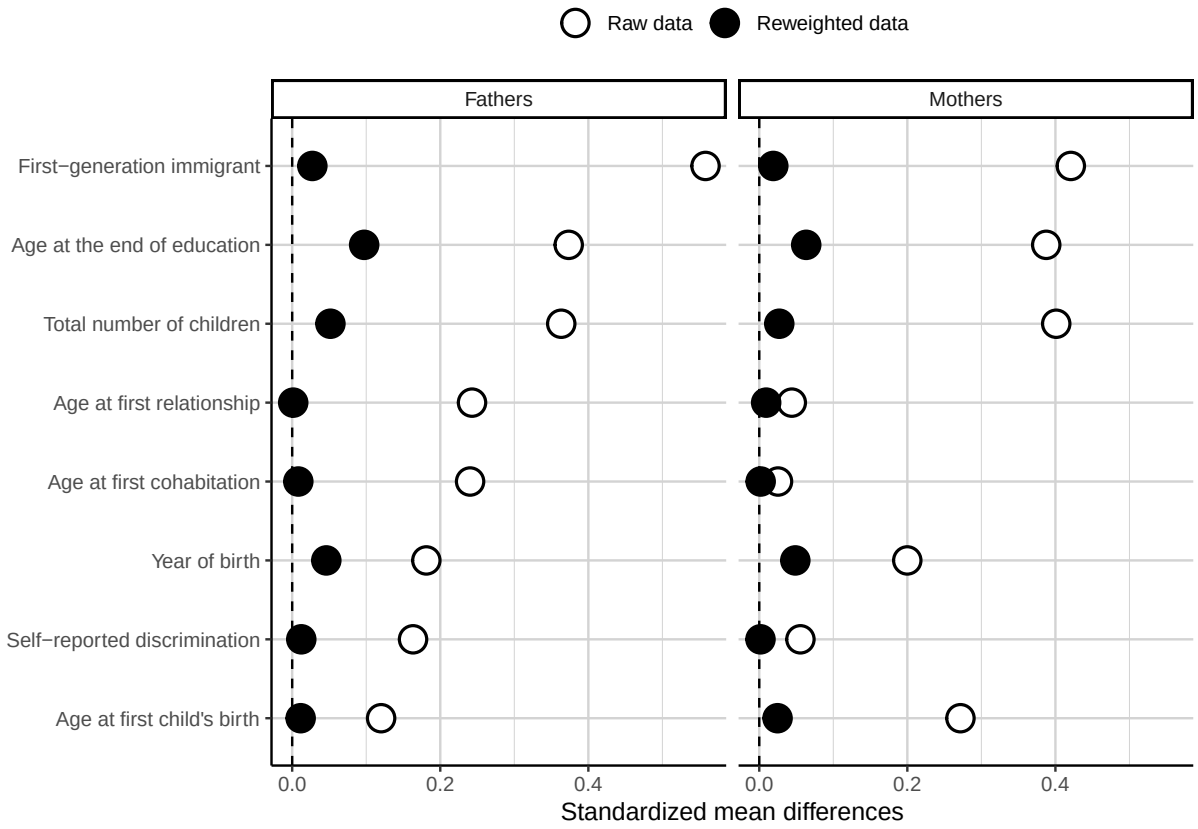


Standardized absolute mean differences along different variables, before and after reweighting. Reweighting variables include migration status (first or second generation), decennial cohort interacted with diploma (7 levels), experience with marital life and discrimination experience on the labor market.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure F.4. Balancing properties of the estimated propensity score: balancing on pre-birth characteristics and fertility decisions



Standardized absolute mean differences along different variables, before and after reweighting. Reweighting variables include migration status (first or second generation), decennial cohort interacted with diploma (7 levels), experience with marital life and discrimination experience on the labor market, as well as decennial cohort interacted with quinquennial age at first child, total number of children, and current life with a partner.

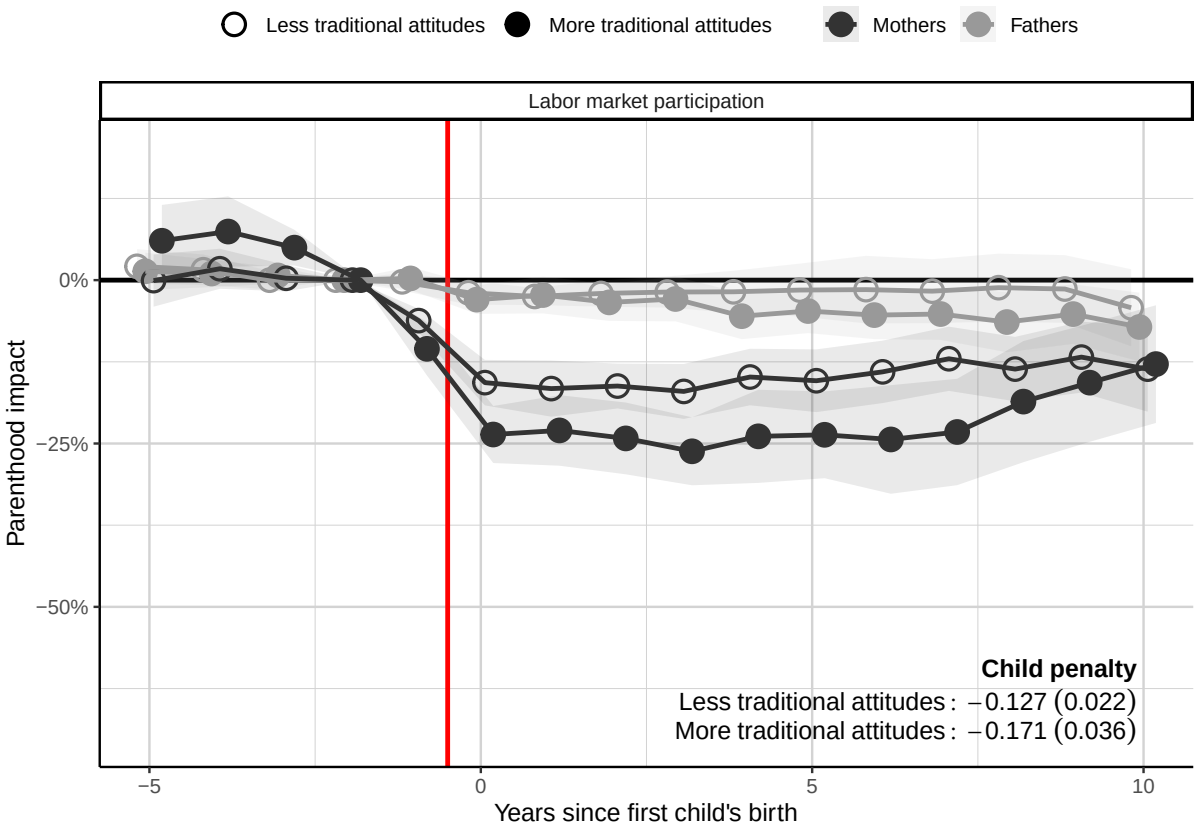
Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

F.4 Influence of the reweighting procedure on the results regarding individual gender-related attitudes

Figures F.5 and F.6 display the child penalty for each attitude group: the first without any reweighting of the data, and the second reweighting only on pre-birth characteristics. In both cases, the difference between the two attitude groups is not significantly different from 0. It is possible that, without reweighting for fertility, the child penalty is stronger for those with more traditional views, although the difference is not large. This is consistent with fertility having a negative impact on mothers' labor force participation and more traditional women having more children.

Figure F.5. Child penalty in labor market participation for immigrant parents: by self-reported attitudes

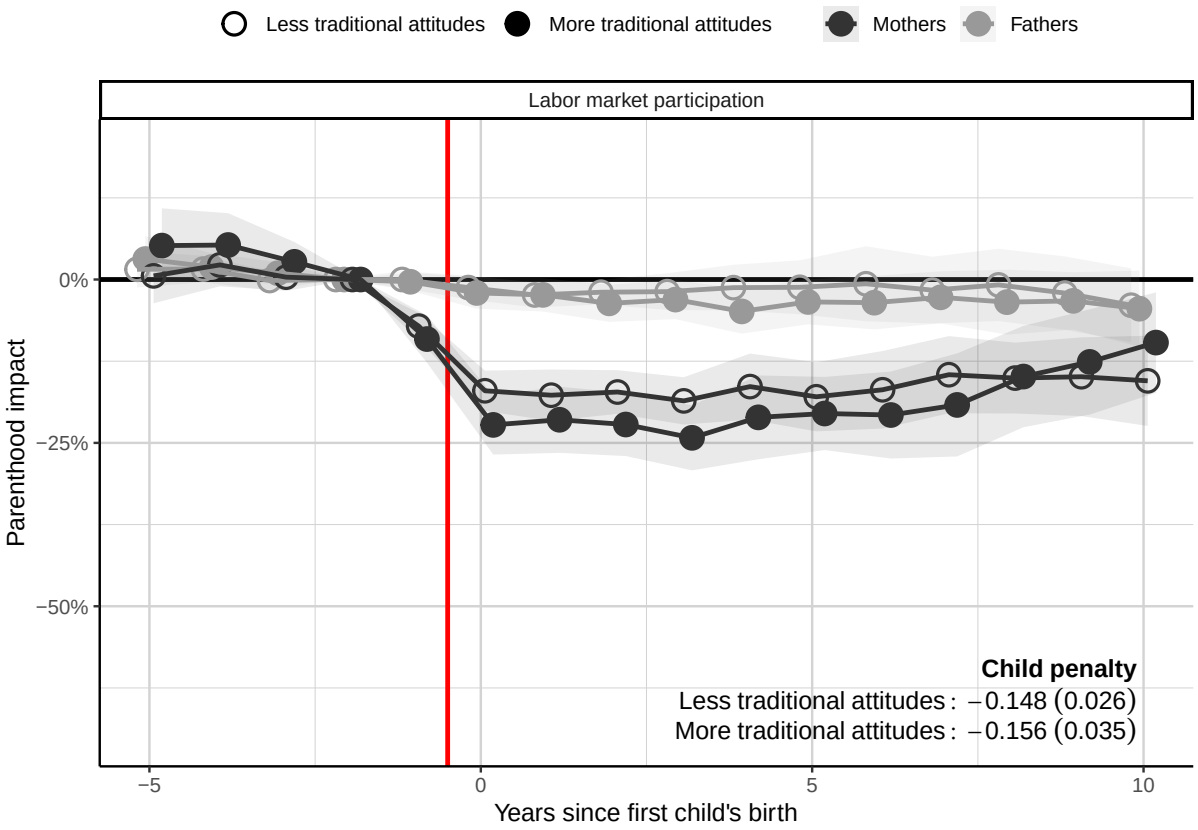


Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure F.6. Child penalty in labor market participation for immigrant parents: by self-reported attitudes, balanced on pre-birth characteristics



Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make attitudes groups similar in terms of pre-birth observables characteristics. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

G Relative and absolute child penalties

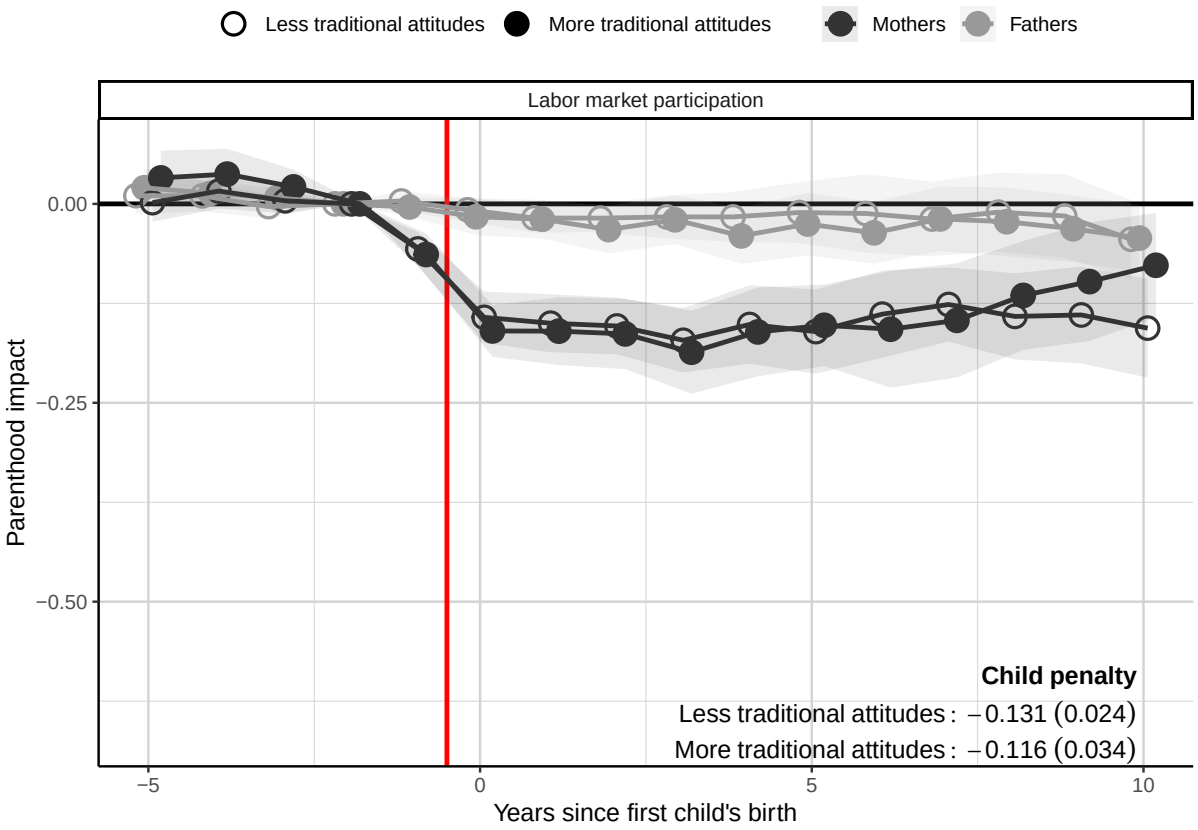
Consistent with the now-standard approach to the child penalty developed by Kleven, Landais, and Søgaard (2019), we display our results in terms of the relative effect of parenthood on labor outcomes, i.e., the change in average labor outcomes between the realized situation and the counterfactual where parents remain childless. When comparing child penalties across groups, this choice may raise the concern that differences could arise from either differences in the absolute effect of parenthood or from differences in baseline labor outcomes. In the particular context of this paper, this is a legitimate concern, given that differences in raw labor market participation rates across groups are not negligible (see Figure 1).

To further investigate this issue, we replicate Figures 3 to 6 considering absolute effects instead of relative ones. Figures G.1 to G.3 display the corresponding estimates. In absolute terms, the child penalty is generally slightly smaller in the more traditional groups, but the difference between more and less traditional groups is not estimated with sufficient precision to draw a firm conclusion. Overall, the data do not suggest substantial differences in the child penalty across attitude-related groups, whether in absolute or relative terms.

To delve deeper into this issue, we examine differences in counterfactual labor market participation rates across attitude groups. Figure G.4 presents our estimates. These help reconcile the fact that the child penalty does not differ across attitude groups with the observation that female labor force participation is much lower across the lifecycle for women with more traditional attitudes. Indeed, we find that, in the absence of children, sizable differences in labor market participation would still exist among women, depending on their gender-related attitudes. Over the first 10 years after their first child is born, women with more traditional attitudes would have an average labor market participation rate of 0.80 (0.03), compared to 0.89 (0.02) for those with less traditional attitudes. The realized rates are 0.66 (0.02) and 0.73 (0.01). By contrast, for men, attitudes do not seem to be related to labor market participation, as the estimated counterfactual rates without children are 0.92 (0.02) and 0.93 (0.02), compared to realized rates of 0.89 (0.01) and 0.91 (0.01).

Finally, to discuss the causal interpretation of these differences in counterfactual labor market participation rates across attitude groups, we turn to differences in counterfactual and realized labor market participation rates across groups defined by their upbringing environment before age 18. Figure G.5 displays our estimates. Differences in counterfactual rates appear to be significant, with rates of 0.82 (0.03) for women who received a more traditional upbringing, compared to 0.88 (0.03) for those with a less traditional upbringing. The realized rates are 0.68 (0.02) and 0.71 (0.02). For men, the corresponding values are 0.91 (0.03) and 0.92 (0.03) for counterfactual rates, and 0.88 (0.02) and 0.89 (0.02) for realized rates. Since comparisons across upbringing groups are less likely to be affected by reverse causality than those relying on current attitudes at the time of the survey, these estimates support the idea that gender-related attitudes impact women's labor supply but not men's. This implies that such attitudes contribute to gender differences in labor market participation, even though they do not affect the child penalty.

Figure G.1. Child penalty in labor market participation for immigrant parents: by self-reported attitudes, balanced on pre-birth characteristics and fertility decisions

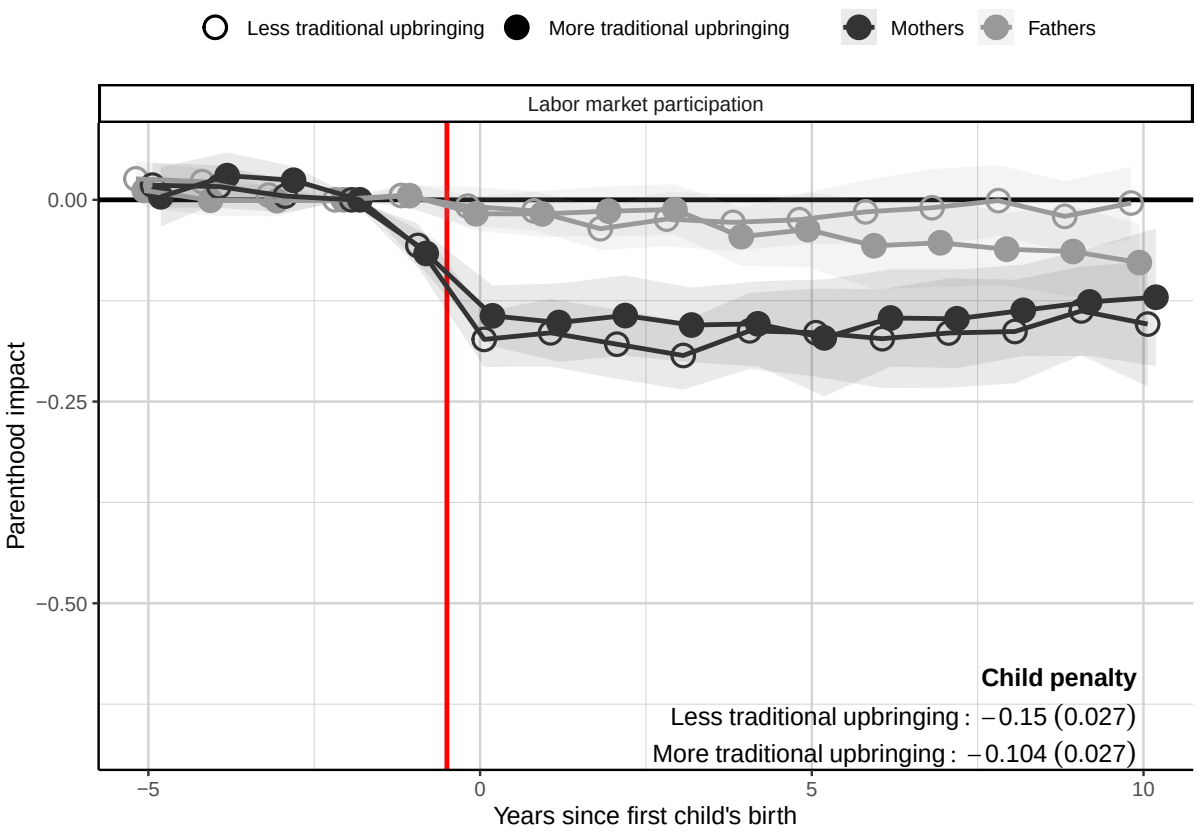


Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed in its absolute value. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make attitudes groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure G.2. Child penalty in labor market participation for immigrant parents: by upbringing environments, balanced on pre-birth characteristics and fertility decisions

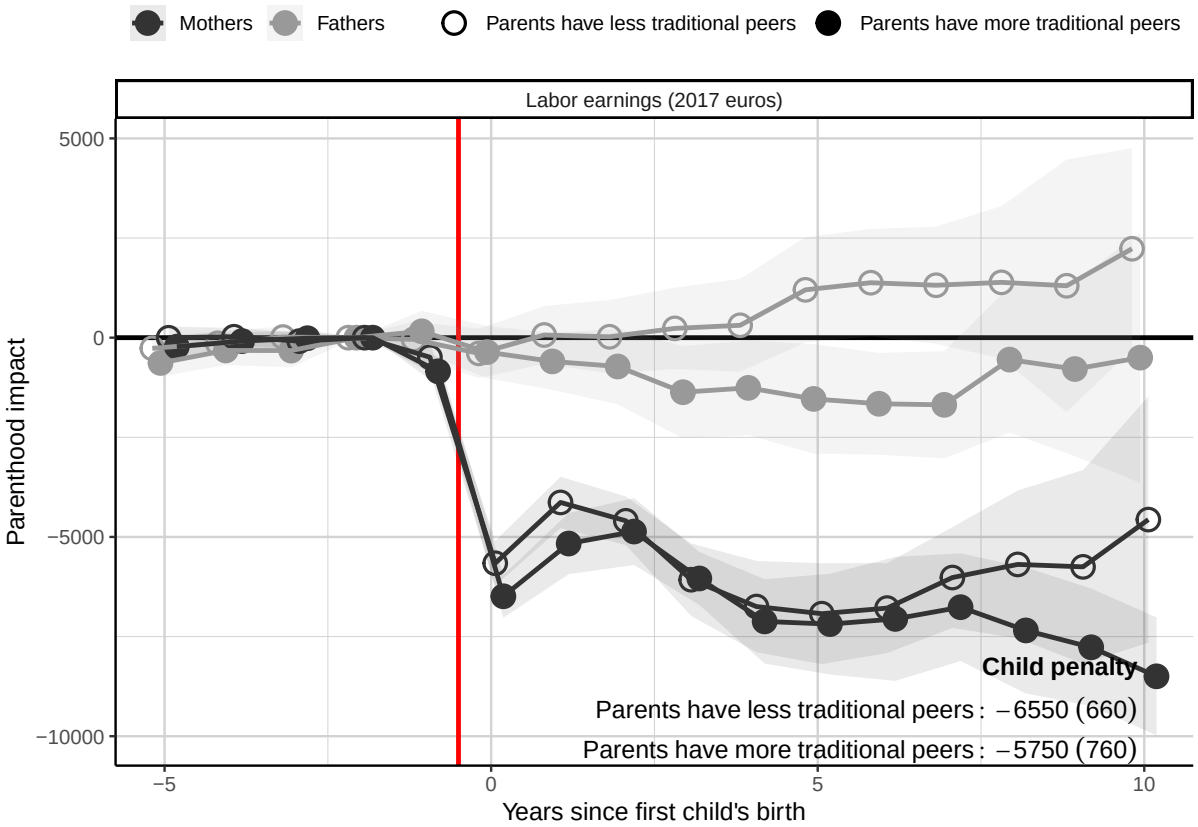


Upbringing environment is measured by *ego*'s father's and mother's having a religion, prominence given to religion in *ego*'s education before the age of 18, gender imbalance between *ego*'s parents' allocation of household chores (daily meals, grocery shopping and clothes washing) before the age of 18 and the number of children born to *ego*'s mother. Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed in its absolute value. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make upbringing environments groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure G.3. Child penalty in labor earnings for second-generation immigrant parents: by second-generation immigrant parents' parents' peers' attitudes, balanced on pre-birth characteristics and fertility decisions

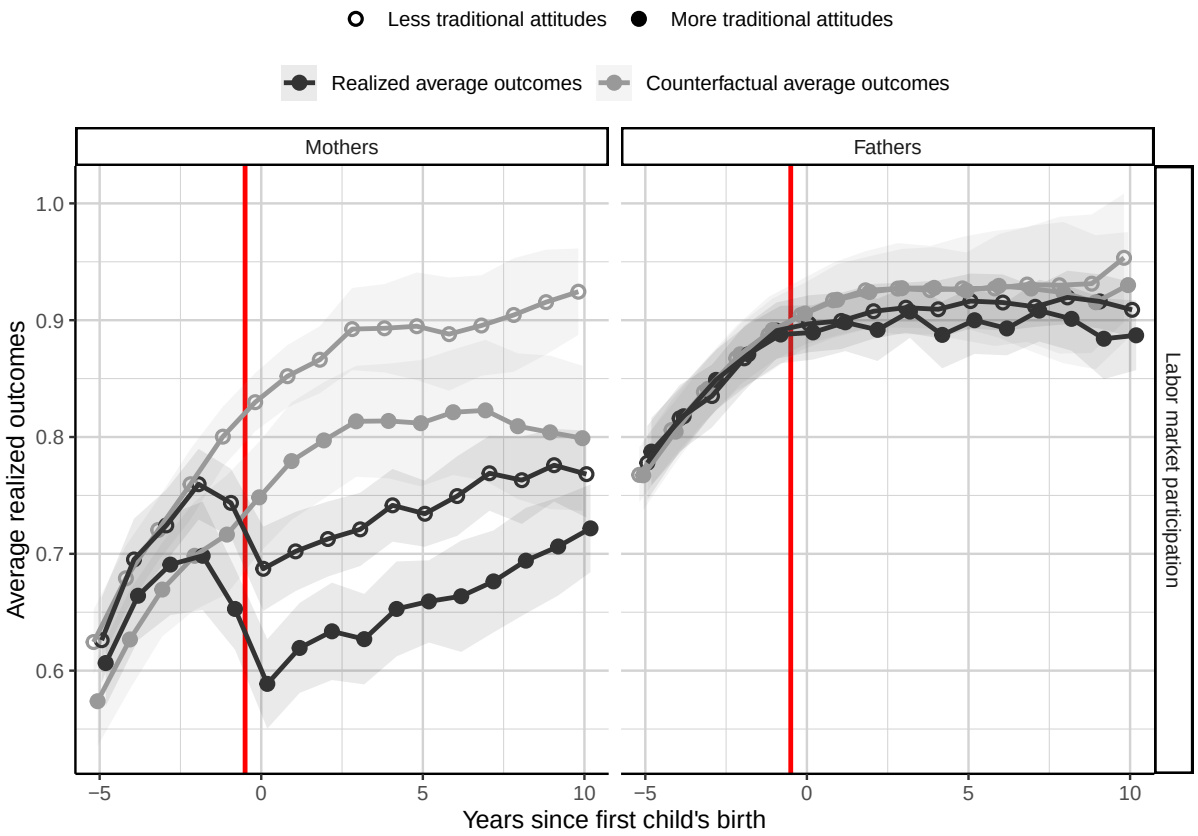


Parents' peers' attitudes are measured as the share of first-generation female (resp. male) immigrants from *ego*'s mother's (resp. fathers') country of birth with above-median traditional gender-related attitudes. Parenthood impact is identified from comparisons between parents of the same cohort (year of birth) who got their first salaried job at the same time, but had their first child at different ages (at least two years after their first salaried job), and displayed in its absolute value in 2017€. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make origin groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level.

Population. Second-generation immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

Figure G.4. Realized and counterfactual labor market participation for immigrant parents: by self-reported attitudes, balanced on pre-birth characteristics and fertility decisions

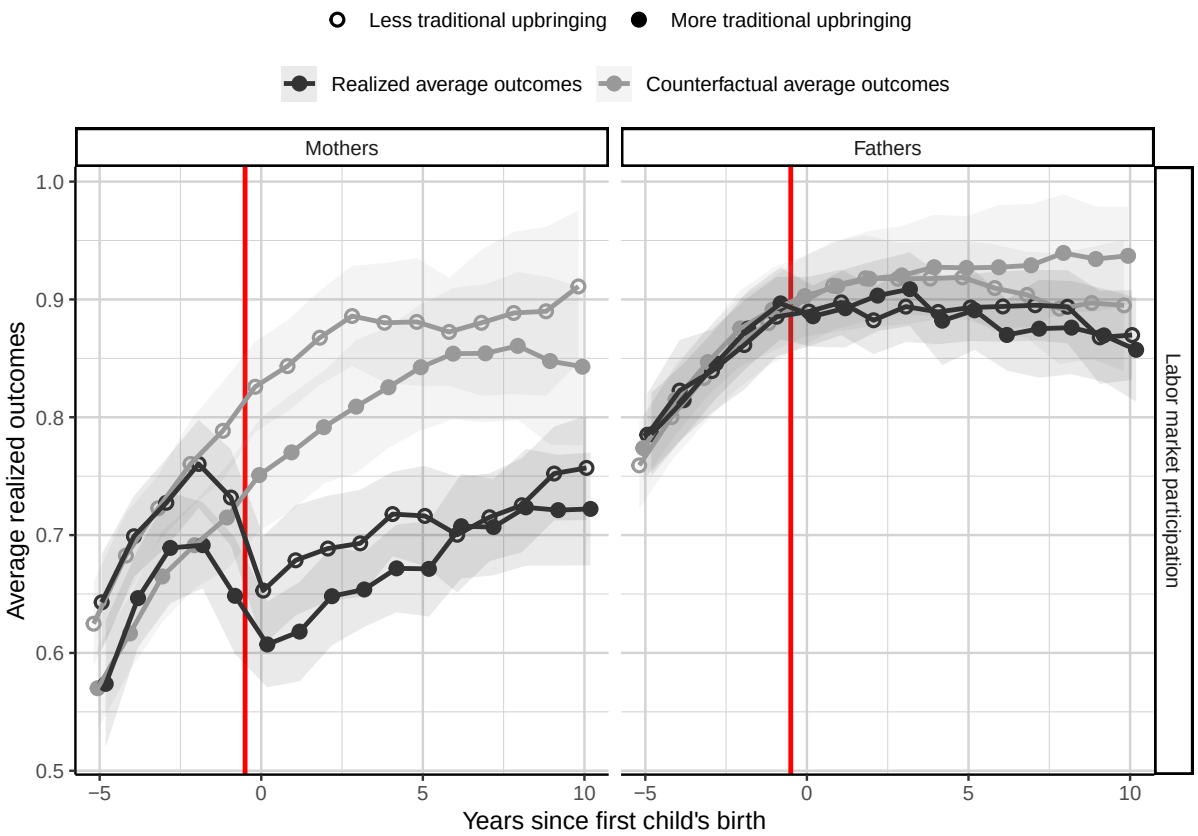


Counterfactual rates are identified from comparisons between parents of the same decennial cohort who had their first child at different ages. The data are reweighted based on an inverse propensity score approach so as to make attitudes groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure G.5. Realized and counterfactual labor market participation for immigrant parents: by upbringing environments, balanced on pre-birth characteristics and fertility decisions



Upbringing environment is measured by *ego*'s father's and mother's having a religion, prominence given to religion in *ego*'s education before the age of 18, gender imbalance between *ego*'s parents' allocation of household chores (daily meals, grocery shopping and clothes washing) before the age of 18 and the number of children born to *ego*'s mother. Counterfactual rates are identified from comparisons between parents of the same decennial cohort who had their first child at different ages. The data are reweighted based on an inverse propensity score approach so as to make upbringing environments groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

H Robustness checks

H.1 Robustness with respect to a different definition of gender-related attitudes

We consider here a different delineation of groups based on self-reported attitudes. Specifically, we split immigrants into two groups based on whether they totally disagreed with the statement "When there are not many jobs, men are more entitled to work than women." Since most respondents totally disagreed with this statement, these alternative attitude groups are less balanced in terms of sample size compared to our baseline specification. Figures H.1 to H.3 display our results. Consistent with the rest of the evidence, we find that the child penalty is very similar across attitude groups.

H.2 Robustness with respect to the inclusion of a particular survey item in the definition of gender-related attitudes

H.2.1 Sensitivity to the inclusion of a particular item

Figure H.4 shows the share of immigrant parents who change attitude groups when one of the six relevant survey items is omitted from the PCA. This share is overall quite low: it reaches a maximum of about 14% for views on non-medical abortion and is typically lower than 10%. The key takeaway from this exercise is that our specification of attitude groups is not driven by any single survey item. This suggests that our approach to gender-related attitudes captures a latent factor that explains opinions on gender inequality and bodily autonomy.

H.2.2 Robustness of the child penalties comparisons

Figures H.5 to H.10 show our estimates of the child penalties in labor market participation, when attitude groups are defined using a PCA that excludes one specific survey item. The results align with our previous finding that immigrant parents with more traditional views do not experience larger child penalties than their less traditional counterparts. This result is unsurprising, as the relevant survey items are strongly correlated, meaning that omitting one item has minimal impact on the attitude groups (see H.2.1).

H.3 Robustness with respect to the threshold choice

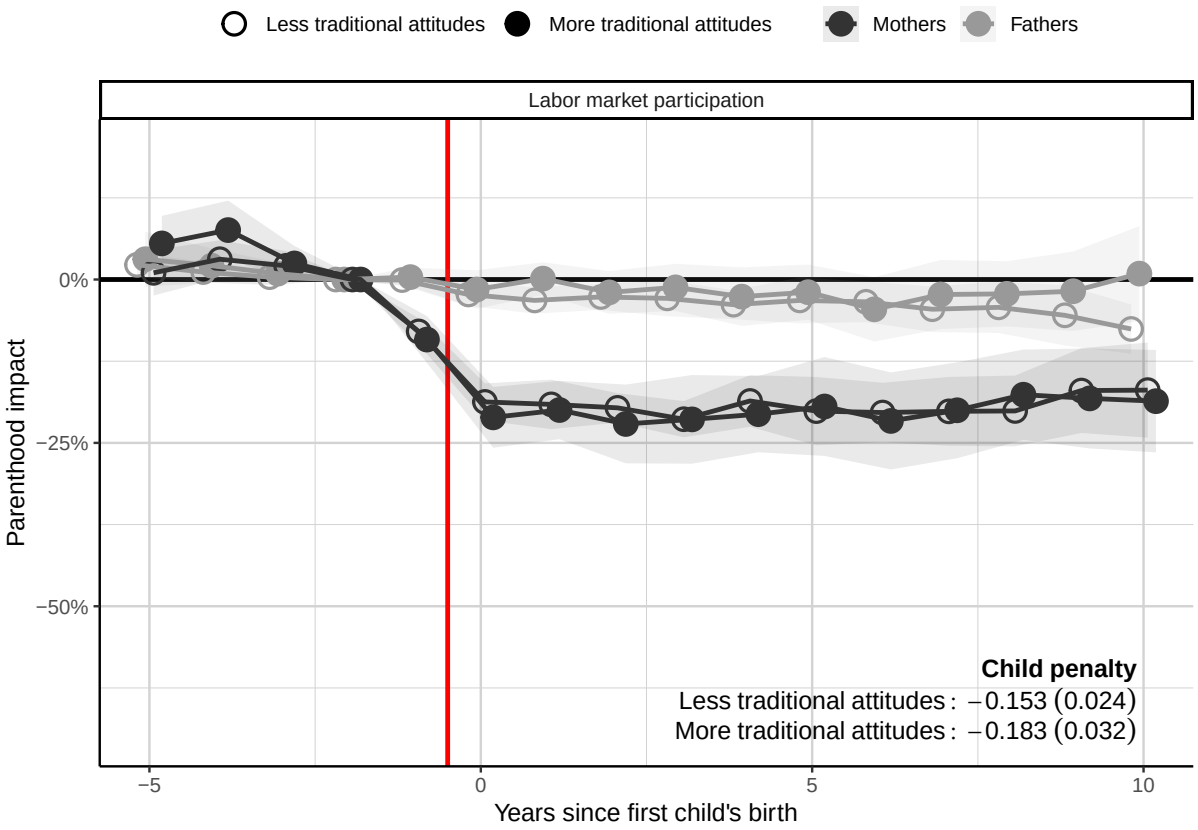
To ensure that our results are not driven by the specific choice to split attitude groups at the median level, we perform an alternative estimation in which we compare groups defined by being below the first tertile or above the last tertile, thereby excluding data from immigrant parents with intermediate views on gender. Figure H.11 presents our estimates. Due to the reduced sample size, the estimated child penalties are less precise than in our baseline estimates. However, consistent with our previous findings, they do not suggest that immigrant parents with more traditional attitudes face a larger child penalty.

H.4 Robustness with respect to the child penalty identification strategy

Our baseline results are based on an approach that improves the event-study approach developed by Kleven, Landais, and Søgaard (2019) in order to identify the child penalty by incorporating insights from the recent difference-in-differences literature. To make the difference between our approach and that of Kleven, Landais, and Søgaard (2019) more explicit, we replicate our estimation, this time following closely their approach when it comes to the estimation of the child penalty.

Specifically, we restrict the sample to immigrant parents who can be observed in the data from five years before to ten years after the birth of their first child. Separately for each gender and each attitude

Figure H.1. Child penalty in labor market participation for immigrant parents: by self-reported attitudes, using only views on gender inequality in the labor market, balanced on pre-birth characteristics and fertility decisions

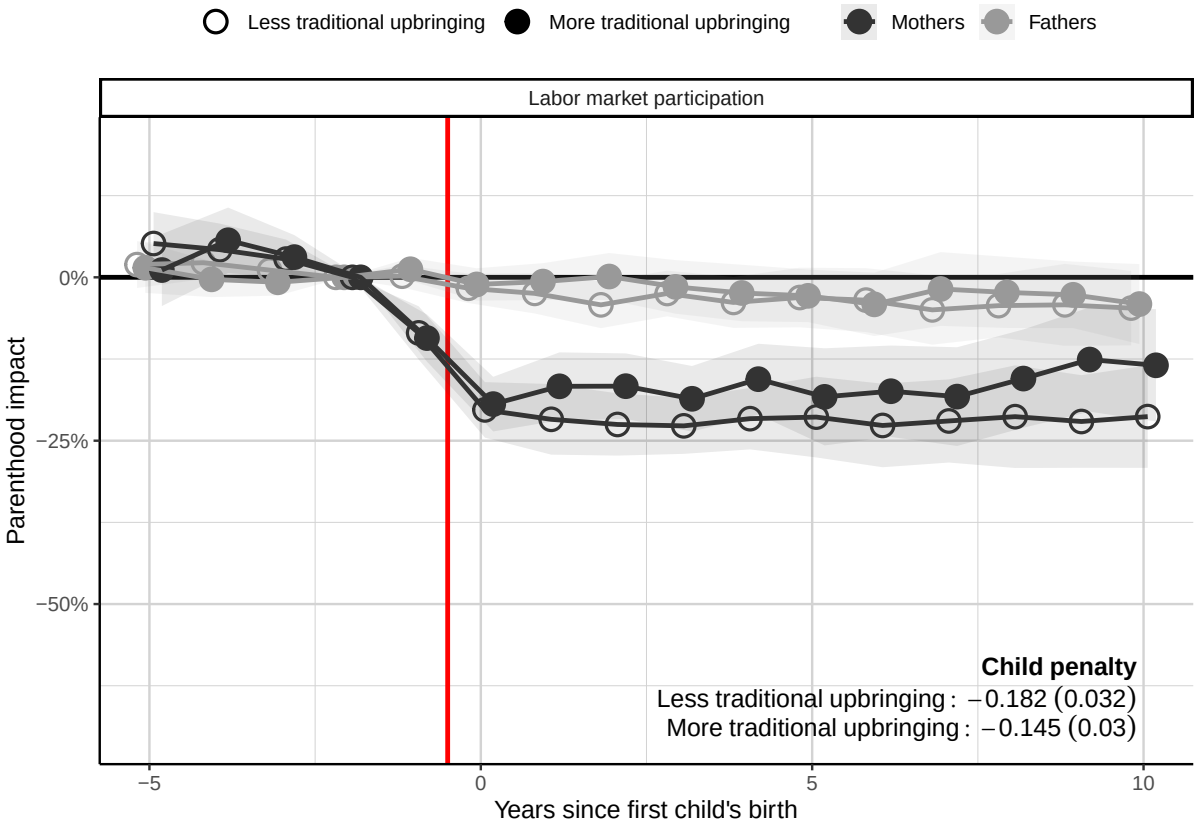


The delineation of attitudes is only based on views regarding gender inequality in the labor market (survey item **I_GENRE**). Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make attitudes groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure H.2. Child penalty in labor market participation for immigrant parents: by upbringing environments, balanced on pre-birth characteristics and fertility decisions, attitudes based only on views on gender inequality in the labor market

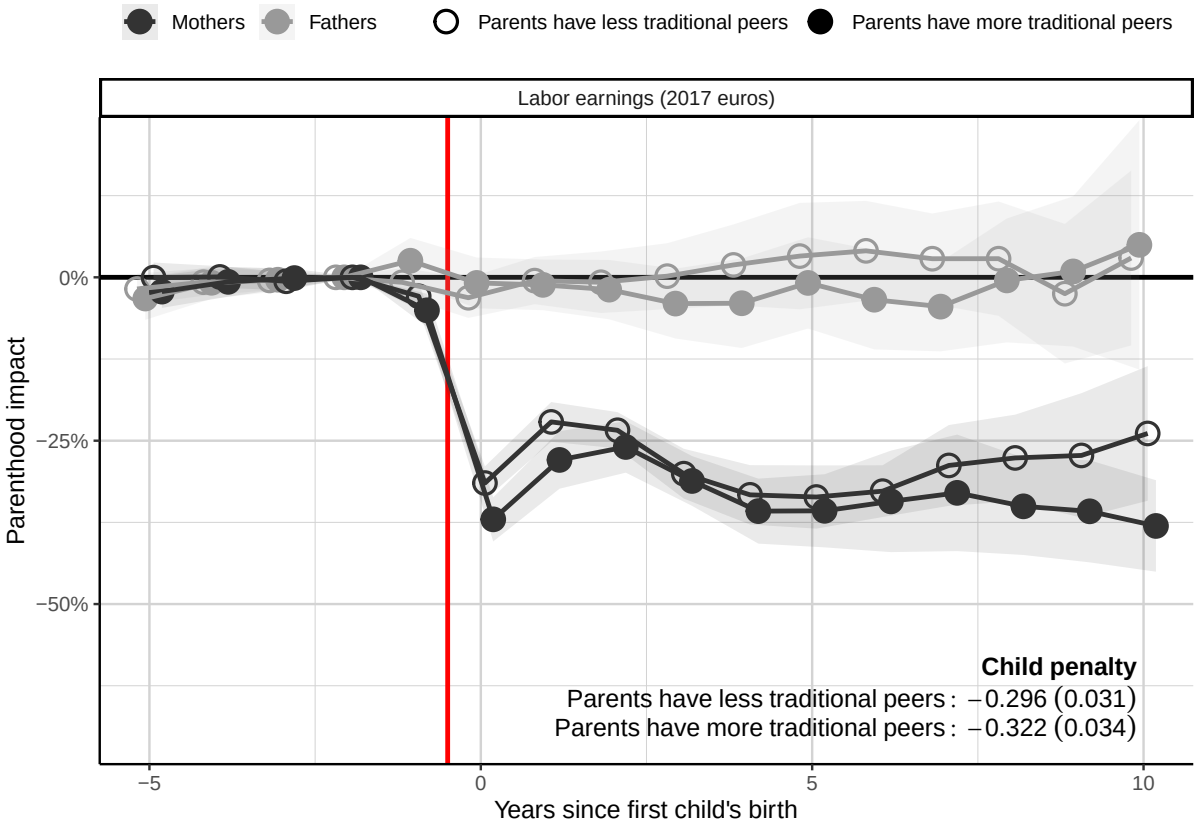


The delineation of attitudes is only based on views regarding gender inequality in the labor market (survey item **I_GENRE**). Upbringing environment is measured by *ego*'s father's and mother's having a religion, prominence given to religion in *ego*'s education before the age of 18, gender imbalance between *ego*'s parents' allocation of household chores (daily meals, grocery shopping and clothes washing) before the age of 18 and the number of children born to *ego*'s mother. Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make upbringing environments groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure H.3. Child penalty in labor earnings for second-generation immigrant parents: by second-generation immigrant parents' parents' peers' attitudes, balanced on pre-birth characteristics and fertility decisions, attitudes based only on views on gender inequality in the labor market

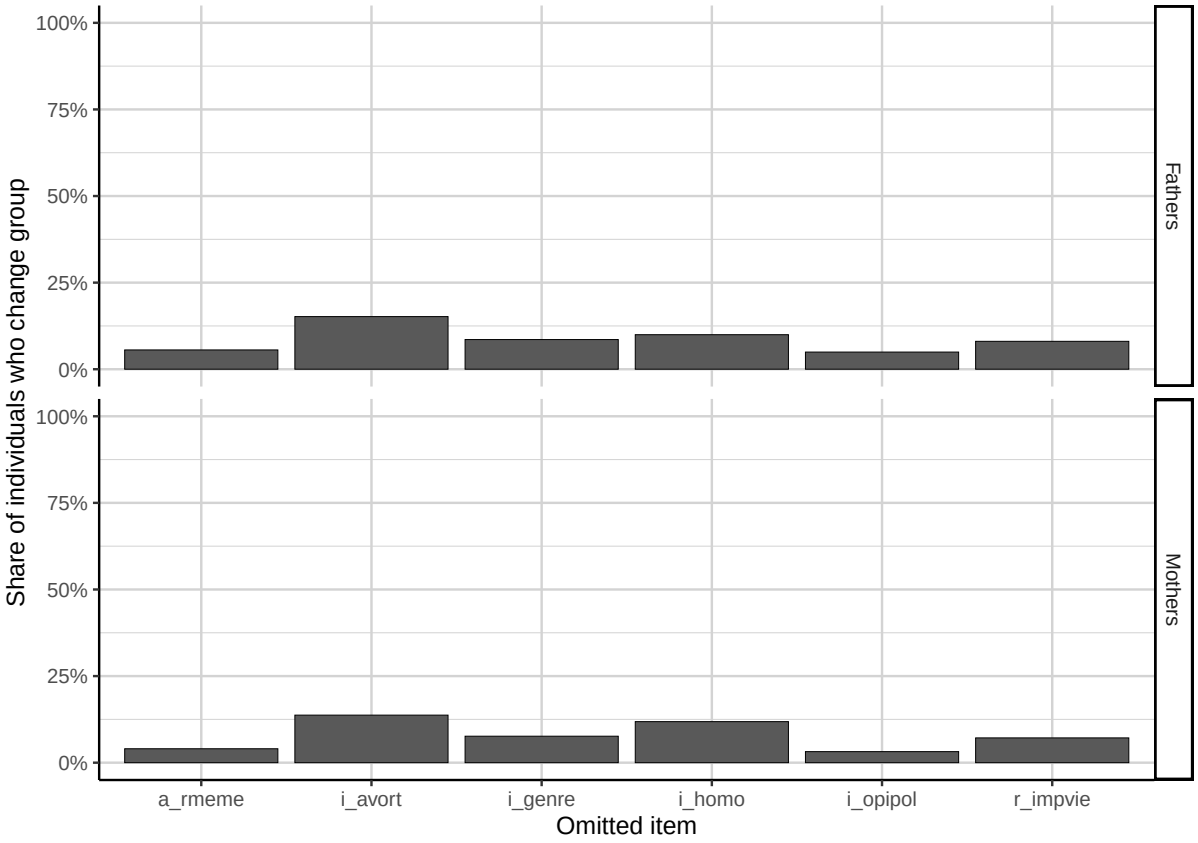


The delineation of attitudes is only based on views regarding gender inequality in the labor market (survey item **I_GENRE**). Parents' peers' attitudes are measured as the share of first-generation female (resp. male) immigrants from *ego*'s mother's (resp. fathers') country of birth with above-median traditional gender-related attitudes. Parenthood impact is identified from comparisons between parents of the same cohort (year of birth) who got their first salaried job at the same time, but had their first child at different ages (at least two years after their first salaried job), and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make origin groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level.

Population. Second-generation immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

Figure H.4. Share of immigrant parents whose attitudes group changes when one survey item is omitted from the PCA: by gender

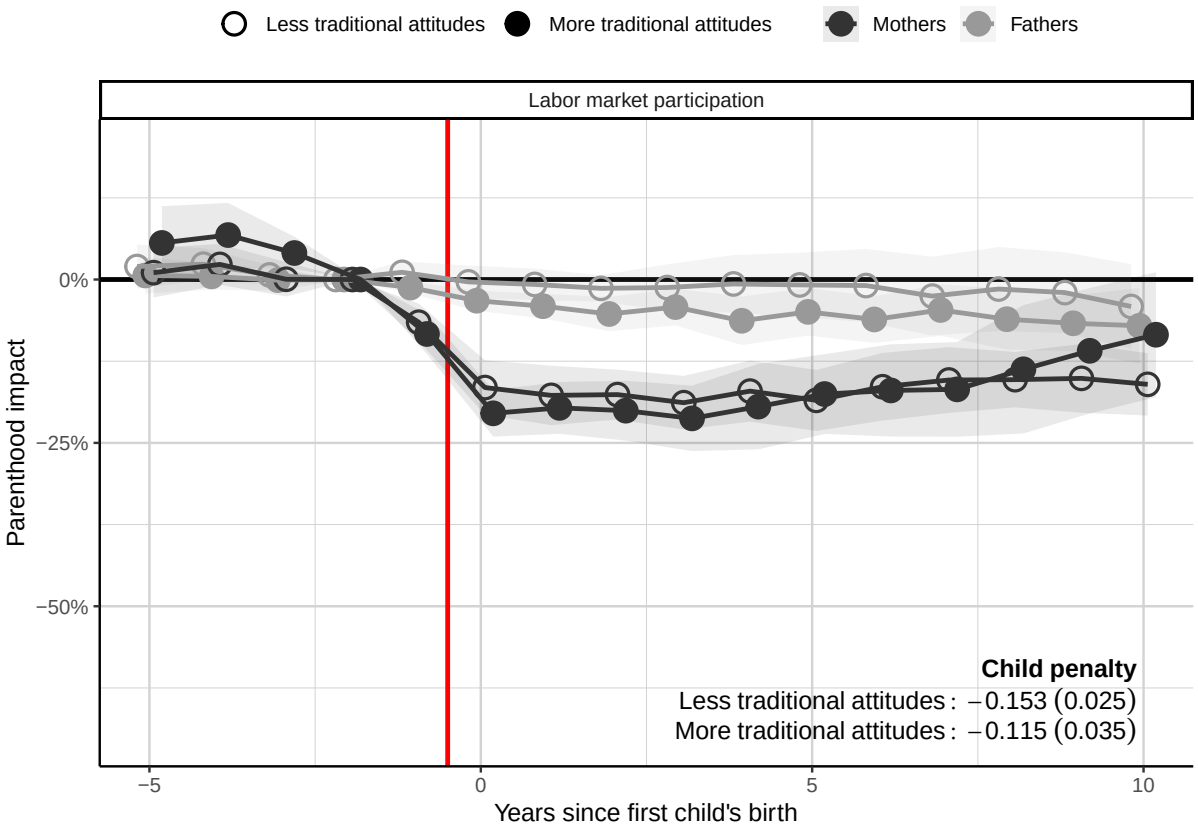


Share of immigrant parents whose attitudes group changes when one of the survey items is omitted from the PCA. The median is that of the first component of the PCA estimated over the five (six minus one) relevant survey items (see 2.2).

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure H.5. Child penalty in labor market participation for immigrant parents: by self-reported attitudes, excluding views on gender inequality in the labor market, balanced on pre-birth characteristics and fertility decisions

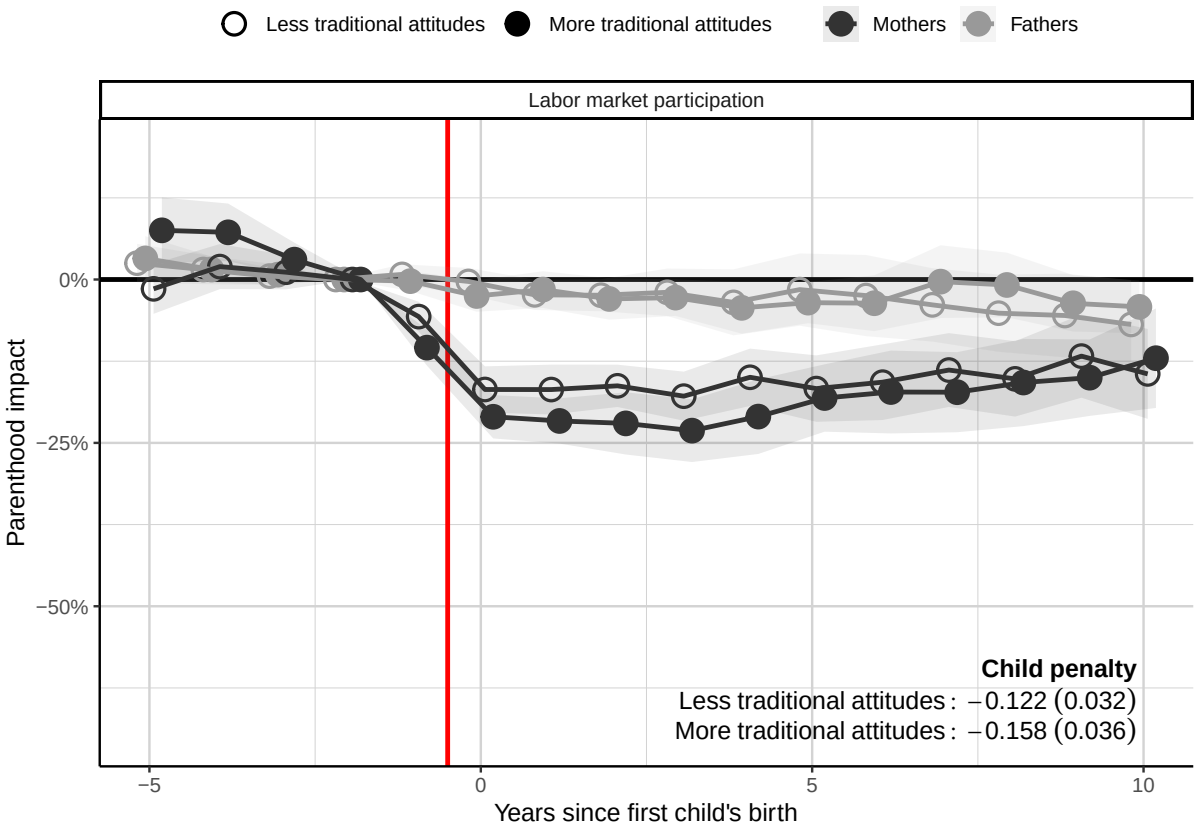


Views regarding gender inequality in the labor market (survey item **I_GENRE**) are not included in the delineation of attitudes. Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make attitudes groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure H.6. Child penalty in labor market participation for immigrant parents: by self-reported attitudes, excluding views on non-medical abortion, balanced on pre-birth characteristics and fertility decisions

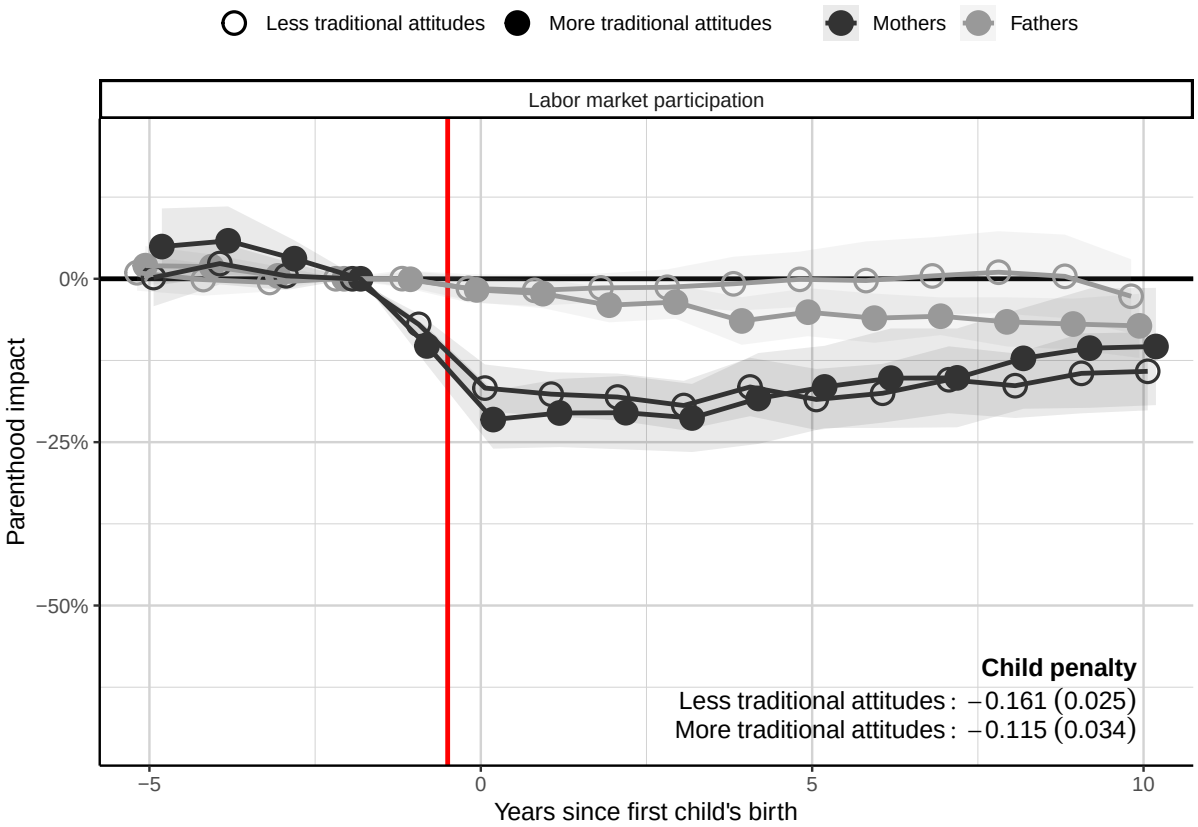


Views regarding non-medical abortion (survey item **I_AVORT**) are not included in the delineation of attitudes. Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make attitudes groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure H.7. Child penalty in labor market participation for immigrant parents: by self-reported attitudes, excluding views on same-sex couples, balanced on pre-birth characteristics and fertility decisions

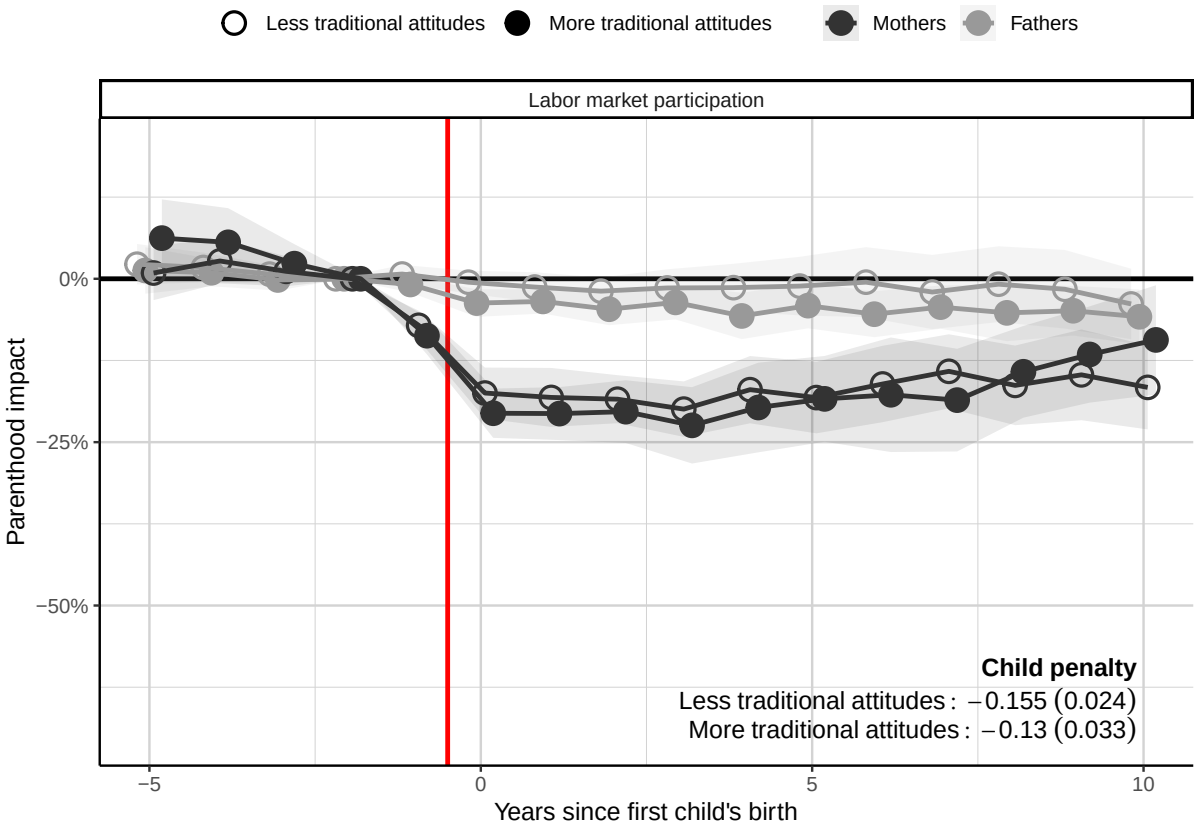


Views regarding equal rights for same-sex couples (survey item **I_HOMO**) are not included in the delineation of attitudes. Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make attitudes groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure H.8. Child penalty in labor market participation for immigrant parents: by self-reported attitudes, excluding friends's gender, balanced on pre-birth characteristics and fertility decisions

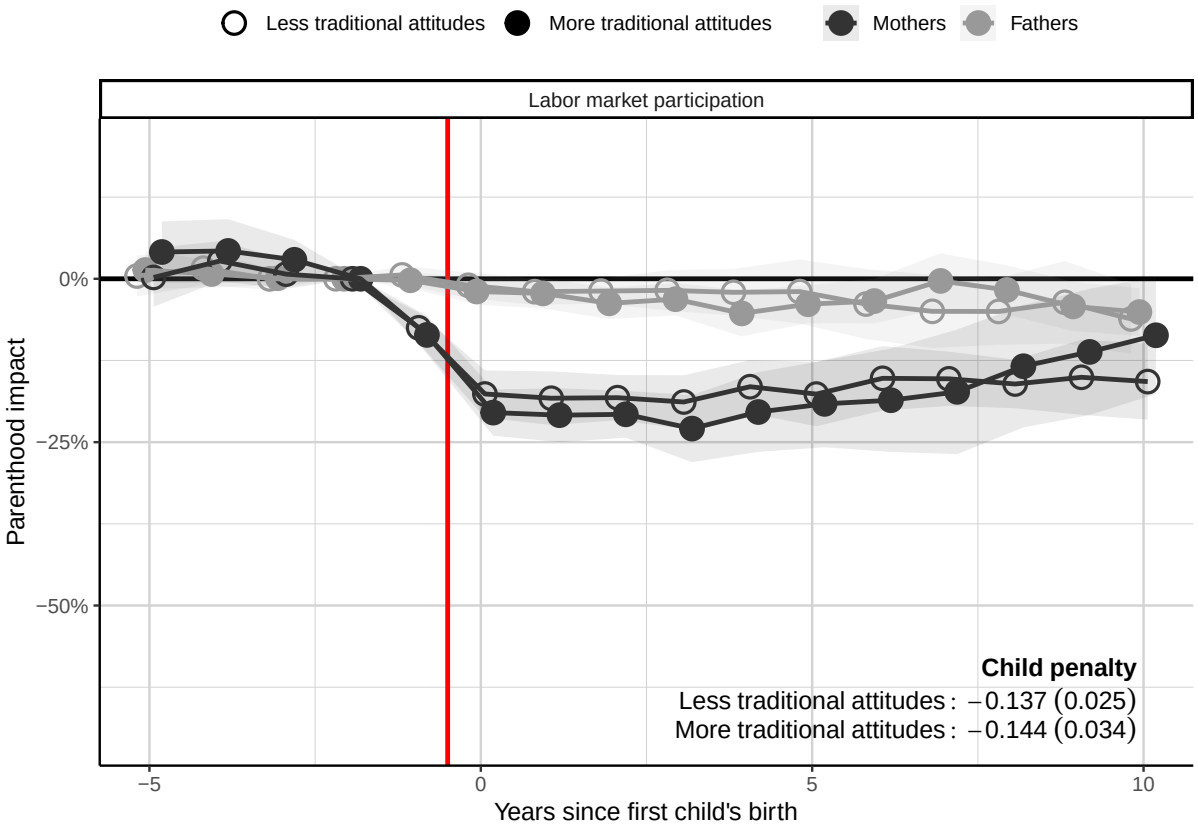


The distribution of immigrants' friends' gender (survey item **A_RMEME**) is not included in the delineation of attitudes. Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make attitudes groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure H.9. Child penalty in labor market participation for immigrant parents: by self-reported attitudes, excluding prominence given to religion, balanced on pre-birth characteristics and fertility decisions

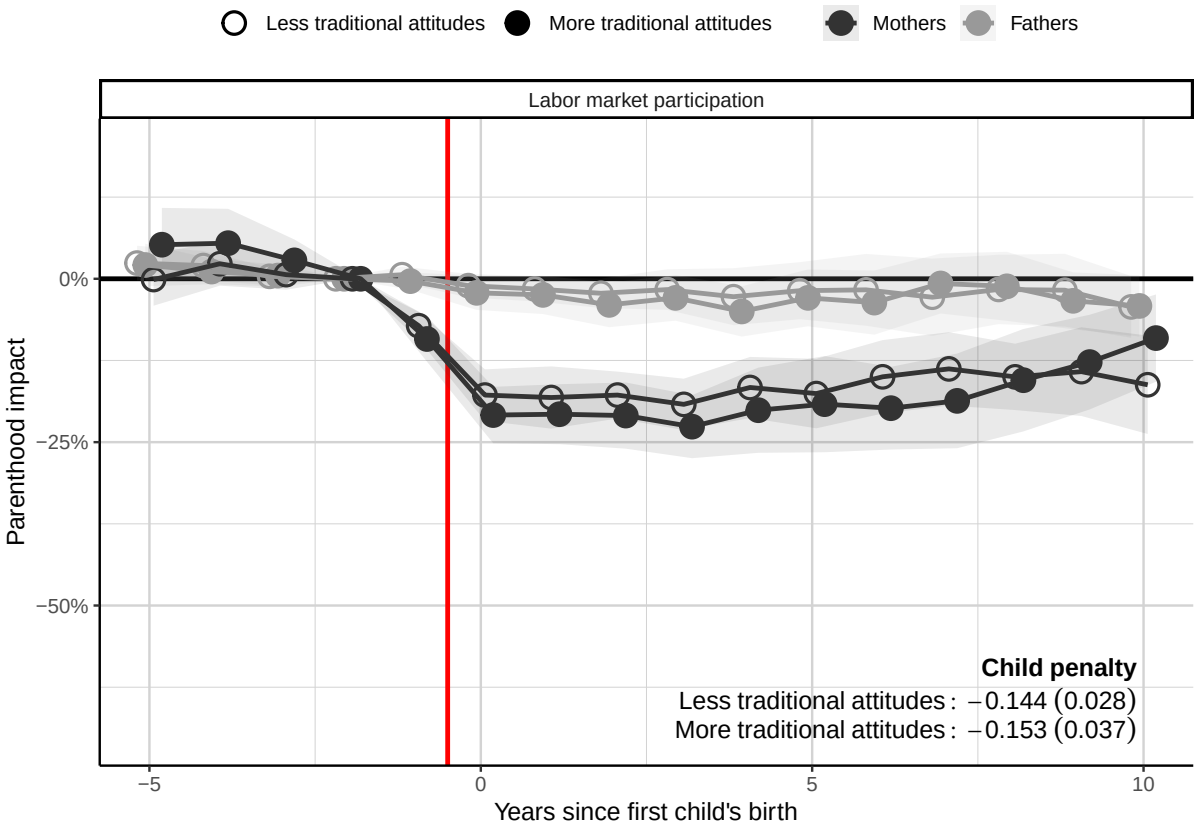


Self-reported prominence given to religion (survey item **R_IMPVIE**) is not included in the delineation of attitudes. Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make attitudes groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure H.10. Child penalty in labor market participation for immigrant parents: by self-reported attitudes, excluding political orientation, balanced on pre-birth characteristics and fertility decisions

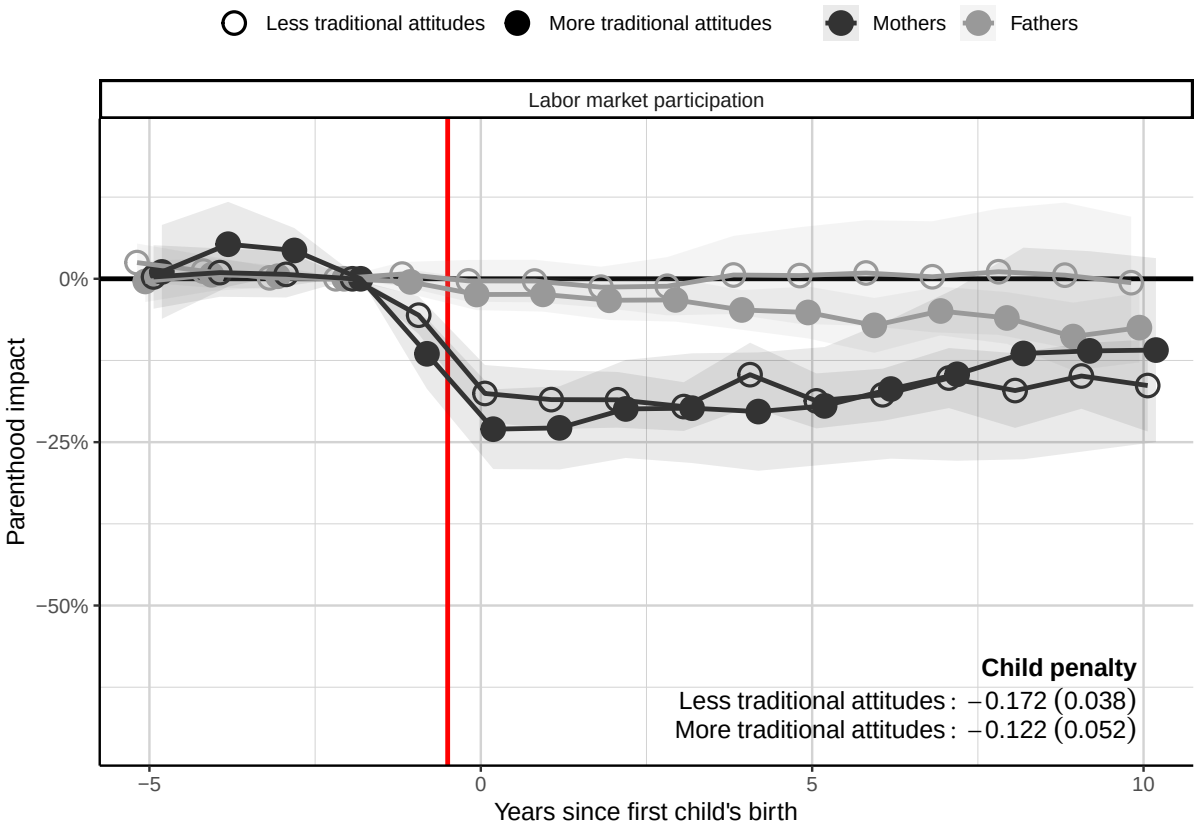


Self-reported political orientation (survey item **I_OPIPOL**) is not included in the delineation of attitudes. Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make attitudes groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure H.11. Child penalty in labor market participation for immigrant parents: by self-reported attitudes, cut at tertiles, balanced on pre-birth characteristics and fertility decisions



Attitudes groups are defined based on tertiles instead of median of the first component of the PCA. Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make attitudes groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

group in this subsample, we estimate:

$$Y_{i,t} = \alpha_t + \sum_{s \neq -2} \beta_s \mathbb{1}\{t = C_i + s\} + \sum_b \gamma_b \mathbb{1}\{B_i + t = b\} + \epsilon_{it} \tag{20}$$

where Y_{it} represents labor force participation for individual i at age t, C_i denotes the age at which she had her first child, and B_i represents the year of her birth. As a result, the β_s coefficients correspond to the child penalty in levels, while the other terms capture non-parametric trends in age and calendar time common to all individuals within a specific gender and attitude group.

Lastly, to ensure consistency with our framework, we consider the relative impact of childbirth in this context, that is:

$$p_s = \frac{\beta_s}{\mathbb{E}[\alpha_t + \sum_b \gamma_b \mathbb{1}\{B_i + t = b\} \mid s = C_i + t]} \tag{21}$$

Figure H.12 presents the corresponding results. The dynamics of labor force participation differ from our baseline estimates: they suggest less negative effects of motherhood on labor supply and a positive effect of fatherhood on labor force participation. They also exhibit a less convincing pre-trend, which may indicate that the additional comparisons upon which this strategy relies are not fully consistent with the additional treatment effects homogeneity assumption upon which this strategy relies. However, we cannot reject the hypothesis that the child penalty is the same in both attitude groups, which is consistent with our conclusion based on a more sophisticated approach.

H.5 Robustness with respect to the inclusion of a particular variable describing the environment in which immigrants were brought up

Our depiction of the upbringing environment encompasses three dimensions: the religiosity of *ego*'s family during her childhood, the imbalance in household chores between her parents, and the number of siblings she had. In this Appendix, we consider less composite representations, distinguishing between these three dimensions.

H.5.1 Changes in the composition of upbringing groups

Figures H.13 to H.15 display the share of immigrant parents characterized as holding more traditional views regarding gender, according to their upbringing environment, each time along one of the aforementioned dimensions. The main takeaway from this exercise is that (i) each of these dimensions makes immigrant parents more likely to hold traditional views; and (ii) this effect is stronger for religiosity and the number of siblings than for gender imbalance in household chores among *ego*'s parents.

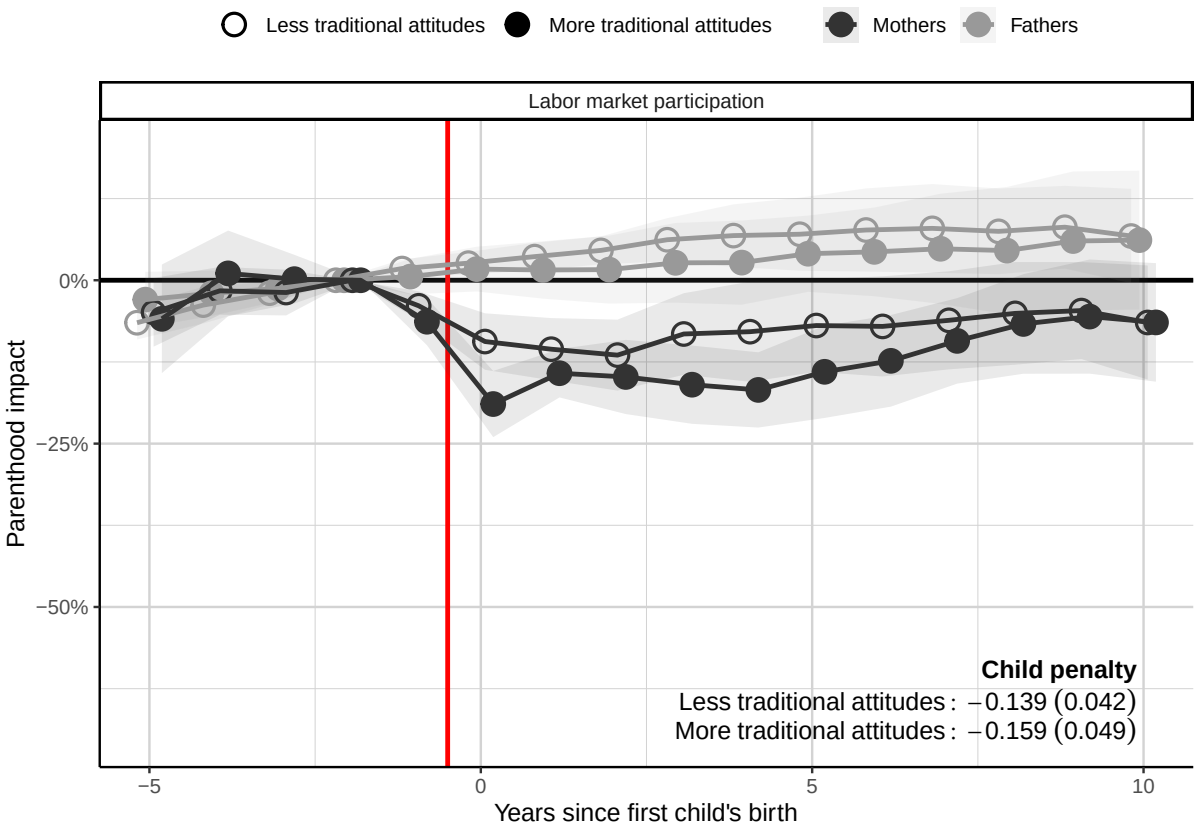
H.5.2 Robustness of the child penalties comparisons

Figures H.16 to H.18 display our estimates of the child penalty for each group defined by our characterizations of the environment in which immigrant parents grew up. Regardless of the dimension we focus on, the child penalties are strikingly similar across groups. This corroborates our finding that growing up in a family where more traditional views were likely to flourish is not correlated with the child penalty in later life.

H.6 Influence of the reweighting procedure on the results regarding different upbringing environments

Figures H.19 and H.20 display our estimates of the child penalties by environment groups, both without any reweighting of the data and after reweighting based on pre-childbirth characteristics, but not on fertility decisions. The key takeaway from this exercise is that our finding—that the child penalty is

Figure H.12. Child penalty in labor market participation for immigrant parents: by self-reported attitudes, balanced on pre-birth characteristics and fertility decisions, child penalty estimation simimlar to Kleven, Landais, and Søgaard (2019)

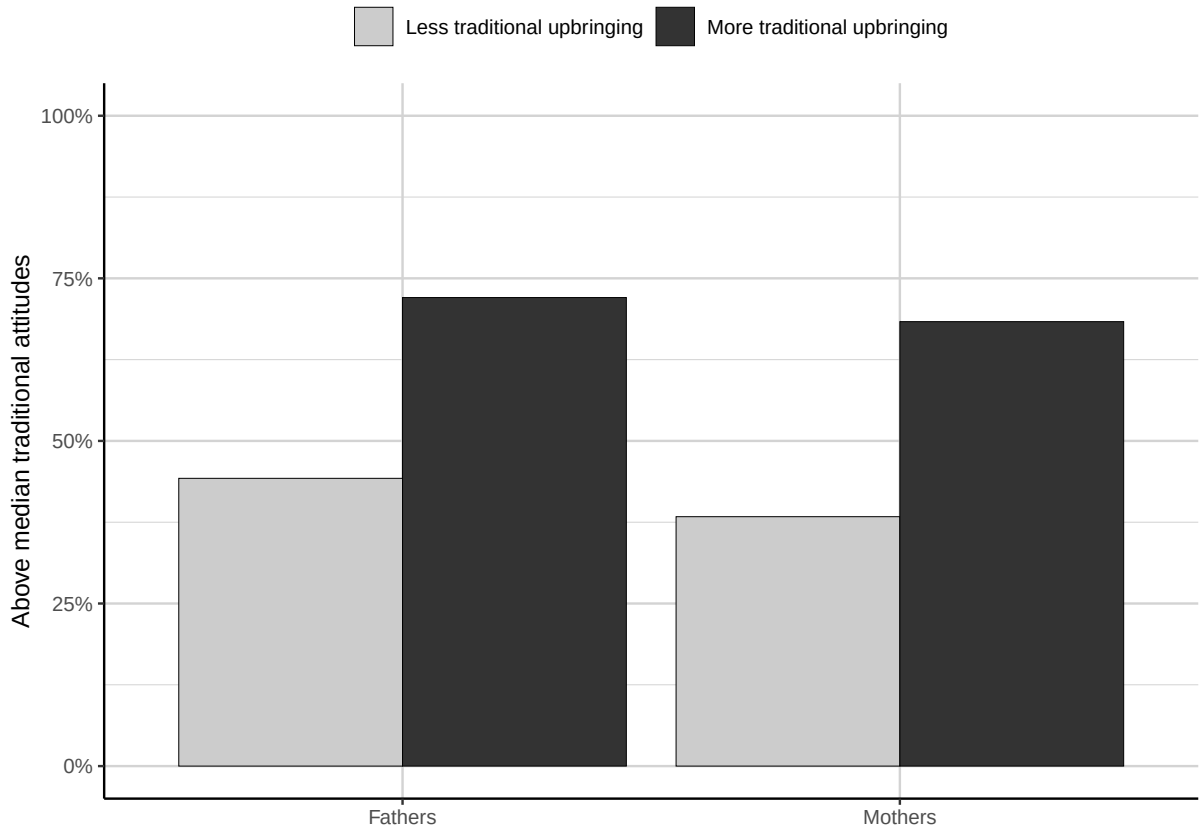


Parenthood impact is identified thanks to Kleven, Landais, and Søgaard (2019)'s approach (see Equation 21), and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make attitudes groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure H.13. Gender-related attitudes by environment in which immigrant parents grew up: by religiosity of the upbringing environment

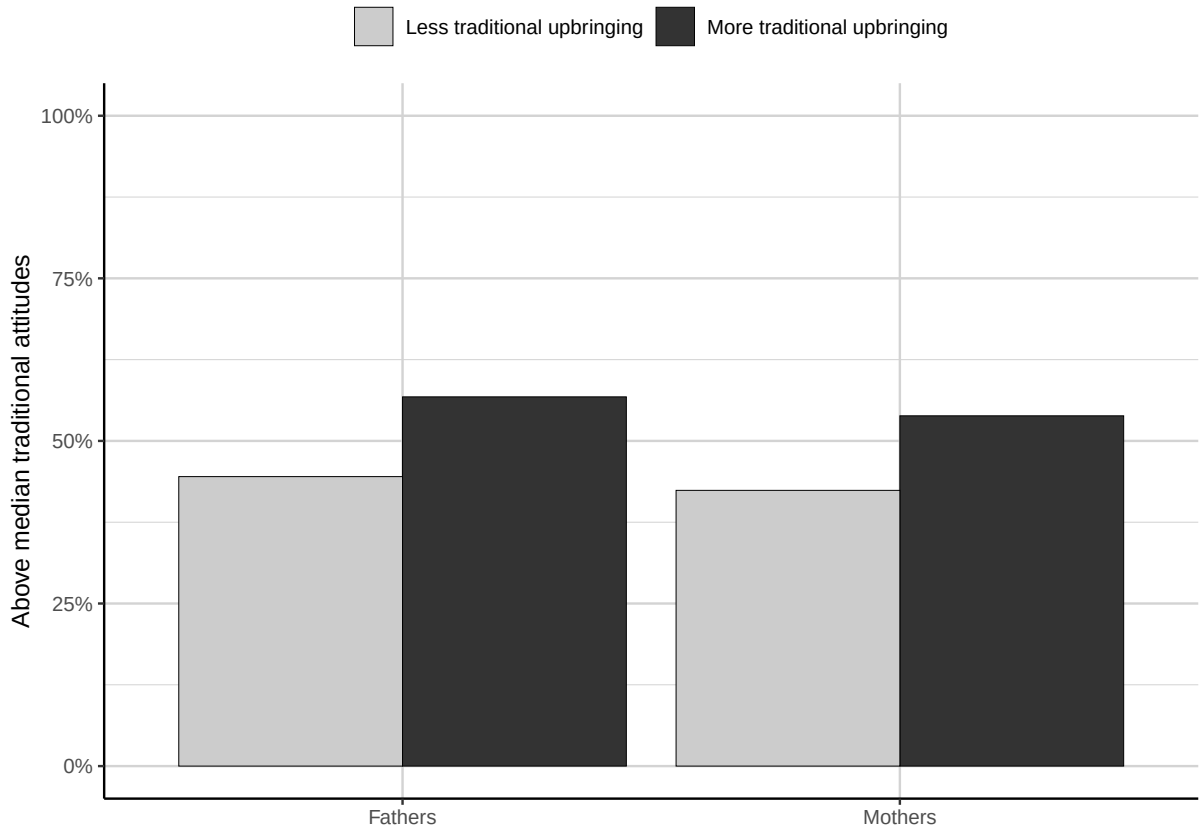


Upbringing environment is measured by *ego*'s father's and mother's having a religion and prominence given to religion in *ego*'s education before the age of 18. Environment groups are defined by immigrants with above or below median predicted gender-related attitudes in the OLS regression of gender-related attitudes on the aforementioned variables. The data are reweighted based on an inverse propensity score approach so as to make upbringing environments groups similar in terms of pre-birth observables characteristics and fertility decisions. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure H.14. Gender-related attitudes by environment in which immigrant parents grew up: : by gender imbalance in household chores

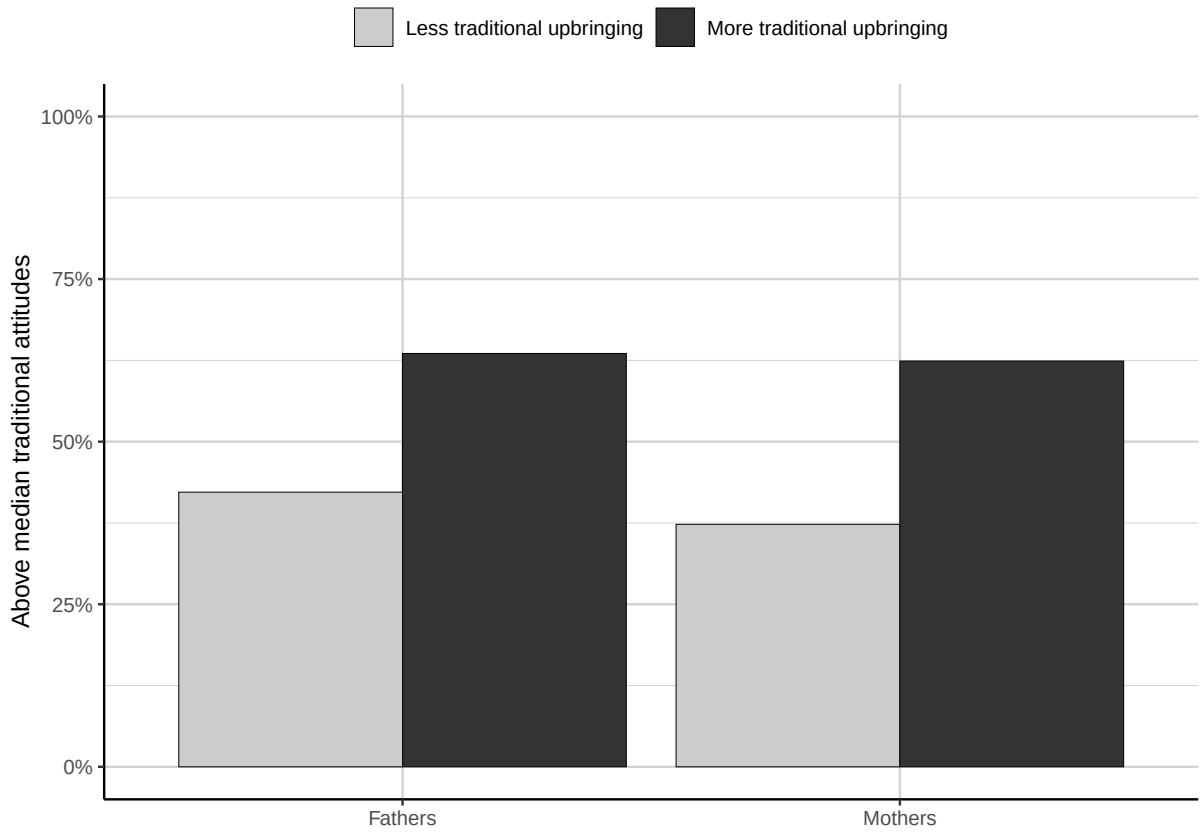


Upbringing environment is measured by gender imbalance between *ego*'s parents' allocation of household chores (daily meals, grocery shopping and clothes washing) before the age of 18. Environment groups are defined by immigrants with above or below median predicted gender-related attitudes in the OLS regression of gender-related attitudes on the aforementioned variables. The data are reweighted based on an inverse propensity score approach so as to make upbringing environments groups similar in terms of pre-birth observables characteristics and fertility decisions. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure H.15. Gender-related attitudes by environment in which immigrant parents grew up: : by number of siblings

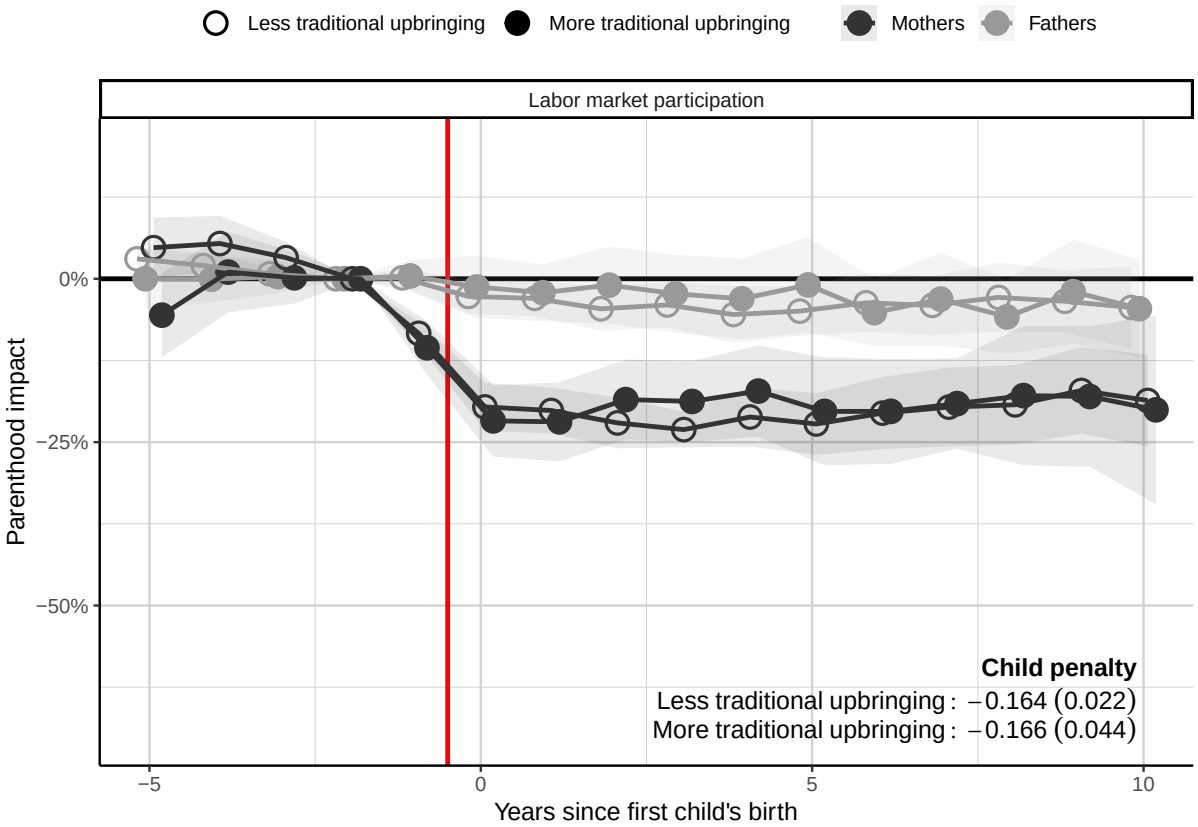


Upbringing environment is measured by the number of children born to *ego*'s mother. Environment groups are defined by immigrants with above or below median predicted gender-related attitudes in the OLS regression of gender-related attitudes on the aforementioned variables. The data are reweighted based on an inverse propensity score approach so as to make upbringing environments groups similar in terms of pre-birth observables characteristics and fertility decisions. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure H.16. Child penalty in labor market participation for immigrant parents: by religiosity of the upbringing environment, balanced on pre-birth characteristics and fertility decisions

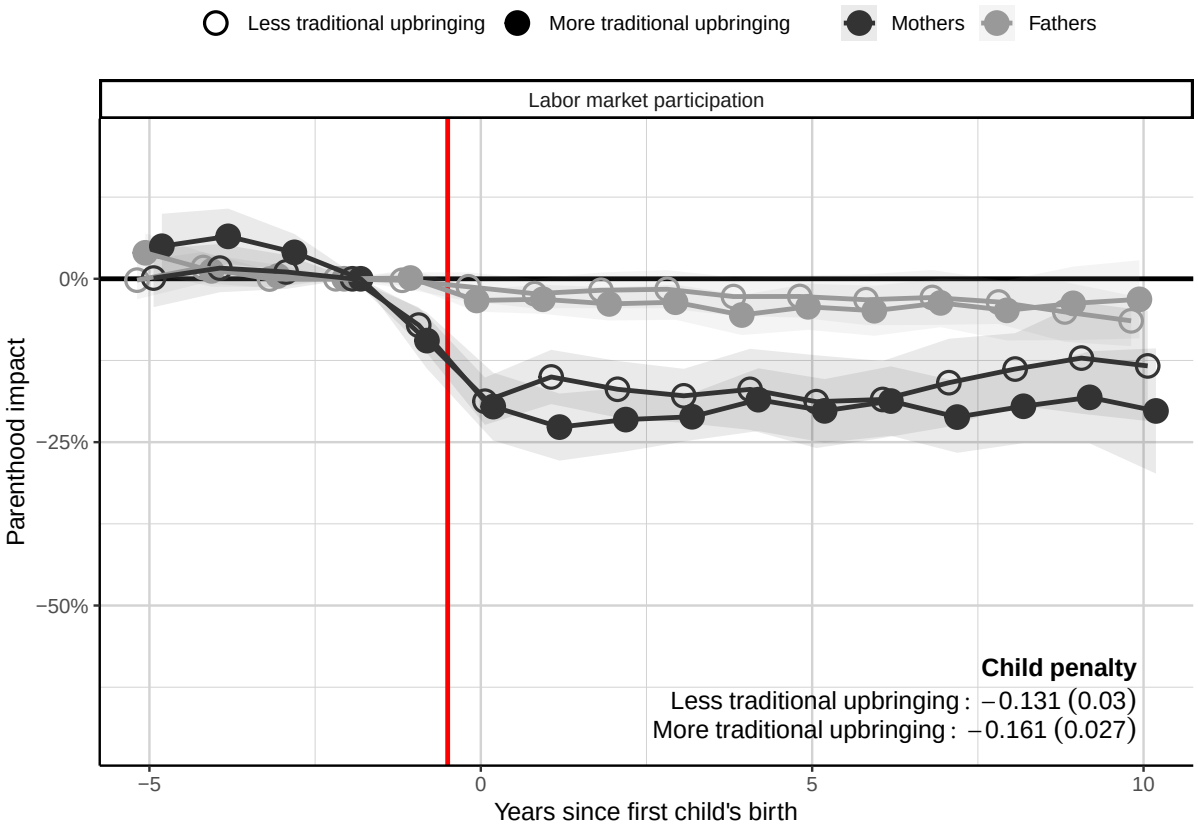


Upbringing environment is measured by *ego*'s father's and mother's having a religion, and prominence given to religion in *ego*'s education before the age of 18. Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make upbringing environments groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure H.17. Child penalty in labor market participation for immigrant parents: by gender imbalance in household chores, balanced on pre-birth characteristics and fertility decisions

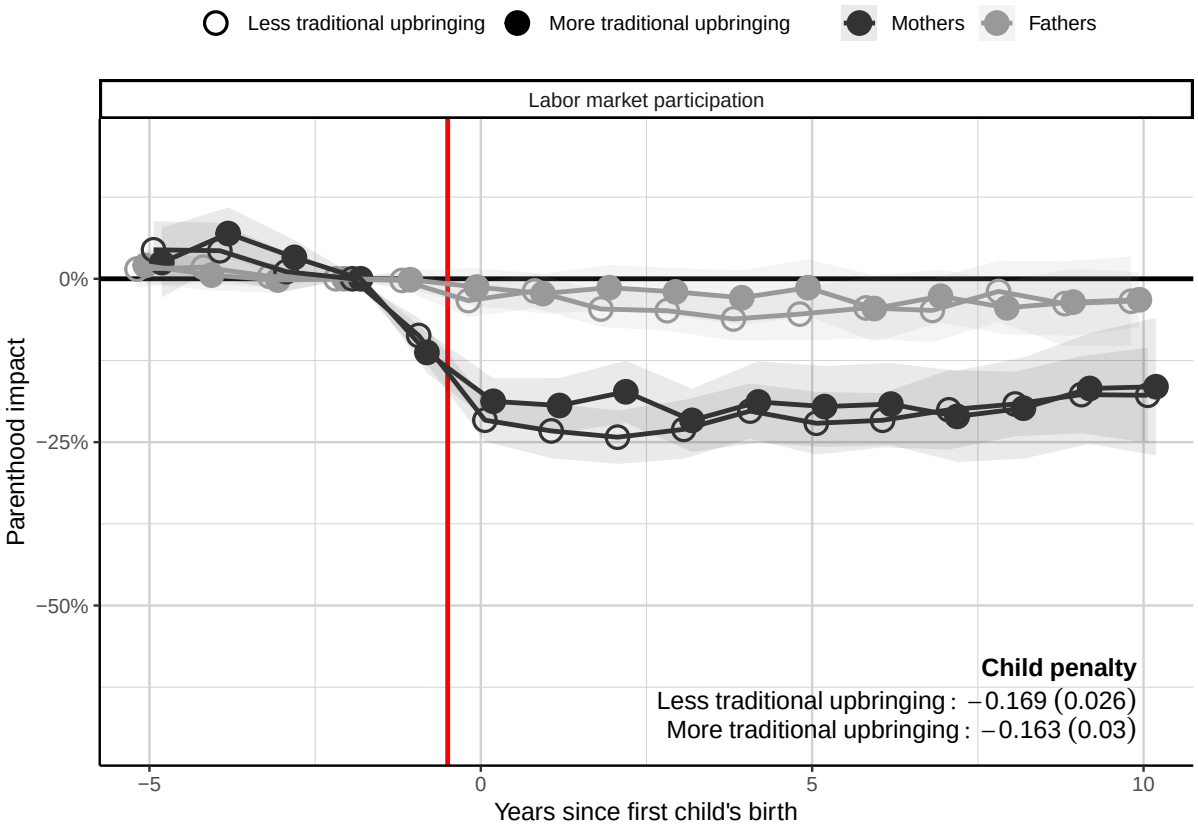


Upbringing environment is measured by gender imbalance between *ego*'s parents' allocation of household chores (daily meals, grocery shopping and clothes washing) before the age of 18. Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make upbringing environments groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

Figure H.18. Child penalty in labor market participation for immigrant parents: by number of siblings, balanced on pre-birth characteristics and fertility decisions



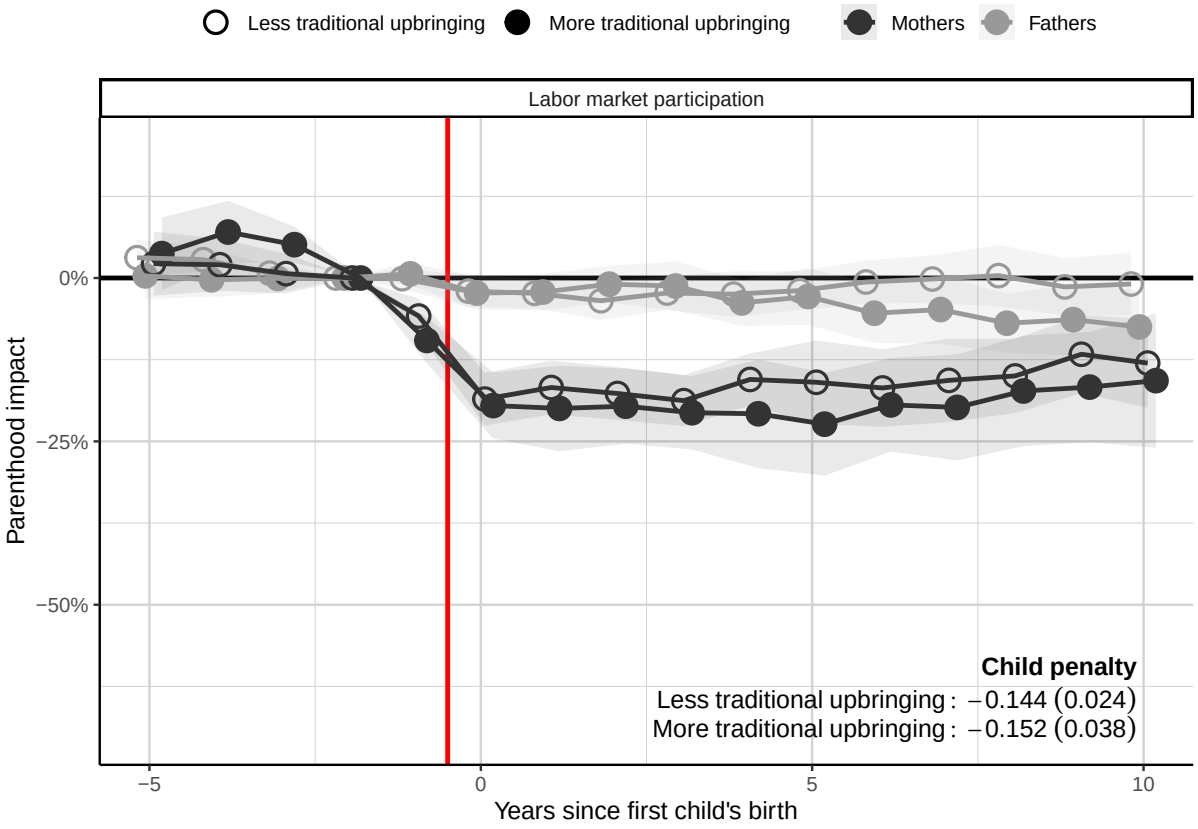
Upbringing environment is measured by the number of children born to *ego*'s mother. Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make upbringing environments groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

not related to the environment in which immigrant parents grew up—does not depend on our choice to reweight the data to make the groups more similar.

Figure H.19. Child penalty in labor market participation for immigrant parents: by upbringing environments, without balancing on observable characteristics



Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

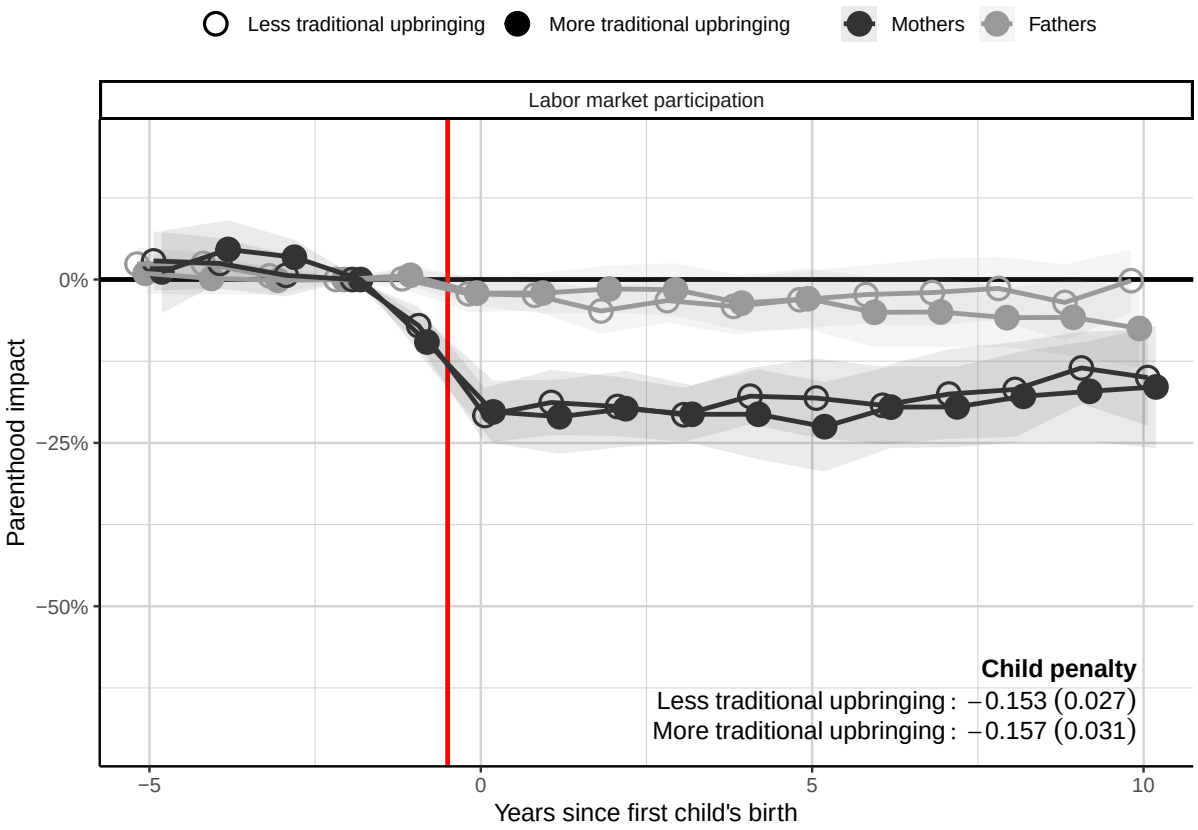
H.7 Robustness with respect to the measurement of parents' peers' attitudes

We focus here on second-generation immigrant parents, defined by the prevalence of traditional views among first-generation immigrants from the same countries as their parents. The purpose of this appendix is to distinguish between transmission through mothers and transmission through fathers.

H.7.1 Changes in the composition of origins groups

Figures H.21 and H.22 display the probability of holding more traditional views regarding gender across groups defined by the attitudes of first-generation immigrant mothers from *ego*'s mother's country of birth, or by those of first-generation immigrant fathers from *ego*'s father's country of birth. The main lesson from this exercise is that, in both cases, the effect of *ego*'s parents' peers' attitudes on *ego*'s

Figure H.20. Child penalty in labor market participation for immigrant parents: by upbringing environments, balanced on pre-birth characteristics



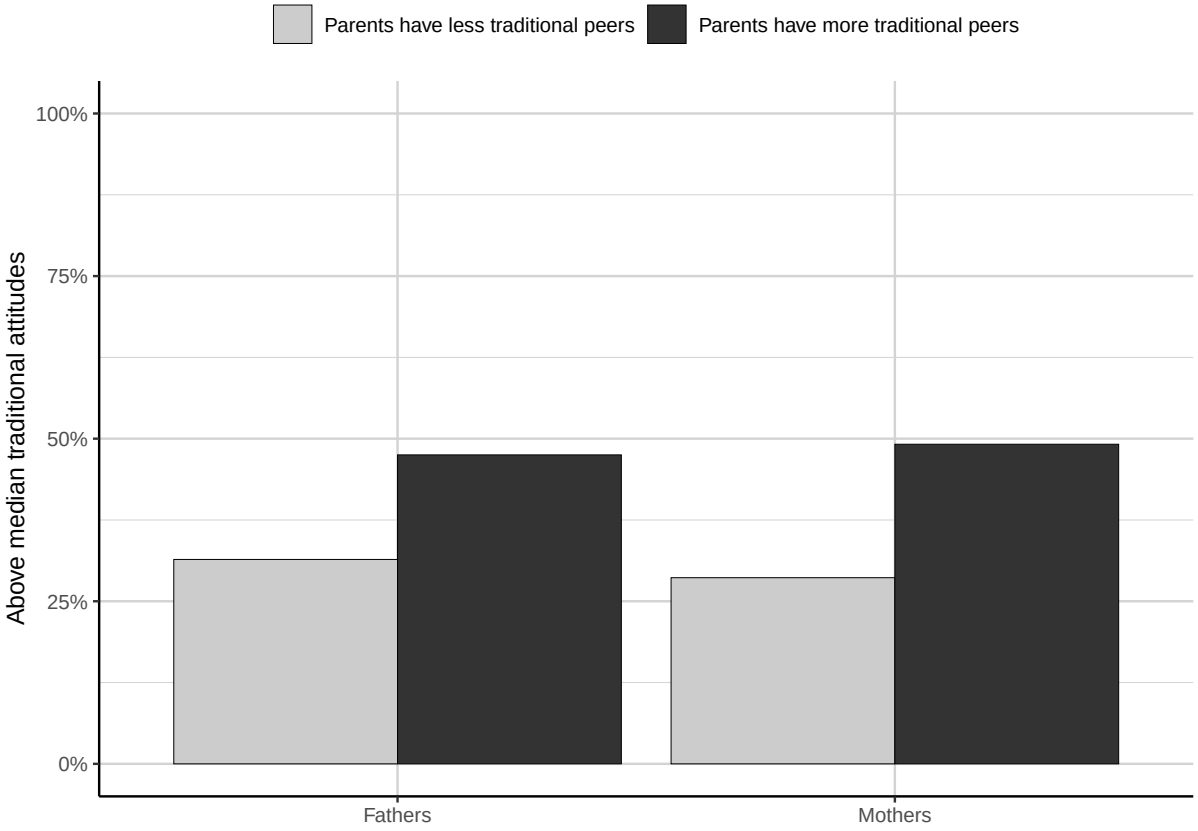
Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual labor market participation rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make upbringing environments groups similar in terms of pre-birth observables characteristics. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.

attitudes is positive and strong. This effect is possibly slightly stronger when considering *ego*'s mother, especially when *ego* is a mother herself.

Figure H.21. Gender-related attitudes by second-generation immigrant parents' mothers' peers' attitudes



Parents' peers' attitudes are measured as the share of first-generation female immigrants from *ego*'s mother's country of birth with above-median traditional gender-related attitudes. Parents' peers' attitudes groups defined by second-generation immigrants with above or below median predicted gender-related attitudes in the OLS regression of gender-related attitudes on the aforementioned variables. The data are reweighted based on an inverse propensity score approach so as to make origins groups similar in terms of pre-birth observables characteristics and fertility decisions.

Population. Second-generation immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

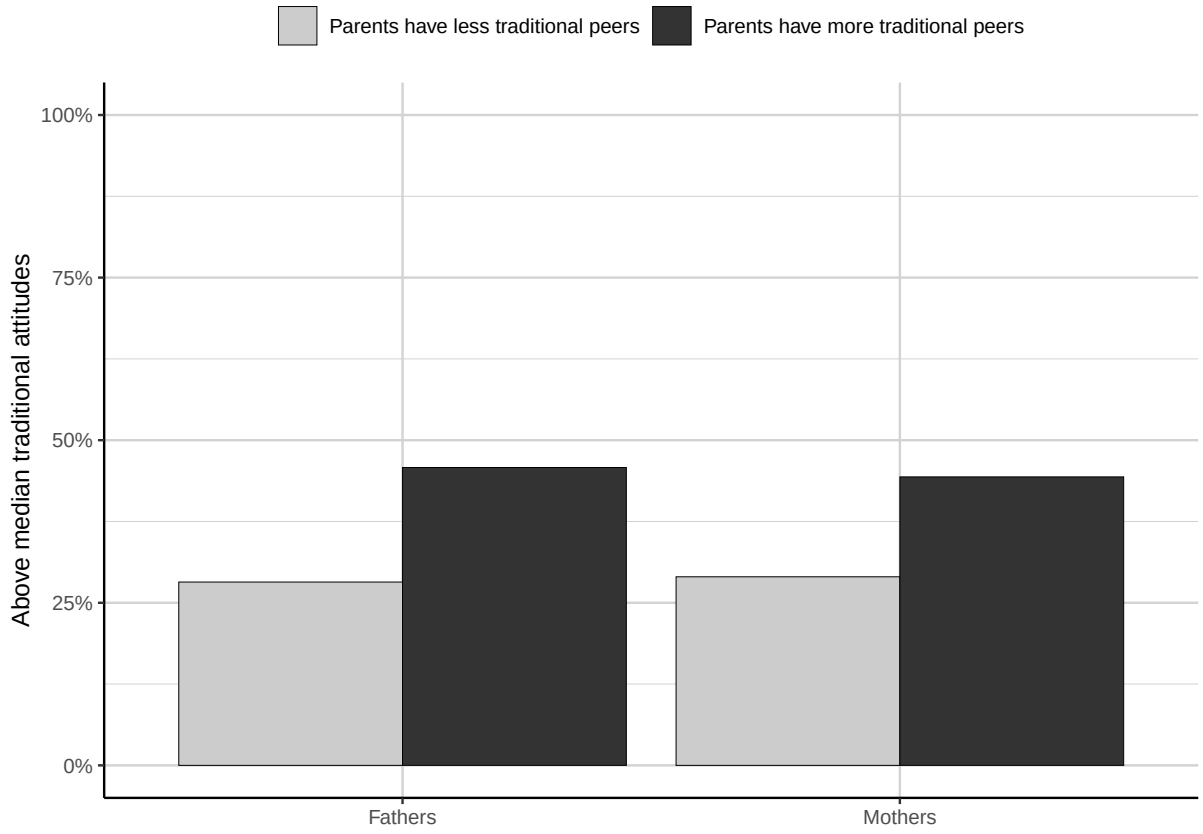
H.7.2 Robustness of the child penalties comparisons

Figures H.23 and H.24 display our estimates of the child penalty across groups defined by *ego*'s parents' peers' attitudes. The main lesson is that the child penalties are very similar across groups, regardless of whether we consider *ego*'s father or *ego*'s mother.

H.8 Influence of the reweighting procedure on the results regarding different countries of origin

Figures H.25 and H.26 display our estimates of the child penalties across groups defined by *ego*'s parents' peers' attitudes regarding gender, depending on whether we reweight the data to make groups more

Figure H.22. Gender-related attitudes by second-generation immigrant parents' fathers' peers' attitudes

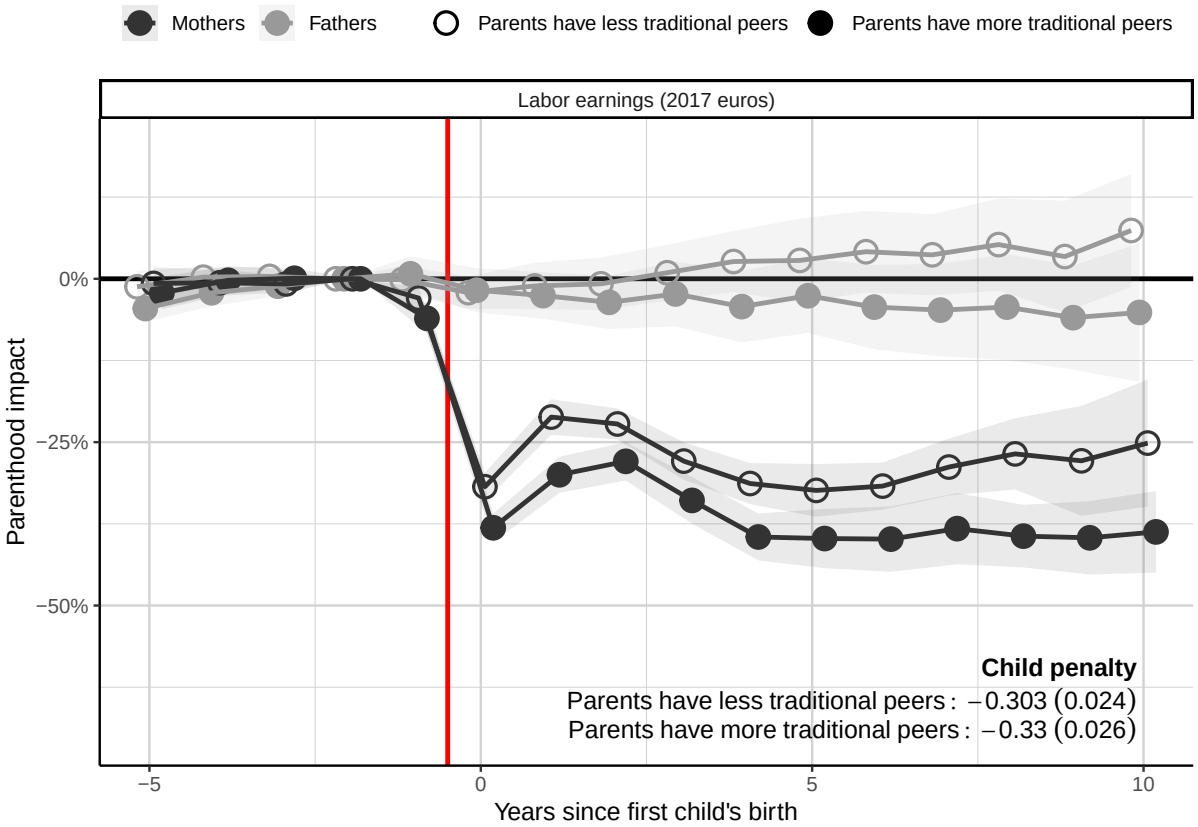


Parents' peers' attitudes are measured as the share of first-generation male immigrants from *ego*'s fathers' country of birth with above-median traditional gender-related attitudes. Parents' peers' attitudes groups defined by second-generation immigrants with above or below median predicted gender-related attitudes in the OLS regression of gender-related attitudes on the aforementioned variables. The data are reweighted based on an inverse propensity score approach so as to make origins groups similar in terms of pre-birth observables characteristics and fertility decisions.

Population. Second-generation immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

Figure H.23. Child penalty in labor earnings for second-generation immigrant parents: by second-generation immigrant parents' mothers' peers' attitudes, balanced on pre-birth characteristics and fertility decisions

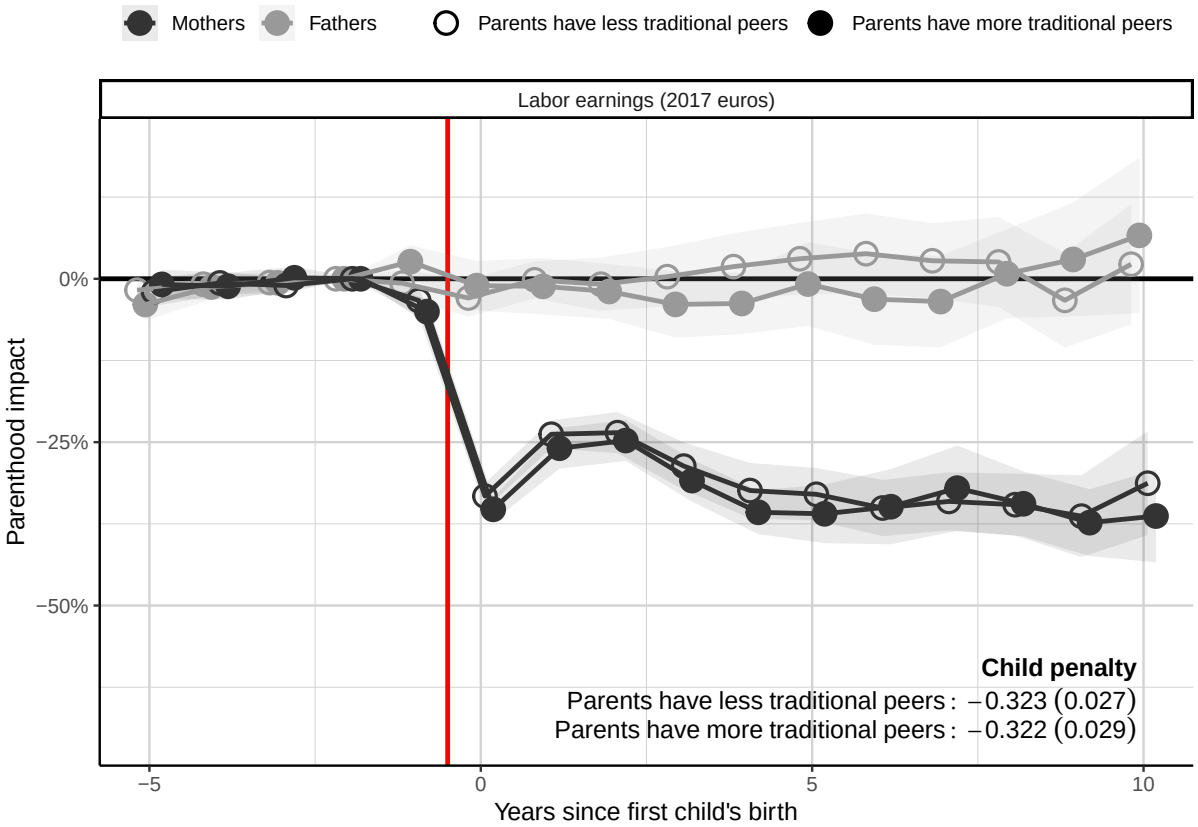


Parents' peers' attitudes are measured as the share of first-generation female immigrants from *ego*'s mother's country of birth with above-median traditional gender-related attitudes. Parenthood impact is identified from comparisons between parents of the same cohort (year of birth) who got their first salaried job at the same time, but had their first child at different ages (at least two years after their first salaried job), and displayed relative to the counterfactual earnings level. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make origin groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level.

Population. Second-generation immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

Figure H.24. Child penalty in labor earnings for second-generation immigrant parents: by second-generation immigrants' fathers' peers' attitudes, balanced on pre-birth characteristics and fertility decisions



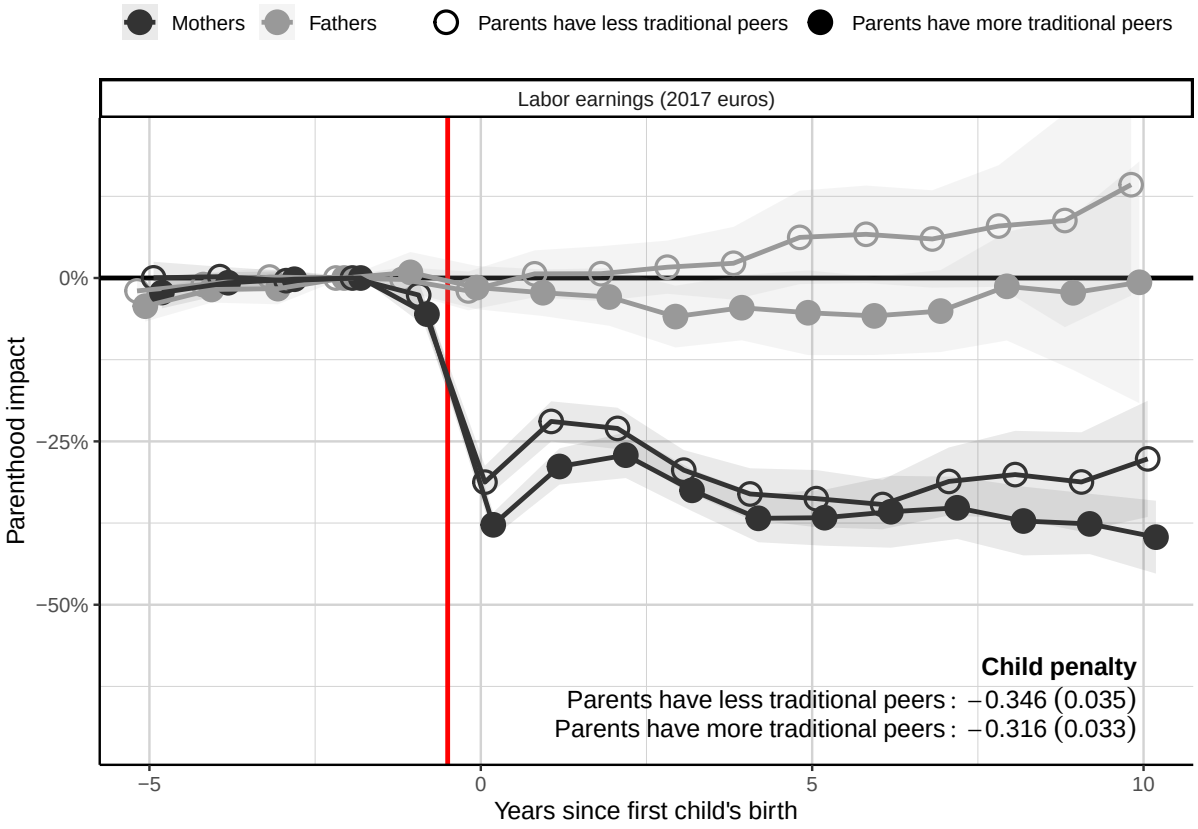
Parents' peers' attitudes are measured as the share of first-generation male immigrants from *ego*'s fathers' country of birth with above-median traditional gender-related attitudes. Parenthood impact is identified from comparisons between parents of the same cohort (year of birth) who got their first salaried job at the same time, but had their first child at different ages (at least two years after their first salaried job), and displayed relative to the counterfactual earnings level. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make origin groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level.

Population. Second-generation immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

similar in terms of observable characteristics, and which characteristics we include. The main lesson is that such choices have little effect on our finding.

Figure H.25. Child penalty in labor earnings for second-generation immigrant parents: by second-generation immigrant parents' parents' peers' attitudes, without balancing on observable characteristics



Parenthood impact is identified from comparisons between parents of the same cohort (year of birth) who got their first salaried job at the same time, but had their first child at different ages (at least two years after their first salaried job), and displayed relative to the counterfactual earnings level. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level.

Population. Second-generation immigrant parents living in mainland France in 2019-2020.

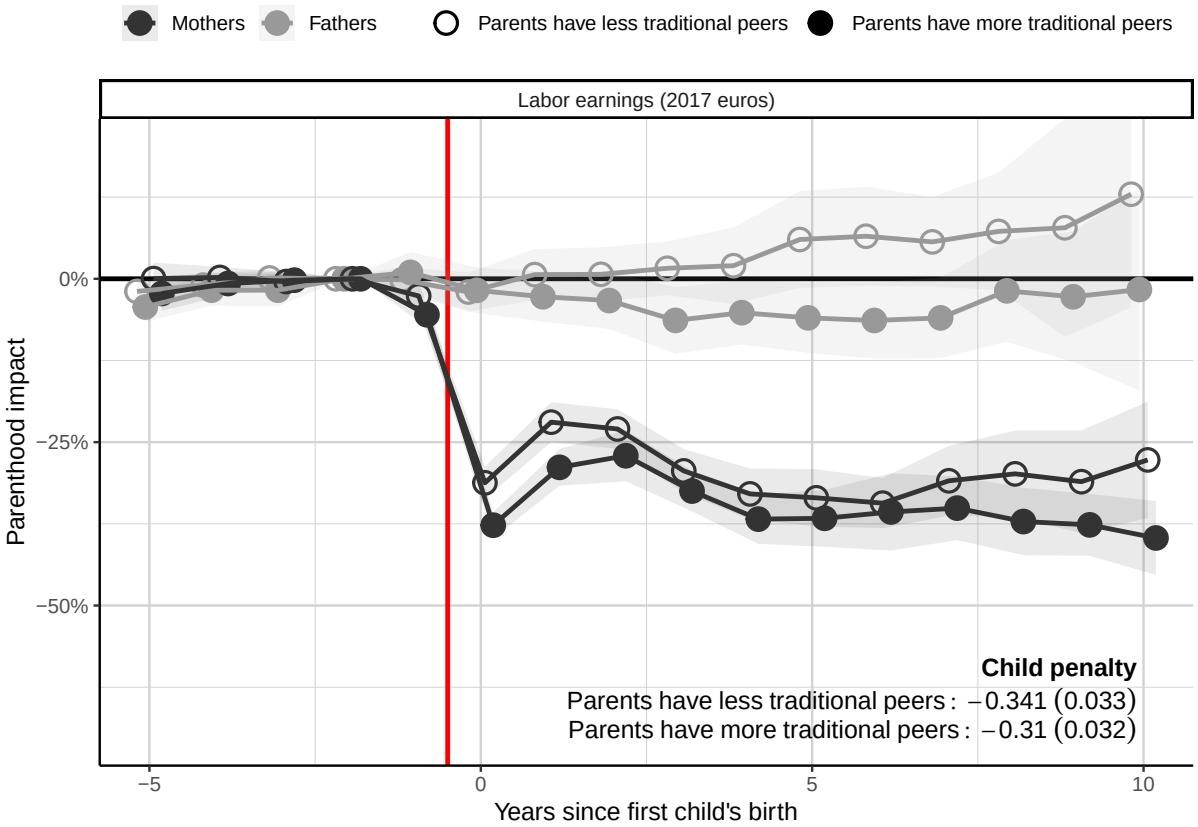
Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

H.9 Robustness with respect to the parallel trends assumption

In this appendix, we compare our estimates of the difference in child penalties across groups defined by *ego*'s parents' peers' attitudes, depending on whether the impact of parenthood is identified from comparisons across parents of the same decennial generation, regardless of other characteristics, or across parents born in the same year who started their first salaried job at the same time.

Figure H.27 displays our estimates in the first case, with the latter being our baseline strategy. The lesson from this exercise is that this choice does change the trend in the impact, but the likely bias remains the same across groups and genders, so it does not affect our finding that the aggregated child penalty is the same across groups.

Figure H.26. Child penalty in labor earnings for second-generation immigrant parents: by second-generation immigrant parents' parents' peers' attitudes, balanced on pre-birth characteristics

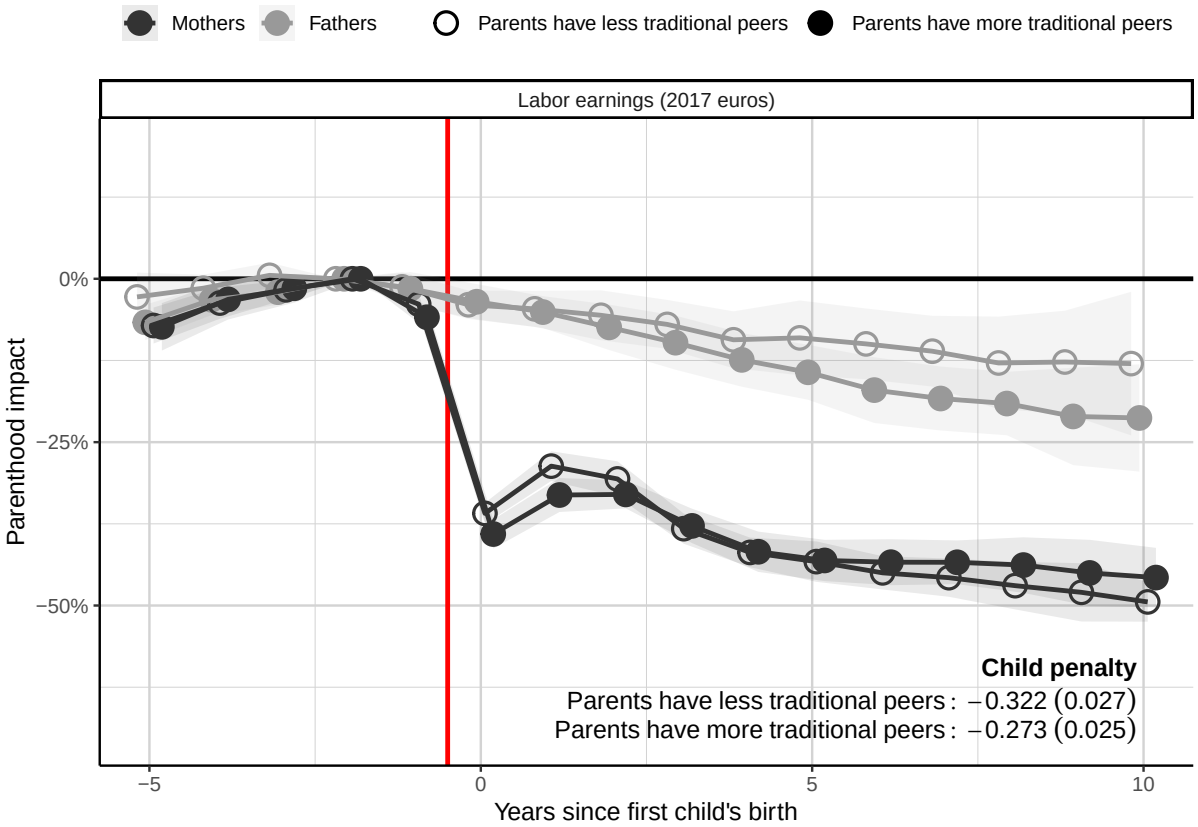


Parenthood impact is identified from comparisons between parents of the same cohort (year of birth) who got their first salaried job at the same time, but had their first child at different ages (at least two years after their first salaried job), and displayed relative to the counterfactual earnings level. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make upbringing environments groups similar in terms of pre-birth observables characteristics. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level.

Population. Second-generation immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

Figure H.27. Child penalty in labor earnings for second-generation immigrant parents: by second-generation immigrants' parents' peers' attitudes, balanced on pre-birth characteristics and fertility decisions



Parents' peers' attitudes are measured as the share of first-generation female (resp. male) immigrants from *ego*'s mother's (resp. fathers') country of birth with above-median traditional gender-related attitudes. Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual earnings level. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make origin groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level.

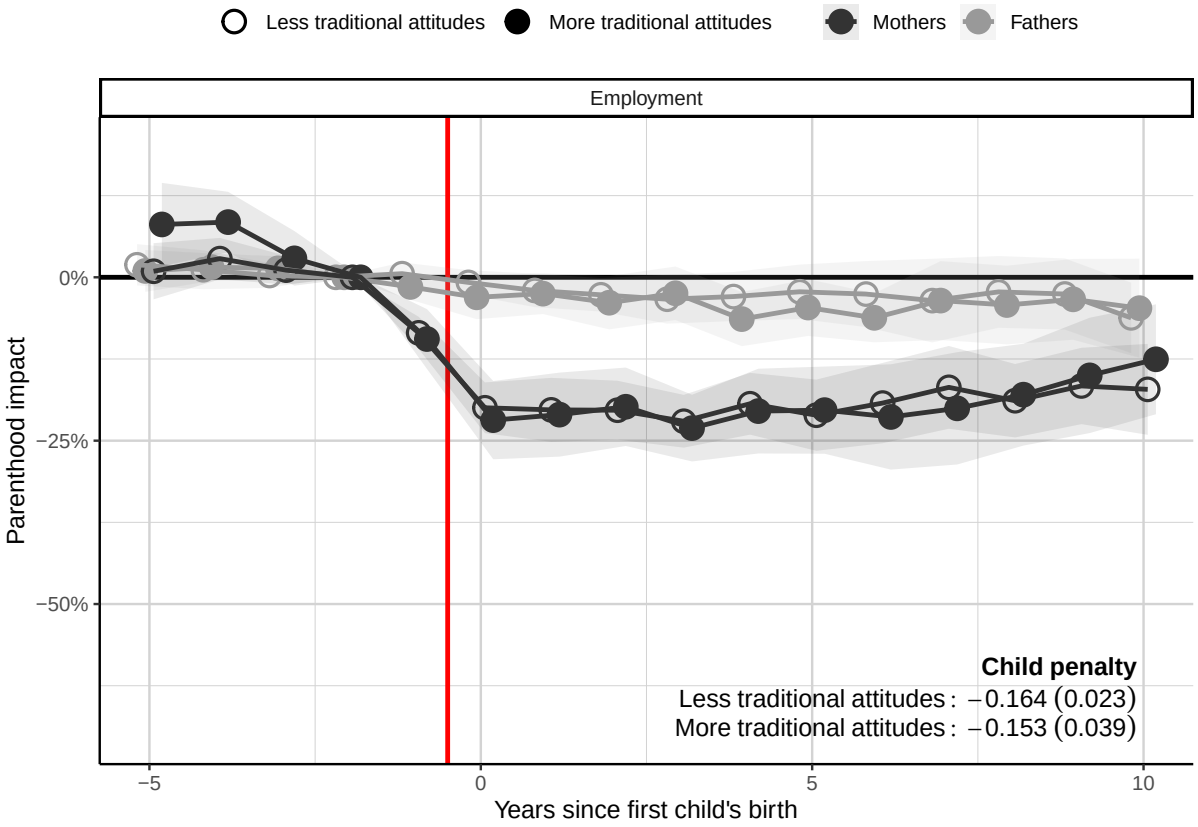
Population. Second-generation immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), CCMSA, Cnaf, Cnav, DGFiP and Insee, permanent demographic sample (EDP), authors' calculation.

H.10 Robustness with respect to the choice of labor outcome

Figure H.28 displays our estimates of the child penalties across attitude groups when the outcome of interest is no longer labor market participation, but actual employment. Our result remains the same: the child penalties are very similar across groups. Since they also closely match those obtained when considering labor market participation, this suggests that unemployment is not a common response to motherhood among immigrants living in France.

Figure H.28. Child penalty in employment for immigrant parents: by self-reported attitudes, balanced on pre-birth characteristics and fertility decisions



Parenthood impact is identified from comparisons between parents of the same decennial cohort who had their first child at different ages, and displayed relative to the counterfactual employment to population rate. The child penalty is the difference in parenthood impact between men and women, averaged over the first 10 years after their first child is born. The data are reweighted based on an inverse propensity score approach so as to make attitudes groups similar in terms of pre-birth observables characteristics and fertility decisions. Shaded areas correspond to 95% confidence intervals; they are based on a reweighted bootstrap approach, clustered at the individual level. First-generation immigrants are only included in the computation after they first arrived in France.

Population. Immigrant parents living in mainland France in 2019-2020.

Source. Ined and Insee, Trajectoires et Origines 2 survey (2019-2020), authors' calculation.