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# The Child Penalty in the Netherlands and its Determinants *

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**Abstract**

Having children can result in large earnings penalties for mothers. Using extensive administrative data from the Netherlands, we assess the magnitude and drivers of the effects of first childbirth on parents' earnings trajectories in the Netherlands. We show that mothers' earnings are 46% lower compared to their pre-birth earnings trajectory, whereas fathers' earnings are unaffected by child birth. We examine the role of two potential determinants of the unequal distribution of parents' labour market costs by gender: childcare policies and gender norms. We find that while child care availability is correlated with lower child penalty, the immediate short-term causal effect of increasing child care availability on the earnings penalty of becoming a mother is small. By taking advantage of variation in gender norms in different population groups, we show that gender norms are strongly correlated with child penalty for mothers.

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# 1 Introduction

Despite important changes towards more gender equality in the past century, the gender pay gap is still large in many countries. The OECD reports that in 2018, men earned 13% more than women in the Netherlands (OECD, 2021).[1] While differences in education between men and women were one of the main drivers of the gender wage gap in the second half of the 20th century, the remaining largest contributors to the gap nowadays is driven by women reducing paid work to care for their children and by men not doing so (Blau and Kahn, 2017).

In this paper, we use administrative data on the full universe of the Dutch population of parents to estimate the effect of children on parents' labour market trajectories, following the methodology of Kleven et al. (2019b) to calculate the so-called *child penalty* - the relative impact of children compared to pre-birth trends for both mothers and fathers; and the relative impact of children for mothers compared to fathers. We also implement a rich set of heterogeneity analyses to investigate the role played by the two main potential contributors of the child penalty: i) childcare availability and ii) norms.

The Netherlands is an interesting setting to study the child penalty, because of particular gender norms with regard to labour supply decisions. Part-time work is wide-spread even among men, they are twice as likely to work part-time as the OECD average (OECD, 2021). Furthermore, female labour market participation is high (OECD, 2019). At the same time, the one-and-a-half earners household model is very prevalent, meaning that men often work (close to) full-time, and women part-time. This implies that it is not clear a priori whether the child penalty in the Netherlands can be expected to be large or small compared to other countries. On the one hand, the part-time work of men may attenuate the child penalty for women and distribute the labour market costs of having children more evenly in couples. On the other hand, extensive part-time work of women may lead to an even larger child penalty, if only women reduce their hours of work after child birth.

There is a large body of literature analysing the effect of children on parents' labour supply (for example Angelov et al., 2016; Angrist and Evans, 1998; Bertrand et al., 2010; Lundborg et al., 2017; Rosenzweig and Wolpin, 1980). This line of research has known important developments in recent years, following Kleven et al. (2019b). They compare the relative impact of children over mothers' and fathers' careers – the child penalty –, and show that the child penalty is large for women and non-existent for men, and that this difference explains much of the remaining gender gap in Denmark. Their approach has

[1] The Netherlands is close to the OECD average of 12.8%. The Dutch gender wage gap is relatively high compared to countries like Denmark (4.9%), but also considerably lower than others such as for example the US (18.5%).

been applied in many other countries,[2] yielding qualitatively consistent yet quantitatively different results. On one hand, for almost all countries we observe a large drop in earnings for mothers at the birth of children, and virtually no effect for fathers. On the other hand, the magnitude of estimated child penalties varies a lot: it is for example three times bigger in Germany than in Denmark (Kleven et al., 2019a).

This large variability in child penalty for mothers across countries raises the question of which factors determine the size of the child penalty. Whereas there exists no study that reunites all potential explanations in one framework, there is separate evidence on two potential institutional drivers that differ by country: family policies and gender norms.[3] First, the child penalty may differ by country because there are different family policies in place. The findings of the existing literature show that policies increasing child care availability have a very limited influence on the child penalty (for example Andresen and Nix, 2020; Kleven et al., 2020). On the other hand, countries with longstanding traditions of generous family policies like the Nordic countries have lower child penalties compared to countries with less generous or more recent family policies (Kleven et al., 2019a; Olivetti and Petrongolo, 2017). This seems contradictory, but may be reconciled if such policies mostly have long-run effects by changing norms.

Second, different culture and norms about gender and parenthood may also contribute to the difference between child penalties by country. For example, in a country where the prevailing norm is that both parents are responsible for child care, the child penalty is likely to be lower than in a country where it is customary that child care is exclusively a women's task. In the literature, it has been established that gender norms impact female labour market outcomes (Akerlof and Kranton, 2000; Bertrand, 2011; Bursztyn et al., 2017; Fernández, 2007; Fernandez and Fogli, 2009; Fortin, 2015; 2005; Steinhauer, 2018; Vella, 1994); and that norms are passed on generation by generation through the family or other social environment (Farré and Vella, 2013; Fernández et al., 2004; Fogli and Veldkamp, 2011; McGinn et al., 2019; Olivetti et al., 2020).

So far, the role played by culture and norms for the child penalty has only sparsely been examined. First, Rellstab (2021) analyses the differences in child penalty in the Netherlands between areas with a high concentration of orthodox protestants other areas. This difference is likely to be driven by gender norms, as there are no meaningful differences on the parent and municipal level before the birth of the first child between the two areas.

[2]Including Kleven et al. (2019a) for Austria, Germany, the US, the UK, Denmark and Sweden, Sieppi and Pehkonen (2019) for Finland, Andresen and Nix (2020) for Norway, Pora and Wilner (2019) for France and de Quinto et al. (2020) for Spain.

[3]An alternative explanation for the child penalty is preferences, i.e. that women have a preference for child care and men do not. Whereas a part of our preferences is likely to be determined genetically and hence may vary by biological sex, another part is determined by the environment we are surrounded by. Since the world we live in is highly gendered, it is impossible to separate preferences from norms.

Second, Kleven et al. (2019b) show that when the grandmother had a low labour market involvement when raising her children, the mother has a larger child penalty. Since this result persists when controlling for socio-economic background, it is likely to be driven by the passing on of gender norms from mother to daugther.[4] Third, there is a set of papers comparing child penalties between same-sex and mixed-sex parent couples (Andresen and Nix, 2020; Moberg, 2016a; Rosenbaum, 2019), finding that same-sex mothers have a considerably lower child penalty than mixed-sex mothers. Since gender norms are less informative for the division of labour in same-sex couples, the child penalty in same-sex couples gives an idea of what child penalties could look like without the influence of gender norms.

We establish the following results. First, we find a relatively large earnings penalty in the Netherlands, as earnings of mothers are estimated to be 46% lower than predicted absent childbirth, and fathers' earnings are not affected. This large penalty in earnings is mainly driven by a reduction in hours worked, and to a lesser extent by women quitting paid work. When exploring the potential contribution of family policies to this child penalty, we find a strong negative correlation between childcare availability and child penalty levels on municipal level, suggesting some connection between childcare policies and child penalties for mothers. We however only find a small effect of childcare expansion on the child penalty, at least in the short run. Third and lastly, we uncover large heterogeneity in child penalty by couple composition, migration background and religiosity of the environment, pointing towards the importance of norms.

We make the following contributions to the literature. First, we add another country to the growing body of analysis of child penalties, using high quality administrative data. As the relative importance of the different determinants of child penalty is still largely unknown and difficult to uncover using natural experiments, international comparisons are key to shed light on the underlying mechanisms. In the case of the Netherlands, we show that part-time work is an important driver of the child penalty, as women reduce their working hours at childbirth whereas men do not. While the institutionalisation of part-time work over the last decades in the Netherlands enabled women to participate in the labour force participation in the first place (Bosch et al., 2010; Tijdens, 2006), it may nowadays act as a brake to further reduction in the gender gap when mainly taken up by women.[5] This is in line with the recent findings of Fernández-Kranz and Rodríguez-Planas (2021) for Spain, who shows that part-time policies can be detrimental to gender

[4]Gender norms of the parents also affect sons. Farré and Vella (2013) finds that mothers' attitude is correlated with sons' attitude; and men with less egalitarian gender norms during their youth are more likely to have spouses with a lower labour supply during adult life.

[5]While Dutch men are the champions in part-time work compared to the other OECD countries (19.4%), the Netherlands is also champion in the gender gap in part-time work: with 38 percentage points, it is the largest among OECD countries (**?**).

equality when they are most often taken up by women.

Second, using geographical variations in the timing of childcare expansion as source of identification, we analyse the causal effect of childcare on the child penalty. We find that a large increase in access to child care leads to a small reduction in mothers' child penalty in the short run. Our results are in line with the recent studies by Kleven et al. (2020) and Andresen and Nix (2020), who find only limited immediate effects of increasing access to childcare on the child penalty. However, we also exhibit strong (negative) correlations between access to childcare and child penalty, suggesting possible more complex interactions between policies and child penalty that cannot be causally captured, for example through long-term impact on gender norms.

Third and lastly, we contribute to the literature on the role of norms for the child penalty by using three types of variations in gender norms that occur in the Dutch population: couple gender composition, religiosity, and migration background. We add to the evidence of more equal division of labour in same-sex couples for the Netherlands that has been established for other countries by Andresen and Nix (2020); Moberg (2016b); Rosenbaum (2019). Second, we use the correlation between non-egalitarian gender norms and religiosity to proxy gender norms on municipality level. This builds on Rellstab (2021), who shows that the child penalty is substantially higher in areas with high concentration of orthodox protestants in the Netherlands despite similar pre-birth parental and municipal characteristics. We analyse a more general indicator of religiosity that includes not only orthodox protestants but also other religious denominations. Finally, we add to the literature on the portability of culture and its effect on parental labour market outcomes (Fernández, 2007; Fernandez and Fogli, 2009) by establishing the child penalty for different migration backgrounds. Contrary to what Nieto (2021) found for Spain, we provide suggestive evidence that differences in child penalty are not driven by socio-economical determinants such as educational attainment, leaving cultural norms as a possible residual explanation. Even if our results are not causally established, taken together they provide evidence that gender norms are likely to be an important driver of the child penalty.

The rest of the paper proceeds as follows. The next section presents the data and the empirical strategy we use. We then present the main estimation results on the child penalty in the Netherlands in section 3. Section 4 presents our analyses on the effect of childcare policies on the child penalty, and section 5 the effect of gender norms. Those results are discussed and put in perspectives in section 6.

# 2 Data and empirical strategy

## 2.1 Data

We use administrative data from Statistics Netherlands (CBS) accessible through a secured remote access. This data is available for the full universe of the Dutch population, and different data sources such as the municipality registers or tax records can be matched through unique individual or household anonymized identifiers. We present below the main variables used and the sample construction. More details on the data sets we use can be found in Appendix C.

**Socio-demographic variables** For each individual, we observe demographics such as gender, date of birth, date of death or migration background.[6] We use information on birth date and linkage between parents and their children to determine the first child for all legal parents, which may include both adoptive or biological parents. We also observe education attainments at each point in time for a subset of parents. We focus on tertiary education, for which we have information on degrees completed since the mid 1980s.[7] Lastly, we have yearly information on the municipality of residency, household composition, and migration spells.

**Labour market outcomes** Our main labour market outcomes are based on the *BAAN* datasets which contain information of the full universe of contract jobs for years 1999-2016. It contains yearly information on wages and job characteristics for 1999-2016 and the percentage of work (share of full time equivalent FTE worktime) for 2001-2016.

We analyse four labour market outcomes: unconditional earnings, the probability of employment, paid hours worked and wage rate. Employment is specified as having a job based on an employment contract between a firm and a person.[8] Second, the earnings data consists of yearly gross earnings after social security contributions, but before taxes and health insurance contribution from official tax data. Third, hours worked and associated wage rates are computed using the the conditional full-time equivalent (FTE), which is a measure of part-time work, and represents working hours as a ratio of reference full-time hours. The full-time reference hours are defined by sector or firm by Statistics Netherlands. In order to determine hours work from the available FTE variable, we impute for each sector a reference full-time hours, based on observed hours distribution

[6] Migration background includes country of birth but also country of births of the parents for descendants of migrants.

[7] As the oldest cohort we consider are born in 1960, we observe high education attainment for a large majority of our sample.

[8] Self-employment is not included because of a change in recording of income from self-employment. We use an alternative measure including income from self-employed as a robustness check. This is not included in the main analysis because it is available for fewer years.

available in another dataset.[9] Appendix B presents the data construction we use in more details, together with some comparisons between the observed and imputed distributions of hours worked.

Finally, we also use data on job location that we use to compute our index of childcare supply per municipality (see Appendix B.2). The job location data set is provided by Statistics Netherlands and contains the municipality of each job, measured in December. When there is more than one establishment for a given firm, the impute the closest establishment to the worker's residential address as the job location.

**Sample(s) construction** Our initial population includes all parents in the Netherlands with children born between 1999 and 2016 that are between 20 and 45 years old when their first child is born. To estimate the effect of childbirth on labour market outcomes (see next subsection), we need to observe individuals' trajectories before and after the birth. For the main estimation, we limit our sample to parents for whom we observe labour market outcome from four years before birth to seven years after birth. Since hours worked are available only from 2001 onward, this implies restricting the sample to parents with children born between 2005 and 2009. We use a balanced sample in event-time, so we only include parents that are observed in every year in that time window. We test for alternative approaches for sample selection in the robustness analysis of section 3.

When conducting our heterogeneity analyses in sections 4 and 5, we estimate the effect of child-birth on smaller units, which requires increasing the sample size and using less strict sample definitions. We first focus on earnings and participation only, in order to includes years 1999-2000 in the estimation. Second, we consider a smaller window for the event, between four years before birth and five years after birth. This implies that we consider parents with a first child-birth born between 2003 and 2011.

## 2.2 Empirical strategy

We estimate the child penalty as suggested by Kleven et al. (2019b). By gender, we estimate Equation (1), where $y_{it}$ is the labour market outcome. We control for a set of event time indicators ranging from 3 years before first child birth up to 7 years after.[10] The baseline is chosen at $q = -3$, three year before child birth, as we observe a change in the earnings trajectory before birth between men and women. This change in earnings

[9] The inputation of the full-time reference hours are based on the hours worked in the *polis* data, which is available from 2006 onward and which covers the full universe of jobs.

[10] This implies that the child penalty measure represents the total impact of all children independent of the total number of children. Kleven et al. (2019b); Sieppi and Pehkonen (2019) show that the penalty tends to be larger the more children a woman has, but that there is no difference by the number of children for men.

trajectories may for example be explained by women sorting into more family friendly jobs, or discrimination. We include controls for age and year fixed effects.

$$y_{it} = \sum_{q=-4}^{7} \alpha_q \mathbb{1}[eventtime_{it} = q] + \sum_{k=18}^{53} \beta_k \mathbb{1}[age_{it} = k] + \sum_{t=2002}^{2016} \gamma_t \mathbb{1}[time_{it} = t] + \nu_{it} \quad (1)$$

In order to estimate the child penalty $P_q$, we compute, by gender, the relative change in the outcome due to the child compared to the pre-birth trajectory. This is expressed as the event time coefficient relative to the counterfactual outcome $\tilde{y}_{it}^q = \sum \hat{\beta}_k \mathbb{1}[age_{it} = k] + \sum \hat{\gamma}_t \mathbb{1}[time_{it} = t]$, which is the predicted labour market outcome at event time $q$ in absence of a child. The counterfactual is thus calculated based on not-yet parents, and not with a formal control group. The child penalty represents the percentage change in the outcome due to the child, and is computed as follows, with $\hat{y}_{it}^q$ the predicted value at event time $q$:

$$P_q = \frac{\tilde{y}_{it}^q - \hat{y}_{it}^q}{\tilde{y}_{it}^q} = \frac{\hat{\alpha}_q}{\tilde{y}_{it}^q} \quad (2)$$

The child penalty $P_q$ specifies the percentage gain/loss in the labour market outcome due to the child compared to people of the same gender and cohort without children yet. To compare mothers with fathers, the relative child penalty $P_q^r$ (the percentage change of women $w$ compared to men $m$) is calculated as follows:

$$P_q^r = \frac{\hat{\alpha}_q^m - \hat{\alpha}_q^w}{\tilde{y}_{it}^{q,w}} \quad (3)$$

We follow five cohorts of parents over seven years after birth. That means that the calendar year estimate of the year 2010 onward, or the age estimate of older parents, (and more generally part of the counterfactual estimates) are based solely on people that are already parents. When comparing child penalty estimates when we follow the same cohorts over a shorter period of time, and hence the problem is smaller, we find very comparable estimates of the child penalty (robustness analysis in the next section). This is evidence that the bias introduced by missing counterfactual is likely to be small.

Having a child is potentially endogenous to income and its determinants. However, a causal effect can be identified if two assumptions are satisfied. First, there needs to be a

sharp discontinuity at the arrival of the child. Second, there are no sharp discontinuities in other determinants of income at child birth that are unrelated to the child. Kleven et al. (2019b) provide evidence that the simple event study framework controlling for age and time fixed effects gives similar results as methods where the arrival of a child is instrumented (using the sex-composition of the first two children as an instrument for the third), and a difference-in-differences model comparing people with and without children.

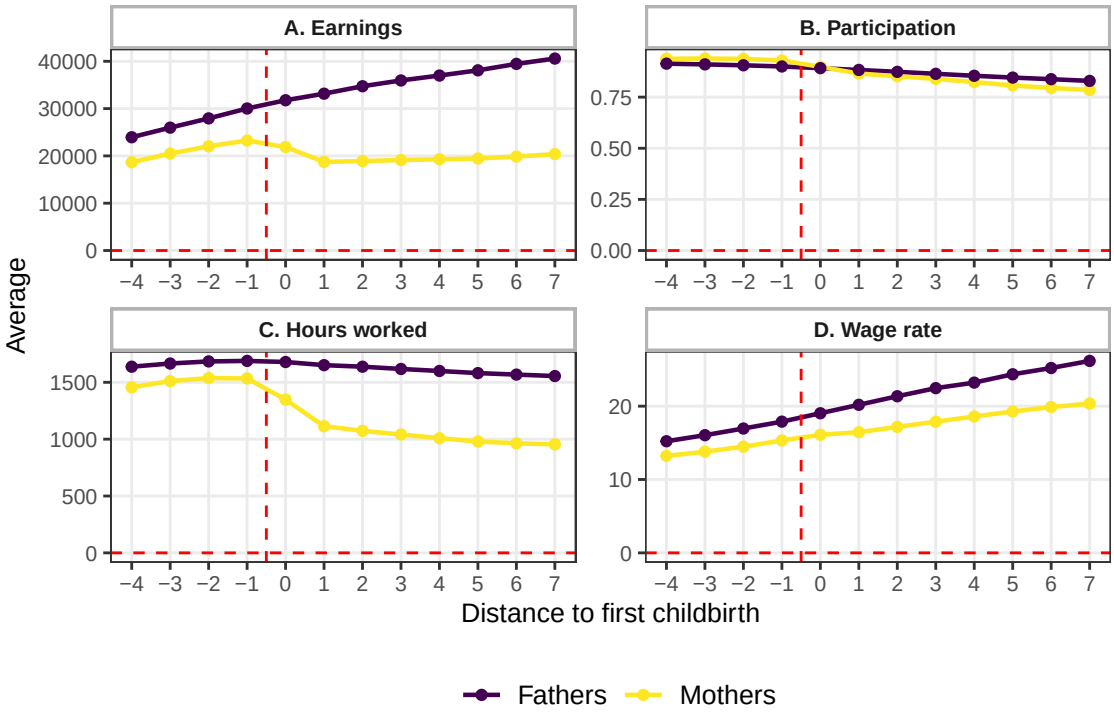
Given the selection of the data and the method of analysis, the estimate is a lower bound estimate of the true child penalty, because it does not take into account pre-child selection more than three years before birth. For example, if women select into less valued educations or lower paying more flexible jobs, then the child penalty would be even larger had they not sorted into low paying jobs.

# 3 The child penalty in the Netherlands

## 3.1 Descriptive statistics

A first assessment of the effect of children on mothers and fathers careers is given in Figure 1, which presents the evolution of labour market outcomes for parents, by time away to the birth of their first child. For all outcomes considered (earnings in panel A, participation in panel B, hours worked in panel C, wage rate in panel D), fathers and mothers exhibit a similar pattern before the birth. Even if the level is higher for men for all outcome except for participation, the evolution is roughly similar for fathers and mothers. The patterns however strikingly diverge at childbirth. Earnings of the fathers evolve smoothly at birth, but mothers experience a large drop at the moment of the birth, and retain a flatter earnings profile afterwards. This differential effect of the first childbirth on earnings is also visible on the hours worked, and to a lesser extent on participation in the labour market. Wage rates slightly diverge after birth, but we do not observe any discontinuity at birth for either fathers or mothers.

Figure 1: Main labour market outcome by distance to first child birth




NOTE: This Figure presents the average by event-time (distance to the first child birth) for the different labour market outcomes we consider: yearly earnings (panel a), labour force participation (panel b), yearly hours worked (panel b) and wage rate defined as earnings/hours worked for those who are working (panel d).

Figure 2: Estimated child penalty by distance to first child birth


NOTE: This Figure presents the estimated child penalty for each event-time, as defined in equation (2), for mothers and fathers and for different labour market outcomes. The relative child penalty (cf. equation 3) in t=7 is added to each plot.

## 3.2 Child penalty estimation

In order to quantify the magnitude of the differential effect of child birth on mothers' and fathers' careers, we compute the child penalties as described in the previous section. We estimate equation (1) and use the estimated coefficient to compute the counterfactual earnings and the associated child penalties. Counterfactual outcomes, *i.e* predicted outcome in absence of childbirth based on age and years dummies, are presented in Figure A.1. We predict that, absent the birth of the child, earnings as well as participation, hours work and wage rate would have increased steeply for mothers. In contrast, counterfactual profiles for fathers are almost identical to the ones predicted in the presence of childbirth, since for fathers, having a child seems to have a limited effect on labour market outcomes. The resulting child penalties are shown in Figure 2 which presents, for each event time, the percentage of the difference between the counterfactual and the predicted earnings (and other labour market outcomes), as expressed in equation (2). We first consider total labour earnings (panel A) and then its decomposition in different channels (panel B to D).

**Earnings penalty** During the first years after the birth, fathers earn slightly less with a child than compared to they had not had one. But this reduction fades away seven years after the birth. Mothers, on the other hand, experience a strong penalty immediately after the birth (35% less earnings than in absence of child after 2 years), which amplifies over the years and reaches 46% after 7 years. Note that this longer run penalty also includes the effect of potential additional births occurring after the first one. From the fathers' (absence of) penalty and the mothers' one we can compute the relative child penalty (cf. equation 3), expressing the relative impact of children for mothers compared to fathers. We present the relative child penalty after 7 years in each panel of Figure 2. Since fathers' penalty is equal to zero, the relative child penalty is of similar magnitude as mothers' child penalty (46%).

**Channels** This penalty in earnings following childbirth can be further decomposed in different channels, namely labour force participation, hours worked and wage rate. For all those outcomes, we qualitatively observe the same divergence pattern after the arrival of the child birth: a large drop for mothers and only mild effects for fathers. The magnitude of the gap, however, varies a lot between the different channels. The long run relative penalties are similar for participation and wage rate (around 15%). The estimated penalty for hours worked is however much bigger (46%). This suggests that the widely spread use of part-time work for mothers is the main driver of the large child penalty in earnings we observe. This is in line with Frey (2019) which suggests that the gender gap in earnings in the Netherlands is mainly explained by a gender differentiated use of part-time work. This is also in line with the findings of Fernández-Kranz and Rodríguez-Planas (2021), who shows, using a recent reform in Spain, that part-time policies can be detrimental to gender equality when they are most often taken up by women.

This result implies that the availability of part-time work, which historically contributed to a decrease in gender inequality, nowadays is a hindering factor for gender equality. The first part-time jobs in the Netherlands were created in a context of labour shortage in the 1960s to enable married women to participate in the labour force, while still being able to do unpaid work at home. This tendency was further institutionalised in the 1980s, and Dutch households transitioned from the male breadwinner model with low female labour market participation into a gendered one-and-a-half-job-households where the man was the breadwinner and the women works part-time and takes care of children and household (Bosch et al., 2010; Tijdens, 2006). This evolution resulted in a gendered take-up of part-time jobs. Even though offering part-time jobs has even been turned into a legal obligation for employers (Wielers and Raven, 2013), part-time options are more readily available in typically female-dominated sectors than in typically male-dominated sectors (Merens and Bucx, 2018). Hence, the gendered one-and-a-half-job-households model with part-time work of women combined with (close to) full-time work of men, contributes to the large

child penalty in the Netherlands.

**Comparison with other countries** How does our estimates for the Netherlands relate to the other child penalties estimated in the literature? Table 1 compares the results obtained in different papers. Results are made comparable by reporting the penalty in the same year (seven years after birth).[11] We do not report the relative child penalty as it is not always computed on the same time window and cannot be directly retrieved from graphs or tables. We only present mothers' penalty as fathers' penalty is negligible in every country. As the different papers estimate the penalty on different years and are not fully consistent regarding sample selection, the following comparison is only suggestive. Yet, the review of the child penalties indicates that the 46% child penalty in the Netherlands is relatively high compared to other countries. It is lower than in Germany (-60%), but comes next with a group of countries with a penalty around 45%-50% (alongside UK and Austria). It is also much larger than the small penalties measured for Denmark and Norway (around -20%).

Table 1: Comparison with other countries

| **Country** | **Birth years** | **Mother penalty in t = 7** | **Source** |
|---|---|---|---|
| Norway[a] | 2001-2017 | -21% | Fig.1.a in Andresen and Nix (2020) |
| Denmark | 1985-2003 | -22% | Fig 1 in Kleven et al. (2019a) |
| Spain | 1994-2009 | -30% | Fig.1 in de Quinto et al. (2020) |
| Sweden | 1997-2011 | -30% | Fig. 1 in de Quinto et al. (2020) |
| Finland | 1992-2007 | -31% | Fig. 1 in Sieppi and Pehkonen (2019) |
| France | 2005-2015 | -32% | Fig. 12 in Meurs and Pora (2019) |
| US | 1967-2006 | -40% | Fig. 2 in Kleven et al. (2019a) |
| UK | 1991-2008 | -44% | Fig. 2 in Kleven et al. (2019a) |
| Netherlands | 2005-2009 | -46% | Figure 2 above |
| Austria | 1985-2007 | -50% | Fig. 3 in Kleven et al. (2019a) |
| Germany | 1989-2005 | -60% | Fig. 3 in Kleven et al. (2019a) |

[a]We report the penalty for t=5, the latest event used for the estimation in this study.

**Robustness** Finally, we conduct a sensitivity analysis to assess the robustness of the estimation of the child penalty to alternative outcome definition and sample selection. Figure 3 presents the long-term penalty for earnings. It corresponds to the value of the mothers' and fathers' penalty in t=7 in the panel A of Figure 2, which is reported again in the "Ref" column. We first compute the penalty of a smaller (resp. larger) windows of birth dates, in order to estimate longer (resp. shorter) run child penalty. In RT1, we use an alternative measure of earnings based on the *secm* dataset,[12] which includes income from self-employed. The estimated penalty is roughly robust to this alternative definition. RT2

[11]Even if they are all computed according to the methodology from Kleven et al. (2019b), differences in the reported penalties can be partly driven by differences in data or time window used in the estimation.

[12]See Appendix B for a description of this data source.

estimates equation (1) for children born between 2003 and 2006, which makes it possible to estimation a penalty until t = 10. This longer term penalty is slightly bigger for mothers, suggesting a small increase in the long-run child penalty. However, we also find a penalty for fathers with this specification, which casts some doubts on the validity of the estimation when too few cohorts are included. RT3 estimates the penalty of a larger set of cohort (births between 2003 and 2011), for which the longer penalty observable is for t = 5. The resulting estimated penalty is equal to 43%, slightly lower than in the reference. RT4 uses t-1 instead of t-3 as the year of reference in the estimation. The estimated penalty is only slightly smaller (45% for earnings), showing that anticipation effect are negligible compared to the actual effect of the birth. Finally, RT5 and RT6 present alternative approaches for the construction of the estimation sample. In RT5, we do not restrict to individuals observed over the full window of observation (t-4 to t+7). In RT6, we further relax the selection of parents, by keeping all the available birth years (2000 to 2016). In both cases, the estimated penalty is higher (around 50%). These robustness checks suggest that, while the child penalty estimate varies slightly according to sample definition and data used, the main conclusion of the analysis does not change: the child penalty for women is substantial, and is likely to lie in the range between 43 and 50%. In contrast, the child penalty for fathers is negligible.

# 4 Effect of childcare policies

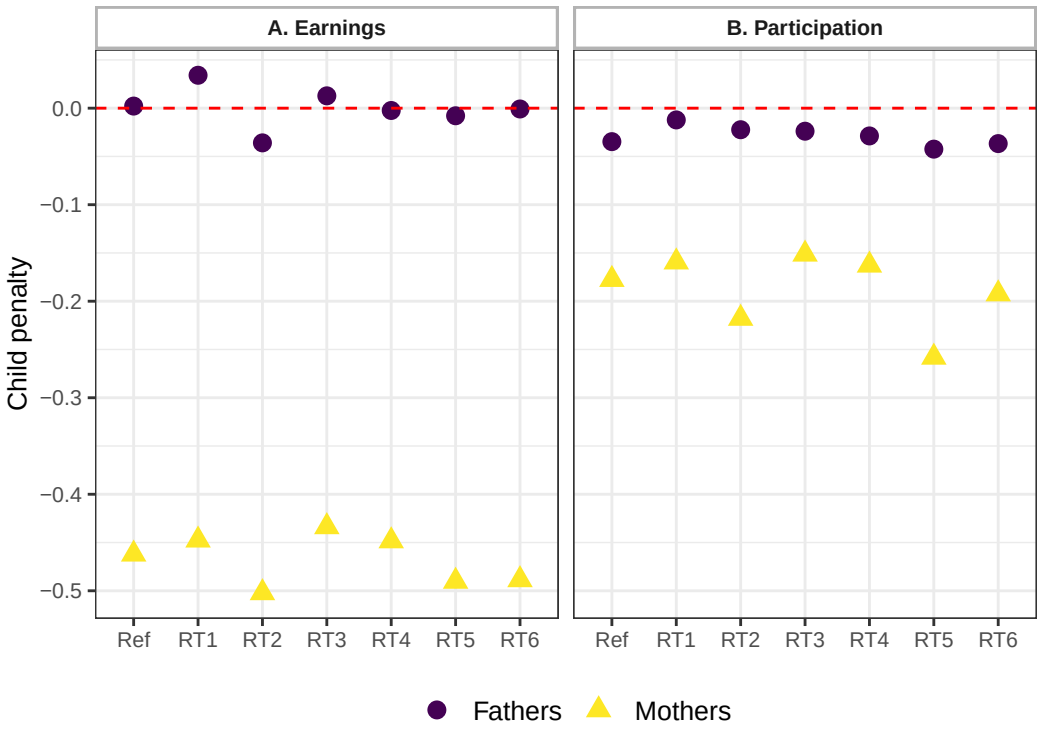
## 4.1 Childcare policies in the Netherlands

In this section we study the effect of childcare policies on the child penalty. We start with a short overview of subsidised childcare and its recent reforms in the Netherlands.[13]

**Before the 2005 reform** Before the introduction of a unified system in 2005, centre-based daycare was subsidised at different rates. The majority of places was subsidised directly by employers and local governments. These places had lower effective parental fees than 'unsubsidized' places (24%), the costs of which were however partly tax deductible for parents. To qualify for the subsidies and tax deduction, all parents in the households must be active on the labour market. The total enrolment rate of children under three in centre-based care was around 25% in 2004. Aside from centre-based care, a large share of children (around 25%) also go to playgroups (*peuterspeelzalen*). This is part-time care for less than 4h per day and is therefore mostly used by households in which one of the parents does not work. Children that are in primary school can go to centre-based out-of-school care and informal care. Like for daycare, before the reform subsidized and unsubsidized

[13] A detailed presentation of the institutional setting can be found in Bettendorf et al. (2015) or Adema et al. (2019).

Figure 3: Robustness tests: long-term earnings child penalty, for different specifications




NOTES: This Figure presents the estimated child penalty 7 years after the birth for different specification. The first column corresponds to the specification used in Figure 2. The next columns correspond to the six alternative specifications (see the text for details):

RT1: Alternative measure of earnings based on *secm* datasets.
RT2: Longer run penalty (t = 10), estimated on births occurring between 2003 and 2006
RT3: Shorter run penalty (t = 5), estimated on births occurring between 2003 and 2011
RT4: Estimation of t-1 instead of t-3 as a reference.
RT5: Estimation without restricting to a balanced sample.
RT6: Estimation on all dates of birth (2003 to 2015).

centre-based care places co-existed, and the costs of unsubsidized places were partly tax deductible for parents. The pre-reform enrolment rate of 4–12 year old in centre-based care was below 6% in 2004.

Before the 2005 reform, the accessibility to child care was not uniform. More or less (sometimes) arbitrary circumstances could determine the access to and the price of child care. There could be situations where the employer of both parents would contribute to the child care costs, whereas sometimes none or only one employer would contribute. Moreover, there were substantial differences between municipalities. Some municipalities would contribute to child care if the employer was also contributing, other municipalities would not. Some municipalities would reserve the child care spots provided by the municipality for a certain target group. There was also substantial variation in the costs for parents, both at the child care providers subsidised by the companies and at those subsidised by municipalities.

**After the 2005 reform** The introduction of 2005 reform unified the subsidies for centre-based care. From 2005 on, all centre-based places qualified for the same subsidy from the government, and subsidies were received by parents using formal care instead of childcare institutions. This increased the subsidy for parents with children going to an unsubsidized place before 2005, since the pre-reform tax deduction was typically lower than the post-reform subsidy. Besides unequal accessibility, another issue addressed by the law was that the quality standards and the quality control were not uniform across municipalities.

## 4.2 Descriptive statistics on childcare policies and child penalty

In this section, we present some correlations between childcare policies and the level of mothers' child penalty. We limit the analysis to mother's, since father's child penalty is non-existent, and hence child care policies are unlikely to matter for father's child penalties. We focus on the pre-reform situations, leveraging the municipality- and sector-based variations embedded in the childcare system at that time. As explained above, before the implementation of a unified childcare system, subsidised childcare was mainly available through municipalities or employers. We construct two indexes of childcare availability before the reform based on municipality and sector, and then analyse how they relate to the level of child penalty.

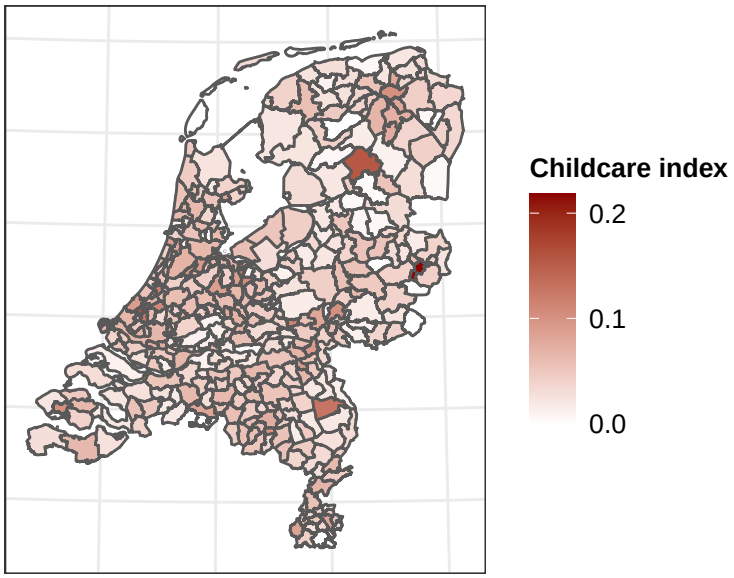
**Childcare supply by municipality** We do not have access to administrative record on municipal spending on childcare or pre-reform child care use. However, we have information on the number of child care workers that we can use to proxy child care supply

by municipality. To construct a municipally-based index on childcare supply, we rely on available information on 5-digits sector available for the universe of jobs in the Netherlands since 2001, to identify child care workers, and information on job location. As detailed in Appendix C.2, the evolution of childcare jobs over time matches quite closely the evolution of aggregate childcare public spending, which suggests that this is a good proxy for childcare availability. We then construct our index for childcare supply by municipality, by dividing the number of childcare jobs in municipality $m$ and year $t$ by the number of children below five years old

$$childcare\_supply^{m,t} = \frac{N_{jobs}^{m,t}}{N_{children}^{m,t}} \tag{4}$$

Figure 4 gives an overview of the variation in child care supply per pre-school child by municipality, averaged over the years 2001 to 2004. We observe a large dispersion in child care workers per pre-school child, varying from 0 to 0.2. Some extreme values may be partly driven by measurement errors, as job location relates to the firm location and may not always correspond to the municipality where the job is actually done. Yet, the figure illustrates that there is large variation in child care availability and take-up by municipality before the child care reform.

Figure 4: Childcare supply by municipality



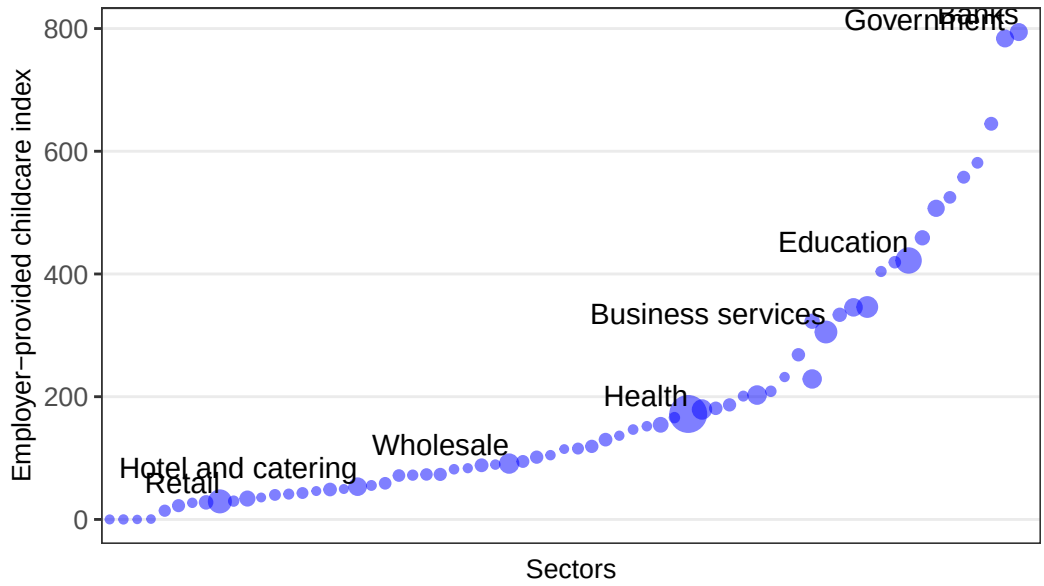

NOTE: This Figure presents the average of the childcare supply index presented in equation 4 by municipality. We compute the average for years 2001 to 2004.

**Childcare employer subsidies by sector** Information on employer-provided childcare is also limited, as we do not observe this variable on the data before 2006 and this type of subsidy was removed in 2007. We use the only available year of data - 2006 -

to construct an index of employer generosity for child care subsidies. We observe, for each worker in the Netherlands, the amount of childcare subsidies she receives from her employer. As these subsidies are largely determined by industry-based agreements, we compute the index at the sectoral level.[14] We compute the index as the average amount of childcare subsidies in 2006 received by mothers of children aged from 1 to 3 (born between 2003 and 2005). Note that this amount should not be interpreted as an exact difference in childcare cost, since part of the costs were tax deductible for the parents that did not get a subsidy from their employer.

Figure 5 presents the distribution of this index for mothers, by sector, highlighting some of the biggest sectors with their names. The difference between sectors is substantial, ranging from no employer-financed subsidies up to an average yearly additional subsidy of 800 euros for the government or the banking sector.

Figure 5: Childcare employer subsidies by sector




NOTE: This figures presents the average amount of chilcare subsidies paid to parents in every sector (*sect* variable in the *baan* datasets). The size of the circles is proportional to the size of the corresponding sectors. We highlight some of the biggest ones.

**Correlations with child penalty** Both the generosity of employer childcare subsidy and municipality level child care supply may be associated with a lower child penalty of women, by making child care more affordable. We correlate the employer generosity and the municipality child care supply index with mother' child penalties, by municipality or sector, to get a first impression of the effectiveness of policies in reducing the child penalty.

[14] We use a variable computed by Statistics Netherlands which aggregates different collective agreements to a total of 70 different sectors.

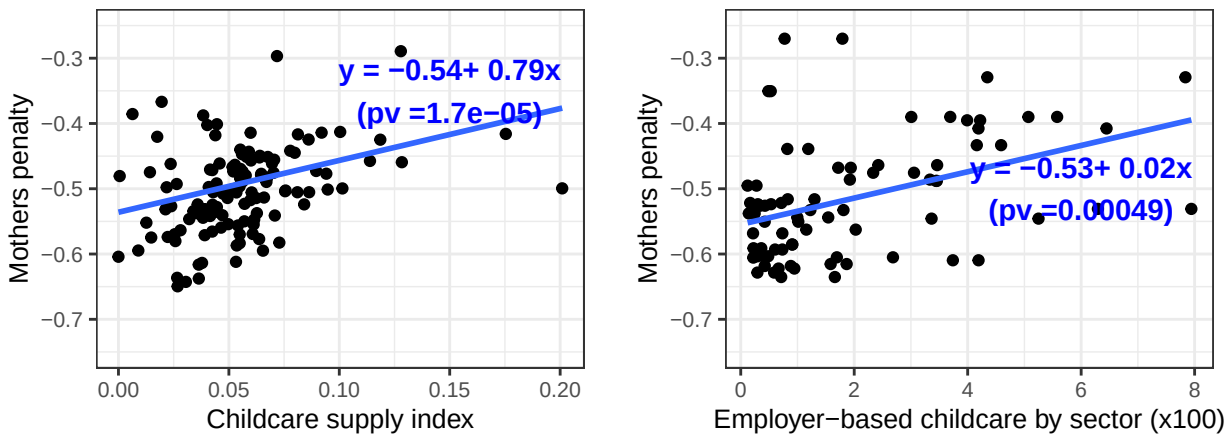
Figure 6 relates our child care affordability indices to the child penalty measured in the respective municipality or sector. We present the average index and child penalty, together with a regression line estimating the linear relationship between the two (weighted by the size of the units). In both cases, we find a clear correlation between childcare affordability and the child penalty. Regarding childcare availability at the municipal level, a 1pp increase in the number of childcare jobs per pre-school children is associated with a 0.8pp reduction in the child penalty. Regarding employer-provided childcare subsidies, a 100 euros increase in the subsidy is associated with a 0.02pp reduction in the child penalty. Even though the correlation is small, the sign goes in the expected direction.

The negative relationship between the magnitude of the child penalty and childcare affordability suggests that access to childcare may be an important determinant of the effect of children on mothers' earnings. However, the correlations presented are not causal. Other potential determinants of the child penalty such as socio-economic background or gender norms can be correlated with individual choices regarding place of residency and sector of employment and the child penalty. For example, women who invested in their career pre-child and who plan on not reducing their hours (much) when having children may be more likely to apply for jobs at firms that do provide subsidies for child care. In the next section we use variation in the timing of childcare expansion following the 2005 reform to causally identify the short term effects of increasing access to childcare over the child penalty.

Figure 6: Correlation between childcare provision and child penalty

(a) Municipalities

(b) Sectors




NOTE: These Figures present the correlation between the mothers' child penalty and the average municipality childcare supply index (panel a) and the average amount of employers-provided childcare subsidies (panel b). We present the slope and coefficients form the estimation of a linear model, including weights for the size of the units. Pv stands for p-value.

## 4.3 Effect of childcare expansion

**Empirical strategy** To identify a causal effect of increased access to childcare on the child penalty, we take advantage of the geographical variation in the timing of childcare expansion following the 2005 reform. A similar approach is implemented in Andresen and Nix (2020) for Norway and Kleven et al. (2020) for Austria. The unification of the childcare system led to a large increase in the amount of public childcare expenditure as well as childcare exposure at the national level (see appendix Figure C.2). The timing and magnitude of the increase however varies across municipality because of the large variation in child care availability before the reform. We leverage these variations to construct our treatment and control group as follows. The treatment group includes municipalities that have experienced a large increase in their childcare supply index from one year to the other (more than 0.05pp[15]). All other cities are put in the control group, and are attributed an *ad hoc* expansion date based on the observed distribution of expansion dates. Figure A.2 of the Appendix presents the distributions of the years for which we observe a large expansion. A large majority of the events occur between 2006 and 2011, the years for which childcare subsidies are gradually expanding.

Spatial variation in the expansion of childcare is likely to be endogenous. We focus on sharp increases in childcare availability, since they are most likely to be driven by reform-induced supply expansions. Thereby, both reverse causality and omitted variable bias are less likely to distort our results.[16]

Figure 7 present the evolution of the childcare supply index before and after the reform-induced expansion, for the treatment and control group. By construction, we observe a large increase in the index for the treatment group when the expansion occurs, and no sharp change for the control group. Interestingly, there is no difference in the evolution of the index before the expansion between the treatment and control group. This is reassuring with respect to our identification assumption: if the expansion were also driven by other factors related to the demand for childcare, we would expect some differences in the trajectories between the two groups before the treatment year.

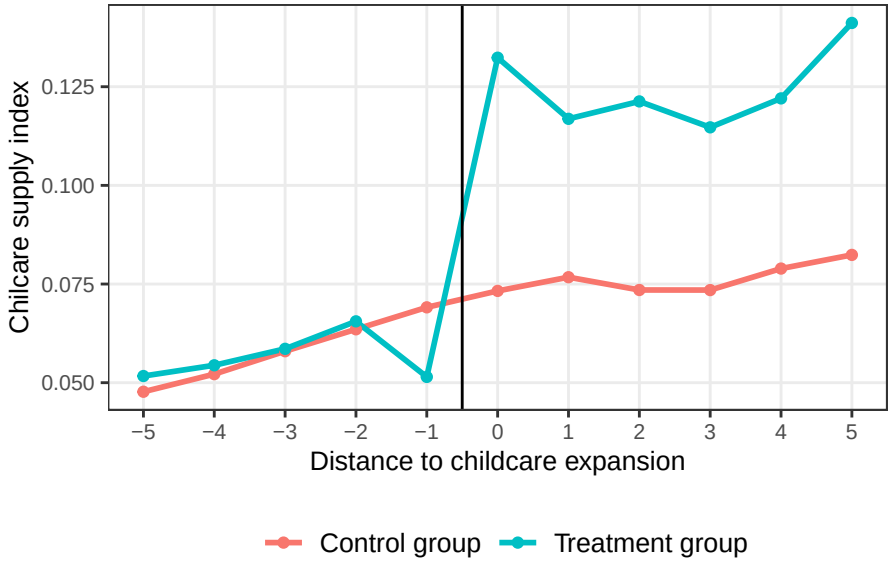
In order to assess the effect of the expansion on the child penalty, we estimate the same model as before on different groups: (i) for mothers whose children were born before the expansion (between -5 and -3) and after the expansion (between +1 and +3) and (ii) separately for the control and treatment group. If the expansion decreases the child

[15] An 0.05 percentage point increase implies for example moving from the median municipality to the 75th percentile pre-reform and thus represents a large increase in child care availability.

[16] Reverse causality may arise if mothers who want to work more push municipalities to provide more child care by voting for politicians promising to provide more child care subsidies. One possibility for omitted variable bias would be that richer cities provide more childcare and mothers living there are working more.

penalty, we expect a larger reduction of the child penalty between the two periods in the treatment group than in the control group.

Figure 7: Evolution of childcare index by distance to the expansion date




NOTE: This Figure present the evolution of the childcare exposure (defined in equation 4) by year before and after the childcare expansion, for the treatment and control group. The treatment group includes cities that have experienced a large increase in their childcare index from one year to the other (more than 0.05pp). All other cities are put in the control group, and are attributed an *ad hoc* expansion date based on the observed distribution.

**Results** The estimated penalties for our four estimation samples are presented in Figure 8. We first present the penalties estimated separately for each group in panel (a). For earnings, we do not observe any difference for the control group, and a small reduction in the penalty in years t=1 and t=2 for the treated group. For participation, we observe a small reduction in the penalty in the control group, and a slightly bigger reduction in the treatment group. Overall, we find that there is at most a very small effect of the childcare expansion on the child penalty. In panel (b), we present the estimated $\delta$ coefficients of the following interacted child penalty estimation, estimated separately for the treatment or control group:

$$y_{it} = \sum_{q=-3}^{8} \alpha_q \mathbb{1}[eventtime_{it} = q] + \omega Post + \sum_{q=-3}^{8} \delta_q \mathbb{1}[eventtime_{it} = q] * Post + \sum_{k=17}^{53} \beta_k \mathbb{1}[age_{it} = k] + \sum_{t=1999}^{2016} \gamma_t \mathbb{1}[time_{it} = t] + \nu_{it} \quad (5)$$

The variable *Post* is a binary indicator that equals one if the birth takes place after the

childcare expansion. We expect the $\delta$ coefficients to be close to zero for the control groups and positive for the treatment groups. The $\delta$ coefficients presented in panel (b) of Figure 8 are roughly in line with what we observe for panel (a) for separated estimation. We observe a similar (and small) decrease the penalty for participation in the control and treatment groups, which is slightly but not significantly higher in the treatment group. The differences for the earnings penalties are more pronounced, and more important for the first years after birth. We find a 500 euros increase in the yearly earnings for the treatment group, to be compared to a slight decrease (around 250 euros) for the control group. This can be interpreted as evidence of an effect of the childcare expansion in reducing the child penalty. The income difference is however small compared to the magnitude of the earnings penalty we observed. The 750 euros gains from the expansion we measure indeed amounts to only 6.5% of the drop in income we observe after birth (around 12,000 euros according to Figure A.1). Overall, those results suggest that the lack of access to childcare may have a small short-term effect on the child penalty. This, however, does not rule out an important role for childcare policies in the long run, as they may have an effect on other determinants of the child penalty – such as gender norms –, that we are not capturing in our empirical setting.

Our results are in line with Andresen and Nix (2020) and Kleven et al. (2020) who implemented similar evaluation of childcare expansions in Norway and Austria respectively. Andresen and Nix (2020) indeed finds small effect of a big childcare reform on earnings penalty, and those effects are concentrated on the first years after birth and fade out from year t+4. Kleven et al. (2020) find no effect at all on either participation or earnings. Our results then provide additional evidence that childcare reforms have a limited impact on the child penalty in the short-run. Lastly, our results are also consistent with Bettendorf et al. (2015), who find a significant but small effect of the 2005 childcare reform on mothers' labour force participation and hours worked.[17] The slight difference in earnings penalty we observe for the control and treated group could translate into a significant difference in the aggregate labour supply of women.

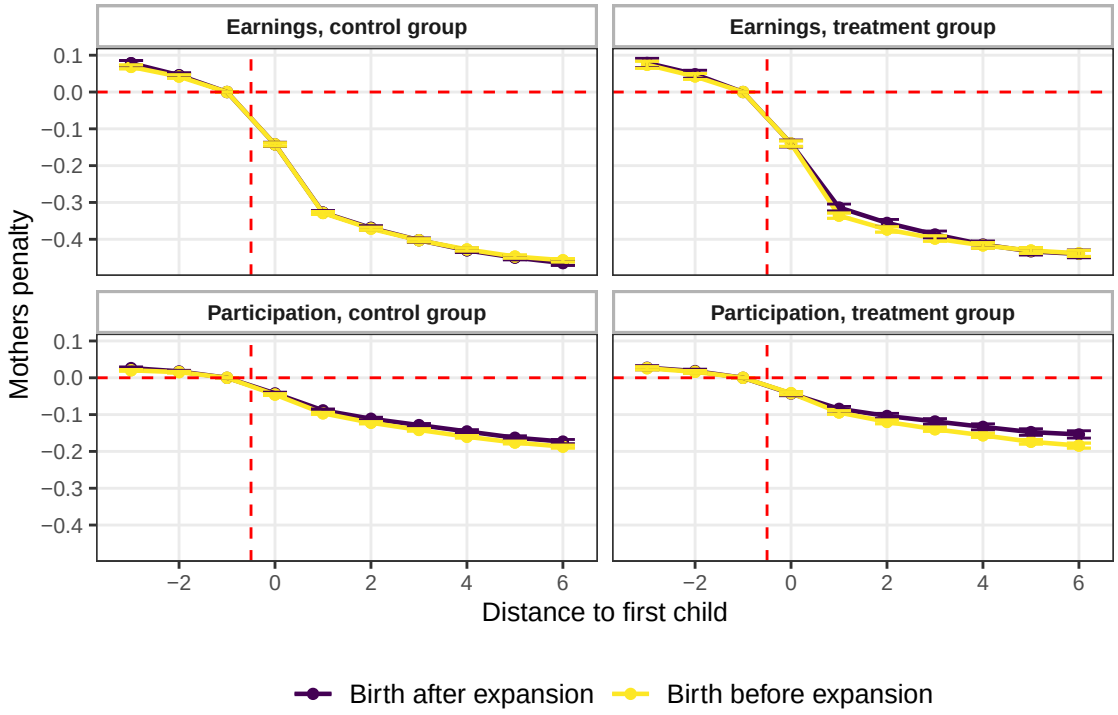
# 5 The role of gender norms

The previous section has shown that family policies have a limited short-run effect on the child penalty. While family policies create the opportunity to freely decide on the division of labour in the couple, this decision does not take place in a cultural void. Indeed, the idea that women are responsible for care tasks whereas men are responsible for paid work is deeply entrenched in our culture and our understanding of gender roles. In order to

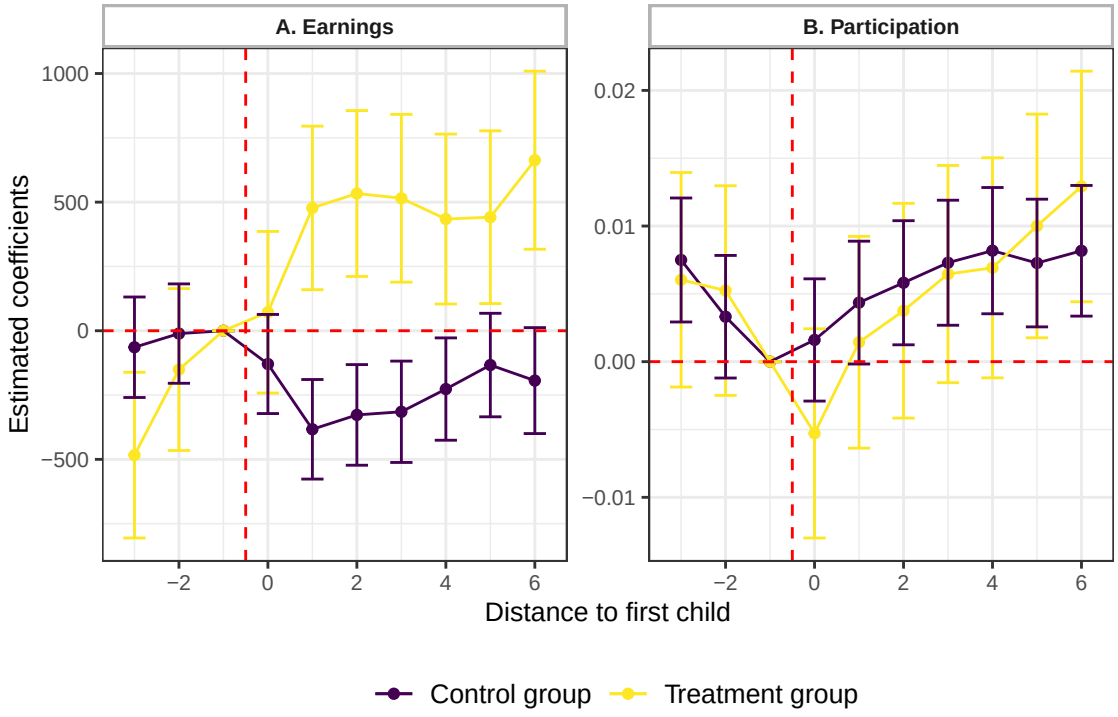
[17]They find a 3.3% increase in the participation rate and 6.6% increase for hours worked for parents with a youngest child 0-11 years of age.

Figure 8: Effect of childcare expansion on the evolution of the child penalty

(a) Separated estimations




(b) Difference-in-difference estimation




NOTE: Panel (a) presents the estimation of the child penalty model (equation 1) for four samples: before and after the childcare expansion, for both the treated and control groups. Panel (b) presents the $\delta$ coefficient from equation (5) for the treatment and control groups

evaluate to what extent gender norms contribute to the division of labour in the couple and ultimately the child penalty, we conduct three analyses. First, we analyse same-sex parents, for whom gender norms are less likely to be informative on how tasks in the household are divided. Second, we take advantage of the correlation between religiosity and non-egalitarian gender norms. Third, we analyse migrants, who are exposed to a different set of gender norms from the culture of their country of origin, while living in the same institutional framework as native Dutch parents.

## 5.1 Same-sex parents

In order to evaluate the role of norms, we compare child penalties of mixed-sex and same-sex parents (Andresen and Nix, 2020; Moberg, 2016a; Rosenbaum, 2019). The idea behind this comparison is the following: if gender norms are influencing the division of labour and thus the child penalty in mixed-sex couples, they are less informative on the division of labour in same-sex couples. Hence, the difference in child penalties between these two types of couples may give an idea on the importance of gender norms for the child penalty. Due to a low number of same-sex fathers, we limit this analysis to same- and mixed-sex mothers.

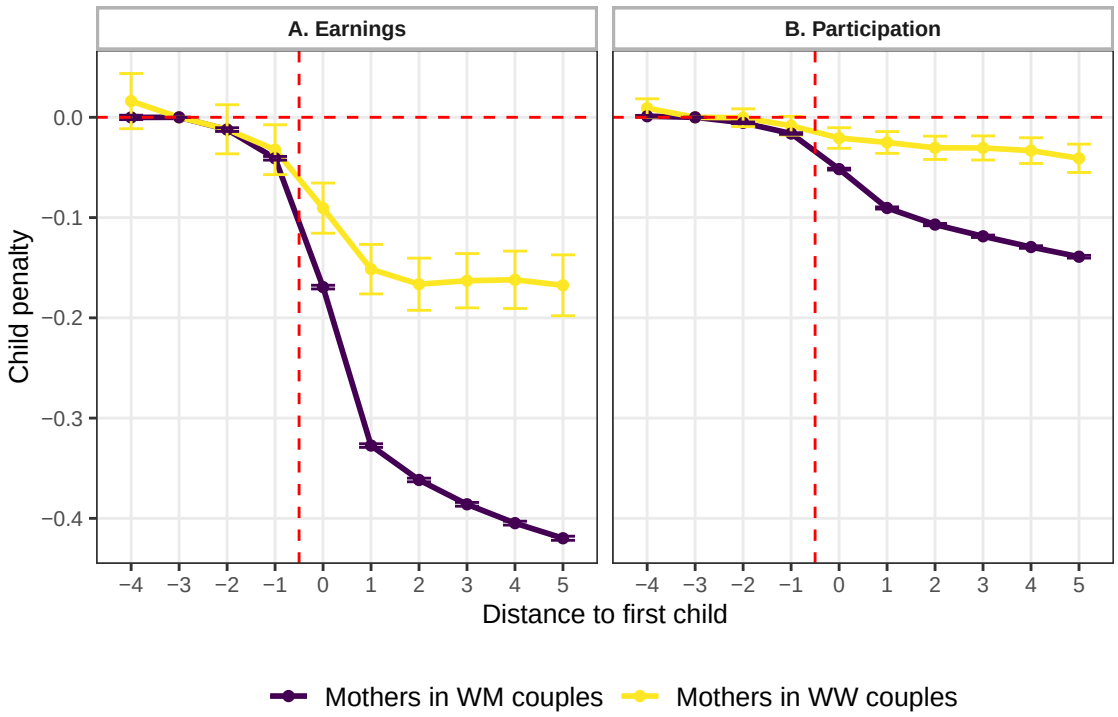
Figure 9 shows that same-sex mothers have about half the child penalty of mixed-sex mothers. However, this figure does not necessarily provide evidence that the labour market costs of parenthood in same-sex women couples are more equally distributed. We do not observed who in the couple is the birth-giving mother. So if same-sex couples specialised in a similar way as mixed-sex couples do, taking the average child penalty of fathers and mothers would give approximately the same child penalty as we find for same-sex mothers. This implies that the average of all same-sex mothers may hide a similar pattern as present for mixed-sex mothers and fathers.

In order to test whether the same division of labour patterns emerge in same-sex and in mixed-sex parents, we divide both groups of parents into the higher and lower educated partner (at t=-3), and in primary and secondary earner (at t=-3). The child penalties of both types of same-sex mothers are still substantially lower than the child penalties of mixed-sex mothers (Figure 10). This is in line with other literature showing that same-sex couples share housework and paid work more equally than heterosexual couples, and that there is less specialisation after child birth (Cudeville et al., 2020; Jaspers and Verbakel, 2013). We cannot differentiate between biological and adoptive mothers in the data, but Stuckradt et al. (2020) show for the Netherlands that while there is a difference in drop in income at birth between birth-giving and co-mother; this difference is small and not comparable to the difference between men and women in mixed sex couples. This suggests that the absence of biological birth for one of the mothers is not explaining the low penalty

for this group.

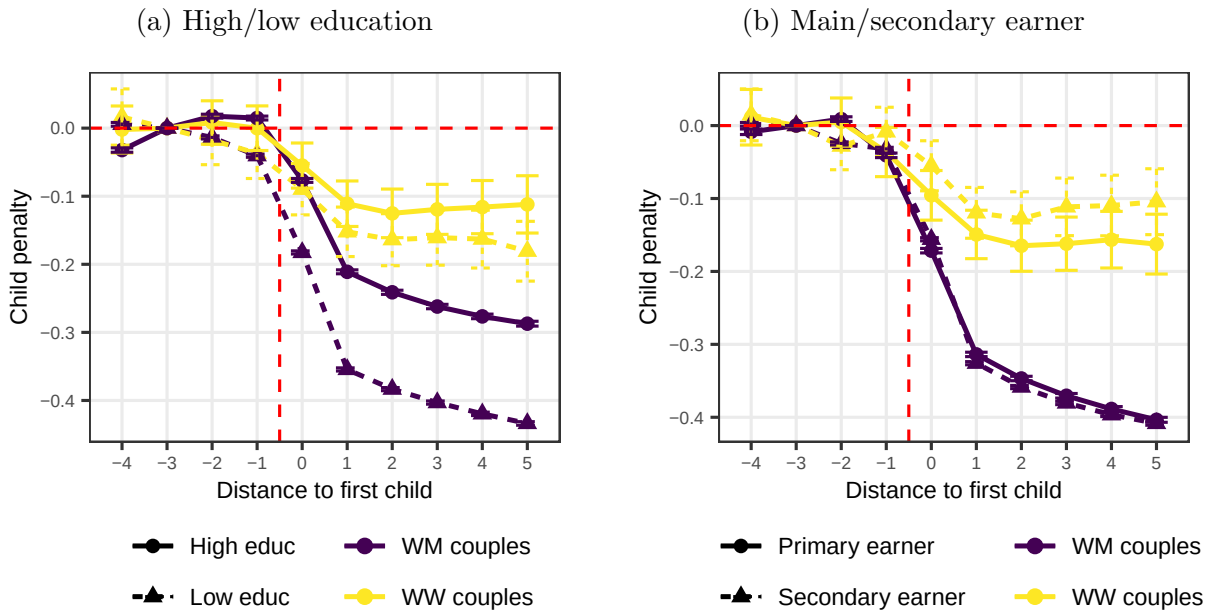
Taken together, these results suggest that gender norms are the most likely explanation for the difference in child penalty between same- and mixed-sex parents. Furthermore, the difference between these two groups of parents is large, suggesting a potentially large role of gender norms for the child penalty.

Figure 9: Mothers' child penalty by type of couples




NOTE: This Figure presents the child penalty in earnings (panel A.) and participation (panel B.) for mothers in same-gender couples (WW couples) and mothers in different-gender couples (WM couples).

Figure 10: Mothers' earnings penalty by type of couples and characteristics



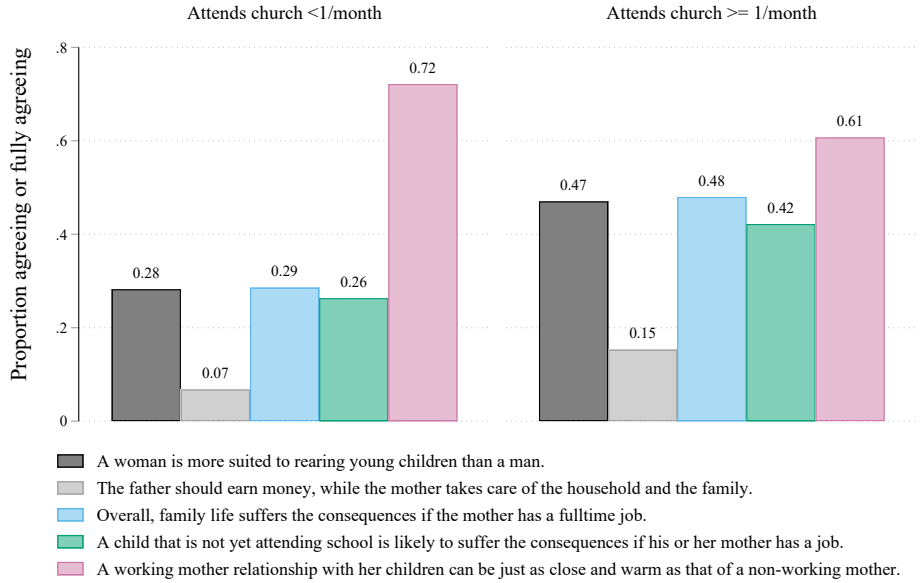

NOTE: This Figure presente the child penalty in earnings for parents in same (WW) or different (WM) gender, depending on their education attainment three years before birth (panel a) and depending on their share of their own earnings in the household total earnings (main or secondary earners, in panel b).

## 5.2 Religiosity

Gender norms do not only vary by couple type, but there is also a lot of variation in prevailing gender norm among mixed-sex parents. While we do not have information on elicited gender norms on the individual level in the administrative data, we take advantage of the correlation between religiosity and non-egalitarian gender norms. We use municipal level data on average religiosity of residents to proxy average prevailing gender norms in a municipality. Then, we compare child penalties by municipality with the level of religiosity.

Based on data from the LISS panel, Figure 11 shows that indeed, individuals who describe themselves as more religious (defined as attending church at least once a month or more), tend to have more non-egalitarian attitudes towards gender roles linked to parenthood. For example, whereas among less religious individuals, 29% are of the opinion that family life suffers when the mother has a full-time job, 48% of more religious individuals are of that opinion.

Figure 11: Elicited gender norms and religiosity

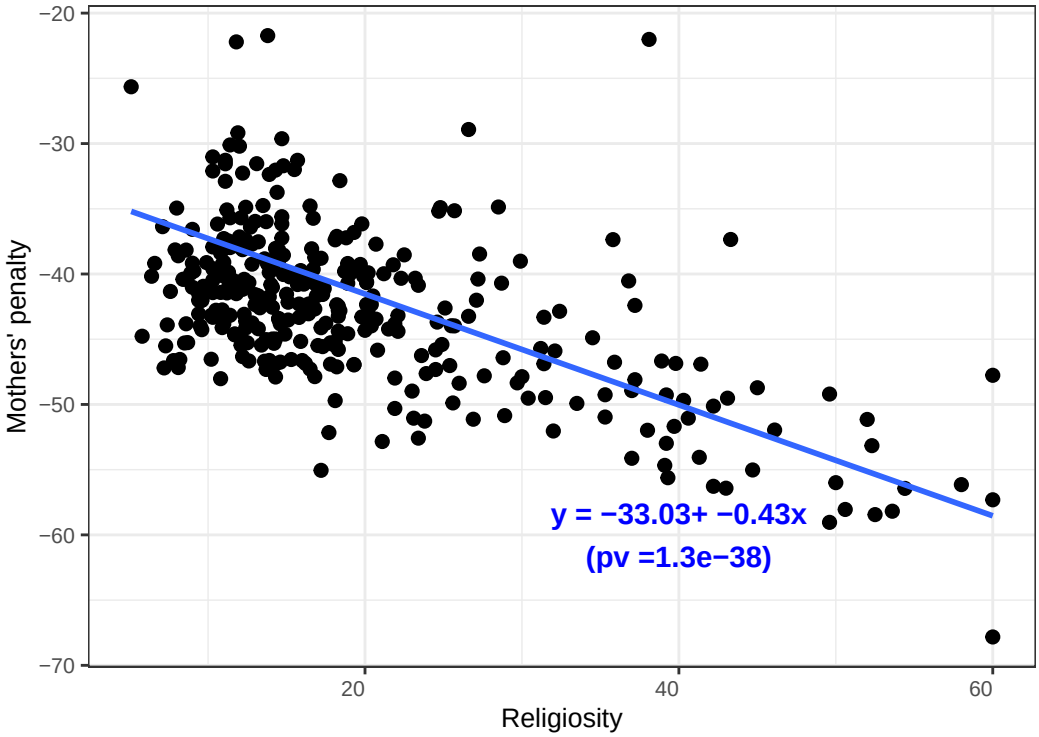



NOTE: Bars show the proportion of respondents agreeing or fully agreeing with the statement. These rates are separated by an indicator of regular church attendance (once a month or more versus less than once a month).
SOURCE: LISS panel 2008-2019.

Data on religious attendance at the municipality level is available in Schmeets (2016) for the year 2013. We use information on the share of individuals who visits a religious place at least once a month. Figure 12 shows the correlation between religiosity and the child penalty on municipal level: the higher the religiosity, the larger the child penalty. The correlation we obtain is strong: a 10pp increase in the average share of the population of a municipality going to a religious place at least once a week increases the child penalty by around 4pp.[18]

[18]Figure A.3 in the appendix shows religiosity and child penalty by municipality.

Figure 12: Correlation between child penalty and religiosity




NOTE: This Figure presents the correlation between the mothers' child penalty and the average religiosity measured at the municipality level (as the share of the population going to a religious place at least once a month, see text for details). We present the slope and coefficients form the estimation of a linear model between the two variables, including weights for the size of the municipalities.

This suggestive evidence is in line with Rellstab (2021), who finds a substantially higher child penalty in areas with a relatively high concentration of Dutch orthodox protestants compared to areas with few or none Dutch orthodox protestants. Since parents-to-be and municipal characteristics are very similar across areas, she concludes that the most likely explanation for the large difference in child penalty are gender norms.

The above associations together with the evidence presented in Rellstab (2021) are compatible with the hypothesis that gender norms influence the child penalty.

## 5.3 Migration background

Migrants provide another valuable source to study the impact of gender norms on the child penalty. Indeed, gender norms can vary between migrant groups (Khoudja, 2018), which could in turn lead to a different child penalty between groups. In this section, we leverage the high quality of the information on migration background available in the Dutch administrative data, to provide detailed heterogeneity analysis for the magnitude of the child penalty by migration background. In the following, we show that differences in

gender norms are a plausible explanation for differences in observed child penalties.

We have information on the migration background of the universe of the Dutch population. More specifically, we know if an individual was born in a different country than the Netherlands (first generation) or if it is the case for one of her parent (second generation); and if so the country of origin. We first compute the child penalty for different countries of origin (independent of the generation). We use the categories often applied by Statistics Netherlands, differentiating between western and not western background and isolating the four most represented sending countries: Antilles, Suriname, Morocco and Turkey. In order to increase our sample size per category, we put together Antilles and Suriname; and Morocco and Turkey, since they are closer in terms of socio-economical background (Jongen et al., 2019).

Figure 13 presents the earnings and participation penalty for mothers of different migration background. Compared to natives, the earnings penalty is lower for all groups except for the Morocco-Turkey group. Regarding participation, the level is homogeneous across all groups (around 15%) and much bigger for the Morocco-Turkey group (30%). As explain in the 2 section, we cannot estimate the effect on hours worked as we estimate the penalty on a larger set of years, including years before 2001 for which information on hours is not available. We can however infer from the comparison between the participation and earnings penalties that participation is driving the difference between Morocco-Turkey and the other groups, and that the more frequent use of part-time work for native Dutch mothers explain the higher earnings penalty for this group.[19]

How can we explain the difference in child penalty observed for those groups? We consider two potential channels, namely different socio-economic status and different gender norms associated to those groups. We argue that the former factor is not likely to drive those differences, based on the following suggestive evidence. First, observed differences in penalties do not match differences in socio-economic backgrounds. As shown in Appendix Table A.1, Western migrant and natives on one hand, and Turks/Moroccans on the other hand are roughly similar in terms socio-demographic characteristics, and yet exhibit strongly differentiated patterns in Figure 13. Secondly, Figure A.4 shows that, if we restrict our samples to more comparable sets of individuals who did not complete tertiary education, the comparison between the different migration groups remains virtually unchanged. Those findings suggest that socio-demographic differences may not be the main driver of the differences in child penalty between migrant group we observe.

Gender norms can, on the other hand, partly explain the differences we observe. Starting

[19]Differentiated wage rate trajectories and associated penalties could also be possible, due for example to labour market discrimination against migrant groups. However, we assume that these mainly manifest in a difference in level, and hence discrimination against migrant groups is unlikely to lead to a difference in child penalty.

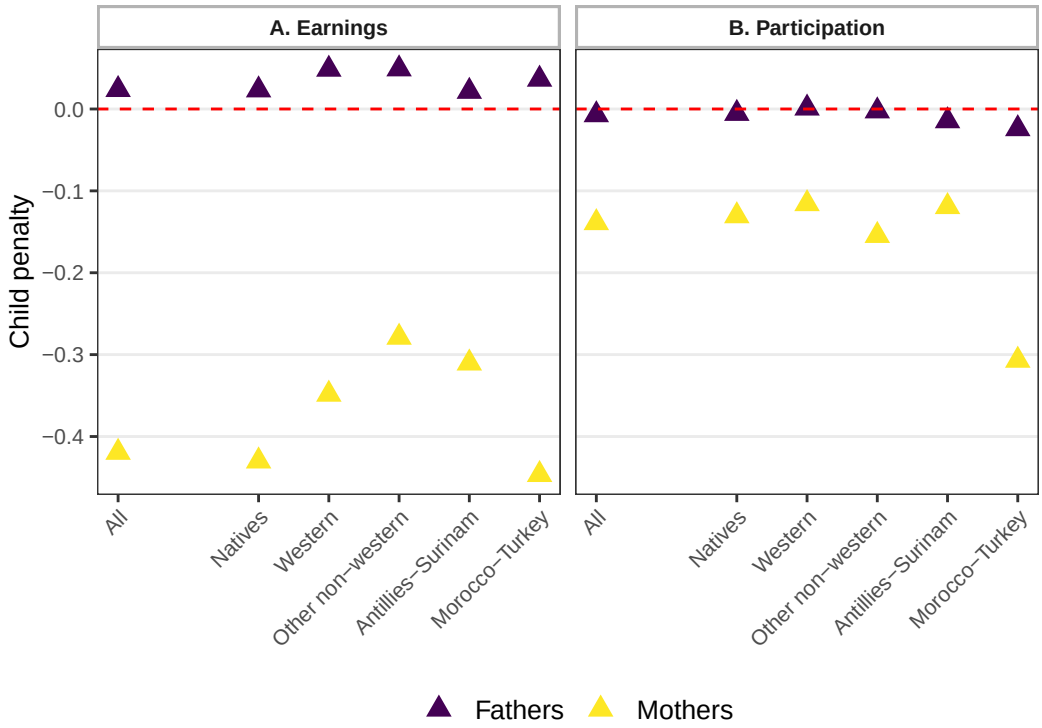
with participation (Panel B. of Figure 13), it appears that the Morocco-Turkey group is a clear outlier, with a two to three times larger child penalty than the other groups. This group is not an outlier in terms of socio-economical status, but more so in terms of norms and values. Khoudja (2018), indeed shows that Moroccan and Turkish migrants have relatively more conservative view about gender norms compared to other migrant groups and to natives, and relates those differences to labour market outcomes. We infer from this that gender norms are likely to partly drive the observed differences in participation child penalty for mothers. Our findings contradict the ones by Nieto (2021) for Spain, who finds that the differences in the child penalty between natives and individuals with migration background are mainly due to socio-economic differences (education), rather than gender norms. The differences between the two studies can come from the difference between Spain and the Netherlands in terms of migration type and integration. It can also be partly driven by the data we use, as we have bigger sample size allowing for a more granular heterogeneity analysis. Our results are on the other hand consistent with Blau et al. (2020), who find that first-generation immigrants from source countries with more gender equality (as measured by the World Economic Forum's Global Gender Gap Index) allocate tasks more equally, while those from less gender equal source countries allocate tasks more traditionally.

This gender norms explanation is, however, at first glance not consistent with what we observe for earnings penalty. It is of similar magnitude for the Natives and the Morocco-Turkey groups, whereas measures of non-egalitarian gender norms are much lower for the former (Khoudja and Fleischmann (2015)). This inconsistency can be understood when adding the Dutch part-time culture to the picture. While working part-time nowadays is legally possible for everyone (Wielers and Raven, 2013), there are clear gender differences in take-up, and when it is taken up, Dutch men work on average still close to full-time, whereas Dutch women work lower part-time percentages.

This implies that the combination of relatively more egalitarian gender norms paired with a gendered part-time culture leads to a large child penalty in earnings for native Dutch women. In contrast, less egalitarian gender norms in the Morocco-Turkey group leads to a large child penalty in participation, resulting in a similarly high earnings child penalty as for Dutch women. Comparing to the Antilles-Suriname group brings additional insights. They are roughly similar in terms of pre-birth characteristics (Table A.1), gender norms index (Khoudja and Fleischmann (2015)) and participation penalty (Panel B. of Figure 13) as Dutch native mothers, but their earnings penalty is much smaller for the Antilles-Suriname group. We speculate that this difference is mainly explained by a much stronger internalisation of the part-time culture by Dutch native women, which results in a large part-time penalty for them. This shows how norms and culture in general, and gender norms in particular, interact with couple's division of labour after birth of the first

child.

Figure 13: Mothers' child penalty by migration background




NOTE: This Figure present the earnings (panel A.) and participation (panel B.) child penalty estimated for different subgroups defined based on their migration background (see text for details on groups definition).

# 6 Conclusion

This paper provides an extensive assessment of the magnitude and drivers of the effects of childbirth on parents' earnings trajectories in the Netherlands. We show that the child penalty for mother is relatively high (46% compared to their potential earnings seven years after birth), whereas fathers' earnings are not affected by the birth. We identify the widespread use of part-time work after birth by mothers as the main component of this important child penalty. We then disentangle the relative role of two potential determinants of the child penalty, childcare policies and gender norms. Regarding the former, we exhibit strong negative correlations between the use of childcare and the penalty for mothers, suggesting that access to childcare is required for limiting the impact of children on careers. In line with the recent literature, we however find a small direct effect on the penalty of large childcare expansion in the short run. This suggests that, even if waiting lists for subsidised childcare can be an issue in the Netherlands especially in large cities, it is not likely to be the main driver of the large penalty observed in the country. In contrast, we find strong heterogeneity in the child penalty between groups – gender composition of the parents, religiosity of the environment, migration backgrounds

– suggesting that gender norms might be an important determinant of the magnitude of the child penalty.

Our study has various policy implications. First, while the development of part-time was an important step in fostering women's labour force participation from the 1960s by curbing the breadwinner model with low female labour market participation, it may nowadays hinder further reduction in the gender wage gap. Reducing the child penalty in the Netherlands requires a more equal use of part-time after childbirth between men and women. Second, even if childcare policies have an impact on parental labour supply, we find that the child penalty is only slightly affected by increasing child care availability immediately after the increase. This suggests that childcare policies do not attenuate the child penalty by a lot. On the other hand, comparisons between countries and between municipalities and sector in the Netherlands show a strong negative correlation between childcare availability and the child penalty. To understand this paradox, more research is needed on the long-run effects of family policies on the child penalty; and how they may interact with gender norms. Third, we show that there is a correlation between non-egalitarian gender norms and the child penalty. Previous literature has shown that gender norms are highly persistent over time (Alesina et al., 2013). Hence, it is unlikely that the child penalty for mothers will be reduced substantially with a *laissez-faire* approach. This calls for a policy response that takes the importance of gender norms and their consequences for the division of labour in couples into account. These type of policies are especially important in the light of the recent Covid19 pandemic, which lead to a trend reversion of increasingly egalitarian gender norms towards more traditional views in France Boring and Moroni (2021).

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# A Additional Tables and Figures

## A.1 Additional Tables

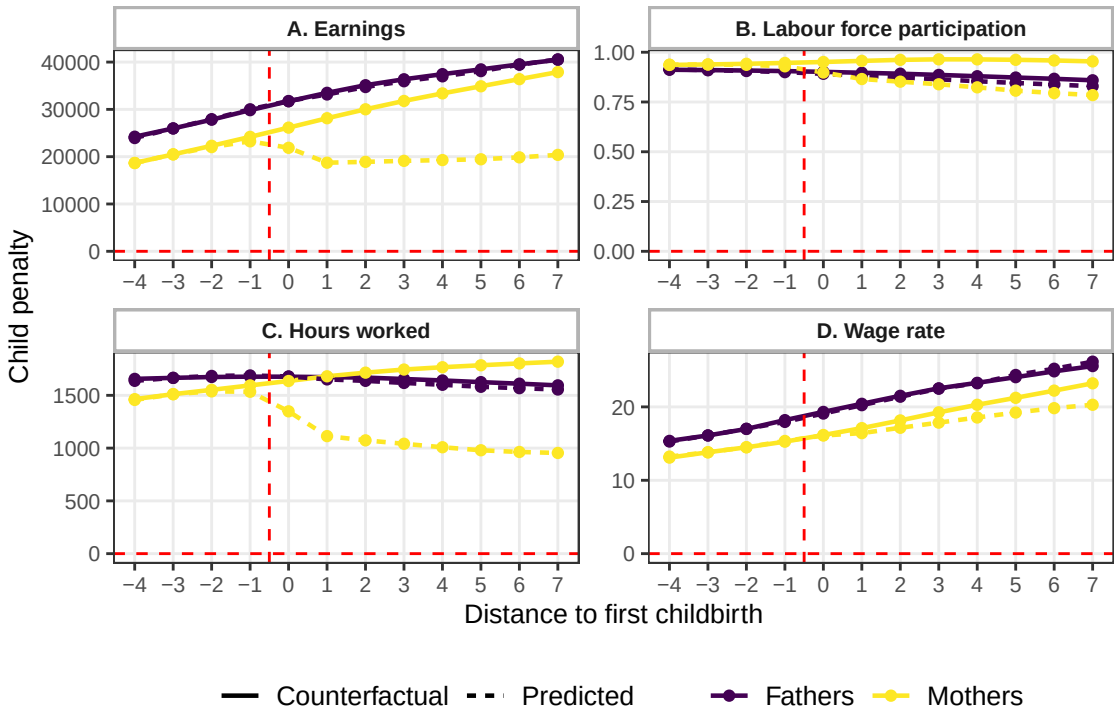
Table A.1: Descriptive statistics by subgroups

| | Nb obs | Child birth date | Birth date | Age | % male | % High educ | LFP | Earnings |
|---|---|---|---|---|---|---|---|---|
| **Gender composition** | | | | | | | | |
| Different gender parents | 1212198 | 2007 | 1976 | 29 | 50 | 29 | 92 | 25229 |
| Same gender parents | 3682 | 2008 | 1973 | 32 | 4 | 48 | 93 | 28561 |
| **Migration background** | | | | | | | | |
| Natives | 1013907 | 2007 | 1976 | 29 | 50 | 31 | 93 | 25985 |
| Western | 88580 | 2007 | 1975 | 30 | 47 | 26 | 90 | 25148 |
| Other non-western | 32769 | 2007 | 1976 | 29 | 53 | 14 | 83 | 17937 |
| Antillies-Surinam | 29813 | 2007 | 1977 | 28 | 48 | 19 | 92 | 21379 |
| Morocco-Turkey | 50801 | 2007 | 1978 | 27 | 50 | 10 | 87 | 17490 |

NOTE: Parents with first child born between 2003 and 2011.

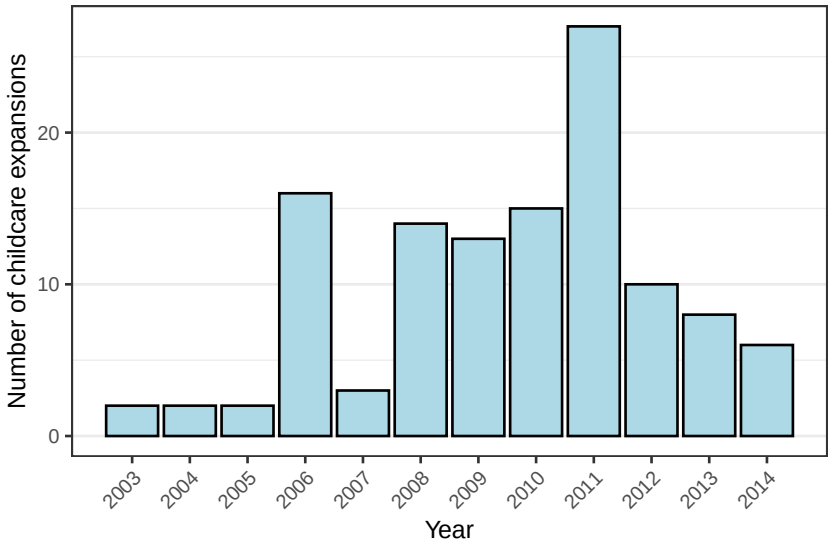
## A.2 Additional Figures

Figure A.1: Impact of children on mothers' and fathers' careers




NOTE: This Figures present the counterfactual and predicted earnings used for the computation of the child penalty, for mothers and fathers and for the different labour market outcomes we consider. Predicted earnings is the predicted value of the outcome based on the estimation of equation (1). Counterfactual outcome is the predicted labour market outcome at event time $q$, without the event dummies: $\tilde{y}_{it}^{q} = \sum \hat{\beta}_k \mathbb{1}[age_{it} = k] + \sum \hat{\gamma}_t \mathbb{1}[time_{it} = t]$.

Figure A.2: Distribution of the years of large childcare expansion

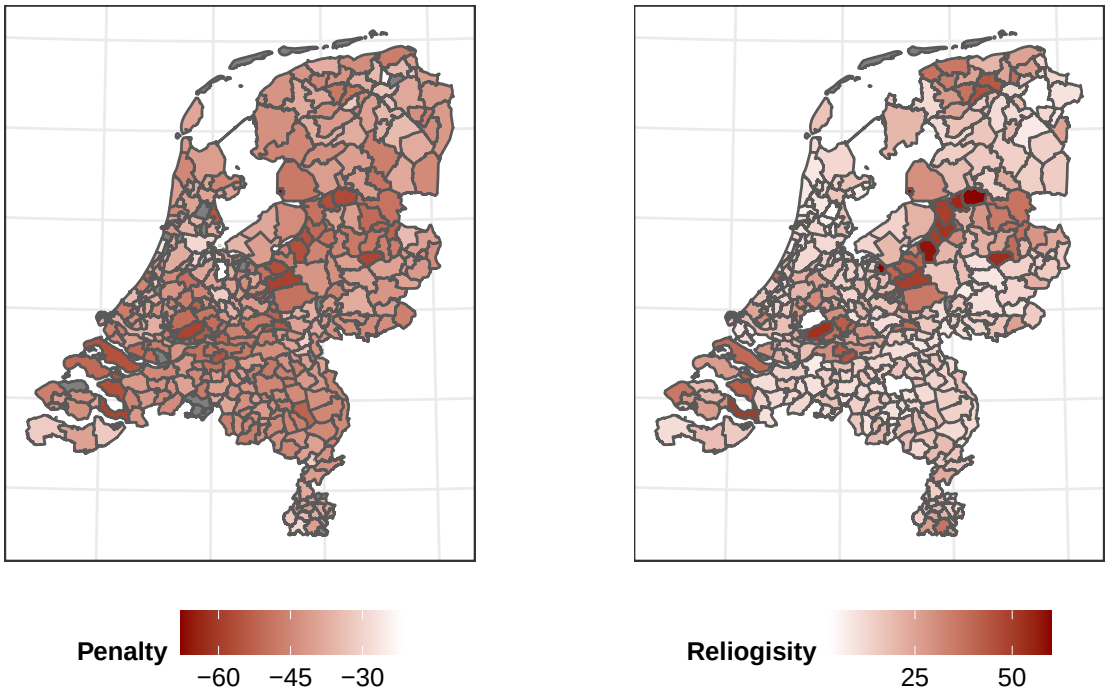



NOTE: This Figure present the distribution of the years for which we observe a childcare expansion (see section 4 for details about the definition of the event).

Figure A.3: Mothers' child penalty and religiosity by city

(a) Child penalty

(b) Religiosity




NOTE: This Figure present, for each municipality, the earnings child penalty for mothers (panel a) and the average religiosity (panel b). Religiosity is defined as the share of individual going to a religious place at least once a month. See text of section 5 for details.

Figure A.4: Mothers' child penalty by migration background : no tertiary education


NOTE: This Figure presents the earning and participation child penalties for different migration groups, restricting the population to individuals we have not completed a tertiary education degree before the birth of their first child.

# B Data construction

## B.1 Construction of the hours worked variable

In the *baan* datasets that we use there is no direct information available on the hours work. We do have access to a variable on the full time equivalent associated to a given job spell. For example, a person working 20 hours when the full-time reference is 40 hours, the % of FTE worked is equal to $\frac{20}{40} = 0.5$. The problem is that we do not observe the full-time reference hours, which would make it possible to retrieve the hours worked from the FTE. We therefore adopt the following approach: we use another data source, the *polis* datasets (see section C above) which contains information on hours actually worked, to impute hours worked in our main estimation sample.[20] More precisely, we compute for each each the distribution the yearly hours worked in each sector using the polis data, and take the mode of the distribution as the reference work duration in the sector. This approach generates some measurement errors since the reference duration is determined at a more disaggregated level (collective agreement or even firm) than the one we are using (sectors, split in 70 categories). Another potential source of measurement errors comes from the fact that the polis dataset starts in 2006, and that we use the distributions observed at that year for the computation of the reference durations for years 2001-2005.

Figure C.1 below compares the observed and imputed distribution, based on year 2010, for which we can observe the hours worked in the *polis* dataset. Panel (a) compares the observed and imputed distribution of yearly hours worked, and panel (b) presents the distribution of the difference between the observed and imputed values. These figures confirm that we have measurement error in our measure of hours worked, but that the imputed values are on average very close to the observed one for the comparison sample.

## B.2 Construction of the childcare index

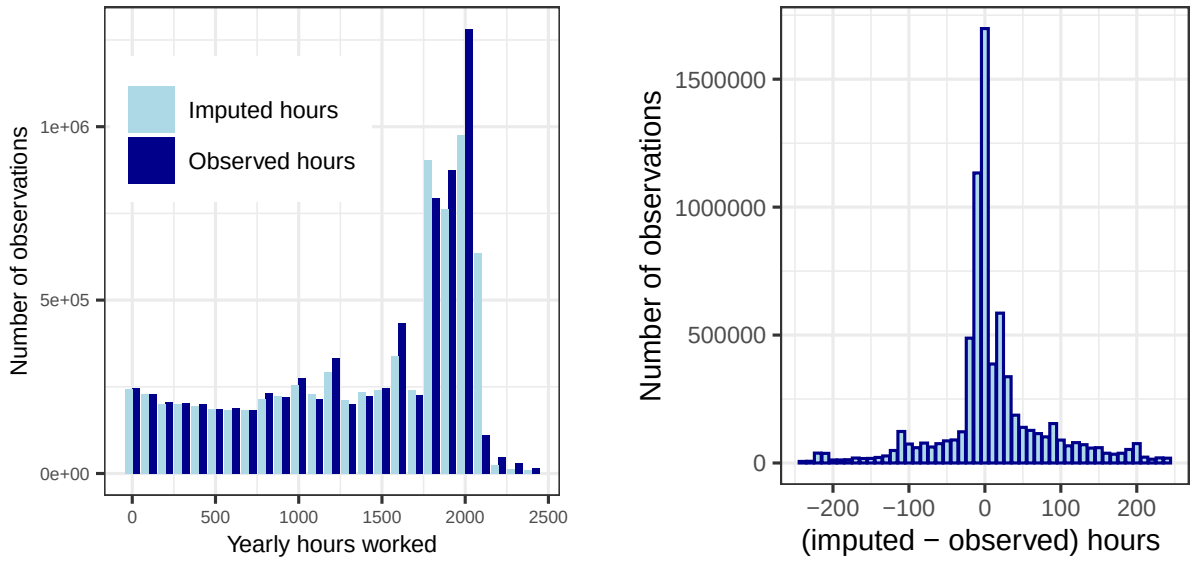
For the construction of our childcare exposure index (equation (4) in section 4), we need to compute the number of childcare jobs existing in each municipality. We do not have direct information on that, for example from municipalities' expenditures as in Andresen and Nix (2020) or Kleven et al. (2020). We therefore use firm-level information on job location combined with sector information to compute our index. More precisely, we compute for each year and city the number of jobs in two different 5-digit sectors: 88911 (day nurseries for pupils) and 88912 (Kindergartens). We then divide this number of jobs by the number of below five years in old by year and city to compute our index.

[20] The *polis* datasets include a richer set of variables but only start from 2006 on, which limit the time window for the estimation of the penalty. This is the reason why we do not use it as main data source in the paper.

Panel (a) of Figure C.2 presents the evolution of the index at the country level. We observe a sharp increase in the number of childcare jobs from year 2006. The overall childcare exposure more than doubles between 2006 and 2012, and then slightly decreases. This evolution is consistent with the timing of implementation of the childcare reform described in section 4.1, as illustrated in panel (b) presenting the evolution of the overall public childcare expenditure. Reassuringly, our index closely follow the evolution of aggregate public expenditure, which suggests that we are capturing the effect of the childcare reform through it.

Figure C.1: Hours worked variable

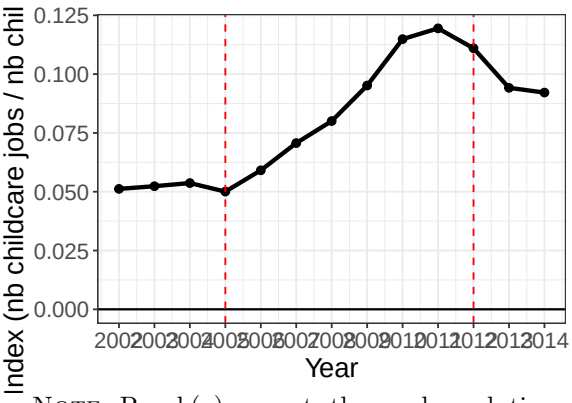
(a) Distributions of observed vs imputed hours (b) Distribution of observed - imputed hours



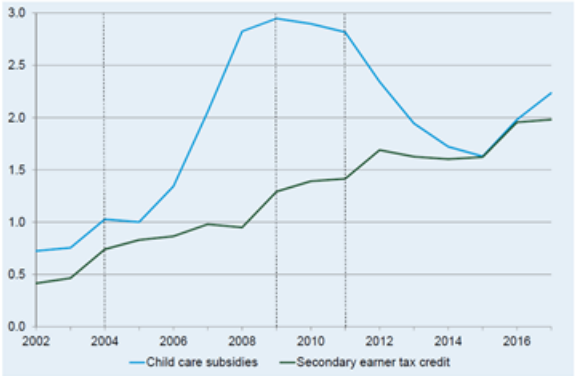

NOTE: These Figures compare the observed and imputed hours for workers observed in 2010 in both the *baan* and *polis* datasets. Panel (a) shows the overalapping distributions of imputed and observed yearly hours. Panel (b) shows the distribution of the gap between observed and imputed hours, computed for each individual.

Figure C.2: Childcare exposure index and childcare expenditure

(a) Childcare index by year




(b) Childcare-related public expenditure




NOTE: Panel (a) presents the yearly evolution of the aggregated childcare exposure index, measured as the ratio between the number of childcare jobs and the number of children below five years old. Panel (b) presents the evolution of the total public spending for childcare (source: Ministry of social affairs).

# C Description of the databases

In this appendix we describe the different data-sources used in the paper. Table B.1 lists all the sources we use, and we then provide more detailed information about each of the data we use.

Table B.1: Description of the datasets used in the analyses

| Data set | Years | Content |
|---|---|---|
| **Socio-demographic variables** | | |
| PARTNERBUS | 2018 | Partner identification |
| GBAPERSOONTAB | 2018 | Basic personal data |
| gbaoverlijdentab | 1999-2019 | Death register |
| gbamigratiebus | 1999-2019 | Migration register |
| GBAADRESOBJECTBUS | 2018 | Address register |
| VSLGWBTAB | 2019 | Address municipality codes |
| KINDEROUDERTAB | 2016 | Children-parents linkages |
| **Income and employment variables** | | |
| BAANKENMERKENBUS | 1999-2016 | Employment |
| BAANSOMMENTAB | 1999-2016 | Earnings |
| SECM | 1999-2019 | Income by income source |
| gemstpl | 1999-2019 | Job location |
| polis and spolis | 2006-2019 | Employment |
| **Other data sources** | | |
| LISS panel data | 2008-2019 | Survey on various domains |
| kerkbezoek | 2015 | Survey on religious practice |

## C.1 Dataset provided by Statistics Netherlands

Information about the administrative micro data sets can be found at https://www.cbs.nl/nl-nl/onze-diensten/maatwerk-en-microdata/microdata-zelf-onderzoek-doen/catalogus-microdata (available in Dutch only)

### gbapersoontab[21]

It contains demographic background data (e.g. gender, year of birth, migration background) for the universe of the Dutch population, that is all persons who appear in the registered in the population register (Basic Register of Persons, BRP) since 1 October 1994.

[21]Link to gbapersoontab documentation in Dutch

**gbaoverlijdentab**[22]

Contains the date of death of all persons who have died since 1 October 1994 and were registered in the population register (Basic Register of Persons, BRP) at the time of death. It also contains the date of death of persons who are not residents but were once residents of the Netherlands since 1 October 1994 and whose information about the death is received in the Register of Non-Residents (RNI). The main source of information for this dataset is the municipal registries (Gemeentelijke Basisadministratie Persoonsgegevens, GBA).

**gbamigratiebus**[23]

It contains all migration spells for the full universe of the Dutch population (as defined in the gbapersoontab). For each immigration (resp. emigration) spell, a date of beginning and end is registered, as well as the country of origin (resp. destination). For each individual, we have as many spells as migration events occurring since 1994. The main source of information for this dataset is the municipal registries (Gemeentelijke Basisadministratie Persoonsgegevens, GBA).

**gbahuishoudensbus**[24]

For the full universe of the Dutch population (as defined in the gbapersoontab), it contains information about the household composition: their place in the household, and the details of the household they belong to (e.g couple or not, married or not, with or without children, etc.). Retrospective information is available, as the data is presented as spells (one additional line when one characteristic of the household changes). The main source of information for this dataset is the municipal registries (Gemeentelijke Basisadministratie Persoonsgegevens, GBA).

**GBAADRESOBJECTBUS**[25]

For the full universe of the Dutch population (as defined in the gbapersoontab), it contains information about the individual address, with a unique object identifiers at the level of the building. Retrospective information is available, as the data is presented as spells (one additional line when one characteristic of the address changes). The main source of information for this dataset is the municipal registries (Gemeentelijke Basisadministratie Persoonsgegevens, GBA).

[22]Link to gbaoverlijdentab documentation in Dutch
[23]Link to gbamigratiebus documentation in Dutch
[24]Link to gbahuishoudensbus documentation in Dutch
[25]Link to gbahuishoudensbus documentation in Dutch

**baankenmerkenbus**[26]

A spell database with information on job characteristics (contract, sector) for the full universe of the jobs. It is available for years 1999-2016.

**baansommentab**[27]

A yearly database with information on wages (gross wage, fiscal wage, full time equivalent) for the full universe of the jobs. It is available for years 1999-2016.

**polisbus**[28] **and spolisbus**[29]

It contains information on the full universe of job in the Netherlands, available from year 2006. There is one line by employment spells, with information on both the individual (wage, hours worked, contributions, etc) and the firm (sector, collective agreement, etc).

**gemstpl**

A yearly database containing information on job location at the municipality level for the full universe of job in the Netherlands existing at the end of the year (31/12). The address is based on the firm address. When there are several establishment for a given firm, the location is imputed by Statistic Netherlands as follows. The location attributed to a given job is the one which is the closest to the home address of the worker.

There are different version of this dataset, depending on the years considered: GEMSTPLAATSBUS for 1999-2005, GEMSTPLBUS for 2006-2014, GEMSTPSBUS for 2015-2019.

## C.2 Other datasets

**LISS panel** The LISS (Longitudinal Internet Studies for the Social sciences) panel administered by CentERdata (Tilburg University, The Netherlands) is a representative sample of Dutch individuals who participate in monthly Internet survey on household background information. The panel is based on a true probability sample of households drawn from the population register. Households that could not otherwise participate are provided with a computer and Internet connection. A longitudinal survey is fielded in the panel every year. The information we use in this paper is retrieved from the religion and ethnicity; and the political views and value module.

[26]Link to baankenmerkenbus documentation in Dutch
[27]Link to baansommentab documentation in Dutch
[28]Link to polis documentation in Dutch
[29]Link to spolis documentation in Dutch

**Survey on religiosity** We use a survey on religious practices conduct by CBS to construct our religiosity index.[30] See Schmeets (2016) for a description of the data and a more complete analyses of religious practices in the Netherlands.

[30]The data can be found here https://www.cbs.nl/nl-nl/maatwerk/2015/20/religie-en-kerkbezoek-naar-gemeente-2010-2014.