Co-Parenting and Careers after Divorce

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Abstract

Joint physical custody (co-parenting) is an increasingly popular post-divorce parenting arrangement, and while it benefits children, its economic implications for parents are theoretically ambiguous. I investigate empirically how co-parenting affects parents' labour market outcomes after divorce, exploiting a custody reform in the Netherlands that encouraged co-parenting and increased its uptake by 7.6 percentage points among parents with young children. I find that mothers who divorce after the reform experience a 0.8% wage decline in an intention-to-treat framework relative to those divorcing before the reform, which implies an average wage loss of about 10% for compliers. This is driven by slower wage growth for mothers in the treatment group, who are less likely to move further away to access better-paid employment. The findings suggest that co-parenting ties both parents to a fixed location, reducing geographical mobility. Treated mothers also reduce their hours temporarily – largely due to rising overtime in the control group – while fathers' wages and hours remain unaffected. The wage penalty is concentrated among mothers who were secondary earners during marriage and younger at the time of divorce. These patterns are consistent with couples placing greater weight on the primary earner's career when making location decisions, which makes the post-divorce location constraint under co-parenting bind more tightly for mothers than fathers, thereby widening the gender wage gap. My results indicate an efficiency cost of location constraints under co-parenting.

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1 Introduction

Parental divorce and separation represent a significant source of disruption in children's lives (Frimmel et al., 2024). Recognising this, many policies aim to safeguard children's well-being in the aftermath of parental separation. One key concern is that children tend to spend significantly less time with their fathers after divorce (Brown et al., 2025). The main reason for this is that, traditionally, physical custody was awarded to only one parent, usually the mother, even when legal custody was shared. More recently, joint physical custody (also termed co-parenting) – an arrangement in which children split their time equally between both parents after separation – has emerged as an increasingly prevalent alternative arrangement in many Western countries, reflecting judicial and legislative shifts toward equal parental responsibility. Co-parenting can indeed benefit children (Vowels et al., 2023; Nielsen, 2018; Spruijt and Duindam, 2009; Bauserman, 2002). However, its economic implications for parents themselves have been largely unexplored.

This paper asks how co-parenting affects parents' labour market trajectories. The effects are theoretically ambiguous: On the one hand, a more equal distribution of childcare responsibilities could free up time for mothers, who are typically the primary caregivers, thereby relaxing their time constraint and allowing them to increase their hours and earnings. On the other hand, the logistical demands of co-parenting, specifically the need for both parents to remain in close geographical proximity to their children, and thus also to their ex-partner, imposes a significant mobility constraint. This constraint could force individuals to remain in a local labour market that is not optimal for their career, potentially limiting their wage growth and career progression. For instance, a parent might be prevented from accepting a promotion that requires relocation, or be unable to pursue a better-paying job in another city or region, effectively shrinking their set of feasible job offers to a smaller area. The empirical question, therefore, is which of these opposing forces dominates.

To understand the causal effect of co-parenting on parents' labour market outcomes, I exploit a custody law reform in the Netherlands that provides an exogenous shock to the prevalence of its uptake. I find that for mothers, the reform leads to a persistent decline in hourly wages, while there is no increase in hours worked. The likely mechanism driving the negative wage effect is the mobility constraint, which prevents mothers from moving towards higher-paid employment opportunities when co-parenting. This finding reveals a previously unexamined economic friction created by non-market institutions and highlights a subtle but important tension between family and economic policy, where an institutional change designed to improve social welfare in one domain unintentionally generates a significant economic cost in another.

To illustrate the mechanism driving these results, I develop a theoretical framework that yields predictions on how a move from sole to joint physical custody affects mothers' and fathers' hourly wages, hours worked, and commuting time. The model treats post-divorce work decisions as a search problem with two different rules depending on the custody arrangement. If one parent keeps sole custody, each ex-partner is free to move anywhere in the country and simply takes the job that pays the highest wage. With joint physical custody, both parents must stay in the same location, which is assumed to be the location in which they lived prior to divorce, and can only accept jobs that are close enough to the location during marriage (or stay in their previous job). The model formalises the trade-off and constraints faced by a parent choosing a job: higher wages from a more distant location (up to a maximum distance under co-parenting) versus lower wages from a more accessible one. I assume that sole custody is only granted to mothers as in my setting, fathers receive sole custody in fewer than 5% of cases.

The model predicts that under co-parenting, mothers' wages will be lower than when they have sole physical custody, while fathers' wages will be unaffected. The intuition is simple: when married, couples typically choose their location based on the best opportunities for the main earner, frequently the father.[1] The family optimises its location to maximise household welfare, which can result in sub-optimal job matches for the secondary earner, who often foregoes better professional opportunities in other locations to accommodate their partner's career. When the marriage ends, a mother is more likely to find a better job match outside of the marital location, but the co-parenting arrangement prevents her from relocating. This creates an immediate friction: her job-search set is now a geographically constricted subset of her original, unconstrained set. My model also predicts that treated mothers will accept a longer commute and that the effect on their hours worked is theoretically ambiguous, as the positive effect of reduced childcare hours is offset by negative substitution effects of lower wages and longer commutes. The remaining part of the paper tests these predictions empirically.

The challenge in answering the question of how co-parenting affects divorced parents' labour market outcomes empirically lies in identifying an appropriate exogenous shift in co-parenting uptake. Purely correlational analyses risk misattributing outcomes based on selection into co-parenting as causal. For example, Augustijn (2023) shows that the positive effects of co-parenting on fathers' life satisfaction are driven by selection into co-parenting. Hence, it is crucial to rely on exogenous variation.

My empirical strategy exploits a nationwide custody law reform, passed in 2009, as a natural experiment. The setting is well-suited for a causal analysis as it provides exogenous variation in the probability of co-parenting uptake. The reform made a joint parenting plan mandatory and signalled a judicial preference for shared residence, thereby increasing the uptake of co-parenting among parents with young children. A key strength of this

[1] In a world with equal sharing of market work and home production within couples, the reform would be expected to affect mothers and fathers in equal proportion.

setting is that the reform was mild enough not to affect divorce rates or the characteristics of the divorcing population, alleviating concerns about selection bias.

My empirical analysis uses granular administrative data from Statistics Netherlands, which provides rich, longitudinal information on the employment histories of ~23,000 divorced individuals with young children. These data, which include precise firm location at the municipality level and 4-digit postcode, allow me to track not only labour market outcomes but also the geographical movements of parents across workplaces over time, which is essential for identifying the role of reduced mobility in shaping labour market outcomes under co-parenting arrangements.

I complement the administrative records with survey data covering a representative sample of parents who divorce just before or after the reform. This survey contains detailed information on post-divorce parenting arrangements, distance between ex-partners, commuting, and re-partnering outcomes. Using these survey data, I first show that the reform led to an increase in co-parenting rates of 7.6 percentage points among parents with young children, a 27% increase relative to the pre-reform share. In addition, I rely on the survey data to test if commuting and re-partnering outcomes change for parents who share physical custody of their children.

The main result of the paper is that the reform led to a persistent 0.8% decline in hourly wages among mothers with young children, in line with the theoretical predictions. Hours worked do not increase; they temporarily drop due to lower overtime before recovering to those in the control group. Fathers' labour market outcomes remain broadly unaffected. The local average treatment effect (LATE), computed by scaling the intention-to-treat (ITT) effects by the share of compliers, implies a 10% wage decrease for treated mothers with young children in co-parenting arrangements after the reform. The observed effects are not driven by business cycle fluctuations: Controlling for local unemployment rates at the time of divorce interacted with a time trend or only comparing mothers within the same month, region, and industry does not change the observed wage decline. A placebo check using divorcing mothers of older children, for whom co-parenting is an unlikely choice, shows no wage decrease.

The likely mechanism behind the negative wage effect is reduced geographical mobility. Co-parenting constrains the ability to move for higher-paying jobs as it introduces restrictions on moving the child without the other parent's consent. I show that the workplaces of treated parents remain in closer proximity to their ex-spouses' workplaces. Using data on employer locations, I quantify this friction, showing that treated parents who take up co-parenting (compliers) work 13 kilometres closer to their ex-partners – compared to an average distance between parents of 27 kilometres. After the reform, ex-spouses are also more likely to be employed in the same region and even the same municipality, and additionally remain working in the same firm for longer after divorce if they worked together while married. This is consistent with co-parenting arrangements tying both parents to one location. The effects are concentrated among mothers who were secondary earners during their marriage and younger mothers – precisely those individuals who would have benefited most from moving for a better professional match.

The fact that the reform did not affect fathers' labour market outcomes can be rationalised through household behaviour during marriage. While married, both partners optimise as a couple, which can involve specialisation and choosing a residential location based on the career of the main earner, who is typically the father. This may result in worse job matches for mothers than fathers during marriage. Mothers may accept this trade-off, knowing that – at least before the reform – they are able to move to better professional opportunities together with their children should their marriage end in divorce.[2] Consequently, when the marriage ends, the father's career is less constrained by the marital location, while the mother, having been the secondary earner, is now stuck in a potentially sub-optimal location and faces a co-location friction.

I contribute to three strands of the literature: First, this paper forms part of the large literature on family formation or dissolution and labour supply (Calvo, 2022; Reynoso, 2019; Calvo et al., 2024; Goussé et al., 2017a,b; Wu and Pollard, 2000; Fernández and Wong, 2014). More specifically, I contribute to the insights on how post-separation legal accommodations affect labour supply decisions (Voena, 2015; Goussé and Leturcq, 2022; Halla, 2013, 2015; Nunley and Seals, 2011; Nguyen et al., 2018; Rasul, 2006; Weiss and Willis, 1985; Altindag et al., 2017; Vuri, 2018; Halla and Hölzl, 2007). While much of this literature sheds light on the effects of changes to divorce laws on still married couples, my contribution is to provide causal evidence on the labour market outcomes of divorcing parents. I show that legal institutions designed to promote child welfare can entail unintended economic costs.

Within this literature, there is scant evidence on how parents' labour market trajectories are affected by co-parenting, especially when it comes to causally identified estimates. The two most closely related papers are those by Halla (2013) and Bonnet et al. (2018). Bonnet et al. (2018) provide correlational evidence that mothers who choose co-parenting arrangements also work more hours. Thanks to the natural experiment provided by the Dutch reform, my results can instead be interpreted as causal estimates. Halla (2013) combines changes to laws regarding legal and physical custody at the level of U.S. states and investigates their effects on the fertility and divorce decisions of still married couples. In contrast, I cleanly isolate the effect of joint physical custody, which may well differ from that of legal custody, and document its effects on the labour market outcomes of divorcing couples. My setting allows me to provide causal evidence of the effect of co-parenting on wages and hours, and clearly illustrates the likely mechanism driving my results.

[2] If divorce risk is taken into consideration at all. Many individuals underestimate it for their own marriage (Berresheim and Koll, 2023).

I also contribute to work on households' labour market and location decisions (Gemici, 2023; Braun et al., 2021), especially those of dual-earner couples (Guler et al., 2012; Foerster and Ulbricht, 2023; Alonzo, 2025; Ranosova, 2025; Rueda and Wilemme, 2025; Venator, 2024; Jayachandran et al., 2024; Foged, 2016; Costa and Kahn, 2000). These papers study how co-location frictions can involve trade-offs between partners' careers and how they limit the job search or migration decisions of one partner, often the woman, resulting in gender disparities in wages and employment. Divorced parents who share physical custody for their children are no longer part of the same household, but I demonstrate that they face similar location constraints, and that this version of co-location frictions also has important implications for gendered wage dynamics.

Finally, this paper also relates to the relevance of outside options for observing gendered effects of policies that are gender neutral on paper. Coviello et al. (2024) show that gender-neutral policies such as a minimum wage can result in unequal labour market outcomes for women and men when they face differences in outside options in the labour market.[3] I show that outside options also matter for creating gendered effects of policies in the domain of family law such as custody. In an on-the-job search and matching framework, my results can be interpreted as smaller offer acceptance sets for co-parents. The effect of co-parenting is a priori gender neutral, but results in gendered outcomes because mothers' current wages rank lower in their distribution of wage offers than the current wages of fathers relative to fathers' distribution of wage offers, i.e., mothers face a worse outside option, creating a differential incentive to move.

The remainder of the paper is organised as follows: Section 2 provides an overview of the Dutch institutional setting and details the 2009 reform. In Section 3, I develop a theoretical framework linking co-parenting to labour market outcomes. Section 4 describes the data sources and construction of the sample. Section 5 presents the empirical strategy and discusses the identification assumptions. Section 6 reports the main results and evidence for the proposed mechanism. Section 7 discusses the broader implications of my results, and Section 8 concludes.

2 Institutional Setting

2.1 2009 Reform

Evolution of Dutch divorce law From 1998, Dutch family law gave both parents joint legal custody after divorce. Thus, prior to 2009, Dutch family law had already favoured legal custody for both parents yet effectively granted sole physical residence only to one parent – typically the mother – unless both parties explicitly agreed to a

[3] Men and women may also react differently to the same outside option (Fredriksson et al., 2025).

shared arrangement. This de facto sole physical custody regime constrained fathers' access to children and limited judicial oversight over custodial schedules. In response to rising joint-residence advocacy by family sociologists and fathers' rights advocacy groups, the Dutch Parliament passed the Promotion of Continued Parenting and Careful Divorce Act (*Wet bevordering voortgezet ouderschap en zorgvuldige scheiding*, Stb. 2008, 500) in late November 2008 and it came into effect on 1 March 2009.

Importantly for my application, unlike in other countries that mandate a minimum period of separation before divorce, in the Netherlands there is no period of separation required before being able to file for divorce, neither before nor after the reform.

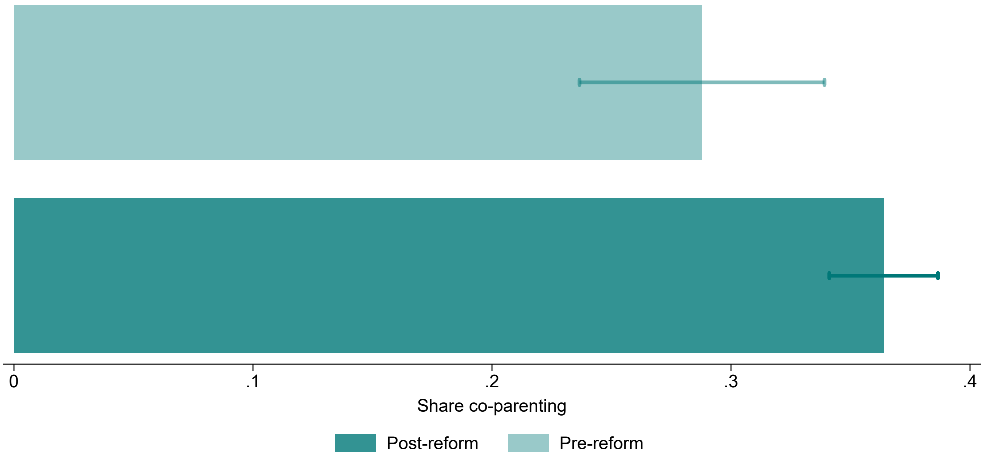
Reform The 2009 reform aimed to promote joint physical custody after divorce, for the sake of children's well-being. The reform amended the Dutch Civil Code, codifying the principle of "equal parenting" (*gelijkwaardig ouderschap*), meaning that children have the right to equal care from both parents, also once they are divorced.[4] It also states that from 1 March 2009 onward, parents filing for divorce (or the dissolution of a registered partnership) must include a signed parenting plan with their divorce petition. The reform reinforced that post-divorce, both parents remain jointly responsible for their children, not only financially but also in terms of parenting time. While the law stopped short of presuming a perfectly equal timeshare, the courts interpreted it as a strong encouragement of shared residence arrangements.[5] In other words, the expectation in 2009 became that, absent contraindications, children should spend a substantial amount of time with each parent. Sole physical custody now had to be justified, especially if the other parent requested a more equal arrangement. Moreover, the assumption of what is the child's best interest shifted the focus from stability of residence (maximised under sole physical custody) to continued equal parental engagement (maximised under co-parenting).

Throughout this paper, I only analyse individuals who experience divorces instead of also considering separations from cohabitation. The reason is that before 2023, unmarried fathers did not automatically receive legal custody of their children; the mother had to agree. Thus, claiming physical custody was more difficult for fathers without previous legal custody. To avoid the selection into joint legal custody of cohabiting couples, I only consider those couples who were married before separating.

[4]The law states (machine-translated) that "A child over whom the parents exercise joint parental authority, after dissolution of marriage (other than by death), dissolution of registered partnership (other than by death), or termination of cohabitation (where a § 252 annotation has been made), has the right to equal care and upbringing by both parents" (*Burgerlijk Wetboek Boek 1, artikel 247, lid 4*).

[5]One Family Court ruling shortly after the law came into effect explicitly noted that since 1 March 2009, Dutch children have the right to equal care from both parents. The ruling by the Dutch Supreme Court (Hoge Raad) on 21 May 2010 clarifies that post-reform, joint physical custody became the "prioriteitsmodel" (default preference), not an absolute rule. *Ruling ECLI:NL:HR:2010:BL7407.*

Figure 1: Co-parenting share increased by 7.6 percentage points



Note. Share of parents choosing co-parenting arrangements after divorce for parents separating between 2007 and 2012. The bars plot coefficients of a linear probability model in which co-parenting status is regressed on a reform indicator. Horizontal capped lines are 95% confidence intervals based on robust standard errors. Own calculations based on NFN data.

Parenting plan The reform also mandated that a parenting plan (*ouderschapsplan*) be submitted as part of the divorce petition of couples with minor children.[6] These parenting plans cover residence schedules (weekday/weekend split, holiday rotation), communication protocols (dispute resolution, information sharing), and financial support arrangements. The idea was to have parents plan in advance for how they would continue to share responsibilities, thereby reducing post-divorce conflicts. Crucially, in this plan, parents specify the living arrangement (either co-parenting or some form of visitation agreement). Importantly, the parenting plan also specifies the maximum mutual living distance (Dutch National Institute for Budget Information). This imposes restrictions on unilateral relocation: Once parents have agreed to joint physical custody, one parent cannot relocate with the child without the other parent's consent. The mandatory nature of the parenting plan also made the reform very salient (as shown in the Google searches for "parenting plan" in Figure C1) and effective: The share of divorcing parents who chose joint physical custody rose by 7.6 percentage points (Figure 1, Table A1).[7]

Distance between ex-spouses One of the requirements for making co-parenting work is that parents continue to live close to each other.[8] Indeed, survey data from the New Families in the Netherlands survey show that ex-spouses who co-parent live less than half as far apart as those who do not co-parent (Figure 2) and that this pattern persists over time (Figure C4). Of course, this pattern alone could either be due to parents who would

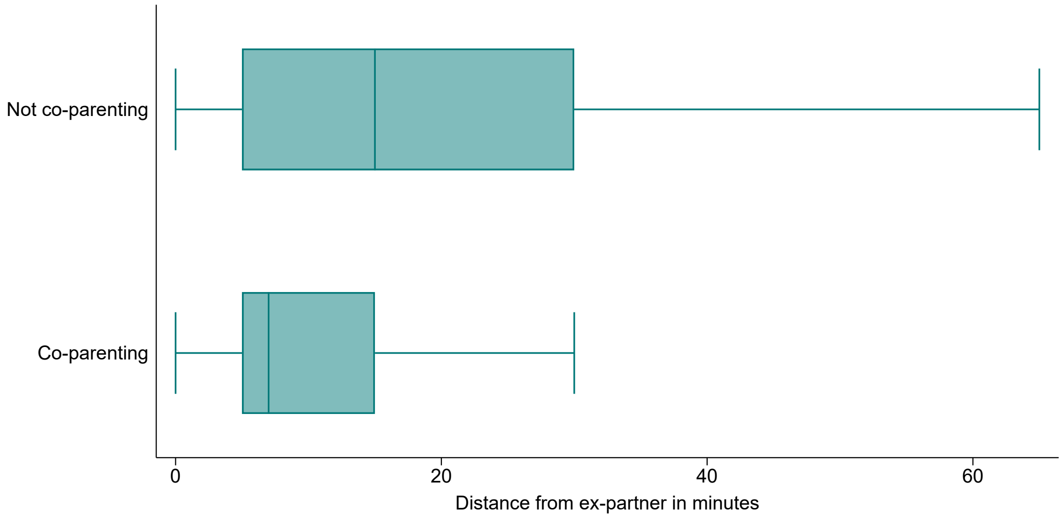
[6]A divorce petition involving children will not proceed without a parenting plan, unless the petitioner convinces the court that it was impossible to obtain one due to the other party's refusal *(Article 815 Rv)*.

[7]Poortman and van Gaalen (2017) also report a 7 percentage point increase in joint physical custody.

[8]They must also be on speaking terms and have little conflict.

choose to live closer to each other in any case deciding to co-parent, or be a consequence of co-parenting arrangements on the acceptable distance to one's former spouse. I provide evidence in support of the latter interpretation in Section 6.2.

Figure 2: Distance between divorced parents with minor children by co-parenting status



Note. This figure shows the travel distance between former spouses in minutes. The box indicates the 25 to 75 percentile, and the horizontal line inside the box denotes the median. Horizontal capped lines are 95% confidence intervals. Own calculations based on NFN data.

Childcare environment As I analyse the labour market behaviour of parents with young children, it is important to understand to what extent the childcare environment may constrain or facilitate parents' employment. At the time of the custody reform, formal childcare in the Netherlands was cheap and relatively widely used. Thanks to a reform in 2005 that heavily subsidised formal childcare usage, take-up had risen. Parents' own contribution is dependent on their income and the number of children, but on average, they only paid for 19% of the childcare cost in 2008 (State Budget Archive). Thus, in the period under analysis, typical out-of-pocket costs to parents were very low by international standards.[9]

3 Theoretical framework

3.1 Intuition

After divorce, mothers work and earn more, compensating for some of the decrease in household earnings (Johnston et al., 2025). At the same time, adopting joint physical

[9]Culturally, using formal childcare was also accepted in the Netherlands by the late 2000s, yet it is still common to have a grandparent day (*"opa- en omadag"*) of informally provided childcare. In terms of quality, survey responses for 2008 indicate that more than 90% of parents were satisfied with the childcare institution their child attended (SEO Economic Research).

custody changes who spends how much time looking after the child and where the parents can live. The size of post-divorce labour market adjustment may thus differ by the prevailing custody regimes at the time of the reform. Figure 3 illustrates the key differences between the two custody regimes. There are three channels to consider when comparing the earnings of mothers choosing joint physical custody vs. sole physical custody:

(i) **Mobility constraint:** Joint physical custody arrangements put a stronger constraint on the distance between parents, restricting job search geography. In particular, those who were secondary earners during their marriage and who would have considered moving for higher wages under sole physical custody may now be locked into a smaller local labour market with lower best wage offers. At the same time, if mobility constraints bind, relatively lower wages could exert a negative substitution effect on hours.[10]

(ii) **Commuting costs:** Commuting across greater distances may be one adaptation to a tighter restriction on the location of residence in order to still be able to access employment opportunities further away. Doing so involves a trade-off between potentially higher hourly wages and time spent on paid work or leisure.

(iii) **Time constraint:** With childcare split more evenly under joint custody, the parent who was the main caregiver (typically the mother) could increase paid hours relative to the sole custody arrangement, as predicted by standard household models (Becker, 1985).

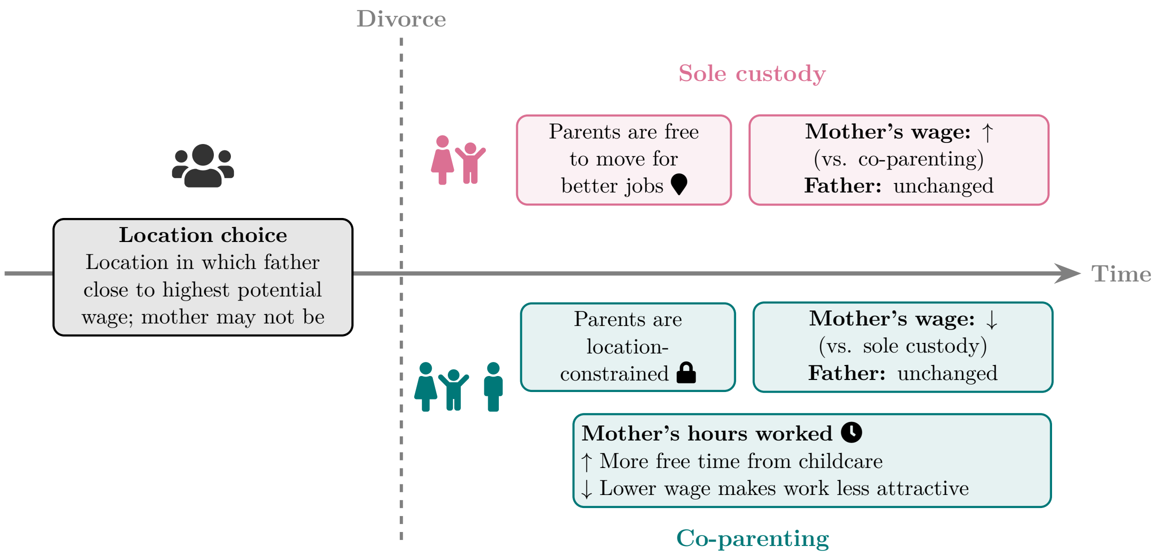
Which force dominates is an empirical question. If the mobility channel is paramount, we expect that implementing joint rather than sole physical custody arrangements would induce: (i) shorter distance moves by treated compared to non-treated parents; (ii) wage declines driven by parents who stay near their ex-spouse; (iii) negligible effects on wages and hours for individuals whose pre-divorce location already matched their job (mostly fathers); (iv) possibly longer commutes for co-parents, and (v) limited changes in hours worked.

3.2 Model setup

The aim of this theoretical framework is to derive predictions on the post-divorce labour market outcomes of moving from sole physical custody (SPC) by mothers to joint physical custody (JPC) by both parents. Section 6 tests these predictions empirically.

[10]Sharing custody could also result in lower child-related expenses and a negative income effect on hours.

Figure 3: Stylised illustration of the two custody regimes



Note. Illustration of differences between sole and joint physical custody for mothers and fathers for the average Dutch family.

Labour market environment Ex-spouses $i \in \{m, f\}$ share a child, where m refers to mothers and f to fathers. There is a continuum (or discrete set) of local labour markets,[11] $x \in X$. Each labour market offers wage draws $w_i(x) \sim F_i(x_i)$ that differ across locations. A job is a pair of location and best wage offer in that location $(x_i, w(x_i))$. Individuals consider one job per location. If they receive multiple job offers in the same local labour market, they only consider the one paying the highest hourly wage.

Time constraint Parents spend their time on work, commuting, leisure, and childcare. There is an exogenous time amount of childcare parents must supply; the split between them varies by custody arrangement. Under regime $R \in \{\text{SPC}, \text{JPC}\}$, parent i supplies q_i^R. Children's total care need is fixed, such that $q_m^{\text{SPC}} + q_f^{\text{SPC}} = q_m^{\text{JPC}} + q_f^{\text{JPC}}$. I define σ_i^R as the share of total childcare that is provided by parent i under custody regime R: $\sigma_i^R = \frac{q_i^R}{q_m^R + q_f^R}$. Commuting time $k_i = d(x_i, x_{r,i})$ is a function of distance between the workplace x_i and the location of residence $x_{r,i}$. $d(x_i, x_{i,r})$ maps the distance between location of the job and location of residence into commuting time.[12] Individuals' time constraint is therefore given by

$$1 = l_i + h_i + k_i + q_i^R,$$

where l_i is leisure, h_i are market hours, k_i is commuting time, and q_i^R is child-care time.

[11]Assuming a continuum only simplifies derivatives; all results hold on a finite grid.

[12]Empirically, the mapping of distance to commuting time also depends on urbanity and the chosen means of transportation, but as this is not relevant for the model, it is omitted to simplify the mapping.

Location constraint Under SPC each parent can pick any location of residence in the country; under JPC both parents must remain in the same location as during marriage $x_{r,i}$ and may only take jobs whose home-to-work distance is no more than a maximum distance $\bar{d}$. They face the following Euclidean distance constraint under JPC:

$$\|x_i - x_{r,i}\| \leq \bar{d}$$

This location constraint binds more strongly for mothers because of household optimisation during marriage: Mothers of young children are overwhelmingly employed part time, whereas the median father works full time. Hence, it is optimal for a representative couple to choose a location in which the father's wage is close to his best wage offer. The clearest example comes from movers: When couples move, they on average move to locations in which the husband sees an earnings increase, whereas his wife experiences no to negative earnings growth (Jayachandran et al., 2024). Once this location becomes the child's centre of life (school, friends, etc.) it becomes likely for the divorcing parents to remain close to that location. If parents cannot relocate unilaterally while maintaining JPC, fathers tend to be better matched to their jobs than mothers, in the sense that their wage is closer to the maximum wage in their job offer set after divorce. Mothers therefore have two job search sets: Under SPC, they search across all locations; under JPC, the only feasible locations are those within a circle of radius $\bar{d}$ around x_r.

Budget constraint Parents also face a period budget constraint:

$$c_i = w_i h_i - M_i^R,$$

where M_i^R is the amount that needs to be spent on the child. This is assumed to be exogenous and depends on the custody arrangement. For mothers, it is lower under JPC than SPC because the child is provided for by the father half of the time, i.e. $M_m^{\text{JPC}} < M_m^{\text{SPC}}$.[13] I abstract from spousal alimony as that is not affected by the choice of custody regime.

3.3 Optimisation problem

Parents maximise expected life-time utility (post-divorce):

$$\max_{\{h_i, x_i\}} U_i = \mathbf{E}\left[\sum_{t=t_{\text{div}}}^{T} \beta^t \left(\nu(w_i h_i - M_i^R) - \eta(h_i) + \phi(\sigma_i^R)\right)\right]$$

[13] I am implicitly assuming that child support payments under SPC are lower than the true cost of raising a child. Given that the level of child support payments is relatively low (about 250-300€ per month, depending on age of the child and income of the paying parent), this is empirically likely on average. The results do not depend on this assumption and hold equally when omitting the term M_i^R.

subject to the time and, under JPC, distance constraints. $\phi(\sigma_i^R)$ denotes the utility derived by parent i from spending share σ_i^R of total childcare time with the child.

Given the static nature of the set-up, the optimisation problem is equivalent to maximising flow utility subject to constraints:

$$u_i = \nu(c_i) - \eta(h_i) + \phi(\sigma_i^R),$$

where utility of consumption $\nu(c_i)$ and utility of child time $\phi(\sigma_i)$ are increasing and concave, and the disutility of working $\eta(h_i)$ is increasing and convex.

Taking the choice of custody arrangement as given, parents choose their optimal job, i.e. the utility-maximising location x_i^* with its wage offer $w_i^*(x_i^*)$ and their optimal hours h_i^*. Under JPC, the mobility constraint binds for mothers if and only if their optimal location x_m^* lies outside the radius $\bar{d}$ around x_r.

3.4 Effect of moving from sole to joint physical custody

What are the model predictions for hours, hourly wages, and commuting when switching from sole to joint physical custody? The model predicts wage decreases and commuting time increases for mothers who become location constrained. The theoretical prediction on mothers' hours is ambiguous. Fathers' hours are predicted to weakly decrease; there are no predicted changes to fathers wages or commuting time. Below, I state each of these propositions in turn. Detailed derivations are provided in Appendix D.

Hourly wages

Think of the wage at a location as a lottery ticket value $w(x)$. SPC lets a mother look at every ticket in the country and pick the highest-paying one. JPC removes any tickets lying beyond the distance $\bar{d}$. A simple "maximum over a subset" rule implies:

- If the best SPC ticket is outside the JPC circle (frequently the case for the lower-earning spouse), the maximum inside the circle must be strictly lower.
- If the best SPC ticket is already inside the circle (assumed for the main earner), the maximum is unchanged.

One can formally show that the wage weakly declines relative to SPC if the mobility constraint binds (refer to Appendix D for details).

Proposition 1. *The switch from SPC to JPC lowers mothers' expected hourly wages:*

$$w_m^{\text{JPC}} \leq w_m^{\text{SPC}}.$$

Proof. See Appendix D. □

Corollary 1 (No wage effect for fathers)**.** *If the father's SPC optimum already satisfies* $||x_f^{\mathrm{SPC}} - x_r\| \leq \bar{d}$*, then the distance constraint is slack,* x_f^{SPC} *remains feasible after the reform and* $w_f^{\mathrm{JPC}} = w_f^{\mathrm{SPC}}$*.*

Commuting

The result on commuting follows trivially from the assumptions, but can also be shown formally.

Assumption 1 (Radial monotonicity of commuting time)**.** *For every pair of residence locations* $x_{r,1}, x_{r,2} \in X$*,*

$$\|x_{r,1} - x_i\| < \|x_{r,2} - x_i\| \implies d(x_{r,1}, x_i) < d(x_{r,2}, x_i).$$

Proposition 2. *Assume radial monotonicity of commuting time and recall that under JPC both parents reside in location* x_r*. Then for every realisation of wage draws*

$$d_m^{\mathrm{JPC}} \geq d_m^{\mathrm{SPC}} = 0, \qquad d_f^{\mathrm{JPC}} = d_f^{\mathrm{SPC}} = 0,$$

with $d_m^{\mathrm{JPC}} > 0$ *whenever the mother's JPC job lies at* $x_m^{\mathrm{JPC}} \neq x_r$ *(strictly positive probability). Hence, under JPC, mothers commute for longer than under SPC. There is no change for fathers.*

Proof. See Appendix D. □

Hours

The theoretical prediction for moving from SPC to JPC for mothers' working hours is ambiguous because it creates several counteracting effects for mothers. For fathers, the predicted effect is weakly negative.

Counteracting effects on hours First, fewer childcare hours translate into a relaxation of the time constraint: The switch to JPC reduces a mother's childcare obligations ($\Delta q_m = q_m^{\mathrm{JPC}} - q_m^{\mathrm{SPC}} < 0$). At the same time, mothers commute weakly more under JPC, reducing the hours available for work. But even if commuting increases, it is reasonable to assume that the reduction in childcare is typically larger, resulting in a net gain in disposable time. As a result, the marginal value of an hour of leisure (its shadow price λ_i) falls. This makes working relatively more attractive, creating an incentive to work more hours.

However, this positive effect on hours worked through a relaxation of the time constraint is counteracted by two forces:

(i) Mothers also experience a negative substitution effect due to lower wages. As established in Proposition 1, the mobility constraint under JPC forces mothers to accept a lower wage ($\Delta w_m = w_m^{\text{JPC}} - w_m^{\text{SPC}} \leq 0$). A lower wage makes working less rewarding compared to leisure. This substitution effect creates a clear incentive to work fewer hours. The strength of this effect depends on the elasticity of labour supply.

(ii) There is also an income effect regarding the cost of raising children when switching from SPC to JPC: For mothers, the monetary cost of children M_m decreases, which in turn lowers the marginal utility of income, and thus the incentive to work more hours. For fathers, the opposite is true.

In total, the effect of moving from SPC to JPC on hours worked is ambiguous for mothers. The ultimate change in a mother's working hours depends on which of these effects dominate. It is only positive if the relaxation of the childcare constraint outweighs (i) the additional commuting time *and* (ii) the negative substitution effect arising from lower wages *and* (iii) the income effect from lower child-related expenses.

For fathers, the predicted change in hours is also ambiguous. They do not experience a change in wages or commuting time, and thus there is no substitution effect for them. They should only experience a tightening of the time constraint due to increased childcare hours, leading to reduced market hours and/or leisure, and a potentially offsetting income effect through higher child-related expenditures that raises the incentive to work more hours.

Proposition 3. *The prediction on hours from moving from SPC to JPC is ambiguous but weakly positive for mothers when assuming that the time constraint effect dominates the substitution effect, and weakly negative for fathers (assuming the time constraint outweighs the positive income effect for fathers).*

Proof. See Appendix D. □

The theoretical predictions of the effect of joint physical custody on wages and hours worked are tested empirically in Section 6. I also provide suggestive evidence on commuting time.

4 Data

The main datasets used are administrative files from Statistics Netherlands (Centraal Bureau voor de Statistiek, CBS). The labour market data consist of linked employeremployee records and provide information on the universe of all Dutch individuals' employment histories. They can be combined with data from the municipality registers that contain characteristics such as age, gender, and household composition. They can also be linked to the number and age of children as well as educational attainment.[14] On the employment side, I observe monthly hours and total earnings for all job spells. Based on these, I compute hourly wages. All employment histories can be linked at the firm level. Employer-side information includes sector, size, and, crucially for my proposed mechanism, the municipality and 4-digit postcode in which a firm is located. I use the municipality as a measure of firm location and compute the distance between firms based on these centroids.[15] I do not observe the municipality of residence. On the household side, I use the information on household composition to construct a panel of marriage spells and combine that with individuals' employment histories. Marriages include registered partnerships as these cannot be differentiated in the data. Legally, registered partnerships confer the same broad rights and responsibilities on partners as marriage, with only minimal differences between the two.[16] The reform also affects them equally.

I complement the administrative data with the New Families in the Netherlands survey (NFN), collected by Anne-Rigt Poortman (Utrecht University) and collaborators, which covers divorcing families before and after the reform. It contains detailed information on the parenting arrangement chosen, travel distance between ex-partners' households, commuting time to work, the time children spend with each parent, and demographic characteristics of parents and their children (Poortman et al., 2014; Poortman and Van Gaalen, 2019).

In the administrative data, I do not observe which custody regime is chosen. Hence, I use the NFN, which allows to observe the exact parenting regime chosen, to identify the share of compliers and for descriptive analyses with variables that are not available in the administrative records. My main analysis remains in an intention-to-treat framework.

4.1 Sample selection

In my sample, I include individuals who divorce between September 2007 and August 2010, i.e., in the 18 months before and after the reform. This window provides a large enough sample while still being relatively close together in terms of divorce date. Those

[14] Information on educational attainment is not available for all individuals.

[15] There were 441 municipalities in the Netherlands on January 1st, 2009, so this measure provides detailed geographical information. The number of municipalities slightly shrinks over time as some are consolidated for administrative reasons. Using municipalities or 4-digit postcodes yields very similar results.

[16] In the administrative data, I observe the first of either 1) the actual date of divorce, 2) the date of separation when still-spouses cease to cohabit but have not yet finalised their divorce. Even when not yet legally divorced, the economically more relevant dimension is cohabitation status (also see Dasgupta et al. 2025).

divorcing between September 2007 and February 2009 form the control group, and those divorcing between March 2009 and August 2010 the treatment group. Extending it to 24 months before and after the reform does not change my results, see Section 6.1. For the main analysis, I restrict the sample to parents aged between 18 and 60 with the youngest child up to age ten at the time of divorce. There are some fathers for which the age ceiling is binding, i.e., they are older than 60 but have a child aged ten or younger. For mothers, it is not binding. I choose this age cut-off as to avoid capturing early retirees. I further restrict the sample to a balanced panel of those whose marriage lasted for at least 18 months, and who do not live with a new partner for at least 24 months after divorce. After applying these restrictions, the sample contains 40,000 divorces, 23,000 of which involve children under the age of ten. Conditioning on at least 18 months of marriage means that everyone married before the reform date, such that the marriage decision should not have been affected by custody considerations. The reason for excluding re-partnered individuals is that including them in the sample might alter labour supply incentives through income effects at the household level.[17] Re-partnering is an interesting and economically relevant outcome nonetheless, which I touch upon separately.

I choose the sample of parents with the youngest child aged ten and younger based on predictions of co-parenting using the NFN data. The group of compliers – those who did not adopt co-parenting before the reform but chose it after – is also highest among this group (Figure C2). Previous studies have also found joint physical custody to be most common when the children are of primary school age (Poortman and van Gaalen, 2017; Weston et al., 2011), specifically up to ten years old (Cancian and Meyer, 1998). Teenagers dislike the weekly moves that co-parenting entails. This is important because from the age of twelve, the child's preference regarding physical custody is strongly considered in the judge's decision.[18][19]

What are characteristics of parents who choose co-parenting? They tend to be well-educated (Figure C3) and enjoy a higher socio-economic status (Bakker and Mulder, 2013). Part of the reason for this are financial considerations: Co-parenting necessitates two bedrooms for the child and other duplicated costs, meaning that the total cost of supporting the child is generally higher than in a scenario in which the child lives only with one parent. Using NFN data, it can also be shown that parents who split market work more equally during marriage are more likely to opt for co-parenting arrangements after divorce.

[17]The results are not sensitive to this and are unchanged when including re-partnered parents.

[18]Parents can also split their children between themselves. However, this option is generally not seen as in the best interest of the children, and therefore not commonly implemented by courts.

[19]The Australian Institute of Family Studies has found a very similar age range that makes co-parenting most likely (AIFS, 2011).

4.2 Variable construction

Using the administrative data, I analyse how the reform affects two main sets of outcomes: labour market outcomes and geographical distance between former spouses. The labour market outcomes are standard: Employment is an indicator variable equal to 1 if an individual is employed in a given month and 0 otherwise; earnings are monthly and capture total compensation, including potential bonuses and allowances; hours worked are monthly contractual hours plus overtime; and hourly wages are constructed as the ratio between earnings and hours. I exclude individuals employed for fewer than 10 hours per week. The data do not cover self-employed workers.

For geographical mobility, I use the following outcomes: Distance between former spouses, the probability of being employed in the same region or same municipality as the former spouse, and the probability of continuing to work in the same firm after divorce. The three measures are calculated based on the location of individuals' employers. For the distance measure, I compute the distance between the municipality centroid of the workplaces of both ex-spouses (in metres). The two measures of being employed in the same region or municipality as one's ex-spouse are indicator variables.[20]

The data contain firm location only at the statistical business unit (head office) level, not at the establishment (branch) level as the municipality or postcode information in the company register data refers to the seat of the administrative and legal unit. This means that firms with multiple establishments within one administrative unit cannot be attributed the correct municipality. Therefore, I limit the analysis of geographical mobility to individuals employed in firms with only one establishment.

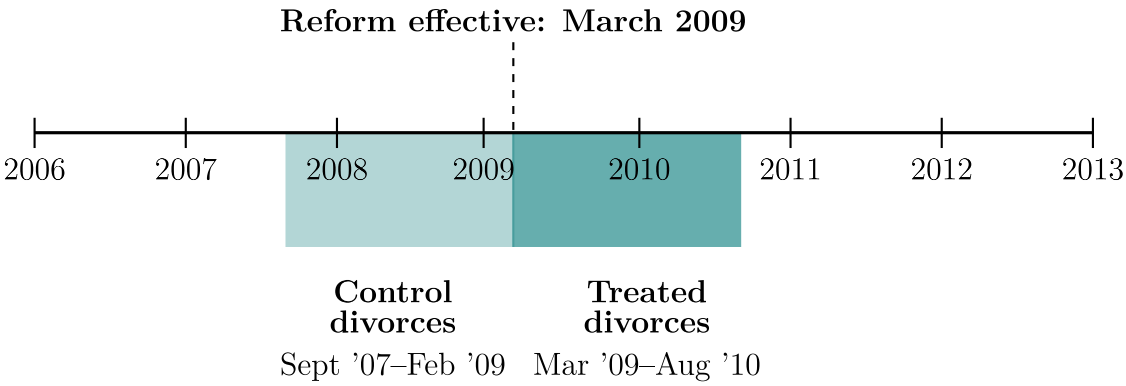
5 Empirical Design

It is generally difficult to disentangle the causal effects of co-parenting from selection. To identify the causal impact on parents' labour market outcomes, I exploit the 2009 custody law reform as an exogenous shifter in the likelihood that parents choose co-parenting after divorce. The empirical strategy is a difference-in-differences (DiD) design: I compare parents of young children who divorced in the 18 months after the reform to those who divorced in the 18 months before the reform (see Figure 4 for a timeline).

The design is an intention-to-treat (ITT) design: The reform did not make co-parenting mandatory, but served more as a nudge. Nonetheless, it led to a 27% increase in its uptake.

[20]Throughout this paper, a region refers to the Dutch COROP regions, which are equivalent to the European NUTS3 level. There are 40 COROP regions in the Netherlands. At the time of the reform, there was no statistical unit for labour market regions. Such *arbeidsmarktregios* were introduced in 2012, and are slightly coarser than the COROP regions (there are 35 labour market regions). However, many *arbeidsmarktregios* were constructed to overlap with the COROP regions.

Figure 4: Timeline and definition of treatment and control groups.



Note. The reform came into effect on 1 March 2009. The 18-month windows before and after define the control and treatment groups.

My empirical design uses the reform-induced variation as a source of identification. The DiD estimates provide reduced-form evidence on how co-parenting affects hours worked, wages, and earnings, as well as the distance between former spouses and the likelihood that parents remain in the same location.

I estimate the following event-study specification:

$$Y_{itr} = \sum_{j \neq -1} \gamma_{\mathbf{j}} \cdot \mathbf{1}[m_{it} = j] \cdot \mathbf{1}[\text{Treated}_i = 1] + \sum_{j \neq -1} \beta_j \cdot \mathbf{1}[m_{it} = j] + \rho_i + \delta_{tr} + \nu_{itr} \tag{1}$$

where Y_{itr} is the outcome of interest ((log) hours, wages, earnings, and later distance from ex-partner) for individual i in month-year t and region r. Treated$_i$ is treatment status, equal to 1 if the individual divorced after the reform. γ_j is the coefficient of interest, comparing outcomes in the treated to those in the control group at the same time relative to divorce, m_{it}. To ensure that observed differences can be attributed to the reform and not to differential trends in local labour markets, I control for month-year×region fixed effects δ_{tr}. These capture macroeconomic and seasonal variation. These time fixed effects are crucial given the proximity of the reform to the Great Recession[21][22] and mean that the variation identifying the effects of the reform comes from comparing individuals divorcing in the same region, netting out region-specific time effects. I also include individual fixed effects ρ_i to account for time-invariant individual characteristics such as ability or preference. Standard errors are clustered at the individual level because treatment is

[21] In the Netherlands, the Great Recession started in the fourth quarter of 2008 (CBS, 2009).

[22] As the primary focus lies on women's labour market outcomes, the Great Recession is somewhat less of a concern as it affected men's labour market outcomes more than women's. Moreover, if anything, women raise their labour supply during recession, cushioning the negative overall employment effects of the Great Recession (Doepke and Tertilt, 2016). The gendered differences were largely due to differential sorting into occupations and industries by women and men (Alon et al., 2022; Hoynes et al., 2012).

assigned once per person and each individual appears in multiple periods. This allows the error terms to be arbitrarily serially correlated within each individual's labour market path (Bertrand et al., 2004). The identifying assumption is that – conditional on the set of fixed effects –, in the absence of the reform, labour market outcomes of parents in the treatment group would have followed the same trend as the labour market outcomes of those in the control group.

My preferred specification includes month-year×region fixed effects. The results are also robust to specifications with only month fixed effects and month-year×region×sector fixed effects. The latter strengthens the argument that the results are not due to the reform coinciding with the Great Recession as this specification compares individuals within the same sector and region, abstracting from macroeconomic conditions. This also rules out that potential effects are driven by changes in labour demand. All three specifications show very similar results. The similarity between the main specification and the month-year×region×sector fixed effects specification should assuage concerns about selection into industries, which may be endogenous. The sector is chosen at the two-digit level, a relatively broad definition to allow for sufficient variation within month-year×region×sector.

In addition to the difference-in-differences design on the sample of divorcing parents of young children, I conduct a placebo analysis comparing individuals divorcing before and after the reform with *older* children (aged 11 to 17). The parents of older vs. younger children should be more similar to each other than those without children, hence the two groups can be expected to be more comparable to each other on unobservables than other possible placebo groups. The difference lies in the fact that parents of teenagers are very unlikely to choose co-parenting arrangements. By demonstrating that the reform had no effect on parents of older children, this specification shows that the post-reform change can plausibly be attributed to the custody-law reform rather than to macroeconomic conditions.

5.1 Identification

No selection into treatment The treatment and control groups are balanced on observable pre-divorce demographic and labour market characteristics, including the number of children, hours worked, and secondary-earner status (see Tables 1 and 2).[23] The only meaningful differences are in earnings and wage levels, which rise over time and are thus higher among the treated group, as well as in age at divorce and the age of the youngest child at divorce, all of which also rises slightly because of rising ages at marriage. For those variables that are not balanced across the two samples, I show that the differences

[23]The samples are also very similar in terms of sectors of employment. Table A2 shows the top ten sectors of employment and their employment shares for mothers and fathers in my sample.

are due to general time trends (Figure C5). This is not a concern for my identification strategy as it does not require balance in levels, but in trends, for the parallel trends assumption to hold.

Table 1: Demographic characteristics of divorcees at the time of divorce

	Pre-Reform		Post-Reform		Post-Pre	*p*-value	
	Mean	Obs	Mean	Obs		unadj.	Holm
Age youngest child	5.40	15162	5.80	13117	0.40	0.000	0.000
Number of children	2.20	15162	2.20	13117	0.00	1.000	1.000
Age	37.20	15162	37.80	13117	0.60	0.000	0.000
Age ex-husband	40.00	15162	40.70	13117	0.70	0.000	0.000
High education	0.28	12103	0.29	10606	0.01	0.097	0.875
High education ex-husband	0.31	10423	0.33	9393	0.02	0.019	0.194

Note. Pre- and post-reform variable means and their difference. The last two columns report conventional (unadjusted) *p*-values and those adjusted for multiple hypothesis testing using the Holm correction. Variables are measured in the semester before divorce. High education refers to the share of individuals with higher vocational training or a university degree. Data on education levels is not available for all individuals.

Table 2: Labour market variables of divorcees around the 18 months before and after the 2009 custody reform

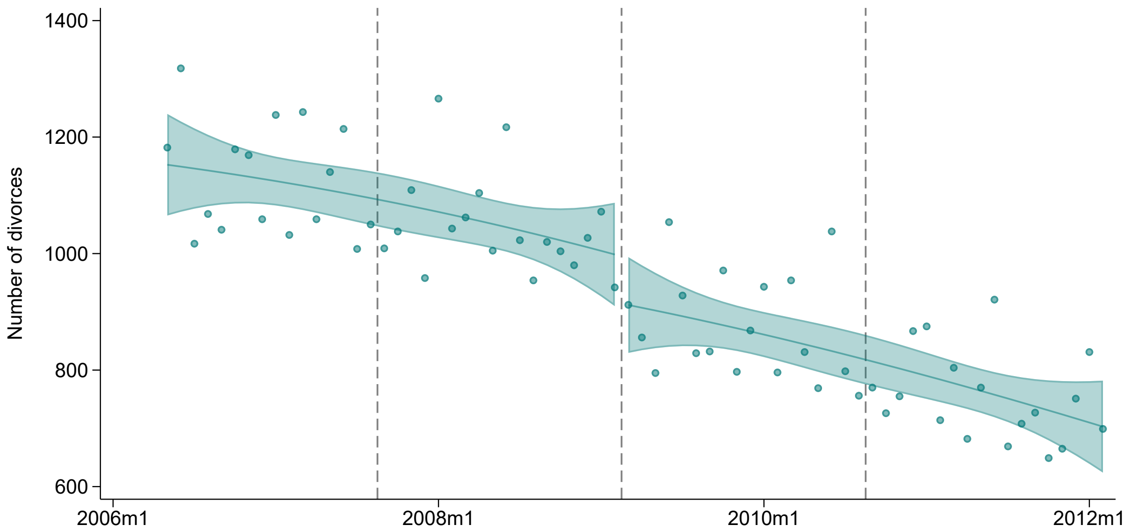
	Pre-Reform		Post-Reform		Post-Pre	*p*-value	
	Mean	Obs	Mean	Obs		unadj.	Holm
Employed	0.58	15162	0.60	13117	0.02	0.001	0.010
Earnings (monthly)	2010.00	8740	2132.00	7850	122.00	0.000	0.000
Hours (monthly)	103.00	8740	104.00	7850	1.00	0.072	0.860
Wage (hourly)	19.05	8740	19.90	7850	0.85	0.000	0.000
Part-time	0.87	8740	0.87	7850	0.00	1.000	1.000
Employed ex-husband	0.75	15162	0.75	13117	0.00	0.990	1.000
Earnings ex-husband	4274.00	11385	4403.00	9781	129.00	0.083	0.916
Hours ex-husband	164.00	11385	164.00	9781	0.00	0.978	1.000
Wage ex-husband	26.00	11385	26.80	9781	0.80	0.013	0.125
Part-time ex-husband	0.16	11387	0.16	9781	0.00	0.926	1.000
Secondary earner	0.72	13297	0.71	11600	-0.01	0.110	0.916
Equal earners	0.13	13297	0.13	11600	0.00	0.756	1.000
Main earner	0.15	13297	0.16	11600	0.01	0.068	0.860

Note. Pre- and post-reform variable means and their difference. The last two columns report conventional (unadjusted) *p*-values and those adjusted for multiple hypothesis testing using the Holm correction. Outcomes are measured one year before divorce. Earnings, hours, and wages are conditional on employment. Wages and earnings are denoted in 2015 prices.

In the NFN survey, the means of the relevant variables in the pre- and post-reform samples are similarly balanced (Table A3). One difference is that the pre-reform sample is smaller than the post-reform one. To account for this and make the samples representative of the national population, sample weights are included. The NFN sample is also similar to the administrative sample in terms of demographic characteristics. One difference lies in how the labour market variables are measured: In the NFN, hours are weekly instead of monthly, but comparable when scaled up. While the NFN asks about paid work in general (including self-employment etc.), in the administrative records, I only observe employees.

No strategic timing of divorce A second condition for identification is that there is no break in the number of divorces around the date of the reform. One concern may be that the reform could have induced divorces that would otherwise not have taken place, prevented some individuals from initiating divorce or led them to anticipate or postpone it. Visual inspection of divorce rates of my sample does not support this (Figure 5) and neither does the Cattaneo-Janssen-Ma density test (Figure C6); there is no discontinuity in the number of divorces at the time of the reform or bunching right before. To further rule out an abrupt change in the number of divorces after the reform, I conduct a Chow test, allowing for heteroskedasticity. The Chow test does not reject the null hypothesis of no discontinuity at the time of the reform (Table A4). On top of providing supporting evidence for the identifying assumption, these results also show that the reform does not seem to have affected the number of divorces.

Figure 5: Monthly number of divorces



Note. Monthly number of divorces among parents with at least one child aged 10 or younger between September 2007 and August 2010. The central vertical line indicates March 2009, the time of the reform. The other two vertical lines indicate the pre- and post-reform horizons that define the control and treatment groups.

Parallel trends The assumption of parallel trends is supported by the lack of pre-trends, as shown in the event studies in Section 6.

6 Results

I start by showing that the reform did not meaningfully affect the extensive margin of labour supply. Mothers and fathers divorcing after the reform are no more or less likely to be employed than those who divorced before. Next, I test the predictions regarding wages and hours made in Propositions 1 and 3. I show that, as predicted, treated mothers experience a reduction in wages relative to those in the control group. The probable mechanism is reduced geographical mobility: Mothers divorcing after the reform are less likely to move to employers further away, but no less likely to change employers overall. They stay closer to their ex-partners, which is consistent with co-parenting arrangements requiring both parents to live close to each other. There is no clear increase in hours worked for mothers with young children after the reform.

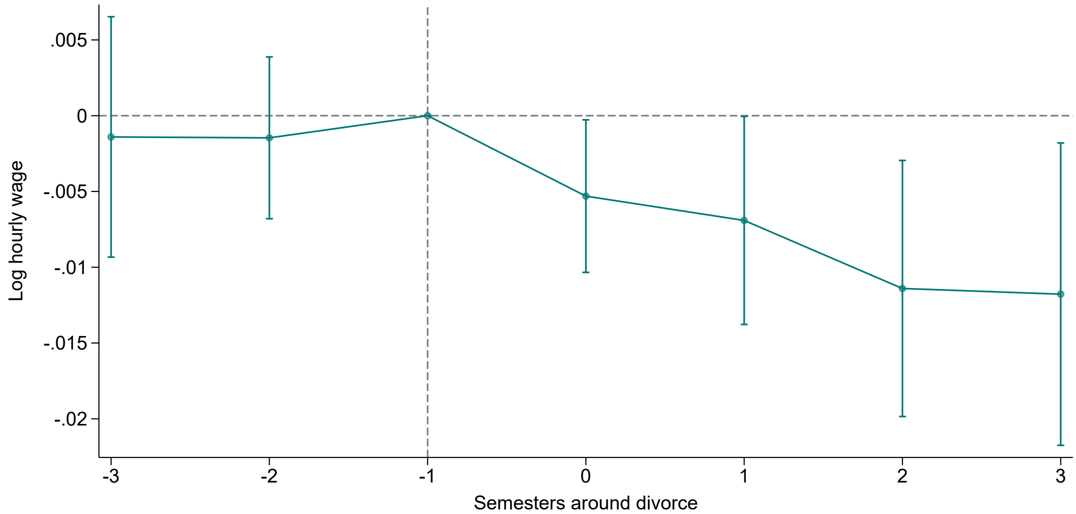
6.1 Labour market outcomes

Employment Comparing mothers divorcing before and after the reform (specification in Equation 1), I find no significant differential effects of the reform on employment outcomes after divorce (Figure B1). Employment in the treated group is minimally lower, but not significantly so. In both the treatment and control group, the probability of being employed rises by 3 percentage points between one year before and one year after divorce (Figure B6). Some of this increase occurs in anticipation of the divorce. Overall, the change in employment status around divorce is relatively small – most mothers had already been employed before the divorce, reflecting high maternal labour force participation in the Netherlands – and, importantly, it does not differ significantly between treatment and control groups.

No pre-trends Figures 6 and 7 plot the estimated interaction coefficients $\hat{\gamma}_j$ of the main specification for monthly hours worked and hourly wages (both in logs), testing Propositions 1 and 3. The pre-divorce difference between those divorcing before and after the reform is very close to zero and insignificant, supporting the identifying assumption of parallel pre-trends. An F-test for joint significance of the pre-divorce coefficients is also insignificant (p-value = 0.32 for the wage result).

Hourly wages The key result is shown in Figure 6. Hourly wages decline for treated mothers, and these decreases are driven by wage gains in the control group, and a flat wage profile in the treatment group (Figure B4). Wages steadily decrease after divorce, from about 0.7% after the first year to 1.2% after two years. This result supports the theoretical prediction stated in Proposition 1. There is no effect on fathers' wages (Figure B10, Table A6). This, too, is in line with theoretical predictions (Corollary 1). The simple pre-post effects of the reform on labour market outcomes for mothers and fathers are summarised in Tables A5 and A6.

Figure 6: Hourly wages decrease persistently



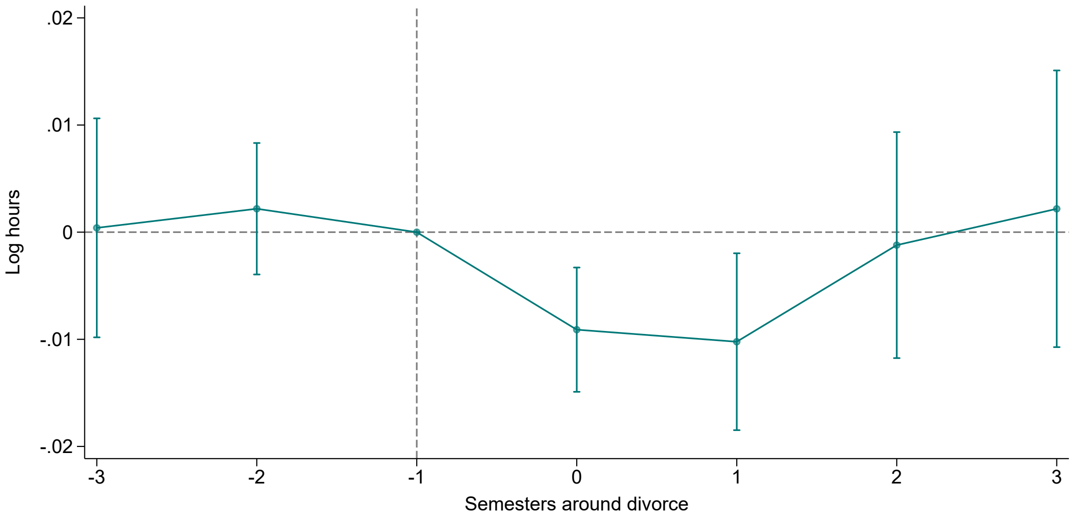
Note. This figure shows event study estimates of the effect of divorcing after the reform on mothers' hourly log wages. The sample includes mothers divorcing between September 2007 and August 2010, and the coefficients represent interactions between post-divorce semester dummies and a treatment indicator for those divorcing after the joint custody reform. The omitted category is the semester immediately before divorce (semester -1). Each point estimate reflects the difference in log wages between treated and control individuals in a given semester relative to this reference period. The specification includes month-year×region and individual fixed effects. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

The observed wage decreases in the treatment group are not driven by business cycle factors: In the regressions, I use specifications controlling for month-year×region fixed effects as well as month-year×region×sector fixed effects, which capture possible regional variation in labour market opportunities over time and sectors. In Table A8, I additionally control for quarterly unemployment rates at the regional level, interacted with a linear time trend, and find an almost identical wage decrease as in the main specification.

The negative wage effect is indeed driven by those with young children. When regressing wages on the event time using the sample of mothers whose youngest child is older than 10 at the time of the divorce, the effect on wages is zero (Figure C7, Table A7). This result affirms that the observed wage decrease is due to the increase in co-parenting, as parents of teenagers rarely co-parent.

Hours Mothers' hours worked did not increase significantly (Figure 7, Table A5).[24] This is in contrast to the idea that a relaxation of the time constraint due to a shift from largely sole physical custody by mothers to shared parenting outweighs the increased commuting time and negative substitution effect from lower wages. It is also in line with Proposition 3. In addition to negative substitution effects, income effects may also contribute: Expenses on children generally exceed what is covered by child support payments under sole physical custody. Such an income effect may further dampen the effect of a relaxation of the time constraint on hours. Finally, as laid out in Section 2.1, the availability and affordability of childcare do not appear to have been dramatic hindrances. Hence, the broadly null result on hours may be different in settings in which the lack of availability of quality childcare is a greater concern. The temporary dip in Figure 7 is due to a larger increase in hours in the control group, largely coming from overtime work.

Figure 7: There is no persistent change in mothers' hours worked



Note. This figure shows event study estimates of the effect of divorcing after the reform on mothers' monthly log hours. The sample includes mothers with young children divorcing between September 2007 and August 2010, and the coefficients represent interactions between post-divorce semester dummies and a treatment indicator for those divorcing after the joint custody reform. The omitted category is the semester immediately before divorce (semester -1). Each point estimate reflects the difference in log hours between treated and control individuals in a given semester relative to this reference period. The specification includes month-year × region and individual fixed effects. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

Finally, the NFN data provide suggestive evidence that those women who commit to co-parenting consume more leisure and report less time pressure (Figure C10).[25] Un-

[24] When conditioning on the sample of continuously employed mothers, hours do increase by about 1%, with the increase concentrated in the last few semesters.

[25] Figure C10 shows the coefficient on co-parenting interacted with a gender dummy for different outcomes related to leisure time based on the following regression:

$$y_i = \beta_0 + \beta_1 \text{ co-parenting}_i + \beta_2 \text{ woman}_i + \beta_3 \text{ co-parenting}_i \cdot \text{woman}_i + \varepsilon_i$$

fortunately, the time use and leisure questions were not asked to individuals divorcing before the reform in the NFN. Therefore, the results should not be interpreted causally, but should instead be viewed as suggestive evidence. They could equally be explained by mothers with more leisure time selecting into co-parenting. Yet, they are consistent with being able to better concentrate on work during "off-duty" days and dedicate quality time to the child during "on-duty" days, whereas sole parents must juggle work and childcare contemporaneously.

Fathers' hours worked decrease slightly compared to before the reform, in line with the fact that those in co-parenting arrangements need to spend more time caring for their children (Figure B9, Table A6). Income effects from possibly spending more on their children may partly explain why the effects are small. Moreover, spousal alimony is not affected by the parenting arrangement, even if fathers wanted to work fewer hours (and thus earn less): Dutch law stipulates that the amount of alimony is not reduced if individuals decide to reduce their income voluntarily, e.g., by working fewer hours.

Earnings Treated mothers experience a significant earnings decline of about 1% for the first year after divorce, which diminishes in later semesters. Fathers' earnings are not affected by the reform (Figure B11). The observed earnings decline for mothers is driven by both slightly lower hours in the immediate semester after divorce, mostly by the persistent wage decreases. It is important to note that mothers in both the treatment and control group work more hours and have higher earnings after divorce. However, the increases are smaller for those treated by the reform. The earnings decline is thus driven by larger earnings increases in the control group, rather than actual decreases of those in the treatment group (Figure B7).

Heterogeneity Survey results have shown that families with more highly-educated mothers are more likely to choose joint physical custody. This suggests stronger effects when restricting the sample to more highly-educated mothers.[26] They, in particular if they were secondary earners during their marriage, may also be able to realise greater wage gains when moving to employers outside of their region. For this reason, I also test if the wage effect is more strongly negative when considering specifically more highly-educated mothers who were the secondary earners during their marriage.[27] The secondary-earner component is potentially very important: In couples in which women are the main earner during marriage, the couple has likely optimised the location choice

[26]High education is defined as having any university degree or higher vocational training, which in the Netherlands also covers professions that require a university degree in other countries, e.g. architecture.

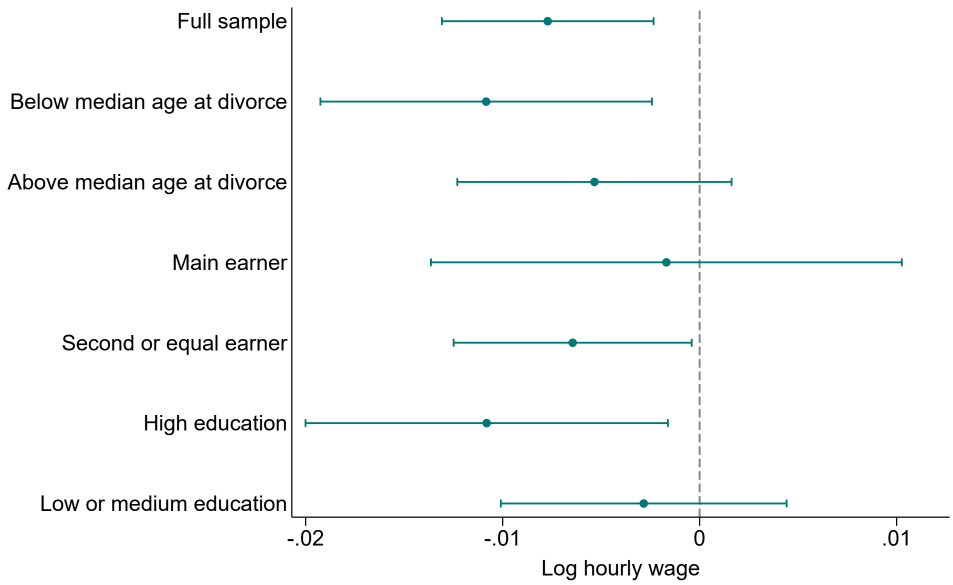
[27]More precisely, towards the end of their marriage: My measure of earnings share is based on the 12 to 18 months before divorce (excluding the year right before divorce to avoid capturing possible anticipation).

for her job, meaning that the scope for earning a higher wage by moving elsewhere is smaller than if the couple chose the optimal location for the (ex-)husband's job.

Indeed, the wage effect is larger for mothers who were secondary earners during their marriage and have above-median earnings potential: These are the women who would have seen the largest wage gains from moving to a job that is a better match, thereby realising their earnings potential. Figure 8 shows the coefficient on log hourly wages for different subsamples. Compared to the full sample, those who were secondary earners during marriage and those with high education see larger wage decreases from the reform. These are plausibly the mothers who could have earned higher wages by moving.

If the effect of lower wages for the women divorcing after the 2009 reform is mediated by lower geographical mobility, we can expect the negative wage effects to be driven by those women who did not move to other regions of the country. Indeed, I find that the negative effect on wages is larger for those mothers who stay in the same region as their ex-partner (Figure C8). Of course, this is only suggestive as the specification conditions on the outcome of moving.

Figure 8: The wage effect is driven by below median age, highly educated, and secondary-earner mothers



Note. This figure shows regression coefficients of a DiD regression of wages on the interaction between a post-reform and a treatment group indicator for different subgroups of mothers with young children. High education refers to university education or high vocational training. The median age at divorce of mothers in the sample is 38. Main earners are defined as earning at least 60% of household income. The sample includes mothers divorcing between September 2007 and August 2010. The specification includes month-year×region and individual fixed effects. Horizontal bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

If secondary earners in general – both mothers and fathers – are those negatively affected by the reform, the effect will show up in the gendered pattern documented because of the much higher share of secondary earners among mother than fathers (72% vs. 15%, see Table 2). Fathers who are secondary earners also experience lower wages after the reform, but the sample is too small to detect statistically significant effects.

Finally, it seems that the inability to relocate freely affects mothers' wages most negatively during the years during which wage growth is generally steepest: The estimated effects on wages are twice as large for mothers below the median age at divorce, which is 38 in my sample, than those above (Figure 8).

Robustness To address the potential concern that I cannot discern the exact date of filing for divorce, I run the same specification using a doughnut-hole design, leaving out the two months immediately before and after the reform date (i.e., omitting those individuals who divorce between January and April 2009). This doughnut design yields results that are slightly larger in magnitude but still very similar to those in my main specification (Table A8). Separating the treatment and control groups further in time also means that there is slightly less overlap in time to estimate the calendar month-year fixed effects. Therefore, the earliest and latest time fixed effects that could previously be identified based on observations of individuals from both the treatment and the control group are only identified from variation within either the control group (for the earliest) or the treatment group (for the latest).

I include all parents with young children in my main specification for hours and wages for the periods during which they are employed. This may raise concerns that the effects are driven by compositional changes. Yet, conditioning on the subsample of mothers who are continuously employed both before and after divorce does not change the results (Figure B12). The downside of this specification is that it excludes more than half of the number of observations.

The cut-off age of 10 years or younger was chosen to include likely compliers based on the NFN data. However, the results do not hinge on this specific age cut-off. Computing the main wage effect for each possible cut-off age (Figure C9) shows that the effects are highest for mothers of younger primary-school-age children and attenuate as the cut-off increasingly includes older children, but do not depend on the specific age cut-off chosen.

An alternative measure of the relative effects on wages by gender is to look at the wage difference between former spouses. This within-couple specification shows that the wages of mothers who were secondary earners are more negatively affected than those of fathers – mothers' wages relative to those of fathers decrease by 1.1-1.4 percentage points. The estimate is less precise than that of the main specification because of additional fixed effect for (ex-)spouse and the month×region of their job, but the results remain significant at the 10% level.

The month-year fixed effects should capture any labour market impacts that affect individuals observed in the same month. To additionally verify that the results are not driven by any remaining cross-sectional correlation among individuals observed in the same month – for example, shocks from nationwide macro news that are imperfectly absorbed by the month-year×region fixed effects – I also report a specification that clusters on both individual and month-year. This two-way clustering is conservative: It nests the individual-level correlation structure while additionally permitting arbitrary correlation across individuals that share the same month. The standard errors increase slightly given the relatively short panel, but remain highly statistically significant.

The fixed-effects specifications imply that the only potential threat to identification may arise from treated and control groups being differentially affected within month×region (×sector). To rule out that divorcing with children is worse for wages per se after March 2009, I look at mothers whose youngest child is between 11 and 17, which makes co-parenting very unlikely. Using these mothers as a placebo group, I find a very small and insignificant (positive) effect of the reform on these mothers' wages (Table A7). This result lends additional support to the claim that the observed effects are attributable to the reform.

6.2 Mechanism: Limited geographical mobility

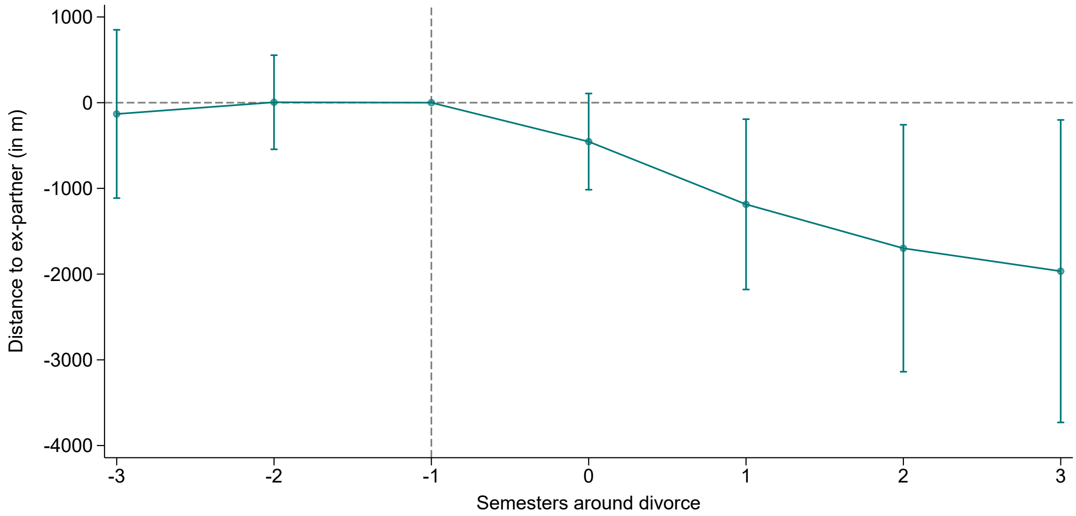
Below, I provide evidence supporting the idea that the lack of post-divorce wage growth in the treated group is driven by limited geographical mobility. I show that the reform reduced the share of moves to municipalities that are different from the ex-spouses' by 1.5 percentage points, and that the average distance between ex-spouses shrinks by over one kilometre (13 kilometres for the compliers) two years post-divorce.[28] This reduced geographic mobility is the likely mechanism behind the relative wage decreases of mothers who were affected by the reform.

Using data on employer location, Figure 9 shows the results of the event study specification with distance to ex-partner as an outcome. It shows that parents of young children divorcing after the reform stay working in greater proximity than before the reform (Figure 9). The effect is persistent and increases over time, consistent with gradual moves to better-paying jobs by mothers in the control group. It is also sizeable: Treated parents work one kilometre closer to each other than before the reform, out of a mean of 27.5km distance. In this sample, I exclude firms with multiple plants, for whom location information is based only on the head office and can thus be imprecise. Estimating the wage effect on the subsample of single-establishment firms yields the same estimate for the wage effect (Table A8, column 4).

Another measure of proximity between former partners is an indicator for employment in the same region or municipality. After the reform, ex-partners who are parents become

[28]The mean of individuals employed in the same municipality as their partner after divorce is 52.9% and the average post-divorce distance 27.5 kilometres.

Figure 9: Distance between ex-spouses' workplaces



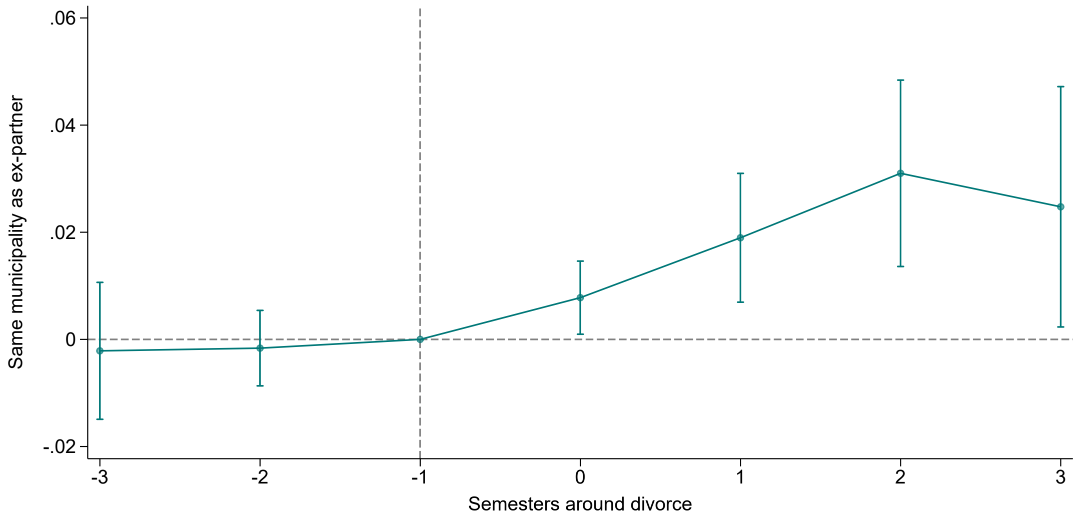
Note. This figure shows event study estimates of the effect of divorcing after the reform on the distance between workplaces of former spouses with young children. The sample includes individuals divorcing between September 2007 and August 2010 who are employed at single-establishment firms, and the coefficients represent interactions between post-divorce semester dummies and a treatment indicator for those divorcing after the joint custody reform. The omitted category is the semester immediately before divorce (semester -1). Each point estimate reflects the difference in the distance (in metres) between ex-spouses' workplaces for treated versus control individuals in a given semester, relative to this reference period. The specification includes month-year×region and individual fixed effects. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

more likely to have employers in the same municipality (Figure 10), in the same region (Table A9, column 3), and even in the same firm. These results are consistent with limited mobility towards better labour market opportunities as the mechanism behind a lack of wage growth for the treated mothers, and the finding that the negative wage effect is driven by those who do not move to employers outside of their previous region (Figure C8).

Do mothers who are further apart from their ex-partner after divorce see larger wage increases? While I cannot answer this causally, descriptive evidence points in this direction. Figure 11 shows mothers' log wages by 10-kilometre bins of post-divorce distance between ex-spouses workplaces. Mothers' wages are increasing in distance from their ex-partner up until 30-40km distance (there are relatively few mothers at distances above 50km from their ex-partner). The wage difference when moving from the <10km bin to the 30-40km one is about 9%. The relationship between distance and log wage does not change before or after the reform, so the observed wage decrease for treated mothers likely arises through fewer distance moves after the reform, as shown in Figure B5.

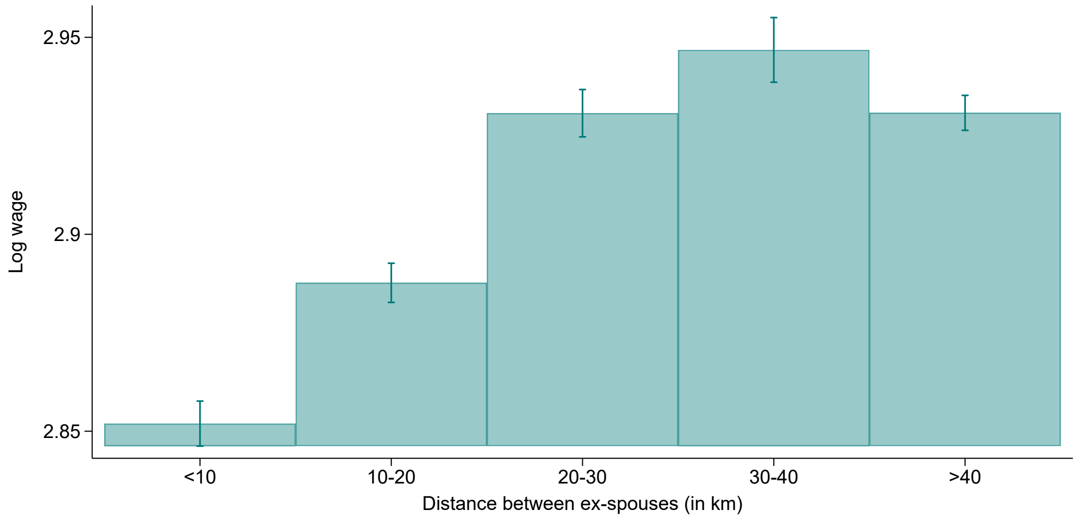
Given that the reform coincided with the Great Recession, one concern might be that decreased mobility could be driven by changes to house prices. To address this concern, I rely on the region×month-year fixed effects. In addition, I also use a specification controlling for yearly municipal house sale prices interacted with a linear trend, and find

Figure 10: Probability of being employed in the same municipality as ex-spouse



Note. This figure shows the probability of working in the same municipality as the former spouse. The sample includes individuals with young children divorcing between September 2007 and August 2010 who are employed at single-establishment firms. The bar heights represent interactions between post-divorce semester dummies and a treatment indicator for those divorcing after the joint custody reform, relative to the semester before divorce. The specification includes month-year and individual fixed effects.

Figure 11: Mothers' log wages by distance from ex-partner



Note. This figure shows mothers' post-divorce log wages by distance from ex-partners' workplaces in bins of 10km.

no change to my main results.

6.3 Ruling out alternative explanations

Income effects through alimony The reform increases fathers' bargaining power, which may be a concern for my identification strategy if they use this to negotiate lower spousal alimony payments, thereby differentially affecting mothers' incentives in the treatment and control groups. I do not observe alimony payments in the administrative data,[29] but can observe whether alimony payments are made and to whom in the NFN sample. Using that, I document that co-parenting status is uncorrelated with the probability of paying or receiving spousal alimony (Table A10), which does not suggest that fathers' newfound bargaining power translates into changes to alimony payments. Moreover, the overall share of couples in which alimony payments are made is relatively low, at less than 20%. Most important to ruling out differential income effects due to changes in alimony payments is the expected direction of income effects: A reduction in alimony would not be able to explain the pattern that wages and earnings *decrease* relative to the pre-reform divorcees. Instead, if alimony was reduced, income effects suggest that mothers should increase their earnings.

Lower productivity If co-parenting lowers mothers' productivity, this may be reflected in lower hourly wages. It seems highly unlikely that the time needed for co-ordination with the other parent more than outweighs the time that would have been spent on weekly childcare under sole physical custody. Co-parenting is not associated with increased time pressure that could make mothers less productive and thereby result in lower wages: On the contrary, leisure and time pressure results from NFN show *less* time pressure for co-parenting mothers (Figure C10).

Search Effort/Intensity An alternative channel is a reduction in search intensity because of an increased cognitive load of co-parenting. If co-parenting mothers search less intensively instead of having a geographically restricted acceptance set, one may expect similar negative effects on wages. While mothers search less intensively in general (Bang and Wang, 2025), shared childcare should lighten the childcare burden and free up additional time for job search among parents with joint physical custody. However, lower search effort is unlikely to explain my results, as there is no differential effect on job separation rates comparing pre-/post-reform – the only difference lies in where they move. Finally, one would expect symmetric effects for mothers and fathers, which I do not find, as demonstrated in Figures 6 and B9.[30]

6.4 LATE: Scaling wage effect for compliers

The ITT estimate of a 0.8% wage decrease is relatively small. However, the share of compliers among parents with children aged up to ten is estimated to be 7.6 percentage points, so the likely effect for compliers is an order of magnitude larger. Therefore, I

[29]CBS data contain information on alimony payments, but only from 2011.

[30]Similarly, if having to frequently interact with the ex-partner causes psychological stress that negatively affects productivity, the expected effects would be the same for mothers and fathers.

also provide estimates for the local average treatment effect for the compliers (LATE) by scaling the estimated ITT by the share of compliers, both for my main sample of parents with children ten and younger and the full sample of divorcing parents with minor children.

Using the NFN dataset that details post-divorce parenting arrangements, I estimate the share of compliers, i.e., the share of parents who chose co-parenting after the reform but would not have chosen it before. I regress the co-parenting status on treatment status. The results show a 6.7 percentage point increase among all parents of minor children, and a 7.6 percentage point increase among those parents whose youngest child is aged ten or younger (Table A1).

I estimate the LATE by scaling the estimated ITT by the estimated share of compliers:

$$\widehat{\text{LATE}} = \frac{\widehat{\text{ITT}}}{\widehat{\text{share of compliers}}} = \frac{0.00771}{0.076} \approx 0.101 \tag{2}$$

Table 3: Estimated local average treatment effects of the reform for compliers

	Wage change	CI	
	relative to control	Delta method	Fieller's theorem
Youngest child under 10	-10.1%	[-0.1984, -0.0035]	[-0.3398, -0.0285]
Any minor children	-6.4%	[-0.1384, 0.0095]	[-0.2153, -0.0030]

Note. LATE estimates, combining estimation results of the effect of the reform on wages in Tables A5 with the share of compliers in Table A1.

The estimated LATE is large: It suggests a 10% reduction in hourly wages for mothers who take up co-parenting due to the reform (Equation 2, Table 3). If considering not only those parents with children 10 and younger, but those with any minor children, the LATE decreases but still implies an economically meaningful 6.4% wage decrease. Given that co-parenting is much less likely for teenagers, the effect on parents with any minor children is still driven by those with younger children, among whom the compliers are concentrated.

LATE assumptions One potential issue with the LATE estimates is that there could be compositional changes. The presence of defiers – those parents who would have chosen co-parenting before the reform but decide against it after –, and thereby a violation of the monotonicity assumption, is unlikely. The balance checks described in Section 3.1 also do not suggest changes in the composition of the sample. Nonetheless, it is important to keep in mind that the compliers are specific types of parents: those with higher education who already shared market hours more equally than the average couples before divorce.[31]

[31] Bakker and Mulder (2013) also find that the couples who select into co-parenting are positively selected on earnings.

The effect may be different if co-parenting was assigned randomly, and is likely to be lower for individuals for whom wages depend less on good matches between their skills and the ones required for their employment.

6.5 Commute

One margin of labour market adaptation to not being able to relocate further away due to co-parenting could be to commute for longer. In the Netherlands, good public transport infrastructure makes it relatively convenient to commute over larger distances. However, even moderately long commutes may not be feasible for parents of young children, given the constraint to only leave for work after the start of school. To better understand if there is some adjustment on the commuting margin for co-parents, I use the NFN data. I compare the commuting duration of mothers and fathers by co-parenting status (Table 4). The regression displays the familiar result that women commute less than men (Le Barbanchon et al., 2021). Men who co-parent have a slightly shorter commute than those who do not. The interaction between co-parenting and women is large relative to the mean dependent variable and significant: Co-parenting mothers commute more. These results are consistent with mothers trying to compensate being location-bound with longer commutes, as predicted by Proposition 2. Nonetheless, based only on these data, I cannot rule out the alternative interpretation that only women with greater willingness to commute enter into co-parenting arrangements.

Table 4: Commuting duration by co-parenting status and gender

	Commuting time (in min)
Co-parenting	-3.816
	(1.277)
Woman	-11.23
	(1.126)
Co-parenting × Woman	3.982
	(1.745)
Mean dep. var.	24.68
Observations	2070

Note. The table shows coefficients for the regression of commuting duration (in minutes) on co-parenting status, controlling for five categories of educational attainment. The sample is that of parents who have been separated for up to three years. Data: NFN.

6.6 Re-partnering outcomes

A change in the presence of children in the household could also affect parents' re-partnering probabilities, whether in new cohabiting or married unions. Below, I investigate this descriptively using those parents who selected into co-parenting. As the time since separation matters strongly for the probability of living with a new partner, and respondents in the NFN were sampled at different times relative to their divorce, I control for the years since separation in the regression.

Table 5: Probability of living with a new partner by co-parenting status

	Women (1)	Men (2)
Co-parenting	-0.0153	-0.0926
	(0.0230)	(0.0233)
Years since separation	0.0320	0.0326
	(0.0076)	(0.0077)
Mean dep. var	0.275	0.341
Observations	3374	3369

Note. The table shows coefficients from regressions of the re-partnering probability on co-parenting status and parent gender, controlling for years since separation. Data: NFN.

There are no large negative effects of co-parenting on the probability of re-partnering for mothers. However, fathers who co-parent are much less likely to move in with a new partner after divorce: 9 percentage points, which is more than a third lower than the re-partnering probability of fathers who are not co-parents (Table 5). Bakker and Mulder (2013) reach the same conclusion drawing on 35 in-depth qualitative interviews. On average, mothers are 6.6 percentage points less likely to live with a new partner than fathers. However, when co-parenting, both parents are equally (un)likely to live with a new partner after divorce. While co-parenting mothers appear disadvantaged relative to fathers in the labour market, when it comes to the dating market for cohabiting relationships, both are equally affected by the presence of children.

7 Discussion

Should co-parenting become even more widely adopted as a post-divorce parenting arrangement? As an expansion would cover parents who currently do not choose co-parenting, and are thus different from the compliers for whom effects are estimated, this question does not have a straightforward answer. On the side of benefits to children's well-being, the quality of parental time matters for child development (Chan and

Liu, 2025). Part of the positive developmental outcomes for children in co-parenting arrangements likely stems from the fact that the parents – including fathers – who choose it are positively selected. Therefore, extending co-parenting arrangements to the full population of divorced parents is likely to entail lower benefits for children. At the same time, the parents selecting into co-parenting are those that have the highest expected gains (in child welfare, time, etc.) net of costs. Thus, mandatory co-parenting may result in worse labour market outcomes for those parents who currently opt out of this arrangement, while yielding lower benefits to children than the ones previously estimated.

There are already small financial incentives for parents to adopt co-parenting. They include extending child tax credits and housing benefits to both co-parents.[32] Yet the fact that joint physical custody comes at a labour market cost to women may limit its uptake. At the same time, it also requires a certain income to be able to afford a bedroom for children in each parent's home etc., suggesting that negative labour market outcomes due to co-parenting will not hit the financially most constrained families. As governments worldwide promote co-parenting arrangements, alternative policy considerations to alleviate geographical labour market constraints could lie in facilitating options to work remotely, but it remains to be determined whether remote working may have adverse career effects itself.

Effectively, the child custody reform trades off child welfare considerations with mothers' career opportunities. Policy could consider spousal compensation for lower earnings in the joint location. Such transfers could function akin to alimony, and are similar to those envisioned by Reynoso (2019) to compensate the partner who specialised in home production for not being able to build human capital that is valued in the labour market. From an aggregate-economy perspective, joint physical custody limits the efficient spatial allocation of workers across the economy.

8 Conclusion

The labour market consequences of co-parenting are ex-ante ambiguous. It may alleviate time constraints for the primary caregiver, potentially expanding their capacity for market work. However, co-parenting also introduces a new constraint: Parents may face

[32] In terms of child-related government benefits and allowances, there are slight advantages to co-parenting, especially for higher earners. The child benefit is paid by the Social Insurance Bank (SVB) to one of the parents, but they may then divide the money among themselves. The child-related budget (KGB) is paid by the Tax Authorities and is paid to the parent with whom the child is registered. It is income-dependent, so it is generally considered advantageous to register the child with the lower-earning parent (Uiteen Family Law). If there are two children or more, they can be registered with different parents, so that both receive some of the child-related budget. Moreover, there is a child tax credit for parents of minor children who are in work. This tax credit is generally granted to the parent with whom the child is registered, but in the case of co-parenting, both parents can apply. Finally, rent allowance can also be claimed by both parents if they have a signed co-parenting agreement.

reduced geographical mobility, limiting career advancement opportunities that require relocation or long commutes. In this paper, I analyse the labour market outcomes of parents following the 2009 custody reform in the Netherlands, which encouraged and raised the share of divorced couples with young children who choose co-parenting.

Comparing parents who divorce before and after the reform in a difference-in-differences framework shows broadly no changes to fathers' labour market outcomes. Mothers, however, experience a decline in hourly wages of 0.8% compared to those who divorced before the reform. When scaled by the share of compliers, the magnitude of this wage effect increases to an economically meaningful -10% for mothers with young children. The wage decrease persists for the entire window of observation and grows over time. It is not driven by business cycle factors such as local unemployment conditions. I do not find strong evidence of a relaxation of time constraints for mothers that would translate into expanded market hours. These empirical findings are consistent with theoretical predictions.

The negative effect on wages is likely rooted in lower geographical mobility. Co-parenting requires both parents to live in close geographical proximity for the arrangement to function. Indeed, after the reform, ex-spouses' workplaces are closer to each other and more likely to be in the same region and same municipality. This channel also helps explain why I find negative wage effects for mothers, but not fathers: While still married, the couple will have optimised at the household level, which for many entails choosing a location based on professional opportunities for the main earner. Given that this is the husband in a majority of couples, he will on average enjoy better professional opportunities in the chosen location than his wife. Once the marriage breaks down and ex-spouses optimise individually, mothers are more likely to face better employment opportunities elsewhere and have a financial incentive to move. Yet when co-parenting, the location of marriage becomes sticky, and the mothers who, with sole physical custody of the children, might have moved for higher-wage jobs are now tied stayers in a possibly sub-optimal location in terms of employment opportunities for themselves. The mobility constraint illustrates an important trade-off between family arrangements and spatial labour market frictions.

This paper estimates the partial equilibrium effects on parents who divorce right after the reform, so for whom the couple-location decision was made before the change to custody law. If individuals have rational expectations about divorce risk and take into account that they may end up in the divorced state, location decisions at the couple-level will give less weight to locations for the jointly optimising couple. Instead, they will attach more weight to locations in which either partner has decent career opportunities, ensuring a minimum satisfactory labour market option for both partners is attainable in the chosen location to insure against the possibility of divorce. This may lead to less efficient specialisation in marriage, but also less disparate labour market outcomes in case of its dissolution.

Regarding mothers' labour market outcomes after the reform, I conclude that the location constraint outweighs the benefits from a lighter care burden. These findings do not imply that co-parenting is not in the best interest of the child(ren) of divorcing parents or should otherwise be discouraged. Prior research finds upsides for child development and well-being, including that of parents. What my analysis does show, however, is that family policies targeting improved outcomes for one demographic (here, children) may have inadvertent economic consequences for another (here, parents, specifically mothers).

My findings also carry implications for policy design, particularly in the context of emerging work arrangements. Flexibility in work location may be a powerful tool for circumventing the geographical constraints of co-parenting, allowing mothers to access better employment opportunities without compromising the post-divorce family arrangement. This insight is particularly salient in a post-pandemic world, where flexible work has become more widespread and offers a potential policy solution to the location constraint. Specifically, the ability to work remotely decouples a worker's physical location from their professional labour market, effectively freeing them from the co-location constraint.

Future research may exploit the reform or a similar setting to understand how co-parenting as a common post-divorce custody arrangement affects the labour supply and location decisions of intact couples in light of the changing outside option. There is already some very interesting work on the co-determination of marriage market and labour market outcomes through location choice (Alonzo, 2025; Foerster and Ulbricht, 2023), but these do not yet take into account post-divorce ties through shared children.

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Appendix

A Additional tables

Table A1: Increase in co-parenting

	Youngest child up to 10 (1)	All minor children (2)
Post-reform	0.076	0.067
	(0.0295)	(0.0218)
Constant	0.286	0.292
	(0.023)	(0.019)
Mean dep. var	0.345	0.327
Observations	2095	3555

Note. This table shows the coefficients of a regression of co-parenting status on a post-reform dummy. Standard errors in parentheses.

Table A2: Top-10 sector shares by gender

	Mothers — Pre-reform		Mothers — Post-reform		Fathers — Pre-reform		Fathers — Post-reform	
Rank	Sector	Share	Sector	Share	Sector	Share	Sector	Share
1	Health & social work	0.282	Health & social work	0.289	Other business activities	0.152	Other business activities	0.138
2	Other business activities	0.155	Other business activities	0.151	Wholesale trade	0.098	Wholesale trade	0.099
3	Retail trade	0.092	Retail trade	0.089	Construction	0.087	Construction	0.088
4	Education	0.079	Education	0.078	Public administration	0.080	Public administration	0.080
5	Public administration	0.077	Public administration	0.073	Land transport & pipelines	0.045	Health & social work	0.051
6	Wholesale trade	0.043	Wholesale trade	0.043	Health & social work	0.045	Land transport & pipelines	0.047
7	Financial intermediation	0.037	Financial intermediation	0.033	Financial intermediation	0.040	Financial intermediation	0.040
8	Hotels & restaurants	0.026	Hotels & restaurants	0.028	Retail trade	0.038	Retail trade	0.036
9	Recreation, culture & sport	0.017	Recreation, culture & sport	0.016	Education	0.035	Education	0.036
10	Other service activities	0.015	Construction	0.014	Computer & related activities	0.030	Computer & related activities	0.034

Note. Labels follow SBI-2003 (aligned with NACE Rev. 1.1) at the 2-digit division level. Shares are means of individual sector shares within each subgroup (pre/post, women/men), measured one year before divorce. Only the top 10 per subgroup are shown.

Table A3: NFN sample: Demographic and labour market characteristics of divorcees before and after the reform

	Pre-Reform		Post-Reform			*p*-values	
	Mean	Obs	Mean	Obs	Post-Pre	unadj.	Holm
MOTHERS							
Age at divorce	36.35	450	36.58	2324	0.22	0.285	1.000
High education	0.30	446	0.29	2304	-0.01	0.654	1.000
Worked before divorce	0.78	450	0.79	2321	0.00	0.813	1.000
Hours (weekly)	18.56	445	19.1	2304	0.53	0.269	1.000
FATHERS							
Age at divorce	39.1	450	39.65	2324	0.55	0.021	0.147
High education	0.33	442	0.34	2291	0.01	0.665	1.000
Worked before divorce	0.93	448	0.92	2322	-0.01	0.412	1.000
Hours (weekly)	38.23	446	36.5	2304	-1.73	0.001	0.006
CHILDREN							
Number of children	2.24	450	1.98	2324	-0.26	0.000	0.000
Age of youngest child	4.61	450	5.04	2324	0.44	0.000	0.001

Note. Pre- and post-reform means of demographic variables at the time of divorce and their difference. The last two columns report conventional (unadjusted) *p*-values and those adjusted for multiple hypothesis testing using the Holm correction. Employment status and hours worked refer to the year before divorce. High education refers to the share of individuals with higher vocational training or a university degree.

Table A4: Chow test for structural break in number of divorces after March 2009

	Number of divorces
Month	-7.151
	(0.667)
Constant	5172.9
	(399.3)
Chow Chi2	3.59
p-value	0.166
Mean dep. var	957.583
Observations	60

Note. Chow test for structural break in the number of divorces after March 2009 using robust standard errors. Standard errors in parentheses.

Table A5: Mothers' labour market outcomes - simple DiD

	Employment (1)	Log earnings (2)	Log hours (3)	Log wages (4)
Post	0.015	0.037	0.033	0.004
	(0.003)	(0.003)	(0.002)	(0.002)
Post × Treat	0.002	-0.014	-0.006	-0.008
	(0.004)	(0.004)	(0.003)	(0.003)
Month	✓			
Month×Region		✓	✓	✓
Observations	1,176,990	702,173	702,173	702,173
Individuals	28,279	22,140	22,140	22,140

Note. Specification includes individual and month-year (Column 1) or month-year×region (Columns 2–4) fixed effects. Estimated across 18 months before and 24 months after the reform. Standard errors are clustered at the individual level.

Table A6: Fathers' labour market outcomes - simple DiD

	Employment (1)	Log earnings (2)	Log hours (3)	Log wages (4)
Post	0.003	0.003	0.003	-0.000
	(0.002)	(0.002)	(0.001)	(0.002)
Post × Treat	-0.006	-0.003	-0.004	0.001
	(0.004)	(0.003)	(0.002)	(0.003)
Month	✓			
Month×Region		✓	✓	✓
Observations	1,086,480	770,165	770,165	770,165
Individuals	26,232	22,160	22,160	22,160

Note. Specification includes individual and month-year (Column 1) or month-year×region (Columns 2–4) fixed effects. Estimated across 18 months before and 24 months after the reform. Standard errors are clustered at the individual level.

Table A7: Wages - mothers of older children

	Young children			Older children		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treat	-0.008	-0.008	-0.006	0.001	0.000	0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Month	✓			✓		
Month×Region		✓			✓	
Month×Region×Sector			✓			✓
Observations	702,173	702,173	702,173	460,561	457,004	434,970
Individuals	22,140	22,140	22,140	13,360	13,350	13,180

Note. "Young children" refers to mothers whose youngest child is 10 years old or younger. "Older children" refers to mothers whose youngest child is aged 11-17. Standard errors are clustered at the individual level.

Table A8: Wages

	Baseline	Doughnut hole	Regional unemployment	Single-establishment firms
	(1)	(2)	(3)	(4)
Post × Treat	-0.008	-0.008	-0.007	-0.008
	(0.003)	(0.003)	(0.003)	(0.005)
Month×Region	✓	✓	✓	✓
Observations	702,173	665,261	702,173	186,623
Individuals	22,140	20,966	22,140	9,422

Note. Specifications include individual and month-year×region fixed effects. Standard errors are clustered at the individual level and shown in parentheses. Unemployment is computed at the quarter-region level using the Dutch Labour Force Survey.

Table A9: Ex-spouses move less to employers further away after the reform

	Distance (in metres) (1)	Same region (2)	Same municipality (3)
Post	823.51	-0.0079	-0.0087
	(208.37)	(0.0029)	(0.0026)
Post × Treat	-990.59	0.0092	0.0115
	(370.01)	(0.0050)	(0.0044)
Mean dep. var	26105.38	0.5437	0.2713
Observations	457168	457168	457168
Individuals	23616	23616	23616

Note. Specifications include individual and month-year fixed effects. Estimated across 18 months before and 24 months after the reform. The sample is restricted to single-establishment firms. Distance in metres. Standard errors are clustered at the individual level.

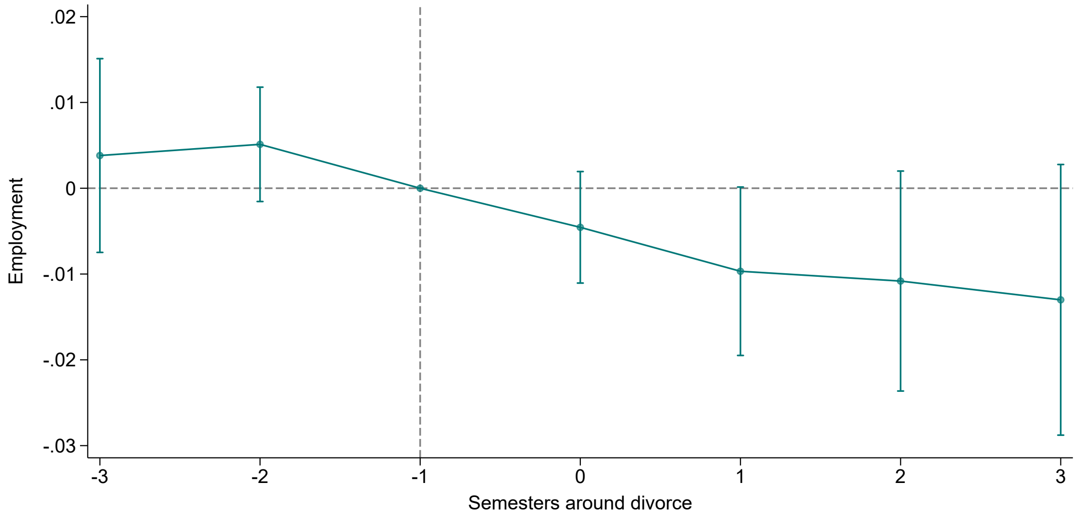
Table A10: Probability of receiving spousal alimony by co-parenting status and gender

	Alimony to ex-wife (1)	Alimony to ex-husband (2)	No alimony payments (3)
Co-parenting	-0.002	0.001	0.001
	(0.012)	(0.002)	(0.012)
Constant	0.131	0.006	0.863
	(0.007)	(0.001)	(0.007)
Mean dep. var.	0.180	0.008	0.813
Observations	5294	5294	5294

Note. The table shows coefficients from regressions of the probability of receiving alimony on co-parenting status by parents' gender. The three mean dependent variables do not add up to one perfectly because of rounding. Data: NFN.

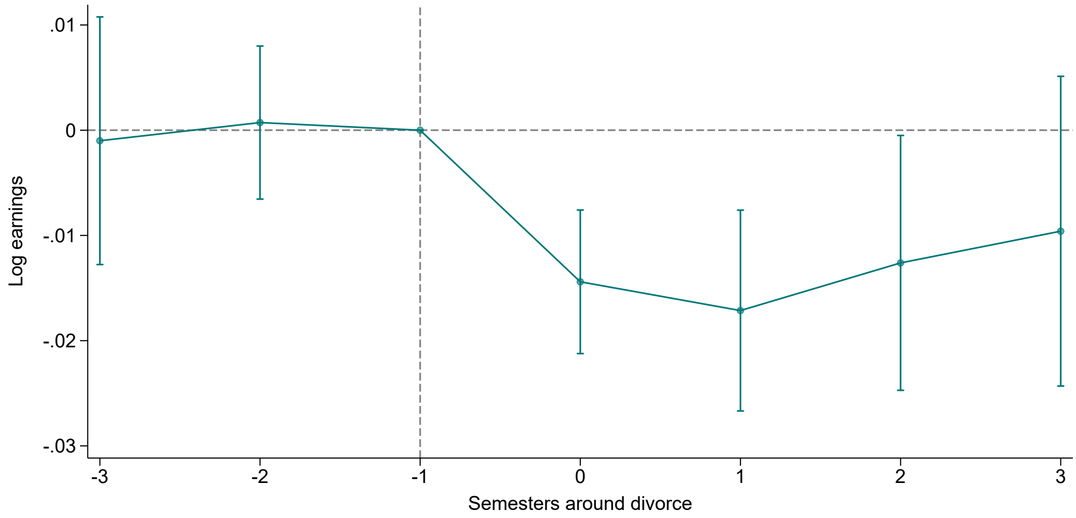
B Additional event studies

Figure B1: The reform does not positively affect mothers' post-divorce employment



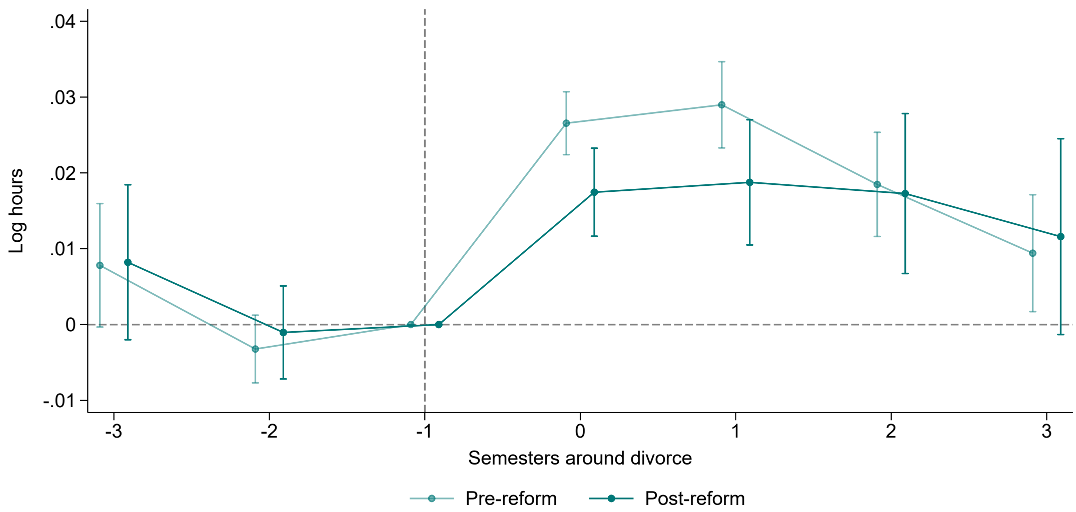
Note. This figure shows event study estimates of the effect of divorcing after the reform on mothers' probability of being employed. The sample includes mothers with young children divorcing between September 2007 and August 2010, and the coefficients represent interactions between post-divorce semester dummies and a treatment indicator for those divorcing after the joint custody reform. The omitted category is the semester immediately before divorce (semester -1). Each point estimate reflects the difference in employment rates between treated and control individuals in a given semester relative to this reference period. The specification includes month-year×region and individual fixed effects. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

Figure B2: Women's earnings decrease by about 1%



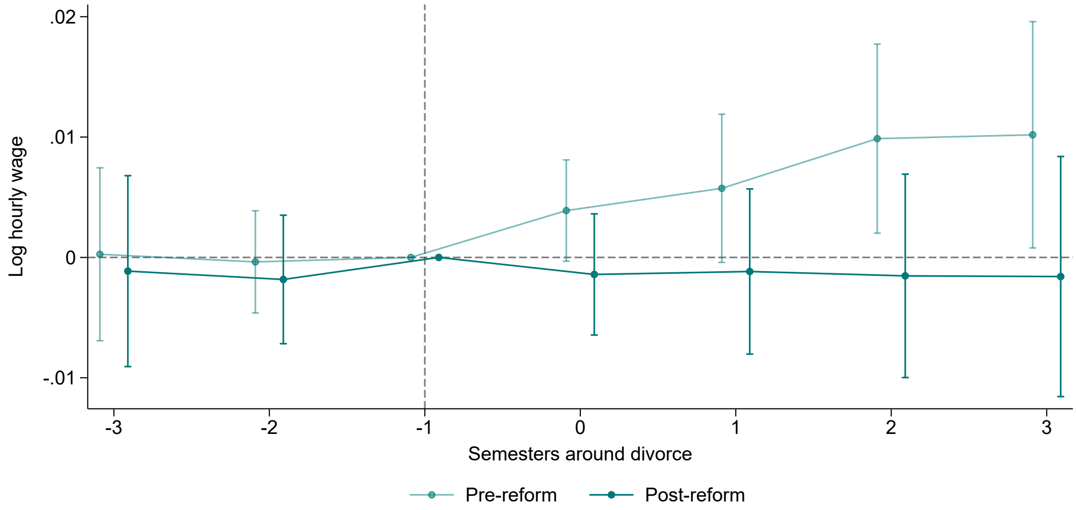
Note. This figure shows event study estimates of the effect of divorcing after the reform on mothers' monthly log earnings. The sample includes mothers with young children divorcing between September 2007 and August 2010, and the coefficients represent interactions between post-divorce semester dummies and a treatment indicator for those divorcing after the joint custody reform. The omitted category is the semester immediately before divorce (semester -1). Each point estimate reflects the difference in log earnings between treated and control individuals in a given semester relative to this reference period. The specification includes month-year×region and individual fixed effects. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

Figure B3: Mothers' hours by treatment status



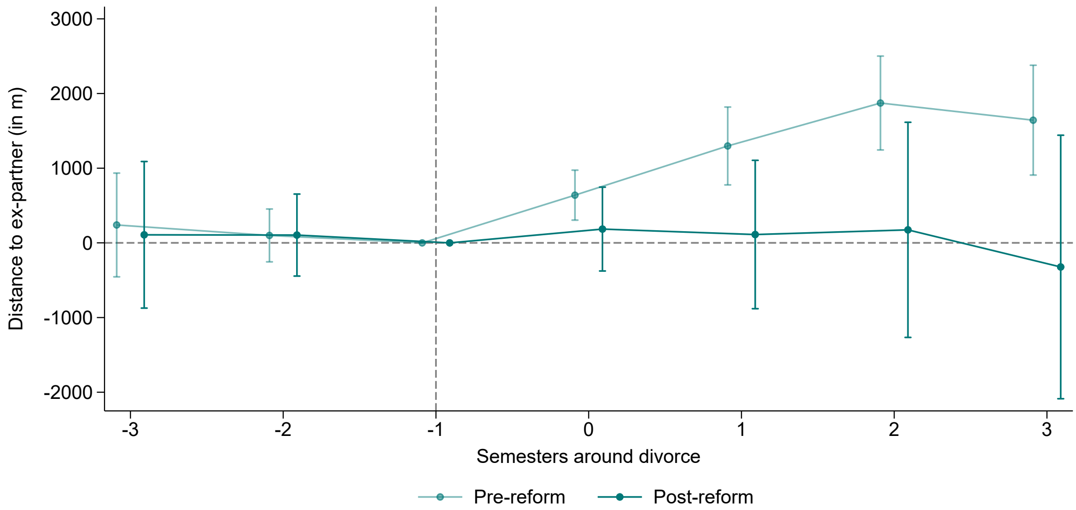
Note. This figure shows event study estimates of the changes in log hours relative to the time of divorce for mothers with young children divorcing before and after the reform. The pre-reform sample includes mothers divorcing between September 2007 and February 2009; the post-reform sample those divorcing between March 2009 and August 2010. The reference category is the semester immediately before divorce (semester -1). The specification includes month-year×region and individual fixed effects. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

Figure B4: Mothers' wages by treatment status



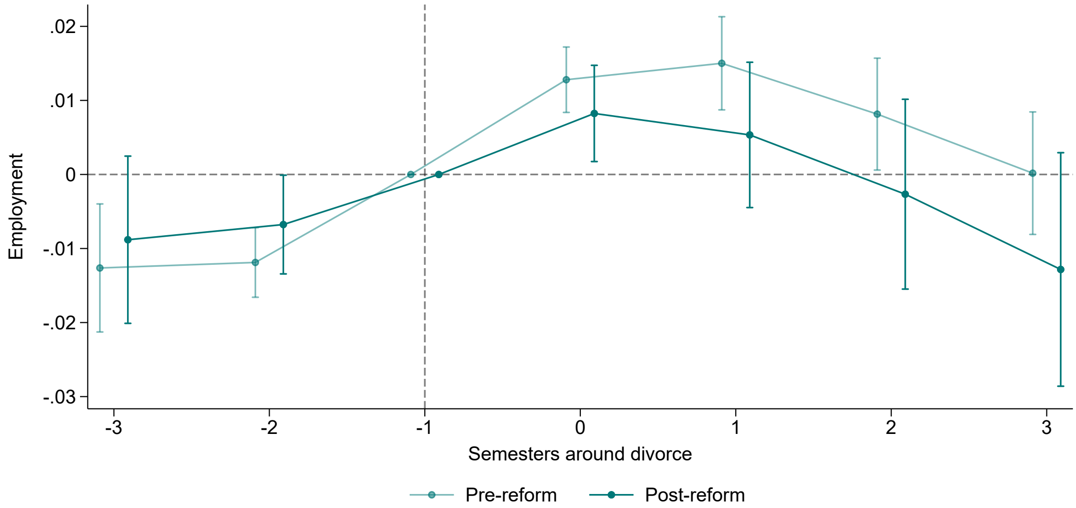
Note. This figure shows event study estimates of the changes in log wages relative to the time of divorce for mothers with young children divorcing before and after the reform. The pre-reform sample includes mothers divorcing between September 2007 and February 2009; the post-reform sample those divorcing between March 2009 and August 2010. The reference category is the semester immediately before divorce (semester -1). The specification includes month-year×region and individual fixed effects. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

Figure B5: Distance between ex-spouses' workplaces by treatment status



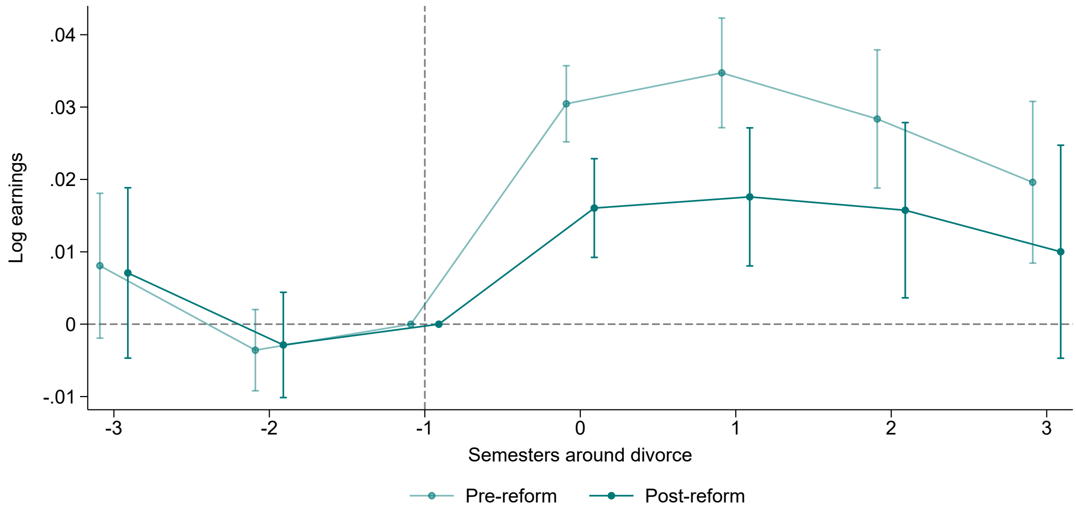
Note. This figure shows event study estimates of the changes in distance between ex-spouses' workplaces (in metres) relative to the time of divorce for parents with young children divorcing before and after the reform. The pre-reform sample includes couples divorcing between September 2007 and February 2009; the post-reform sample those divorcing between March 2009 and August 2010. The reference category is the semester immediately before divorce (semester -1). The specification includes month-year×region and individual fixed effects. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

Figure B6: Mothers' employment by treatment status



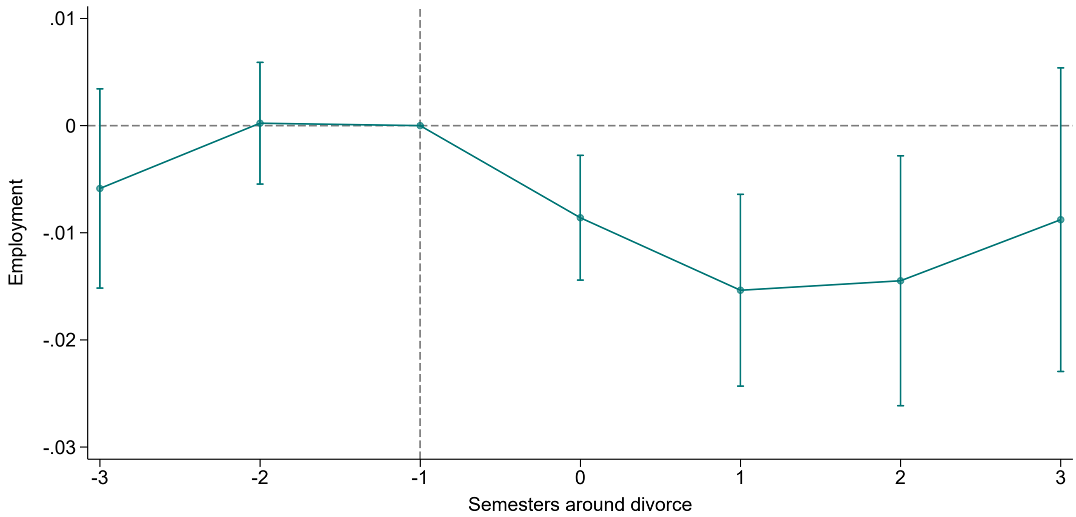
Note. This figure shows event study estimates of the changes in employment probability relative to the time of divorce for mothers with young children divorcing before and after the reform. The pre-reform sample includes mothers divorcing between September 2007 and February 2009; the post-reform sample those divorcing between March 2009 and August 2010. The reference category is the semester immediately before divorce (semester -1). The specification includes month-year×region and individual fixed effects. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

Figure B7: Mothers' earnings by treatment status



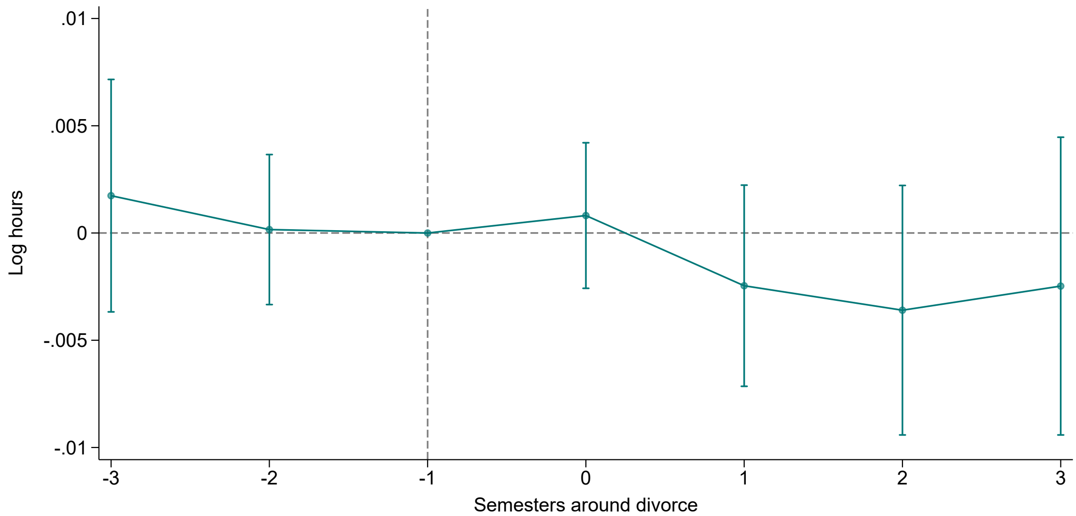
Note. This figure shows event study estimates of the changes in log earnings relative to the time of divorce for mothers with young children divorcing before and after the reform. The pre-reform sample includes mothers divorcing between September 2007 and February 2009; the post-reform sample those divorcing between March 2009 and August 2010. The reference category is the semester immediately before divorce (semester -1). The specification includes month-year×region and individual fixed effects. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

Figure B8: Fathers' employment



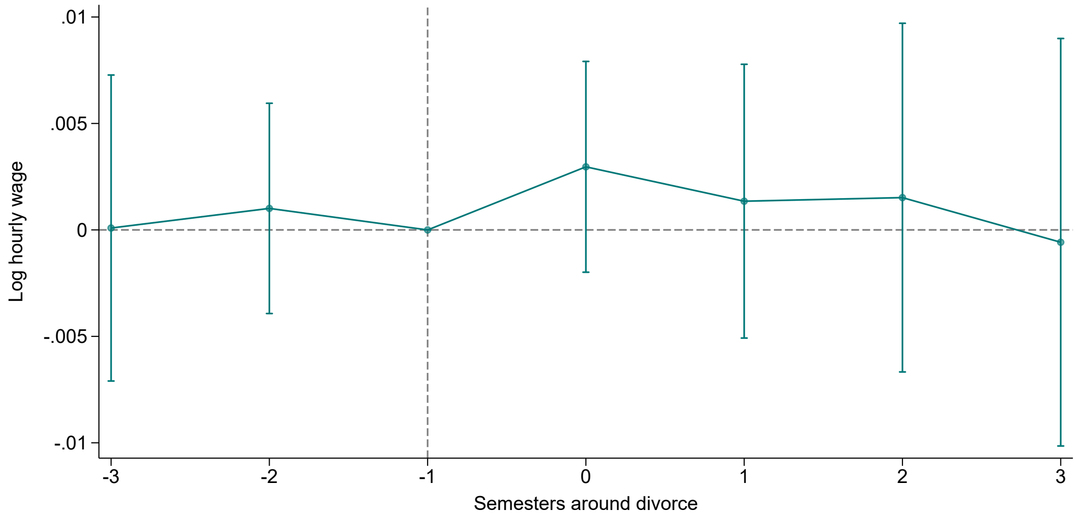
Note. This figure shows event study estimates of the effect of divorcing after the reform on fathers' probability of being employed. The sample includes fathers with young children divorcing between September 2007 and August 2010, and the coefficients represent interactions between post-divorce semester dummies and a treatment indicator for those divorcing after the joint custody reform. The omitted category is the semester immediately before divorce (semester -1). Each point estimate reflects the difference in employment rates between treated and control individuals in a given semester relative to this reference period. The specification includes month-year×region and individual fixed effects. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

Figure B9: Fathers' hours



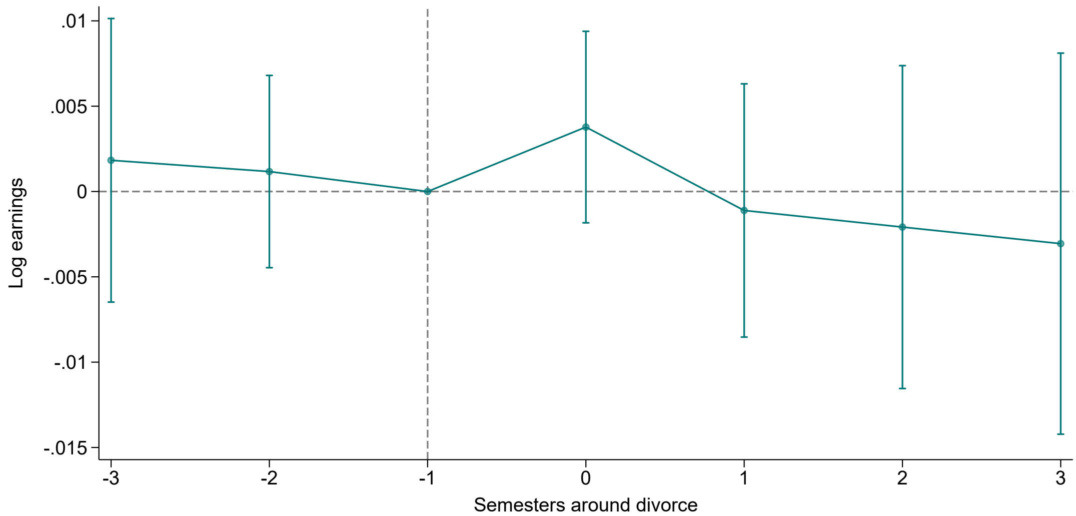
Note. This figure shows event study estimates of the effect of divorcing after the reform on fathers' monthly log hours. The sample includes fathers with young children divorcing between September 2007 and August 2010, and the coefficients represent interactions between post-divorce semester dummies and a treatment indicator for those divorcing after the joint custody reform. The omitted category is the semester immediately before divorce (semester -1). Each point estimate reflects the difference in log hours between treated and control individuals in a given semester relative to this reference period. The specification includes month-year×region and individual fixed effects. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

Figure B10: Fathers' wages



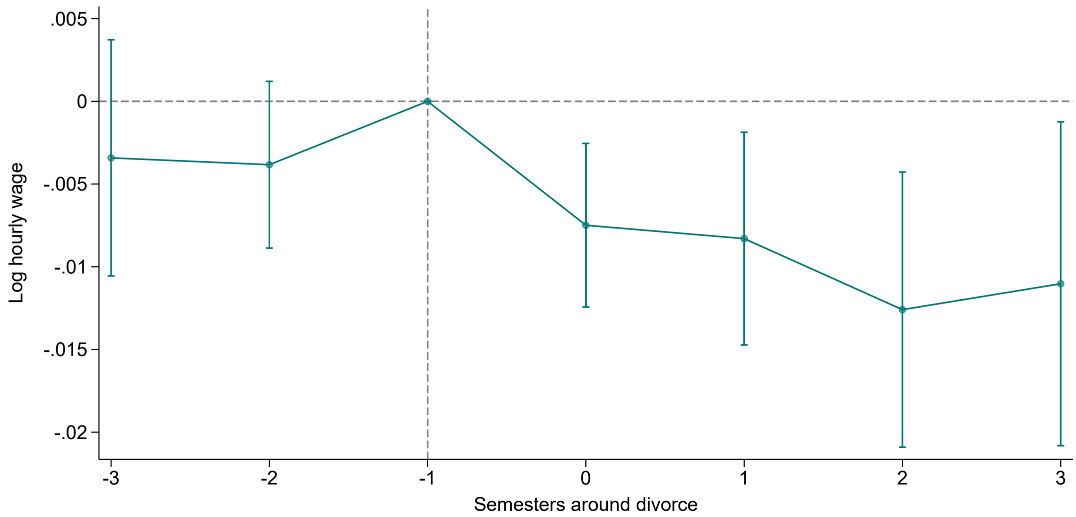
Note. This figure shows event study estimates of the effect of divorcing after the reform on fathers' hourly log wages. The sample includes fathers with young children divorcing between September 2007 and August 2010, and the coefficients represent interactions between post-divorce semester dummies and a treatment indicator for those divorcing after the joint custody reform. The omitted category is the semester immediately before divorce (semester -1). Each point estimate reflects the difference in log wages between treated and control individuals in a given semester relative to this reference period. The specification includes month-year×region and individual fixed effects. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

Figure B11: Fathers' earnings



Note. This figure shows event study estimates of the effect of divorcing after the reform on fathers' monthly log earnings. The sample includes fathers with young children divorcing between September 2007 and August 2010, and the coefficients represent interactions between post-divorce semester dummies and a treatment indicator for those divorcing after the joint custody reform. The omitted category is the semester immediately before divorce (semester -1). Each point estimate reflects the difference in log earnings between treated and control individuals in a given semester relative to this reference period. The specification includes month-year×region and individual fixed effects. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

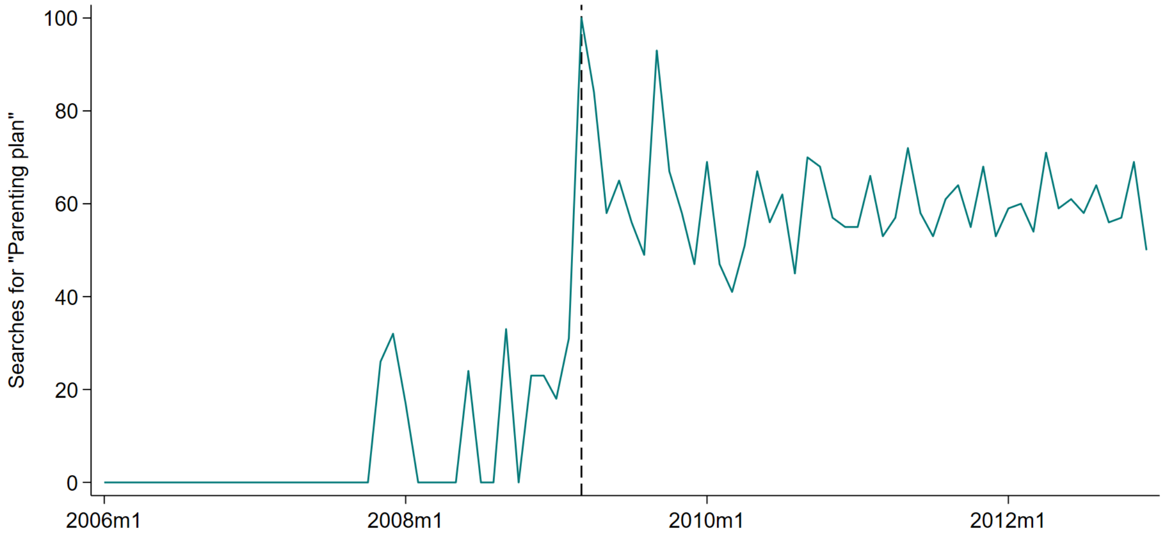
Figure B12: Continuously employed mothers



Note. This figure shows event study estimates of the effect of divorcing after the reform on mothers' hourly log wages. The sample includes mothers with young children divorcing between September 2007 and August 2010 who are continuously employed during the event time around divorce, and the coefficients represent interactions between post-divorce semester dummies and a treatment indicator for those divorcing after the joint custody reform. The omitted category is the semester immediately before divorce (semester -1). Each point estimate reflects the difference in log earnings between treated and control individuals in a given semester relative to this reference period. The specification includes month-year×region and individual fixed effects. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

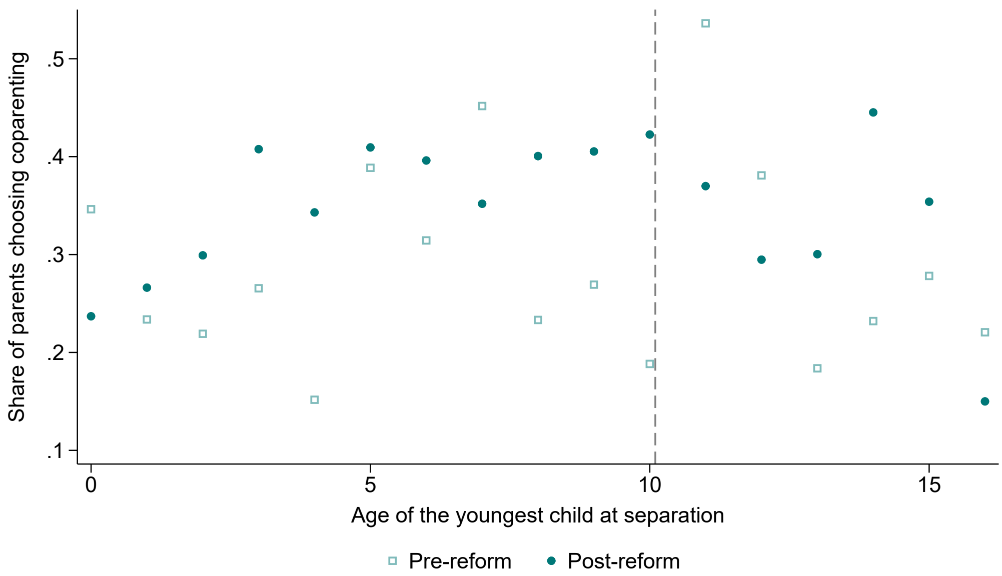
C Additional figures

Figure C1: Searches for “parenting plan” (*ouderschapsplan*) spike in March 2009 and remain elevated thereafter.



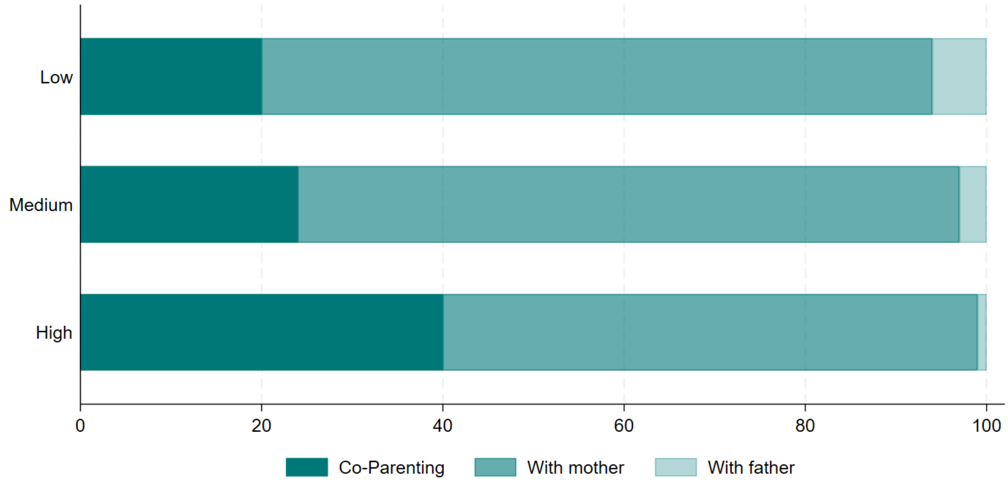
Note. Google trends data for the Dutch term for “parenting plan” between 2006 and 2012. Google trends data are reported as a value relative to the highest search volume, which receives a value of 100 (here, in March 2009). (Data: Google Trends, Netherlands; search term ’ouderschapsplan’).

Figure C2: Compliers are mostly parents with children aged up to 10



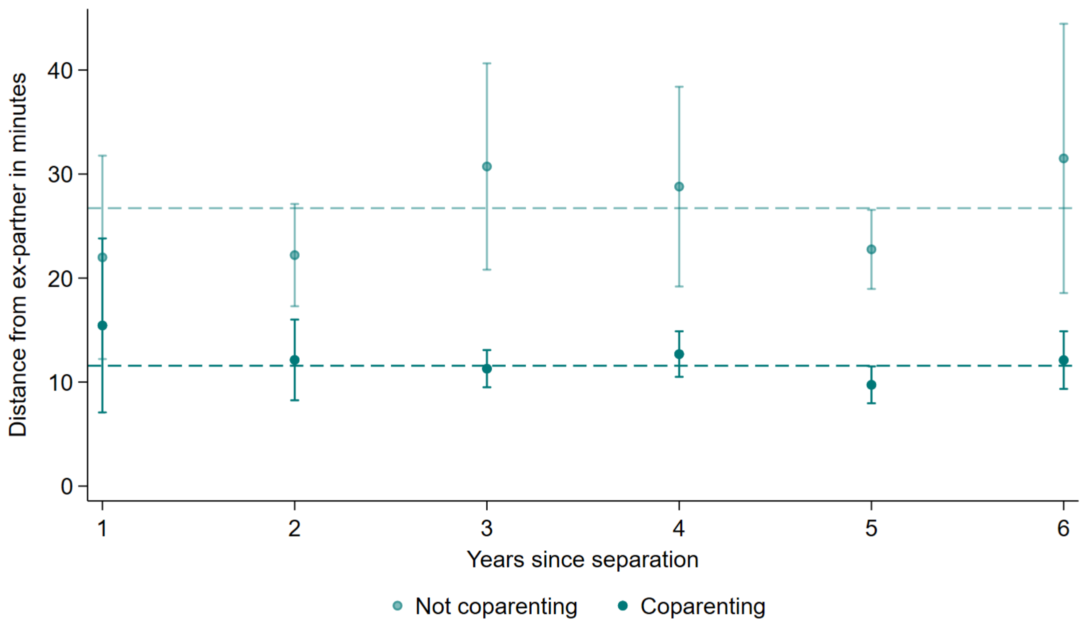
Note. This figure shows the share of parents who choose a co-parenting arrangement after divorce by the age of the youngest child and whether they were affected by the reform (Data: NFN).

Figure C3: Post-divorce child custody arrangements in the Netherlands by education of the mother, 2010.



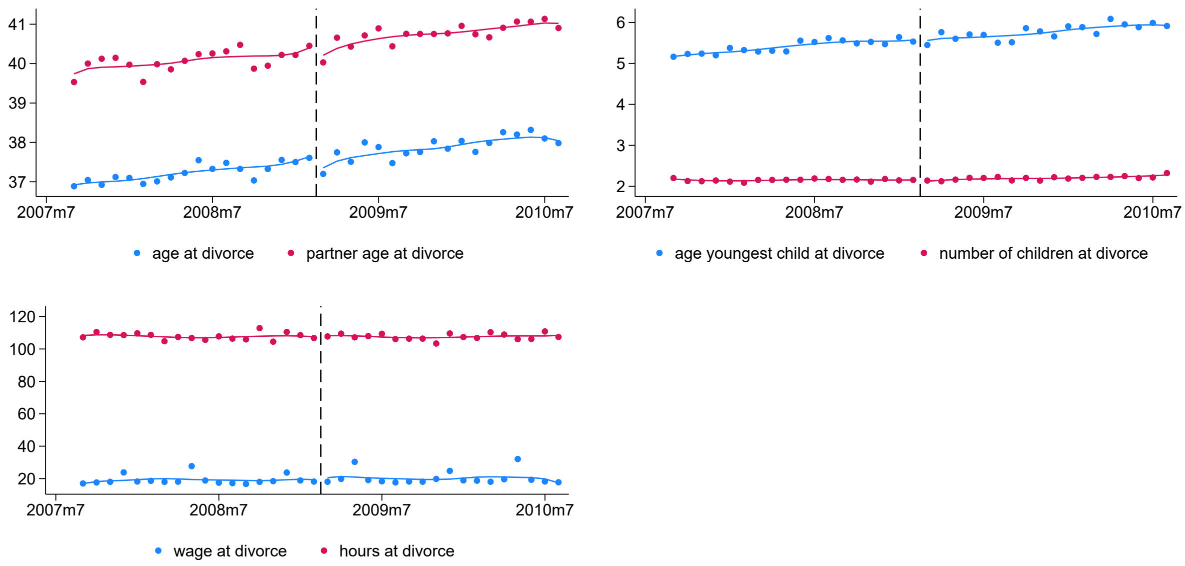
Note. This figure shows the share of mothers who choose co-parenting arrangements by educational attainment (Data: CBS). Co-parenting is most common among highly-educated mothers.

Figure C4: Distance between divorced parents with minor children by co-parenting status



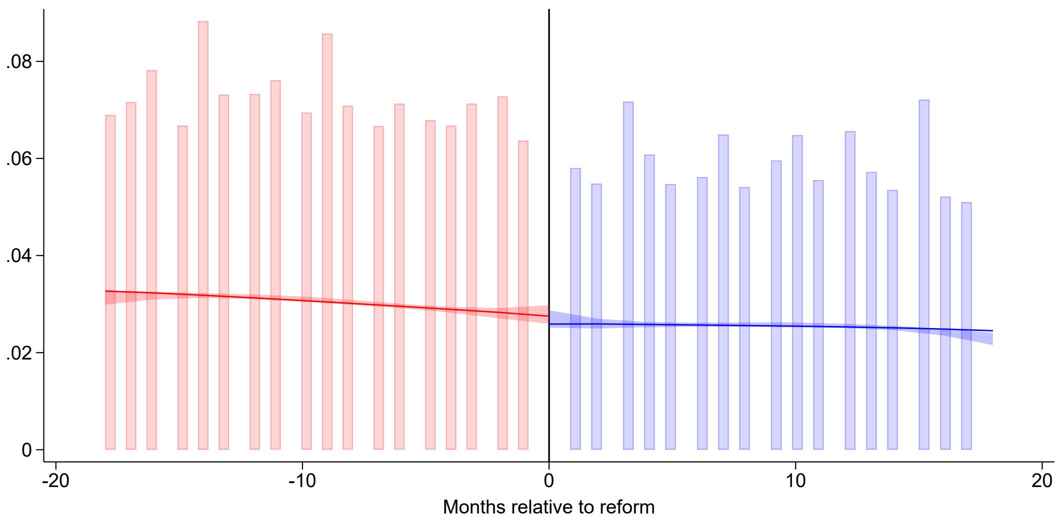
Note. This figure shows the travel distance between former spouses in minutes by year of separation. Markers plot coefficients of a linear model in which distance between ex-partners is regressed on years since separation by co-parenting status. The horizontal lines indicate the mean distances by co-parenting status. Vertical capped lines are 95% confidence intervals based on robust standard errors. Own calculations based on NFN data.

Figure C5: Treatment and control groups are balanced in trends



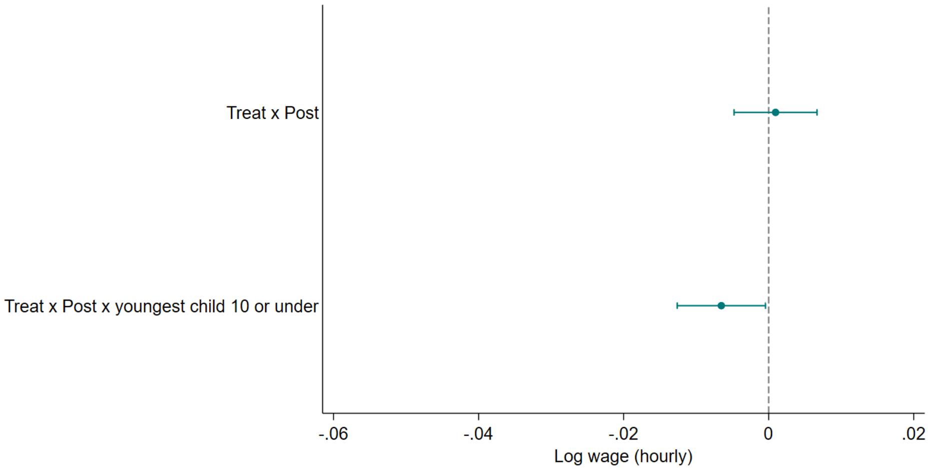
Note. This figure shows time trends of the variables for which the treatment and control samples are not balanced in levels.

Figure C6: There is no bunching right before the reform



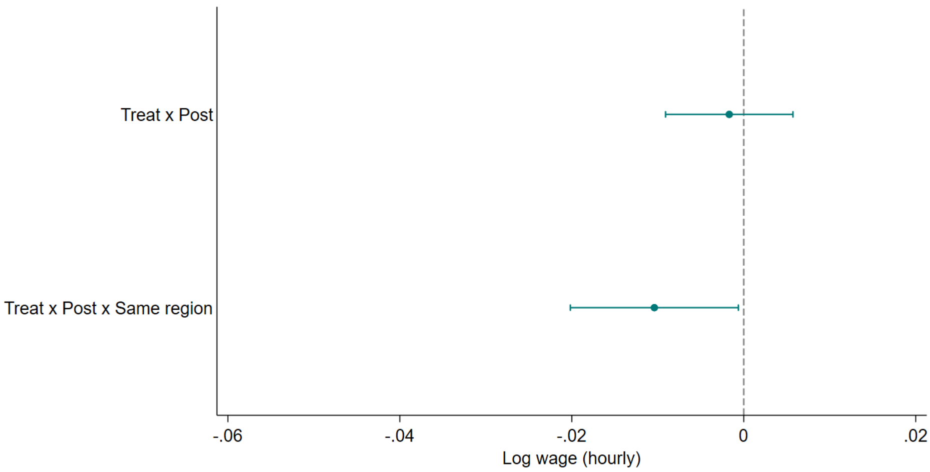
Note. This figure shows the density of divorces by month in the 18 months before and after the reform, and the 95% confidence bands based on the Cattaneo-Janssen-Ma density test.

Figure C7: The wage effect is driven by mothers with young children



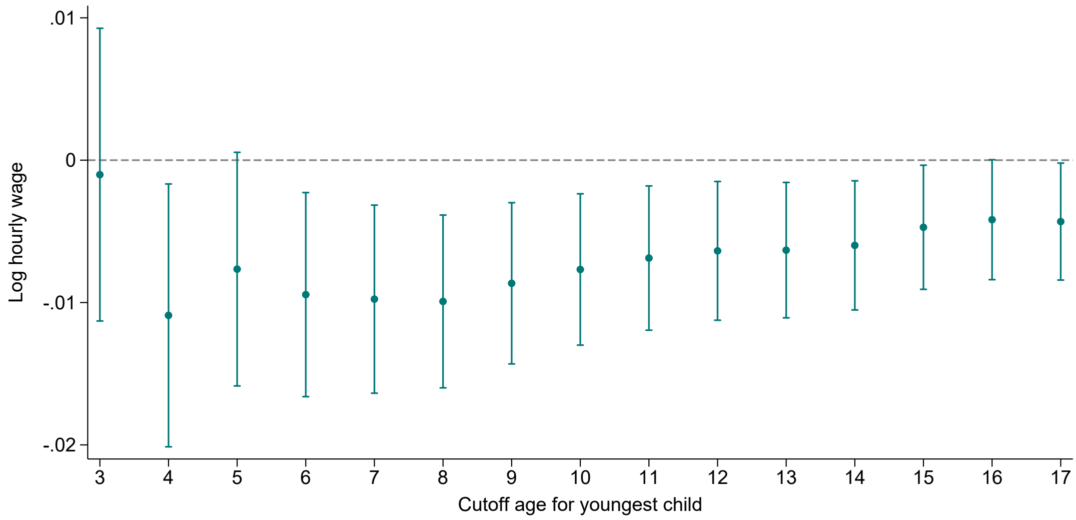
Note. This figure shows regression coefficients of a DiD regression of wages on a triple interaction between a post-reform indicator, a treatment group indicator, and an indicator for the age of the youngest child. The sample includes mothers divorcing between September 2007 and August 2010. The specification includes month-year×region and individual fixed effects. Horizontal bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

Figure C8: Wage decreases are driven by those who stay in the same region as their partner: triple difference



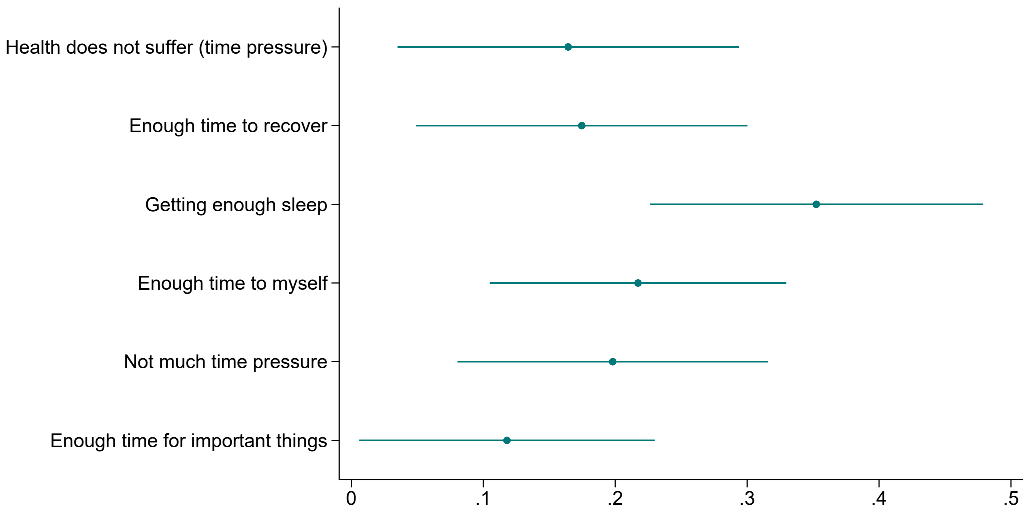
Note. This figure shows regression coefficients of a DiD regression of wages on a triple interaction between a post-reform indicator, a treatment group indicator, and an indicator for whether the ex-spouses work in the same region. The sample includes mothers with young children divorcing between September 2007 and August 2010. The specification includes month-year×region and individual fixed effects. Horizontal bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

Figure C9: Wage effect by cut-off age



Note. This figure shows the effect of the reform on mothers' wages by cut-off age for inclusion in the sample. The specification includes month-year×region and individual fixed effects. Vertical bars indicate 95% confidence intervals, with standard errors clustered at the individual level.

Figure C10: Mothers who co-parent report less time pressure



Note. This figure shows coefficients of a DiD regression of different outcomes on the interaction between indicators for co-parenting and being a woman (Data: NFN).

D Detailed derivations

Setup

Having substituted the period budget constraint, parents maximise expected life-time utility (post-divorce):

$$\max_{\{h_i,x_i\}} U_i = \mathbf{E}\left[\sum_{t=t_{div}}^{T} \beta^t \Big(\nu(w_i h_i - M_i^R) - \eta(h_i) + \phi(\sigma_i)\Big)\right]$$

subject to the time and possible distance constraints as well as commuting:

$$\begin{aligned} l_i + h_i + k_i + q_i^R &= 1 && \text{time constraint} \\ \|x_i - x_{r,i}\| &\leq \bar{d} && \text{distance constraint (under JPC only)} \\ k_i &= d(x_i, x_{r,i}) && \text{commuting, where } d'(x_i, x_{r,i}) > 0 \end{aligned} \tag{3}$$

In addition to the environment outlined in Section 3, I make the assumption that the utility function is moderately concave in consumption: $-\nu''(c) \cdot c < \nu'(c)$. This bound on the degree of concavity of the utility function implies that the function is concave, but not sharply curved, i.e. that marginal utility does not decline rapidly. A common example is CRRA utility with relative-risk aversion parameter $\gamma < 1$.[2] This assumption is used in the derivation of the result on hours as it allows to determine the sign of the derivative of hours worked with respect to wages.

Solving

Lagrangian

$$\max_{\{x_i,h_i\}} \mathcal{L} = \nu(w_i(x_i)h_i - M_i^R) - \eta(h_i) + \phi(\sigma_i) + \lambda_i(1 - l_i - h_i - d(x_i) - q_i^R) + \mu_i(\bar{d}^2(x_i, x_{r,i}) - \|x_i - x_{r,i}\|^2)$$

FOCs

$$\frac{\partial \mathcal{L}}{\partial h_i} : \nu'(w_i(x_i)h_i - M_i^R) \cdot w_i(x_i) = \eta'(h_i) + \lambda_i \tag{FOC 1}$$

Intra-temporal Euler equation: marginal utility of consumption of working one additional hour = marginal cost of working an additional hour in terms of disutility of working and

[2] Connecting to expected utility theory, this is equivalent to saying that the utility function exhibits bounded absolute risk aversion, with curvature constrained by $A(x) < \frac{1}{c}$. The utility function $u(\cdot)$ is strictly increasing and concave, with bounded absolute risk aversion:

$$A(x) := -\frac{\nu''(c)}{\nu'(c)} < \frac{1}{c} \quad \forall\ c > 0.$$

the opportunity cost of leisure. Importantly, there is also an income effect of moving from SPC to JPC: For mothers, M_m decreases, decreasing the marginal utility of income (the left-hand side of the first order condition). Thus, from the right-hand side, mothers' hours decrease. The opposite holds for fathers.

$$\frac{\partial \mathcal{L}}{\partial x_i} : \nu'(w_i(x_i)h_i - M_i^R) \cdot w_i'(x_i) \cdot h_i = \lambda_i \cdot d'(x_i, x_{r,i}) + 2\mu_i(x_i - x_{r,i}) \tag{FOC 2}$$

Parents relocate for higher wages until the expected wage gradient benefit is exactly offset by (i) the commuting cost ($\lambda_i \cdot d'(x_i, x_{r,i})$) and/or, under joint custody, (ii) the distance constraint bites. Under SPC, the last term drops out as $\mu_i = 0$.

KKT

$$\mu_i(\bar{d}^2(x_i) - \|x_i - x_{r,i}\|^2) = 0$$

Under SPC, $\mu_i = 0$; under JPC, $\mu_i > 0$.

Comparative Statics

What happens to hours, wages, and commuting when switching from SPC to JPC? Wages should decrease for mothers who become location constrained and they should commute more. The theoretical prediction on hours is ambiguous. No effects are expected for fathers. Below, I derive each of these predictions in turn.

Hourly wages

Think of the wage at a location as a lottery ticket value $w(x)$. SPC lets a mother look at every ticket in the country and pick the highest-paying one. JPC removes any tickets lying beyond the distance $\bar{d}$. A simple "maximum over a subset" rule implies:

- If the best SPC ticket is outside the JPC circle (empirically common for Dutch mothers), the maximum inside the circle must be strictly lower.
- If the best SPC ticket is already inside the circle (assumed for fathers), the maximum is unchanged.

Below, I formally show that the wage weakly declines relative to SPC if the mobility constraint binds:

Proposition 1. *The switch from SPC to JPC lowers mothers' expected wages:*

$$w_m^{\text{JPC}} \leq w_m^{\text{SPC}}.$$

Proof. Let the mother's unconstrained optimum be x_m^{SPC}. The feasible set of locations under JPC is $\chi^{\text{JPC}} := \{x : \|x_i - x_{r,i}\| \leq \bar{d}\}$. Then

$$w_i^{\text{JPC}} = \max_{x_i \in \chi_{\bar{d}}^{\text{JPC}}} w_i(x_i) \leq w_i^{\text{SPC}} = \max_{x_i \in \chi^{\text{SPC}}} w_i(x_i)$$

because utility is increasing in wages ($\nu_w > 0$) and

$$\chi_{\bar{d}}^{\text{JPC}} \in \chi^{\text{SPC}}$$

i.e. $x_{\bar{d}}^{\text{JPC}}$ could have been chosen under SPC, but was not. By revealed preferences, $w_{\bar{d}}^{\text{JPC}} \leq w_m^{\text{SPC}}$.

The relation holds with strict inequality whenever

$$w_i^{\text{SPC}} \notin \chi_{\bar{q}}^{\text{JPC}}.$$

□

Corollary 1 (No wage effect for fathers)**.** *If the father's SPC optimum already satisfies* $||x_f^{SPC} - x_{r,i}^{SPC}\| \leq \bar{d}$*, then* $\mu_f = 0$*,* x_f^{SPC} *remains feasible after the reform and* $w_f^{JPC} = w_f^{SPC}$*.*

Commuting

This follows rather trivially from the assumptions, but can also be shown formally:

Assumption 1 (Radial monotonicity of commuting time)**.** *For every pair of locations* $x_{r,1}, x_{r,2} \in \mathcal{X}$*,*

$$\|x_{r,1} - x_i\| < \|x_{r,2} - x_i\| \implies d(x_{r,1}, x_i) < d(x_{r,2}, x_i).$$

Proposition 2. *Assume radial monotonicity of commuting time and recall that under JPC both parents reside in location* x_f*. Then for every realisation of wage draws*

$$d_m^{\text{JPC}} \;\geq\; d_m^{\text{SPC}} \;=\; 0, \qquad d_f^{\text{JPC}} \;=\; d_f^{\text{SPC}} \;=\; 0,$$

with $d_m^{\text{JPC}} > 0$ *whenever the mother's JPC job lies at* $x_m^{\text{JPC}} \neq x_f$ *(strictly positive probability). Hence, under JPC, mothers commute for longer than under SPC. There is no change for fathers.*

$$d_m^{JPC} > d_m^{SPC}$$

and

$$d_f^{JPC} \approx d_f^{SPC}.$$

Proof. Under SPC the mother can relocate to her chosen job, hence $d_m^{\text{SPC}} = 0$. Under

JPC the residence is fixed at x_f, so her accepted job x_m^{JPC} satisfies $0 < \|x_m^{\text{JPC}} - x_r\| \leq \bar{d}$ unless the wage-maximising job is co-located with the home. Assumption 1 then implies $d(x_m^{\text{JPC}}) \geq d(x_r) = 0$; strict inequality holds if $x_m^{\text{JPC}} \neq x_r$. Fathers' residence and job coincide in both regimes, so their commute is unchanged. □

Hours

Proposition 3. *The prediction on hours from moving from SPC to JPC is ambiguous but weakly positive for mothers and weakly negative for fathers.*

Proof. To obtain the derivative of hours with respect to the shadow value of leisure ($\frac{\partial h_i}{\partial \lambda_i}$), first note that h is implicitly a C^1 function of (λ, w) by the implicit function theorem because $F_h = \nu''(wh)w^2 - \eta''(h) < 0$ as ν is concave and η convex. I rewrite the first-order condition with respect to hours (equation FOC 1) as

$$F(h_i, w_i, \lambda_i) = \nu'(w_i h_i - M_i^R)w_i - \eta'(h_i) - \lambda_i = 0 \tag{4}$$

By the implicit function theorem:

$$\frac{\partial h_i}{\partial \lambda_i} = -\frac{F_\lambda}{F_h} < 0$$

as $F_\lambda = -1$ and $F_h = \nu''(w_i h_i - M_i^R) \cdot w_i^2 - \eta''(h_i) < 0$ given concavity of ν and convexity of η ($\nu'' < 0, \eta'' > 0$).

How is λ_i affected? Taking the total derivative of the time constraint

$$-dl_i - dh_i - dd(x_i, x_{r,i}) - dq_i^R = 0$$

$$dh_i = -dl_i - dd(x_i, x_{r,i}) - \Delta q_i\,, \tag{5}$$

where $\Delta q_i = q_i^{\text{JPC}} - q_i^{\text{SPC}}$. On average, $\Delta q_i < 0$ for mothers and $\Delta q_i > 0$ for fathers.

Totally differentiating the first-order condition with respect to hours (equation FOC 1) yields

$$\begin{aligned}
&\nu''(w_i(x_i)h_i - M_i^R) \cdot w_i(x_i)^2 \cdot dh + \nu''(w_i(x_i)h_i - M_i^R) \cdot h_i \cdot dw + \nu'(w_i(x_i)h_i - M_i^R) \cdot dw \\
&= \eta''(h_i) \cdot dh + d\lambda_i \\
&\Big[\nu''(w_i(x_i)h_i - M_i^R) \cdot w_i(x_i)^2 - \eta''(h_i)\Big]\, dh + \Big[\nu''(w_i(x_i)h_i - M_i^R) \cdot h_i + \nu'(w_i(x_i)h_i - M_i^R)\Big]\, dw \\
&= d\lambda_i
\end{aligned} \tag{6}$$

Substituting equation 5, I obtain

$$d\lambda_i = \underbrace{\left[\nu'' \cdot w^2 - \eta''\right]}_{<0} \cdot(-dl_i - dd(x_i, x_{r,i}) - \Delta q_i) + [\nu'' \cdot h_i + \nu']\, dw \tag{7}$$

Wages and commuting: effect of mobility restriction

Let x_i^{SPC} be an unconstrained maximiser of expected utility. The feasible set under JPC is:

$$\chi_i^{\text{JPC}} = \left\{x : ||x_i - x_{r,i}^{\text{SPC}}|| \leq \bar{d}\right\}$$

Given that $\chi_i^{\text{JPC}} \in \chi_i^{\text{SPC}}$:

$$w_i^{\text{JPC}} \leq w_i^{\text{SPC}}$$
$$d_i^{\text{JPC}} \geq d_i^{\text{SPC}}$$

Empirically, $\chi_i^{\text{SPC}} \notin \chi_i^{\text{JPC}}$ more likely for mothers than fathers.

Define

$$\Delta w_i = w_i^{\text{JPC}} - w_i^{\text{SPC}} \leq 0$$
$$\Delta d_i = d_i^{\text{SPC}} - d_i^{\text{JPC}} \geq 0$$

Again, using the implicit definition of h from the first-order condition with respect to hours (equation 4) and linearising F_h around the SPC point $(\lambda^{\text{SPC}}, w^{\text{SPC}})$ using the implicit function theorem yields the two partial derivatives:

$$\begin{aligned}
\frac{\partial h}{\partial \lambda} &= -\frac{F_\lambda}{F_h} = -\frac{-1}{\underbrace{\nu''(wh - M^R)w^2 - \eta''(h)}_{<0}} < 0 \\
\frac{\partial h}{\partial w} &= -\frac{F_w}{F_h} = -\frac{\nu''(wh - M^R)hw + \nu'(wh - M^R)}{\underbrace{\nu''(wh - M^R)w^2 - \eta''(h)}_{<0}} \geq 0 \quad \text{if } -\nu''(wh - M^R)wh \leq \nu'(hw - M^R)
\end{aligned} \tag{8}$$

The second result holds with inequality as long as the assumption about bounded absolute risk aversion is satisfied. This is the case for standard classes of utility such as CRRA. Under log utility, the numerator is zero. I also assume that earnings always cover the cost of children under any custody arrangement, i.e. that $wh \geq M_i^R$.

First-order Taylor expansion for small values of $\Delta\lambda, \Delta w$:

$$\begin{aligned}
\Delta h &\equiv h\left(\lambda^{\text{JPC}}, w^{\text{JPC}}\right) - h\left(\lambda^{\text{SPC}}, w^{\text{SPC}}\right) \\
&\approx \underbrace{\frac{\partial h}{\partial \lambda}}_{<0} \cdot \Delta\lambda + \underbrace{\frac{\partial h}{\partial w}}_{\geq 0} \cdot \Delta w + \mathcal{O}\left((\Delta\lambda, \Delta w)^2\right)
\end{aligned} \tag{9}$$

Depending on the size of the income effect of the custody regime (ΔM_i), the change in hours will be attenuated relative to a scenario in which the choice of custody regime has no income effect. This assumes that the substitution effect dominates the income effect. If the income effect were larger than the substitution effect, the sign on the change in hours could reverse.

Moving from SPC for mothers to JPC implies

$$\begin{aligned} \Delta\lambda_m < 0 &\qquad \text{for mothers} \\ \Delta\lambda_f > 0 &\qquad \text{for fathers} \end{aligned} \tag{10}$$

On the wage side

$$\begin{aligned} \Delta w_m < 0 &\qquad \text{for mothers} \\ \Delta w_f \approx 0 &\qquad \text{for fathers} \end{aligned} \tag{11}$$

Finally, putting everything together:

$$\begin{aligned} \Delta h_m \geq 0 &\qquad \text{for mothers} \\ \Delta h_f \leq 0 &\qquad \text{for fathers} \end{aligned} \tag{12}$$

$\square$

E Joint physical custody in other countries

In 2021, about 20.7% of separated parents in Europe had some form of joint physical custody, with the highest rates in Northern European countries, and the lowest in Eastern and Southern Europe (Claessens and Mortelmans, 2025; Hakovirta et al., 2023).[3] Canadian data document a rise in joint physical custody from about 10% in 2006 to 31% in 2018/19 (Department of Justice Canada, 2021). In France, about a quarter of divorce cases involving children resulted in co-parenting arrangements. In the United States, joint physical custody is estimated to have covered 34% of children after divorce between 2010 and 2014. This rise has been attributed to changing norms and policies favouring co-parenting (Meyer et al., 2022). Since 2018, several U.S. states have introduced the legal presumption of splitting parenting time 50/50 unless it is not in the child's best interest, and other U.S. states have debated the adoption of similar legislation in recent years. In short, co-parenting has become a widespread post-divorce parenting arrangement in many Western jurisdictions over the past two decades, though the pace and extent of its adoption vary.

In Asia and the Middle East, joint physical custody is much less prevalent. For example, Japan does not legally recognise joint custody after divorce (one parent receives sole custody by law), and culturally the concept of equal co-parenting post-divorce remains uncommon. However, even in such jurisdictions there is growing debate about fathers' access to their children and reforms to facilitate more contact with non-resident parents.

In most countries other than the Netherlands, the increase in shared physical custody occurred gradually, without a clear policy reform. Legal regimes differ in whether they mandate, presume, or simply allow such arrangements. Coupled with its administrative data records, the reform for a legal preference for co-parenting makes the Netherlands a good country to study its labour market effects.[4]

The idea that co-parenting is the ideal post-divorce parenting arrangement has been codified in international policy, most notably in the Council of Europe's 2015 landmark statement on "Equality and shared parental responsibility: the role of fathers."[5] This resolution urges all 47 member states to "introduce into their laws the principle of shared residence following a separation," with exceptions only for cases of abuse or violence. It emphasises that equal involvement of both parents is beneficial for children's development and gender equality, and calls on governments to facilitate shared residence (for example, through enforcing contact orders, encouraging mediation, and adjusting social benefits to accommodate dual-household families).

[3] In Sweden, the share with equal parenting is as high as 42.5%.
[4] Similar reforms were passed in Australia and Belgium, both in 2006.
[5] Council of Europe Parliamentary Assembly Resolution 2079.

F Data construction

First, I identify all divorces between 2006 and 2013. Then, I keep all observations in that time window related to those ever divorcing during it in the dataset. I proceed by creating a monthly relationship panel with partner ID.

For all individuals in the sample, I collect their employment history between 2006-2013, at the monthly level. If individuals hold multiple jobs in the same month, I keep the highest-paying one. If the jobs are paid equally, I choose the one with more hours worked.

In a final step, I combine the relationship and employment panels and keep only individuals aged 18 to 60 divorcing in heterosexual marriages.