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Commuting, gender and children☆

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ABSTRACT

We demonstrate that women with children are much more likely to leave their job when they have a long commute, which is not true for men. Interpreting these results through the lens of a dynamic search model, we demonstrate that commuting costs increase substantially for women after they have children. For women with children, a 12 kilometer increase in commuting distance induces costs equivalent to about 20% of their wage.

1. Introduction

Over the last decades, earnings for men and women have converged due to the reduced gap in education, skills, and labor participation (Altonji and Blank, 1999; Blau and Kahn, 2017; Maasoumi and Wang, 2019; Gallen et al., 2019). However, women still earn substantially less than men, despite decades of equal-pay laws. This gender pay gap has been argued to be essentially a child penalty for women because the childbirth induces career interruptions and reduced working hours (Manning and Petrongolo, 2008; Blau and Kahn, 2017; Kleven et al., 2019b; Cortés and Pan, 2020; Card and Hyslop, 2021).

Using administrative register data for the full working population in Denmark for the years 2003–2013 we apply an event study methodology – the birth of the first child – and demonstrate, that women not only earn substantially less but also strongly decrease their commute after the birth of the child relative to men. This finding makes sense, as for many workers, adjusting the length of the commute through a job move is an important behavioral margin to optimize time devoted to labor as they are severely constrained in their choice of working hours (Böheim and Taylor, 2004).

Consistent with this finding, we show that women with a long commute are *several times* more likely to change jobs when they have a child, which is not true for men.[2] We also show that workers with a higher wage are less likely to move jobs. Interpreting these results through the lens of a dynamic search model as in Gronberg and Reed (1994), Van Ommeren and Fosgerau (2009) and Le Barbanchon et al. (2021), we estimate how much workers are willing to trade off wages for a shorter commute, i.e. we estimate the marginal cost of commuting.

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[2] Petrongolo and Ronchi (2020) show that women are more likely than men to move job given a long commute, but ignore the role of children. The reduction in commuting distance for mothers has also been documented in Germany, see Skora et al. (2020).

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We show that this cost is about the same for men and women before the birth of a child, but after the birth, it is substantially higher for women.

Our study refers to a range of literature. First, our paper refers to a literature emphasizing that women have higher commuting costs, resulting in restrictive job search and shorter commutes (Le Barbanchon et al., 2021; Farré et al., 2022; Petrongolo and Ronchi, 2020). Employing revealed preference data, we demonstrate that women *with children* bear higher marginal commuting costs. Consistent with that we show that gender differences in the length of the commuting distance come into existence after the birth of the first child.[3]

Second, we contribute to the urban economics literature aiming at estimating the marginal cost of commuting, i.e. the marginal willingness to pay for commuting. Commuting costs are fundamental as they determine the urban spatial structure by influencing the size as well as the structure of cities (Wheaton, 1974; Fujita, 1989; Lucas and Rossi-Hansberg, 2002; Baum-Snow, 2010; Ahlfeldt et al., 2015; Heblich et al., 2020), but surprisingly few estimates of the commuting costs exist.

Third, our paper also relates to a large literature on the value of non-wage job attributes for workers (Ophem, 1991; Gronberg and Reed, 1994; Bonhomme and Jolivet, 2009; Sullivan and To, 2014). Important non-wage job attributes include health insurance (Gruber and Madrian, 2004; Aizawa and Fang, 2020), employer-provided retirement benefits (Altonji and Paxson, 1992), employer-provided cars (Gutiérrez-i-Puigarnau and Van Ommeren, 2011), and employer-provided parking (Van Ommeren and Wentink, 2012).

In the current paper, we apply the methodology introduced by Gronberg and Reed (1994) to estimate the marginal cost of commuting derived from information about the effects of commuting distance and wages on job mobility given assumptions on the job search environment (as in Van Ommeren et al. (2000), Manning (2003a), Van Ommeren and Fosgerau (2009), and Le Barbanchon et al. (2021)).

Our first, and main, improvement is that we improve the Gronberg-and-Reed methodology to estimate the cost of commuting as applied in Van Ommeren et al. (2000), Manning (2003b) and Van Ommeren and Fosgerau (2009). In essence, this approach estimates the effects of non-wage job characteristics (i.e., commuting distance) and wages on job mobility. The ratio of these effects provides information about the willingness to pay for these non-wage characteristics. The underlying idea is that workers search for a job where the distribution of wages of alternative jobs is given (Pissarides, 2000). Consequently, workers with higher wages are less likely to move jobs, because alternative jobs have become less attractive.

The fundamental econometric problem with this approach is that workers are heterogeneous, so the worker's wage is an increasing function of the worker's productivity level. But a higher level of productivity shifts the distribution of wage offers to the right. For example, if one observes a worker with a high wage, then it may be the case that this worker is also more productive to other firms, or that this worker had just a lucky draw (Barlevy, 2008). Only in the latter case, there would be a strong incentive not to move to another job. Consequently, not controlling for worker productivity will result in an estimate of the marginal effect of wages which is biased towards zero. This bias may be large, because it is generally thought that the relationship between wages and productivity is tight (and even one-to-one according to fully competitive labor market models without search). The urban economics literature is aware of this bias, and in empirical applications, workers' characteristics (e.g. education, age, sector) are used as controls (Manning, 2003b; Van Ommeren and Fosgerau, 2009). However, many characteristics of the worker are then still unobserved. In the current paper, we improve on the empirical method by including worker fixed effects.

The inclusion of worker fixed effects, which controls for time-invariant unobserved heterogeneity, is not sufficient (and may make it even worse): workers' wages strongly vary over time, and if workers' productivity changes, also the wage offer function changes over time.[4] For example, if we observe an associate professor who receives a wage increase from her current employer because of a top-five publication, it is plausible that her wage offer distribution would also be affected by this publication. We aim to solve the econometric problem by combining the worker fixed effects with an IV approach. In essence, we are looking for an instrument that determines a worker's wage, but not directly the wage offer distribution of this worker, as this would directly affect job mobility.

Here we follow Bassier et al. (2022) and use the average wage of other workers with similar positions within the same firm as an instrument, while we control for firm characteristics – sector and firm size – which are known to correlate with non-wage amenities (Oi and Idson, 1999). Hence, the identifying assumption we make is that changes over time in the wage offer distribution of a worker are not related to changes over time in the average wage of other workers in the same firm, conditional on sector and firm size.[5]

This assumption is subject to criticism because of the presence of unobserved non-wage amenities that correlate with the average wage within the firm, but not with sector or firm size. We deal with this by adding many other firm-level controls, such as more detailed sector controls, average educational level, the proportion of female employees, and the presence of female top management. The latter control aims to capture amenities particularly important to female employees with children, such as flexible working hours. Similar to Bassier et al. (2022), we also control for measures that aim to capture the value of all non-wage amenities combined, as developed by Sorkin (2018) and Bagger and Lentz (2018). We demonstrate that our results are robust when adding controls, which adds confidence to the estimation strategy.

Our second improvement is that our study presents a significant advance in data quality compared to previous studies. We use administrative register data for the universe of the working population of Denmark (rather than survey data), and we observe a precise measure of commuting distance. This allows the econometric analysis to control for unobserved time-invariant worker characteristics using worker fixed effects and to calculate our instrument, whereas previous studies rely on cross-section identification. One exception is the stated-preference study of Le Barbanchon et al. (2021), which uses a 8% sample drawn from matched French unemployment and employment registers.

When interpreting our results, we assume that the labor market is characterized by search frictions and, therefore, not competitive. This interpretation is consistent with our main theoretical framework where we estimate the marginal cost of commuting, as this framework relies on a labor market where workers get offers from a wage distribution. This is fundamental because frictions in the matching between workers and jobs imply that the workers' evaluation of these job attributes are *not* equal to the compensating wage differentials of these job attributes as estimated in hedonic wage literature (Hwang et al., 1992;

[3] Le Barbanchon et al. (2021) find seemingly contradictory effects of children. On the one hand, using a structural job search model, they do not find any effect of children on the marginal cost of commuting. On the other hand, the reservation commute (i.e. the stated maximum commute) is much smaller for women with children. In this paper, it must be noted that these effects are not interpreted as causal. Another important difference is that the focus is on the commute of workers currently unemployed (which is a selective sample) with or without children, whereas our focus is on full-time employed workers that recently have their first child.

[4] In line with that, the monopsony literature shows that approaches using worker fixed effects provide job-separation elasticities that are to be too low to be believable. For a detailed discussion, see Bassier et al. (2022).

[5] Using an IV approach also reduces other econometric issues, such as measurement error in the net wage because the tax on labor income depends on non-labor activities such as house ownership.

Mulalic et al., 2013; Mas and Pallais, 2017). This criticism applies in principle to all job attributes, but in particular to commuting, as job search frictions are thought to be essential to explain large variation in commuting distances of otherwise identical workers (Manning, 2003a; Le Barbanchon et al., 2021).

The remainder of the paper is organized as follows. In Section 2, we present and describe the data. In Section 3, we establish the relevance and extent of the gender pay and commuting gaps using an event study methodology. In Section 4, we provide the theoretical foundation of how to estimate the marginal cost of commuting. We introduce the econometric approach and provide the empirical results in Section 5. Finally, Section 6 presents the main conclusions.

2. Data

Our sample consists of longitudinal administrative register data for the full working population in Denmark. We observe all workers' demographic information (such as gender, number of children, and education) and labor market outcomes (such as annual wage, occupation, and sector).

We restrict our sample to workers who are employed between 2003 and 2013 and we censor observations of workers who move into non-employment, so all our job moves refer to job-to-job moves. This restriction makes it likely that the job moves (observed by us) tend to be voluntary, which will be a requirement of the approach introduced later on. Furthermore, we select observations of individuals who experience the birth of their first child either in this period or within up to 9 years before or 4 years after this period. This restriction is useful because workers without children may face different labor market conditions. We also impose a standard set of sample selection criteria of workers, i.e. we exclude workers younger than 19 or older than 45, workers who are in ongoing education, teleworkers, workers with an extremely low income (the lowest percentile), and workers with commuting distances exceeding 50 km. Commuting distance is calculated for each worker as the shortest route between the worker's residence and workplace location taking into account changes in road infrastructure.[6] In our analyses, we capture wages using annual *net* labor income, which includes a commuting tax deduction.[7] Commuters in Denmark are entitled to a tax deduction when the commute exceeds 12 km, which disproportionately benefits male commuters.[8]

We focus on full-time workers, which facilitates the interpretation of our empirical findings because for part-time workers we do not observe the exact number of hours worked. We define job mobility as a move from a (full-time) job to another job (which can be full-time or part-time).[9] We have slightly more than 3 million observations.[10] Due to the childbirth and age selections, we focus on workers at the beginning of their career: workers are, on average, about 28 years in the period before the birth of their first child and about 35 years in the period after.

Table 1
Summary statistics by gender and period (birth of first child).

	Men		Women	
	Mean	Std. dev.	Mean	Std. dev.
Before childbirth				
Commute (km)	13.20	11.91	12.26	11.72
Annual net income (DKK)	336,469	114,670	293,826	98,184
Job move	0.18	0.38	0.17	0.37
Residence move	0.19	0.39	0.19	0.39
Job tenure	2.80	2.63	2.43	2.14
Age	28.45	4.99	27.94	4.44
N	501,478		443,408	
After childbirth				
Commute (km)	15.04	12.24	12.65	11.03
Annual net income (DKK)	394,345	137,048	298,310	113,435
Job move	0.15	0.36	0.15	0.36
Residence move	0.09	0.29	0.09	0.28
Job tenure	4.36	3.90	3.94	3.43
Age	36.20	4.30	34.91	4.22
N	1,140,917		1,179,652	

Notes: Full-time workers in the ten years around the birth of the first child. Observations from the year of childbirth are excluded. 1 DKK ≈ 0.15 USD.

Table 1 shows that the average commute for men and women before the birth of their first child is quite similar: men commute 13.2 km and women commute 12.3 km, so a difference of about 8%. After the childbirth, however, it increases for men by almost 2 km to 15.0 km, while for women it increases by only 0.4 km to 12.7 km. The commuting distance for men after the birth of the first child is around 2.4 km longer, which is substantial. The men's time devoted to commuting is then approximately 30 min per week higher.[11]

Wages for men exceed wages for women before and after the childbirth, but their difference is larger after the childbirth: the gender pay gap amounts to 12% before the childbirth and 24% after. It further appears that the Danish job market is characterized by high labor turnover and therefore by short job durations (on average 3 years). Around 16% of workers move to another job within a year. The shares of men and women that move job before and after the childbirth are similar. In Fig. 1(a), we show distributions of log wage by gender and presence of a child. A remarkable feature of the distributions is that they are similar for men and women before the event, but not after: in particular the share of women with low wages increases, while for men the whole distribution moves to the right. In Fig. 1(b) we show the commuting distributions by gender and child. It appears that after the childbirth the share of men with short commutes strongly drops, while for women this does not occur.

So how important is the role of residential moving behavior for commuting? Changes in commuting distance are predominantly a labor market phenomenon: the average (absolute) change in the commuting distance is about 7.0 km given a residential move, less than the 9.4 km when changing job. Furthermore, residential moving behavior is

[6] This is important because alterations to transport infrastructure can significantly impact commuting patterns and spatial organization of cities (Ahlfeldt et al., 2015; Börjesson et al., 2019; Mulalic and Rouwendal, 2020; Tsivanidis, 2019).

[7] In 2019, commuters were entitled to deduct 1.96 DKK, about 0.20 USD, from gross income per kilometer driven, so about 4 DKK per (one-way) commuting distance over 15 km. A range of other European countries have similar commuting tax deductions (Potter et al., 2006; Paetzold, 2019).

[8] In terms of deduction incidence, gender differences are moderate: without children, 41% of men and 37% of women receive the deduction, and with children 49% of men and 41% of women. However, on average, the implied subsidy is several times larger for men. With children, the average annual commuting subsidy for men is 652 DKK whereas for women it is only 163 DKK; without children, the annual subsidy is 255 DKK for men and 61 DKK for women. This subsidy is small and even for men with children it is only 0.17% of their income. Consequently, the subsidy implied by the deduction is too low to notably affect wage setting and job mobility. 1 DKK ≈ 0.15 USD.

[9] Take note that the share of part-time workers is generally low (10%–15%) and stays more or less the same before and after having a child, and across genders, see Fig. B.1 in Appendix B. Out-of-sample job mobility is also limited at 6%–10%. Therefore, the focus on full-time workers is less restrictive in Denmark compared to other countries (Kleven et al., 2019a).

[10] Our original sample consists of about 10 million observations. We exclude observations with commuting distances outside the range (about one million observations), observations not referring to parents (about 4 million observations), part-time (about 0.5 million observations), censoring income (about 0.1 million observations), and observations with missing values (about 0.2 million observations).

[11] In Appendix C, using survey data, we show that the marginal effect of distance (in kilometers) on commuting time (in hours per trip) is about 0.025. We then multiply 0.025 by the increase in commuting distance (2.4 km) x 10 trips.

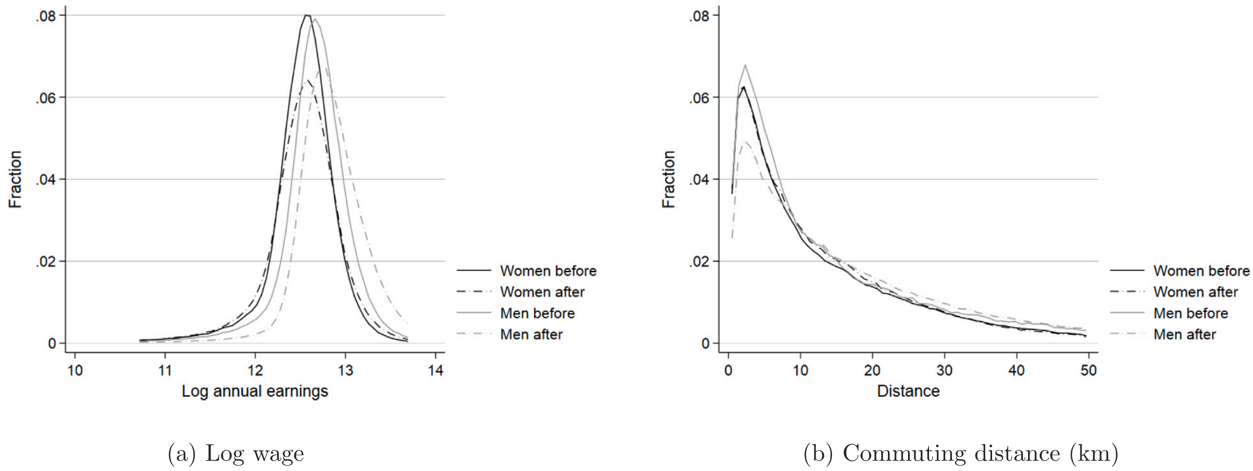


Fig. 1. Distribution of wages and commuting distance by gender and first child.

important before the childbirth – about 19% of workers move residence each year – but this drops to 9% after the childbirth. So, residential moves tend to be more local, and are less frequent when having children, but still play an important role for commuting, and will receive special attention later on.

3. Gender, wage and commuting gap

We first establish the relevance and extent of a gender commuting gap using a standard event study methodology based on the birth of the first child, following studies such as Kleven et al. (2019b). We use individual-level variation in the timing of childbirth. Observed sharp changes in wage and commuting for mothers relative to fathers around the birth of the first child are likely orthogonal to unobserved determinants of these outcomes as they evolve smoothly over time. To reduce the selection effects of the childbirth, we only select individuals who become a parent for the first time either during the period of observation or in the 10 years before or after the childbirth.

Event time is denoted by t (measured in years) and we observe the childbirth at time $t = 0$ (the actual the childbirth occurs between -1 and 0). We focus on two outcome variables of worker i: wage and the length of the commute, both denoted by $y^g_{i,s,t}$. We then estimate the effect of the childbirth at $t = 0$ on $y^g_{i,s,t}$, for each gender g separately, controlling for year s and age $h_{i,s}$:

$$y^g_{i,s,t} = \sum_{j \neq t'} \alpha^g_j \cdot \mathbb{I}[j = t] + \sum_k \beta^g_k \cdot \mathbb{I}[k = h_{i,s}] + \sum_l \gamma^g_l \cdot \mathbb{I}[l = s] + v^g_{i,s,t}, \qquad (1)$$

where event time effects are captured by α^g_j which yield the event time effect in relation to the year of the birth and $\mathbb{I}$ denotes an indicator variable.[12] In (1) we exclude α^g_j for $j \neq t'$ which is the reference category. This implies that the event time coefficients measure the impact of the birth of the first child relative to t'. When we focus on commuting distance then $t' = -1$, i.e. the last year before the worker is affected by the child birth. When we focus on wage then $t' = -2$, as we wish to allow for reduced wages due to maternity leave in the year before the childbirth. β^g_k captures the effects of a set of age dummies (to control for life cycle), γ^g_l a set of year dummies (to control for time trends), and $v^g_{i,s,t}$ is a (gender-specific) error term.[13] The estimated $\tilde{\alpha}^g_j$ are converted to percentage changes by $\tilde{\alpha}^g_t / \tilde{y}^g_{i,s,t}$, where $\tilde{y}^g_{ist}$ is the predicted outcome using the estimated coefficients (while excluding α^g_j), i.e. $\tilde{y}^g_{i,s,t} = \sum_k \tilde{\beta}^g_k \cdot \mathbb{I}[k = h_{i,s}] + \sum_l \tilde{\gamma}^g_l \cdot \mathbb{I}[l = s]$. It captures the event time effect at t as a share of the counterfactual outcome (i.e. no child at t').

In Fig. 2, we show $\tilde{\alpha}^g_t / \tilde{y}^g_{i,s,t}$ based on the estimates of (1). Fig. 2(a) shows a gender pay gap of about 15% immediately after the childbirth compared to the year before pregnancy. It also shows that the wages of women and men follow the same trend before (and after) birth. Women's wages drop substantially after the childbirth, while in contrast men's wages only slightly decrease. Moreover, the figure also shows that the effect of the birth of the first child is very persistent, i.e. it remains at the same level 10 years after the child's birth. These results are not novel to the literature. For example, they are consistent with (Kleven et al., 2019b) who find that the gap remains after 20 years.[14]

We now focus on the role of the childbirth on commuting distance, which is of interest here. Fig. 2(b) shows that the commuting distances of women and men follow the same upward trend before the birth of the child, but after the childbirth, women's commuting distance gradually reduces, while men's commuting distance uninterruptedly follows the trend a few years after the childbirth and then stagnates. The gender commuting distance gap ranges from about 5% immediately after the childbirth (compared to the year before pregnancy) to about 15% ten years after. The resulting difference in commuting patterns after the childbirth hints towards an increase in the cost of commuting for women after having a child.

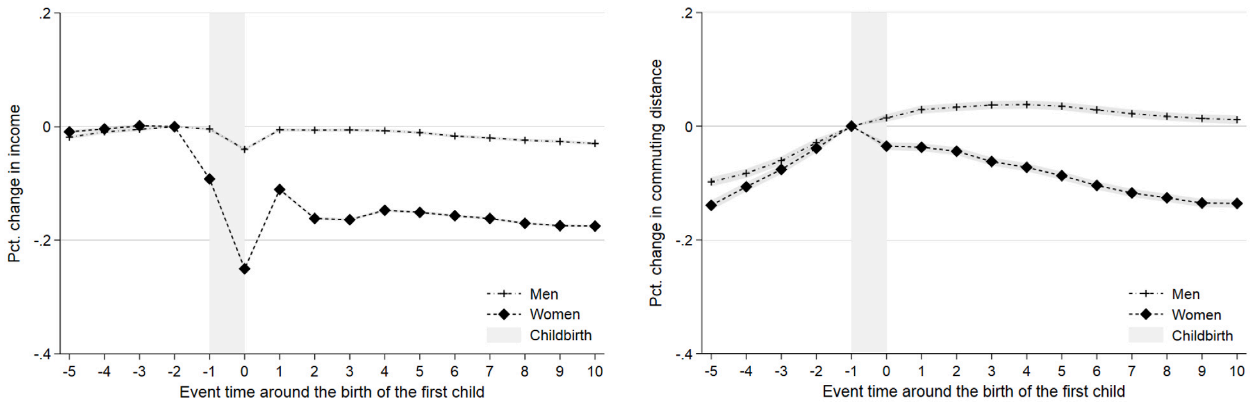
Additionally, we have tested whether the gender difference in commuting distance after the childbirth is sensitive to additional controls. For example, we have performed the same analysis with two additional control variables: education and the number of workers at the firm level. The results remain robust.

The previous descriptives suggest that the increased gender differences in the length of the commute are predominantly due to job moving. To investigate this in more detail, we apply the event study methodology on sub-samples of workers that either do not move job or do not move residence (in the period starting 3 years before the birth), see Fig. B.3 in Appendix B. These figures corroborate the idea that gender differences in commuting distance after the childbirth are predominantly due to gender differences in job moving and less due to gender differences in residential moving behavior. Nevertheless, residential moves still play an important role for commuting, and will receive special attention later on.

[12] In our application, α^g_j range from -10 until $+9$. This specification does not include worker fixed effects but they will be included in later analysis.

[13] Age dummies are important because women are often younger than man when having their first child.

[14] When we include part-time workers, our results do not fundamentally change. We also estimate models on sub-samples of workers that either do not move jobs or do not move residence or both (in the period starting 3 years before the birth), see Fig. B.2 in Appendix B. The results remain the same.



(a) Wage (b) Commuting distance

Fig. 2. Wage, commuting and the first child. *Notes:* wage and commuting distance event time effects around the birth of the first child. The gray area marks the time interval of the birth of the first child. The shaded 95 percent confidence intervals are based on robust standard errors.

4. Theoretical foundations

It is intuitive to estimate the marginal cost of commuting, defined as the marginal monetary valuation of commuting distance, using the ratio of the effects of commuting distance and wages on job-to-job mobility (Van Ommeren et al., 2000; Van Ommeren and Fosgerau, 2009). In this section, we discuss more precisely the theoretical foundations for estimating the marginal cost of commuting.

The theoretical starting point is a labor market where workers have to search for jobs located at different locations, and where workers maximize their utility by moving jobs from one location to another location (Manning, 2003b). Jobs are characterized by wages and commuting distance and employers post wage offers drawn from a given wage distribution. In the basic setup, (i) the wage offer distribution does not depend on the current wage, which is equivalent to assuming that the current wage does not change while staying in the same job, (ii) space is homogeneous, (iii) workers are not allowed to move residence, (iv) there are no job moving costs, (v) job search costs are absent, (vi) job-to-job moves are voluntary and there are no moves into unemployment or non-participation, (vii) the environment is stationary, and (viii) non-wage amenities are absent. We will relax these assumptions later on, and discuss the consequences for the estimation procedure.

4.1. Basic model

Workers get utility from wages, w, and disutility from distance to work, x. Utility is additive in the *logarithm* of wages and commuting. Hence, $v = log(w) - \alpha x$. Here, α can be interpreted as the marginal disutility of commuting. Job offers, implying an offer of w^* and x^*, arrive at an exogenous arrival rate λ. Wage offers come from a continuous wage offer distribution $F(w^*)$. Workers will accept all jobs that offer a utility increase. The job moving rate, so the rate of job offers that are accepted, is denoted by θ. Workers maximize lifetime utility V, while discounting the future at a given discount rate.[15]

[15] Comparative statics analysis shows that α has an ambiguous effect on the job moving rate (see Appendix A.1). This makes sense, because an increase in commuting costs reduces the utility of the current job. This effect is proportional to the length of the commute, so this effect is weak for short commutes and strong for long commutes. At the same time, an increase in α makes all job offers to become less attractive. The latter effect is convex (because space is two-dimensional), so the job moving rate can be shown to be inversely proportional to α^2. This suggests that a higher α (e.g. due to having a child) may strongly restrict job moving, in line with (Le Barbanchon et al., 2021).

We are interested to estimate the value of the instantaneous marginal cost of commuting, MCC, defined by $-(\partial v/\partial x)/(\partial v/\partial w)$. It appears that MCC can be estimated using information about job mobility (see Appendix A.2), as:

$$MCC \equiv -\frac{\partial v/\partial x}{\partial v/\partial w} = -\frac{\partial\theta(w,x)/\partial x}{\partial\theta(w,x)/\partial w} = -\frac{\partial\theta(w,x)/\partial x}{\partial\theta(w,x)/\partial log(w)} w = \alpha w. \tag{2}$$

Consequently, MCC is equal to αw, and can be estimated by the ratio of the marginal effect of commuting distance on job mobility, $\partial\theta(w,x)/\partial x$, and the marginal effect of log wage on job mobility, $\partial\theta(w,x)/\partial log(w)$.

In the current paper, our econometric methodology to estimate MCC is to derive estimates for α using estimates of the effects of log wages and commuting distance on the job moving rate. Our key interest is to examine to what extent α depends on the presence of children, and whether this differs by gender.

4.2. Extensions

We have derived Eq. (2), given a number of restrictive assumptions. We will now discuss these assumptions one by one, and the consequences of these assumptions for the estimation strategy.

The wage offer distribution does not depend on the wage level of the worker. For heterogeneous workers with different productivity levels, this assumption is unlikely to hold, because the wage offer distribution is a function of the worker's productivity. In the introduction, we gave as an example the associate professor who receives a wage increase from her current employer because of a top-five publication. It is plausible that her wage offer distribution would also be affected by this publication. This is particularly problematic, because, in wage data, one frequently observes that worker's wage change without changing job. We deal with this issue empirically by using an IV approach, using the mean wage of the co-workers, following Bassier et al. (2022). This instrument can be justified by assuming that there is productivity shock *at the firm level*, which is *not* correlated to the worker's wage offer distribution. This assumption makes sense because the latter wage offer distribution is determined by other firms, but requires the absence of industry-wide productivity shocks. We deal with the later by controlling for sector and other firm level controls (e.g. firm size).

Space is homogeneous and endogenous job search costs are absent. It can be shown that the result also holds when space is not homogeneous, as shown by Van Ommeren et al. (2000). This result is intuitive, because the immediate utility associated with a job does not depend on the immediate utility offered by other jobs. The latter study also shows that it also holds given endogenous job search costs, as long as the job search costs are additive in the utility function. So, these two assumptions have little consequences for the estimation procedure.

Job moving costs are absent. The result still holds in the presence of job moving costs, provided these costs do not depend on the current wage or commuting costs (Van Ommeren et al., 2000). This assumption is slightly restrictive historically when the worker's pension depended on the latest wage, but nowadays in Denmark, particularly for younger workers, the assumption is nonrestrictive at all.

Job-to-job moves are voluntary and there are no (voluntary or involuntary) moves into unemployment or non-participation. It is assumed that all job-to-job moves are voluntary, but it is well known from the labor market literature that some workers move from job-to-job, because they know they will be fired soon. In this case, it is difficult to interpret job-to-job moves as entirely voluntary (see, for example, Sorkin (2018)). We relax this assumption here by assuming that the probability of making an involuntary job-to-job move is nonzero, but not related to the current wage or commuting distance. This assumption will not hold, for example, if workers with a high wage are more likely to be fired and to make involuntary job-to-job moves. It will also not hold, when applying the IV approach using mean wages of co-workers, when firms which pay high wages are more likely have a workforce which makes an involuntary job-to-job moves. It can be shown that allowing for (voluntary or involuntary) moves into unemployment or non-participation does not change the result (Van Ommeren et al., 2000).

The environment is stationary. The above result depends on the assumption that the environment of the worker is stationary. This is unlikely true for example because of business cycles, or other events that change over time (e.g., a change in policy), so the arrival rate of jobs may vary over time. In this case, expectations regarding future changes in job arrival rates start to play a role in the decision of workers to change job. Allowing for non-stationarity in the job arrival rate or the wage distribution does not affect the above result (Van Ommeren et al., 2000).

It is straightforward to show however that Eq. (2) does *not* hold when a worker expects changes in the cost of commuting, conditional on staying in the same job (Van Ommeren et al., 2000). The primary example is when a worker expects to move residence or have a baby. In that case, the ratio of the marginal effects on job mobility can be shown to be equal to the *expected* marginal cost of commuting, defined by $\mathbb{E}[MCC] \equiv -\frac{\partial V/\partial x}{\partial V/\partial w}$, which is also equal to αw, see Appendix A.2. Hence, there is a subtle difference in interpretation.

When workers expect to move residence, and therefore expect a change in the length of the commute, workers attach less value to their current commute, hence $\mathbb{E}[MCC] < MCC$. Note however that if the residential move is local – which applies to a large share of residential moves – so the (expected) change in commuting distance is small, then $\mathbb{E}[MCC] \approx MCC$.

In contrast, when workers expect to have a child, it is plausible that the commuting costs go up, so we expect that $\mathbb{E}[MCC] > MCC$. To examine whether the difference between the instantaneous and the expected monetary valuation of the commuting distance is important, we examine whether anticipation of residential moves or the childbirth plays a role for our results.

Non-wage amenities are absent. The above model assumes that non-wage amenities are absent. This is an extreme assumption, as we know that non-wage amenities play a role. This is particularly problematic because these non-wage amenities are likely correlated to the mean wages of co-workers, that are used as an instrument. We will deal with this in several ways. First, we will have a number of arguments why due to the institutional environment of Denmark, these non-wage amenities are less important in Denmark than in other countries. Second, more convincingly, we deal with this by controlling for a large number of firm level controls (e.g. number of workers in the firm, average age of the workers, and detailed sector controls) that are well known to capture a large range of non-wage amenities. Third, we add control variables for family friendliness, which capture unobserved non-wage amenities that are particularly attractive for young workers with children. Fourth, we add control variables for the value of non-wage amenities combined based on the work of Sorkin (2018) and Bagger and Lentz (2018). These control variables will be labeled as "Sorkin amenities" and "poaching controls".

5. Marginal cost of commuting: empirical application

In this section, we turn to the estimation of the marginal cost of commuting. The first two subsections show how the marginal cost of commuting can be estimated using our econometric approach which is supported by a graphical approach. Section 5.3 reports our main findings of estimating the marginal cost of commuting, Section 5.4 discusses the marginal cost of commuting time, and Section 5.5 presents robustness checks. Finally, in Sections 5.6 and 5.7, we discuss the implications of allowing for residential moves on estimating the marginal cost of commuting and we estimate the effect of commuting distance on the probability of leaving the labor market and moving into part-time employment.

5.1. Econometric approach

We aim to estimate the causal effects of wage and commuting distance on job mobility.[16] We employ a linear probability model, as in Manning (2003b), which offers two advantages. First, it estimates the average causal marginal effect (Angrist and Pischke, 2008, p.93), in which we are interested. Second, it allows us to include many fixed effects for a very large dataset, which is computationally cumbersome for the non-linear approaches, such as survival analysis and discrete choice models which have been applied in this context (Van Ommeren et al., 2000; Van Ommeren and Fosgerau, 2009).

We differentiate both effects by gender, g, and the presence of a child, c. One complication, as is common with annual data, is that we observe the commuting distance at the end of the year and the average wage of a worker per year. Consequently, in the year that the worker moves, the average wage is a combination of the before-the-move wage and after-the-move wage, which is problematic because we wish to know the effect of the before-the-move wage on job mobility. To deal with this, we define a job move in year t, when the actual move takes place the year after. Given this definition, we use a job moving dummy indicator $J_{i,t}$ which captures whether a worker, i in year, t, moves job. We then use the following linear probability model, to estimate the effects of log wage and commuting distance on job mobility:

$$J_{i,t} = \alpha_{g,c} \cdot x_{i,t} + \beta_{g,c} \cdot log(w_{i,t}) + \gamma \cdot X'_{i,t} + \delta_{g,c} + \lambda_i + \kappa_t + \varepsilon_{i,t}, \quad (3)$$

where our main interest is in the marginal effects of commuting distance, $x_{i,t}$ and log wage, $log(w_{i,t})$, which are captured by the coefficients $\alpha_{g,c}$ and $\beta_{g,c}$, respectively.[17] Importantly, $\alpha_{g,c}$ and $\beta_{g,c}$ are both gender

[16] This is not the first study that exploits information on job mobility to derive the marginal cost of commuting (Van Ommeren et al., 2000; Manning, 2003b; Van Ommeren and Fosgerau, 2009; Le Barbanchon et al., 2021). We make two fundamental contributions. First, we employ a large panel of workers over a long period, so we can identify the parameters of interest using worker fixed effects, whereas the previous studies essentially rely on strategies identifying parameters of interest without worker fixed effects. Second, we apply an instrumental variable approach inspired by Bassier et al. (2022) to deal with the issue that workers' wage offer distribution is unobserved and correlated to their current wage.

[17] We allow for differential gender effects in wages. Such a specification is in line with the labor economics literature, where there is a discussion to what extent the effects of wage on job mobility are gender specific, as these differences might be indicative of monopsony power by firms. A general finding in that literature is that these effects are very similar, see

and child-specific. We also include $\delta_{g,c}$, which is a gender and child interaction term, which allows job mobility to change over time for reasons not captured by commuting distance or wages. This is essential, as the literature has shown that wages discretely jump around the birth of a child, a characteristic which also holds in our data, suggesting that also other factors than only wage may discretely change.[18] We emphasize here that in the above equation, we do *not* include information about future job characteristics after a move (e.g. the commuting distance after moving), because any future job characteristic would be endogenous.

$X_{i,t}$ consists of a vector of additional controls, which includes marital status, broad sector controls (NACE 1), firm size, the average age of workers at the firm, and job tenure. We include worker λ_i and year κ_t fixed effects, and $\varepsilon_{i,t}$ is an idiosyncratic error term. Standard errors are clustered at the firm level. We emphasize that we include worker fixed effects to control for time-invariant worker characteristics. Consequently, we examine whether changes in the wage levels of workers affect their job mobility.

In the reviewed literature that aims to estimate the marginal cost of commuting, to deal with the endogeneity of wages, empirical approaches rely on identification by using control variables. Given that we include worker fixed effects this implies that changes in wages are not correlated to changes in the wage offer distribution. This assumption is unlikely to hold. For example, if we observe that a worker receives a higher wage while staying at the same job, it is plausible that the productivity of this worker has increased, and therefore the wage offer distribution of this worker also has changed.

To address this issue, following Bassier et al. (2022), we use an instrumental variable approach, where we use (log of) the average wage of *similar* workers that work at the same firm as an instrument, where similar is defined as belonging to the group of workers who have children during the observed time interval and who are in the same job position, where we distinguish between 7 broad job positions (e.g., manager). To be more precise, as we have 4 endogenous variables, i.e. the wage for 4 different groups (the interaction of gender and before and after having a child), we use 4 instrumental variables in the first stage (the average wage in the firm interacted with group).

The underlying idea of this instrument is that productivity improvements *at the firm level* reflect into individual workers' wage increases, which do not affect the wage offer distribution of this worker. These productivity improvements at the firm level should be contrasted with the productivity improvements at the individual level, which do affect the wage distribution of a worker. In order to argue that the average wage is exogenous, we take two steps: we exclude the wage of the worker and we only include firms with at least 10 workers, which refers to about 95% of all workers. By excluding small firms, we avoid the inclusion of workers who are owners of the firm rather than employees, or workers who are family members, for which the wage does not reflect market wages.

The underlying assumption to justify the IV approach is that the average wage of the firm does not directly affect individual job-moving decisions, except through its effect on the individual wage of the worker. To minimize the possibility that the average wage is correlated to the presence of unobserved non-wage amenities, we control for firm size and sector that are known to correlate to wages and non-wage amenities (Oi and Idson, 1999).

One may argue that also when we control for firm size and sectors, we do not fully address for the presence of non-wage amenities.[19] Arguably, there might be more subtle unobserved non-wage characteristics (e.g. training opportunities) that are relevant to workers and which correlate with the wage in the firm, and which are not picked up by our set of controls (e.g. flexible working times). To test for this, we will add controls, such as very detailed sector controls (NACE 3), average education, share of females and the presence of female top managers which proxies for amenities important to female workers with children.

In addition, we add "Sorkin amenities" and "poaching controls", based on the works of Sorkin (2018) and Bagger and Lentz (2018). Sorkin amenities are derived from a tool from numerical linear algebra to measure workflows between firms, which reveals the ranking of firms based on workers' choices. The intuition here is that if more workers move from firm A to firm B than the other way around, then firm B is, on average, preferred to firm A. This ranking can be interpreted as an index of the workers' value of the firm, mainly a combination of the wage and non-wage amenities. As we include wages, Sorkin amenities control for the value of non-wage amenities combined.

There are two issues with this approach. First, the ranking can only be derived for firms that are strongly connected, meaning that at least one worker has moved to, and at least one worker has left, the firm. Hence we can only use Sorkin amenities for a selected sample of workers (about 65% of the original sample of workers, mainly excluding workers employed at smaller firms). Second, as the firm's ranking is derived from job mobility data, it implies that Sorkin amenities are potentially endogenous when used as controls in our application. For that reason, we also instrument the Sorkin amenities using public ownership as an instrument, where we control for industrial sector. The identification assumption is here that in industrial sectors where both private and public organizations are active (mainly schooling and healthcare), the private organizations typically offer higher levels of compensation and non-wage amenities than public organizations.

Poaching controls also aim to measure non-wage amenities, and are based on the firm's share of workers that were not employed before moving to the firm. The idea is here that firms that are less attractive to workers are less likely to poach from other firms, and are therefore more likely to recruit workers that were previously not employed.[20]

Another issue with the instrument is that *job-level* wage increases that are driven by technological changes that are shared with other jobs in the same firm *and* in other firms.[21] In this case, the exclusion restriction would not hold. To address this, we use an alternative instrument, where we use the average wage of workers at the same

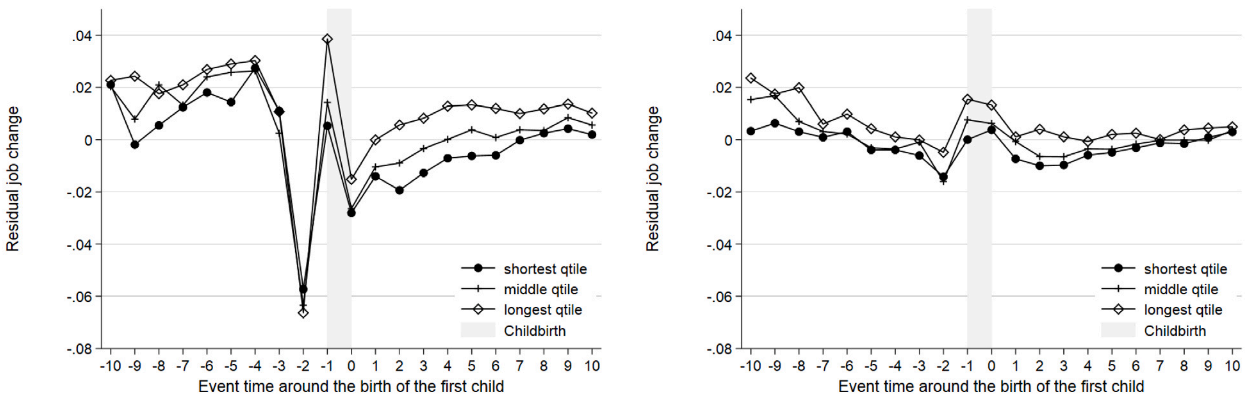
for example Manning (2003a) (an exception is Barth and Dale-Olsen (2009) that differentiates firms based on their gender composition, but not how firms differentiate between workers with a different gender).

[18] For example, we allow for the situation that women with children receive fewer job offers for unobserved reasons.

[19] According to the admittedly somewhat outdated US literature, non-wage amenities are hardly important to workers, except for pensions and health care (Turner, 1987). In Denmark, healthcare and pensions are mandatory (Gruber and Lettau, 2004), suggesting that non-wage amenities may not be relevant in our context. Nevertheless, this literature ignores other more recent non-wage amenities, such as childcare benefits, that are likely important for young workers with children. Company cars have been shown to be relevant as these fringe benefits are relevant to workers in other European countries (Gutiérrez-i-Puigarnau and Van Ommeren, 2011). We note here that studies for Denmark rule out the importance of these two types of non-wage amenities. In Denmark, company cars are hardly offered, as the Danish tax system offers little advantage of having a company car (Harding, 2014). Also, childcare is rarely supplied by firms as a fringe benefit. In Denmark, only 1%–2% of firms offer (paid) childcare or any additional childcare allowance (Galanaki and Papalexandris, 2012).

[20] We do *not* include firm fixed effects. In that case, one effectively uses differences in the average wage growth experienced by the same worker at different firms as an instrument of the wage change. The first-stage effect of the average wage is then close to zero, resulting in an instrument that is either weak or not robust to minor changes in specification.

[21] We are less worried about *firm-level* wage increases that are driven by technological changes that are shared with other firms, as our results remain robust given additional detailed sector controls.



(a) Women (b) Men

Fig. 3. Job changes by distance quantiles. *Notes:* We estimate a regression as in Eq. (3), but where we exclude commuting distance as an explanatory variable, i.e. $J_{i,t} = \beta_{g,c} \cdot log(w_{i,t}) + \gamma \cdot X_{i,t} + \delta_{g,c} + \lambda_i + \kappa_t + \varepsilon_{i,t}$. The figures display the estimated job mobility residuals $\hat{\varepsilon}_{i,t}$. The distance quantiles are calculated based on the average commuting distance before childbirth.

firm, who are in job positions that are *not* similar to the worker. Using this instrument, we get similar results, but with a somewhat larger marginal cost of commuting.[22] So our approach that relies on using the wages of workers in similar positions as an instrument is the more conservative estimate.

To challenge the instrumental variable approach, we also examine a range of alternative specifications. For example, we have also examined other specifications with other definitions of "similar workers". When we include older workers in the same job position, then the first-stage impact of the instrument becomes smaller, but the results remain robust. Finally, note that if the exclusion restriction does not strictly hold, then the bias in the estimates is unlikely large, as our instrument is strong. Here we use arguments developed by Conley et al. (2012), Angrist and Krueger (1991) and Bound et al. (1995) which show that the bias from violation of the exclusion restriction is negatively related to the strength of the instrument.

5.2. Graphical approach

To support our econometric specification, we have examined the effect of commuting distance on job mobility graphically for several distance quantiles definitions (e.g., 3 quantiles, 5 quantiles, etc.). Here, we control for worker fixed effects and the same controls used in our econometric approach, later on, so we show results for the job move *residuals*.[23] The results for these different quantiles definitions are very similar. In Fig. 3 we show job mobility for three distance quantiles, so we show the job mobility residuals for a group of workers with a short commuting distance, for a group with a long commuting distance, and for a third group which is in the middle with respect to commuting distance.

There are several messages in this figure. First and most importantly, workers in the long commuting distance quantile tend to change jobs more frequently, especially women with children. Second, there is an extreme drop in job mobility of females just before the birth, which is likely due to a combination of reasons, including the effect of a Danish law which states that if women announce that they are pregnant, they cannot be fired, which reduces the incentives to search for another job. Third, for women who (expect to) become pregnant in the year after, we do not observe that the job mobility residual is higher for those with a long commuting distance. One possible explanation is that those women realize that during maternity leave they will not commute at all.

5.3. Empirical results

Our main results using different specifications to identify the marginal cost of commuting by estimating Eq. (3) can be found in Table 2. As we have seen that job mobility around the childbirth is extremely volatile, especially for women, which may potentially affect the estimates of the econometric analysis, we exclude observations in the year before the birth, the year of the birth, as well as the year after the birth. All coefficients are estimated precisely and have expected signs. In the first five specifications, the wage is instrumented and it appears that all 4 instruments are very strong with high F-values and have the expected positive sign. For example, for the specification shown in column [1], the effect of the log average wage on the individual's log wage for females with children is about 0.35 (std. err. is 0.003), with an F-value equal to 9532.

In column [1], which is our preferred specification, it is shown that the effects of commuting distance on job mobility are very similar for men and women before they have children, with coefficients equal to 0.0014 and 0.0011, respectively. Hence, given a hypothetical increase of about one standard deviation in the length of the commute, which is equal to almost 12 km, job mobility rates increase by about 0.013–0.016. After the birth of the child, the estimated effect of distance is about the same for men and equal to 0.0015. For women, the estimated effect is about 0.0028, so about twice the estimated effect for their male counterparts, and almost 3 times the estimated effect for females without children. Consequently, given a hypothetical increase of about one standard deviation in the length of the commute, job mobility rates increase by about 0.034, which is very substantial given a job moving rate of 0.17 for this group. This supports our claim that gender differences regarding commuting play an important role after the birth

[22] This instrument also addresses the issue that the exclusion restriction will also fail if job mobility decisions of workers directly depend on the wages of co-workers in similar job positions, for example, because of jealousy. If these co-workers receive a raise in wages, and the worker does not, the worker could feel worse off and leave. Then the instrument would be invalid.

[23] Because we use controls, we apply the following two-step procedure. We first estimate a regression as in Eq. (3), but where we exclude commuting distance as an explanatory variable:

$$J_{i,t} = \beta_{g,c} \cdot log(w_{i,t}) + \gamma \cdot X_{i,t} + \delta_{g,c} + \lambda_i + \kappa_t + \varepsilon_{i,t}.$$

In the figure, we show the estimated residuals $\hat{\varepsilon}_{i,t}$.

Table 2
Job mobility.

Dep. var.: Job change		[1]	[2] Women	[3] Men	[4] No anticipated childbirth	[5] OLS
Distance (km)						
Women	No child	0.0011***	0.0011***		0.0015***	0.0010***
		(0.0001)	(0.0001)		(0.0002)	(0.0001)
	Child	0.0028***	0.0028***		0.0030***	0.0029***
		(0.0001)	(0.0001)		(0.0001)	(0.0001)
Men	No child	0.0014***		0.0014***	0.0013***	0.0013***
		(0.0001)		(0.0001)	(0.0001)	(0.0001)
	Child	0.0015***		0.0015***	0.0015***	0.0015***
		(0.0001)		(0.0001)	(0.0001)	(0.0001)
Log. wage						
Women	No child	−0.374***	−0.367***		−0.343***	−0.099***
		(0.026)	(0.041)		(0.028)	(0.004)
	Child	−0.171***	−0.168***		−0.166***	−0.039***
		(0.021)	(0.036)		(0.023)	(0.002)
Men	No child	−0.319***		−0.316***	−0.331***	−0.085***
		(0.024)		(0.025)	(0.025)	(0.004)
	Child	−0.178***		−0.174***	−0.187***	−0.028***
		(0.023)		(0.025)	(0.025)	(0.003)
F-statistic for IV						
Women	No child	7,858	14,532		5,470	
	Child	9,532	12,120		8,659	
Men	No child	8,464		15,404	6,238	
	Child	10,878		14,748	9,677	
Wage instrumented		Yes	Yes	Yes	Yes	No
Time-variant controls		Yes	Yes	Yes	Yes	Yes
Age and educ. controls		No	No	No	No	Yes
Worker fixed effects		Yes	Yes	Yes	Yes	Yes
Year fixed effects		Yes	Yes	Yes	Yes	Yes
No. of observations		2,243,915	1,136,004	1,107,911	2,086,894	2,243,915
Marginal cost of commuting (% of annual wage) per 12 km increase (1 std. dev.)						
Women	No child	−0.035	−0.037		−0.051	−0.120
		(0.005)	(0.006)		(0.007)	(0.016)
	Child	−0.199	−0.203		−0.218	−0.893
		(0.026)	(0.044)		(0.031)	(0.066)
Men	No child	−0.053		−0.053	−0.049	−0.179
		(0.006)		(0.006)	(0.006)	(0.018)
	Child	−0.100		−0.103	−0.097	−0.617
		(0.014)		(0.016)	(0.014)	(0.087)
Average job change		0.1675	0.1637	0.1714	0.1698	0.1675

Notes: The sample consists of full-time workers. We exclude observations in the year before the birth, the year of the birth, as well as the year after the birth. All specifications include the following controls: marital status, job tenure in linear and squared form, number of workers in the firm, the average age of workers at the firm, and gender and child interaction term. Log wage is instrumented using the average wage of similar workers of the same firm. MCC is estimated using the ratio of the marginal effect of commuting distance on job mobility and the marginal effect of log wage on job mobility, see Eq. (2). Standard errors are clustered at the firm-year level and can be found in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

of the first child. *Women who have a child are much more likely to leave their job when they have a long commute, which is not true for men*. This result is novel to the literature, as previous studies speculated about this effect, but failed to show this, see e.g. Van Ommeren and Fosgerau (2009).

Focusing on the same column, it appears that the effects of log wage on job mobility are negative for all groups. It appears that (i) the wage effects are hardly gender specific, but (ii) differ whether or not the worker has a child. The former finding is of interest to the labor economics literature, where it is hypothesized that employers have more monopsony power over women than men suggesting that the wage effect differs between men and women. More specifically, it hypothesizes that the wage effect of women is smaller in absolute terms (Manning, 2003a; Barth and Dale-Olsen, 2009). Clearly, our data do not confirm this hypothesis (and this specification even suggests that the effect for women is slightly larger in absolute terms).

It appears that the effect of log wage on job mobility is approximately the same for males and females, but varies with having a child. We will show later on that the latter interpretation is spurious, i.e. there is no evidence that effect of wage discretely jumps when the worker becomes a parent. The wage effect seemingly depends on having a child, because the wage effect becomes less pronounced with the age of the worker (which, by design, is positively correlated with having a child).

Around the birth, the effect of log wage is about −0.25. This estimate implies that a 10% increase in the current wage decreases the job mobility rate by roughly 0.025, which is about 15% of the mean job mobility rate of 0.17. The order of magnitude of this estimate seems to make sense intuitively. For example, it suggests that a doubling of the wage in the current job would prevent workers from leaving voluntarily ($0.17 - ln(2) \times 0.25 \approx 0$). These estimates imply job moving elasticities with respect to the wage of about −1.5 ($0.25/0.17$), which is slightly higher than the estimates obtained by Barth and Dale-Olsen (2009) for workers in the manufacturing industry in Norway (using a different methodology with different types of instruments), but somewhat lower than those obtained by Bassier et al. (2022).

We are particularly interested to derive the marginal cost of commuting, i.e. the marginal willingness to pay for a (one-way) commuting

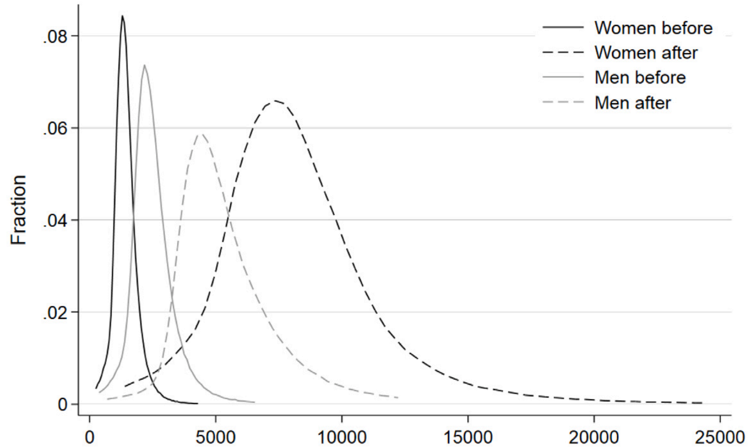


Fig. 4. Distribution of the Marginal Cost of Commuting (MCC) per 12 kilometers in DKK. *Notes:* The marginal cost of commuting MCC per 12 kilometers in DKK has been computed using the estimated coefficients from model [1] in Table 2 and the observed distribution of annual wage.

distance of 1 km, using Eq. (2). The results for MCC are shown in the panel below the estimated coefficients. To improve interpretation, we focus here on a one-way commuting distance increase of 12 km, approximately the standard deviation of commuting distance.

Our headline results, using Eq. (2) and our estimates of column [1] of Table 2, demonstrate that for men and women before having a child, the MCC given a 12 km increase of commuting distance is about 3%–5% of the wage. When having a child, the MCC given a 12 km increase is substantially higher for women and equal to 20% of the wage. In comparison, for men, the MCC rises to 10% of their wage. This finding is in line with the idea that (full-time) women with children often have more childcare and household responsibilities than men (Daly and Groes, 2017), hence their marginal dis-utility of commuting will be higher. Clearly, the estimated marginal costs of commuting are very similar across different model specifications shown in Table 2.

Our assumption that utility is additive in the logarithm of wages and commuting implies that MCC is proportional to the wage, see Eq. (2), hence our estimate implies that there is a distribution of marginal commuting costs. Fig. 4 shows the estimated distributions of the *annual* marginal commuting costs per 12 kilometers (in DKK), using the estimated coefficients from model [1] in Table 2 and the distribution of annual wage. It shows that the MCC distributions are similar for women and men before they have children, with a mean of about 1400 DKK and 2500 DKK, respectively. For women, the distributions before and after having a child are quite different: after the birth of the child, the MCC distribution for women shifts to right with the mean of about 8200 DKK (about USD 1200). For men after the birth of the child, the MCC distribution shifts to right with the mean of about 5400 DKK (about 770 USD).

We show now that in all other specifications (including ones not presented), women with children have a much higher MCC. One alternative specification is motivated by the labor economics literature, where it is hypothesized that employers have more monopsony power over women than men suggesting that the wage effect may differ between men and women, which has not been substantiated by empirical research (Manning, 2003a; Barth and Dale-Olsen, 2009). To allow for this possibility, we re-estimate the model separately for women and men, see columns [2] and [3]. We note that the effects of commuting distance and wage are essentially identical to those of column [1]. The main result that the loss in MCC is substantially higher for women with children is also supported by this specification.

In the next column of the table, we estimate model for more selective samples. In Section 4.2, we have explained that the interpretation of the marginal cost of commuting as defined by Eq. (2) changes if workers expect to have a child after accepting a new job because the estimate refers then to the expected marginal commuting costs, $\mathbb{E}[MCC]$ which may differ from the MCC.

As we do not observe the expectations of households, we investigate this issue by making additional assumptions about their expectations. We assume that households have perfect information in the near future, defined here as less than three years, and are completely myopic about the far future, defined here as more than three years. If one then excludes observations of households for an interval of three years before having a child, then one essentially has a sample of households who do not expect to have a child.

In column [4], we focus on a subsample of workers who did not anticipate of having a child. We find that the estimates of the wage effect are very similar, but the effect of the commuting distance of women without children is somewhat higher, but still substantially below the marginal cost of commuting for women with children. One possible reason why we find that the MCC for women who do not expect children exceeds the $\mathbb{E}[MCC]$ for women who expect children, is that the latter do not expect to commute during the maternity leave period, which is typically 12 months in Denmark. This interpretation is supported by Fig. 3 which shows that women just before they get pregnant are hardly sensitive to the length of the commuting distance. It could also be that women who expect children expect to change residence, so they do not care as much about the current commute. We address this issue later on.

In column [5], we show a specification where we do not instrument the wage. It appears now that the estimated effect of wage is substantially lower, suggesting that workers are hardly sensitive to wage increases, inconsistent with the monopsony literature. This specification is clearly biased because it does not account for the fact that a wage increase also shifts the worker's wage offer distribution.[24]

5.4. The marginal cost of commuting time

In this study, we estimate commuting costs using commuting distance. The main advantage of using commuting distance over commuting time, as used in the literature, is that distance does not depend on

[24] We have also estimated specifications where we do *not* control for worker fixed effects, but control for a range of other variables including age, gender, and education. It appears that the effect of distance is robust. In contrast, although the instrument is very strong, it appears that there is a positive effect of the wage on job mobility, which does not make sense from an economic point of view. Clearly, the instrument is invalid without worker fixed effects, because of worker sorting.

the mode of transport, which is endogenously chosen. However, it also has a disadvantage as it does not directly give insight into the marginal cost of commuting *time* (rather than distance), which may be either expressed in terms of (leisure) time lost or in monetary terms, which are also useful measures.

To calculate the marginal cost of commuting time, we have to make additional assumptions. We assume that workers commute daily between their residence and workplace without combining these trips with other activities (e.g., dropping children at school, which may reduce effective commuting time) and that full-time workers work 7.4 h per day, in line with other studies. Furthermore, we need to have information about the effect of a marginal increase in commuting distance on commuting time. To derive the latter, we use the Danish National Travel Survey (NTS), which provides information on the commuting behavior of about 80,000 randomly selected individuals who fill out a one-day travel diary.

For the population of young workers we are interested in, the marginal effect of distance on one-way commuting time (in hours) is about 0.025, see Appendix C.[25] This estimate implies, given i.e. a 40 km increase, the (one-way) commuting time increases exactly by one hour, which makes sense. It follows that the marginal effect of distance on *daily* commuting time is about 0.050. The implied MCC for one hour of commuting per day before childbirth is then about half the wage, i.e., 43% and 65% of the hourly wage for women and men without children, respectively.[26] For female workers with children, the MCC for commuting time is substantially higher, about 2.25 times the hourly wage, while for man with children it is about 1.23 times the hourly wage, i.e., it exceeds the hourly wage for both. We have assumed that workers commute each day. Note that given the, maybe more plausible, assumption that workers do not commute to work one day a week, e.g. because of working from home or because of a business trip, then the MCC for commuting time is about 25% higher.

How do these estimates compare with the literature? Note that in most previous studies (Ophem, 1991; Van Ommeren and Fosgerau, 2009; Manning, 2003b), commuting time rather than commuting distance was used as a proxy for commuting costs, so one can only compare with our implied commuting time estimates. Nevertheless, it appears that our implied estimates of MCC for commuting time are somewhat lower than the estimates obtained in those studies. Arguably, the estimated coefficients of log wage were downward biased in the previous studies, as is also concluded in the monopsony literature.

We are aware of only two studies that also use distance. Van Ommeren et al. (2000) finds roughly the same point estimate, but the confidence interval of this estimate is very large, so their point estimate must be interpreted as suggestive only. Le Barbanchon et al. (2021) focus on the commuting patterns of currently unemployed workers in a stated-preference setting. They do not find any effect of having children on the marginal cost of commuting using a structural job search model, but they do not interpret the presence of children as causal. However, they do find that the reservation commute distance is significantly shorter for women with children. Important for the current study which focuses on the role of children and gender, the current study is the first revealed-preference study that is able to differentiate between the MCC for men and women and demonstrates the importance of the presence of children with precisely estimated point estimates.

5.5. Sensitivity analysis

We have performed several sensitivity analyses of our preferred specification [1] of Table 2. First, we have also applied an event time methodology, where we let the distance coefficients vary per year. Second, we examine the importance of additional firm level controls, in particular for non-wage amenities, to examine the robustness of using our firm level instrument. Third, we examine a range of alternative specifications, including the non-linear effect of distance.

5.5.1. Event time methodology results

In the previous analyses, we have assumed that the estimated distance coefficients discretely jumps after the birth of the first child, implicitly assuming that these coefficients do not vary over time otherwise. To investigate this further, we also estimate models that exploit an event time methodology, i.e. we re-estimate our preferred specification, but we allow the (gender-specific) distance coefficients $\alpha_{g,j}$ to vary over time, i.e. these coefficients vary by year j relative to the event of the birth. Thus, we estimate:

$$J_{i,t} = \sum_j \alpha_{g,j} \cdot x_{i,t} + \beta_{g,c} \cdot log(w_{i,t}) + \gamma \cdot X'_{i,t} + \delta_{g,c} + \lambda_i + \kappa_t + \varepsilon_{i,t}, \tag{4}$$

where we instrument $log(w_{i,t})$.

In Fig. 5, we show the estimated distance coefficients for men and women around the year of the birth. It clearly shows that the coefficients for men are very similar for the different years before and after the event. Additionally, the coefficients for women are indistinguishable from the male coefficients before the childbirth but jump discretely after childbirth. Consequently, we believe that the jump in the coefficients for women when they have a child supports our methodology, and therefore our findings.

In contrast, there are no good reasons to believe that the effect of wages will also jump discretely around birth. This is supported by our data. To test whether the change in the level of the wage coefficient is due to a discrete jump around the childbirth or that it varies continuously over event time, we interact the instrumented wage with event time (21 year dummies) and gender. To estimate 42 interactions of the same instrumented variable, we use the procedure discussed in Bun and Harrison (2019), and applied in Levkovich et al. (2020), which imposes restrictions in the first stage, by assuming that the relationship between wages and the instrument does not depend on event time, which makes the procedure more efficient. This means that we assume that the pass-through of increases in co-workers' average wages at the firm to ones own wage is independent of the event time or gender. Fig. 6 shows the results from this procedure. It shows that the effect of wage slowly changes over event time, without showing a discrete jump around the birth of the first child.

5.5.2. Additional firm level control variables

In our IV approach, we use as an instrument the average wage (of similar workers) within the firm. In these estimations, we control for firm size as well as the average age of the workers belonging to the firm to avoid the criticism that the average wage has a direct effect on individual wages. Nevertheless, the main criticism of this estimation procedure is that we do not control sufficiently for firm characteristics, including non-wage amenities, which may invalidate the instrument if these firm characteristics are correlated to the instrument and affect job mobility directly.

[25] According to the speed literature, the effect of travel distance on travel time is diminishing, because the marginal increase in travel time is less for longer distances, see, for example, Couture et al. (2018). In line with that, we estimate the marginal effect of distance on travel time using a log–log specification, see Table C.2 in Appendix C. We find a coefficient of 0.58, almost identical to the estimates reported for the United Kingdom by Van Ommeren and Dargay (2006). For this specification, the average marginal effect is equal to the product of the estimated coefficient and the average inverse speed (the ratio of travel time and travel distance). Given an estimate of 0.58 (see Table C.2) and an average inverse speed of about 0.043 (see Table C.1), it appears that the average marginal effect is 0.025.

[26] For example, given our estimates of column [1] of Table 2, the MCC (per km) for man before the childbirth is about 0.0044 (−0.053/12) of the daily wage. The MCC for one hour of commuting per day is then 0.088 (0.0044/0.050) of the daily wage, as the marginal effect of distance on daily commuting time is 0.050. Given the typical number of hours worked per day (7.4), the MCC for one hour of commuting per hour worked is about half the hourly wage (7.4 × 0.068 = 0.65).

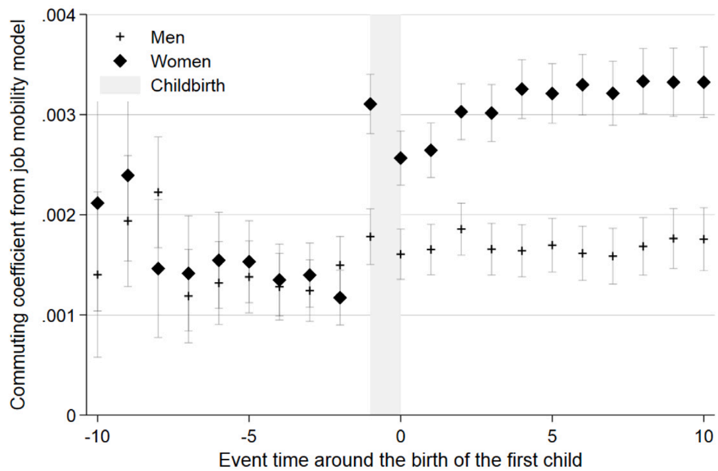


Fig. 5. Job mobility: commuting distance coefficients. *Notes:* Estimated coefficients of commuting distance on job mobility around the birth of the first child when including worker fixed effects and other controls. The gray area marks the birth of the first child. Standard errors are clustered by firm and year.

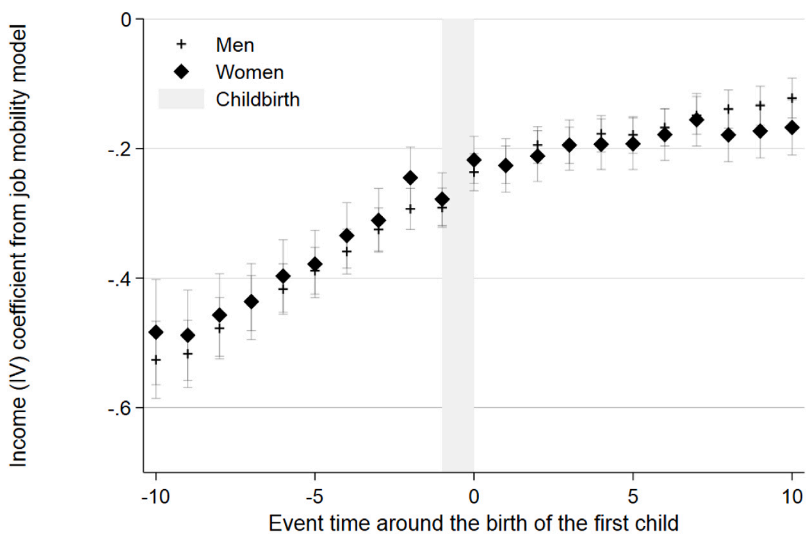


Fig. 6. Event time specific wage effects. *Notes:* Wage (IV) event time effects around the birth of the first child when including worker fixed effects and other controls. The gray area marks the birth of the first child. Standard errors are clustered by firm and year.

To address this issue, we have estimated several model specifications with additional firm-level control variables. First, we add more detailed sector controls, average education shares and region dummies which aim to control for the confounding bias of unobserved firm characteristics. Second, we control for the share of female workers and a proxy for family friendliness, which aim to control for the confounding bias of unobserved characteristics that tend to be appreciated by female workers with children. Third, we control for Sorkin amenities and a poaching index which capture non-wage amenities. These results are shown in Table 3.

Controlling for detailed sector levels is potentially important, as it has been known for many years that wages are structurally higher in certain sectors, whereas there are also substantial job mobility differences between sectors. If sectoral wage differences and sectoral job mobility differences are correlated, the instrument would be invalid. To address this, we add additional controls for sectors at NACE 3 (272 sectors), as shown in column [1]. The effects of commuting distance remain the same, whereas the effect of wage is slightly less pronounced. Consequently, the estimates are rather insensitive to sector controls, even when we control for sector in a very detailed way.

Similarly, adding controls for the share of workers with a certain educational level or the share of female workers results in almost identical results (see columns [2] and [3]). Finally, in column [4], we follow Kleven et al. (2019a) and include a proxy variable to measure the family friendliness, which is based on whether the management team includes women with young children (under 15 years of age). This aims to proxy many non-wage amenities, such as tolerance for taking sick days off, flexible working hours, and the option of working remotely. Again the overall estimation results remain unchanged. In conclusion, it appears that for all these additional specifications, the effects of commuting distance and wage are robust, reducing the likelihood that our effects are confounded by unobserved non-wage amenities.

Table 3
Job mobility models: Additional firm level control variables.

Dependent variable: Job change		[1] Sector FE NACE 3	[2] Education	[3] Share men	[4] Family friendliness	[5] Poaching index	[6] Sorkin index
Distance (km)							
Women	No child	0.0011***	0.0011***	0.0011***	0.0011***	0.0010***	0.0010***
		(0.0001)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0002)
	Child	0.0028***	0.0029***	0.0029***	0.0028***	0.0028***	0.0029***
		(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Men	No child	0.0014***	0.0014***	0.0014***	0.0014***	0.0014***	0.0011***
		(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)
	Child	0.0015***	0.0015***	0.0015***	0.0015***	0.0015***	0.0015***
		(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Log. wage							
Women	No child	−0.312***	−0.360***	−0.336***	−0.374***	−0.430***	−0.396***
		(0.028)	(0.026)	(0.027)	(0.026)	(0.029)	(0.037)
	Child	−0.135***	−0.161**	−0.138***	−0.171***	−0.212***	−0.188***
		(0.023)	(0.022)	(0.022)	(0.021)	(0.024)	(0.031)
Men	No child	−0.266***	−0.307***	−0.286***	−0.319***	−0.353***	−0.312***
		(0.024)	(0.024)	(0.024)	(0.024)	(0.027)	(0.035)
	Child	−0.143***	−0.166***	−0.145***	−0.178***	−0.217***	−0.174***
		(0.025)	(0.024)	(0.024)	(0.023)	(0.027)	(0.034)
F-statistic for IV							
Women	No child	7,526	7,827	7,711	7,857	7,044	5,573
	Child	9,542	9,514	9,527	9,531	8,699	5,976
Men	No child	8,475	8,410	8,388	8,461	7,393	5,276
	Child	10,320	10,863	10,798	10,880	9,606	6,915
Controls		Yes	Yes	Yes	Yes	Yes	Yes
Worker fixed effects		Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects		Yes	Yes	Yes	Yes	Yes	Yes
No. of observations		2,243,915	2,243,915	2,243,915	2,243,915	2,034,809	1,506,226
		Marg. cost of comm. (% of annual wage) per 12 km increase (1 std. dev.)					
Women	No child	−0.043	−0.036	−0.039	−0.035	−0.029	−0.029
		(0.006)	(0.005)	(0.005)	(0.005)	(0.004)	(0.006)
	Child	−0.251	−0.213	−0.249	−0.199	−0.157	−0.183
		(0.044)	(0.030)	(0.041)	(0.026)	(0.019)	(0.031)
Men	No child	−0.065	−0.055	−0.059	−0.053	−0.048	−0.044
		(0.008)	(0.006)	(0.007)	(0.006)	(0.005)	(0.007)
	Child	−0.124	−0.106	−0.122	−0.100	−0.086	−0.103
		(0.022)	(0.016)	(0.021)	(0.014)	(0.012)	(0.022)

Notes: The sample consists of full-time workers. All specifications include the following controls: marital status, job tenure in linear and squared form, number of workers in the firm, average age of workers at the firm, and gender and child interaction term. Specification [4] in addition also includes control for family friendliness, i.e. whether the firm's management includes women with young children (below 15 years of age). Poaching and Sorkin indexes are defined and discussed in Section 5.5.3. Log wage is instrumented using the average wage of similar workers of the same firm. MCC is estimated using the ratio of the marginal effect of commuting distance on job mobility and the marginal effect of log wage on job mobility, see Eq. (2). Standard errors are clustered at the firm-year level and can be found in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Next, similar to Bassier et al. (2022), we estimate specifications that incorporate measures that aim to capture unobserved non-wage amenities. We first include the poaching rank index developed by Bagger and Lentz (2018), see column [5]. The effect of wages appears to be slightly higher in absolute terms, resulting in slightly lower values of MCC. We then control for Sorkin amenities (Sorkin, 2018). Note that we can only estimate these amenities for a subset of strongly connected firms. To be in this set, a firm has to both hire a worker from, and have a worker hired by a firm in the strongly connected set. About 75% of workers are in the strongly connected set. Fig. B.4 in Appendix B shows histogram of the estimated Sorkin amenity index. The results change slightly when controlling for these amenities, see column [6], but it appears that this change is entirely due to having a different subset of firms, and not because of this additional control, see column [5] of Table B.2 in Appendix B. Consequently, these results reinforce our main conclusion that the MCC is substantially higher for female workers with a child.

5.5.3. Alternative specifications

We have also investigated whether the effect of distance on job mobility is linear by including the square of the (demeaned) distance, see Table B.1 in Appendix B. It appears that linearity is a reasonable assumption for our data.

We have also estimated a range of other alternative specifications. First, we have estimated a specification where we add household fixed effects, so we additionally control for unobserved time-invariant household characteristics. This essentially means that we identify the effects of interest by comparing the behavior of men and women within the same household (i.e. a husband and wife). The results reported in column [1] in Table B.2 of Appendix B demonstrate that the MCC results are almost identical by including these additional controls.

Second, we have investigated the robustness of the results using a specification that appears in the literature, by including household fixed effects rather than worker fixed effects. See column [2] in Table B.2. Again, we find that the effects of commuting distance remain the same. However, the impact of wage is notably reduced in absolute terms, leading to substantially higher values of MCC. Hence, in conclusion, it appears that the effects of commuting distance are robust to the methodology used, whereas the effect of wage is *not* and depends on the methodology used. It appears essential to use worker fixed effects.

Column [3] includes controls for occupational rank and management status. Note that these controls are potentially endogenous, as changes in occupational rank are frequently closely linked to changes in wages. Nevertheless, also controlling for these factors, the results remain unchanged. In the next column, we address the issue that

Table 4
Job mobility model: competing risks models.

Dep. var.: Transition from full-time job			[1] Transition to part-time job	[2] Transition to full-time	[3] Transition to non-employment
Distance (km)					
	Women	No child	0.0001**	0.0010***	0.00004
			(0.00004)	(0.0001)	(0.00003)
		Child	0.0005***	0.0026***	0.00002
			(0.00003)	(0.0001)	(0.00002)
	Men	No child	0.00001**	0.0014***	0.000003
			(0.00004)	(0.0001)	(0.000002)
		Child	0.0002***	0.0014***	0.000006
			(0.00004)	(0.0001)	(0.000008)
Log. wage					
	Women	No child	−0.024**	−0.338***	−0.0004
			(0.009)	(0.027)	(0.0002)
		Child	−0.010	−0.146***	−0.0002
			(0.007)	(0.022)	(0.0001)
	Men	No child	−0.040***	−0.269***	−0.0002*
			(0.008)	(0.024)	(0.0001)
		Child	−0.020***	−0.145***	−0.0002
			(0.008)	(0.024)	(0.0001)
F-statistic for IV					
	Women	No child	6,711	7,618	6,598
		Child	8,356	9,311	8,251
	Men	No child	6,858	8,322	6,774
		Child	9,358	10,714	9,228
Wage instrumented			Yes	Yes	Yes
Time-variant controls			Yes	Yes	Yes
Worker fixed effects			Yes	Yes	Yes
Year fixed effects			Yes	Yes	Yes
No. of observations			1,833,871	2,196,983	1,799,631
	Marginal cost of commuting (% of annual wage) per 12 km increase (1 std. dev.)				
	Women	No child	−0.056	−0.034	−0.142
			(0.029)	(0.005)	(0.061)
		Child	−0.577	−0.211	−0.136
			(0.426)	(0.033)	(0.124)
	Men	No child	−0.028	−0.061	−0.016
			(0.013)	(0.007)	(0.012)
		Child	−0.107	−0.115	−0.032
			(0.045)	(0.020)	(0.038)
Average change			0.0156	0.1545	0.0083

Notes: The sample consists of full-time workers. We exclude observations in the year before the birth, the year of the birth, as well as the year after the birth. All specifications include the following controls: marital status, job tenure in linear and squared form, number of workers in the firm, the average age of workers at the firm, and gender and child interaction term. Log wage is instrumented using the average wage of similar workers of the same firm. MCC is estimated using the ratio of the marginal effect of commuting distance on job mobility and the marginal effect of log wage on job mobility, see Eq. (2). Standard errors are clustered at the firm-year level and can be found in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

controlling for Sorkin amenities is only possible for a subset of firms, we re-estimate the model for this subset while not controlling for Sorkin amenities, which facilitates interpretation of the results with Sorkin amenities. Finally, the last column shows that including region fixed effects (5 regions) does not affect the estimation results.

Finally, we investigate further our source of identification. In essence, we rely on an instrument observed at the firm level, the firm's average wage (while including worker fixed effects). This instrument differs between firms but also varies over time within firms. The variation in the instrument between firms is much larger than the variation within firms.[27] This suggests that a between-firms analysis – i.e. an analysis where for each firm, the (time-invariant) average of each variable is used – generates similar results. This is exactly what we find. We refer to Table B.3 in Appendix B for details.

5.6. Part-time work and labor market participation

Above we have estimated models for full-time workers, where we examine the effect on moving to another job, which may be either full-time or part-time. The distinction between full-time and part-time is potentially relevant, as Fig. B.1 in Appendix B shows that the probability of moving into part-time jobs is systematically higher for women compared to men, except in the years immediately before the childbirth. We therefore estimate competing risks models, allowing workers to move either into full-time or part-time jobs.

The results, reported in the first two columns of Table 4, demonstrate that the marginal effects of moving into a part-time job are an order of magnitude lower than those moving into full-time jobs. To interpret this marginal effect, it is important to realize that only about 10% of the job moves are into part-time positions. If we multiply the reported coefficients by a factor of 10 to compare the effect on the probability of moving relative to the mean moving rate, it appears that the wage effects are similar in magnitude. Additionally, the commuting distance effects for women with children are almost twice as high compared to previous estimates, suggesting an even higher marginal cost of commuting for this selected group. This makes sense as female workers with a high value of time are likely more sensitive to commuting distance and long working hours.

An important finding in the literature is that the effect of commuting times on labor participation, which varies across cities and depends on residential location within the city, differs between men

[27] The within-firm variance is 0.03, while the between-firms variance is 0.09.

Table 5
Job mobility model: no anticipation of residential moving.

Dep. var.: Job change			[1] No anticipated residence move 1 year	[2] No anticipated residence move 2 years	[3] No anticipated residence move 3 years	[4] Control for residential move
Distance (km)						
	Women	No child	0.0010***	0.0011***	0.0011***	0.0011***
			(0.0001)	(0.0002)	(0.0002)	(0.0001)
		Child	0.0029***	0.0030***	0.0031***	0.0029***
			(0.0001)	(0.0002)	(0.0002)	(0.0001)
	Men	No child	0.0015***	0.0015***	0.0015***	0.0014***
			(0.0001)	(0.0002)	(0.0002)	(0.0001)
		Child	0.0015***	0.0015***	0.0015***	0.0015***
			(0.0001)	(0.0001)	(0.0001)	(0.0001)
Log. wage						
	Women	No child	−0.304***	−0.287***	−0.278***	−0.353***
			(0.032)	(0.034)	(0.034)	(0.031)
		Child	−0.098***	−0.091***	−0.084**	−0.123**
			(0.026)	(0.027)	(0.027)	(0.025)
	Men	No child	−0.250***	−0.230***	−0.256***	−0.284***
			(0.029)	(0.030)	(0.031)	(0.028)
		Child	−0.126***	−0.118***	−0.108***	−0.151***
			(0.029)	(0.030)	(0.030)	(0.028)
Residential move						−0.009***
	within 3 years					(0.001)
F-statistic for IV						
	Women	No child	6,521	5,822	5,413	7,591
		Child	9,532	9,053	8,721	10,330
	Men	No child	7,028	6,150	5,666	8,203
		Child	11,274	10,585	10,173	12,195
Wage instrumented			Yes	Yes	Yes	Yes
Time-variant controls			Yes	Yes	Yes	Yes
Worker fixed effects			Yes	Yes	Yes	Yes
Year fixed effects			Yes	Yes	Yes	Yes
No. of observations			1,617,741	1,503,497	1,436,361	2,243,915
Marginal cost of commuting (% of annual wage) per 12 km increase (1 std. dev.)						
	Women	No child	−0.038	−0.047	−0.049	−0.037
			(0.007)	(0.009)	(0.009)	(0.006)
		Child	−0.352	−0.399	−0.436	−0.282
			(0.094)	(0.119)	(0.143)	(0.059)
	Men	No child	−0.071	−0.080	−0.081	−0.061
			(0.010)	(0.013)	(0.013)	(0.008)
		Child	−0.142	−0.153	−0.170	−0.121
			(0.034)	(0.039)	(0.048)	(0.023)

Notes: The sample consists of full-time workers. We exclude observations in the year before the birth, the year of the birth, as well as the year after the birth. All specifications include the following controls: marital status, job tenure in linear and squared form, number of workers in the firm, the average age of workers at the firm, and gender and child interaction term. Log wage is instrumented using the average wage of similar workers of the same firm. MCC is estimated using the ratio of the marginal effect of commuting distance on job mobility and the marginal effect of log wage on job mobility, see Eq. (2). Standard errors are clustered at the firm-year level and can be found in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

and women, at least in the US (Black et al., 2014; Farré et al., 2022). To examine this further in the context of our model, we use the same set of full-time workers but now focus on the effects of commuting distance and wages on the probability of moving to non-employment. Despite very small standard errors, we do not find any (statistically significant) evidence that Danish women, conditional on having a full-time job, are more likely to move into non-participation when they have longer commutes. This suggests that moves into non-employment are predominantly involuntary, and that the probability of being fired does not depend on commuting distance. The conclusion that moves into non-employment are predominantly involuntary is supported by Fig. B.1 in Appendix B, which shows that most workers who quit into non-employment (or part-time employment) later return to full-time employment. However, another interpretation is that the estimates only statistically insignificant, because of a lack of data, and therefore a relatively large standard errors, consistent that very few workers move into non-employment, as the annual moving rate into non-employment is only 0.0083. If we increase the wage of women without children with 10%, and we use the point estimate of 0.0004, then accordingly, the mobility rate into non-employment reduces by 0.5%, which is a meaningful change. For other groups, the suggested effect is in the order of 0.25% of the mobility rate into an unemployment.

5.7. Anticipation of residential moving

At the end of Section 3, we demonstrated that gender differences in commuting distance after the childbirth are predominantly due to gender differences in job mobility rather than residential mobility. However, as emphasized in Section 4.2, the interpretation of marginal commuting costs, as defined by Eq. (2), changes when workers anticipate relocating after accepting a new job. In this situation, the expected marginal commuting costs, $\mathbb{E}[MCC]$, become the relevant measure for workers and may differ from the current MCC. For instance, if a worker expects to move closer to their new job to reduce the length of the commute, the expected commuting costs will be less than the current commuting costs.

To investigate this further, we will make specific assumptions about expectations regarding residential moves. To be more specific, we will assume that households have perfect information about their residential moving behavior in the near future but are fully myopic about their behavior after that. The "near future" will be defined alternatively as one year, two years, or three years. Based on this definition, by excluding observations of workers within one, two, or three years before a residential move, we are left with a sample of workers who know they will not move in the near future and who ignore future residential moves. Consequently, $\mathbb{E}[MCC] = MCC$.

In Table 5, we address the anticipation of residential moves. In columns [1-3], we examine subsamples of workers who did not anticipate moving residences within 1, 2, and 3 years, respectively. We find that the estimates of commuting distance are hardly affected, despite removing a substantial share of the data. If anything, the effect for women with children becomes somewhat larger.[28] Finally, in column [4], rather than making selections, we use the full sample and control for residential moves within the next three years. Note that this control is potentially endogenous, as a job move may trigger a residential move, see e.g. Gutiérrez-i-Puigarnau et al. (2016). However, even after accounting for anticipated residential moving behavior, the results remain robust.

6. Conclusion

A large literature shows that the gender wage gap strongly increases after the birth of the first child. We provide complementary analyses of the role of the birth of the first child on gender differences in commuting distance as well as in preferences for commuting distance using administrative register data for the full working population in Denmark.

Employing the childbirth as an event for identification, we demonstrate that women with children are much more likely to leave their job when they have a long commute – the marginal effect of distance on job mobility is almost three times higher – which is not true for their male counterparts with children. Furthermore, we apply an IV approach to estimate the effect of wages on job mobility. Employing a dynamic search model, these results imply that the marginal cost of commuting increases substantially for women after the birth of the first child. A 12-kilometer increase in commuting distance induces costs equivalent to about 20% of wages for women with children. Consequently, women with children bear a higher cost of commuting.

Our findings are consistent with the notion that gender differences in the costs of commuting are important as argued, for example, by Le Barbanchon et al. (2021). A subtle, but important, contribution here is that we show that these gender differences are only important when children are present.

CRediT authorship contribution statement

Malte Borghorst: Writing – review & editing, Writing – original draft, Formal analysis, Data curation, Conceptualization. **Ismir Mulalic:** Writing – review & editing, Writing – original draft, Validation, Software, Methodology, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Jos van Ommeren:** Writing – review & editing, Writing – original draft, Validation, Methodology, Formal analysis, Conceptualization.

[28] The effect of wages is somewhat smaller now (in absolute terms), suggesting that the current MCC exceeds the $\mathbb{E}[MCC]$. This makes sense, as, by moving residence, it is possible to reduce the current cost of commuting. However, an alternative interpretation is that the change in the wage effect is due to having a selected sample.

Appendices

In the following appendices, we present additional information and empirical results. Appendix A provides more details on the theoretical foundations for estimating the marginal cost of commuting, including comparative statics. Appendix B presents additional estimation results. Finally, Appendix C provides estimates of the marginal effect of distance on commuting time.

Appendix A. Marginal cost of commuting: theory

A.1. Comparative statics

In this section we derive the effect of α on the voluntary job-to-job rate, θ, i.e. the arrival rate of jobs which increases utility. To derive this effect, we introduce $\lambda(v^*)$ which defines the arrival rate of job offers that offer utility v^*.[29] Workers will accept all job offers for which hold that $log(w^*) - \alpha x^* > log(w) - \alpha x$.[30] Note that when job is at distance x^*, then the (log) wage offer is $v^* + \alpha x^*$. This arrival rate can therefore be written as (space is two-dimensional and we, therefore, multiply the job offer density function with $2\pi x^*$):

$$\lambda(v^*) = \lambda \int_0^\infty f(v^* + \alpha x^*) 2\pi x^* dx^*. \tag{A.1}$$

We change the variable of integration to $log(w^*)$, so we get:

$$\lambda(v^*) = \frac{2\pi\lambda}{\alpha^2} \int_v^\infty (log(w^*) - v^*) f(log(w^*)) dlog(w^*). \tag{A.2}$$

Now consider a worker with a job offering $log(w^*)$ at a distance equal to x^*, i.e. a job which offers exactly utility v^*. This worker will accept all job offers v^* which exceed v. The job moving rate θ is then defined by:

$$\begin{aligned}\theta(w,x) &= \int_v^\infty \lambda(v^*) dv^* \\ &= \frac{2\pi\lambda}{\alpha^2} \int_v^\infty \int_v^\infty (log(w^*) - v^*) f(log(w^*)) dlog(w^*) dv^*.\end{aligned} \tag{A.3}$$

Eq. (A.3) allows us to do comparative statics. Given (A.3), it is straightforward to see that the job moving rate depends negatively on v ($\partial\theta(w,x)/\partial v < 0$). Furthermore, v depends positively on wages while negatively on distance. Consequently, an increase in the current wage or a decrease in the length of the commute will result in a lower job moving rate, i.e. $\partial\theta(w,x)/\partial w < 0$ and $\partial\theta(w,x)/\partial x > 0$. Such a result is in line with intuition.

It also allows us to investigate how α affects the job moving rate. Given (A.3), it appears that v, the job moving rate is directly proportional to the ratio of the arrival rate λ and the *square* of the marginal cost of commuting α. However, the effect of α on the job moving rate is ambiguous, as it reduces v. For workers with a short commute, an increase in α reduces the job moving rate, whereas for those with a long commute, an increase in α increases the job moving rate.

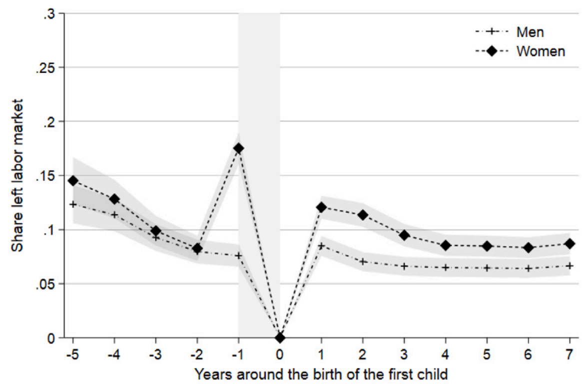
One can show this formally by differentiating $\theta(w,x)$ with respect to α:

$$\frac{\partial\theta(w,x)}{\partial\alpha} = -\frac{2\theta(w,x)}{\alpha} + \frac{\partial\theta(w,x)}{\partial x}\left(\frac{\partial v}{\partial x}\right)^{-1}\frac{\partial v}{\partial\alpha} = -\frac{2\theta(w,x)}{\alpha} + \frac{\partial\theta(w,x)}{\partial x}\frac{x}{\alpha}. \tag{A.4}$$

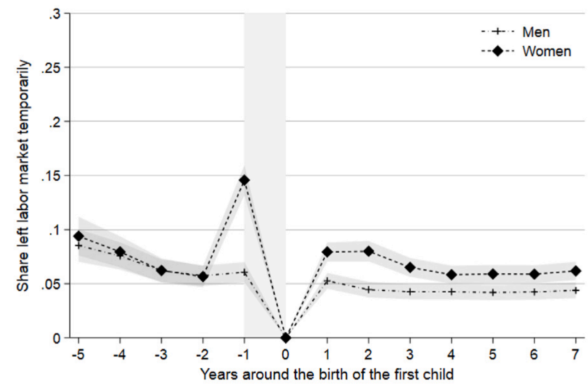
For x equals to 0, the expression is negative, whereas for large values of x, the second term exceeds the first term, as the first term is bounded.

[29] Job offers come from a continuous wage offer cumulative distribution denoted by $F(w^*)$ with the corresponding density function denoted by $f(w^*)$. We assume that this distribution is given for individual workers. Job offers at a distance x^* arrive at an exogenous Poisson arrival rate λ.

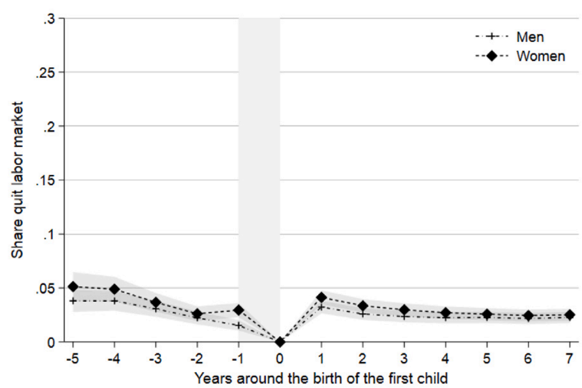
[30] Recall, workers get utility from wages, w, and disutility from distance to work, x. The utility is additive in the *logarithm of wages* and commuting. Hence, utility v can be written as an increasing function of $log(w) - \alpha x$, where α is a parameter. For simplicity we assume that $v = log(w) - \alpha x$.



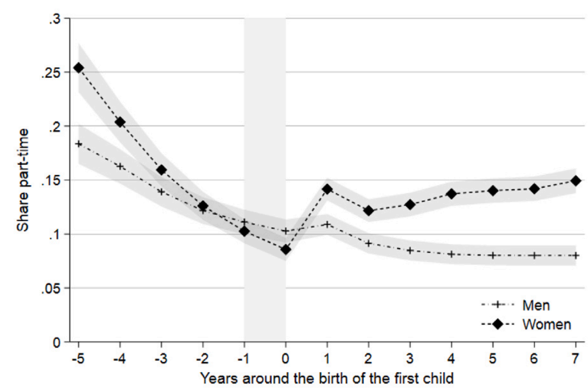
(a) Share of workers leaving the final sample



(b) Share of workers leaving - returning



(c) Share of workers leaving - not returning



(d) Shares part-time before full-time selection

Fig. B.1. Share of sample quits and part-time selection. *Notes:* The gray area marks the birth of the first child and the observation of the birth is the seed of our sample, meaning we observe everyone at t = 0. The part-time shares have been calculated before the selection on full-time has been made, the share of sample quits is calculated on the final sample.

A.2. Deriving marginal cost of commuting

Given Eq. (A.3), one can write $\theta(w,x)$ as $\theta(v(w,x))$, so one may derive the marginal cost of commuting, MCC, as:

$$MCC \equiv -\frac{\partial v/\partial x}{\partial v/\partial w} = -\frac{\partial\theta(w,x)/\partial x}{\partial\theta(w,x)/\partial w} = -\frac{\partial\theta(w,x)/\partial x}{\partial\theta(w,x)/\partial log(w)}w = \alpha w, \quad \text{(A.5)}$$

where we have used the chain rule that implies that:

$$-\frac{\partial\theta(w,x)/\partial x}{\partial\theta(w,x)/\partial w} = -\frac{[\partial\theta/\partial v]\partial v/\partial x}{[\partial\theta/\partial v]\partial v/\partial w} = -\frac{\partial v/\partial x}{\partial v/\partial w}. \quad \text{(A.6)}$$

Consequently, MCC can be estimated using the ratio of the marginal effect of commuting distance on job mobility and the marginal effect of log wage on job mobility.

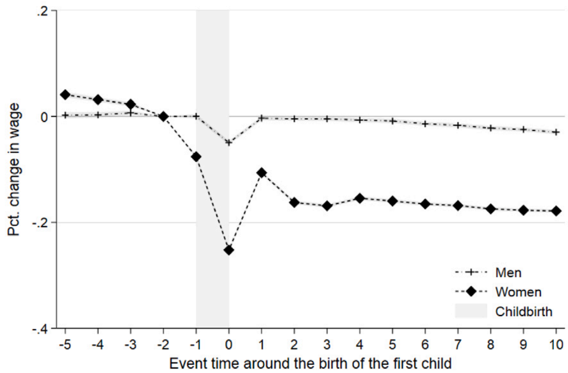
Appendix B. Job mobility and the birth of the first child

This appendix presents additional estimation results, which are mentioned and discussed in the main text. Fig. B.1 shows results for sample quits and part-time selection. For example, Panel (a) indicates that the proportion of part-time workers is generally low (10%–15%) and remains relatively stable both before and after having a child, as well as across genders. The other panels suggest that the majority of workers who transition to non-employment (or part-time employment) eventually return to full-time employment. Additionally, Panel (c) reveals that out-of-sample job mobility is limited. Fig. B.2 presents the estimation results for models based on sub-samples of workers who either did not change jobs, did not relocate, or both, within the period starting three years before the birth and Fig. B.3 shows the estimation results of the impact of childbirth on commuting distance for sub-samples of workers who either did not change jobs or did not relocate during the period starting three years before the birth. Finally, Fig. B.4 displays histogram of the estimated Sorkin amenity index discussed in Section 5.5.2.

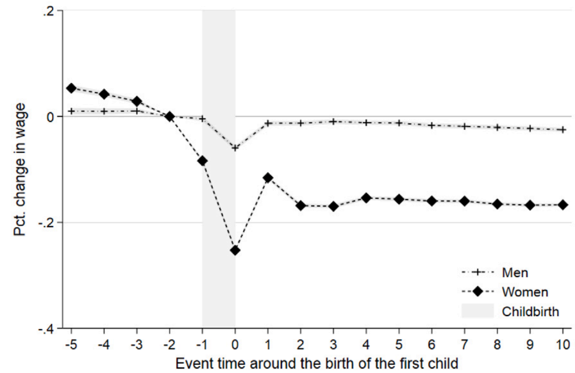
Table B.1 presents the estimation results of models that explore whether the effect of distance on job mobility is linear by including the square of the demeaned distance. Table B.2 shows the results of estimating alternative specifications of the job mobility model. Finally, in Table B.3 we provide the empirical evidence that a between-firms analysis generates robust results, see also Section 5.5.3.

Appendix C. Marginal effect of distance on commuting time

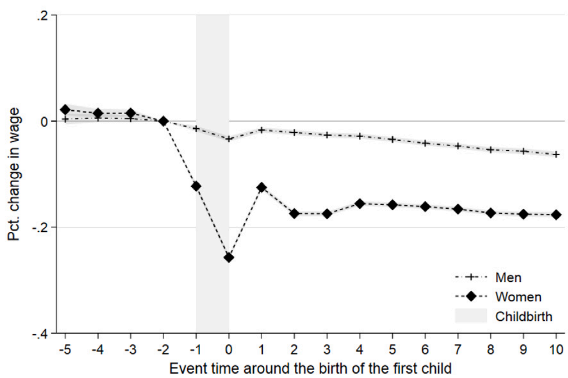
We use the Danish National Travel Survey (NTS) to estimate the marginal effect of distance on commuting time. The NTS provides information on the travel behavior of randomly selected individuals who fill out a one-day travel diary. Information is collected continuously



(a) Job fixed and residence flexible

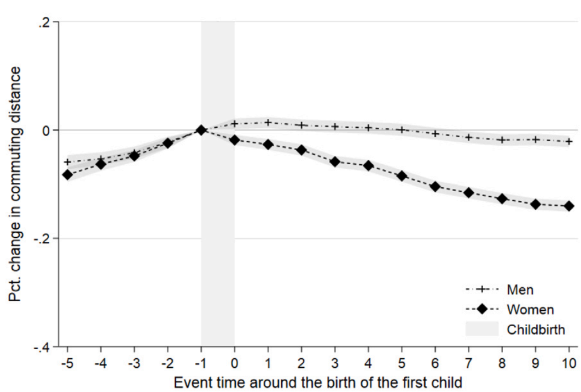


(b) Job flexible and residence fixed (t=-3)

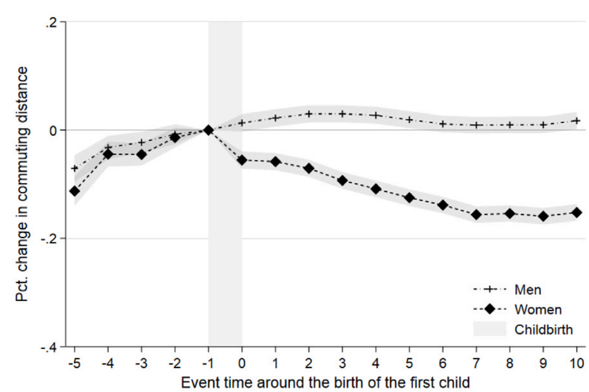


(c) Job and residence fixed

Fig. B.2. Event study results for different samples: wage. *Notes:* Wage event time effects around the birth of the first child. The gray area marks the time interval of the birth of the first child. The shaded 95 percent confidence intervals are based on robust standard errors.



(a) Job fixed (from t=-3)



(b) Residence fixed (from t=-3)

Fig. B.3. Event study results for different samples: commuting distance. *Notes:* Commuting distance event time effects around the birth of the first child. The gray area marks the time interval of the birth of the first child. The shaded 95 percent confidence intervals are based on robust standard errors.

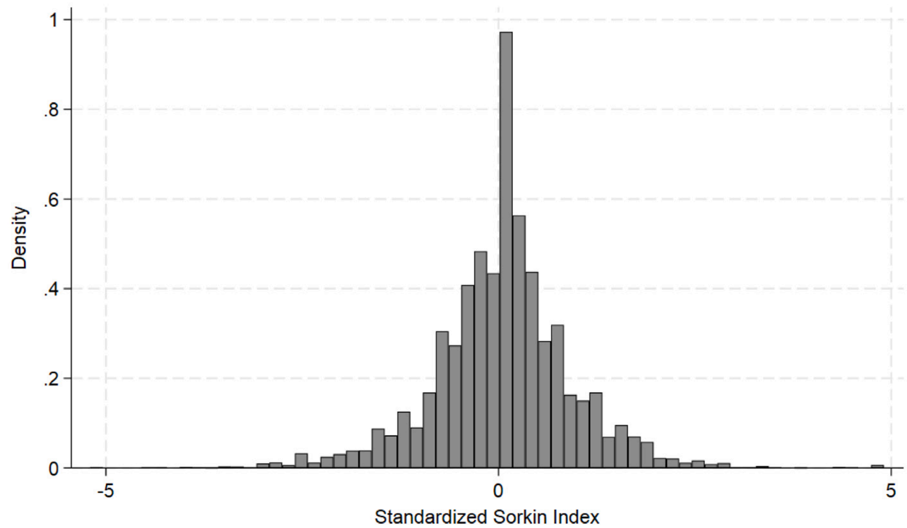


Fig. B.4. Histogram of the estimated Sorkin index. *Notes:* Figure shows the ranking of firms revealed by workers' choices developed by Sorkin (2018) for the subset of strongly connected firms by employer-to-employer mobility.

Table B.1
Job mobility model with polynomial specification for commuting distance.

Dep. var.: Job change			[1]
Distance (km)			
	Women	No child	0.0008***
			(0.0002)
		Child	0.0029***
			(0.0001)
	Men	No child	0.0014***
			(0.0002)
		Child	0.0016***
			(0.0002)
(Distance - Average Distance)^2			
	Women	No child	0.00003*
			(0.00001)
		Child	−0.00001
			(0.0001)
	Men	No child	−0.000001
			(0.0001)
		Child	−0.00001
			(0.0001)
Log. wage			
	Women	No child	−0.373***
			(0.026)
		Child	−0.171***
			(0.021)
	Men	No child	−0.319***
			(0.024)
		Child	−0.178***
			(0.023)
F-statistic for IV			
	Women	No child	7,832
		Child	9,546
	Men	No child	8,447
		Child	10,882
Controls			Yes
Worker fixed effects			Yes
Year fixed effects			Yes
No. of observations			2,243,915

Notes: The sample consists of full-time workers. All specifications include the following controls: marital status, job tenure in linear and squared form, number of workers in the firm, average age of workers at the firm, and gender and child interaction term. Log wage is instrumented using the average wage of similar workers of the same firm. Standard errors are in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B.2
Alternative specifications of the job mobility model.

Dependent variable: Job change		[1] Worker and household FE	[2] Household FE	[3] Occupational rank	[4] Strongly connected	[5] Regional FE
Distance (km)						
Women	No child	0.0013***	0.0014***	0.0011***	0.0010***	0.0011***
		(0.0002)	(0.0001)	(0.0001)	(0.0002)	(0.0001)
	Child	0.0030***	0.0027***	0.0028***	0.0028***	0.0029***
		(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0001)
Men	No child	0.0014***	0.0013***	0.0014***	0.0011***	0.0014***
		(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0001)
	Child	0.0015***	0.0016***	0.0015***	0.0015***	0.0015***
		(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Log. wage						
Women	No child	−0.361***	−0.127***	−0.355***	−0.394***	−0.382***
		(0.033)	(0.013)	(0.029)	(0.037)	(0.026)
	Child	−0.170***	−0.027**	−0.155***	−0.184***	−0.174***
		(0.025)	(0.009)	(0.024)	(0.031)	(0.022)
Men	No child	−0.328***	−0.153***	−0.303***	−0.307***	−0.325***
		(0.030)	(0.023)	(0.026)	(0.035)	(0.024)
	Child	−0.190***	−0.023***	−0.161***	−0.174***	−0.181***
		(0.028)	(0.008)	(0.026)	(0.035)	(0.024)
F statistic for IV						
Women	No child	5,325	9,971	7,808	6,973	7,866
	Child	7,994	17,148	9,566	7,498	9,538
Men	No child	5,396	8,828	8,107	6,607	8,468
	Child	9,066	23,461	10,772	8,671	10,895
Time-variant controls		Yes	Yes	Yes	Yes	Yes
Worker fixed effects		Yes	No	Yes	Yes	Yes
Household fixed effects		Yes	Yes	No	No	No
Year fixed effects		Yes	Yes	Yes	Yes	Yes
No. of observations		2,243,915	2,243,915	2,243,915	1,506,226	2,243,915
		Marg. cost of comm. (% of annual wage) per 12 km increase (1 std. dev.)				
Women	No child	−0.043	−0.130	−0.037	−0.029	−0.035
		(0.007)	(0.017)	(0.005)	(0.006)	(0.005)
	Child	−0.214	−1.217	−0.220	−0.185	−0.197
		(0.033)	(0.385)	(0.035)	(0.033)	(0.026)
Men	No child	−0.050	−0.103	−0.056	−0.044	−0.053
		(0.007)	(0.012)	(0.006)	(0.008)	(0.006)
	Child	−0.098	−0.844	−0.109	−0.103	−0.099
		(0.016)	(0.298)	(0.019)	(0.022)	(0.014)

Notes: The sample consists of full-time workers. Time variant controls include our standard set of controls, see Table 2. Occupational rank includes: general managers (CEOs), managers, workers at non-managerial level, and employees (excl. young people and trainees). The set of strongly connected firms is defined in Section 5.5.3. Log wage is instrumented using the average wage of similar workers of the same firm. MCC is estimated using the ratio of the marginal effect of commuting distance on job mobility and the marginal effect of log wage on job mobility, see Eq. (2). Standard errors are clustered at the firm-year level and can be found in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

throughout the year. We use NTS for the years 2006–2019 and select individuals (18–70 years old) who report commuting trips and exclude observations with missing information and observations for which the one-way commuting distance exceeds 108 km (99 percentile), the one-way commuting time exceeds 95 min (99 percentile), or the average commuting speed is below 3.6 km/h (1 percentile) or above 79.5 km/h (99 percentile). Given these selection criteria, we exclude 6.7% of commuting trips. Our final sample includes 81,577 commuting trips.

Table C.1 provides descriptives. On average, the one-way commuting time is 21 min, the one-way commuting distance is about 14 km and the speed is 36 km/h. The mean inverse speed is 0.045. The most popular commuting mode is the car (65%), while only 9% of workers commute with public transport. Bicycle use is very common: more than 21% of workers commute by bicycle. For the sample of workers between 25–45 years, which is the relevant population for our paper, the descriptive statistics are almost identical.

According to the speed literature, the effect of distance on travel time is diminishing, because the marginal increase in travel time is less for longer distances, see, for example, Couture et al. (2018). In line with that, when we regress travel time on travel distance, we use a log–log specification, see Table C.2. In the first model [1], we find a coefficient of 0.58, slightly higher than the value reported for the United Kingdom by Van Ommeren and Dargay (2006). When we estimate the models for the sample of workers between 25–45 years, the estimated coefficients are almost identical, see column [2]. Finally, we re-estimate the latter model separately for women and men, see columns [3] and [4]. Again it appears that the coefficient is about 0.58.

We are interested in the marginal effect of commuting distance (measured in km) on commuting time (measured in hours). Given a log–log specification, the average marginal effect is equal to the product of the estimated coefficient and the average inverse speed (the ratio of travel time and travel distance). Given an estimate of 0.58 (see Table C.2) and an average inverse speed of about 0.045 and 0.043 respectively (see Table C.1), it appears that the mean marginal effect is 0.026 for the full sample and 0.025 for the sample of commuters 25–45 years, respectively.

Table B.3
Job mobility model: Between firms estimation.

Dependent variable: Job change			[1]
Distance			
	Women	No child	0.0005***
			(0.0001)
		Child	0.0015***
			(0.0001)
	Men	No child	0.0008***
			(0.0001)
		Child	0.0009***
			(0.0001)
Log. wage			
	Women	No child	−0.128***
			(0.020)
		Child	−0.092***
			(0.016)
	Men	No child	−0.133***
			(0.019)
		Child	−0.156***
			(0.017)
F-statistic for IV			
	Women	No child	6,239
		Child	10,988
	Men	No child	6,650
		Child	10,339
Time variant controls			Yes
Worker fixed effects			Yes
Year fixed effects			Yes
No. of observations			2,243,915
Marginal cost of commuting (% of annual wage) per 12 km increase (1 std. dev.)			
	Women	No child	−0.044
			(0.013)
		Child	−0.201
			(0.035)
	Men	No child	−0.069
			(0.013)
		Child	−0.072
			(0.009)

Notes: The sample consists of full-time workers. All variables are averaged across workers' job spells. Time variant controls include our standard set of controls, see Table 2. Log wage is instrumented using the average wage of similar workers of the same firm. MCC is estimated using the ratio of the marginal effect of commuting distance on job mobility and the marginal effect of log wage on job mobility, see Eq. (2). Standard errors are clustered at the firm-year level and can be found in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.1
Descriptive statistics for Danish national travel survey.

	All commuters		Comm. 25-45 years	
	Mean	Std. dev.	Mean	Std. dev.
Trip length (km)	14.20	15.40	14.60	15.58
Trip time (minutes)	21.35	16.51	21.73	16.44
Trip speed (km/h)	35.82	20.03	36.35	20.12
Trip inverse speed (h/km)	0.045	0.040	0.043	0.038
Car (share)	0.65	0.48	0.65	0.48
Public transport (share)	0.09	0.20	0.09	0.29
Walking (share)	0.04	0.04	0.04	0.19
Bicycle (share)	0.21	0.41	0.22	0.41
Male (share)	0.49	0.50	0.49	0.50
Age (year)	43.43	12.08	36.50	5.77
Number of obs. (commuting trips)	81,577		37,524	

Table C.2
Travel distance and travel time.

	All commuters	Commuters 25-45 years		
			Man	Woman
Dep. variable	log(time) [1]	log(time) [2]	log(time) [3]	log(time) [4]
log(distance)	0.5792***	0.5846***	0.5945***	0.5771***
	(0.0013)	0.0019	(0.0027)	(0.0027)
const.	−2.5231***	−2.5354***	−2.5755***	−2.5028***
	(0.0030)	0.0045	(0.0066)	(0.0062)
R-squared	0.7197	0.7200	0.7316	0.7086
Number of obs.	81,577	37,524	18,443	19,081

Notes: Standard errors are in parentheses, *** $p < 0.01$.

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