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Couples, Careers, and Spatial Mobility

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Lea Nassal and Marie Paul[1]

Couples, Careers, and Spatial Mobility

Abstract

We investigate the effects of long-distance moves of married couples on both spouses' earnings, employment and job characteristics based on a new administrative dataset from Germany. Employing difference-in-difference propensity score matching and accounting for spouses' premove employment biographies, we show that men's earnings increase significantly after the move, whereas women suffer large losses in the first years. Men's earnings increases are mainly driven by increasing wages and switches to slightly larger and better paying firms. Investigating effect heterogeneity with respect to pre-move relative earnings or for whose job opportunity couples move, confirms strong gender asymmetries in gains to moving.

JEL-Codes: J61, J16, R23

Keywords: Long-distance moves; labor market careers; gender gap

September 2022

1 Lea Nassal, UDE; Marie Paul UDE, RGS Econ, and CReAM. - We thank participants of several conferences and seminars for helpful comments and the Research Data Center of the Institute for Employment Research (RDC-IAB) for generously providing the data and support with running programs remotely. RDC-IAB prepared the data in the priority program 1764 "The German Labor Market in a GlobalizedWorld: Challenges through Trade, Technology, and Demographics" of the German Research Foundation (DFG). Lea Nassal gratefully acknowledges financial support from the DFG. The usual caveat applies. This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors. There are no interests to declare. – All correspondence to: Marie Paul, University of Duisburg-Essen, Lotharstr. 65, 47057 Duisburg, Germany, e-mail: marie.paul@uni-due.de

1 Introduction

Over the past half a century, women's participation in the labor market rose sharply and dual-earner couples have become the norm[1]. Due to this development, many couples will face a trade-off between both spouses' careers. Typically, location preferences within couples will differ because spouses have different preferences for amenities as well as different job opportunities in a particular region. They will therefore face the so-called *"colocation problem"* (Costa and Kahn, 2000). If both spouses are employed and live together, both will have to find acceptable jobs in the same region, which will sometimes involve sacrificing the career of one spouse for the sake of the other. If such decisions are not taken in a gender neutral way or if potential relocation gains are lower for women, the colocation problem may be a driving force of the remaining gender wage and employment gap, in addition to other explanations stressed in recent studies.

A number of papers have found that child penalties play an important role in the remaining gender gap (Angelov et al. (2016), Cortes and Pan (2022), Kleven et al. (2019a,b)). Women, who typically take over more care responsibilities than men, have disadvantages when long working hours or working particular hours is rewarded (Bolotnyy and Emanuel (2022), Goldin (2014)). Women also show a lower willingness to commute (Le Barbanchon et al. (2020)). In addition, social norms or psychological attributes such as being willing to compete, risk preferences and self-confidence may directly affect job search and wages (e.g. Bertrand et al. (2015), Buser et al. (2014), Cortes et al. (2021), Wiswall and Zafar (2017)). A further potential explanation, which is the focus of this paper, is that especially married women may take less advantage of career enhancing long-distance moves or may even experience earnings losses as a tied mover.

In this paper, we investigate the career effects of *long-distance moves* for both partners of married couples as a potential explanation of the gender earnings gap, drawing on a new administrative dataset from Germany (Goldschmidt et al. (2017)). We deal with the empirical challenge of self-selection into moving by flexibly controlling for selection on observables. In addition, we account for time-invariant unobservables through subtracting pre-move outcomes. To implement this, we use difference-in-difference propensity score matching (Heckman et al. (1997), Heckman et al. (1998)). We estimate the average treatment effect for the moving couples on spouses' earnings, employment, daily wages, and job characteristics for the five years after the move. The treatment is defined as experiencing a long-distance move concurring with a job change of at least one partner. Couples used as controls must not experience a long-distance move in the time period under study, but may or may not change jobs. We use our rich administrative dataset to precisely account for spouses' personal characteristics, their labor market

[1]In 1970, 97% of German men and 47% of women were in the labor force, but the gender gap in labor force participation has narrowed considerably. By 2010, men's labor force participation rate fell to 93%, while that of women increased to 81%. (Measured statistic is the total labor force participation of men and women aged 25-54, see OECD statistics: `https://stats.oecd.org`.)

histories before the move, as well as for regional characteristics.

Our main results show that women experience large negative labor market effects in the first years after the move. One year after the move, the yearly labor earnings of moving women decline on average by around €2,000 (10.5%) compared to matched non-movers. Earnings recover slowly over time. In contrast, men experience significant positive returns from moving, with an increase in yearly labor earnings of around €450 (1.0%) in the first year after the move and even around €2,600 (5.1%) five years thereafter. We show that the negative effect on women's earnings is mainly driven by a decrease in employment, whereas the positive effect on men's earnings is driven by a large increase in wages and a small increase in employment. We then take a look at household labor earnings and expect spouses' joint earnings to increase after the move. In the standard human capital framework, couples will only move if the returns to moving exceed the costs (Mincer (1978)). In line with this, we observe that household earnings start to increase significantly in year two after the move. However, spouses' individual earnings and earnings potential are also relevant. According to non-cooperative bargaining models, the spouse with higher earnings has more say in the decision-making process (Browning and Chiappori (1998), Chiappori (1992)). Hence, a move going along with an individual income loss may harm women through decreasing their bargaining power within the couple. In addition, the woman's individual income loss may also be important in case of a separation or husband's death.

For a deeper understanding of these results, we then turn to analyzing heterogeneity in treatment effects. In a first step, we investigate differences in treatment effects, depending on whether couples move in favor of the man's or the woman's job opportunities, which we approximate by who starts a job in the target region first. While men can realize significant increases in labor earnings if couples move in favor of the man, estimated returns are much lower for women if couples move in favor of the woman. Earnings losses are also much larger for women if couples move in favor of the man's career than for men if couples move for the woman's career. In a second step, we examine whether treatment effects vary by spouses' pre-move relative earnings. We show that returns to moving are the largest for men who only earn a small proportion of household earnings before the move. However, for women with low pre-move relative earnings, the estimated returns are much smaller. These results are not in line with what we would expect from gender neutral standard collective models. From these models, we would expect that spouses with high pre-move relative earnings are those with high bargaining power within the couple and therefore returns to moving should be the largest for them. However, one needs to take into account that spouses who already have high (relative) earnings before the move may find it difficult to realize large moving gains. In addition to this, we observe that men can realize moving gains regardless of their relative pre-move earnings, while this does not necessarily apply to women. We take this as additional evidence for gender asymmetries in the returns to moving. We also examine the role of other important characteristics: the gains from moving are larger for spouses starting off in regions with high unemployment rates and for couples in

which no partner holds a university degree. Female movers for whom we observe a childbirth experience high earnings losses, but these are not necessarily a causal effect of the move. While men gain from moving, whether employed or non-employed before moving, women gain on average only if they are non-employed before the move. In a final step, we use available information in our data on firm characteristics together with our matching approach to shed light on underlying mechanisms that may drive spouses' earnings and employment responses. We hypothesize that men's large positive returns to moving may be explained by men moving from small and low-paying firms to larger and higher-paying firms. We built weak support for this hypothesis by showing that men tend to move to firms that on average pay higher daily wages and have slightly more employees. In contrast, we cannot document this finding for women. In addition, we investigate whether spouses' job requirement level changes after the move. In particular, we explore if spouses move from low-skilled tasks toward higher-skilled tasks or vice versa. Our results show that transitions to a higher job requirement level are slightly more pronounced for men than for women. However, this small difference does not explain the large gender differences in the returns to moving. Finally, we investigate whether spouses switch industries or occupations after the move. We show that the probability to switch industries and occupations increases significantly after the move for both spouses.

Overall, our results suggest that long-distance moves lead to an increase in long-run household income through wage gains or employment prospects of husbands, at the cost of wives' employment stability. Gender asymmetries pertain, also conditional on for whom couples move and conditional on relative earnings. Men reach their wage gains through targeting slightly larger and better-paying firms. The contrary situation – women realizing wage gains while accepting a worse job situation for men – is rare and, if at all, rather occurs to escape unemployment than to improve wages. Long-distance moves are thus one factor that enhances wage and employment differences and our study therefore contributes to the recent literature on understanding the remaining gender gap in the labor market (Cortes et al. (2021), Cortes and Pan (2022), Goldin (2014), Kleven et al. (2019b), Kleven et al. (2019a), Le Barbanchon et al. (2020), Illing et al. (2021), Huttunen et al. (2018)).

Our paper is also strongly related to several, mainly less recent, studies that investigated the effects of joint moves on couples' labor market outcomes (Blackburn (2010b), Cooke et al. (2009), LeClere and McLaughlin (1997), Sandell (1977), Blackburn (2010a), Cooke (2003a), Spitze (1984), Rabe (2009)). These studies show that women experience large earnings losses in the first years after the move and that these losses are mainly driven by employment interruptions of women (LeClere and McLaughlin (1997), Blackburn (2010b)). Even if women have a higher earnings potential before the move, they cannot realize moving gains (Cooke (2003b)). Methodologically, these studies used either Heckman two-stage models (LeClere and McLaughlin (1997), Rabe (2009)) or fixed effects/lagged-variable models (Cooke et al. (2009), Blackburn (2010a), Blackburn (2010b), Cooke (2003b), Sandell (1977), Spitze (1984)) to control for self-selection into migration. The approach in this study is different. Our data

and matching approach enable us to control more precisely for spouses' personal and job characteristics before the move, since we have detailed information about the labor market histories of both spouses for many years before the move. Most of the existing studies largely focus on overall effects and study the effects on earnings, employment or wages. In this study, we investigate effect heterogeneity in treatment effects along many dimensions. As our dataset includes some firm information, we can examine if spouses relocate to different types of firms after the move which, to our knowledge, has not been studied in the literature. In sum, looking into the "black box" of moving and providing evidence on the underlying mechanisms and heterogeneities while precisely aligning movers to controls is our main contribution to this strand of literature.

Finally, our paper relates to a number of studies on the determinants of family migration (Nivalainen (2004), Tenn (2010), Compton and Pollak (2007), Eliasson et al. (2014), Duncan and Perrucci (1976), McKinnish (2008), Rabe (2009), Foged (2016)). Several studies have shown that couples' migration decision is primarily driven by the husband's career. Evidence shows that factors such as the husband's education (Compton and Pollak (2007), Nivalainen (2004)) and occupation characteristics (Duncan and Perrucci (1976), McKinnish (2008)) are more important in the migration decision than those of the wife. Exceptions are the studies of Foged (2016) and Rabe (2009), which show that the migration decision is not husband-centered. However, these two studies focus on dual-earner couples. Although the determinants of couples' migration decision are not the main focus of this research, they are important for our propensity score specification. Specifically, our specification includes a rich set of personal and job characteristics of both spouses as well as labor market histories and regional characteristics which the literature has considered as important factors in couples' migration decision.

The rest of the paper is organized as follows: the next section describes the data and the definition of long-distance moves. Section 3 lays out the empirical strategy and identifying assumptions. Section 4 presents our empirical results of the labor market effects of long-distance moves, along with a number of robustness checks, whereafter section 5 concludes.

2 Data, Long-distance Moves and Sample Characteristics

2.1 German Administrative Data

Our analysis is based on German administrative data. We use a dataset called *FEMPSO COUPLE* which contains a 10% random sample of married couples that can be identified in the *Integrated Employment Biographies* (IEB) plus a 15% oversample of couples who experienced a long-distance move from 2008 to 2012.[2] The IEB includes all employees subject to social

[2] *FEMPSO COUPLE* is a custom-shaped dataset produced by the Research Data Center of the *Institute of Employment Research (IAB)* as part of the DFG priority program 1764. The data are processed and kept by the IAB according to Social Code III. The data contain sensitive information and are therefore subject to the confidentiality regulations

security, all people who receive unemployment benefits and those who have been registered as searching for a job. Only civil servants and self-employed people are missing. For FEMPSO COUPLE, married couples are identified according to the method of Goldschmidt et al. (2017): for two people to be matched as a couple, the spouses have to live at the same location, have a matching last name, are of different sexes and have an age difference of less than 15 years[3]. The identification of couples was done once, on June 30, 2008, which implies that our dataset only includes couples of whom both spouses have a record in the IEB for that particular date. It also means that it is not certain whether two individuals are indeed a couple in the years before, and that the further the observation is from 2008, the more uncertain it will become. We therefore study long-distance moves happening from July 1, 2008 to December 31, 2012 (*treatment period*).

Three characteristics make this dataset especially attractive for our analysis: first, the data include detailed geographic information on the place of residence for each spouse that is necessary to investigate the effects of joint moves. Second, our data include detailed labor market histories of both spouses from 1998 to 2017, which allows us in our matching specification to precisely account for spouses' pre-move employment dynamics. In particular, the dataset consists of day-to-day information on every period in employment covered by social security, every period of receiving unemployment insurance benefits, as well as information on periods of job search and participation in subsidized employment and training measures. For each period, it contains information on the corresponding wages and benefit levels. The wage information is very accurate, as the employer has to report wages for social security purposes. In addition, the data include a rich set of personal characteristics such as occupation, nationality, year of birth, education, and job requirement level. For each employee, we also observe information on the employers, such as firm size, average wage at the firm and industry, obtained from the *Establishment History Panel* (BHP)[4]. In our analysis, we use this link between employees and firms to examine whether spouses relocate to different types of firms after the move. Finally, the sample size is much larger than in panel data based on surveys, like the German Socioeconomic Panel (SOEP). This is crucial, as long-distance moves are relatively rare, therefore the available survey data would not enable a precise estimation of treatment effects. In contrast to most previous studies, we can even analyze heterogeneities for different types of couples.

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of the German Social Code (Book I, Section 35, Paragraph 1). The data are held by the IAB, Regensburger St 104, D-490478 Nuremberg, email: iab@iab.de, phone: +49/9111790. If you wish to access the data for replication purposes, please get in contact with the authors and the Research Data Center of the IAB.

[3]This identification method increases the likelihood of identifying certain types of couples: i) older couples, ii) more conservative couples and iii) couples living in smaller buildings (Goldschmidt et al. (2017)). For more information on the dataset and data processing, see online appendix section A.1 and A.2.

[4]Throughout this paper, we use the term firm for simplicity. Note that we can only identify establishments and are unable to link them to firms.

2.2 Long-distance Moves

To identify *long-distance moves*, we use district-level information on spouses' place of residence[5]. We follow the existing literature and view long-distance moves as *changes of local labor markets* (Dahl (2002), Ham et al. (2011), Gabriel and Schmitz (1995)). We define a long-distance move as occuring if a couple moves across district borders with a distance of at least 50 km (30 miles) (between district centroids) and a job change of at least one spouse. The latter can be either a job change between firms or a transition from unemployment to employment. We only require a job change of one spouse to allow for the possibility that the other spouse might become unemployed due to a move in favor of the other spouse's job opportunity. Since for most spells the information on the place of residence is only determined at the end of each year,[6] we allow for the possibility that one spouse moves in year t while the other follows in year $t+1$. The distance between districts is calculated as the distance between the centroids of each district:

$$dist = r \cdot \arccos\left[\sin(lat_t) \cdot \sin(lat_{t-1}) + \cos(lat_t) \cdot \cos(lat_{t-1}) \cdot \cos(long_{t-1} - long_t)\right], \quad (1)$$

where r is the radius of the earth ($6,378$ km or $3,963$ miles), lat_t is the latitude of the district in t, lat_{t-1} is the latitude of the district in $t-1$, $long_t$ is the longitude of the district in t and $long_{t-1}$ is the longitude of the district in $t-1$.

As shown in the literature (Ham et al. (2011), Blackburn (2010b)), using a distance-based definition of moves is more precise relative to definitions based on moving across state or district borders. Still, our definition leaves a little room for potential measurement error. For example, there are a few large districts with areas of over $3,000$ square km. Couples who live in these districts could experience a long-distance move without crossing district borders, and this means we cannot identify these moves. On the other hand, we could falsely assign long-distance moves to couples who live close to district borders. As we use the distance between centroids, the moving distance of those couples could be smaller. These cases are exceptions, though. For more information on the identification of long-distance moves, see online appendix section A.3.

2.3 Sample Definition

In our analysis, we consider all joint moves of couples happening from July 1, 2008 to December 31, 2012. During the observation period, a few couples experienced multiple long-distance moves. We consider only their first move (since 2008), because future outcomes may

[5]In Germany, there are currently 401 districts and 16 states.

[6]For employment spells (BeH), which form the bulk of observations, the information on the place of residence is determined at the end of each year. For job seeker spells (ASU), unemployment benefit spells (LeH) and participant in training measures spells (MTH and XMTH), the information on the place of residence applies to the beginning of the original period. Only for unemployment benefit II recipient spells (LHG) and XASU spells (ASU spells reported by municipal institutions) the information applies to the entire period of original observation.

be influenced by the first move. We therefore abstract from repeated migration. We exclude couples from our sample who no longer lived together for at least three years after 2008, as they may have separated. Finally, we restrict the age of each spouse to 20 to 50 in the pre-move year. This upper age limit is chosen to exclude moves that are related to spouses' retirement decisions. We construct a balanced panel that includes all couples we observe three years before the move to five years thereafter. Our final sample consists of $164,782$ couples, of whom $3,744$ (2.3%) experience a long-distance move.

2.4 Outcome Variables and Sample Characteristics

The main outcome variables that we consider in our analysis are *gross yearly labor earnings* (in 2015 euros) and *days employed per year* of each spouse. The latter refers to the total days employed in all jobs subject to social security contributions and the former refers to the total gross yearly labor earnings from all those jobs. For non-working spouses, the yearly labor earnings and days employed are zero. Changes in employment and earnings may therefore be either due to changes at the extensive or intensive margin.

Table 1 provides sample characteristics for *movers* and *non-movers* before and after matching by depicting raw means of selected variables measured in the pre-move year by treatment status and gender. Column 1 reports sample characteristics for moving men, column 2 for all non-moving men and column 3 for matched non-moving men, while columns 4 to 6 refer to women. Before matching, there are large differences between movers and non-movers.

Table 1: Sample Descriptives, Moving and Non-moving Couples

	Men			*Women*		
	Movers	All non-movers	Matched non-movers	Movers	All non-movers	Matched non-movers
	(1)	(2)	(3)	(4)	(5)	(6)
Age (years)	37.29	41.17	37.26	35.03	39.06	34.99
	(6.95)	(6.11)	(6.92)	(7.10)	(6.36)	(7.10)
Age group (years)						
20-29	0.15	0.05	0.15	0.26	0.09	0.25
	(0.36)	(0.22)	(0.35)	(0.44)	(0.29)	(0.44)
30-39	0.46	0.29	0.46	0.45	0.38	0.46
	(0.50)	(0.46)	(0.50)	(0.50)	(0.49)	(0.50)
40-50	0.39	0.65	0.40	0.29	0.53	0.29
	(0.49)	(0.48)	(0.49)	(0.46)	(0.50)	(0.45)
Education						
No/unrecognized education, basic/	0.17	0.17	0.18	0.23	0.23	0.23
general secondary education	(0.38)	(0.38)	(0.38)	(0.42)	(0.42)	(0.42)
In-company/school-based training,	0.55	0.71	0.56	0.58	0.71	0.59
abitur	(0.50)	(0.45)	(0.50)	(0.49)	(0.45)	(0.49)
University degree	0.27	0.11	0.26	0.19	0.07	0.18
	(0.45)	(0.32)	(0.44)	(0.39)	(0.25)	(0.38)
Non-German	0.11	0.08	0.11	0.13	0.09	0.14
	(0.31)	(0.28)	(0.32)	(0.34)	(0.28)	(0.34)
Yearly labor earnings	40860	43565	40608	18203	17834	18268
	(35623)	(30664)	(36318)	(20784)	(16692)	(20837)
Yearly labor earnings <4000€	0.14	0.07	0.15	0.32	0.21	0.32
	(0.35)	(0.25)	(0.36)	(0.47)	(0.40)	(0.47)
Yearly labor earnings 4000-20000€	0.16	0.09	0.16	0.32	0.42	0.31
	(0.37)	(0.29)	(0.36)	(0.47)	(0.49)	(0.46)
Yearly labor earnings 20000-60000€	0.47	0.66	0.48	0.33	0.35	0.33
	(0.50)	(0.48)	(0.50)	(0.47)	(0.48)	(0.47)

(continued)

Table 1: Sample Descriptives, Moving and Non-moving Couples (continued)

	Men			*Women*		
	Movers	All non-movers	Matched non-movers	Movers	All non-movers	Matched non-movers
	(1)	(2)	(3)	(4)	(5)	(6)
Yearly labor earnings > 60000€	0.23	0.19	0.22	0.04	0.02	0.04
	(0.42)	(0.39)	(0.41)	(0.19)	(0.14)	(0.19)
Days employed	301.05	337.26	300.84	258.35	314.17	256.91
	(118.20)	(84.84)	(119.54)	(146.33)	(112.83)	(148.11)
Days receiving benefits	14.99	6.45	14.81	11.45	5.88	11.24
	(51.37)	(33.45)	(52.56)	(46.78)	(33.96)	(46.87)
Tenure at current job (years)	2.98	5.92	3.32	2.44	4.39	2.66
	(3.34)	(4.28)	(3.77)	(3.01)	(3.84)	(3.38)
Employment status						
Full-time	0.79	0.90	0.79	0.60	0.64	0.60
	(0.41)	(0.31)	(0.41)	(0.49)	(0.48)	(0.49)
Part-time	0.05	0.03	0.05	0.14	0.23	0.14
	(0.21)	(0.18)	(0.21)	(0.35)	(0.42)	(0.35)
Marginal	0.02	0.01	0.02	0.02	0.02	0.02
	(0.13)	(0.08)	(0.13)	(0.15)	(0.15)	(0.15)
Non-employed	0.15	0.06	0.15	0.23	0.10	0.23
	(0.36)	(0.25)	(0.36)	(0.42)	(0.30)	(0.42)
Job requirement level						
Unskilled	0.08	0.06	0.09	0.15	0.16	0.15
	(0.28)	(0.25)	(0.28)	(0.36)	(0.36)	(0.36)
Skilled	0.54	0.70	0.55	0.61	0.71	0.63
	(0.50)	(0.46)	(0.50)	(0.49)	(0.45)	(0.48)
Complex	0.12	0.11	0.12	0.06	0.05	0.06
	(0.32)	(0.32)	(0.32)	(0.23)	(0.21)	(0.23)
Highly complex	0.26	0.12	0.25	0.18	0.09	0.17
	(0.44)	(0.32)	(0.43)	(0.38)	(0.28)	(0.37)

(continued)

Table 1: Sample Descriptives, Moving and Non-moving Couples (continued)

	Men			*Women*		
	Movers	All non-movers	Matched non-movers	Movers	All non-movers	Matched non-movers
	(1)	(2)	(3)	(4)	(5)	(6)
Years in region (censored in 1998)	8.19	9.24	8.34	8.16	9.23	8.33
	(2.11)	(1.60)	(2.11)	(2.09)	(1.60)	(2.12)
Children	0.24	0.62	0.23	0.24	0.62	0.23
	(0.43)	(0.49)	(0.42)	(0.43)	(0.49)	(0.42)
Area unemployment rate	8.41	7.82	8.42	8.41	7.82	8.42
	(3.87)	(3.65)	(3.74)	(3.87)	(3.65)	(3.74)
Area GDP per capita	32.86	30.11	32.79	32.86	30.11	32.79
	(15.20)	(12.59)	(14.90)	(15.20)	(12.59)	(14.90)
Observations	3744	161038	24614	3744	161038	24614

Notes: Statistics shown are means with standard deviations in parentheses. Movers are couples that experienced a move with a distance of at least 50 km and a job change of at least one spouse. The sample consists of couples aged 20-50 in the pre-move year $t-1$. See section 2.2 for more details about the restrictions. All variables are measured in pre-move year $t-1$. Days employed and days receiving benefits refer to the total days employed per year and the total days receiving unemployment benefits, respectively. There is one observation per couple.

We observe that movers are on average about four years younger than non-movers and that women are about two years younger than men. In addition, movers are better educated compared to non-movers – 27% of moving men hold a university degree, while this applies to only 11% of non-movers. The same pattern is observed for women, but they are generally less educated than men. We also see that about twice as many movers work in highly complex job tasks compared to non-movers. However, while movers are on average younger and better skilled, they work on average fewer days per year and receive unemployment benefits more often during the years before the move. Relative to non-movers, moving men receive on average 8.5 days more unemployment benefits in the year before the move, while moving women receive only 5.6 days more. With respect to yearly labor earnings, earnings of moving men are lower than those of non-movers in the year before the move. Looking at yearly earnings divided into four bins, it becomes clear that moving men and women are more likely to be represented in the lowest and highest earnings group than non-movers. Finally, relative to non-moving couples, fewer moving couples have at least one child.

Overall, the descriptive statistics show that the group of moving couples is very heterogeneous. It seems that moving couples are either people who are unemployed before moving or they are highly skilled people who move to further improve their careers. We therefore believe that matching is better suited than more parametric approaches, as it flexibly controls for couples' individual heterogeneity. After matching, we observe only minor differences between the group of moving couples and their matched controls, which shows that the matching does well in choosing suitable control couples.

3 Empirical Strategy

We are interested in the effect of a job-related long-distance move (as defined above) on both partners' labor market careers. In our baseline specification, the counterfactual is defined as not experiencing a long-distance move in the time period under study, while a job change is allowed but not a must. We are estimating the effect of moving for job reasons of at least one partner versus not having or not taking this opportunity. The effect to be identified is therefore the effect of (searching for and) receiving and accepting a job in a different region versus not accepting (and likely also not searching and/or not receiving) a job offer apart.

The empirical challenge in estimating the effects of long-distance moves stems from the fact that couples decide whether to move or not. We attempt to solve this problem using a selection on observables strategy. In addition, we control for time-invariant unobservables through subtracting pre-move outcomes. On average, there are large differences in the characteristics of moving and non-moving couples. As those differences potentially relate to future labor market outcomes, this may lead to biased estimates for the returns of moving. If couples with low labor market prospects are more likely to move, we may falsely attribute these lower prospects to moves and underestimate the returns. Conversely, if couples with high labor market prospects are more likely to move, the estimated returns may be upward biased. To overcome this bias, we apply *difference-in-difference matching* (Heckman et al., 1998, 1997), which we believe is particularly suited in our setting. First, our administrative data allow for a rich propensity score specification that takes advantage of information on both spouses' labor market histories. Together with personal, job, and regional characteristics, this enables us to control for important differences. Second, we believe that matching is better suited than other empirical strategies to control for observables because movers are a heterogeneous group. Finally, the large sample size of our data allows us to choose suitable control couples from the large set of non-moving couples and ensures that the matched sample is well-balanced. The use of the difference-in-difference matching estimator, as opposed to a pure matching estimator, will take into account time-invariant unobservables that affect earnings, for example innate ability.

3.1 Parameter of Interest and Identifying Assumptions

Our goal is to estimate the *average treatment effect on the treated* (ATT) (moving couples) who are experiencing a long-distance move. Following Rubin (1974), we define treatment effects in terms of potential outcomes. Let T_i denote whether a couple moves, with $T_i = 1$ if couple i moves and $T_i = 0$ if it does not. Further, let $\tilde{Y}_{it}(1)$ denote the potential outcome in year t if couple i moves and $\tilde{Y}_{it}(0)$ if it does not. Using the *difference-in-difference matching estimator*, we measure potential outcomes relative to year t' (pre-move year $t-3$), to avoid

disturbance by anticipation effects (Ashenfelter (1978))[7]. The potential outcome in year t is therefore given by $\tilde{Y}_{it}(1) = Y_{it}(1) - Y_{it'}(1)$ if couple i moves and by $\tilde{Y}_{it}(0) = Y_{it}(0) - Y_{it'}(0)$ if it does not move. The ATT parameter in year t is then given by:

$$\tau_t = E\{Y_{it}(1) - Y_{it'}(1)|T_i = 1\} - E\{Y_{it}(0) - Y_{it'}(0)|T_i = 1\}. \tag{2}$$

Because we can observe the labor market outcomes of moving couples, it is possible to calculate the first term of the right-hand side of equation (2). However, we do not observe the second term of the right-hand side of equation (2) – the potential outcome for the moving couples had they not moved. To obtain these counterfactual outcome, we use propensity score matching. For each moving couple, we assign 10 control couples using the 10 non-moving couples with the closest propensity scores. The propensity score is defined as $p(X) = Pr(T_i = 1|X)$ where X is a set of covariates realized prior to moving. Rather than matching exactly on covariates, matching on the propensity score reduces the dimensionality problem, because the set of covariates we use in our specification is large.

To estimate our parameter of interest, two assumptions have to hold. The main assumption underlying our approach is the *conditional independence assumption* (CIA):

Assumption 1. $E\{Y_{it}(0) - Y_{it'}(0)|p(X), T_i = 1\} = E\{Y_{it}(0) - Y_{it'}(0)|p(X), T_i = 0\}$

which has to hold for all periods t and t'. Assumption 1 states that, conditional on the propensity score, the potential outcomes of moving couples, had they not moved, have to be the same as for control couples. Note that in assumption 1 the potential outcomes are measured relative to pre-move year t'. This allows for time-invariant differences in outcomes between movers and non-movers and therefore eliminates bias due to time-invariant unobserved heterogeneity. As our parameter of interest is the ATT, this assumption only needs to hold for $\tilde{Y}_{it}(0)$ and not for $\tilde{Y}_{it}(1)$.

The CIA involves that we observe everything that drives the decision to move and the *change* in outcomes, for example earnings. With recent and long-term labor market histories, age, education, occupation, and further personal and regional characteristics, we observe important drivers of the treatment decision (Nivalainen (2004), Ham et al. (2011), Compton and Pollak (2007), Eliasson et al. (2014)) and earnings development. However, an exemplary case for the CIA to fail might be information about potential future job loss, which may increase the probability to move. While this information is available to the individuals, it cannot be observed in the data. In this case, movers may be matched with control people with too stable employment prospects. This would lead to downward biased estimates in the case of positive treatment effects. Still, we believe that accounting for the combination of recent labor market history, the

[7] Smith and Todd (2005) show in an application of training program evaluation that difference-in-difference matching estimators are substantially less biased than cross-sectional matching estimators. See also Ham et al. (2011).

region's unemployment rate, and occupation, our matched controls will do relatively well, even in specific cases where we cannot observe relevant pieces of information. In all those cases where people experience unemployment before moving, we can of course use this information in the propensity score.

In addition to assumption 1, we need to make the *common support assumption*:

Assumption 2. $p(X) = Pr(T_i = 1|X) < 1$.

This assumption requires that we can find a non-moving (control) couple for each moving couple. In our setting, this assumption is satisfied. For the full sample, an illustration of the propensity score distributions before and after matching is shown in online appendix section C.

3.2 Propensity Score Specification

We use propensity score matching to choose a suitable control group for the moving couples that is as similar as possible to the group of movers. For each moving couple, the potential pool of control couples includes all couples that do not move during the treatment years. Note that there is neither a restriction that couples in the control group have to experience a job change, nor that they cannot move in future years. In a robustness check, we also show results for a control group of couples experiencing a job change to show that our results are determined by the move and not by one spouse's job change.

Using a large set of covariates, we estimate propensity scores separately for the full sample of couples and for each subsample. We estimate logit models based on covariates that according to theory may affect the probability of moving as well as the outcome variables. We estimate these models by pooling over the five treatment years to ensure that we have enough observations which turns out to be important for subsample analyses. In our specification, we include personal characteristics of both spouses, characteristics of the region of origin as well as detailed labor market histories. As recent studies have shown in different contexts, conditioning on past labor market histories is crucial in estimating treatment effects (Andersson et al. (2013), Caliendo et al. (2017), Leung and Pei (2020)). Finally, we include 53 covariates in our specification and pay particular attention to control for spouses' past employment [8] [9]. Coefficients of the logit

[8] Our specification includes the following covariates:
(i) *Personal characteristics (for both spouses)*: age, age^2, education (four dummies), tenure at current job (five dummies, censored in 1998), dummy for non-German citizenship, years in region (four dummies, censored in 1998), dummy for employment relationship (four dummies), requirement level of job (five dummies), occupation (10 dummies) (all measured in pre-move year $t-1$), days employed per year in $t-1$, $t-2$ and $t-3$, interaction between days employed in $t-1$ and $t-2$ ($t-3$), yearly labor earnings in $t-1$, $t-2$ and $t-3$, interaction between yearly labor earnings in $t-1$ and $t-2$ ($t-3$), days receiving unemployment benefits per year in $t-1$, $t-2$ and $t-3$, dummy for positive yearly labor earnings in $t-1$, $t-2$ and $t-3$
(ii) *Household characteristics*: age of first child (8 dummies), dummy for moving year (all measured in $t-1$)
(iii) *Regional characteristics*: district unemployment rate, GDP per capita (all measured in $t-1$).

[9] If necessary, we adjust the logit specification for subsamples in which the propensity score models do not converge to a solution with the full set of covariates. In these cases, we estimate the model by eliminating a few covariates.

model for the full sample are shown in online appendix section B.1.

3.3 Matching Balance Check

To show covariate balance, we apply two balancing tests to check whether our matched sample is sufficiently balanced. First, we test whether the means of all covariates (included in the propensity score specification) differ significantly between treatment and control group using t-tests. Second, we report the standardized differences for each covariate between treatment and control group[10]. Online appendix B.2 displays the results of the balancing tests. For each covariate, columns 3 and 4 show the standardized differences and the p-values of the t-tests before matching, while columns 6 and 7 show the analogous statistics after matching. The balancing tests show that the matched sample is well-balanced with respect to the included covariates. For all included covariates, t-tests fail to reject equality of means despite the large sample size and the standardized differences are close to 0. Out of 104 characteristics, only 4 are not within the 0.03 rule-of-thumb, and even for those characteristics standardized differences never exceed 0.035[11].

3.4 Matching Estimator

Based on the predicted propensity scores from the logit models, for each moving couple we choose the ten closest matches from the set of control couples. Note that we use uniform weights and allow each control couple to be matched to more than one treated couple (matching with replacement). By using more than one control couple as counterfactual, bias of the estimated treatment effect increases but variance is reduced. Because we have a very large set of control couples, we think it is appropriate to use more than one control couple to increase the precision of the estimates. The ATT parameter in each year t is then given by:

$$\hat{\tau}_t = \frac{1}{N}\sum_{i=1}^{N} W_i\Big(\tilde{Y}_{it} - \frac{1}{10}\sum_{j\in J(i)} \tilde{Y}_{jt}\Big), \tag{3}$$

where $N = \sum_{i=1}^{N} W_i$ is the number of moving couples, $\tilde{Y}_{it} = Y_{it} - Y_{it'}$ is the outcome for moving couple i, $\tilde{Y}_{jt} = Y_{jt} - Y_{jt'}$ is the outcome for control couple j, $J(i)$ is the set of controls for couple i and $t \in [-3, 5]$ are the years before or after the move.

It is well-known that when estimating the ATT the usual variance estimation is not valid (Abadie and Imbens, 2006, 2008). The problem is that the estimated variance of the ATT should also

[10] The standardized difference is given by: $\frac{(\bar{X}_1 - \bar{X}_0)}{(S_1^2 - S_0^2)^{\frac{1}{2}}}$, where $\bar{X}_1$ and $\bar{X}_0$ are the sample means of covariate X in the treatment and control group and S_1^2 and S_0^2 are the sample variances in the treatment and control group, respectively (Rosenbaum and Rubin (1985)).

[11] According to Caliendo and Kopeinig (2008), a standardized difference below 0.03 or 0.05 can be regarded as sufficient.

include the variance due to the estimation of the propensity score that is estimated prior to matching. Couples are matched based on the estimated value of the propensity score rather than on the true value. The standard bootstrap is not valid when using matching with replacement and a fixed number of matches (Abadie and Imbens, 2006, 2008), as we do in this study. Recently, Abadie and Imbens (2016) derived a variance adjustment for these matching estimators that takes into account the first step estimation of the propensity score. This variance adjustment was implemented in this paper.

4 Effects of Long-distance Moves

4.1 Effects on Labor Earnings, Employment and Wages

To measure the effects of long-distance moves, we begin by investigating the effects on yearly labor earnings, daily wages, and days employed per year for moving men and women. We graphically present the effects of moving from three years before the move up to five years after the move for a balanced panel of couples.

Yearly Labor Earnings

Panel A of figure 1 shows the effects on yearly labor earnings for the full sample of moving men and women, respectively. It plots the average treatment effects for the treated (mover) with the corresponding 95% confidence intervals. The solid vertical line marks the time of the move. Note that treatment effects are defined as outcome differences between movers and matched controls relative to year $t-3$, which implies that the estimated effects are zero in this year, by construction. The effects on yearly labor earnings are close to zero prior to the move, as expected due to including pre-move earnings in the propensity score. The effects for moving men are shown in blue. We observe that men's earnings increase after the move. Moving men earn around €450 (1.0%%) more compared to the matched control group (non-mover) in the first year after the event. The treatment effect increases considerably over time and moving men earn around €2,600 (5.1%) more relative to controls in year $t+5$. In contrast, women (in red) suffer significant earnings losses in the first two years after the move. Compared to their matched controls, moving women earn around €2,000 (10.5%) less in the first year after the move. Their earnings recover slowly over time, but even in year $t+5$ moving women cannot realize significant moving gains. For an illustration of the mean yearly labor earnings of movers and matched non-movers, see online appendix section C.

In a next step, we look at total household labor earnings (figure 1 panel B), defined as the joint labor earnings of both spouses. We expect spouses' joint earnings to increase after a move, as in the standard human capital framework, couples will only move if the returns of moving exceed the costs (Mincer (1978)). We observe a significant negative effect on total household earnings in the first year after the move. This effect is driven by the large losses in moving women's earnings, which cannot be offset by increases in moving men's earnings. Over time, the total

household earnings of moving couples increase sharply. In year $t + 5$, the total household earnings of moving couples are around €3,000 higher than those of matched control couples. However, even if couples can realize significant positive returns to moving some years after the move, the negative effect on women's individual earnings potentially have severe negative implications on a woman's position in the couple. A large number of papers has modeled the decision-making process of households through collective models in which the spouse with more bargaining power has more weight in the decision-making process (Browning and Chiappori (1998), Chiappori (1992)). Women's bargaining power within the couple, typically proxied by their relative earnings, is expected to decrease after the move due to the decrease in their earnings. Joint moves of couples can therefore negatively influence a woman's position within the couple.

Days Employed per Year

Panel C of figure 1 shows the effects of a move on the total days employed per year. In the first year after the move, men (in blue) experience a small drop in working days per year. Relative to their matched controls, moving men work about five fewer days per year. After this initial drop, the days employed increase significantly in the following years. However, we do not observe that the treatment effects on employment increase over time as observed for men's earnings. In contrast, moving women's employment (in red) decreases significantly in the first two years after the move. In the first year after the move, moving women work about 31 fewer days per year than their matched controls. Note that the outcome variables include zeros for non-working spouses. Hence, this sharp drop in working days may be driven by women who do not work at all in a given year. The employment of moving women then starts to recover and employment losses are small at the end of the observation period.

Daily Wages

To investigate whether spouses' earnings responses are driven by changes in employment or in wages, panel D of figure 1 shows the effects of a move on the daily wages of men and women. Note that by construction wages are only defined for working spouses. Because we use a difference-in-difference matching estimator and evaluate outcomes relative to $t - 3$, this figure only includes spouses who are working in $t - 3$ (2,286 moving and 117,749 non-moving couples). From figure 1, we observe that men's daily wages (in blue) rise significantly after the move and that this effect increases over time. The large increases in men's earnings are therefore largely driven by increases in their wages rather than by increases in employment. In contrast, effects on women's wages (in red) are close to zero in all post-move years. The large decrease in women's earnings can therefore be explained by the large decrease in employment.

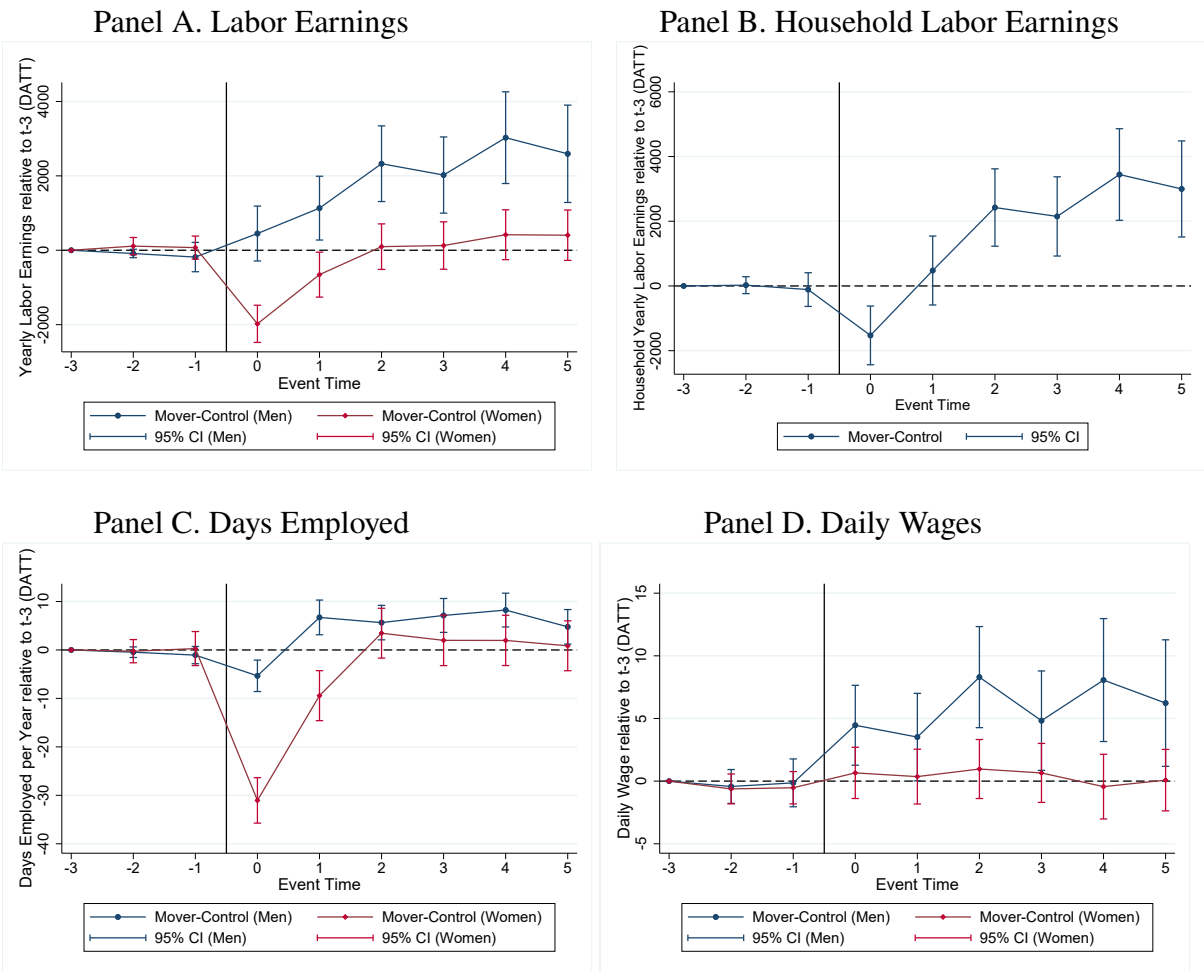

Figure 1: Effects on Labor Earnings, Employment, and Wages

Notes: Panel A displays the ATT on the yearly labor earnings for men (blue) and women (red) relative to year $t-3$ (after propensity-score matching) with the corresponding 95% confidence intervals. Standard errors are computed following Abadie and Imbens (2016). Panel B displays the ATT on the household yearly labor earnings, panel C the ATT on the days employed per year and panel D the ATT on daily wages.

4.2 Heterogeneity

Our results based on the full sample indicate that moving men realize significant positive returns from moving, while women suffer large losses in the first years after the move. In this section, we investigate heterogeneity in treatment effects for different types of couples. In a first step, we examine how returns to moving differ, depending on whether couples move in favor of men's or women's job opportunities. In a second step, we seek to capture differences in treatment effects by spouses' relative pre-move earnings. Finally, we investigate heterogeneity among other dimensions, such as the role of spouses' pre-move employment status, the effect of age and education as well as the impact of childbirth and differences by regions and moving years. To estimate the average treatment effect on the moving couples for the respective subgroups, as before, we match moving and non-moving couples by choosing ten control couples for each moving couple. However, we do not use all non-moving couples as potential controls. For each non-moving couple, we then only allow for matches within the same subgroup, for

example the potential controls for a couple in the first age group are all non-moving couples in the same age group.

Move for Men's versus Women's Job Opportunities

In the standard human capital framework, couples will only move if the returns of moving exceed the costs (Mincer (1978)). This implies that couples will also move if the returns of one spouse exceed the losses of the other spouse. Typically, we do not expect that both spouses will receive job offers at the same future location. Couples will often move in favor of one spouse's job opportunities while the other spouse will be the "tied mover" (Mincer (1978), Sandell (1977)). In this section, we present results depending on whether couples move in favor of men's or women's job opportunities.

We classify a move in favor of the man as a move in which the man is the spouse who starts his new job first after the move. A move in favor of the woman is defined accordingly. In our data, we have precise information on the start and end date of each job, which the employer has to report. Using this information on job start to define for which spouse the couple moved, is clearly an approximation, as we do not have direct information on the reasons why couples move. For each moving couple, we classify moves into two categories: i) move in favor of the man or ii) move in favor of the woman. This leaves us with 1,931 (51.6%) moves in favor of the man and 1,540 (41.1%) in favor of the woman [12]. For each non-moving couple, we also classify whether the couple experienced a job change in favor of the man or the woman. We then only allow for matches within the same cell, for example the potential controls for a couple who moves in favor of the man are all non-moving couples who also experienced a job change in favor of the man.

Figure 2 panel A displays the effects of a long-distance move on yearly labor earnings for the two subgroups of moving men and women, respectively. We observe large differences in the estimated treatment effects between the two groups of couples. For couples moving in favor of men's job opportunities, the yearly labor earnings of moving men (solid blue) increase significantly after the move relative to their matched controls. In contrast, for the women of those couples (solid red), we observe large decreases in their yearly labor earnings. For couples moving in favor of women's job opportunities, increases in women's yearly labor earnings (dashed orange) are much smaller than those of men if couples move for the man. If couples move in favor of the woman, men's labor earnings (dashed light blue) decrease in the first two years after the move. However, men realize small positive returns in later years. This shows that there is an asymmetry in the returns to moving between men and women. In figure 2 panel B, we also present results for spouses' employment and in section C of the online appendix we show the means of yearly labor earnings and days employed for movers and matched non-movers for the two subgroups.

[12] We identify 274 (7.3%) moves in which both spouses start their new job at exactly the same date. We do not use these couples in this subgroup analysis.

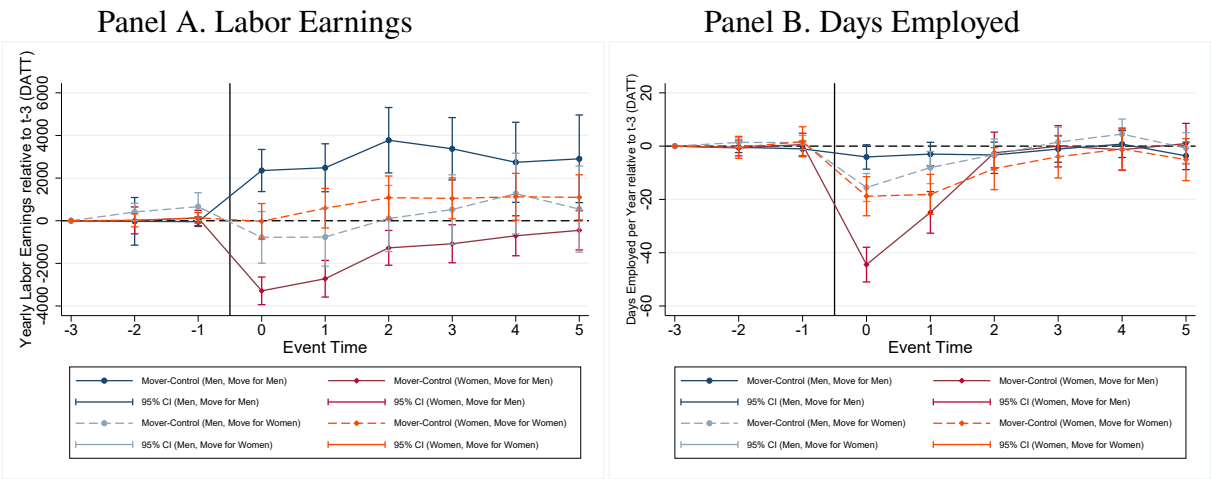

Figure 2: Effects on Individual Labor Earnings and Employment (by Move for Men versus Move for Women)

Note: Panel A displays the ATT on the yearly labor earnings for men (blue) and women (red) relative to year $t-3$ for subgroups defined by whether couples move in favor of the man or the woman (after propensity-score matching) with the corresponding 95% confidence intervals. Standard errors are computed following Abadie and Imbens (2016). Panel B displays the ATT on the days employed per year.

If we look at the effect on household labor earnings between the two subgroups (figure 3), we observe only small differences in the estimated treatment effects. This indicates that, while there are higher gains as well as higher losses if the couple moves in favor of the man's career, on the household level returns to moving are not significantly different, whether a couple moves in favor of the man or the woman.

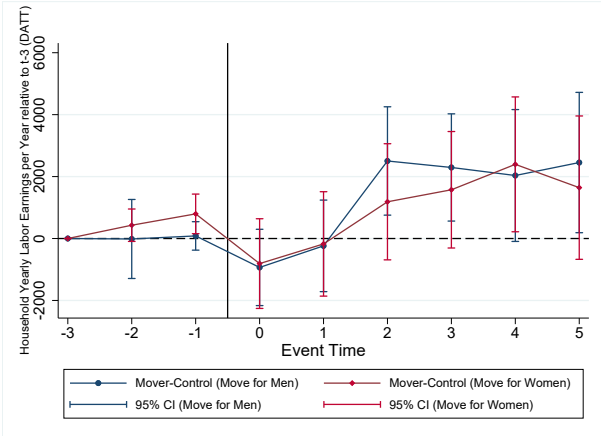

Figure 3: Effects on Household Labor Earnings (by Move for Men versus Move for Women)

Notes: This figure displays the ATT on the household yearly labor earnings relative to year $t-3$ for subgroups defined by whether couples move in favor of the man (blue) or the woman (red) (after propensity-score matching) with the corresponding 95% confidence intervals. Standard errors are computed following Abadie and Imbens (2016).

Pre-move Relative Earnings

We expect that the returns to moving depend on spouses' relative household earnings. Whether a partner is the main breadwinner of the household or contributes a minor share of household earnings should influence spouses' post-move labor market outcomes. From collective models, we would expect that returns to moving are larger for spouses who are the main breadwinners, as they would have more bargaining power in the couple and couples should move in favor of those spouses (Browning and Chiappori (1998), Chiappori (1992)). However, it is also possible to expect that returns to moving are the largest for spouses who only earn a small proportion of household earnings. For example, if a spouse is unemployed before the move, her/his relative household earnings are zero. If the unemployed spouse finds a job after the move, the returns to moving will be large. We expect her/his returns to moving to be considerably higher than those of a spouse with high relative earnings. Ex ante, the expected effects are not clear.

For each couple, we begin by defining spouses' relative earnings as: $\frac{\text{individual labor earnings}}{\text{total household labor earnings}}$. We compute the relative earnings for each couple over the pre-event years $t-5$ to $t-2$ and take the average over those years. Pre-move year $t-1$ is not included to exclude potential pre-event effects on spouses' earnings (Ashenfelter (1978)). We then define four groups based on spouses' relative earnings before the move: (i) woman $0-24\%$, man 75-100%; (ii) woman $25-49\%$, man 50-74%; (iii) woman $50-74\%$, man 25-49% and (iv) woman $75-100\%$, man 0-24%. In our sample, we identify $1,614$ moving couples in group (i), $1,163$ in group (ii), 671 in group (iii) and 297 in group (iv), which shows that for most moving couples the man is the main breadwinner before the move. We then match moving and non-moving couples by only allowing matches within the same earnings group.

Figure 4 shows the results for the treatment effects on spouses' yearly labor earnings.

To the left, panel A displays the results for two groups of couples: First, those in which the woman earns $0-24\%$ and the man $75-100\%$ of household earnings and second, the opposite

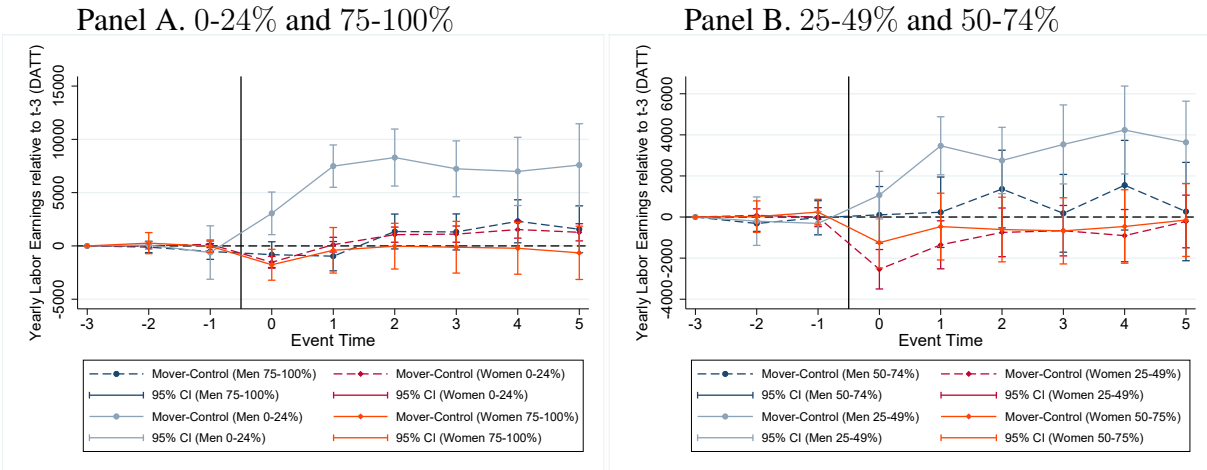

Figure 4: Effects on Individual Labor Earnings (by Relative Household Earnings before Move)

Notes: This figure displays the ATT on the yearly labor earnings for men (blue) and women (red) relative to year $t-3$ for subgroups defined by pre-move relative household earnings (after propensity-score matching) with the corresponding 95% confidence intervals. Standard errors are computed following Abadie and Imbens (2016).

case, in which the woman earns $75-100\%$ and the man $0-24\%$. From figure 4, we observe that the yearly labor earnings of moving men who earn only $0-24\%$ of households earnings before the move (solid light blue) increase significantly after the move relative to their matched controls. In the first year after the move, the yearly labor earnings of those men are about €3,100 (15.3%) higher than those of matched controls and in year $t+5$ the labor earnings are even about €7,600 (26.4%) higher. In contrast, the labor earnings of moving women who earn $0-24\%$ of the pre-move households earnings (solid orange) only increase significantly in years $t+2$ to $t+5$. Even in those years, the estimated treatment effects of women are much smaller than those of men in the same group of relative earnings. In year $t+5$, the estimated treatment effect on the earnings of those women is about €1,300 (8.8%), compared to €7,600 (26.4%) for men in the same group of relative pre-move earnings. Looking at the treatment effects for the group of men and women who earn $75-100\%$ of household earnings, we observe that the treatment effects for those men (dashed blue) are much smaller than for men who earn only $0-24\%$ (solid light blue). For women in this group of relative earnings (dashed red), the estimated treatment effects are also smaller compared to women who earn only $0-24\%$ (solid orange) but the difference in the estimated effects between the two groups is much smaller than for men. Even in this group of relative earnings, gender differences in returns to moving are observable. To the right, figure 4 panel B shows the analogous results for the other two groups of couples: those in which the woman earns $25-49\%$ of household earnings and the man 50-75% and those in which the woman earns $50-74\%$ and the man $25-49\%$. From this figure, we also observe that the estimated effects on labor earnings are the largest for men with low relative earnings before the move (solid light blue). The treatment effects for men who earn $50-74\%$ (dashed blue) of household earnings are much smaller. With respect to women in the same earnings groups, the estimated effects are much smaller for both groups, compared to

men.

To summarize, we find that the significant increase in men's labor earnings after a move is largely driven by significant increases in labor earnings of men with low relative earnings before the move. For women with low relative earnings, increases in labor earnings are however much smaller. For each group of relative earnings, we observe gender differences in the estimated treatment effects. The results for the effects on spouses' employment are shown in figure 5.

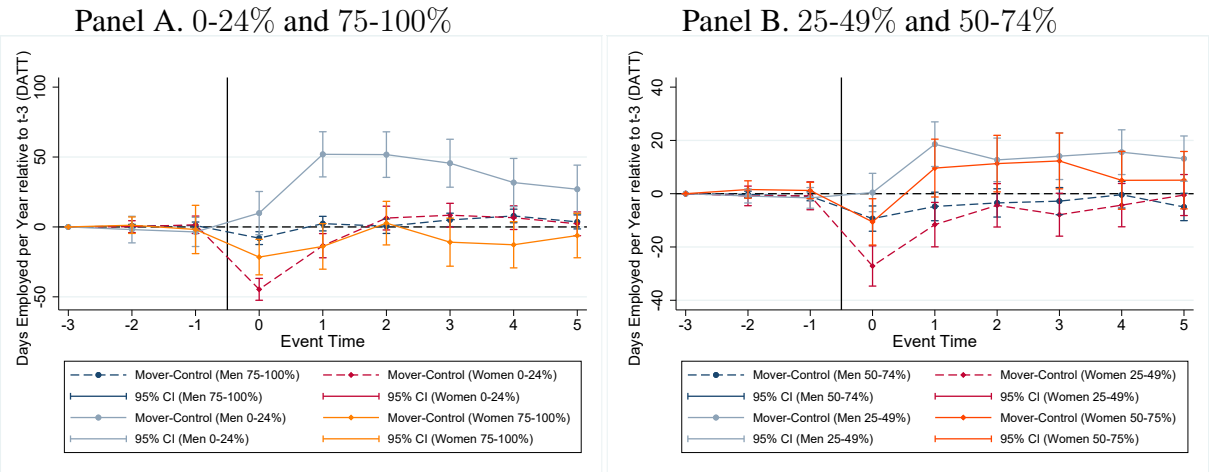

Figure 5: Effects on Individual Employment (by Relative Household Earnings before Move)

Notes: This figure displays the ATT on the days employed per year for men (blue) and women (red) relative to year $t-3$ for subgroups defined by pre-move relative household earnings (after propensity-score matching) with the corresponding 95% confidence intervals. Standard errors are computed following Abadie and Imbens (2016).

Section C of the online appendix shows the means of yearly labor earnings and days employed for movers and matched non-movers for the different subgroups. Finally, if we look at household labor earnings between the different subgroups (figure 6), we observe large differences in the estimated treatment effects between the four groups of couples. On the household level, the estimated returns are the largest for couples in which the man only earns $0-24\%$ of pre-move relative earnings and the woman $75-100\%$ (dashed blue), while the returns are the smallest for couples in which the man earns $50-74\%$ and the woman $25-49\%$ (solid orange).

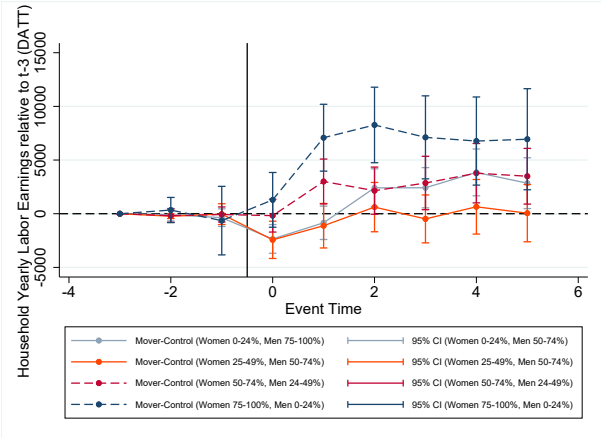

Figure 6: Effects on Household Labor Earnings (by Relative Household Earnings before Move)

Notes: This figure displays the ATT on the yearly household labor earnings relative to year $t-3$ (after propensity-score matching) with the corresponding 95% confidence intervals for subgroups defined by pre-move relative earnings. Standard errors are computed following Abadie and Imbens (2016).

Other Heterogeneities

So far, we have shown heterogeneity in treatment effects with respect to spouses' pre-move relative earnings and by whether couples move in favor of the man or the woman's job opportunities. Now, we turn to investigating heterogeneity with respect to other important characteristics of spouses. To study the effects of moving for different subgroups, as before, we match moving and non-moving couples but only allow for matches within the same subgroup. Table 2 shows the effects on men's and women's yearly labor earnings, respectively. Columns 1 and 2 of table 2 show the average treatment effect for the first two post-move years and columns 3 and 4 the average treatment effects for years $t+2$ to $t+5$ after the move. Note that the treatment effects are measured as differences in outcomes between movers and matched controls relative to the average outcome in the pre-move period ($t-3$ to $t-1$). The treatment effects in the pre-move period are zero by construction.

Table 2: Effects on Yearly Labor Earnings by Subgroups

	DATT post-move years 1-2		*DATT post-move years 3-6*		
	Men	Women	Men	Women	No. movers
	(1)	(2)	(3)	(4)	(5)
All	881.127	-1377.682	2582.499	200.589	3744
	(336.774)	(223.967)	(441.266)	(272.913)	
District unemployment rate					
Below median	-483.676	-2137.185	789.482	-535.875	1668
	(517.418)	(347.453)	(676.905)	(416.112)	
Above median	2289.354	-623.075	4373.259	888.671	2074
	(422.163)	(296.197)	(547.631)	(348.668)	

(continued)

Table 2: Effects on Yearly Labor Earnings by Subgroups (continued)

	DATT post-move years 1-2		*DATT post-move years 3-6*		
	Men	Women	Men	Women	No. movers
	(1)	(2)	(3)	(4)	(5)
Age group					
20-29	2614.580	115.568	4550.261	850.238	738
	(537.976)	(444.203)	(882.705)	(586.495)	
30-39	1359.777	-1038.717	2499.349	449.059	1760
	(489.693)	(344.017)	(636.306)	(401.177)	
40-50	-600.795	-2078.735	1310.207	290.854	1243
	(626.646)	(346.718)	(773.344)	(403.203)	
Education group					
Power	758.349	-2548.352	1179.005	-1880.954	452
	(1386.564)	(1136.117)	(1843.725)	(1341.130)	
Part-power man	-810.991	-2536.215	2332.328	424.047	574
	(1153.534)	(522.740)	(1547.219)	(661.358)	
Part-power woman	2907.818	-1888.450	2979.822	-2948.568	245
	(1387.122)	(1224.458)	(1561.953)	(1420.100)	
Low-power	1133.861	-775.674	2470.470	944.773	2469
	(276.891)	(198.438)	(390.124)	(252.644)	
Children					
No birth in event years	828.235	-1516.074	2314.822	-8.780	2863
	(377.858)	(214.488)	(475.189)	(267.743)	
Birth in pre-move years	834.606	-2302.767	2727.378	-1320.255	383
	(1049.710)	(732.693)	(1265.428)	(889.489)	
Birth in post-move years	1732.152	-2203.232	4277.792	-2304.025	499
	(831.842)	(743.107)	(1327.941)	(729.149)	
Employment status					
Both non-emp.	6603.255	2925.752	8048.576	4856.889	159
	(942.887)	(546.090)	(1177.479)	(883.015)	
Man non-emp., woman emp.	8795.391	-2172.789	9209.506	-1989.913	204
	(1196.911)	(617.233)	(1828.656)	(776.768)	
Woman non-emp., man emp.	1867.407	1235.137	3973.225	2619.035	587
	(721.466)	(414.303)	(1049.426)	(504.672)	
Both emp.	-6.773	-1863.537	1687.160	-441.069	2791
	(397.417)	(274.502)	(522.473)	(328.741)	

(continued)

Table 2: Effects on Yearly Labor Earnings by Subgroups (continued)

	DATT post-move years 1-2		*DATT post-move years 3-6*		
	Men	Women	Men	Women	No. movers
	(1)	(2)	(3)	(4)	(5)
Moving year					
2008	647.114	-1082.521	3357.589	1011.207	1002
	(571.790)	(408.437)	(775.416)	(487.439)	
2009	1211.745	-1509.145	1670.049	-993.217	949
	(625.976)	(431.140)	(806.168)	(507.236)	
2010	746.982	-1487.008	1941.848	-377.629	724
	(752.222)	(503.254)	(925.897)	(590.629)	
2011	1533.084	-1005.114	3888.552	138.372	625
	(817.438)	(555.5457)	(1086.712)	(682.918)	
2012	1186.203	-1506.760	2474.741	492.523	436
	(1069.433)	(640.930)	(1322.577)	(780.560)	

Notes: Columns 1 and 2 show the estimated treatment effect on the yearly labor earnings of moving men and women in the two years after the move, relative to the pre-move period. Columns 3 and 4 show the analogous results for years 3 to 6 after the move. Column 5 shows the number of moving couples in the respective group. Standard errors (in parentheses) are computed following Abadie and Imbens (2016).

Region

In regions with a depressed labor market, spouses' job opportunities are limited. Even if spouses are willing to change jobs for higher earnings, there might be only a few opportunities to do so. Moving to a new local labor market could increase spouses' opportunity set. It is therefore expected that returns to moving will be larger for couples who previously lived in regions with poor labor market conditions. To evaluate the role of local labor market conditions, we divide our sample of couples into two groups: couples who have lived in regions with unemployment rates above median (in pre-move year $t-1$) and couples who have lived in regions with below-median unemployment rates. As expected, our results in table 2 show that returns to moving are considerably larger for both spouses if couples have lived in regions with above-median unemployment rates. Still, the estimated treatment effects on labor earnings are much larger for men than for women.

Age Group

Previous research showed that young individuals are more likely to move (Polachek and Horvath (2012)) and that the returns to moving are larger for younger individuals (Bartel (1979)). To test whether the treatment effects vary with respect to spouses' age, we define age groups based on the average of the spouses (in pre-move year $t-1$). We define age groups as follows: (i) $20-29$, (ii) $30-39$ and (iii) $40-50$. The results in table 2 show that the returns to moving decline with increasing age. For both spouses, the average treatment effects on yearly labor earnings are the largest for younger couples and the lowest for older couples. Again, we observe gender differences between men and women in the same age group.

Education Group

In the literature on migration of couples, some studies focused on investigating the relationship between the probability to migrate to large metropolitan areas and the education profile of

spouses (Compton and Pollak (2007), Costa and Kahn (2000)). While those studies focused on how the migration decision varies by spouses' education profile, we now examine how the returns to migration vary with respect to spouses' education. To do so, we follow Compton and Pollak (2007) and Costa and Kahn (2000) and distinguish between four types of couples (measured in pre-move year $t-1$): (i) power (man and woman hold a university degree), (ii) part-power man (only the man holds a university degree), (iii) part-power woman (only the woman holds a university degree) and (iv) low-power (neither spouse holds a university degree). In our sample, most couples are low-power couples $(2,469)$, while only 452 couples are power couples and 574 are part-power man and 245 are part-power woman. The results of the yearly labor earnings for the different types of moving men and women are shown in table 2. We observe that for men, the estimated treatment effects on earnings are the lowest for couples in which the man holds a university degree but the woman does not while effects are the largest for couples in which the woman holds a university degree but the man does not. For women, earnings losses from moving are the largest for couples in which the woman holds a university degree (either power or part-power woman couples) while small earnings gains are observed for low-power couples. Note that spouses' education is likely to be correlated with spouses' pre-move employment status. If spouses holding a university degree are more likely to be employed, we would expect that the returns to moving are smaller for them compared to non-employed spouses, which is in fact what we observe.

Childbirth

If a couple's fertility decision is correlated with their decision to move, the large earnings losses of women could be due to the birth rather than the move. Unfortunately, our data does not include detailed information on the number of children and their time of birth. In our data, we identify the birth of a first child through maternity leave spells following Müller and Strauch (2017).[13] We show the frequency of first births of moving couples relative to control couples over the event time in online appendix section C. From this figure, it can be observed that there exists a correlation between the birth of the first child and the move. As shown in table 2, it can be observed that earnings losses after a move are lower for women who do not give birth during the observation period compared to women who give birth. For those who give birth during the observation period, earnings losses are slightly larger for women who give birth in the post-move years. This might be a causal effect of moving, as women may find themselves in a less stable job situation due to the move when getting pregnant or the fertility decision itself may be part of the effect of the move. However, it is also possible that family planning is endogenously related to the moving decision.

Employment Status

[13] Our data have the drawback that we can only identify the date of birth for women who are either employed, unemployed, or in training measures before they give birth. The algorithm of Müller and Strauch (2017) is most reliable for identifying the date of birth for the first child, as women often give birth to a second child before returning to the labor market. We therefore focus on the firstborn child throughout our analysis.

Non-employment is an important factor that affects spouses' careers. It is also a decisive factor that influences the returns to moving, which we expect to be considerably larger for non-employed spouses compared to employed ones. To present heterogeneity based on spouses' employment status in pre-move year $t-1$, we consider two employment states: (i) employed and (ii) non-employed. We define a spouse to be non-employed if she/he receives unemployment benefits for more than 182 days in the year before the move or if her/his yearly labor earnings are zero. Since we observe one employment status for each spouse, this leaves us with four groups: (i) both non-employed, (ii) man non-employed, woman employed, (iii) woman non-employed, man employed and (iv) both employed. Note that in most couples both spouses are employed in the year prior to the move (2,791), while the number of couples in which both spouses are unemployed is considerably lower (159). As expected, it can be seen from table 2 that the returns to moving are much smaller for employed spouses than for unemployed ones. However, we also observe gender differences. While men can realize positive returns from moving even if they are employed before moving, this does not apply to women. Employed women suffer large losses in labor earnings.

Moving Year

In our analysis, we have so far pooled all observations over the treatment years. To investigate whether there are differences in the estimated effects between the five treatment years, we split our sample by moving year and present the results in table 2[14]. There are some differences in the estimated treatment effects on spouses' earnings between the different moving years showing no specific pattern.

In online appendix section D, we also present the results of the average treatment effects on the days employed per year by the subgroups that were studied previously.

4.3 What Drives Spouses' Earnings Responses?

The previous sections showed that women suffer large losses in yearly labor earnings in the first years after the move, while men realize significant positive returns. In this section, we first examine whether the earnings responses of spouses are driven by spouses moving towards larger or higher-paying firms. Second, we investigate if spouses change their job requirement level or switch industry or occupation after the move. Note that in this part of the analysis, we only consider couples in which spouses are employed from $t-3$ to $t+5$[15].

Average daily wage of firm

Panel A of figure 7 shows the effects of a move on the average daily wage of the firm for moving men and women, respectively. More precisely, the firm's average daily wage is defined as the mean imputed gross daily wage of all its full-time employees. Note that this measure neither includes the wages of marginally or part-time employed workers, nor those of apprentices. For

[14]We identify 1,002 long-distance moves in 2008, 949 in 2009, 724 in 2010, 625 in 2011 and 436 in 2012.

[15]This results in a balanced panel of 2,286 moving couples and 117,749 control couples.

men, we observe increases in the average daily wage paid by the firm for each year after the move (compared to matched controls), although at the 5% level effects are insignificant. For women, we observe decreases in the average daily wage of the firm after the move.

Firm size

Panel B of figure 7 displays the effects on firm size, which we measure as its total number of employees. The results show that after a move men tend to switch to firms (not significant at the 5% level) that are on average slightly larger, while for women we do not observe an effect on firm size. Overall, our results show that men tend to move to slightly larger and on average higher-paying firms, which may explain some of their large earnings increases after a move.

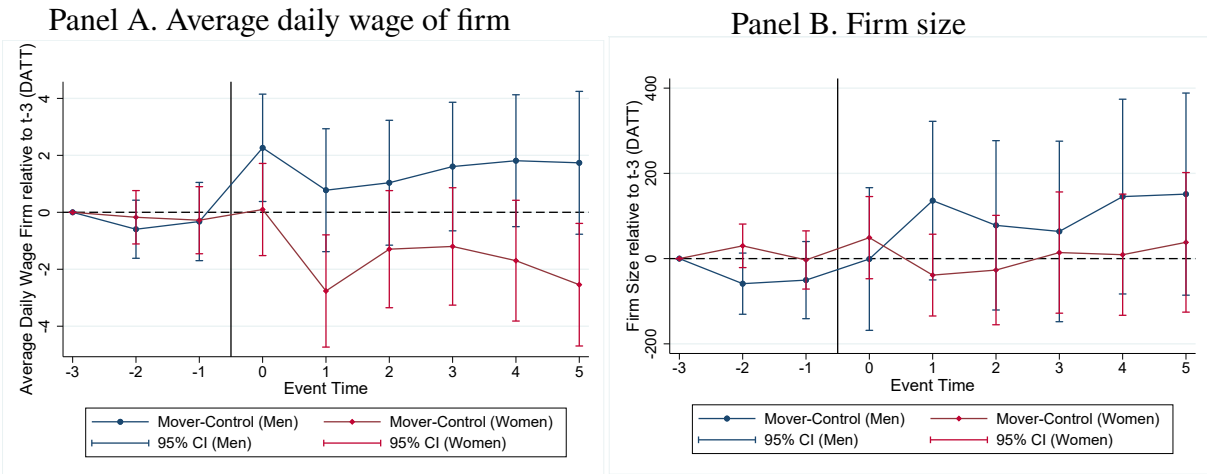

Figure 7: Effects on Firm Characteristics

Notes: Panel A displays the ATT on the average daily wage of the firm for men (blue) and women (red) relative to year $t-3$ (after propensity-score matching) with the corresponding 95% confidence intervals. Standard errors are computed following Abadie and Imbens (2016). Panel B shows the ATT on the firm size.

Job Requirement Level

We also investigate whether the requirement level of the job changes after the move and if the transition probabilities differ between moving men and women. For this analysis, we classify the requirement level of jobs into four groups: (i) unskilled or semi-skilled tasks, (ii) skilled tasks, (iii) complex tasks and (iv) highly complex tasks. We then assign the job transition of each moving man and woman to one of the 16 transition cells based on the job requirement level in the year before and after a move. This results in a matrix showing the transition probabilities. Figure 8 shows the transition probabilities for each transition cell (as percentages) of moving women (panel A) and men (panel B) relative to matched controls.

For example, in panel A the gray bar on the far left on the x-axis refers to moving women who do unskilled or semi-skilled tasks before the move and who stay in those types of jobs after the move. The dark-blue bar on the far left on the x-axis refers to moving women who work in highly complex tasks before the move and who switch to unskilled or semi-skilled tasks after

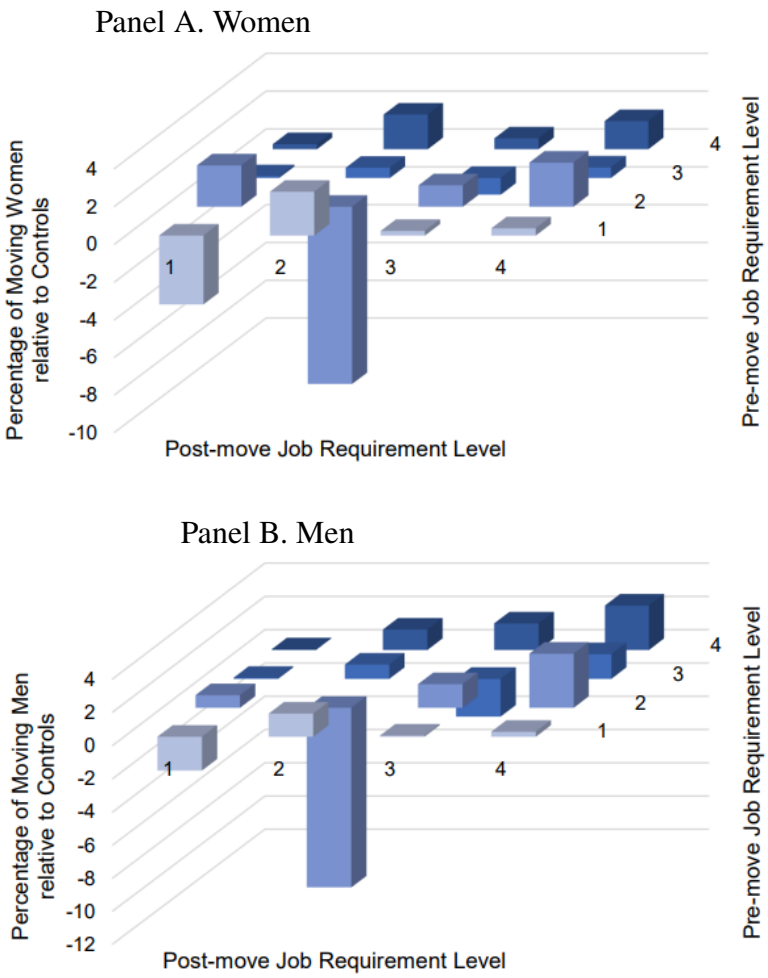

Figure 8: Transitions Job Requirement Level

Notes: Panel A displays the transition probabilities for the moving women's job requirement level and panel B displays the transition probabilities for the moving men's job requirement level (relative to matched control couples). The post-move job requirement level refers to the job requirement level in year $t+1$ and the pre-move job requirement level refers to the job requirement level in year $t+1$.

the move. The bars on the diagonal (from the bottom left to the upper right) refer to women who do not change their requirement level of the job after a move.

We observe that moving men and women are more likely to work in highly complex tasks before the move relative to matched control couples – 5.52% more moving men work in highly complex tasks before the move (relative to controls) and 4.15% of moving women. Moving men as well as women are less likely to work in unskilled or semi-skilled tasks and in skilled tasks (relative to controls) before moving. After a move, we observe that 7.70% more moving men work in highly complex tasks (relative to controls) whereas this applies to only 4.75% of moving women. In general, while 7.92% of moving men can realize a one-level increase in job requirement level (relative to controls), 6.96% of moving women can do the same. With respect to a one-level decrease in the job requirement level, 4.53% of moving men and 5.49% of moving women experience a one-level decrease in the requirement level of the job. To summarize, it seems that changes in job requirement levels cannot explain the observed large

gender differences in returns on moving.

Industry/Occupation Switch

In a final step, we examine if spouses switch industry or occupation after the move. In figure 9, we show the probability that spouses switch their occupation or industry (relative to $t-3$).

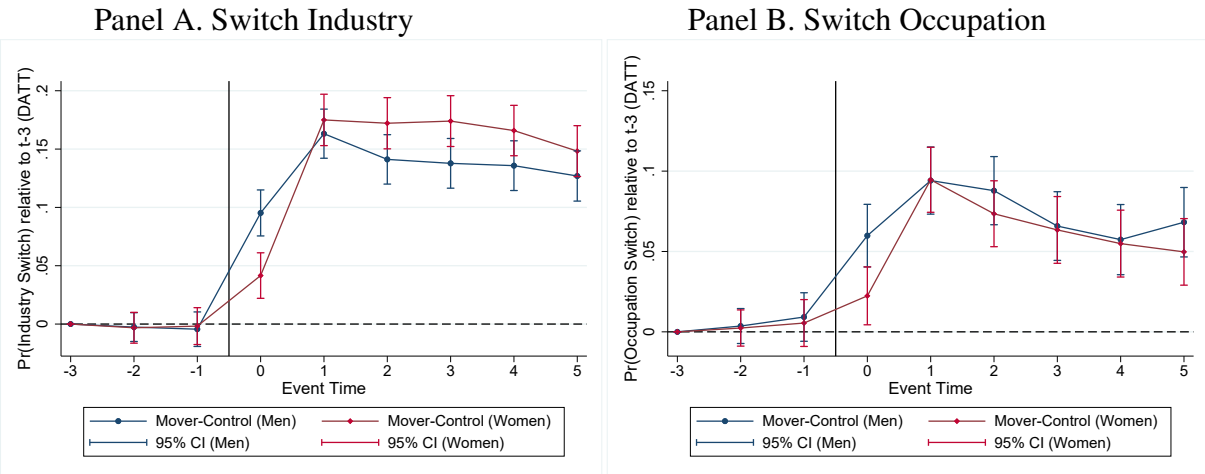

Figure 9: Switch Industry/Occupation

Notes: Panel A displays the ATT on the probability of switching industries for men (blue) and women (red) relative to year $t-3$ (after propensity-score matching) with the corresponding 95% confidence intervals. Standard errors are computed following Abadie and Imbens (2016). Panel B shows the ATT on the probability of switching occupations.

From figure 9 panel A, it can be observed that the probability for an industry switch increases significantly for both spouses after the move. One year after the move, moving men are around 10% and moving women around 4% more likely to switch industry compared to matched controls. For years two to five after the move, this probability is even higher for both spouses, but slightly higher for moving women than for moving men.

From panel B, we observe that the probability of switching occupation also increases significantly after the move. In the first years after the move, moving men are more likely to switch occupations compared to moving women. In later years, however, the probability of switching occupation is approximately equal for male and female movers. Compared to matched controls, moving men are around 7% more likely to switch occupation in year $t+5$, whereas this amounts to 5% for women. Overall, for moving men as well as women, a move has a large effect on the probability of switching industry or occupation.

4.4 Robustness

In this section, we examine a number of robustness checks and show that our results are highly robust to all of these tests.

Alternative Control Group - Job Changer

Our goal is to estimate the effect of a long-distance move on both spouses' labor market outcomes. To do so, we evaluate the treatment, moving to a new local labor market, against the

alternative of staying. In our main analysis, we use all non-moving couples as potential control couples. Note that there is neither a restriction that couples in the control group have to experience a job change, nor that they cannot move in future years. To show that the returns to moving are determined by geographic mobility rather than by job changes, we now present the results for a control group of couples who also experience a job change. Since voluntary job changes are typically associated with an increase in earnings, we expect that the estimated treatment effects are lower compared to our main specification. Figure 10 shows the effects of a move on yearly labor earnings (panel A) and days employed per year (panel B) of moving couples using our alternative control group of job changers. Overall, we observe only small differences in the estimated effects between our main specification and the alternative control group. For moving men as well as women, the estimated treatment effects on yearly labor earnings and days employed are only slightly lower for the alternative control group compared to the main specification.

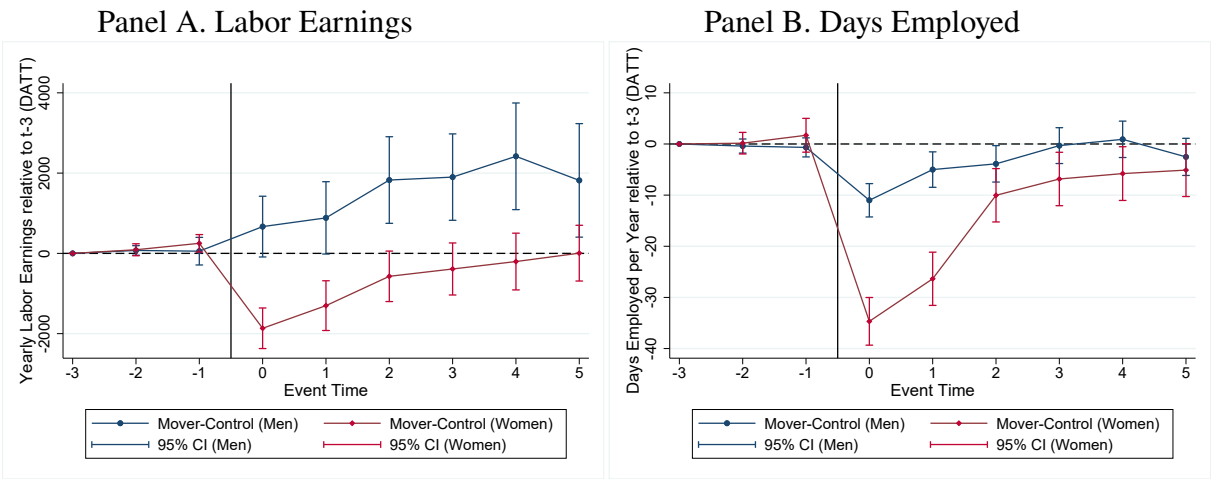

Figure 10: Effects on Individual Labor Earnings and Employment for Control Group of Job Changer

Notes: Panel A displays the ATT on the yearly labor earnings for men (blue) and women (red) relative to year $t-3$ (after propensity-score matching) with the corresponding 95% confidence intervals. Standard errors are computed following Abadie and Imbens (2016). Panel B displays the ATT on the days employed per year.

Moving Distance

In our main analysis, long-distance moves are defined as moves across district borders with a distance of at least 50 km and a job change of at least one spouse. To show that our results are robust to the choice of the cutoff distance, figure 11 shows the results of yearly labor earnings (panel A) and days employed (panel B) for varying cutoff distances of 50, 60, 70, 80, 90, and 100 km, respectively[16]. For readability, we exclude the confidence intervals. Figure 11 shows that the estimated treatment effects vary only slightly between the different cutoff distances.

[16]Using these cutoff distances, we could identify 3,744 moves with a distance of at least 50 km, 3,424 moves with a distance of at least 60 km, 3,232 with a distance of at least 70 km, 3,098 with a distance of at least 80 km, 2,970 with a distance of at least 90 km and 2,862 with a distance of at least 100 km.

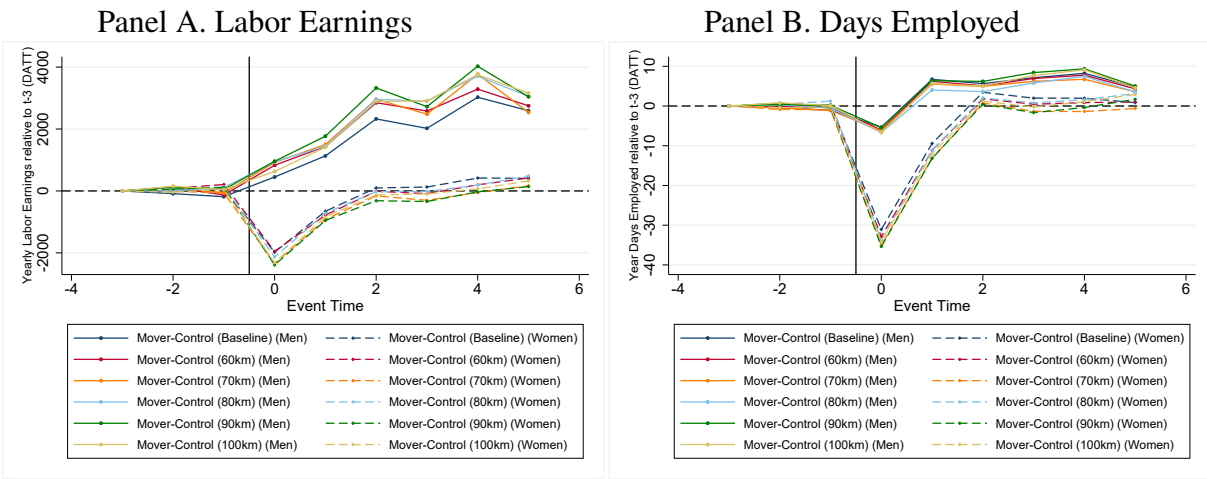

Figure 11: Effects on Individual Labor Earnings and Employment by Varying Cutoff Distance

Notes: Panel A displays the ATT on the yearly labor earnings for men (point) and women (diamond) relative to year $t-3$ (after propensity-score matching). For readability the confidence intervals are omitted. Panel B displays the ATT on the days employed per year.

5 Conclusion

Over the past half a century, women have made great strides in the labor market. However, despite substantial gender convergence, there are still large differences between men and women. In this paper, we investigate an aspect that contributes to gender differences in the labor market which has not received much attention in the recent literature: gender differences in the returns to moving. Using a new administrative dataset from Germany, we apply difference-in-difference propensity score matching to estimate the labor market effects of couples' long-distance moves. We use a matching approach to compare the labor market outcomes of couples who have moved (treated) with those of observably similar couples who have not moved (matched non-movers). Our approach takes advantage of detailed administrative data that helps us to choose control couples that are as similar as possible to the moving couples. In particular, our data allows us to precisely account for spouses' personal characteristics, their labor market histories as well as for household and regional characteristics before the move.

Our results show that while men's earnings increase significantly after the move, women suffer large losses in the first years after the move. This is in line with previous studies (Blackburn (2010b), Cooke et al. (2009), LeClere and McLaughlin (1997), Sandell (1977), Blackburn (2010a), Cooke (2003a), Spitze (1984), Rabe (2009)). According to our results, men benefit almost exclusively through higher daily wages, while women's losses are mostly due to being employed on fewer days. The effect on household income as a whole is positive in the medium and long run, which is as expected from a standard human capital framework (Mincer (1978)). Going beyond the existing studies, we provide an in-depth analysis of the underlying mechanisms and effect heterogeneities. First, we examine whether the treatment effects vary by whether couples move in favor of the man's or the woman's job opportunities and present evidence for

gender asymmetries. While men experience large increases and women large losses in yearly labor earnings after the move if couples move in favor of the man, increases in earnings are much lower for women if couples move in favor of the woman. Men experience only a short-lived small loss in labor earnings. In a second step, we investigate how treatment effects vary by spouses' relative household earnings before the move. While men can realize significant positive returns from moving regardless of their pre-move relative earnings, women can only realize small positive returns if they have low pre-move relative earnings. Returns to moving are also much larger for men than for women, even if both have similar relative earnings before the move. The highest gains are estimated for men who have low relative earnings before the move. These come with gains in the number of days employed, suggesting that these couples move for a (stable) job of a man without stable employment. Focusing on whose job starts first as well as on pre-move relative earnings therefore lead to similar insights. In addition, we find that couples starting off in regions with high unemployment rates and those in which no partner holds a university degree experience larger returns to moving. Female movers for whom we observe a childbirth in the data in post-move years lose much, but this is not necessarily a causal effect of the move. While men gain from moving also if they are employed before moving, women gain on average only if they are unemployed before the move.

Finally, we investigate what drives the wage gains from moving. Because our data include some firm information, we could take a look into this "black box" and provide evidence on the underlying mechanisms. We find that men tend to move to slightly larger and higher-paying firms, while this does not apply to women. Changes in the job requirement levels cannot explain the gender asymmetry in the returns to moving. Further, we show that for both spouses the move goes along with an increased probability of switching industry or occupation.

Overall, our results indicate that long-distance moves go along with an increase in long-run household income through wage gains or employment prospects of the man at the cost of the woman's employment stability. Men work in slightly larger and better-paying firms after the move. The opposite situation – realizing wage gains for the woman while accepting a worse job situation for the man – is rare and if it occurs at all it is to escape unemployment rather than to improve wages.

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Online Appendix

Couples, Careers, and Spatial Mobility

Lea Nassal
Marie Paul

Contents

A Dataset and Data Processing

A.1 Dataset

For our analysis, we use German administrative data in which couples are identified according to the method of Goldschmidt et al. (2017). Couples are defined as individuals who live at the same location, have a matching last name, are of different sexes and fewer than 15 years apart in age. Only couples in which both spouses have a record in the IEB on June 30, 2008 could be identified. This sample is not fully representative of all German couples due to the nature of the IEB and the matching algorithm. Couples of whom one spouse works in a civil service job, is unemployed (not registered), self-employed or already retired (in 2008) could not be identified. In addition, couples who do not share their last name could not be identified. According to Goldschmidt et al. (2017) around 85 to 90% of German couples share their last name and therefore only a few couples could not be identified. However, it is often assumed that couples who share their last name have more traditional gender roles and are slightly older. According to Goldschmidt et al. (2017) it is more likely that their algorithm picks up married couples who live in smaller buildings. For example, it could happen that a couple lives at a location where an other (unrelated) person also shares their last name. In this case, the couple could not be identified. In total, they can identify about 17% of all married couples in Germany and about 35% of married couples in the IEB. For a comparison between the sample of couples identified in the IEB and the microcensus, see table 6 in Goldschmidt et al. (2017).
The matching of individuals is only done for that particular year, therefore the sample size decreases the further the observation is from that year. For an illustration of the total sample size, see figure A1 panel A. The decrease in the total sample size after 2008 is mainly driven by the older cohorts (figure A1 panel C-D). Since we restrict our analysis sample to couples aged $20 - 50$ in the pre-move year, this is only a minor problem in our sample. Before 2008, the decrease in the sample size is more pronounced for women than for men. This is possibly driven by periods of non-employment due to childbirth. For our analysis, we consider all long-distances moves from 2008 to 2012. We show the effects from three years prior to the move up to five years thereafter – the first year we consider for our outcome variables is 2005, while it is 1998 for covariates used in the matching. From figure A1, it can be seen that the problem of decreasing sample size is especially pronounced for earlier years which we do not consider as outcome variables in our analysis.

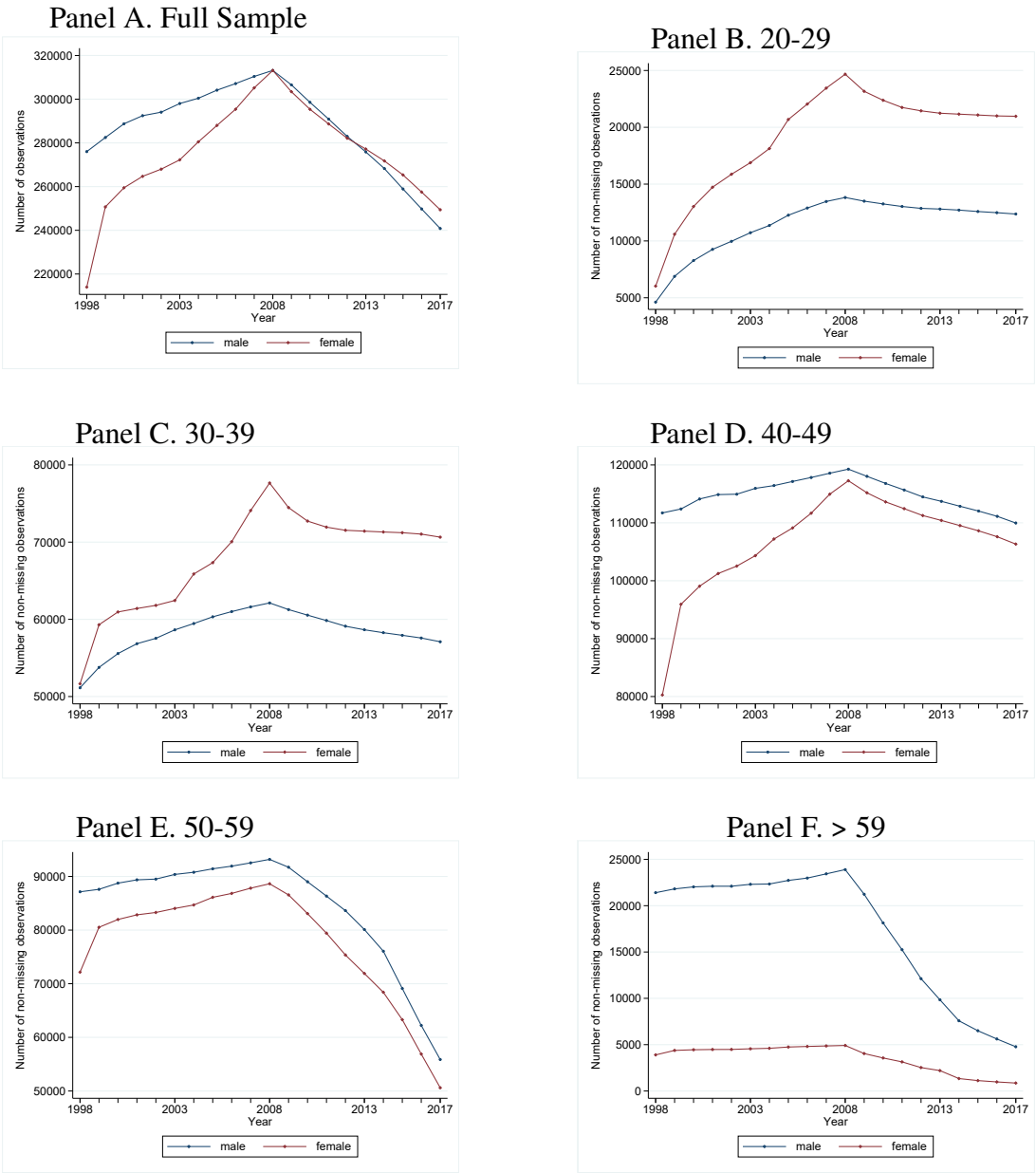


Figure A1: Sample Size

Notes: This figure shows the total sample size (panel A) and the sample sizes by age groups (panel B-F). Age groups are measured in 2008.

A.2 Data Processing

We start with the sample of married couples identified in the IEB from 1998 to 2017. The data is provided at the spell level and each record includes a couple ID, a person ID, a firm ID, a start and end date of the spell, a daily wage or benefit level, various demographic characteristics, and information on firm characteristics.

As the wage information is generated from employer submitted employment records, the wage information in our data is highly reliable. However, we can only observe wages up to the social security contribution ceiling, which implies that wages are right-censored. Hence, right-censored wages are imputed using a two-step procedure following Dustmann et al. (2009) and Card et al. (2013). Specifically, in a procedure using "leave-one-out-means", we fit 320 tobit models separately by year, education (four groups: missing, no qualification/unrecognized qualification, some post-secondary, university degree), sex and region (east/west), including the following variables: age, age^2, tenure, tenure^2, dummy for 20 or more employees, dummy for age above 40, interaction between dummy for age above 40 and age (age^2). We deflate prices to 2015 prices using the consumer price index.

We follow Dauth and Eppelsheimer (2020) and create a yearly panel by taking one record for each spouse, using the spell that includes June 30 of the respective year. If an individual has multiple spells that include June 30, we first sort spells by source and keep only one record per year.[1] If an individual has multiple employment spells, we keep the spell with the highest tenure at the current employer. In our analysis there might be spouses who work only during some months of the year, therefore it could be that spells do not include June 30. In this case, we sort those spells and keep one record per year, even if in that case firm characteristics are not measured at the respective spell.

For each year, we merge spouses using the couple ID. Some spouses drop out of our sample because they are not covered in the IEB anymore. This may happen for several reasons. They could start working in a civil service job, become self-employed, drop out of the labor force, become unemployed (not registered), move abroad, go into early retirement or die. In our analysis, we keep individuals as long as they work in covered employment or receive unemployment benefits at least once after they temporarily drop out of the sample. In cases of a temporary drop, we assume zero earnings for that individual. For individuals receiving benefits, we use information on the reason of termination at the last observation to identify individuals with transitions from registered to unregistered unemployment.[2] We keep observations for

[1] The sorting order is the following: BeH, LeH, LHG, MTH, XMTH, ASU, XASU.

[2] We use the following reasons to identify transitions from registered to unregistered unemployment: 2003 Expiration of entitlement, 2009 Expiration of partial unemployment benefit, 2010 Lack of cooperation of job seeker, 2011 Lack of willingness to work, 2020 Termination of period for which benefit is allowed ALHI, 2023 Period of exclusion from benefit (short), 2028 Entitlement exhausted, 2035 Stock (of ALG4), 2043 Period of shortfall, 2045 Non-appearance of notification, 2049 End of unemployment assistance (Bund-ALHI), 2059 Period of exclusion from benefits 3/6/12 weeks, 2062 End of availability, 2063 3rd shortfall of notification, 3014 End of need for financial support, 5044 Lack of cooperation, 5074 Lack of availability/ cooperation, 5087 Non-activation according to §10

individuals in unregistered unemployment until the end of the obervation period and assume zero labor earnings. We drop couples after the last observation for both spouses.

A.3 Identifying Long-distance Moves

To identify long-distance moves, we use information on the place of residence of each spouse that is available at the district level in our data. To secure consistent regional allocations over the observation period, the information on the district is recoded with reference to the territorial allocation on December 31, 2017. We use GIS software to identify the GPS coordinates of the center of each German district. A few districts consist of mainland and small islands. For those, we assign the center of the district to the center of the mainland. With the GPS coordinates of each district, we calculate moving distances. For illustration of the 401 German districts with the associated GPS coordinates, see figure A2.

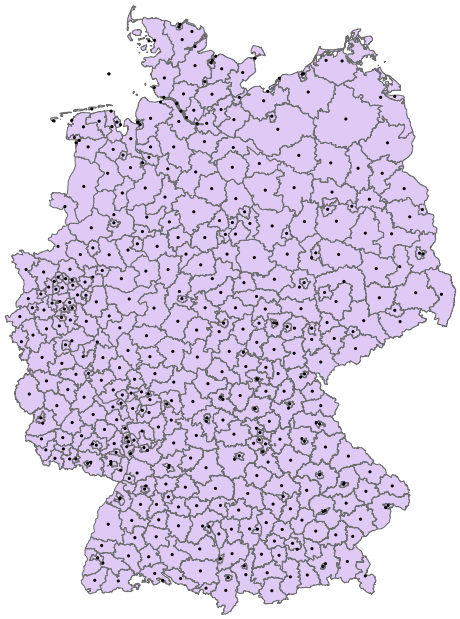


Figure A2: Districts with GPS Coordinates of the Center

SGBI, 6018 Lack of cooperation, 6026 Lack of availability/ cooperation.

B Propensity Score and Matching Diagnostics

This section presents the estimation results for the logit model and the balancing tests for the full sample. Results for the different subsamples are available from the authors as electronic files on request.

B.1 Logit Parameters for Propensity Score Specification

Table B1: Partial Effects of Logit Model

	(1) Full sample
Man characteristics	
Age	0.136
	(4.17)
Age^2	-0.00203
	(-4.67)
No/unrecognized education, basic/general secondary education	0
	(.)
In-company/school-based training, abitur	0.190
	(3.51)
University degree	0.566
	(7.61)
Full-time	-0.809
	(-0.76)
Part-time	-0.846
	(-0.80)
Marginal	-0.842
	(-0.80)
Non-employed	0
	(.)
Unskilled	0
	(.)
Skilled	0.0231
	(0.33)
Complex	0.267
	(2.98)
Highly complex	0.457
	(5.16)

(continued)

Table B1: Partial Effects of Logit Model (continued)

	(1) Full sample
< 4 years in region	0
	(.)
4-7 years in region	-0.390
	(-1.65)
8-12 years in region	-0.974
	(-4.00)
> 12 years in region	-1.242
	(-4.16)
Non-German	-0.236
	(-3.74)
< 1 year tenure	1.057
	(0.99)
< 2 years tenure	0.809
	(0.76)
< 3 years tenure	0.755
	(0.71)
>= 3 years tenure	0.322
	(0.30)
Missing tenure	0
	(.)
Agriculture and military	0.390
	(2.30)
Resource extraction and production	0.200
	(1.66)
Construction and architecture	0.179
	(1.34)
Science, geography and computer science	0.309
	(2.20)
Transport, logistics, safety and security	0.381
	(3.11)
Commercial services, merchandise trade and tourism	0.573
	(4.40)
Corporate organization, accounting and law	0.688
	(5.51)
Health, social affairs and education	0.943
	(7.05)
Social sciences, media, arts and culture	0.662
	(4.15)
Missing occupation	0
	(.)
Yearly labor earnings	0.00000435
	(2.84)

(continued)

Table B1: Partial Effects of Logit Model (continued)

	(1) Full sample
Yearly labor earnings $t-2$	-0.00000124
	(-0.56)
Yearly labor earnings $t-3$	0.00000618
	(2.67)
Days benefits per year	0.00160
	(3.48)
Days benefits per year $t-2$	0.000163
	(0.33)
Days benefits per year $t-3$	-0.000197
	(-0.45)
Year days employed	-0.00239
	(-4.02)
Year days employed $t-2$	0.000235
	(0.36)
Year days employed $t-3$	0.000704
	(1.28)
Yearly labor earnings $\times$yearly labor earnings $t-2$	3.66e-12
	(0.20)
Year labor earnings$\times$yearly labor earnings $t-3$	-4.50e-11
	(-2.12)
Year days employed $\times$year days employed $t-2$	0.000000961
	(0.46)
Year days employed $\times$year days employed $t-3$	-0.00000358
	(-2.25)
Yearly labor earnings > 0	0.737
	(6.67)
Yearly labor earnings $t-2$ > 0	-0.110
	(-0.94)
Yearly labor earnings $t-3$ > 0	-0.0454
	(-0.44)
Woman characteristics	
Age	0.109
	(3.43)
Age^2	-0.00174
	(-3.99)
No/unrecognized education, basic/general secondary education	0
	(.)
In-company/school-based training, abitur	0.0912
	(1.77)
University degree	0.311
	(4.19)
Full-time	13.19
	(0.02)

(continued)

Table B1: Partial Effects of Logit Model (continued)

	(1) Full sample
Part-time	13.29
	(0.02)
Marginal	13.56
	(0.02)
Non-employed	0
	(.)
Unskilled	0
	(.)
Skilled	0.132
	(1.95)
Complex	0.169
	(1.68)
Highly complex	0.208
	(2.35)
< 4 years in region	0
	(.)
4-7 years in region	0.233
	(0.97)
8-12 years in region	-0.259
	(-1.04)
> 12 years in region	-0.822
	(-2.71)
Non-German	-0.252
	(-4.31)
< 1 year tenure	-13.39
	(-0.02)
< 2 years tenure	-13.25
	(-0.02)
< 3 years tenure	-13.30
	(-0.02)
>= 3 years tenure	-13.67
	(-0.02)
Missing tenure	0
	(.)
Agriculture and military	0.390
	(2.30)
Resource extraction and production	0.200
	(1.66)

(continued)

Table B1: Partial Effects of Logit Model (continued)

	(1) Full sample
Construction and architecture	0.179
	(1.34)
Science, geography and computer science	0.309
	(2.20)
Transport, logistics, safety and security	0.381
	(3.11)
Commercial services, merchandise trade and tourism	0.573
	(4.40)
Corporate organization, accounting and law	0.688
	(5.51)
Health, social affairs and education	0.943
	(7.05)
Social sciences, media, arts and culture	0.662
	(4.15)
Missing occupation	0
	(.)
Yearly labor earnings	-0.00000135
	(-0.49)
Yearly labor earnings $t-2$	-0.00000685
	(-1.95)
Yearly labor earnings $t-3$	-0.000000893
	(-0.29)
Days benefits per year	0.00160
	(3.63)
Days benefits per year $t-2$	-0.000129
	(-0.28)
Days benefits per year $t-3$	0.000977
	(2.39)
Year days employed	-0.000704
	(-1.55)
Year days employed $t-2$	0.0000113
	(0.02)
Year days employed $t-3$	0.000377
	(0.90)
Yearly labor earnings $\times$ yearly labor earnings $t-2$	6.16e-11
	(1.53)
Yearly labor earnings $\times$ yearly labor earnings $t-3$	-1.06e-11
	(-0.24)
Year days employed $\times$ year days employed $t-2$	-0.00000271
	(-1.80)
Year days employed$\times$ year days employed $t-3$	-0.00000130
	(-1.07)

(continued)

Table B1: Partial Effects of Logit Model (continued)

	(1) Full sample
Yearly labor earnings > 0	0.0936
	(0.98)
Yearly labor earnings $t-2$ > 0	0.116
	(1.30)
Yearly labor earnings $t-3$ > 0	0.0871
	(1.03)
Household characteristics	
No child/age first child > 24	0
	(.)
Age first child = 0	-0.0528
	(-0.52)
Age first child 1-2	-0.692
	(-8.01)
Age first child 3-5	-1.482
	(-16.98)
Age first child 6-10	-2.037
	(-23.55)
Age first child 11-15	-2.246
	(-22.63)
Age first child 16-18	-2.444
	(-13.98)
Age first child 19-24	-1.516
	(-9.72)
Moving year 2008	0
	(.)
Moving year 2009	0.344
	(6.93)
Moving year 2010	0.446
	(8.13)
Moving year 2011	0.681
	(11.71)
Moving year 2012	1.251
	(15.89)
Regional characteristics	
District unemployment rate	0.0255
	(5.09)
District GDP per capita	0.00282
	(2.28)
Constant	-7.087
	(-13.30)
Observations	164783

Notes: All covariates are measured in pre-move year $t-1$ if not stated differently. T-statistic is in parentheses.

B.2 Balancing Tests

Table B2: Balancing Tests (Full Sample)

		Before matching			*After matching*		
	Movers	Non-movers	Standard. diff.	P-value	Non-movers	Standard. diff.	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Man characteristics							
Age	37.290	41.167	-0.592	0.000	37.262	0.004	0.860
Age^2	1438.800	1732.100	-0.586	0.000	1436.300	0.005	0.828
In-company/school-based training, abitur	0.552	0.713	-0.338	0.000	0.560	-0.015	0.524
University degree	0.273	0.114	0.413	0.000	0.259	0.037	0.160
< 1 year tenure	0.255	0.124	0.337	0.000	0.256	-0.003	0.916
< 2 year tenure	0.132	0.087	0.146	0.000	0.134	-0.007	0.788
< 3 year tenure	0.099	0.070	0.102	0.000	0.099	-0.002	0.929
>= 3 year tenure	0.366	0.654	-0.603	0.000	0.361	0.011	0.648
Skilled	0.541	0.704	-0.339	0.000	0.550	-0.017	0.470
Complex	0.116	0.112	0.014	0.403	0.116	0.002	0.937
Highly complex	0.259	0.120	0.362	0.000	0.249	0.027	0.298
Non-German citizenship	0.110	0.084	0.088	0.000	0.113	-0.008	0.749
Full-time	0.788	0.896	-0.299	0.000	0.786	0.006	0.810
Part-time	0.046	0.033	0.000	0.000	0.047	-0.009	0.734
Marginal	0.017	0.007	0.092	0.000	0.017	0.005	0.850
4-7 years in region	0.314	0.082	0.608	0.000	0.313	0.003	0.901
8-12 years in region	0.612	0.827	-0.492	0.000	0.618	-0.014	0.598
> 12 years in region	0.048	0.087	-0.155	0.000	0.046	0.010	0.608
Year days employed	301.050	337.260	-0.352	0.000	300.840	0.002	0.940
Year days employed $t-2$	299.430	334.320	-0.324	0.000	298.620	0.008	0.777
Year days employed $t-3$	290.650	330.540	-0.350	0.000	289.380	0.011	0.675
Yearly labor earnings	40860	43565	-0.081	0.000	40608	0.008	0.762
Yearly labor earnings $t-2$	39242	42632	-0.105	0.000	38893	0.011	0.664
Yearly labor earnings $t-3$	37342	41570	-0.131	0.000	36906	0.014	0.587
Year days benefits	14.991	6.455	0.197	0.000	14.815	0.004	0.883
Year days benefits $t-2$	11.586	7.066	0.113	0.000	12.025	-0.011	0.674
Year days benefits $t-3$	11.305	7.972	0.082	0.000	11.910	-0.015	0.558

(continued)

Table B2: Balancing Tests (Full Sample) (continued)

		Before matching			*After matching*		
	Movers	Non-movers	Standard. diff.	P-value	Non-movers	Standard. diff.	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Yearly labor earnings > 0	0.927	0.961	-0.146	0.000	0.925	0.008	0.764
Yearly labor earnings $t-2$ > 0	0.903	0.956	-0.208	0.000	0.904	-0.003	0.900
Yearly labor earnings $t-3$ > 0	0.883	0.949	-0.238	0.000	0.881	0.009	0.753
Yearly labor earnings # labor earnings $t-2$	2.6e+09	2.6e+09	0.006	0.713	2.6e+09	0.002	0.938
Yearly labor earnings # labor earnings $t-3$	2.5e+09	2.5e+09	-0.011	0.512	2.5e+09	0.003	0.914
Year days employed # year days employed $t-2$	1.0e+05	1.2e+05	-0.405	0.000	1.0e+05	0.005	0.852
Year days employed # year days employed $t-3$	95853	1.2e+05	-0.443	0.000	95560	0.006	0.811
Agriculture and military	0.018	0.017	0.012	0.469	0.018	0.002	0.918
Resource extraction and production	0.260	0.384	-0.267	0.000	0.260	-0.002	0.937
Construction and architecture	0.058	0.082	-0.093	0.000	0.061	-0.009	0.661
Science, geography and computer science	0.064	0.048	0.068	0.000	0.066	-0.007	0.775
Transport, logistics, safety and security	0.155	0.176	-0.056	0.001	0.158	-0.008	0.732
Commercial services, merchandise trade and tourism	0.086	0.066	0.074	0.000	0.088	-0.008	0.733
Corporate organization, accounting and law	0.199	0.143	0.150	0.000	0.199	-0.001	0.970
Health, social affairs and education	0.107	0.042	0.248	0.000	0.099	0.030	0.264
Social sciences, media, arts and culture	0.029	0.013	0.113	0.000	0.026	0.020	0.460
Woman characteristics							
Age	35.033	39.056	-0.597	0.000	34.991	0.006	0.795
Age^2	1277.700	1565.900	-0.583	0.000	1274.800	0.006	0.805
In-company/school-based training, abitur	0.584	0.708	-0.261	0.000	0.591	-0.013	0.584
University degree	0.187	0.067	0.368	0.000	0.178	0.027	0.328
< 1 year tenure	0.237	0.165	0.180	0.000	0.237	0.000	1.000
< 2 year tenure	0.144	0.111	0.099	0.000	0.140	0.011	0.662
< 3 year tenure	0.096	0.090	0.021	0.203	0.096	0.001	0.969
>= 3 year tenure	0.296	0.530	-0.490	0.000	0.296	-0.000	0.984

(continued)

Table B2: Balancing Tests (Full Sample) (continued)

		Before matching			*After matching*		
	Movers	Non-movers	Standard. diff.	P-value	Non-movers	Standard. diff.	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Skilled	0.614	0.710	-0.203	0.000	0.627	-0.026	0.277
Complex	0.057	0.046	0.050	0.001	0.055	0.007	0.786
Highly complex	0.180	0.088	0.274	0.000	0.167	0.038	0.142
Non-German citizenship	0.134	0.088	0.144	0.000	0.136	-0.009	0.723
Full-time	0.604	0.642	-0.078	0.000	0.605	-0.001	0.964
Part-time	0.145	0.232	-0.022	0.000	0.141	0.009	0.658
Marginal	0.024	0.022	0.009	0.596	0.023	0.003	0.885
4-7 years in region	0.318	0.085	0.607	0.000	0.318	0.011	0.693
8-12 years in region	0.612	0.824	-0.485	0.000	0.620	-0.018	0.491
> 12 years in region	0.046	0.087	-0.163	0.000	0.044	0.008	0.677
Year days employed	258.350	314.170	-0.427	0.000	256.910	0.011	0.673
Year days employed $t-2$	255.170	305.830	-0.374	0.000	254.280	0.007	0.796
Year days employed $t-3$	248.750	294.890	-0.327	0.000	247.600	0.008	0.745
Yearly labor earnings	18203	17834	0.020	0.184	18268	-0.030	0.892
Yearly labor earnings $t-2$	17766	17194	0.031	0.039	17791	-0.010	0.957
Yearly labor earnings $t-3$	17303	16556	0.041	0.007	17440	-0.007	0.767
Year days benefits	11.448	5.876	0.136	0.000	11.239	0.005	0.847
Year days benefits $t-2$	10.114	6.576	0.089	0.000	10.149	-0.001	0.972
Year days benefits $t-3$	10.985	7.512	0.082	0.000	11.181	-0.005	0.854
Yearly labor earnings > 0	0.818	0.914	-0.286	0.000	0.814	0.010	0.703
Yearly labor earnings $t-2$ > 0	0.806	0.899	-0.263	0.000	0.802	0.011	0.667
Yearly labor earnings $t-3$ > 0	0.786	0.874	-0.235	0.000	0.781	0.013	0.623
Yearly labor earnings # yearly labor earnings $t-2$	6.7e+08	5.6e+08	0.082	0.000	6.8e+08	-0.005	0.863
Yearly labor earnings # yearly labor earnings $t-3$	6.2e+08	5.3e+08	0.069	0.000	6.3e+08	-0.011	0.693
Year days employed # year days employed $t-2$	80537	1.1e+05	-0.472	0.000	80276	0.005	0.846
Year days employed # year days employed $t-3$	75187	1.0e+05	-0.457	0.000	75071	0.002	0.931
Agriculture and military	0.011	0.010	0.006	0.729	0.010	0.007	0.778
Resource extraction and production	0.067	0.083	-0.063	0.000	0.066	0.002	0.930
Construction and architecture	0.007	0.004	0.033	0.026	0.007	-0.002	0.934
Science, geography and computer science	0.025	0.017	0.052	0.001	0.025	-0.002	0.935

(continued)

Table B2: Balancing Tests (Full Sample) (continued)

		Before matching			*After matching*		
	Movers	Non-movers	Standard. diff.	P-value	Non-movers	Standard. diff.	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Transport, logistics, safety and security	0.108	0.145	-0.111	0.000	0.109	-0.006	0.798
Commercial services, merchandise trade and tourism	0.176	0.164	0.032	0.052	0.179	-0.009	0.712
Corporate organization, accounting and law	0.268	0.296	-0.062	0.000	0.270	-0.004	0.868
Health, social affairs and education	0.264	0.246	0.043	0.009	0.256	0.019	0.420
Social sciences, media, arts and culture	0.032	0.012	0.135	0.000	0.030	0.008	0.764
Household characteristics							
Child 0 years old	0.045	0.011	0.206	0.000	0.043	0.014	0.635
Child 1-2 years old	0.057	0.027	0.149	0.000	0.060	-0.011	0.690
Child 3-5 years old	0.042	0.060	-0.083	0.000	0.043	-0.003	0.890
Child 6-10 years old	0.041	0.152	-0.384	0.000	0.035	0.021	0.167
Child 11-15 years old	0.030	0.021	-0.557	0.000	0.024	0.018	0.129
Child 16-18 years old	0.009	0.093	-0.387	0.000	0.007	0.008	0.413
Child 19-24 years old	0.012	0.056	-0.248	0.000	0.010	0.009	0.498
Moving year 2009	0.254	0.260	-0.014	0.403	0.256	-0.005	0.844
Moving year 2010	0.193	0.199	-0.014	0.400	0.193	0.002	0.944
Moving year 2011	0.167	0.163	0.011	0.498	0.169	-0.006	0.802
Moving year 2012	0.118	0.107	0.034	0.033	0.114	0.013	0.586
Regional characteristics							
District unemployment rate	8.409	7.817	0.157	0.000	8.406	0.001	0.970
District GDP per capita	32.861	30.112	0.197	0.000	32.791	0.005	0.840

Notes: This table includes balancing tests for all covariates that are included in the propensity score specification. All covariates are measured in pre-move year $t-1$ if not stated differently.

C Additional Figures

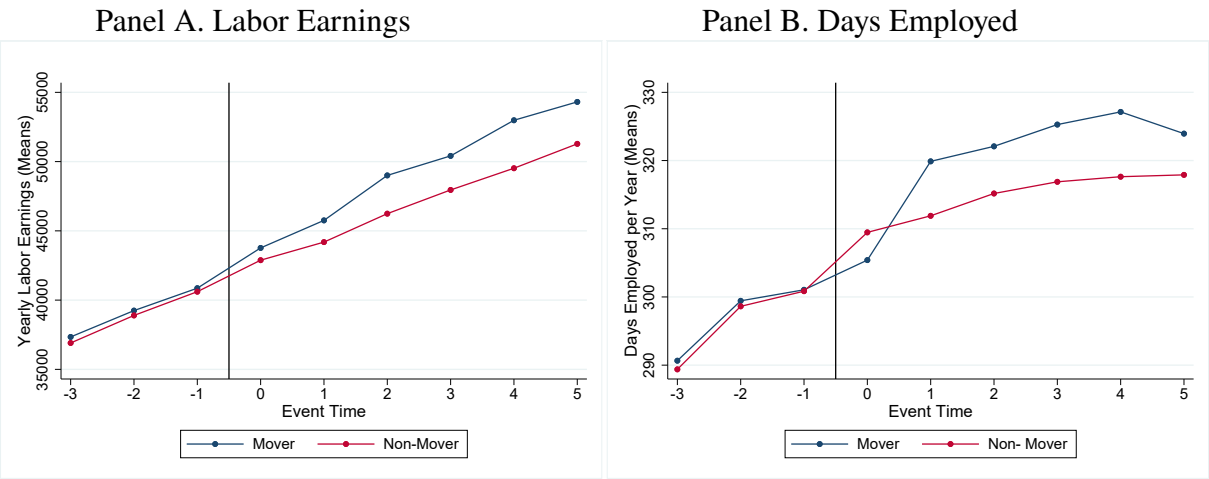


Figure C1: Means of Individual Labor Earnings and Employment (Full Sample Men)

Notes: To the left, panel A displays the means of the yearly labor earnings for moving men (blue) and matched non-moving men (red). To the right, panel B displays the means of the days employed per year.

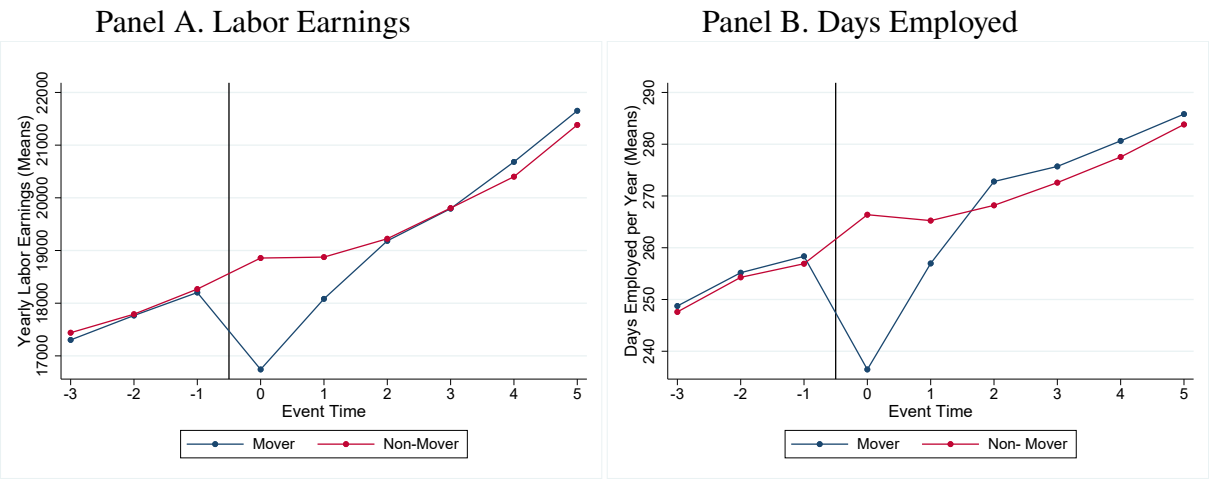


Figure C2: Means of Individual Labor Earnings and Employment (Full Sample Women)

Notes: To the left, panel A displays the means of the yearly labor earnings for moving women (blue) and matched non-moving women (red). To the right, panel B displays the means of the days employed per year.

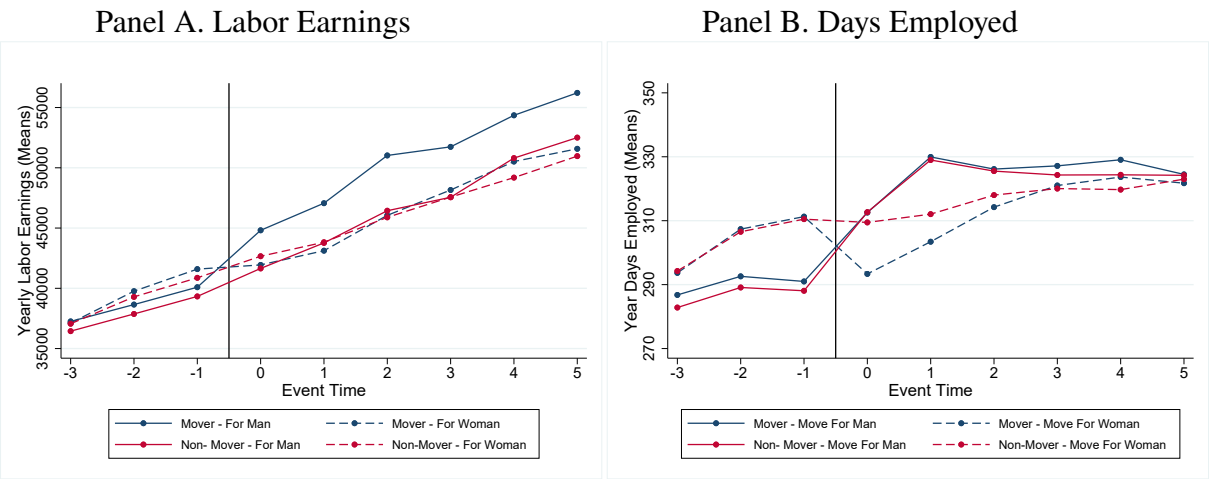


Figure C3: Means of Individual Labor Earnings and Employment (Men by Move for Man versus Woman)

Notes: To the left, panel A displays the means of the yearly labor earnings for moving men (blue) and matched non-moving men (red) (for subgroups defined by whether couples move in favor of the man or the woman). To the right, panel B displays the means of the days employed per year.

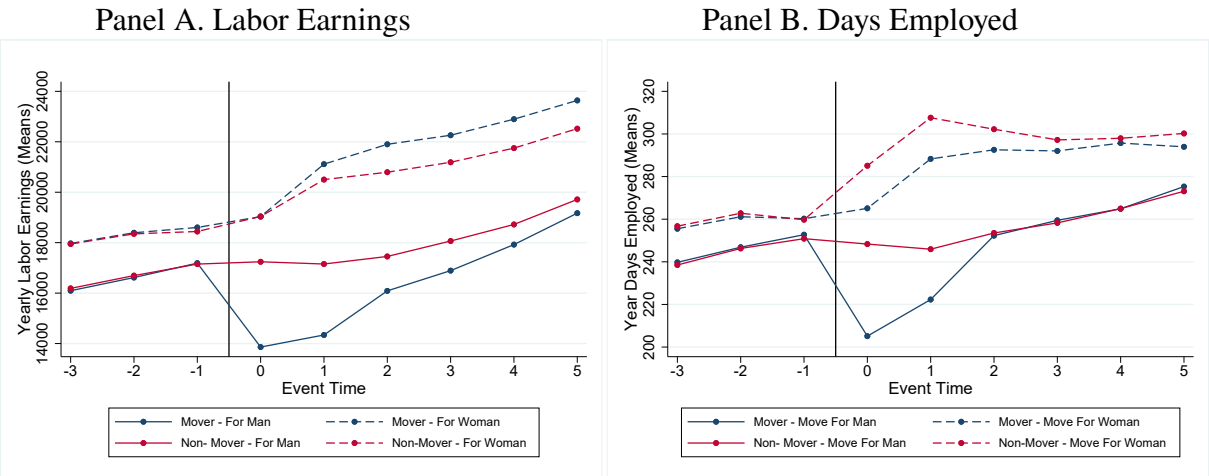


Figure C4: Means of Individual Labor Earnings and Employment (Women by Move for Man versus Woman)

Notes: To the left, panel A displays the means of the yearly labor earnings for moving women (blue) and matched non-moving women (red) (for subgroups defined by whether couples move in favor of the man or the woman). To the right, panel B displays the means of the days employed per year.

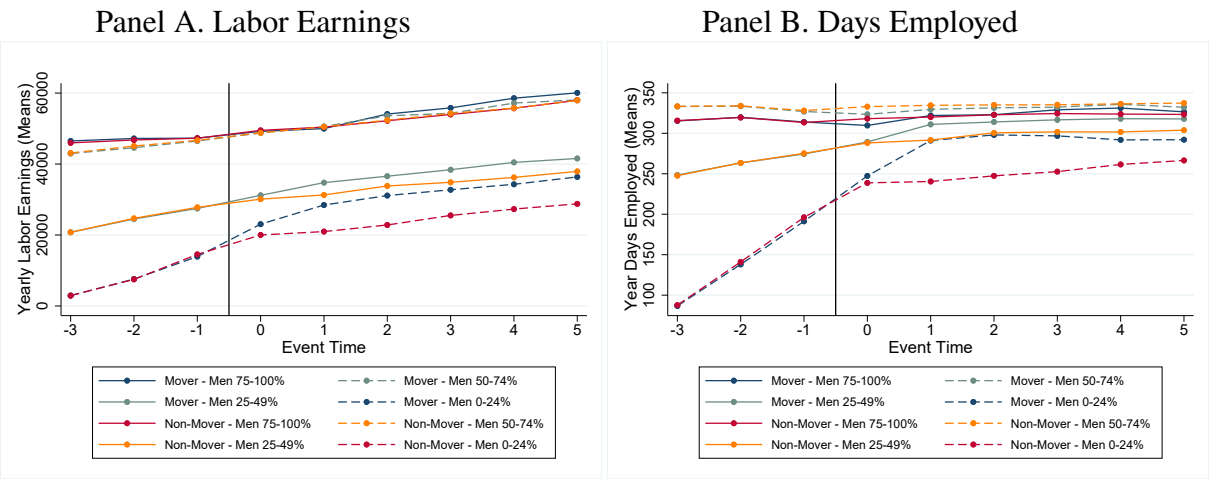


Figure C5: Means of Individual Labor Earnings and Employment (Men by Intra-household Earnings before Move)

Notes: To the left, panel A displays the means of the yearly labor earnings for moving men and matched non-moving men (for subgroups defined by pre-move relative household earnings). To the right, panel B displays the means of the days employed per year.

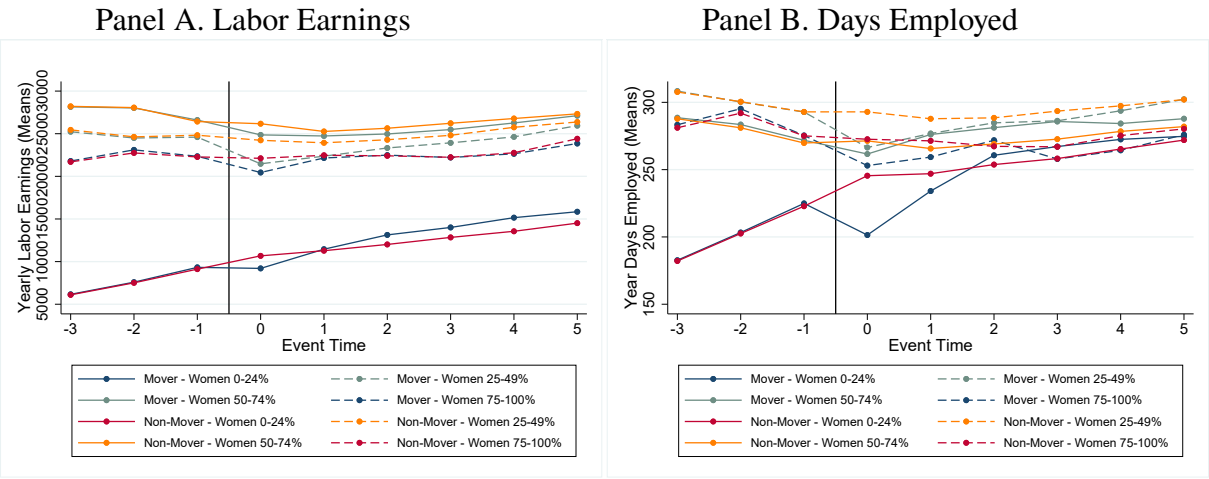
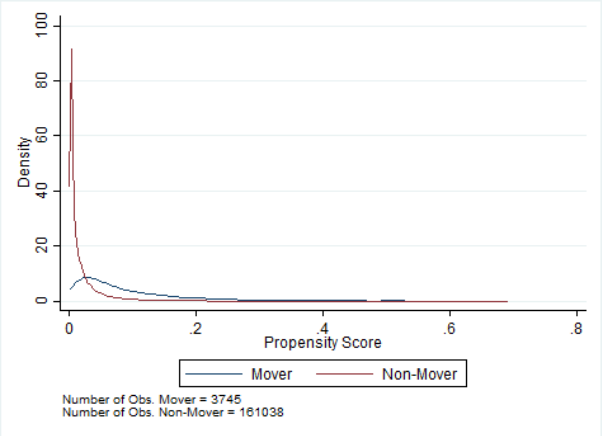


Figure C6: Means of Individual Labor Earnings and Employment (Women by Intra-household Earnings before Move)

Notes: To the left, panel A displays the means of the yearly labor earnings for moving women and matched non-moving women (for subgroups defined by pre-move relative household earnings). To the right, panel B displays the means of the days employed per year.

Panel A. Propensity Score Distribution before Matching



Panel B. Propensity Score Distribution after Matching

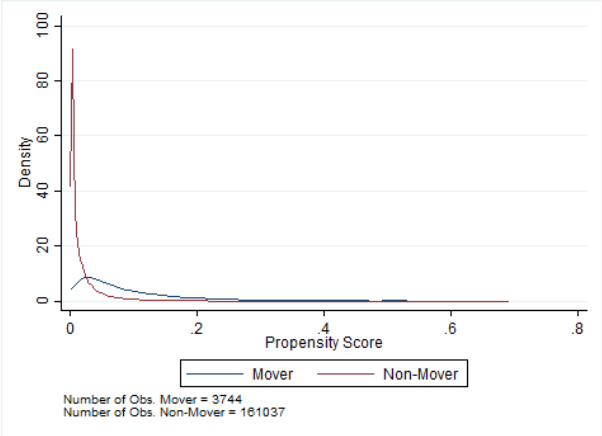


Figure C7: Propensity Score Distributions

Notes: To the left, panel A displays the distributions of the estimated propensity scores for moving (blue) and non-moving couples (red) before matching. To the right, panel B displays the distributions of the estimated propensity scores for moving (blue) and matched non-moving couples (red).

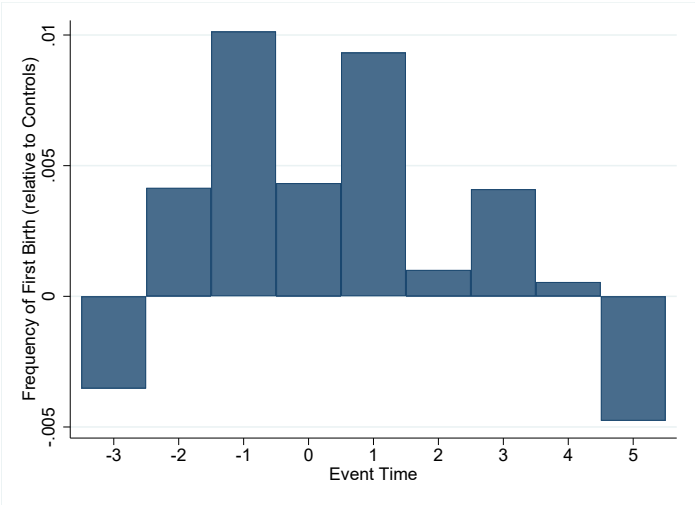


Figure C8: Move and First Birth

Notes: This figure displays the frequency of the first births of moving couples relative to matched control couples over the event years (in yearly bins).

D Additional Tables

Table D1: Effects on Days Employed per Year by Subgroups

	DATT post-move years 1-2		*DATT post-move years 3-6*		
	Men	Women	Men	Women	No. movers
	(1)	(2)	(3)	(4)	(5)
All	1.197	-20.248	6.952	2.056	3744
	(1.429)	(1.873)	(1.440)	(1.998)	
District unemployment rate					
Below median	-8.834	-29.200	-2.503	-9.372	1668
	(2.033)	(2.882)	(1.954)	(2.980)	
Above median	7.799	-10.946	12.481	12.686	2074
	(1.984)	(2.585)	(1.978)	(2.612)	
Age group					
20-29	13.370	-3.373	13.424	4.669	738
	(3.794)	(4.307)	(3.666)	(4.851)	
30-39	0.877	-16.534	6.980	2.996	1760
	(2.051)	(2.924)	(2.106)	(3.045)	
40-50	-6.640	-32.642	3.340	1.742	1243
	(2.263)	(2.871)	(2.389)	(2.764)	
Education group					
Power	-0.419	-19.065	2.066	-15.911	452
	(3.431)	(5.663)	(3.181)	(5.787)	
Part-power man	6.835	-28.826	8.330	4.357	574
	(2.377)	(4.967)	(2.560)	(5.043)	
Part-power woman	-6.256	-15.530	-4.916	-2.821	245
	(4.400)	(6.366)	(3.817)	(6.435)	
Low-power	0.389	-17.826	7.801	6.442	2469
	(1.848)	(2.282)	(1.866)	(2.432)	
Children					
No birth in event years	1.294	-26.989	9.231	1.080	2863
	(1.658)	(2.117)	(1.629)	(2.183)	
Birth in pre-move years	0.500	-6.767	-1.129	-4.543	383
	(2.970)	(6.244)	(3.419)	(5.363)	

(continued)

Table D1: Effects on Days Employed per Year by Subgroups (continued)

	DATT post-move years 1-2		*DATT post-move years 3-6*		
	Men	Women	Men	Women	No. movers
	(1)	(2)	(3)	(4)	(5)
Birth in post-move years	-0.162	-20.081	1.241	-13.106	499
	(3.639)	(4.297)	(3.944)	(4.686)	
Employment status					
Both non-emp.	90.494	53.620	81.987	61.907	159
	(8.345)	(8.318)	(10.161)	(10.351)	
Man non-emp., woman emp.	66.735	-39.485	51.956	-14.520	204
	(6.767)	(7.408)	(7.921)	(7.048)	
Woman non-emp., man emp.	1.216	32.989	14.021	44.049	587
	(3.944)	(5.307)	(4.066)	(6.066)	
Both emp.	-9.603	-31.806	-3.935	-9.541	2791
	(1.317)	(2.091)	(1.378)	(2.082)	
Moving year					
2008	4.049	-12.645	10.636	14.146	1002
	(2.625)	(3.361)	(2.832)	(3.832)	
2009	-2.340	-25.216	-0.228	-3.054	949
	(2.804)	(3.721)	(2.776)	(3.987)	
2010	0.412	-19.708	5.196	0.097	724
	(3.031)	(4.133)	(2.998)	(4.104)	
2011	1.635	-25.008	8.438	-7.466	625
	(3.178)	(4.550)	(3.159)	(4.634)	
2012	-0.496	-17.244	8.407	7.956	436
	(3.771)	(4.901)	(3.629)	(4.818)	

Notes: Columns 1 and 2 show the estimated treatment effect on the days employed per year of moving men and women in the two years after the move, relative to the pre-move period. Columns 3 and 4 show the analogous results for years 3 to 6 after the move. Column 5 shows the number of moving couples in the respective group. Standard errors (in parentheses) are computed following Abadie and Imbens (2016).

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