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## Couples, Careers, and Spatial Mobility

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# Couples, Careers, and Spatial Mobility

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## Abstract

We examine the employment and earnings effects of long-distance moves for married couples based on a new administrative data set from Germany. Using difference-in-difference propensity score matching, we estimate the average treatment effect for moving couples while precisely accounting for pre-move employment dynamics. Our results show that men's earnings increase significantly after the move, while women suffer large losses in the first years after the move. We shed light on potential mechanisms and show that spouses' earnings response is driven by men moving to larger, higher paying establishments, whereas women move to smaller, lower paying establishments. We explore effect heterogeneity by spouses' relative earnings before the move and find evidence for gender asymmetries.

**Keywords:** Long-distance moves, labor market careers, gender identity norms

**JEL Codes:** J61, J16, R23

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# 1 Introduction

Over the past half a century, women’s participation in the labor market has risen sharply and dual-earner couples have become the norm. Whereas in 1970, 97% of German men and 47% of women were in the labor force, the gender gap in labor force participation has narrowed considerably. By 2010, men’s labor force participation rate has fallen to 93%, while that of women has increased to 81%<sup>1</sup>. Due to this development, most 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. Hence, couples will 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 often involve sacrificing the career of one spouse for the sake of the other.

In this paper, we investigate the labor market effects of *long-distance moves* of couples, drawing on a new administrative data set from Germany (Goldschmidt et al., 2017). As a first step, we estimate the earnings and employment effects of joint moves by using a matching strategy that compares the labor market outcomes of couples who move (treated) versus similar couples who do not (matched non-movers). More specifically, we apply *difference-in-difference propensity score matching* (Heckman et al., 1997) to estimate the average treatment effect for the treated (the moving couples) while precisely accounting for spouses’ labor market histories before the move. In our matching specification, we take advantage of rich information on spouses’ personal characteristics, as well as on employer and regional characteristics which helps us to match well moving and non-moving couples. Whereas most previous studies (Blackburn (2010); Cooke et al. (2009); Rabe (2009)) relied on survey data, we use high quality administrative data which enable us to identify a large number of moves - much more compared to most of the existing studies. Further, the detailed location information of this data allow us to apply a distance-based definition of moves which have turned out to be more precise compared to definitions based on moving across state or district borders (Ham et al. (2011)).

Our main results show that women experience large negative labor market effects in the first years after move. One year after move, yearly labor earnings of moving women decline on average by around € 1800 compared to matched non-movers. In contrast, men experience significant positive returns from moving with an increase in yearly labor earnings of around € 1600 in the first year after move and even around € 4000 five years after.

We then turn to analyzing heterogeneity in treatment effects which has been mainly neglected in the literature. First, we examine whether treatment effects vary by spouses’ within household relative earnings before the move. Bertrand et al. (2015) show that the distribution of income earned by the woman drops sharply at  $\frac{1}{2}$ . They argue that couples avoid the situation where the woman earns more than the man and only *gender identity norms* (Akerlof and Kranton, 2000) could explain this pattern. Evidence from the World Value

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<sup>1</sup>Measured statistic is the total labor force participation for men and women aged 25-54, see OECD statistics: <https://stats.oecd.org>

Survey indeed shows the prevalence of traditional gender norms. In 2013, almost one-fifth of German survey respondents agreed with the statement “If a woman earns more money than her husband, it’s almost certain to cause problems.”, whereas the share of agreement is considerably lower in other countries (see Figure 15). In this paper, we test the hypothesis that couples move to restore the situation in which the husband earns more than the wife and present supporting evidence for this hypothesis. While men could realize significant positive returns from moving regardless their relative pre-move earnings within the household, women’s post-move earnings only increase if they earn a small proportion of households’ earnings. Further, we examine heterogeneity by spouses’ pre-move employment status and show that returns to moving are considerably higher for couples with at least one unemployed spouse compared to couples where both spouses are working prior to the move. In a final step, we use the matching approach to shed light on underlying mechanisms that may drive the spouses’ earnings and employment responses. We hypothesize that the large positive returns of men may be explained by men moving from small, lower paying establishments to larger, higher paying establishments. We built support for this hypothesis by showing that men move to establishments that, on average, pay a higher daily wage and employ more workers. Contrastly, we could not document this finding for women. Finally, we investigate if spouses’ job requirements changes after move. Specifically, we explore if spouses move from low skilled tasks towards higher skilled tasks. We find that transitions to a higher job requirement level are somewhat more pronounced for men compared to women.

Our paper is related to several literatures. First, we contribute to the literature that investigates the effects of joint moves on labor market outcomes of couples (Blackburn (2010); Cooke et al. (2009); Rabe (2009)). Using high-quality administrative data, we could identify a large number of moves that is needed to study heterogeneities, something that has been largely neglected in previous studies. Moreover, this linked employer-employee data set allows us to examine what may drive the spouses’ earnings responses which, to our knowledge, has not been studied in the literature. Second, our paper relates to the literature on the determinants of family migration (Compton and Pollak (2007); Eliasson et al. (2014); Nivalainen (2004); Tenn (2010)). Although the determinants of migration are not the main focus of our paper, they play an important role in our empirical strategy, more precisely in our propensity score specification. In our specification, we use a rich set of characteristics of both spouses, including their employment dynamics before the move. Accounting for the dynamics before the move is crucial to match well treated and non-treated couples. Finally, we also contribute to the large literature on gender identity norms and in particular how they shape labor market outcomes of men and women (Bertrand (2020); Bertrand et al. (2015); Fernández et al. (2004); Kleven et al. (2019)). By presenting evidence for gender differences in the returns to moving, we explore the role of social norms and gender identities in a somewhat different context. Evidence on how gender norms affect joint moves of couples is rare and has been mainly explored in the sociology literature (Bielby and Bielby (1992); Bird and Bird (1985); Cooke (2003); Shihadeh (1991)).

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 effects of long-distance moves along with a number of robustness checks. Finally, section 6 concludes.

## 2 Data, Long-distance Moves and Sample Characteristics

### 2.1 German Administrative Data

For our analysis, we use German administrative data from the *Institute for Employment Research* (IAB). Specifically, the data are a 10% sample of married couples from the *Integrated Employment Biographies* (IEB) plus a 15% oversample of couples experiencing a long-distance move in the years 2008 – 2012.<sup>2</sup> In this data set, married couples are identified according to the method of [Goldschmidt et al. \(2017\)](#): The spouses have to live at the same location, have a matching last name, are of different sex and less than 15 years apart in age. Only couples in which both spouses have a record in the IEB on June 30<sup>th</sup> 2008 are identified and the matching of individuals is only done for that particular date. Note that the IEB includes records of nearly all employees working in the private sector in Germany and only excludes civil servants, self-employed and military personnel. For more information on the dataset and data processing see appendix [subsection A.1](#) and [subsection A.2](#).

Three characteristics make this data set 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, the data include complete labor market histories of both spouses from 1998 – 2017 which allows us to precisely account for pre-move employment dynamics. Finally, the large sample size enables us to identify a large number of moves, much more compared to most of the existing studies.

This data set 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 employment and training measures. For each spell, it contains information on the corresponding wages and benefit levels, respectively. In addition, we observe a rich set of personal characteristics such as occupation, nationality, year of birth, education and place of residence. We use the latter to define moves and merge regional characteristics. For each employee, we also have information on establishment characteristics from the *Establishment History Panel* (BHP) measured on June 30<sup>th</sup> in each respective year. Our sample includes all spells of both spouses from 1998-2017.

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<sup>2</sup>The data is a custom shaped sample from the IEB which have been provided by the Research Data Center as part of the DFG priority program 1764. They are processed and kept by IAB according to Social Code III. The data contain sensitive information and are therefore subject to the confidentiality regulations of the German Social Code (Book I, Section 35, Paragraph 1). The data are held by the Institute for Employment Research (IAB), Regensburger Str.104, D-490478 Nürnberg, email: [iab@iab.de](mailto: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 Centre of the IAB.

## 2.2 Long-distance Moves

We make use of district-level residence information to identify *long-distance moves* of couples.<sup>3</sup> We view long-distance moves as *changes of local labor markets* and require a move across district borders with a distance of at least 50 km (30 miles) as the crow flies and a job change of at least one spouse. A job change is defined as a change of establishment or a transition from unemployment to employment. We do not impose restrictions on the other spouse to allow for the possibility that one spouse might get unemployed due to a move in favor of job opportunities of the other spouse. Since for most spells the information on the place of residence is only determined at the end of each year, we allow for the possibility that one spouse moves in year  $t$  while the other follows in year  $t + 1$ .<sup>4</sup> The distance between districts is calculated as the distance between the centroids of each district:

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

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  $t - 1$ .

Our definition leaves some small room for potential measurement error. For example, there exist few large districts with distances of over 3,000 sq.km. Couples living in these districts could potentially experience a long-distance move without crossing district borders and therefore we could not identify these moves. On the other hand, we could falsely assign long-distance moves to couples living close to district borders. Since we use the distance between centroids, the actual moving distance of those couples could be smaller. However, these cases are clearly exceptions. For more information on the identification of long-distance moves see appendix [subsection A.3](#).

### Baseline restrictions

In our analysis, we consider all long-distance moves of couples from 2008 - 2012. We use only these years because couples are identified in 2008 and therefore it is not certain that two individuals form a couple in the years before. We exclude couples who no longer live together for at least three years after 2008, as these couples may have separated. During the observation period, few couples experience multiple long-distance moves. For these couples, we consider only the first move because future outcomes may be influenced by the first move. Thus, we abstract from repeated migration and return migration. Our analysis is limited to

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<sup>3</sup>In Germany, there currently exist 401 districts.

<sup>4</sup>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.

A one year delay in the moving year applies to approximately 18.04 % of all moves. In 10.26 % of moves the husband moves in year  $t$  and the wife follows in year  $t + 1$ , whereas in 7.78 % the wife is the first spouse who moves.

moves within Germany, as it is not possible to determine the exact moving distance for couples living or moving abroad. Further, the sample is restricted to couples living together in the pre-move year and two years after the move since only joint moves are considered and since we allow a one year delay in the moving year for one spouse. Finally, we define the pre-move year as *reference year  $y$*  and restrict the age of each spouse to 20 - 50 in this year. This upper age limit is chosen to exclude spouses who have access to partial retirement programs. We construct a balanced panel that includes all couples who we observe three years before the move to five years after. Our final sample consists of 166, 723 couples of which 4, 108 experience a long-distance move.<sup>5</sup>

## 2.3 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. Days employed refer to total days employed in all jobs subject to social security contributions and labor earnings refer to total gross yearly labor earnings from all those jobs. For non-working spouses, yearly labor earnings and days employed are zero. Hence, changes in employment and earnings may be either due to changes at the extensive or intensive margin.

Table 1 provides sample characteristics for *movers* and *non-movers* before matching. More precisely, table 1 shows the raw means of selected variables measured in reference year  $y$  by treatment status and gender. The first column reports sample characteristics for moving men and column two for non-moving men, whereas columns three and four report sample characteristics for women. Before matching, there exist large differences between movers and non-movers. We observe that movers are on average about four years younger than non-movers and that women are in general about two years younger than men. In addition, movers are better educated compared to non-movers. Around 28% of moving men hold a university degree, while this applies to only 12 % of non-movers. The same pattern is observed for women, but they are generally less educated than men. With respect to labor market histories, movers work on average less days per year and receive unemployment benefits more often during the three years before the move. Labor earnings of moving men are lower than those of non-movers in the years before move, while moving women earn on average more relative to their non-moving counterparts. Note that compared to non-moving women, moving women work in larger establishments. Finally, movers have on average less tenure at their current job and less moving couples have at least one child.

## 3 Empirical Strategy

The empirical challenge in estimating the effects of a long-distance move stems from the fact that moving couples are a selected sample. Each couple decides whether to move or not to move and we observe large

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<sup>5</sup>Specifically, we identify 1, 362 moves in 2008, 951 in 2009, 723 in 2010, 625 in 2011 and 444 in 2012.

differences in characteristics between moving and non-moving couples. Since those differences potentially relate to future labor market outcomes, this may lead to biased estimates in 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, 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 especially suited in our setting. Our administrative data allows for a rich propensity score specification that takes advantage of information on spouses' past labor market histories and accounts for their employment dynamics. Together with demographic, establishment and regional characteristics, this enables us to well predict counterfactual outcomes. Furthermore, the large sample size of our data allows us to choose suitable controls from a large set of non-moving couples and secures that the matched sample is well balanced.

### 3.1 Parameter of Interest and Identifying Assumptions

Following Rubin (1974), we define treatment effects in terms of potential outcomes.  $Y_i^1 = Y_{it}^1 - Y_{it'}^1$  is defined as the potential outcome if a couple moves and  $Y_i^0 = Y_{it}^0 - Y_{it'}^0$  as the potential outcome if a couple does not move. Since we use a difference-in-difference matching estimator <sup>6</sup>, outcomes are measured relative to year  $t'$  which refers to pre-move year  $y - 2$ . We view the treatment as *moving to new local labor market versus staying* and therefore require a job change of at least one spouse for the treated couples. Note that there is neither restriction that couples in the control group have to experience a job change, nor that they cannot move in future years.

Our goal is to estimate the average treatment effect on the treated (ATT) of experiencing a long-distance move:

$$\tau_t = E\{Y_{it}^1 - Y_{it'}^1 | W = 1\} - E\{Y_{it}^0 - Y_{it'}^0 | W = 1\}, \quad (2)$$

where  $W = 1$  indicates that a couple moves and  $t$  measures the years before/after treatment.

Since outcomes for moving couples are observed, 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 outcomes for the moving couples if they had not moved. These outcomes are estimated using propensity score matching. To estimate these counterfactual outcomes, the following assumptions have to hold:

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

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<sup>6</sup>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 if participants and non-participants are located in different local labor markets. Since moving and non-moving couples are located in different regions, we follow them and use a difference-in-difference matching estimator. See also Ham et al. (2011).



where  $p(X) = Pr(W = 1|X)$  is the propensity score (ex ante probability of experiencing a long-distance move) and  $X$  is a set of covariates realized prior to moving.

Assumption 1 states that conditional on the propensity score, outcomes of moving couples, had they not moved, have to be the same as for control couples. Note that this condition only needs to hold after time-invariant unobserved characteristics have been differenced out. Further, since our parameter of interest is the ATT, this assumption only needs to hold for  $Y^0$  and not for  $Y^1$ . Ideally, our propensity score specification would capture all observable and unobservable characteristics of couples that drive both, the decision to move and future labor market outcomes. In this case, we could interpret the estimated coefficients as the causal effect of a long-distance move for moving couples. Admittedly, we are not able to control for unobserved time-variant characteristics. For example, if the decision to move is determined by private information of spouses such as knowledge about future job loss which is unobservable for the researcher, then our identification strategy would fail. However, we think that our approach should substantially reduce potential bias because we take advantage of rich information on spouses' characteristics as well as their past labor market histories and control for time-invariant unobservable characteristics. In addition to assumption 1, we also need to make the following assumption:

**Assumption 2.**  $p(X) < 1$ .

This assumption requires that we could find a non-treated match for each treated couple. In our setting, this assumption is satisfied. The set of control couples is much larger than the group of treated couples.

### 3.2 Propensity Score Specification

Using a large set of variables as possible covariates, we estimate propensity scores separately for the full sample of couples and for each subsample. We estimate logit models based on covariates which according to theory may affect both, the probability of moving and the outcome variables. We estimate the models by pooling over the five treatment years to ensure that we have enough observations which turns out to be especially important for the subsample analyses. This implicitly assumes that treatment effects are constant over the treatment years. We think that this is a plausible assumption due to the short treatment period that we study. Our specifications include personal characteristics of both spouses, their past labor market histories and characteristics of the region of origin.<sup>7</sup> Particularly, our specification accounts for employment

<sup>7</sup>Our specification for the full sample includes the following covariates:

(i) *Man characteristics*: Age, age<sup>2</sup>, education (4 dummies), tenure at current job (5 dummies, censored in 1998), establishment size, dummy for non-German citizenship, years in region (4 dummies, censored in 1998) (all measured in reference year  $y$ ), days employed in  $y$ ,  $y - 1$  and  $y - 2$  (linear/squared terms), interaction between days employed in  $y$  and  $y - 1$  ( $y - 2$ ), yearly labor earnings in  $y$ ,  $y - 1$  and  $y - 2$ , interaction between yearly labor earnings in  $y$  and  $y - 1$  ( $y - 2$ ), dummy for unemployment benefits in  $y$ ,  $y - 1$  and  $y - 2$ , dummy for positive labor earnings in  $y$ ,  $y - 1$  and  $y - 2$ , dummy for employment in  $y$ ,  $y - 1$  and  $y - 2$

(ii) *Woman characteristics*: Age, age<sup>2</sup>, education (4 dummies), tenure at current job (censored in 1998), total days employed (censored 1998), dummy for non-German citizenship, years in region (4 dummies, censored in 1998) (all measured in reference year  $y$ ), days employed in  $y$ ,  $y - 1$  and  $y - 2$ , interaction between days employed in  $y$  and  $y - 1$  ( $y - 2$ ), yearly labor earnings in  $y$ ,  $y - 1$  and  $y - 2$  (linear/squared terms), interaction between yearly labor earnings in  $y$  and  $y - 1$  ( $y - 2$ ), dummy for unemployment

dynamics before the move. Parameters of the logit model for the full sample are shown in appendix section [B.1](#). Balancing tests for each sample show that the matched samples are well balanced with respect to the included covariates. All  $t$ -tests fail to reject equality of means despite the large sample size (see appendix [B.2](#) for the full sample).

### 3.3 Matching Estimator

Based on the predicted propensity scores from the logit models, we choose for each moving couple the ten closest matches from the set of control couples. Here, we 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, the bias of the estimated treatment effect increases but the variance is reduced. Since we have an extremely large set of control couples, we think that it is worth using more than one control couple to increase the precision of estimates. The ATT parameter in each period is then given by:

$$\hat{\tau}_t = \frac{1}{N} \sum_{i=1}^N W_i \left( Y_i - \frac{1}{10} \sum_{j \in J(i)} Y_j \right), \quad (3)$$

where  $N = \sum_{i=1}^N W_i$  is the number of moving couples,  $Y_i = Y_{it} - Y_{it'}$  is the outcome for moving couple  $i$ ,  $Y_j = Y_{jt} - Y_{jt'}$  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/after move.

The variances of the period ATT's have to be adjusted because the propensity score has been estimated prior to matching. Couples are matched using the estimated value of the propensity score rather than the true one. [Abadie and Imbens \(2016\)](#) show that ignoring the variance adjustment will lead to confidence intervals that are either too large or too small depending on the data generating process. Therefore, we follow their variance adjustment.

## 4 Effects of Long-distance Moves

To measure the effect of long-distance moves, we first investigate the effects on yearly labor earnings, daily wages and days employed per year for men and women. We show the period effects from three years prior move to five years after move for a balanced panel of couples. We then study heterogeneity in treatment effects along two dimensions: (i) spouses' relative earnings within the household before the move and (ii) spouses' employment status in the year before the move. Further, we examine what may account for spouses' earnings responses. In particular, we investigate whether positive labor market returns could be explained by

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benefits in  $y$ ,  $y - 1$  and  $y - 2$ , days receiving unemployment benefits in  $y$ ,  $y - 1$  and  $y - 2$ , dummy for positive labor earnings in  $y$ ,  $y - 1$  and  $y - 2$ , dummy for employment in  $y$ ,  $y - 1$  and  $y - 2$

(ii) *Household characteristics*: Dummy for at least one child (measured in  $y$ )

(iv) *Region characteristics*: District unemployment rate in  $y$ ,  $y - 1$  and  $y - 2$ , interaction between unemployment rate in  $y$  and  $y - 1$  ( $y - 2$ ).

If necessary, we adjusted the logit specification for each subsample.

spouses moving from small, lower paying establishments to larger, higher paying establishments. We also explore if spouses' job requirement level changes after the move. Finally, we show a number of robustness checks.

## 4.1 Effects on Labor Earnings, Employment and Wages

### 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 with the corresponding 95% confidence intervals. Effects on earnings are close to zero prior to move, as expected due to inclusion of pre-move earnings in the propensity score. Note that treatment effects are defined as outcome differences between movers and matched controls relative to year  $t - 3$  which implies that period effects are zero in this year. We observe that men's earnings increase significantly after the move. Moving men earn around €1,600 more relative to the control group in the first year after the event. This effect is increasing over time and moving men earn around €4,000 more relative to their matched controls in year  $t + 5$ . In contrast, women suffer significant earnings losses, especially in the first two years after the move. Compared to their non-moving counterparts, moving women earn around €1,800 less in the first year after move and earnings recover only slowly. While men can realize significant moving gains, this does not apply to women. Looking at total household labor earnings (figure 2 panel A), we see a significant increase in total earnings beginning two years after the move. This implies that the positive effect on men's earnings offsets the negative effect on women's earnings.

### Days employed per year

Panel B of figure 1 shows the results for employment. In the first year after move, men experience a small drop in working days. However, after this initial drop days employed increase significantly in the following years. By contrast, women's employment falls dramatically after the move. Note that the outcome variables include zeros for non-working spouses. Hence, this sharp drop could be due a decrease in working days at the extensive rather than the intensive margin. Women's employment starts to recover, but estimated coefficients are always negative and we observe large gender differences.

### Daily wages

To investigate whether the sharp drop in women's yearly labor earnings and employment after the move is driven by decreases at the extensive or the intensive margin, Panel B of figure 2 shows the effects on daily wages of spouses. We observe a significant increase in men's daily wages. In the first year after moving, daily wages of moving men are on average €3.95 higher than those of matched controls and in year  $t + 5$  they are even €14.95 higher. Contrastly, for women, we see a significant decrease in daily wages in the first two years after the move, while in years two to five effects on daily wages are not statistically significant. The results on wages show that the large increases in yearly labor earnings of men are not only driven by

an increase in employment, but also by an increase in wages. Losses of women are not only driven by a decrease in employment, but also by a decrease in wages.

## 4.2 Heterogeneity

### By Pre-move Intra-households Earnings

Bertrand et al. (2015) show that relative income earned by women drops sharply if women's income exceed men's income. They argue that *gender identity norms* are an important factor in marriage that might limit convergence in the labor market. In this section, we examine whether treatment effects vary by pre-move relative earnings of spouses and whether gender identity norms might play an important role for joint moves. For each couple, we define the relative labor earnings of the woman as:  $\frac{\text{woman labor earnings}}{\text{woman labor earnings} + \text{man labor earnings}}$ . Conversely, the relative labor earnings of the man is defined. We compute the relative earnings for each couple over the pre-event years  $t-5$  -  $t-2$  and take the average of those years. Pre-move year  $y$  is not included to exclude potential pre-event effects on spouses' earnings (Ashenfelter (1978)). We then define four groups based on spouses average relative earnings before the move: (i) 0 – 24%, (ii) 25 – 49%, (iii) 50 – 74% and (iii) 75 – 100%. As before, we match treated and control couples by choosing ten control couples for each treated couple. However, now we require that treatment couples are matched to control couples in the same subgroup.

Figure 3 shows results for the ATT on labor earnings, separately for each of the four groups of couples. To the upper left, panel A displays results for men and women who earn less than 25% of households labor earnings before the move. We observe large increases in men's earning in each post-move year. Relative to controls, moving men in this group earn about € 6,400 more in the first year after move and even € 11,800 more in year  $t + 5$ . In contrast, the estimated coefficients for women are much smaller. For comparison, in year  $t + 5$  they only earn € 1,200 more relative to the controls. However, note that this is the only group for which we could observe positive returns of moving for women. The results for the other three groups of couples are very similar. For each group, we observe large increases in yearly labor earnings of men whereas women experience sharp losses, especially in the first two years after move. Overall, direct comparison between men and women within the same group reveals large gender differences in the returns to moving. Note that the confidence intervals for the coefficients of women are the largest in panel D as we observe only few couples in which the woman earns almost all of the household earnings.

The effects of spouses' employment are shown in Figure 4. We observe a significant increase in employment for all post-move years for men who earn less than 50% of pre-move households earnings (panel A and B) with the largest positive effect for men who earn less than 25% of households earnings. For men who earn more than 50% of pre-move households earnings, days employed per year decrease significantly in the first year after move while effects are close to zero in the years thereafter. For women, we see large decreases in days employed in the first two years after move with the largest reduction for women who earn less than

25% of pre-move households earnings. This contrasts the findings of men whose employment increase is the largest for this group. From year  $t + 3$  onward, effects on women's employment are close to zero and statistically not significant.

### **By Pre-move Employment Status**

We now investigate the effects of long-distance moves by spouses' employment status before the move. Non-employment is an important factor influencing spouses' careers and it is also important in couples' decision to move. Hence, the employment status of spouses is a decisive factor that influences the returns to moving which we expect to be considerably larger for non-employed compared to employed.

For this analysis, we consider two employment states for each spouse: (i) employed and (ii) non-employed. We define a spouse to be non-employed if he or she receives unemployment benefits for more than 182 days in the year before the move or if his or her yearly labor earnings are zero. Results for the average treatment effect on labor earnings of men and women are shown in figure 5. Panel A displays results for couples where both spouses are working in the year before the move, whereas panel B shows results for couples with at least one unemployed spouse prior to moving. In our sample, the majority of moving couples have two employed spouses in the year before the move (3,029 moving couples). For those couples, we observe large increases in men's labor earnings while women suffer large and statistically significant losses in the first two years after move. Women's earnings slowly recover, but they never return to the pre-move level. For couples with at least one unemployed spouse (1,076 moving couples), earnings of both spouses increase significantly after the move.<sup>8</sup> However, even for those couples, we observe large gender differences in the magnitude of estimated coefficients.

The results for the average treatment effect on employment are shown in figure 6. We observe that days employed per year decrease significantly for both, men and women, in the first two years after move if both spouses are employed in  $t - 1$  (panel A). However, women's losses are much larger compared to those of men. From year  $t + 2$  onward, the effects on men's employment are close to zero while women's employment never returns to the pre-move level. For couples with at least one unemployed spouse in  $t - 1$ , we observe large increases in employment for both spouses. Since we observe much more couples where both spouses are employed in  $t - 1$ , the overall effects are mainly driven by those couples. Overall, our results of the heterogeneity analysis confirm our finding of large gender differences in the returns to moving.

## **4.3 What Drives Spouses' Earnings Responses?**

The previous sections have shown 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 examine whether the earnings response of spouses is driven by spouses moving towards larger, higher paying establishments or

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<sup>8</sup>Specifically, we observe that in 181 of the moving couples both spouses are unemployed, in 232 couples only the husband is unemployed, and in 663 couples only the wife is unemployed, respectively.

towards smaller, lower paying establishments, respectively. For this analysis, we consider a balanced panel of couples in which both spouses are working from three years before move up to five years after the move. This leaves us with 2,430 moving and 132,611 non-moving couples. As before, for each moving couple, we choose a set of 10 control couples.

### **Average daily wage of establishment**

Panel A of figure 7 shows the effects on average daily wage of establishments for the subsample of employed men and women. More precisely, the average daily wage of an establishment is defined as the mean imputed gross daily wage of all full-time employees in that establishment. Note that this measure neither includes the wages of marginally or part-time employed workers, nor those of apprentices. For men, we observe a large and statistically significant increase in the average daily wage paid by the establishment for each year after the move (compared to matched controls). In contrast, for women, the average daily wage paid by the establishment decreases significantly after move.

### **Establishment size**

Panel B of figure 7 displays the effects on establishment size which we measure as the total number of employees. The results show that after move, men switch to establishments that are on average larger while women move to smaller establishments. Gender differences in estimated coefficients are large in the years after move, but the coefficients are not statistically significant. Overall, our results show that men move to larger and on average higher paying establishments which may explain their large earnings increases after move.

### **Requirement level of job**

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/ semi-skilled task, (ii) skilled task, (iii) complex task and (iv) highly complex task. We then assign the job transition of each moving man and woman to one of the 16 transition cells, based on the requirement level of the job three years before the move and two years after. 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). For example, in panel A the light blue bar far left on the x-axis refers to moving women who work in unskilled/ semi-skilled jobs (i) before the move and who stay in those types of jobs after the move. The gray bar far left on the x-axis refers to moving women who work in unskilled/ semi-skilled jobs (i) before the move and who switch to skilled jobs (ii) after 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.

We observe that over 40% of women work in skilled tasks prior to move and that they also stay in those jobs after the move. In general, around 18% of moving women move to a higher requirement level after move while around 16% of women move to a lower requirement level. We observe most transitions from pre-move

skilled tasks to lower or higher requirement levels. In comparison to men, women are observed less often in complex and highly complex tasks. For moving men, around 23% move to a higher requirement level after move whereas around 16% of men move to a lower requirement level. While the percentage of moving men and women who move to lower requirement level is nearly the same, a little more men move to a higher requirement level compared to women.

## 4.4 Robustness

We now examine a number of robustness checks and show that the results are highly robust to all of these tests.

### By Moving Distance

In our main analysis, long-distance moves are defined as moves across district borders with distances of at least 50km as the crow flies and a job change of at least one spouse. We choose this cutoff distance since it has been commonly used in the migration literature. In this section, we show that our results are robust with respect to varying cutoff distances. Figure 9 shows results for yearly labor earnings (panel A) and days employed per year (panel B) for cutoff distances of 50, 60, 70, 80, 90 and 100 km, respectively. For readability, we exclude confidence intervals. We observe that estimated coefficients vary only slightly depending on the chosen cutoff distance.

### By Number of Matches

We now present evidence that our results are robust with respect to the chosen matching procedure. In our main analysis, we have chosen 10 non-moving couples for each moving couple (with replacement) who resemble the movers in observable characteristics. Figure 10 displays results for a different number of matches (1, 5 and 15). Panel A shows results for men's labor earnings and panel B for men's employment while panel C displays results for women's earnings and panel D for women's employment, respectively. We observe that the estimated coefficients vary not much depending on the number of matches. Note that, as expected, confidence intervals are the largest for the specification in which we choose only one non-moving couple for each moving couple.

### Cross-sectional Matching Estimator

In our main analysis, we have chosen a difference-in-difference matching estimator. For this estimator, the conditional independence assumption (CIA) only has to hold after unobserved time invariant characteristics of couples have been differenced out and this estimator relies on the additive separability of the error term. In contrast, the cross sectional matching estimator does not rely on the latter assumption. However, the CIA has to hold without removing unobserved time invariant characteristics (Ham et al., 2011). Figure 11 shows that the treatment effects do not vary much between the two estimators.

## 5 Conclusion

Over the past half a century, women have made great strides in the labor market. However, despite substantial gender convergence, there still exist large inequalities between men and women. In this paper, we investigate gender inequalities in the labor market in a somewhat different context: gender differences in the returns to moving. Using a new administrative data set from Germany, we apply difference-in-difference propensity score matching to estimate the earnings and employment effects of long-distance moves of couples. More specifically, we apply a matching approach to compare the labor market outcomes of couples who experience a move (treated) versus observably similar couples who do not (matched non-movers). Our approach takes advantage of personal, employer and regional information as well as on past labor market histories to precisely account for spouses' pre-move employment dynamics which helps to match well moving and non-moving couples.

We show that while men's earnings increase significantly after the move, women suffer large losses in the first years after the move. Whereas this finding is mostly consistent with previous studies ([Blackburn, 2010](#); [Cooke et al., 2009](#); [Rabe, 2009](#)), much less is known about the underlying mechanisms. Using linked employer-employee data, we present evidence what may drive spouses' responses. We find that men move to larger, higher paying establishments, while women move to smaller, lower paying establishments. Further, we show that after move men switch to jobs with a higher requirement level more often compared to women. Finally, we explore effect heterogeneity by spouses' pre-move relative earnings and find evidence for large gender asymmetries. If a man and a woman earn the same share of household earnings prior to move, the estimated returns to moving are much larger for the man than for the woman. This suggest that gender identity norms may be an important factor in joint moves which influence both spouses' careers.



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**Table 1: Sample descriptives, moving and non-moving couples**

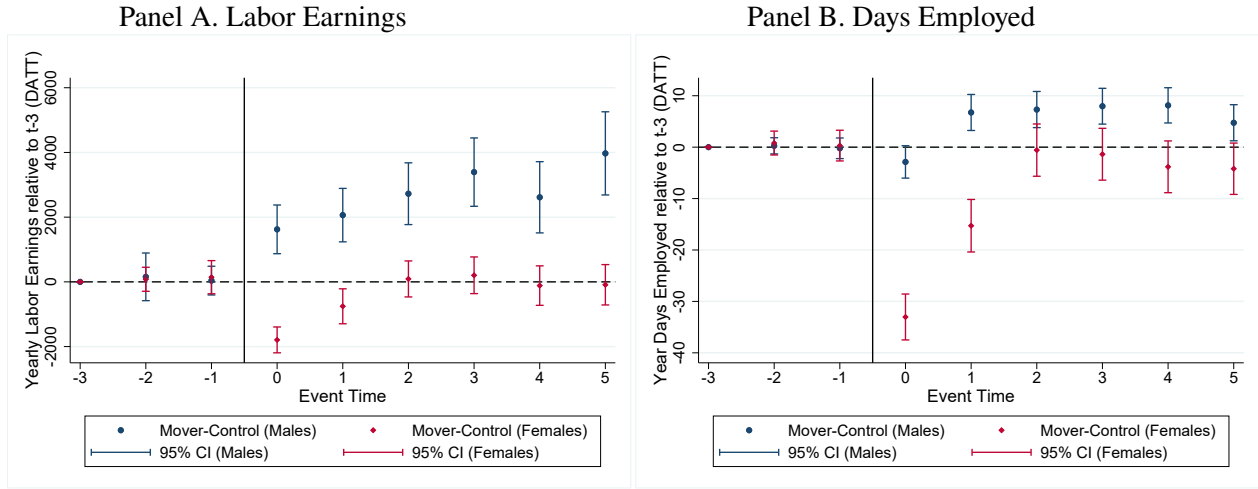
	Men		Women	
	Mover (1)	Non-Mover (2)	Mover (3)	Non-Mover (4)
Age (years)	37.17 (6.95)	41.11 (6.14)	34.92 (7.10)	38.99 (6.38)
<i>Age group (years)</i>				
20-29	0.15 (0.36)	0.05 (0.22)	0.26 (0.44)	0.10 (0.30)
30-39	0.46 (0.50)	0.30 (0.46)	0.45 (0.50)	0.38 (0.49)
40-49	0.36 (0.48)	0.60 (0.49)	0.28 (0.45)	0.51 (0.50)
50-59	0.03 (0.16)	0.05 (0.21)	0.01 (0.10)	0.02 (0.12)
<i>Education</i>				
No/ not recognised education	0.18 (0.38)	0.15 (0.36)	0.20 (0.40)	0.18 (0.38)
In-company/ school-based training	0.52 (0.50)	0.71 (0.45)	0.56 (0.50)	0.71 (0.45)
University degree	0.28 (0.45)	0.12 (0.32)	0.19 (0.40)	0.07 (0.25)
Non-German	0.11 (0.32)	0.08 (0.28)	0.13 (0.34)	0.09 (0.28)
Real yearly labor earnings $y$ (€)	41,016.08 (36,237.91)	43,395.95 (30,419.91)	18,191.11 (20,746.76)	17,752.74 (16,764.52)
Real yearly labor earnings $y-1$ (€)	39,364.10 (37,246.30)	42,460.29 (29,536.73)	17,686.99 (19,993.51)	17,085.58 (16,610.96)
Real yearly labor earnings $y-2$ (€)	34,307.22 (34,307.22)	41,451.53 (28,655.05)	17,344.06 (20,232.44)	16,457.24 (16,489.00)
Days employed $y$	299.44 (119.34)	336.96 (85.30)	256.19 (147.47)	313.07 (113.79)
Days employed $y-1$	297.45 (125.07)	333.75 (89.92)	252.52 (150.29)	303.69 (122.65)
Days employed $y-2$	288.57 (132.46)	329.89 (95.31)	246.60 (152.31)	292.66 (131.75)
Days receiving benefits $y$	15.09 (51.36)	6.30 (32.93)	11.25 (46.25)	6.04 (34.42)
Days receiving benefits $y-1$	11.49 (44.10)	7.21 (35.35)	10.08 (42.89)	6.65 (36.43)

(continued)

**Sample descriptives, moving and non-moving couples (*continued*)**

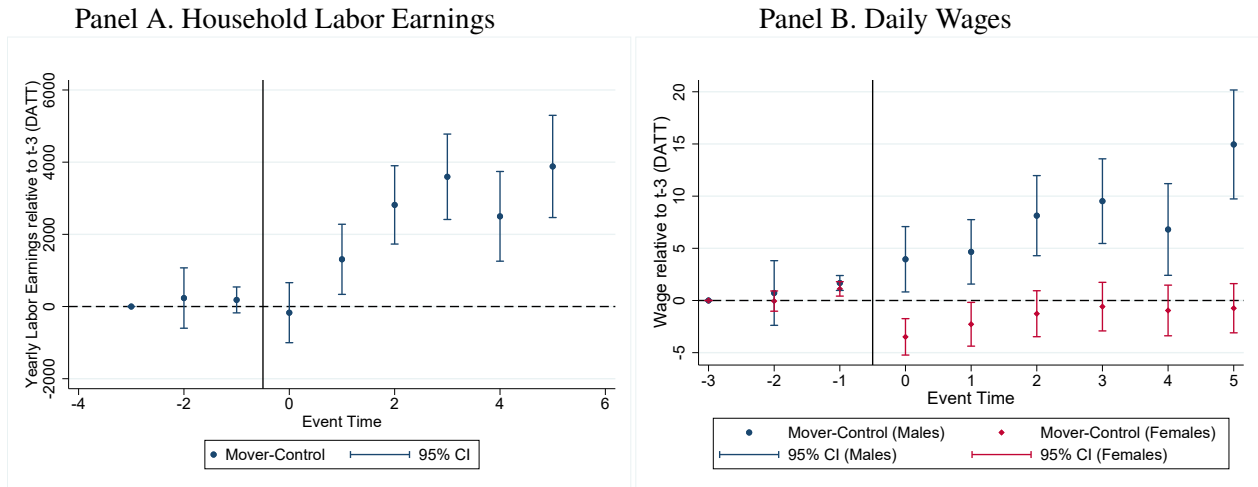
	Men		Women	
	Mover (1)	Non-Mover (2)	Mover (3)	Non-Mover (4)
Days receiving benefits $y-2$	11.49 (44.22)	8.21 (37.46)	11.05 (45.46)	7.73 (39.62)
Tenure at current job (years)	2.94 (1,210.76)	5.85 (1,544.63)	2.40 (1,091.38)	4.33 (1,390.36)
Plant size	726.99 (2,706.38)	1,126.61 (4,544.62)	502.96 (2,002.03)	487.20 (2,288.29)
Total days employed (censored 1998)	2,634.21 (1,334.92)	3,392.61 (1,073.87)	2,140.60 (1,327.29)	2,743.97 (1,214.46)
Children	0.40 (0.49)	0.67 (0.47)	0.40 (0.49)	0.67 (0.47)
Years in region (censored 1998)	8.05 (2.11)	9.09 (1.61)	8.02 (2.10)	9.08 (1.61)
Area unemployment rate	8.52 (3.93)	7.91 (3.70)	8.52 (3.93)	7.91 (3.70)
Area GDP per capita	32.93 (15.33)	30.06 (12.64)	32.93 (15.33)	30.06 (12.64)
Observations	4,108	162,615	4,108	162,615

*Note:* 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 reference year  $y$ . See section 2.2 for more details about the restrictions. All variables are measured in year  $y$  if not stated differently. Days employed and days receiving benefits refer to total days employed per year and total days receiving unemployment benefits, respectively. There is one observation per couple.



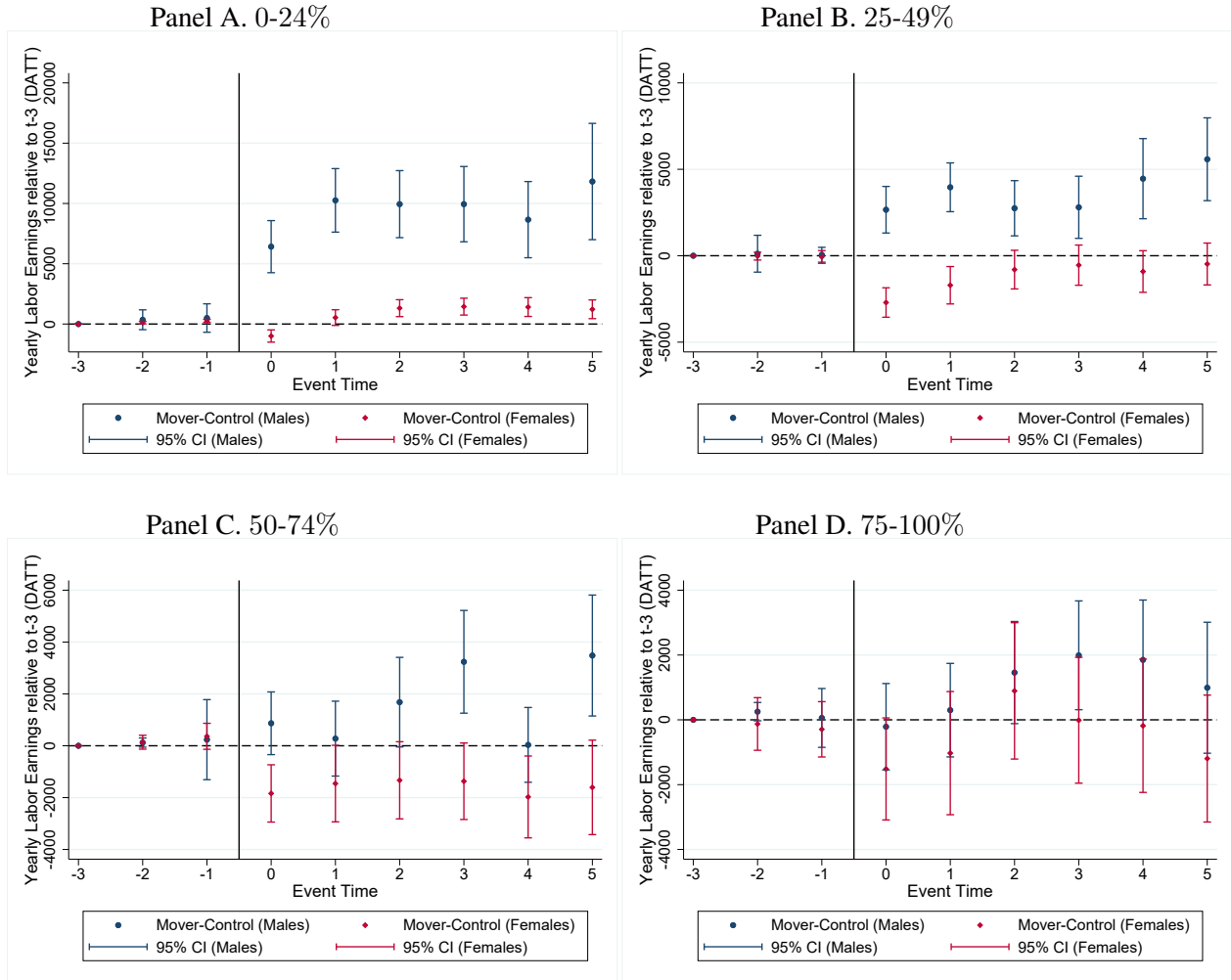
**Figure 1: Effects on Individual Labor Earnings and Employment**

*Note:* To the left, panel A displays the period ATT's on yearly labor earnings for men (blue) and women (red) relative to year  $t-3$  (after propensity-score matching) with the corresponding 95% confidence intervals. To the right, panel B displays the period ATT's on days employed per year.



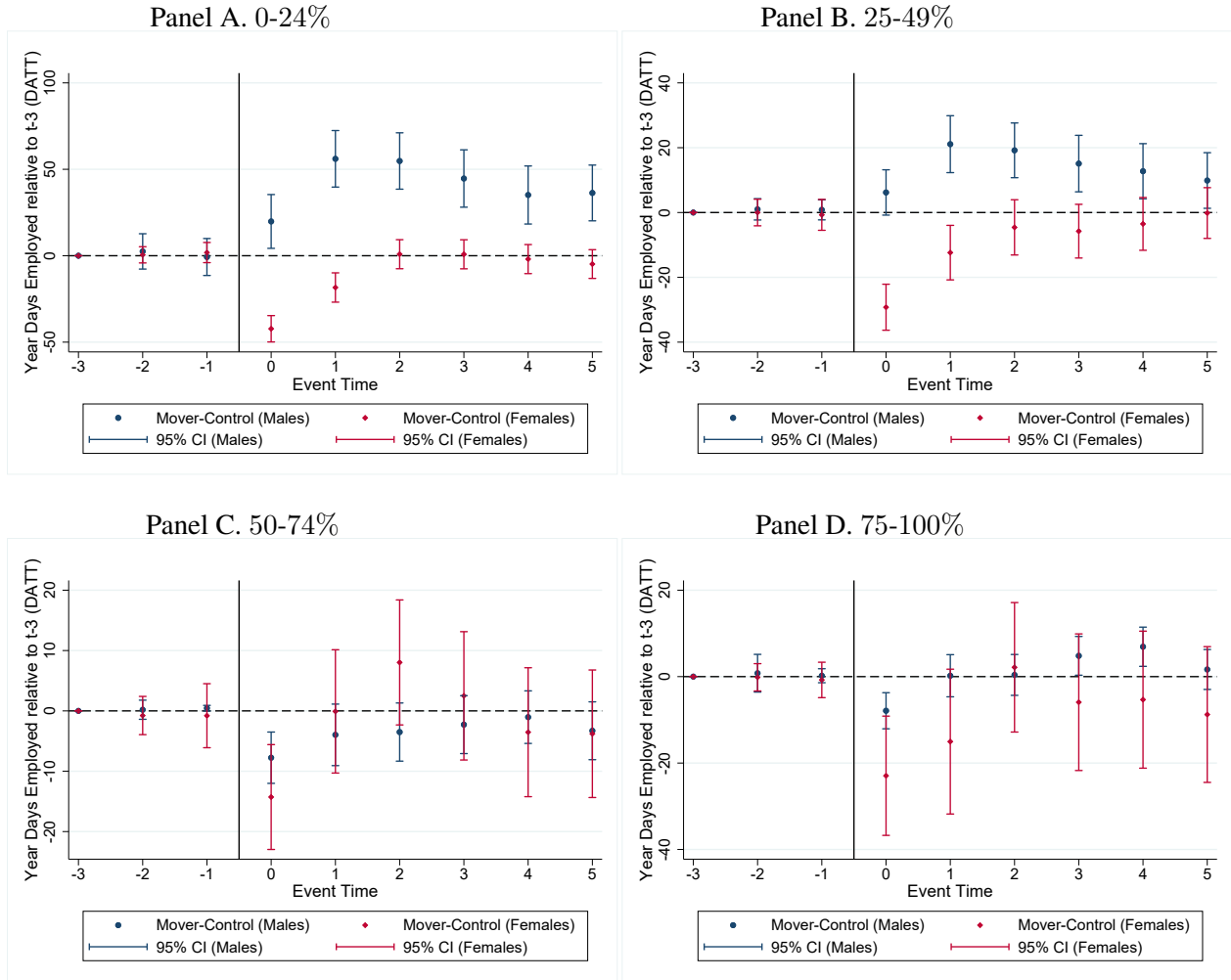
**Figure 2: Effects on Household Labor Earnings and Daily Wages**

*Note:* To the left, panel A displays the period ATT's on yearly household labor earnings for men (blue) and women (red) relative to year  $t-3$  (after propensity-score matching) with the corresponding 95% confidence intervals. To the right, panel B displays the period ATT's on daily wages.



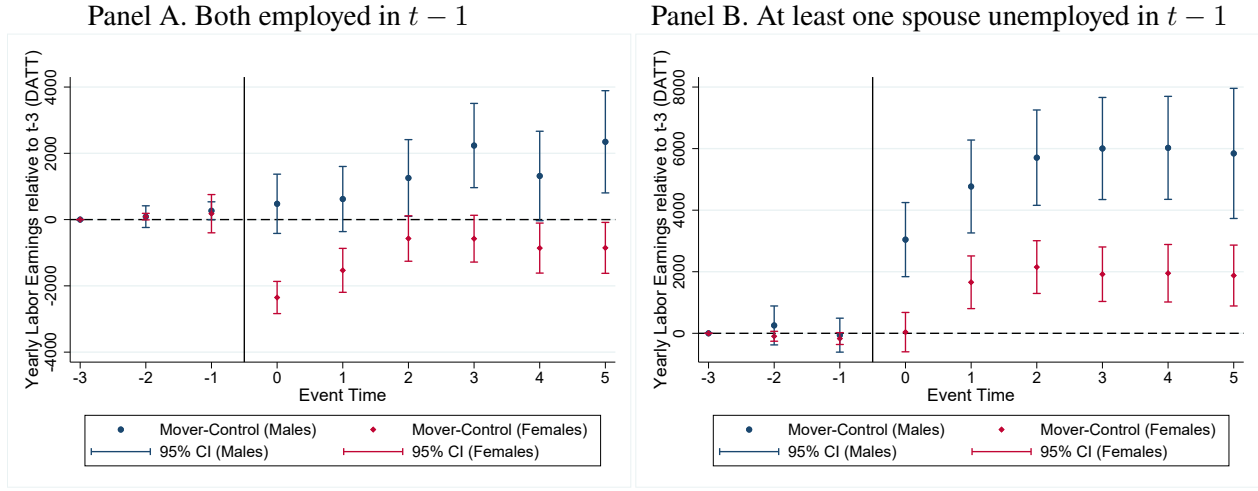
**Figure 3:** Effects on Individual Labor Earnings (by intra-household earnings before move)

*Note:* This figure displays the period ATT's on yearly labor earnings for men (blue) and women (red) relative to year  $t - 3$  for subgroups defined by pre-move intra-household earnings (after propensity-score matching) with the corresponding 95% confidence intervals.



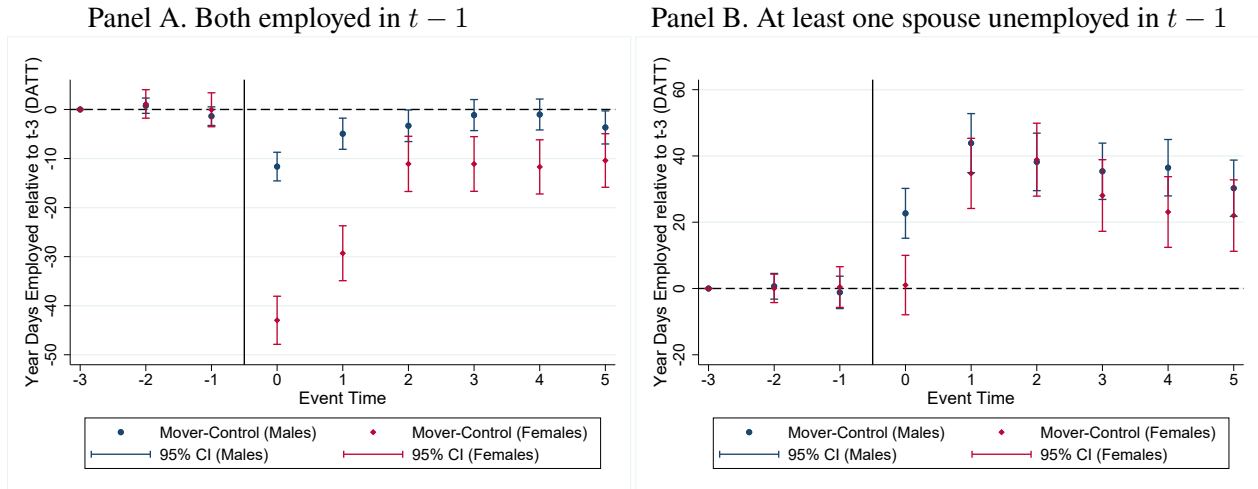
**Figure 4:** Effects on Individual Employment (by intra-household earnings before move)

*Note:* This figure displays the period ATT's on year days employed for men (blue) and women (red) relative to year  $t - 3$  for subgroups defined by pre-move intra-household earnings (after propensity-score matching) with the corresponding 95% confidence intervals.



**Figure 5: Effects on Individual Labor Earnings (by Pre-move Employment Status)**

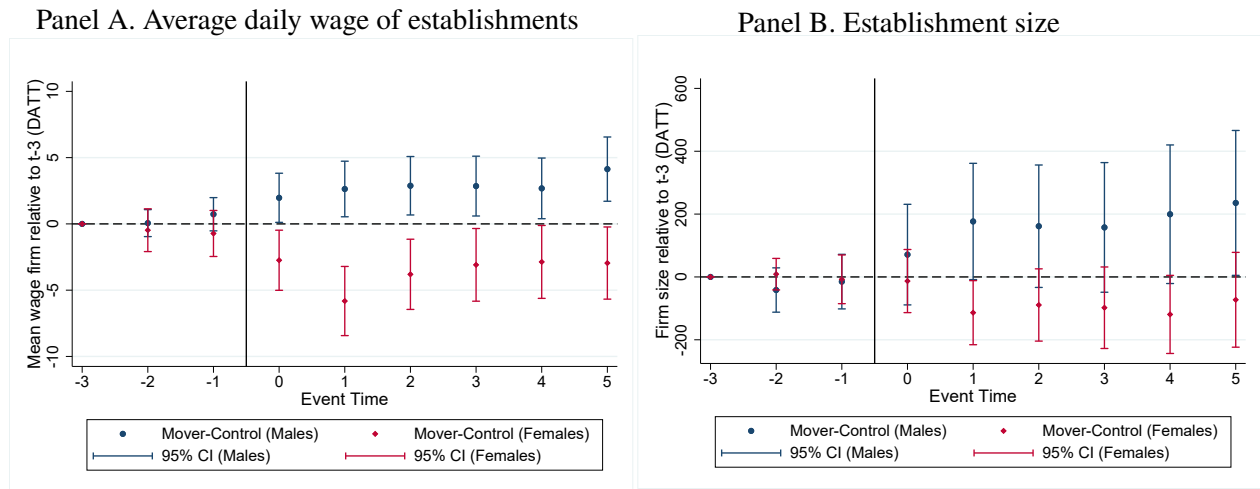
*Note:* This figure displays the period ATT's on yearly labor earnings for men (blue) and women (red) relative to year  $t - 3$  for subgroups defined by pre-move employment status (after propensity-score matching) with the corresponding 95% confidence intervals.



**Figure 6: Effects on Individual Employment (by Pre-move Employment Status)**

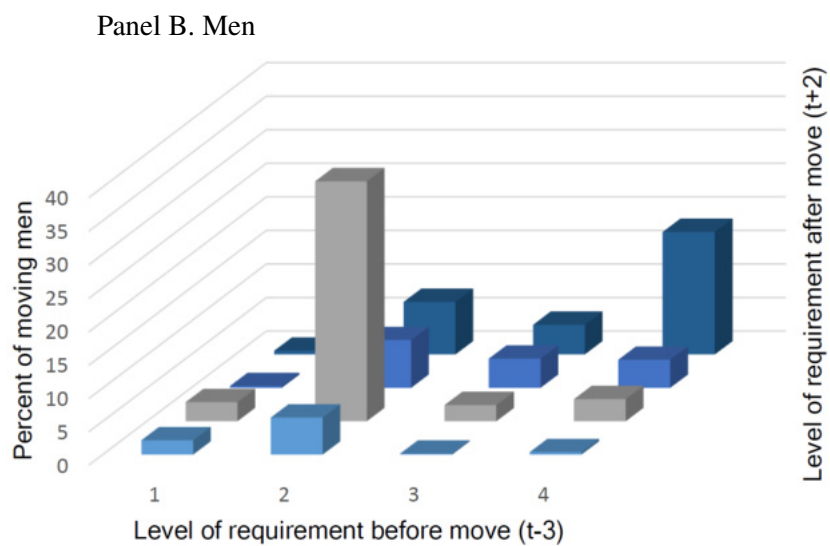
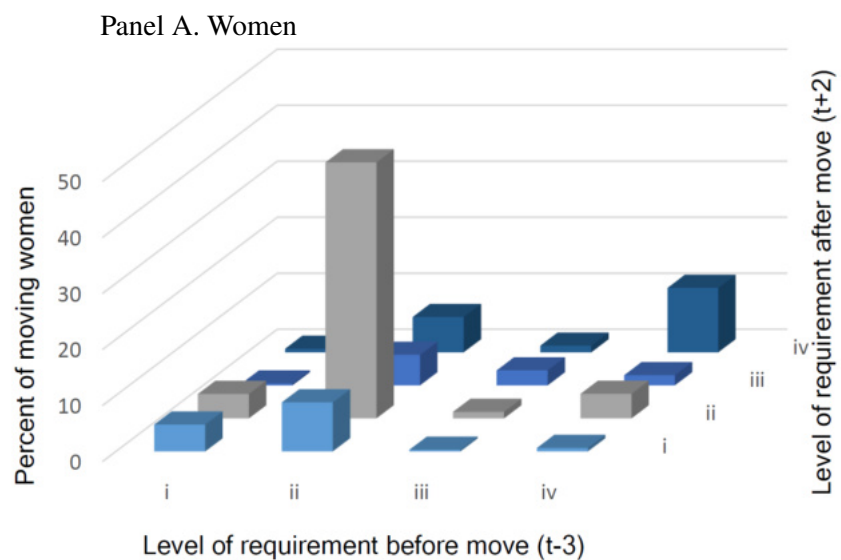
*Note:* This figure displays the period ATT's on year days employed for men (blue) and women (red) relative to year  $t - 3$  for subgroups defined by pre-move employment status (after propensity-score matching) with the corresponding 95% confidence intervals.





**Figure 7: Effects on Establishment Characteristics**

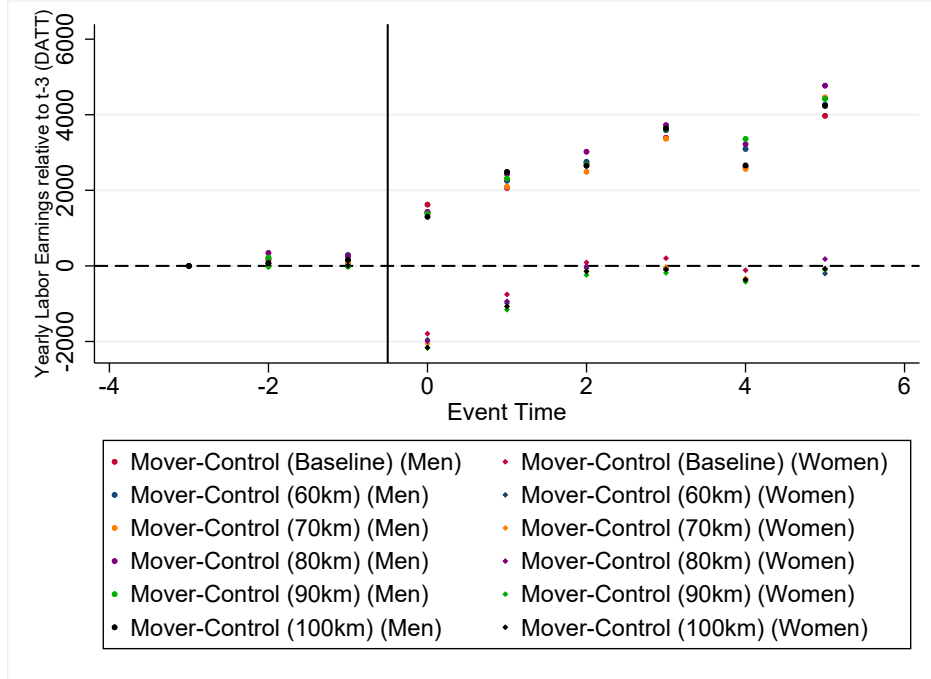
*Note:* Panel A displays the period ATT's on average daily wage of establishment for men (blue) and women (red) relative to year  $t - 3$  (after propensity-score matching) with the corresponding 95% confidence intervals. Panel B shows the period ATT's on establishment size.



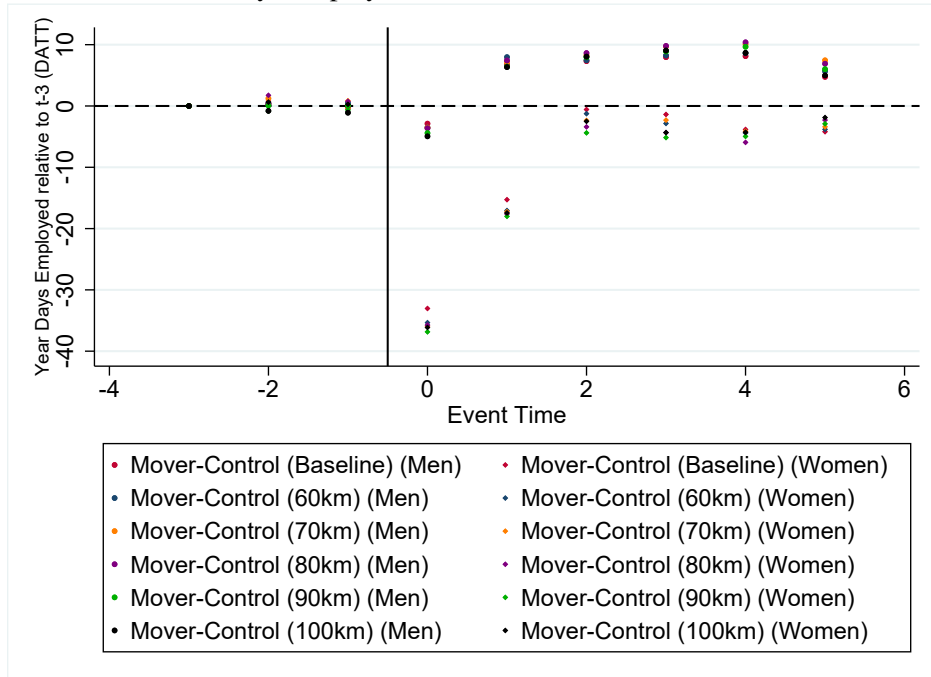
**Figure 8:** Transitions requirement level of job

*Note:* Panel A displays the transition probabilities for women's level of requirement and panel B men's level of requirement, respectively.  $T - 3$  refers to the pre-move period while  $t + 2$  refers to the post-move period.

Panel A. Labor Earnings

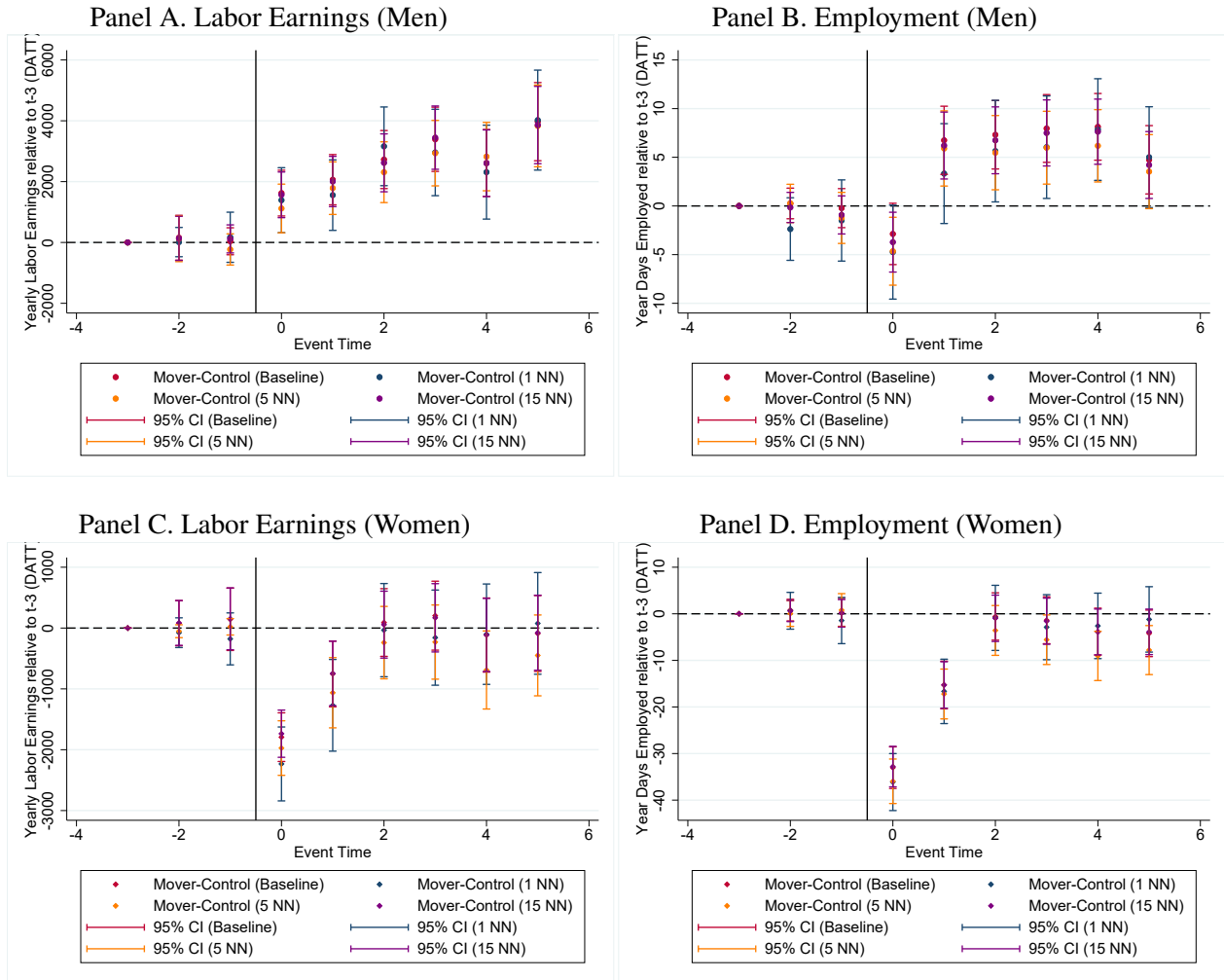


Panel B. Days Employed



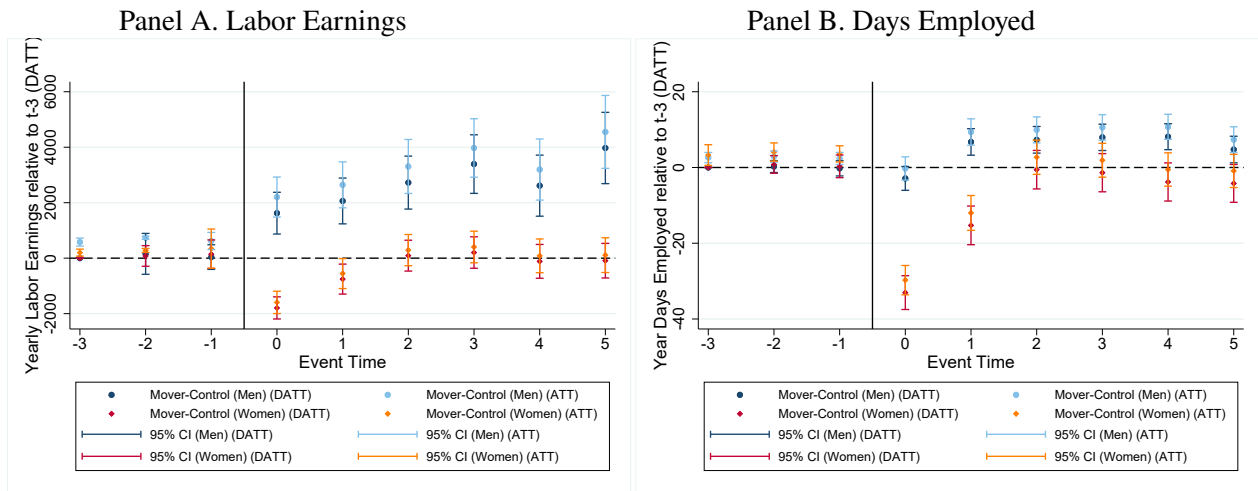
**Figure 9: Effects on Individual Labor Earnings and Employment by Varying Cutoff Distance**

Note: Panel A displays the period ATT's on 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 period ATT's on days employed per year.



**Figure 10: Effects on Individual Labor Earnings and Employment by Varying Number of Matches**

*Note:* To the left, panel A displays the period ATT's on yearly labor earnings for men relative to year  $t - 3$  (after propensity-score matching) with the corresponding 95% confidence intervals. To the right, panel B displays the period ATT's on days employed per year for men. Panel C and D show results for women.



**Figure 11: Effects on Individual Labor Earnings and Employment (DATT vs. ATT)**

*Note:* To the left, panel A displays the period ATT's on yearly labor earnings for men (blue) and women (red) relative to year  $t - 3$  (after propensity-score matching) with the corresponding 95% confidence intervals. To the right, panel B displays the period ATT's on days employed per year, respectively.

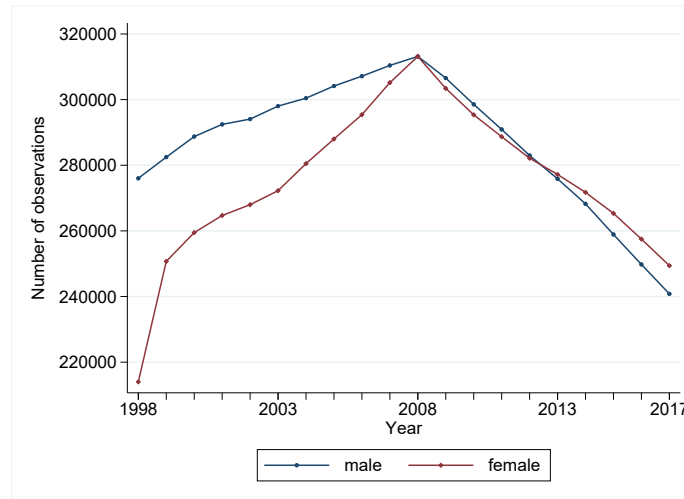
## A Dataset and Data Processing

### 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 sex and less than 15 years apart in age. Only couples in which both spouses have a record in the IEB on June 30<sup>th</sup> 2008 could be identified. This sample of couples is not fully representative for all German couples due to the nature of the IEB. Couples in which one spouse works in a civil service job, is unemployed (not registered), self-employed or already retired (in 2008) could not be identified. Furthermore, according to [Goldschmidt et al. \(2017\)](#) it is more likely to pick up married couples who live in smaller buildings due to the methodology which is used to identify couples. For example, it could happen that a couple lives at a location where an other person also shares their last name. In this case, the couple 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 – 90% of German couples share their last name and therefore only few couples could not be identified. However, it is often assumed that couples who share their last name are couples with more traditional gender roles and this fact has to be taken into account. In total, they could 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. \(2017\)](#).

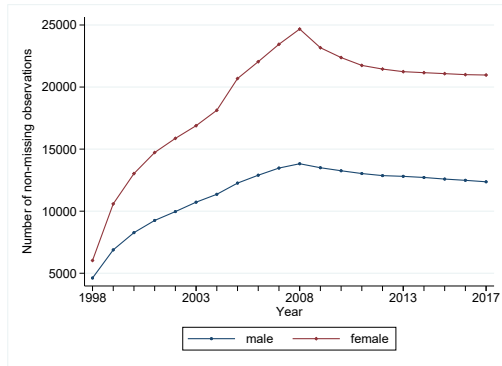
The matching of individuals is only done for that particular year and therefore the sample size is decreasing the further the observation is apart from that year. For illustration of the total sample size see figure [Figure 12](#). The decrease in the total sample size after 2008 is mainly driven by the older cohorts (see [Figure 13](#)). Since we restrict our sample to couples aged 20 – 50 in the reference year, this is only a minor problem in our sample. Before 2008, the decrease in the sample size is more pronounced for females than for males. This is possibly driven by periods of non-employment due to the birth of a child. For our analysis, we consider long-distances moves occurring between 2008 and 2012. We show the effects from three years prior to the move up to five years after the move. Hence, the first year that we consider is 2005. From [Figure 12](#), it is visible that the problem of decreasing sample size is especially pronounced for the early years which we do not consider in our analysis.

**Figure 12: Total Sample Size**

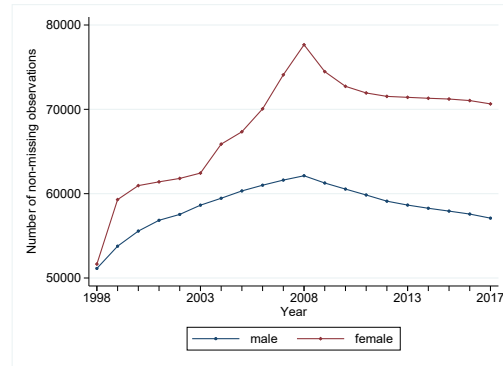


Note: The figure shows the total sample size over the observation period.

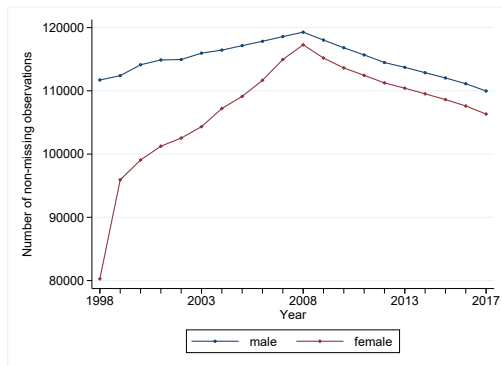
**Figure 13: Sample Size by Age Group**



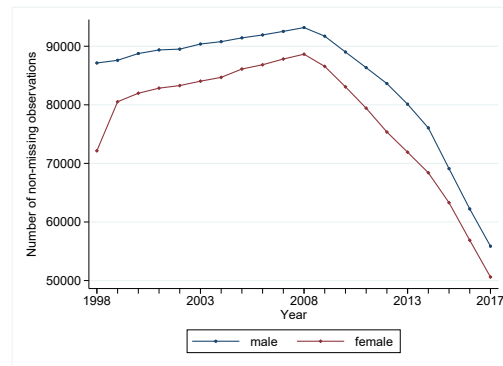
**(a) 20-29**



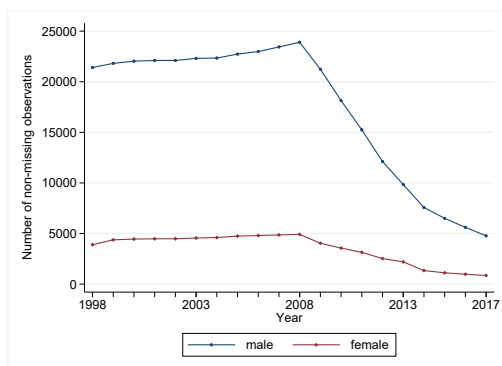
**(b) 30-39**



**(c) 40-49**



**(d) 50-59**



**(e) >59**

*Note:* The figures show the sample sizes by age groups. 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, person ID, establishment ID, start and end date of the spell, daily wage or benefit level, various demographic characteristics and information on establishment characteristics.

Since the wage information is generated from employer submitted employment records, the wage information in our data is highly reliable. However, we could 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 (4 groups: missing; no qualification/ qualification not recognised; some post secondary; and university degree), sex and region (east/west) including the following variables: age, age<sup>2</sup>, tenure, tenure<sup>2</sup>, dummy for 20 or more employees, dummy for age above 40, interaction between dummy for age above 40 and age (age<sup>2</sup>), respectively. We deflate prices to 2015 prices using the consumer price index.

We take one record for each spouse in each year using the spell that includes June 30<sup>th</sup> of the respective year. If an individual has multiple spells which include June 30<sup>th</sup>, we first sort spells by source and keep only one record per year.<sup>9</sup> 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 and therefore it could be that spells do not include June 30<sup>th</sup>. In this case, we sort those spells and also keep one record per year even if in that case establishment characteristics are not measured at the respective spell.

For each year, we merge spouses using the couple ID. There are spouses which drop out of our sample because they are simply not covered in the IEB anymore. This may happen for multiple reasons. They could start working in a civil service job, start working in self-employment, drop out of the labor force, get 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.<sup>10</sup> We keep observations for individuals in unregistered unemployment until the end of the observation period and assume zero labor earnings. We then drop couples after the last observation for both spouses.

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<sup>9</sup>The sorting order is the following: BeH, LeH, LHG, MTH, XMTH, ASU, XASU.

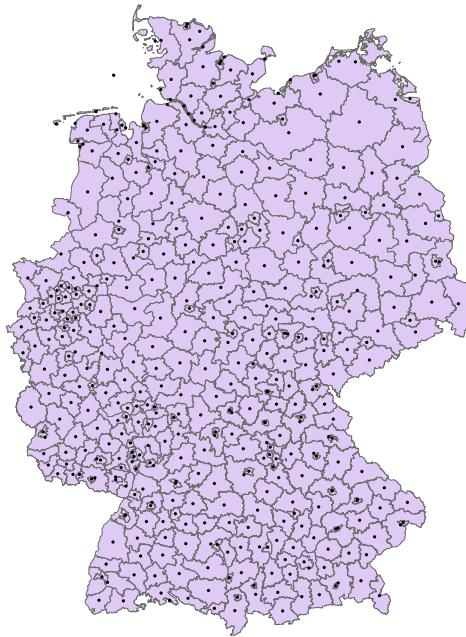
<sup>10</sup>We use the following reasons to identify transitions from registered to unregistered unemployment: 2003 Expiration of entitlement, 2009 Expiration of partial unemployment benefit, 2010 Lack of cooperation of job seeker, 2011 Lack of willingness to work, 2020 Termination of period for which 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 3<sup>rd</sup> 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 SGBI, 6018 Lack of cooperation, 6026 Lack of availability/ cooperation.



### A.3 Identifying Long-distance Moves

To identify long-distance moves, we use information on the place of residence of each spouse which is available at the district level in the IEB. 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<sup>th</sup> 2017. We use a GIS software to identify the GPS coordinates of the center of each German district. 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 14](#).

**Figure 14:** Districts with GPS Coordinates of the Center



## B Propensity Score and Matching Diagnostics

This section presents the estimation results for the logit model and 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 Specifications

**Table B1: Partial Effects of Logit Model**

	(1) Full sample
<i>Man characteristics</i>	
Age	0.192*** (6.25)
Age <sup>2</sup>	-0.00277*** (-6.73)
Education 1	-0.168 (-1.42)
Education 2	-0.0969 (-0.84)
Education 3	0.581*** (4.83)
Tenure 1	-0.0436 (-0.46)
Tenure 2	-0.306** (-2.77)
Tenure 3	-0.314** (-2.76)
Tenure 4	-0.788*** (-7.45)
Firm size	-0.0000255*** (-4.44)
Non-German citizenship	-0.238*** (-3.95)
Years in region 1	-0.199 (-0.89)
Years in region 2	-0.865*** (-3.75)
Years in region 3	-1.197*** (-4.16)
Year days employed	0.000737 (0.53)
Year days employed $y - 1$	-0.00112 (-0.79)
Year days employed $y - 2$	0.000418 (0.30)

(continued)

**Table B1: Partial Effects of Logit Model (continued)**

	(1) Full sample
Yearly labor earnings	0.00000599*** (4.30)
Yearly labor earnings $y - 1$	0.00000417* (2.02)
Yearly labor earnings $y - 2$	0.00000148 (0.67)
Dummy for benefits	0.315*** (4.37)
Dummy for benefits $y - 1$	-0.00955 (-0.12)
Dummy for benefits $y - 2$	-0.0704 (-1.01)
Dummy for employment	1.084** (2.81)
Dummy for employment $y - 1$	-1.264 (-1.73)
Dummy for employment $y - 2$	0.646 (1.79)
Dummy for positive earnings	-0.531 (-1.40)
Dummy for positive earnings $y - 1$	1.277 (1.76)
Dummy for positive earnings $y - 2$	-0.629 (-1.77)
Yearly labor earnings # Yearly labor earnings $y - 1$	-3.08e-11 (-1.78)
Yearly labor earnings # Yearly labor earnings $y - 2$	-1.86e-11 (-0.99)
Year days employed # Year days employed $y - 1$	0.00000325 (1.56)
Year days employed # Year days employed $y - 2$	-0.00000443** (-2.90)
Year days employed <sup>2</sup>	-0.00000804** (-2.61)
Year days employed <sup>2</sup> $y - 1$	0.00000155 (0.48)
Year days employed <sup>2</sup> $y - 2$	0.000000585 (0.19)
<i>Woman characteristics</i>	
Age	0.0378 (1.24)
Age <sup>2</sup>	-0.000865* (-2.07)
Education 1	0.0761 (0.84)
Education 2	0.182* (2.05)
Education 3	0.558*** (5.66)

*(continued)*

**Table B1: Partial Effects of Logit Model (continued)**

	(1) Full sample
Tenure	-0.000214*** (-10.72)
Total days employed	0.0000101 (0.44)
Non-German citizenship	-0.0725 (-1.29)
Years in region 1	0.122 (0.54)
Years in region 2	-0.473* (-2.03)
Years in region 3	-1.089*** (-3.76)
Year days employed	-0.00110** (-2.71)
Year days employed $y - 1$	0.000611 (1.29)
Year days employed $y - 2$	-0.000442 (-1.09)
Yearly labor earnings	0.00000369 (1.09)
Yearly labor earnings $y - 1$	0.00000140 (0.31)
Yearly labor earnings $y - 2$	0.0000110** (2.99)
Year days benefits	-0.000927 (-1.46)
Year days benefits $y - 1$	-0.00172** (-2.62)
Year days benefits $y - 2$	-0.000228 (-0.36)
Dummy for benefits	0.170 (1.60)
Dummy for benefits $y - 1$	0.176 (1.63)
Dummy for benefits $y - 2$	0.189 (1.83)
Dummy for employment	0.372 (1.91)
Dummy for employment $y - 1$	-0.0507 (-0.24)
Dummy for employment $y - 2$	0.283 (1.48)
Dummy for positive earnings	-0.191 (-1.04)
Dummy for positive earnings $y - 1$	0.124 (0.61)
Dummy for positive earnings $y - 2$	-0.142 (-0.77)
Yearly labor earnings # Yearly labor earnings $y - 1$	-2.49e-10** (-2.92)

*(continued)*

**Table B1: Partial Effects of Logit Model (continued)**

	(1) Full sample
Yearly labor earnings # Yearly labor earnings $y - 2$	-1.06e-11 (-0.16)
Year days employed # Year days employed $y - 1$	-0.00000396** (-2.80)
Year days employed # Year days employed $y - 2$	-0.000000589 (-0.51)
Yearly labor earnings <sup>2</sup>	7.77e-11** (2.79)
Yearly labor earnings <sup>2</sup> $y - 1$	7.05e-11 (1.57)
Yearly labor earnings <sup>2</sup> $y - 2$	-3.85e-11 (-1.07)
<i>Household characteristics</i>	
Dummy for at least one child	-1.141*** (-30.95)
Dummy for event in 2009	0.437*** (9.12)
Dummy for event in 2010	0.645*** (10.51)
Dummy for event in 2011	0.896*** (14.54)
Dummy for event in 2012	1.538*** (19.43)
<i>Regional characteristics</i>	
Unemployment rate	-0.0322 (-1.66)
Unemployment rate $y - 1$	-0.0862** (-2.97)
Unemployment rate $y - 2$	0.0970*** (4.04)
Unemployment rate # Unemployment rate $y - 1$	0.0123*** (5.00)
Unemployment rate # Unemployment rate $y - 2$	-0.00913*** (-4.06)
Constant	-5.374*** (-10.58)
<i>N</i>	166723

*Note:* All covariates are measured in reference year  $y$  if not stated differently. Education 1 refers to no/ no recognised education, education 2 to in-company/ school-based training, education 3 to university/ university of applied sciences, missings as reference category. Tenure 1 refers to less than 1 year tenure at the current job, tenure 2 to 1 to less than 2 years, tenure 3 to 2 to less than 3 years, tenure 4 to more than 3 years, missings as reference category. Years in region 1 refers to 4 to less than 8 years, years in region 2 to 8 to less than 12 years and years in region 3 to more than 12 years, less than 4 years as reference category. Dummy for event in 2008 as reference category.  $T$ -statistics in parentheses, \*, \*\*, \*\*\* denote significance at the 10%-, 5%- and 1%, respectively.

## B.2 Balancing Tests

**Table B2: Balancing Tests Full Sample**

	Mean			
	Mover (1)	Non-Mover (2)	%-Bias (3)	p-value (4)
<i>Man characteristics</i>				
Age	37.174	37.109	1.0	0.671
Age <sup>2</sup>	1430.1	1425.1	1.0	0.656
Education 1	.17637	.18614	-2.6	0.251
Education 2	.52058	.52524	-1.0	0.673
Education 3	.28088	.2666	3.7	0.147
Tenure 1	.25993	.26071	-0.2	0.936
Tenure 2	.12984	.13296	-1.0	0.676
Tenure 3	.09695	.09603	0.3	0.887
Tenure 4	.36054	.35084	2.0	0.359
Firm size	727.36	670.63	1.5	0.344
Non-German citizenship	.11181	.12041	-2.9	0.224
Years in region 1	.33861	.33391	1.2	0.652
Years in region 2	.59099	.59949	-1.9	0.433
Years in region 3	.04409	.04314	0.4	0.833
Year days employed	299.46	297.07	2.3	0.368
Year days employed $y - 1$	297.46	294.59	2.6	0.302
Year days employed $y - 2$	288.59	285.97	2.3	0.374
Yearly labor earnings	41132	40512	1.8	0.449
Yearly labor earnings $y - 1$	39338	38601	2.2	0.358
Yearly labor earnings $y - 2$	36891	36310	1.8	0.438
Dummy for benefits	.12497	.12519	-0.1	0.976
Dummy for benefits $y - 1$	.10183	.10351	-0.6	0.802
Dummy for benefits $y - 2$	.09647	.10285	-2.3	0.334
Dummy for employment	.92619	.92007	2.7	0.298
Dummy for employment $y - 1$	.89915	.89323	2.3	0.379
Dummy for employment $y - 2$	.88015	.87703	1.1	0.665
Dummy for positive earnings	.924	.91842	2.4	0.348
Dummy for positive earnings $y - 1$	.89866	.89294	2.2	0.396
Dummy for positive earnings $y - 2$	.87771	.87486	1.0	0.695
Yearly labor earnings # Yearly labor earnings $y - 1$	2.7e+09	2.7e+09	0.8	0.721
Yearly labor earnings # Yearly labor earnings $y - 2$	2.5e+09	2.5e+09	0.6	0.813

(continued)

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

	Mean		%Bias	p-value
	Mover (1)	Non-Mover (2)		
Year days employed # Year days employed $y - 1$	99895	98848	2.3	0.355
Year days employed # Year days employed $y - 2$	95136	94146	2.1	0.403
Year days employed <sup>2</sup>	1.0e+05	1.0e+05	2.1	0.422
Year days employed <sup>2</sup> $y - 1$	1.0e+05	1.0e+05	2.6	0.302
Year days employed <sup>2</sup> $y - 2$	1.0e+05	99789	2.3	0.369
<i>Woman characteristics</i>				
Age	34.923	34.846	1.1	0.626
Age <sup>2</sup>	1270	1264.8	1.0	0.643
Education 1	.20024	.21013	-2.5	0.267
Education 2	.56443	.56594	-0.3	0.890
Education 3	.19513	.18149	4.1	0.114
Tenure	876.25	847.02	2.3	0.227
Total days employed	2141.4	2119.5	1.7	0.462
Non-German citizenship	.13423	.14146	-2.3	0.342
Years in region 1	.34202	.33428	2.0	0.458
Years in region 2	.59026	.59963	-2.1	0.387
Years in region 3	.04214	.04139	0.3	0.864
Year days employed	256.28	252.69	2.7	0.274
Year days employed $y - 1$	252.52	248.44	3.0	0.221
Year days employed $y - 2$	246.65	243.36	2.3	0.332
Yearly labor earnings	18218	17874	1.8	0.454
Yearly labor earnings $y - 1$	17694	17415	1.5	0.527
Yearly labor earnings $y - 2$	17358	17158	1.1	0.654
Year days benefits	11.257	10.856	1.0	0.690
Year days benefits $y - 1$	10.092	10.275	-0.5	0.847
Year days benefits $y - 2$	11.061	11.075	-0.0	0.989
Dummy for benefits	.08575	.08356	0.9	0.721
Dummy for benefits $y - 1$	.08185	.08268	-0.3	0.891
Dummy for benefits $y - 2$	.08526	.08499	0.1	0.965
Dummy for employment	.82144	.81228	2.7	0.283
Dummy for employment $y - 1$	.80487	.79591	2.5	0.310
Dummy for employment $y - 2$	.78904	.78063	2.3	0.354
Dummy for positive earnings	.81267	.80361	2.7	0.297
Dummy for positive earnings $y - 1$	.79781	.78814	2.7	0.280
Dummy for positive earnings $y - 2$	.78051	.77079	2.6	0.291
Yearly labor earnings # Yearly labor earnings $y - 1$	6.7e+08	6.6e+08	0.7	0.783
Yearly labor earnings # Yearly labor earnings $y - 2$	6.3e+08	6.2e+08	0.5	0.838
Year days employed # Year days employed $y - 1$	79755	78236	2.8	0.239
Year days employed # Year days employed $y - 2$	74543	73352	2.2	0.359

(continued)

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

	Mean		%Bias (3)	p-value (4)
	Mover (1)	Non-Mover (2)		
Yearly labor earnings <sup>2</sup>	7.7e+08	7.5e+08	0.9	0.737
Yearly labor earnings <sup>2</sup> $y - 1$	7.1e+08	7.0e+08	0.7	0.770
Yearly labor earnings <sup>2</sup> $y - 2$	7.1e+08	7.0e+08	0.7	0.767
<i>Household characteristics</i>				
Dummy for at least one child	.40024	.39043	2.0	0.363
Dummy for event in 2009	.23167	.23179	-0.0	0.990
Dummy for event in 2010	.17613	.17727	-0.3	0.892
Dummy for event in 2011	.15225	.15946	-2.0	0.368
Dummy for event in 2012	.10816	.10675	0.5	0.836
<i>Regional characteristics</i>				
Unemployment rate	9.4913	9.4675	0.0	0.995
Unemployment rate $y - 1$	9.4913	9.4675	0.6	0.804
Unemployment rate $y - 2$	10.427	10.408	0.4	0.855
Unemployment rate # Unemployment rate $y - 1$	97.073	96.722	0.4	0.853
Unemployment rate # Unemployment rate $y - 2$	105.3	105.2	0.1	0.964

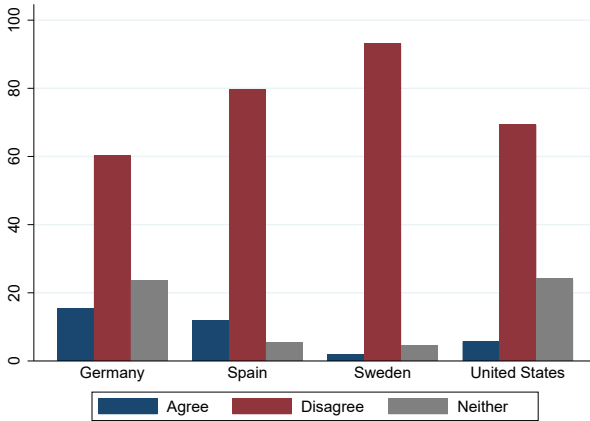
*Note:* All covariates are measured in reference year  $y$  if not stated differently. Education 1 refers to no/ no recognised education, education 2 to in-company/ school-based training, education 3 to university/ university of applied sciences, missings as reference category. Tenure 1 refers to less than 1 year tenure at the current job, tenure 2 to 1 to less than 2 years, tenure 3 to 2 to less than 3 years, tenure 4 to more than 3 years, missings as reference category. Years in region 1 refers to 4 to less than 8 years, years in region 2 to 8 to less than 12 years and years in region 3 to more than 12 years, less than 4 years as reference category. Dummy for event in 2008 as reference category.



## C Additional Figures

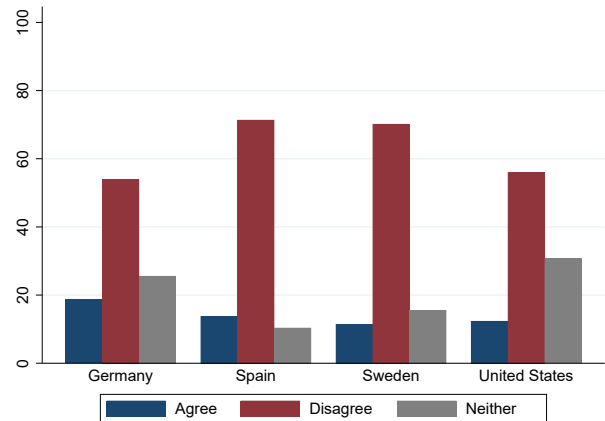
Panel A.

*When jobs are scarce, men should have more right to a job than women.*



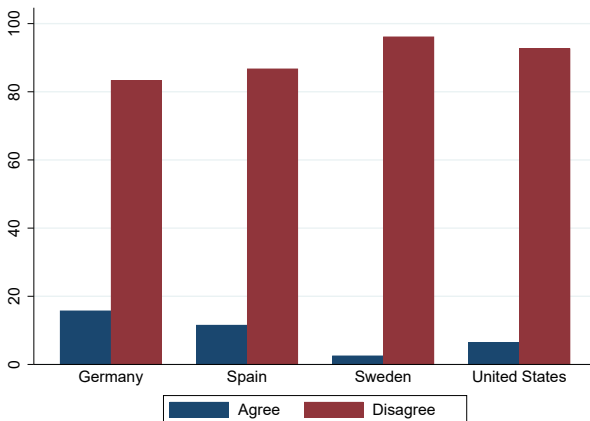
Panel B.

*If a woman earns more money than her husband, it's almost certain to cause problems.*



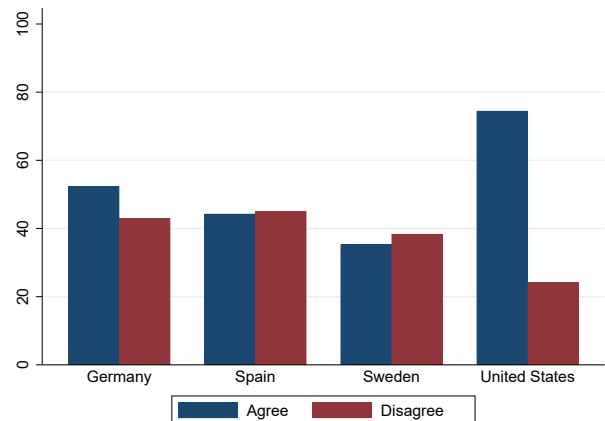
Panel C.

*A university education is more important for a boy than for a girl.*



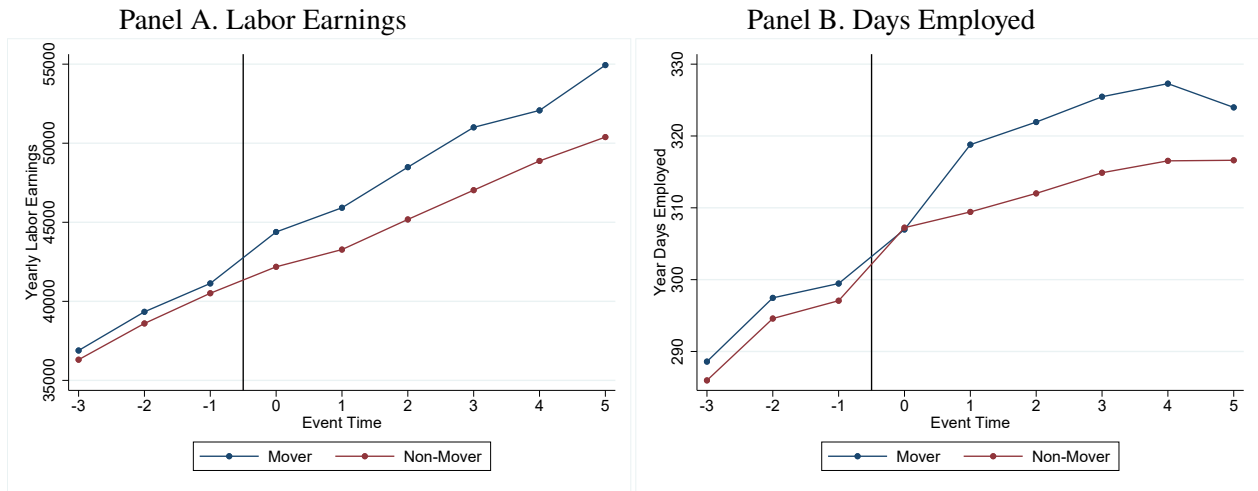
Panel D.

*Being a housewife is just as fulfilling as working for pay.*



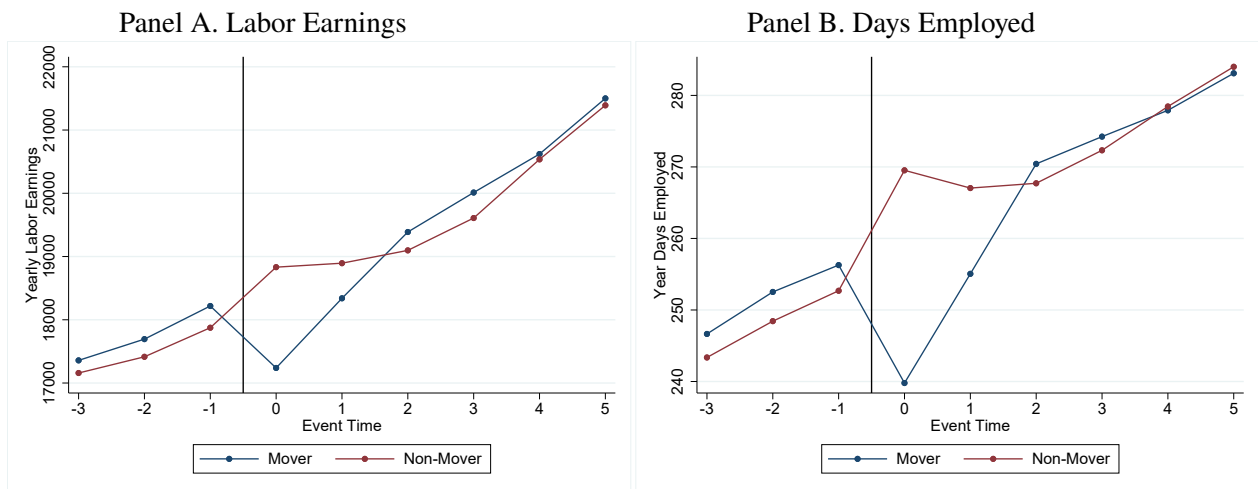
**Figure 15: Gender Identity Norms in Selected Countries**

*Note:* The figure is based on data from the World Value Survey in 2013 (Germany) and 2011 (Spain, Sweden and United States), respectively. Each panel shows percentage shares of respondents, both men and women. In Panel C and D the categories (1) strongly agree and (2) agree and (3) strongly disagree and (4) disagree are aggregated. The total number of observation is 6,673. See: <http://www.worldvaluessurvey.org/WVSDocumentationWV6.jsp>



**Figure 16: Means Individual Labor Earnings and Employment (Men)**

*Note:* To the left, panel A displays the mean yearly labor earnings for men. To the right, panel B displays the mean days employed per year.



**Figure 17: Means Individual Labor Earnings and Employment (Women)**

*Note:* To the left, panel A displays the mean yearly labor earnings for women. To the right, panel B displays the mean days employed per year.