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Working Paper

Safety net or helping hand? The effect of job search assistance and compensation on displaced workers

IWH Discussion Papers, No. 18/2023

Provided in Cooperation with:

Halle Institute for Economic Research (IWH) – Member of the Leibniz Association

Suggested Citation: Fackler, Daniel; Stegmaier, Jens; Upward, Richard (2023) : Safety net or helping hand? The effect of job search assistance and compensation on displaced workers, IWH Discussion Papers, No. 18/2023, Halle Institute for Economic Research (IWH), Halle (Saale), <https://nbn-resolving.de/urn:nbn:de:gbv:3:2-988488>

This Version is available at:

<https://hdl.handle.net/10419/276237>

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Safety Net or Helping Hand? The Effect of Job Search Assistance and Compensation on Displaced Workers

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ISSN 2194-2188

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IWH Discussion Papers are indexed in RePEc-EconPapers and in ECONIS.

† Daniel Fackler died, far too young, during the completion of this project. We have lost a calm but outstanding scientist and friend.

Safety Net or Helping Hand? The Effect of Job Search Assistance and Compensation on Displaced Workers*

Abstract

We provide the first systematic evidence on the effectiveness of a contested policy in Germany to help displaced workers. So-called “transfer companies” (*Transfergesellschaften*) employ displaced workers for a fixed period, during which time workers are provided with job-search assistance and are paid a wage which is a substantial fraction of their pre-displacement wage. Using rich and accurate data on workers’ employment patterns before and after displacement, we compare the earnings and employment outcomes of displaced workers who entered transfer companies with those that did not. Workers can choose whether or not to accept a position in a transfer company, and therefore we use the availability of a transfer company at the establishment level as an IV in a model of one-sided compliance. Using an event study, we find that workers who enter a transfer company have significantly worse post-displacement outcomes, but we show that this is likely to be the result of negative selection: workers who lack good outside opportunities are more likely to choose to enter the transfer company. In contrast, ITT and IV estimates indicate that the use of a transfer company has a positive and significant effect on employment rates five years after job loss, but no significant effect on earnings. In addition, the transfer company provides significant additional compensation to displaced workers in the first 12 months after job loss.

Keywords: earnings, employment, job loss, transfer companies

JEL classification: J63, J65, J68

* Upward thanks Lutz Bellmann for facilitating visits to the IAB to access data. We would like to thank Jens Härpfer for providing the information on the transfer company data and Bernd Fitzenberger for helpful conversations. We thank seminar participants at Universität Konstanz, Prague University of Economics and Business and participants at [SASE 2019](#) and [COPE 2019](#). Any remaining errors are our own.

1 Introduction

For economies to grow, reallocation of jobs is essential (e.g. Asturias et al., 2023; Haltiwanger et al., 2014). But job creation and job destruction places a heavy cost on a small group of displaced workers. These workers suffer large wage losses, on average, and also extended spells of non-employment.¹ However, while there is a large literature on the impact of active labour market programmes (ALMPs) (see Card et al. (2018) for a detailed overview), this literature usually examines the impact of such policies on unemployed workers rather than on displaced workers. Since displaced workers can be expected to differ from other job losers,² it is important to know how ALMPs affect displaced workers.

Against this background we evaluate a policy which is specifically designed to protect workers who have been laid off, which has been used in Germany since at least the early 1990s. So-called “transfer companies” (*Transfergesellschaft*) are created by employers who have a masslayoff event. The transfer company employs displaced workers for a fixed period after job loss, during which time they are provided with training and job-search assistance, and are paid a wage which is a large fraction (often up to 90%) of their pre-displacement wage. Workers employed by a transfer company can effectively delay, by up to 12 months, the point at which they become formally unemployed and start claiming time-limited unemployment benefits. The value of this policy has been contested for many years. Despite the fact that transfer companies have been operating in Germany for 30 years, no formal systematic evaluation of their effectiveness is currently available.³ In addition, stakeholders in the policy (employer and worker organisations) disagree about their benefits.

In Figure 1 we plot the number of workers and establishments which use transfer companies in Germany since 2007. The stock of workers employed by transfer companies fluctuated between about 10,000 and 35,000 with large increases shortly after the 2008 global financial crisis and the COVID pandemic.

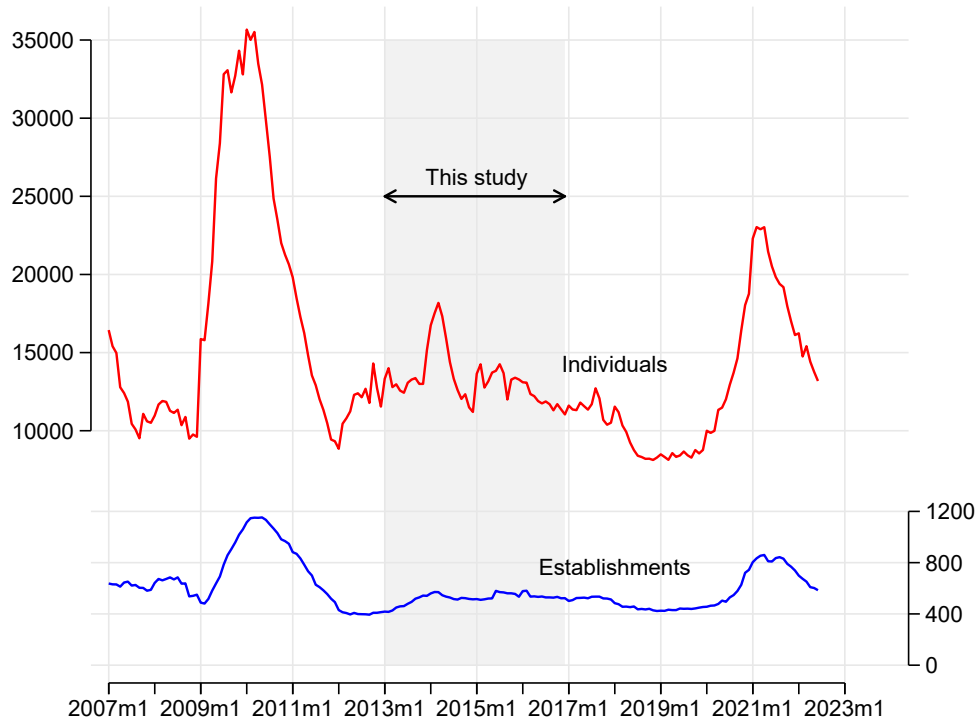
The effect of transfer companies on displaced workers will depend on three features of the system. First, workers who are employed by a transfer company receive a large increase in the potential duration of unemployment insurance (UI), because they are entitled to claim unemployment benefit after their spell in the transfer company ends. Sec-

¹See e.g. Jacobson et al. (1993) and numerous subsequent papers for the US and Schmieder et al. (2022) or Fackler et al. (2021) for recent evidence for Germany.

²Jacobson et al. (2005c, p 47) note that “displaced workers differ from other job losers, in that temporary earnings losses associated with unemployment constitute only a small portion of the income losses associated with their layoffs.”

³We discuss evaluations of assistance for displaced workers in more detail in Section 2.

Figure 1. Number of individuals and establishments using transfer companies



Source: Statistik der Bundesagentur für Arbeit. Angezeigte und realisierte Kurzarbeit, Nürnberg, Februar 2023.

ond, workers employed by a transfer company typically receive higher levels of benefit for the period of the transfer company. Third, workers employed by a transfer company receive job search assistance. It is an important empirical question (a) whether the additional payments received can buffer the wage losses from displacement; (b) whether the job search assistance and retraining provide benefits in terms of post-displacement employment outcomes. We thus consider whether transfer companies have a role as a safety net or as an active helping hand.

We use data on every spell in a transfer company in Germany over a four year period (January 2013 to December 2016) and combine them with administrative records of workers' earnings and employment status for several years before and after the job loss event in order to evaluate the effectiveness of this policy. A comparison of displaced workers who join a transfer company with displaced workers who do not join a transfer company is unlikely to provide a causal estimate: workers whose employers set up a transfer company can choose whether or not to join, and therefore workers with better outside options will be less likely to use transfer companies, which would invalidate any worker-level comparison of outcomes. We therefore utilise the fact that the availability

of a transfer company is dependent on an agreement at the level of the workplace. If this agreement is exogenous with respect to individual workers' outside opportunities, we can compare workers whose employers make a transfer company available with workers for whom a transfer company is not available. This intention to treat can be used as a highly relevant instrument in the individual outcome equation.

However, the decision to set up a transfer company by an employer is unlikely to be random with respect to the potential labour market outcomes of the workers who separate from their employer during a masslayoff event. The decision depends on negotiations between the employer and workers' representatives (Works Councils), and the outcome of these negotiations seem likely to depend on the costs and benefits of the scheme to the employer. A key factor is the extent to which the transfer company allows the employer to avoid (a) lengthy periods of notice and (b) redundancy payments, by contributing instead payments to the transfer company which are subsidised by the Federal Employment Agency. For example, employers whose workers have better outside options may be less likely to set up a transfer company.

To deal with this selection issue we draw on the population of German establishments who experience similar mass-separation events and, from these, select those whose layoff costs are likely to be similar, by creating inverse probability weights as a function of worker characteristics and the dynamics of establishment employment patterns. These weights effectively balance the characteristics of treated and controls (and their establishments) in the pre-displacement period, and make it more plausible that the decision to set up a transfer company is independent of potential labour market outcomes.

We show that transfer companies are set up by establishments during a mass-separation event which typically reduces employment by about 15% within one month. More than half of all workers who separate from these establishments in the month of the mass-separation event join a transfer company. We then compare establishments which set up a transfer company with those who do not, but which also experience a sudden mass-separation event. Transfer companies are set up by establishments which are much larger, concentrated in manufacturing industries, have a higher proportion of men and a much older and more experienced workforce. As expected, we find that separators who join a transfer company have significantly worse outcomes than separators who could have joined a transfer company but chose not to and separators who could not enter a transfer company. In contrast, ITT estimates indicate that the availability of a transfer company has a small positive effects on employment and labour market attachment and no effect on earnings up to five years after separation. In contrast with the recent

findings on the effect of UI extensions (Schmieder et al., 2016, for example), our results suggest that the training and job search advice which transfer companies offer may compensate for any negative effect from extended benefit duration. Furthermore, since transfer companies also provide workers with substantially improved post-displacement earnings, this institution provides a meaningful safety-net for displaced workers. Transfer company payments are significantly more generous, reducing wage loss by as much as one-third for the first 12 months after job loss.

The structure of the paper is as follows. In Section 2 we describe the existing literature which evaluates the impact of additional compensation and assistance for displaced workers, as well as considering more generally the effect on job search outcomes. In Section 3 we explain the particular institutional context of transfer companies in Germany. In Section 4 formalises the selection issues we face and explains our empirical methodology. Section 5 describes our data and explains how we construct comparable treatment and control groups. Section 6 presents our results and Section 7 concludes.

2 Literature

Existing evidence on transfer companies is mainly restricted to case studies. It is reported that workers who participate in transfer companies often experience wage losses but are at least “partially satisfied” with their new job (e.g. Mühge et al., 2012; Mühge, 2016). Based on that, Mühge et al. (2012, p 40) state that this institution is a helpful labour market policy. On the other hand, Schneider et al. (2007) argue that transfer companies do not enhance the employment prospects of the participants compared to the services provided by the Federal Employment Agency. At the same time, however, they also note that an observation period of at least four years as well as administrative data would be necessary for a conclusive evaluation. Härpfer (2014) compares participants of transfer companies with unemployed workers who received other measures of labour market integration and comes to a similar conclusion. Overall, it is difficult to draw general conclusions from these studies as they typically analyse a small number of transfer companies, have relatively short observation periods and struggle to find a suitable control group, or do not use a control group at all. We can overcome these limitations with our study.

The use of a transfer company entitles participants to a substantial increase in the potential duration of unemployment benefit and also typically involves more generous compensation for the period of the transfer company itself. Participants also receive job-search assistance and retraining. Our paper therefore also relates closely to the broader

literature which considers the effect of increased UI duration and generosity on exit rates from unemployment and post-unemployment wages; and the effect of job search assistance and retraining programmes. However, as noted by OECD (2018, p 126), while there is a vast literature on each of these aspects, “this research rarely assesses the specific situation of displaced workers, as distinct from that of other participants of labour market programmes, making it difficult to draw firm conclusions about which general measures work best for this particular group and whether they also require special support measures.”

The conclusion of the literature on benefit generosity and duration is that benefits reduce the exit rate from unemployment (and therefore increases unemployment durations) and that post-unemployment wages are, if anything, reduced. A survey is provided by Tatsiramos and van Ours (2014). The balance of evidence does not support the finding of a positive effect on post-unemployment wages, although there are some papers which find a positive effect. The two main identification methods are (a) changes in policy which affect different groups or different geographic areas and (b) discontinuities in eligibility by age or job history. In contrast, our approach relies on a comparison of displaced workers who enter a transfer company and those who do not, using the availability of the policy at the establishment level as our identification method. With only a few exceptions — Rebollo-Sanz and Rodríguez-Planas (2020) and Fackler et al. (2019) for example — this literature does not relate specifically to displaced workers. Displaced workers are much more likely to qualify for UI (and for more generous UI) because they have previously been in regular employment. They are also more likely to be in receipt of severance pay.

The literature on job search assistance and retraining (ALMPs) is analysed by Card et al. (2018). Here, we focus only on the small subset of that literature which considers displaced workers specifically.⁴ There were a number of evaluations of displaced worker programmes in the US in the 1980s. These programmes tended to be a mixture of job search assistance and retraining, and some may also have included extensions to unemployment benefits. Bloom (1990) is a study of a randomised experimental evaluation of a re-employment programme for displaced workers in Texas in the 1980s. The programme consisted of job-search assistance and retraining, and was found to have positive effects, with significantly larger effects for women. Results of a range of randomised trials are summarised in Leigh (1994). His conclusion was that job search assistance speeded up the reemployment of displaced workers. These results are supported by Dar and Gill (1998), who summarised the effectiveness of retraining programmes for

⁴There is also a literature on wage insurance for displaced workers (e.g. Hyman, 2018), but this is not a feature of the transfer company system.

eleven OECD countries. They conclude that “retraining programmes are generally no more effective than job search assistance in increasing reemployment probabilities or post-intervention earnings.” Kodrzycki (1997) argues similarly. More recent evidence on the effectiveness of retraining comes from Heinrich et al. (2008), who evaluate the Adult and Dislocated Worker Programs in the US. Again, these programs provided a mixture of job search assistance and retraining. The conclusion from this US literature seems to be that job search assistance is quite effective in helping displaced workers, but additional training less so. However, Jacobson et al. (2005a,2005b) have looked at the efficacy of community college for displaced workers in the US. They find that enrolment in Community College after job loss provides quite high rates of return.

In Europe, van den Berge (2016) considers the joint effect of severance pay and job search assistance on unemployment duration and job quality in the Netherlands. The effects are identified by comparing workers who were displaced because of bankruptcy (who do not get severance pay) and workers displaced with a “social compensation plan”. van den Berge notes that there are various selection problems in evaluating the performance of post-displacement outcomes. For example, displaced workers who enter unemployment (and who therefore might receive job search assistance) are a selected sample from all displaced workers. The identifying assumption is quite similar to that used in this paper: that workers do not select into a type of firm (in this case whether the firm is bankrupt or not). The main difference is that van den Berge considers the joint effect of severance pay and job search assistance which results from the formation of a social plan. Andersson (2018) investigates the effects of the individual counselling and job search assistance provided through the Employment Security Agreement Swedish blue-collar workers that were displaced during a mass layoff. The findings suggest that there are no effects on the unemployment probability, unemployment duration or income while there are positive effects for the duration of the subsequent job. Winter-Ebmer (2006) evaluates a retraining programme for workers displaced from the steel industry in Austria, and finds that participants had considerable wage gains (5%) and improved employment.

Transfer companies may also be seen as a type of extended “advanced notice” to the extent that participants remain employed during their spell in a transfer company. Addison and Portugal (1987), Ruhm (1994) and Friesen (1997) consider the effect of advance notice on subsequent outcomes for displaced workers. Ruhm (1994) finds that workers who receive an advance notice earn 10% more three to five years after job loss, but this effect may be explained by the fact the workers who receive advance notice also receive other kinds of assistance from their employer. Friesen (1997) finds that workers who are covered by advance notice regulation find work more quickly than those that

do not.

In the light of this literature, we remain agnostic as to the likely effects of transfer companies on subsequent earnings and employment. The evidence that more generous benefits damage job finding rates and post-unemployment wages may be balanced by the extent to which transfer companies offer meaningful job search assistance and human capital formation.

3 Institutional detail

A transfer company is a temporary organisation established by the employer to provide support for displaced workers. Worker dismissals have to be “permanent and unavoidable”, which means that transfer companies are not used in cases of temporary or seasonal fluctuations in labour demand.⁵ The aim of a transfer company is to help displaced workers find new employment via job placement, advice and the provision of training. Transfer companies are typically set up after the negotiation of a ‘social plan’ between workers’ representatives and their employer.⁶

According to Social Code III (*Sozialgesetzbuch III* §111), the process of setting up a transfer company involves negotiations between workers and the employer and also consultation with the German Federal Employment Agency. If a transfer company is set up, it is typically run by a third party service provider. Under a transfer company, workers agree to end the employment relationship with their current employer and sign a new fixed-term contract with the service provider. At the same time, the employer, the workers and the service provider agree upon training and job placement measures to be offered to the workers while they are in the transfer company. The former employer has to bear the costs of these measures, but up to 50% of these costs are reimbursed by the Federal Employment Agency.

⁵Recalls are explicitly ruled out by law. It is forbidden to receive funding in case of recalling a worker not only within an establishment but also if she switches to another establishment within the same corporation.

⁶Works Constitution Act (*Betriebsverfassungsgesetz* §111) prescribes that works councils and managers have to engage in negotiations if the establishment is exposed to legally defined, substantial changes (*Betriebsänderung*) e.g. lasting reductions of operations, the closure of an establishment or important departments of an establishment, a mass layoff, or changes in the organisation. This process typically involves a social plan, an agreement to alleviate the effects for dismissed employees. Although works councils are not a legal prerequisite for setting up a transfer company, the existence and bargaining position of the works council is an important determinant, as such negotiations take time and require legal knowledge. Despite the strong position of works councils in this process, they cannot enforce transfer companies as the employer could potentially go to court which would lengthen the process in a prohibitive way. See Kania and Koch (2012) or Schelz and Shahatit (2013) for details on the institutional background.

Workers in a transfer company also receive compensation. These payments consist of a compensation scheme (*Transferkurzarbeitergeld*, paid by the Federal Employment Agency) which corresponds to unemployment benefits (*Arbeitslosengeld I*) and amounts to 60 percent (67 percent in case the worker has a child) of the workers last wage. In most cases the former employer also pays a markup so that workers receive between 80 and 90% of their former wage. Workers receive compensation for a maximum of 12 months, but the exact duration of a transfer company is determined during the negotiation process. In practice, most transfer companies have a duration between 6 and 12 months. Whilst employed in a transfer company, workers are not formally unemployed, but are obliged to register as a job seeker at the Federal Employment Agency.⁷ A worker can leave the transfer company at any point in time, typically because a new job has been obtained. At the end of the maximum duration of the transfer company each worker is still entitled to claim the unemployment benefit that was due at the end of the original employment contract. This implies that a spell in a transfer company is effectively a significant extension of unemployment benefit duration, without the disadvantage of having to formally register as unemployed.

All workers of the downsizing establishment are free to choose whether they want to enter the transfer company or not. If a worker chooses not to join the transfer company his employment relationship is terminated in accordance with statutory periods of notice.

Why do employers set up a transfer companies? Transfer companies have the advantage for employers that they can avoid the risk of redundancies due to operational reasons. The ‘voluntary’ transfer of employees to a transfer company avoids the difficult process to select workers to layoff according to social criteria and the associated risk of being sued. As workers move immediately into the transfer company, employers avoid also statutory periods of notice. Additionally, in some cases employees contribute to the financing of a transfer company by means of their severance pay. And as already noted above, part of the measures will be paid by the Federal Employment Agency. All in all, this leads to an increase in the volume of the social plan or saves the employer part of the costs of redundancies. As a rule of thumb, a one month reduction in employment duration can finance two months in a transfer company. Finally, the use of a transfer company may also help the employer to reduce the workforce without encountering opposition from works councils and public opinion. However, as the process for setting up a transfer company requires substantial knowledge and advice and usually has to be implemented under serious time pressure it is not the case that transfer companies are

⁷This status enables the Employment Agency to sanction the worker according to §107 and §98 (Social Code III).

set up automatically under certain conditions or in certain firms only.⁸

The effect of transfer companies on displaced workers post-displacement outcomes will depend on three features. First, displaced workers who enter a transfer company receive a significant increase in the potential duration of UI, because they are entitled to claim unemployment benefit after their spell in the transfer company ends. Second, displaced workers who enter a transfer company usually receive higher levels of benefit for the period of the transfer company. Third, displaced workers who enter a transfer company receive job search assistance and retraining.

4 Identifying the causal impact of transfer companies

We face two selection issues. First, workers in establishments which set up a transfer company are free to choose whether or not to join. Displaced workers with good outside options, such as a job offer, are unlikely to choose to enter a transfer company, and therefore we expect negative selection on future employment prospects in the group which enter a transfer company. These workers may have worse employment outcomes than a control group because they are a selected group of displaced workers who did not have good outside opportunities.

Second, establishments which are making layoffs have some choice as to whether to set up a transfer company or not. As noted in Section 3, the decision to set up a transfer company depends partly on the costs and benefits to the establishment of doing so, as well as on factors related to the outcome of negotiations between the establishment and the works council. It is less obvious what direction this will bias the estimated effect. The bias will depend on how the decision to set up a transfer company is related to the post-displacement performance of the displaced workers in that establishment. For example, an establishment which employs a large fraction of high-tenure workers may be more likely to set up a transfer company. If high-tenure workers experience worse post-displacement outcomes, this will bias our intention-to-treat estimates downwards.

The first selection issue is dealt with by the identifying assumption that individual workers do not choose whether the establishment they work for will set up a transfer company in the event of a displacement. Our view is that the decision to set up a transfer company is taken after the decision to downsize,⁹ and therefore individual workers are unlikely to select into an establishment because they think that a transfer company

⁸We thank Klaus Müller from the Federal Employment Agency for details on employers' decision-making.

⁹Or at least this decision is unknown to a worker who decides to join the establishment.

might be an option in the event of a downsizing. Therefore, from the point of view of an individual displaced worker, the fact that there is a transfer company available is exogenous and can be used as the intention-to-treat (ITT).

To formalise these ideas, our intention-to-treat is a comparison of outcomes between those who are displaced from an establishment which makes a transfer company available and those who are displaced from an establishment which does not. Separation takes place at $t = 0$. We observe post separation earnings at various points in time after separation, $t > 0$. A simple ITT estimate can be written as

$$y_{it} = \pi_0 + \sum_k \pi_k (D_{kt} \cdot TC_j) + \delta_t + \varepsilon_{it}, \quad (1)$$

where y_{it} is a measure of some labour market outcome for individual i at time t , D_{kt} is a dummy variable indicating that the observation is at time $t = k$ and TC_j is a dummy variable indicating that the establishment $j = J(i)$ of individual i at time $t = 0$ set up a transfer company. We refer to establishments with $TC_j = 1$ as *sending establishments*. If TC_j was as good as randomly assigned, then π_k would be the causal impact of having a transfer company available at the time of the job loss on outcomes at time $t = k$. These estimates can be scaled by the proportion of separators who actually enter a transfer company. The first stage regressions in this case would be

$$TC_i = \gamma_{k0} + \gamma_{k1} TC_j + \eta_{ki} \quad k = 1, \dots, K \quad (2)$$

where TC_i is a dummy variable indicating that individual i entered a transfer company after being laid off. In principle there is a separate first stage for each time period $t = k$, we have a balanced panel for up to four years after the layoff event, and so the first stage estimates γ_{k1} are equal. Displaced workers with $TC_j = 0$ are unable to enter a transfer company, so we have a situation with one-sided noncompliance (Angrist and Pischke, 2009, p.161). All those with $TC_j = 0$ must be compliers; those with $TC_j = 1$ and $TC_i = 1$ are also compliers and those with $TC_j = 1$ and $TC_i = 0$ are never-takers. Our IV estimates can therefore be interpreted as the average treatment effect on the treated. This also means that our first stage estimates will be highly significant.

Conditional on our assumption that TC_j is as good as randomly assigned, the extent to which the Wald ratios π_k/γ_{k1} represent the causal impact of entering the transfer company depends on whether the assignment of TC_j has effects on y_{it} other than through the individual's use of the transfer company.

However, the second selection issue implies that the probability that an establishment uses a transfer company is not independent of their workers' potential outcomes

after displacement. We therefore relax the assumption that TC_j is as good as randomly assigned and use a selection-on-observables approach to control for differences between establishments which use transfer companies and those that do not. We use inverse probability weighting to produce a like-for-like comparison of establishments which set up a transfer company and those that do not. We allow for a dynamic selection process up to the point at which displacement occurs so that treated and control establishments, once weighted, have observably similar characteristics at various points in time leading up to the mass layoff event. We use a rich set of observable characteristics, including their size in the three years before and two years after the layoff event; their industry and region; their skill and occupational composition; characteristics of their workforce such as their tenure, and so on. Ideally, balancing the sample on these characteristics ensures that the type of establishment a worker is in is independent of their workers' potential outcomes after displacement.

5 Data

We use two sources of data. First, data on every spell in a transfer company in Germany which started between January 2013 and December 2016. Because our data only record spells which start from January 2013 onwards, the stock builds up substantially for the first 12 months. From January 2014 onwards we have a record on every spell in a transfer company in Germany, and the total number of spells matches almost exactly the official statistics reported in Figure 1. Our data covers about 87,000 workers who experience a spell in a transfer company.

Second, we use data from the Integrated Employment Biographies (IEB) provided by the Institute for Employment Research (IAB). The IEB covers all individuals in Germany who have spells of employment subject to social security, spells of marginal part-time employment, spells of benefit receipt, spells registered as job-seeking at the German Federal Employment Agency, and spells in active labour market programs.¹⁰ The IEB data contains start and end dates of all spells in each of the labour market states described above. The transfer company spell data can be linked to the IEB data by a consistent worker identifier based on their social security number. The IEB data is used to measure employment outcomes for workers. The IEB data runs up to December 2020, which implies that we have at least four years of post-separation labour market information for all workers.

¹⁰The 2% random sample from the IEB is described in Antoni et al. (2016). Note that we use the full population.

The IEB is also used to create a monthly panel of all German establishments which have workers subject to social security.¹¹ We keep those establishments which have a minimum size threshold of 10 workers in at least one month from January 2007 onwards, and which have positive employment in at least one month from January 2013 onwards. The monthly panel contains just over 1 million establishments and 120m establishment-months. For each establishment-month we measure all permanent worker separations.¹² Using this panel we can identify establishments which used a transfer company and then identify establishments which experienced similar patterns of worker separations but which did not use a transfer company.

5.1 Constructing the treatment and comparison groups

Treated workers ($TC_i = 1$) are those who appear in the transfer company data, indicating that they had a spell in a transfer company. We match these workers to the IEB (biography) data, allowing us to measure their employment and earnings before and after the treatment. Of the 87,193 workers identified in the transfer company data, 87,042 can be located in the IEB data. From these, we select only those workers whose biography is consistent with being laid-off at the appropriate time. That is, we select those who have a permanent separation from a regular employment spell subject to social security¹³ between January 2013 and December 2016, which occurs less than one month before the transfer company spell start date.¹⁴ 70,700 of the transfer company participants (81%) have consistent biography records.

We identify 3,127 establishments from which these 70,700 treated workers separate shortly before their transfer company spell starts. These are the sending establishments defined in Section 4 by $TC_j = 1$. To construct comparison groups, we need to ensure that workers who separate from sending establishments (both those who join a transfer company and those that do not) have similar potential outcomes as those who separate from non-sending (control) establishments.¹⁵ Separators from sending establishments will comprise workers who are displaced and workers who leave continuing jobs.

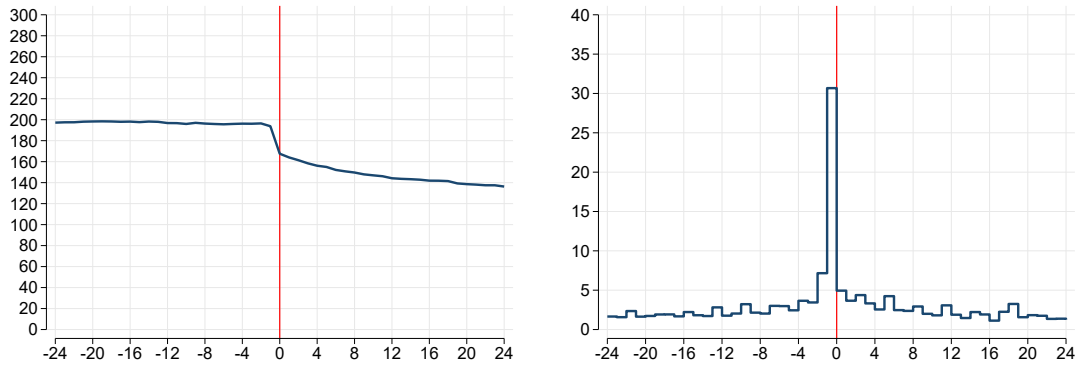
¹¹The definition of an establishment is provided by Ganzer et al. (2020).

¹²Separations which do not return to the same establishment.

¹³We therefore exclude separations from marginal part-time employment (so-called “mini-jobs”).

¹⁴For 90% of treated workers, the separation date is the day before the transfer company spell start date.

¹⁵Note that we are not comparing displaced and non-displaced workers, as is typical in the literature which followed from Jacobson et al. (1993). In fact, only a small fraction of sending establishments would be defined as an establishment which experiences a mass-layoff event Schmieder et al. (e.g. 2022) because the establishment does not meet the required size threshold or the required threshold for employment reduction.



(a) Average employment in sending establishments (b) Average number of separations from sending establishments

Figure 2. Employment and separations from sending establishments. In each panel, the x -axis indicates months relative to the start of the modal transfer company start date in that establishment. In panel (a), average employment includes establishments with zero employment. Separations in panel (b) count only those separations from jobs subject to social security where the individual does not return to the same establishment.

In order to construct a control group, we use the fact that sending establishments typically experience a very sudden and unusual increase in separations immediately before the transfer company is set up. This is illustrated in Figure 2. To draw this picture, we match our list of sending establishments to the monthly panel of establishments created from the IEB.¹⁶ In each panel, the vertical red line indicates the modal transfer company start date.¹⁷

Panel (a) of Figure 2 shows that employment is extremely stable in the 24 months leading up to the transfer company start date. Employment falls sharply by 15% on average immediately before the transfer company starts. Over the next 24 months employment declines by a further 15%. Panel (c) shows that the fall in employment coincides with a sudden increase in separations. There is a small increase in separations two months before the start of the transfer company, but it is clear that the great majority of separations occur immediately before the transfer company starts.

We therefore limit our analysis to workers who separate from their establishment in a month in which their establishment experiences a large and sudden increase in separations. The number of separations from establishment j in month t , S_{jt} should be

¹⁶92% (2,887) of the sending establishments appear in the monthly panel. A small number of establishments do not appear because they do not meet a minimum size threshold.

¹⁷Most transfer company spells occur at the same date for each establishment. In other words, the setting up of a transfer company can be thought of as a single event for a sending establishment. In those cases where there are multiple transfer company start dates from the same sending establishment we use the modal start date.

more than R times greater than the average number of separations in the previous 12 months, \bar{S}_j .¹⁸ Our rule is therefore that a mass-separation event occurs if

$$\frac{S_{jt}}{\max(1, \bar{S}_j)} > R. \quad (3)$$

This implies a minimum size threshold of at least R , albeit one which is much smaller than typically used in the displacement literature.¹⁹ In our baseline model we use a threshold of $R = 5$. In a robustness exercise in Appendix B we also consider $R = 10$ and $R = 20$. When measuring separations, we consider only permanent separations from jobs subject to social security. Our window for mass-separation events is restricted to the period January 2013-December 2016. The start of the window is determined by the availability of transfer company information. The end of the window is determined by the fact that we wish to follow separators for up to five years after the separation event. From the approximately 1 million establishments in the monthly panel, we identify about 100,000 who have a mass-separation event during this four year window. Of the 3,127 sending establishments, just under 1,300 have a mass-separation event.

We then select the biographies of all workers who permanently separate from these mass-separation establishments in the month of the mass-separation event. From these biographies we identify the three groups of workers defined in Section 4. There are 40,189 separators from sending establishments who enter a transfer company ($TC_i = 1$, $TC_j = 1$), who we label TC1 separators. There are 29,976 separators from sending establishments who do not enter a transfer company ($TC_i = 0$, $TC_j = 1$), who we label TC2 separators. There are over 2.3m separators from control establishments ($TC_j = 0$) who we label C separators.²⁰ Our analysis sample of TC1 separators is smaller than the full sample of 70,700 described earlier because we require that separations occur in the month of a mass-separation event.²¹

¹⁸For establishments with less than one separation per month in the previous 12 months, we require a minimum of $R + 1$ separations in the mass-separation month. This ensures that very small establishments with minimal turnover are excluded.

¹⁹An establishment must have more than R employees in order to reach the threshold.

²⁰It is possible for a worker to have multiple separations during a mass-separation event. In these cases we take the first separation.

²¹In Appendix A we compare the employment patterns of the TC1 separators who are in our analysis sample (those who separate in the month of a mass-separation event) and those who are not in our analysis sample (those who do not separate during a mass-separation event). We find that employment patterns are almost identical before and after the separation, while earnings are lower in the analysis sample before and after the separation.

5.2 Outcomes

To evaluate the effectiveness of transfer companies, we consider a range of post-displacement outcomes all measured at 1–5 years after the displacement date. The IEB biography data we use includes spells up to the end of 2020, and our window for displacement events runs from January 2013 to December 2016. We therefore observe post-displacement outcomes up to 4 years after the displacement date for our entire sample, and outcomes 5 years after the displacement date for the sample whose displacement event occurs before 2016.

First, we use daily earnings from all employment spells in progress at each point in time. Individuals not in employment are assumed to have zero earnings. This means that individuals who subsequently enter employment which is not covered by social security (self-employment, civil service) will have zero earnings.²² Earnings are censored at the social security threshold, so we follow Dustmann et al. (2009) (and others) and impute earnings above the threshold using the method described in Dauth and Eppelsheimer (2020). The displaced workers in our sample are not particularly highly-paid, and because we are comparing displaced workers to other displaced workers, the results with or without imputation are quite similar.²³

Second, we use binary measures of employment status to measure the probabilities of employment, unemployment and non-employment. As before, the employment indicator measures any employment spell covered by the social security data. The unemployment indicator indicates that someone appears in the social security statistics but does not have a job and is not in a transfer company. The non-employment indicator measures the absence from the social security statistics at that point.

6 Results

6.1 A comparison of sending and control establishments

In Figure 3 we plot the employment behaviour of the selected sending and control establishments which experience a mass-separation event. Panels (a) and (b) plot a count of the number of establishments with positive employment in each month before and after the event. Our sample selection process ensures that each establishment is observed in

²²Individuals who are in a TU are assumed to have zero earnings. This means that individuals who enter a TU and who also have a mini-job have their earnings underestimated.

²³This is one of the robustness checks we carry out in Appendix B.

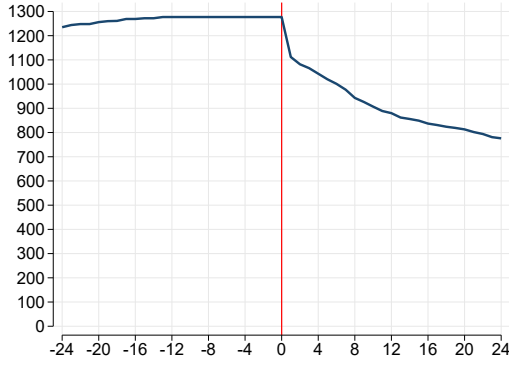
the data for at least 12 months leading up to the mass-separation event, and so establishment numbers are completely stable leading up to the event. Unsurprisingly there is a sudden fall in establishment numbers at the time of the event. A larger proportion of control establishments exit immediately, but sending establishments exit at a faster rate over the next two years. Panels (c) and (d) plot average employment, including zero employment in those establishments which have exited. Sending establishments are more than three times larger than control establishments before the mass-separation event. Sending establishments' employment falls by about 20%; control establishments' employment falls by about 30%. Sending establishments continue to experience a decline in employment after the event, but comparison with panel (a) suggests that this is more than accounted for by establishment exit, which implies that employment in surviving establishments is increasing. Sending and control establishments also experience different trends in employment growth before the mass-separation event, with control establishments growing by about 12% in the two years leading up to the mass-separation event.²⁴ In panels (e) and (f) we see that separations are concentrated almost entirely in the month of the mass-separation event, but sending establishments continue to experience more separations after this point.

These differences between sending and control establishments tell us that we cannot assume that TC_j is random with respect to separators' potential outcomes. Sending establishments are much larger and experience larger separation events (in absolute terms). Most importantly, we cannot assume that separators from sending and control establishments are equally likely to be displaced. While TC1 separators are by definition displaced, TC2 and C separators may include those who separate from a continuing job or who move to another establishment in the same firm. Further, because separations are identified by changes in establishment identifier within a worker's biography, changes in establishment identifier may create spurious separations.²⁵ We can shed some light on the extent to which separators are likely to be displaced by the extent to which they move together to the same post-separation establishment, called "clustered outflows". Separators who move together are more likely to be the result of within-firm mobility, or of separations which are not the result of job loss. For each separator from an establishment j in month t , we record the establishment identifier j' in their biography 90 days after the separation.²⁶ We count the number of workers in the same establishment

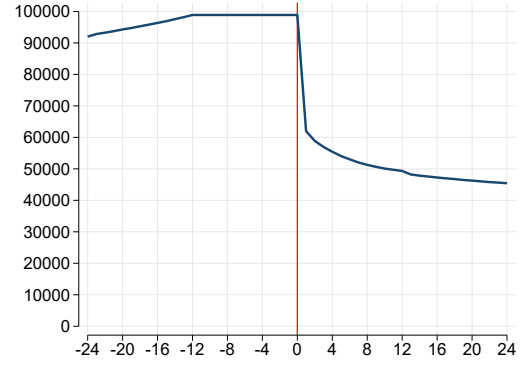
²⁴Fackler et al. (2018) also show that establishments grew in the years preceding the mass-separation event.

²⁵This problem is well-recognised in the literature. For example, Hethey and Schmieder (2010) note that establishment identifiers may change due changes in ownership or legal form and because of mergers and acquisitions.

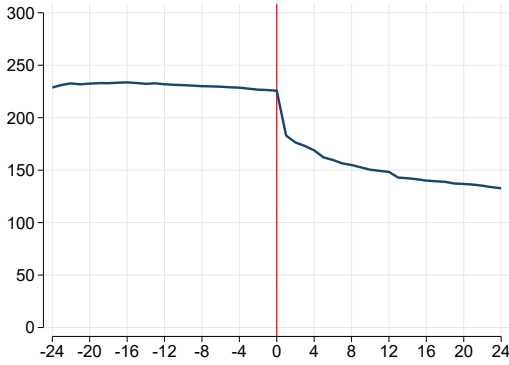
²⁶90 days is an arbitrary point. We find that workers' biographies are sometimes quite noisy immediately after a separation and may include additional short-lived employment spells. In a robustness exercise



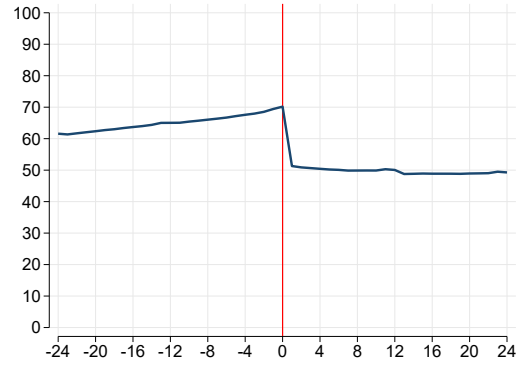
(a) Number of sending establishments with positive employment



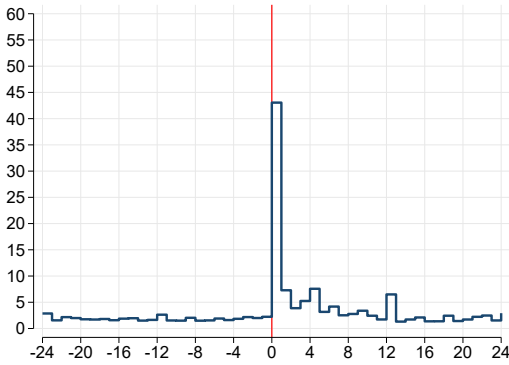
(b) Number of control establishments with positive employment



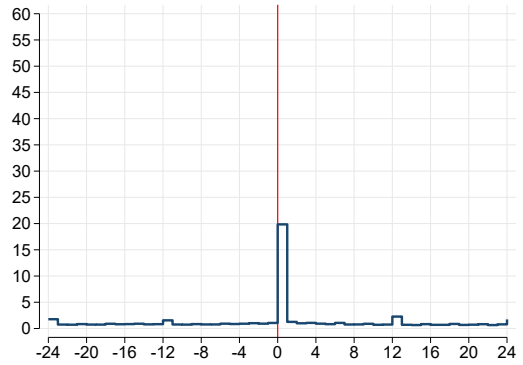
(c) Average employment in sending establishments



(d) Average employment in control establishments



(e) Average number of separations from sending establishments



(f) Average number of separations from control establishments

Figure 3. Employment and separations in sending and control establishments. In each panel, the x -axis indicates days relative to the month in which the mass-separation event occurs. In panels (c) and (d), average employment includes establishments with zero employment. Separations in panel (e)–(f) count only those separations where the individual does not return to the same establishment.

j at t who join the same next establishment j' . We then divide this count by the total number of separators in that establishment.²⁷

Figure 4 shows the distribution of our clustered outflow measure for separators from sending and control establishments. In panel (a), a large fraction of separators from sending establishments do not move in a cluster because these separators are dominated by TC1 separators, who by definition have a clustering measure of zero. However, even in sending establishments — which have a verified mass-layoff event — some fraction of workers move in a cluster. This is clear in panel (c), which shows the distribution of clustered outflows for TC2 separators. This indicates that some separators who leave an establishment at the time of a mass-layoff event may move to another establishment in the same firm, or that the mass-layoff event occurs in conjunction with a change in establishment id for some fraction of the workforce. In panel (b) we see that separators from control establishments — which do not have a verified mass-layoff event — also often move together to the same establishment. We do not want to exclude these separators from our analysis. Instead, we wish to create a comparison group from the $TC_j = 0$ separators whose characteristics are as similar as possible to the $TC_j = 1$ separators to ensure that they have similar potential outcomes.

6.2 A comparison of treated and untreated workers

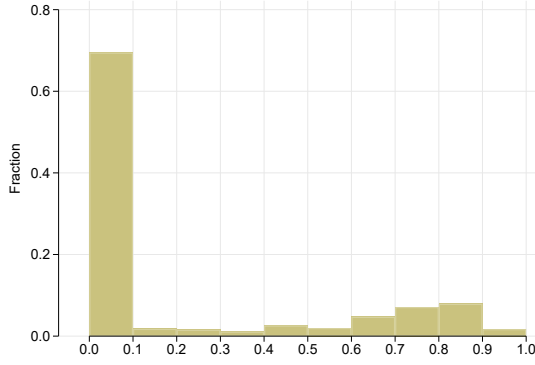
In Figure 5 we plot the raw means of two key outcome variables for three years before and five years after the separation. Panel (a) shows that daily earnings are much higher for separators from sending establishments. Separators who join a transfer company (TC1) have very slightly higher earnings than separators who choose not to (TC2). Because all TC1 separators join a transfer company (and we are not including transfer company payments in their earnings), their daily earnings fall to almost zero immediately after the separation.²⁸ In panel (b), TC1 separators' employment rate falls to below 0.2 1 month after the separation.²⁹ In contrast, TC2 separators experience much smaller falls in earnings, reflecting the fact that after 1 month nearly 90% are in em-

in Appendix B we also consider patterns of clustering after 180 days.

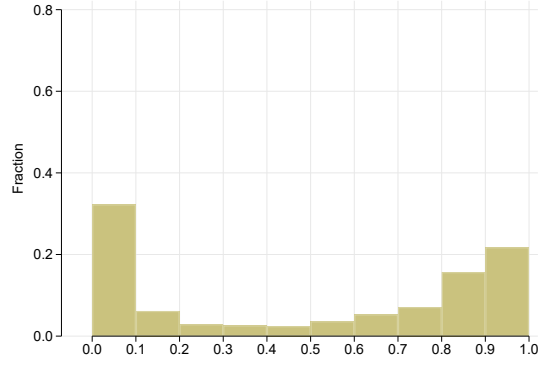
²⁷As an example, consider an establishment $j = 1$ which has 3 separators. Separators $i = 1, 2$ leave $j = 1$ and join establishment $j = 2$. Separator $i = 3$ leaves $j = 1$ and joins $j = 3$. Define $C(J(i), J'(i))$ to be the flow from $j = J(i)$ to $j' = J'(i)$. In this case $C(J(1), J'(1)) = C(J(2), J'(2)) = 2$, while $C(J(3), J'(3)) = 1$. Our measure of clustering is then $2/3$ for $i = 1, 2$ and $1/3$ for $i = 3$. Individuals who are not in employment (or in a transfer company) after 90 days are given a clustering measure of zero.

²⁸A small number have mini-jobs concurrent with their transfer company spell, and a small number spend less than 30 days in the transfer company, which is why earnings after 30 days for this group are not exactly equal to zero.

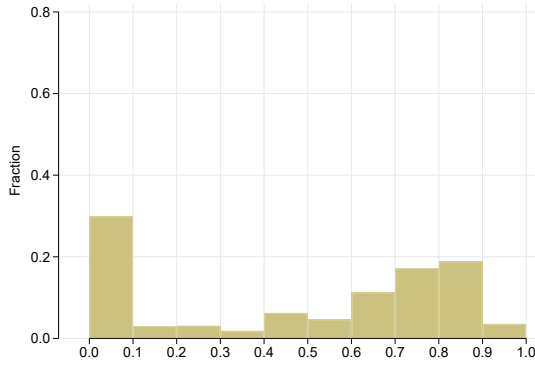
²⁹The sharp fall in earnings and employment rates of TC1 separators after 24 months is accounted for by a single large employer.



(a) Separators from sending establishments (TC1 and TC2)



(b) Separators from control establishments (C)



(c) Separators from sending establishments (TC2 only)

Figure 4. Clustered outflows of separators. For each separator i from establishment j , the clustered outflow is the proportion of all separators from j who move to the same new establishment j' within 90 days. Separators who move individually therefore have a cluster of $1/S_j$ where S_j is the total number of separators from j . Separators who do not move to another establishment within 90 days, and separators who join a transfer company are given a clustered outflow of zero. Even establishments which experience a verifiable mass-layoff event in panel (a) have some fraction of workers who move in clusters to new establishments. These are the TC2 workers in panel (c). Separators from control establishments in panel (b) are more likely to move in clusters, consistent with the hypothesis that a higher fraction are not displaced workers.

ployment, shown in panel (b). Separators from control establishments (C separators) also have small falls in earnings because they also have high employment rates within one month of separating. Note that the outcomes for C separators are effectively a weighted average of TC1 and TC2 separators. If a transfer company had been available to these separators, some fraction of them would have chosen to enter a transfer company. The patterns of employment and earnings for C and TC2 separators are quite different to those observed in “displaced workers” as conventionally defined in the literature because some fraction of these separators may be separating from continuing jobs or moving to another establishment in the same firm.³⁰

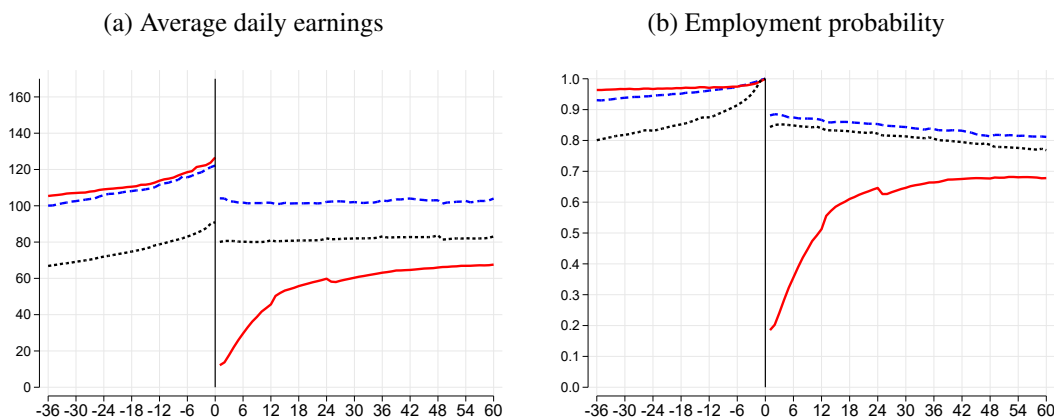


Figure 5. Earnings and employment outcomes before and after separation. In each panel, the x -axis indicates months relative to the date of separation, indicated by the vertical line. Spells of non-employment (including spells in transfer companies) are assigned daily earnings of zero. TC1 separators: —, TC2 separators: ---- and C separators:

These differences in earnings and employment between TC1, TC2 and C workers motivates our methodology. A comparison of TC1 and TC2 separators is very unlikely to be a causal estimate partly because TC2 workers have better outside options. We therefore use the availability of a transfer company as an intention-to-treat, where the reduced form is a comparison of all separators from sending establishments with all separators from control establishments. We use inverse probability weighting to ensure that observations in the two groups have observably similar characteristics.

6.3 Reweighting sending and control establishments

In Table 1 we report a comparison of means between displaced workers in treated (sending) and control (control) establishments before and after re-weighting by the inverse

³⁰See for example Schmieder et al. (2022, Figure 1).

probability of being in a sending establishment at the time of separation, as a function of observable characteristics. In a first step, we estimate the probability of being in a sending establishment using a Logit regression on the set of characteristics reported in Table 1. Observations which are off the common support are removed after the first step. In a second step the sample is reweighted by the inverse probability of being in a sending establishment.

Time-varying characteristics (such as employment status and earnings) are measured at three points in time before the separation event, to ensure that we account for different dynamics into selection. For the same reason, we also include a measure of whether each separator was registered for job search with the Federal Employment Agency at $t = 0$, $t = -6$ and $t = -12$. The set of characteristics also includes establishment employment one and two years *after* the displacement event. Typically, one would not include post-displacement characteristics because they themselves could be outcomes of the treatment. In this case however, these are outcomes of the establishments from which workers are separating, and we wish to compare individuals who were displaced in similar circumstances, and therefore we wish to balance on the size of the employment reduction between $t = 0$ and $t > 0$.

Columns (1) and (2) in Table 1 confirm that there are large and significant differences between workers from sending and control establishments. Separators from sending establishments are more likely to be in employment 6 and 12 months before the separation, less likely to be unemployed, less likely to be in a mini-job³¹ and have substantially higher earnings. Separators from control establishments are about four times more likely to be registered for job search 12 months before the separation date (0.101 vs. 0.028), but this reverses as the separation date approaches, to the extent that separators from sending establishments are four times more likely to be registered for job search at the separation date (0.666 vs. 0.159). This strongly suggests that separators from sending establishments are in more stable employment relationships which are unexpectedly ended by a mass-separation event, while separators in control establishments are more likely to be separating for other reasons. Separators from sending establishments worked for very large establishments compared to separators from control establishments, but separators from control establishments worked for establishments which experienced larger proportional falls in employment after 12 months. Separators from sending establishments have much longer tenure, are much more likely to be men and are older. Finally, note that separators from sending establishments move in smaller clusters, as shown in Figure 4. In other words, separators from control establishments

³¹Jobs with monthly earnings below a threshold which have reduced social security contributions and income tax.

	Unweighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
	Control	Sending	p -value	Control	Sending	p -value
	Mean	Mean		Mean	Mean	
	C	TU1 + TU2		C	TU1 + TU2	
in employment $t = 0$	1.000	1.000	[.]	1.000	1.000	[1.000]
in employment $t = -6$	0.931	0.975	[0.000]	0.977	0.975	[0.321]
in employment $t = -12$	0.896	0.967	[0.000]	0.968	0.967	[0.738]
unemployed $t = 0$	0.000	0.000	[.]	0.000	0.000	[.]
unemployed $t = -6$	0.030	0.004	[0.000]	0.004	0.004	[0.986]
unemployed $t = -12$	0.042	0.007	[0.000]	0.007	0.007	[0.775]
registered for job search $t = 0$	0.159	0.666	[0.000]	0.637	0.666	[0.353]
registered for job search $t = -6$	0.093	0.045	[0.000]	0.051	0.045	[0.624]
registered for job search $t = -12$	0.101	0.028	[0.000]	0.033	0.028	[0.543]
minijob $t = 0$	0.077	0.076	[0.805]	0.076	0.076	[0.956]
minijob $t = -6$	0.083	0.065	[0.000]	0.064	0.065	[0.755]
minijob $t = -12$	0.087	0.065	[0.000]	0.064	0.065	[0.809]
wages $t = 0$	93.994	125.876	[0.000]	127.387	125.872	[0.655]
wages $t = -6$	86.715	118.338	[0.000]	118.948	118.338	[0.834]
wages $t = -12$	82.567	113.896	[0.000]	114.271	113.896	[0.890]
employment $t = -24$	354.373	575.773	[0.129]	465.243	575.777	[0.453]
employment $t = -12$	367.475	560.484	[0.185]	450.126	560.488	[0.451]
employment $t = 0$	376.546	515.071	[0.303]	403.122	515.075	[0.404]
employment $t = +12$	167.368	157.080	[0.751]	128.740	157.087	[0.350]
employment $t = +24$	163.305	129.916	[0.274]	113.617	129.923	[0.560]
manufacturing	0.178	0.705	[0.000]	0.665	0.705	[0.297]
tenure	7.117	13.153	[0.000]	12.970	13.154	[0.832]
female	0.465	0.276	[0.000]	0.296	0.276	[0.290]
age	42.417	46.621	[0.000]	46.700	46.621	[0.806]
nongerman	0.112	0.094	[0.001]	0.090	0.094	[0.532]
high qualified worker	0.157	0.109	[0.000]	0.121	0.109	[0.288]
med qualified worker	0.760	0.808	[0.000]	0.794	0.808	[0.258]
low qualified worker	0.082	0.082	[0.982]	0.085	0.082	[0.746]
high skilled worker (Blossfeld)	0.238	0.165	[0.000]	0.172	0.165	[0.443]
med skilled worker (Blossfeld)	0.355	0.390	[0.061]	0.404	0.390	[0.526]
low skilled worker (Blossfeld)	0.407	0.445	[0.089]	0.424	0.445	[0.414]
cluster size 90	0.498	0.204	[0.000]	0.221	0.204	[0.421]
	2,008,005	63,954		1,790,337	63,951	

Table 1. Comparison of unweighted and weighted (balanced) samples. Table shows means of each variable and the p -value of the test that the means are equal across the two samples. Standard errors are clustered by the mass-separation establishment identifier. The unweighted samples (columns 1 and 2) comprise all workers who separate from control and sending establishments. The weighted samples (columns 4 and 5) are weighted by inverse-probability weights. Observations not on the common support are removed in columns 4 and 5. Additional variables included in the balancing procedure were a full set of 2-digit industry dummies and *Bundesland* dummies. Skills are groupings based on Blossfeld (1987, Table 1).

are more likely to move together to the same subsequent establishment.

In columns (4) and (5) we report the re-weighted means and the p -value of a comparison of those means.³² The re-weighting procedure removes any significant difference in pre-displacement values for all of these observable characteristics. For example, note that after re-weighting the time-path of job search registration of separators from sending establishments is almost identical to the time-path of separators from control establishments. Re-weighting also makes the separators in the two groups similar in terms of the pattern of clustering. After reweighting, a comparison of cluster size in columns (4) and (5) is insignificantly different from zero. Further evidence that reweighting achieves balance is shown in Figure 6, which shows that separators from sending and control establishments have very similar patterns of clustering.

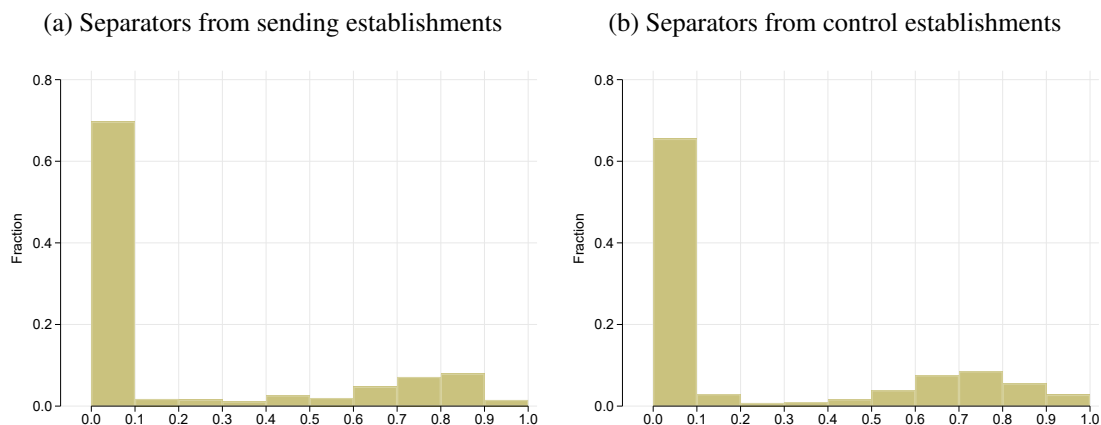


Figure 6. Clustered outflows of separators, weighted by inverse probability weights. After reweighting, the distribution of clustering is similar between the two groups of separators.

6.4 Earnings and employment effects of transfer companies

Using these reweighted samples we now estimate the earnings and employment effects of transfer companies on separators' subsequent earnings and employment. Although we have an extremely large sample of separators (approx 1.7m) the number of mass-layoff establishments is much smaller (approx 90,000). It seems likely that subsequent earnings and employment are correlated across separators from the same establishment, so throughout we cluster standard errors at the level of the masslayoff establishment.

In panel (a) of Table 2 we report estimates of the labour market earnings effect of participation in a transfer company. As in Figure 5, our definition of labour market

³²Standard errors are robust-clustered at the level of the mass-separation establishment. That is, the errors are allowed to be correlated for separators from the same establishment.

earnings is total earnings from any spells of employment, with earnings from all other spells set to zero.³³ Column (1) reports a simple comparison of earnings for workers who enter a transfer company (TC1) with all those who do not (TC2 and C). As was clear from Figure 5, TC1 workers have much lower earnings than the comparison group, and this gap persists for at least five years after the separation. This large negative effect represents the strong selection bias inherent in any comparison of TC1 workers with a control group, because the TC1 group will consist of workers who entered a transfer company because they had worse outside options e.g. those who could not find a subsequent job by the time of separation.

Column (2) reports estimates of π_k from Eqn (1). This is a comparison of average earnings, after job loss, between all workers who separate from sending establishments and all workers who separate from control establishments. If the process of reweighting has successfully removed the selection bias so that ε_i is uncorrelated with TC_j , these represent causal ITT effects. After 12 months, separators from sending establishments have earnings about 7% lower than separators from control establishments. At this point in time, a substantial fraction of TC1 workers are finishing their spell in a transfer company and are not yet in employment. After 24 months the earnings gap reduces to 3% and is insignificantly different from zero. The earnings gap continues to shrink and after five years is very close to zero. These ITT results suggest that the availability of a transfer company to workers at the time of separation does not significantly change their earnings five years later, and we can rule out any large gains or losses.

Estimates of γ_{k1} from the first stage models given in Equation (2) are reported below the ITT estimates. In this case, displaced workers from establishments who do not set up a transfer company are unable to enter a transfer company, and so we have a situation with one-sided noncompliance (Angrist and Pischke, 2009, p.161). Those with $TC_i = 1$ and $TC_j = 1$ are compliers and those with $TC_i = 0$ and $TC_j = 1$ are never-takers, which means that our IV estimates can be interpreted as the average treatment effect on the treated. This also means that our first stage estimates will be highly significant. In fact, the estimates of γ_{11} to γ_{41} are identical in this case because we have a balanced panel of observations up to four years after the displacement event.³⁴ The estimates of γ_{k1} tell us that 59% of displaced workers from sending establishments enter a transfer company. Column (3) reports 2SLS estimates which in this case are almost exactly equal to the

³³The model is estimated in levels and estimates are then expressed as a proportion of average earnings at the point of separation.

³⁴After five years, our sample is no longer balanced because individuals who are displaced in 2016 are not observed in 2021, but this makes little difference to the first stage estimates. Recall that the biography data runs to the end of 2020.

	(a) Labour market earnings			(b) Labour market earnings conditional on employment		
	OLS	ITT	2SLS	OLS	ITT	2SLS
$TC1_i$ 12 months	-0.444*** (0.040)		-0.125* (0.072)	-0.197*** (0.022)		-0.078 (0.067)
$TC1_i$ 24 months	-0.314*** (0.025)		-0.057 (0.057)	-0.210*** (0.020)		-0.100* (0.053)
$TC1_i$ 36 months	-0.292*** (0.028)		-0.055 (0.058)	-0.205*** (0.019)		-0.092* (0.050)
$TC1_i$ 48 months	-0.261*** (0.026)		-0.028 (0.055)	-0.202*** (0.019)		-0.082* (0.048)
$TC1_i$ 60 months	-0.239*** (0.029)		0.014 (0.057)	-0.187*** (0.019)		-0.061 (0.045)
$TC_{J(i)}$ 12 months		-0.074* (0.044)			-0.035 (0.031)	
$TC_{J(i)}$ 24 months		-0.034 (0.034)			-0.052* (0.029)	
$TC_{J(i)}$ 36 months		-0.032 (0.035)			-0.050* (0.027)	
$TC_{J(i)}$ 48 months		-0.016 (0.033)			-0.045* (0.027)	
$TC_{J(i)}$ 60 months		0.009 (0.035)			-0.034 (0.026)	
First-stage estimate		0.593*** (0.023)			0.450*** (0.022)	
Kleibergen-Paap Wald F-statistic		673.29			436.98	

Table 2. Estimated effect of transfer companies on earnings after job loss. All estimates weighted by inverse probability weights (see Section 4). In panel (a) the dependent variable is calculated as earnings from all spells of employment; earnings from other spells (such as UI payments or transfer company payments) are set to zero. In panel (b) all non-employment spells are excluded from the sample. Column (3) represents the results of the three first stage regressions (Eqn. 2). The model uses unlogged earnings with estimates expressed as a proportion of average earnings at the point of separation. Regressions in panel (a) have a sample size of 8,812,162 observations, 1,854,288 individuals and 88,688 masslayoff establishments. All regressions in panel (b) have a sample size of 7,215,462 observations, 1,727,062 individuals and 88,282 masslayoff establishments. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the level of establishment from which separation occurred.

ITT scaled by the first stage coefficients.³⁵ We therefore find no significant effect on earnings of participation in a transfer company 2 or more years after separation.

The earnings effects in panel (a) are the product of the probability of employment and earnings conditional on employment. In panel (b) we exclude all spells of non-employment from the regression sample to estimate the effect on earnings conditional on employment. The ITT and the 2SLS estimates are now slightly larger (more negative), implying that there will be a positive effect on employment probability. The 2SLS estimate is now -6% , but still insignificant at any conventional level. We can therefore rule out the hypothesis that participation in a transfer company leads to higher-paid employment.

In Table 3 we report estimates from linear probability models of the impact of transfer companies on the probability of being in employment, unemployment and out of the labour market. From column (1), we see that participants of transfer companies have significantly lower employment probabilities for up to five years after separation, although the effect is declining. As before, we believe that this reflects selection into transfer companies by those who have worse employment opportunities. This effect is reversed in column (2) when we use the availability of transfer companies as an instrument. After five years transfer company increases employment by 3 percentage points with a standard error of 1.2 percentage points. The resulting IV estimate is therefore slightly larger and implies that participation in a transfer company increases employment by 5 percentage points after five year.

This positive employment effect is not the only difference between transfer company participants and non-participants. In column (4) of Table 3 we see that transfer company participants have a lower probability of unemployment after 1 year because at this point the transfer company is just ending. Transfer company participants then experience significantly higher unemployment rates as they re-enter the labour market and this effect declines over the next five years. Column (5) and column (6) report the ITT and 2SLS estimates, which are precisely estimated zeros after five years. Since the employment effect is positive and the unemployment effect is zero, this implies that there must be a negative effect on the probability of disappearing from the social security data, shown in panel (c).

Our reduced form and 2SLS estimates imply that transfer companies offer a small helping hand to displaced workers who are treated in the form of higher employment rates, although those jobs do not pay higher wages. However, our estimates of earnings effects do not take into account the additional earnings which transfer company partici-

³⁵They are not exactly equal because of the inclusion of time dummies δ_t .

	(a) <i>Employment</i>			(b) <i>Unemployment</i>			(c) <i>Other</i>		
	(1) OLS	(2) ITT	(3) 2SLS	(4) OLS	(5) ITT	(6) 2SLS	(7) OLS	(8) ITT	(9) 2SLS
$TC1_i$ 12 months	-0.222*** (0.028)		-0.036 (0.039)	-0.063*** (0.012)		-0.217*** (0.020)	-0.055*** (0.004)		-0.087*** (0.007)
$TC1_i$ 24 months	-0.114*** (0.011)		0.016 (0.019)	0.129*** (0.007)		0.051*** (0.012)	-0.016** (0.006)		-0.068*** (0.010)
$TC1_i$ 36 months	-0.094*** (0.013)		0.016 (0.021)	0.096*** (0.011)		0.064*** (0.013)	-0.002 (0.007)		-0.080*** (0.013)
$TC1_i$ 48 months	-0.066*** (0.011)		0.032* (0.019)	0.032*** (0.005)		0.016** (0.007)	0.035*** (0.008)		-0.048*** (0.015)
$TC1_i$ 60 months	-0.055*** (0.013)		0.050** (0.021)	0.018*** (0.006)		0.003 (0.007)	0.037*** (0.010)		-0.053*** (0.018)
$TC_{J(i)}$ 12 months		-0.021 (0.024)			-0.129*** (0.011)			-0.052*** (0.004)	
$TC_{J(i)}$ 24 months		0.010 (0.011)			0.030*** (0.008)			-0.040*** (0.006)	
$TC_{J(i)}$ 36 months		0.009 (0.012)			0.038*** (0.009)			-0.047*** (0.008)	
$TC_{J(i)}$ 48 months		0.019* (0.011)			0.010** (0.004)			-0.029*** (0.008)	
$TC_{J(i)}$ 60 months		0.030** (0.012)			0.002 (0.004)			-0.032*** (0.010)	
First-stage estimate		0.593*** (0.023)			0.593*** (0.023)			0.593*** (0.023)	
Kleibergen-Paap Wald F-statistic		673.29			673.29			673.29	

Table 3. Estimated effect of transfer companies on employment outcomes after job loss. All estimates weighted by inverse probability weights (see Section 4). Estimates are linear probability models. In panel (a) the dependent variable is equal to one if the individual has any spell of employment in progress at 1, 2, ..., 5 years after separation. In panel (b) the dependent variable is equal to one if the individual has no spell of employment but is in receipt of some UI or is registered for job search. In panel (c) the dependent variable is equal to one if the individual does not appear in the biography data at that point in time. It may therefore include spells of self-employment, unpaid work, retirement and so on. All sample sizes are the same as in panel (a) of Table 2, and the first stage estimates are identical to those reported in panel (a) of Table 2.

pants receive. The social security data does not record the value of these payments, but they are typically significantly higher than the equivalent unemployment benefits: 80–90% of the pre-displacement wage, compared to 60-67% from unemployment benefits. This safety net provides an important buffer against lost earnings as a result of job loss. Assuming an average pre-displacement wage of € 120 (see panel (a) of Figure 5), this implies that those who enter a transfer company have post-displacement earnings one-third higher than those who do not enter a transfer company and receive unemployment benefits. Thus, the safety net appears more important than the helping hand, at least in the short-run.

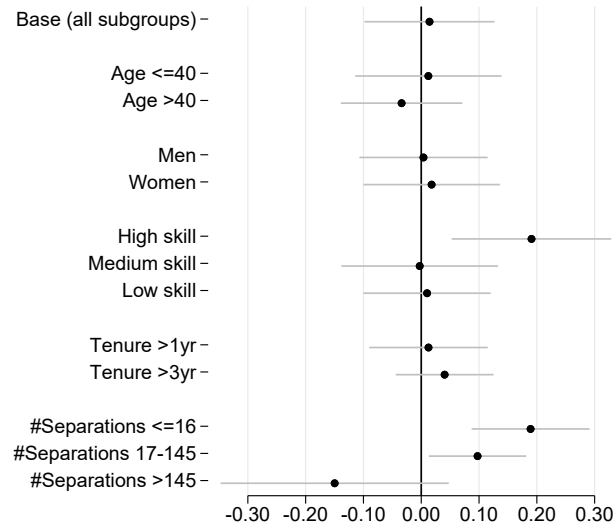
The results reported in Tables 2 and 3 far represent averages for all treated workers. In our data we know very little about the characteristics of the transfer companies themselves, but nevertheless we can explore the extent to which the average treatment effect varies across different kinds of displaced workers and different kinds of displacement events. In Figure 7 we plot estimated treatment effects five years after displacement by age, sex, skill, tenure and the size of the masslayoff event. For each subsample we re-estimate the probability of being in a sending establishment and use inverse probability weights from that subsample, so that in each case our (sub) sample with $TC_j = 1$ is observationally similar to the (sub) sample with $TC_j = 0$.

Panel (a) of Figure 7 shows that earnings effects are also insignificantly different from zero for most of the sub-groups we consider, and no subgroup has a significant negative earnings effect. An exception is high-skill workers³⁶ (defined as Technicians and engineers, Semi-professionals, Professionals and Managers), who have significantly positive earnings effects. Panel (b) shows that younger workers, women and higher-skilled separators have larger and significantly positive employment effects. Employment effects are monotonically declining in skill level. We find very little difference between workers according to their level of tenure.

Perhaps most importantly, we find significant differences in the effectiveness of transfer companies when we split the sample by the size of the layoff event. We split the sample at the 25th and 75th percentile of the distribution of number of separations at the time of the layoff event. Separators from small layoff events who join a transfer company do significantly better than separators from small layoff events who do not join a transfer company, both in terms of employment (+9pp) and earnings (+19%) five years after the separation. In contrast, the effect of transfer companies on separators in large layoff events is negative both for employment and earnings, although both effects are imprecisely estimated.

³⁶Skills are defined based on 3-digit occupation from Blossfeld (1987, Table 1).

(a) Effect on labour market earnings after 5 years



(b) Effect on probability of employment after 5 years

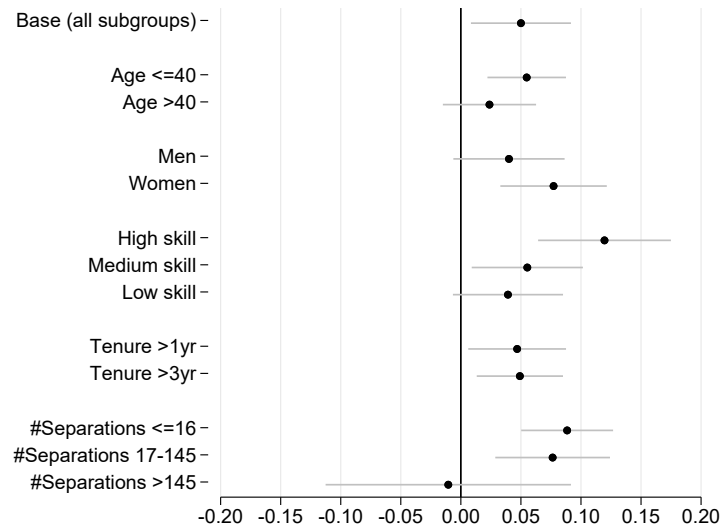


Figure 7. 2SLS estimates of earnings and employment effects of transfer companies. In each panel, the black dot indicates the estimated effect for each sample and the grey bar indicates the 95% confidence intervals.

7 Conclusions

We provide the first systematic evidence on the effectiveness of transfer companies as a policy to help displaced workers. Transfer companies are intended to provide both a “helping hand” and a “safety net”. The helping hand comes from a delay in the point at which the displaced worker officially enters unemployment, during which time the displaced worker receives additional job search assistance and retraining. The safety net comes from additional compensation in excess of their entitlement to unemployment benefit.

A challenge in evaluating this policy is that displaced workers can choose whether or not to enter a transfer company if one is available, and establishments have some choice about whether or not to make a transfer company available. To deal with the first selection issue we utilise the fact that displaced workers from establishments which do not set up a transfer company are unable to use them, providing a model of one-sided non compliance where the establishment’s decision is a highly relevant instrument for individual treatment. To deal with the second selection issue we use a massive comparison group of workers who separate from their employer during a mass-separation event but who do not have a transfer company available, and reweight using the propensity of treatment. We argue that our reweighted sample of separators from the comparison group are sufficiently similar that the availability of a transfer company is independent of potential labour market outcomes.

As expected, at the individual level, displaced workers who enter a transfer company have significantly worse labour market outcomes for up to five years after separation, compared to the reweighted control group. These effects are large and significant. Participants have 24% lower earnings, 19% lower wages and 6% lower employment rates after five years. However, the intention-to-treat results show that displaced workers who had the opportunity of entering a transfer company have a higher probability of being in employment after five years, albeit in jobs which pay slightly (insignificantly) lower wages, leading to quite precisely estimated zero effect on earnings. To interpret this as a causal effect requires us to assume that the reweighted comparison group of workers who suffered a mass-separation event, but who did not have a transfer company available, had similar potential outcomes. In an IV setting, this translates into our main finding that workers who enter a transfer company have a higher probability of employment after five years (+5pp with a standard error of 2.1pp) and insignificantly higher earnings (+1.4% with a standard error of 5.7%).

The positive employment effect we find comes about because transfer company par-

ticipants are 5.3pp less likely to leave the social security system, but not more likely to be in receipt of unemployment benefits. In contrast with the findings (also from the German labour market) on the effect of UI extensions (Schmieder et al., 2016), this suggests that the training and job search advice which transfer companies offer compensate for the extended benefit duration. If we also add in imputed payments made to workers who join a transfer company, the effect of this policy seems unambiguously positive for displaced workers.

The effectiveness of transfer companies appears to differ quite widely across types of workers and the size of the layoff event. Highly-skilled workers who separate in smaller layoff events gain from entering a transfer company both in terms of employment and earnings.

This paper represents the first systematic evaluation of the effectiveness of transfer companies. However, an important caveat to our findings is that, in our data, the transfer company is a black-box: we do not know the details of how compensation, search assistance and training varies from one company to another. Our data at present do not allow us to identify the nature of the agreement which set up the transfer company, such as the maximum length of the program (this was typically 6- or 12-months), the financial compensation or the training services provided. In future research we hope to be able to identify the third-party service providers who run the training and job search assistance. This would allow us to assess the extent to which the outcomes of the treatment vary across different service providers. We also hope to identify the agreed length of the programmes to see if this affects post-displacement outcomes in ways which are consistent with the established findings on the effect of extension of unemployment benefits.

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A Comparison of analysis sample with excluded sample

In Section 5 we showed that only about 60% of the workers who have a spell in a transfer company separate from their employer during a mass-separation event (as defined in Equation 3). We focus on these 60% because for this group a plausible counterfactual comparison group is available. However, it is of interest to understand how the outcomes of the analysis sample differs from those who are treated but who do not appear in our analysis sample.

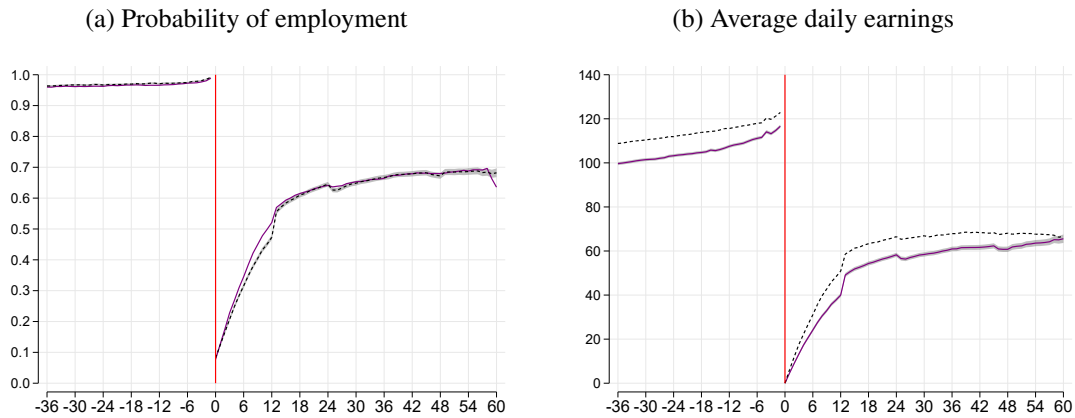


Figure A1. Comparison of transfer company workers' employment and earnings between those who appear in the analysis sample (solid line) and those who do not (dashed line). The vertical line indicates the date of the transfer company spell start. The (very narrow) shaded area around the solid line represents the 95% confidence interval around the difference in the means. Wages are assumed to be zero during transfer company spells. Note that in Figure 5 the vertical line indicates the date of the separation, which is why the gap is not in exactly the same place.

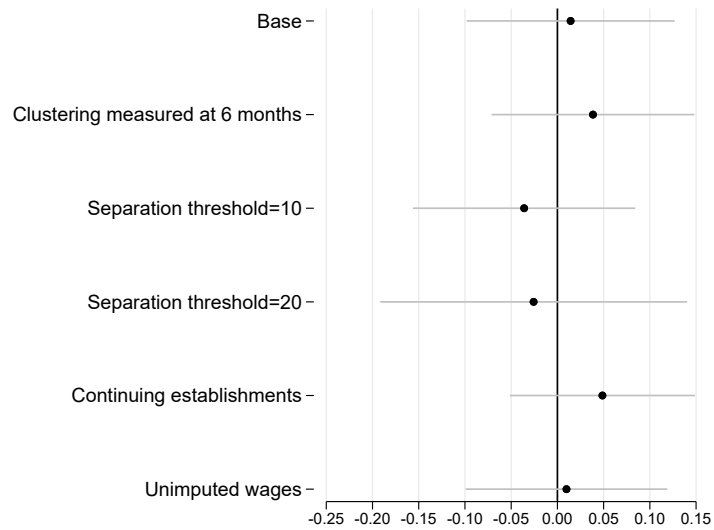
Panel (a) of Figure A1 shows that workers who enter a transfer company have identical employment rates beforehand, whether or not their employer has a mass-separation event (i.e. whether or not they appear in our analysis sample). After entering the transfer company, employment rates recover slightly more quickly for the analysis sample, but after about two years the employment rate is almost identical. In panel (b) we do find some systematic differences in earnings: workers who enter a transfer company after a mass separation event have significantly lower earnings before and after the separation. This highlights the fact that our analysis sample is based on larger separation events which appear to have worse effects on post-separation earnings.

B Robustness of results to sample definitions

A number of decisions were made when constructing the data which might affect our conclusions. In particular, we chose a threshold to determine whether a mass-separation event has occurred (see Equation 3), we chose a point in time after the separation to record the first post-separation employer, we imputed wages using the method described in Dauth and Eppelsheimer (2020) and we included exiting and continuing firms in our sample of mass-separation establishments.

Figure B1 provides a graphical summary of the effects of these decisions. Panel (a) shows the effects of transfer companies on earnings after five years, while panel (b) shows the effect on the probability of employment. At the top of each panel we show the estimate from our preferred baseline specification, shown in Tables 2 and 3. Within each panel, we plot the estimated effect for various specifications, from which we draw the following conclusions. First, measuring the identity of the post-separation employer after 6 months instead of 3 months makes almost no difference to the employment effect and slightly increases the earnings effect. Second, increasing the size of the separation event reduces the earnings and employment effect. This is consistent with our findings in Figure 7, albeit using a different definition of the size of the mass-separation event. Third, when we restrict the sample only to continuing establishments the earnings effect becomes slightly more positive with very little effect on employment. Finally, the result using unimputed earnings is almost identical to the base model.

(a) Effect on labour market earnings 5 years after separation



(b) Effect on probability of employment 5 years after separation

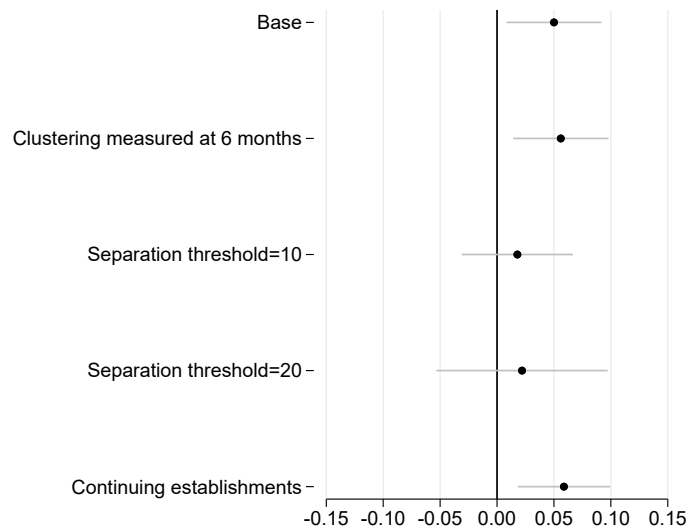


Figure B1. 2SLS estimates of employment and earnings effects of transfer companies under different sample decisions. In each panel, the black dot indicates the estimated effect for each model and each time period. 95% confidence intervals are shown by the grey bars.

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ISSN 2194-2188



The IWH is funded by the federal government and the German federal states.