The effect of job search assistance and compensation on displaced workers*

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Abstract

We provide the first systematic evidence on the effectiveness of a widely-used 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 detailed data on workers' employment patterns before and after displacement, we measure the wages and employment outcomes of displaced workers for up to three years after job loss. 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. Compared to a matched control group, 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 transfer company raises wages and labour market participation 24 and 36 months after displacement.

1 Introduction

It is widely established that displaced workers suffer large wage losses and also extended spells of non-employment.[1] But, despite the pervasiveness of job destruction in modern economies,[2] far less is known about the effectiveness of policies to ameliorate these losses. In this paper we examine a particular form of compensation and job search assistance that operates in Germany, whereby so-called "transfer companies" employ displaced workers for a fixed period 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 who attend 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.

Despite the fact that transfer companies have been well-established in Germany since the mid-1990s, no formal systematic evaluation of their effectiveness is currently available.[3] The effect of transfer companies on displaced workers will depend on three features of the system. First, displaced workers who enter 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. Second, displaced workers who enter a transfer company receive higher levels of benefit for the period of the transfer company. Third, displaced workers who enter 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 additional payments, job search assistance and retraining provide benefits in terms of post-displacement employment outcomes.

In order to estimate the effectiveness of this policy on subsequent labour market outcomes, we need to deal with the problem that displaced workers whose employers set up a transfer company can choose whether or not to enter. Workers with better outside options (e.g. a permanent job) will therefore be less likely to use transfer companies, which would invalidate any worker-level comparison of outcomes. To deal with this problem, we utilise the fact that the availability of a transfer company is dependent on an agreement at the establishment-level. 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

[1] See e.g. Jacobson et al. (1993) and numerous subsequent papers for the US, Upward and Wright (2018) for recent evidence for the UK and Schmieder et al. (2018) for recent evidence for Germany.

[2] A large literature, starting with Davis and Haltiwanger (1992) has established that gross job flows (i.e. job creation and destruction) far exceed net job flows.

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

available. This intention to treat can be used as a highly relevant instrument in the individual outcome equation.

A possible threat to identification is the fact that the decision to set up a transfer company by establishments may depend on the potential labour market outcomes of its displaced workers. The decision to set up a transfer company depends partly on negotiations between the employer and workers' representatives (Works Councils), but also 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 lengthy periods of notice and redundancy payments by contributing to payments to the transfer company which are subsidised by the Federal Employment Agency. To deal with this selection issue we draw on the population of German establishments who experience observably similar mass-layoff events and, from these, select those whose layoff costs are likely to be similar, by matching on worker age, tenure, wages and the employee's job search behaviour. We argue that our matched sample of control establishments are then sufficiently similar that the decision to set-up a transfer company is independent of potential labour market outcomes.

By aggregating detailed spell-level social security data, we first describe the behaviour of establishments before and after setting up a transfer company. We verify that transfer companies are typically set up in response to a sudden mass-layoff event which reduces employment by about 15% on average within one month, and that approximately half of the displaced workers in these establishments enter a transfer company. We then show graphically how the wages and employment patterns of displaced workers differ between those who enter transfer companies (the treated), those who could have entered but chose not to (the non-compliers) and those who could not enter a transfer company (the controls). The treated group experience significantly worse post-displacement outcomes, but this is plausibly the result of negative selection. In contrast, ITT estimates indicate that the availability of a transfer company improves post-displacement outcomes both in terms of wages and labour market attachment, which leads to positive and significant IV estimates 24 and 36 months after displacement.

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 we explain how we identify workers who enter transfer companies, how we identify the establishments which displace these workers, and how we identify a comparable group of displaced workers who do not enter transfer companies. Sec-

tion 5 formalises the selection issues we face and explains our empirical methodology. Section 6 presents our results and Section 7 concludes.

2 Literature

As far as we are aware, there have been no formal evaluations of the effectiveness of transfer companies in Germany.[4] Existing evidence is mainly restricted to case studies. It is reported that workers who participate in transfer companies experience often 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) states that this institution is a helpful labour market policy. On the other hand, Schneider et al. (2007) point out that transfer companies do not enhance the employment prospects of the participants compared to the services provided by the Federal Employment Agency. 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 and struggle to find a suitable control group, or do not find a control group at all.

As noted in the introduction, the use of a transfer company effectively 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: (a) the effect of increased UI duration and generosity on exit rates from unemployment and post-unemployment wages; and (b) the effect of job search assistance and retraining programmes for displaced workers.

A substantial empirical literature has investigated the effect of increased unemployment duration and generosity on exit rates from unemployment; a recent survey is provided by Tatsiramos and van Ours (2014). Meyer (1990) uses variation in the duration of benefit entitlement and generosity across US states and within states across time. He notes that in the US, recalls — which is not a feature of our data — are a large fraction of unemployment exits (perhaps as much as 60%). A disadvantage of Meyer's data

[4]Transfer companies are — indirectly — defined in §111 of the Social Code III. §110 defines other transfer measures (*Transferagentur*) of smaller scale that can be granted to workers who will be displaced, but we are not aware of data or evaluations on that issue either. More details regarding the institutional setup can be found in chapter 3.

is the behaviour after the exhaustion of benefits is not included. Meyer (1990, Figure 4) illustrates that the hazard rate rises sharply shortly before benefit entitlement ends. However, one explanation for this is that firms and workers agree to a recall at the point where benefits run out (p.775). Meyer estimates quite a large generosity elasticity of -0.88 (the hazard falls by 8.8 percent if benefits increase by 10 percent). There is a clear spike in the hazard as benefits approach exhaustion, but the generosity effect is more important because most job-seekers find a job before exhaustion. Katz and Meyer (1990) also stress the importance of recalls for unemployment exits. They show that those who initially expect to be recalled but who are not account for a large fraction of long-term unemployment spells in the US.

A recent literature uses discontinuities in the UI system to investigate the causal impact of the duration of benefit entitlement on unemployment duration and post-unemployment wages. These discontinuities are often caused by age thresholds, as in e.g. Lalive (2007), Schmieder et al. (2012*b*), and Nekoei and Weber (2017), or thresholds from previous job history as in Card et al. (2007) and Le Barbanchon (2016). Card et al. (2007) consider the effect of severance pay as well as benefit entitlement. Their findings on severance pay show that a substantial fraction of the response to UI generosity may be a wealth effect rather than a distortionary substitution effect. They also find very little effect of benefit duration on subsequent match quality. Card and Levine (2000) utilise a change in UI duration in New Jersey, and argue that this change was not subject to the problem of policy endogeneity. Their estimates suggest that the increase in UI duration caused the hazard from unemployment to shift down by about 17%.

Lalive and Zweimüller (2004) utilise a very large change in UI duration in Austria and implement a DiD method (actually DiDiD) which is possible because the reform affected different age groups and regions in different ways. They make the particular point that identifying the causal effect of UI extension using policy reform is potentially biased if the reform is endogenous. For example, if the reform occurs because of worsening labour market conditions. The fact that the reform in this case varied geographically and socioeconomically as well as over time means they can deal with this issue. They find a small but significant effect of UI extension: an increase in entitlement from 30 to 209 weeks caused an increase in expected unemployment duration by 9 weeks.

Van Ours and Vodopivec (2006) utilise a large and sudden change in UI duration eligibility in Slovenia to identify the effect on exits from unemployment. They show a spike in the hazard at the point where benefits are exhausted, but also a DiD effect.

Schmieder et al. (2012*b*) consider the effects of increasing UI duration on both the

initial unemployment spell and future unemployment spells. If increased UI improves subsequent job matches then the effect on the initial unemployment spell may be larger than the total effect. But if increased UI leads to worse subsequent outcomes the reverse is true. They say that little is known about the long-term effects. Schmieder et al. (2012*a*) considers how the effects of UI vary over the business cycle, again using RDD methods.

Le Barbanchon (2016) notes that evidence on the effects of UI level and duration on subsequent match quality is "scarce and mixed". Perhaps the most reliable evidence comes from regression discontinuities. Lalive (2007), Card et al. (2007) and Centeno and Novo (2009) all find negative effects on wages. Schmieder et al. (2016) provides evidence that extending benefit duration does *not* lead to better job matches, and in fact leads to lower post-unemployment wages. They show that there are two effects of UI. First, UI may change the reservation wage (and hence change expected unemployment durations). Second, unemployment duration may itself change the wage-offer distribution. If the (positive) reservation wage effect is small and the (negative) duration effect is large, this would explain why post-unemployment wages decline in benefit duration. Most recently Nekoei and Weber (2017) analyse the effect of UI on reemployment wages. They find a nine-week increase in potential benefit duration causes workers to stay jobless two days longer. However, in contrast to previous studies, they find that the benefit extension also causes workers to obtain jobs that pay on average 0.5% higher wages. Le Barbanchon (2016) also uses a RDD and finds large effects of benefit extension on exits to employment, but no effect on subsequent match quality.

Overall, the conclusion is that benefits reduce the exit rate from unemployment (and therefore increases unemployment durations) and that post-unemployment wages are, if anything, reduced. 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. One striking point about this literature is that none of it relates 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 second relevant strand of the literature relates to the impact of job search as-

sistance and retraining. In contrast to the literature on the effects of UI extensions or benefit generosity, the literature on the effectiveness of job search assistance or retraining is often specifically focused on displaced workers.

There were a number of evaluations of displaced worker programmes in the US. 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 "Evidence provided by the displaced worker demonstration projects indicated clearly that job search assistance speeded up the reemployment of displaced workers." Some of the trials allow one to separately identify the effect of job search assistance as opposed to training.

These results are supported by Dar and Gill (1998), who summarised the effectiveness of retraining programmes for eleven OECD countries. They conclude that "retraining programmes are generally no more effective than job search assistance in increasing reemployment probabilities or postintervention 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 results for participants who receive only job search assistance (outreach, job search and placement assistance, assessments, development of employment plans, counselling, career planning) is quite positive, with gains in earnings which are large relative to the small cost of providing these services. However, the benefits for job search assistance are smaller for displaced workers as opposed to the "Adult program", which deals with disadvantaged adults more generally.

Thus, 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. (2005*a*,2005*b*) 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. Winter-Ebmer (2006) is another study which finds a positive effect of a retraining programme for displaced workers.

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. van den Berge says that we know "little about the net effects of these policies". The

effects are identified by comparing workers who displaced because of bankruptcy (who do not get severance pay) and workers displaced with a "social compensation plan". 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.

3 Institutional detail

A transfer company (*Transfergesellschaft*) is a temporary organisation established by the employer to provide support for displaced workers. The 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 §111 (Social Code III) the process of setting up a transfer company involves not only negotiations between workers and the employer but also consultation

[5]Recalls 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.

[6]German labour law (§111 BetrVG) 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.

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[7]. 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.

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.[8] 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 choose to set up a transfer company? 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

[7] These measures are also mandatory to some extent.

[8] This status enables the Employment Agency to sanction the worker according to §107 and §98 (SC III).

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 advise and usually has to be implemented under serious time pressure it is not the case that transfer companies are set up automatically under certain conditions or in certain firms only.[9]

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 Data

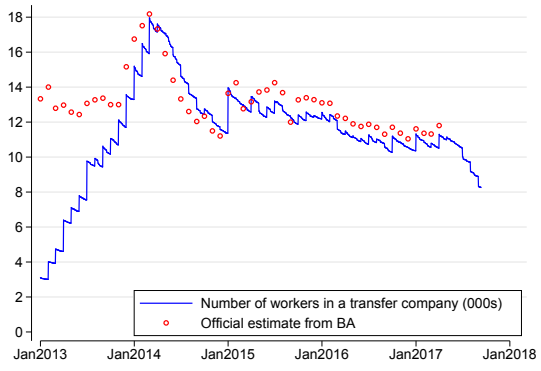
We first describe the information we have on individual spells in transfer companies. We then explain how we identify the establishments which lay off workers and subsequently set up a transfer company. We describe the patterns of employment and worker flows in these establishments, and show that they are consistent with the fact that workers enter transfer companies as a result of sudden employment falls. Once we have identified establishments which use transfer companies, we can identify all workers who are displaced from those establishments during the downsizing event. This group are crucial in establishing the causal impact of the use of transfer companies, because individuals who are laid off from an establishment which uses a transfer company have a choice about whether to enter the transfer company or not. Finally, we explain how we select a comparable control group: workers who are laid off by establishments which do not use a transfer company.

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

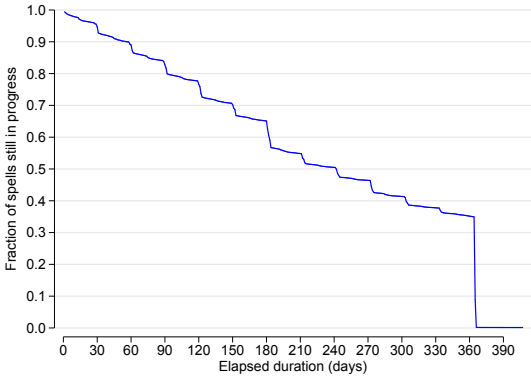
4.1 Spells in transfer companies

We have data on every spell in a transfer company in Germany from January 2013 until September 2017. There are 89,350 of these spells. Each observation contains the start and end date of the spell and almost all contain an identifier of the individual. 87,520 of the spells have a valid individual identifer with 87,194 unique values, indicating that in almost every case an individual has only one spell in a transfer company.

Panel (a) of Figure 1 plots the total number of spells in progress in transfer companies. Because our data only record spells which start from January 2013 onwards, the stock builds up substantially for the first 12 months. The maximum duration of a spell is 12 months, and so from January 2014 onwards we have a record on every spell in a transfer company in Germany. For comparison, we also plot the official recorded number of individuals in transfer companies in Germany, which gives a very similar picture. The number of spells tends to jump at the end of each month because most spells start on the first of the month. Within the month, we observe a decline in the stock as individuals exit their transfer company. Exits also tend to be clustered at the end of each month.



(a) Number of individuals in transfer companies January 2013–September 2017.



(b) Proportion of spells in transfer companies surviving after t days

Figure 1. Description of spells in transfer companies.

Panel (b) of Figure 1 plots the survival rate of spells in transfer companies. Of the 89,350 spells, the 81,018 have completed by the end of sample period; the remaining observations are marked as censored.[10] About 65% of spells end before 365 days, and almost all the rest end on 365 days. Very few spells end after 365 days, as we would expect since we believe the maximum length of a spell is 365 days. Note that the end

[10] 20 spells appears to have a miscoded start date, leading to a negative spell duration. We drop these spells.

of a spell in a transfer company does not necessarily indicate the start of a spell of employment, as we will see in Section 6.2.

4.2 Identifying establishments which use transfer companies

Establishments which set up a transfer company when they make a mass-layoff are labelled as *sending establishments*. The spell data described above includes an id number of this establishment. To check the accuracy of this id number, we examine the complete employment biographies of those individuals who have a spell in a transfer company. These biographies are available from the Integrated Employment Biographies (IEB) provided by the Institute for Employment Research (IAB). The IEB covers all individuals in Germany who have any of the following: spells of employment subject to social security since 1975; spells of marginal part-time employment since 1999; benefit receipt; registered as job-seeking at the German Federal Employment Agency; participation in programs of active labour market policies (in the data since 2000).[11] The IEB data contains start and end dates of all spells in each of the labour market states described above.

Of the 87,194 individuals who have a single spell in a transfer company, we keep only those whose spell ends on or before 30 June 2016.[12] Of the remaining 62,026 individuals just 42 (less than 0.1%) cannot be found in the IEB data. This leaves us with a sample of 61,982 individuals with a single spell in a transfer company for whom we have a record in the IEB data. We search their biographies for a spell of employment in an establishment whose id number matches the id number of the sending establishment given in the transfer company spell data. 50,383 (82%) of individuals have a spell of employment in the identified sending establishment. For these individuals we check whether the final employment spell in that establishment ends shortly before or after the start date of the spell in the transfer company. In 86% of cases the spell in the sending establishment ends within 30 days of the start of the transfer company spell. For the remaining 11,134 individuals who do not have a spell in an establishment listed in the transfer company spell data, we identify the final spell of employment before the transfer company spell starts and keep only those individuals whose final employment spell is within 30 days of the start of the transfer company spell. We are left with a sample of 55,056 individuals for whom we can identify a spell in a sending establishment which

[11] The 2% random sample from the IEB (SIAB) is described in Antoni et al. (2016). Note that we use the full population in order to identify every participant in a transfer company.

[12] At the time of writing the IEB provides biography data up to the end of 2016, so for those who enter a transfer company in 2016 we do not yet know their labour market outcomes.

ends just before the start date of the transfer company spell. This provides us with a list of 2,333 sending establishments.

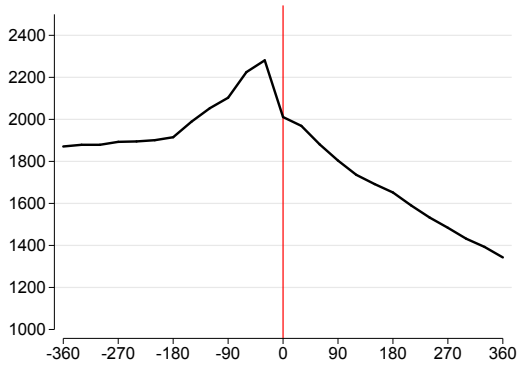
The IEB can be aggregated to the establishment level by taking snapshots of all spells at different points in calendar time. This allows us to examine the precise development of employment and separations before and after workers enter the transfer company. We record the modal transfer company date: the date on which the largest number of workers in that establishment start a spell in a transfer company.[13] Figure 2 plots the behaviour of the 2,333 sending establishments in the 12 months before and after this modal start date.

Panel (a) shows that the number of establishments with positive employment falls from over 2,300 to about 2,000 in the month before the transfer company starts. In other words, about 13% of establishments who use a transfer company have reduced their employment to zero at the point where the transfer company starts. The number of establishments with positive employment continues to fall after the transfer company date, indicating that many sending establishments faced continuing difficulties.[14] Panel (a) also shows that about 15% of establishments appear to "enter" shortly before the transfer company is set up. This may indicate that some establishments are administrative units created as part of a restructuring process at the establishment level (which we cannot observe). In our analysis of worker-level outcomes we will focus only on long-lasting employment relationships, and so these short-lived establishments will not be part of our main analysis sample.

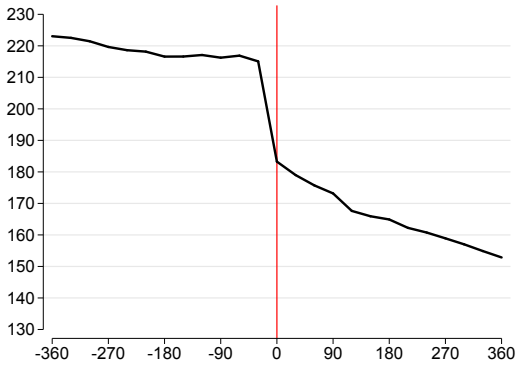
Panel (b) shows a similar pattern for average employment, which falls by an average of 14% in the month before the transfer company date. Employment continues to fall in the following year by about 1.1% per month. Most of these post-transfer company falls in employment are accounted for by the increasing number of establishments with zero employment, rather than by falls in employment in continuing establishments.

Panel (c) shows that the use of a transfer company is very tightly associated with an increased number of separations from the establishment. The separation rate in the year leading up to the transfer company date is less than 2% per month, jumps to 15% in the month of the transfer company start date, and then falls back to 1.5% per month in the year after the transfer company. Panel (d) shows that more than half of these separators enter a transfer company, and they do so almost entirely in the month of the transfer company start date.[15] Panel (e) shows that there is also a jump in the number of workers

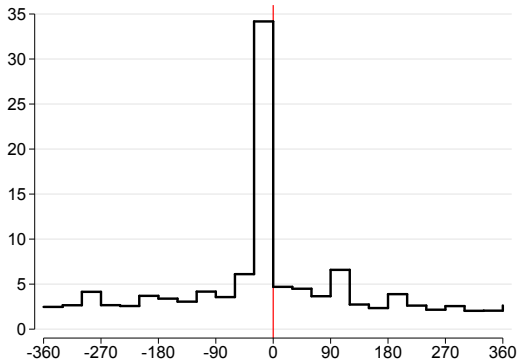
[13] In practice, most establishments have a single date on which they send workers to a transfer company.
[14] They do not, however, continue to send workers to transfer companies, as shown in panel (d).
[15] In fact, the great majority enter on a single day.



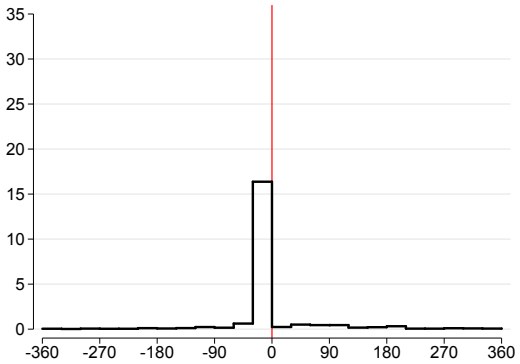
(a) Number of establishments with positive employment



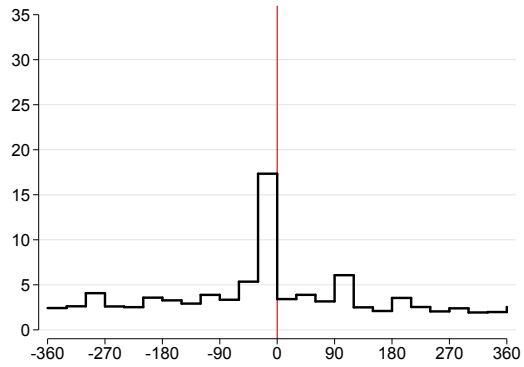
(b) Average employment (includes establishments with zero employment)



(c) Average number of separations in each 30 day period



(d) Average number of separators who enter a transfer company



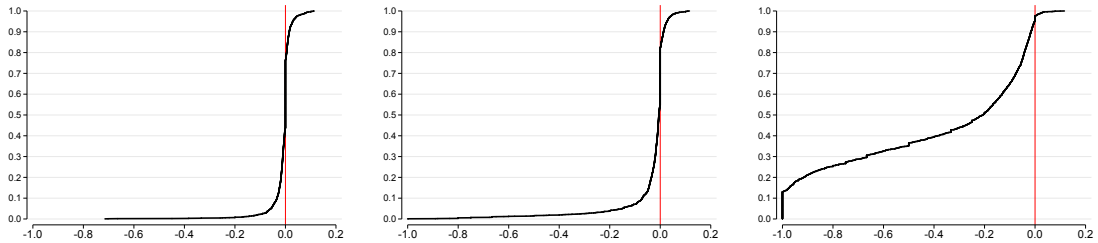
(e) Average number of separators who do not enter a transfer company

Figure 2. Employment and separations in establishments which use transfer companies.

Notes: In each panel, the *x*-axis indicates days relative to the start of the modal transfer company start date in that establishment. In panel (b), average employment includes establishments with zero employment. Separations in panels (c)–(e) count only those separations where the individual does not return to the same establishment.

who separate from the establishment but who do not enter a transfer company.

A final piece of descriptive evidence on the behaviour of establishments which use transfer companies is the distribution of employment growth before and after the transfer company is set up. In the literature on the effect of job loss on workers, it has become standard to consider only workers whose employer experiences falls in employment of 30% or more (Jacobson et al., 1993, and many papers since). Because we have a verified source of information on job loss (workers who enter a transfer company must have lost their job) we can examine what fraction of these displaced workers are included in the standard measure. In Figure 3 we plot the cumulative distribution of employment growth at different points in time around the transfer company date.



(a) 1 year before transfer company date

(b) 3 months before transfer company date

(c) On transfer company date

Figure 3. Cumulative distribution of monthly employment growth rate in establishments which use a transfer company.

Notes: The employment growth rate is calculated as $(N_{i,t} - N_{i,t-30})/N_{i,t-30}$ where $N_{i,t}$ is total employment in establishment i on day t. Exiting establishments therefore have a growth rate of -1.

Figure 3 shows how the distribution of employment growth shifts left as the establishment approaches the transfer company date. In panel (a) we see that about 40% of establishments have stable employment one year before, falling to 30% three months before. In panel (c) we see that 95% of establishments experience employment falls in the 30 days leading up to the use of a transfer company. About 15% of establishments exit, and about 45% have employment falls of more than 30%. Therefore slightly less than half of our establishments are "mass-layoff" establishments according to the standard definition.

4.3 Identifying displaced workers in sending establishments

Having identified these sending establishments, we select the employment biographies of *all* individuals who have a spell in a sending establishment. From these biographies

we select two groups. TC1 workers are those who themselves enter a transfer company (15,961 individuals). These are a subset of the 55,056 individuals defined earlier and work for a selected subgroup of sending establishments (see Section 4.4). TC2 workers are those who separate from the sending establishment at approximately the same time,[16] but who do not themselves enter a transfer company (13,573 individuals). We assume that these TC2 separators are also displaced workers because they exit at a time when the establishment is making a mass-layoff. The fact that we observe approximately the same number of TC1 and TC2 workers is consistent with the evidence in Panels (d) and (e) in Figure 2 which shows that approximately the same number of separators enter a transfer company as do not.

4.4 Identifying a comparable group of displaced workers

Finally, we need to identify a sample of comparable displaced workers whose establishments did not set up a transfer company. This is challenging, because, unlike our TC1 and TC2 samples, we do not have an independent measure of a mass-layoff event (i.e. we do not observe these establishments setting up a transfer company). We therefore need to rely on the conventional approach to identifying displaced workers, which considers workers who separate from establishments which experience large falls in employment as being displaced.

To do this, we select a subset of the 2,333 sending establishments which satisfy a standard set of criteria for mass-layoff establishments.[17] These criteria are:

1. They have at least 50 employees five years before the mass-layoff event;
2. They have no large ($> |30\%|$) fluctuations in employment in the five years before the mass-layoff event.

This ensures that we focus on relatively large establishments which experience a sudden and unexpected layoff event which causes the creation of the transfer company. 470 of the 2,333 sending establishments satisfy these criteria. The median employment loss of these establishments in the 12 months during which the transfer company is set up is -22%, and 75% of these establishments experience employment reductions of 6% or more.

[16] We consider all those who separate within 30 days of the modal transfer company start date for that establishment.

[17] See Schmieder et al. (2018, p.6) for a discussion of these criteria.

We then construct a control group of mass-layoff establishments which have similar characteristics to the 470 sending establishments. To do this we use the population of establishments in Germany contained in the Establishment History Panel (BHP). The BHP contains employment information of all establishments which have any employees in the IEB on the 30 June in each year.[18] We first remove the 2,333 sending establishments. From the remaining establishments (those which do not set up a transfer company) we select a control group of large, stable establishments which experience an annual employment reduction of at least 6% during the period from June 2013 to June 2016 (i.e. during the same period as the transfer companies were created).

Having identified a list of potential control establishments, we select the employment biographies of those individuals, labelled group C, who separate from those establishments during the relevant period e.g. a separator between June 2013 and June 2014 from an establishment which shrank between June 2013 and June 2014. This results in a sample of 318,311 potentially displaced workers.

4.5 Descriptive statistics

Table 1 provides a description of the sending and control establishments we have identified. In column (1) we report statistics for all sending establishments whose workers enter a transfer company between 1 January 2013 and 31 December 2014. We apply this restriction in order to ensure that we have a reasonable period of time after the displacement event. In column (2), we consider only large and stable sending establishments. Finally in column (3) we consider large and stable control establishments.

It is clear that, even after restricting the sample to large stable establishments which experience sudden employment falls, there are substantial differences between those which set up a transfer company and those that do not. Sending establishments are much larger and experience larger falls in employment on average. They are much more likely to be manufacturing establishments, to have a smaller fraction of female workers, and have an older and higher-paid workforce.

Having created a sample of 470 large, stable sending establishments and 24,861 large, stable control establishments, we now compare the characteristics of displaced workers, described in Table 2. Comparing TC1 and TC2 workers, we see that TC1 workers have three more years of tenure and are four years older, they are more likely to be in low-skilled occupations and to be in the middle education group, and their wages

[18]The BHP is described in more detail in Schmucker et al. (2018). It comprises snapshots of the IEB biography data aggregated to establishment level.

	(1) All sending establishments	(2) Selected sending establishments	(3) Control establishments
Number of establishments:			
on June 30 $t-2$	1,220	470	24,861
on June 30 $t-1$	1,262	470	24,861
on June 30 t	1,317	470	24,861
on June 30 $t+1$	1,083	410	24,267
on June 30 $t+2$	898	360	23,469
Average employment:			
on June 30 $t-2$	296.94	399.12	137.55
on June 30 $t-1$	303.86	400.53	137.84
on June 30 t	293.31	383.38	138.44
on June 30 $t+1$	227.79	265.91	112.13
on June 30 $t+2$	211.69	234.73	106.48
Manufacturing	0.49	0.65	0.20
West Germany	0.85	0.90	0.81
Proportion female	0.27	0.27	0.48
Proportion non-German	0.07	0.08	0.08
Proportion high-qualified workers	0.21	0.19	0.14
Proportion medium-qualified workers	0.69	0.70	0.69
Proportion low-qualified workers	0.09	0.11	0.13
Proportion high-skilled occupations	0.26	0.25	0.21
Proportion non-standard workers	0.09	0.09	0.23
Mean age	44.82	44.50	42.71
Mean tenure in establishment	9.97	12.92	6.20
Median wage in Euro	125.77	123.45	94.37

Table 1. Characteristics of sending and control establishments.

Notes: A sending establishment is an establishment whose displaced workers are able to enter a transfer company. Selected sending establishments are those with employment of at least 50 five years before the year in which the transfer company is set up, are observed every year in the BHP from $t=-5$ to $t=-1$ and which do not have large employment fluctuations in any of those years. A non-standard worker is a worker who is not fully liable for social security payments, and includes workers in so-called "mini-jobs" which are marginal part-time jobs and apprentices. Highly qualified workers are those who have a university degree or higher. Medium qualified workers are those whose highest qualification is *Abitur* or vocational training. Low-qualified workers are those who have neither *Abitur* nor vocational training. High-skilled occupations are defined according to the Blossfeld classification (Blossfeld, 1987).

are about 7% lower. Comparing all TC separators (column 1) with all C separators (column 4) confirms the establishment level comparison in Table 1, although here the means are weighted by the number of separators in each establishment. TC separators are paid higher wages, have five additional years of tenure, are less likely to be in non-standard and part-time employment.

	Sending establishments			Control establishments
	(1)	(2) TC1	(3) TC2	(4) C
	All separators	Separators who enter transfer companies	Other separators	All separators
Number of workers	29,534	15,961	13,573	318,311
Number of establishments	466	458	416	22,752
Daily wage in Euro	125.37	121.21	130.27	98.71
Age	46.42	48.16	44.37	43.24
Proportion female	0.24	0.24	0.24	0.46
Proportion non-German	0.08	0.08	0.08	0.07
Proportion high-qualified workers	0.16	0.10	0.24	0.20
Proportion medium-qualified workers	0.77	0.84	0.68	0.74
Proportion low-qualified workers	0.07	0.07	0.08	0.06
Proportion high-skilled occupations	0.22	0.17	0.28	0.28
Proportion medium-skilled occupations	0.37	0.34	0.40	0.37
Proportion low-skilled occupations	0.42	0.49	0.33	0.35
Tenure in establishment (years)	16.20	17.69	14.44	11.11
Proportion of previous three years:				
in employment	0.98	0.98	0.98	0.96
in unemployment	0.02	0.02	0.02	0.04
in non-standard employment	0.06	0.06	0.06	0.08
in part-time employment	0.08	0.08	0.08	0.26

Table 2. Characteristics of workers in sending and control establishments.

Notes: Workers are sampled at the date at which they separate from their establishment. To calculate the proportion of time in each labour market state, we recorded the labour market status of each worker every month for the 36 months before separation. Workers are defined as unemployed if they are not employed in a given month but do have a spell of job search or unemployment benefit receipt.

5 Identifying the causal impact of transfer companies

We face two selection issues. First, individual workers in sending establishments are free to choose whether or not to enter the transfer company. Displaced workers with

better outside options are unlikely to choose to enter a transfer company, and therefore we expect negative selection on future employment prospects into the TC1 group. This will lead to estimates of the impact of transfer companies which are downward-biased. TC1 workers will tend to have worse employment outcomes than a control group not because entering a transfer company has a negative effect, but because these are a selected group of displaced workers who did not have good outside opportunities.

Second, establishments which are going to make 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 the extent to which 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 event. Our view is that the decision to set up a transfer company is taken after the decision to downsize,[19] and therefore individual workers are unlikely to select into an establishment because they think that a transfer company 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). However, for this to be a valid instrument we require that the type of establishment a worker is in is independent of their potential post-displacement outcomes. This assumption may be violated because of the second selection issue: establishments which use transfer companies might differ in ways which are correlated with post-displacement outcomes.

Therefore we also need to control for differences between establishments which use transfer companies and those that do not.[20] To do this we match sending establishments with control establishments on the basis of 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

[19] Or at least this decision is unknown to a worker who decides to join the establishment

[20] Ideally, we would also have an instrument for this, but at the time of writing this is not yet available.

workforce such as their tenure, and so on. Ideally, balancing the sample on these characteristics will ensure that the type of establishment a worker is in is independent of their potential post-displacement outcomes.

To formalise these ideas, our intention-to-treat is a comparison of outcomes between those who are displaced from a sending establishment and those who are displaced from a control establishment. Separation takes place at $t = 0$. We observe post separation wages at three points in time after separation, $t = 1,\ldots,3$. A simple ITT estimate can be written as

$$y_{it} = \pi_0 + \sum_{k=1,\ldots,3} \pi_k (D_{kt} \cdot TC_{J(i)}) + \delta_t + \varepsilon_i, \tag{1}$$

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 $TC_{J(i)}$ is a dummy variable indicating that the establishment $j = J(i)$ of individual i at time $t = 0$ set up a transfer company.

If $TC_{J(i)}$ was as good as randomly assigned, then $\pi_1,\ldots,\pi_3$ would be the causal impact of having been displaced from a sending establishment, compared to having been displaced from a control establishment. These estimates can be scaled by the proportion of separators who actually enter a transfer company i.e. those who have $TC1_i = 1$. The stylised first stage regressions in this case would be

$$D_{kt} \cdot TC1_i = \gamma_{k0} + \gamma_{k1}(D_{kt} \cdot TC_{J(i)}) + \delta_t + \eta_{ki} \;\; k = 1,2,3. \tag{2}$$

Conditional on our assumption that $TC_{J(i)}$ is as good as randomly assigned, the extent to which the Wald ratios π_k/γ_{k1} represent the causal impact of $TC1_i$ (the individual entering the transfer company) depend on whether the assignment of TC_j has effects on y_{it} other than through the individual's use of the transfer company. One threat to this assumption is the possibility that large-scale layoffs might have effects on the local labour market generally, which affect post-displacement outcomes for all workers regardless of their use of a transfer company. We regard this threat as less serious than the problem that $TC_{J(i)}$ is not randomly assigned. *to discuss: what happens if some of the displaced workers never had the possibility to enter a TC?*

Note that a difference-in-difference extension is not helpful in this case. This is because the use of a transfer company is positively correlated with size and hence pre-displacement wages. As shown in Table 1, sending establishments are different from non-sending establishments: they are larger, experience larger falls in employment, are more likely to operate in the manufacturing sector, and so on. Because of this, the wage *losses* from displacement will tend to be larger for displaced workers with $TC_{J(i)} = 1$

even if their post-displacement wage levels are similar.[21]

6 Results

6.1 Matching treated and control establishments

In Table 3 we report a comparison of means between displaced workers in treated (sending) and control (non-sending) establishments before and after propensity score matching. In columns (1) and (2) we confirm that sending establishments are far larger than control establishments before matching; the difference is almost 0.8 log-points at the time of the displacement event. Note that we also match on establishment employment one- and two-years after the displacement event. Typically, one should not match on post-displacement characteristics because they themselves could be outcomes of the treatment. In this case however, we wish to compare individuals who were displaced in similar circumstances, and therefore we wish to match on the size of the employment reduction between $t = 0$ and $t > 0$. The difference in log employment between the sending and control establishments after matching is shown in columns (4) and (5). The matching procedure greatly reduces the difference, which is not significant at conventional levels. However, we note that there is still a substantial gap of 0.4 log-points which matching does not eliminate.

The second crucial characteristic which we match on is the proportion of displaced workers who are registered for job search with the Federal Employment Agency. Registering for job search is far more likely for displaced workers who do not have outside opportunities. We match not only on the proportion registered for job search at the time of displacement ($t = 0$), but also at monthly intervals leading up to displacement. In the unmatched samples, a much higher proportion of displaced workers register for job search in sending establishments (over 50% in sending establishments compared to under 20% in control establishments). After matching, these proportions are more balanced and again we cannot reject the null of equality,

We also match on the displaced workers' pre-displacement wages at the time of displacement and at monthly intervals leading up to displacement. In the third panel of Table 3 we see that displaced workers from sending establishments have significantly higher pre-displacement wages, and that after matching these differences are reduced.

[21] Empirical evidence suggests that the cost of job loss is strongly associated with the size of the establishment (Fackler et al., 2017).

	Unmatched			Matched		
	(1)	(2)	(3)	(4)	(5)	(6)
	Control	Sending	p-value	Control	Sending	p-value
log estab. employment $t=-2$	5.565	6.414	[0.001]	5.898	6.237	[0.158]
log estab. employment $t=-1$	5.568	6.396	[0.002]	5.874	6.212	[0.164]
log estab. employment $t=0$	5.564	6.336	[0.004]	5.817	6.149	[0.173]
log estab. employment $t=+1$	4.357	4.315	[0.915]	3.807	4.045	[0.554]
log estab. employment $t=+2$	3.969	3.325	[0.127]	2.822	3.009	[0.649]
Prop. registered for job search $t=0$	0.197	0.538	[0.000]	0.596	0.559	[0.566]
Prop. registered for job search $t=-30$ (days)	0.173	0.344	[0.008]	0.274	0.357	[0.240]
Prop. registered for job search $t=-60$ (days)	0.147	0.277	[0.054]	0.215	0.287	[0.329]
Prop. registered for job search $t=-90$ (days)	0.120	0.225	[0.080]	0.178	0.232	[0.411]
log wages $t=0$	4.491	4.810	[0.000]	4.763	4.796	[0.242]
log wages $t=-30$ (days)	4.406	4.746	[0.000]	4.633	4.730	[0.008]
log wages $t=-60$ (days)	4.370	4.704	[0.000]	4.598	4.688	[0.008]
log wages $t=-90$ (days)	4.348	4.686	[0.000]	4.596	4.672	[0.014]
Tenure (years)	11.112	16.198	[0.000]	16.043	16.232	[0.885]
Age (years)	43.245	46.418	[0.000]	46.524	46.570	[0.929]
1=female	0.456	0.237	[0.000]	0.272	0.236	[0.248]
1=non-German nationality	0.073	0.082	[0.216]	0.078	0.082	[0.761]
1=low education	0.063	0.074	[0.427]	0.073	0.075	[0.889]
1=high education	0.202	0.160	[0.097]	0.160	0.142	[0.405]
Sample size	318,311	29,534		28,158	28,158	

Table 3. Comparison of matched and unmatched samples

Notes: Table shows means of each variable and the p-value of the test that the means are equal across the two samples. The matching procedure used was kernel matching. The unmatched samples (columns 1 and 2) comprise all workers who separate from control and sending establishments. The matched samples (columns 4 and 5) are weighted by the resulting matching weights. Additional variables included in the matching procedure were a full set of 2-digit industry dummies and *Bundesland* dummies. Mean comparisons for these variables are reported in Table A1 in Appendix A.

Our current matching procedure is however not able to entirely balance the samples across pre-displacement wages in the 90 days leading up to displacement.

The final panel of Table 3 shows that displaced workers in sending establishments have more than five years additional tenure in the establishment, are more than three years older and are disproportionately male. All three of these characteristics are balanced across the control and sending establishments after matching.

6.2 Graphical comparison of outcomes

In this section we plot outcomes for the three relevant groups in our analysis, namely TC1, TC2 and C workers. For each worker, we calculate their employment status and their wage at monthly intervals up to and after the date on which they lose their job. The job loss date for TC1 workers is defined as the date at which they leave the establishment which precedes their spell in a transfer company. The job loss date for TC2 workers is defined as the date at which they leave an establishment if it is within 30 days of the modal transfer company start date. The job loss date for C workers is defined as the date at which they leave an establishment if they separate during the year in which the establishment is shrinking. We restrict the sample to workers who are aged between 20–50 and who have at least three years tenure in their establishment at the time of displacement.[22]

Panel (a) shows that the employment rate is almost 1 for the three years preceding the job-loss date. This is because we restrict the sample based on those who have at least five years tenure in their displacement establishment.[23] After job loss, TC1 workers enter a transfer company and therefore are not in employment by definition. As TC1 workers exit transfer companies over the next 12 months their employment rate increases quickly, and then jumps after 12 months. This pattern matches the pattern of transfer company spell lengths shown in panel (b) of Figure 1. In contrast, TC2 workers' employment pattern is quite different. Over 80% are in employment within one month of the job loss date, which confirms our assertion that workers who choose not to enter a transfer company tend to be those who have better outside options. However, we do not observe any subsequent recovery in the employment rate from this point on. As we would expect, workers in the control group have a pattern of employment which

[22] We recognise that this reduces the sample size considerably and reduces the generalisability of our results. However, restricting the sample to high tenure worker increases the probability that we identify workers who would have remained in the establishment if the downsizing event had not occurred.

[23] The fact that the proportion is not exactly 1 is because individuals may have gaps in their employment record.

lies in between the TC1 and TC2 group. About 60% of this group are in employment one month after displacement, and this recovers somewhat over the next 2 years.

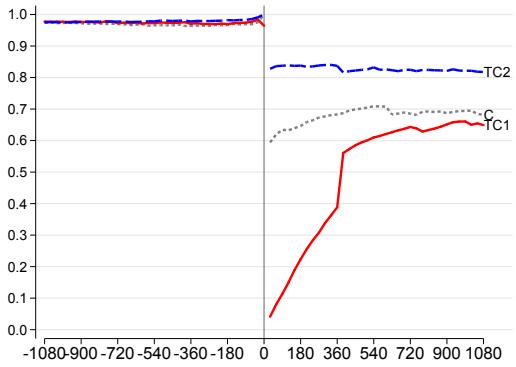
Panel (b) of Figure 4 shows the proportion of the sample who are registered for job search. Consistent with our claim that we have identified a group of displaced workers (rather than those who voluntarily left their establishment) we find that the proportion registered for job search is very low in the three years before the displacement date, and increases suddenly about six months before. The TC1 group have the highest rate of registration and the TC2 group the lowest rate.

Panel (c) shows the proportion of the sample in unemployment. An individual is defined as unemployed at a point in time if they have no employment spells, are not in a transfer company spell, but do appear in the IEB data as receiving UI, or registered for job search, or in an Employment Agency funded training programme. By definition, none of the TC1 group are unemployed immediately after job loss, but a small fraction enter unemployment by leaving a transfer company over the next 12 months. Then, after 12 months, there is a jump as the maximum duration is reached. Comparing panel (a) and panel (c) we can see that slightly less than 60% of TC1 workers are in employment after 12 months and slightly less than 40% are in unemployment.

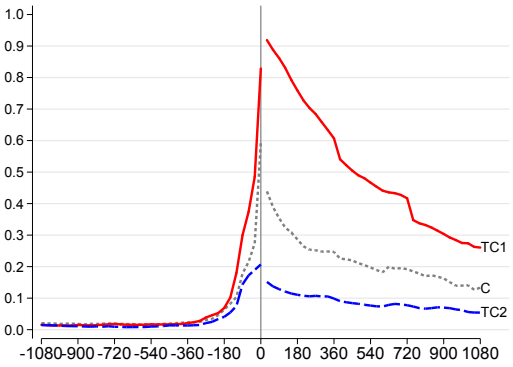
Panel (d) shows the proportion of the sample who do not appear in the biography data in each month. Unsurprisingly this is initially zero for the TC1 group because spells in transfer companies appear as spells in the biography data. The sudden jump after 12 months represents those workers who end their transfer company spell and exit the labour market. The proportion of the TC2 and C groups who are out of the labour market is slightly higher.

The final two panels of Figure 4 show the familiar patterns of wages before and after job loss which have been calculated by Jacobson et al. (1993) and many others. In panel (e) we ignore any unemployment benefit receipt and set wages to zero for any spells of non-employment (including spells in transfer companies). Wages from employment then follow very closely the pattern of employment in panel (a). Note that the TC1 sample are recorded as having zero wages during their TC spell, which accounts for the fact that their wage loss is much larger but jumps up as their transfer company spell finishes.

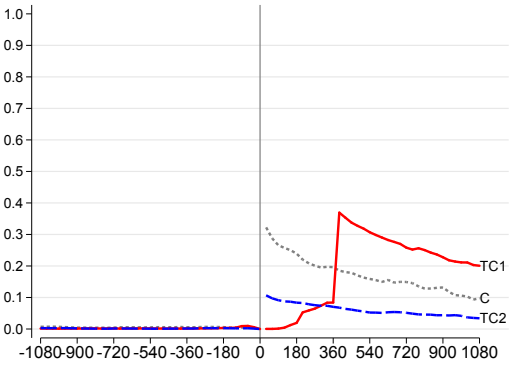
In panel (f) we plot wages for those who are in employment. For those who find work immediately wage losses are quite small in the C and TC2 groups. Again, this indicates that these displaced workers had outside options. However, average wages of the C group decline slightly over the next three years, which is largely driven by the composition effect from workers re-entering employment with lower wages after



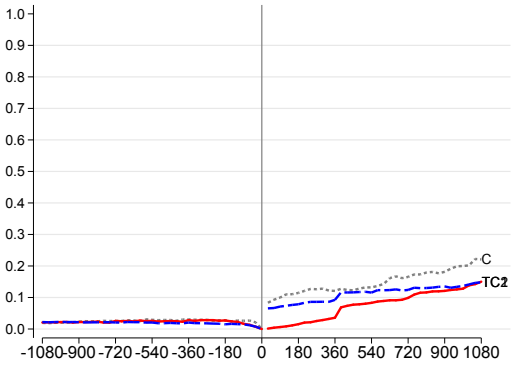
(a) Proportion in employment



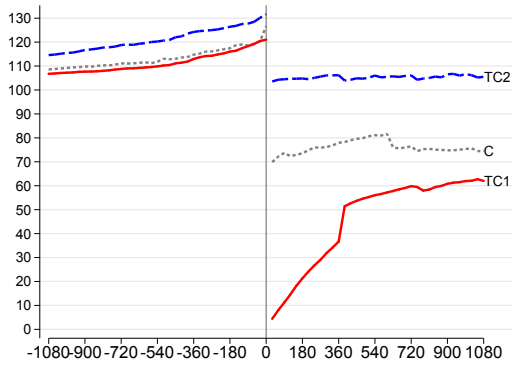
(b) Proportion registered for job search



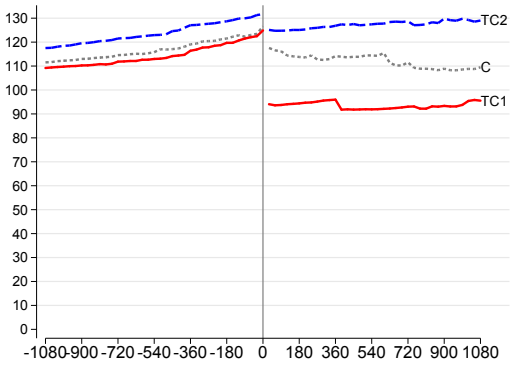
(c) Proportion unemployed



(d) Proportion not in data



(e) Average daily earnings (€) ($w = 0$ for non-employment spells)



(f) Average daily wage in employment spells (€)

Figure 4. Employment and wage outcomes for workers who are displaced.

Notes: the x-axis indicates time relative to the day on which the employment spell ends. Wages in panels (e) and (f) are average daily wage for the spell in progress at that point in time. The line labelled "TC1" indicates workers who enter a transfer company. The line labelled "TC2" indicates workers who do not enter a transfer company but who separated from a sending establishment. The line labelled "C" indicates workers who separated from a non-sending establishment.

longer periods of job search. Wages in the TC1 group dip slightly after 12 months; this will reflect the effect of those workers whose transfer company spell ends and enter employment on slightly lower wages than those workers who left transfer companies before the maximum 12 month duration.

6.3 Regression results

In Table 4 we report estimates of the labour market earnings effect of participation in a transfer company. As in panel (e) of Figure 4, our definition of labour market earnings is total earnings from any spells of employment, with earnings from all other spells set to zero. Column (1) confirms, unsurprisingly, that TC1 workers have significantly lower earnings than the control group, and that this gap remains after 36 months. This is clear from panel (e) of Figure 4, because this regression is a comparison of TC1 group with the control group. It seems plausible that this large negative effect represents the strong selection bias inherent in any comparison of TC1 displaced workers with a control group, because the TC1 group will consist of workers who entered a transfer company because they did not have an outside option.

Column (2) reports estimates of π_1, π_2 and π_3 from Eqn (1). This is a comparison of average earnings, after job loss, between all workers who are displaced from sending establishments and all workers who are displaced from control establishments. If the process of matching has successfully removed the selection bias so that ε_i is uncorrelated with $TC_{J(i)}$, these represent causal ITT effects. After 12 months, displaced workers from sending establishments have significantly lower earnings than displaced workers from control establishments. At this point, a substantial fraction of TC1 workers are finishing their spell in a transfer company, and we have assigned them a wage of zero at this point. After 24 and 36 months, however, the wage gap becomes positive and significant, with a difference of 8.8 log points after 36 months.

Column (3) reports estimates of γ_{k1} from the three first stage regressions given in Equation (2). 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 $TC1_i = 1$ are compliers and those with $TC2_i = 1$ are never-takers, which means that our IV estimates will be estimates of the average treatment effect on the treated. This also means that our first stage estimates will be highly significant, as shown in column (3). In fact, the estimates of γ_{11} and γ_{21} are identical in this case because we have a balanced panel of observations at 12 and 24 months. At 36 months our sample is no longer balanced

	(1) OLS	(2) ITT	(3) First stage	(4) 2SLS
(a) Labour market earnings				
$TC1_i$ 12 months	-0.572^{***}			-0.221^{***}
	(0.012)			(0.026)
$TC1_i$ 24 months	-0.300^{***}			0.074^{***}
	(0.012)			(0.028)
$TC1_i$ 36 months	-0.257^{***}			0.165^{***}
	(0.018)			(0.044)
$TC_{J(i)}$ 12 months		-0.126^{***}	0.569^{***}	
		(0.015)	(0.003)	
$TC_{J(i)}$ 24 months		0.042^{***}	0.569^{***}	
		(0.016)	(0.003)	
$TC_{J(i)}$ 36 months		0.088^{***}	0.535^{***}	
		(0.024)	(0.004)	
Number of obs.	658,898	658,898	658,898	658,898
Number of ind.	296,658	296,658	296,658	296,658
(b) Wages from employment				
$TC1_i$ (12 months)	-0.203^{***}			-0.073^{***}
	(0.006)			(0.015)
$TC1_i$ (24 months)	-0.228^{***}			-0.010
	(0.007)			(0.017)
$TC1_i$ (36 months)	-0.195^{***}			0.077^{**}
	(0.012)			(0.035)
$TC_{J(i)}$ 12 months		-0.042^{***}	0.578^{***}	
		(0.008)	(0.003)	
$TC_{J(i)}$ 24 months		-0.005	0.508^{***}	
		(0.009)	(0.004)	
$TC_{J(i)}$ 36 months		0.037^{**}	0.477^{***}	
		(0.017)	(0.005)	
Number of obs.	540,045	540,045	540,045	540,045
Number of ind.	261,639	261,639	261,639	261,639

Table 4. Estimated effect of transfer companies on post-displacement wages.

Notes: 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) observations with zero earnings are excluded from the sample. Column (3) represents the results of the three first stage regressions (2).

because individuals who are displaced in 2014 are not observed in 2017.[24] The estimates of γ_{k1} tell us that 54–57% of displaced workers from sending establishments enter a transfer company.

Column (4) then reports 2SLS estimates which in this case are almost exactly equal to the ITT scaled by the first stage coefficients.[25] We therefore estimate sizeable and significant positive impacts on earnings of participation in a transfer company 24 (7.4 log points) and 36 months (16.5 log points) after displacement. We stress again that in order to interpret this as a causal impact requires us to assume that, after matching, the workers in sending establishments do not have higher potential earnings than those in control establishments.

In panel (b) we examine whether these positive impacts on earnings are the result of higher employment rates or higher wages conditional on employment. We do this by excluding non-employment observations from the regression sample. Here we see that the ITT and the 2SLS estimates are reduced, but 36 months after displacement the impact of transfer companies is still positive and significant (7.7 log points). Thus, participation in transfer companies increases earnings partly through higher wages, and partly through higher employment rates.

In Table 5 we use linear probability models to estimate the impact of transfer companies on the probability of being in employment, unemployment and out of the labour market. From panel (a), we see that treatment increases the probability of employment by about 9 percentage points after 36 months. However, from panel (b) we see that this is not because treatment reduces the probability of unemployment. Instead, from panel (c) we see that treatment reduces the probability of non-employment by 13.3 percentage points after 36 months. Thus, the positive earnings effect observed in Table 4 is the result of higher labour market attachment rather than lower risk of unemployment. An important caveat here is that "non-employment" simply captures non-appearance in the biography data, and may therefore represent spells of self-employment or even retirement. If this was the case, our earnings effect may be overestimated.

[24] Recall that the biography data currently runs to the end of 2016.

[25] They are not exactly equal because of the inclusion of time dummies δ_t.

	(1) OLS	(2) ITT	(3) First stage	(4) 2SLS
(a) Probability of employment				
$TC1_i$ 12 months	-0.339^{***}			-0.175^{***}
	(0.008)			(0.018)
$TC1_i$ 24 months	-0.090^{***}			0.066^{***}
	(0.009)			(0.021)
$TC1_i$ 36 months	-0.081^{***}			0.088^{***}
	(0.013)			(0.034)
$TC_{J(i)}$ 12 months		-0.099^{***}	0.569^{***}	
		(0.010)	(0.003)	
$TC_{J(i)}$ 24 months		0.037^{***}	0.569^{***}	
		(0.012)	(0.003)	
$TC_{J(i)}$ 36 months		0.047^{***}	0.535^{***}	
		(0.018)	(0.004)	
(b) Probability of unemployment				
$TC1_i$ 12 months	-0.075^{***}			-0.209^{***}
	(0.007)			(0.016)
$TC1_i$ 24 months	0.143^{***}			0.035^{*}
	(0.008)			(0.018)
$TC1_i$ 36 months	0.125^{***}			0.046^{*}
	(0.010)			(0.026)
$TC_{J(i)}$ 12 months		-0.119^{***}	0.569^{***}	
		(0.009)	(0.003)	
$TC_{J(i)}$ 24 months		0.020^{*}	0.569^{***}	
		(0.010)	(0.003)	
$TC_{J(i)}$ 36 months		0.025^{*}	0.535^{***}	
		(0.014)	(0.004)	
(c) Probability of non-employment				
$TC1_i$ 12 months	-0.077^{***}			-0.108^{***}
	(0.005)			(0.012)
$TC1_i$ 24 months	-0.053^{***}			-0.101^{***}
	(0.007)			(0.017)
$TC1_i$ 36 months	-0.044^{***}			-0.133^{***}
	(0.011)			(0.030)
$TC_{J(i)}$ 12 months		-0.061^{***}	0.569^{***}	
		(0.007)	(0.003)	
$TC_{J(i)}$ 24 months		-0.057^{***}	0.569^{***}	
		(0.009)	(0.003)	
$TC_{J(i)}$ 36 months		-0.071^{***}	0.535^{***}	
		(0.016)	(0.004)	
Number of obs.	658,898	658,898	658,898	658,898
Number of ind.	296,658	296,658	296,658	296,658

Table 5. Estimated effect of transfer companies on post-displacement labour market outcomes

Notes: 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 12, 24 or 36 months after employment. 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.

7 Conclusions

In this paper we provide the first systematic evidence on the effectiveness of transfer companies as a policy to assist displaced workers. The policy is intended to provide a temporary "buffer" which effectively delays the point at which the displaced worker officially enters unemployment. During the period of employment in the transfer company, the displaced worker receives compensation in excess of their entitlement to unemployment benefit, additional job search assistance and retraining.

A challenge in evaluating the 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 match with the population of German establishments who experience observably similar mass-layoff events and whose layoff costs are likely to be similar. We argue that our matched sample of control establishments are then sufficiently similar that the decision to set-up 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 12, 24 and 36 months after displacement. However, the intention-to-treat results show that displaced workers from sending establishments have significantly higher wages and higher employment probabilities than workers from control establishments. This translates into significant and economically meaningful positive effects in an IV setting. In contrast with the recent findings on the effect of UI extensions (Schmieder et al., 2016), this seems to suggest that transfer companies do more than simply extend benefit duration.

There are two important caveats to our findings, which require further examination. First, we are relying on a selection-on-observables approach to deal with establishment selection. If, after matching, the error term in (1) is correlated with future labour market outcomes then our ITT estimate is biased. It is not obvious however that the decision to set up a transfer company will be *positively* correlated with workers' potential outcomes post-displacement; in fact the reverse seems more plausible. We also note that we have matched on a rich set of individual and establishment-level characteristics in the period before displacement. The second caveat is that the transfer company is something of a black-box: we do not know the details of how compensation, search assistance and

retraining varied from one company to another.

There are a number of ways in which we can extend this analysis. First, if any endogeneity in the ITT equation comes from establishment-specific characteristics which affect subsequent labour market outcomes, a within-establishment comparison of displaced workers and stayers in sending establishments against a within-establishment comparison of displaced workers and stayers in control establishments would remove this effect. This would essentially combine the ITT with a within-firm difference-in-differences approach. Second, we understand from communication with the Federal Employment Agency that a barrier to setting up a transfer company was simply a lack of knowledge about this institution. If this is the case, then we may be able to use data on pre-existing use of transfer companies as an instrument for the use of this policy in the current analysis period. Third, we would like to examine particular features of transfer companies. Because we have information on all participants, it will be possible to estimate the maximum agreed length of each programme 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. Finally, we also have information on the identity of the service providers. A service provider may be responsible for many different transfer companies, and it would be of great interest to policy makers to understand how the effectiveness of the policy varies with service provider.

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Appendix A Additional matching results

	Unmatched			Matched		
	(1)	(2)	(3)	(4)	(5)	(6)
	Control	Sending	*p*-value	Control	Sending	*p*-value
Land- und Forstwirtschaft	0.003	0.000	[0.000]	0.000	0.000	[.]
Bergbau	0.005	0.036	[0.213]	0.012	0.038	[0.342]
Verarbeitendes Gewerbe	0.227	0.734	[0.000]	0.702	0.722	[0.744]
Energieversorgung	0.019	0.001	[0.000]	0.003	0.001	[0.003]
Wasserversorgung, Entsorgung	0.009	0.001	[0.000]	0.002	0.001	[0.281]
Baugewerbe	0.027	0.020	[0.315]	0.036	0.020	[0.255]
Handel, Reparatur	0.118	0.065	[0.073]	0.067	0.068	[0.974]
Verkehr, Lagerei	0.062	0.021	[0.000]	0.026	0.022	[0.692]
Gastgewerbe	0.028	0.000	[0.000]	0.000	0.000	[.]
Information, Kommunikation	0.044	0.036	[0.677]	0.032	0.038	[0.746]
Finanz- und Versicherungs DL	0.041	0.006	[0.000]	0.009	0.006	[0.317]
Grundstücks- und Wohnungswesen	0.006	0.004	[0.373]	0.003	0.004	[0.623]
Freiberufliche, wiss. und techn. DL	0.081	0.033	[0.004]	0.042	0.034	[0.738]
Sonstige wirtschaftl. DL	0.089	0.014	[0.000]	0.022	0.015	[0.259]
Öffentliche Verwaltung, SV	0.068	0.000	[0.000]	0.000	0.000	[.]
Erziehung und Unterricht	0.041	0.002	[0.000]	0.005	0.001	[0.008]
Gesundheit- und Sozialwesen	0.110	0.009	[0.000]	0.022	0.010	[0.058]
Kunst, Unterhaltung, Erholung	0.006	0.000	[0.000]	0.000	0.000	[.]
Sonstige DL	0.016	0.002	[0.000]	0.004	0.002	[0.377]
Ext. Organisationen	0.001	0.018	[0.188]	0.013	0.019	[0.692]
Schleswig-Holstein	0.024	0.030	[0.602]	0.039	0.031	[0.705]
Hamburg	0.032	0.012	[0.002]	0.015	0.013	[0.714]
Niedersachsen	0.086	0.047	[0.005]	0.044	0.050	[0.699]
Bremen	0.018	0.008	[0.136]	0.008	0.008	[0.992]
Nordrhein-Westfalen	0.238	0.349	[0.133]	0.328	0.366	[0.651]
Hessen	0.073	0.057	[0.393]	0.082	0.060	[0.369]
Rheinland-Pfalz	0.041	0.017	[0.001]	0.018	0.018	[0.997]
Baden-Wuerttemberg	0.131	0.184	[0.180]	0.215	0.193	[0.762]
Bayern	0.152	0.201	[0.388]	0.153	0.163	[0.853]
Saarland	0.012	0.009	[0.473]	0.012	0.009	[0.711]
Berlin	0.056	0.018	[0.001]	0.017	0.019	[0.810]
Brandenburg	0.026	0.019	[0.519]	0.015	0.020	[0.699]
Mecklenburg-Vorpommern	0.017	0.000	[0.000]	0.000	0.000	[.]
Sachsen	0.040	0.025	[0.131]	0.028	0.027	[0.899]
Sachsen-Anhalt	0.028	0.006	[0.000]	0.008	0.006	[0.733]
Thueringen	0.026	0.017	[0.319]	0.020	0.018	[0.836]
Sample size	318,311	29,534		28,158	28,158	

Table A1. Comparison of matched and unmatched samples: results for industry and region dummies. Refer to Table 3 for comparison of all other covariates.