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Are Temporary Work Agency Jobs Stepping Stones? Exploiting Variation in the Timing of Bankruptcy

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Are Temporary Work Agency Jobs Stepping Stones? Exploiting Variation in the Timing of Bankruptcy*

Abstract

This study analyses whether temporary work agency (TWA) employment is a stepping stone to better labour market outcomes for displaced workers. Using monthly Dutch administrative data for the period 2010–2022, we focus on a sample of incumbent employees who are displaced because of a firm bankruptcy, and analyse the causal effect of the first job after displacement being a TWA job vs. another job. The main contribution of this study is that we exploit variation in firm bankruptcy dates in a novel instrumental variable approach to deal with the endogeneity of TWA employment. We find that TWA causes negative effects on employment and working hours, which dissipate around 30 months after firm bankruptcy. Effects on hourly wages are negative (20%) and consistent up until seven years after firm bankruptcy. TWA employment also reduces the probability of having a permanent contract by around 20 percentage points in the long run.

JEL classification

J31, J64

Keywords

temporary work agency, stepping stone, labour market flexibilization, instrumental variables, displaced workers

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1 Introduction

The share of workers in atypical forms of work has been steadily increasing worldwide (OECD, 2019; Katz and Krueger, 2019). Atypical work is generally characterised by lower job security, lower wages and less attractive secondary benefits than jobs with a permanent contract (Neugart and Storrie, 2006; Boeri and Garibaldi, 2024). There is a long debate in labour economics on whether such jobs can be a "stepping stone" towards permanent employment, or are "dead ends" in which workers are caught in a spiral of continuous short-term contracts and low wages; see e.g. Booth et al. (2002); Neugart and Storrie (2006); Esteban-Pretel et al. (2011). Concerns about workers moving between atypical jobs and being locked in as labour market "outsiders" are amplified by recent evidence that transition rates from temporary to permanent contracts have been declining in several countries (Barbieri et al., 2019; Biegert, 2019). Empirically, results have provided evidence both in favour of the stepping stone hypothesis and the dead-end hypothesis (Filomena and Picchio, 2022). Causal evidence on the impact of atypical work remains rare, however.

The aim of this paper is to analyse the causal effect of re-employment through a temporary work agency (TWA) after job displacement (versus re-employment through a non-TWA job) on future labour market outcomes. We focus on TWA jobs because these jobs combine the two features underlying the stepping-stone versus dead-end hypotheses: TWA employment provides a route back into work, but may offer relatively low job quality. The main challenge in empirical research on this topic is that selection into atypical work is endogenous. Most existing studies either do not correct for unobserved heterogeneity or assume that it is time-invariant.[1] We use a novel two-step approach to tackle these methodological challenges. First, we build on the literature on job displacement by focusing on 79,971 workers that are displaced because of firm bankruptcy in the period 2012-2020. This ensures that all workers face an external shock, alleviating concerns with respect to selection into unemployment. Second, we build on the stylised fact that there is cyclicality in the prevalence of TWA jobs, which allows us to exploit differences in the timing of firm bankruptcies in an Instrumental Variable (IV)

[1] See Jahn and Rosholm (2018) for a comprehensive overview of the empirical literature.

approach.

Theoretically, several mechanisms have been proposed for both the stepping stone hypothesis and the dead-end hypothesis.[2] These mechanisms can largely be understood in terms of the counterfactual against which TWA employment is evaluated. Relative to remaining unemployed, TWA employment may facilitate human capital accumulation, provide a positive signal to prospective employers, expand professional networks, and improve information about suitable vacancies. Relative to obtaining regular employment, TWA jobs may involve less employer investment in training, poorer job matches, weaker accumulation of firm- or sector-specific human capital, and potentially negative signalling effects. Moreover, accepting a TWA job may reduce search intensity for regular employment. This distinction is reflected in the empirical literature, in which different studies compare TWA employment with unemployment, regular employment (including fixed-term or permanent contracts), or a broader mix of alternative labour market states.[3] Consistent with these different counterfactuals, estimated effects tend to be most favourable in studies comparing TWA employment with unemployment and least favourable in studies comparing TWA employment with regular employment. Our design identifies the effect of entering TWA employment as the first post-displacement job relative to obtaining a non-TWA first job, conditional on eventual re-employment. Accordingly, our estimates should be interpreted as the causal consequences of accepting a TWA job, rather than transitioning into another form of employment, as the initial route back into work following exogenous job loss.

The heterogeneity in findings in the empirical literature also reflects strongly differing identification approaches, covering mixed proportional hazard models, matching models, dose-

[2]For a discussion of these theoretical arguments; see, e.g., Segal and Sullivan (1997); Houseman et al. (2003); Ichino et al. (2008); Autor and Houseman (2010); Jahn et al. (2012); Jahn and Rosholm (2014). We note that we evaluate the effect of TWA work strictly from a worker perspective. From a firm-perspective, hiring temporary workers through an intermediary such as a TWA can be a flexible and effective way to adjust labour input and manage risk (Forde and Slater, 2005). In addition, TWA spells offer benefits to client firms in terms of screening (Ichino et al., 2008), which is especially attractive in countries with relatively high firing costs.

[3]Examples of studies (largely) comparing to the (initially) unemployed are Ichino et al. (2008); Heinrich et al. (2005); Hveem (2013). On the other hand, Autor and Houseman (2010); Forde and Slater (2005); Amuedo-Dorantes et al. (2008); Givord and Wilner (2015); Carrasco et al. (2024); Laß and Wooden (2019) compare TWA work to alternative contractual situations (fixed-term contracts, permanent contracts, direct hires), while other empirical approaches allow for mixed counterfactual groups that could either by unemployed or working in a different contractual arrangement (Jahn and Rosholm, 2013, 2014; Auray and Lepage-Saucier, 2021; De Graaf-Zijl et al., 2011).

response models and control function approaches. None of these approaches rely on exogenous variation and as such do not (fully) correct for time-variant unobserved heterogeneity (e.g. shocks to health, motivation, liquidity, preferences) which limits their causal interpretation. A rare exception is provided by Autor and Houseman (2010), who employ a judge leniency IV design to estimate the short-run causal effects of (non-profit) agency work on employment and earnings for welfare recipients in the United States. Whereas their study holds particular relevance for welfare-to-work settings, our study is especially relevant for the context of reintegration of displaced workers with stronger labour market attachment.

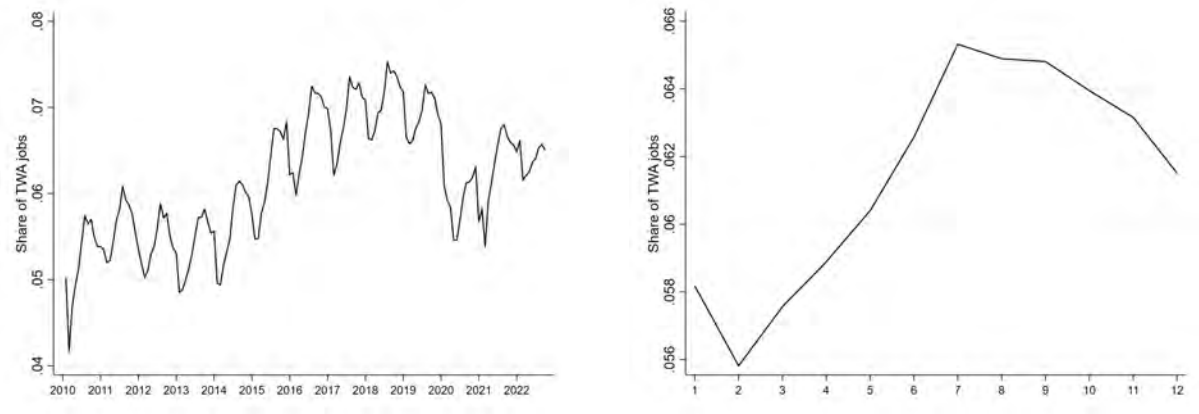
The intuition behind the IV approach of this study is provided in Figure 1. It presents the number of TWA jobs relative to all other jobs in the period 2010-2022 in the Netherlands, based on administrative microdata.[4] Figure 1a shows a strong seasonal pattern in jobs, which is also substantially stronger than for non-TWA jobs. When averaging data by calendar month (see Figure 1b), the share of TWA jobs is shown to be lowest in February and highest in July, with a particular strong rise in early summer and a particular strong drop throughout winter. The average share of TWA jobs is around 17% higher in July than in February, which represents around 86,000 more jobs.[5] The pattern in Figure 1 is used to satisfy the instrument relevance condition of the IV approach: job loss in summer and fall is linked to higher availability of TWA jobs, and thus to a higher probability of TWA employment compared to other employment. We note that the pattern observed in Figure 1b exhibits heterogeneity by (pre-bankruptcy) sector. In the main analysis, we also exploit this sector-specific variation by constructing the instrument by sector and month, while simultaneously controlling for sector fixed effects.

The instrument exogeneity condition requires that the timing of firm bankruptcy is not related to other determinants of labour market outcomes. Since we specifically focus on displacement due to firm bankruptcies, job loss is not driven by individual decision, but forced upon the employee. Additionally, we base the instrument on the timing of the bankruptcy, rather than the timing of job loss, as the exact timing of job loss around the bankruptcy date may be endogenous. Furthermore, we condition on firm sector and firm size. Some sectors may have

[4]Data from PIAAC shows that the share of TWA workers in the Netherlands is close to the OECD average (Diris and van Vliet, 2022).

[5]Appendix Figures A1 and A2 provide relative inflows and outflows by month.

Figure 1: Relative stock of TWA jobs among all employees



(a) By period

(b) By calendar month

Notes: The figure shows the share of employees in TWA jobs relative to all employees, per year and month (left-hand side) and by calendar month (right-hand side), based on Dutch administrative microdata.

higher bankruptcy propensities in certain months, e.g. when economic activity in such sectors is seasonal. Our identifying assumption is thus that, conditional on firm sector-size combination, the timing of a bankruptcy is exogenous to the determinants of future labour market outcomes of displaced workers. Put differently, we effectively compare, e.g., a worker employed in a medium-sized retail firm that goes bankrupt in February with a worker employed in a medium-sized retail firm that goes bankrupt in July. Projecting that the latter worker faces a higher labour demand from temporary work agencies, this worker is more likely to start a TWA job. Assuming that the two workers are otherwise similar, we can attribute any differences in post-bankruptcy labour market outcomes to the treatment, i.e. taking a TWA job after job loss caused by firm bankruptcy.

Applying this approach, we find that an initial post-bankruptcy spell of TWA employment relative to non-TWA employment negatively affects short-term employment and hours worked. These negative effects dissipate in full after around two and a half years post-bankruptcy. In contrast, hourly wages and the likelihood of a permanent contract are negatively affected by TWA employment in both the short and long run. Transitioning to a TWA job leads on average to a 20% lower hourly wage and a 30 percentage points lower likelihood of obtaining a permanent contract, across the period of 12 months to 84 months since firm bankruptcy. While the vast majority of treated individuals exits TWA jobs in the longer run, there remains a negative

and sustained impact on wages, job security, job tenure, and probability of working in the same sector as before the bankruptcy. The results prove robust to a range of specifications designed to account for seasonal and cyclical labour market conditions, including calendar-month fixed effects, macroeconomic controls, more granular sector-by-time fixed effects, and sector-month job quality indicators.

The main contribution of this study to the literature on the stepping stone and dead-end effects is to provide unique causal evidence on the effect of TWA employment after forced displacement. Another important strength of this study lies in its rich data. We use Dutch administrative monthly microdata from 2010 up to 2022. These data allow us to administratively identify all TWA workers in the Netherlands over this period. This contrasts with many other studies in this literature, which often have to rely on self-reported employment status based on (small-sample) survey data, or have to combine various sources of atypical work that strongly differ in their degree of precariousness. Moreover, the data allow us to estimate a wide scope of relevant labour market outcomes, including employment, working hours, hourly wages, monthly earnings and job security, over a period of seven years post-bankruptcy.

Our findings also provide insights into two related strands of labour economics literature. First, we contribute to the literature on job matching and wage dynamics by providing causal evidence on how the type of initial post-displacement employment shapes subsequent labour market trajectories. In particular, our study complements job- and wage-ladder models (e.g., Postel-Vinay and Robin (2002); Lentz et al. (2023)), which emphasize job mobility, sorting, and wage progression over the employment career. By comparing workers who enter TWA employment with those who transition directly into non-TWA jobs after displacement, we show how initial job placement can have persistent consequences for subsequent wages and employment outcomes.

Second, we contribute to research on job loss. This literature typically examines the consequences of mass lay-offs and firm bankruptcies for employment, paid working hours, hourly wages, and earnings. Previous research documents substantial heterogeneity in the effects of job loss across worker groups, including differences by age (Hijzen et al., 2010; Ichino et al.,

2017), gender and family circumstances (Meekes and Hassink, 2022; Illing et al., 2024), and migrant status (Bratsberg et al., 2018; Balgova and Illing, 2024). By analysing labour market trajectories after job loss by exogenously determined TWA employment status, we identify the type of first post-displacement job as an additional source of heterogeneity in the long-run consequences of job loss.

This paper is organised as follows. Section 2 discusses the data sources and the selected sample for the analysis. Section 3 discusses the methodological approach, while Section 4 discusses the results of the empirical analysis. Section 5 concludes.

2 Data

We use Dutch administrative data from Statistics Netherlands over the period 2010-2022. The data include the entire population of the Netherlands, and cover information on individual demographic characteristics and educational level, job-spells (paid working hours, monthly wage, contract type and job tenure), firm size, and economic sector. For our independent variable of interest, we use information on the contract type to distinguish between TWA employment and non-TWA employment as the first job after firm bankruptcy, where TWA employment contains staffing, seconded and payrolling employees.[6]

Using our 2010-2022 data, we analyse incidences of firm bankruptcies that occurred in the period 2012 to 2020 to ensure that we have labour market information for at least 24 months before and at least 24 months after bankruptcy. We use precise information on the date of the firm bankruptcy and the date of job displacement. Displaced employees are defined as those who lost their job between 6 months before and 12 months after the bankruptcy date. This time window is chosen to include early leavers but also those who remain on the sinking ship, following Schwerdt (2011).[7] Consistent with the job loss literature, we limit the sample in several ways based on firm-level information and individual-level information; see, e.g.,

[6]Staffing workers and seconded workers are hired by a TWA and assigned to client firms for work. Payrolling workers are recruited by a client firm but formally employed by the TWA, which handles the employment contract and administration.

[7]Appendix Figure A4 shows the distribution of the difference between bankruptcy and job loss. It shows that about 75% of all workers lose their job in the window from one month before until one month after bankruptcy.

Schwerdt (2011); Meekes and Hassink (2022).

At the firm level, we only consider bankrupt firms with 10 or more employees, measured 12 months before the firm bankruptcy.[8] For firms with relatively few employees, one may be concerned that workers can influence the exact timing of the bankruptcy. In addition, we exclude bankrupt firms that still operate with at least half of their pre-bankruptcy work force one year after the court bankruptcy date. As firms may declare bankruptcy but still operate for a substantial amount of time, the actual date of bankruptcy has little relevance in cases where employees are still employed. We also exclude bankrupt firms who are engaged in a merger or acquisition, approximated by the condition that at least 40% of the bankrupt firm's employees switch to the same new employer.

At the individual level, we only consider individuals who are employed for at least a full year at the time of firm bankruptcy, work on a permanent contract and work for at least 20 hours per week (measured twelve months before bankruptcy). This ensures that our sample of analysis contains individuals with a strong attachment to the labour market that are involuntarily displaced by the firm bankruptcy. In addition, we exclude individuals who experience two or more bankruptcies in the period 2012-2020. We also limit the sample to workers aged between 21 and 60 at the time of bankruptcy.

These selections are applied as per order discussed below and reduce the number of displaced workers as follows. There are a total of 461,342 workers employed with a bankrupt firm within the [-6,12] months time-frame, when considering all bankruptcies between 2012 and 2020. Excluding bankruptcies from firms with less than 10 workers reduces the group by 18.3%. Another 16.4% works at firms that continue to operate more than 50% for at least another year and are excluded. Excluding mergers further reduces the sample by 3.7%, not considering those with multiple bankruptcies by 3.5%, only considering those with permanent contracts by 48.1%, only considering those with at least one year of tenure at the bankrupt firm by 18.6%, excluding those with working hours below 20 hours by 19.3%, and excluding those with any

[8]Firm size is measured categorically: under 10, 10–19, 20-49, 50–99, 100–149, 150-199, 200-249, 250-499, 500-999, 1,000-1,999 and equal to or larger than 2,000 employed workers. This is based on information of the preceding calendar year.

TWA work in the year before bankruptcy by 0.7%. Another 14.4% of the sample is never re-employed as a dependent employee and therefore cannot be assigned a treatment status.[9] Finally, another 5.4% of the sample is excluded because we only consider those aged 21-60. That leaves us with an analysis sample of 79,971 employees. The analysis sample contains substantially more workers in manufacturing and the private sector, and less in the service sector and public sector, since bankruptcies are less common there.[10] Construction workers are especially overrepresented.

The 79,971 displaced workers are spread across a total of 4,312 bankrupt firms. Table 1 shows summary statistics for all displaced workers together, as well as separate means for individuals whose first job after displacement is a TWA job ($TWA = 1$) and those who transition to other job types ($TWA = 0$). All summary statistics are measured in the twelfth month before firm bankruptcy. Table 1 shows that those who transition to a TWA job differ statistically significantly on all observed characteristics from those who transition to other jobs. They are more likely to be older, to be male, to have a migrant background, to be low educated, and to have lower wages and more working hours. These results are consistent with the key identification challenge of endogenous selection into TWA jobs, also when we limit the sample to displaced workers. Consequently, we apply a novel IV approach to estimate the causal effect of TWA employment on labour market outcomes.

[9] We identify no statistically significant relation between our instrument and ever being re-employed, see Appendix Table B5.

[10] The distribution of the sample across the eight defined (pre-bankruptcy) sectors is provided in Appendix Figure A5, which also provides a comparison to the entire population of employees. A comparison to the full population of employees on other characteristics is provided in Appendix A1.

Table 1: Summary statistics

	Mean	Std. dev.	TWA=0	TWA=1	Diff	p-value
Age	42.09	10.28	41.87	43.25	1.38	0.000
Female	0.358	0.479	0.374	0.275	-0.098	0.000
Migrant	0.168	0.374	0.159	0.213	0.054	0.000
High education level	0.254	0.435	0.275	0.142	-0.133	0.000
Low education level	0.152	0.359	0.136	0.238	0.102	0.000
Hourly wage	17.41	7.08	17.79	15.44	-2.34	0.000
Hours worked	154.85	32.24	154.34	157.49	3.15	0.000

Notes: Summary statistics, measured twelve months before firm bankruptcy (bankruptcy occurring in the period from 2012 to 2020), based on 79,971 displaced workers. TWA job is defined based on the first job after job loss due to firm bankruptcy. The number of individuals who become re-employed at a non-TWA equals 67,160 and the number of individuals who become re-employed at a TWA firm equals 12,811. The number of unique firms equals 4,312.

3 Methodology

3.1 IV model

Our goal is to estimate the causal effect of TWA employment after job loss (TWA_i) on future labour market outcomes ($Y_{i\tau}$), for each period τ measured relative to the bankruptcy (month-by-year) period t_i^B, where bankruptcies can occur between January 2012 and December 2020. A generic model is shown in (1), where Y represents employment, hours worked, hourly wage, or monthly earnings.

$$Y_{i\tau} = \alpha_\tau + \delta_\tau TWA_i + X_i\beta_\tau + F_f + \kappa_\tau t_{i\tau} + \phi_y + \varepsilon_{i\tau} \tag{1}$$

$i \in 1,2,...,N$; $\tau \in -24,-23,.....,+84$; $f \in 1,2,...,F$; $t \in 1,2,...,156$; $y \in 2010,2011,...,2022$

where i, τ, f and t denote the employee, event time period, firm sector-size combination, and month-by-year period, respectively, and $t_{i\tau} = t_i^B + \tau$. The parameters of interest are δ_τ, which represent the effect of having a TWA job relative to a non-TWA job as the first job after job loss, on a given outcome variable, estimated for each event time period τ in separate regressions. The vector of individual time-invariant controls X_i includes gender, migrant background, education level and the worker's age at firm bankruptcy. We control for characteristics of the

bankrupt firm, captured by interactions between firm size and sector (F_f). These are measured 12 months before the bankruptcy of the corresponding firm and thus also time-invariant. The sector classification is a reduced version of the one-digit ISIC. Since our instrument is calculated separately by both bankruptcy calendar month and sector, we categorize sectors such that they are of comparable size.[11] To capture natural growth in labour income as well as business cycle effects, we include calendar year fixed effects (ϕ_y) and a linear monthly time trend ($t_{i\tau}$) from January 2010 to December 2022 in which the labour market outcome is observed. Standard errors are clustered at the level of the bankrupt firm, to allow for arbitrary correlation in the error terms among workers displaced from the same bankrupt firm.

The sample on which we estimate equation (1) can change across τ. For one because post-event windows vary depending on t_i^B and secondly because some outcome variables are conditional on employment. Thus, changes in the estimated effects across τ reflect both the evolution of treatment effects within the sample and changes in the composition of the sample. Appendix Figure A3 provides an overview of sample size by τ and type of outcome.

To overcome the key identification problem of endogenous TWA employment, we instrument the variable TWA_i with the (left-out) mean TWA transition rate per combination of bankruptcy month and sector of the bankrupt firm.[12] For each sector-by-month cell s,m, and letting s_j and m_j denote the sector and bankruptcy month of worker j's bankrupt firm, we define:

$$A_{sm} = \sum_j TWA_j \cdot \mathbf{1}\{s_j = s, m_j = m\},$$
$$B_{sm} = \sum_j \mathbf{1}\{s_j = s, m_j = m\},$$

so that A_{sm} denotes the total number of workers in cell (s,m) who enter TWA employment, and B_{sm} denotes the total number of displaced workers in that cell. The instrument for worker i is

[11] In addition to combining adjacent ISIC-sectors, we split the ISIC sector G in Wholesale and Retail, as it comprises near 30% of the sample. See Appendix Figure A5 for more details on the classifications. Robustness analysis shows that the results are consistent for alternative classifications.

[12] The minimum number of unique bankrupt firms for a given calendar month and sector equals 11. The maximum equals 89.

then given by:

$$\overline{TWA}_{-i,sm} = \frac{A_{sm} - TWA_i}{B_{sm} - 1}. \tag{2}$$

This approach pools workers across all firms within a given sector-by-month cell and excludes the focal worker to avoid mechanical correlation between the instrument and the treatment variable. We thereby exploit that the seasonal pattern of demand for the TWA job is heterogeneous by (origin) sector, thus increasing first stage power. We specifically exploit the variation across *bankruptcy* month, rather than displacement month or re-employment month, because the latter is under the influence of the employee. Since we simultaneously condition on (origin) sector and firm size fixed effects, we effectively exploit that for a bankrupt firm of given size and sector, the probability of workers transitioning to a TWA job is dependent on the month the firm goes bankrupt. The first-stage of the IV model is shown in (3):

$$TWA_i = \gamma + \rho \overline{TWA}_{-i,sm} + X_i\theta + F_f + \lambda t_{i\tau} + \phi_y + \eta_i \tag{3}$$

Note that Equation (3) presents the generic first-stage specification. As stated above, exact samples can deviate from the baseline 79,971 depending on τ and type of outcome. When that applies, first stage are re-estimated for each event time using the corresponding analysis sample, such that the first and second stages are estimated on identical samples. Standard errors are clustered at the level of the bankrupt firm.

3.2 Instrument relevance

Figure 1 has shown that there is a strong seasonality of TWA jobs in the Dutch labour market for the entire population of employees in the Netherlands. However, the seasonality in Figure 1 could be driven by, e.g., student jobs and temporary migrant jobs, which are rare in our sample of displaced workers with a strong attachment to the labour market. Appendix Figure A6 shows, for our sample of displaced workers, the share of TWA transitions by calendar month in which the new job is started. The figure shows a pattern similar to that in Figure 1: TWA transitions are low at the beginning of the calendar year, start to increase in February after

which they peak in July, and steadily decline again until the end of the year. The results in Figure A6 confirm that our estimation sample is subject to the same within-year seasonality in TWA employment as is shown in Figure 1.

However, we argue that it is the timing of the bankruptcy, not the timing of re-employment, that is exogenous. Bankruptcy dates may be earlier than re-employment dates since workers may leave before the official court bankruptcy or they may be later than re-employment rates because it takes time to find new work. Appendix Figure A7 shows that, on average, displaced workers become re-employed three months after firm bankruptcy. Thus, we would expect that the pattern of TWA transition rates across bankruptcy months is shifted more to the left compared to Figure A6, and that the pattern is more volatile given the dispersion shown in Figure A7. Figure 2 shows the TWA transition pattern by month and quarter of the bankruptcy. The pattern indeed shows a shift of around three months to the left: with a peak in the 1st quarter, and the strongest increase at the turn of the year. Compared to the re-employment pattern in Figure A6, Figure 2 is indeed also more noisy, since the timing between displacement and re-employment is dispersed.[13]

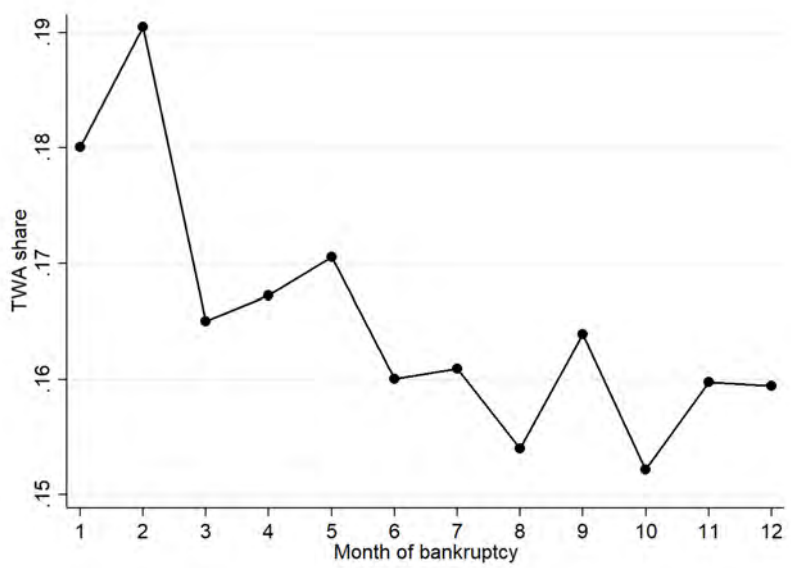
Another source of volatility in Figure 2 is the fact that monthly patterns differ by sector. The patterns by sector can be observed in Appendix Figures A8 (by re-employment timing) and A9 (by bankruptcy timing). Because of this heterogeneity, we choose to construct the instrument by month and by sector to increase the first-stage power (while still controlling for sector fixed effects).

Table 2 shows the first stage estimates and test statistics for this approach. It shows that the instrument is strongly predictive of making a TWA transition after job displacement.[14] The statistical significance weakens somewhat when we include controls for background characteristics, sector fixed effects, and sector-size fixed effects. In the preferred specification, the first-stage

[13]Results for an alternative approach that constructs the instrument by month of re-employment are in the Appendix. These estimates are qualitatively similar (and somewhat more precise, as expected given the discussion above).

[14]Additional analysis shows a significant correlation between mean TWA transition rates by sector in period τ and period $\tau - 12$ (correlation of 0.31 with p=0.001 and 144 observations). This provides additional evidence that the instrument is not spurious and captures consistent seasonal patterns.

Figure 2: TWA transition rate by timing of bankruptcy



Notes: TWA share is defined for the 79,971 displaced workers as the total number of individuals starting TWA employment as their first job after firm bankruptcy relative to all individuals experiencing firm bankruptcy. Month of bankruptcy represents the calendar month.

F-statistic equals 22.8, indicating a relevant instrument.[15] This is close to, but slightly below, the corresponding 10% critical value of 23.1 for limiting size distortions (Olea and Pflueger, 2013). We therefore complement the conventional IV estimates with weak-identification-robust Anderson–Rubin confidence intervals, which are discussed in Section 4.2.[16]

3.3 Instrument exogeneity

The exogeneity assumption is that the timing of the bankruptcy affects future labour market outcomes only through the probability of TWA transitions. We note that all models condition on sector fixed effects, firm size, and their interactions, with respect to the bankrupt firm. We thus do not require that bankruptcies are completely random events, but that their timing does not correlate with other determinants of future labour market outcomes, conditional on observable individual characteristics and firm characteristics.

Appendix Figures A10 through A13 show the distribution of bankruptcy months and weeks in the full sample and by sector, both unweighted and weighted by firm size. While a uniform

[15] We note that the conventional Kleibergen–Paap rk Wald F-statistic, and the heteroskedasticity- and cluster-robust Olea and Pflueger (2013) effective F-statistic are identical here.

[16] See also Lee et al. (2022) on weak-identification-robust inference in applied IV settings.

Table 2: First stage coefficients: left-out mean TWA transition rate effect on TWA employment

	(1)	(2)	(3)	(4)
$\overline{TWA_{-i}}$	0.969***	0.835***	0.891***	0.793***
	(0.065)	(0.078)	(0.169)	(0.166)
Age		-0.004***	-0.004***	-0.004***
		(0.001)	(0.001)	(0.001)
Age^2		0.000***	0.000***	0.000***
		(0.000)	(0.000)	(0.000)
Female		-0.008	-0.013*	-0.010
		(0.009)	(0.007)	(0.007)
Migrant		0.062***	0.061***	0.061***
		(0.006)	(0.006)	(0.006)
Low education level		0.068***	0.068***	0.067***
		(0.006)	(0.006)	(0.005)
High education level		-0.055***	-0.057***	-0.059***
		(0.004)	(0.004)	(0.004)
Sector: Construction			-0.010	-0.053**
			(0.013)	(0.026)
Sector: Wholesale			-0.006	0.006
			(0.019)	(0.027)
Sector: Retail			0.012	0.041
			(0.031)	(0.038)
Sector: Food & transport			-0.011	-0.007
			(0.022)	(0.033)
Sector: Commercial services			0.014	0.014
			(0.026)	(0.023)
Sector: Business services			0.008	-0.023
			(0.023)	(0.030)
Sector: Public services			0.015	-0.006
			(0.031)	(0.033)
Sector-size interactions	No	No	No	Yes
F-statistic	223.64	113.55	27.85	22.80
N	79,971	79,971	79,971	79,971

Notes: The table shows first stage estimates (equation 3), for different inclusions of control variables. Sector-size interactions refer to interactions between the sector that the bankrupt firm belongs to and the size (number of employees at $\tau = -12$) of the bankrupt firm. Reference category for sectors is Industry. Age measures the age at the moment of firm bankruptcy. F-statistic is based on Wald test on the instrument only. Standard errors are robust and corrected for clustering at the level of the bankrupt firm.

distribution of bankruptcy dates is neither necessary nor sufficient for instrument exogeneity, the figure provides reassurance that bankruptcies are not concentrated in particular periods of the year. Moreover, we identify neither statistically significant differences in the average bankruptcy timing across sectors nor statistically significant correlations between bankruptcy timing and worker characteristics. This is supportive evidence that the variation in TWA transition rates across bankruptcy months reflects dynamics in the demand for TWA workers, rather than differences in characteristics across displaced workers with different bankruptcy times.

A more formal way to test instrument validity is to regress the instrument on observable characteristics. A strong advantage of our data is that we can test this both for individual traits and for pre-treatment labour market outcomes. Results for this quasi-experimental test are presented in Table 3. Pre-treatment labour market outcomes are taken 12 months before firm bankruptcy; results are consistent for other pre-treatment periods (monthly pre-treatment estimates will be shown in all the graphical portrayals of the treatment effects for all pre-treatment periods in Section 4). Table 3 shows that, excluding firm size and sector controls, higher TWA transition rates are associated with a higher likelihood of being male, native, lower educated, and working more hours. These correlations disappear once sector fixed effects are included, indicating that they are driven by differences in workforce composition across sectors rather than by within-sector seasonal variation. The results remain unchanged when sector-by-size fixed effects are added, and a joint significance test fails to reject the null that the observable characteristics are unrelated to the instrument. We further examine whether the instrument captures broader macroeconomic conditions by including contemporaneous measures of GDP, the unemployment rate, and the vacancy rate in the month of bankruptcy. None of these variables is significantly associated with the instrument. Taken together, these findings provide support for the identifying assumption that, conditional on our fixed effects, the instrument is orthogonal to other determinants of labour market outcomes. At the same time, these results do not rule out that the instrument captures within-sector seasonal labour market conditions (such as changes in the quality of available job opportunities) that independently affect subsequent outcomes. We investigate this potential threat to the exclusion restriction in Section 4.5.

In the preferred specification, we rely on the controls specified in column (3) of Table 3. The

Table 3: Quasi-randomization test: regression of the instrument on covariates

	(1)	(2)	(3)	(4)
Age	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Age^2	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Female	-0.034***	0.001	0.001	0.001
	(0.003)	(0.002)	(0.002)	(0.001)
Migrant	-0.005***	0.001	0.001	0.001
	(0.002)	(0.001)	(0.001)	(0.001)
Low education level	0.007***	0.002*	0.002	0.002*
	(0.002)	(0.001)	(0.001)	(0.001)
High education level	-0.021***	0.001	0.001	0.001
	(0.002)	(0.001)	(0.001)	(0.001)
Lagged wage	0.005	0.001	-0.001	-0.001
	(0.004)	(0.002)	(0.002)	(0.002)
Lagged hours	0.023**	0.000	0.002	0.002
	(0.010)	(0.004)	(0.003)	(0.003)
Sector: Construction		-0.033***	-0.066***	-0.058***
		(0.005)	(0.014)	(0.010)
Sector: Wholesale		-0.090***	-0.088***	-0.082***
		(0.006)	(0.019)	(0.019)
Sector: Retail		-0.098***	-0.088***	-0.091***
		(0.009)	(0.013)	(0.011)
Sector: Food & transport		-0.103***	-0.108***	-0.113***
		-0.014)	(0.009)	(0.010)
Sector: Commercial services		-0.139***	-0.110***	-0.105***
		(0.013)	(0.008)	(0.009)
Sector: Business services		-0.119***	-0.120***	-0.123***
		(0.003)	(0.006)	(0.007)
Sector: Public services		-0.184***	-0.191***	-0.189***
		(0.006)	(0.009)	(0.008)
GDP per capita				0.000
				(0.000)
Unemployment rate				0.009
				(0.006)
Vacancy rate				0.001
				(0.002)
Sector-by-size FE	No	No	Yes	Yes
Joint F-test (excl. FE)	0.000	0.151	0.403	0.294
N	79,971	79,971	79,971	79,971

Notes: Reference category for sectors is Industry. Sector-size interactions refer to interactions between the sector that the bankrupt firm belongs to and the size (measured in number of employees) of the bankrupt firm. The joint tests are consistently run on the first eight variables listed in the table. "Joint F-test" reports p-value of Wald test on all (eight) covariates, excluding the sector and sector-by-size fixed effects. Standard errors are robust and corrected for clustering at the level of the bankrupt firm.

macro-economic controls are excluded from the baseline IV model, because they are not pre-determined and potentially endogenous to the treatment. The appendix shows results when we also include the macro-economic controls for sensitivity analysis.

3.4 Instrument monotonicity

We finally address instrument monotonicity. This requires that when the mean TWA transition rate increases for a certain calendar month-sector cell, the treatment status of the displaced worker cannot change from TWA transition to non-TWA transition. The monotonicity assumption provides another argument to construct the instrument separately by sector, since different sectors can have different seasonal patterns. Appendix Table B1 shows first-stage estimates separately by different demographic groups. The absence of negative relations across subgroups provides evidence in favour of the monotonicity assumption; see Imbens and Angrist (1994).

At the same time, Table B1 shows that compliance is stronger for men versus women, medium educated versus highly educated, and in manufacturing versus service sector. This implies that the local average treatment effect will be predominantly reflective of male, medium educated, manufacturing employees. Moreover, Table B1 shows that the F-stat for women falls below conventional thresholds. Heterogeneity analysis in Section 5 will report results by gender, but these should be interpreted with caution.

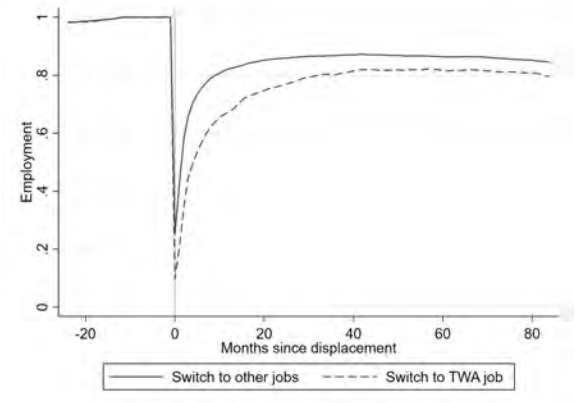
4 Results

4.1 Descriptive evidence on labour market outcomes

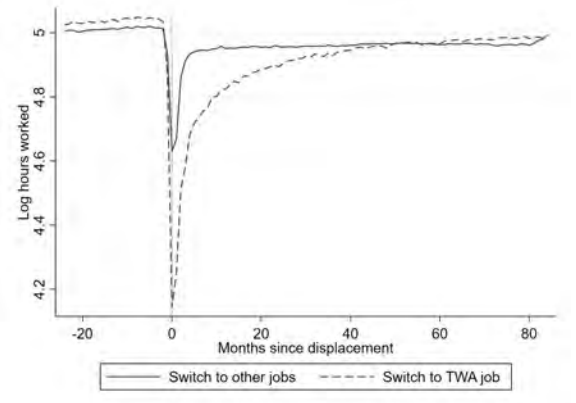
We first descriptively analyse how labour market outcomes develop before and after job displacement, for displaced workers who (first) transition to a TWA job and those who do not. Results can be observed in Figure 3. The estimated models mimic a difference-in-differences (DiD) framework, in which we can observe how initial differences between the two groups

respond to job displacement.[17] Observing how the difference between the groups changes corrects for baseline differences in the outcome variable, but still assumes that these capture all of the unobserved heterogeneity between TWA and non-TWA entrants. The IV approach relaxes this assumption by using the exogenous variation in bankruptcy month to isolate the TWA-employment impact.

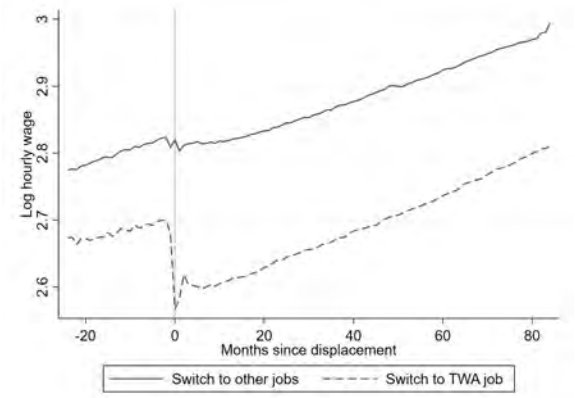
Figure 3: Difference in labour market outcome means: TWA jobs vs. other jobs



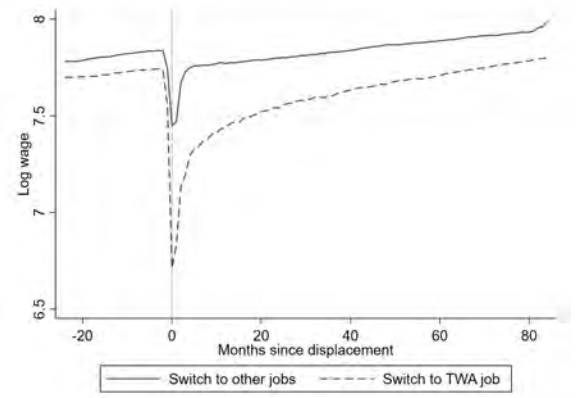
(a) Employment



(b) Log hours worked



(c) Log hourly wage



(d) Log earnings

Notes: The figure shows averaged labour market outcomes for the sample of displaced workers, separately for those whose first job after job loss due to firm bankruptcy was a TWA job and those whose first job after job loss due to firm bankruptcy was any other job. Outcomes are averaged by month since displacement.

The descriptive results in Figure 3 show that there are baseline differences between the two groups. They are not apparent for employment in the two years before displacement, because individuals are included in our sample of analysis if they have a pre-treatment employment period of at least one year. Baseline differences in working hours are small, and slightly in

[17] Estimates from a formal difference-in-difference model can be observed in Appendix C.

favour of TWA entrants before displacement. TWA entrants work at substantially lower hourly wages before job loss, which also results in substantially lower monthly earnings.

After job loss, labour market outcomes initially worsen for both groups, but consistently more so for TWA entrants. The employment difference is around 10 percentage points in favour of non-TWA entrants just after displacement, widens in the period between 6 and 12 months after displacement, and somewhat (but not fully) converges afterward. TWA entrants also exhibit a substantially stronger negative shock to hours worked, but quickly converge in later periods. They catch up after around three years, and work slightly more hours than non-TWA entrants at the end of the time frame. Hourly wages initially show a stronger negative shock, but no convergence. The 10% difference in hourly wages before treatment doubles at displacement and stays at 20% throughout the period. The pattern for earnings shows a slight but not full convergence, as it combines the dynamics of figures (b) and (c).

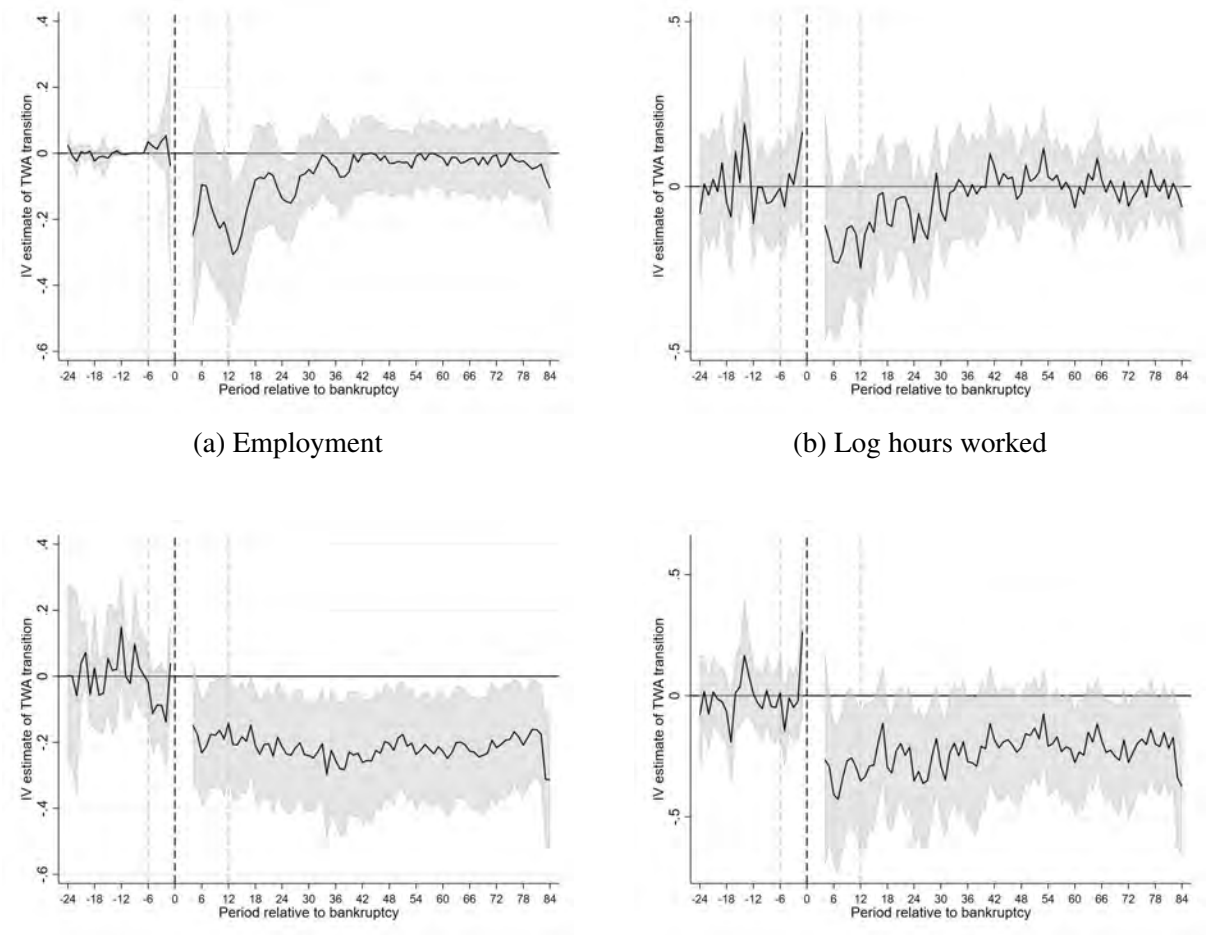
4.2 IV analysis

Figure 4 plots the IV estimates across event time for the four main labour market outcomes. Reassuringly, all pre-treatment coefficients are statistically insignificant, providing further support for the validity of the instrument. The pre-treatment period effectively extends until $\tau = -6$, after which workers gradually begin to leave the bankrupt firm in anticipation of, or following, bankruptcy. Consequently, the estimates between $\tau = -6$ and $\tau = +12$ should be interpreted with caution, as workers transition to new jobs at different points in time and the timing of displacement and re-employment may itself be influenced by the treatment. After approximately one year, the vast majority of workers has entered post-displacement employment, making the interpretation of the estimates more straightforward.

The estimates for $\tau = 0$ to $\tau = +3$ deserve particular caution. During this period, many workers are not yet re-employed. This substantially reduces estimation samples for outcomes observed conditional on employment (see also Appendix Figure A3). Moreover, treatment status has not yet fully materialised, as many workers have not yet entered their first post-displacement job, further reducing the precision of the estimates. As a result, the corresponding confidence

intervals are very wide. To preserve the readability of the figures, these event times are omitted from the main results and reported separately in Appendix Figure B2.[18]

Figure 4: IV estimates of the effect of post-bankruptcy TWA employment



(a) Employment

(b) Log hours worked

(c) Log hourly wage

(d) Log earnings

Notes: The figure shows effects (point estimates and confidence intervals) of transitioning to a TWA job after job loss because of firm bankruptcy on labour market outcomes, based on IV estimation. Effects are estimated by month relative to bankruptcy. Analysis of outcomes is conditional on employment (other than employment itself). Standard errors are robust and corrected for clustering at the level of the bankrupt firm. The 95% confidence interval is provided. Results for $\tau = 0$ to $\tau = 3$ are not shown because of the dominant size of confidence intervals. They can be found in Appendix Figure B2.

Panel (a) shows that switching to a TWA job negatively affects employment in the short run. Estimates are more negative between $\tau = 6$ and $\tau = 12$, and are statistically significant around 12 months since bankruptcy. This may be indicative of the lower security that TWA jobs provide, leading to short employment spells in their first job after displacement. These negative employment effects decrease in the second year since bankruptcy, although they are still neg-

[18] We report an overview of first stage F-statistics in Appendix Figure B1.

ative, and statistically significant in some periods around the two-year mark. Point estimates converge to near-zero after around three years, after which they stabilize.

The pattern of results is similar for hours worked. Initial point estimates are substantially negative and tend to be at the margin of traditional statistical significance thresholds. After around three years, convergence is achieved and point estimates are at or slightly above zero, and consistently statistically insignificant. This relatively small initial impact and near-zero long-run impact is likely related to the fact that many treated workers only shortly remain in a TWA job but also to the fact that TWA jobs in the Netherlands often are full time.[19]

IV estimates of TWA jobs on hourly wages are consistently negative and statistically significant after bankruptcy. They indicate that becoming re-employed in a TWA job versus a non-TWA job results in a 20% loss in wages just after bankruptcy and still so seven years later. The results for monthly earnings in panel (d) essentially mimic those for hourly wages (although somewhat less precise), which is consistent with the near-zero estimates for hours worked. The estimates for the six months before bankruptcy, in which workers can already transition, tend to be less precise. Point estimates for these periods tend to be positive for employment and hours worked, and negative for hourly wages, but these are at best marginally significant.

In Table 4, we summarize the graphical results by averaging outcomes for the period from $\tau = +12$ until $\tau = +84$, and then applying the IV model. They confirm the statistically insignificant results for employment and hours worked, and the 20% decrease in hourly wages which is statistically significant.[20]

Compared to the results from the DiD analysis of Appendix C, the IV estimates are slightly larger although falling within the confidence interval of the DiD results. If we assume that selection on unobservables operates in the same direction as the selection on observables identified in Table 1, the DiD results for hourly wages would be negatively biased.[21] This is sug-

[19] On average, half of all TWA workers in the Netherlands work 36 hours or more (De Wit et al., 2018).

[20] We report the Anderson–Rubin intervals in Appendix Table B8, which lead to the same substantive conclusions as the conventional inference, providing additional reassurance that the results are not driven by weak-instrument concerns. The t-stat of the hourly wage effect (t=2.57) is also above the alternative critical value for 5% significance from Lee et al. (2022).

[21] Estimation of the pre-trends for hourly wages in the DiD analysis in Appendix C also suggest a small negative bias.

Table 4: IV estimates of effect of TWA job: averaged outcomes

Main outcomes				
	Employment	Log hours worked	Log hourly wage	Log monthly earnings
TWA effect	-0.049	0.026	-0.201**	-0.180*
	(0.049)	(0.063)	(0.078)	(0.104)
Supplementary outcomes				
	TWA job	Permanent contract	Origin sector	Job tenure
TWA effect	0.329***	-0.305**	-0.451**	-9.93***
	(0.080)	(0.124)	(0.202)	(3.10)

Notes: The table shows estimates from the IV model for the effect of transitioning to a TWA job on averaged labour market outcomes. Outcomes are averaged across $\tau = +12$ and $\tau = +84$. "Origin sector" measures whether the job held in period t is in the same sector as the sector of the bankrupt firm. "Tenure" measures tenure in period t for the job currently held, in months. Sample size equals 79,971 for employment and 78,678 for all other outcomes, which are conditional on employment. Standard errors are robust and corrected for clustering at the level of the bankrupt firm.

gestive evidence that the (unbiased) average treatment effect is somewhat smaller than the local average treatment effect.

4.3 Additional outcomes and dynamics

To better understand the channels through which initial TWA employment affects longer-term labour market outcomes, we examine four complementary outcomes. These outcomes serve different purposes. First, the probability of holding a permanent contract is analysed as a labour market outcome that is directly related to the stepping-stone versus dead-end debate. Second, current TWA employment is assessed to characterize the persistence of TWA careers and to analyse whether the treatment primarily reflects temporary entry into, or continued attachment to, agency work. Third, working in the pre-displacement sector provides evidence on sectoral mobility and the potential role of sector-specific human capital. Fourth, tenure at the current employer offers insight into job stability and the accumulation of firm-specific human capital. Thereby, these supplementary outcomes give a more comprehensive overview of the broader impact of TWA employment after forced displacement. Estimates by event time τ for these complementary outcomes can be observed in Appendix Figure B3, while averaged effects are

provided in the lower panel of Table 4.

The first three of these outcomes show very strong short-term effects by construction, since contract status and sector are directly related to TWA employment. TWA employment leads to an initial reduction of around 50 percentage points in the probability of obtaining a permanent contract. The negative effect gradually decreases over time, but is still statistically significant seven years after bankruptcy, and amounts to around 20 percentage points. These effect sizes are similar to those identified by Amuedo-Dorantes et al. (2008). For holding a TWA job, effects decrease rapidly, but are still statistically significant after seven years, and amount to around 10-15 percentage points.[22] Since TWA jobs are typically short-lived, this implies that there is substantial exit and re-entry into TWA work for individuals who started a TWA employment spell as their first job after job displacement.

The pattern for working in the sector of origin is similar. Effect sizes tend to be larger than for holding a TWA job, suggesting that also those who leave a TWA job are less likely to work in the origin sector (although these effect sizes are not statistically distinguishable given the substantial confidence intervals). Per-period estimates for tenure at the current job indicate that TWA employment consistently leads to lower job tenure. The average effect amounts to around 10 months lower tenure (Table 4). Relatedly, Appendix Table B2 shows that the treatment leads to a higher number of total jobs post-treatment, and a shorter length of not only the first job but also subsequent jobs. These results are in line with findings of Autor and Houseman (2010), who find greater job churn (lower job tenure and more multiple-job holdings) from placement in temporary jobs. They suggest that part of the negative treatment effect on wages may be through less sector-specific and job-specific human capital.

Furthermore, we descriptively examine whether the negative wage patterns identified in Section 4.2 are concentrated among workers who remain in TWA employment ("stayers") or also persist among workers who subsequently transition out of TWA employment ("leavers"). This distinction is informative because persistent effects among leavers would suggest that the consequences of TWA employment extend beyond the duration of the TWA spell itself. The com-

[22]The initial effect does not equal 1 since the timing of displacement is spread across a period of 18 months around bankruptcy.

parison provides descriptive evidence on whether negative wage effects persist after workers exit TWA employment but should not be interpreted causally, as leaving TWA employment is likely selective.[23] Moreover, the split samples are relatively small, resulting in imprecise estimates. Estimates are shown in Appendix Figure B4. The negative wage effects are substantially larger among stayers. Still, the point estimates for leavers remain consistently negative and are statistically significant in some periods. Unless leavers are negatively selected relative to stayers, this pattern suggests that the consequences of initial TWA employment are not limited to workers who remain in TWA jobs, but may extend to their subsequent labour market trajectories.[24]

4.4 Heterogeneity analysis

We further analyse potential heterogeneity in treatment effects. Previous literature on stepping stone effects generally finds that effects can differ strongly across personal characteristics. A common observation is that the effects of TWA employment are more favourable for those with weaker ties to the labour market, such as the long-term unemployed, migrants, low-skilled workers, younger and older workers (compared to middle-aged), or those with few (pre-treatment) working hours; see, e.g., Jahn et al. (2012); Jahn and Rosholm (2014); Barbieri et al. (2019); Auray and Lepage-Saucier (2021); De Graaf-Zijl et al. (2011). Several studies have also identified relatively more favourable effects for women compared to men, see, e.g., Albanese and Gallo (2020); Booth et al. (2002); Jahn et al. (2012).[25]

Table 5 shows results for when we interact the treatment variable by worker and (bankrupt) firm characteristics, of which some are dichotomous (gender, migrant status, low education dummy, manufacturing sector dummy), and some are continuous (age, firm size, pre-treatment wage,

[23] Stayers have somewhat lower pre-treatment wages than leavers, although differences are relatively modest and are mainly driven by very early leavers, see Appendix Figure A15.

[24] Appendix Figure C2 shows a similar split analysis between leavers and stayers for a DiD approach, which includes a triple interaction term between treatment, post-displacement time dummies, and currently working a TWA job. This model also shows substantially larger negative wage effects for stayers, but still consistently negative effects for leavers as well. The interpretation of this exercise is more limited, however, because current TWA employment is endogenous and the selection of leavers relative to stayers is unclear. We therefore interpret this analysis as descriptive evidence only.

[25] These findings are not always consistent; e.g. De Graaf-Zijl et al. (2011) find more favourable effects for men.

pre-treatment working hours). The interaction between the treatment variable and the worker or firm characteristic is instrumented with an interaction between the instrument $\overline{TWA_{-i}}$ and the worker or firm characteristic.

Effects on employment and working hours are generally small and statistically insignificant across subgroups. We do identify heterogeneity in the hourly wage effects. The negative hourly wage effect amounts to -26.7 percent for men, and to -4.4 percent for women (-0.267+0.223=-0.044), which is statistically insignificant. However, we observe in Table B1 that the first stage for women is low, so these results should be interpreted as only suggestive. The negative hourly wage effects are also stronger for smaller (bankrupt) firms compared to larger firms. Additionally, we observe that negative wage effects are stronger for native workers compared to migrant workers, and for manufacturing workers compared to service workers; these differences are marginally significant but of substantial size. Treatment effects do not differ by educational level; interaction effects are low, statistically insignificant and with inconsistent sign. Point estimates with respect to age are consistently negative (i.e. effects are more negative for older workers) but statistically insignificant.

The strongest heterogeneity is observed by pre-treatment wages: the negative effects are substantially larger for workers with higher initial wages, and the interaction term is highly statistically significant. This pattern may partly reflect floor effects, as workers with low wages have less scope for further wage declines and may be constrained by minimum wage regulations and collective bargaining agreements. However, the magnitude of the heterogeneity suggests that floor effects alone are unlikely to fully explain the differences. The results are consistent with the broader literature on temporary and atypical employment, which generally finds more favourable labour market consequences for workers with weaker labour market attachment.

4.5 Robustness analysis

The preferred specification used in this study relies on specific choices made in the definition of sample, treatment variable and instrument. We extensively analyse the sensitivity towards these choices, in Appendix Tables B3 and B4. The effects for employment, hours worked and hourly

Table 5: IV estimates of effect of TWA job: heterogeneity

		Employment	Hours worked	Hourly wage	Earnings
Female=1	Baseline	-0.066	0.003	-0.267***	-0.274**
		-0.059	(0.070)	(0.085)	(0.118)
	Interaction	0.091	0.068	0.223**	0.306*
		(0.094)	(0.119)	(0.107)	(0.169)
Migrant=1	Baseline	-0.032	0.053	-0.231***	-0.182
		(0.054)	(0.070)	(0.088)	(0.113)
	Interaction	-0.061	-0.055	0.122*	0.060
		(0.075)	(0.108)	(0.070)	(0.143)
Low educated=1	Baseline	-0.033	0.045	-0.206**	-0.168
		(0.052)	(0.067)	(0.086)	(0.112)
	Interaction	-0.063	-0.095	0.020	-0.065
		(0.080)	(0.114)	(0.077)	(0.146)
Manufacturing=1	Baseline	-0.052	0.034	-0.085	-0.048
		(0.061)	(0.083)	(0.096)	(0.138)
	Interaction	0.030	-0.010	-0.273*	-0.306
		(0.103)	(0.124)	(0.149)	(0.204)
Age	Baseline	-0.054	0.024	-0.230*	-0.200**
		(0.052)	-0.068	(0.120)	(0.101)
	Interaction	-0.035	-0.012	-0.045	-0.082
		(0.036)	-0.053	(0.059)	(0.079)
Firm size	Baseline	-0.003	0.040	-0.378**	-0.241
		(0.078)	(0.096)	(0.150)	(0.150)
	Interaction	-0.030	0.008	0.267**	0.256**
		(0.062)	(0.076)	(0.121)	(0.123)
Pre-treatment wage	Baseline	-0.038	0.013	-0.214***	-0.209**
		(0.048)	(0.060)	(0.071)	(0.088)
	Interaction	0.011	0.026	-0.207***	-0.207**
		(0.044)	(0.056)	(0.067)	(0.090)
Pre-treatment hours	Baseline	-0.043	0.027	-0.224**	-0.185*
		(0.050)	(0.064)	(0.108)	(0.102)
	Interaction	-0.008	-0.035	-0.048	-0.142
		(0.048)	(0.070)	(0.079)	(0.096)

Notes: The table shows heterogeneity in the estimates from the IV model for the effect of transitioning to a TWA job on averaged labour market outcomes, using interactions between characteristic and treatment. These interactions are instrumented by the interaction between the characteristic and the instrument. Results are based on separate regressions per interacted characteristic. For dichotomous characteristics (female, migrant, low educated, manufacturing) the baseline estimate refers to the treatment effect when the characteristic equals 0, and the interaction estimates the addition to the treatment when the characteristic equals 1. For continuous characteristics (age, firm size, wage, hours), characteristics are set to have mean zero and standard deviation of 1. Standard errors are robust and corrected for clustering at the level of the bankrupt firm.

wages are consistent across specifications, indicating that these are not driven by how we define sample and instrument. Estimates for employment are consistently negative across Tables B3 and B4, and they are statistically significant in some of the alternatives. Thus, results with respect to employment are somewhat inconclusive although suggestive of a negative impact of TWA jobs.

There are two broader identification threats to our approach that we analyse in more detail. First, the instrument may affect not only the type of job obtained after displacement, but also the timing of re-employment, for example if lower relative demand for TWA workers prolongs job search. This may have an independent effect on labour market dynamics. When using the time to re-employment as outcome in our main specification, we obtain statistically insignificant results (Appendix Table B5). Moreover, including time to re-employment fixed effects as additional controls leads to similar estimates (Appendix Table B6), as does varying the re-employment window for sample inclusion (Appendix Table B7).

A second threat is that the instrument may capture within-sector seasonal variation in labour market conditions that independently affects subsequent outcomes, thereby violating the exclusion restriction. We address this concern in two complementary ways. First, we estimate specifications with more granular sector-by-time fixed effects, including sector-by-year and sector-by-quarter fixed effects, which absorb broader sector-specific cyclical shocks and differential trends. These additional fixed effects leave our estimates qualitatively unchanged (Appendix Table B6). Second, we estimate whether the instrument is associated with several sector-month indicators of job quality among newly started jobs (measured across the full Dutch labour market). As reported in Appendix Table B5, we find no statistically significant relationship with starting wages, average educational attainment, the share of permanent contracts, or re-employment timing. We do find a positive association with firm size, although this is only marginally significant (10% level). Importantly, including these job-quality indicators and re-employment timing as additional controls leaves the estimated treatment effects virtu-

ally unchanged (Appendix Table B6).[26] While these analyses cannot fully rule out unobserved seasonal shocks, they provide evidence that our results are driven primarily by seasonal variation in the availability of TWA jobs rather than broader seasonal differences in job quality or job-finding dynamics.

5 Conclusion

This study examines the causal effect of temporary work agency (TWA) employment after job loss due to firm bankruptcy on subsequent labour market outcomes. Using an instrumental variables approach based on seasonal variation in TWA demand, we find that entering TWA employment as the first post-displacement job leads to persistent reductions in hourly wages, monthly earnings, and the probability of obtaining a permanent contract. TWA employment also reduces employment and working hours in the short run, although these effects largely disappear after approximately two years. In addition, workers entering TWA employment are less likely to remain in their pre-displacement sector and accumulate less tenure with their current employer.

Overall, the results provide causal evidence that, for the group of displaced workers affected by our instrument, TWA employment is more consistent with a dead-end trajectory than with a stepping stone toward stable, higher-quality employment. The observed patterns are consistent with several mechanisms discussed in the literature, including differences in subsequent job mobility, match quality, and the accumulation of sector- and firm-specific human capital. While our design does not allow us to disentangle these mechanisms, the persistence of the wage and employment effects suggests that the consequences of the initial post-displacement job extend beyond the duration of the TWA spell itself.

The estimated effects should be interpreted in light of the population and treatment margin identified in our study. First, the analysis is restricted to incumbent workers displaced by

[26] Another implicit probe of the exclusion restriction is that we estimate the model separately for periods with above- and below-median unemployment rates. The results of this heterogeneity analysis are portrayed in Appendix Table B3. We do not find evidence that the qualitative conclusions depend on the state of the business cycle.

firm bankruptcy and therefore does not generalize to all TWA employment. Relative to the broader population of TWA workers, our sample consists predominantly of prime-age, male workers employed in manufacturing and commerce, who generally have stronger labour market attachment. Second, our IV strategy identifies a local average treatment effect for workers whose post-displacement transition into TWA employment is influenced by seasonal variation in TWA demand. Both considerations suggest caution when extrapolating the results to other populations, such as young labour market entrants or the long-term unemployed, for whom previous studies often report more favourable effects of TWA employment (Booth et al., 2002; Jahn et al., 2012; Jahn and Rosholm, 2014). Consistent with this interpretation, treatment compliance in our setting is strongest among men and manufacturing workers, while our heterogeneity analyses point to more favourable effects for groups that are less strongly represented in the complier population. Future research should examine the extent to which these causal effects generalize across worker groups, institutional settings, and phases of the business cycle.

Although specific, the context of this study has important policy relevance. Many employees face forced displacement due to a bankruptcy, and subsequent reintegration into the labour market. Throughout the 2010s, around 450,000 employees in the Netherlands experienced a transition to a new job because of a forced displacement through bankruptcy, and around 90,000 of those transition to a TWA job as their next job (authors' calculations). Our results are indicative that reintegration through TWA jobs is detrimental to the short and long run earnings perspectives of these initially entrenched workers. These findings show interesting similarities with recent work by Degenhardt and Nimczik (2025), who find that greater availability of gig work for refugees in Austria lowers their job security and earnings in the long run. While the target groups differ, both their and our study highlight that more job opportunities can in fact have negative labour market consequences, when that expansion comes from jobs that be considered of poor quality. It would be interesting to shed more light on what causes compliance with the instrument. Is this guided by the job search behaviour of the displaced workers, who face more TWA job opportunities in certain months versus other months, and/or does the employment agency also play an active role by guiding unemployed workers towards TWA jobs when these are more abundant? Future research, possibly focusing on variation among

employment agency caseworkers in job search assistance, may shed more light on this.

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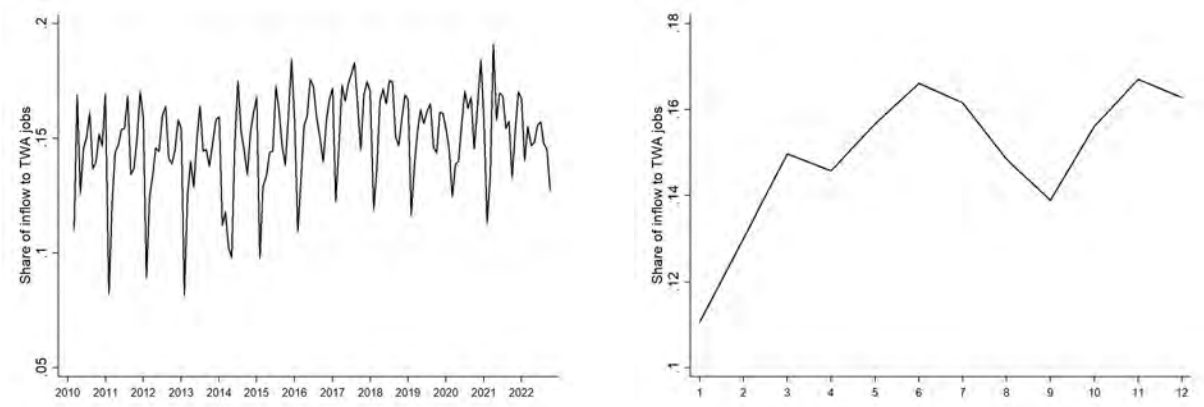
Appendix A: Additional descriptives

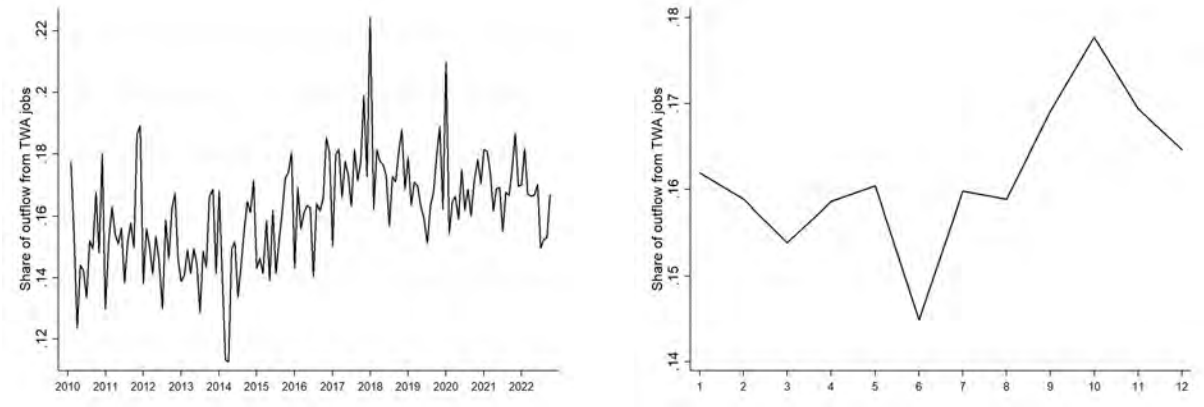
Figure A1: Inflow to TWA jobs relative to all inflow to new jobs



(a) By period (b) By calendar month

Notes: The figure shows the share of employees who started a new TWA job relative to all employees who started a new job, per year and month (left-hand side) and by calendar month (right-hand side)

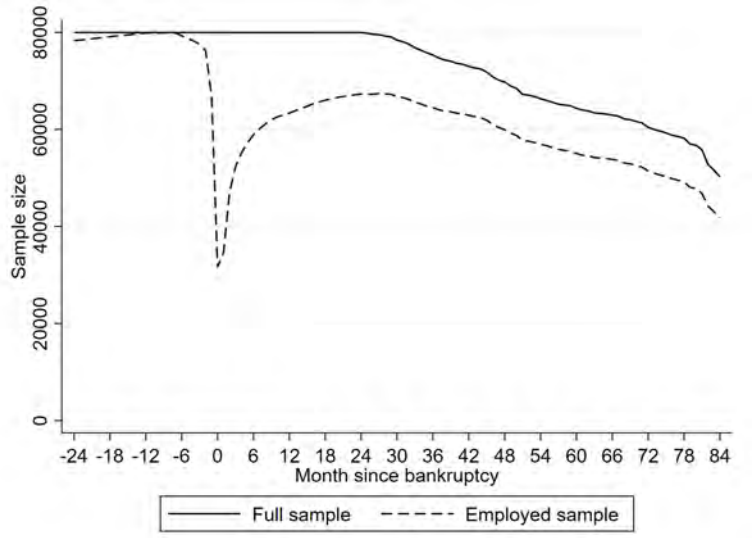
Figure A2: Outflow from TWA jobs relative to all job outflow



(a) By period (b) By calendar month

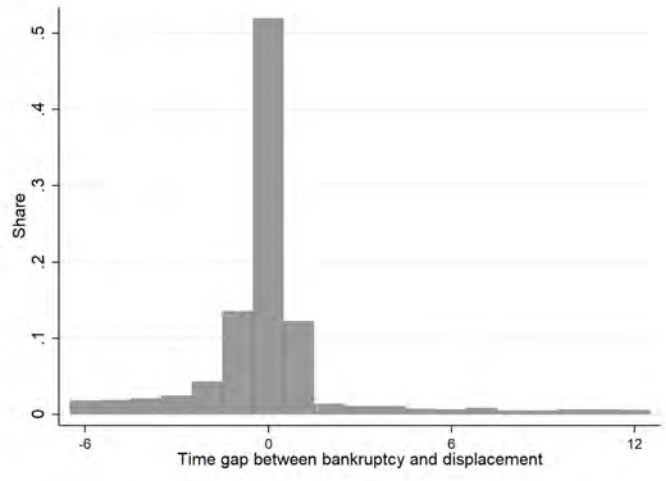
Notes: The figure shows the share of employees who ended a TWA job relative to all employees who ended a job, per year and month (left-hand side) and by calendar month (right-hand side)

Figure A3: Sample size per period relative to bankruptcy



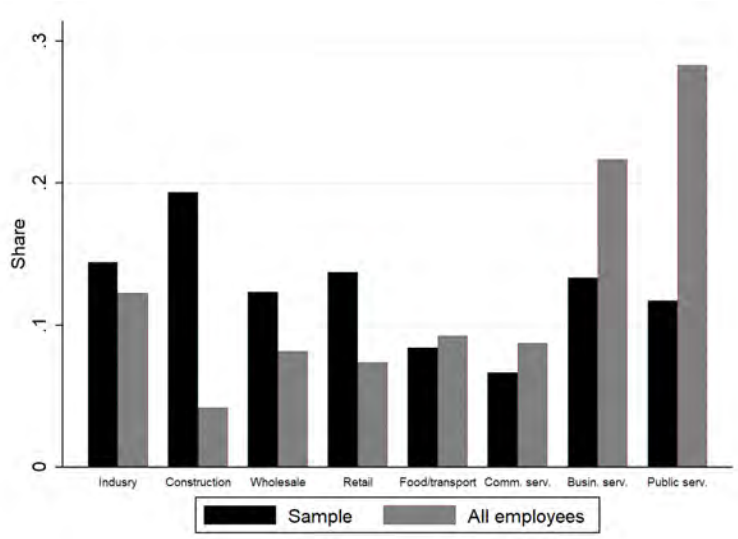
Notes: The figure shows the number of individuals present in the sample across τ, separately for all observed individuals ('Full sample', corresponding to the sample used to estimate employment effects) and all employed individuals ('Employed sample', corresponding to the sample used to estimate effects for outcomes conditional on employment).

Figure A4: Timing of displacement month relative to bankruptcy month



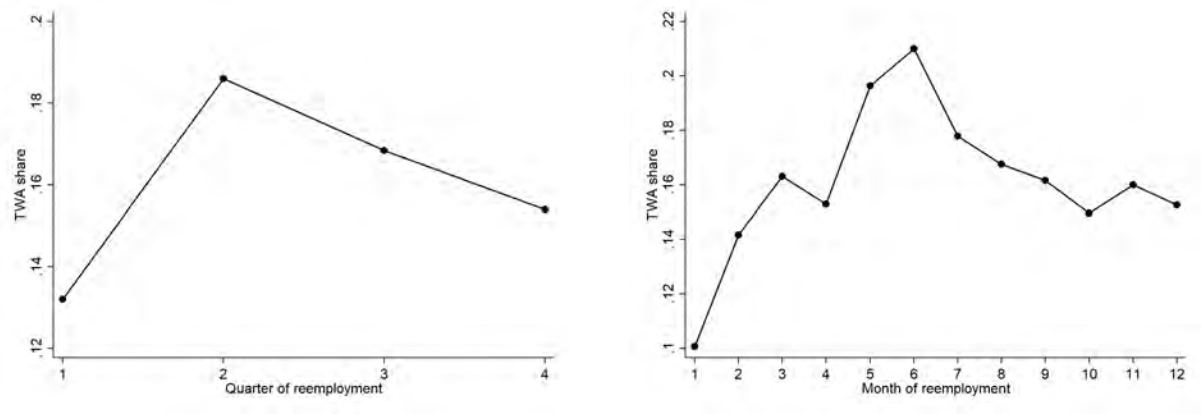
Notes: The figure shows the differences in months between the period of job displacement and the period of bankruptcy (positive values mean that job loss occurs after bankruptcy).

Figure A5: Sector shares in sample and in population



Notes: Sector categorization is done by reducing the one-sigit ISIC, which consists of 21 categories: Industry (ISIC-A through ISIC-E), construction (F), wholesale (G45-G46), retail (G47), food services and transport (H-I), commercial services (J-L), business services (M-N), public services (P-U). There are no bankrupt firms that fall into sector O (public administration and defence).

Figure A6: TWA transition rate by timing of re-employment

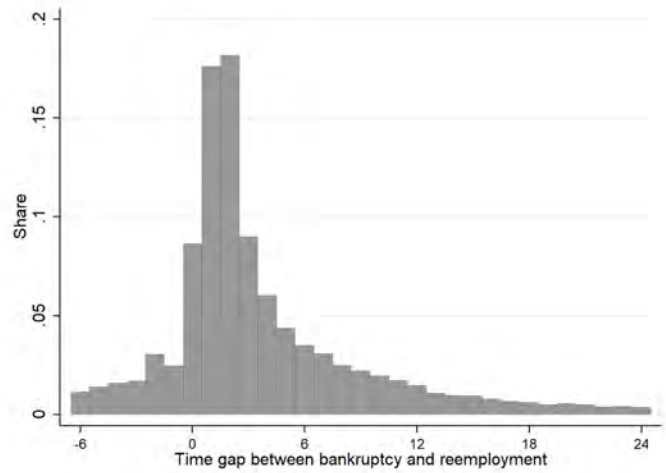


(a) By quarter of re-employment

(b) By month of re-employment

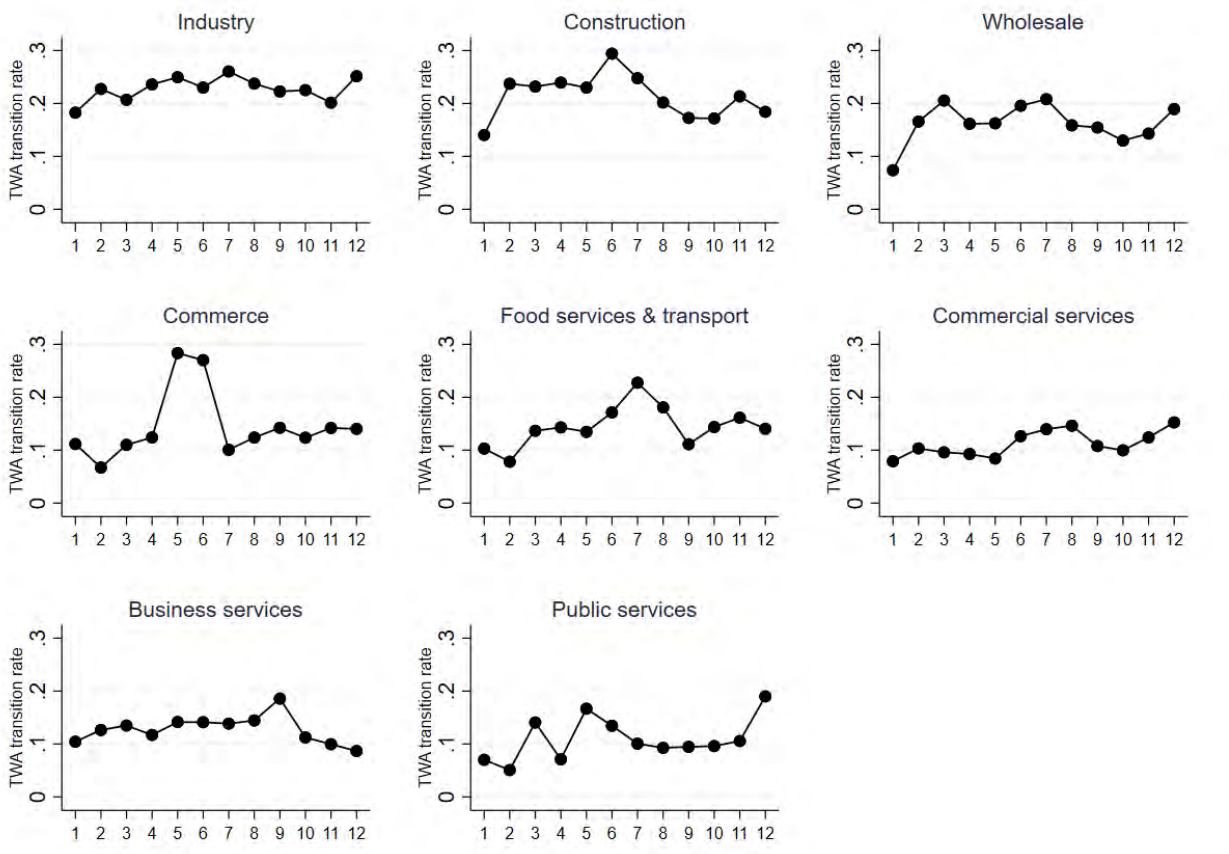
Notes: The figure shows the share of employees who transitioned to a TWA job as a share of all employees in the analysis sample who transitioned to a new job. Relative shares are calculated per calendar quarter (a) or calendar month (b) in which the new job is started.

Figure A7: Timing of re-employment month relative to bankruptcy month



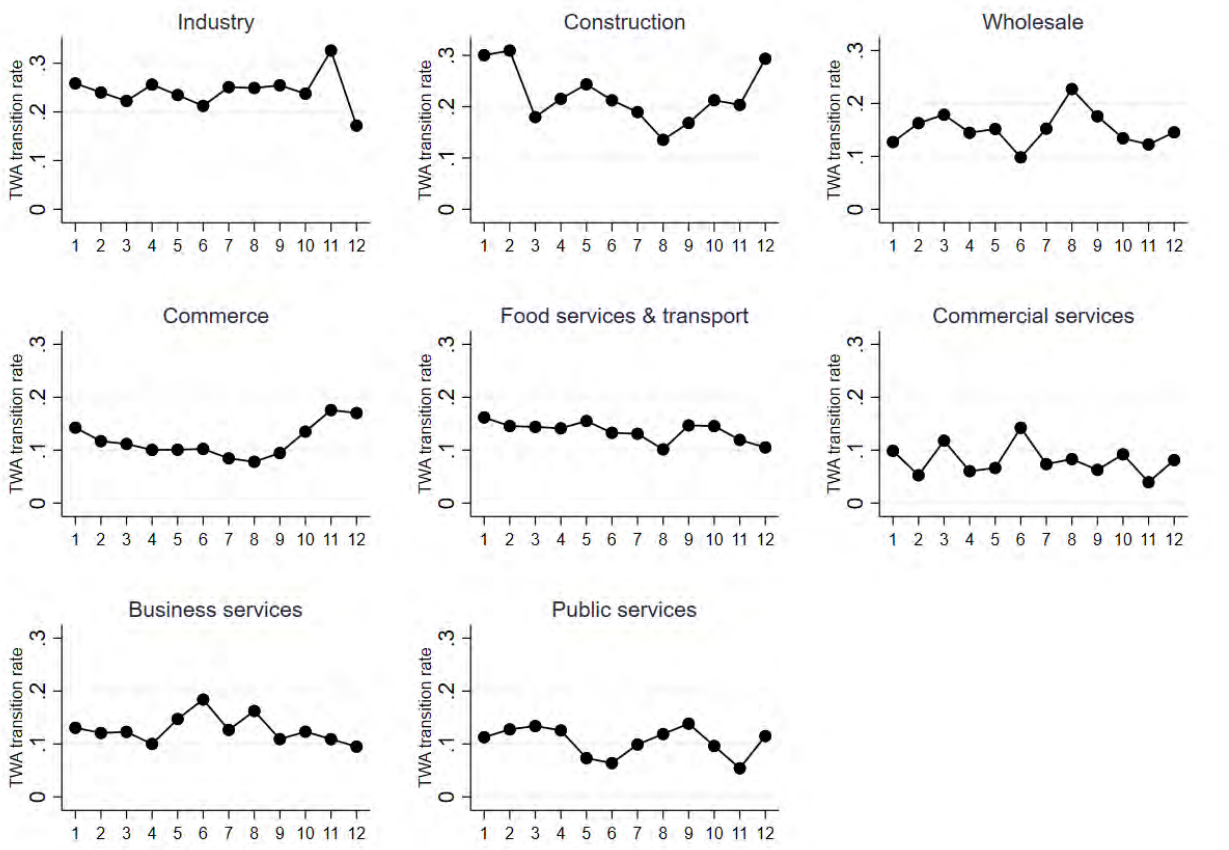
Notes: The figure shows the differences in months between the period of re-employment and the period of bankruptcy (positive values mean that re-employment occurs after bankruptcy). For better graphical portrayal, values beyond 24 months are not included in the figure; these represent 6.5% of the sample. Around 86% of the sample is re-employed within 12 months.

Figure A8: TWA transition rate by re-employment month: by sector



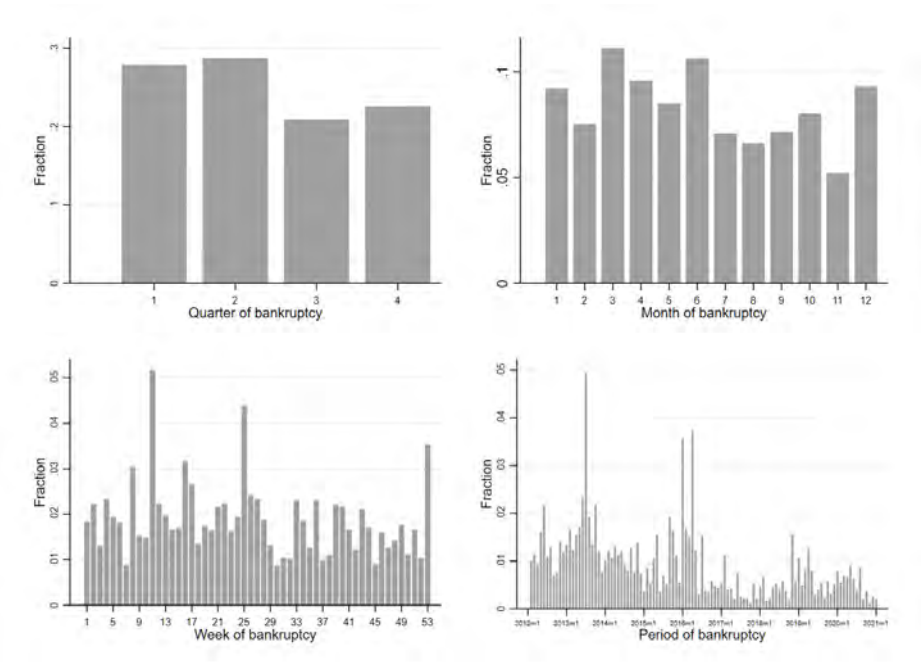
Notes: The figure shows the share of employees who transitioned to a TWA job as a share of all employees in the analysis sample who transitioned to a new job, separately by sector of the bankrupt firm. Relative shares are calculated per calendar month in which the new job is started.

Figure A9: TWA transition rate by bankruptcy month: by sector



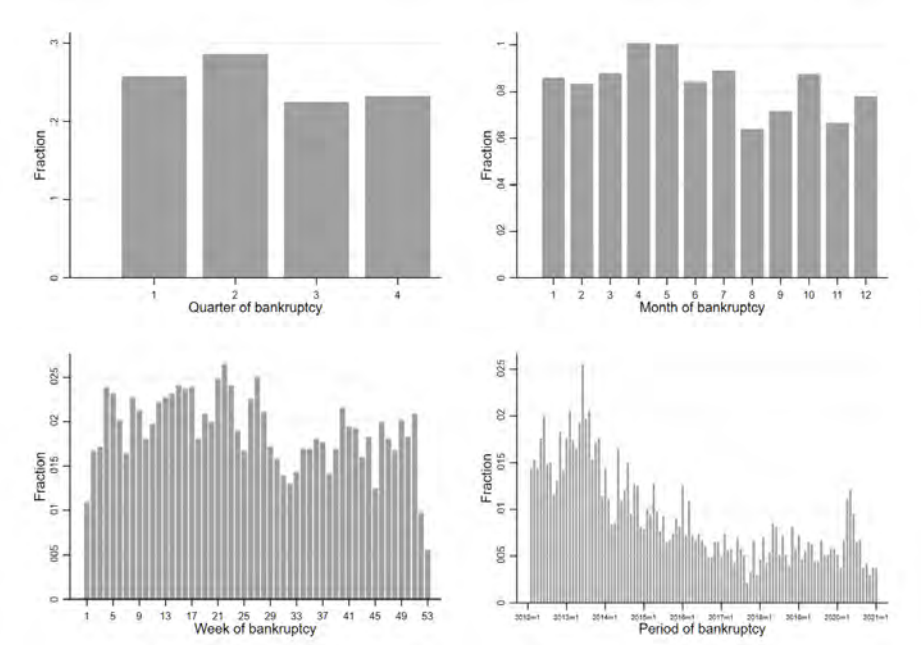
Notes: The figure shows the share of employees who transitioned to a TWA job as a share of all employees in the analysis sample that transitioned to a new job, separately by sector of the bankrupt firm. Relative shares are calculated per calendar month in which the firm bankruptcy occurred.

Figure A10: Distribution of bankruptcy timing (full sample; employee-level)



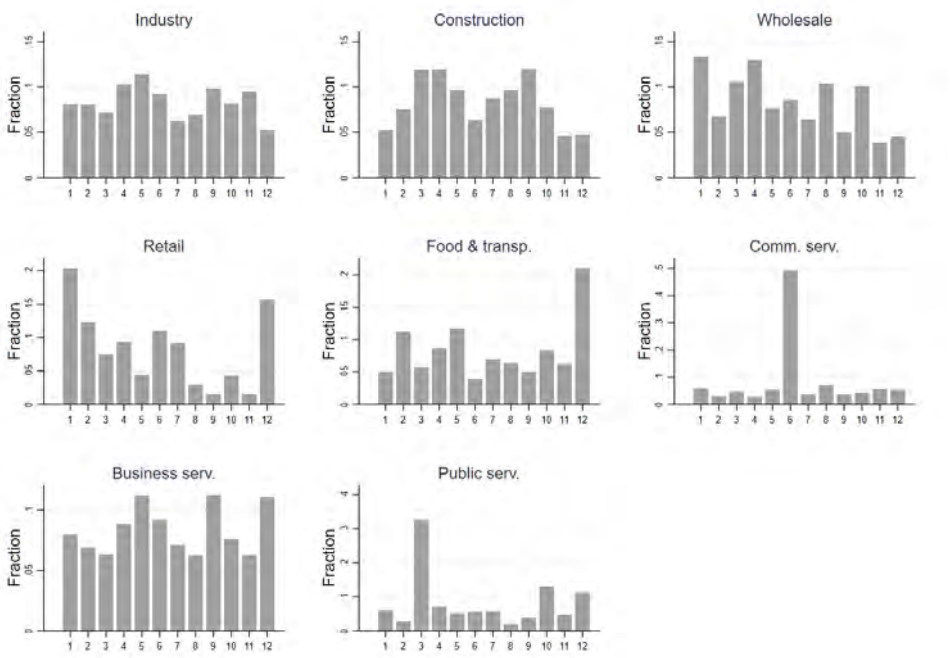
Notes: The figure shows the frequency of bankruptcies, across different time measures. Individual observations are not weighted.

Figure A11: Distribution of bankruptcy timing (full sample; firm-level)



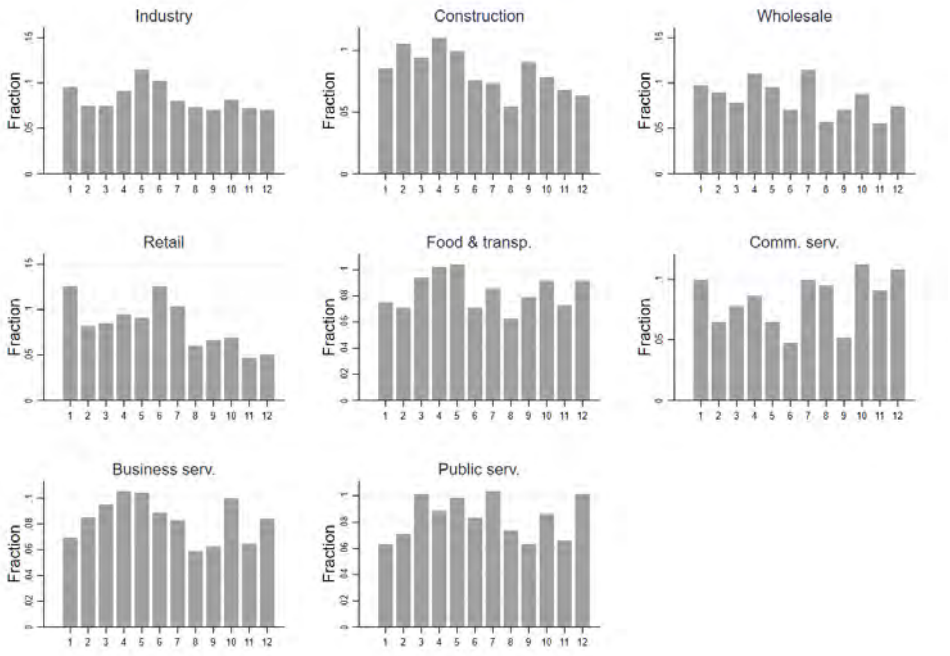
Notes: The figure shows the frequency of bankruptcies, across different time measures. Individual observations are weighted by the inverse of firm size.

Figure A12: Distribution of bankruptcy timing (by sector; employee-level)



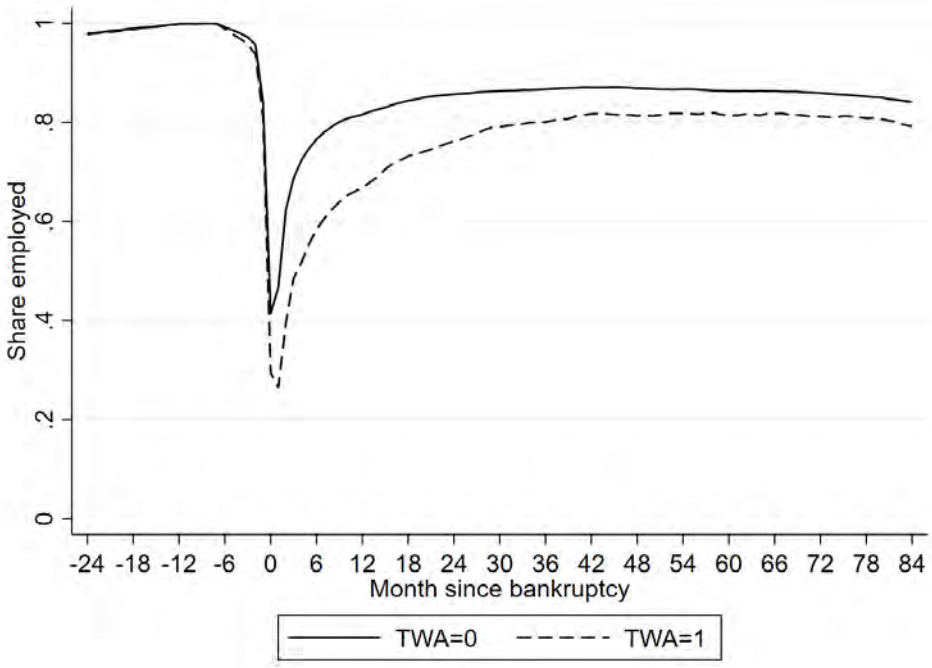
Notes: The figure shows the frequency of bankruptcies, across different time measures, separately by sector of the bankrupt firm. Individual observations are not weighted.

Figure A13: Distribution of bankruptcy timing (by sector; firm-level)



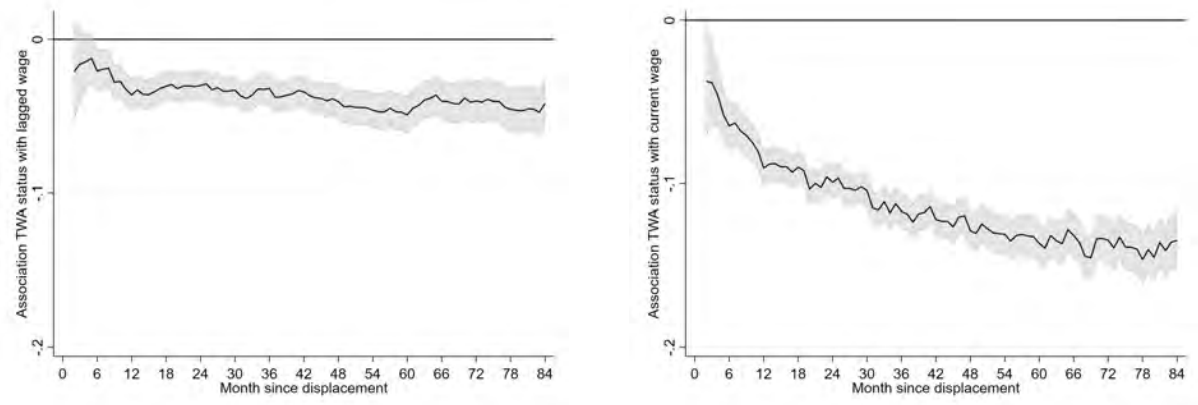
Notes: The figure shows the frequency of bankruptcies, across different time measures, separately by sector of the bankrupt firm. Individual observations are weighted by the inverse of firm size.

Figure A14: Employment rates by treatment status and by τ



Notes: The figure shows the share of employed workers at each τ, separately for those whose first post-displacement job is a TWA job and those whose first post-displacement job is a non-TWA job.

Figure A15: Association between currently holding TWA job and wages



(a) Association with pre-treatment wage

(b) Association with current wage

Notes: Estimates are based on regression of either (averaged) pre-treatment wage or wage in period t on holding a TWA job in period t. Regressions further control for individual and firm characteristics and time fixed effects.

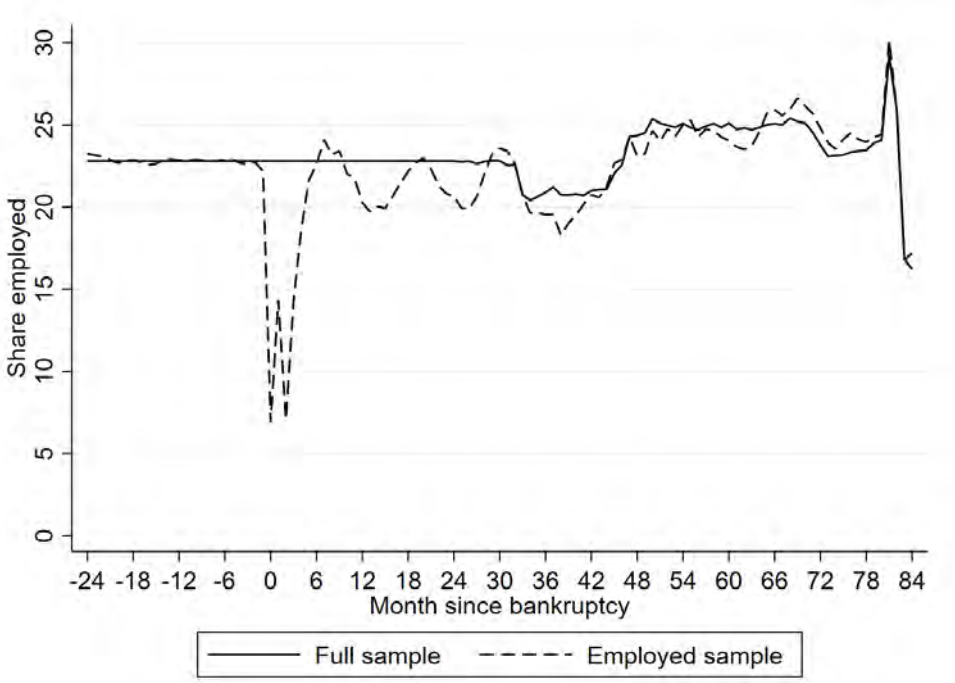
Table A1: Summary statistics: all employees vs. sample (2016)

	All employees		Analysis sample	
	Mean	Std. dev.	Mean	Std. dev.
Age	40.286	11.507	42.500	10.719
Female	0.479	0.500	0.355	0.479
Migrant	0.221	0.415	0.174	0.379
High educ	0.468	0.499	0.267	0.443
Low educ	0.124	0.329	0.168	0.374
Hourly wage	18.295	9.454	17.044	7.137
Hours worked	127.583	50.578	144.599	41.113
N	6,794,843		79,971	

Notes: Because of computational limitations, statistics are provided for all those employed in 2016. Accordingly, labour market statistics for the analysis sample are also based on 2016 only.

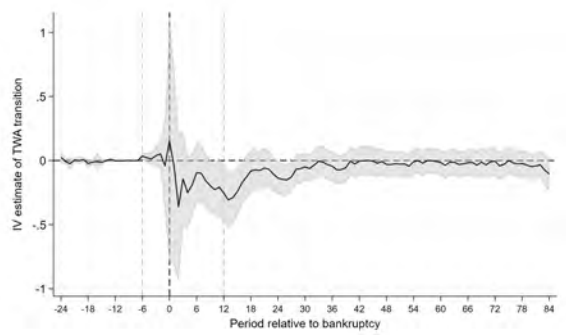
Appendix B: Additional IV estimates

Figure B1: First stage F-statistics by τ

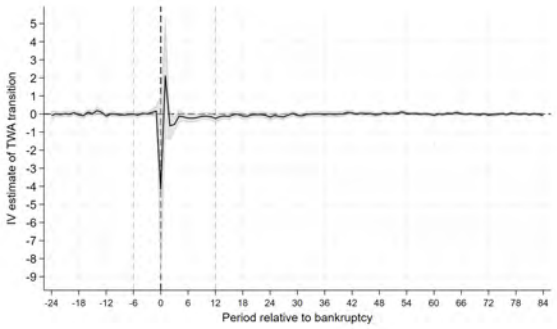


Notes: The figure shows first stage Kleibergen–Paap rk Wald F-statistic of first stage power across τ, separately for first stage regressions on all observed individuals at τ, and first stage regressions for all employed individuals at τ. See Appendix Figure A3 for on overview of the underlying observations across τ and sample type.

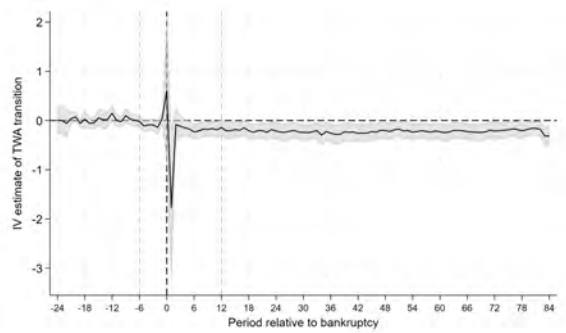
Figure B2: IV estimates of the effect of post-bankruptcy TWA employment: complete confidence intervals



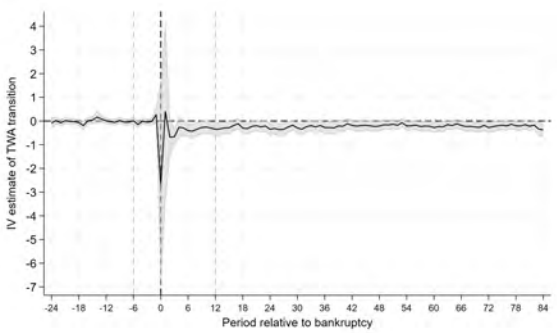
(a) Employment



(b) Log hours worked



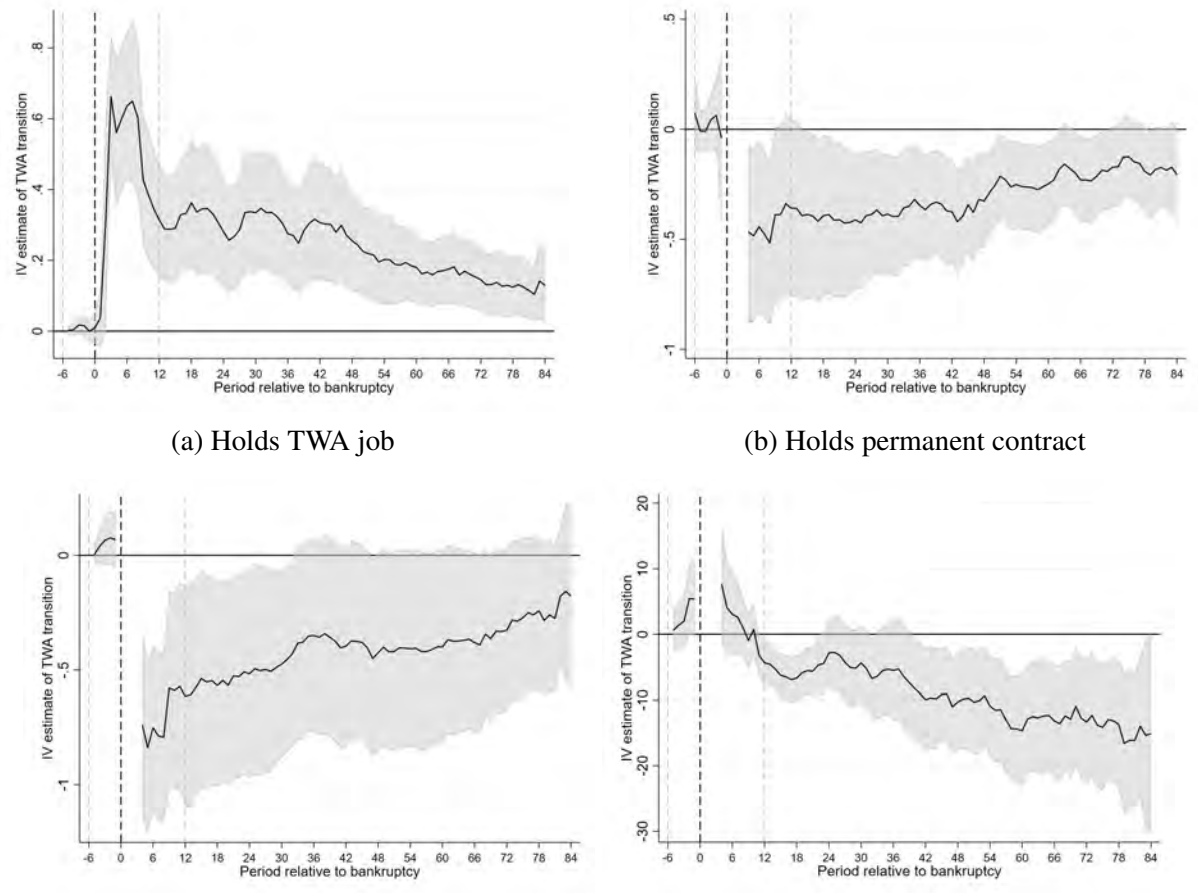
(c) Log hourly wage



(d) Log earnings

Notes: The figure shows effects (point estimates and confidence intervals) of transitioning to a TWA job after job loss because of firm bankruptcy on labour market outcomes, based on IV estimation, now also including results for $\tau = 0$ to $\tau = 3$. Effects are estimated by month relative to bankruptcy. Analysis of outcomes is conditional on employment (other than employment itself). Standard errors are robust and corrected for clustering at the level of the bankrupt firm. The 95% confidence interval is provided.

Figure B3: Effect of transitioning to TWA job on additional labour market outcomes



(a) Holds TWA job

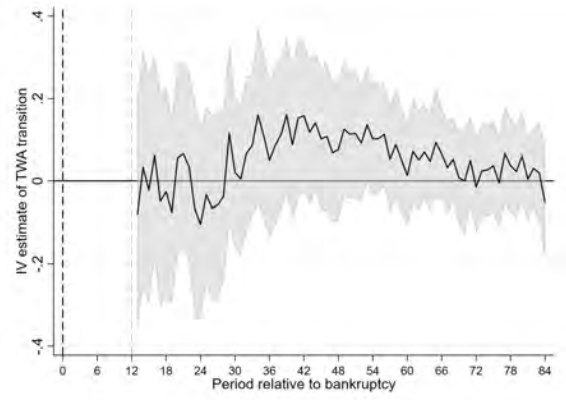
(b) Holds permanent contract

(c) Works in sector of origin

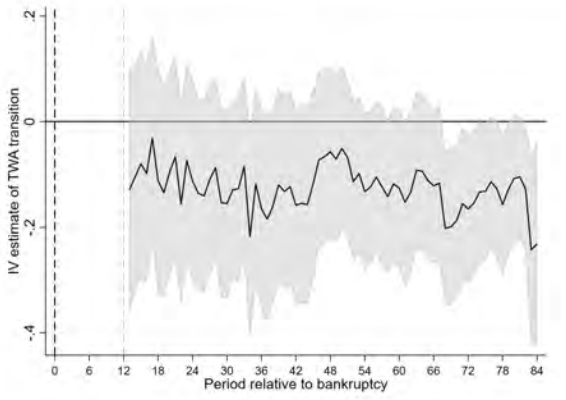
(d) Job tenure (in months)

Notes: The figure shows effects (point estimates and 95% confidence intervals) of transitioning to a TWA job after displacement on secondary labour market outcomes, based on IV estimation. Analysis of outcomes is conditional on employment. Effects are estimated by month since bankruptcy. Standard errors are robust and corrected for clustering at the level of the bankrupt firm. Results for periods 0 through period +3 are not shown because of the dominant size of confidence intervals. Results are not shown for periods before -6 because it could not be determined (job tenure) or because sample selection criteria lead to 0 effect by construction (all other outcomes).

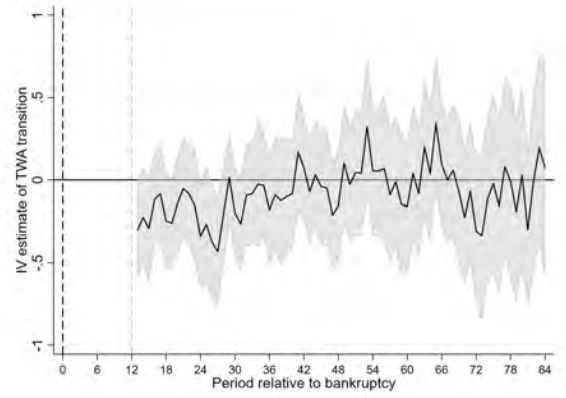
Figure B4: IV effects for labour market outcomes: stayers and leavers



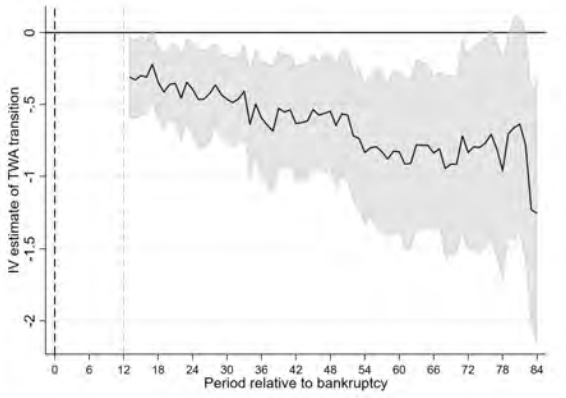
(a) Leavers: hours worked



(b) Leavers: hourly wage



(c) Stayers: hours worked



(d) Stayers: hourly wage

Notes: The figure shows results for the IV approach when the treatment group is either restricted to only those who do not hold a TWA job in period t (top panel; 'leavers') or those who do hold a TWA job in period t (bottom panel; 'stayers').

Table B1: Monotonicity test

	Coefficient	se	F-stat	N	Dependent mean
Full sample					
Full sample	0.793	0.166	22.80	79,971	0.160
Gender					
Male	0.754	0.132	32.52	51,352	0.181
Female	0.832	0.324	6.47	28,619	0.123
Age					
Under 43	0.839	0.203	17.14	39,794	0.142
43 and older	0.728	0.177	16.85	40,177	0.178
Education					
Low educ	0.815	0.229	12.72	12,192	0.250
Medium educ	0.870	0.185	22.03	47,481	0.167
High educ	0.455	0.121	14.09	20,298	0.089
Migrant status					
Native	0.726	0.169	18.36	66,559	0.151
Migrant background	1.086	0.210	26.69	13,412	0.203
Sector					
Industry	0.659	0.231	8.15	11,531	0.246
Construction	0.676	0.192	12.42	15,455	0.213
Wholesale	0.624	0.249	6.29	9,892	0.158
Retail	0.927	0.409	5.15	10,960	0.151
Food & transport	0.654	0.482	1.84	6,745	0.140
Comm. serv.	0.563	0.285	3.90	5,342	0.109
Business serv.	0.685	0.303	5.12	10,659	0.127
Public serv.	0.237	0.262	0.82	9,387	0.063
Manufacturing	0.691	0.150	21.13	26,986	0.227
Services	0.837	0.242	11.97	52,985	0.126

Notes: The table shows first stage regressions separately by subgroup. Sector results are shown first across the main eight sectors, and subsequently for manufacturing (Industry and Construction) and services (all other sectors).

Table B2: IV estimates of effect of TWA job (cross-period outcomes)

	Number of jobs	Length 1st job	Length 2nd job	Length last job
TWA effect	1.00***	-6.42**	-9.40**	-6.94***
	(0.378)	(2.76)	(3.69)	(2.64)

Notes: The table shows estimates from the IV model for the effect of transitioning to a TWA job on post-treatment labour market outcomes. Job length is measured in months. Standard errors are robust and corrected for clustering at the level of the bankrupt firm.

Table B3: Robustness tests: varying estimation sample

	Employment	Hours worked	Hourly wage	Earnings	N
Baseline	-0.049	0.026	-0.201**	-0.180*	79,971
	(0.049)	(0.063)	(0.078)	(0.103)	
Include temp workers	-0.078	0.003	-0.187**	-0.190*	103,336
	(0.054)	(0.072)	(0.086)	(0.104)	
Include low FTE	-0.069*	0.054	-0.187***	-0.138	95,979
	(0.041)	(0.073)	(0.071)	(0.113)	
Exclude firm size <20	-0.055	0.037	-0.177**	-0.145	67,351
	(0.047)	(0.064)	(0.074)	(0.091)	
Exclude firm size <50	-0.119**	0.003	-0.200***	-0.197*	62,001
	(0.046)	(0.061)	(0.073)	(0.102)	
Exclude TWA sector	-0.034	0.041	-0.163**	-0.127	66,814
	(0.044)	(0.059)	(0.075)	(0.094)	
Balanced panel	-0.074**	-0.063	-0.207***	-0.279***	54,506
	(0.037)	(0.045)	(0.063)	(0.076)	
Age 30-55	-0.062	-0.004	-0.178***	-0.186**	50,944
	(0.044)	(0.056)	(0.061)	(0.078)	
Pre-covid	-0.085	-0.020	-0.202**	-0.231**	68,931
	(0.052)	(0.062)	(0.079)	(0.107)	
Low unemployment	-0.087	0.004	-0.229***	-0.232*	40,207
	(0.061)	(0.082)	(0.086)	(0.125)	
High unemployment	-0.077*	-0.048	-0.192***	-0.249***	39,764
	(0.042)	(0.053)	(0.061)	(0.089)	

Notes: The table shows IV estimates of the effect of transitioning to a TWA job after job loss because of firm bankruptcy, for varying sample compositions. Outcomes are averaged across $\tau = +12$ and $\tau = +84$. Temporary worker, FTE status and firm size are all measured one year before displacement. Exclude TWA sector = exclude those for which the bankrupt firm belongs to the TWA sector (ISIC code 78); Balanced panel = only include those observed in all periods from [-24,84]; Age 30-55 = only include those aged 30-55 at time of bankruptcy. Low and high unemployment separate the sample in periods with below and including median unemployment and above-median unemployment (measured across the Dutch labour market). Standard errors are robust and corrected for clustering at the level of the bankrupt firm.

Table B4: Robustness tests: alternative instrument construction

	Employment	Hours worked	Hourly wage	Earnings
Baseline	-0.049	0.026	-0.201**	-0.180*
	(0.049)	(0.063)	(0.078)	(0.103)
Z by 21 sectors	-0.023	0.027	-0.222***	-0.206**
	(0.050)	(0.065)	(0.074)	(0.101)
Z by 4 sectors	-0.139*	-0.023	-0.211**	-0.241
	(0.076)	(0.093)	(0.106)	(0.152)
Z by week and sector	-0.096***	-0.045	-0.241***	-0.288***
	(0.030)	(0.040)	(0.055)	(0.073)
Alternative bankruptcy month	-0.088*	-0.104*	-0.111**	-0.216**
	(0.048)	(0.057)	(0.055)	(0.087)
Bankruptcy adjacent months	-0.177*	-0.015	-0.326**	-0.363**
	(0.104)	(0.131)	(0.138)	(0.183)
Split sample approach	-0.044	0.029	-0.171**	-0.144
	(0.050)	(0.063)	(0.075)	(0.103)
Z excluding same year	-0.141	-0.051	-0.186*	-0.252*
	(0.091)	(0.100)	(0.112)	(0.145)
Z by re-employment month	-0.075*	-0.052	-0.159**	-0.213**
	(0.040)	(0.053)	(0.064)	(0.083)

Notes: The table shows IV estimates of the effect of transitioning to a TWA job after job loss because of firm bankruptcy, for varying definitions of the instrumental variable. Outcomes are averaged across $\tau = +12$ and $\tau = +84$. Baseline = main IV results; 21 sectors = use 21 sectors (ISIC one-digit) instead of 8 to calculate instrument; 4 sectors = use 4 sectors (ISIC A-F; G-I; J-N; O-U) instead of 8 to calculate instrument; Z by week and sector = using calendar weeks of bankruptcy rather than calendar months to calculate instrument; Alternative bankruptcy month = defining bankruptcy month by month in which largest share of employees is laid off; Bankruptcy adjacent months = calculating TWA shares as average of bankruptcy month, previous month and following month; Split sample approach = within each sector-month cell, the instrument is calculated from a random half-sample and assigned to the complementary half (and vice versa); Z excluding same year = excluding the observations from the own bankruptcy calendar year from the construction of the instrument; Z by re-employment month= uses the month of re-employment for the construction of the instrument instead of the bankruptcy month. Sample size equals 79,971 for employment and 78,678 for all other outcomes, which are conditional on employment. Standard errors are robust and corrected for clustering at the level of the bankrupt firm.

Table B5: IV estimates of effect of TWA job: tests of exclusion restriction

	Ever re-employed	Re-empl. time	Education level (firm)	Starting wage (firm)	Firm size	Permanent contract (firm)
TWA effect	0.039	2.510	0.003	0.006	8.135*	0.009
	(0.053)	(1.775)	(0.004)	(0.004)	(4.931)	(0.078)

Notes: The table shows estimates from main IV model, using alternative outcomes variables to test the exclusion restriction. Effects for "Ever re-employed" are estimated in a reduced form model, as those with value 0 have no determined treatment status (and are excluded in the main estimation). 'Re-employment time' measures the number of months between bankruptcy and re-employment. Other outcomes are all measured at the firm-level, and are calculated among those that started a new job within the sector-calender month cell. Sample size equals 88,983 for 'Ever re-employed' and 79,971 for all other outcomes. Standard errors are robust and corrected for clustering at the level of the bankrupt firm.

Table B6: Robustness tests: additional control variables

	Employment	Hours worked	Hourly wage	Earnings
Baseline	-0.049	0.026	-0.201**	-0.180*
	(0.049)	(0.063)	(0.078)	(0.103)
Macro controls	-0.047	0.018	-0.200***	-0.189*
	(0.048)	(0.063)	(0.078)	(0.105)
Month fixed effects	-0.056	-0.001	-0.246***	-0.252**
	(0.051)	(0.066)	(0.078)	(0.106)
Sector-year fixed effects	-0.010	0.024	-0.164**	-0.145
	(0.049)	(0.057)	(0.079)	(0.104)
Sector-quarter fixed effects	-0.006	0.050	-0.223**	-0.178
	(0.055)	(0.070)	(0.092)	(0.119)
Time to re-employment fixed effects	-0.006	0.067	-0.210***	-0.148
	(0.054)	(0.075)	(0.078)	(0.106)
Job quality controls	-0.052	0.022	-0.202**	-0.185*
	(0.048)	(0.063)	(0.079)	(0.106)
All	0.048	0.077	-0.264***	-0.191*
	(0.065)	(0.077)	0.084)	(0.110)

Notes: The table shows IV estimates of the effect of transitioning to a TWA job after job loss because of firm bankruptcy, including additional control variables. Outcomes are averaged across $\tau = +12$ and $\tau = +84$. Macro controls are GDP, unemployment rate and vacancy rate. Job quality controls are starting wages, average educational attainment, the share of permanent contracts and firm size (see Appendix Table B5 for definitions). Sample size equals 79,971 for employment and 78,678 for all other outcomes, which are conditional on employment. Standard errors are robust and corrected for clustering at the level of the bankrupt firm.

Table B7: Robustness tests: varying bankruptcy and re-employment windows

	Employment	Hours worked	Hourly wage	Earnings	N
Bankruptcy window					
Baseline: $[-6,12]$	-0.049	0.026	-0.201**	-0.180*	79,971
	(0.049)	(0.063)	(0.078)	(0.103)	
$[-6,6]$	-0.060	-0.004	-0.230***	-0.240**	76,867
	(0.050)	(0.065)	(0.074)	(0.102)	
$[0,12]$	-0.048	-0.036	-0.241***	-0.280***	58,579
	(0.047)	(0.061)	(0.073)	(0.102)	
$[0,6]$	-0.050	-0.030	-0.224***	-0.258***	55,475
	(0.046)	(0.059)	(0.068)	(0.095)	
$[0,3]$	-0.047	-0.045	-0.187***	-0.236**	53,364
	(0.042)	(0.053)	(0.064)	(0.091)	
Re-employment window					
Baseline: $[-6,.]$	-0.049	0.026	-0.201**	-0.180*	79,971
	(0.049)	(0.063)	(0.078)	(0.103)	
$[-6,12]$	-0.043	-0.001	-0.195***	-0.199**	68,737
	(0.044)	(0.057)	(0.067)	(0.084)	
$[-6,6]$	-0.055	-0.042	-0.175***	-0.220***	58,920
	(0.040)	(0.053)	(0.062)	(0.080)	
$[0,12]$	-0.041	-0.013	-0.192***	-0.209**	60,174
	(0.043)	(0.056)	(0.067)	(0.084)	
$[0,6]$	-0.058	-0.056	-0.172***	-0.232***	50,357
	(0.039)	(0.052)	(0.061)	(0.081)	
$[0,3]$	-0.024	-0.092*	-0.138**	-0.236***	39,934
	(0.042)	(0.049)	(0.060)	(0.085)	

Notes: The table shows IV estimates of the effect of transitioning to a TWA job after job loss because of firm bankruptcy, varying the bankruptcy and re-employment windows that are used to define the composition of the analysis sample. Outcomes are averaged across $\tau = +12$ and $\tau = +84$. Standard errors are robust and corrected for clustering at the level of the bankrupt firm.

Table B8: Anderson-Rubin confidence intervals

	Lower bound	Upper bound	P-value	MOP stat
Employment	-0.148	0.070	0.351	22.80
Hours worked	-0.091	0.189	0.669	23.12
Hourly wage	-0.420	-0.062	0.007	23.12
Earnings	-0.432	0.030	0.093	23.12

Notes: The table shows Anderson-Rubin confidence intervals Anderson and Rubin (1949) and corresponding p-values for the effect of transitioning to a TWA job after job loss because of firm bankruptcy, based on the main IV specification. Outcomes are averaged across $\tau = +12$ and $\tau = +84$. The final column provides the Montiel Olea–Pflueger (MOP) effective F-statistic Olea and Pflueger (2013). Sample size equals 79,971 for employment and 78,678 for all other outcomes, which are conditional on employment. Critical values for the MOP statistics are 37.42 for 5%, 23.11 for 10%, 15.06 for 20% and 12.04 for 30%.

Appendix C: Difference-in-differences estimates

Below are results from a formal DiD model, which interacts the treatment variable (TWA_i), which equals 1 if the individual transitions to a TWA-job as the first job post-bankruptcy, with all time dummies (D_{ik}). Time periods k are here measured relative to *displacement*, rather than bankruptcy as in the IV model (which uses τ). The time periods run from $k = -23$ to $k = +84$, using $k = -24$ as the reference period.

$$Y_{ik} = \alpha + \beta TWA_i + \sum_{k=-23}^{84} \sigma_k D_{ik} + \sum_{k=-23}^{84} \omega_k \left(D_{ik} \times TWA_i\right) + X_i\theta + F_f + \phi_y + \varepsilon_{ik}. \quad (4)$$

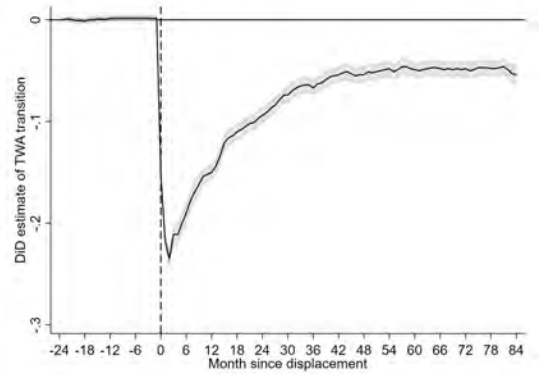
The estimates ω_k give the impact of job loss for individuals who become employed through a TWA relative to individuals who are in non-TWA employment in their first post-displacement job, over time since job loss relative to the baseline period -24. In contrast, the IV approach defines periods relative to the period of firm bankruptcy.

Subgraph (c) of the first figure shows the absence of parallel pre-treatment trends for the log hourly wage outcome. This emphasizes the selection into TWA employment and underscores the relevance of our IV empirical design to overcome this endogeneity problem.

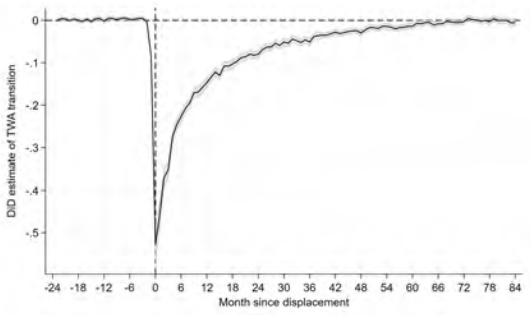
The second figure below separately estimates treatment effects for treated individuals who are still working in a TWA job in period k, and treated individuals who do not work anymore in a TWA job in period k. For this purpose, we add a triple interaction between treatment (TWA_i), time dummies (D_{ik}), and currently working a TWA job ($CTWA_{ik}$). By definition, we can only define these two different status for periods $k > 0$. Since there are still very few "leavers" just after displacement, leading to very wide confidence intervals, we the figures only portray estimates from $k = 3$ onwards.

$$\begin{aligned} Y_{ik} = \alpha + \beta TWA_i + \sum_{k=-23}^{84} \sigma_k D_{ik} + \sum_{k=-23}^{84} \omega_k \left(D_{ik} \times TWA_i\right) + \sum_{k=-23}^{84} \psi_k \left(D_{ik} \times CTWA_{ik}\right) \\ + X_i\theta + F_f + \phi_y + \varepsilon_{ik}. \end{aligned} \quad (5)$$

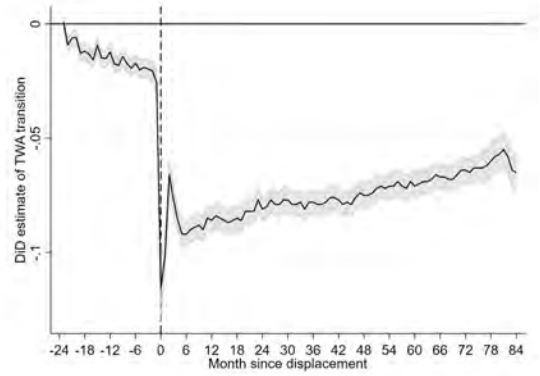
Figure C1: Difference-in-differences estimates



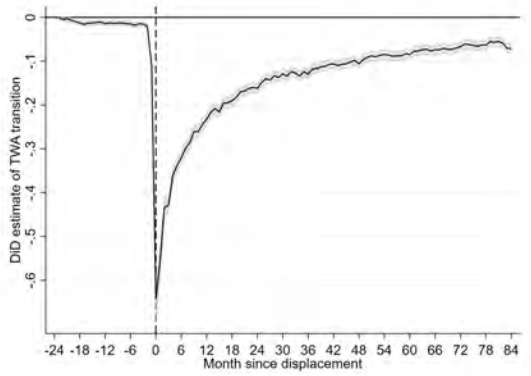
(a) Employment



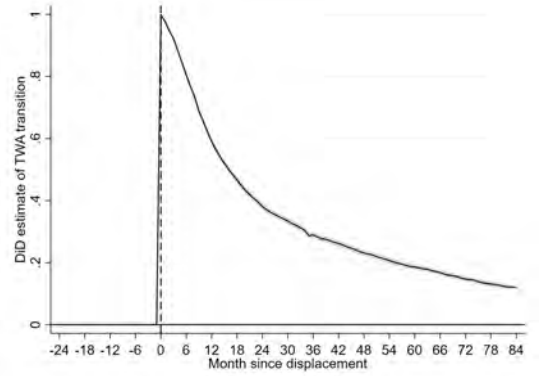
(b) Log hours worked



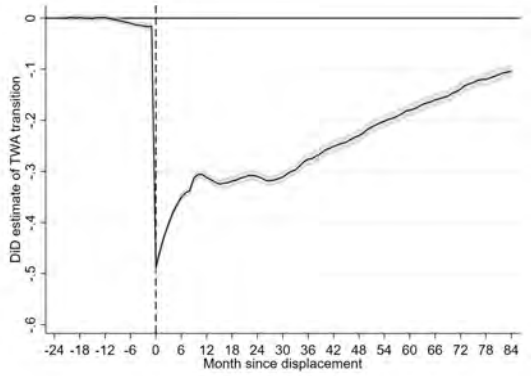
(c) Log hourly wage



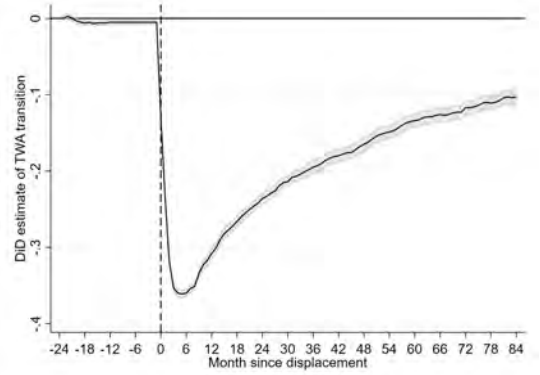
(d) Log earnings



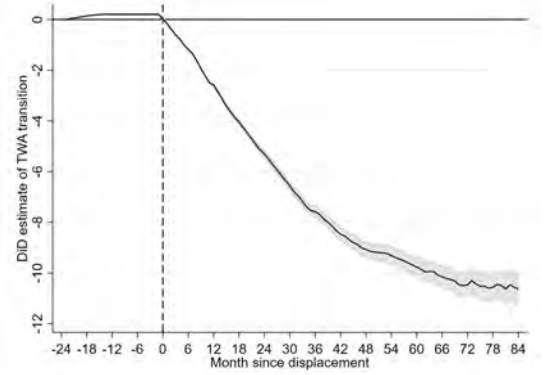
(e) Holds TWA job



(f) Holds permanent contract

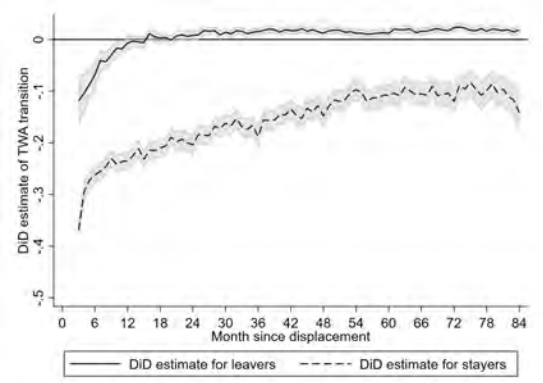


(g) Works in sector of origin

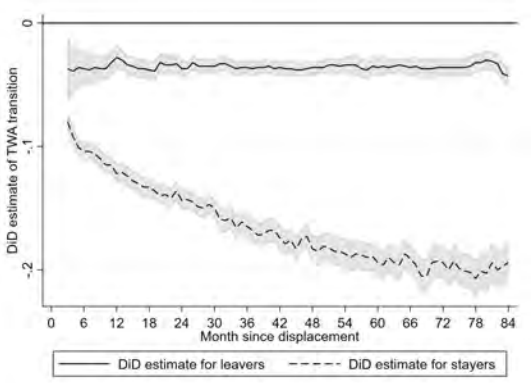


(h) Job tenure

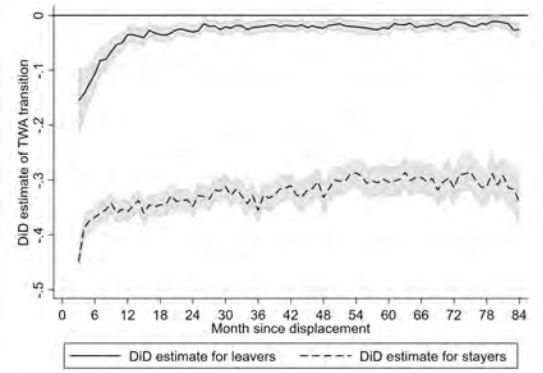
Figure C2: Difference-in-differences estimates: stayers and leavers



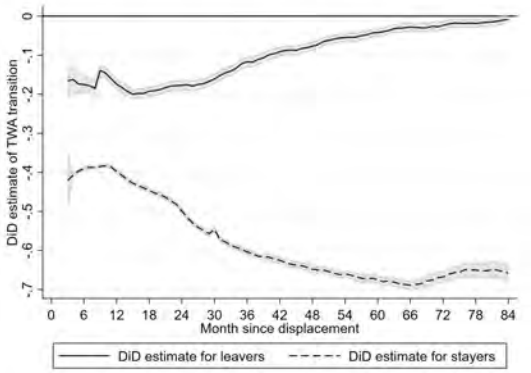
(a) Log hours worked



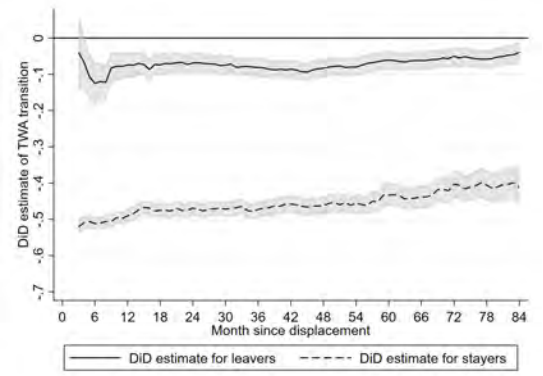
(b) Log hourly wage



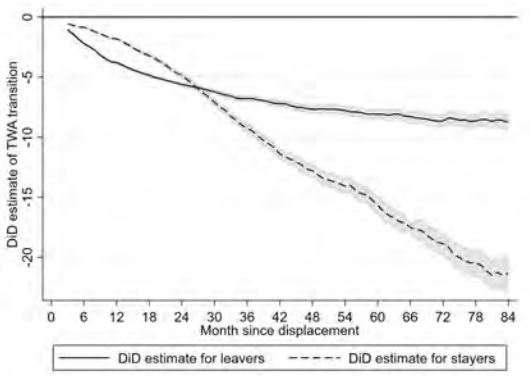
(c) Log earnings



(d) Holds permanent contract



(e) Works in sector of origin



(f) Job tenure

Notes: Leavers are those whose first job is a TWA job but they are not in a TWA job in period k. Stayers are those whose first job is a TWA job and who are still in a TWA job in period k. We do not show estimates for the first three periods because the group of leavers is still very small and standard errors very large.