Are temporary work agency jobs stepping stones? Exploiting variation in the timing of bankruptcy*

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Abstract

This study estimates the effect of re-employment in a temporary work agency job for displaced workers using monthly Dutch administrative data for the period 2010-2022. We use a sample of incumbent employees that 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. To deal with the endogeneity of TWA employment, we exploit variation in firm bankruptcy dates in a novel instrumental variable approach. We find that TWA employment leads to negative effects in the short-run on employment and working hours, which dissipate around 30 months after firm bankruptcy. Effects on hourly wages are negative (15-20%) and consistent up until seven years after firm bankruptcy. We further identify that transitioning to a TWA job has long-lasting negative impacts on job security.

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

JEL Classification: J31, J64

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1 Introduction

The share of workers in atypical forms of work has been steadily increasing worldwide (OECD, 2019). Atypical work has been shown to provide lower job security, lower wages and less attractive secondary benefits than jobs with a permanent contract (Neugart and Storrie, 2006). There is a long debate in labour market research 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 from atypical work to atypical work 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). Atypical work represents different forms of non-standard employment including employment through a temporary work agency (TWA), being part-time employed and having a fixed-term contract. In this paper, we specifically focus on TWA jobs, as these jobs can be labelled as particularly precarious and thus as especially relevant in the context of providing potential stepping stones for workers who may otherwise face unemployment.

From the perspective of the individual worker, several theoretical mechanisms have been proposed for both the stepping stone hypothesis and the dead-end hypothesis.[1] Mechanisms in favour of the former generally follow the argument that individuals in atypical work may alternatively be unemployed. As such, atypical work helps to accumulate human capital, provides a positive signal to future employers, and helps to build and expand relevant networks or to acquire information about suitable vacancies. Potential mechanisms for the dead-end hypothesis point to negative aspects of atypical work versus regular employment. Employers have lower incentive to invest in the human capital of temporary workers, there may be a stigma or negative signal towards future employers from having held an atypical job, and there is higher potential

[1] 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).

for skill mismatch in atypical work and thus also lower accumulation of specific human capital on the job. Furthermore, relative to unemployment, atypical work may crowd out job search (Jahn and Rosholm, 2013). From a firm-perspective, hiring temporary workers through an intermediary such as a TWA is a flexible and effective way to adjust labour input and manage risk (Forde and Slater, 2005). Evidence shows that TWA work enabled firms to better cope with the effects of the Great Financial Crisis (Baumgarten and Kvasnicka, 2017).[2]

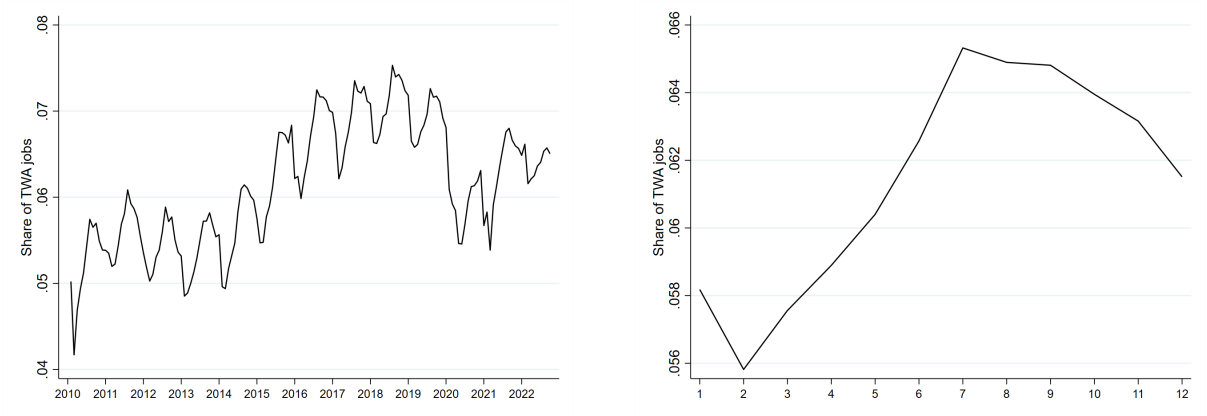
This paper uses full-population administrative data from the Netherlands and a novel methodological approach to estimate the causal effect of TWA employment on future labour market outcomes. The administrative data classify the TWA status for all employees in the Netherlands. Additionally, labour market information is available on a monthly basis for the period 2010-2022, allowing us to estimate effects in short, medium and long-run up to 84 months since firm bankruptcy, and to identify the dynamics of treatment effects.

Estimating the causal impact of atypical work is not straightforward. The main methodological challenge is that atypical work employment is endogenous. Studies thus have to rely on a rich set of control variables, or have to make strong assumptions on individual labour market dynamics, e.g. that unobserved heterogeneity is time-invariant. Reliable causal estimates of the impacts of atypical work, including the impacts of TWA employment, are thus rare. With respect to TWA employment in particular, poor data registration presents another limitation, often leading studies to rely on survey data with relatively small sample sizes.

Our methodological approach exploits the stylized fact that the frequency of TWA jobs exhibits strong seasonal patterns. Figure 1 gives the number of TWA jobs relative to all other jobs in the period 2010-2022 in the Netherlands (data points are by month), based on administrative micro data. Aside from the strong general increase in TWA jobs in the period 2010-2019, Figure 1a shows a strong seasonal pattern in jobs. Moreover, this seasonality is substantially stronger than for non-TWA jobs. Relative shares are typically higher in the third and fourth quarter. When averaging data by calendar month (see Figure 1b), the share of TWA jobs is shown to

[2]The benefits in terms of risk management for client firms are large as the intermediary is financially liable for the human resource services and risks associated with dismissal and sickness absence. In addition, TWA spells offer benefits to client firms in terms of screening (Ichino et al., 2008), which is disproportionately attractive in countries with relatively high firing costs.

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)

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.

Based on this aspect of TWA employment, we develop a novel approach to deal with the endogeneity of TWA employment. First, integrating insights of the literature on job displacement, we focus on 79,987 individuals who lost their job because of firm bankruptcy. Second, building on the stylized fact that there is cyclicality in the relative prevalence of TWA jobs, we exploit differences in the timing of firm bankruptcies in an instrumental variable (IV) estimation. The likelihood of getting a TWA job after job displacement will be partially driven by the (relative) demand for TWA work in the labour market. 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.

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-level characteristics (firm sector and firm size).

Some sectors may have 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 and size, 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 restaurant that goes bankrupt in February with a worker employed in a medium-sized restaurant that goes bankrupt in July. Projecting that the latter worker faces a higher relative supply of TWA jobs, 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 novel approach, results show 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. On average over the period of 12 months to 84 months since firm bankruptcy, transitioning to a TWA job leads to a 20% lower hourly wage and a 30 percentage points lower likelihood of obtaining a permanent contract. 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, and job tenure, as well as lower odds of working in the same sector as before the bankruptcy.

At present, empirical evidence on the medium-to-long-term effects of atypical work is strongly mixed. The mixed evidence can be explained by various reasons: (i) pooling different types of workers in a single group that represents atypical work, which includes TWA workers, part-time employed workers and fixed-term contract employees, although these subgroups differ in the degree of precariousness (De Graaf-Zijl et al., 2011; Auray and Lepage-Saucier, 2021),[3] (ii) using different identification strategies such as a timing-of-events approach, matching tech-

[3]In addition, in research on the impact of employment through an intermediary on individual labour market outcomes, there are also differences in whether the intermediary is a not-for-profit or public employment agency (Autor and Houseman, 2010; Autor et al., 2017; Auray and Lepage-Saucier, 2021) or a private TWA (Ichino et al., 2008; Jahn and Rosholm, 2014; Givord and Wilner, 2015).

niques, difference-in-differences approaches, and logit models (see Jahn and Rosholm (2018) for a comprehensive overview), (iii) focusing on different geographical locations that vary in employment protection legislation, (iv) using different time periods (Filomena and Picchio, 2022), and (v) using different reference groups of atypical workers.[4] Taken together, some findings are consistent with the stepping stone hypothesis while others are consistent with the dead-end hypothesis.[5]

Also studies that specifically focus on TWA employment have shown mixed findings. Autor and Houseman (2010) use 1999-2003 US data and find no effect of temporary-help job placements on future employment outcomes, and a slightly negative effect on earnings. Jahn and Rosholm (2014) use 1997-2006 Danish data on unemployed job seekers and find that TWA shortens the initial unemployment duration, but no positive effects are found on employment and wages based on subsequent jobs after the TWA-spell.[6] On the other hand, Ichino et al. (2008) use 2001-2002 Italian data and find a positive impact of TWA employment on future permanent employment. Givord and Wilner (2015) use 2002-2010 French data and show workers employed through a TWA do better in terms of finding employment than individuals remaining unemployed, at least in non-crisis years. Using recent data from 2010 up to 2022, we contribute to this literature by providing a comprehensive overview of labour market effects of TWA employment on future employment, hours worked, hourly wages, monthly earnings and permanent employment, over a period of up to seven years post-bankruptcy.

We also contribute to the empirical labour economics literature by introducing a methodological innovation, integrating insights from the literature on job displacement and the literature on

[4]See, e.g., Booth et al. (2002); Jahn et al. (2012); Jahn and Rosholm (2014); Filomena and Picchio (2022).

[5]E.g., Forde and Slater (2005) find that TWA workers converge rather quickly towards the labour market outcomes of other workers. Auray and Lepage-Saucier (2021) find a positive impact of atypical work (part-time employment, fixed-term contracts) on finding regular work, and a null effect on wages. In contrast, Autor and Houseman (2010) and Givord and Wilner (2015) find that atypical workers' long-term outcomes are not markedly better than those of the (initially) unemployed. De Graaf-Zijl et al. (2011) provide a mixed picture for atypical workers in the Netherlands, identifying positive employment effects but no improvements in job security compared to the initially unemployed. Relatedly, Chadi and Hetschko (2016) use 2001-2010 German data and show a negative impact of fixed-term employment on job satisfaction.

[6]In addition, Jahn and Pozzoli (2013) use 2000-2008 German data and find that TWA workers experience a 15% to 25% daily wage gap compared to other workers, and an important finding is that this wage gap decreases in the length of the period an individual works through a TWA. Amuedo-Dorantes et al. (2008) use 1998-2004 data for Spain and find a negative impact of TWA employment on future permanent employment. Jahn and Rosholm (2013, 2014) show that TWA employment effects differ by subgroup, and immigrants tend to have a relatively high employment probability after a TWA-employment spell.

atypical work. In the analysis of atypical work, our paper is the first to account for selection into unemployment by using information on an exogenous negative employment shock caused by firm bankruptcy. In addition, we estimate the causal impact of TWA employment by using a novel IV strategy that exploits differences in the timing of firm bankruptcies. This method enables us to quantify the causal effect of TWA employment on a sample of workers who lost their job because of firm bankruptcy.

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 contain the entire population of Dutch individuals, and cover information on individual characteristics (the employee's sex, age, educational attainment, and migrant status (i.e. born outside of the Netherlands)), job-spells (type of contract, job tenure and employment relationship), firms (firm size (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) and economic sector (Industry, Construction, Wholesale, Commerce, Food services and transport, Commercial services, Business services and Public services)) and other monthly national statistics (GDP, unemployment rate and vacancy rate). The firm-level data are based on information of the preceding calendar year. For our independent variable of interest, we use information on the employment relationship 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.

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.[7] We use precise information on the date of

[7] See Appendix Figure A2 for the sample size by period of observation.

the firm bankruptcy and the date of job displacement. Following Schwerdt (2011), displaced employees are defined as those who lost their job between 6 months before and 12 months after the bankruptcy date. Consistent with the job displacement literature, we limit the sample in several ways based on firm-level information and individual-level information.[8]

At the firm level, we only consider bankrupt firms with 10 or more employees in the year before the firm bankruptcy. For firms with relatively few employees, the assumption that the bankruptcy timing is independent of characteristics of the individual employee may be violated. 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, in fact, 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] 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% work 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 con-

[8] See, e.g., Schwerdt (2011); Meekes and Hassink (2022).

sidering 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 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 distribution of the sample across sectors (based on the bankrupt firm) is provided in Appendix Figure A1, which also provides a comparison to the entire population of employees. In general, 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. Note that 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 have categorized sectors such that they are of comparable size.[11]

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 which is that TWA employment is endogenous, also when we limit the sample to displaced workers. Consequently, we apply a novel IV approach, which takes into account selection in TWA employment, 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.

[10] Additional comparisons between population sample and analysis sample can be found in Appendix Table A1.

[11] In addition to combining adjacent ISIC-sectors, we split the ISIC sector G in Wholesale and Retail, as it is would otherwise comprise near to 30% of the sample. See Appendix Table A1 for more details on the classifications. Robustness analysis shows that the results are consistent for alternative classifications.

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

Our goal is to estimate the causal effect of TWA employment after job loss (TWA_i) on future labour market outcomes (Y_{it}). A generic model is shown in (1), where Y represents employment, hours worked, hourly wage, or monthly earnings.

$$Y_{it} = \alpha_0 + \delta TWA_i + X_i\beta + F_f + \tau T_t + \phi_y + \varepsilon_{it} \tag{1}$$

$i \in 1,2,...,N$; $f \in 1,2,...,F$; $t \in 1,2,...,156$; $y \in 2010,2011,...,2022$

where i, f and t denote the employee, firm and month-by-year period, respectively. The parameter of interest is δ, which represents 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. The vector of individual time-constant 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 sector and firm size (F_f). To capture natural growth in labour income as well as business cycle effects, we include a linear monthly time trend from January 2010 to December 2022 in which the labour market outcome is observed (T_t) and calendar year fixed effects (ϕ_y).

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] In this way, we exploit that the seasonal pattern of demand for the TWA job is somewhat heterogeneous by (origin) sector, thus increasing first stage power. 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 (2):

$$TWA_{it} = \gamma_0 + \rho\overline{TWA_{-i}} + X_i\theta + F_f + \omega T_t + \phi_y + \eta_{it} \tag{2}$$

Standard errors are clustered at the level of the bankrupt firm, since the value of the instrument depends on the firm and is the same for all individuals who worked at the bankrupt firm around the time of displacement. We specifically exploit the variation across *bankruptcy* month, rather than displacement month or re-employment month, because the former is not guided by decisions of the individual employee. This model is estimated for each month relative to firm bankruptcy τ, which ranges from -24 up to +84. Hence, depending on τ, the month-by-year period in the analysis sample changes.

3.1 Instrument relevance

Our IV approach is based on the observation that the demand for TWA workers is seasonal within a calendar year, and thus the timing of firm bankruptcy affects the relative amount of TWA employment prospects that one faces upon job displacement.

Figure 1 shows 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 largely 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. We therefore analyse whether the seasonal pattern is still present for our estimation sample. Appendix Figure A3 shows, for our sample of displaced workers, the share of TWA transitions by calendar month

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

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 A3 confirm that our estimation sample is subject to the same within-year seasonality in TWA employment as is shown in Figure 1.

However, we have argued 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 A5 shows the distribution of the difference between the re-employment month and the bankruptcy month. On average, displaced workers become re-employed three months after firm bankruptcy.[13] Thus, we would expect that the pattern of TWA transition rates across bankruptcy months is shifted more to the left compared to Figure A3, and that the pattern is more volatile given the dispersion shown in Figure A5. 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 A3, Figure A4 is indeed more noisy, since the timing between displacement and re-employment is dispersed.[14]

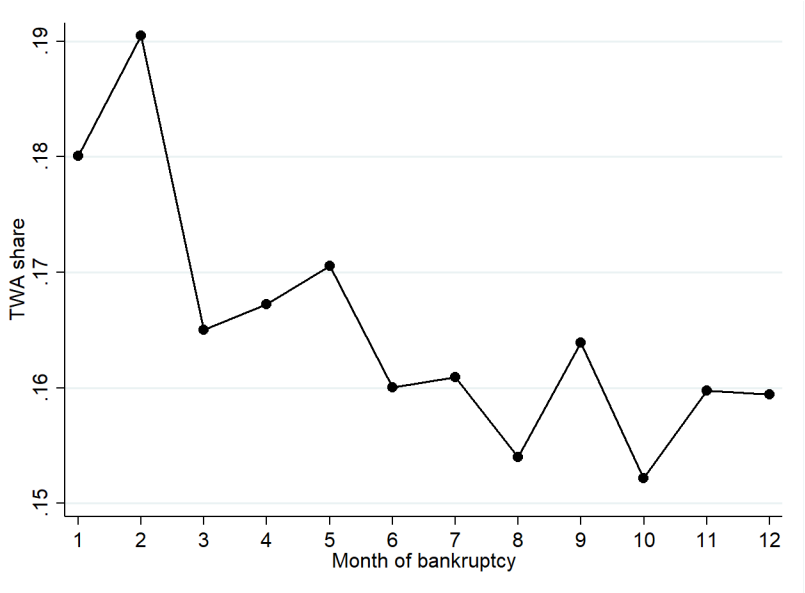
Another source of volatility in Figure A4 is the fact that monthly patterns differ by sector. This applies both to TWA transitions by re-employment timing and TWA transitions by bankruptcy timing. For example, there are sectors that show peaks in September, and for many sectors, December is either a peak or a trough. The patterns by sector can be observed in Appendix Figures A11 (by re-employment timing) and A12 (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 the instru-

[13] Appendix Figure A6 shows the distribution of the difference between bankruptcy and job loss. It shows that more than half of all workers lose their job in the month of bankruptcy.

[14] Results for alternatively constructing 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).

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.

ment is strongly predictive of making a TWA transition after job displacement. The statistical significance weakens somewhat when we include controls for background characteristics, sector fixed effects, and sector-size fixed effects. Still, F-statistics are above 20 also in the most extensive specification with sector-size fixed effects.[15] Hence, we consider our instrument to be relevant.

3.2 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 A7 through A10 show the distribution of bankruptcy months and weeks in

[15] Additional analysis shows a significant correlation between mean TWA transition rates by sector in period T and period T-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.

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***	0.982***	0.793***
	(0.065)	(0.078)	(0.170)	(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	20.80
N	79,987	79,987	79,987	79,987

Notes: The table shows first stage estimates (equation 2), 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 t=-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.

the full sample and by sector, both unweighted and weighted by firm size. While a uniform distribution of bankruptcy dates is neither a necessary nor a sufficient condition for instrument exogeneity, the figure is informative in that it shows no strong peaks or variations across the year. Moreover, we identify neither statistically significant differences in the average bankruptcy timing across sectors nor statistically significant correlations with 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. Since the latter are also driven by relevant unobservable characteristics, testing whether pre-treatment labour market outcomes are balanced is a strong indicator for instrument exogeneity. Results for this quasi-experimental test are in Table 3. Pre-treatment labour market outcomes are taken 12 months before firm bankruptcy; results are consistent for other pre-treatment periods (note that 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, the instrument correlates strongly with observable characteristics. Higher TWA transition rates by calendar month and sector are associated with a higher likelihood of being male, native and low educated, and more hours worked. When controlling for sector fixed effects, these correlations disappear, and a joint significance test on all covariates fails to be rejected.[16] This remains the case when we include sector-size interactions. Additionally, one may be concerned that the instrument reflects not just a seasonal pattern in demand but also volatility in macro-economic circumstances. We therefore include measures of GDP, the unemployment rate and the vacancy rate from the month in which the bankruptcy took place in column (4). These coefficients are statistically insignificant as well.

[16]Note that column (1) does not include sector-size fixed effects, while the instrument is still calculated by sector. In other words, the correlations in column (1) reflect that sectors with higher rates of transition towards TWA work (irrespective of the bankruptcy month) tend to be also sectors with more lower educated workers.

In the preferred specification, we rely on the controls specified in column (3) of Table 3. While the joint significance test supports the instrument also when including only sector fixed effects, we include sector-size fixed effects for additional robustness (and higher precision). The macro-economic controls are excluded from the IV model, because they are not pre-determined and potentially endogenous to the treatment. While therefore not the preferred specification, the appendix shows results when we also include the macro-economic controls for sensitivity analysis.

3.3 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 displayed 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. Table B1 in the appendix 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.

Table 3: Quasi-randomization test: regression of the (left-out) mean TWA transition rate 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				3.74e-07
				(2.77e-07)
Unemployment rate				0.009
				(0.006)
Vacancy rate				0.001
				(0.002)
Sector-size interactions	No	No	Yes	Yes
Joint F-test (excl. fixed effects)	0.000	0.151	0.403	0.294
N	79,987	79,987	79,987	79,987

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 F-statistic of Wald test on all (eight) covariates, excluding the sector and sector-size fixed effects. Standard errors are robust and corrected for clustering at the level of the bankrupt firm.

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 essentially 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 switchers. The IV approach relaxes this assumption by using the exogenous variation in bankruptcy month to isolate the TWA-employment impact.

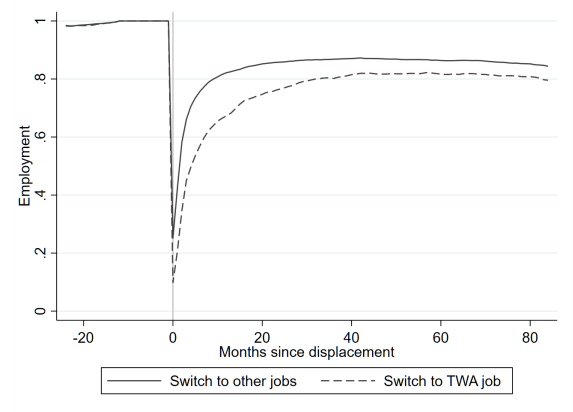
The descriptive results in Figure 3 show that there are baseline differences between the two groups. They are not apparent for employment in the two years before displacement, because individuals are included in our sample of analysis if they have a pre-treatment employment period of at least one year. Baseline differences in working hours are small, and slightly in favour of TWA switchers before displacement. TWA switchers 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 switchers.[18] The initial employment difference is around 10 percentage points stronger for TWA switchers, widens in the period between 6 and 12 months after displacement, and somewhat (but not fully) converges afterward. TWA switchers also exhibit a substantially stronger negative shock to hours worked, but quickly converge in later periods. They catch up after around three years, and even work slightly more hours than non-TWA switchers at the end of the time frame. In contrast, hourly wages initially show a stronger negative shock, but not convergence. The 10% difference in hourly wages before treatment doubles at displacement

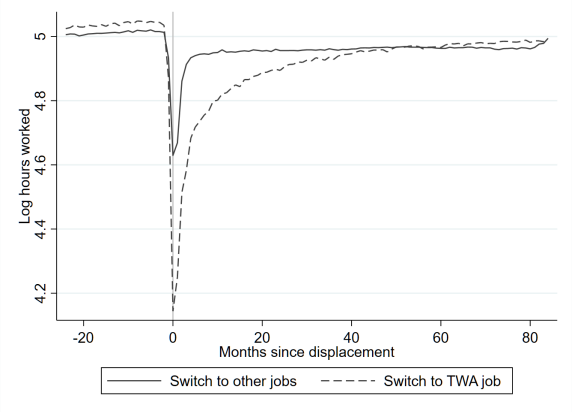
[17] Estimates from a formal difference-in-difference model can be observed in Appendix C.

[18] Note that changes in post-displacement outcomes such as hourly wages and earnings, especially in the first periods after job loss, are characterised by composition effects. That is, changes in outcomes for individuals who were already re-employed for some time for example due to wages growth of job transitions, and changes in outcomes because of new individuals becoming re-employed for the first time.

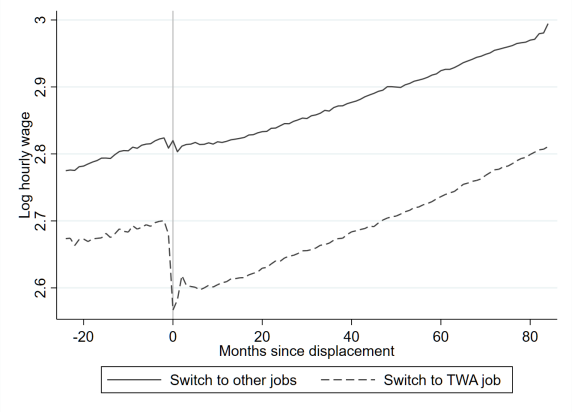
Figure 3: Difference in labour market outcome means: TWA jobs vs. other jobs



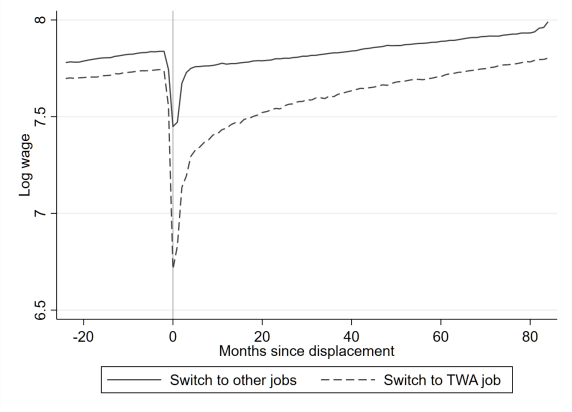
(a) Employment



(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.

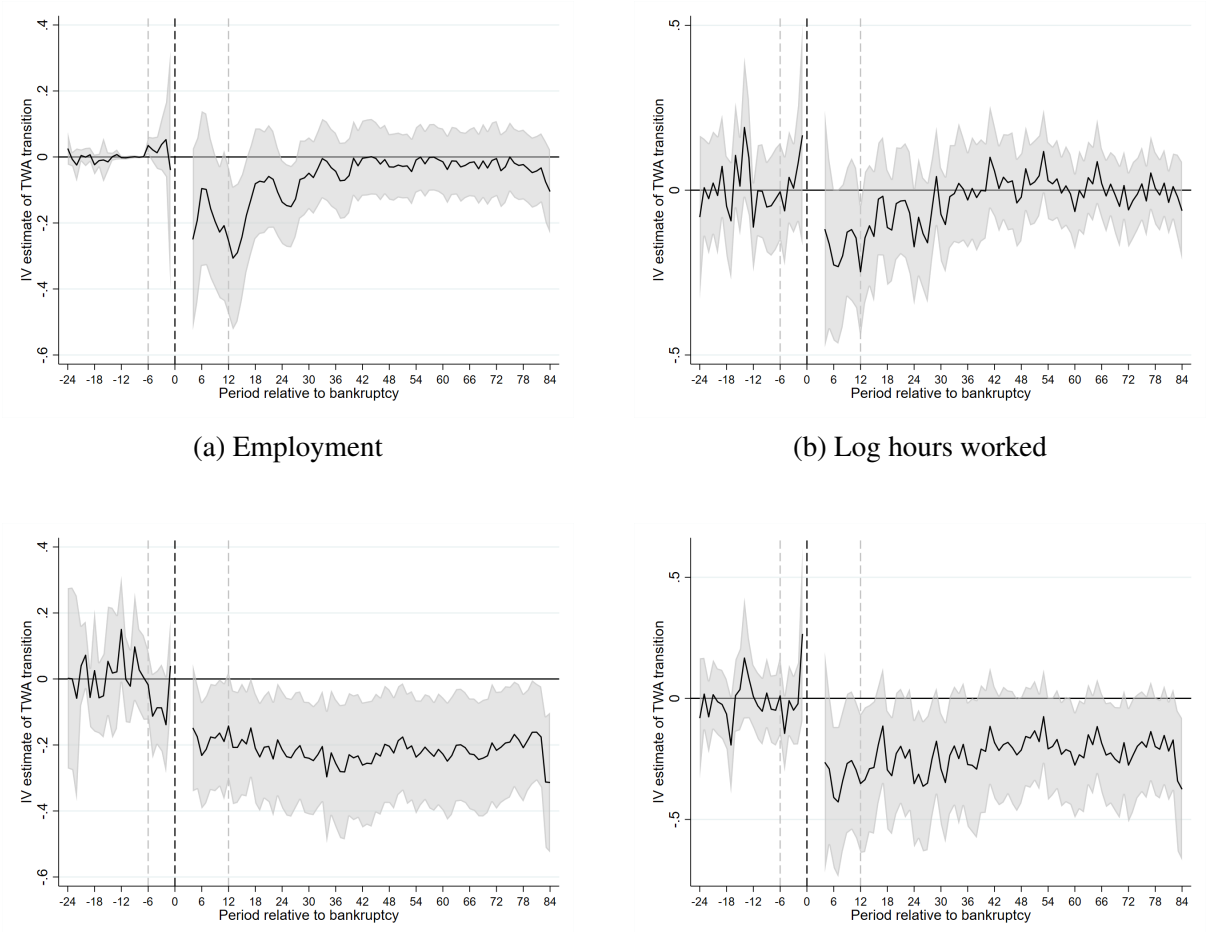
and stays at 20% throughout the period. The pattern for earnings shows a slight but not full convergence, as it combines the dynamics of figures (b) and (c).

4.2 IV analysis

Figure 4 plots coefficients for the per-period estimates of the IV model, across the four main labour market outcomes. For all outcomes, all pre-treatment estimates are statistically insignificant, which provides further evidence for instrument validity. Note that the pre-treatment period essentially runs until T=-6, since this is the time that workers start to switch jobs or be displaced. The treatment effects for the period between T=-6 an T=12 should be interpreted

with caution, as workers switch jobs at different points in time, which can be endogenous to the treatment. After T=12, the vast majority of workers has switched to a new job, as shown in Figure 3a, and the interpretation of the estimates is more transparent. We note that the estimates for the three periods after bankruptcy are highly imprecise. Since the large confidence intervals at those points strongly distort the scale of the figure, these estimates (which are all statistically insignificant) have been excluded.

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 the 95% confidence interval is provided. Results for periods 0 through period +3 are not shown because of the dominant size of confidence intervals.

Panel (a) shows that switching to a TWA job negatively affects employment in the short run. Estimates tend to worsen somewhat in the period between 6 and 12 months, at which point they

are statistically significant. This may be indicative of the lower security that TWA jobs provide, leading to short employment spells in their first job after displacement. The TWA effects on employment improve after 12 months, although they are still significantly negative in some (but not all) 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. The results indicate that becoming re-employed in a TWA job, as compared with becoming re-employed in a non-TWA job, as the first job after job displacement, results in a 20% loss in wages just after bankruptcy and still so seven years later. This result is largely in line with the descriptive evidence in Figure 3. 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. We note that 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 T=12 until T=84, and then applying the IV model. They confirm the low and statistically insignificant results for employment and hours worked, and the 20% decrease in hourly wages. Compared to the results from the DiD analysis, the IV estimates are slightly larger although the former fall within the confidence interval of the IV results. The DiD results for hourly wages are in all likelihood negatively biased, if we assume that selection on unobservables operates in the same

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

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 averages across the period of 12 months to 84 months after bankruptcy. "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. 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.

direction as the selection on observables identified in Table 1. This is suggestive evidence that the (unbiased) average treatment effect is somewhat smaller than the local average treatment effect.

4.3 Additional outcomes and dynamics

We explore dynamics in additional outcomes to shed more light on what drives the TWA job effects. First, we show the per-period estimates for job characteristics that are closely tied to the treatment variable: namely holding a TWA job, having a permanent contract, and working in the same sector as before displacement (Appendix figures B1a, B1b and B1c, respectively; average effects are reported in Table 4). All of these outcomes show very strong short-term effects by construction, since contract status and sector are directly related to TWA employment. For holding a TWA job, effects decrease rapidly, but are still statistically significant after seven years, and amount to around 10-15 percentage points.[20] 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.

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

TWA employment leads to an initial reduction of around 50 percentage points in the odds of obtaining a permanent contract. The negative effect gradually decreases over time, but there is still evidence of negative effects even seven years after bankruptcy, of around 20 percentage points. These effect sizes are similar to those identified by Amuedo-Dorantes et al. (2008). 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 that 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). Appendix Figure B1d also shows the per-period estimates for tenure at the current job, which indicates TWA employment consistently leads to lower job tenure. Table 4 shows that the average effect amounts to around 10 months lower tenure. 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.

Second, we analyse whether the patterns identified in Section 4.2 are exclusive to those that remain in a TWA job ("stayers"), or also experienced by those who transition out of TWA employment ("leavers"). This is difficult to isolate because leaving a TWA job is likely selective. Indeed, we find that stayers have lower pre-treatment wages than leavers, although the difference is relatively small and mainly present when compared to very early leavers (see Appendix Figure A13). A split analysis between stayers and leavers is therefore not causal. Moreover, it is relatively imprecise. Nonetheless, they can be indicative for seeing whether negative treatment effects persist when out of TWA employment. Estimates are shown in Appendix Figure B2. Results for stayers are indeed substantially larger. While relatively imprecise, the point estimates for leavers are nonetheless consistently negative, and (marginally) significant in some periods. Given that leavers are in all likelihood positively selected among treated individuals, this suggests that the negative hourly wage effects are not exclusive to those who remain in a

TWA job, and could be indicative of long-run scarring or negative signalling effects.[21]

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 effect 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 low (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).[22]

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, 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 low and statistically significant across subgroups. We do identify heterogeneity in the hourly wage effects. Specifically,, effects are substantially more negative for men than for women. For the latter, hourly wage effects are statistically insignificant. However, we have observed in Table B1 that the first stage for women is low, so these results should be interpreted as only suggestive. The negative hourly wage

[21] 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. These estimates are more difficult to interpret, however, because it is not clear whether leavers in the treatment group are positively or negatively selected compared to the (full) control group. For the IV approach, treatment and control are balanced pre-treatment, which means that positive selection of leavers compared to stayers must imply positive selection of leavers compared to the control group as well.

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

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

		Employment	Hours worked	Hourly wage	Earnings
Female	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)
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)
Migrant	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	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	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)
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.

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. When we interact with the higher education dummy instead, results are also statistically insignificant although consistently positive for the interaction term (not shown). Point estimates with respect to age are consistently negative (i.e. effects are more negative for older workers) but statistically insignificant.

The strongest degree of heterogeneity is identified across pre-treatment wages. Effects are substantially stronger for those with higher initial wages; the interaction is highly statistically significant. Part of this may be due to a floor effects; if wages are low, they are less likely to fall further also given that they are constrained by minimum wage laws and collective bargaining agreements. However, the size of the heterogeneity suggests that these are more than floor effects. The results are consistent with other heterogeneity results in both Table 5 and the broader literature, that effects of TWA work or atypical work in general are more favourable, or less harmful, for more marginal workers.

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 have extensively analysed sensitivity towards these choices. Appendix Table B3 shows robustness analysis with respect to the specification, and Table B4 with respect to the sample. The tables show the main qualitative conclusions – negative effects for hourly wages and no marked impact on working hours – are consistent across specifications and sample. The estimates for employment are consistently negative, and statistically significant in some of the alternatives (and in a few of those also statistically significant in the long run). Thus, results with respect to employment are somewhat inconclusive although suggestive of a negative impact of TWA jobs.

5 Conclusion

This study has estimated the causal effect of TWA employment after job loss because of firm bankruptcy on various labour market outcomes including employment, hours worked, hourly wages, earnings and permanent employment. Using a novel IV method on a sample of individuals who lost their job because of firm bankruptcy in the period 2012 to 2020, we estimate a local average treatment effect for employees for which TWA employment after displacement depends on the bankruptcy timing along the calendar year. We find that TWA employment after bankruptcy has persistent negative effects on hourly wages, monthly earnings and job security for our sample of incumbent workers. TWA employment also causes negative effects on employment and working hours in the short run, but these effects approach zero when we move beyond two years post-treatment. We further identify negative effects on working in the same sector as before bankruptcy and on job tenure, which may be suggestive of lower sector-specific and job-specific human capital. Taken together, we provide evidence in favour of the dead-end hypothesis rather than the stepping stone hypothesis.

The results we identify pertain to a specific context, and might not be generalizable to all TWA jobs. For one, we limit the sample to incumbent employees who are displaced because of a bankruptcy. Secondly, we estimate a local average treatment effect for employees for whom taking up a TWA job after displacement depends on the bankruptcy timing along the calendar year. Our sample of displaced workers contains relatively more middle-aged, male workers, who are employed in manufacturing and commerce, and have a comparably strong attachment to the labour market. From a theoretical perspective, the comparatively lower precariousness of our sample may imply that negative mechanisms such as scarring and negative signalling are more important, and positive mechanisms such as human capital acquisition and networking are less important. Results may thus be more favourable for younger workers, service workers and individuals with weaker ties to the labour market. Other literature does point towards more favourable effects for those groups in particular (see, e.g., Booth et al. (2002); Jahn et al. (2012); Jahn and Rosholm (2014)), and the heterogeneity analysis in this study largely supports these conclusions. With respect to instrument compliance, the story is largely similar. Compliance

is stronger for men and manufacturing workers. Average treatment effects may thus be lower than our local average treatment effect, which is also supported by supplementary analysis in a DiD framework.

Particular aspects of our setting may also contribute to the negative effects we identify. For example, effects of atypical work tend to be more negative for studies on the 2010s (and more generally periods with higher unemployment) compared to earlier time periods (Filomena and Picchio, 2022). Where the 2010s generally had higher unemployment, this is amplified in our approach because bankruptcies are especially concentrated in economic downturns. Additionally, the Dutch institutional context of high employment protection legislation for permanent jobs and low employment protection legislation for temporary jobs may also contribute to a lack of a stepping stone effect.

Although specific, the context of this study has important policy relevance. Many employees each year face forced displacement due to a bankruptcy, and subsequently have to reintegrate into the labour market. Throughout the 2010s, around 450,000 employees in the Netherlands experience 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 incumbent workers. 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 there are more TWA vacancies? Future research, possibly focusing on variation among employment agency caseworkers, may shed more light on this issue.

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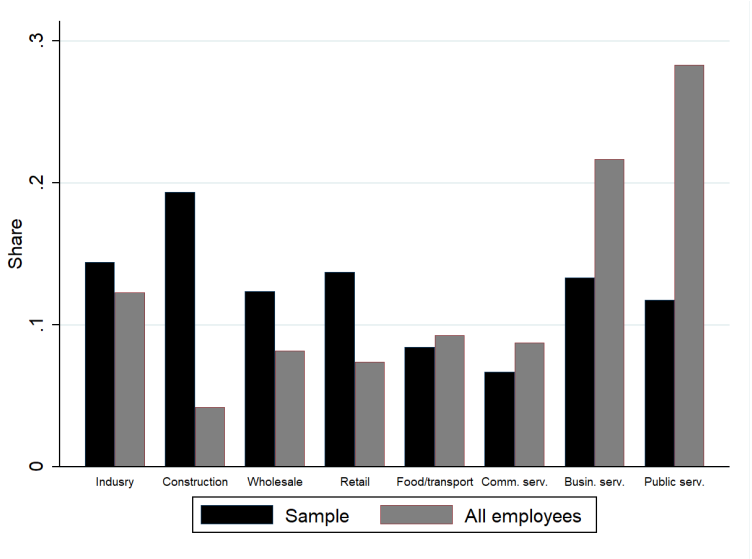
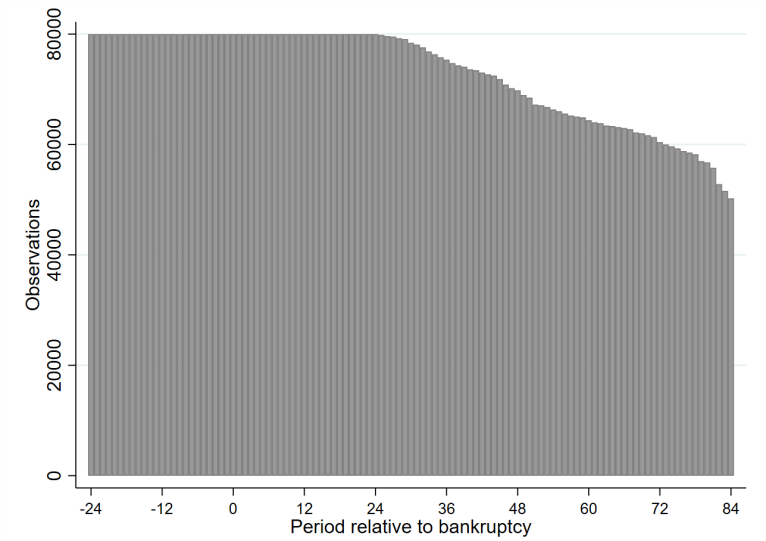
Appendix A: Additional descriptives

Figure A1: Sector shares in sample and in population



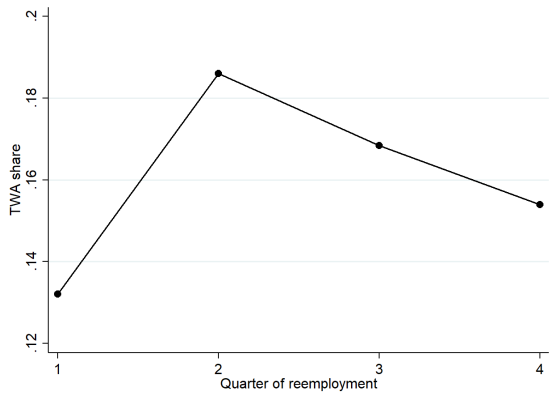
Notes: Sector categorization is done by reducing the one-sigit ISIC, which consists of 21 categories: Industry (ISIC-A through ISIC-E), construction (F), wholesale (G45-G46), retail (G47), food services and transport (H-I), commercial services (J-L), business services (M-N), public services (P-U). Note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

Figure A2: Sample size per period relative to bankruptcy

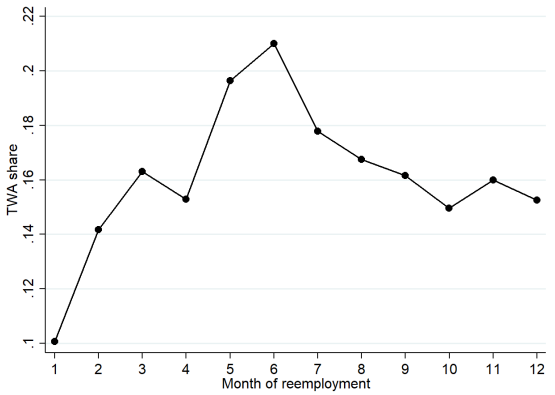


Notes: The figure shows the number of individuals present in the sample at different time periods (in months) relative to the bankruptcy date.

Figure A3: TWA transition rate by timing of re-employment



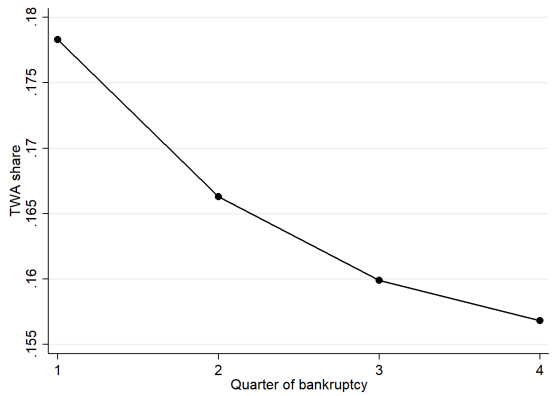
(a) By quarter of re-employment



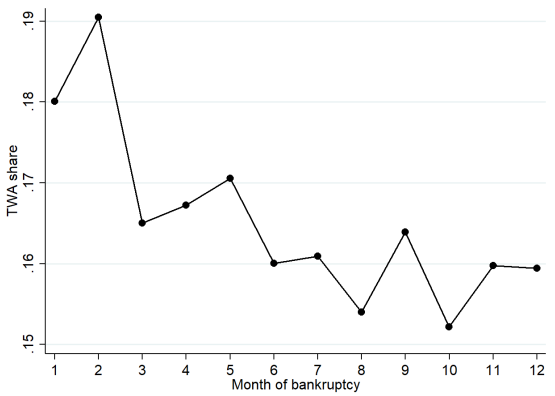
(b) By month of re-employment

Notes: The figure shows the share of employees that transitioned to a TWA job as a share of all employees in the analysis sample that 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 A4: TWA transition rate by timing of bankruptcy



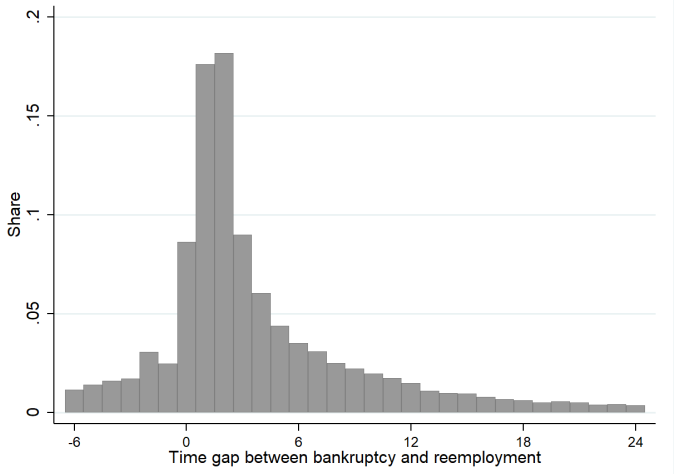
(a) By quarter of bankruptcy



(b) By month of bankruptcy

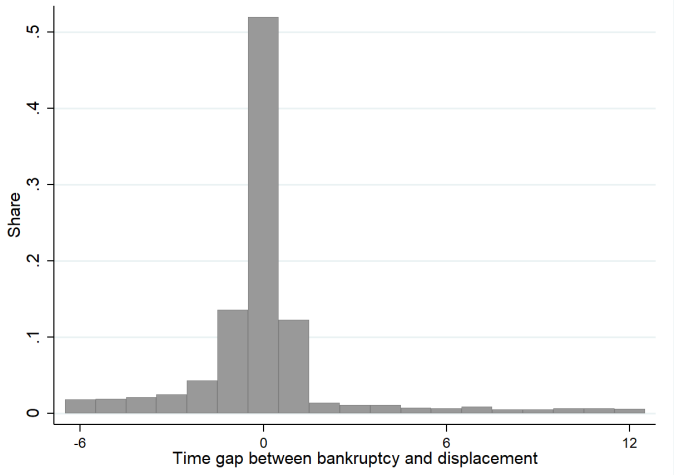
Notes: The figure shows the share of employees that transitioned to a TWA job as a share of all employees in the analysis sample that transitioned to a new job. Relative shares are calculated per calendar quarter (a) or calendar month (b) in which the firm bankruptcy occurred.

Figure A5: Timing of re-employment month relative to bankruptcy month



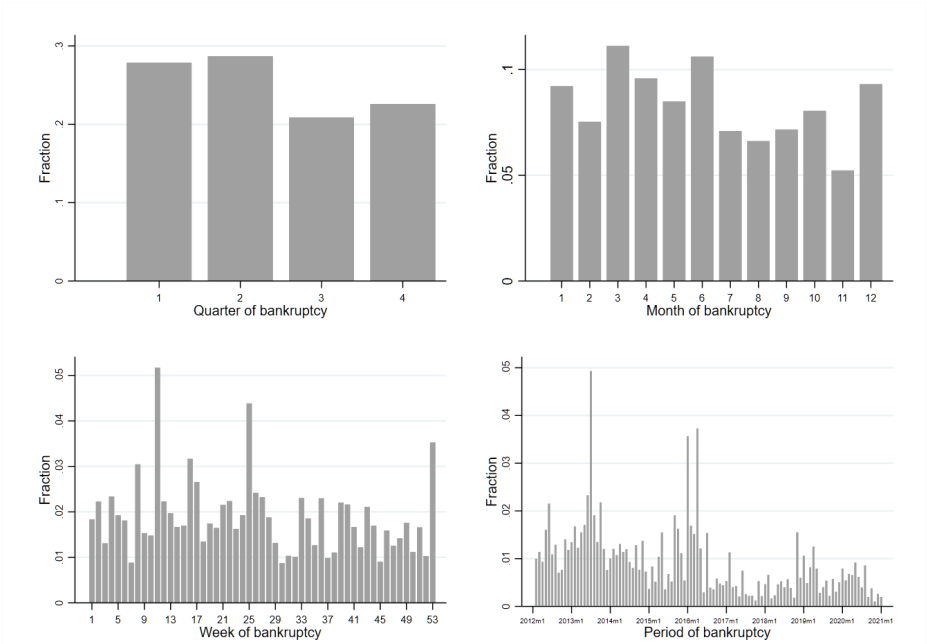
Notes: The figure shows the differences in months between the period of reemployment and the period of bankruptcy (positive values mean that re-employment occurs after bankruptcy).

Figure A6: Timing of displacement month relative to bankruptcy month



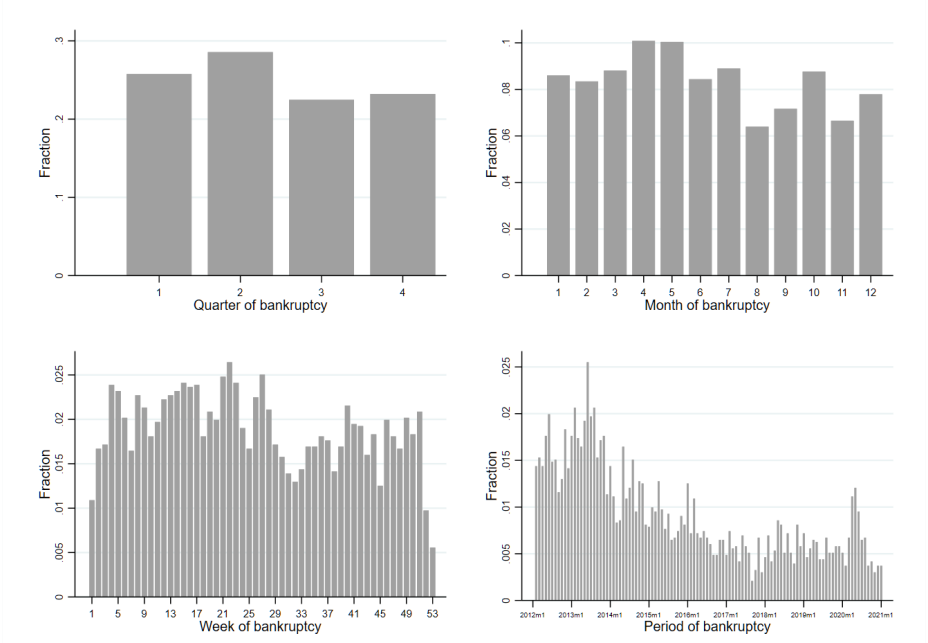
Notes: The figure shows the differences in months between the period of job displacement and the period of bankruptcy (positive values mean that job loss occurs after bankruptcy).

Figure A7: Distribution of bankruptcy timing (full sample; employee-level)



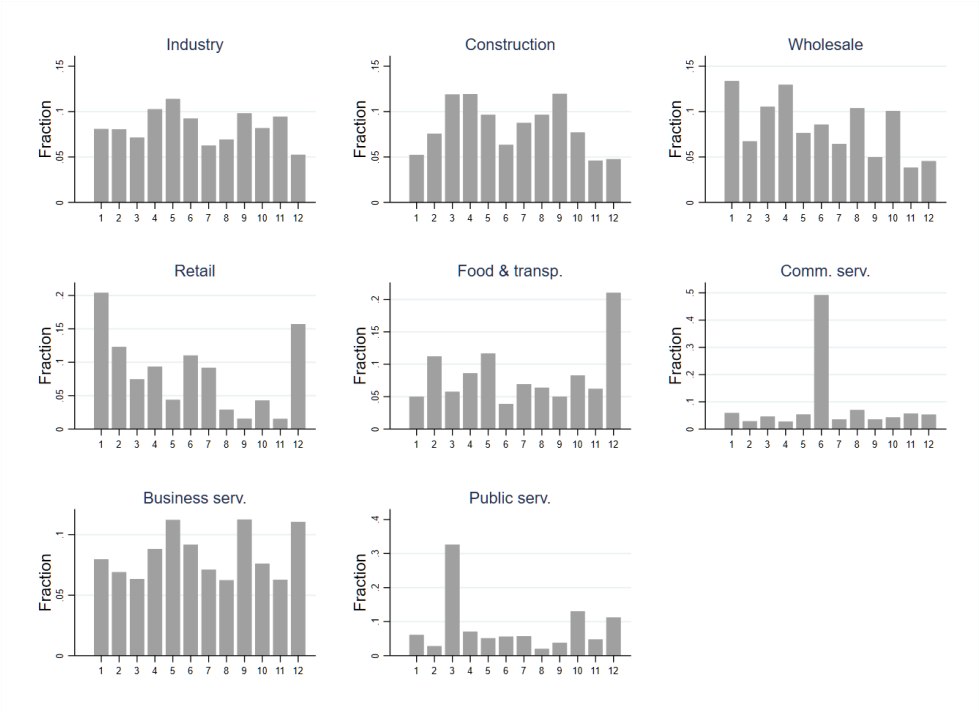
Notes: The figure shows the frequency of bankruptcies, across different time measures. Individual observations are not weighted.

Figure A8: Distribution of bankruptcy timing (full sample; firm-level)



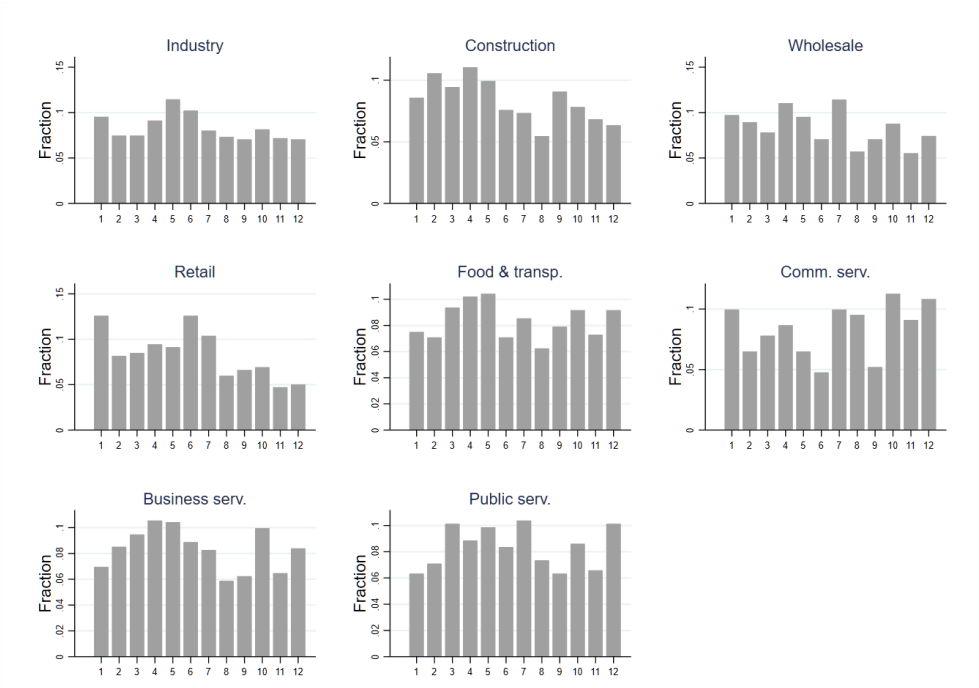
Notes: The figure shows the frequency of bankruptcies, across different time measures. Individual observations are weighted by the inverse of firm size.

Figure A9: Distribution of bankruptcy timing (by sector; employee-level)



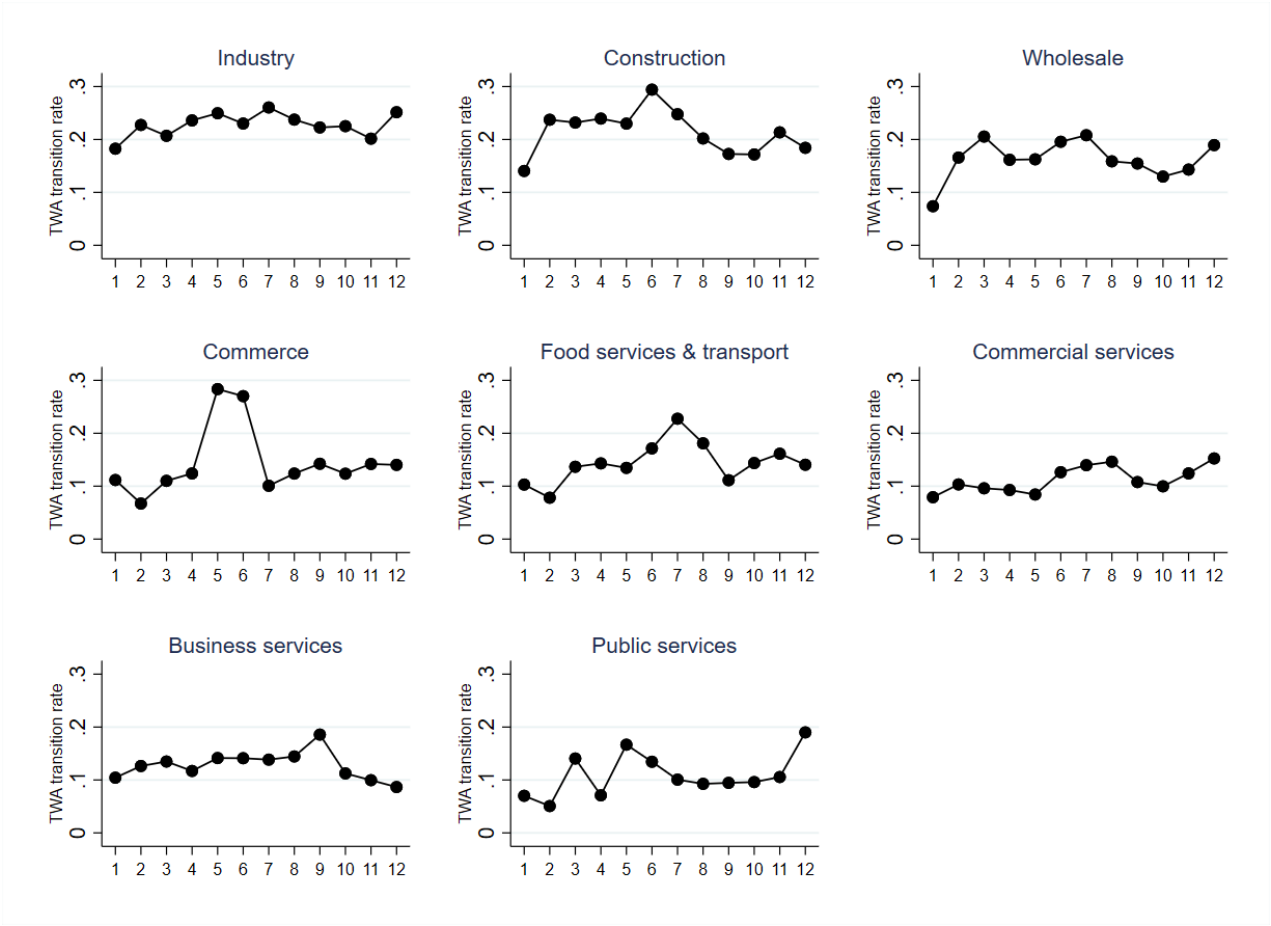
Notes: The figure shows the frequency of bankruptcies, across different time measures, separately by sector of the bankrupt firm. Individual observations are not weighted.

Figure A10: Distribution of bankruptcy timing (by sector; firm-level)



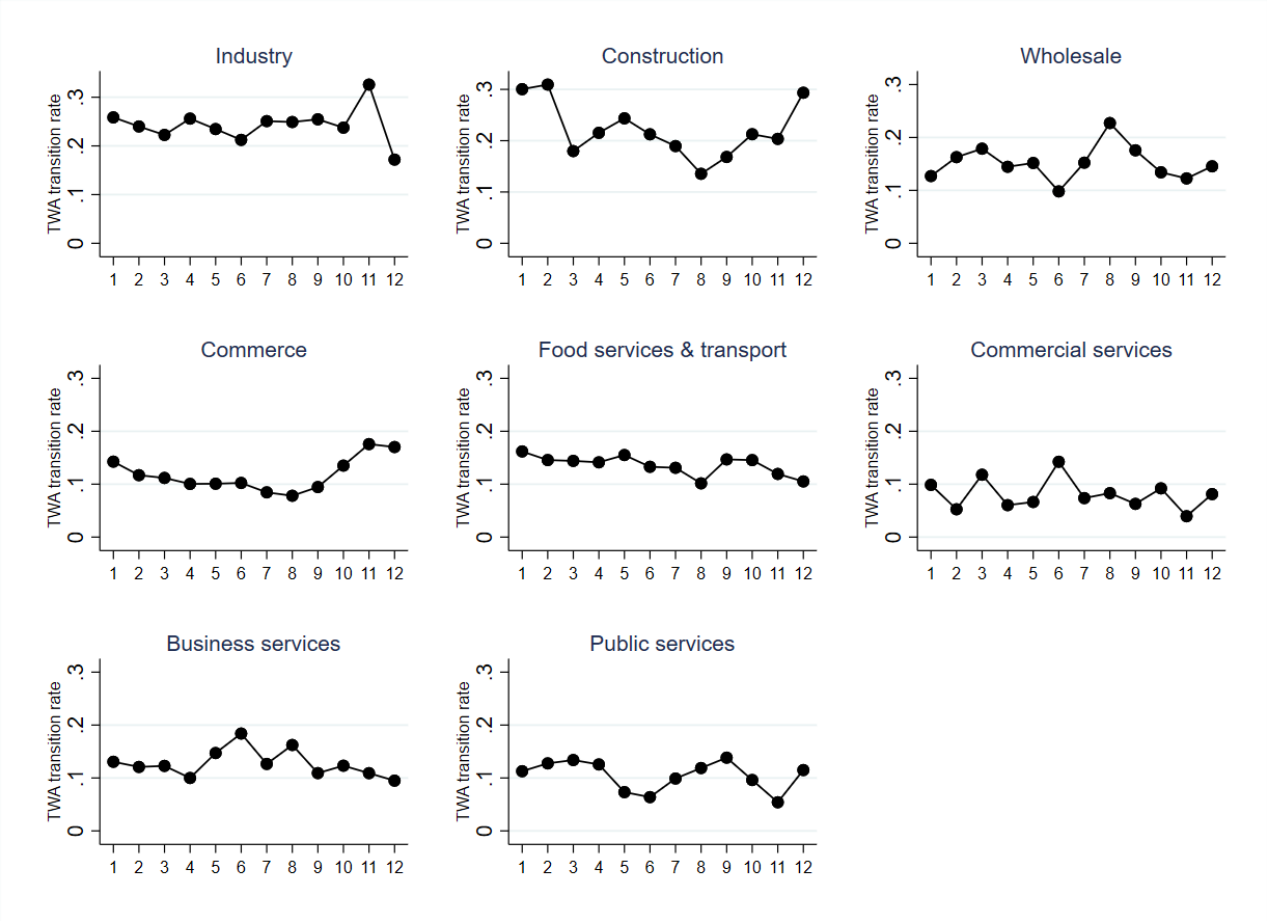
Notes: The figure shows the frequency of bankruptcies, across different time measures, separately by sector of the bankrupt firm. Individual observations are weighted by the inverse of firm size.

Figure A11: TWA transition rate by re-employment month: by sector



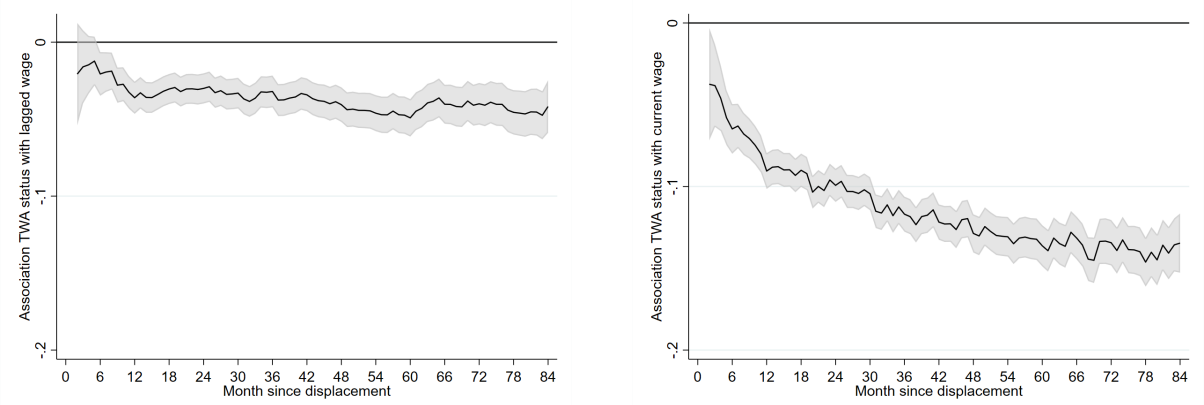
Notes: The figure shows the share of employees that transitioned to a TWA job as a share of all employees in the analysis sample that transitioned to a new job, separately by sector of the bankrupt firm. Relative shares are calculated per calendar month in which the new job is started.

Figure A12: TWA transition rate by bankruptcy month: by sector



Notes: The figure shows the share of employees that 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 A13: 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.

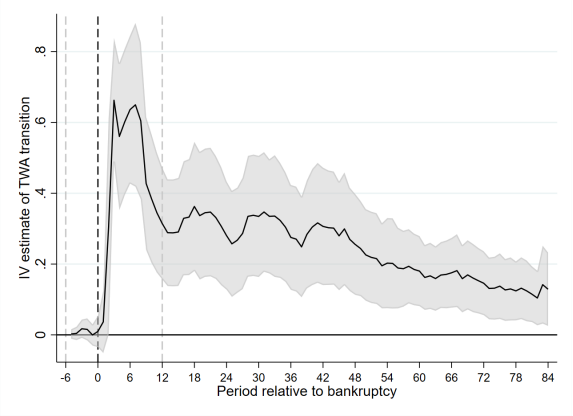
Table A1: Summary statistics: all employees vs. sample (2016)

	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	

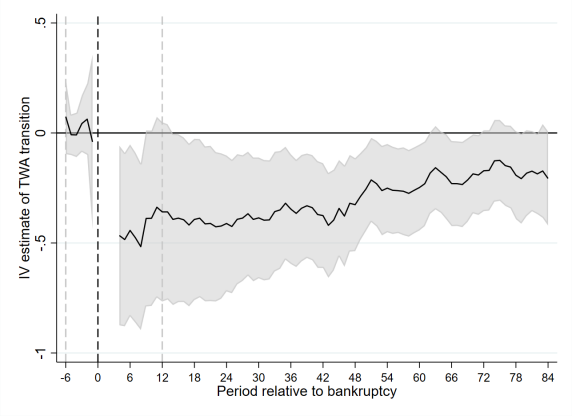
Notes: Because of computational limitations, statistics are provided for all those employed in 2016. Accordingly, labour market statistics for the analysis sample are also based on 2016 only.

Appendix B: Additional IV estimates

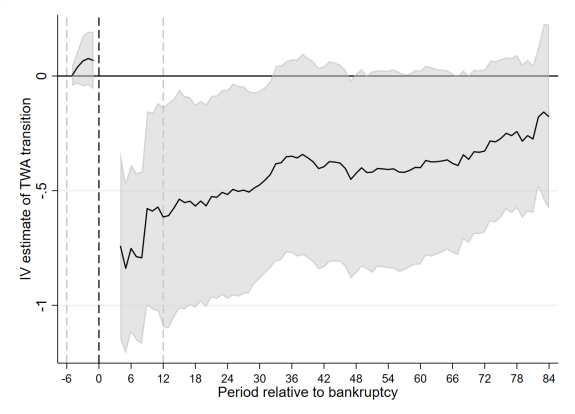
Figure B1: Effect of transitioning to TWA job on additional labour market outcomes



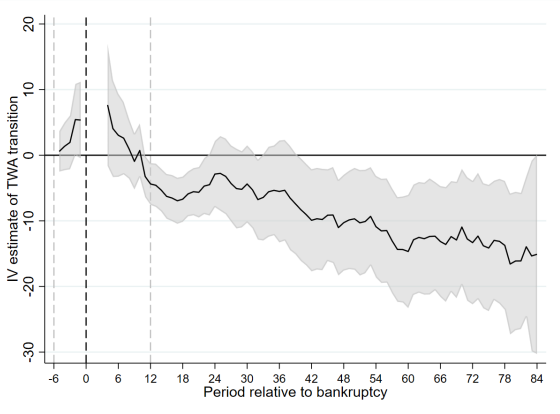
(a) Holds TWA job



(b) Holds permanent contract



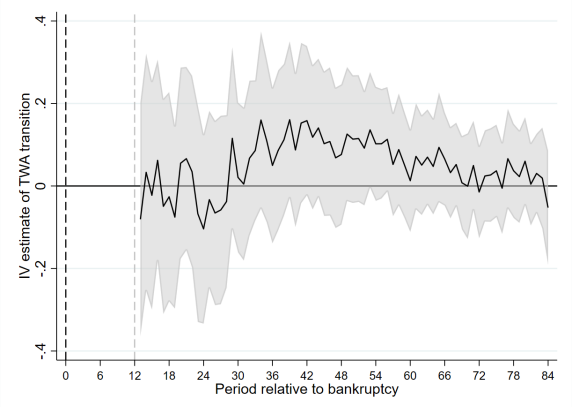
(c) Works in sector of origin



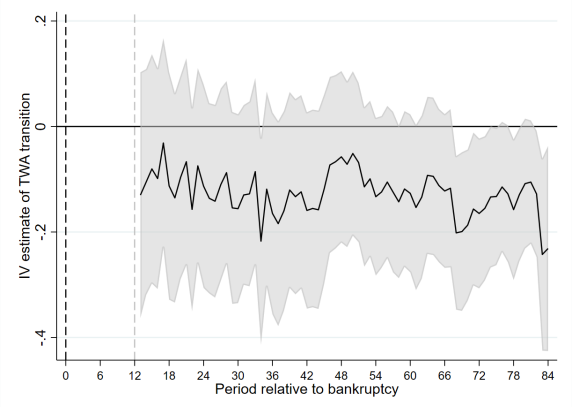
(d) Job tenure (in months)

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).

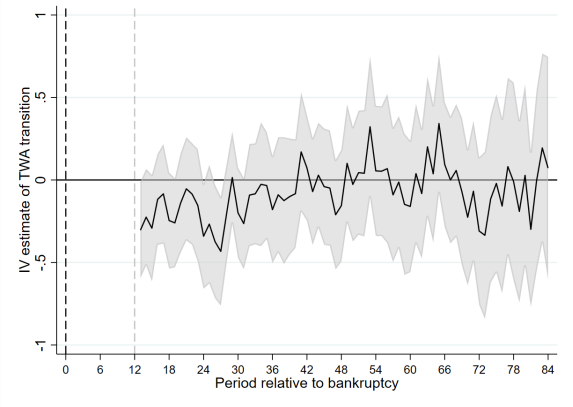
Figure B2: IV effects for labour market outcomes: stayers and leavers



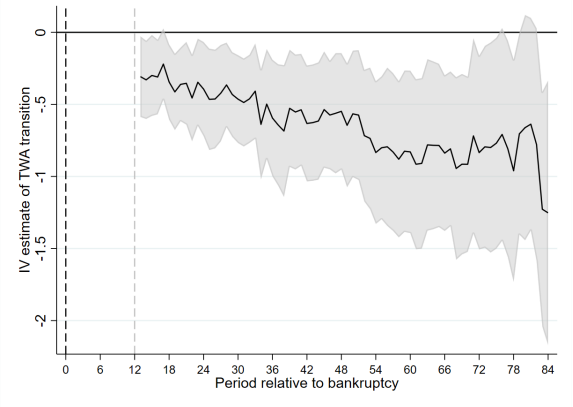
(a) Leavers: hours worked



(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 meausured in months. Standard errors are robust and corrected for clustering at the level of the bankrupt firm.

Table B3: Robustness tests: varying estimation approach

	Baseline	Macro controls	21 sectors	4 sectors	Z by week and sector	Other TWA definition
Employment	-0.049	-0.047	-0.023	-0.139*	-0.096***	-0.111**
	(0.049)	(0.048)	(0.050)	(0.076)	(0.030)	(0.044)
Hours worked	0.026	0.018	0.027	-0.023	-0.045	-0.050
	(0.063)	(0.063)	(0.065)	(0.093)	(0.040)	(0.053)
Hourly wage	-0.201***	-0.200***	-0.222***	-0.211**	-0.241***	-0.132**
	(0.078)	(0.078)	(0.074)	(0.106)	(0.055)	(0.064)
Earnings	-0.180*	-0.189*	-0.206**	-0.241	-0.288***	-0.185**
	(0.104)	(0.105)	(0.101)	(0.152)	(0.073)	(0.082)
	Alternative BM	BM adjacent months	Pre-covid	Split sample	Month fixed effects	Re-empl. month
Employment	-0.088*	-0.177*	-0.085	-0.044	-0.056	-0.075*
	(0.048)	(0.104)	(0.052)	(0.050)	(0.051)	(0.040)
Hours	-0.104*	-0.015	-0.020	0.029	-0.001	-0.052
	(0.057)	(0.131)	(0.062)	(0.063)	(0.066)	(0.053)
Hourly wage	-0.111**	-0.326**	-0.202**	-0.171**	-0.246***	-0.159**
	(0.055)	(0.138)	(0.079)	(0.075)	(0.078)	(0.064)
Earnings	-0.216**	-0.363**	-0.231**	-0.144	-0.252**	-0.213**
	(0.087)	(0.183)	(0.107)	(0.103)	(0.106)	(0.083)

Notes: Baseline = main IV results; Macro controls = include additional control variables for GDP, unemployment rate and vacancy rate; 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; Other definition TWA = defining TWA work by ISIC code of firm; Alternative BM = defining bankruptcy month by month in which largest share of employees is laid off; BM adjacent months = calculating TWA shares as average of bankruptcy month, previous month and following month; Pre-covid = excluding income periods and bankruptcies from 2020 and beyond; Split sample = split sample approach to calculating instrument; Month fixed effects = including calendar month fixed effects as additional controls. Re-empl. month = uses the month of re-employment for the construction of the instrument instead of the bankruptcy month. Sample size equals 79,987 for all estimates.

Table B4: Robustness tests: varying estimation sample

	Baseline	With temp workers	Include low FTE	Exclude firms 10-19 workers	Exclude firms 10-49 workers
Employment	-0.049	-0.078	-0.069*	-0.055	-0.119**
	(0.049)	(0.054)	(0.041)	(0.047)	(0.046)
Hours worked	0.026	0.003	0.054	0.037	0.003
	(0.063)	(0.072)	(0.073)	(0.064)	(0.061)
Hourly wage	-0.201***	-0.187**	-0.187**	-0.177**	-0.200**
	(0.078)	(0.086)	(0.071)	(0.074)	(0.073)
Earnings	-0.180*	-0.190*	-0.138	-0.145	-0.197*
	(0.104)	(0.113)	(0.091)	(0.102)	(0.103)
N	79,971	103,366	95,979	67,351	49,593
	Exclude TWA sector	Age 30-55	Displacement window [-3,3]	Re-employment window [-6,12]	Balanced panel
Employment	-0.034	-0.062	-0.048	-0.025	-0.074**
	(0.044)	(0.044)	(0.044)	(0.044)	(0.037)
Hours worked	0.041	-0.004	-0.013	0.038	-0.063
	(0.059)	(0.056)	(0.054)	(0.057)	(0.045)
Hourly wage	-0.163**	-0.178***	-0.155**	-0.152**	-0.207***
	(0.075)	(0.061)	(0.074)	(0.071)	(0.063)
Earnings	-0.127	-0.186**	-0.174*	-0.120	-0.279***
	(0.094)	(0.078)	(0.100)	(0.091)	(0.076)
N	66,827	50,960	69,992	68,737	41,506

Notes: Baseline = main IV approach; With temp workers= including those who were temporary workers at time of displacement; include low FTE = including those who worked less than 20 hours at time of displacement; exclude TWA sector = exclude those for which the bankrupt firm was registered as part of TWA sector; Age 30-55 = only include those aged 30-55 at time of bankruptcy; Displacement window [-3.3] = only consider employees who leave bankrupt firm between [-3,3] months rather than [-6,12]; Re-employment window [-6,12] = only consider employees who find new employment within 12 months; Balanced panel = only include those observed in all periods from [-24,84].

Appendix C: Difference-in-differences estimates

Below are results from a formal DiD model, which interacts the treatment variable, which equals 1 if the individual transitions to a TWA-job as the first job post-bankruptcy, with all post-displacement time dummies.

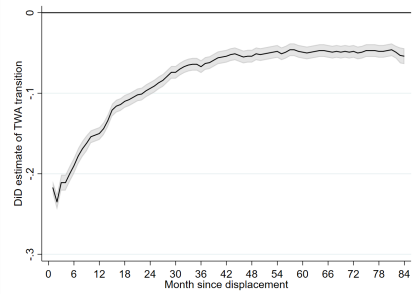
$$Y_{it} = \alpha_0 + \beta TWA_i + \gamma_t + \sum_{t=0}^{t=84} \delta_t(\gamma_t * TWA_i) + X_i\beta + F_f + \varepsilon_{it} \tag{3}$$

Note that time periods $gamma_t$ run from $T = -12$ to $T = 84$ and are defined here relative to the period of displacement. In contrast, the IV approach defines periods relative to the period of firm bankruptcy.

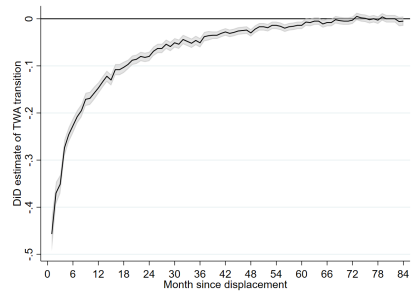
The second figure below separately estimates treatment effects for treated individuals that are still working in a TWA job in period t, and treated individuals who do not work anymore in a TWA job in period t. For this purpose, we add a triple interaction between treatment, post-displacement time dummies, and currently working a TWA job:

$$Y_{it} = \alpha_0 + \beta TWA_i + \gamma_t + \sum_{t=0}^{t=84} \delta_t(\gamma_t * TWA_i) + \sum_{t=0}^{t=84} \theta_t(\gamma_t * TWA_i * TWA_{it}) + X_i\beta + F_f + \varepsilon_{it} \tag{4}$$

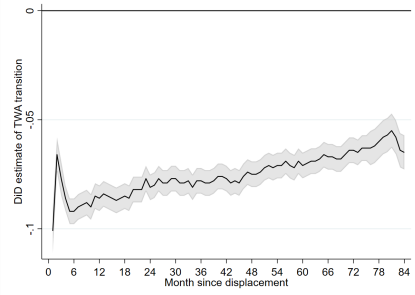
Figure C1: Difference-in-differences estimates



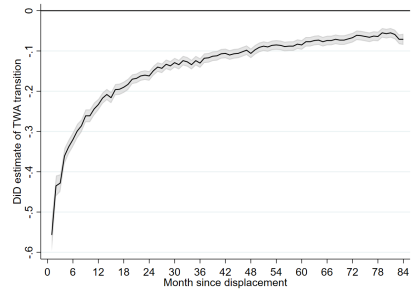
(a) Employment



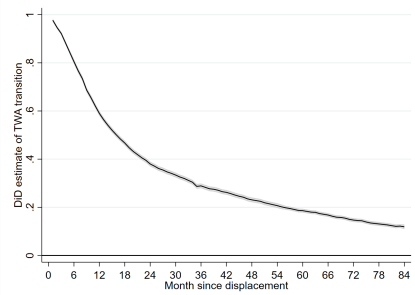
(b) Log hours worked



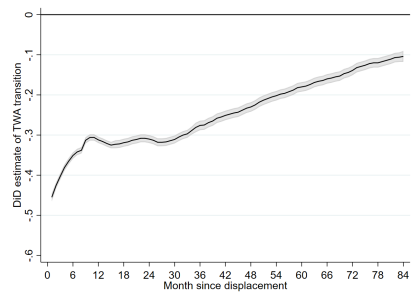
(c) Log hourly wage



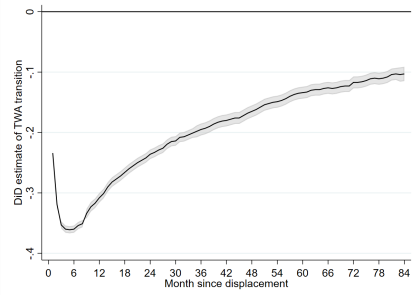
(d) Log earnings



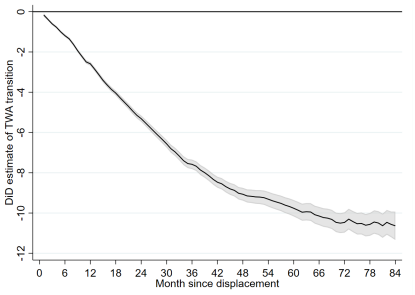
(e) Holds TWA job



(f) Holds permanent contract

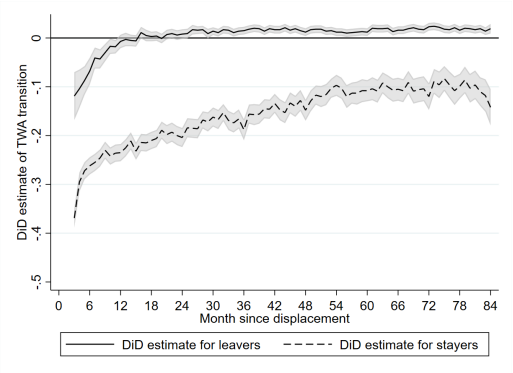


(g) Works in sector of origin

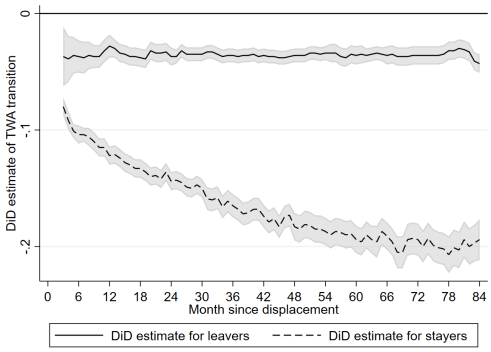


(h) Job tenure

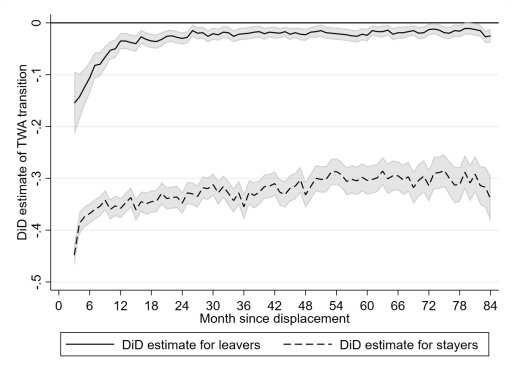
Figure C2: Difference-in-differences estimates: stayers and leavers



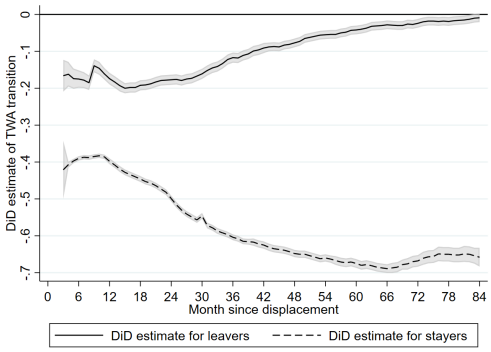
(a) Log hours worked



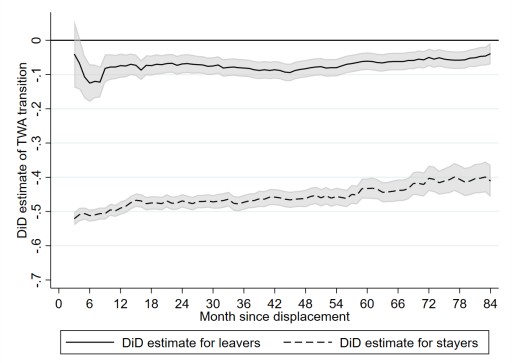
(b) Log hourly wage



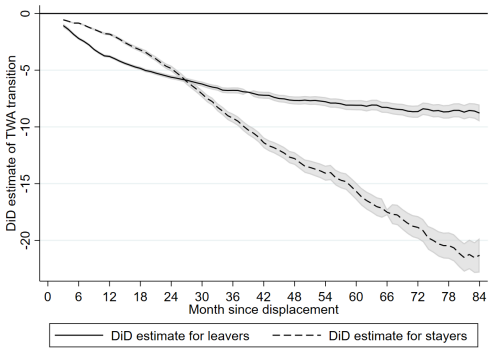
(c) Log earnings



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