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Consequences of job loss for routine workers

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Abstract

Routine-biased technological change has led to the worsening of labour market prospects for workers in exposed occupations as their work has increasingly been done by machines. Routine workers who have lost their jobs in mass displacement events are likely to have been a particularly affected group, due to potential difficulties in finding new employment that matches their skills and experience. In this study, the annual earnings, employment, monthly wages and days of unemployment of displaced routine workers are compared to those of displaced non-routine workers using Swedish matched employer-employee data. The results show substantial routine-occupation penalties among displaced workers, which persist in the medium to long term. Compared to displaced non-routine workers, displaced routine workers lose an additional year's worth of pre-displacement earnings and spend 180 more days in unemployment. A possible channel for this effect is the loss of occupation- and industry-specific human capital, as routine workers are unable to find jobs similar to those they had before becoming displaced. I do not find evidence that switching to a non-routine occupation reduces routine workers' losses, but rather there are indications that switchers do worse in the short-to-medium run. The findings suggest that the effects of labour-replacing technological change on the most exposed individuals can be severe and difficult to ameliorate.

Keywords: Routine-biased technological change; Mass layoffs

JEL codes: J63; O33

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1. Introduction

Over the last several decades, automation of tasks that had previously required human labour has taken place at a rapid pace. While this has led to increased labour productivity (Graetz and Michaels, 2018), concerns as to the effects of labour-replacing technology on worker welfare and income inequality have been raised both within academia and in the broader public debate (Acemoglu and Autor, 2011). The consensus in the literature is that automation has indeed been a contributing factor to increased income inequality in developed countries in recent decades, with the main effect operating through its impact on the occupational distribution of the workforce. Machines have tended to replace workers in middle-skill, middle-wage manufacturing and clerical occupations, while complementing labour in high-skilled managerial and professional positions. The share of employment in low-wage service jobs, which have been relatively unaffected by automation, has also increased. Overall, this has led to the workforce being increasingly polarised in terms of occupational skills and wages (Autor, et al., 2003; Goos et al., 2016). The reduction in routine employment has likely involved a large number of involuntary job separations, as firms have laid off workers whose input is no longer required in production. However, evidence on how routine workers fare following involuntary job loss has been scarce. Indeed, most previous work has focused on the aggregate labour market effects of technological change rather than the impact on exposed workers. Nevertheless, there are studies suggesting that workers in declining occupations have suffered from reduced employment and earnings (Edin et al., 2019) and that workers in routine occupations have seen lower wage growth than other worker categories (Cortes, 2016). Theory suggests that displaced routine workers should do worse than displaced non-routine workers, as they are likely to have a harder time finding a new job that fits their occupation-specific skills, in addition to facing the loss of good employer-employee matches, firm-specific human capital and rents as all workers do. Indeed, the direct exposure of involuntarily displaced routine workers suggests they could be among the biggest losers of automation and technological change. The magnitude of their losses might provide an approximate upper bound on the detrimental effects of labour-replacing technology. In this paper, I seek to establish whether routine workers are more affected by a common type of involuntary job displacement, namely establishment-level closures and mass layoffs.

The evidence on how establishment shutdowns and mass layoffs affect worker outcomes is extensive and overwhelmingly negative. Since the pioneering paper by Jacobson et al. (1993), studies have almost invariably found that job loss has severe impacts on workers' subsequent employment, earnings and even health (Sullivan and von Wachter, 2009; Davis and von Wachter, 2011). Worker outcomes do not regain the levels of comparable controls who avoid losing their jobs for many years, resulting in a seemingly permanent scarring effect. These results hold in practically all countries where this question has been investigated; Eliason and Storrie (2006) show that Swedish workers do not recover in terms of labour market outcomes even 12 years after losing their jobs. There is evidence that displaced workers fare worse when demand for either labour in general or for their particular occupation- or industry-related skills is low due to aggregate economic conditions (Davis and von Wachter, 2011), local occupation-specific labour demand (Galaasen and Kostol, 2018) and import competition (Dauth et al., 2021). This suggests that if some occupations experience rising demand due to complementary technological change, while others decline due to automation, the experiences of workers in these occupations should be different following job loss. Furthermore, there are indications that displaced workers whose skills are not in demand suffer larger losses than their peers

(Nedelkoska et al., 2022). The first comparison of the post-layoff outcomes of routine and non-routine workers is conducted in a recent paper by Blien et al. (2021), whose results point to substantial penalties for routine workers in terms of earnings and employment, but only insignificant effects on their wages.

In order to assess differences in the size of routine and non-routine workers' post-layoff losses, I use a standard difference-in-difference event study approach. Displaced individuals' labour market outcomes at different time points preceding and following layoff are compared to those of similar non-displaced workers. Routine and non-routine workers who lose their jobs are compared to corresponding groups of non-displaced peers. Matching on a large set of characteristics, including age, gender, education, tenure, size of closing establishment, size of local labour market as well as broad industry and occupation categories ensures that the groups of displaced and non-displaced workers are observationally comparable. Detailed Swedish matched employer-employee data enable me to identify all those who lost their jobs in plant shutdowns or mass displacement events during the 1997-2014 period, although occupational information is missing for a fraction of individuals. Individual worker outcomes are tracked for ten years following the year an establishment shuts down or experiences a mass layoff. The aim is to be as representative of all displaced workers as possible, including small workplaces (5-49 employees), older workers aged 51-62 and all public sector workers (including civil servants).

The results show that layoff penalties are significantly more adverse for routine workers than for non-routine workers. Their labour income falls by 20 percentage points more than that of non-routine laid off workers in the year following displacement, and remains significantly lower for eight years. This drop is mostly due to lower re-employment probabilities for displaced routine workers; the probability of not being employed in the year following displacement is 11 percentage points higher for routine workers than for their non-routine counterparts. The monthly wages of laid off routine workers also drop five log points more than what is the case for comparable non-routine workers. Seen from another perspective, routine workers spend 90 additional days in unemployment in the first post-displacement year. Overall, the evidence suggests that workers exposed to automation suffer greatly when they are displaced from their jobs. The estimated effects are larger than those found in studies that have considered individuals in routine or otherwise declining occupations in general, without focusing specifically on mass layoffs (Cortes, 2016; Edin et al., 2019). A share of these losses may be due to losses of occupation and industry-specific human capital, as routine workers are more likely to find new employment outside of their original occupation and sector. This view is reinforced by the fact that displaced routine workers are likely to move to lower-paying industries and to end up with lower earnings compared to other workers in their new occupation. Switchers from routine to non-routine occupations do worse in terms of earnings than those who continue doing routine work. This is in line with earlier results on costs of occupational mobility increasing in task and skill distance (Cortes and Gallipoli, 2018; Robinson, 2018), but contrasts with Cortes' (2016) findings that switchers from routine to non-routine cognitive occupations see wage increases. This difference could be due to a higher prevalence of involuntary switchers among displaced workers.

The analysis is important for establishing the external validity of the findings of Blien et al. (2021), as it is conducted using high-quality data from another country. Furthermore, the number of days spent in unemployment is studied as an outcome, providing more concrete evidence as to whether reductions in employment and earnings are involuntary. Finally, unlike Blien et al. (2021), I find significant negative effects on displaced routine workers' wages and show that they are more likely to transition across industries.

The remainder of this paper is structured as follows. Section 2 describes the data used, explains how routineness is defined, provides descriptive statistics for displaced and non-displaced workers and covers the labour market outcomes included in the study. The empirical model estimated is presented in Section 3 and the results, along with robustness checks, heterogeneity analysis and a discussion of mechanisms are shown in Section 4. Section 5 concludes.

2. Data

2.1 Selection of displaced and control samples

I use a rich micro-level dataset created by Statistics Sweden which contains information on all Swedish employment relationships. Data on occupation are collected in the Wage Structure Statistics dataset and are available for all public sector and a large sample (about half) of private sector workers. The probability of a private firm being sampled is determined by its size, with large firms overrepresented. If there is no information on a worker's occupation in the current year, it is imputed using reported occupations in the three preceding years on the condition that the worker has remained at the same establishment. Years before 1996 are excluded because converting the old occupational codes to the new system is very difficult. In all cases, the focus is on a worker's main place of employment during a given year. This is defined as the establishment where the worker had his or her highest source of earnings that year.

The displacement and control groups of establishments are defined based on the change in the number of workers for whom they are the main place of employment. Shutdowns are defined as cases where an establishment ceases to be the main workplace of any worker. The establishment is required to exist in year t_{-1}, to be the main workplace of at least one individual in the event year t_0, and to no longer be the main workplace for anyone in t_1, the year after the event. If the number of individuals who have their main place of employment at an establishment falls by at least 80 percent from t_{-1} to t_1, this is classified as a mass layoff event.[1] Events where more than 30 percent of the displaced workers end up at other establishments in the old workplace's firm or in the same unique establishment at a different firm in t_1 are excluded from both the displacement and control groups. This is standard in the literature because these cases are likely to be firm mergers, acquisitions or reorganisations rather than real job displacement events (Hethey-Maier and Schmieder, 2013). Establishments with fewer than five workers in t_{-1} are also excluded in order to reduce the possibility of individual worker characteristics having a large impact on overall plant performance. This size restriction is among the most permissive used in the literature. The control group of establishments consists of those that had at least five employees in t_{-1} and did not experience a shutdown or mass layoff from t_{-1} to t_0 or from t_0 to t_1.

Workers who had their main place of employment at a closing establishment in the year t_{-1} immediately preceding the shutdown or layoff event are categorised as displaced. Early leavers are thus captured in the displaced sample as there is no requirement that individuals work at the

[1] Similar cutoffs are used by e.g. Davis and von Wachter (2011). A number of studies also include events such as employment decreases of 30 percent or more. However, it is problematic to use such a cutoff when including small establishments, as changes in the establishment not related to mass layoffs might lead to such employment shifts.

shutting establishment in t_0, the year of shutdown or layoff. Also, workers who stay at their old workplaces following a mass layoff are included in the displaced sample to avoid the issue of selectiveness in terms of who gets laid off. To ensure that the individuals studied have a sufficiently strong connection to the shutting establishment, they are required to have a tenure of at least two years (defined as having their main place of work at the shutting establishment in the years t_{-2} and t_{-1}). This restriction is less stringent than what is typically used in the literature and aims to minimise the number of workers with a strong degree of attachment to the closing establishment who are excluded. The control pool consists of those who were employed at a control establishment in the year t_{-1} and had tenure of at least two years. There are no conditions on what happens to control workers or their establishments beyond t_{-1}; it is possible for them to themselves become displaced at a later point in time. This avoids the downward bias on displacement loss estimates that appears when the control group is defined conditional on never being displaced (Krolikowski, 2018). Workers who are younger than 22 or older than 62 in the year prior to layoff are dropped from both the displaced and control groups. The sample thus includes older workers, who are sometimes excluded in other studies. Older workers are not followed after they reach the age of 65, as this is the typical retirement age. In order to ensure that the workers considered are at least somewhat consistently attached to the labour market, the sample is limited to those who earn at least three times the tenth percentile-level blue-collar monthly wage in each of the years t_{-4} through t_{-1}.[2] This restriction also entails dropping workers who are not continuously observed in the Swedish registry data in the four years prior to the real or placebo displacement event. As an additional safeguard against including individuals only tenuously attached to the labour market, workers who were registered as unemployed for 183 days or more in any of the years t_{-4} through t_{-2}, or for 330 days or more in the year t_{-1} are excluded. The more liberal restriction on the year t_{-1} aims to exclude as few early leavers as possible; this concern arises because days spent in unemployment begin rising for displaced workers already in t_{-1}. Workers who are not observed in both the years t_0 and t_1 are also removed from the sample because their post-layoff outcomes are not known. Finally, individuals for whom occupational data are missing even after imputation are excluded as the routineness of their jobs cannot be determined. This final condition is the most restrictive, as occupational data are missing for 52 percent of eligible displaced and 28 percent of eligible controls. After restrictions are imposed, the eligible sample of displaced workers consists of 84,896 individuals who lose their jobs in 4,866 shutdown or mass layoff events.

2.2 Routineness definition

Routineness is defined based on the Dictionary of Occupational Titles (DOT), as has been standard in the routine-biased technological change literature since the seminal study by Autor et al. (2003). The US occupations whose task intensities are determined using the DOT are translated to the ISCO-88 international classification, which is in turn matched to corresponding Swedish occupations. Routineness is measured as the sum of an occupation's intensities in tasks that are routine cognitive ("set[ting] limits, tolerances, or standards" according to the DOT) and routine manual ("finger dexterity" according to the DOT). The sum of intensities in these two task categories is normalised by the occupation's total intensity in all tasks. This provides a

[2] The tenth percentile of blue-collar monthly wages is used as a measure of the "minimum wage", as minimum wage legislation is absent in Sweden. Wage levels are instead agreed through collective bargaining between unions and employer organisations.

measure of the share of routine tasks in the total number of tasks involved in the occupation, which should be a good measure of its exposure to automation. In the main specification, the cutoff for being classified as routine is set at the upper quartile of routineness among workers displaced in 2005, in the middle of the studied period. One quarter of the workers displaced in 2005 are therefore classified as routine and three quarters as non-routine. This results in 15 three-digit occupations being categorised as routine and 81 as non-routine. This grouping of routine and non-routine occupations is also applied for those displaced in other years. As expected, routine occupations consist exclusively of machine operating, clerical, elementary, and some crafts jobs, while non-routine occupations are typically managerial, high-skilled or service jobs. Setting a high cutoff for routineness makes it more likely that the occupations classified as such are indeed exposed to labour-replacing technological change; nevertheless any threshold is somewhat arbitrary and alternative definitions are tested. These are setting the cutoff for routineness at the occupation of the displaced worker with median routineness in 2005 and dropping the occupations of the middle two quartiles of workers entirely to only include high-routine and low-routine occupations. These alternative definitions do not qualitatively affect the results.

2.3 Descriptive statistics and matching

Descriptive statistics for displaced and non-displaced workers in terms of routineness, demographics, education, occupation, industry and location are shown in Table 1. All individual characteristics are defined based on the year t_{-1} preceding the year t_0 in which the mass layoff takes place. This should reduce the risk of changes immediately related to plant closure having an effect. The first two columns of Table 1 show that displaced workers tend to be in slightly more routine occupations than non-displaced ones. This is mainly explained by an extreme overrepresentation of manufacturing workers among the displaced; almost half of them were in a manufacturing job prior to layoff. This affects the occupational composition of the displaced, which is skewed towards operators, assemblers and crafts workers. On the other hand, it is very rare for displaced workers to be found in typically public sector industries, such as education, health and public administration. High-skilled professional workers are also underrepresented.

Because of these discrepancies, I use propensity score matching to make the two groups of workers more comparable. In the main specification, displaced and control individuals are matched on routineness, age, gender, education level, tenure, broad industry, establishment size, a measure of their municipality's urban character, and earnings in periods t_{-4} through t_{-1}. Because the analysis focuses on occupations, workers are matched within broad one-digit occupational groups. To avoid comparing trajectories of workers who were displaced in different years, matching is done within cohorts defined by the calendar year of the real or placebo event. Each displaced worker is assigned one match from the pool of controls with replacement. Workers whose propensity scores lie outside of the common support region where the propensity score distributions of the displaced and controls overlap are trimmed away. As can be seen in the top panel of Appendix Figure A1, the propensity scores of the unmatched controls skew heavily towards zero, while those of the displaced are more spread out. However, as the size of the control pool is much larger than the number of displaced, over 99 percent of the displaced workers are within the common support region. Good matches are available for practically all displaced workers, as can be seen in the bottom panel of Appendix Figure A1. The propensity score distributions among matched displaced and controls overlap almost perfectly.

Descriptive statistics for the matched samples are presented in the last two columns of Table 1. Matching within broad occupational groups ensures perfect balance in that dimension. The matched sample of controls is also very similar to the displaced in terms of routineness, demographics, education level, industry, and municipality type. To ensure robustness of results, I test alternative matching specifications where either the entire sample is used without any restrictions, or matching is done based on the covariates listed above, but excluding pre-period earnings. Neither of these other specifications produces results qualitatively different from those given by the main specification. The pre-displacement values of the outcome variables on which I do not match (employment probability, monthly wages and days of unemployment) are presented in Table A1 in the Appendix. Table A2 contains the post-matching characteristics for routine and non-routine workers (as defined by the cutoff used in the main specification) separately.

TABLE 1. DESCRIPTIVE STATISTICS FOR THE MATCHED AND UNMATCHED SAMPLES OF CONTROLS AND DISPLACED

	Controls (Unmatched)	**Displaced** (Unmatched)	**Controls** (Matched)	**Displaced** (Matched)
N individuals	1,035,499	84,896	65,069	84,325
Routine intensity	0.51	0.58	0.58	0.58
Year t_{-1}	2005.0	2004.3	2004.3	2004.3
Age	45.3	43.3	43.3	43.3
Tenure	6.2	5.6	5.5	5.6
Female	0.54	0.37	0.37	0.37
Immigrant	0.10	0.11	0.11	0.11
Education level (percentages)				
Less than compulsory	4.14	7.00	7.16	6.99
Compulsory, 9 years	7.97	12.91	13.04	12.91
High school, 2 years	31.15	32.68	33.07	32.70
High school, 3 years	16.71	21.74	21.16	21.74
Some post-secondary	13.87	12.73	12.22	12.74
University	24.29	12.14	12.52	12.14
PhD	1.86	0.79	0.82	0.78
Occupations (percentages)				
Officials & Managers	5.55	6.88	6.87	6.87
Professionals	24.16	12.47	12.47	12.47
Technicians	17.98	17.35	17.38	17.38
Clerks	9.36	11.45	11.46	11.46
Service & Sales	20.60	10.14	10.12	10.12
Crafts	6.60	11.34	11.34	11.34
Operators & Assemblers	10.79	23.70	23.80	23.80
Elementary Occupations	4.96	6.67	6.57	6.57
Industries (percentages)				
Primary	0.74	0.40	0.46	0.40
Manufacturing	21.98	48.77	48.17	48.88
Construction	2.52	2.21	2.22	2.21
Utilities & telecom	6.42	9.71	9.88	9.68
Wholesale & retail	6.96	10.36	10.67	10.35
Business services	10.63	17.09	16.92	17.03
Health, social work	29.16	7.53	7.59	7.53
Education	14.64	1.52	1.61	1.51
Public administration	6.96	2.42	2.47	2.42

Type of municipality (percentages)				
Rural municipalities	14.47	15.64	15.88	15.66
Commuter municipalities	4.45	5.62	5.64	5.51
Towns	16.28	15.00	14.88	15.05
Other cities	33.38	32.61	31.87	32.66
Suburbs of 3 largest cities	10.52	10.04	10.43	10.06
3 largest cities	20.91	21.09	21.29	21.07
Pre-period earnings (SEK thousands)				
t_{-1}	323	334	333	334
t_{-2}	316	321	323	321
t_{-3}	306	308	311	309
t_{-4}	293	294	296	295

Note: Characteristics evaluated in year t_{-1} unless stated otherwise. Unmatched control group consists of 5% random sample of the eligible control pool. One-to-one propensity score matching with replacement implemented based on characteristics listed in the table. Propensity scores estimated using logit. Matched control sample statistics weighted by the number of times a control worker was drawn as the best match for a displaced worker. Sum of matched control weights is 84,325.

2.4 Outcomes studied

The annual earnings outcome is normalised by the mean of the worker's earnings in t_{-4} through t_{-1}. This provides an individual baseline for each worker and makes the size of the estimates independent of absolute differences in pre-period earnings and wages of routine and non-routine workers. Annual earnings are measured before income tax. The employment outcome is a dummy for earning at least three times the tenth percentile-level blue-collar monthly wage within a given year.[3] Unemployment is measured as the number of days the individual is registered as unemployed or taking part in a labour market programme at the Public Employment Service. In Sweden, one must register as unemployed in order to receive benefits, meaning that instances of unemployed individuals abstaining from registering should be minimised. The days of unemployment measure represents the total number of days, including weekends and holidays, rather than only working days. Wages are the worker's monthly wages at their main workplace, measured in the second half of the year. Data on wages are available only for workers who were sampled into the Wage Structure Statistics that year. This is the same sample as the one from which occupational information is obtained (it contains all public sector workers and about half of private sector workers, with large firms overrepresented). This means that wage data are missing for many individuals in at least some years, while the other outcomes are always observed for the population of displaced and control workers.

3. Empirical specification

An event study approach typical for the literature is used. The main effect of job displacement is estimated first using a differences-in-differences model:

$$y_{it} = \sum_{\tau=-4,\tau\neq-1}^{10} [\alpha_\tau I(t = t_0 + \tau) + \beta_\tau I(t = t_0 + \tau) \times D_i] + \lambda_i + \mu_t + \varepsilon_{it} \qquad (1)$$

The model given by equation (1) regresses a labour market outcome, such as earnings or wages, on a set of dummies $I(t = t_0 + \tau)$ for years relative to the year of real or placebo displacement, which is indexed by t_0. The coefficients on year t_{-1} have been normalised to zero. The main

[3] This is analogous to the definition of employment for the purposes of determining attachment to the labour market in the pre-period. The tenth percentile of blue-collar monthly wages is once again used as a measure of the "minimum wage", as minimum wage legislation is absent in Sweden.

effects of interest are given by the set of β_τ which measure the size of the interaction effect between year dummies and actual displacement. Individual fixed effects λ_i are included to remove influences from time-invariant individual characteristics, which can affect the estimates as the panel of workers is unbalanced. General economic conditions in a given year are controlled for by calendar year dummies μ_t.

The main specification is based on Equation (1), but adds a full set of routine-time-to-event and routine-displacement indicators. A full set of routine-calendar year interactions is also included to control for general trends in routine labour market outcomes in the economy, which is necessary as the panel is not fully balanced (results using a fully balanced sample of individuals who are observed during the entire t_{-4} to t_{10} period are presented in the Appendix for comparison). The following equation results:

$$y_{it} = \sum_{\tau=-4,\tau\neq-1}^{10} [\alpha_\tau I(t = t_0 + \tau) + \beta_\tau I(t = t_0 + \tau) \times D_i + \delta_\tau I(t = t_0 + \tau) \times R_i + \gamma_\tau I(t = t_0 + \tau) \times D_i \times R_i] + \lambda_i + \mu_t + \mu_t \times R_i + \varepsilon_{it} \qquad (2)$$

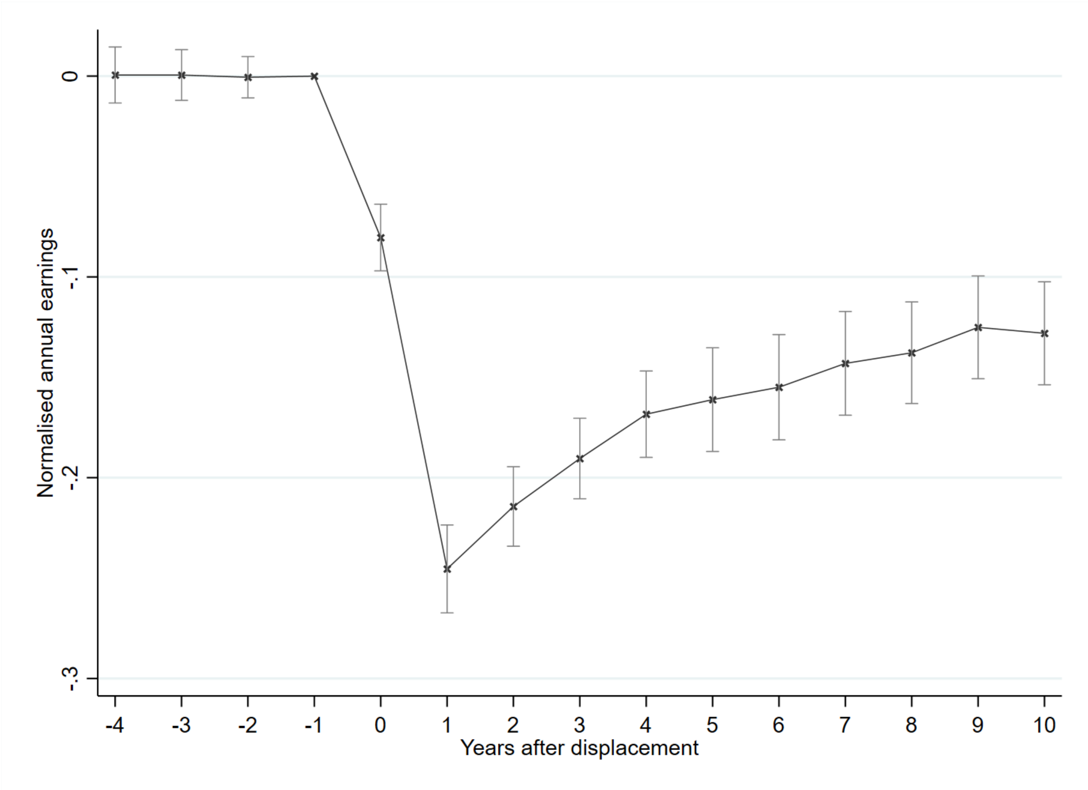
Now, the effects of displacement on non-routine workers relative to non-routine workers who are not displaced are given by the set of β_τ. The effects of displacement on routine workers relative to non-displaced routine workers are given by $\beta_\tau + \gamma_\tau$, with γ_τ capturing any differences in the displacement penalty between routine and non-routine workers. In the figures below, the non-routine series plot the estimates β_τ, while the routine series show $\beta_\tau + \gamma_\tau$. Standard errors are clustered at the level of the t_{-1} establishment in all cases.

4. Results

4.1 Post-layoff outcomes of routine and non-routine workers

The baseline estimated effects of job loss on real earnings from Equation (1) are shown in Figure 1. Earnings evolve in a very similar fashion for displaced and non-displaced workers through t_{-1}. The relative earnings of the displaced then decrease somewhat in t_0 (the last year the closing establishment is observed) before falling sharply to 25 percent less than the pre-displacement earnings level in t_1. While there is some recovery in the following years, displaced workers' earnings never regain the trajectories of their non-displaced peers, remaining 13 percent lower in t_{10}. This pattern is similar to what previous studies have found (Jacobson et al., 1993; Eliason and Storrie, 2006; Davis and von Wachter, 2011). This is in spite of some differences regarding sampling restrictions, suggesting that they do not have a qualitative effect on the findings.

FIGURE 1. BASELINE ESTIMATE OF EFFECTS OF JOB LOSS ON EARNINGS

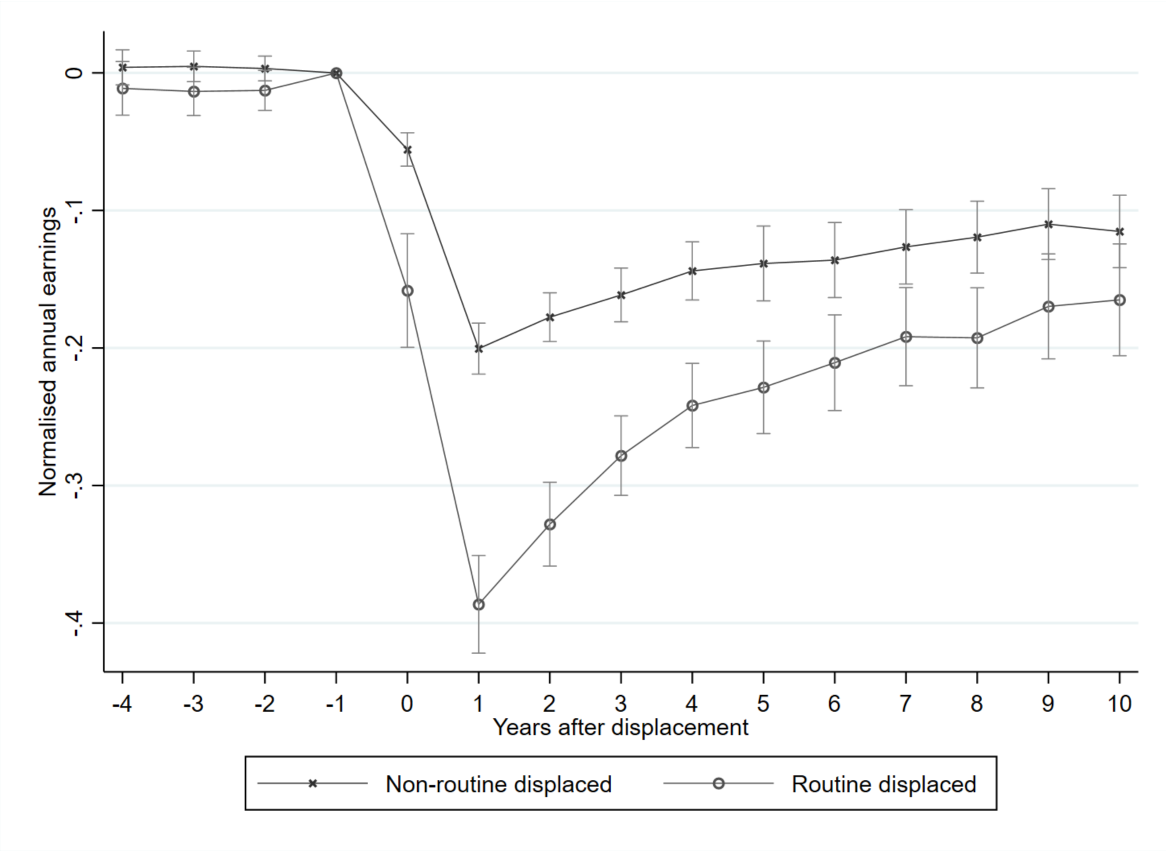


Note: The baseline period is the year before displacement, t_{-1}. Outcome of displaced relative to matched control group in each period. Standard errors clustered at the level of t_{-1} establishments. 95 percent confidence intervals shown.

The results of the main earnings specification as estimated by Equation (2) are presented in Figure 2 (the point estimates are also shown in Table A3 in the Appendix). Although the trajectories of earnings for routine and non-routine workers follow each other closely in the period up to displacement, they diverge clearly in t_0. While non-routine workers lose 20 percent of their pre-displacement earnings in t_1, their worst post-displacement year, for routine workers the corresponding share is 39 percent. However, the earnings of laid off routine workers converge with those of their non-displaced counterparts more quickly than those of non-routine workers. This means that the gap between the two groups of displaced workers narrows over time. Nevertheless, the additional penalty suffered by routine workers remains statistically significant for eight years after establishment closure. Cumulatively, non-routine workers are estimated to lose 1.26 times the amount of a year's worth of pre-displacement earnings over the t_0 to t_8 period. Routine workers are estimated to lose 2.22 times worth of their pre-displacement annual labour income over the same time frame. The convergence seems to be driven by the fact that many non-displaced routine workers have disadvantageous earnings trajectories; their real earnings are only 5.7 percent higher in t_{10} than during the t_{-4} to t_{-1} period, while the real earnings of non-routine control workers grow by 20 percent over this timeframe.[4] Indeed, by t_{10} displaced non-routine workers are estimated to have higher earnings relative to the baseline period than non-displaced routine workers.

[4] See Table A1.

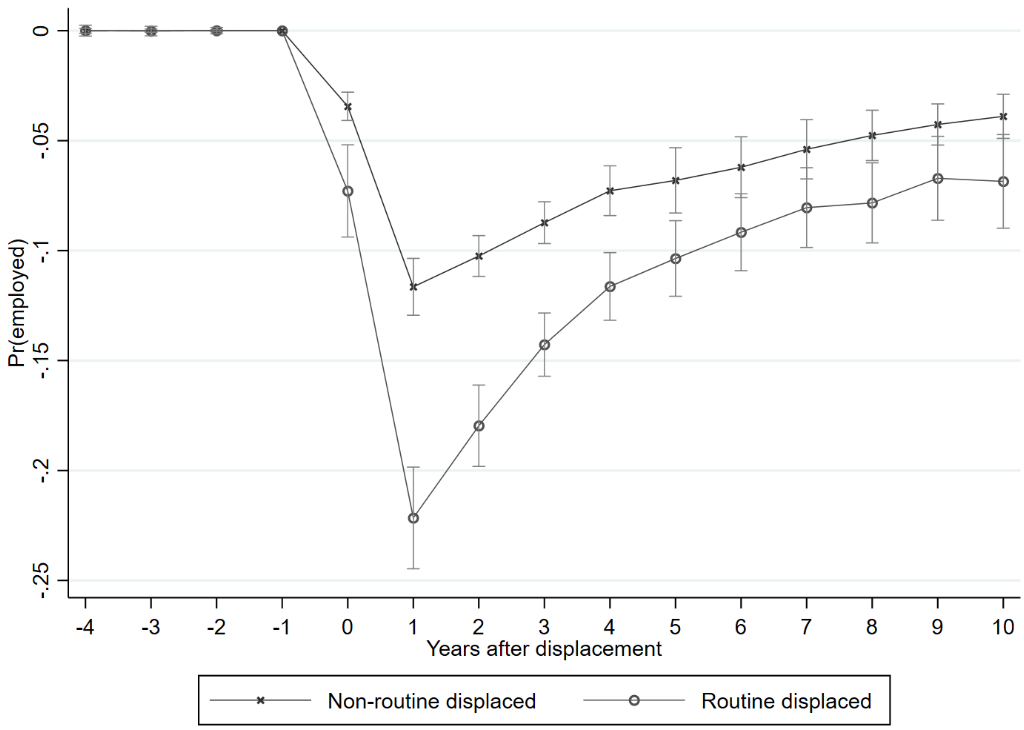
FIGURE 2. ESTIMATED EFFECTS OF JOB LOSS ON EARNINGS FOR ROUTINE AND NON-ROUTINE WORKERS, RELATIVE TO NON-DISPLACED WORKERS IN THE RESPECTIVE CATEGORY



Note: The baseline period is the year before displacement, t_{-1}. Outcome of routine and non-routine displaced relative to routine and non-routine matched controls in each period. Standard errors clustered at the level of t_{-1} establishments. 95 percent confidence intervals shown.

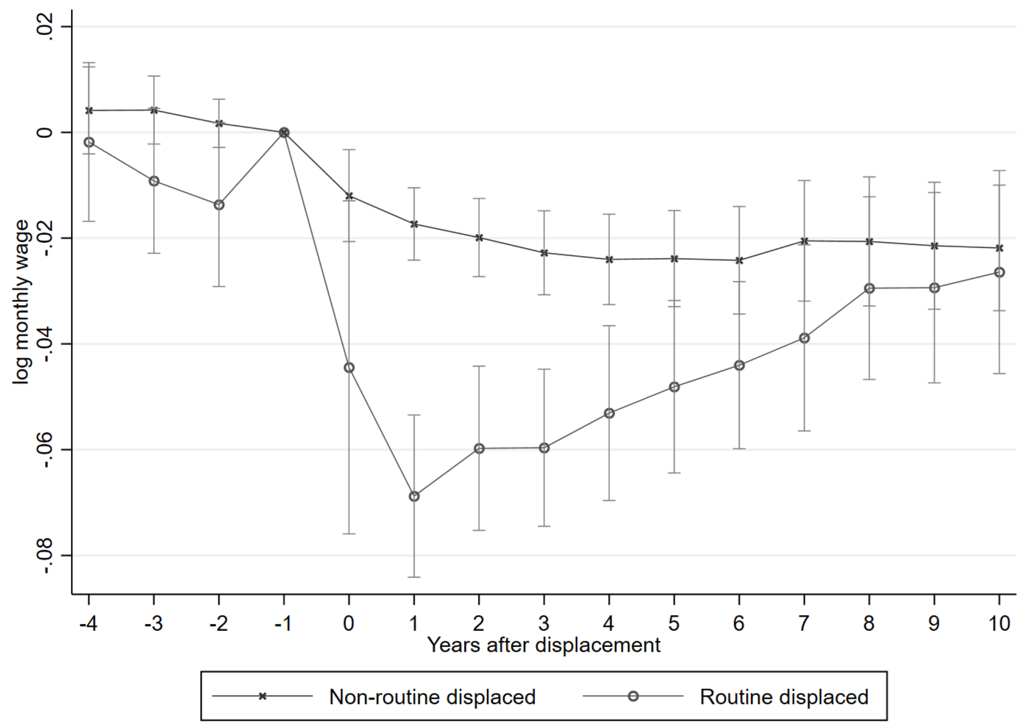
A breakdown of annual earnings losses reveals substantial adverse effects of displacement on both the probability of being employed and on wages conditional on employment. Figure 3 shows the estimated effect of displacement on the employment probability for routine and non-routine workers (the baseline employment effect for all workers is plotted in Figure A2 in the Appendix). All workers are employed by construction in the four years from t_{-4} through t_{-1} and there is thus no difference between routine and non-routine workers in this regard. However, by t_1 displaced routine workers are 11 percentage points less likely to be employed than displaced non-routine workers. This difference is persistent, and even though it narrows over time, is statistically significant through the fifth post-layoff year. Just like in the case of earnings, neither group of workers fully recovers from the shock of losing their jobs. In the case of monthly wages, results for which are shown in Figure 4, estimates are somewhat noisy because wage data are not available for the full sample of workers each year (the baseline wage effect can be seen in Figure A2). They do however indicate that routine workers suffer much more following displacement, suffering a 6.9 log point drop in wages in t_1, while non-routine workers only see wages drop by 1.7 log points. The difference remains significant for the first four post-layoff years. It seems that routine workers' wages converge more quickly to the level of their non-displaced peers than is the case for non-routine workers, whose wages do not seem to converge at all. However, it is difficult to draw any definitive conclusions about this as the point estimates for different years are noisy and not statistically distinguishable in most cases.

FIGURE 3. EFFECTS OF JOB LOSS ON THE PROBABILITY OF BEING EMPLOYED FOR ROUTINE AND NON-ROUTINE WORKERS, RELATIVE TO NON-DISPLACED WORKERS IN THE RESPECTIVE CATEGORY



Note: Baseline period is the year before displacement, t_{-1}. Outcome of routine and non-routine displaced relative to routine and non-routine matched controls. Standard errors clustered at the level of t_{-1} establishments. 95 percent confidence intervals shown.

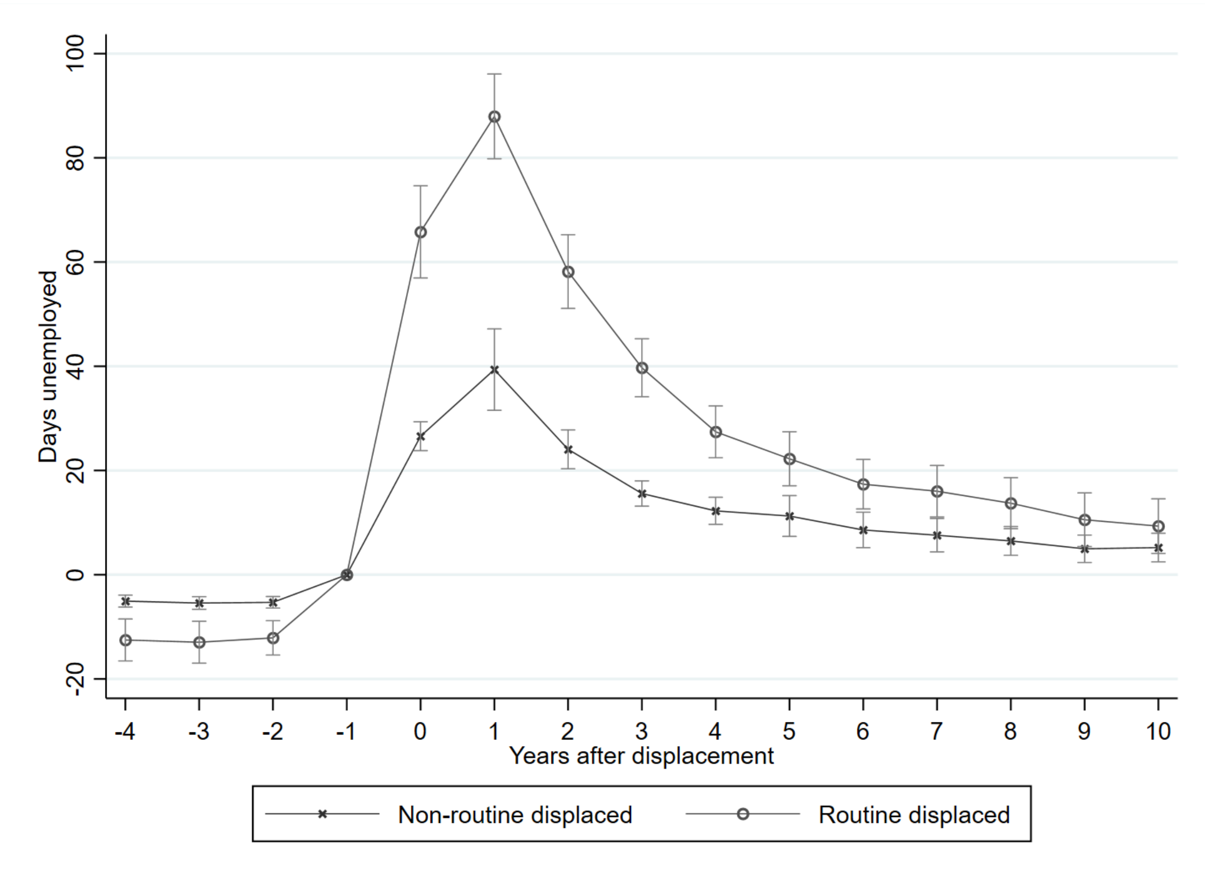
FIGURE 4. EFFECTS OF JOB LOSS ON LOG MONTHLY WAGES (CONDITIONAL ON BEING EMPLOYED) FOR ROUTINE AND NON-ROUTINE WORKERS, RELATIVE TO NON-DISPLACED WORKERS IN THE RESPECTIVE CATEGORY



Note: Baseline period is the year before displacement, t_{-1}. Outcome of routine and non-routine displaced relative to routine and non-routine matched controls. Standard errors clustered at the level of t_{-1} establishments. 95 percent confidence intervals shown.

Finally, I turn toward an alternative way of measuring adverse labour market outcomes, namely the number of days in a year registered as unemployed. According to this metric, routine workers also suffer more following displacement than non-routine ones do, as can be seen in Figure 5 (Figure A2 shows the average unemployment effects of displacement). The largest unemployment effects are observed in the year t_1, when non-routine displaced workers spend 39 more days in unemployment than their non-displaced counterparts. At the same time, displaced routine workers experience 88 additional days of unemployment. The difference in time spent unemployed is persistent and remains statistically significant, although quantitatively smaller, until the sixth post-displacement year. By this time, displaced routine workers have on average spent a total of 307 additional days in unemployment, compared to 126 days for displaced non-routine workers.

FIGURE 5. ESTIMATED EFFECTS OF JOB DISPLACEMENT ON DAYS SPENT IN UNEMPLOYMENT FOR ROUTINE AND NON-ROUTINE WORKERS, RELATIVE TO NON-DISPLACED WORKERS IN THE RESPECTIVE CATEGORY



Note: The baseline period is the year before displacement, t_{-1}. Outcome of routine and non-routine displaced relative to routine and non-routine matched controls in each period. Standard errors clustered at the level of t_{-1} establishments. 95 percent confidence intervals shown.

4.2 Robustness checks

To ensure that the results are not sensitive to the definition of routineness used or the type of matching employed, several robustness checks are employed. While the definition of routineness used in Section 4.1 makes it more likely that truly routine occupations are classified as routine due to the stringent cutoff used, this definition might be too narrow. For this reason, Equation 2 has been re-estimated using an alternative routineness definition where the workers are split by median routineness instead of classifying only the most routine quartile as routine.[5] According to this definition, 43 three-digit occupations are routine and 53 are non-routine. Another possibility is that the presence of occupations that are close to one another in terms of routineness on both sides of the threshold attenuates the results. To make sure that this is not the case, Equation 2 is estimated using only those individuals whose occupations were either in the top or bottom quartiles of displaced workers ordered by routineness in t_{-1}. This leaves the 15 occupations classified as routine in the main analysis and 30 low-routineness occupations.

These alternative definitions yield results almost identical to those given by the main specification, as can be seen in Figure 6. The top panel plots the baseline estimates of the effects of displacement on earnings, reproducing Figure 2. The panel on the bottom left shows the results when the median is used as the threshold for the routine category and the panel on the bottom right shows the results when only the top and bottom quartiles of routineness are included. Using a less stringent definition of routineness reduces the size of the estimated routine penalty in the years immediately following layoff. Also, limiting the non-routine sample to those in the lowest quartile of routineness leads to slightly larger routineness penalty estimates. These apparent differences are in line with what theory predicts. The routine penalty estimates and the post-layoff earnings trajectories are very similar in the three specifications, confirming that the way routineness is defined is not of key importance for the results. A final alternative specification where post-layoff outcomes are plotted for each of the four routineness quartiles separately is shown in Figure A3 in the Appendix. The fourth routineness quartile, which contains the workers classified as routine in the main specification, does clearly worse than the other three quartiles. The differences between the first, second and third quartiles are not as clear. In t_1, workers from the second and third routineness quartiles appear to suffer larger penalties than those in the first quartile, but the trajectories of these groups converge over the medium and long run.

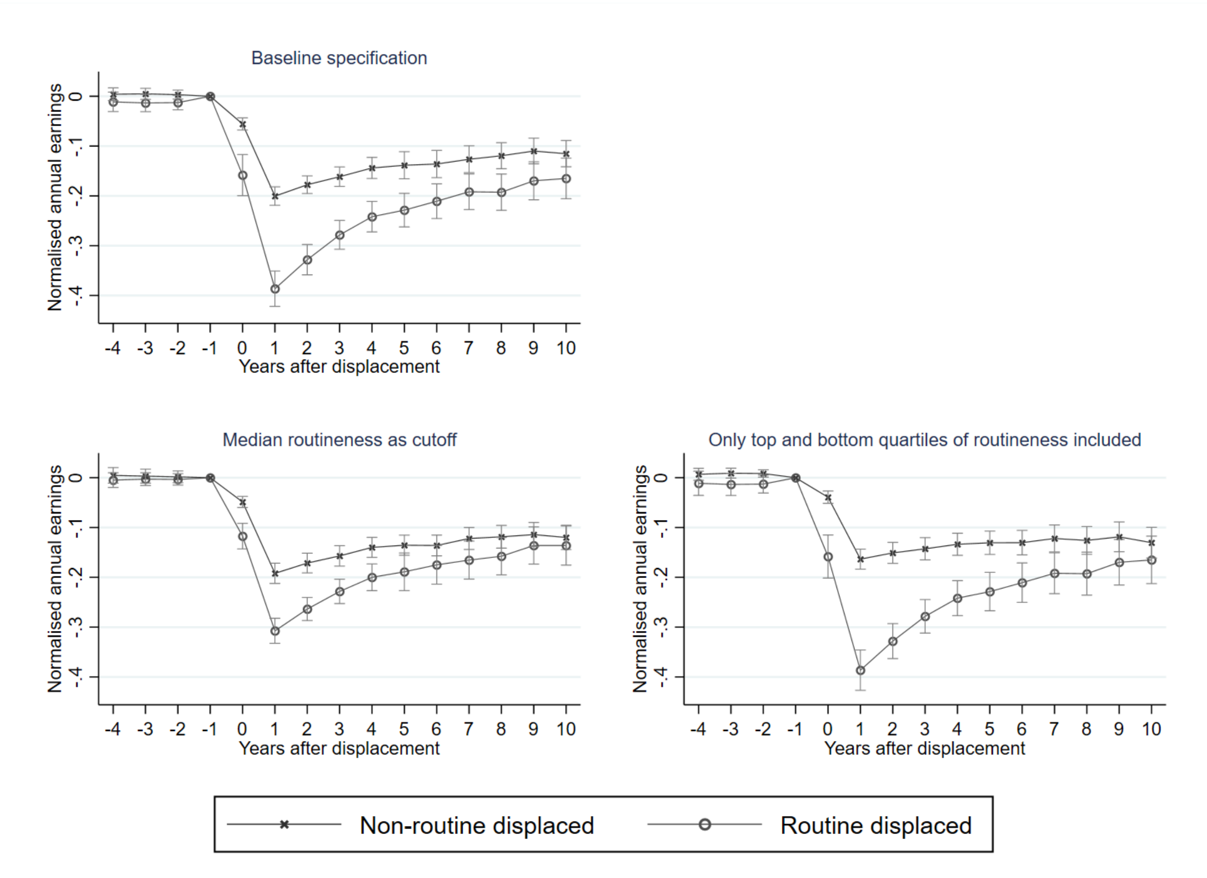
Graphs corresponding to Figure 6 for the employment and monthly wage outcomes are presented in Figures A4 and A5 in the Appendix. In the case of employment, the differences between the different definitions are small, although there are indications that the routine penalty is smaller if all workers with above-median routineness are categorised as routine. However, with this definition of routineness, the routine wage penalty becomes insignificant in all years except for t_1. However, the confidence intervals are wide enough to contain the estimates from the main specification. Results for days of unemployment using the different routineness definitions are shown in Appendix Figure A6. The definitions give similar results, except for a somewhat smaller routine penalty in t_1 when the median cutoff is used.

In addition to estimating Equation 2 using a sample matched on covariates and earnings in t_{-4} through t_{-1}, I estimate it in turn using the full unmatched sample and a sample matched only on covariates, but not pre-period earnings. These alternative samples of workers give results very similar to those obtained using the preferred sample. Their results are presented in Figure A7 in the Appendix. Point estimates of earnings penalties when the unmatched sample is used are also provided in Appendix Table A3 for all three definitions of routineness.

[5] Just like in the main specification, displaced workers who are in their t_{-1} period in 2005 are ordered by routineness, but the split is at the occupation of the median worker rather than the occupation of the third quartile worker.

Finally, Equation 2 has been re-estimated using a fully balanced panel, leaving only those workers who are observed in the Swedish registry data and are younger than 65 years of age in each of the years t_{-4} through t_{10}. This entails reducing the sample to shutdowns and mass layoffs that took place in 1997-2006, as data for years after 2016 is not available. Also, workers older than 52 years of age in t_{-1} are excluded. The results of this exercise for the outcomes of earnings and unemployment are shown in Appendix Figure A8. Using the fully balanced panel reduces the size of penalties immediately following layoff for both routine and non-routine workers (the differences from the full panel estimates are rarely statistically significant), but has no effect on penalty estimates for later years. The routine penalty remains large and statistically significant.

FIGURE 6. ESTIMATES OF DISPLACEMENT EARNINGS PENALTIES USING DIFFERENT DEFINITIONS OF ROUTINE AND NON-ROUTINE OCCUPATIONS.



Note: The baseline period is the year before displacement, t_{-1}. Outcome of routine and non-routine displaced (according to different definitions of routineness) relative to routine and non-routine matched controls in each period. Standard errors clustered at the level of t_{-1} establishments. 95 percent confidence intervals shown.

4.3 Heterogeneity in routine penalties

I consider heterogeneity in the size of routine penalties for workers with different levels of education, by sector, and within broad occupational categories. The top panel of Figure 7 shows post-displacement earnings trajectories among workers with high school education or less and among workers with more than a high school education.[6] Among less educated workers, the trajectories of routine and non-routine workers are very similar to those among the full displaced sample. Among highly educated workers, initial losses among the non-routine group are initially somewhat smaller than in the full sample, but routine workers' losses are not. The size of the penalty for routine highly educated workers decreases before seeming to actually increase again at the very end of the period studied, but this is likely to be an artefact of the small sample size, as confidence intervals are very wide.

The middle panel of Figure 7 shows earnings trajectories of workers who are displaced in the manufacturing[7] and services sectors. The results for manufacturing are similar to the findings for the full sample, albeit with indications that non-routine workers who are displaced in manufacturing do slightly worse. In the service sector, penalties for both routine and non-routine workers are lower. Also, it seems that convergence of routine workers' losses to the level experienced by non-routine workers is quicker. The difference between the two groups only remains significant through t_2 and the point estimates for t_6 and t_7 are almost identical for the groups of routine and non-routine displaced.

In the bottom panel, I test for heterogeneity depending on whether the workers are displaced in blue-collar occupations (service and sales, crafts, operators and assemblers, elementary occupations) or in white-collar occupations (managers, professionals, technicians and clerks).[8] Routine penalties among blue-collar workers are similar to those found for the entire sample. On the other hand, I find no evidence of routine penalties for white-collar displaced workers. This indicates that routine cognitive workers are able to cope with layoffs better than routine manual workers. The mechanisms behind this would be an interesting topic for further study.

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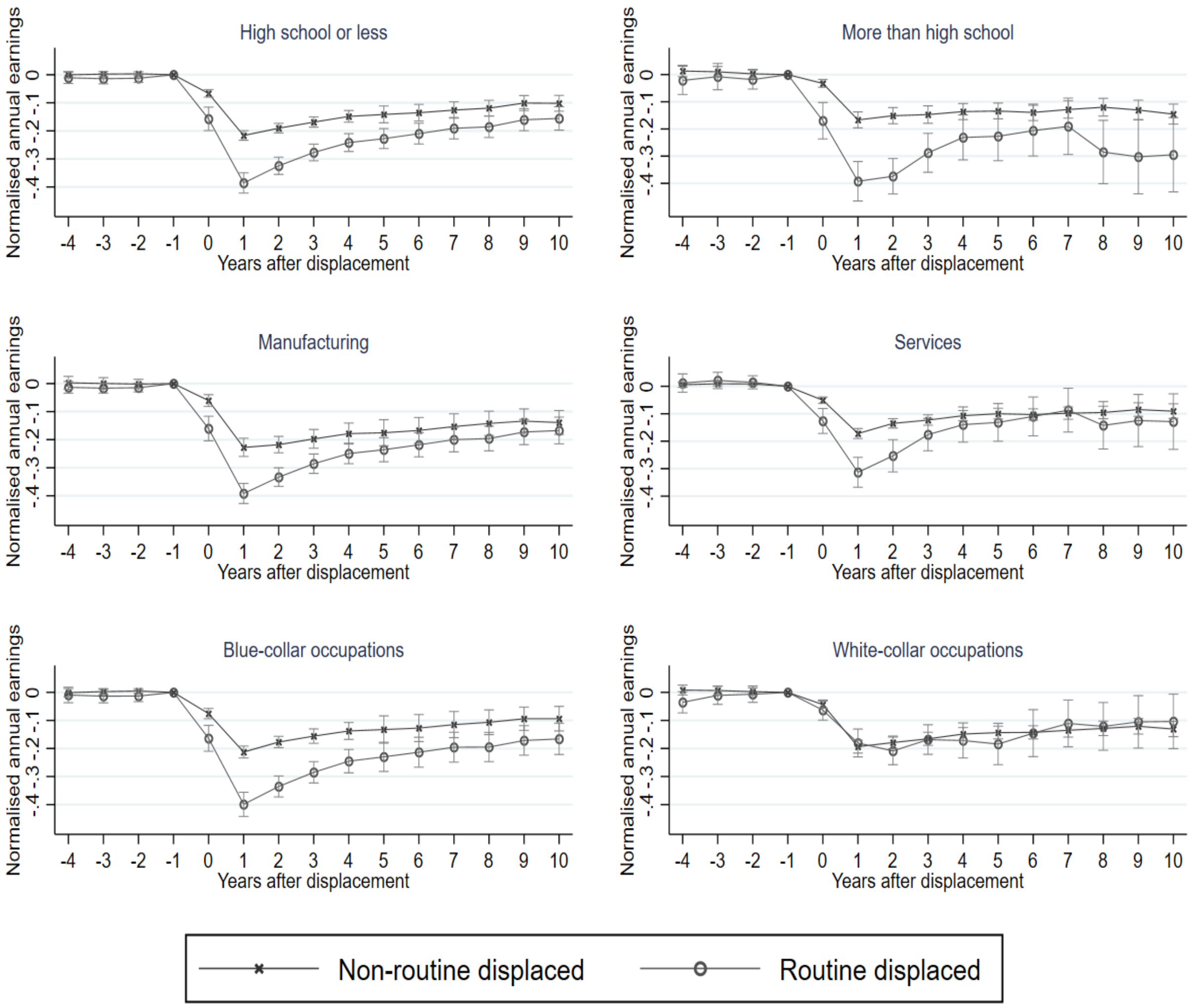
[6] Only a quarter of the displaced workers have more than high school education and routine workers are underrepresented within this group. However, placing the threshold at a lower education level is problematic due to the changes to the Swedish primary and secondary schooling systems that affected different cohorts of workers.

[7] Including primary industries.

[8] Among blue-collar workers, routine three-digit occupations (according to the main definition) are found in the broad groups of crafts, operators and assemblers and elementary occupations. Among white-collar workers, routine three-digit occupations are found among clerks.

FIGURE 7. HETEROGENEITY IN ROUTINENESS PENALTY IN TERMS OF POST-DISPLACEMENT EARNINGS BY EDUCATION LEVEL, INDUSTRY AND OCCUPATIONAL GROUP



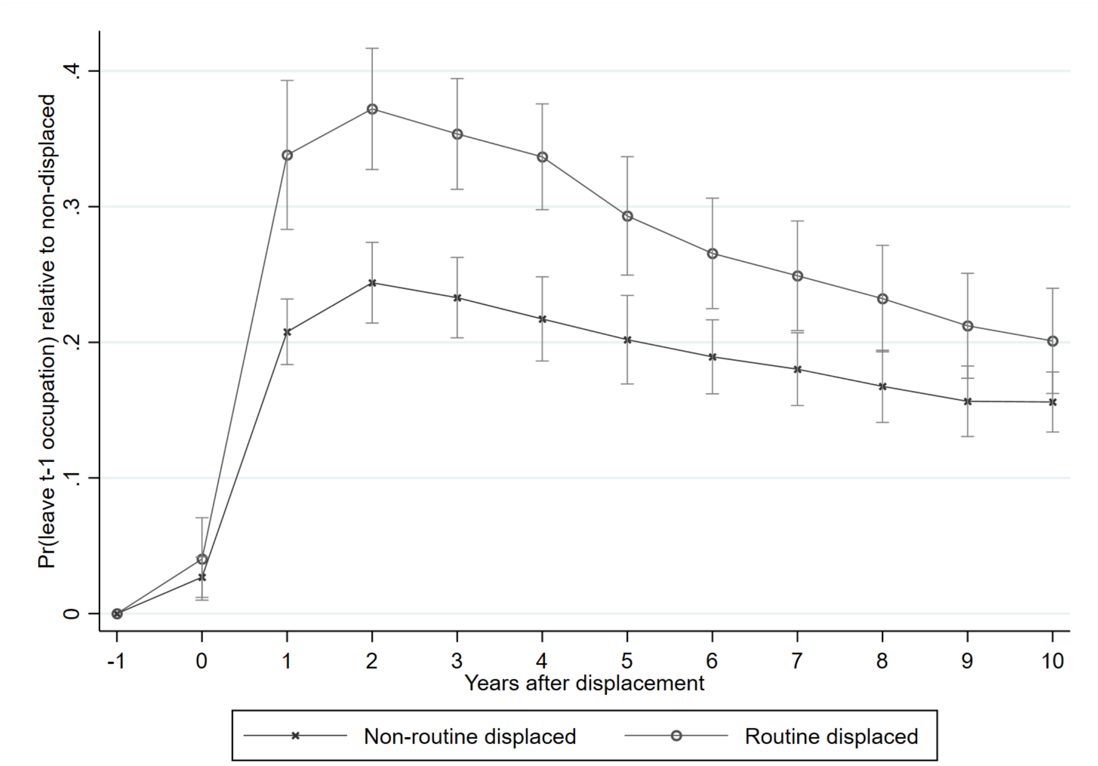
Note: The baseline period is the year before displacement, t_{-1}. Outcome of routine and non-routine displaced within the high/low educational groups, manufacturing/service industries and blue-collar/white-collar occupations relative to routine and non-routine matched controls in each period. Standard errors clustered at the level of t_{-1} establishments. 95 percent confidence intervals shown.

4.4 Mechanisms

Since routine occupations have been declining as a share of total employment, theory predicts that displaced routine workers should have a hard time finding a new job in their old occupation and may have to switch to another one when re-entering employment. This may lead both to adverse consequences in terms of earnings and wages relative to displaced non-routine workers in the short run as occupation-specific human capital is lost and to better long-run outcomes relative to routine workers who are not displaced and stay in declining occupations (Cortes, 2016). As routine occupations are concentrated in declining industries like manufacturing, displaced routine workers should be more likely to switch industry as well. The effects of industry switching are predicted to be qualitatively similar to those of occupation switching.

The probabilities that displaced routine and non-routine workers are employed in the same three-digit occupation and three-digit industry as in the t_{-1} period are shown in Figures 8 and 9 respectively. Probabilities are conditional on the workers being employed in the given period; the occupational outcome is known only for a subset of employed workers, sampled according to the description in Section 2.[9] The results show that routine workers are more likely to change both occupation and industry in the years following displacement. For occupations, this effect is estimated to be 13 percentage points one year after displacement, when it peaks. It remains statistically significant until the sixth post-displacement year, but declines to an insignificant 4.5 percentage points by t_{10}. In the case of industries, routine workers are 20 percentage points less likely to be employed in their original industry than non-routine workers in year t_1. The gap remains at this level for the duration of the period over which the workers are followed. The results are qualitatively unaffected if a fully balanced panel consisting only of workers who are observed in each of the years t_{-4} to t_{10} is used, as can be seen in the Appendix Figure A9.

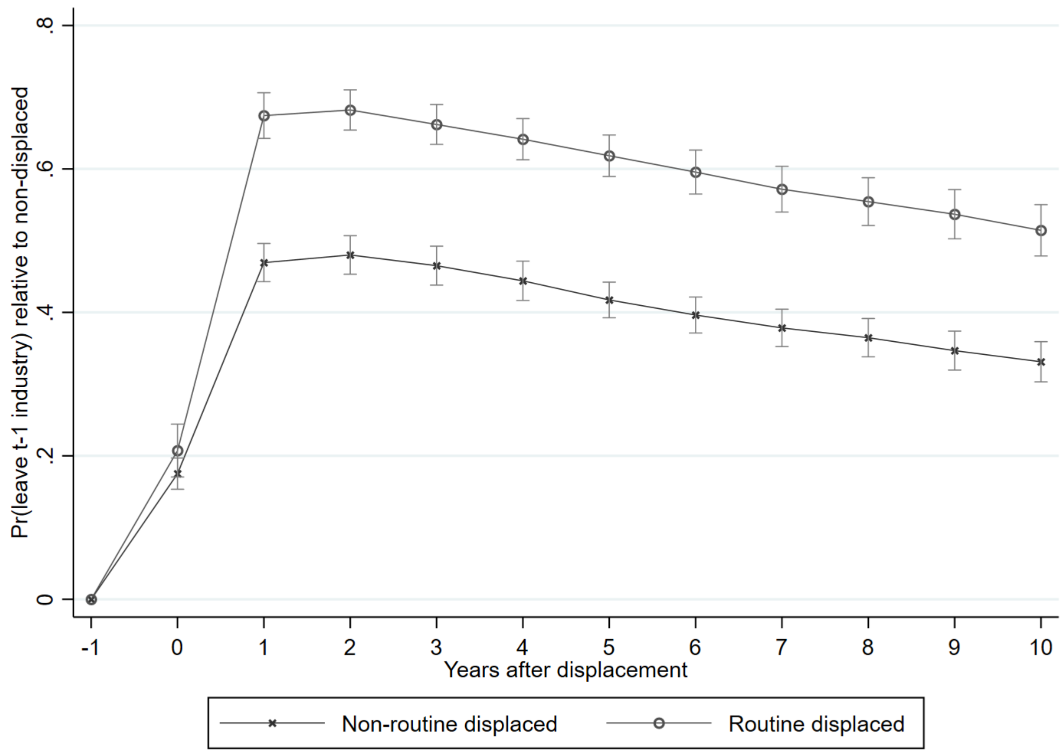
FIGURE 8. EFFECT OF DISPLACEMENT ON THE PROBABILITY OF ROUTINE AND NON-ROUTINE WORKERS BEING IN ANOTHER THREE-DIGIT OCCUPATION THAN IN t_{-1}, CONDITIONAL ON BEING EMPLOYED AND OCCUPATIONAL DATA BEING AVAILABLE.



Note: Outcome of routine and non-routine displaced relative to routine and non-routine matched controls in each period. Standard errors clustered at the level of t_{-1} establishments. 95 percent confidence intervals shown.

[9] If occupation data is missing for an employed worker in the post-period, it is imputed according to the same procedure as is followed for t_{-1} occupations, and described in Section 2. If the occupation is still unknown post-imputation, the individual is dropped from the occupation-switching regression.

Figure 9. Effect of displacement on the probability of routine and non-routine workers being in another three-digit industry than in the t_{-1} period, conditional on being employed.



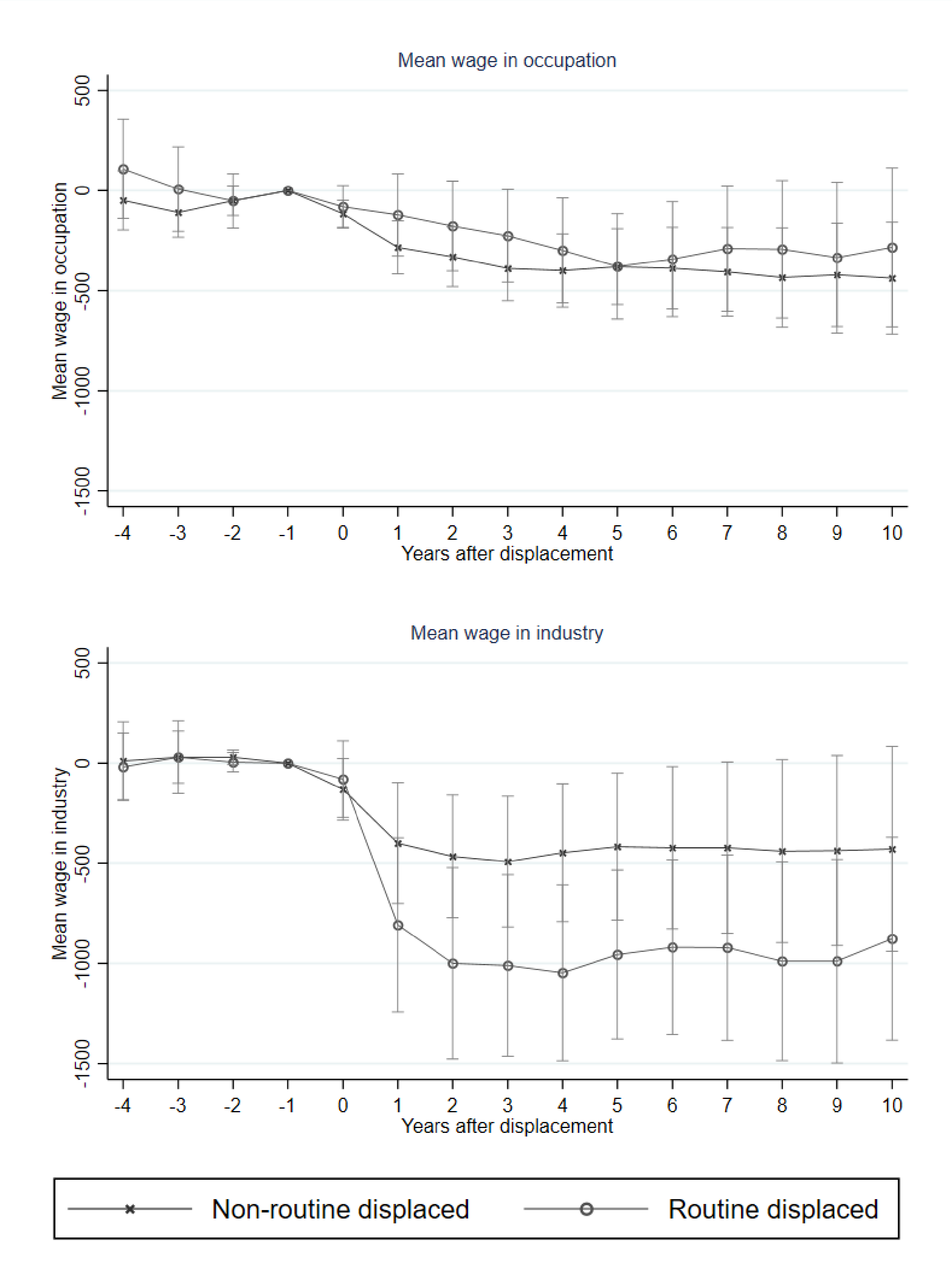
Note: Outcome of routine and non-routine displaced relative to routine and non-routine matched controls in each period. Standard errors clustered at the level of t_{-1} establishments. 95 percent confidence intervals shown.

Both the occupation and industry-switching results point to routine workers being more likely to become re-employed in jobs less similar to their pre-displacement jobs. Loss of occupation- and industry-specific human capital could therefore explain at least part of the additional short-run penalties that they suffer compared to their non-routine counterparts. However, higher rates of occupation and industry switching could also explain the faster long-term convergence of displaced routine workers' outcomes to those of their non-displaced peers. This is the case if their new occupations and industries see higher rates of wage growth than their original routine jobs; Cortes (2016) shows that routine workers who switch to non-routine jobs fare better over long time horizons than those who stay in routine occupations.

Does the higher switching rate among routine workers mean that they are more likely to move into high-paying occupations and industries? I analyse this in Figure 10, which plots mean wages in the displaced individual's occupation and industry, relative to t_{-1}. This is based on the occupations and industries of both stayers and switchers, with the restriction that the individual must be employed and have available occupation and industry information respectively. The top panel shows that both routine and non-routine displaced workers tend to end up in lower-paying occupations than they were in in t_{-1}. The estimates are imprecise however, and there is no evidence that routine workers end up in lower-paying occupations than non-routine ones. When it comes to industry, there is a clearer pattern of routine workers ending up in lower-paying industries than the one that they were displaced from. This could be seen as evidence that they lose good industry matches or industry-specific rents. Many routine workers are displaced from manufacturing industries, which tend to provide high wages for less-educated individuals. Importantly, neither group of displaced workers manages to move up to better-paid sectors.

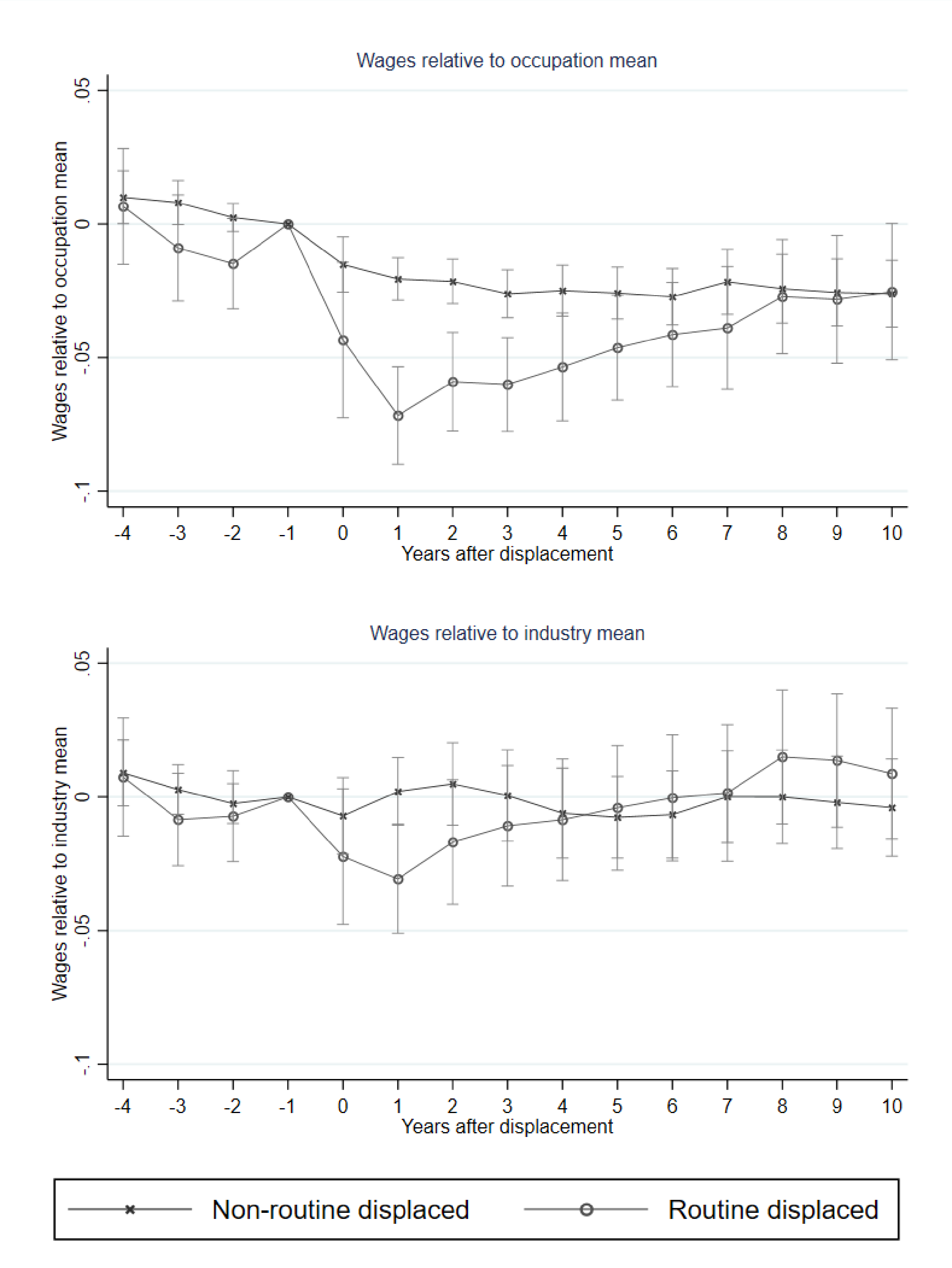
Is there evidence that routine workers move into jobs for which their human capital is less suited? I test this by considering the worker's wages as a percentage of the mean for their occupation and industry. The results are shown in Figure 11. Routine workers earn about seven percentage points less in terms of their occupation's mean in t_1, compared to about two percentage points for non-routine workers (relative to what had been the case in t_{-1}). The difference between the two groups remains significant until t_4, but the estimates for routine and non-routine workers converge in the long run. This could be due to routine workers having to enter occupations which differ more from their original one, resulting in a period of more rapid human capital accumulation. There are no clear patterns when it comes to wages relative to the industry mean.

FIGURE 10. WAGE LEVELS IN DISPLACED ROUTINE AND NON-ROUTINE WORKERS' OCCUPATIONS AND INDUSTRIES (CONDITIONAL ON BEING EMPLOYED AND OCCUPATIONAL INFORMATION AVAILABLE)



Note: Outcome of routine and non-routine displaced relative to routine and non-routine matched controls in each period. Standard errors clustered at the level of t_{-1} establishments. 95 percent confidence intervals shown.

FIGURE 11. WAGE LEVELS OF DISPLACED ROUTINE AND NON-ROUTINE WORKERS AS SHARE OF THE OCCUPATION OR INDUSTRY MEAN (CONDITIONAL ON BEING EMPLOYED AND OCCUPATIONAL INFORMATION AVAILABLE)



Note: Outcome of routine and non-routine displaced relative to routine and non-routine matched controls in each period. Standard errors clustered at the level of t_{-1} establishments. 95 percent confidence intervals shown.

As a final test of how occupational switching affects workers, the outcomes of displaced workers who are in a different occupation in t_5 are compared to those of displaced workers who remain in their t_{-1} occupation. This exercise is clearly endogenous to factors such as worker skill, motivation and local labour market conditions and is limited by the fact that occupations are observed for only a fraction of employed workers in the post-period. Nevertheless, it yields interesting indicative results as shown in Table 2, where the outcome is average relative earnings over the t_1 - t_5 period. Almost four fifths of the employed displaced routine workers with available occupation data were in another three-digit occupation in t_5; a full 70 percent of these switchers had gone to a non-routine occupation. Among non-routine workers, only six out of ten had switched out of their initial line of work. Of these, 92 percent went to another non-routine occupation. There is no evidence that switching occupations leads to higher earnings over the years t_1 - t_5. On the contrary, switching seems to be especially detrimental for routine workers, as they expect to lose seven percent of pre-displacement income annually if they switch to another routine occupation and 18 percent if they switch to a non-routine occupation. The losses for non-routine workers are smaller, at two percentage points if they are in another non-routine occupation and nine percentage points if they are in a routine occupation. Confidence intervals are tight and the estimates for stayers and switchers within each category are distinguishable at conventional significance levels. The results are evidence of loss of occupation-specific human capital hurting all workers, but especially routine ones. The better long-term prospects of non-routine occupations do not seem to help routine workers who switch into them in the short and medium run. These workers instead appear to lose more than those who switch to other routine occupations, which are more similar to the pre-displacement occupation in terms of tasks.

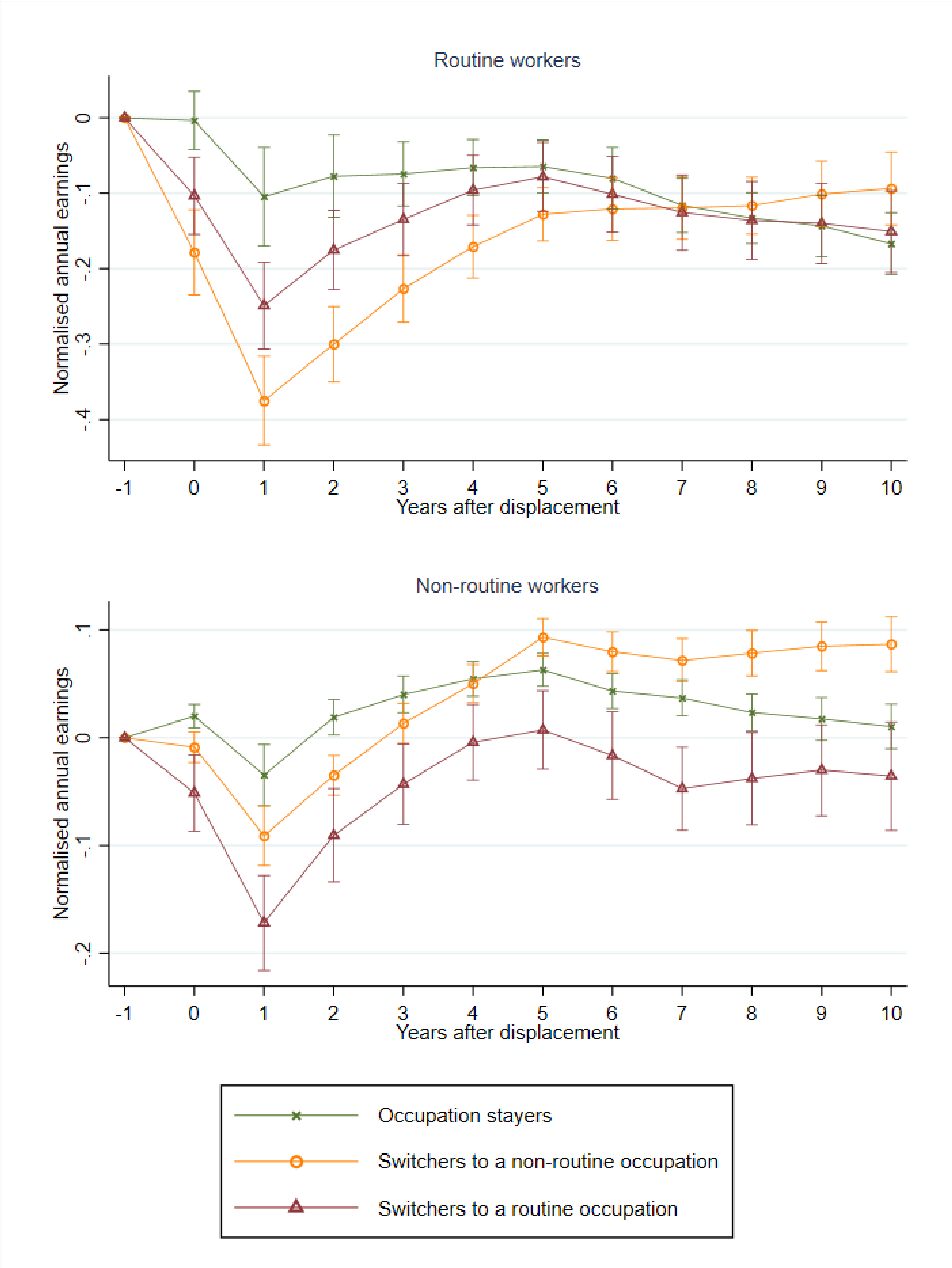
TABLE 2. AVERAGE ANNUAL EARNINGS OF DISPLACED WORKERS IN t_1 - t_5 RELATIVE TO THE t_{-4} - t_{-1} PERIOD DEPENDING ON INITIAL OCCUPATION ROUTINENESS, WHETHER THE WORKER STAYED IN THEIR INITIAL OCCUPATION AND THE ROUTINENESS OF THE NEW OCCUPATION CONDITIONAL ON SWITCHING

	Stayed in routine occupation	**Stayed in non-routine occupation**	**Switched to (other) routine occupation**	**Switched to (other) non-routine occupation**
Routine occupation initially	1.07 (0.006) *N*=1,569		1.01 (0.007) *N*=1,742	0.90 (0.006) *N*=3,991
Non-routine occupation initially		1.14 (0.003) *N*=9,890	1.06 (0.010) *N*=1,241	1.12 (0.003) *N*=13,536

Note: Workers must be employed in t_5, with occupational information available, to be included in the analysis. Standard errors clustered at the level of t_{-1} establishments.

Further evidence on how earnings develop over time depending on displaced workers' occupation in t_5 is provided by the plots in Figure 12. To be included, workers must have an observed occupation in t_5. By definition, this means that they are employed in that period, which means that they are positively selected among displaced workers. Because of this, the comparison group of non-displaced workers is also limited to individuals whose occupations were observed in t_5. As occupation switching is endogenous to displacement, all three groups of displaced workers are compared to the entire sample of non-displaced workers, which is not split according to t_5 occupation. The caveat of the groups being non-randomly selected among displaced individuals still applies, but there nevertheless are interesting suggestive results. Both routine and non-routine workers seem to suffer in the short run if they switch occupation, but there are indications that each group suffers more if they switch to the other type of occupation. This is expected for non-routine workers if they switch to a routine occupation with bad prospects, but expectations are not quite as clear for routine workers who switch to non-routine occupations. That switchers do worse than stayers should be seen as a piece of evidence favouring the hypothesis that losses of occupation-specific human capital are an important component of displacement losses. Such losses should be larger if the worker switches to a more dissimilar occupation, which is what is indicated by the results. Occupation switchers, especially those who go to (other) non-routine occupations, do appear to gain on stayers over time. However, for routine workers, such gains are at most small and appear only towards the end of the period studied.

Figure 12. Development of earnings for displaced workers over time, for occupation stayers, occupation switchers to non-routine occupations and occupation switchers to routine occupations (as defined by the occupation of individuals in t_5)



Note: Workers must be employed in t_5, with occupational information available, to be included in the analysis. Outcomes compared to those of controls which were employed and for whom occupational data were available in t_5. Standard errors clustered at the level of t_{-1} establishments. 95 percent confidence intervals shown.

5. Conclusion

While large bodies of literature have identified that routine occupations have declined due to technological change and that workers lose out greatly in terms of their labour market outcomes following involuntary job loss, research connecting these two strands has been lacking. This paper attempts to conjoin the two lines of inquiry by comparing how workers initially in routine and non-routine occupations fare on the labour market following layoff. The findings imply substantial earnings, employment, wage and unemployment penalties of displacement for routine workers, up to several times the size of the penalties faced by non-routine displaced workers. These differences in losses persist in at least the medium run. There are indications that the additional losses suffered by routine workers are due to them being unable to find new jobs which provide a good match for their occupation- and industry-specific human capital, as they switch occupations and industries to a higher degree than displaced non-routine workers. This is reflected in routine workers moving to lower-paid industries and ending up on lower rungs in their occupations' wage distributions. Occupation switchers appear to do worse in terms of earnings than stayers, even if they switch to non-routine occupations. This is a somewhat disheartening piece of evidence for policy, which often aims to make displaced workers more flexible in terms of their job search and to re-educate and retrain them so that they can shift out of declining occupations and industries. A potential interpretation is that retraining programmes are insufficiently focused on the needs of displaced routine workers. Singling out this group as a target for such efforts and tailoring suitable courses might be a possible remedy.

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Appendix

TABLE A1. PRE-LAYOFF LEVELS OF OUTCOMES NOT USED IN MATCHING

	Controls (Unmatched)	**Displaced** (Unmatched)	**Controls** (Matched)	**Displaced** (Matched)
N individuals	1,035,499	84,896	65,069	84,325
Pre-period employment (probability)				
t_{-1}	1	1	1	1
t_{-2}	1	1	1	1
t_{-3}	1	1	1	1
t_{-4}	1	1	1	1
Pre-period log monthly wages				
t_{-1}	10.2	10.2	10.2	10.2
t_{-2}	10.2	10.1	10.2	10.1
t_{-3}	10.1	10.1	10.1	10.1
t_{-4}	10.1	10.1	10.1	10.1
Pre-period days of unemployment				
t_{-1}	1.7	9.7	2.5	9.7
t_{-2}	1.0	1.7	1.4	1.7
t_{-3}	1.7	2.5	2.5	2.5
t_{-4}	3.2	4.9	4.5	4.8

Note: Characteristics evaluated in year t_{-1} unless stated otherwise. Unmatched control group consists of 5% random sample of the eligible control pool. One-to-one propensity score matching with replacement implemented based on characteristics listed in the table. Propensity scores estimated using logit. Matched control sample statistics weighted by the number of times a control worker was drawn as the best match for a displaced worker. Sum of matched control weights is 84,325.

TABLE A2. DESCRIPTIVE STATISTICS FOR THE MATCHED ROUTINE AND NON-ROUTINE SAMPLES OF CONTROLS AND DISPLACED

	Non-Routine (Matched controls)	**Non-Routine** (Matched displaced)	**Routine** (Matched controls)	**Routine** (Matched displaced)
N individuals	50,802	64,084	14,267	20,241
Routine intensity	0.50	0.50	0.81	0.81
Year t_{-1}	2004.3	2004.3	2004.3	2004.3
Age	43.5	43.4	42.8	43.1
Tenure	5.3	5.4	6.3	6.4
Female	0.38	0.38	0.34	0.32
Immigrant	0.10	0.11	0.14	0.15
Education level (percentages)				
Less than compulsory	5.72	5.41	11.92	12.01
Compulsory, 9 years	10.78	10.36	20.53	21.00
High school, 2 years	30.88	30.17	40.31	40.73
High school, 3 years	21.29	22.30	20.73	19.96
Some post-secondary	14.42	15.23	4.94	4.86
University	15.84	15.52	1.54	1.41
PhD	1.05	1.02	0.03	0.02
Occupations (percentages)				
Officials & Managers	8.94	9.04	0.00	0.00
Professionals	16.24	16.41	0.00	0.00

Technicians	22.63	22.86	0.00	0.00
Clerks	12.22	13.14	8.93	6.13
Service & Sales	13.18	13.31	0.00	0.00
Crafts	14.15	14.42	2.01	1.57
Operators & Assemblers	7.36	5.74	78.18	80.98
Elementary Occupations	5.27	5.07	10.88	11.33
Industries (percentages)				
Primary	0.59	0.50	0.05	0.08
Manufacturing	36.31	35.72	87.42	90.54
Construction	2.84	2.83	0.14	0.26
Utilities & telecom	11.67	12.11	3.99	2.00
Wholesale & retail	13.19	13.05	2.31	1.77
Business services	21.08	21.61	3.15	2.52
Health, social work	9.19	9.13	2.31	2.47
Education	2.08	1.95	0.07	0.09
Public administration	3.05	3.10	0.56	0.27
Type of municipality (percentages)				
Rural municipalities	12.82	11.68	26.00	28.27
Commuter municipalities	5.29	4.91	6.79	7.42
Towns	13.32	13.22	20.06	20.83
Other cities	32.07	33.60	31.22	29.67
Suburbs of 3 largest cities	11.81	11.50	5.87	5.48
3 largest cities	24.69	25.09	10.06	8.33
Pre-period earnings (SEK thousands)				
t_{-1}	323	334	333	334
t_{-2}	316	321	323	321
t_{-3}	306	308	311	309
t_{-4}	293	294	296	295

Note: Characteristics evaluated in year t_{-1} unless stated otherwise. Workers subdivided according to the main definition of routineness, as defined in Section 2.1. Matched control sample statistics weighted by the number of times a control worker was drawn as the best match for a displaced worker.

Estimates of annual earnings in different periods for different worker groups are shown in Table A3. The first set of estimates presents the period effects relative to t_{-1} for non-routine non-displaced workers. The second set contains interactions of periods with routineness, and provides estimates of the difference between routine and non-routine workers' relative earnings in each period. The third set contains interactions of each period with displacement; these estimates show the difference between displaced and non-displaced non-routine workers. Finally, the fourth set of estimates is the focus of this paper, as it shows the difference between routine and non-routine displaced workers. The total size of the displacement effect for routine workers is found by adding the period-displacement and the period-displacement-routine effect for the period in question.

The main definition of routineness used in this paper (the quarter of workers with the highest share of routine tasks in their occupations classified as routine, the others as non-routine) is used in columns (1) and (2). In columns (3) and (4), the definition of routine occupations is made less stringent and the median routineness of workers' t_{-1} occupations is used as the cutoff. The final two columns provide estimates in the case when only the top and bottom quartile of routineness are included, so as to ensure that truly routine workers are compared to truly non-routine ones. For each definition of routineness, estimates are provided for the full unmatched sample as well as for a sample that has been matched on covariates and earnings pre-trends as described in Section 4. The main specification used in this study is the one in column (2).

TABLE A3. POINT ESTIMATES OF PERIOD EFFECTS FOR DIFFERENT GROUPS OF WORKERS

	(1) Baseline, unmatched	(2) Baseline, matched	(3) Median cutoff, unmatched	(4) Median cutoff, matched	(5) High and low only, unmatched	(6) High and low only, matched
Periods:						
t_{-4}	-0.11***	-0.13***	-0.11***	-0.14***	-0.12***	-0.14***
	(0.0006)	(0.003)	(0.0006)	(0.004)	(0.0009)	(0.005)
t_{-3}	-0.063***	-0.079***	-0.063***	-0.082***	-0.069***	-0.091***
	(0.0005)	(0.003)	(0.0005)	(0.003)	(0.0007)	(0.004)
t_{-2}	-0.025***	-0.036***	-0.025***	-0.039***	-0.028***	-0.046***
	(0.0003)	(0.002)	(0.0003)	(0.002)	(0.0005)	(0.002)
t_0	0.0052***	-0.000073	0.0055***	0.0025	0.011***	0.0092***
	(0.0004)	(0.002)	(0.0004)	(0.002)	(0.0006)	(0.003)
t_1	0.013***	0.0073**	0.014***	0.014***	0.022***	0.020***
	(0.0006)	(0.003)	(0.0007)	(0.003)	(0.0009)	(0.004)
t_2	0.029***	0.021***	0.030***	0.030***	0.039***	0.033***
	(0.0009)	(0.004)	(0.0009)	(0.004)	(0.001)	(0.005)
t_3	0.048***	0.043***	0.050***	0.053***	0.059***	0.057***
	(0.001)	(0.004)	(0.001)	(0.005)	(0.002)	(0.006)
t_4	0.068***	0.061***	0.071***	0.074***	0.083***	0.081***
	(0.001)	(0.005)	(0.001)	(0.005)	(0.002)	(0.007)
t_5	0.091***	0.081***	0.094***	0.099***	0.11***	0.10***
	(0.002)	(0.006)	(0.002)	(0.006)	(0.003)	(0.008)

t_6	0.12***	0.10***	0.12***	0.12***	0.13***	0.13***
	(0.002)	(0.007)	(0.002)	(0.007)	(0.003)	(0.009)
t_7	0.14***	0.12***	0.14***	0.14***	0.16***	0.15***
	(0.002)	(0.008)	(0.002)	(0.008)	(0.003)	(0.01)
t_8	0.17***	0.15***	0.17***	0.17***	0.18***	0.18***
	(0.003)	(0.009)	(0.003)	(0.009)	(0.004)	(0.01)
t_9	0.19***	0.17***	0.20***	0.19***	0.21***	0.20***
	(0.003)	(0.010)	(0.003)	(0.010)	(0.004)	(0.01)
t_{10}	0.22***	0.20***	0.23***	0.22***	0.24***	0.23***
	(0.003)	(0.01)	(0.003)	(0.01)	(0.005)	(0.01)
Period-routine interactions:						
t_{-4}	0.022***	0.033***	0.012***	0.035***	0.030***	0.047***
	(0.002)	(0.006)	(0.001)	(0.005)	(0.002)	(0.007)
t_{-3}	0.016***	0.021***	0.0083***	0.019***	0.022***	0.034***
	(0.001)	(0.005)	(0.0010)	(0.004)	(0.001)	(0.006)
t_{-2}	0.0086***	0.0090**	0.0040***	0.011***	0.012***	0.019***
	(0.0007)	(0.003)	(0.0006)	(0.003)	(0.0008)	(0.004)
t_0	-0.011***	-0.017***	0.015*	-0.013***	-0.016***	-0.027***
	(0.001)	(0.004)	(0.007)	(0.003)	(0.001)	(0.005)
t_1	-0.022***	-0.042***	0.0061	-0.034***	-0.031***	-0.054***
	(0.001)	(0.006)	(0.006)	(0.005)	(0.002)	(0.006)
t_2	-0.036***	-0.060***	0.0017	-0.047***	-0.046***	-0.072***
	(0.002)	(0.007)	(0.005)	(0.006)	(0.002)	(0.008)
t_3	-0.050***	-0.075***	-0.0052***	-0.057***	-0.062***	-0.089***
	(0.002)	(0.009)	(0.0007)	(0.007)	(0.003)	(0.009)
t_4	-0.061***	-0.087***	-0.011***	-0.070***	-0.076***	-0.11***
	(0.003)	(0.01)	(0.001)	(0.009)	(0.003)	(0.01)
t_5	-0.073***	-0.094***	-0.017***	-0.082***	-0.088***	-0.12***
	(0.003)	(0.01)	(0.002)	(0.010)	(0.004)	(0.01)
t_6	-0.083***	-0.11***	-0.025***	-0.092***	-0.10***	-0.13***
	(0.004)	(0.01)	(0.002)	(0.01)	(0.005)	(0.01)
t_7	-0.095***	-0.12***	-0.032***	-0.095***	-0.11***	-0.15***
	(0.005)	(0.02)	(0.002)	(0.01)	(0.005)	(0.02)
t_8	-0.11***	-0.13***	-0.038***	-0.10***	-0.12***	-0.16***
	(0.005)	(0.02)	(0.003)	(0.01)	(0.006)	(0.02)
t_9	-0.12***	-0.14***	-0.044***	-0.11***	-0.13***	-0.17***
	(0.006)	(0.02)	(0.004)	(0.02)	(0.007)	(0.02)
t_{10}	-0.13***	-0.15***	-0.051***	-0.12***	-0.15***	-0.18***
	(0.006)	(0.02)	(0.004)	(0.02)	(0.008)	(0.02)

Period-displaced interactions:						
t_{-4}	-0.018**	0.0041	-0.026***	0.0047	-0.027***	0.0069
	(0.007)	(0.007)	(0.008)	(0.008)	(0.005)	(0.006)
t_{-3}	-0.012*	0.0048	-0.019**	0.0031	-0.018***	0.0091
	(0.006)	(0.006)	(0.007)	(0.007)	(0.004)	(0.005)
t_{-2}	-0.0096*	0.0033	-0.014*	0.0017	-0.010***	0.0084*
	(0.004)	(0.005)	(0.006)	(0.006)	(0.003)	(0.004)
t_0	-0.059***	-0.056***	-0.050***	-0.049***	-0.040***	-0.039***
	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)
t_1	-0.20***	-0.20***	-0.19***	-0.19***	-0.16***	-0.16***
	(0.009)	(0.009)	(0.01)	(0.01)	(0.01)	(0.01)
t_2	-0.18***	-0.18***	-0.17***	-0.17***	-0.15***	-0.15***
	(0.009)	(0.009)	(0.01)	(0.01)	(0.01)	(0.01)
t_3	-0.16***	-0.16***	-0.15***	-0.16***	-0.14***	-0.14***
	(0.010)	(0.010)	(0.01)	(0.01)	(0.01)	(0.01)
t_4	-0.15***	-0.14***	-0.14***	-0.14***	-0.14***	-0.13***
	(0.010)	(0.01)	(0.010)	(0.01)	(0.01)	(0.01)
t_5	-0.15***	-0.14***	-0.13***	-0.14***	-0.14***	-0.13***
	(0.01)	(0.01)	(0.009)	(0.01)	(0.01)	(0.01)
t_6	-0.15***	-0.14***	-0.13***	-0.14***	-0.14***	-0.13***
	(0.01)	(0.01)	(0.010)	(0.01)	(0.01)	(0.01)
t_7	-0.14***	-0.13***	-0.12***	-0.12***	-0.13***	-0.12***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
t_8	-0.14***	-0.12***	-0.12***	-0.12***	-0.13***	-0.13***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
t_9	-0.13***	-0.11***	-0.12***	-0.11***	-0.13***	-0.12***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
t_{10}	-0.13***	-0.12***	-0.12***	-0.12***	-0.14***	-0.13***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Period-displaced-routine interactions:						
t_{-4}	-0.012	-0.015	0.015*	-0.0096	-0.0029	-0.018
	(0.009)	(0.010)	(0.007)	(0.008)	(0.01)	(0.01)
t_{-3}	-0.018*	-0.018*	0.0061	-0.0060	-0.013	-0.023*
	(0.008)	(0.009)	(0.006)	(0.007)	(0.01)	(0.01)
t_{-2}	-0.017*	-0.016*	0.0017	-0.0051	-0.016	-0.021*
	(0.007)	(0.007)	(0.005)	(0.006)	(0.009)	(0.009)

t_0	-0.11***	-0.10***	-0.077***	-0.013***	-0.13***	-0.12***
	(0.02)	(0.02)	(0.01)	(0.003)	(0.02)	(0.02)
t_1	-0.20***	-0.19***	-0.14***	-0.034***	-0.24***	-0.22***
	(0.02)	(0.02)	(0.01)	(0.005)	(0.02)	(0.02)
t_2	-0.17***	-0.15***	-0.12***	-0.047***	-0.19***	-0.18***
	(0.02)	(0.02)	(0.01)	(0.006)	(0.02)	(0.02)
t_3	-0.13***	-0.12***	-0.10***	-0.057***	-0.15***	-0.14***
	(0.01)	(0.01)	(0.01)	(0.007)	(0.02)	(0.02)
t_4	-0.11***	-0.098***	-0.096***	-0.070***	-0.12***	-0.11***
	(0.02)	(0.02)	(0.01)	(0.009)	(0.02)	(0.02)
t_5	-0.094***	-0.090***	-0.094***	-0.082***	-0.11***	-0.098***
	(0.02)	(0.02)	(0.02)	(0.010)	(0.02)	(0.02)
t_6	-0.077***	-0.075***	-0.081***	-0.092***	-0.089***	-0.080***
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
t_7	-0.071***	-0.065***	-0.081***	-0.095***	-0.081***	-0.070***
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
t_8	-0.067***	-0.073***	-0.075***	-0.10***	-0.073***	-0.067**
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
t_9	-0.058***	-0.060**	-0.059***	-0.11***	-0.058**	-0.051*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
t_{10}	-0.044*	-0.050*	-0.052***	-0.12***	-0.039	-0.035
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)

Note: Standard errors clustered at the level of t_{-1} establishments shown in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

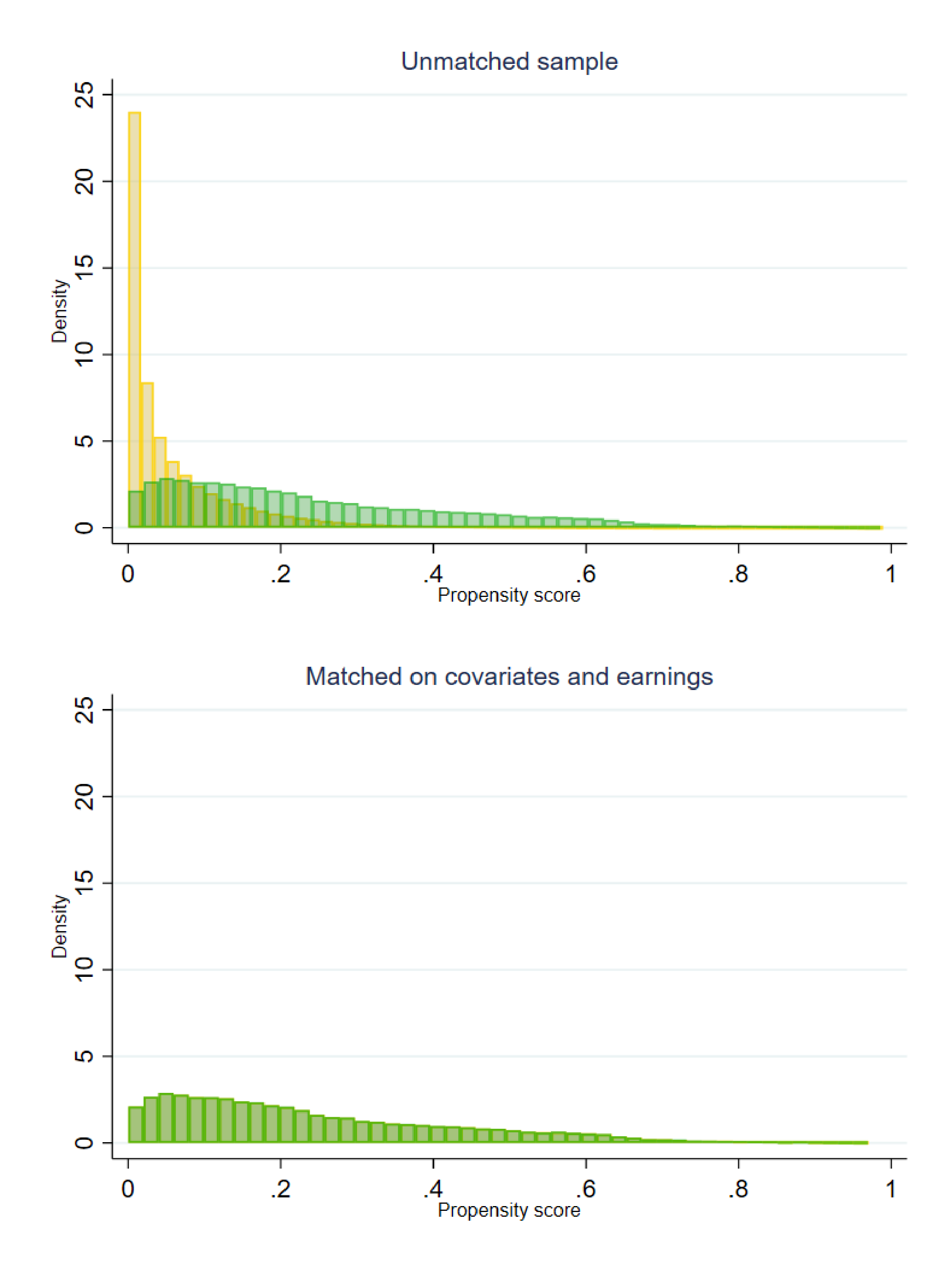

FIGURE A1. HISTOGRAMS OF PROPENSITY SCORES FOR THE CONTROL AND DISPLACED SAMPLES BEFORE AND AFTER MATCHING

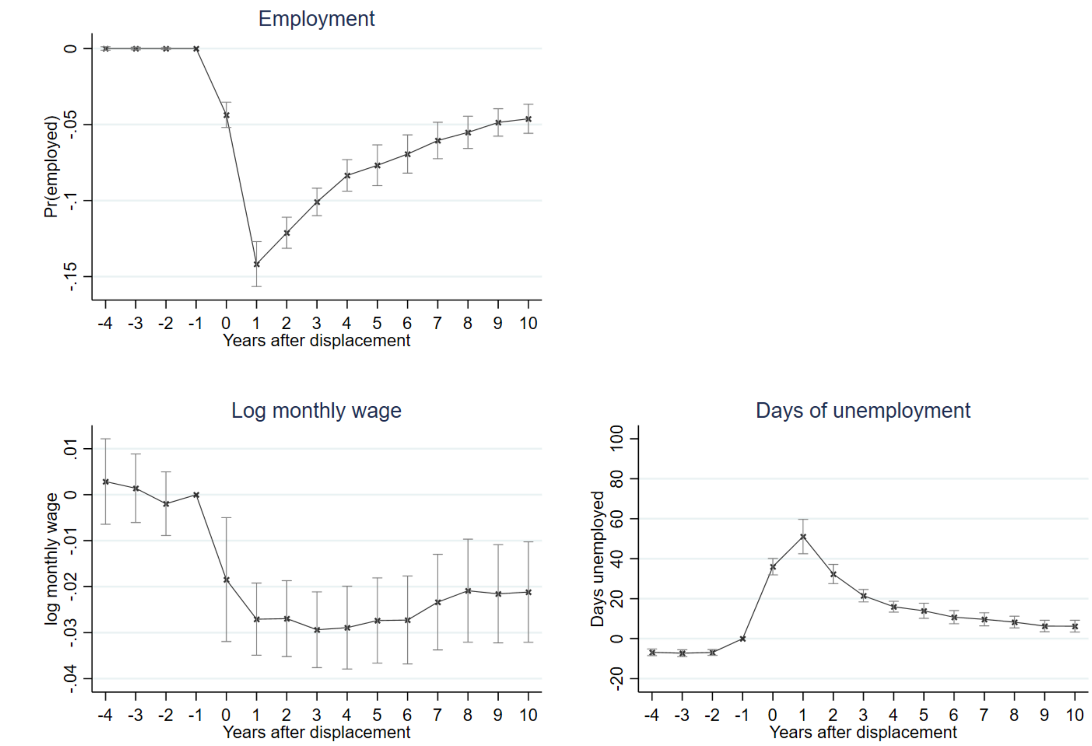

FIGURE A2. ESTIMATES OF AVERAGE DISPLACEMENT EFFECTS, WITHOUT ROUTINE INTERACTIONS, ON EMPLOYMENT, MONTHLY WAGES AND DAYS OF UNEMPLOYMENT

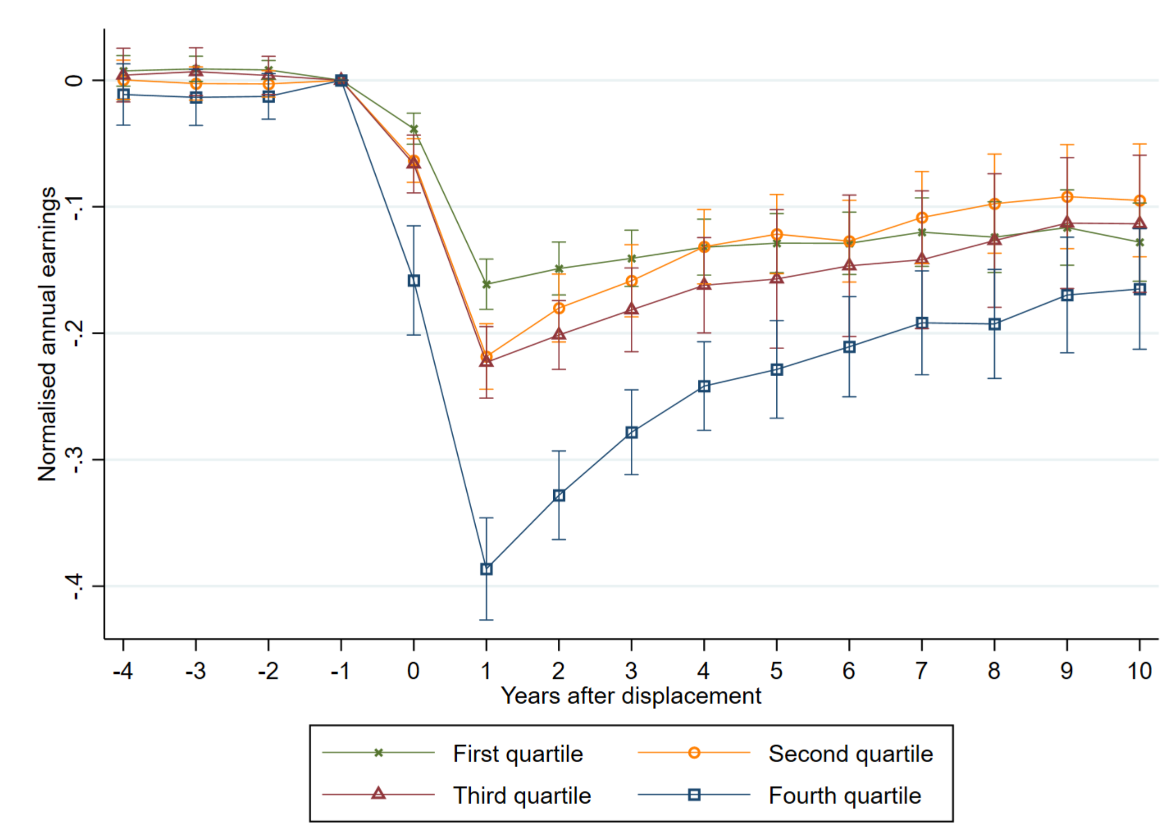

FIGURE A3. ESTIMATES OF DISPLACEMENT EARNINGS PENALTIES FOR THE FOUR ROUTINENESS QUARTILES

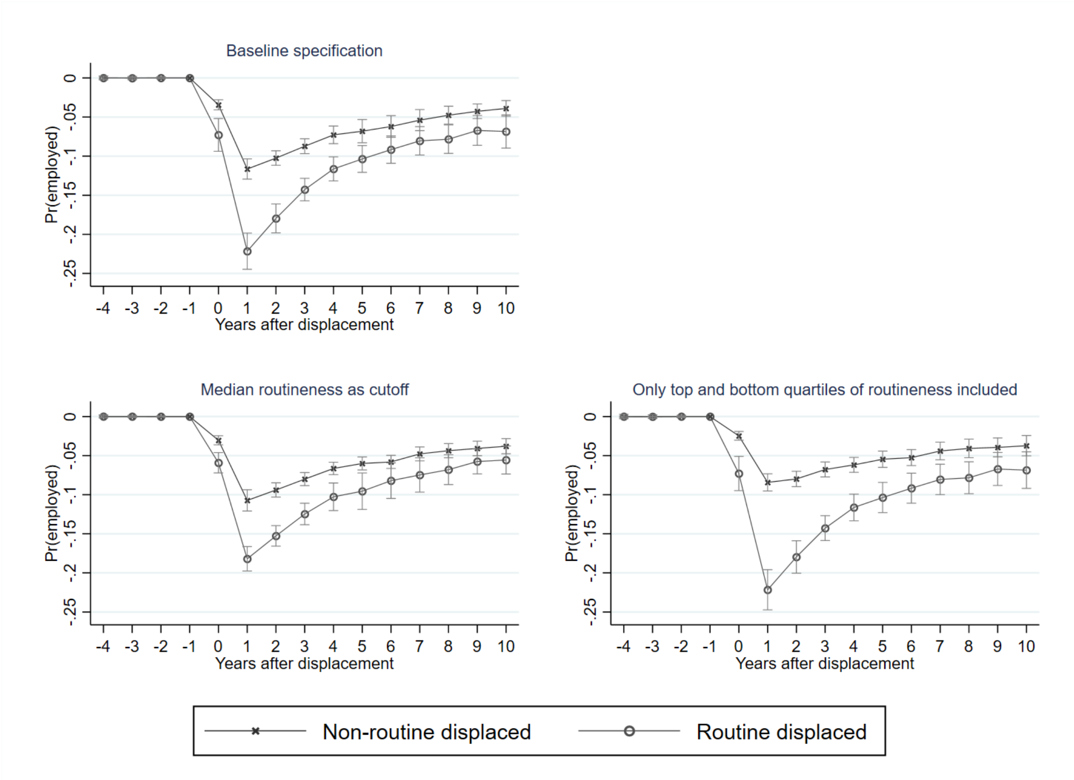

FIGURE A4. ESTIMATES OF DISPLACEMENT EMPLOYMENT PENALTIES USING DIFFERENT DEFINITIONS OF ROUTINE AND NON-ROUTINE OCCUPATIONS.

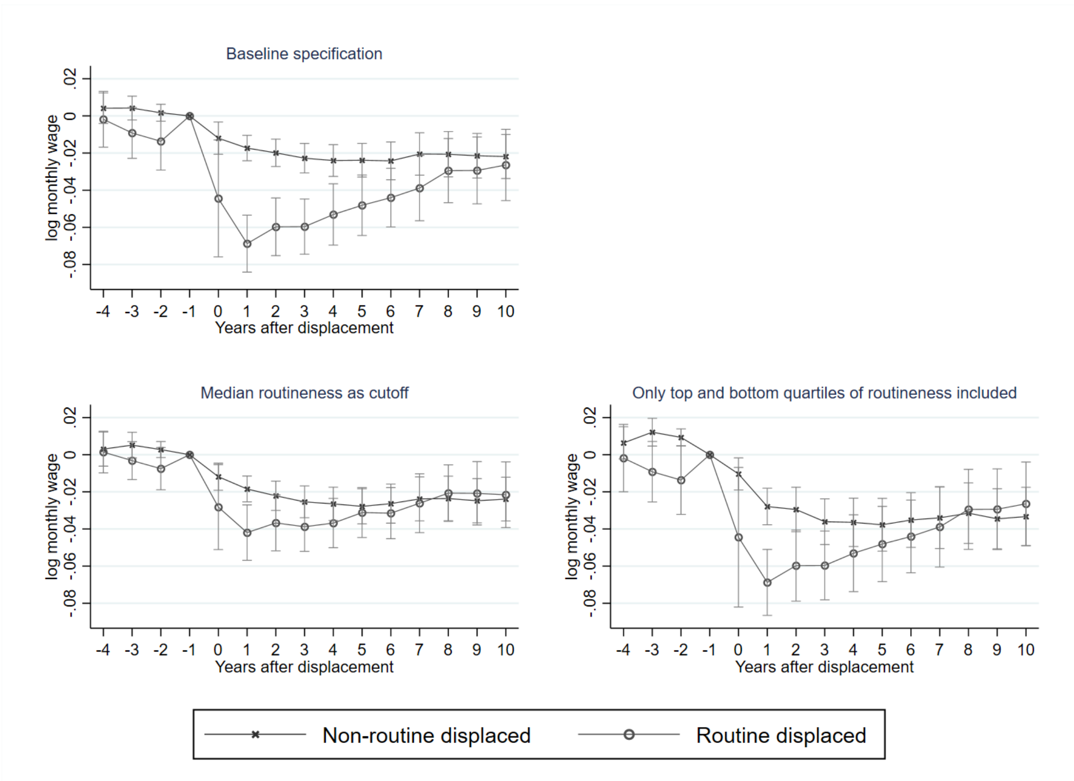

FIGURE A5. ESTIMATES OF DISPLACEMENT MONTHLY WAGE PENALTIES USING DIFFERENT DEFINITIONS OF ROUTINE AND NON-ROUTINE OCCUPATIONS.

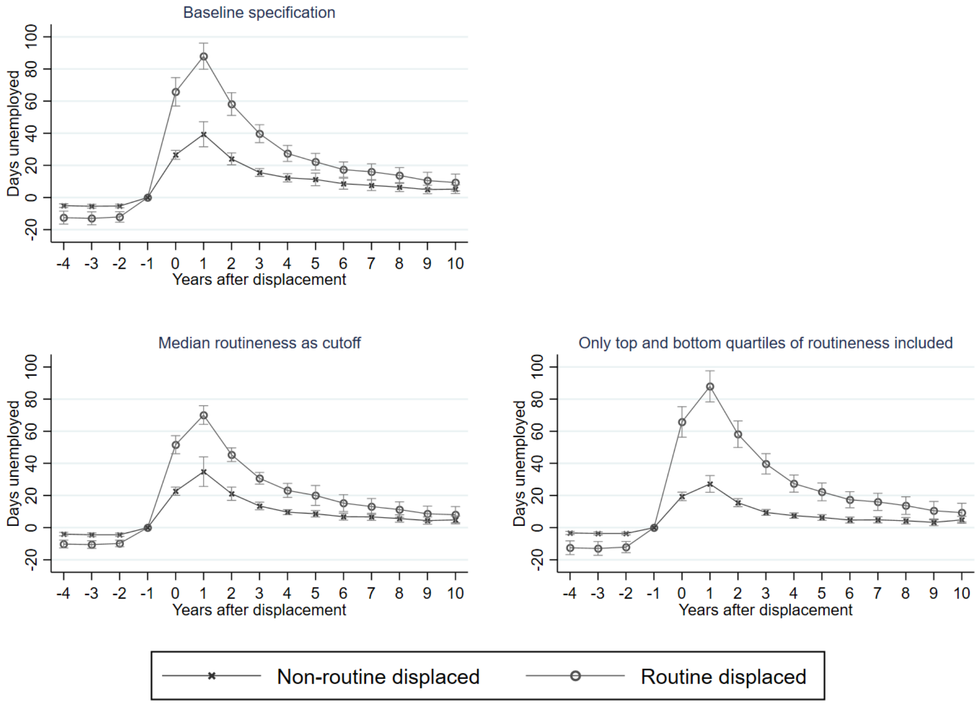

FIGURE A6. ESTIMATES OF DISPLACEMENT UNEMPLOYMENT PENALTIES USING DIFFERENT DEFINITIONS OF ROUTINE AND NON-ROUTINE OCCUPATIONS.

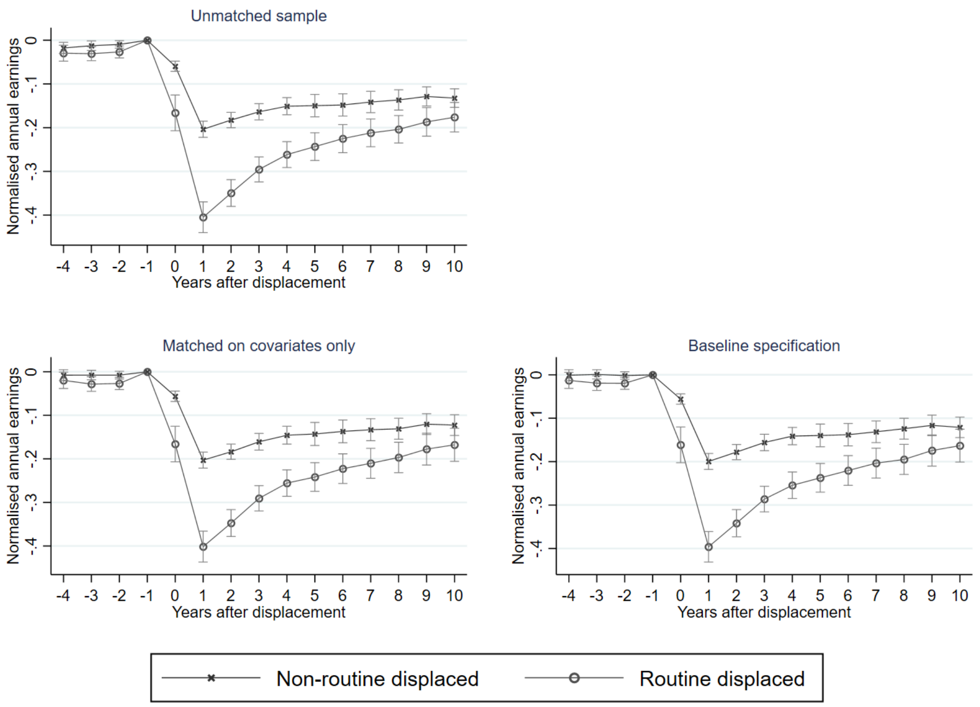

FIGURE A7. EFFECTS OF DISPLACEMENT ON ANNUAL EARNINGS USING THE UNMATCHED SAMPLE, A SAMPLE MATCHED ON A BROAD SET OF CONTROL VARIABLES AND THE BASELINE SPECIFICATION WITH A SAMPLE MATCHED ON A BROAD SET OF CONTROL VARIABLES AND EARNINGS PRE-TRENDS

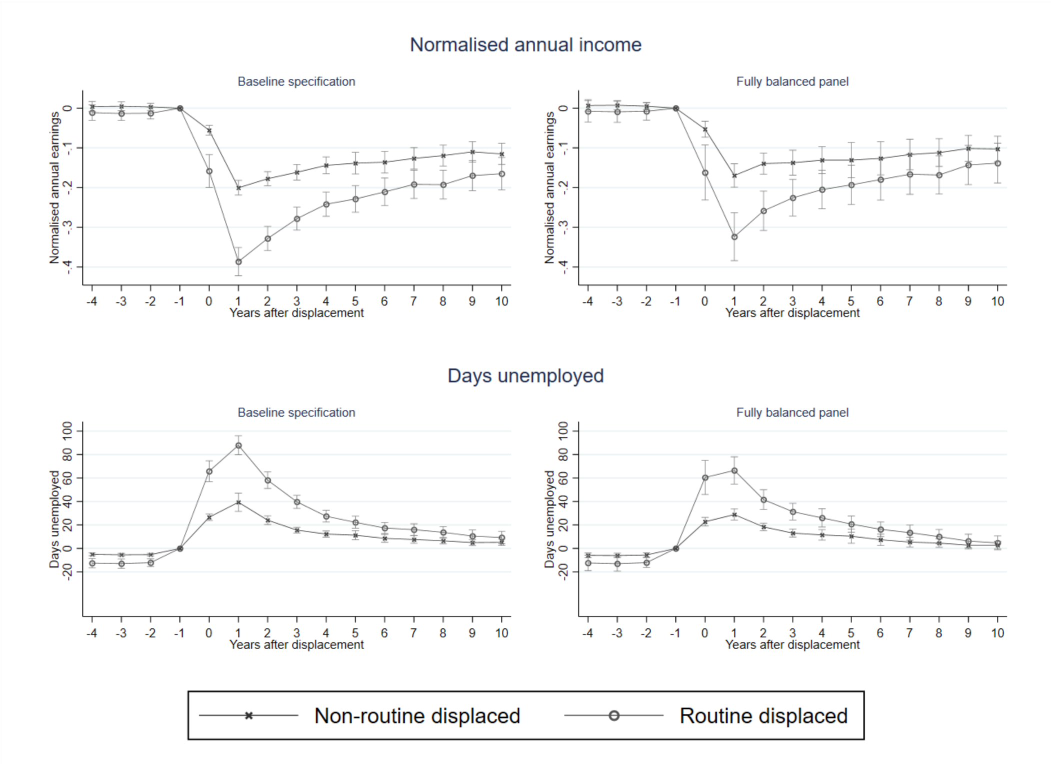

FIGURE A8. ESTIMATED EFFECTS OF DISPLACEMENT ON ROUTINE AND NON-ROUTINE WORKERS USING THE FULL SAMPLE (LEFT COLUMN) AND ONLY THOSE INDIVIDUALS OBSERVED IN EACH OF THE YEARS t_{-4} TO t_{10} (RIGHT COLUMN) ON ANNUAL EARNINGS AND DAYS IN UNEMPLOYMENT

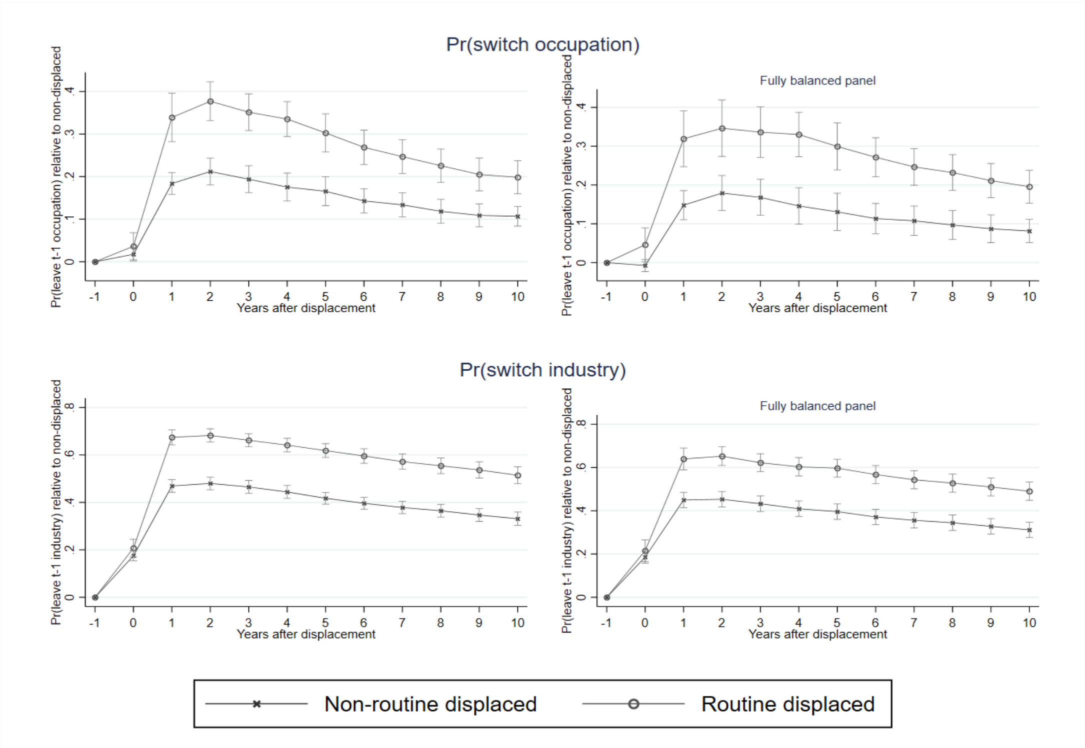

FIGURE A9. ESTIMATED EFFECTS OF DISPLACEMENT ON ROUTINE AND NON-ROUTINE WORKERS USING THE FULL SAMPLE (LEFT COLUMN) AND ONLY THOSE INDIVIDUALS OBSERVED IN EACH OF THE YEARS t_{-4} TO t_{10} (RIGHT COLUMN) ON PROBABILITY OF SWITCHING OCCUPATION AND SWITCHING INDUSTRY