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Severing a labour reserve: Employment adjustment and the loss of external labour access in the West Bank, Palestine after October 2023

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Abstract
The suspension of Palestinian labour access after October 2023 war left many West Bank workers without their external jobs while the domestic economy was also contracting. I examine how these workers adjusted and how much employment they found inside the West Bank. Using the Palestinian Central Bureau of Statistics Labour Force Survey, I construct 8,752 matched links between men employed at baseline and their labour-force status four quarters later. The adjusted permit–domestic employment gap falls by 42.0 percentage points in crisiscrossing cohorts, or by 38.9 points with a linear trend. Among wage employees matched in detailed occupations, the decline is about 34–37 points. In the standardised crisis-crossing permit group, 18.6 per cent are employed at follow-up, 74.5 per cent are unemployed and 9.6 per cent hold jobs in the West Bank. Domestic work accounts for 10.6 per cent of those no longer employed externally. Repeated cross-sections show employment among resident men aged 18–59 at 59.2 per cent in 2025 Q4, against 77.9 per cent in 2023 Q3. Domestic employment offers a limited fallback, and the wider labour market remains weak through the end of 2025.

The views expressed in this paper are mine alone and do not represent the views of the Palestine Monetary Authority.

Keywords
Palestinian labour, work permits, labour-market adjustment, domestic absorption, West Bank, political economy

JEL classification
J21, J61, J46, O53, D74

1 Introduction

The suspension of Palestinian employment access to Israel after 7 October 2023 sharply reduced a major source of work for residents of the West Bank. Many affected workers had spent years working outside the local economy while living and supporting their households in the West Bank. Once access was withdrawn, they had to look for work in an economy that was itself losing jobs. This raises the question of how much of the lost external employment could be replaced by work at home?

Labour access has been part of the economic relationship between Israel and the occupied Palestinian territory for decades. External wages support household spending, while the conditions for reaching a job are determined outside the worker's domestic labour market. Earlier research describes both the higher returns to external work and the vulnerability created by this dependence (Angrist, 1996; Farsakh, 2005; Berda, 2018). The October 2023 interruption provides a particularly complete episode in which to examine employment adjustment after access was largely removed. The suspension came with war, greater internal movement restrictions and a broad decline in demand (Habbas, 2024; Rabee, 2026).

I use quarterly Labour Force Survey records from the Palestinian Central Bureau of Statistics (PCBS) to compare men in permit-based external jobs with men employed in the West Bank at baseline. Both groups are resident men aged 18–59. I link their baseline jobs to their labour-force status four quarters later and match them within baseline quarters on industry, occupation, age band and marital status. Ordinary and crisis-crossing cohorts are then placed on the same supported cell distribution. The analysis contains 8,752 one-year links from 6,108 distinct person keys.

Employment among baseline permit workers contracts sharply relative to domestic workers. The adjusted change in the employment gap is −42.0 percentage points, or −38.9 with a linear trend. The decline remains large among wage employees matched in detailed occupations. Yet the destination figures are more revealing about domestic absorption. In crisis-crossing links, 9.0 per cent of the permit group are employed in Israel or the settlements at follow-up, 9.6 per cent work in the West Bank and 74.5 per cent are unemployed. The domestic-employment share was already 8.4 per cent in ordinary-period links. Therefore, much of the positive relative coefficient for West Bank employment comes from the domestic comparator's own loss of jobs.

The empirical strategy draws on three bodies of work. Studies of Palestinian commuting and restrictions show how external labour flows affect workers inside the West Bank (Mansour, 2010; Fallah, 2018; Calì and Miaari, 2018). Labour-market research on migration, mobility and local demand helps explain why residents can lose workplace employment without moving their residence, and why adjustment may be slow even among workers with relevant skills (Dustmann and Kirchkamp, 2002; Artuç, Chaudhuri and McLaren, 2010; Monte, Redding and Rossi-Hansberg, 2015). Finally, methodological research on matching, overlap weighting and difference-in-differences informs how I compare the observed groups (Abadie, 2005; Iacus, King and Porro, 2012; Li, Morgan and Zaslavsky, 2018). The closest empirical study is Fallah (2018), which examines the effects of returning commuters on rural non-commuters during the Second Intifada, extending the earlier labour-supply analysis of Mansour (2010). I ask a different but connected question: what are the subsequent employment destinations of workers who had relied on external work, and how do those destinations compare with outcomes for initially domestic workers? I also classify current domestic jobs by workers' observed origin and use employment relative to the resident population. Hinn (2025) and Rabee (2026) provide recent Palestinian evidence on obstacles to domestic hiring and the wider adjustment under the current war.

The paper contributes linked evidence on workers who had relied on external employment and on the domestic jobs they subsequently obtained. It compares their employment with workers in supported baseline job categories and examines whether the result survives narrower job definitions and adjustments for prior histories. It also brings the relative estimates together with absolute destinations and employment relative to the resident population. This gives a clearer account of domestic absorption when the local economy is contracting too.

Section 2 discusses labour dependence and domestic alternatives. Section 3 describes the survey and empirical approach. Section 4 presents worker destinations, employment gaps and the wider labour-market account. Section 5 brings together the main robustness findings and limitations. Section 6 discusses their implications and concludes. The appendices contain the weighting formulas, full comparison tables and sensitivity calculations.

2 Labour access and the domestic fallback

2.1 Earnings and the institutions governing access

Employment in Israel and the settlements has historically offered an important source of earnings for Palestinian workers, particularly when domestic job opportunities are weak but the wage earned at the external workplace is only one part of the employment relationship. Whether the worker can reach that workplace also depends on permits, entry decisions and wider restrictions on movement. Angrist (1996) documents the demand and wage dimensions of Palestinian labour in Israel, while Farsakh (2005) relates labour movement to employment and land under occupation. Berda (2018) explains the administrative structure through which access is allocated.

These institutions create an unusual dependence where the worker remains a resident of the West Bank, while an important employment opportunity is controlled elsewhere. Roy (1987) distinguishes the income generated through external dependence from the development of domestic productive capacity. Although her work examines Gaza in an earlier period, the distinction remains useful for the West Bank. Arnon and Weinblatt (2001) and Dana (2021) also examine how political and economic relationships condition Palestinian development. Earlier disruption under Covid-19 also exposed the insecurity of Palestinian labour mobility (Samour, 2020). Adnan and Etkes (2019) document payments associated with traded work permits before the current crisis. In a different migrant labour market, Naidu, Nyarko and Wang (2016) show why limits on changing employers matter for bargaining and employment conditions. These studies do not measure the October 2023 shock, but they show why the availability of external jobs should not be treated as an ordinary labour-market choice. Work authorisation also affects earnings and labour-market outcomes in other settings (Kossoudji and Cobb-Clark, 2002; Pope, 2016), though those legalisation policies are different from a sudden interruption of Palestinian commuting access. More recently, Bondy and Preminger (2026) describe the conflict among political authorities, employers, professional institutions and other actors over the employment of Palestinian and other noncitizen workers after October 2023. Labour demand may continue, even while Palestinian access is restricted.

The paper refers to the October war as a *suspension of effective labour access*. Habbas (2024) discusses the interruption after 7 October and Bondy and Preminger (2026) describe exceptions, including some employment in settlements. The survey identifies reported employment destinations and baseline channels; it cannot establish the administrative status of every permit at the follow-up date.

2.2 Why domestic work is not an automatic alternative

A person who chooses when to return from migration may plan the move around savings, job prospects or self-employment. Dustmann and Kirchkamp (2002) model migration duration and activity after return as related decisions. This is different from losing commuting access unexpectedly. Palestinian commuters are already resident in the West Bank, and they may have little time to arrange another job before external earnings stop. Spatial labour-market models are helpful because they distinguish the place where a person lives from the place where employment is located. Monte, Redding and Rossi-Hansberg (2015) study how commuting and migration connect local labour markets. Their model and estimates are not transferred to the present setting, but the residence–workplace distinction is directly relevant: West Bank residents can lose an external job without returning from residential migration.

Skills gained in construction or services outside the West Bank do not guarantee that a domestic employer has a vacancy. A worker who wants to change occupation may face search costs, limited information or a lack of

funds. Artuç, Chaudhuri and McLaren (2010) show how costly sector switching can slow labour adjustment in a different setting. Their model does not account for the very high unemployment observed here, but it helps explain why workers do not immediately move to another sector because their previous employment has stopped. After external access is interrupted, a worker may enter domestic employment, continue or resume external work, become unemployed, or leave the labour force. The relative size of these outcomes must be established empirically. It cannot be inferred from the existence of similar occupations in the two economies or from the external wage premium before the crisis.

2.3 Domestic workers face the crisis too

Other residents may be searching for the same jobs, while local firms lose demand when external earnings and household spending fall. Mansour (2010) and Fallah (2018) study these interactions during the Second Intifada. Fallah finds adverse wage and unemployment outcomes for rural non-commuters, especially among lower-skilled job seekers. This evidence is an important reason not to treat domestic workers as an unaffected control group. The extent of competition depends partly on whether employers see displaced commuters and domestic workers as substitutes. Research on migration and labour demand suggests that similarity in observed characteristics does not establish perfect substitution (Ottaviano and Peri, 2012). Matching occupation and industry makes the groups more comparable, but it does not measure the degree to which their labour is interchangeable in local production. Domestic hiring also depends on demand. Moretti (2010) shows the importance of local employment spillovers in another economy. Here, external wages may sustain purchases inside the West Bank, so the interruption can weaken local demand at the same time as job search increases. Hinn (2025) argues that unmet housing needs do not directly translate into construction employment if projects lack finance and effective demand. Rabee (2026) connects lost external earnings with movement restrictions and pressure on the domestic economy. Calì and Miaari (2018) document the labour-market effects of internal mobility restrictions before the current war. Together, these studies indicate several reasons why local employers may not absorb displaced commuters.

2.4 Household consequences

Loss of adult employment can affect the whole household. Where families cannot borrow easily, a decline in income may change schooling and work decisions for children. Baland and Robinson (2000) provide a theoretical account of how financial constraints affect child labour. For Palestine, Di Maio and Nandi (2013) examine school attendance and child work during closure, while Di Maio and Nisticò (2019) study schooling after parental job loss. Hallaq and Khalifah (2022, 2024) connect earlier school performance with subsequent paid child work in the West Bank.

3 Data and empirical approach

3.1 Survey links and the comparison sample

I use PCBS quarterly Labour Force Survey records from 2021 Q1 to 2025 Q4 (PCBS, 2023, 2024, 2025; PCBS, various years). The main analysis links workers at baseline to a record four quarters later. Baseline cohorts run from 2021 Q1 through 2023 Q3. Seven have endpoints before 2023 Q4 and four cross the crisis, with endpoints from 2023 Q4 to 2024 Q3. These are one-year employment links with different lengths of crisis exposure. The later 2025 records extend the repeated cross-sectional account.

The sample covers resident West Bank men aged 18–59. PCBS collection for 2023 Q3 ended on 6 October, and Q4 collection began on 7 October. The survey continued in the West Bank during the last quarter of 2023, while collection in Gaza stopped (PCBS, 2023). This timing defines the crisis boundary used here.

A one-year link must have a unique longitudinal key in each quarter and a plausible change in reported age, between zero and two years. Ambiguous person-quarter duplicates are removed. Some people contribute more

than one one-year link, so the 8,752 links in the matched sample are not 8,752 independent workers. The survey rotation means that the file is also not a complete panel observed every quarter.

All workers in the main matched comparison are employed at baseline. The permit group reports a permit employment channel and an Israel-or-settlement workplace. The domestic group reports domestic employment and no external workplace. Workers who report external employment without a permit are excluded from the main contrast and used in an alternative comparison. The identity-document category is not used, because different statuses cannot be reliably separated within it.

Follow-up employment, unemployment and nonparticipation come from the reported labour-force status. For employed workers, the workplace is Israel or settlements (PWORK = 3), West Bank (PWORK = 1), or other workplace. Missing workplace responses remain in other employment. Previous jobs reported by unemployed respondents are excluded from current employment. Destination shares use all baseline workers with an observed endpoint as the denominator.

The baseline match uses broad industry, occupation, four age bands (18–24, 25–34, 35–44 and 45–59) and marital status (married or other). I retain cells containing both groups within the relevant cohorts and represented in ordinary and crisis-crossing periods. Of 15,315 eligible observed links, 8,752 remain in 70 supported cells and 498 cell–cohort observations. Table 1 gives the group counts and effective sample sizes.

Table 1 Observed one-year links and common support

Period	Baseline group	Eligible links	Matched links	Effective N
Ordinary	Domestic	7,820	3,597	1,442.4
Ordinary	Permit	1,827	1,717	736.8
Crisis crossing	Domestic	4,517	2,349	920.5
Crisis crossing	Permit	1,151	1,089	706.0

Notes: Counts are links; the sample contains 6,108 distinct person keys across 11 baseline cohorts. Effective N reflects the concentration of analysis weights. Its formula and the support and weighting rules are in Appendix A1.

Person weights in the extract are relative survey weights. I normalise them within baseline quarters and use them to construct the matched comparison. The extracts lack original strata, primary sampling units and rotation-eligibility fields. Reported intervals are conditional on the observed links and constructed weights; they do not incorporate the full PCBS survey design.

Two further accounts place these links in the wider labour market. First, I classify current domestic jobs by the worker's observed state four quarters earlier, using endpoint survey weights and leaving unavailable origins unresolved. Second, I calculate employment per 100 residents from the raw quarterly records, including external workplaces and showing West Bank jobs separately. The main population is men aged 18–59; a separate series covers all residents aged 15 and above. Education and health are included in services in the six-sector comparison.

3.2 Employment destinations and domestic absorption

For each baseline group, employment at follow-up is divided between Israel or the settlements, the West Bank and other workplaces. Unemployment and nonparticipation complete the account:

$$E = X + D + O, \qquad X + D + O + U + N = 1 \tag{1}$$

Here E is employment, X external employment, D West Bank employment, O other workplace employment, U unemployment and N nonparticipation. All shares use the same baseline-linked group.

I measure the domestic fallback among workers without an external job as

$$A = \frac{D}{1-X}, \qquad X < 1 \tag{2}$$

This ratio gives the share in West Bank work among baseline workers no longer employed externally at the endpoint. The two survey dates capture their destination after a year, including any moves that would have occurred in ordinary times.

The relative comparison answers a separate question where A positive West Bank employment coefficient can arise when domestic workers lose jobs, even if few permit workers move into domestic employment. I therefore report the destination shares alongside the employment-gap estimates. Appendix C sets out the relationship algebraically and discusses how added search and weaker local demand can constrain hiring.

3.3 The matched employment comparison

Within each baseline quarter, I give permit and domestic workers equal total weight in supported job cells. Cells with little representation from either group receive less weight. I then standardise ordinary and crisis-crossing periods to the cell distribution of the crisis overlap sample. The comparison follows the common-support logic of coarsened exact matching (Iacus, King and Porro, 2012) and empirical overlap weighting (Li, Morgan and Zaslavsky, 2018). Appendix A1 gives the construction.

For each cell c and baseline cohort b, I calculate the weighted permit-minus-domestic difference in follow-up employment, G_{cb}. I estimate the following model by weighted least squares, using the common group mass in each cell–cohort as its weight:

$$G_{cb} = \alpha_c + \lambda_{q(b)} + \theta s_b + Z_{cb}'\beta + s_b Z_{cb}'\gamma + \varepsilon_{cb} \tag{3}$$

Cell effects α_c allow persistent differences across baseline job categories, and $\lambda_{q(b)}$ are baseline-quarter-of-year effects. The vector Z_{cb} contains residual demographic differences between the two groups within the cell–cohort. Its interaction with s_b permits these associations to differ in crisis-crossing cohorts. A second specification adds linear baseline calendar time. The coefficient θ measures the adjusted change in the permit-minus-domestic follow-up employment gap associated with the crisis-crossing cohorts. All workers are employed at baseline, so the outcome is employment retention four quarters later. The model adjusts for baseline age, centred age squared, education, locality type and refugee status through their within-cell group differences. Interactions allow these associations to change during the crisis. Conditional DiD methods provide the basis for adjustment for observables and the assumed path of the untreated gap (Abadie, 2005).

The suspension coincided with wider wartime disruption. Domestic workers also faced movement restrictions and weaker demand, while external workers faced conditions in the Israeli labour market. The coefficient measures the groups' relative employment contraction during this period. The destination account shows how much domestic work the permit group obtained.

Main intervals cluster by the 11 baseline quarters and use t critical values with ten degrees of freedom. Fixed-worker comparisons use household-aggregated scores. Appendix A documents inference and numerical checks, including the comparison with repeated-person clustering.

4 Employment outcomes and domestic absorption

4.1 Where baseline workers were employed

Table 2 shows the main result directly. In ordinary-period links, 87.4 per cent of baseline permit workers are employed one year later. In crisis-crossing links, the share is 18.6 per cent. External employment falls from 79.0 to 9.0 per cent, while West Bank employment changes from 8.4 to 9.6 per cent. Unemployment reaches 74.5 per

cent of the crisis-crossing permit group. Initially domestic workers also lose employment, with their employed share falling from 87.2 to 61.2 per cent.

Table 2 Standardised destinations at the one-year follow-up

Destination	Ordinary permit	Ordinary domestic	Crisis permit	Crisis domestic
Employed	87.4	87.2	18.6	61.2
Israel or settlements	79.0	9.3	9.0	0.6
West Bank	8.4	77.5	9.6	60.6
Other workplace	0.0	0.4	0.0	0.0
Unemployed	9.5	8.8	74.5	34.0
Outside labour force	3.1	4.0	6.9	4.8

Notes: Percentages of baseline-employed workers with observed endpoints, using the matched standardised weights. Employed equals the three workplace categories; these categories, unemployment and nonparticipation sum to 100 before rounding. Link counts are 1,717, 3,597, 1,089 and 2,349 across the columns.

Using equation (2), the domestic fallback ratio is 10.6 per cent for crisis-crossing permit workers. Around one in ten workers without an external job at follow-up are working in the West Bank. The ordinary-period ratio is 40.0 per cent, when leaving external work involved a smaller and differently selected group. Domestic employment replaces only a small part of the external work lost in the crisis-crossing sample.

There are 113 crisis-crossing permit links that end in West Bank employment. Among these workers, construction accounts for 37.9 per cent of local jobs, manufacturing 17.9, agriculture 16.4 and commerce 15.8. About 50.3 per cent work in a different broad sector than at baseline, but those sector switchers are only 4.8 per cent of all observed crisis-crossing permit links. It is possible to see sizeable switching among the small group that finds work while reallocation remains limited among the displaced group.

These sector shares describe the 113 links ending in domestic work. Their small number gives a limited basis for drawing conclusions about the composition of reallocation. Sector changes also provide little information about the pay or productivity of the jobs obtained.

4.2 Relative employment contraction

The adjusted permit–domestic employment gap falls by 42.0 percentage points, with a 95 per cent interval of [−47.1, −36.9]. Adding a linear trend gives −38.9 points [−50.1, −27.7]. The unadjusted change calculated from Table 2 is −42.8 points. Baseline season and demographic adjustment account for the difference between the unadjusted and adjusted figures.

Table 3 decomposes the adjusted gap change using the same observations, weights and covariates for each outcome. External employment contributes −61.0 points, West Bank employment +18.8 and other employment +0.2. The increase in unemployment is 37.6 points, with another 4.4 points in nonparticipation.

Table 3 Decomposition of the relative employment-gap change

Follow-up outcome	Adjusted, pp [95% CI]	With trend, pp [95% CI]
Employment	−42.0 [−47.1, −36.9]	−38.9 [−50.1, −27.7]
Israel or settlements	−61.0 [−66.6, −55.4]	−61.1 [−72.9, −49.2]
West Bank	+18.8 [+12.1, +25.5]	+22.1 [+7.9, +36.4]
Other workplace	+0.2 [−0.1, +0.6]	+0.0 [−1.1, +1.1]
Unemployed	+37.6 [+32.2, +43.0]	+39.6 [+29.3, +50.0]
Outside labour force	+4.4 [−2.3, +11.1]	−0.7 [−10.4, +9.0]

Notes: Percentage-point changes in the permit-minus-domestic gap, using the same 8,752 links and regression design for every outcome. The workplace coefficients sum to employment; employment, unemployment and nonparticipation coefficients sum to zero. Intervals use baseline-cohort clustering.

The positive coefficient for West Bank employment is largely explained by the comparator. West Bank employment among initially domestic workers falls from 77.5 to 60.6 per cent, a loss of about 17 points. For permit workers it rises by about one point. The +18.8-point coefficient captures this relative movement. Actual domestic absorption remains the 9.6 per cent observed in West Bank jobs at follow-up.

The comparison also holds among workers with more similar baseline jobs. Restricting both groups to wage employees and matching detailed occupations leaves 4,952 links. The coefficient is −33.6 points without a trend and −35.7 with one (Table 4). The contraction remains substantial in this narrower population. The full set of occupation, employment-status and hours specifications appears in Table B2.

Table 4 Employment-gap changes in the main and detailed wage-employee comparisons

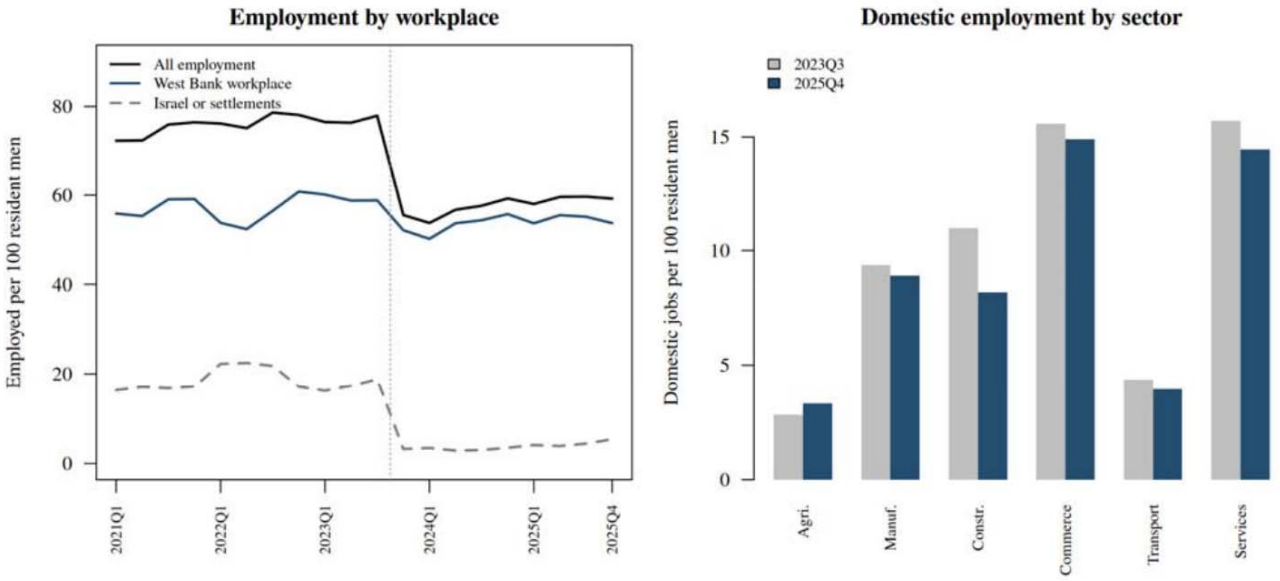
Baseline comparison	Links	Adjusted, pp [95% CI]	With trend, pp [95% CI]
Main baseline match	8,752	−42.0 [−47.1, −36.9]	−38.9 [−50.1, −27.7]
Detailed occupation, wage employees	4,952	−33.6 [−43.1, −24.1]	−35.7 [−50.0, −21.3]

Notes: Both specifications adjust baseline demographics and seasons. The detailed comparison uses PW39 and EMPSTATS = 3. Each sample is separately matched, so the overlap populations differ. Table B2 reports all job and hours comparisons.

4.3 Domestic jobs and the resident labour market

The repeated cross-sections show how little the domestic labour market expanded to meet the loss of external work. Domestic employment falls from 58.8 jobs per 100 resident men aged 18–59 in 2023 Q3 to 52.1 in 2023 Q4. It is 53.7 in 2025 Q4. External employment falls from 18.8 to 3.2 per 100 men during the immediate crisis quarter and reaches 5.4 by the end of 2025 (Figure 1).

Figure 1 Employment relative to the resident male population



Notes: Resident West Bank men aged 18–59; quarterly PCBS records and relative person weights. The left panel shows employment per 100 resident men. Total employment includes other workplaces. The right panel shows domestic sector jobs per 100 men, with education and health included in services. The series are repeated cross-sections.

Domestic construction employment falls from 11.0 jobs per 100 men in 2023 Q3 to 8.3 in 2024 Q3 and 8.2 in 2025 Q4. Including external workplaces, construction employment falls from 23.4 to 10.4 per 100 over the full period. Agriculture rises modestly, from 2.8 to 3.3 domestic jobs per 100 men. These changes leave little evidence of domestic sectors taking up the scale of employment previously provided outside the West Bank.

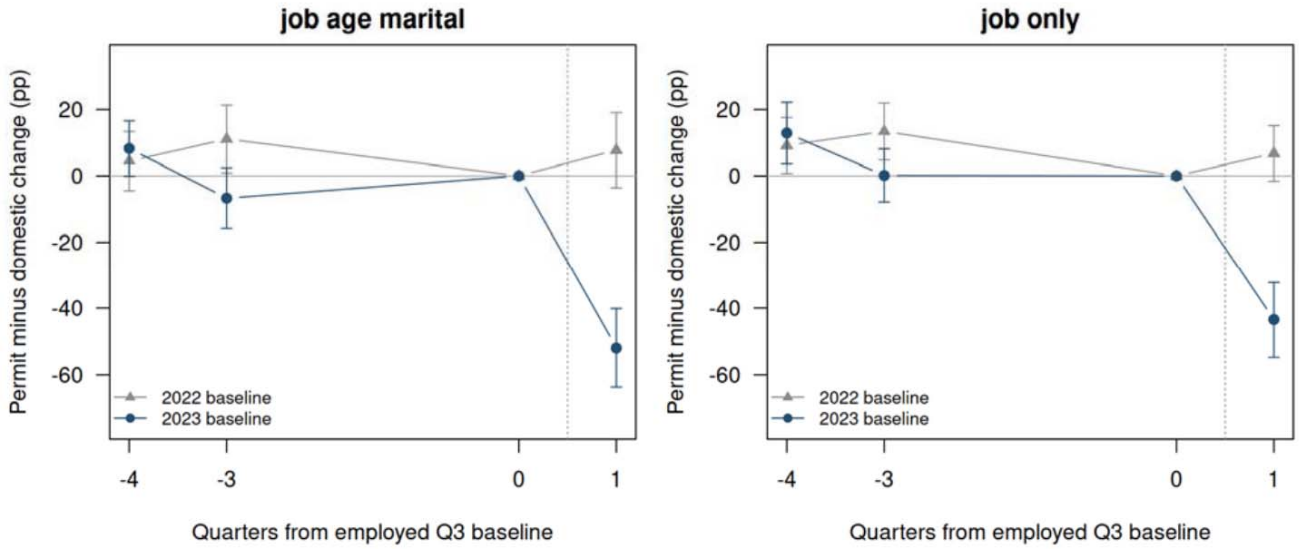
The origin account gives another view of domestic employment. At 2024 Q3, workers observed in the same domestic sector a year earlier account for 35.9 per cent of current domestic jobs, those from another domestic sector for 4.1 per cent and those from permit-based external work for 1.6 per cent. Origins remain unresolved for 54.4 per cent. Table B3 reports the full classification. Its large unresolved share limits the account to recorded origins, including in 2025 when the earlier observation refers to 2024.

4.4 Timing and subsequent adjustment

To examine the immediate change, I also follow workers employed in 2023 Q3 who have records in 2022 Q3, 2022 Q4 and 2023 Q4. Their baseline classification and weights remain fixed across visits. Matching jobs, age and marital status gives 313 workers from 287 households; matching jobs alone gives 466 workers from 382 households. All are employed at the selected baseline, so the baseline employment gap is zero.

The gap opens sharply in 2023 Q4 (Figure 2). It is −51.9 points [−63.7, −40.1] in the match including age and marital status, and −43.5 points [−54.8, −32.2] in the job-only match. Corresponding next-quarter gaps for a 2022 Q3 baseline are +7.7 and +6.7 points, with intervals including zero. Table B4 reports the estimates and sample sizes. Earlier observed gaps vary, especially in the smaller match, but the large deterioration appears at the onset of the crisis.

Figure 2 Employment gaps for fixed baseline workers at observed visits



Notes: Permit-minus-domestic employment changes in percentage points, relative to the employed Q3 baseline. Visits are observed at offsets −4, −3, 0 and +1. The 2022 and 2023 samples are separately matched. Whiskers use 1.96 household-score standard errors; Table B4 uses household t intervals. Lines connect observed visits.

There is some subsequent recovery among workers who can be followed. A sample observed in 2023 Q4, 2024 Q3 and 2024 Q4 contains 309 men in 36 supported cells: 111 baseline permit workers and 198 domestic workers. With the workers and baseline weights fixed, permit-group employment rises from 10.6 to 21.0 to 34.9 per cent. West Bank employment rises from 7.0 to 13.0 to 21.3 per cent. Even at the final visit, 61.1 per cent of the permit group are unemployed. The effective permit-group sample size is 77.

The 2025 records provide the wider population picture. Employment among resident men aged 18–59 is 59.2 per 100 in 2025 Q4, compared with 77.9 in 2023 Q3. Table 5 also reports the conventional unemployment rate for all residents aged 15 and above. The reconstructed 27.5 per cent in 2025 Q4 agrees with the PCBS published figure (PCBS, 2026). No worker key from the 2023 Q3 baseline appears in the supplied 2025 quarters, so recovery of that original group can be followed only through the earlier linked visits.

Table 5 Employment and unemployment in West Bank resident populations

Population	Quarter	Employed per 100	West Bank jobs per 100	External jobs per 100	Unemployment rate (%)
Men 18–59	2023 Q3	77.9	58.8	18.8	9.9
Men 18–59	2023 Q4	55.5	52.1	3.2	35.3
Men 18–59	2024 Q3	57.6	54.3	3.0	31.6
Men 18–59	2025 Q4	59.2	53.7	5.4	27.7
All residents 15+	2023 Q3	41.2	32.9	8.1	12.9
All residents 15+	2023 Q4	31.1	29.5	1.4	33.2
All residents 15+	2024 Q3	32.5	31.0	1.4	30.7
All residents 15+	2025 Q4	33.0	30.6	2.3	27.5

Notes: Quarterly PCBS records with relative person weights. Employment and workplace columns use the resident population as denominator; unemployment uses the employed-plus-unemployed labour force. Other workplaces explain small differences between total employment and the two workplace columns.

5 Robustness and limitations

The main employment contraction survives the common-support and cohort checks. A stricter support rule gives −41.5 points. Construction and nonconstruction comparisons are both negative, and leaving out one crisis-crossing cohort at a time gives estimates between −39.4 and −43.2 points (Table B5). Detailed occupation matching and wage-employee restrictions also leave a large decline. Baseline hours adjustment changes the detailed wage-employee estimate only modestly (Table B2). These checks cover different supported populations.

Prior histories have a larger influence on the estimated magnitude. Among the 2,926 links with a prior observation, the coefficient is −43.5 points without a trend and −31.1 with one. On the same sample, controlling for previous employment and hours gives −40.2 and −23.7 points. Table B1 reports these results and exact-history rematching. The direction is stable, while the size depends partly on the workers observed and the path used for their earlier employment gap.

The main pre-crisis gap trend is −3.6 points per year, with an interval from −11.5 to +4.2. Its imprecision matters for a counterfactual based on a flat gap (Roth, 2022). Artificial pre-crisis cutoffs give small estimates around zero. Other comparisons are less stable: the same-household sample has a large earlier trend, and the permit–undocumented contrast changes sign after trend adjustment. Tables B6 and B7 give these diagnostics, including the fee-payment comparisons.

Appendix A4 examines bounded departures from the earlier gap path in the fixed-worker samples, following the sensitivity principle in Rambachan and Roth (2023). Under the smaller bounds, conclusions depend on the match and confidence construction. At twice the pre-gap slope measure, all reported sets include zero. These are custom projections. They show how much the attribution depends on the allowed counterfactual movement.

Endpoint coverage is the other main limitation. Within supported cells, weighted availability is 42.3–48.5 per cent across groups and periods. The unavailable records combine survey rotation, nonresponse and extract coverage, and rotation eligibility is absent from the supplied files. The estimates therefore describe workers with observed endpoints. Assigning every unavailable employment outcome zero or one produces broad all-baseline bounds that include zero (Appendix A5 and Table B8). Recovering the scheduled reinterview groups would help separate planned rotation from outcome-related loss of observations.

6 Discussion and conclusion

The withdrawal of external labour access left West Bank workers with few domestic alternatives. In the matched crisis-crossing sample, only 9.6 per cent of baseline permit workers hold a West Bank job at follow-up and 74.5 per cent are unemployed. The adjusted relative employment contraction is 42.0 percentage points, or 38.9 with a linear trend. Wage employees matched in detailed occupations also show a decline of about 34–37 points. Employment recovers partly in the smaller group followed through 2024 Q4, while unemployment remains high.

These results connect the immediate adjustment to the longer history of Palestinian labour dependence. Farsakh (2005), Roy (1987) and Berda (2018) describe how earnings and employment can be tied to an external institutional system while domestic productive capacity remains weak. The survey evidence shows what this means when access is withdrawn. Most observed workers without an external job remain without domestic work, and initially domestic workers lose employment too. The shared deterioration is consistent with Fallah's (2018) evidence from an earlier period.

Domestic hiring depends on the conditions facing local firms. Hinn (2025) describes financing and effective-demand obstacles in construction; Rabee (2026) discusses wider restrictions and contraction. Workers may have relevant skills and still face employers unable to finance projects or reach their markets. The limited domestic employment observed here gives a reason to put job creation and firms' ability to hire alongside training in the policy response.

The loss of earnings also reaches households. Earlier Palestinian studies link employment disruption and household resources to children's schooling and work (Di Maio and Nandi, 2013; Di Maio and Nisticò, 2019; Hallaq and Khalifah, 2022, 2024). These findings support monitoring household consequences during the present crisis and considering income support alongside employment measures.

Access remains an institutional question even where external demand returns. Habbas (2024) and Bondy and Preminger (2026) describe disagreement over Palestinian employment after October 2023. Quzmar and AL-Labadi (2025) discuss the conditional incorporation of Palestinian labour. A worker and an employer may both want employment to continue while entry remains restricted. The insecurity of access is part of the dependence documented here. The positive relative coefficient for West Bank employment comes mainly from the comparator's decline. The absolute destination shares show how little work the permit group obtained locally. By 2025 Q4, domestic employment among resident men is still below its pre-crisis level relative to population. The domestic economy has provided a limited fallback after the loss of external labour access.

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Appendix A Construction and inference

A1 Standardisation weights and matched-gap estimation

Let b denote a baseline quarter, c the cell formed from baseline occupation, industry, age band and marital status, and g the baseline group, with $g = 1$ for permit workers and $g = 0$ for domestic workers. Let $\widetilde{w}_i$ be the survey person weight divided by its baseline-quarter mean. For a given cell and cohort, define W_{gcb} as the sum of $\widetilde{w}_i$ for group g. I give both groups the overlap mass.

$$H_{cb} = \frac{W_{1cb}W_{0cb}}{W_{1cb}+W_{0cb}} \tag{A1}$$

This construction reduces the influence of cells in which one group is very large but the other has little support. It has a direct connection to empirical overlap weighting (Li, Morgan and Zaslavsky, 2018). In particular, if the cell-level permit share is $e_{cb} = W_{1cb}/(W_{1cb} + W_{0cb})$, the within-cell rescaling factors are $1 - e_{cb}$ for permit workers and e_{cb} for domestic workers. This gives the same factor as overlap weights calculated from these cell proportions. The survey-weight normalisation and later period standardization make the full estimator a separate construction.

For each cell c, the target T_c is the total overlap mass H_{cb} across crisis-crossing cohorts. If s(b) indicates the period containing cohort b, the individual analysis weight is

$$\omega_i = \widetilde{w}_i \frac{H_{cb}}{W_{gcb}} \frac{T_c}{\sum_{b\prime:s(b\prime)=s(b)} H_{cb\prime}}. \tag{A2}$$

The first adjustment gives permit and domestic workers equal mass within a supported cell–cohort. The second makes the total mass of cell c equal to T_c in both ordinary and crisis-crossing periods. It follows algebraically that the matching variables are jointly balanced within each cell and on the period-level target. The maximum numerical discrepancy in these mass identities is below 10^{-14} in the numerical balance checks. The maximum standardised difference in residual demographic variables is approximately 0.37, which is why the cell-gap regressions also adjust for the remaining demographic gaps. The main model uses 498 cell–cohort observations. The WLS weight of each observation is the common analysis mass of either group in that cell–cohort. It includes cell fixed effects, baseline season effects, the crisis-crossing indicator, group differences in demographic controls and their interactions with the crisis indicator. The trend specification adds baseline calendar time in years and no follow-up sector or employment outcome is used to select matching cells.

The effective sample size is $(\sum_i \omega_i)^2 / \sum_i \omega_i^2$ within each group and period. The matched sample has 70 cells and 11 baseline cohorts. All baseline workers are employed resident men aged 18–59.

A2 Accounting checks and inference

The main conditional intervals cluster the cell-gap regression by the eleven baseline quarters and use a t critical value with ten degrees of freedom. The reported intervals are conditional on the constructed weights, the links observed and the regression specification. The individual weighted implementation gives the same point coefficients. The fixed-worker analyses use household-aggregated scores, described in Appendix A3.

Each destination equation uses the same matched links, weights and regression design as the employment equation. Current employment equals external plus West Bank plus other employment; these three shares, unemployment and nonparticipation add to one for every respondent. In the accounting audit, the employment-coefficient identity differs by less than 10^{-15} and the five-category coefficient sum by less than 2×10^{-15}.

The unadjusted standardised gap change calculated directly from group means is −0.4277317774. The equivalent regression without seasonal adjustment gives the same coefficient to less than 10^{-15}. An individual

weighted regression with cell–cohort intercepts and signed gap regressors reproduces the aggregate estimate to less than 5×10^{-11}. Standard-error corrections can differ between implementations because they count observations and absorbed degrees of freedom differently.

A two-way covariance constructed from cohort clustering and person-key clustering by inclusion–exclusion gives a positive scalar variance for the crisis coefficient. However, the full matrix has a minimum eigenvalue of approximately −0.025 and is not positive semidefinite. As Cameron, Gelbach and Miller (2011) explain, this can arise with multiway covariance estimates. Table B9 reports the scalar comparison, while the main confidence intervals use cohort clustering in the cell-gap model.

A3 Fixed-worker histories

The main matched estimates compare different baseline cohorts. To see the timing more directly, I fix workers' baseline classification and their weights across the visits that are actually observed. For the 2023 comparison, workers must be employed in 2023 Q3 and have valid records in 2022 Q3, 2022 Q4, 2023 Q3 and 2023 Q4. The detailed matching sample contains 313 workers from 287 households; the job-only match contains 466 workers from 382 households. The fixed classification prevents workers being reassigned to a different baseline group when their employment changes.

The fixed-worker sample requires unique records at four visits: baseline quarter minus four, minus three, baseline and plus one. Workers are employed at baseline, and the same group assignment and weights are used at each visit. The gap at each nonbaseline visit is a weighted permit–domestic difference in within-worker employment change from the selected baseline. An independent weighted change regression with cell effects reproduces the fixed-worker point estimates to less than 6×10^{-16}. Inference uses sums of weighted-mean influence scores aggregated by household. The finite-sample factor is $G/(G-1) \times (N-1)/(N-2)$, where G denotes households and N workers. The original three by three covariance matrices for the nonbaseline visits are positive definite.

Figure 2 uses 1.96-standard-error whiskers for presentation, whereas Table B4 uses household t critical values. The baseline gap is exactly zero because the sample is employed then, and it is not given an independent confidence interval.

In separately matched endpoint samples, baseline permit workers' employment is 11.7 per cent in 2023 Q4, 20.8 in 2024 Q3 and 35.8 in 2024 Q4. The corresponding West Bank employment shares are 5.5, 12.4 and 21.3 per cent. Because the overlap population differs at each endpoint, this sequence alone is not a within-person recovery path.

The separately matched endpoint samples have different overlap populations. Section 4.4 instead reports the recovery sequence for the smaller sample whose workers and weights remain fixed across the three endpoints.

A4 Counterfactual sensitivity sets

The fixed-worker comparisons offer a way to ask how large a departure from the earlier gap path would be needed to weaken an attribution to the crisis exposure. The approach follows the general insight of Rambachan and Roth (2023): one can report what happens under specified bounds on departures from the counterfactual trend. My implementation here is a custom projection based on three observed gap coefficients and is not the official DiD confidence procedure.

Let δ_t be the permit-minus-domestic employment gap that would have been observed without the crisis exposure, expressed relative to the employed Q3 baseline. The observed earlier gaps inform the scale of its possible movement. Define L as the larger of the one-quarter average change between the two earlier observations and the average change over the three quarters from the later pre-observation to baseline. I allow the absolute untreated post-baseline gap to be at most ML:

$$L = \max\left\{|\delta_{-3} - \delta_{-4}|, \frac{|\delta_{-3}|}{3}\right\}, \qquad |\delta_{+1}| \leq ML. \tag{A3}$$

Here M is a sensitivity parameter. The baseline gap is zero by sample construction. The restriction is stronger when M is small and permits more differential untreated movement when M rises. It uses the irregular visits that are observed.

Let $\hat{b} = \left(\hat{\delta}_{-4}, \hat{\delta}_{-3}, \hat{\delta}_{+1}\right)'$ be the three observed nonbaseline coefficients for the 2023 fixed-worker comparison, with household-score covariance matrix V. Equation (A3) bounds the untreated post gap by M times the larger of two observed pre-gap slope measures. The assumption is about the *untreated* gap; it does not exclude differential effects of the war.

For the simultaneous t box, each element of $\hat{b}$ is allowed to move within its Bonferroni interval using $t_{1-0.05/(2\times3)}$ and household degrees of freedom. For the joint normal ellipse, the radius is $r = \sqrt{\chi^2_{3,0.95}}$. The pre-slope facets are $a \in \{(-1,1,0)', (1,-1,0)', (0,1/3,0)', (0,-1/3,0)'\}$. For each facet let $c = (0,0,1)' + Ma$. The projected lower and upper endpoints are the minimum over facets of $c'\hat{b} - r\sqrt{c'Vc}$ and the maximum of $c'\hat{b} + r\sqrt{c'Vc}$, respectively. The symmetric facets cover both signs of the possible untreated departure.

An independent constrained-optimisation check reproduces the analytic projection at $M = 1$ to less than 10^{-12}. These procedures produce approximate confidence sets conditional on the selected sample, fixed weights and household asymptotics. For the detailed and job-only matches, respectively, the sensitivity calculation gives negative conclusion breakdown values of about 1.55 and 1.44 for the ellipse, and 1.03 and 0.87 for the simultaneous box. Untruncated sets may go beyond the range of an individual employment probability because the reported parameter is a contrast of gaps.

I construct two conditional 95 per cent confidence sets: a projection of a joint normal covariance ellipse for the three gap coefficients, and a Bonferroni simultaneous t box. Their numerical results are in Table A1. At $M = 1$, the ellipse's upper endpoints remain negative for both matches, whereas the t box includes zero for the job-only match. At $M = 2$, both constructions include zero for both samples. Thus, the inference about crisis attribution depends on how much counterfactual movement is considered possible and on the confidence construction.

Table A1 Confidence sets under bounded counterfactual departures

Matching	M	Joint normal ellipse, 95% set (pp)	Simultaneous t box, 95% set (pp)
Detailed	0	[−68.7, −35.1]	[−66.4, −37.4]
Detailed	0.5	[−78.2, −26.3]	[−84.6, −19.2]
Detailed	1	[−90.3, −14.6]	[−102.8, −1.0]
Detailed	2	[−117.9, +12.7]	[−139.2, +35.4]
Job only	0	[−59.5, −27.4]	[−57.3, −29.7]
Job only	0.5	[−67.4, −19.8]	[−74.3, −12.7]
Job only	1	[−77.4, −9.8]	[−91.2, +4.3]
Job only	2	[−100.8, +13.5]	[−125.2, +38.3]

Notes: Custom projections for the 2023 fixed-worker samples. The absolute untreated post gap is bounded by M times the pre-gap slope measure. Intervals are conditional on fixed weights and household-score inference. These are not official HonestDiD conditional or hybrid intervals.

A5 Unavailable endpoint bounds

For the missing-outcome exercise I return to the eligible baseline records in the main supported cells, with both groups present in each retained cell–cohort. All matching variables are measured before the endpoint. The all-baseline records are standardised to the main crisis overlap-cell target. An endpoint is available only when the four-quarter-later longitudinal key is observed with plausible age progression.

Weighted endpoint availability is 42.3–48.5 per cent across the four group-period categories. Missing endpoints include survey rotation, nonresponse and extract coverage. Table B8 reports the group-specific rates and employment bounds. The files lack rotation eligibility, so this exercise treats all unavailable outcomes as unrestricted.

To obtain an employment lower bound for a group, each unavailable binary outcome is assigned zero; to obtain its upper bound, each is assigned one. The lower gap-change bound combines the crisis-permit lower, crisis-domestic upper, ordinary-permit upper and ordinary-domestic lower. This gives [−131.9, +88.9] points for an unadjusted all-baseline target. It has no accompanying sampling interval and should not be confused with the adjusted regression estimate for observed links. As in the partial-identification logic discussed by Horowitz and Manski (2000), the breadth of the interval illustrates what cannot be learned without assumptions on missing outcomes.

The bounds include zero. They describe an unadjusted all-baseline target, while the main regression estimates use observed links. The missing-outcome exercise therefore addresses a separate population question from the conditional intervals reported in the main text.

Appendix B Additional comparisons

B1 Prior employment histories

Prior observations can be used to control for employment history and hours, or to match workers more closely on those states. Requiring a previous visit also selects a different group of workers. I compare adjusted and unadjusted results on the same history sample to separate changes caused by history adjustment from changes caused by the sample restriction. Exact matching on prior employment makes the earlier employment gap zero by construction.

Table B1 reports the change in the permit–domestic employment gap. In the main matched sample the adjusted coefficient is −42.0 percentage points. Allowing the baseline gap to follow a linear trend gives −38.9 points. Both estimates indicate that employment among initially permit-based workers deteriorated strongly relative to initially domestic workers. The result is not determined by one history specification. When I restrict the sample to links from baseline 2021 Q2 onward, the estimates remain close to those in the full sample. Requiring a prior observation reduces the sample to 2,926 links and gives −43.5 points without a trend and −31.1 with one. On this identical history sample, adding prior employment and hours gives −40.2 and −23.7 points. The difference shows that prior labour-market position matters for the estimated counterfactual, especially with a linear trend. Exact-history rematching remains negative but changes the workers being compared.

Table B1 Changes in employment gaps and prior histories

Specification	Links	Adjusted, pp [95% CI]	With trend, pp [95% CI]
Main baseline match	8,752	−42.0 [−47.1, −36.9]	−38.9 [−50.1, −27.7]
Audited history window	8,037	−42.8 [−47.5, −38.1]	−39.5 [−53.1, −25.9]
Prior history observed	2,926	−43.5 [−53.7, −33.2]	−31.1 [−50.4, −11.8]
Same sample + history controls	2,926	−40.2 [−51.1, −29.4]	−23.7 [−40.8, −6.6]
Exact prior employment	2,623	−42.9 [−52.9, −33.0]	−29.2 [−45.0, −13.5]
Exact prior employment + hours	1,733	−42.4 [−49.9, −34.8]	−32.8 [−53.2, −12.5]
Job-only + history controls	4,556	−43.4 [−64.1, −22.7]	−35.5 [−59.5, −11.5]

Notes: Permit-minus-domestic gap changes. Specifications adjust baseline demographic gaps and seasons. The audited window begins at 2021 Q2; history rows require a prior-quarter observation. History controls use previous employment and hours categories with crisis interactions. Rematching changes the common-support target.

B2 Detailed jobs and hours

There is an obvious concern that an external worker and a domestic worker in the same broad occupation may still hold quite different jobs. Table B2 therefore tightens comparability in stages. Detailed occupation matching gives a contraction of −40.9 points without a trend. Restricting the broad match to wage employees gives −38.0 points. When I combine the detailed occupation match with the wage-employee restriction, 4,952 links remain and the coefficient is −33.6 points [−43.1, −24.1]. With a linear trend it is −35.7 [−50.0, −21.3].

The adjusted ordinary-period gap trend is approximately zero when matching detailed occupations with all statuses. In the detailed-occupation wage-employee sample it is +3.1 percentage points per year, with an interval of [−2.5, +8.6].

Table B2 Job and hours comparisons

Baseline comparison	Links	Cells	Adjusted, pp [95% CI]	With trend, pp [95% CI]
Broad jobs, all statuses	8,752	70	−42.0 [−47.1, −36.9]	−38.9 [−50.1, −27.7]
Detailed jobs, all statuses	7,020	84	−40.9 [−46.9, −35.0]	−38.2 [−50.5, −26.0]
Broad jobs, wage employees	6,220	62	−38.0 [−46.2, −29.9]	−38.3 [−51.4, −25.1]
Detailed jobs, wage employees	4,952	74	−33.6 [−43.1, −24.1]	−35.7 [−50.0, −21.3]
Broad jobs, all; hours adjusted	8,752	70	−38.8 [−43.8, −33.7]	−35.6 [−47.2, −24.0]
Detailed jobs, all; hours adjusted	7,020	84	−39.6 [−46.7, −32.5]	−36.9 [−50.5, −23.3]
Broad jobs, wage; hours adjusted	6,220	62	−37.0 [−44.8, −29.2]	−36.9 [−50.5, −23.2]
Detailed jobs, wage; hours adjusted	4,952	74	−34.5 [−43.6, −25.4]	−37.0 [−50.7, −23.2]

Notes: Detailed occupation uses PW39; wage employees have EMPSTATS = 3. Hours adjustment adds baseline actual hours and its square with crisis interactions. All matched links have usable baseline hours. Rows are separately matched and represent different overlap targets.

B3 Observed origins of domestic jobs

Table B3 Observed origins of West Bank jobs

Origin four quarters earlier	2023 Q3	2023 Q4	2024 Q3	2025 Q4
Same domestic sector	30.8	34.5	35.9	35.9
Another domestic sector	4.3	4.2	4.1	2.5
Permit-based external	1.0	1.1	1.6	0.2
Other external	2.6	0.9	1.0	0.3
Previous nonemployment	4.1	3.3	3.0	6.0
Other previous workplace	0.0	0.0	0.0	0.0
No valid prior observation	57.1	56.0	54.4	55.1

Notes: Percentages of all current West Bank jobs held by resident men aged 18–59, using endpoint relative person weights. Missing and age-implausible histories remain unresolved, with no inverse-observation adjustment. Same-sector observations at two dates can include intervening employment changes. The 2025 endpoint uses 2024 origins.

The unresolved category is left as observed. The permit-origin share covers workers whose earlier permit employment can be linked. These records classify workers' origins; changes of employer or worker between observations mean that the account cannot establish job creation. The 2025 classification looks back to 2024 and covers a different origin period from the pre-October 2023 workforce.

B4 Fixed baseline workers

Everyone in the sample is employed in 2023 Q3, so the permit–domestic employment gap at that baseline is zero by construction while the earlier gaps can be different from zero. In the detailed match they are +8.2 points in 2022 Q3 and −6.7 in 2022 Q4. After the crisis starts, the 2023 Q4 gap is −51.9 points, with a conditional interval from −63.7 to −40.1. The job-only comparison gives −43.5 points at the same endpoint and earlier gaps of +12.9 and +0.1.

I repeat this design for a 2022 Q3 baseline and the corresponding next-quarter changes are +7.7 and +6.7 points and neither is statistically distinguishable from zero under the reported conditional intervals. The difference between the two years is consistent with the abrupt timing of the 2023 deterioration.

Table B4 Fixed baseline workers and next-quarter employment change

Q3 baseline	Matching	Workers	Households	Gap change, pp [95% CI]
2022	Jobs, age and marital status	355	330	+7.7 [−3.7, +19.1]
2022	Jobs only	528	451	+6.7 [−1.7, +15.2]
2023	Jobs, age and marital status	313	287	−51.9 [−63.7, −40.1]
2023	Jobs only	466	382	−43.5 [−54.8, −32.2]

Notes: Workers have observations at baseline offsets −4, −3, 0 and +1. They are employed at the Q3 baseline, so that gap is zero by construction. Intervals use household-aggregated scores and household t critical values. The 2022 and 2023 samples are separately matched.

B5 Sample and support checks

Table B5 Employment sample and common-support checks

Specification	Links	Adjusted, pp [95% CI]	With trend, pp [95% CI]
Main	8,752	−42.0 [−47.1, −36.9]	−38.9 [−50.1, −27.7]
Two-cohort support	8,120	−41.5 [−45.8, −37.1]	−37.2 [−49.0, −25.4]
Construction	3,826	−47.0 [−54.4, −39.6]	−39.8 [−56.6, −23.0]
Nonconstruction	4,926	−44.5 [−50.7, −38.3]	−49.5 [−64.6, −34.4]
Work-permit wage employees	6,026	−38.3 [−46.9, −29.7]	−37.8 [−51.0, −24.6]
Omit 2022 Q4	7,919	−43.2 [−50.0, −36.4]	−39.5 [−55.6, −23.4]
Omit 2023 Q1	7,894	−39.4 [−45.7, −33.2]	−36.4 [−47.1, −25.6]
Omit 2023 Q2	7,893	−41.7 [−46.8, −36.7]	−39.0 [−50.3, −27.7]
Omit 2023 Q3	7,557	−42.8 [−50.2, −35.4]	−39.1 [−51.0, −27.2]

Notes: Samples are separately rematched using baseline variables and have different overlap targets. Construction and nonconstruction rows describe separate supported populations. The last four rows omit one crisis-crossing cohort at a time. Intervals use cohort clustering.

B6 The same-household comparison

An alternative comparison uses permit and domestic workers from the same baseline household. It contains 381 household–cohort pairs and 886 worker links. Without a trend, the adjusted change is −44.4 points. With a trend, it becomes −83.2 points. The ordinary-period gap in this sample has an estimated trend of +32.0 points per year, with a conditional interval of [+21.0, +42.9]. This large earlier movement weakens the household comparison as a counterfactual. Household composition may change, and the domestic member can also be affected by lost family earnings. I therefore report the household result as a diagnostic that fails a stability check.

Table B6 Same-household comparison and its pretrend

Comparison	Cohort-clustered, pp [95% CI]	Household-clustered, pp [95% CI]
Adjusted crisis crossing	−44.4 [−56.9, −31.9]	−44.4 [−58.3, −30.5]
Adjusted with linear trend	−83.2 [−97.7, −68.7]	−83.2 [−106.5, −59.9]
Ordinary-period gap trend, pp/year	+32.0 [+21.0, +42.9]	+32.0 [+16.8, +47.1]

Notes: 381 household–cohort pairs, 271 household keys and 886 worker links. Each pair contains both baseline groups. The pretrend uses 224 ordinary pairs and 166 households. The columns use alternative one-way cluster dimensions. The large earlier trend weakens the household counterfactual.

B7 Alternative external comparisons and artificial cutoffs

I examine two artificial crisis dates using only observations whose endpoints come before the actual October 2023 disruption. Assigning a cutoff at 2022 Q4 gives an adjusted coefficient of +0.6 percentage points; a cutoff at 2023 Q1 gives −0.3. Both intervals include zero and run approximately from −8 to +8 points. These results make it less likely that a similarly large discontinuity was already visible in the recent ordinary period. In the short pre-crisis window, the artificial-cutoff indicator is collinear with a combination of season and linear-trend controls, so trend versions are not identified and are not presented as additional tests.

External workers without permits appear to offer a closer comparison in terms of destination labour demand, but their employment is also affected by closure, enforcement and changes in demand, and their pre-crisis employment gap relative to permit workers already follows a material trend. The adjusted permit-minus-undocumented change is −12.5 points without a trend, but +1.7 points with one, with an interval spanning [−21.3, +24.6].

Pooling permit and undocumented workers into an external employment group and comparing it with domestic workers yields −36.7 points without a trend and −44.7 with one. This supports a broader description of external employment dependence. Fee-payment comparisons within the permit group are also unstable when tenure and trends are included. The evidence on traded permits (Adnan and Etkes, 2019) gives institutional context, not an instrument in these data. I do not use fee payment as an instrumental variable or interpret its association as the cause of later job loss. Table B7 gives the numerical diagnostics.

Table B7 Other comparison groups and earlier artificial cutoffs

Comparison	Links	Gap change, pp [95% CI]
Permit vs undocumented	3,102	−12.5 [−22.3, −2.7]
Permit vs undocumented, with trend	3,102	+1.7 [−21.3, +24.6]
All external vs domestic	10,978	−36.7 [−40.4, −33.0]
All external vs domestic, with trend	10,978	−44.7 [−50.3, −39.2]
Fee payer at baseline	2,050	−3.9 [−7.6, −0.1]
Fee payer, with trend	2,050	−0.4 [−11.1, +10.2]
Fee payer, with tenure	2,050	−2.0 [−5.7, +1.7]
Fee payer, tenure and trend	2,050	+5.8 [−3.1, +14.6]
Artificial cutoff 2022 Q4	5,052	+0.6 [−7.2, +8.4]
Artificial cutoff 2023 Q1	5,226	−0.3 [−7.8, +7.2]

Notes: Other external workers include workers without permits. Fee comparisons use work-permit wage employees. Fee payment and undocumented work are comparison variables, not instruments for the suspension. Artificial cutoffs use only pre-crisis endpoints and seven baseline clusters.

B8 Endpoint availability

Table B8 Availability of endpoints and unadjusted employment bounds

Period	Group	Baseline cases	Observed (%)	Employment bounds (%)
Ordinary	Domestic	9,653	42.3	36.9 to 94.6
Ordinary	Permit	3,739	44.4	39.0 to 94.6
Crisis	Domestic	6,306	44.0	27.3 to 83.2
Crisis	Permit	2,400	48.5	9.1 to 60.6

Notes: All eligible baseline records in supported cells, standardised to the main overlap-cell target. Missing endpoints include rotation, response and extract coverage. Assigning unavailable employment outcomes zero or one yields the all-baseline gap-change range [−131.9, +88.9] points, without regression adjustment or sampling intervals.

B9 Inference with repeated person links

Table B9 Diagnostic comparison of inference with repeated person links

Linear trend	Covariance method	Gap change, pp [95% CI]
No	Cell gap clustered by cohort	−42.0 [−47.1, −36.9]
No	Individual, clustered by cohort and person	−42.0 [−47.2, −36.8]
Yes	Cell gap clustered by cohort	−38.9 [−50.1, −27.7]
Yes	Individual, clustered by cohort and person	−38.9 [−49.8, −28.1]

Notes: Both implementations give the same point coefficients. Finite-sample corrections explain some interval differences. The full two-way covariance is not positive semidefinite; the table reports positive scalar crisis-coefficient variances. Intervals remain conditional on the observed links and constructed weights.

Appendix C Employment adjustment framework

C1 Search vacancies and local demand

Consider a domestic labour market with S effective searchers and V vacancies. Following the matching-function literature reviewed by Petrongolo and Pissarides (2001), suppose matches are produced by a constant-returns function:

$$M = \mu S^{\eta} V^{1-\eta}, \qquad f = \frac{M}{S} = \mu \left(\frac{V}{S}\right)^{1-\eta}, \quad 0 < \eta < 1. \tag{C1}$$

The parameter μ reflects matching efficiency and f is the match rate per effective searcher. The survey does not contain vacancies or effective search effort. If R displaced external workers increase effective local search by σR, where $\sigma \geq 0$, then $S = S_0 + \sigma R$. Holding other conditions fixed,

$$\frac{\partial \log f}{\partial R} = (1-\eta)\left[\frac{\partial \log V}{\partial R} - \frac{\sigma}{S_0 + \sigma R}\right] \tag{C2}$$

If vacancies do not expand as displaced workers search, the vacancy to searcher ratio falls. If lost external earnings also reduce vacancies, job finding becomes more difficult. Movement restrictions could reduce μ as well. These channels are consistent with the Palestinian evidence on demand and mobility (Calì and Miaari, 2018; Hinn, 2025; Rabee, 2026).

C2 The relative employment gap

The matched comparison measures the employment gap between the baseline permit group and the baseline domestic group. Let x be the permit group's external-employment share at follow-up, p_P its probability of employment at another workplace conditional on not being employed externally, and p_D the domestic comparator's overall probability of employment. The gap is

$$G = x + (1-x)p_P - p_D \tag{C3}$$

If $p_P = p_D$, the gap becomes $x(1-p_D)$. Therefore, weaker domestic employment prospects can influence the relative gap even when the two groups have the same non-external employment probability. More generally, a positive coefficient for domestic employment may reflect declining domestic employment in the comparison group. Nevertheless, if the arrival of displaced searchers reduces employment among domestic workers, then the comparison group is affected by the disruption. A permit–domestic contrast measures relative incidence and does not recover the total effect of the suspension on either group. This problem is consistent with the earlier Palestinian evidence on labour-supply spillovers (Mansour, 2010; Fallah, 2018).