The Job Diversion Matrix

How Workers' Alternatives Determine the Harm from Layoffs and Mergers*

Theodor Friederiszick† Axel Malmcrona‡ Nelson Sunuwar§

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Abstract

Where would workers go if their jobs disappeared? The answer is central to understanding workers' losses after displacement and the labor market effects of mergers. We propose a method that uses matched employer-employee data to measure how important one job is as an alternative for another job's workers, for every pair of jobs in an economy. We recover these alternatives from a structural model of job choice estimated on Swedish administrative data covering 2.5 million workers and 590,000 jobs per year. The estimates form the Job Diversion Matrix, from which we compute each job's number of effective alternatives, a summary of how many meaningful alternatives its workers have. Following plant closures, workers from jobs with few effective alternatives earn 18% less than matched controls one year later, nearly three times the loss for workers with many alternatives. Applying the Job Diversion Matrix to mergers, we find that only 10% of the Swedish mergers combine employers whose jobs are close alternatives. Five years later, relative to matched controls, workers whose jobs have a close alternative at the merger partner earn 7% less and are 5 percentage points more likely to be unemployed, and establishments are 23 percentage points more likely to have closed. Workers whose jobs divert little to the merger partner experience no changes in earnings or unemployment. Because diversion ratios can be estimated before a merger, the Job Diversion Matrix can flag mergers that warrant closer review.

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†University of Zurich. Email: theodor.friederiszick@econ.uzh.ch

‡University of Gothenburg. Email: axel.malmcrona@gu.se

§University of Zurich. Email: nelson.sunuwar@econ.uzh.ch

1 Introduction

Labor market shocks hit workers differentially. Following the China Shock, high-wage workers were more likely to move out of manufacturing and experienced much smaller earnings losses (Autor et al., 2014). A wave of hospital consolidations led to decreased wage growth for nurses and pharmacists, while janitors and cafeteria workers were largely unaffected (Prager and Schmitt, 2021). Germany's transition away from combustion engines puts many well-paid manufacturing jobs at risk, while new jobs in the industry require different skills (Die Zeit, 2024). Across all these settings, a common theme emerges: what matters for workers is whether they have other jobs to turn to. Yet no existing measure tells us, job by job, which other jobs workers would consider instead or how important each one is.

The need for such a measure is especially clear when merger review turns to labor markets. The key question is whether the merger combines employers that workers consider close job alternatives. This concern was central to the Federal Trade Commission's labor market case against the proposed merger between Kroger and Albertsons, two of the largest US supermarket chains.[1] While the merging parties argued that union grocery workers could readily move to other retail jobs, the Federal Trade Commission argued that their relevant alternatives were primarily other union grocery jobs and that Kroger and Albertsons were among the major employers offering them. The court ultimately found the labor market analysis insufficiently developed, noting that it was "not aware of any standard economic analysis used to measure employee diversion" (United States District Court for the District of Oregon, 2024, p. 62).

To this end, we develop the Job Diversion Matrix, which measures how close an alternative any two jobs are in an economy. We construct the matrix using a structural model of job choice estimated from Swedish matched employer-employee data. Using plant closures, we show that the matrix's predictions closely track displaced workers' moves across commuting zones and occupations. Closures also reveal why these alternatives matter. Workers from jobs with few alternatives lose nearly three times as much in earnings in the first year as those with many alternatives and recover more slowly. Applying the Job Diversion Matrix to mergers, we find that only one in ten Swedish mergers combines close job alternatives. But five years later, workers in those jobs earn seven percent less and are five percentage points more likely to be unemployed than their matched controls. Work-

[1] Similar questions arose for authors in the proposed merger of Penguin Random House and Simon & Schuster (United States District Court for the District of Columbia, 2022), for film and television writers in Paramount's acquisition of Warner Bros. Discovery (Writers Guild of America, 2026), for journalists in DPG Media's acquisition of RTL Nederland (Netherlands Authority for Consumers and Markets, 2025), and for delivery drivers in New Jersey's monopsony case against Amazon (Wagener, 2026).

ers whose jobs have no close alternative at the merger partner experience no such losses. Estimated before a merger, the matrix can flag mergers that warrant closer review.

The object of interest is the job diversion ratio, where we define a job as an occupation at an establishment. For a given pair of jobs, the diversion ratio measures the share of workers at one who would choose the other if their current job disappeared. Observed job-to-job flows would be a natural starting point for estimating diversion ratios, but at the level of individual jobs they are sparse: most job pairs never record a move, even when they are close alternatives. Moreover, observed moves combine voluntary quits with involuntary separations and therefore do not directly reveal where workers would go if their job disappeared. We recover this counterfactual by estimating a structural model of job choice using discrete choice methods (Train, 2009). Workers value jobs according to their wages and amenities, the commute they require, their occupational and educational fit, and the cost of switching jobs. We allow these valuations to vary with observed worker characteristics. By relating choices to these characteristics, the model assigns a choice probability to every feasible worker-job pair, including job pairs between which no move is observed. We can then remove a job from the choice set, hold all other jobs fixed, and calculate where its workers would go.

We estimate the model on Swedish matched employer-employee data covering 2.5 million workers and 590,000 jobs per year. Collecting the estimated diversion ratios between every pair of jobs produces the Job Diversion Matrix. Each entry is the diversion ratio from a particular origin job to a particular destination job, while the diagonal records the diversion ratio to nonemployment. To summarize the full set of alternatives to each job, we compute its number of effective alternatives: the number of equally important alternatives that would generate the same concentration of diversion ratios as the job's actual alternatives.[2] This produces an economy-wide map of how broadly or narrowly workers' alternatives are distributed. Most jobs have many effective alternatives, but the variation is substantial. The median job has 97 effective alternatives in Stockholm, compared with 9 in the smallest commuting zones. Yet one in ten jobs in Stockholm has fewer than 15 effective alternatives, while one in ten jobs in the smallest zones has more than 24. Across occupations, the ranking does not follow a simple high-skill versus low-skill divide. Data scientists and cashiers are among the occupations with the broadest sets of alternatives, while medical doctors and nurses are among those with the narrowest. What matters is not simply skill level, but how portable workers' skills are across employers.

We then test the predictions of the Job Diversion Matrix using plant closures. A closure

[2]This is the same construction as the inverse Herfindahl index, which expresses market concentration as an equivalent number of equally sized firms.

forces workers to leave their jobs, so their moves reveal where workers go when their job disappears. We estimate the model using workers' choices across the economy before each closure, aggregate its predicted diversion ratios to commuting zones and occupations, and compare them with where displaced workers move. At both levels, predicted diversion ratios closely track realized moves. We compare these predictions with two benchmarks. The first is a logit model with only job fixed effects and no worker heterogeneity, so all workers value jobs in the same way. The second is a model-free benchmark based on where workers with similar locations, occupations, and education already work. Both are substantially less successful at identifying workers' most important geographic and occupational alternatives.

We then return to mergers, where the pairwise structure of the matrix becomes essential. When two independently owned employers offer close job alternatives to the same workers, each provides workers with an employment option independent of the other. Combining them can leave workers who stay with a weaker outside option, while workers laid off through establishment consolidation can no longer turn to what would otherwise have been one of their most important alternatives. The diversion ratios between the merging employers' jobs measure how important each employer's jobs are as alternatives for the other's workers and provide the object needed to evaluate both channels. Among the 1,242 Swedish mergers we study, only 10 percent combine employers whose jobs are close alternatives. Five years later, relative to matched controls, workers whose jobs have a close alternative at the merger partner earn 7 percent less and are 5 percentage points more likely to be unemployed, and establishments with a job that diverts strongly to the partner are 23 percentage points more likely to have closed. Workers whose jobs divert little to the merger partner experience no changes in earnings or unemployment. Measures based on occupations within commuting zones do not recover this split. Because diversion is estimated before the merger, the Job Diversion Matrix can identify the small subset of mergers that warrant closer scrutiny while they are still under review.

More broadly, we show that workers' alternatives strongly determine their losses after labor market shocks, and we provide a tool to measure these alternatives using matched employer-employee data. This tool is directly relevant to policy. Industrial policy can target the workers who would suffer most from closures, and antitrust authorities can scrutinize mergers that generate substantial earnings losses. Beyond these cases, the Job Diversion Matrix lets policymakers identify the jobs most vulnerable to trade shocks, the green transition, and technological shifts such as generative artificial intelligence.

Related Literature We contribute to the literature that quantifies imperfect competition in the labor market, often measured by the finite own-wage labor supply elasticities

that firms face (Manning, 2003; Card, 2022; Caldwell et al., 2026a). Estimation procedures that explicitly model labor supply often assume a (nested) multinomial logit model (Card et al., 2018; Berger et al., 2022; Lamadon et al., 2022; Felix, 2022). Such specifications generate diversion ratios, which are a function of own- and cross-wage elasticities, but the resulting substitution patterns depend on a parsimonious set of parameters and are proportional to employment shares. This paper allows job diversion ratios to depend on a variety of well-documented features of the labor market. Jobs differ in pay and amenities (Card et al., 2018; Sorkin, 2018). Commuting, family ties, and switching costs limit which jobs are within reach (Manning, 2003; Le Barbanchon et al., 2021). Skills limit which jobs a worker can do (Gathmann and Schönberg, 2010; Fan, 2025; Macaluso, 2025). All of them feed into diversion ratios. The closest existing measure is the Outside Options Index developed by Caldwell and Danieli (2024). It is a worker-level index built from where similar workers work. Our object compares jobs rather than workers. The measures are complementary because the Outside Options Index tells us which workers have good outside options, whereas the Job Diversion Matrix identifies the alternatives to each job.

An alternative method to measure worker alternatives is to define a relevant labor market. This is akin to a restricted diversion ratio where a worker only moves to jobs within their labor market. Much of the labor market concentration literature defines markets as occupations within commuting zones (Azar et al., 2022a; Azar and Marinescu, 2024). The resulting estimates can be sensitive to these boundaries, and misclassifying jobs across markets can bias estimates of labor market power (Fogel et al., 2024). Recent work therefore constructs data-driven market definitions that group together labor market cells linked by observed worker flows (Nimczik, 2023; Fogel and Modenesi, 2023; Schubert et al., 2024). These approaches improve on predefined markets, but they still treat every job within a market as an equally close alternative.[3] Product market analysis addresses the analogous problem using diversion ratios, which measure direct substitution between particular products without first requiring a market boundary (Farrell and Shapiro, 2010; Conlon and Mortimer, 2021). The Job Diversion Matrix brings this logic to the labor market.[4] It directly estimates a continuous and directional measure of substitution between jobs rather than assigning each pair to the same or different markets. Unlike a Herfindahl-Hirschman index (HHI) based on employment shares within a predefined market, it is built from pre-

[3]Moreover, competition can vary within a correctly defined market because vacancies controlled by a worker's current employer are not equivalent to opportunities at an independent employer (Jarosch et al., 2024).

[4]Unlike consumers, who can in principle buy any product, workers cannot take every job. We therefore restrict each worker's choice set to occupations for which they are formally qualified and let the value of each job depend on its occupational and educational fit.

dicted substitution between jobs. Because the matrix can subsequently be aggregated over any proposed boundary, it allows market definitions to be evaluated rather than imposed.

Our research also contributes to the job displacement literature (Jacobson et al., 1993; Schmieder et al., 2023; Athey et al., 2023). Recent work points to the alternatives available after displacement as an important reason why earnings losses differ across displaced workers. Moretti and Yi (2024) show that workers displaced in thicker labor markets experience shorter spells out of work and smaller earnings losses. Macaluso (2025) shows that workers displaced from jobs whose skills are remote from those demanded by other local jobs suffer more persistent losses. Dodini et al. (2024) find worse outcomes after mass layoffs in markets where demand for workers' tasks is concentrated among fewer employers. These papers identify market thickness, skill portability, and employer concentration as important components of workers' alternatives. We integrate these dimensions into a single job-specific measure derived from the full distribution of counterfactual destinations.

We also contribute to understanding which mergers harm workers. Using Swedish data, Beuschlein et al. (2025) document persistent earnings losses following acquisitions, reflecting both displacement and wage cuts among workers who stay. They find no evidence that increased labor market power explains the wage cuts among stayers and instead present evidence consistent with rent redistribution. Other evidence highlights the role of workers' job alternatives. Prager and Schmitt (2021) find that hospital mergers slow wage growth for nurses and pharmacists, but not for janitors and cafeteria workers who can more readily work outside hospitals. Arnold (2025) finds earnings declines when mergers increase concentration in already concentrated local labor markets, but little effect otherwise. These findings suggest that specialization and employer concentration matter because they limit the jobs workers can turn to. We measure directly how important each merging employer's jobs are as alternatives for the other's workers and show that worker harm is concentrated among workers whose jobs have a close alternative at the merger partner. These ratios therefore provide an ex ante screen for mergers that warrant closer scrutiny.

Finally, our methodological contribution is to recover job diversion ratios at economy-wide scale from matched employer-employee data. We make estimation feasible by combining two tools. Sampling of alternatives reduces each worker's choice set to a random subset of jobs (McFadden, 1978; Train et al., 1987; Frejinger et al., 2009), and the equivalence between the multinomial logit and a Poisson model lets us absorb hundreds of thousands of job fixed effects with a fast high-dimensional fixed-effects estimator (Baker, 1994; Guimarães et al., 2003; Correia et al., 2020). We build on discrete choice methods (McFadden, 1973; Train, 2009), which empirical industrial organization also uses to estimate substitution patterns among differentiated products (Berry et al., 1995, 2004).

We estimate the model directly from individual job choices rather than aggregate market shares, using rich demographic information to capture how substitution patterns between jobs vary across workers. Related labor market applications also use discrete choice methods to estimate worker preferences, drawing on job applications (Azar et al., 2022b) or wage offers and acceptance decisions on a hiring platform (Roussille and Scuderi, 2025). Closest to our methodology is Geuna (2026), who deploys machine learning to estimate a discrete choice model of workers' transitions and uses it to characterize their outside options. In companion work, we apply these methods to realized job choices in the Swedish pharmacy labor market to study wage setting (Friederiszick et al., 2025).

The rest of the paper proceeds as follows. Section 2 defines job diversion ratios and the number of effective alternatives. Section 3 describes the estimation, and Section 4 describes the data. Section 5 presents the estimated preferences and the Job Diversion Matrix and validates it with plant closures. Section 6 applies the framework to mergers. Section 7 concludes.

2 Job Diversion Ratios

Our central object is the job diversion ratio, which measures how important one job is as an alternative for another job's workers. Consider two jobs, j and k. The diversion ratio from j to k is the fraction of workers currently employed at j who would move to k if j ceased to be available.

That is:

$$D_{jk} = \frac{\#\text{ workers moving from } j \text{ to } k \text{ if } j \text{ is removed}}{\#\text{ workers employed at } j}. \tag{1}$$

Diversion is inherently directional. Job k may be an important alternative for workers displaced from job j, even if job j is not an important alternative for workers displaced from k. Consider cooks at a small restaurant and at the large hotel in the same town. If the restaurant closed, its cooks would move overwhelmingly to the hotel, the largest kitchen in the area; if the hotel closed, its cooks would spread across many restaurants, so the diversion from the hotel to any single restaurant would be small. The full Job Diversion Matrix collects these directional substitution patterns across all jobs. We reserve its diagonal for diversion to nonemployment, the outside option (D_{j0}).

Diversion ratios are widely used in competition policy to measure substitution between products (Farrell and Shapiro, 2010; Conlon and Mortimer, 2021). Their key advantage in our setting is that they measure substitution between jobs directly, without first assigning jobs to a relevant market. Equation (1) is therefore well defined whether or not jobs j and k share an occupation, industry, or geographic labor market.

We can rewrite Equation (1) in terms of labor supply as a function of the set of available jobs $\mathcal{J}$:

$$D_{jk} = \frac{1}{L_j(\mathcal{J})} \left(L_k(\mathcal{J} \setminus \{j\}) - L_k(\mathcal{J}) \right). \tag{2}$$

Here $L_j(\mathcal{J})$ and $L_k(\mathcal{J})$ denote the number of workers employed at j and k, respectively, when all jobs are available, and $L_k(\mathcal{J} \setminus \{j\})$ the number employed at k when job j is removed. The difference $L_k(\mathcal{J} \setminus \{j\}) - L_k(\mathcal{J})$ is therefore the number of workers who move to k as a result of removing job j. While $L_j(\mathcal{J})$ and $L_k(\mathcal{J})$ are observed, $L_k(\mathcal{J} \setminus \{j\})$ is a counterfactual object which motivates the need for a model.

The Number of Effective Alternatives Our measure links a job with every other job in the economy, so that every job is an alternative to every other, even if only at a microscopic margin. We therefore propose a new measure that estimates the number of effective alternatives. For each job, we take the inverse of the sum of squared diversion ratios:

$$A_j = \frac{1}{\sum_k D_{jk}^2}. \tag{3}$$

We can interpret this value as the number of alternatives each job would have if all its diversion ratios were equally sized. This measure has a footing in both industrial organization and political science.[5]

2.1 Diversion Ratios under a Discrete Choice Labor Supply

We consider an environment of workers ($i \in N_t$), jobs ($j \in \mathcal{J}_t$), and periods ($t \in T$), where N_t and $\mathcal{J}_t$ denote the workers present and the jobs available in period t.

In each period t, a worker can choose either a job from their feasible set $\mathcal{J}_{it} \subseteq \mathcal{J}_t$ or the outside option, nonemployment, denoted by $j = 0$. The worker chooses the alternative with the highest utility U_{ijt}:

$$y_{ijt} = \begin{cases} 1 & \text{if } U_{ijt} > U_{ikt} \text{ for all } k \in (\mathcal{J}_{it} \cup \{0\}) \setminus \{j\}, \\ 0 & \text{otherwise.} \end{cases} \tag{4}$$

Individual choice probabilities are then given by:

$$s_{ijt} = \mathbb{E}\big[y_{ijt} \mid \mathcal{J}_{it}, X_{it} \big]. \tag{5}$$

[5]This construction is the inverse Simpson index of the diversion ratios (Simpson, 1949). The same functional form appears as the inverse Herfindahl index in industrial organization, where it counts the number of equally sized firms consistent with a given level of concentration, and as the effective number of parties in political science (Laakso and Taagepera, 1979).

We now let utility take the following parametric form:

$$U_{ijt} = \delta_{jt} + \mu_{jt}(X_{it}) + \varepsilon_{ijt}. \tag{6}$$

Utility thus decomposes into three components. The job-specific utility δ_{jt} captures average preferences for wages as well as observed and unobserved non-wage amenities. The heterogeneous utility $\mu_{jt}(X_{it})$ captures, for example, commuting costs, switching costs, and skill fit, each interacted flexibly with the worker characteristics X_{it}.[6] Finally, ε_{ijt} denotes the idiosyncratic component. Assuming ε_{ijt} is distributed as a Type 1 Extreme Value, the choice probability in Equation (5) takes the following form:

$$s_{ijt} = \frac{\exp(\delta_{jt} + \mu_{jt}(X_{it}))}{1 + \sum_{k \in \mathcal{J}_{it}} \exp(\delta_{kt} + \mu_{kt}(X_{it}))}, \tag{7}$$

where we have normalized the systematic utility of the outside option to zero, $\delta_{0t} = \mu_{0t}(X_{it}) = 0$ for all workers and periods, so that $U_{i0t} = \varepsilon_{i0t}$ and the outside option contributes $\exp(0) = 1$ to the denominator.

Employment at job j in period t, as a function of the set of available jobs $\mathcal{J}_t$, is given by the sum of the choice probabilities in Equation (7):

$$L_j(\mathcal{J}_t) = \sum_{i \in N_t \,:\, j \in \mathcal{J}_{it}} \frac{\exp(\delta_{jt} + \mu_{jt}(X_{it}))}{1 + \sum_{k \in \mathcal{J}_{it}} \exp(\delta_{kt} + \mu_{kt}(X_{it}))}. \tag{8}$$

Equation (2) then admits the following form:[7]

$$D_{jk} = \frac{1}{L_j(\mathcal{J}_t)} \sum_{i \in N_t} \frac{s_{ijt}\, s_{ikt}}{1 - s_{ijt}}. \tag{9}$$

Diversion ratios are hence functions of individual and aggregate choice probabilities and can be readily calculated once the labor supply system is estimated. Because the choice probabilities are period-specific, so are the diversion ratios. We suppress the period index on D_{jk} for readability.

[6] We do not add random coefficients. The interactions with observed worker characteristics already let substitution patterns differ across workers, while a mixing distribution would require simulating each worker's choice probabilities over hundreds of thousands of jobs. It would also give up the two features that make estimation feasible at this scale: the equivalence with a Poisson model that absorbs the job fixed effects, and the simple correction for sampled choice sets, which holds only for the logit (Section 3).

[7] To see this, observe that $1+\sum_{k \in \mathcal{J}_{it} \setminus \{j\}} \exp(\delta_{kt}+\mu_{kt}(X_{it})) = (1-s_{ijt})\big(1+\sum_{k \in \mathcal{J}_{it}} \exp(\delta_{kt}+\mu_{kt}(X_{it}))\big)$.

3 Estimation Procedure

We estimate the model by maximum likelihood.[8] Recall that y_{ijt} denotes worker i's choice, equal to one if i chooses job j in period t and zero otherwise. Given the logit choice probabilities s_{ijt} in Equation (7), the period-t log-likelihood is

$$\ell(\delta_t, \mu_t) \;=\; \sum_{i \in N_t} \sum_{j \in \mathcal{J}_{it} \cup \{0\}} y_{ijt} \, \ln s_{ijt}, \tag{10}$$

where $\delta_t \equiv (\delta_{jt})_{j \in \mathcal{J}_t}$ and $\mu_t \equiv (\mu_{jt})_{j \in \mathcal{J}_t}$ collect the job-specific and heterogeneous utility components of period t.

Maximizing Equation (10) directly is computationally challenging. First, with $|N_t|$ workers and $|\mathcal{J}_{it}|$ feasible jobs per worker, the choice dataset has $\sum_{i \in N_t} |\mathcal{J}_{it}|$ rows.[9] Second, we need to estimate the high-dimensional vector of job fixed effects δ_t. We address the two problems as follows. We use sampling of alternatives to reduce the size of the choice set and exploit the multinomial-Poisson equivalence for a fast estimation of the job fixed effects.

Sampling of Alternatives Following Train et al. (1987) and Frejinger et al. (2009), we estimate on a subset of alternatives: for each worker i we draw m jobs uniformly at random from the feasible choice set $\mathcal{J}_{it}$ and add the chosen alternative, the previous-period job, and the outside option.[10] Each alternative j in the sampled set K_{it} receives a weight inversely proportional to the probability that it is drawn,

$$q_{K_{it}|j} = \begin{cases} \dfrac{|\mathcal{J}_{it}|}{m} & \text{if } j \text{ is drawn uniformly at random,} \\ 1 & \text{if } j \in \{\text{chosen job, previous-period job, outside option}\}. \end{cases} \tag{11}$$

Randomly drawn jobs receive weight $|\mathcal{J}_{it}|/m$ and the always-included alternatives receive weight one. Adding $\ln q_{K_{it}|j}$ as an offset gives the sampling-corrected choice probability

$$s^K_{ijt} \;=\; \frac{\exp\big(\delta_{jt} + \mu_{jt}(X_{it}) + \ln q_{K_{it}|j}\big)}{\sum_{k \in K_{it}} \exp\big(\delta_{kt} + \mu_{kt}(X_{it}) + \ln q_{K_{it}|k}\big)}, \tag{12}$$

[8] This requires worker-level data, which is available wherever matched employer-employee registers exist. In the absence of such microdata, Conlon and Gortmaker (2020) provides an alternative estimation using aggregate data.

[9] In the Swedish setting, the education screen of Section 4.2 leaves the median worker with 235 of the 323 four-digit occupations in their feasible choice set. With 2.5 million workers choosing among 590,000 jobs per year, the full choice dataset would therefore still contain on the order of 10^{12} worker-job observations.

[10] We add the previous-period job when the specification includes state dependence.

which keeps the estimates consistent with estimation on the full choice set. We obtain the following adjusted likelihood:

$$\ell^K(\delta_t, \mu_t) \;=\; \sum_{i \in N_t} \sum_{j \in K_{it}} y_{ijt} \, \ln s^K_{ijt}. \tag{13}$$

Poisson Pseudo-Maximum Likelihood Maximizing the conditional logit likelihood in Equation (10) is equivalent to estimating a log-linear Poisson model (Baker, 1994), which lets us absorb the job fixed effects with a fast high-dimensional fixed-effects estimator (Correia et al., 2020). We treat the choice indicators $y_{ijt} \in \{0, 1\}$ as independent Poisson counts with mean:

$$\mathbb{E}\big[y_{ijt}\big] \;=\; \exp\big(\upsilon_{it} + \delta_{jt} + \mu_{jt}(X_{it})\big), \qquad j \in \mathcal{J}_{it} \cup \{0\}. \tag{14}$$

This introduces a new parameter υ_{it}, which absorbs the individual-specific normalization term and acts as a nuisance fixed effect, so that maximizing the Poisson likelihood recovers the same (δ_t, μ_t) as Equation (10).

Combining this observation with Equation (13) gives the following likelihood:

$$\begin{aligned} \ell^K_{\text{Poisson}}(\upsilon_t, \delta_t, \mu_t) \;=\; \sum_{i \in N_t} \sum_{j \in K_{it}} \Big[& y_{ijt} \left(\upsilon_{it} + \delta_{jt} + \mu_{jt}(X_{it}) + \ln q_{K_{it}|j} \right) \\ & - \; \exp\big(\upsilon_{it} + \delta_{jt} + \mu_{jt}(X_{it}) + \ln q_{K_{it}|j}\big) \Big]. \end{aligned} \tag{15}$$

Our estimator is then given by:

$$(\hat{\upsilon}_t, \hat{\delta}_t, \hat{\mu}_t) \;=\; \arg\max_{\upsilon_t, \delta_t, \mu_t} \ell^K_{\text{Poisson}}(\upsilon_t, \delta_t, \mu_t). \tag{16}$$

4 Data

We estimate the model on administrative registers from Statistics Sweden (SCB) that cover the universe of Swedish workers and employers. Section 4.1 describes the matched employer-employee data and the sample, Section 4.2 the two data sources we use to measure which jobs a worker is qualified for and how transferable skills are across occupations, and Section 4.3 reports summary statistics.

4.1 Matched Employer-Employee Data

The core data are two matched employer-employee registers from Statistics Sweden. JOBB (Employment Spells Register) records every employment relationship in Sweden with its

earnings and employer, and RAMS (Register-Based Labour Market Statistics) identifies each worker's establishment in November of each year. We link these registers to LISA (Longitudinal Integration Database for Health Insurance and Labour Market Studies), which records each person's age, family composition, residence, employment status in November, education level and subject, and four-digit occupation.[11] We also observe the coordinates of each person's home and workplace and the identity of their mother, from which we compute commuting distance and distance to the mother.[12] Wage structure statistics (Lönestrukturstatistiken) provide full-time adjusted monthly wages for about half of all employees, and firm financial data (FEK) provide value added and other financial variables at the establishment level. Together, the establishment identifier and the occupation code define the alternatives workers choose among: a job is a four-digit occupation at an establishment.

The estimation uses eight annual cross-sections, 2006 to 2013, each paired with the previous year. Each cross-section contains individuals aged 24 to 54 who are either employees or registered as not gainfully employed in November. The latter form the outside option. Appendix A.1 details the sample construction. Each cross-section covers about 2.5 million individuals, of whom 2.1 million hold about 590,000 jobs, or 3.6 workers per job on average (Table 1).

4.2 Education and Occupation Match

The model uses two further data sources. An education-occupation matrix from Statistics Sweden classifies which occupations a worker's education qualifies for. It restricts the choice set and supplies the over-qualification and subject-match terms in utility. Occupation similarity scores from the Swedish Public Employment Service measure the skill overlap between any two occupations. They enter utility as the skill match between a worker's previous occupation and the occupation of each alternative job.

Education-Occupation Match Statistics Sweden maintains a matrix that classifies the fit between education programs and occupations. It combines assessments by education experts with observed employment patterns. Mapped to our data, it covers 93 education

[11] Occupation codes in LISA come from Statistics Sweden's Occupational Register. The register combines employer reports from two sources: the wage structure statistics, which cover all public employers, all private firms with at least 500 employees, and a sample of smaller firms, and a supplementary survey that reaches each remaining firm about every four to five years. If no employer reports a code for the reference year, Statistics Sweden carries over the most recent code from the same employer. About 70 percent of employees' codes refer to the reference year, and four-digit coverage is 91 percent in 2005 and 95 percent from 2008 on (Statistics Sweden, 2011, 2019).

[12] If the mother's location is missing, we set it to the worker's own residence, so that the distance to the mother equals the commuting distance.

groups and 323 four-digit occupations.[13]

The matrix rates each education-occupation pair on two dimensions (Appendix Figure A.1).[14] Level match records whether the education is below the occupation's requirement (under-qualified), at it (correctly qualified), or above it (over-qualified). Subject match records whether the field of study prepares for the occupation completely, partially, or poorly.

We use the level dimension to restrict each worker's choice set. Occupations for which the worker is under-qualified are infeasible.[15] For licensed occupations such as physician, nurse, and teacher, feasibility also requires a complete subject match. Workers therefore choose among jobs for which they are at least formally qualified.[16] The screen removes many occupations but leaves most. The median worker retains 235 of the 323 four-digit occupations, and the 90th percentile retains 288. Panel B of Appendix Table A.1 reports, for the largest education groups, the number of feasible occupations and how many of them the matrix rates as a complete subject match. Consider dentists and dental nurses. For the dentist occupation, only a university degree in dentistry gives a complete subject match. Vocational dental care training has the right subject but leaves the worker under-qualified.[17] For the dental nurse occupation, both educations give a complete subject match, but the dentistry degree is over-qualified and the dental care training correctly qualified. The matrix thus captures that dentists can move into dental nursing while dental nurses cannot move into dentistry.

Occupation-Occupation Similarity The Swedish Public Employment Service (Arbetsförmedlingen) computes occupation similarity scores from job advertisements and uses them to suggest related occupations to job seekers (Arbetsförmedlingen, 2026). The scores draw on more than 6 million Swedish job postings from 2016 to 2024, from which skill requirements are extracted and coded in the Service's labor market taxonomy. Each four-digit occupation receives a profile of its most characteristic skills, weighted by relevance,

[13]The education groups follow Statistics Sweden's SUN2000 classification, documented at `https://www.scb.se/dokumentation/klassifikationer-och-standarder/svensk-utbildningsnomenklatur-sun/`.

[14]Appendix Tables A.2 and A.3 define the broad education and occupation groups used in the figure.

[15]Agarwal and Somaini (2025) develop the econometric framework for demand estimation when choice constraints are latent. We instead take the constraints from the education-occupation matrix and treat them as known. The matrix is an approximation. About 12 percent of employed observations hold a job for which it classifies the worker as under-qualified, and we exclude them (Section 4.3).

[16]The matrix does not rate 25 education groups, mostly compulsory schooling, general upper-secondary programs, and university credits without a degree. For these groups we treat as feasible every occupation for which all rated educations are at least correctly qualified, and we assign a poor subject match on every job.

[17]Dentistry is a licensed occupation, so practicing requires a university degree in dentistry.

and the similarity between two occupations is the cosine similarity of their profiles.[18] Because the scores come from the text of advertisements rather than from observed worker flows, they measure task similarity without importing the mobility patterns we later seek to explain. They range from 0 to 1, with higher values indicating greater skill overlap. In the dentist and dental nurse example, the two occupations are similar in tasks, but the education screen rules out moves into dentistry for workers without the degree.

Appendix Figure A.2 visualizes the similarity structure as a network where nodes represent occupations and edges connect each occupation to its ten most similar counterparts for readability. The layout positions similar occupations closer together, revealing skill-based clusters. Healthcare occupations form a tight cluster, as do IT and technology occupations.

4.3 Summary Statistics

Table 1 presents summary statistics, pooled across the eight estimation years 2006 to 2013. In a given year, about 2.1 million employed workers hold 590,000 jobs, so the average job has 3.6 workers. Workers, however, are concentrated in larger jobs. Half of all employed workers hold a job with at least 9 workers, and half work at an establishment with at least 34. Employment is spread across many occupations, and the ten largest account for 29 percent of it (Appendix Table A.1). Nonemployment, the outside option, accounts for 15.8 percent of individual-years.[19]

Among employed observations, we require current and lagged jobs to be feasible given the worker's education, which removes roughly 300,000 employed observations per cross-section, about 12 percent. Hence, no employed worker in the sample is under-qualified, and 96.6 percent are correctly qualified for their job. About half of workers have a complete subject match between their education and occupation, while the other half work in fields unrelated to their training.[20]

The median commute is 6.8 kilometers and the median distance to the mother is 13.5 kilometers, consistent with non-negligible family-location ties. Most workers keep their job, and those who switch tend to keep their occupation or move to a similar one. Among

[18] The scores use the SSYK 2012 classification, which we map to SSYK96. Occupations with fewer than 200 postings receive no score. The Service also publishes a filtered version in which licensing, management, and education requirements remove some pairs. We use the unfiltered, symmetric scores and introduce directionality through the education-occupation screen instead.

[19] Among all residents aged 24 to 54, before any sample restriction, 13.0 percent are not gainfully employed. Among employees and the nonemployed, the two groups in our sample, the share is 14.0 percent. The sample share is higher because most sample restrictions, such as the exclusion of managers and of infeasible jobs, remove employed workers.

[20] The second share is partly mechanical. We assign a poor subject match on every job to workers whose education the classification does not rate, mostly compulsory schooling and general upper-secondary programs.

workers employed in two consecutive years, 76 percent hold the same job in the second year. About half of those who switch keep their occupation and move to another establishment. Among those who change occupation, the median new occupation is the 23rd most similar of the 322 other occupations to the previous one, and a quarter of moves go to one of the five most similar. Because the similarity scores come from job advertisements rather than from worker flows, this pattern also indicates that they capture which occupations are close.

Observed moves nonetheless link very few pairs of jobs. About 490,000 workers switch jobs each year, fewer than one for every job, while each job has more than half a million other jobs as potential destinations. This sparsity is why we estimate diversion from a model of job choice rather than from observed moves.

Table 1: Summary Statistics

Panel A: Worker Characteristics		*Panel B: Distance and Education Match*	
Share female	52.0	Median distance to job (km)	6.8
Share foreign-born	17.9	Median distance to mother (km)	13.5
Share with a child under 3	17.2	Share under-qualified (by construction)	0.0
Average number of children (0–17)	0.84	Share correctly qualified	96.6
Share in a big city	19.8	Share over-qualified	3.4
Share aged 24–29	19.4	Share with complete subject match	45.7
Share aged 30–39	30.6	Share with partial subject match	4.5
Share aged 40–49	32.2	Share with poor subject match	49.9
Share aged 50–54	17.8		
Share with pre-secondary education (<9 years)	2.4		
Share with pre-secondary education (9 years)	10.4		
Share with upper-secondary education	49.3		
Share with post-secondary education (<2 years)	6.2		
Share with post-secondary education (≥2 years)	30.5		
Share with postgraduate (doctoral) education	1.2		
Panel C: Sample Overview (pooled, 2006–2013)		*Panel D: Year-over-Year Mobility*	
Estimation years	8	Share changing job	23.9
Total individual-years	20,008,171	Same occupation, new establishment	12.8
Employed individual-years	16,842,060	New occupation	11.1
Nonemployed individual-years	3,166,111	*Occupation changers*	
Job-years	4,740,268	Median similarity rank	23
Average job size	3.6	Share to one of 5 most similar	26.3
Median job size	9		
Median establishment size	34		

Notes: Sample of Swedish individuals aged 24–54, pooled over the eight estimation years 2006–2013. Each individual is observed in an estimation year and the previous one. Panel A reports shares over all individual-years. Panel B reports distances and the education-occupation match among employed individual-years. Panel C reports pooled counts, with a job counted once per year. Average job size is employed individual-years per job-year. The median job and establishment sizes are those of the median employed individual, counting sample workers. Panel D reports year-over-year mobility among individuals employed in both years. A job is an establishment-occupation combination, so workers who change job but keep their occupation have changed establishment. The similarity rank of the new occupation orders the 322 other four-digit occupations by their cosine similarity to the previous occupation (Section 4.2), with 1 the most similar.

5 Estimation and Results

We now turn to estimating our model using our cross-sectional data on job choice. We first estimate the preference parameters (δ_t, μ_t), parameterized below, and then compute the diversion ratios, which we validate using plant closures.

5.1 Specification

Before solving Equation (13), we first need to parameterize the heterogeneous utility component $\mu_{jt}(X_{it})$. As explained in Section 2, a flexible heterogeneous utility dictates how flexible our substitution patterns are across jobs, which in turn is crucial for calculating diversion ratios. We therefore make sure to include the key worker-specific margins of job choice, i.e., commute, distance to mother, skill match, frictions, and demographics, in our model.

In particular, we parameterize it as follows:

$$\mu_{jt}(X_{it}) = g_d(d_{ijt}^{\text{commute}}, X_{it}) + g_f(d_{ijt}^{\text{family}}, X_{it}) + g_s(X_{it})\mathbf{1}\{j \neq j_{i,t-1}\} + g_e(E_{ijt}, X_{it}) + g_o(O_{ijt}, X_{it}), \tag{17}$$

where X_{it} collects worker demographics, d_{ijt}^{commute} and d_{ijt}^{family} are the commuting distance and the distance to the worker's family, $\mathbf{1}\{j \neq j_{i,t-1}\}$ equals one for every job other than the worker's previous-period job, so that workers who were not employed in the previous period incur the switching cost on every job, E_{ijt} holds indicators for over-qualification and partial or poor subject match, and O_{ijt} measures the similarity between the worker's previous occupation and the occupation of job j. Each of these characteristics is a function of job j's attributes and the worker's period-t state (residence, family location, education, and previous-period job), which X_{it} subsumes. The distance terms capture worker preferences, the switching term the cost of changing jobs, and the education and occupation terms the skill match between worker and job; each enters through a flexible function $g(\cdot)$ of the characteristic and worker demographics.

In the main specification, g_d and g_f are splines over 0–5, 5–20, 20–50, and 50+ km (commuting) and 0–5, 5–50, and 50+ km (family). We let skill match, short- and medium-distance commuting costs, switching costs, and the disutility of distance to mother vary with worker demographics. The interactions are with gender, low and high education, age, foreign-born status, big-city residence (the municipalities of Stockholm, Gothenburg, and Malmö), the presence of a young child, and the number of children. The switching cost also depends on prior job tenure, with medium tenure denoting two to four years and high tenure five or more years, relative to a short-tenure base. For workers entering from

nonemployment, who have no previous occupation, occupation similarity is set to zero. Low education denotes at most compulsory schooling, medium education (the omitted category) upper-secondary schooling, and high education a university degree or higher.

We estimate this equation using `ppmlhdfe` in Stata (Correia et al., 2020), absorbing worker and job fixed effects, and recover the job fixed effects after estimation to construct predicted choice probabilities and diversion ratios.[21]

The remaining coefficients identify preferences over distance, match quality, and state dependence, all from within-worker variation across the sampled choice set, holding the job fixed effects constant.[22] The reported standard errors are the conventional maximum-likelihood standard errors of the Poisson estimation, with the worker entering through the absorbed fixed effect rather than as a clustering dimension.

5.2 Worker Preferences

Table 2 reports the preference estimates for the 2010 cross-section.[23] Panel A reports the base coefficients, which apply to a reference worker: a 40-year-old man with upper-secondary education, born in Sweden, living outside the three largest cities, without children, and with short tenure in his previous job. Panels B to F report how skill match, the commuting slopes over 0–5 and 20–50 km, the switching cost, and the distance to mother vary with worker characteristics. For any other worker, the effect of a characteristic is the base coefficient plus the relevant interaction terms. The estimated forces have the expected signs. Workers prefer jobs close to home, are reluctant to switch jobs, and value occupational continuity and a good fit between their education and the job. Appendix A.5 expresses these forces on a common utility scale.

Commuting The distance coefficients imply substantial but nonlinear commuting frictions. For the reference worker, a job 15 km from home is chosen half as often as an otherwise identical job 5 km from home, and a job 30 km away about a quarter as often

[21] Because the diversion ratios and the number of effective alternatives are themselves estimated objects that we later use to classify workers and establishments, their sampling error in principle carries into the closure and merger event studies. We treat the measure as known in that second stage: it is estimated on millions of observations, with sampling error negligible relative to the coarse top-versus-bottom-quartile split it feeds, and we leave a full propagation of first-stage uncertainty to future work.

[22] The commuting- and family-distance coefficients are identified from how a worker's choice probabilities vary with each alternative's location relative to the worker's home and family, the switching cost from the elevated probability of choosing the previous-period job, and the skill-match and education-fit coefficients from how choices vary with an alternative's occupation relative to the worker's previous occupation.

[23] The estimates are nearly identical across the eight separate annual estimations from 2006 to 2013 (Appendix A.4).

Table 2: Estimation Results

Variable	Coeff.	S.E.
Panel A: Base Coefficients		
Distance (0–5 km)	0.417^{***}	(0.010)
Distance (5–20 km)	-0.069^{***}	(0.00064)
Distance (20–50 km)	0.0026^{***}	(0.00080)
Distance (50+ km)	-0.0052^{***}	(0.000036)
Dist. to mother (0–5 km)	0.045^{***}	(0.0022)
Dist. to mother (5–50 km)	-0.011^{***}	(0.00025)
Dist. to mother (50+ km)	-0.00038^{***}	(0.000028)
Switching cost	-4.096^{***}	(0.043)
Skill match	1.133^{***}	(0.0054)
Educ. level too high	-0.691^{***}	(0.011)
Partial subj. match	-0.426^{***}	(0.011)
Poor subj. match	-0.829^{***}	(0.0071)
Panel B: Skill match × Demo.		
Female	-0.051^{***}	(0.0016)
Low educ.	0.035^{***}	(0.0025)
High educ.	-0.039^{***}	(0.0018)
Age	0.0038^{***}	(0.000082)
Panel F: Dist. to mother × Demo.		
Young child	0.0012	(0.0042)
Num. children	0.043^{***}	(0.0014)
Big city × young child	-0.062^{***}	(0.0051)

Variable	Coeff.	S.E.
Panel C: Dist. (0–5 km) × Demo.		
Female	-0.012^{***}	(0.0027)
Age	0.00031^{**}	(0.00015)
Low educ.	-0.123^{***}	(0.0043)
High educ.	-0.0069^{**}	(0.0031)
Big city	-0.254^{***}	(0.0020)
Young child	-0.020^{***}	(0.0035)
Num. children	0.022^{***}	(0.0012)
Foreign born	-0.218^{***}	(0.0027)
Skill match	-0.033^{***}	(0.0011)
Panel D: Dist. (20–50 km) × Demo.		
Female	-0.015^{***}	(0.00038)
Age	-0.00077^{***}	(0.000020)
Low educ.	0.0036^{***}	(0.00060)
High educ.	0.018^{***}	(0.00043)
Big city	0.031^{***}	(0.00049)
Young child	-0.0065^{***}	(0.00058)
Num. children	-0.0075^{***}	(0.00022)
Foreign born	0.0042^{***}	(0.00051)
Skill match	-0.0033^{***}	(0.000068)
Panel E: Switching cost × Demo.		
Female	-0.109^{***}	(0.011)
Age	-0.034^{***}	(0.00060)
Low educ.	-0.207^{***}	(0.018)
High educ.	0.263^{***}	(0.012)
Foreign born	-0.501^{***}	(0.012)
Distance (0–5 km)	-0.659^{***}	(0.0088)
Distance (20–50 km)	-0.134^{***}	(0.00061)
Medium tenure	0.129^{***}	(0.0089)
High tenure	0.028^{***}	(0.0100)
Constant	2.169^{***}	(0.042)

Notes: Poisson pseudo-maximum likelihood estimates with worker and job fixed effects. Panel A reports base preference coefficients. Panel B reports interactions of skill match with worker demographics. Panels C and D report interactions of the 0–5 km and 20–50 km distance slopes with worker demographics. Panel E reports interactions of the switching cost with worker demographics and with prior job tenure. Panel F reports interactions of distance to mother with worker demographics. Distance and distance to mother enter as linear splines, and all interactions with age are linear. Low education denotes at most compulsory schooling, medium education (the omitted category) upper-secondary schooling, and high education a university degree or higher. Standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

as one 20 km away.[24] Utility from the jobs a worker could move to falls at every distance, most steeply within 5 km of home and again between 20 and 50 km, and is almost flat beyond 50 km.[25] Short-distance frictions are markedly steeper for workers in big cities and for foreign-born workers, and also for women, lower-educated workers, and those with young children. Distance to mother has the same structure but matters much less.

Switching Cost The switching-cost coefficient implies substantial inertia in job choice. For the reference worker, moving to an otherwise identical job costs as much utility as a commute about 37 km longer, and about half the gain from moving to a job in the worker's own occupation (Appendix A.5). Inertia is stronger for women and for older, lower-educated, and especially foreign-born workers, is weaker for highly educated workers, and varies little with prior job tenure.

Education and Skill Fit The skill-match coefficient implies that workers strongly value occupational continuity. Because it multiplies a similarity index that ranges widely, a move within the worker's own occupation yields the largest utility gain in the model, as much as a commute about 80 km shorter for the reference worker.[26] Workers also avoid jobs that do not fit their formal training. Over-qualification and a poor subject match each cost about as much utility as a commute 6 to 7 km longer, and a partial subject match about half as much as a poor one. Skill match matters somewhat less for women and highly educated workers, and more for lower-educated and older workers (Panel B).

5.3 The Job Diversion Matrix

We use the estimated choice probabilities to construct the job diversion ratios described in Section 2. These ratios answer the question that motivates the paper: where would workers go if their jobs disappeared? Together, they form the Job Diversion Matrix. From this matrix, we compute each job's number of effective alternatives, the number of equally important alternatives that would produce the same concentration of diversion. We first describe how these alternatives vary across commuting zones and occupations and how

[24] In the logit model, the ratio of the choice probabilities of two jobs equals the exponential of their utility difference.

[25] For these jobs, the 0–5 km and 20–50 km slopes combine the base coefficient with the switching-cost interactions in Panel E. The positive base coefficient on the 0–5 km segment applies only to a worker's current job. Evaluated at the sample averages of worker characteristics, utility from any job other than the current one also falls with distance on every segment.

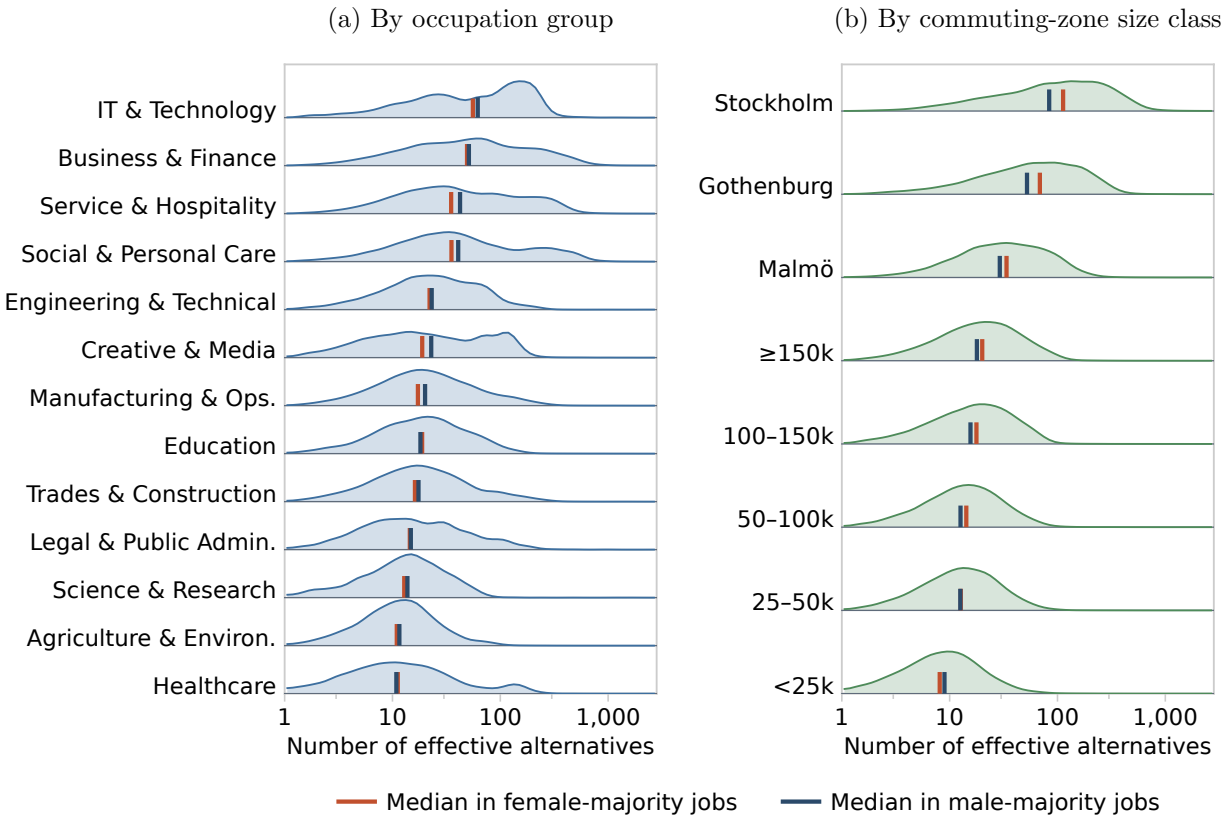
[26] The skill-match variable is a monotone transform of the occupation-similarity score of Section 4.2. We replace the raw cosine score $s \in [0, 1]$ with a recentered log-odds index, $\log(s + 0.02) - \log(1 - s + 0.02)$ normalized to zero at $s = 0$, which spreads out a score otherwise concentrated near its bounds without adding spline parameters to the choice model. On this scale a different-occupation alternative takes the value zero and a same-occupation alternative a value of about 7.9, so the utility gain from full occupational continuity is roughly 7.9 times the tabulated coefficient.

predicted moves are distributed across employers. We then aggregate diversion ratios to commuting zones and occupations to compare the model's predictions with workers' realized moves after plant closures, and test whether the number of effective alternatives predicts their earnings losses. Throughout, we treat the diversion ratios as known, since they are estimated from millions of choices and their sampling error is small.

5.3.1 Job Alternatives across Geography and Occupations

The Job Diversion Matrix reveals substantial variation in the breadth of alternatives across jobs. The median job has 27 effective alternatives, while a quarter of jobs have fewer than 12 and another quarter more than 71. Figure 1 shows the full distributions by occupation group in Panel (a) and by commuting-zone size class in Panel (b). Alternatives differ systematically across occupations and geography, but the variation within each remains wide.

Figure 1: Number of Effective Alternatives



Notes: Distribution of the number of effective alternatives, $A_j = 1/\sum_k D_{jk}^2$, across the 612,656 jobs of 2010, smoothed and on a common vertical scale. Ticks mark the medians of female-majority jobs, in which women make up at least half of the 2010 workforce, and of male-majority jobs. Occupation groups are ordered by their median. Commuting-zone size classes are Statistics Sweden's local labor market classes by population, in thousands, with the three metropolitan regions shown separately.

Jobs in larger commuting zones tend to have more effective alternatives. The median job has 97 effective alternatives in Stockholm, compared with 9 in zones with fewer than 25,000 inhabitants. Yet the distributions overlap substantially. A tenth of jobs in Stockholm have fewer than 15 effective alternatives, while a tenth of jobs in the smallest zones have more than 24. The size of a commuting zone is therefore an incomplete guide to a particular job's alternatives.

Across occupation groups, effective alternatives do not follow a simple high-skill versus low-skill divide. Information technology, business and finance, and service and hospitality have the most, with medians of 37 to 61. Healthcare, science and research, and agriculture have the fewest, with medians of 11 to 13. Both sets include occupations with high and low skill requirements. This pattern suggests that the portability of skills across employers matters more than skill level alone. Wide variation also remains within occupation groups. The middle half of jobs spans a threefold range of effective alternatives in agriculture and environment and an eightfold range in creative and media.

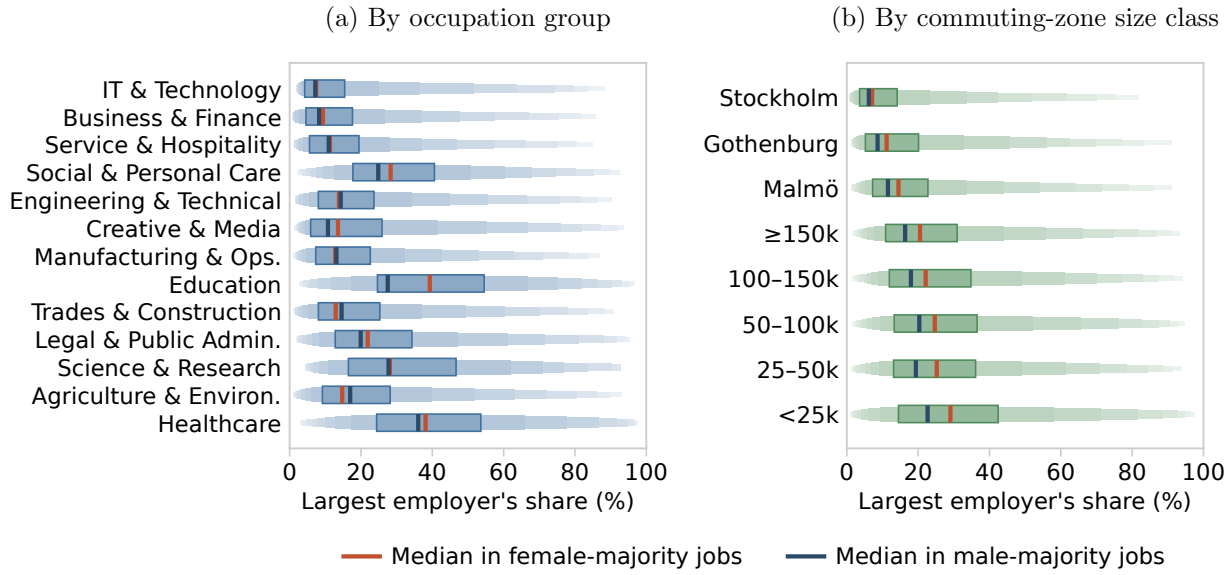
Gender differences in job alternatives depend on occupation and geography. Female-majority jobs, in which women make up at least half the workforce, have a higher median number of effective alternatives overall, 29 compared with 25 for male-majority jobs. This ordering reverses in the smallest commuting zones and in eleven of the thirteen occupation groups. In service and hospitality, for example, female-majority jobs have a median of 35 effective alternatives, compared with 42 for male-majority jobs. The national comparison therefore reflects the sorting of women and men across occupations and geography.

5.3.2 Concentration of Alternatives across Employers

A broad set of job alternatives can still be concentrated at a few employers. We therefore sum each job's diversion ratios over the destination jobs of each employer and identify the employer that receives the largest share of predicted moves. The current employer is included because its other jobs remain possible destinations when the origin job disappears. Figure 2 shows the distribution of this share by occupation group and commuting-zone size class. The darkest boxes contain the middle half of jobs, while lighter boxes extend into the tails. For the median job, the largest employer receives 14 percent of its predicted moves.

Concentration across employers varies sharply by occupation and geography. The median share going to the largest employer is 38 percent in healthcare, compared with 7 percent in information technology. For more than a quarter of healthcare jobs, the largest employer receives more than half of predicted moves. Across commuting zones, the median share is 26 percent in the smallest zones, compared with 7 percent in Stockholm, consistent with workers having fewer employers within reach.

Figure 2: Share of Diversion to the Largest Employer



Notes: Letter-value plots (Hofmann et al., 2017) of the share of a job's diversion that goes to its largest employer, the current employer included, across the jobs of a group. The darkest box contains the middle half of jobs. Successively lighter boxes extend into the tails, each halving the share of jobs left outside. Boxes stop at the middle 99.8 percent, and a box is drawn only if at least 10 jobs lie beyond each end. Ticks mark the medians of female-majority and male-majority jobs as defined in Figure 1. Rows as in Figure 1.

Concentration across employers also differs by gender composition. In every commuting-zone size class, the median share going to the largest employer is higher for female-majority jobs than for male-majority jobs, 29 compared with 23 percent in the smallest zones. Across occupation groups, the gap is widest in education, 39 compared with 28 percent. We next ask whether differences in the number of effective alternatives are reflected in workers' earnings and employment after plant closures.

5.3.3 Plant Closures as Validation

We validate the Job Diversion Matrix using plant closures, which force workers to reveal where they turn when their jobs disappear. We construct the matrix using only workers' choices observed before each closure, so their subsequent moves provide an out-of-sample test. We use this setting for two complementary exercises. First, we compare predicted diversion with realized moves across commuting zones and occupations. Second, we ask whether the number of effective alternatives to the lost job predicts the size and persistence of workers' earnings losses.

Closure Sample Following Athey et al. (2023), we identify plant closures between 2006 and 2012 and track their workers' subsequent employment and earnings.[27] Appendix Table A.5 reports the worker and plant counts at each sample restriction, and Appendix A.6 provides further details.

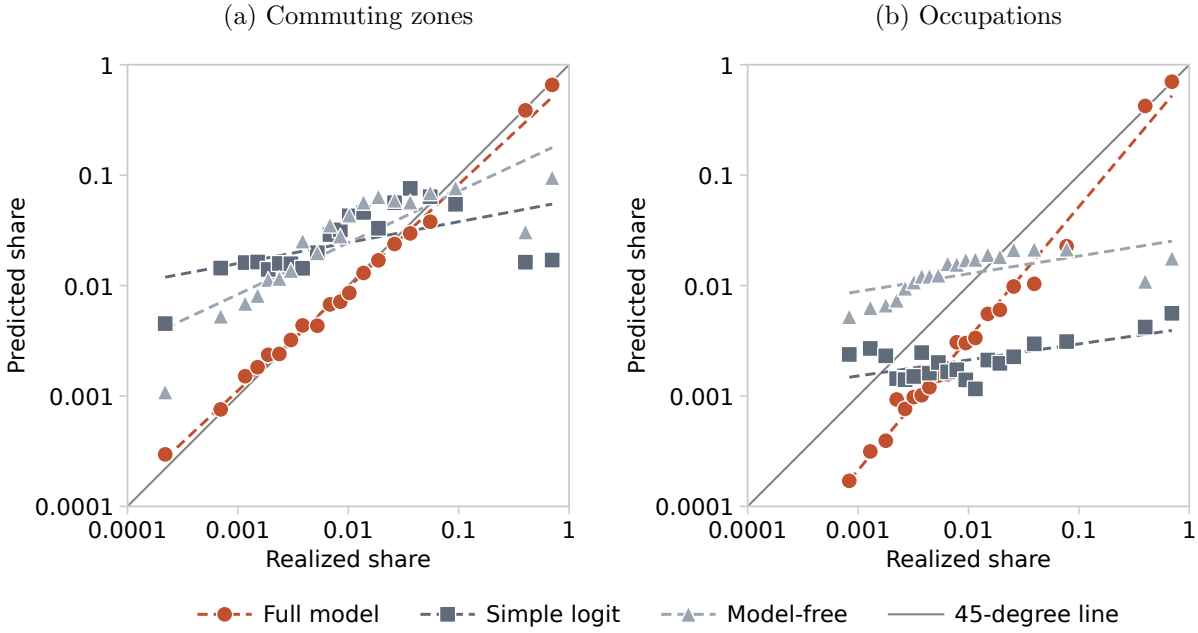
Diversion Across Groups For the first exercise, we aggregate the job diversion ratios separately to pairs of commuting zones and pairs of four-digit occupations (Appendix A.7) and compare these predicted shares with the realized shares of displaced workers moving between those origins and destinations. We focus on moves to other jobs and measure each worker's destination at their first departure from the closing plant within two years of the closure year.

We benchmark the full model's predictions against two alternatives. The first is a simple logit model with only job fixed effects, which makes diversion proportional to destination employment shares. This comparison tests how much it matters that different workers value the same jobs differently. The second is a model-free benchmark that estimates diversion from employment shares within groups of workers with similar locations, occupations, and education (Appendix A.8). It uses the same pre-closure employed cross-section as the

[27] We classify a plant as closed if, having employed at least five workers, it disappears entirely or shrinks by at least 90 percent within two years. Following Hethey-Maier and Schmieder (2013), we exclude cases consistent with relabeling or internal reorganization. We restrict the sample to displaced workers with earnings above the 10th percentile and exclude military personnel and senior government officials. We end the closure window in 2012 because occupation-code coverage drops sharply in 2014, so 2013 is the last year in which we observe workers' occupation switches.

full model, so the comparison shows what the structural approach adds to the observed employment patterns of similar workers. Appendix A.9 reports aggregate diversion shares and destination accuracy for all three models.

Figure 3: Model Fit: Predicted versus Realized Shares



Notes: Binned comparisons of predicted and realized shares of displaced workers moving between commuting zones in Panel (a) and four-digit occupations (SSYK96) in Panel (b). Each marker is a bin of origin-destination pairs, with realized shares on the horizontal axis and predicted shares on the vertical axis, both on log scales. Dashed lines are linear fits of the log binned predicted shares on the log binned realized shares, the solid line marks a perfect fit, and missing predictions are set to zero. Destinations are measured at the worker's first departure from the closing plant within two years of the closure year, pooled over closures from 2006 to 2012. Moves into nonemployment are excluded. Appendix Figure A.4 shows the underlying matrices, Appendix Figures A.7 and A.8 the simple logit benchmark, Appendix Figures A.5 and A.6 the same- and different-cell splits, and Appendix A.8 the model-free benchmark.

Figure 3 plots the predicted shares against the realized shares for commuting zones in Panel (a) and occupations in Panel (b). The 45-degree line marks a perfect fit. In both panels, the full model closely tracks the realized shares, including the destinations that absorb the most displaced workers. The simple logit produces a nearly flat relationship and substantially underpredicts moves to these destinations. The model-free benchmark captures some geographic variation but remains far below the full model, while its occupational predictions are nearly flat. The Job Diversion Matrix therefore captures the relative importance of workers' geographic and occupational alternatives, rather than merely allocating diversion in proportion to destination employment shares.

Appendix Figure A.4 shows the underlying matrices of realized and predicted shares

for commuting zones and occupations. Both the realized and the predicted matrices have strong diagonals, because displaced workers who find another job tend to remain in their original commuting zone and occupation. The full model also captures the broad patterns of moves between commuting zones and between occupations, so its fit does not rest on the diagonals alone (Appendix Figures A.5 and A.6).

Earnings Impact of Job Loss Having shown that the matrix predicts where displaced workers go, we next examine whether workers displaced from jobs with fewer effective alternatives suffer larger and more persistent earnings losses.[28] We compute the number of effective alternatives for each worker's job in the year before closure and compare subsequent outcomes for workers in the bottom and top quartiles of this measure.

For this analysis, we restrict displaced workers to those aged 24–60 with at least three years of tenure at the closing plant and define the quartiles among these workers. Following Schmieder et al. (2023), we match each displaced worker to one non-displaced worker at a surviving plant using a propensity score estimated within each closure year and one-digit industry. The propensity score includes pre-closure plant size, log earnings in the two years before closure, education, tenure, and age. Displaced workers and their matched controls are closely balanced on these characteristics and on gender (Appendix Table A.6). We follow both groups from five years before to seven years after closure, using the year before closure as the baseline.

We estimate matched difference-in-differences event studies separately for the bottom quartile (Q1) and the top quartile (Q4), assigning each control to the quartile of its matched displaced worker:

$$y_{ict} = \alpha_i + \lambda_{ct} + \sum_{\tau \neq -1} \beta_\tau^g \, \mathbf{1}[t = \tau] \, T_i + \varepsilon_{ict}, \tag{18}$$

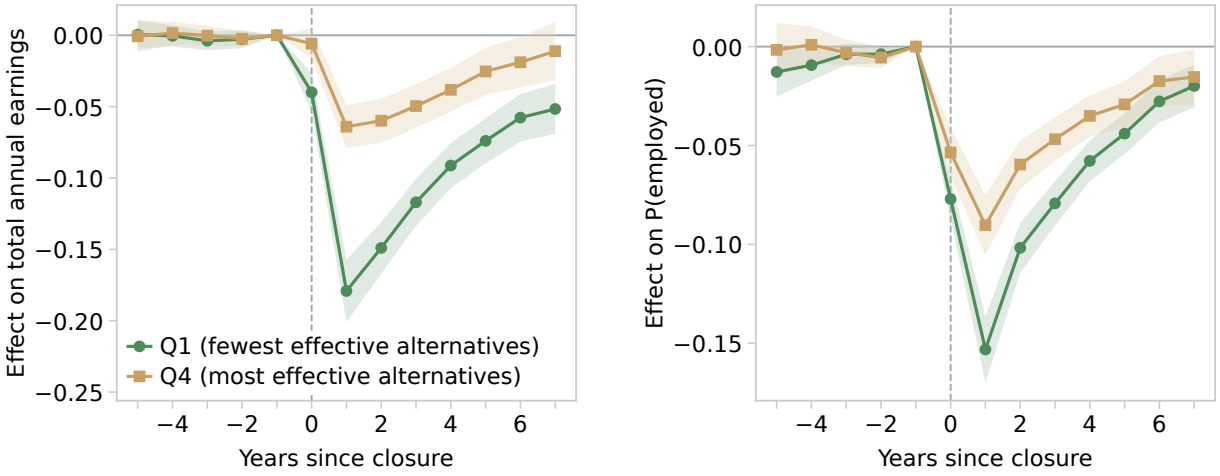
where y_{ict} is the outcome of worker i from closure cohort c at event time t, α_i is a worker fixed effect, and λ_{ct} is a closure-cohort-by-event-time fixed effect. The outcome is either annual earnings relative to earnings in the year before closure or an employment indicator. T_i equals one for displaced workers and zero for their matched controls, and $g \in \{Q1, Q4\}$ indexes the quartile group. Each coefficient β_τ^g measures the change in displaced workers' outcomes relative to their controls, compared with the year before closure. Comparing the Q1 and Q4 paths shows how displacement effects differ between workers whose lost jobs had few or many effective alternatives. Standard errors are two-way clustered by worker

[28] Plant closures displace many workers simultaneously. If hiring capacity is limited, workers with few alternatives may remain unemployed longer. Where employers have wage-setting power, the inflow of displaced applicants may also lower the wages these workers are offered. The diversion ratios do not capture these responses, because they hold all other jobs fixed.

and closing plant.

Panel (a) of Figure 4 shows that workers displaced from jobs with few effective alternatives suffer larger earnings losses and recover more slowly. Before closure, both groups' earnings track those of their matched controls closely. One year after closure, the estimated earnings loss relative to matched controls is about 18 percent of baseline earnings for Q1 workers, nearly three times the 6 percent loss for Q4 workers. Six years after closure, the loss for Q1 workers remains around 6 percent, while Q4 workers have largely recovered.

Figure 4: Earnings and Employment After Displacement, by Effective Alternatives



(a) Total annual earnings

(b) Employment

Notes: Event studies of the effect of displacement on total annual earnings relative to the worker's earnings in the year before closure (Panel (a)) and on the probability of being employed (Panel (b)). Workers are split by the number of effective alternatives of the lost job, defined in Equation (3), into the bottom quartile (Q1, fewest effective alternatives) and the top quartile (Q4, most). Each panel plots the two group paths from Equation (18), estimated separately for each group against its matched controls: the Q1 path β_τ^{Q1} and the Q4 path β_τ^{Q4}. A worker counts as employed when registered as an employee in November with monthly earnings at the main job of at least 14,000 SEK in 2010 prices. Effects are relative to the year before closure ($t = -1$), with $t = 0$ the closure year. Shaded areas indicate 95% confidence intervals. Controls are matched workers at surviving plants.

The differences are evident in both employment and the earnings of employed workers. Panel (b) of Figure 4 shows that, one year after closure, employment is 15 percentage points lower for Q1 workers than for their matched controls, compared with 9 percentage points for Q4 workers. These employment losses decline over time, and the two paths largely converge by year six. Appendix Figure A.3 shows that annual earnings are also lower among employed workers. One year after closure, employed Q1 workers earn about 12 percent less than their matched controls, compared with 5 percent for employed Q4 workers, and earnings losses persist in both groups six years after closure.

Together, these exercises show that the matrix predicts where displaced workers go and that the number of effective alternatives predicts the size and persistence of their earnings losses. Because it is computed from pre-closure data alone, this measure can identify in advance the jobs whose workers are most exposed to the costs of job loss. We next turn to mergers, where the key question is whether the jobs of one merging employer are close alternatives for the workers of the other.

6 Mergers in the Labor Market

We now apply the framework to the question that motivates the paper. When a merger combines two employers, it can harm their workers by removing a close alternative.[29] We ask whether diversion ratios identify such mergers in advance and whether standard market definitions distinguish them as clearly.

Competition Policy in Sweden Following EU competition rules, the Swedish Competition Authority reviews mergers that could significantly impede effective competition. Notification is mandatory only when the merging firms' combined turnover in Sweden exceeds SEK 1 billion (about USD 100 million) and at least two of them each exceed SEK 200 million (about USD 20 million). Neither the EU nor the Swedish merger guidelines explicitly take labor markets into account. However, the European Commission's draft revised merger guidelines recognize for the first time that a merger may harm workers through lower wages or worse working conditions. The Commission argues that such "anticompetitive effects are more likely to occur where workers have access to few alternative employment options that are comparable to those offered by the merging firms" (European Commission, 2026). To identify such situations, the Commission points to defining labor markets by "occupation and narrow geographic area" (European Commission, 2025). This is the benchmark we evaluate below.

6.1 Job Diversion Ratios Across Mergers

To assess a merger between a target and an acquirer, we use the diversion ratios between the two firms' jobs. A high diversion ratio means that the merger combines jobs that workers regard as close alternatives. Diversion ratios require no predefined labor market, and they are directional. The target's workers may regard the acquirer's jobs as close alternatives

[29] We focus on the harm a merger does to workers. A merger can also raise welfare through efficiencies and lower consumer prices, and jurisdictions differ on whether antitrust should weigh worker harm against those gains. United States doctrine treats harm to workers in the input market as sufficient for liability, regardless of downstream effects on consumers (Hemphill and Rose, 2018). The European Union has not settled the question, and its ongoing merger review asks whether labor market harm is a sufficient theory of harm on its own (European Commission, 2025).

even when the reverse does not hold.

Using year-to-year changes in establishment ownership in the establishment register, we identify about 1,800 mergers between 2007 and 2014 in which both the target and the acquirer are present in our data with workers we can follow, involving roughly 18,400 establishments that together employ more than one million workers. About 8.5% of these establishments close within two years of the merger, around 1,600 establishments over the period (Appendix Figure A.9).[30] Post-merger closures are one channel through which a merger reaches workers. The second works even when no establishment closes: a merger of two employers that workers regard as close alternatives puts those alternatives under one owner and strengthens the merged firm's wage-setting power. Both channels turn on the same primitive, namely whether the merging firms' jobs are close alternatives for workers, and both are invisible to a market-share statistic that does not know where workers can move.

We observe these diversion ratios for 1,242 mergers, those for which both firms appear in the labor-supply model.[31] In the event studies of Section 6.2, a worker is exposed when the largest job diversion ratio from their own job to a single job at the merger partner is at least 0.02.[32] Workers in the same merger can therefore differ in exposure. To show how many mergers expose any of their workers, Figure 5 plots the distribution across mergers of the largest job diversion ratio from any job at one merging firm to a single job at the other. This value is at least 0.02 whenever at least one job, at either firm, has a close alternative at the partner. For 71 percent of mergers, all job diversion ratios toward the merger partner fall below 0.002, while only 131, or 10 percent, reach 0.02. This is the labor market counterpart of the familiar product market fact that most mergers do not raise competitive concerns, and it is reassuring for the tractability of a diversion-based screen: applied to these mergers, such a screen would flag few transactions for closer review.

6.2 Effects of Mergers on Establishments and Workers

To test whether workers whose jobs have a close alternative at the merger partner are the ones harmed, we use a matched difference-in-differences design following Arnold (2025), adapting the merger definition to the Swedish register and matching treated units to controls as in our closure analysis (Appendix A.11).[33] Each worker and establishment at a

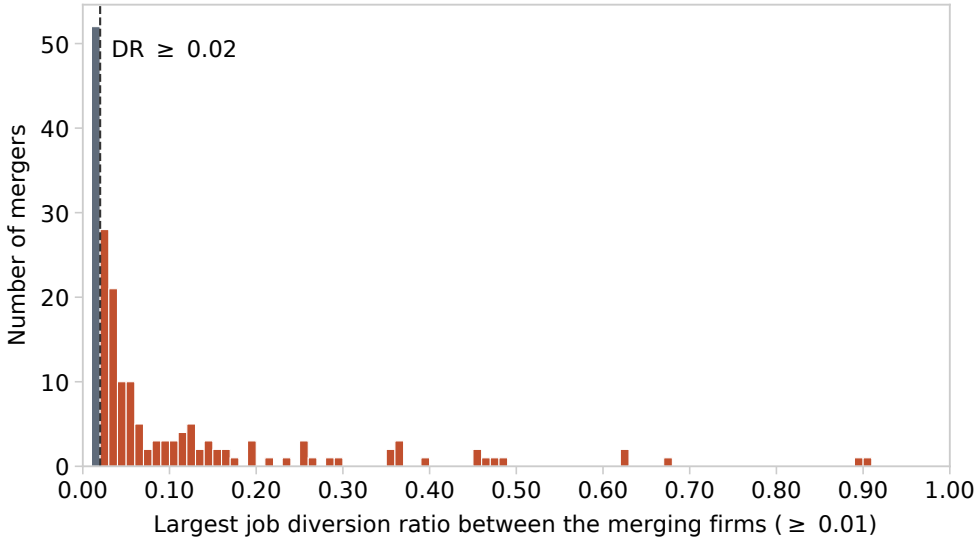
[30]The closure rate among establishments not involved in a merger in any given year is of similar size.

[31]We identify these mergers, clean the sample, and match treated units to controls following the procedures detailed in Appendices A.10 and A.11.

[32]We examine how worker outcomes vary across alternative diversion ratio thresholds in Table 4.

[33]For establishments, we follow Arnold (2025) more closely (Appendix A.11). For workers, we use the propensity-score matching of Schmieder et al. (2023), which finds a control for almost every treated worker. This coverage matters because the high-diversion group is small. Matching workers as in Arnold (2025)

Figure 5: Job Diversion Ratios Across Mergers



Notes: Distribution of the largest job diversion ratio between the merging firms across the 1,242 merger events between 2007 and 2014 for which it can be computed, shown for the 183 events with a ratio of at least 0.01. For each event we take the largest job diversion ratio from any job at one merging firm to a single job at the other, in either direction. The 1,059 events (85%) in which every job diversion ratio to the partner lies below 0.01 are omitted from the chart. Orange bars mark events at or above the 0.02 cutoff (dashed line) used in the event studies. DR denotes the job diversion ratio. Appendix A.10 describes the construction of the merger sample.

merging firm is matched to observably similar counterparts at non-merging firms on pre-event characteristics, so that the matched controls trace the path the treated workers would have followed absent the merger. We then split the treated workers by their overlap with the merger partner in two ways. The first split uses the diversion ratio: a worker is in the high-diversion group when the merger partner offers a close alternative to the worker's job, which we operationalize as a job diversion ratio of at least 0.02 from the worker's own job to a single job at the merger partner. We compare this group with a low-diversion group, workers whose own job's largest diversion ratio to a job at the merger partner is below 0.002, dropping the intermediate group. We set this lower cutoff at a tenth of the high-diversion cutoff, so that the comparison group contains only workers for whom the merger partner is at most a negligible alternative. We compute these diversion ratios from the labor-supply estimates for the year before the merger, so, as in the closure analysis (Section 5.3.3), the split uses only pre-merger information. In total, 1,842 workers fall in the high-diversion group, 773 at target establishments and 1,069 at acquirer establishments, compared against roughly 219,000 workers in the low-diversion group. The second split uses labor markets defined by commuting zone and occupation, following a common approach in the concentration literature (Azar et al., 2022a) and consistent with the occupational and geographic segmentation discussed by the European Commission (European Commission, 2025). A worker has overlap under this split when the merger partner operates a job in the worker's own commuting zone and occupation. The commuting-zone-by-occupation split is far broader: about 17% of treated workers share a commuting zone and occupation with the merger partner, against fewer than 1% with a job diversion ratio of at least 0.02, so it classifies a much larger set of workers as same-market. For each split we compare the high-overlap and low-overlap workers, each against their matched controls, in a difference-in-differences event study, and we ask which split better separates the workers who go on to bear the harm.

Table 3 reports baseline characteristics of the treated workers and their matched controls. The two groups are similar in earnings, education, and age (Panel A). Panel B shows their industry composition, and Panel C summarizes the treated workers' overlap with the merger partner, including the share whose partner operates in their commuting zone and occupation.

Separately for each overlap group under a given split, we estimate the matched difference-

yields the same pattern (Appendix Figure A.16).

Table 3: Merger Sample: Worker Characteristics

	Merger workers	Control workers	Normalized difference
Panel A: Worker characteristics			
Annual earnings (2010 SEK)	331,215	344,117	−0.06
Post-secondary education	0.38	0.38	−0.01
Age	43.8	44.0	−0.02
Share target	0.09	–	–
Panel B: Industry of worker			
Wholesale & retail trade	0.23	0.19	0.08
Health & social work	0.22	0.18	0.08
Manufacturing	0.22	0.21	0.00
Education	0.06	0.08	−0.09
Real estate & business services	0.05	0.06	−0.01
Finance & insurance	0.05	0.03	0.10
Construction	0.05	0.07	−0.09
Transport & storage	0.05	0.07	−0.11
Information & communication	0.04	0.03	0.06
Public administration	0.01	0.04	−0.16
Other industries	0.04	0.04	−0.02
Panel C: Merger characteristics			
Partner in same commuting zone	0.40	–	–
Partner in same CZ × occupation	0.38	–	–
Diversion ratio to merger partner	0.0012	–	–
Share with diversion ratio ≥ 0.02	0.008	–	–
Unique workers	269,562	269,562	

Notes: Workers at merging firms whose baseline job has a diversion value in the labor-supply model, and their 1:1 propensity-score-matched controls at non-merging firms, in the last full pre-merger year. Matching follows Schmieder et al. (2023) within 1-digit industry on establishment size, lagged earnings, education, tenure, and age. Earnings are total annual labor earnings in 2010 SEK. Industry is that of the worker's establishment, in the groups of Table A.9. Other industries pools groups below 0.02 in both columns. Diversion ratio to merger partner is the largest job diversion ratio from the worker's baseline job to a single job at the merger partner. The normalized difference is the difference in means divided by the square root of the average of the two variances.

in-differences event study

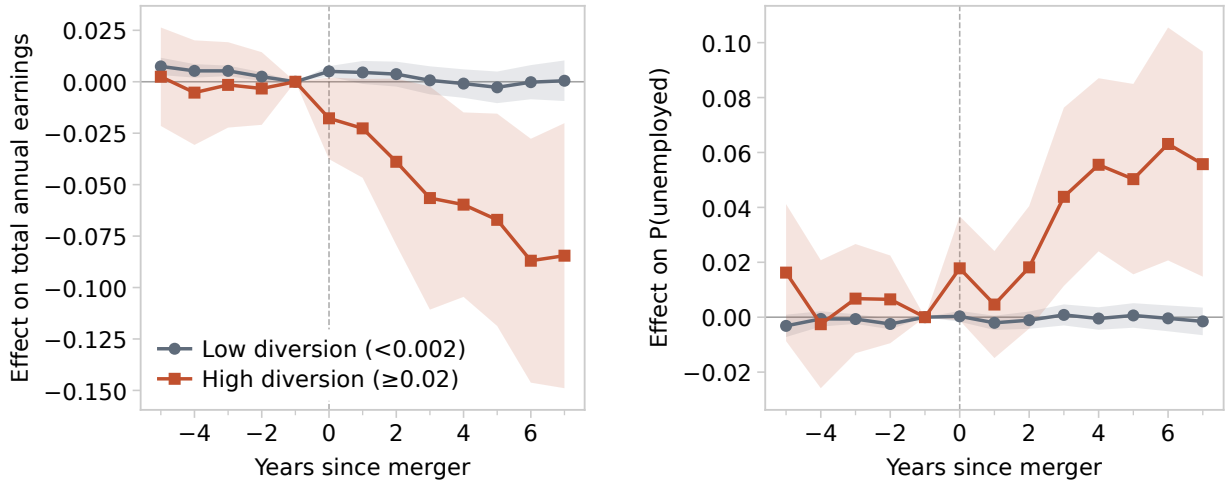
$$y_{ict} = \alpha_i + \lambda_{ct} + \sum_{\tau \neq -1} \beta_\tau^g \, \mathbf{1}[t = \tau] \, T_i + \varepsilon_{ict}, \tag{19}$$

where y_{ict} is the outcome for worker i from merger cohort c at event time t, with $t = 0$ the first year under the new owner and $t = -1$, the last year under the old owner, the omitted reference year, α_i a worker fixed effect, λ_{ct} a merger-cohort-by-event-time fixed effect identified off the matched controls, and T_i an indicator for workers at a merging firm that is zero for their matched controls. The superscript g indexes the high- or low-overlap group under the split in question, with the specification estimated separately on each group's treated workers and their matched controls, so the comparison of the two paths β_τ^g shows whether overlap with the merger partner predicts harm. The worker outcomes are total annual earnings, expressed as a share of the worker's earnings at $t = -2$, and an indicator for an unemployment spell in the year. Standard errors are two-way clustered on the worker and the baseline establishment.

We apply the same design to establishments. An establishment is in the high-diversion group when any of its jobs has a diversion ratio of at least 0.02 to a single job at the merger partner, and in the low-diversion group when none of its jobs reaches 0.002. This gives 112 high-diversion and 4,346 low-diversion establishments (Appendix Table A.10). An establishment closes when its employment falls to 10 percent or less of its pre-merger level. No establishment is closed before the merger, so we estimate Equation (19) at the establishment level without the unit fixed effect and with post-merger event-time indicators only. Standard errors are clustered by establishment.

Results Across all mergers in our sample, workers at merging firms earn on average about 1 percent less than their matched controls five years after the merger and are about 0.4 percentage points more likely to have an unemployment spell (Appendix A.12). These modest averages conceal persistent losses among workers whose jobs have a close alternative at the merger partner (Figure 6). Their earnings track those of their controls before the merger but decline steadily afterward, to a loss of about 7 percent of baseline earnings five years later. Their probability of an unemployment spell rises by about 5 percentage points. By contrast, workers whose jobs divert little to the merger partner experience no corresponding changes in earnings or unemployment. Job diversion ratios estimated before the merger therefore distinguish workers who subsequently experience persistent losses from those who are largely unaffected. Appendix A.13 reports the outcomes separately for workers at the target and acquirer.

Figure 6: Merger Effects by Job Diversion Ratio to the Merger Partner



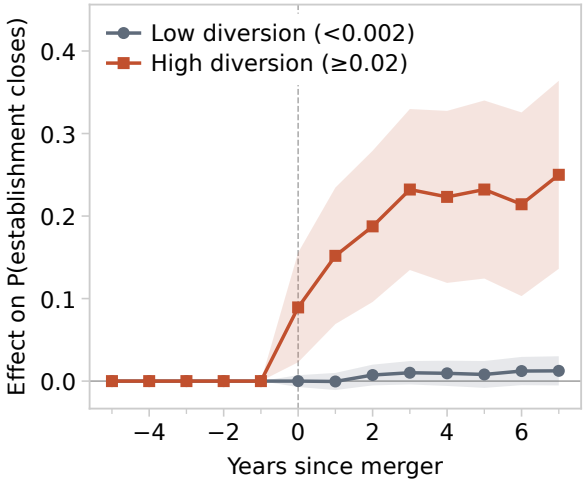
(a) Total annual earnings

(b) Unemployment

Notes: Event studies around a merger for workers at merging firms, split by job diversion ratios to the merger partner: workers whose largest job diversion ratio to a job at the merger partner is at least 0.02 versus a low-diversion group (below 0.002), dropping the intermediate group. Panel (a) plots effects on total annual earnings, expressed as a share of each worker's earnings at $t = -2$, and Panel (b) effects on the probability of an unemployment spell. Each line is a group's difference-in-differences path from Equation (19), estimated against the group's own propensity-score-matched controls, with matching following Schmieder et al. (2023) as described in Appendix A.11. $t = 0$ is the first year under the new owner, and $t = -1$, the last year under the old owner, is the omitted reference year. Effects are measured relative to $t = -1$, and shaded areas indicate 95 percent confidence intervals.

Establishment closures reveal a similar distinction. Five years after the merger, establishments with a job that diverts strongly to the merger partner are 23 percentage points more likely than their matched controls to have closed. Establishments whose jobs divert little to the partner close about as often as their controls (Figure 7). The increase appears in the first year under the new owner and levels off after about three years. Closures are therefore concentrated among establishments whose jobs have close alternatives at the merger partner.

Figure 7: Merger Effects on Establishment Closure



Notes: Event study around a merger for establishments at merging firms, split by job diversion ratios to the merger partner: establishments with a job whose diversion ratio to a single job at the merger partner is at least 0.02 versus a low-diversion group (below 0.002), dropping the intermediate group. The figure plots effects on the probability that the establishment has closed, defined as employment at 10 percent or less of its pre-merger level. This probability is zero before the merger by construction. Each line is a group's difference-in-differences path from the establishment-level version of Equation (19), estimated against the group's own propensity-score-matched control establishments, with matching following Arnold (2025) as described in Appendix A.11. $t = 0$ is the first year under the new owner, and all pre-merger years form the reference period. Shaded areas indicate 95 percent confidence intervals.

We also examine whether earnings losses appear among workers who stay at their pre-merger establishment. Adapting the stayer definition of Arnold (2025) to establishments, we find that high-diversion stayers earn about 3 to 4 percent less than control stayers from one year after the merger onward, while low-diversion stayers experience little change (Appendix A.14). This pattern is consistent with the wage-setting channel, but the estimates are imprecise, with few high-diversion stayers three years after the merger. Moreover, because the merger affects who stays, the earnings comparison does not separately identify the contribution of increased wage-setting power to the overall worker losses.

The event studies fix the exposure threshold at a job diversion ratio of 0.02. Panel

A of Table 4 varies this threshold, reporting pooled estimates of the differential effect of exposure on earnings and unemployment over successively longer post-merger windows, and the estimates show a clear dose-response. At the 0.02 cutoff, the differential earnings loss averages about 3.7 percent of baseline earnings over years 0 to 4 and 5 percent over years 0 to 8. The corresponding increases in unemployment are about 2 and 3 percentage points, respectively. The effects emerge at the 0.01 cutoff and deepen at 0.02, while the lowest cutoff of 0.002 shows no statistically significant effect. The harm is concentrated among workers whose jobs divert strongly to the merger partner. Appendix A.15 reports the full event-study versions under each cutoff, both for the baseline matching and for the alternative matching of Arnold (2025) (Appendix Figures A.15 and A.16).

Comparing Market Definitions Panel B of Table 4 runs the same pooled design for the alternative market definitions, asking whether any of them can identify the same harmed workers. For each candidate definition, we split the treated workers by whether the merger partner operates in the worker's own market under that definition and re-estimate the matched difference-in-differences design, pooling the pre-period and the indicated post-merger windows. Each cell reports the differential effect for the high-overlap group relative to the low-overlap group, both measured against their matched controls. Alongside the diversion ratio splits, we consider the administrative boundaries used in practice (the worker's occupation, commuting zone, their interaction, three-digit industry, and industry by commuting zone) and a data-driven benchmark that delineates markets from observed worker flows (Nimczik, 2023). Panel C turns to employer concentration, measured by the Herfindahl-Hirschman index (HHI) within markets defined by commuting zone and occupation. We test whether workers whose markets see larger merger-induced increases in the HHI suffer greater harm, limiting the high group to markets that were already more concentrated before the merger. This split produces no statistically significant differences in earnings or unemployment effects at any reported horizon.

Only the diversion ratio isolates the harm: no administrative or flow-based definition detects an earnings loss at any horizon, and the few significant coefficients go in the opposite direction, with workers who share an occupation, commuting zone, or industry with the merger partner slightly less likely to be unemployed in the first year. Sharing a market label with the merger partner is too coarse a notion of exposure: it pools the few workers for whom the partner offers a close alternative with a large majority for whom it does not, so the harm averages away. Appendix A.16 shows the full event studies under the commuting-zone-by-occupation split, for workers and for establishment closure.

Table 4: Merger Effects by Diversion Ratio Cutoff and Market Definition

	Total earnings			Unemployment		
Market definition	0	0 to 4	0 to 8	0	0 to 4	0 to 8
Panel A. Job Diversion Ratios						
DR $\geq$ 0.002	0.87	−0.58	−0.76	0.27	0.55	0.79
	(0.58)	(0.83)	(1.40)	(0.48)	(0.55)	(0.73)
DR $\geq$ 0.01	−1.26*	−2.86**	−4.18***	0.51	1.64**	2.54**
	(0.73)	(1.13)	(1.51)	(0.76)	(0.82)	(1.01)
DR $\geq$ 0.02	−1.81**	−3.71***	−5.21***	0.85	2.04**	2.98**
	(0.84)	(1.44)	(1.93)	(0.95)	(1.04)	(1.36)
DR $\geq$ 0.05	−3.31***	−5.29**	−6.82**	2.16	2.70**	3.57**
	(1.12)	(2.43)	(3.23)	(1.46)	(1.34)	(1.75)
Panel B. Alternative Market Definitions						
Same occupation	−0.11	−0.55*	−0.72	−0.69***	−0.23	−0.19
	(0.22)	(0.34)	(0.45)	(0.16)	(0.20)	(0.25)
Same commuting zone	0.49*	0.48	0.51	−0.47**	−0.12	0.01
	(0.30)	(0.41)	(0.54)	(0.19)	(0.26)	(0.31)
Same occupation × CZ	0.38	−0.37	−0.64	−0.26	0.16	0.32
	(0.49)	(0.56)	(0.77)	(0.25)	(0.35)	(0.41)
Same industry (SNI3)	0.22	−0.36	−0.56	−0.52***	−0.04	0.03
	(0.21)	(0.34)	(0.46)	(0.16)	(0.19)	(0.25)
Same industry × CZ	0.11	−0.59	−0.51	0.09	0.30	0.22
	(0.38)	(0.53)	(0.75)	(0.29)	(0.37)	(0.43)
Data-driven market boundaries	0.36	0.53	0.71	0.15	0.17	0.39
(Nimczik, 2023)	(0.42)	(0.56)	(0.79)	(0.27)	(0.39)	(0.45)
Panel C. Merger-Induced Concentration						
HHI increase, occupation × CZ	0.35	−1.01	−1.56	−0.53	0.07	0.06
(high vs. low)	(0.53)	(0.85)	(1.24)	(0.42)	(0.49)	(0.58)

Notes: Each cell reports the difference in the merger effect between the high and the low group, each measured against its matched controls, from a pooled difference-in-differences. The pre-period pools $t = -6$ to -1, where $t = -1$ is the last year under the old owner, and each column pools the post-merger years shown, where $t = 0$ is the first year under the new owner. Standard errors in parentheses are clustered by worker and baseline establishment. Earnings effects are in percent of earnings at $t = -2$ and unemployment effects in percentage points. In Panel A the high group consists of workers whose largest job diversion ratio (DR) from their baseline job to a single job at the merger partner is at least the row's cutoff, and the low group of workers for whom it is below 0.002. Workers in between are dropped. In Panel B the groups are same versus different market. In Panel C the high group's occupation × CZ cell has a merger-induced HHI increase of at least the 75th percentile of positive increases across two-sided mergers and an above-median pre-merger HHI. The low group's increase is below that cutoff, and the remaining workers are dropped. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

7 Conclusion

This paper develops a method for estimating job diversion ratios from matched employer-employee data. Applied to Swedish administrative data covering 2.5 million workers and 590,000 jobs per year, it produces the Job Diversion Matrix without imposing predefined labor market boundaries. Out-of-sample tests using plant closures show that predicted geographic and occupational destinations closely track displaced workers' realized moves. The number of effective alternatives also predicts their earnings losses. One year after closure, workers displaced from jobs with few effective alternatives lose nearly three times as much as those from jobs with many, and their earnings remain depressed six years later.

A direct application of the Job Diversion Matrix is merger review. Here the diversion ratios between the merging firms' jobs can serve as a screen, showing whether a merger combines employers whose jobs are close alternatives. We find that only 10 percent of the Swedish mergers we study do. Workers whose jobs have a close alternative at the merger partner earn less and are more likely to be unemployed than workers whose jobs do not, and establishments are more likely to close. Neither standard market definitions nor a concentration screen based on the Herfindahl-Hirschman index identifies this pattern. The Job Diversion Matrix therefore gives competition authorities a practical tool for assessing a merger's effects on workers, one that measures workers' alternatives directly rather than presuming them from a shared occupation and commuting zone.

Estimating the full labor supply system to obtain job diversion ratios may be infeasible for an agency evaluating a merger. The agency may lack the time or a high-quality matched employer-employee dataset. Surveys are one natural alternative. In product markets, authorities already run purpose-built surveys that elicit consumers' second choices (Conlon and Mortimer, 2022). The labor market analog is to survey the merging firms' workers about where they would apply if their plant closed. A recent literature in labor economics illustrates their potential. When Caldwell et al. (2025) ask workers to name the firms they would consider, those firms are the ones their coworkers later moved to. Pairing the named firms with randomized wage offers even measures how large a raise it takes to move a worker (Caldwell et al., 2026b). In a merger review, the share of one merging firm's workers who name the other is a direct estimate of the diversion ratio between them, the object our screen relies on.

Beyond mergers, the Job Diversion Matrix can inform policy responses to trade shocks, technological change, and the green transition. When a plant producing internal combustion engine cars closes, new plants producing electric cars offer little protection if they require different skills. The matrix identifies which existing jobs provide meaningful alter-

natives for the plant's workforce. Combined with measures of exposure to tariffs, import competition, or automation, it can show whether workers could move to unaffected jobs or whether their alternatives are also threatened. These patterns can inform industrial policy and regional aid by identifying jobs and places where displacement would leave workers with few alternatives, and help tailor retraining and reemployment support to the jobs those workers could turn to.

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A Appendix

A.1 Sample Definition

We estimate the model separately for each year from 2006 to 2013 and report the 2010 cross-section as the representative year throughout (Appendix A.4). The window starts in 2006, when four-digit occupation coverage in the registers becomes near complete, and ends in 2013, the last year of the SSYK96 occupation classification. Each annual cross-section consists of individuals aged 24–54 observed in that year and the previous one, with non-missing education and demographic information; the previous-period job enters through state dependence. We exclude military personnel, politicians, police, and managers.[34] We also exclude individuals whose current-period or previous-period job is infeasible given their education (Section 4.2).

A job is defined as an establishment-occupation combination observed in the estimation year, where occupations are classified at the four-digit level using the SSYK96 classification.[35] The final estimation sample contains roughly 2.5 million individuals per year. Of these, about 2.1 million are employed and distributed across approximately 590,000 jobs, while roughly 400,000 choose the outside option.

Table A.1 lists the ten largest occupations and education groups in the estimation sample.

[34] We exclude managers as this represents more a career progression than a job choice.

[35] There are 323 four-digit occupations in our matched data. A complete list can be found here: `https://www.scb.se/ssyk`

Table A.1: Largest Occupations and Education Groups

Panel A: Occupations

	Employment	Jobs	Median size	
	share (%)	per year	Job	Establishment
5132 Assistant nurses	5.2	8,657	38	65
5133 Home-based personal care workers	3.2	14,547	17	46
5222 Shop salespersons, non-food	3.1	24,738	4	7
5131 Child-care workers	2.9	17,538	6	17
3415 Sales representatives	2.7	19,519	5	19
4190 Other office clerks	2.7	25,376	3	31
3310 Pre-primary teaching associates	2.5	11,736	7	16
2131 Systems designers and programmers	2.4	8,111	24	59
2330 Primary school teachers	2.2	5,709	14	35
5134 Psychiatric care attendants	2.0	8,142	9	13
All occupations	100.0	592,534	9	34

Panel B: Education groups

	Individuals (%)		Occupations	
	Share	Employed	Feasible	Complete match
02Z Pre-secondary (9 years)	10.4	67.5	235	0†
03A General upper-secondary, social science	6.8	80.7	235	0†
33H Vocational, commerce and administration	5.9	85.1	235	47
73O Vocational, nursing and care	5.0	89.0	235	21
53V Vocational, workshop training	3.8	85.8	235	0†
53E Vocational, electrical and IT	3.2	91.9	238	108
34P Social science credits, no degree	2.5	85.6	235	0†
01Z Pre-secondary (less than 9 years)	2.4	39.5	235	0†
35E Economics, university	2.4	87.8	288	45
75N Nursing, university	2.3	95.8	282	28
All individuals	100.0	84.2	235	

Notes: Pooled over the estimation years 2006–2013. Panel A lists the ten four-digit occupations (SSYK96) with the largest shares of employed individual-years. Jobs is the mean number of jobs, that is establishment-occupation combinations, per year. The median job and establishment sizes are those of the median employed individual in the occupation, counting sample workers. Panel B lists the ten education groups (SUN2000) with the largest shares of all individual-years and the share of each group's individual-years in employment. Feasible occupations counts the 323 four-digit occupations that the education-occupation screen leaves feasible for the group, and for all individuals reports the median. Complete match counts the feasible occupations the matrix rates as a complete subject match for the group. †Education group not rated by Statistics Sweden's matrix. Its feasible occupations are those for which every rated education is at least correctly qualified, and all are assigned a poor subject match, so the zero holds by construction.

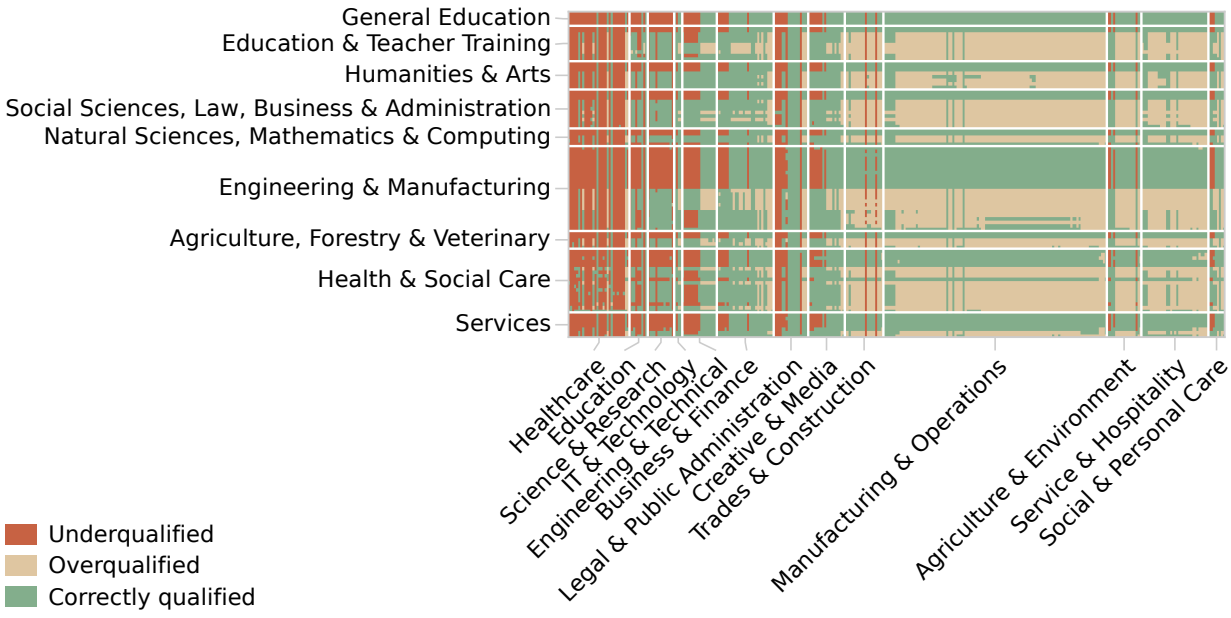
A.2 Education-Occupation Matrix and Occupation Similarity

Statistics Sweden's education-occupation matching combines normative and statistical methods. The qualitative method defines in advance which education programs fit which professions, independent of actual distributions. The statistical method analyzes how education backgrounds are distributed within occupations to detect over- or under-represented combinations. Uncommon combinations can still be accepted if statistically over-represented.

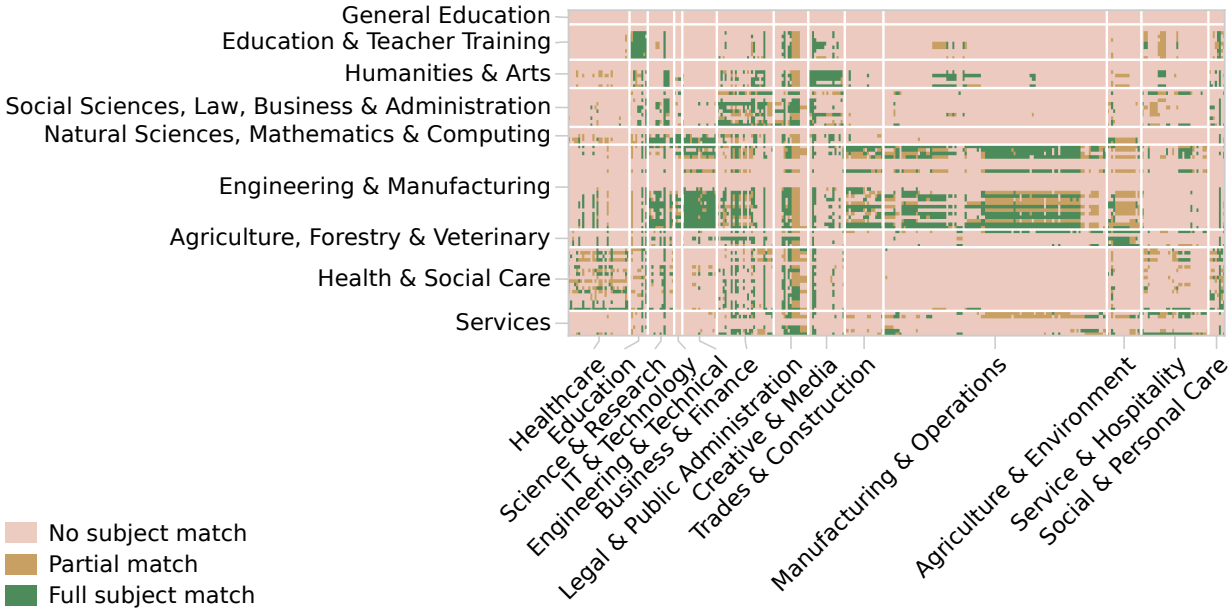
The classification is based on approximately 120 education groups and 140 occupation groups, forming several thousand combinations. It uses data from education-occupation distributions, job advertisements, and training providers.

Each education-occupation pair is evaluated along two dimensions. Subject matching captures how well the education's field fits the job, classified as complete, partial, or no match. Level matching captures how well the education's level fits job requirements, classified as lower, correct, or higher than required.

Figure A.1: Education-Occupation Match Quality



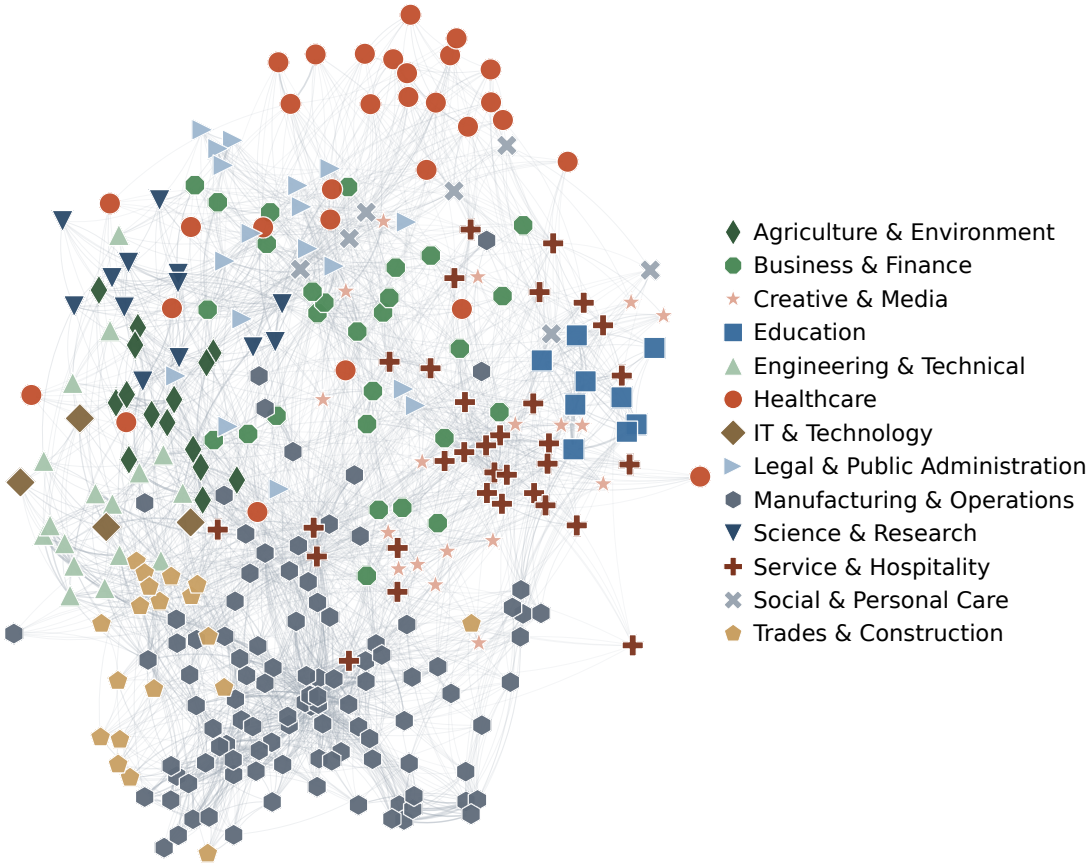
(a) Feasibility



(b) Subject Fit

Notes: Each row corresponds to a detailed education code and each column to a detailed occupation code, ordered by broader education fields and occupation groups. Panel (a): Feasibility. Red indicates under-qualified, sand over-qualified, and green correctly qualified. Panel (b): Subject Fit. Green indicates a full subject match, sand a partial match, and pale red no subject match. White lines separate groups.

Figure A.2: Occupation Similarity Network



Notes: Network visualization of occupation similarity based on skill overlap in job advertisements. Each node represents a four-digit occupation (SSYK96), colored by occupation group. Edges connect each occupation to its ten most similar occupations for visualization only; the estimation uses the full similarity matrix. Edge thickness reflects similarity score. Spring layout positions similar occupations closer together.

A.3 Education and Occupation Group Definitions

Table A.2 lists the nine education field groups used in Figure A.1, with the individual SUN2000 education codes assigned to each group. Table A.3 lists the thirteen occupation groups, with the individual SSYK96 four-digit codes assigned to each group.

Table A.2: Education Field Groups

Group	SUN2000 Codes
General Education	01Z, 02Z, 03A, 03N
Education & Teacher Training	13Z, 14P, 15B, 15F, 15G, 15H, 15P, 15S, 15V, 15X
Humanities & Arts	23E, 23M, 24P, 25H, 25K, 25M, 25T, 25X
Social Sciences, Law, Business & Admin.	33E, 33H, 34P, 35B, 35E, 35F, 35J, 35M, 35P, 35S, 35X
Natural Sciences, Math. & Computing	43Z, 44P, 45D, 45N, 45X
Engineering & Manufacturing	53A, 53B, 53E, 53F, 53K, 53L, 53P, 53R, 53T, 53V, 53X, 54P, 55A, 55C, 55D, 55E, 55F, 55G, 55H, 55I, 55J, 55K, 55L, 55X
Agriculture, Forestry & Veterinary	63Z, 64P, 65J, 65S, 65V, 65X
Health & Social Care	73B, 73O, 73T, 73X, 74P, 75A, 75B, 75D, 75F, 75H, 75J, 75L, 75N, 75O, 75P, 75T, 75V, 75X
Services	83R, 83T, 83X, 84P, 85M, 85P, 85T, 85X

Table A.3: Occupation Groups

Group	SSYK96 Codes
Healthcare	2221, 2222, 2223, 2224, 2225, 2229, 2231, 2233, 2234, 2235, 2236, 2491, 3134, 3221, 3222, 3223, 3224, 3225, 3226, 3227, 3228, 3229, 3231, 3232, 3233, 3234, 3235, 3239, 5132, 5135
Education	2321, 2322, 2323, 2330, 2340, 2351, 2359, 3310, 3320
Science & Research	2111, 2112, 2113, 2114, 2121, 2122, 2211, 2212, 2310, 2442, 2443, 3111, 3240
IT & Technology	2131, 2139, 3121, 3122
Engineering & Technical	2141, 2142, 2143, 2144, 2145, 2146, 2147, 2148, 2149, 3112, 3113, 3114, 3115, 3116, 3117, 3118, 3119
Business & Finance	2411, 2412, 2413, 2414, 2419, 2441, 3411, 3412, 3413, 3415, 3416, 3417, 3418, 3419, 3421, 3423, 3429, 3431, 3433, 3476, 4111, 4112, 4120, 4190, 4212, 4214, 4215, 5227
Legal & Public Admin.	2421, 2422, 2423, 2429, 2470, 2480, 3151, 3152, 3432, 3441, 3442, 3443, 3449, 5151, 5152, 5153, 5159
Creative & Media	2431, 2432, 2444, 2451, 2452, 2453, 2454, 2455, 2456, 3131, 3132, 3133, 3471, 3472, 3473, 3474, 4140, 5210
Trades & Construction	7121, 7122, 7123, 7124, 7129, 7131, 7132, 7133, 7134, 7135, 7136, 7139, 7141, 7142, 7143, 7241, 7242, 7243, 9310
Manufacturing & Operations	3141, 3142, 3143, 3144, 3145, 3422, 4131, 4132, 4150, 7111, 7112, 7211, 7212, 7213, 7214, 7215, 7216, 7221, 7222, 7223, 7224, 7231, 7232, 7233, 7311, 7312, 7313, 7321, 7322, 7323, 7324, 7330, 7341, 7342, 7343, 7344, 7411, 7412, 7413, 7421, 7422, 7431, 7432, 7433, 7434, 7435, 7441, 7442, 8111, 8112, 8121, 8122, 8123, 8124, 8125, 8130, 8141, 8142, 8143, 8144, 8150, 8160, 8170, 8211, 8212, 8221, 8222, 8223, 8224, 8229, 8231, 8232, 8240, 8251, 8252, 8253, 8261, 8262, 8263, 8264, 8265, 8269, 8271, 8272, 8273, 8274, 8275, 8276, 8277, 8278, 8279, 8281, 8282, 8283, 8284, 8285, 8290, 8311, 8312, 8321, 8322, 8323, 8332, 8333, 8334, 8340, 9141, 9150, 9320, 9330
Agriculture & Environment	2213, 2214, 3211, 3212, 6111, 6112, 6113, 6121, 6122, 6129, 6130, 6140, 6151, 6152, 6153, 8331, 9210
Service & Hospitality	3414, 3462, 3475, 4211, 4213, 4221, 4222, 4223, 4224, 5111, 5112, 5113, 5121, 5122, 5123, 5141, 5142, 5149, 5221, 5222, 5223, 5224, 5225, 5226, 7137, 9110, 9121, 9122, 9123, 9130, 9142, 9143, 9190
Social & Personal Care	2460, 2492, 3461, 3480, 5131, 5133, 5134, 5139

A.4 Estimation Window and Stability

We estimate the model separately for each of the eight years from 2006 to 2013. Because utility depends on the previous-period job, each annual estimation conditions on each worker's state in the prior year, a job or nonemployment. The 2010 estimation, for example, uses choices in 2010 given workers' states in 2009.

The eight estimations are entirely separate, with no restriction linking the preference coefficients across years. Even so, the estimates are nearly identical across all eight years, so their stability is an empirical result rather than an assumption. We still estimate every year because the diversion ratios are year specific even though the coefficients are nearly constant: they depend on the job fixed effects and on the set of available jobs, which changes from year to year. Because the annual estimates are so close, we report results for the 2010 cross-section as a representative year.

A.5 Implied Magnitudes of Preference Estimates

Table A.4 expresses the estimated forces on a common scale: each row reports the change in utility from an interpretable change in a job's attributes, for a worker choosing among feasible moves, together with its equivalent in commuting kilometers. Occupational continuity is the strongest single force: a same-occupation move is worth more than twice the switching cost and about four times a 20 km longer commute. The switching cost ranks second. The desirability of the destination firm and commuting distance come next and are comparable to each other, while subject mismatch, over-qualification, and distance to mother are progressively smaller. Workers weigh geography, inertia, and match quality together, but occupational continuity dominates the ranking.

Table A.4: Implied Utility Magnitudes of Model Contrasts

Contrast	Δ (utils)	$\approx$ commute-km
Same occupation vs. different occupation	+8.91	−79.6
New job vs. current job (same location)	−4.10	+36.6
More vs. less desirable firm (job fixed effects)	+2.73	−24.4
Commute 20 km longer (for a switcher)	−2.24	+20.0
Poor field-of-study match	−0.83	+7.4
Over-qualified (education level too high)	−0.69	+6.2
20 km farther from mother	+0.07	−0.6

Notes: Implied utility magnitudes from the conditional-logit choice model. Each row is the change in utility from a single discrete change, evaluated for a move to an alternative job at a fixed reference profile (a 40-year-old, medium-educated, native man with no children and short tenure) at which all demographic interactions are zero. The commute-km column rescales utils by the switcher's commuting disutility over the first 20 km, 0.112 utils per km; a positive entry is equivalent to that many added commuting kilometers and a negative entry to a commute of that length avoided. The firm row is the employment-weighted interquartile range of the estimated job fixed effects δ_j across jobs, excluding the nonemployment outside option.

A.6 Displacement Sample and Earnings of the Employed

Sample Construction Following Athey et al. (2023), we identify plant closures using establishment snapshots between 2006 and 2012. A plant is classified as closed if it either disappeared entirely or shrank by at least 90% of its workforce within two years. Following Hethey-Maier and Schmieder (2013), we use worker flows to exclude closures consistent with relabeling (the same workers reappearing under a new establishment identifier) or internal reorganization within the same firm. Only plants with at least five employees prior to closure are included. Displaced workers are defined as individuals aged 24–60 with at least three years of continuous tenure at the closing plant, whose annual earnings exceed the 10th percentile of the earnings distribution. We exclude military personnel and senior government officials. Table A.5 reports the worker and plant counts at each restriction; the binding step is the requirement that the closing job carry computed diversion ratios, and hence a number of effective alternatives.

Control Group Following Schmieder et al. (2023), we match each displaced worker to a single non-displaced worker at a surviving plant by propensity score, estimated within closure year and one-digit industry from pre-closure plant size, log earnings in the two years before closure, education, tenure, and age. Both groups are followed from $t = -5$ to $t = +7$ relative to the closure, where $t = 0$ is the closure year, using November employment snapshots. For each outcome, a matched pair contributes a year only when both the displaced worker and the control are observed in it. Event-study effects are measured

Table A.5: Closure Analysis Sample Construction

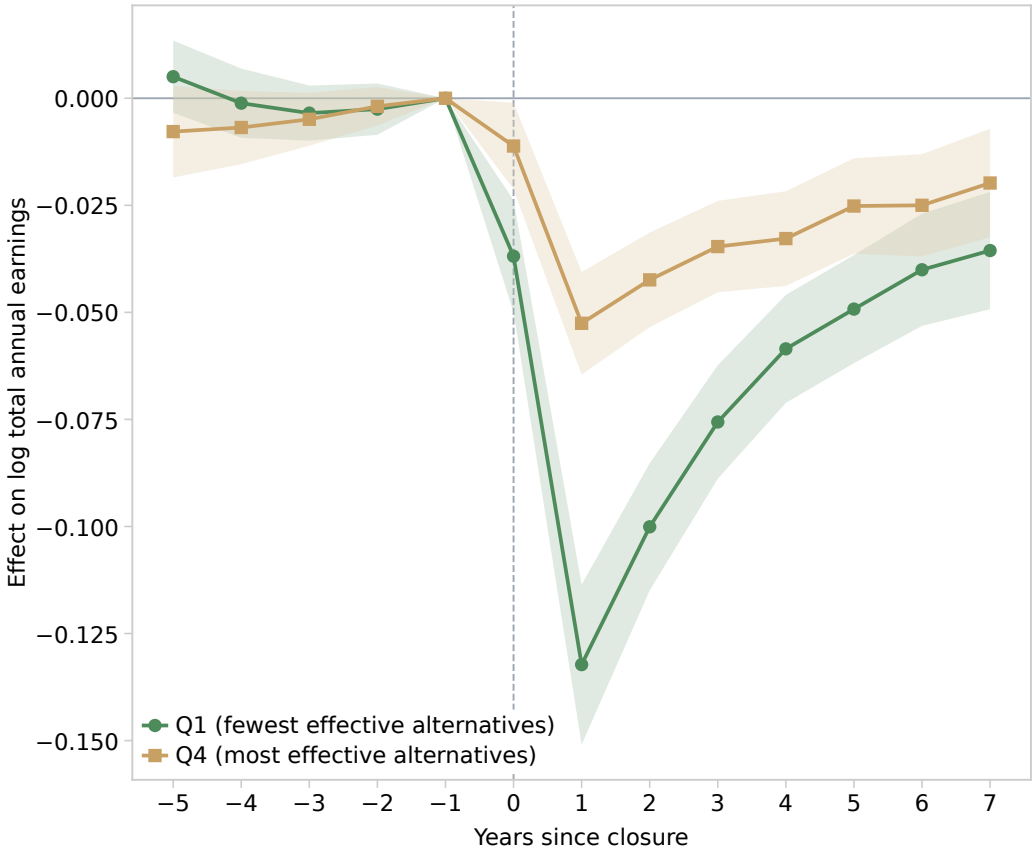
	Displaced workers	Closing plants
Linked displaced-worker panel	147,422	14,201
Closures 2006–2012 (occupation coverage)	128,952	12,258
Employed and qualified at baseline	106,994	11,877
Diversion ratios available	61,097	7,991

Notes: Worker and plant counts at each restriction used to build the closure analysis sample, measured in the baseline year. Each displaced worker is matched one-to-one to a non-displaced control at a surviving plant, so the control count equals the displaced count at every step. "Closures 2006–2012" excludes later closures, for which occupation-code coverage falls sharply in 2014. "Employed and qualified at baseline" requires positive employment and real establishment earnings of at least 150,000 SEK (2010 prices). "Diversion ratios available" requires the closing job to carry computed diversion ratios, and hence a number of effective alternatives; this is the binding restriction, reflecting occupation and establishment coverage in the structural estimation sample.

relative to the year before closure ($t = -1$). For total-earnings outcomes, workers who are nonemployed in a given year receive zero earnings; the outcome in Figure A.3 below is defined for employed workers only. Table A.6 reports baseline means for the displaced and matched-control workers: normalized differences are at most 0.07 in absolute value on every characteristic, including sex, which the match does not target.

Earnings of the Employed Figure A.3 restricts the sample in each year to workers who are employed, as defined in Figure 4, and plots the effect of displacement on log annual earnings. One year after closure, employed Q1 workers earn about 12 percent less than their matched controls, against 5 percent for employed Q4 workers; six years out the gaps are about 4 and 2.5 percent. Part of the first-year gap reflects months not worked by workers who started their new job during the year. The closure year itself ($t = 0$) combines pay from the closing plant, including separation pay, with pay from new employers.

Figure A.3: Earnings of the Employed After Displacement



Notes: Event study of the effect of displacement on log total annual earnings among workers who are employed in the given year, with employment defined as in Figure 4. Workers are split by the number of effective alternatives of the lost job into the bottom quartile (Q1, fewest effective alternatives) and the top quartile (Q4, most). The figure plots the two group paths from Equation (18), estimated separately for each group against its matched controls. Effects are relative to the year before closure ($t = -1$), with $t = 0$ the closure year. Shaded areas indicate 95% confidence intervals.

Table A.6: Baseline Balance: Displaced and Matched-Control Workers

	Displaced	Control	Norm. diff.
Age	43.1 (9.6)	43.7 (9.6)	−0.07
Female	0.30 (0.46)	0.33 (0.47)	−0.07
Education (SUN level, 1–6)	3.71 (1.31)	3.77 (1.31)	−0.04
Plant size	82.7 (114.4)	92.2 (270.9)	−0.05
Tenure (years)	7.9 (5.0)	8.0 (4.9)	−0.02
Annual earnings (SEK 000s)	349 (147)	363 (235)	−0.07
Workers	61,097	61,097	

Notes: Means with standard deviations in parentheses, measured in the baseline year (the last full year before closure), for workers displaced by a plant closure between 2006 and 2012 and their propensity-score-matched controls at surviving plants (Appendix A.6). The normalized difference is the difference in means divided by the square root of the average of the two within-group variances; absolute values below 0.1 are conventionally taken to indicate good balance. Female is the share of women; plant size is headcount at the worker's plant; earnings are total annual labor earnings in thousands of 2010 SEK; education is the Swedish SUN level code. The propensity score is estimated following Schmieder et al. (2023) on plant size, lagged earnings, education, tenure, and age, so balance on these characteristics is partly by construction; sex is not targeted by the match.

A.7 Aggregating Diversion Ratios

For an origin set of jobs G and a destination set of jobs H, for example two occupations, regions, or firms, we aggregate the job-level diversion ratios of Equation (9) by removing one job at a time:

$$D_{jk} = \frac{1}{L_j(\mathcal{J}_t)} \sum_{i \in N_t} \frac{s_{ijt}\, s_{ikt}}{1 - s_{ijt}} \tag{20}$$

$$= \frac{1}{L_j(\mathcal{J}_t)} \left(L_k(\mathcal{J}_t \setminus \{j\}) - L_k(\mathcal{J}_t) \right) \tag{21}$$

$$D_{GH} = \frac{1}{\sum_{j \in G} L_j(\mathcal{J}_t)} \sum_{j \in G} \sum_{k \in H} \left(L_k(\mathcal{J}_t \setminus \{j\}) - L_k(\mathcal{J}_t) \right) \tag{22}$$

$$= \frac{\#\text{ workers moving from } G \text{ to } H \text{ if their job is removed}}{\#\text{ workers employed at } G}. \tag{23}$$

D_{GH} is the employment-weighted average of the job-level diversion ratios from jobs in G to jobs in H: the share of workers at G who would move to H if their particular job disappeared.

A.8 Model-Free Diversion Ratios

As a benchmark that imposes no choice model, we estimate diversion ratios directly from observed employment shares within cells of similar workers. We partition workers by a recursive median split: at each step we divide a cell at the median of the worker characteristic with the highest within-cell variance, among home location, occupation, and education, and we continue until every cell holds at least 500 workers. Within a cell c we estimate the probability of holding job k by its employment share $s_{k|c}$, and we form the diversion ratio from job j to job k as $D_{jk} = \sum_c (N_{jc}/N_j)\, s_{k|c}/(1 - s_{j|c})$, where the weight N_{jc}/N_j is job j's share of its workers in cell c. This benchmark uses the same employed cross-section as the structural estimates and differs only in replacing the estimated choice probabilities with raw within-cell shares, so the gap between the two isolates the contribution of the structural model.

A.9 Aggregate Diversion and Destination Accuracy

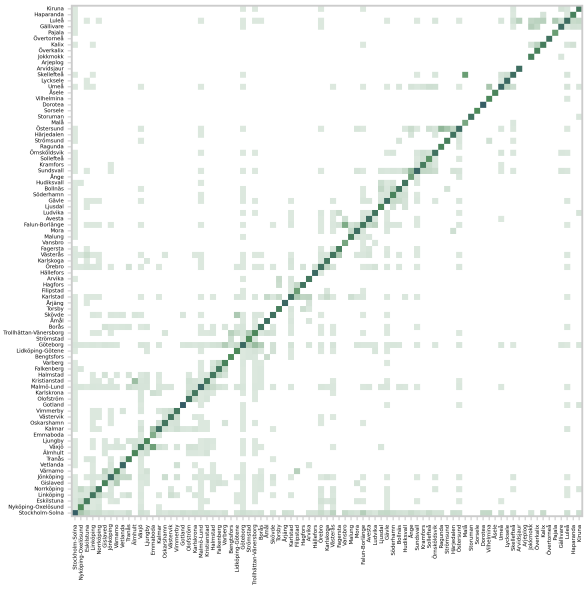
This appendix reports aggregate diversion shares and destination-accuracy statistics supporting the validation in Section 5.3.3. Table A.7 reports aggregate diversion shares and Table A.8 how often each model ranks a displaced worker's realized destination first. Figures A.4–A.6 compare realized and model-implied diversion, and Figures A.7 and A.8 show the simple logit diversion matrices.

Table A.7: Aggregate Diversion Shares

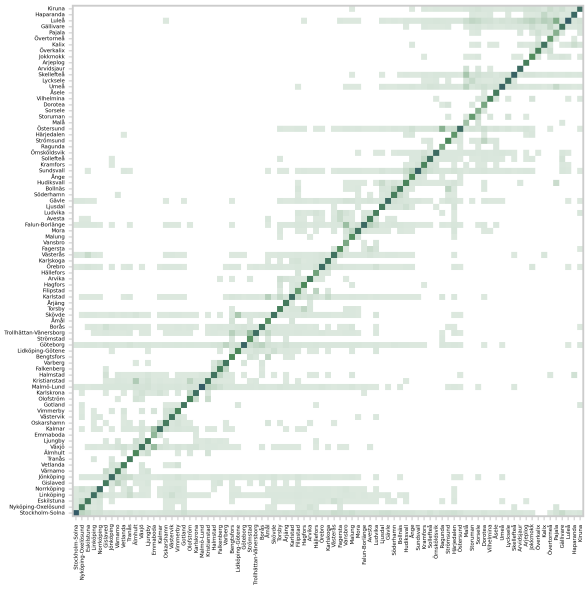
	Same CZ	Same occupation	Same CZ and occupation	Nonemployment
Closures (data)	0.82	0.70	0.62	0.07
Full Model	0.72	0.77	0.68	0.06
Simple Logit	0.05	0.01	0.00	0.00
Model-Free	0.39	0.04	0.02	0.07

Notes: Each entry is the share of workers displaced by a plant closure between 2006 and 2012 who reallocate to the indicated margin. Closures reports observed transition shares; Full Model, Simple Logit, and Model-Free report the shares implied by the corresponding diversion estimates, aggregated over the same closing jobs using employment weights. The first three columns summarize reallocation across commuting zones and occupations; the last reports transitions into nonemployment. The simple logit uses only job fixed effects; the model-free benchmark reads reallocation from within-cell employment shares, with cells from a recursive median split on location, occupation, and education.

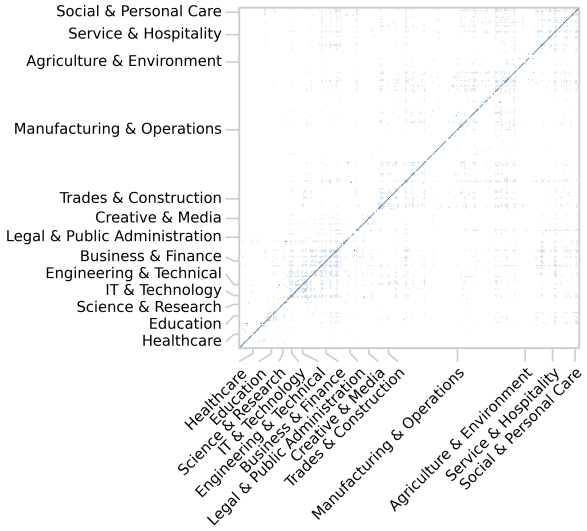
Figure A.4: Empirical Reallocation and Model-Implied Diversion Matrices



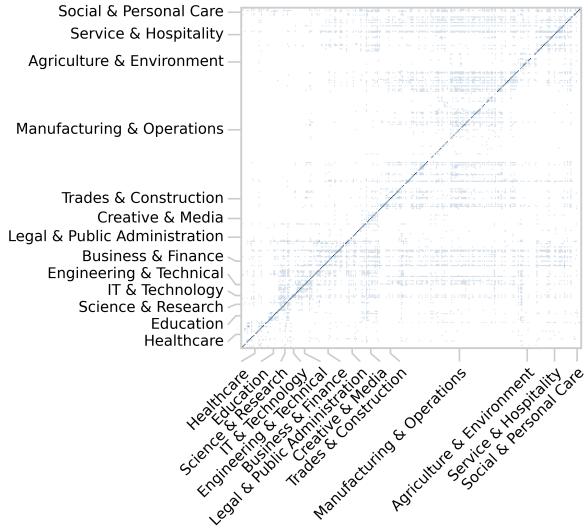
(a) Plant closures (data), commuting zones



(b) Model diversion, commuting zones



(c) Plant closures (data), occupations



(d) Model diversion, occupations

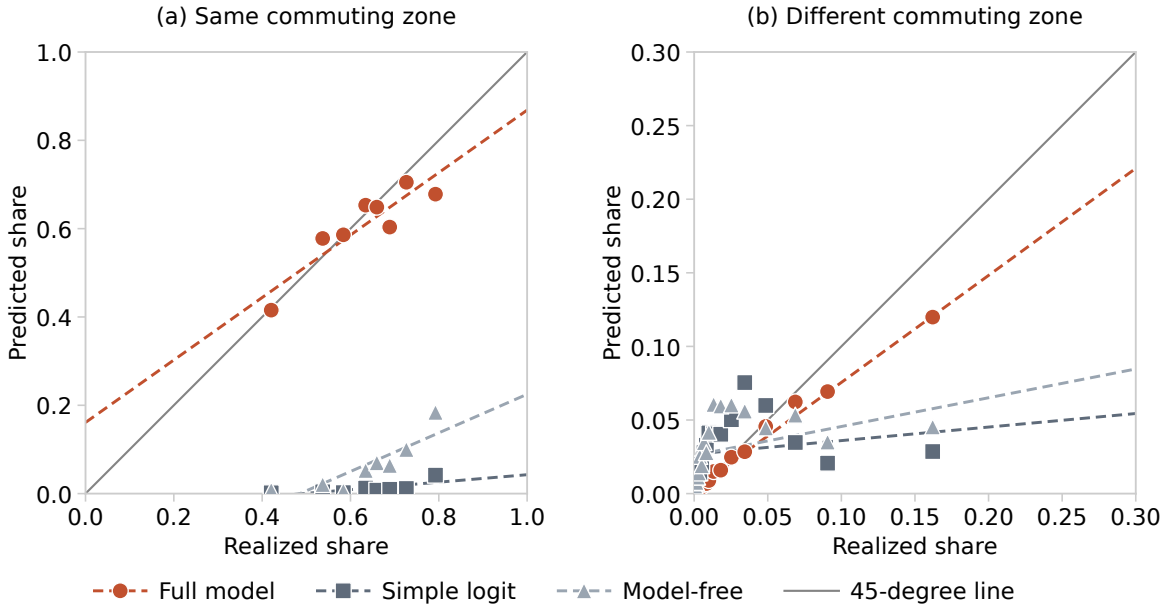
Notes: Realized reallocation matrices for workers displaced by plant closures (left column) and model-implied diversion matrices (right column), across commuting zones (top row) and four-digit occupations (bottom row, SSYK96). Destinations are measured at the worker's first departure from the closing plant within two years of the closure year, pooled over the 2006–2012 closure cohorts. The x-axis is the origin and the y-axis the destination; transitions into nonemployment are excluded, and model cells with diversion below 0.002 are left blank. Figure 3 summarizes the comparison in binned form; Appendix Figures A.7 and A.8 report the corresponding simple logit matrices.

Table A.8: Predicting Each Worker's Most Likely Destination

	Full Model	Simple Logit	Model-Free
Realized destination commuting zone ranked first	0.757	0.130	0.510
Realized destination occupation ranked first	0.605	0.002	0.120

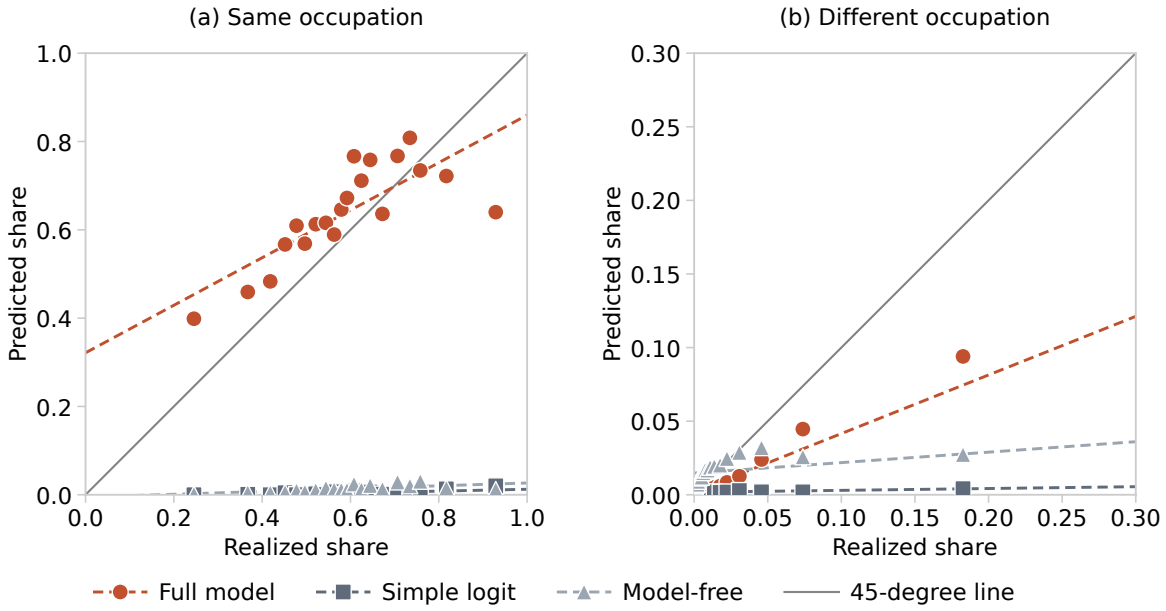
Notes: For each worker displaced by a plant closure between 2006 and 2012, alternatives are ranked by model-implied diversion. The table reports the share of displaced workers whose realized reemployment commuting zone (first row) or occupation (second row) is the top-ranked alternative, for the full model, a simple logit benchmark, and a model-free benchmark that reads diversion from within-cell employment shares (recursive median split on location, occupation, and education).

Figure A.5: Predicted versus Realized Shares Across Commuting Zones: Diagonal and Off-Diagonal



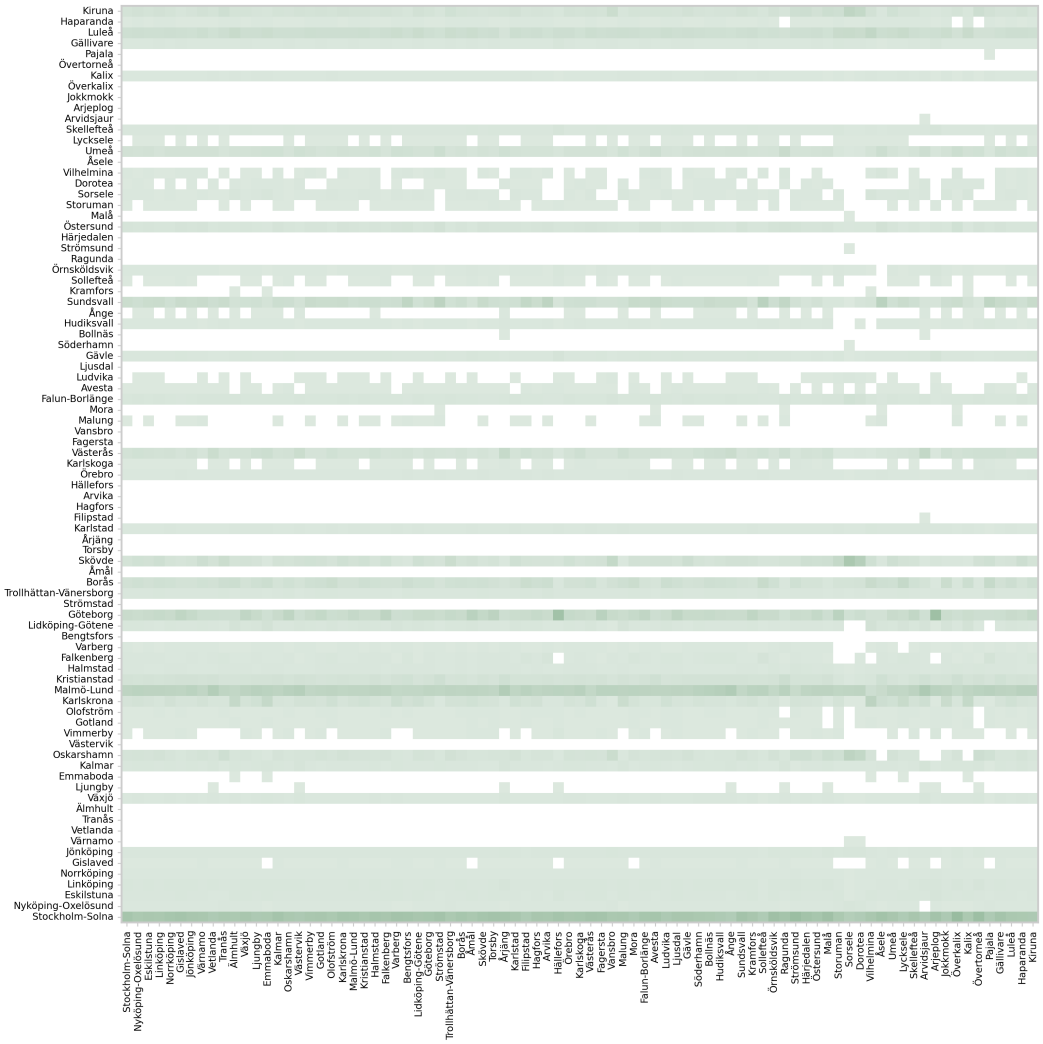
Notes: Binned comparisons of predicted and realized shares across commuting zones, as in Panel (a) of Figure 3, separating same-commuting-zone pairs (Panel (a), the diagonal) from different-commuting-zone pairs (Panel (b), the off-diagonal). Bins are quantiles of realized shares. Dashed lines are linear fits, the solid line marks a perfect fit, and missing predictions are set to zero.

Figure A.6: Predicted versus Realized Shares Across Occupations: Diagonal and Off-Diagonal



Notes: Binned comparisons of predicted and realized shares across four-digit occupations, as in Panel (b) of Figure 3, separating same-occupation pairs (Panel (a), the diagonal) from different-occupation pairs (Panel (b), the off-diagonal). Bins are quantiles of realized shares. Dashed lines are linear fits, the solid line marks a perfect fit, and missing predictions are set to zero.

Figure A.7: Simple Logit Diversion Matrix Across Commuting Zones



Notes: Diversion matrix across commuting zones implied by the simple logit benchmark used in Section 5.3.3; cells with diversion below 0.002 are left blank.

Figure A.8: Simple Logit Diversion Matrix Across Occupations



Notes: Diversion matrix across occupations implied by the simple logit benchmark used in Section 5.3.3; cells with diversion below 0.002 are left blank.

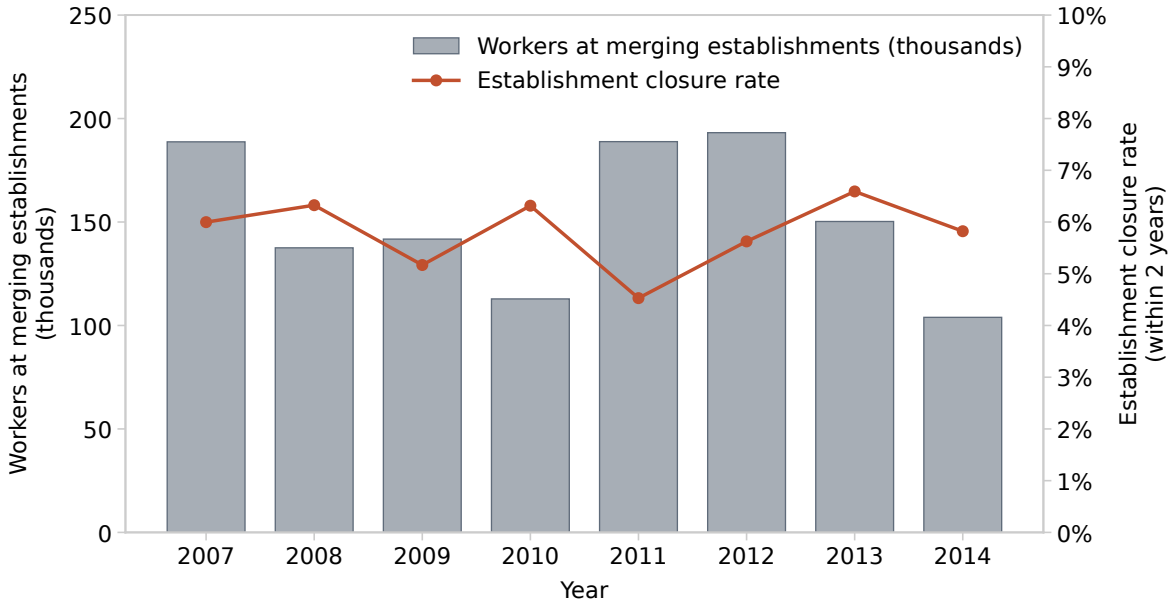
A.10 Merger Sample Construction: Events

We identify mergers using administrative ownership changes in the Swedish establishment register. Each establishment (CFAR) is linked to a firm identifier (PeOrg). We define a merger event as a year-to-year change in an establishment's PeOrg identifier across consecutive years; the establishment (CFAR) keeps its own identifier through the ownership change, so we track the same workplace before and after. All establishments sharing the same old PeOrg, new PeOrg, and year are grouped into a single merger event. This procedure yields roughly 7,500 events.

We apply several filters to exclude false or economically insignificant transitions. Both the target and the acquiring firm must employ at least five workers. Both firms must be present in our data the year before the merger. At least 50% of the old firm's total employment must be involved in the transition, and the old firm must lose at least 50% of its employment at the time of the transition. Events where the old and new firm already share more than 50% of their establishments are dropped, as these likely reflect internal reorganizations rather than mergers. Together with the requirement that we can follow workers on both sides, these filters leave 1,795 two-sided events.

Finally, we require that each event have at least one establishment on each side that fulfills the data requirements for our diversion ratio sample, such as non-missing occupation and geographic coordinates. This leaves 1,310 events, which constitute the diversion ratio sample. For 1,242 of them, both firms appear in the labor-supply model, so the diversion ratios between their jobs are available. Figure A.9 shows, by merger year, the number of workers at merging establishments and the share of those establishments that close.

Figure A.9: Merger-Related Closures in Sweden



Notes: Bars give the number of workers at merging establishments by year; the line gives the share of those establishments that close within two years of the merger. An establishment is classified as closed if employment falls to 10% or less of its pre-merger level within two years of the merger, or if it disappears from the register entirely.

A.11 Merger Sample Construction: Matching

We match the treated workers and establishments to observably similar controls at non-merging firms in a matched difference-in-differences design. For workers, our baseline uses the propensity-score matching of Schmieder et al. (2023) that we also apply to displaced workers in Section 5.3.3 (Appendix A.6): within merger year and one-digit industry, we take the single nearest control, without replacement, on a propensity score estimated from pre-event establishment size, log earnings in the two preceding years, education, tenure, and age, with current-year earnings excluded so that pre-period earnings provide a parallel-trends check. Both treated and control workers must have at least three years of tenure at their baseline establishment, and the reference year is the year before the merger. Because these cells are coarse, almost every treated worker finds a control, so the match rate is 99.9%.

As a robustness check we also match following Arnold (2025), on exact cells of four-digit industry, a coarse geography (Stockholm, Gothenburg, Malmö, or elsewhere), gender, a five-year age band, and a firm-size quintile, with a linear propensity score fit from pre-event firm size, firm age, worker age, and local labor market size, again excluding earnings. We depart

from Arnold (2025) in coarsening the firm-size strata from deciles to quintiles, replacing the full set of geographic cells with the four-way split while absorbing the remaining geographic variation through the local labor market size term, lowering the minimum establishment-size threshold, and keeping the acquirer's own workers rather than dropping mergers in which the acquirer is much larger than the target. These finer cells leave 40% of treated workers without a feasible match, so this design pairs only 60% of them, against 99.9% under the baseline. We favor the baseline for its far higher coverage, which matters because the high-diversion group is small. Appendix Figure A.16 shows that the event-study pattern is the same under the Arnold matching.

We match establishments following Arnold (2025). Within cells of four-digit industry, broad region (Stockholm, Gothenburg, Malmö, or elsewhere), and quintiles of employment and of earnings per worker in the last year before the merger, we take the single nearest control establishment, without replacement. The propensity score comes from a linear probability model in employment, payroll, and establishment age, each with its square, an indicator for multi-establishment firms, and the size of the local labor market. Treated and control establishments must have positive employment in each of the four years before the merger, and control establishments are not involved in any merger in our sample. An establishment involved in mergers in several years enters only in its first merger year. We depart from Arnold (2025) in using quintiles rather than deciles and a four-way regional split rather than states, in imposing no minimum of 50 employees, and in keeping mergers in which one firm is much larger than the other.

Table A.10 reports the number of treated workers and establishments in the estimation samples at each diversion ratio cutoff.

Table A.9: Merger Sample: Establishment Characteristics

	Merger establishments	Control	Normalized difference
Panel A: Establishment characteristics			
Payroll (1,000s 2010 SEK)	12,791	11,024	0.04
Employment	43.9	39.3	0.03
Median employment	18	17	–
Earnings per worker (1,000s 2010 SEK)	269	264	0.05
Share target	0.17	–	–
Panel B: Industry of establishment			
Wholesale & retail trade	0.31	0.29	0.04
Health & social work	0.19	0.19	0.01
Real estate & business services	0.09	0.10	−0.03
Finance & insurance	0.08	0.07	0.07
Education	0.08	0.09	−0.06
Construction	0.06	0.05	0.04
Manufacturing	0.05	0.06	−0.03
Accommodation & food	0.04	0.04	0.01
Transport & storage	0.03	0.04	−0.03
Information & communication	0.02	0.03	−0.01
Arts & other services	0.02	0.03	−0.03
Other industries	0.03	0.03	−0.02
Panel C: Characteristics of the merger			
Partner in same commuting zone	0.31	–	–
Partner in same CZ × occupation	0.28	–	–
Partner in same industry group	0.66	–	–
Diversion ratio to merger partner	0.0013	–	–
Share with diversion ratio ≥ 0.02	0.013	–	–
Unique establishments	8,041	5,419	
Unique firms	1,739	3,379	

Notes: Establishments in the matched merger sample whose jobs have a diversion value in the labor-supply model, and their propensity-score-matched controls at non-merging firms, in the last full pre-merger year. A control matched to several merging establishments enters once, so the column counts differ. Industry groups are harmonized across the 2008 SNI revision. Other industries pools groups below 0.02 in both columns. Diversion ratio to merger partner is the worker-weighted mean of the job-level diversion from the establishment's jobs to the partner firm. The normalized difference is the difference in means divided by the square root of the average of the two variances.

Table A.10: Merger Sample by Diversion Cutoff

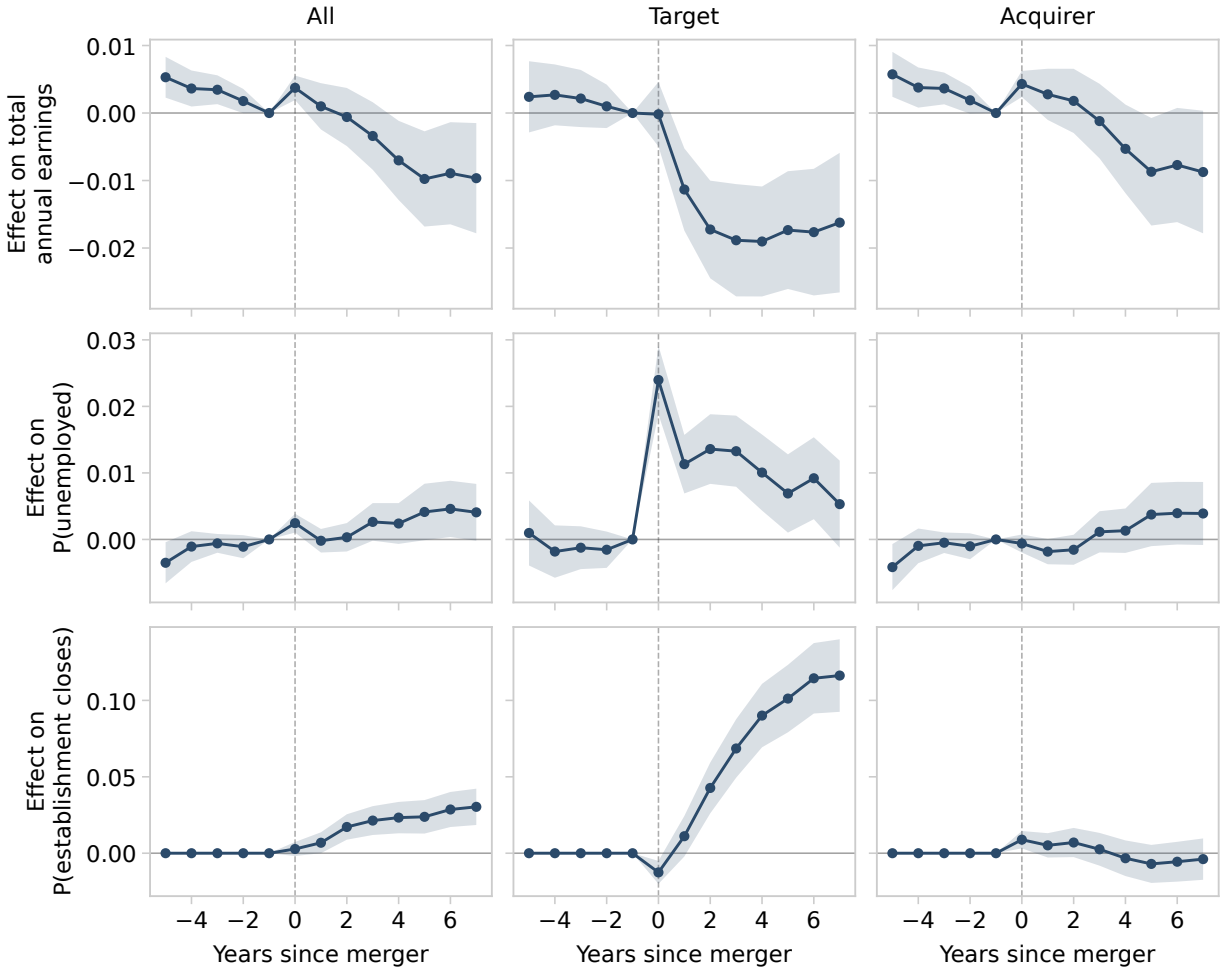
	Workers			Establishments		
	All	Target	Acquirer	All	Target	Acquirer
Diversion ratio < 0.002	218,816	18,032	200,784	4,346	809	3,537
Diversion ratio ≥ 0.002	12,778	3,146	9,632	544	210	334
Diversion ratio ≥ 0.01	2,797	1,107	1,690	191	85	106
Diversion ratio ≥ 0.02	1,842	773	1,069	112	58	54
Diversion ratio ≥ 0.05	857	299	558	52	34	18

Notes: Treated workers and establishments in the merger estimation samples, each paired with one matched control. Workers are classified by the largest job diversion ratio from their baseline job to a single job at the merger partner, as in Figure 6 and Table 4. Establishments are classified by the largest job diversion ratio of any of their jobs, as in Figure 7. The rows with a job diversion ratio of at least a cutoff are nested, and the group whose largest job diversion ratio is below 0.002 is the comparison group at every cutoff.

A.12 Baseline Merger Event Studies

Figure A.10 reports the baseline event studies underlying Section 6: the average effect of a merger on the three main outcomes, with no diversion ratio or market-definition split, for all affected workers and establishments (left column) and separately for the target (middle) and acquirer (right) sides. Earnings fall on both sides, more sharply and immediately for the target, where the drop reaches about 2% against roughly 1% for the acquirer. By contrast, unemployment and establishment closure concentrate on the target side. Target unemployment jumps at the merger year, and the closure probability climbs by about 11 percentage points by year seven, while the acquirer side barely moves on either.

Figure A.10: Baseline Merger Effects, by Side

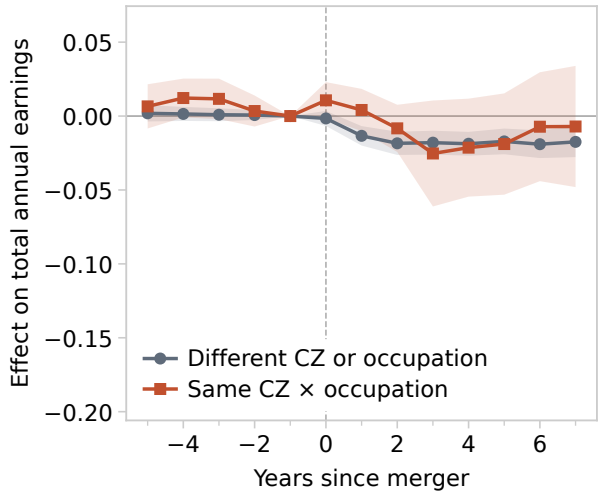


Notes: Matched difference-in-differences event studies of the average merger effect, with no overlap split. Rows are total annual earnings and the probability of unemployment (worker level), and the probability of establishment closure (establishment level); columns are all, target-side, and acquirer-side units. Each line is the treated-versus-matched-control difference; rows share a vertical scale. Earnings are expressed as a share of each worker's earnings at $t = -2$. $t = 0$ is the first year under the new owner, and $t = -1$, the last year under the old owner, is the omitted reference year. Shaded areas indicate 95% confidence intervals.

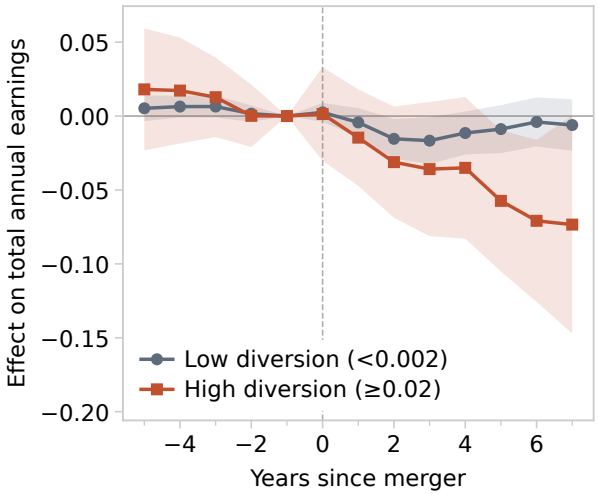
A.13 Merger Effects by Side: Target and Acquirer

The main text pools all workers, and all establishments, at the merging firms. Figures A.11–A.13 reproduce the three event studies separately for the target and acquirer sides, each split by both the diversion ratio and the commuting-zone-by-occupation benchmark. Within every outcome the four panels share a common vertical scale, so the target-acquirer asymmetry is directly visible.

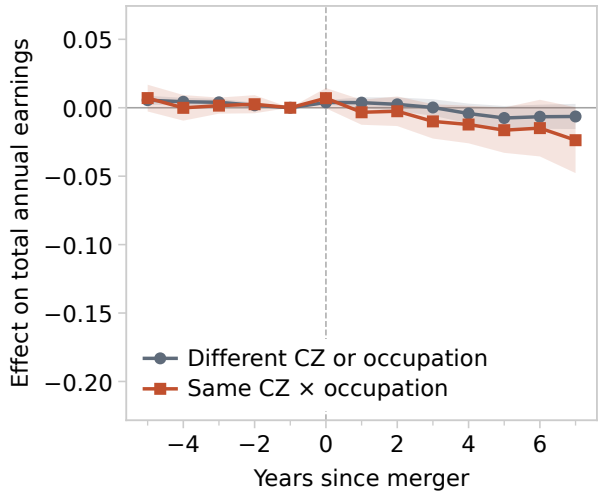
Figure A.11: Merger Effects on Total Annual Earnings, by Side



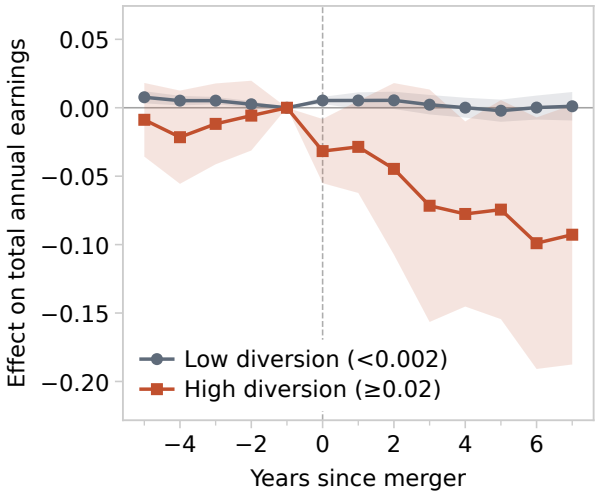
(a) Target, commuting-zone-by-occupation split



(b) Target, diversion-ratio split



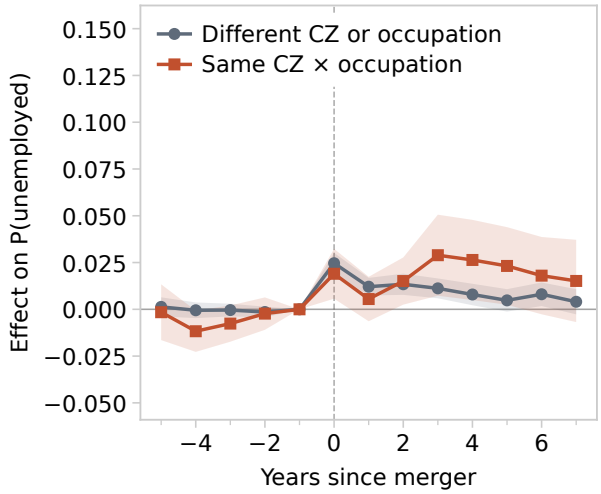
(c) Acquirer, commuting-zone-by-occupation split



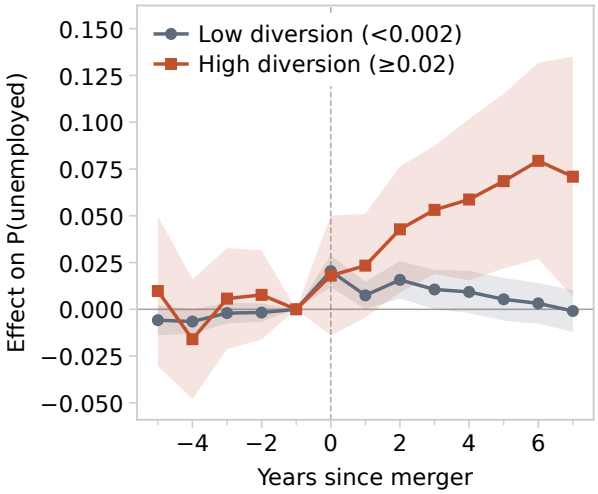
(d) Acquirer, diversion-ratio split

Notes: As Panel (a) of Figures 6 and A.17 (Appendix A.16), estimated separately for target-side (top row) and acquirer-side (bottom row) workers; all four panels share a common y-axis. Within each side the left panel splits workers by commuting-zone-by-occupation overlap with the merger partner and the right panel by job diversion ratio (below 0.002 versus a largest job diversion ratio ≥ 0.02, dropping the intermediate group). $t = 0$ is the first year under the new owner, and $t = -1$, the last year under the old owner, is the omitted reference year. Shaded areas indicate 95% confidence intervals.

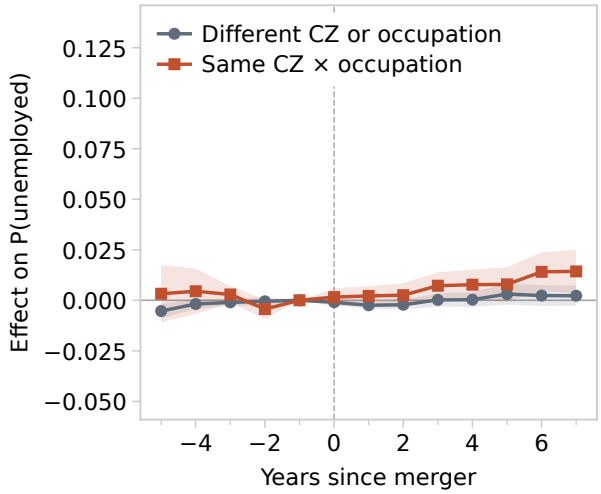
Figure A.12: Merger Effects on Unemployment, by Side



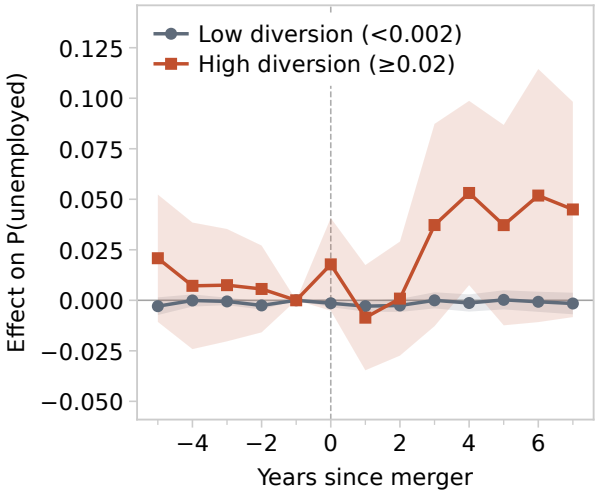
(a) Target, commuting-zone-by-occupation split



(b) Target, diversion-ratio split



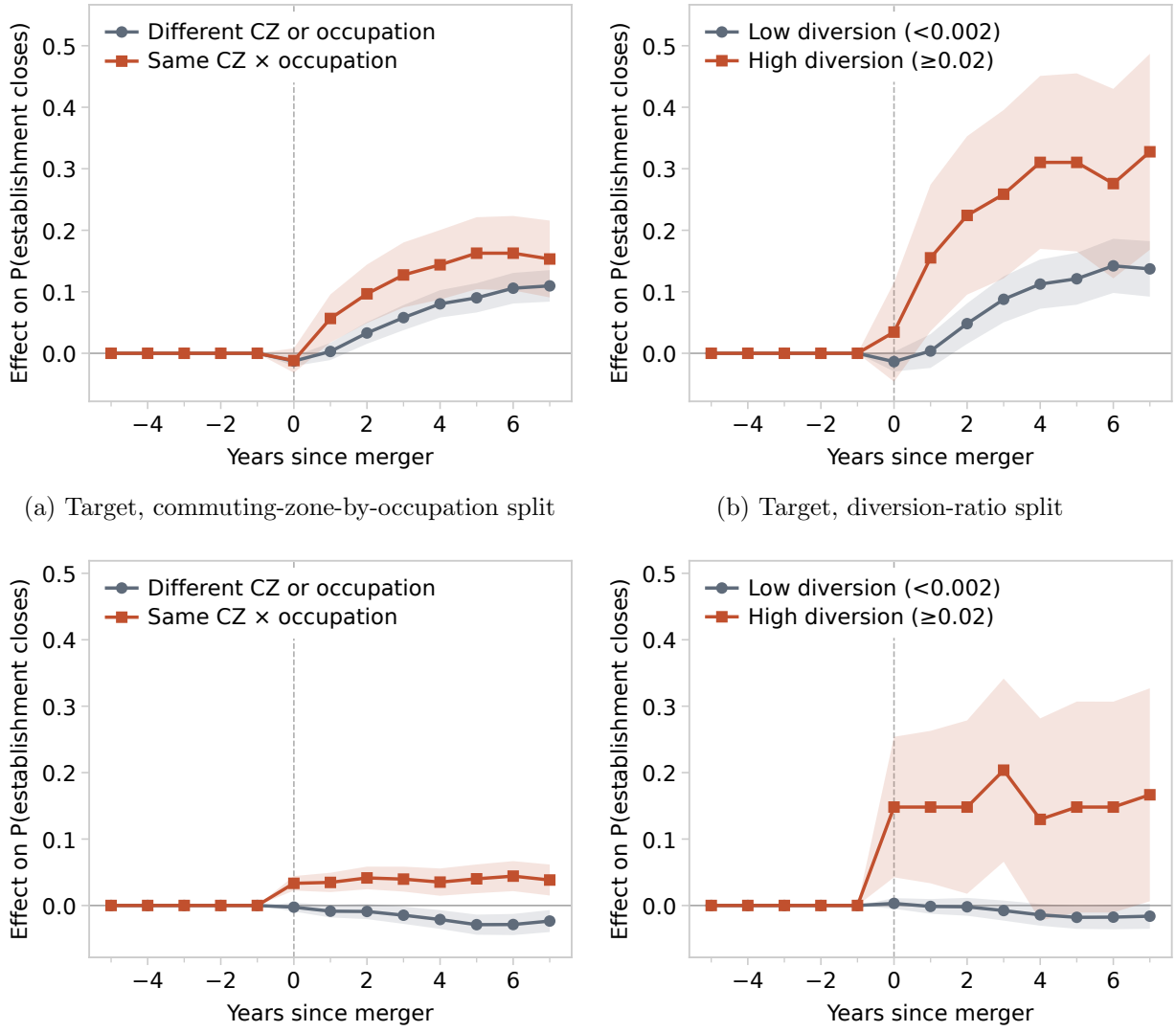
(c) Acquirer, commuting-zone-by-occupation split



(d) Acquirer, diversion-ratio split

Notes: As Panel (b) of Figures 6 and A.17 (Appendix A.16), estimated separately for target-side (top row) and acquirer-side (bottom row) workers; all four panels share a common y-axis. Splits and conventions as in Figure A.11.

Figure A.13: Merger Effects on Establishment Closure, by Side



(a) Target, commuting-zone-by-occupation split

(b) Target, diversion-ratio split

(c) Acquirer, commuting-zone-by-occupation split

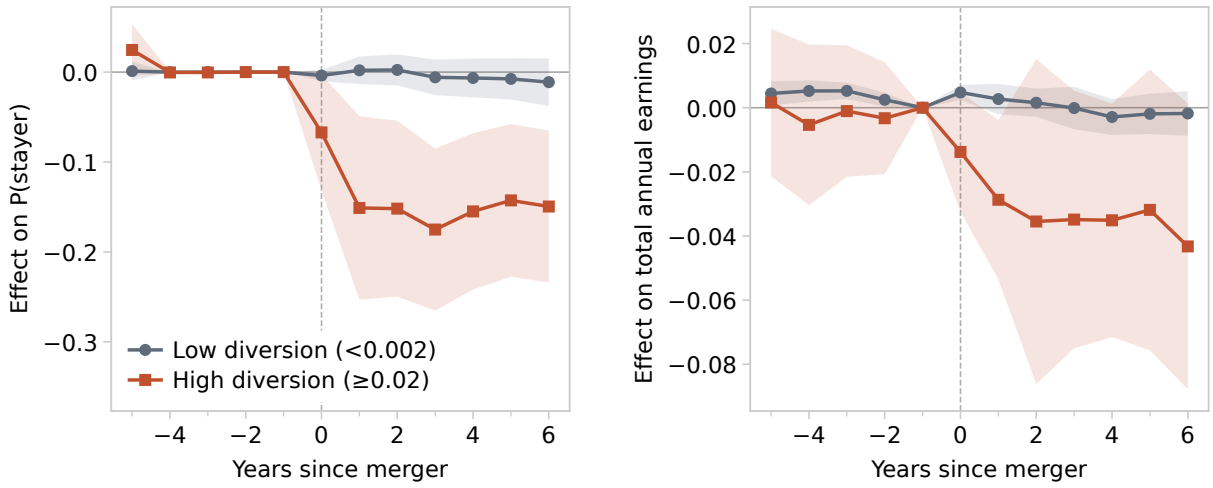
(d) Acquirer, diversion-ratio split

Notes: As Figures 7 and A.18 (Appendix A.16), estimated separately for target-side (top row) and acquirer-side (bottom row) establishments; all four panels share a common y-axis. Splits and conventions as in Figure A.11.

A.14 Merger Effects on Stayers

We define a stayer as a worker employed in year t at the same establishment as in the last year before the merger, adapting the firm-level definition of Arnold (2025) to establishments. Figure A.14 reports the diversion-split event studies for the probability of being a stayer (Panel (a)) and for the total annual earnings of stayers (Panel (b)). From one year after the merger onward, workers in the high-diversion group are between 14 and 18 percentage points less likely than their matched controls to be stayers, while the low-diversion group stays within about 1 percentage point of its controls. Among stayers, earnings in the high-diversion group fall by about 3 to 4 percent relative to control stayers, and earnings in the low-diversion group change by less than 1 percent. The stayer sample is small, with 930 high-diversion stayers three years after the merger, and it is selected, because high-diversion workers leave more often. We therefore read the earnings result as suggestive.

Figure A.14: Merger Effects on Stayers



(a) Probability of staying

(b) Earnings of stayers

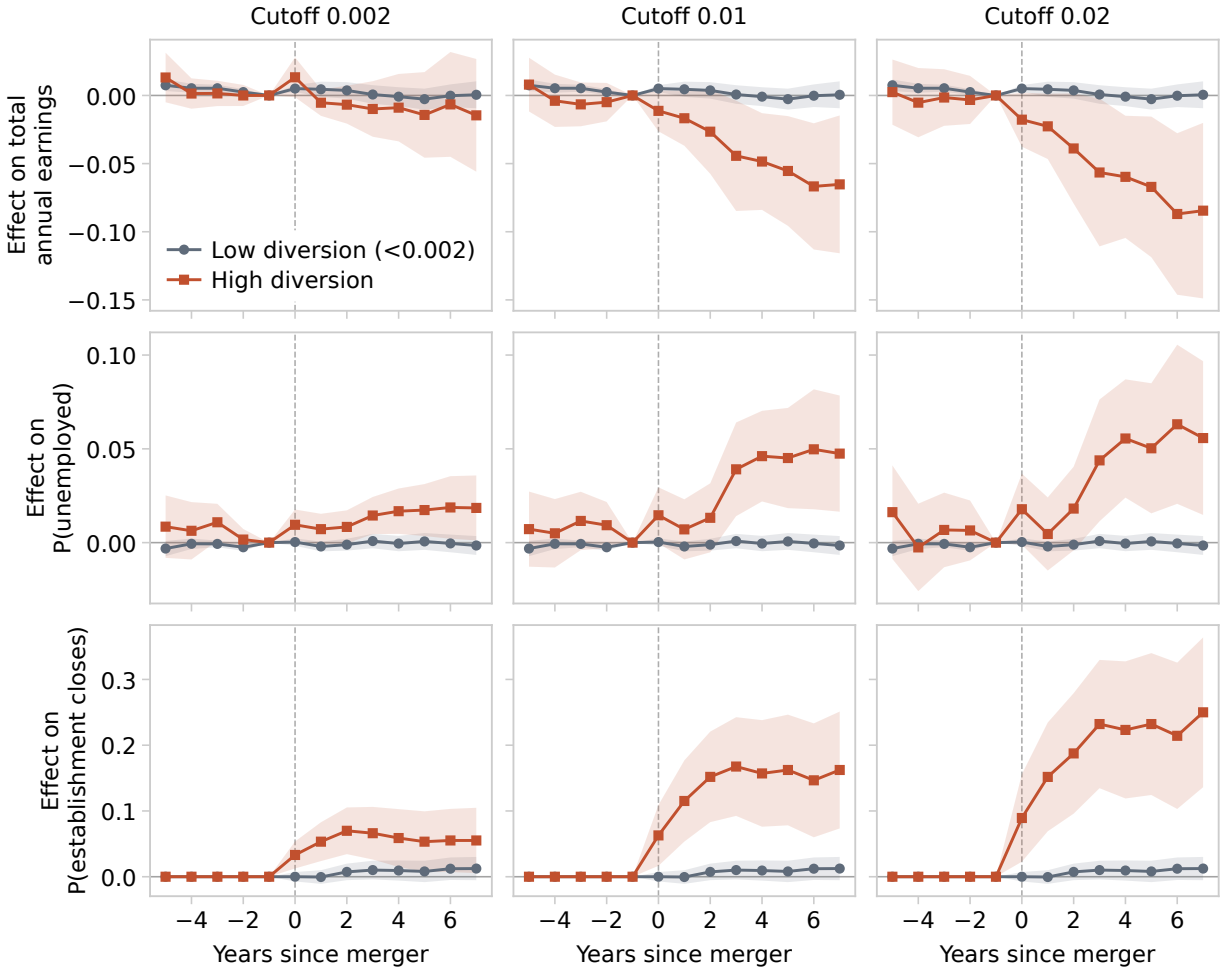
Notes: Event studies around a merger for workers at merging firms, split by job diversion ratios to the merger partner as in Figure 6. Panel (a) plots effects on the probability of being a stayer, defined as employment in year t at the same establishment as in the last year before the merger. Panel (b) plots effects on total annual earnings, expressed as a share of each worker's earnings at $t = -2$, for stayers among both treated workers and controls. Each line is a group's difference-in-differences path from Equation (19), estimated against the group's own propensity-score-matched controls. $t = 0$ is the first year under the new owner, and $t = -1$, the last year under the old owner, is the omitted reference year. Both panels end at $t = 6$ because the earnings estimate for $t = 7$ rests on few stayers. Shaded areas indicate 95 percent confidence intervals.

A.15 Merger Event Study: Robustness to the Diversion Ratio Cutoff

Section 6 classifies a worker, or establishment, as exposed when its largest job diversion ratio to the merger partner is at least 0.02. Figure A.15 re-estimates all three event studies, for all workers and establishments, under cutoffs of 0.002, 0.01, and 0.02 (columns), with the low-diversion group as the comparison throughout. Reading each row from left to right, the harm to the high-diversion group grows as the cutoff rises, so the result is not specific to the 0.02 cutoff. For establishment closure, the baseline enters each establishment only in its first merger year. Entering it in every merger year instead, as in the stacked design of Arnold (2025), yields 147 high-diversion establishments rather than 112 and a closure effect of 20 rather than 23 percentage points five years after the merger.

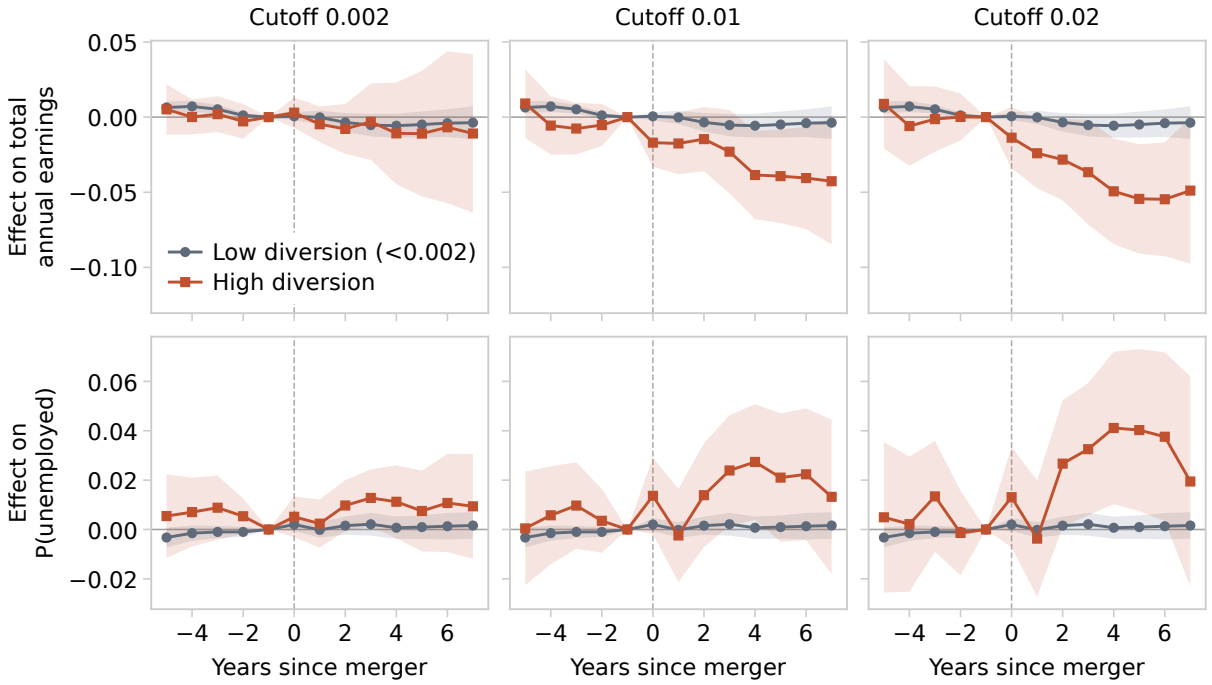
The same dose-response holds under the alternative matching. Figure A.16 re-estimates the earnings and unemployment event studies for all workers under the Arnold (2025) matching, which pairs only 60% of treated workers against the baseline's 99.9%, at cutoffs of 0.002, 0.01, and 0.02. Despite drawing on a very different, and smaller, set of controls, the pattern is the same: the high-diversion group's earnings fall and its unemployment rises, both deepening as the cutoff tightens, so the result is not an artifact of the baseline controls.

Figure A.15: Merger Effects by Diversion Ratio Cutoff



Notes: Diversion-ratio event studies for all workers (total annual earnings, unemployment) and all establishments (closure) at merging firms, by the diversion ratio cutoff. Each panel compares the low-diversion group to the workers (establishments) whose largest job diversion ratio to the merger partner is at least the column cutoff (0.002, 0.01, or 0.02), dropping the intermediate group; the low-diversion comparison group is common across columns. Rows share a vertical scale. Earnings are expressed as a share of each worker's earnings at $t = -2$. $t = 0$ is the first year under the new owner, and $t = -1$, the last year under the old owner, is the omitted reference year. Shaded areas indicate 95% confidence intervals.

Figure A.16: Merger Effects by Diversion Ratio Cutoff, Arnold Matching

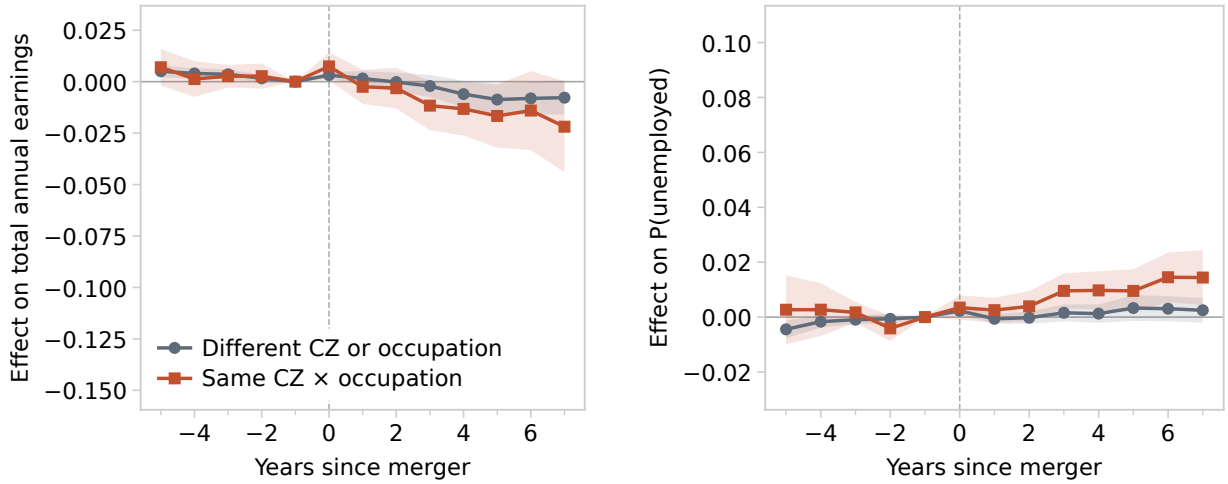


Notes: Diversion-ratio event studies for all workers (total annual earnings and unemployment) at merging firms under the matching of Arnold (2025), by the diversion ratio cutoff. Each panel compares the low-diversion group (largest job diversion ratio below 0.002) to the workers whose largest job diversion ratio to the merger partner is at least the column cutoff (0.002, 0.01, or 0.02), dropping the intermediate group; the low-diversion comparison group is common across columns. Rows share a vertical scale. $t = 0$ is the first year under the new owner, and $t = -1$, the last year under the old owner, is the omitted reference year. Shaded areas indicate 95% confidence intervals.

A.16 Commuting-Zone-by-Occupation Splits

This appendix collects the commuting-zone-by-occupation splits for earnings, unemployment, and establishment closure, all discussed in Section 6.

Figure A.17: Merger Effects: Commuting-Zone-by-Occupation Split

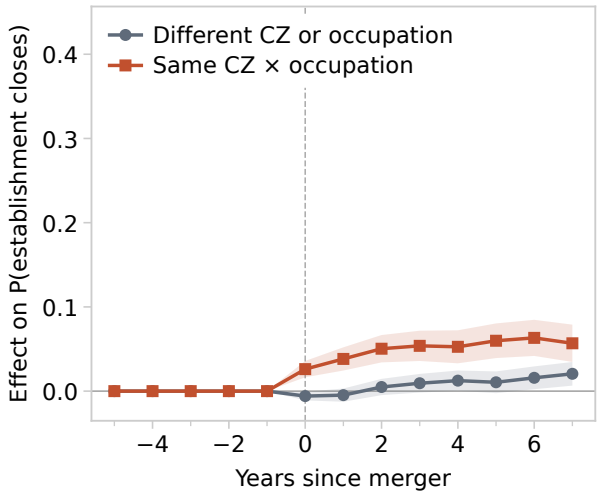


(a) Total annual earnings

(b) Unemployment

Notes: Event studies around a merger for all workers at merging firms relative to propensity-score-matched controls at non-merging firms (matching following Schmieder et al. (2023); see Appendix A.11). Panel (a) plots effects on total annual earnings, expressed as a share of each worker's earnings at $t = -2$, and Panel (b) effects on the probability of an unemployment spell. $t = 0$ is the first year under the new owner, and $t = -1$, the last year under the old owner, is the omitted reference year. The high- and low-overlap groups' difference-in-differences paths are estimated separately for each group against its own matched controls, splitting workers by whether the merger partner operates a job in their commuting zone and occupation. Shaded areas indicate 95% confidence intervals.

Figure A.18: Merger Effects on Establishment Closure: Commuting-Zone-by-Occupation Split



Notes: Event study of the probability that an establishment closes following a merger, for all establishments at merging firms relative to propensity-score-matched control establishments (matching following Arnold (2025); see Appendix A.11). $t = 0$ is the first year under the new owner. Closure is zero before the merger by construction, so all pre-merger years form the reference period. The figure plots the high- and low-overlap groups' difference-in-differences paths from the establishment-level analogue of the worker design, estimated separately for each group against its own matched control establishments. Establishments are split by whether any of their jobs shares a commuting zone and four-digit occupation with a job at the merger partner. Shaded areas indicate 95% confidence intervals.