The Effect of Public Sector Relocations on Regional Development in Germany

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Can governments create private sector jobs in lagging regions by relocating public sector employees? I study a program moving about 5,200 public sector jobs from thriving cities to lagging towns in one of Germany's largest states. Using linked employer-employee data, I find that public sector jobs move, but incumbent workers largely do not. I estimate place-level effects using event studies with entropy balancing for receiving locations and synthetic controls for sending locations. Receiving locations gain about 1.7 private jobs per additional public job. Sending locations also gain private employment. A quantitative spatial model rationalizes these effects through public-to-private productivity spillovers. A 10% increase in nearby public employment density raises the productivity of private firms by about 0.4%.

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1 Introduction

Can governments narrow regional disparities in economic opportunities by moving public sector jobs from thriving to lagging regions? Governments have long paid subsidies to firms to locate in targeted lagging regions. Countries such as Germany and South Korea are now moving public sector jobs to these regions to stimulate local economic development instead. [1] In 2019, Germany's public wage bill amounted to 8.1% of GDP (Eurostat 2026b), whereas federal and state spending on the country's main place-based policy program[2] amounted to 0.03% of GDP (German Bundestag 2021; Eurostat 2026a). Because political and administrative power has historically been centralized, much of this wage bill is concentrated in national and subnational capitals (Galiani and Kim 2008; Dascher 2000). Relocating just 1% of the German public wage bill would move about €2.9 billion a year across space. How redistributing public employment as a place-based policy affects the lagging regions that gain this wage bill and the thriving regions that lose it is therefore a first-order policy question.

My paper answers this question with five contributions by studying an ongoing public employment reallocation policy in Bavaria, in the south of Germany. The policy is moving 5,196 public sector jobs and 1,170 apprenticeships from thriving to lagging regions in two phases from 2015 to 2030. The jobs alone equal roughly 1.7% of the state's pre-policy direct workforce.[3] I examine *worker-level* and *place-level adjustment* in both *locations gaining and locations losing public jobs*, analyze the policy's *channels and welfare effects in general equilibrium*, and extend a quantitative spatial model to incorporate the *public sector's contribution to agglomeration economies* and its use of *floor space*.

I trace worker adjustment using the Federal Employment Agency's Integrated Employment Biographies, which link workers to establishments over time. Building on Goldschmidt and Schmieder (2017), I use establishment and worker identifiers to follow employment through changes in public employers. I combine public administration industry codes with the workplace postcodes of the program's origins and destinations to identify public sector agencies that close or substantially contract and agencies that open or substantially expand. I follow the original agencies' 2014 workforce to 2024, distinguishing workers who move, commute, remain employed elsewhere without changing residence, or leave regular employment. I then identify workers hired from outside the original workforce and compare the characteristics and wages of employees at closed and newly opened agencies. These comparisons describe how the workforce adjusts and how recruitment changes across locations.

To estimate place-level effects, I use the policy program's pre-announced eligibility criteria to construct control groups. Every municipality within the policy state is

[1] See Freitas (2026) for a cross-country analysis of past and current public employment relocation programs.

[2] The Gemeinschaftsaufgabe Verbesserung der regionalen Wirtschaftsstruktur (GRW), or Joint Task for the Improvement of Regional Economic Structures.

[3] These figures describe intended final staffing. Phase I's scheduled implementation period ended in 2025, although staffing continued to build up. Phase II is still being implemented (Bavarian Ministry of Finance 2026).

benchmarked against the state average on a socioeconomic distress index. Municipalities scoring below 90% of the state average are classified as lagging and become eligible to receive relocated public agencies. I reproduce the index for municipalities elsewhere in Germany and compare receiving municipalities with entropy balancing controls among municipalities that would have been eligible had they been located in the policy state. I compare sending municipalities with synthetic controls built from municipalities outside the policy state.[4] I combine a self-collected dataset of relocated agencies and the state's official public-agency register with a yearly municipal employment panel constructed from the Federal Employment Agency's linked employer-employee records for 2007 to 2024. I compare employment in sending and receiving municipalities with their controls before and after the relocations.

I use a quantitative spatial model to evaluate the policy's channels and welfare effects throughout Germany, including regions that neither send nor receive public jobs. The model distinguishes productivity spillovers within the private sector from those between the public and private sectors and accounts for competition between public agencies and private firms for commercial floor space. These channels help explain why private employment rises both in locations that receive public jobs and in locations that lose them. In the model, the government assigns public jobs to workplaces, public employees choose where to live and commute, and public agencies occupy commercial floor space. The model distinguishes agglomeration economies within the private sector from productivity spillovers between the public and private sectors. At the origins, an agency's departure removes public-to-private spillovers but releases space for private activity. At the destinations, the agency generates these spillovers but competes with private firms for commercial floor space. I match the model to the reduced-form private employment responses to quantify these competing forces.

My worker-level results show that public sector jobs move but public sector workers largely do not. Few incumbents change residence to follow their jobs, while about 10% work at the new locations without moving. Many remain employed near their previous residence, typically at another public agency, while others leave regular employment, largely through retirement. As the baby-boom generation retires, the policy state shifts its hiring from urban centers to small towns where the state's uniform pay schedule buys more qualified workers. Public sector agencies opening in these towns recruit almost entirely from outside the original workforce. Compared with the former sending workforce, their employees are younger, more often women, more educated, and more often in specialist and expert jobs, yet earn less per day. Many already have public sector experience. The program thus brings comparatively well-paid public employment to destination towns while changing who fills these jobs.

I now turn to the place-level results. Relocating public employment raises private employment in receiving locations. The estimates imply about 1.7 additional private jobs per public job. Private jobs build up slowly in receiving locations, with large gains five to six years after the first relocation, and rents and purchase prices follow people with the

[4] I additionally exclude municipalities with the strongest commuting links to the policy state from the control pool.

same lag. Residents fill the new local jobs, and in-commuting does not rise. Rents rise by 5 to 7%, against private wage gains of 1 to 2.5%, suggesting that landowners capture a large part of the gain. Employment effects around receiving locations are localized. Population gains spill over to towns 10 to 30 minutes from receiving locations. Because every receiving location is in the policy state and every control is outside it, a change specific to the policy state that coincides with treatment years could appear as an effect of the relocations. However, applying triple differences to remove the trend in the policy state from outcomes strengthens the gains in total workplace employment, employment density and net migration of young adults in receiving locations.

Relocating public employment also raises private employment in locations that lose public jobs. Munich's employment density increases by 3.6 workers per 100 inhabitants after the relocations compared with its synthetic control. The number of jobs relocated may be small relative to its labor market, but almost all relocated agencies occupied scarce office space in the city center. The relocations may free up this space for more productive private sector firms, helping explain the rise in employment density despite the loss of public jobs. The government estimates that the program's first phase alone freed about 37,000 square meters of office space, mainly in the state capital (Bavarian Ministry of Finance 2026). Removing the trend in the policy state raises Munich's employment density gain from 3.60 to 5.31 workers per 100 inhabitants.

Matching the model to the employment response in receiving municipalities yields an estimated public-to-private productivity elasticity of 0.0375. This elasticity is the contribution of the government to agglomeration. For a receiving municipality, a 10% increase in surrounding public employment density, weighted by proximity, raises private firms' productivity by about 0.36%, holding other determinants fixed. The model separates the consequences of these spillovers from those of reallocating commercial floor space. The program redistributes economic activity at a small national output cost. Private output in Germany falls by 0.010%, and welfare rises by 0.0045%. Among eligible destinations, the program's choices come close to the most welfare improving allocation. 37 of the 39 receiving towns raise welfare, and replacing the two that lower it with the nearest untreated eligible towns raises the welfare gain by 11%.

This paper contributes to the literature on worker adjustment to employer shocks, public employment as a place-based policy, equal public sector pay and unequal recruitment quality and the spatial allocation of economic activity.

My first contribution is to show how the workforce adjusts when public employers relocate. Gathmann et al. (2020) find that mass layoffs cause persistent local employment losses, while initially employed workers suffer smaller losses. Geographic mobility protects younger workers in that initial workforce. I distinguish the adjustment of the original agency workforce from recruitment at receiving agencies. Goldschmidt and Schmieder (2017) find wage losses of about 10–15% after outsourcing, associated with the loss of firm-specific rents. In my setting, average public workplace wages decrease at receiving locations and increase at sending locations. The worker records show that receiving agencies recruit a lower-paid workforce, while many incumbents remain at sending locations and earn higher nominal wages. I follow the workers employed at relocated agencies and show that public sector jobs move but public servants largely do not. Few

incumbents change residence, and receiving agencies hire mostly from outside the original workforce.

My second contribution is to the literature on place-based policies, which asks whether spatially targeted interventions improve economic outcomes in lagging regions (Moretti 2012; Kline and Moretti 2014b; Ehrlich and Seidel 2018; Duranton and Venables 2018; Ehrlich and Overman 2020). Most of this work studies subsidies, tax incentives, and infrastructure investment. Relocating public sector jobs differs from these instruments because the government moves its own activity rather than altering private firms' incentives. The closest literature to my work studies how public employment affects private employment, using expansions, contractions, and relocations of the public sector (Faggio and Overman 2014; Senftleben-König 2014; Jofre-Monseny et al. 2020; Auricchio et al. 2020; Freitas 2026). Much of the relocation evidence concerns large interventions concentrated in a single destination, such as the German government's moves to Bonn and back to Berlin and the BBC's move from London to Salford (Becker et al. 2021; Faggio, Schlüter, et al. 2025; Nathan et al. 2024). In another study (Freitas 2026), I review public employment decentralization programs and find gains of about 7 private jobs per 10 relocated public jobs, concentrated in non-tradable activities (ibid.). This paper studies public employment relocation as a place-based policy targeting many lagging locations. It is therefore closer to studies of programs targeting multiple locations in the United Kingdom, South Korea, and Denmark (Faggio 2019; Lee et al. 2024; Jensen et al. 2026). I add evidence from a program whose eligibility rules were announced before implementation, and I estimate effects in the municipalities that send the jobs as well as in the lagging municipalities that receive them.

My third contribution concerns recruitment under uniform public sector pay across different local labor markets. Propper and Van Reenen (2010) and Britton and Propper (2016) show that stronger private outside wage opportunities worsen hospital and school performance under nationally regulated pay in England. Dal Bó, Finan and Rossi (2013) find that higher public wages attract more able applicants without reducing public-service motivation and help fill positions in less attractive municipalities. Di Porto, Dustmann, Giannetto and Incoronato (2026) show that salary caps induce disproportionately able public managers to leave where private sector opportunities are available. I add the location of public employment as a policy margin. When administrative functions can serve users from different locations, the government can change the labor markets in which it recruits while retaining a common pay schedule. My findings suggest that relocation can strengthen the state's position relative to competing employers, although access to specialized workers may constrain recruitment in smaller labor markets.

My fourth contribution is to bring the public sector into quantitative spatial models (Ahlfeldt, Redding, et al. 2015; Redding and Rossi-Hansberg 2017). Earlier work models public employment in spatial equilibrium in a two-city economic geography model of Bonn (Becker et al. 2021), in a search model of the average Spanish city (Jofre-Monseny et al. 2020), and in models of public sector pay and local public goods (Guillouzouic et al. 2021; Guillouzouic et al. 2024). Rothenberg et al. (2025) study the relocation of Indonesia's capital in a dynamic quantitative spatial model with public employment. I build a quantitative spatial model in which the government assigns public sector jobs

to workplaces, public employees choose where to live and commute, and public agencies occupy floor space. A novel feature of the model is that public agencies occupy floor space, allowing me to quantify how their relocation affects real estate markets and private activity. Matching the model to a quasi-experiment, I estimate agglomeration economies between the public and the private sector separately from those within the private sector, and find them to be small. In their two-city model of Bonn, Becker et al. (2021) find positive amenity spillovers from public employment, while the fitted productivity spillover to the private sector is close to zero and slightly negative.

My fifth contribution is to the debate on whether place-based policies trade efficiency for equity (Kline and Moretti 2014b; Kline and Moretti 2014a; Busso et al. 2013; Gaubert et al. 2025). Relocating public employment may be a case in which a place-based policy removes a misallocation instead of creating one. Public employment is concentrated not only in capitals but also at their centers. The location of city hall is even a common proxy for a city's central business district (Holian 2019). Political accountability benefits from capitals that are close to their citizens (Campante and Do 2014), but many administrative jobs need neither proximity to political decisions nor proximity to the people they serve.[5] Faggio (2019) describes such jobs as providing tradable public services. They resemble the tradable private sector more than the usual view of the public sector suggests. Yet the literature mostly studies the size of the public sector or the provision of public goods (Alesina et al. 2001; Algan et al. 2002; Guillouzouic et al. 2021). The program I study moves the state's back office, such as tax processing, payroll, land registries, geodata, and IT services, out of thriving centers. These functions serve users statewide without face-to-face contact. Many of these jobs occupy prime locations, the hyper-dense clusters of economic activity where commercial rents and productivity peak (Ahlfeldt, Albers, et al. 2026; Ahlfeldt, Heblich, Seidel, and Yin 2026). By moving these jobs out of prime locations and making room for more productive private firms, the program may correct a historical misallocation of this type of back office public employment.

Section 2 describes the policy program and Section 3 the data. Section 4 shows how the workforce adjusts when agencies relocate. Section 5 presents the empirical strategy and Section 6 the effects in receiving and sending municipalities. Section 8 develops the quantitative spatial model and evaluates the relocations for the economy as a whole. Section 12 concludes.

2 Institutional Setting

The public sector is a major employer in Germany and accounts for a substantial share of government expenditure.[6] By choosing where to locate public jobs, governments can

[5] Studying cuts in Italian public employment, which includes local public good providers such as schools and health services and has long served as a regional subsidy (Alesina et al. 2001), Auricchio et al. (2020) find that an inflated public sector crowds out private employment, mostly in tradable sectors.

[6] According to the OECD, general government employment accounted for approximately 11% of total employment in Germany, 15% in the United States, 21% in France, and 17% in the United Kingdom in 2021 (OECD 2023, Figure 12.1). In the same country order, the World Bank reports wage bills of 17.4%, 25.8%, 22.1%, and 23.4% of public expenditure in 2019 (World Bank 2024).

direct employment and recurring wage income toward disadvantaged places. The state of Bavaria used this instrument to address spatial inequality within its borders.

Economic opportunities differ sharply across Bavaria. Wealthy municipalities around Munich and in the south contrast with struggling areas in the north and along the Czech border.[7] The state thus faces a familiar regional-policy problem. The question is how to bring jobs to places that have fallen behind its economic centers. In 2015, Bavaria began reallocating public employment from its major agglomerations to these places as part of its *Heimatstrategie*, a regional development strategy.

To identify eligible destinations, the government benchmarked all municipalities within the state in terms of economic and demographic performance against the state average of an index. This index consists of five indicators of population forecasts, unemployment, employment density, income, and migration among young adults. Municipalities whose index falls below 90 form the eligible set for the reallocation policy. A municipality qualifies through either its own index value or its district's index value, with a threshold of 90 relative to a state average of 100 (Bavarian Ministry of Finance 2015). Panel (a) of Figure 7 in Appendix A shows the municipalities meeting these eligibility criteria. Appendix A reports the index components and their construction.

Within the eligible set, the government favored towns officially designated as service centers for surrounding communities in the state's regional development plan and places that had not already received substantial support through other place-based higher-education policies (Bavarian Ministry of Finance 2025). Panel (b) of Figure 7 in Appendix A adds the service-center designations. Figure 1 shows the selected destinations and origins. Some selected destinations lie outside the index-defined eligible set.

The agencies selected for relocation perform functions that are not tied to local public good provision. Tax processing, information technology, and other specialized administrative functions can serve the same users from a different municipality. Ordinary schools and local police stations are therefore excluded from the relocation program (Kaltenhauser 2023). The program includes whole and partial agency moves, as well as the establishment or expansion of offices. For agency moves, Munich is an important origin. Relocating an office shifts public jobs away from the state's largest employment center while adding them to a receiving town.

I study both phases of the public employment reallocation program. Together, they are moving 5,196 jobs and 1,170 apprenticeships, equivalent to about 1.7% of Bavaria's pre-policy direct state workforce when counting jobs alone.[8] I assembled a relocation dataset that records agencies, destinations, origins, staffing, and implementation dates. Appendix A documents the program's administrative components and intended final staffing.

[7] Before the program, average annual wages in high-wage municipalities, concentrated around Munich and Starnberg, were more than twice those in low-wage municipalities, concentrated in Upper Franconia and Lower Bavaria (Bavarian Statistical Office 2015).

[8] The government's March 2026 statement reports intended final staffing of 2,511 jobs and 730 apprenticeships in phase I, and 2,685 jobs and 440 apprenticeships in phase II. Phase II remains under way, with implementation extending to 2030 (Bavarian Ministry of Finance 2026). The employment benchmark uses the direct state workforce before the program. Appendix A gives the calculation.

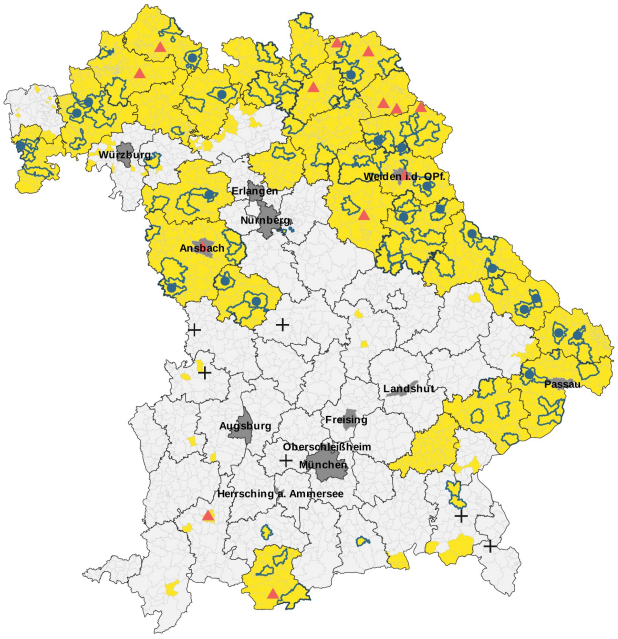


Figure 1. Receiving and sending locations. Blue circles mark service centers, red triangles other municipalities, and black crosses municipalities outside the index-defined eligible set. Sending locations appear in gray throughout.

Most workers in Bavaria's direct state administration are civil servants. Before the program, civil servants and judges accounted for 68.4% of this workforce. The remaining workers held employment contracts (Statistical Offices 2014; Bavarian Statistical Office 2014). Civil servants have a distinct legal status and are absent from the social-security employment records used in the empirical analysis[9].

Public base pay does not vary across the policy state's municipalities for a given position and experience level.

Agencies build up employment at their destinations gradually. An office may open with a small team, recruit additional staff, and move into permanent premises only later. Positions can shift as workers leave or retire at the origin, allowing employment at the destination to grow over several years (Bavarian Ministry of Finance 2026). I date treatment to the government press release announcing an office's opening. This convention captures responses that occur while staffing is still being built up, and the event studies allow me to check for responses before the announced opening.

Employees do not have to move when their agency relocates. The personnel framework rules out compulsory permanent transfers, relocation-related dismissal, and involuntary downgrading. Incumbents can therefore stay, commute, or move, while agencies can also recruit at the destination. Public jobs therefore need not bring an equal number of new residents. Section 4 analyzes how these conditions are translated into realized worker

[9]Municipal civil-service employment data from the statistical offices will allow me to extend the analysis to both groups of public employees in the future.

responses.

Relocations also change the use of office space. Receiving agencies rent, adapt, or construct premises, often before reaching full staffing. At the origins, the government estimates that phase I freed about 37,000 square meters of office space, mainly in Munich, the state capital (Bavarian Ministry of Finance 2026). Vacated premises create scope for other uses, including private-sector offices. Sending locations thus lose public jobs while potentially releasing space for other employers.

3 Data

Studying public employment relocation requires knowing where agencies move and what happens to the people and firms around them. I assemble a record of the Bavarian program, use successive editions of the Bavarian public-agency register to document agency locations, and measure employment with linked employer–employee data from the Institute for Employment Research (IAB). The empirical analysis follows municipalities over time. The same records provide the baseline for the quantitative model, which I quantify for all German municipalities.

3.1 Data on the Relocation Program

I construct the relocation dataset by combining official program records. Starting from the BayernAtlas records, I collect the agency name, responsible ministry, origin and destination, intended staffing, program phase, and implementation status (State Office for Digitization, Broadband and Geodetic Measurement 2024). I then follow each project through the annual *Heimatstrategie* reports, government announcements, agency websites, and local newspaper coverage. These sources turn a list of planned relocations into a record of where the policy was implemented and when employment could begin to change.

I complement these project histories with a new source for studying the geography of public employment. This source is the Bavarian public-agency register (Bavarian State Statistical Office 2026). Its records identify agencies and their addresses, administrative affiliations, municipality codes, and headquarters or branch status. The register and IAB data serve separate purposes. The former documents agency locations, while the latter measures employment at the establishment level. Appendix B describes the registry reconciliation.

3.2 Employment Data

The main employment source is the IAB's Integrated Employment Biographies (IEB), from which I use linked worker and establishment records for 2007–2024. The IEB combine employers' social-security notifications with administrative records on benefit receipt, job search, and labor-market programs (IAB FDZ 2026; IAB 2017). This source offers a combination essential for the analysis. It provides broad coverage of insured employment, detailed locations, and worker histories that continue across employers. It

allows me to study small local labor markets and to connect changes in local employment with the movements of individual workers.

The records distinguish the municipality where a person works from the municipality where that person lives. I use this distinction to construct annual workplace and residence employment measures and to observe commuting across municipal boundaries. The same person and establishment identifiers link employment spells over time. The records also report industry, earnings, education, and demographic characteristics. These dimensions make it possible to ask whether employment grows around a receiving agency, whether local residents benefit, and whether the adjustment involves changes in residence or commuting. County-level aggregates provide the corresponding measures for the model comparison.

Industry and employment status answer different questions. Industry codes identify public administration, defense, and social security, but do not identify every publicly owned employer. Public schools and hospitals, for example, fall in other industries. Civil servants are also outside the IEB employment records because their jobs are exempt from social-security contributions, so relocated civil-service positions are not observed in the employment data. Self-employment is not covered either.

The worker histories also provide evidence on how agencies adjust their workforce.

I then use worker identifiers to follow employees of candidate sending establishments in 2014 through 2024. Section 4 presents this evidence. Appendix B gives the sample definitions, including the treatment of postcodes that occur at both ends of the program.

The model uses employment from the same records in 2014, the year before the first relocation.

3.3 Matching Covariates

For matching, I assemble the indicators used to assess regional disadvantage. These are the population outlook, unemployment, workplace employment density, income per taxpayer, and net migration of young adults. I use administrative data from the German Regional Database and Federal Employment Agency, together with the government's index documentation, to reproduce the policy benchmark outside Bavaria (German Statistical Offices 2026). I also account for municipal size and pre-policy outcome trajectories.

3.4 Travel Costs

Commuting connects the local effects of the policy across space. I use origin–destination flows to measure these connections and road travel times to characterize the cost of reaching a workplace. I compute road travel times with OpenStreetMap routing through OSRM.

3.5 Rents, Housing Prices, and Commercial Floor Space

To measure the price of residential space, I use the micro-geographic rent indices of Ahlfeldt, Heblich, and Seidel (2023).

Office rents provide a separate measure of the cost of space used by firms. I use the municipal office-rent series associated with Ahlfeldt, Heblich, Seidel, and Yin (2026). Developable land in each municipality comes from the CORINE 2012 land-cover data.

4 Worker Adjustment and Public Capacity

The program moved public jobs, not public workers. As the baby-boom generation retired, Bavaria shifted its rehiring from urban centers to small towns where its uniform pay buys more qualified workers. The new public sector agencies are staffed almost entirely by new employees who are younger, mostly women, more educated, and more often in specialist and expert jobs, yet earn less per day. Destination towns gained a stream of stable, well-paid public jobs that locals, especially women, can take without commuting to the city or leaving.

4.1 Tracing Public Sector Agencies and Their Workers

Linked employer-employee records at the postcode level let me locate public sector agencies that closed and opened and follow workers through the relocation.

Appendix B gives the details.

I follow the 2014 workforce of the closed and shrinking public sector agencies to 2024 and compare it with the workforce that replaced it, distinguishing six groups.

To compare the old and the new workforce, I contrast the workers of closed public sector agencies in 2014 with those of opened public sector agencies in 2024. The 2014 averages of the sending and receiving municipalities serve as local benchmarks. Appendix C shows in detail which sectors stayers moved into and which sectors new employees came from, and the public sector agency profiles as figures.[10]

4.2 Worker Adjustment

Few incumbents followed their jobs. Most kept working near their previous place of residence or left regular employment, largely through retirement.

Only 1.8% of incumbents moved their place of residence and work at a destination public sector agency in 2024, and another 10.3% work at the new locations without having moved (Table 1). In total, incumbents make up 0.9% of the destination workforce.

Following was a choice between two equally paid jobs. Followers keep their pay grade and experience step, and the personnel framework rules out compulsory transfers

[10]The public sector agency groups count public administration workers, while the municipal benchmarks count all sectors, so each gap partly compares public administration with the whole economy. Opened public sector agencies are measured in 2024 and the benchmarks in 2014, so any general shift toward specialist and expert jobs over the decade may explain part of the gaps. Civil servants are not in the IAB social-security data, so the public sector agencies' expert shares are understated and their skilled shares overstated, and this bias may differ between closed and opened public sector agencies. Closures and openings may include changes of establishment identifiers or local reorganizations unrelated to the program. The occupation shares cover regular employees with a valid occupation code, which leaves out apprentices and marginal jobs.

Table 1. Worker adjustment and recruitment shares

Worker outcome	Share (%)
Original sending workforce, observed in 2014	
Receiving employment, changed residence	1.8
Receiving employment, unchanged residence	10.3
Employment elsewhere, unchanged residence	40.7
No longer observed in regular employment	40.0
Other or unclassified outcomes	7.2
Receiving workforce, observed in 2024	
Workers from outside the original sending workforce	99.1
Workers from the original sending workforce	0.9

Notes: The first panel reports 2024 outcomes as shares of the 2014 workforce at closed or substantially contracting sending establishments. Residence refers to the recorded municipality of residence. Receiving employment refers to employment at opened or substantially expanding receiving establishments. The second panel reports shares of the 2024 receiving workforce.

(personnel framework, Section 2). Wages could not compensate for a longer commute either. Adjustment therefore had to run through following, switching jobs, moving home, or leaving employment. Leaving was about as common as switching. In 2024, 40.7% of incumbents kept working near home and 40.0% were no longer in regular employment.

Retirement explains much of the leaving.

For those who switched jobs, the origin's public sector absorbed the shock. In 2024, 78% of stayers still worked in the public sector after the relocation (Figure 8 in Appendix C). Especially Munich is a thick public labor market, and no one was legally displaced. After mass layoffs, Gathmann et al. (2020) find that workers under 50 avoid employment losses through geographic mobility. Here, mobility within the local public sector does the same job.

Those who gain are destination residents, a majority of them women. Women hold 64% of the jobs at opened public sector agencies, against 48% of all jobs in the receiving municipalities in 2014 (Table 19). These jobs also pay well by local standards, about 10 euros per day more than the receiving municipalities' average.

4.3 Public Capacity

The retirement wave let the policy state redraw its map of public jobs at low cost. It lost experience that retirement would have taken anyway and moved its hiring to towns where it competes less for skilled workers against the private sector.

The one moment it becomes cheap to redraw the map of public employment is when workers leave voluntarily. The program ran from 2015 to 2030, as the baby-boom generation retires, and positions could shift as workers left or retired at the origin (Section 2). Without layoffs, origin positions had to empty on their own, and an older workforce provided that attrition. The state did not move individual workers. It moved

the place where the next generation is hired.

The new public sector agencies are staffed by a different workforce. Compared with closed public sector agencies in 2014, opened public sector agencies in 2024 employ younger workers, with a mean age of 44.3 against 47.3 years and more workers under 35 (26% against 15%, Table 2). Their staff are more often women (64% against 48%), more educated, and more often in specialist or expert jobs (32% against 20%). Yet they earn 5.2 euros less per day.

Table 2. Closed and opened public sector agencies

	Closed agencies (2014)	Opened agencies (2024)
Mean worker wage (gross EUR/day)	108.7	103.5
Share women	0.48	0.64
Mean education (1–3)	2.16	2.30
Mean age (years)	47.3	44.3
Share under 35	0.15	0.26
Share specialist or expert	0.20	0.32

Definitions follow Tables 18 and 19 in Appendix C.

Part of the pay gap could be explained through a gap in experience. A gap of 3 to 5% in daily wages is consistent with lower seniority, although part-time work, which these records do not separate, could also contribute to it. Opened public sector agencies have more specialist and expert jobs and more young workers, yet lower daily pay, consistent with hiring at low steps of the German public sector pay schedule. Read this way, the gap is the price of lost experience, not of lower grades. The experience was lost because many workers retired, which would have happened without the relocation as well.

New public sector agencies have more demanding jobs in towns where expert labor is less common. Their skill mix is close to that of the large origin cities, with 32% of workers in specialist or expert jobs, against 33% in the sending municipalities and 21% in the receiving municipalities (Tables 18 and 19). Public sector wages are more or less the same everywhere, but local private sector wages differ, and the opened public sector agencies pay about 10 euros per day more than the receiving municipalities' average. By moving its hiring to these towns, the state faces less competition from private employers for skilled workers. What the state gained is a better draw of new staff per euro. The public sector agencies employ experts at 1.6 times the share in their host towns (16% against 10%).[11]

Less competition for the state means more competition for local firms. Whether the public sector agencies pull the best-educated workers out of the local private sector remains open. Answering it requires following the firm side over the long run, once the programs are fully implemented.

[11] Part of the higher expert share may also reflect contract type. If expert posts at new public sector agencies were filled with employees rather than civil servants, which are not observed, the social-security records would show more experts even if the true job mix had not changed.

5 Empirical Methodology

To estimate place-level effects, I use the pre-announced eligibility criteria of the policy program to construct control groups. I use the policy rule to construct comparison groups in the potential outcomes framework (Rubin 1974; Imbens and Rubin 2015). The policy state announced the eligibility rule before the relocations, and the same rule can be computed for every municipality in Germany. I reproduce the policy rule for municipalities outside the policy state and compare receiving locations with entropy balancing controls among municipalities that would have been eligible had they been located in the policy state. I compare sending locations with synthetic controls built from municipalities outside the policy state. Event studies then trace the outcomes of both groups around the first relocation.

5.1 Receiving Locations

The policy rule

The policy rule benchmarks every municipality against the policy state as a whole on a composite index of five indicators (Bavarian Ministry of Finance 2015; StMWi 2014). The indicators measure the population forecast, unemployment, employment density, income and net migration of young adults aged 18 to 29, with weights of 30%, 30%, 10%, 20% and 10%, respectively. Each indicator is expressed relative to its value for the policy state as a whole, and the weighted indicators sum to 100 for the policy state. A municipality is eligible if the index of its district is below 90. A municipality in a district at or above 90 is eligible if its own index is below 90, unless it forms a district of its own.

With the scaling in Equation 16 in Appendix D, my reconstruction reproduces all 79 official district values with a correlation of 0.9997 and a root mean squared error of 0.63 index points, and every district lies on the same side of 90 as in the official list.

The reconstructed policy rule classifies 2,011 of 2,046 municipalities of the policy state as in the official list.

The policy state also favored central places as destinations. These are towns that the regional development plan of the policy state designates as service centers for surrounding communities. Other states designate central places by different rules, so I predict the status a municipality would have under the rules of the policy state.

Cells and donors

The comparison groups follow the policy rule. I define cells by the route to eligibility under the policy rule and by central place status. Receiving locations are compared only with donors outside the policy state in the same cell.

The donors are municipalities outside the policy state. I exclude municipalities in the 20 counties outside the policy state with the strongest commuting links to the policy state to limit exposure to spillovers from the relocations.[12] The donor pool contains 5,164 municipalities, of which 1,071 are central places under the rules of the policy state.

[12] Figure 12 in Appendix D shows these counties.

Entropy balancing

Within each cell, entropy balancing weights the donors so that their weighted means equal the means of the receiving locations in the cell on the five indicators of the municipal index (Hainmueller 2012). Entropy balancing belongs to the matching and weighting estimators of the treatment effect on the treated (Abadie and Imbens 2006; Stuart 2010). All receiving locations in a cell share these weighted donors, but each matched set is anchored at its own first relocation year. A treated location and its weighted controls form a matched set.

Table 3 shows that the weighting works. After entropy balancing, the receiving locations lie within 0.15 standard deviations of their weighted controls on every indicator of the municipal index. The changes before the relocations and the size of the local labor market were not part of the balancing, yet they are also within 0.15 standard deviations.

Variable	Receiving	Weighted controls	SMD
Population forecast term	76.30	77.23	−0.03
Unemployed per 100, 2012	5.42	5.93	−0.15
Employment density, 2012	49.18	49.52	−0.02
Income per taxpayer, 2007	29,334	29,467	−0.02
Net migration of young adults, 2011	−5.32	−4.12	−0.02
Not used in the balancing			
Log population, 2014	9.41	9.26	+0.10
Change in log employment density, 2011 to 2013	0.02	0.04	−0.09
Change in log population, 2011 to 2013	−0.00	−0.00	+0.01

Table 3. Balance after entropy balancing within cells of route to eligibility under the policy rule and central place status, with controls outside the policy state. Entries are means of receiving locations and their weighted controls and standardized mean differences.

5.2 Sending Locations

Why controls within the policy state do not work

Five sending locations are among the eight largest cities of the policy state, and the policy state contains no city of comparable size that is not part of the relocation programs. The donors for sending locations therefore lie outside the policy state.

The sample contains Munich, Freising, Herrsching, Oberschleißheim, Landshut, Erlangen, Augsburg, Nürnberg and Würzburg. Passau is excluded because its district also contains receiving locations, while Ansbach and Weiden are excluded because they switch between sending and receiving.

Synthetic control

Each sending location receives its own synthetic control (Abadie, Diamond, et al. 2010; Arkhangelsky et al. 2021). The synthetic control is the weighted mix of donors whose outcome path best reproduces the path of the sending location before its first relocation.

Table 25 in Appendix D reports the synthetic controls for workplace employment. They reproduce the path of workplace employment before the first relocation exactly for every sending location, with a root mean squared prediction error below 0.00001.

5.3 Event Studies

Event studies trace each outcome year by year around the first relocation. I compare each receiving location only with its own weighted controls and each sending location with its own synthetic control. Already treated locations therefore never serve as controls, and the response can differ across cohorts first treated in different years. This follows the stacked comparisons of each treated cohort with its own clean controls (Cengiz et al. 2019; Wing et al. 2024). I estimate receiving and sending effects separately.

For each year relative to the first relocation, the event study measures how far a treated location has moved away from its comparison group since the year before the first relocation. Let G_s denote the first relocation year of location $i(s)$ in matched set s. The comparison outcome $\bar{Y}_{C(s),t}$ is the mean outcome of its controls weighted by entropy balancing for receiving locations and the synthetic control outcome for sending locations. The gap at event time ℓ is

$$D_{s\ell} = \left(Y_{i(s),G_s+\ell} - \bar{Y}_{C(s),G_s+\ell}\right) - \left(Y_{i(s),G_s-1} - \bar{Y}_{C(s),G_s-1}\right). \tag{1}$$

I average the gaps over the G_ℓ matched sets observed at event time ℓ, giving

$$\hat{\theta}_\ell = \frac{1}{G_\ell} \sum_{s \text{ observed at } \ell} D_{s\ell}. \tag{2}$$

The number G_ℓ appears below each point in the figures. For tax income, municipal data for 2011 and 2012 are missing, so the reference is the latest available year before first treatment.

I compute standard errors at the level of the matched set. The matched set is the unit of treatment assignment, and its controls enter every year of the set, so the years of a set share the same shocks. Standard errors should therefore allow any correlation within a set and treat different sets as independent (Abadie, Athey, et al. 2023). For matching without replacement, the variance across matched sets gives valid inference (Abadie and Spiess 2022).

A causal interpretation requires that, without the relocation, treated municipalities would have followed the same outcome trends as their matched controls.

The design also requires that anticipation does not change outcomes during the comparison's pre-treatment period. Agencies may prepare their premises and employees may adjust before operations begin, so I examine pre-treatment coefficients.

Appendix D provides the policy-rule reconstruction, weighting problems, full balance table, synthetic-control diagnostics and inference details.

Appendix D provides additional results using stacked difference-in-differences and long differences.

The next section uses these estimates to evaluate the policy targets and then the adjustment of local labor markets and factor prices.

6 Place-Level Effects

This chapter studies how redistributing public employment as a place-based policy affects the lagging regions that gain these jobs and the thriving regions that lose them.

I first evaluate whether the relocations move the policy targets that determine which locations can receive public jobs.

The index weights determine the order in which I evaluate these targets. A one-point gain in an indicator with a weight of 30% moves the index three times as much as the same gain in an indicator with a weight of 10%. I therefore interpret the indicator weights as the policymaker's tiered priority list of each policy goal. I present the results in this order, starting with population growth and finishing with net migration of young adults.

The policy targets do not show who fills the new jobs or who gains from the relocations. I therefore examine local employment, residence employment and commuting to distinguish residents who already lived in the location, newcomers and commuters who live elsewhere. Human capital, measured by employment among workers with tertiary education and employment in specialist and expert jobs, provides further evidence on how the local labor market adjusts. Residential rents and purchase prices, commercial rents and wages by sector then show whether workers or landowners capture the benefits.

Relocating public employment raises private employment both in locations that receive public jobs and in locations that lose them. The estimates imply about 1.7 additional private jobs per public job. Private jobs build up slowly in receiving locations, with large gains five to six years after the first relocation, and rents and purchase prices follow people with the same lag. Residents fill the new local jobs, and in-commuting does not rise.

Munich's employment density increases by 3.6 workers per 100 inhabitants after the relocations compared with its synthetic control. The number of jobs relocated may be small relative to its labor market, but almost all relocated agencies occupied scarce office space in the city center. The relocations may free up this space for more productive private sector firms, helping explain the rise in employment density despite the loss of public jobs. Appendix E reports the long-difference results and the additional figures.

6.1 Policy Evaluation of the 'Heimatstrategie'

The relocations move three of the five policy targets in receiving municipalities. Net migration of young adults responds at once, tax income after two years, and employment density and total population after five to six years, while the unemployment rate does not change. Table 4 reports the average effects, and Figure 14 in Appendix E shows the event studies, comparing receiving locations with entropy balancing controls and sending locations with synthetic controls, reporting Munich separately from the other eight sending locations.

Priority One. Population and Unemployment (30% Each)

Priority one in the public employment reallocation policy is population growth and the unemployment rate, which together account for 60% of the index (30% each). Receiving locations grow, and the growth comes from working-age residents. Total population rises

Table 4. Average effects on the policy targets

	Receiving	Munich	Other sending
Total population (100 x log)	**0.56****	−0.28	**1.03***
	(0.28)	(1.35)	(0.54)
Unemployment rate (pp)	0.04	−0.03	0.33
	(0.09)	(0.79)	(0.19)
Tax income (100 x log)	1.01	2.39	−0.99
	(0.64)	(1.93)	(0.71)
Employment density (per 100 inhabitants)	0.59	**3.60****	−0.36
	(0.55)	(1.73)	(0.64)
Net migration 18 to 29 (per 1,000)	**8.58****	**−15.75***	−3.74
	(3.65)	(8.95)	(3.26)

Average effects over event times zero to eight. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations are compared with synthetic controls. Standard errors are in parentheses. Unemployment effects are in percentage points, employment density in workers per 100 inhabitants, and net migration of young adults in people per 1,000. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Figure 14 in Appendix E reports the full event studies.

by about 0.5%, while working-age population rises by about 1%. Both increases are statistically significant.

The unemployment rate does not change in receiving locations. Jobs and residents rise while the unemployment rate stays flat, suggesting that the new jobs go to people who already work or to newcomers rather than to the unemployed.

Munich does not lose residents, and its labor market keeps absorbing workers. Total population is unchanged. Working-age population grows by 2.59% on average, although the estimate is not significant. The unemployment rate is unchanged. In the other sending locations, total population rises, as does working-age population. Both increases are statistically significant.

Priority Two. Tax Income (20%)

Priority two is tax income (20%). Tax income rises in receiving locations two years after the first relocation, before population grows. The average increase is 1.02% and is not significant.

Priority Three. Employment Density and Net Migration of Young Adults (10% Each)

Priority three is employment density and net migration of young adults between 18 and 29 years of age (each get 10% index weight). Receiving locations become thicker local labor markets three years after the first relocation. Employment density rises by 0.59 workers per 100 inhabitants on average. The average decline of 0.36 workers per 100 inhabitants in other sending locations is not significant.

Net migration of young adults responds faster than total population. It rises significantly by 8.6 per 1,000 on average. In Munich, net migration of young adults falls significantly by 15.8 per 1,000 on average and by 23.3 in year eight. The average decline of 3.7 per 1,000 in other sending locations is not significant.

Appendix E reports the cohort comparisons in detail.

6.2 Effects on the Local Labor Markets

Residents take the new jobs in receiving locations. Public jobs arrive first, private jobs follow after about five years, and in-commuting does not rise. I compare workplace employment, local employment, residence employment and commuting to trace these adjustments.

Workplace Employment

Public workplace employment in receiving locations builds up over seven years as the agencies fill their positions (Figure 2). The average increase is 11%. The increase grows from 6.6% in the year of the first relocation to a significant 20.8% in year seven. With an average of 531 public workplace employees in 2014, a rise of about 11% amounts to roughly 55 to 60 jobs per receiving location. This is close to the roughly 50 positions per receiving location in the relocation data, with a median of 31 and a maximum of 230 in Gunzenhausen. The first stage thus appears consistent with the program's size. Its steady growth mirrors the gradual staffing of agencies in interim offices and later in new buildings. Gunzenhausen's State Office for Schools, for example, moves into its new building for about 200 staff only in 2027.

Private workplace employment follows public employment with a lag of about five years. The average increase is 1.11% and is not significant. Effects remain close to zero through year four, then reach significant increases of 2.7% in year five and 4.7% in year eight. The average increase in total workplace employment is 1.85% and is not significant. The additional total workplace employment estimates are also imprecise on average, but show significant increases of 1.9% in year four and 3.8% in year eight. The average private effect is small because the response starts late. Private jobs may respond as agencies approach full staff and new residents spend locally.

Public-to-Private Jobs Multiplier. The estimates imply about 1.7 additional private jobs per public job, providing a gauge of the jobs multiplier. This calculation compares the private employment increase of 1.11% on a base of 9,093 employees with a public employment increase of 10.9% on a base of 531 employees.

Munich gains private jobs relative to its synthetic control, with a significant average increase of 5.94% and a significant increase of 11.5% in year eight. Total workplace employment rises significantly by 6.03%, while the additional estimate indicates an increase of 4.5% and is not significant. Public workplace employment also increases, but the estimate is not statistically significant. In other sending locations, public workplace employment rises significantly, showing that the public sector keeps growing despite the relocations, while private workplace employment remains flat.

The number of jobs relocated from Munich may be too small to matter for its local labor market as a whole, but their location within the city may matter. Almost all relocated public agencies occupied office space in Munich's city center (Figure 3). Using private sector wages as a proxy for productivity, Munich is one of the most productive cities in Germany, and its city center is one of the most productive areas of the city. Office

space in these areas is scarce. The relocations likely freed up space for more productive private sector firms, which may explain why employment density in Munich increased despite the loss of public jobs.

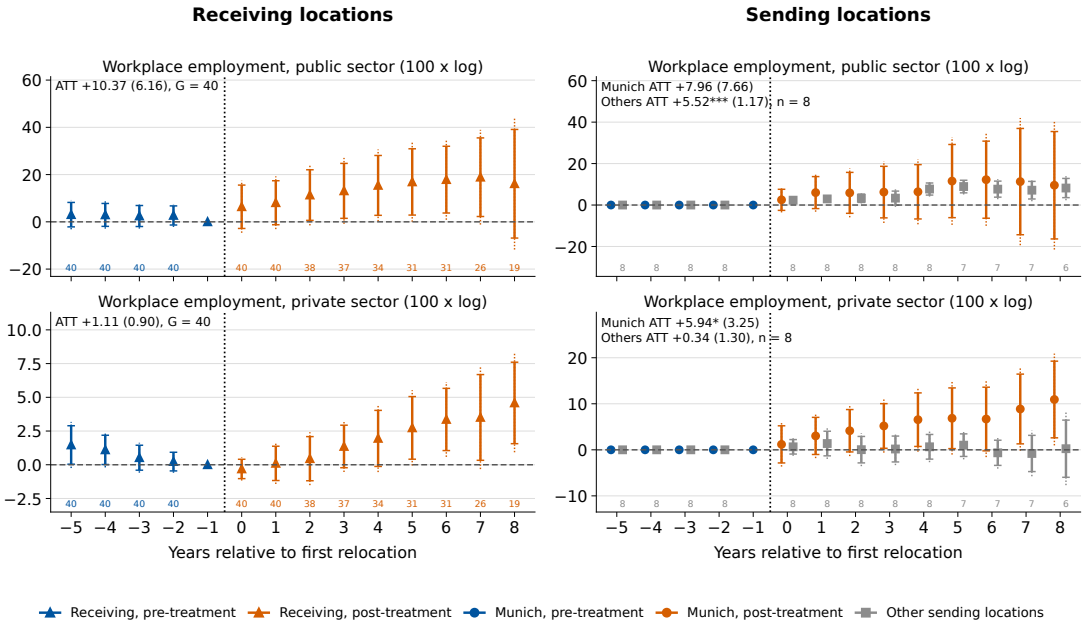


Figure 2. Public and private workplace employment. Event studies relative to the first relocation. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations are compared with synthetic controls, with Munich reported separately from the other eight sending locations. Log outcomes are multiplied by 100. Because relocations occur in different years, the small numbers below the estimates show how many treated locations contribute at each event time.

Local Employment

In receiving locations, new local jobs go to residents, while some newcomers keep jobs elsewhere. Local employment counts the employees who live and work in the same municipality, and local private employment rises significantly by 2.35% on average (Figure 18 in Appendix E).

Residence employment and out-commuting rise alongside local employment. Residence employment increases significantly by 1.37%, and out-commuters increase significantly by 1.7%. In-commuters do not increase significantly. More residents work in their own town, more residents commute out, and in-commuting does not rise. The policy may therefore enlarge the resident labor force. New residents who out-commute could be partners of agency staff who keep jobs elsewhere.

Munich residents increasingly work in Munich. These results suggest that the relocation does not weaken Munich's labor market.

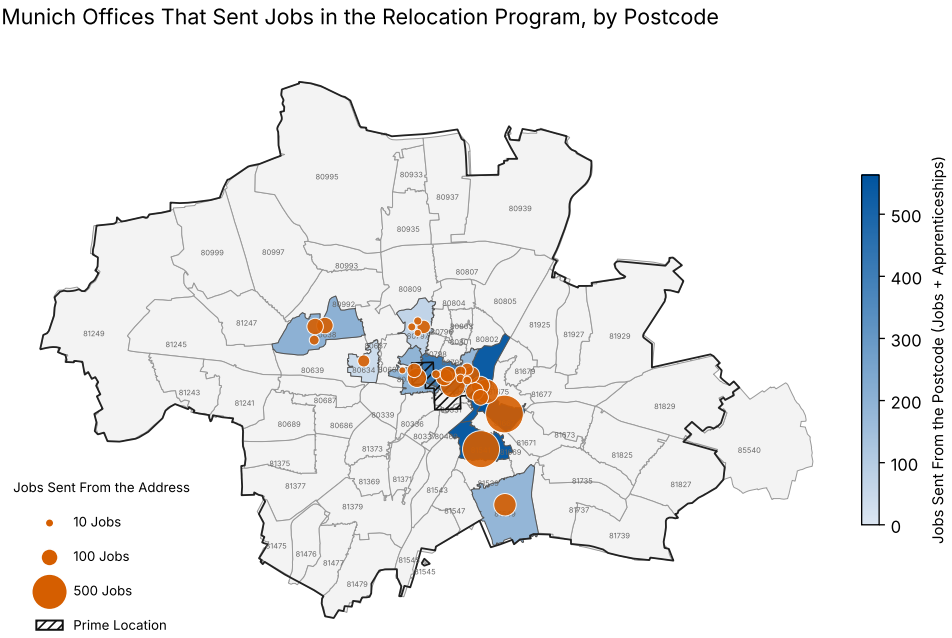


Figure 3. Locations of public agencies in Munich before relocation. Circle sizes show jobs sent from each address, shading shows jobs and apprenticeships sent from each postcode, and hatching marks prime locations. Prime locations are unusually dense clusters of economic activity within a city (Ahlfeldt, Albers, et al. 2026).

Sector Employment

I now turn to workplace employment by sector. The private response in receiving locations comes from manufacturing employment and knowledge-intensive business services, while other services shrink when the agencies open.

Manufacturing

Manufacturing employment in receiving locations rises by about 5% after the first relocation (Figure 22 in Appendix E). The increase is significant at 5.5% in year two and around 10.5% in year eight.

Manufacturing employment increases more where the state builds new offices than where public agencies rent.

Neighboring towns show no significant manufacturing employment gains (Appendix E).

Manufacturing employment effects in sending towns are not statistically significant. Significant manufacturing gains appear only in treated receiving locations.

Services

Services employment falls when the agencies open and then recovers (Figure 22 in Appendix E). The average decline is significant at 2.22%. Within services, knowledge-intensive business services grow significantly by 6.73%.

Human Capital

Private employers in receiving locations employ more workers with tertiary education after the first relocation (Figures 28 and 32 in Appendix E). Private employment at a high requirement level rises by 2.12%. New public agencies increase the local demand for workers with tertiary education, while nearby private employers upgrade their workforce.

6.3 Effects on Factor Prices

Prices follow people. Rents and purchase prices in receiving locations rise five to six years after the first relocation, mostly where the state built a new office, while private wages rise only a little. In a spatial equilibrium, a local labor shock does not stay with workers. Part of it shows up in rents and land values, so landowners capture some of the benefit and newcomers pay more to live there. Wages and rents together show what happens to real income, because comparing wages with the rise in rents shows whether the gains reach workers or are absorbed by higher housing costs. This section reports rents, purchase prices and wages.

Residential Rents and Purchase Prices

Rents and purchase prices rise in receiving locations with a lag (Figure 38 in Appendix E).

Relieving Munich's housing market was a stated reason for the program, yet these results show no relief.

Wages

Private wages in receiving locations rise a little and early, while public wages do not change (Figure 38 in Appendix E). The average private wage increase is significant at 0.68%. The significant increase grows from 0.45% in the year of the first relocation to 2.5% in year eight. Public pay is set uniformly across the state, so the local wage response shows up only in the private sector.

Private wages rise in the year of the first relocation and the following three years, before rents move. Later, rents rise by 5 to 7%, against private wage gains of 1 to 2.5%, providing suggestive evidence of capitalization into rents. The average rent effect is not statistically significant, so this comparison does not measure the share of total gains captured by landowners.

7 Robustness

The preceding chapter compares receiving and sending locations with controls outside the policy state. I now examine whether the effects extend to neighboring towns or reflect a trend that is idiosyncratic to the policy state, Bavaria. I find that employment effects around receiving locations are localized. Population gains appear 10 to 30 minutes away with Conley standard errors, although no band within 30 minutes is statistically significant with standard errors clustered by district region. I find no evidence of crowding out. Removing the trend in the policy state raises Munich's workplace employment gain from 4.50% to 7.39%.

7.1 Spatial Spillovers

I therefore use 30 minutes as a conservative buffer for reference towns in the policy state. The population spillover also lies within this buffer. No employment-weighted ring total is significantly negative at cutoffs of 15, 30 or 45 minutes, providing no evidence of crowding out.

Appendix F reports the spatial-spillover specifications, commuting exposure and rings around sending sites.

7.2 The Trend in the Policy State

Removing the trend in the policy state increases the employment effects in receiving locations. Every treated location is in the policy state and every control is outside it. A change specific to Bavaria that coincides with treatment could therefore appear as an effect of the relocations, especially in Munich. At the same time, the distance of reference towns from relocation sites matters. If nearby towns grow because of the policy, subtracting their growth would reduce the estimated effect.

I estimate a triple difference by imputation. I compare receiving locations with their controls outside the policy state, subtract the corresponding gap for untreated reference towns in the policy state, and compare outcomes before and after relocation.

Total workplace employment rises by 0.93% in the main analysis and by 3.75% after adjustment. Employment density rises by 0.59 workers per 100 residents in the main analysis and by 2.27 after adjustment. Net migration of young adults rises by 8.58 per 1,000 in the main analysis and by 11.86 after adjustment. All three adjusted effects are statistically significant. Residence employment changes from a significant increase of 1.36% to an insignificant decline of 0.71% (Figure 4, left column).

Manufacturing employment rises by 2.07% before removing the trend and by 2.79% after removing it. Neither estimate is statistically significant.[13]

Munich's workplace employment gain rises from 4.50% in the main analysis to 7.39% after removing the trend. Its local employment gain rises from 9.44% to 10.55%, and its residence employment gain from 5.65% to 6.34% (Figure 43 in Appendix F). The

[13] Manufacturing employment in this comparison covers mining, manufacturing, energy, water and construction in both estimates.

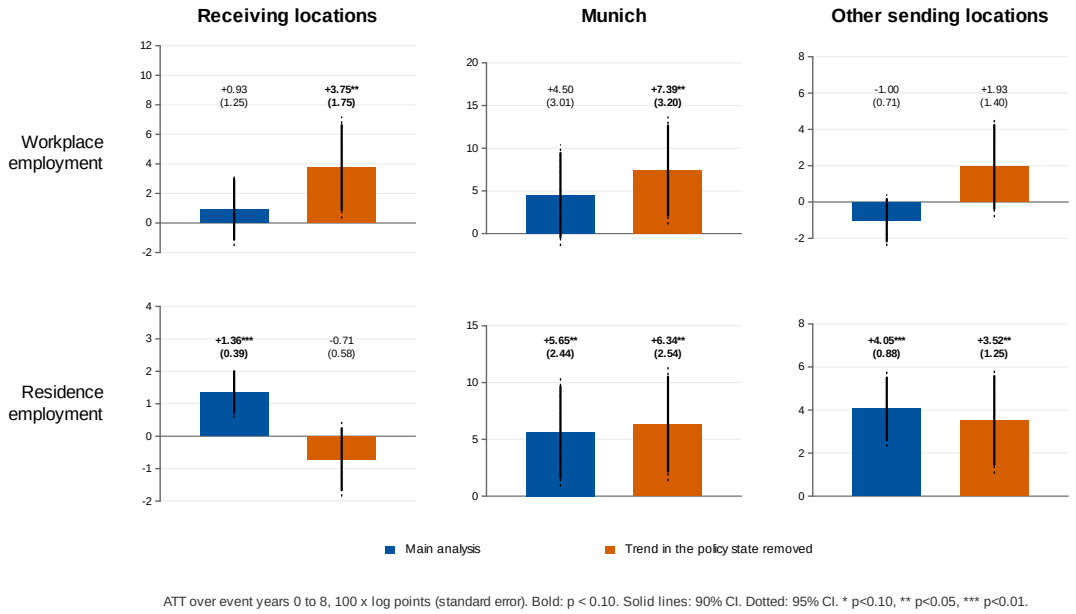


Figure 4. Employment in receiving and sending locations before and after removing the trend in the policy state

Blue bars show the main analysis and orange bars the estimates after removing the trend in the policy state. For receiving locations (left), the triple difference is estimated by imputation, with reference towns at least 30 minutes from every relocation site, entropy balanced within policy cells using the smallest feasible tolerance, and standard errors clustered by set. For Munich (middle) and the eight other sending locations pooled (right), the synthetic control is estimated again after subtracting the trend measured in seven untreated towns in the policy state with at least 20,000 inhabitants in 2014, at least 30 minutes from every relocation site. Entries are averages over event years zero to eight, measured in 100 times log. The numbers above each bar give the estimate with its standard error in parentheses, in bold when significant at the 10% level. Solid lines show 90% and dotted lines 95% confidence intervals. Each panel has its own scale.

three adjusted effects are statistically significant (Figure 4, middle column). In the other sending locations, workplace employment changes from a decline of 1.00% to an increase of 1.93%, neither statistically significant (Figure 4, right column).

The positions transferred from Munich account for only 0.14% of its workplace employment, so its growth may reflect its own dynamics rather than a response to the relocations. The urban reference group contains only seven towns at least 30 minutes from every relocation site. The small reference group of big cities available in Bavaria is one of the main reasons why the main analysis uses controls outside the policy state.

The employment effects around receiving locations are localized, while population spillovers extend up to 30 minutes from receiving locations. Removing the trend in the policy state strengthens the gains in total workplace employment, employment density and net migration of young adults in receiving locations. I now use the model to examine whether redistributing public employment can reduce regional disparities while improving aggregate efficiency.

8 Two-Sector Quantitative Spatial Model

Can moving public jobs to lagging regions reduce regional disparities while improving aggregate efficiency?

The location of government activity reflects the historical concentration of political and administrative power. Public offices occupy central locations that also attract private firms. City halls are sufficiently close to central business districts to serve as proxies for their location (Holian 2019). Public offices in these dense locations occupy space that private firms could use, so congestion from public occupancy can hinder private expansion. Relocating public administration could therefore resolve this inefficiency by freeing up space at dense central locations and, at the same time, help increase density in lagging regions.

I extend the quantitative spatial framework of Ahlfeldt, Redding, et al. (2015) to distinguish public and private employment. The central extension allows public workplaces to generate productivity spillovers for private firms separately from the agglomeration economies generated within the private sector. The relative marginal contributions of public and private employment depend on their relative densities in and around each location. An additional public job can therefore have different effects across locations with different employment compositions. Public agencies also occupy floor space, so their relocation changes the space available to private firms and households. These mechanisms interact as employment, residence choices, wages, and rents adjust throughout the economy.

I implement the relocation program as a counterfactual change in the geography of public employment. My model contributes to the evaluation of place-based policies by accounting jointly, in general equilibrium, for effects in locations gaining jobs, the rest of the policy state, and the rest of Germany.

8.1 Set Up

Locations are German municipalities, with i indexing residences and j workplaces. Subscripts M and R denote workplace and residence quantities. Each location has land area $K_i > 0$ and effective floor-space stock $L_i > 0$. The national mass of workers, $\bar{H}$, is fixed. Each worker supplies one unit of labor. All workers belong to one pool and choose a residence and a workplace among all jobs, public and private. The single pool is a proxy for sector switching, because workers move freely between public and private jobs at the same workplace. The equilibrium reallocates workers across locations.

8.2 Preferences

Workers value consumption, housing, amenities, and access to work. They spend share β of disposable labor income on a freely traded numeraire good and share $1 - \beta$ on housing. Let Q_i^R denote residential rent, t the common proportional labor-income tax, and z_{ijo}

the preference draw of worker o. Indirect utility is

$$u_{ijo} = \frac{z_{ijo} B_i (1-t) w_j}{(Q_i^R)^{1-\beta} d_{ij}}, \qquad d_{ij} = \exp(\kappa \tau_{ij}), \tag{3}$$

where w_j is the wage at workplace j, B_i is the amenity level at residence i, τ_{ij} is road travel time in minutes, and κ governs commuting costs per minute. Commuting reduces utility in non-monetary terms. Independent Fréchet preference draws have shape $\epsilon > 1$ and scales $T_i E_j$ for residence–workplace pairs.

Every job at workplace j, public or private, pays the same gross nominal wage w_j. The wage clears the local labor market. All wages in this section are nominal wages.

8.3 Workplace Choice and Labor Supply

Workers compare all residence–workplace pairs. With $D_{ij} = \exp(-\kappa \epsilon \tau_{ij})$, choice probabilities and bilateral worker flows are

$$\begin{aligned} \phi_{ij} &= T_i B_i^{\epsilon} (Q_i^R)^{-\epsilon(1-\beta)} D_{ij} E_j [(1-t) w_j]^{\epsilon}, \qquad \Phi = \sum_{r,s} \phi_{rs}, \\ \pi_{ij} &= \frac{\phi_{ij}}{\Phi}, \qquad N_{ij} = \bar{H} \pi_{ij}. \end{aligned} \tag{4}$$

Summing N_{ij} across residences gives workplace employment, $H_{Mj} = \sum_i N_{ij}$. Summing across workplaces gives resident employment, $H_{Ri} = \sum_j N_{ij}$. Higher wages make workplaces more attractive. Higher housing costs and longer commutes make the corresponding residence–workplace pairs less attractive. The diagonal flow N_{jj} is local employment in municipality j.

The choice set determines expected utility. With $\Gamma_\epsilon = \Gamma(1 - 1/\epsilon)$,

$$\bar{U} = \Gamma_\epsilon \Phi^{1/\epsilon}. \tag{5}$$

Expected utility values access to the full set of residence–workplace pairs. It incorporates disposable wages, housing costs, amenities, and commuting.

8.4 Public Job Assignment

The public sector is introduced as follows. The government assigns $\bar{H}^G_{Mj}$ public jobs to workplace j, with $\sum_j \bar{H}^G_{Mj} = \bar{H}^G$. Public jobs are a fixed slice of the jobs at each workplace. Private workplace employment is total workplace employment minus public jobs,

$$H^P_{Mj} = H_{Mj} - \bar{H}^G_{Mj}.$$

Resident employment refers to individuals living in a location, wherever they work. Public agencies occupy office space at their workplace in proportion to the public jobs assigned there. This space is not available to private firms or households.

I implement the relocation program as a counterfactual change in the assignment of public jobs. Relocated jobs take their office space with them, and workers choose new residences and workplaces. The government finances public payroll and office rents with a common proportional tax on labor income, which adjusts to keep its budget balanced.

8.5 Residential Choice and Demand

Amenities depend on where workers live. They are given by

$$B_i = b_i \Omega_i^{\eta}, \qquad \Omega_i = \sum_s \exp(-\delta_B \tau_{is}) \frac{H_{Rs}}{K_s}. \tag{6}$$

All residents contribute to surrounding resident density. The amenity fundamental b_i is fixed, η is the amenity elasticity, and δ_B governs spatial decay per minute. A relocation can consequently change workplace productivity more than residential amenities when workers commute to the new jobs from their existing homes.

8.6 Supply Side: Firms and Floor Space

Private firms produce the freely traded good using labor and private commercial floor space L_j^P. Their output is

$$Y_j^P = A_j^P (H_{Mj}^P)^{\alpha} (L_j^P)^{1-\alpha}. \tag{7}$$

Firms take productivity A_j^P, wages, and commercial rent q_j as given. Competition and constant returns imply

$$w_j H_{Mj}^P = \alpha Y_j^P, \qquad q_j L_j^P = (1-\alpha) Y_j^P. \tag{8}$$

Public offices are separate from L_j^P. Vacated offices can therefore increase the space available for private production or housing, with rents and demand determining its use.

The key tradeoff of relocating public jobs runs through private sector productivity and floor space. A public agency can raise the productivity of nearby private firms, but its offices occupy space those firms could use. The central extension of the model, and its main contribution, is that private sector productivity depends separately on nearby private sector and public sector workplace density. Productivity at workplace j is

$$A_j^P = a_j (\Upsilon_j^P)^{\lambda_{PP}} (\Upsilon_j^G)^{\lambda_{PG}}, \tag{9}$$

$$\Upsilon_j^m = \sum_s \exp(-\delta_A \tau_{js}) \frac{H_{Ms}^m}{K_s}, \qquad m \in \{P, G\}, \quad H_{Ms}^G = \bar{H}_{Ms}^G. \tag{10}$$

The productivity fundamental a_j is fixed across policy scenarios. The elasticities λ_{PP} and λ_{PG} govern private-to-private and public-to-private spillovers, while δ_A governs their decay with travel time in minutes. Each source contributes employment per unit of its own land area. In the spillover kernels, the travel time within a municipality is zero, so a municipality's own density enters with weight one. Moving public jobs changes the public density term directly. The subsequent response of private sector firms and workers changes the private sector density term. I maintain the private sector elasticity from Ahlfeldt, Redding, et al. (2015) and estimate the public sector elasticity from the relocation responses.

The elasticity λ_{PG} is the contribution of the government to agglomeration.

Public offices occupy $\bar{L}_i^G$, leaving private firms and households to share the residual stock. Floor space therefore satisfies

$$L_i = L_i^P + \bar{L}_i^G + L_i^R, \qquad F_i = L_i - \bar{L}_i^G > 0. \tag{11}$$

Office requirements are assigned with public employment. At baseline, a public job uses fraction γ_G of the floor space used per private worker in the same location. Baseline office space per public job and per location is

$$\ell_i^{G,0} = \gamma_G \frac{1-\alpha}{\alpha} \frac{w_i^0}{q_i^0}, \qquad \bar{L}_i^{G,0} = \ell_i^{G,0} \bar{H}_{Mi}^{G,0}. \tag{12}$$

I set $\gamma_G = 0.256$. The median floor area per workplace in the five office buildings the state constructed for the program is 30 m^2, and the model-implied floor space per private worker in Munich is 117.3 m^2. Office requirements remain fixed during the equilibrium adjustment. A transferred position keeps its baseline office requirement.

Income earned at workplaces finances housing at residences. Gross resident income is $I_i = \sum_j w_j N_{ij}$, and disposable income is $v_i = (1-t) I_i$. Housing demand satisfies $Q_i^R L_i^R = (1-\beta) v_i$. Residential floor space stays fixed at its baseline level. Commercial floor space responds to commercial rent,

$$C_i = (C_i^0 + O_i^{\text{free}} + O_i^{\text{new}}) \left(\frac{q_i}{q_i^0} \right)^{\sigma}, \tag{13}$$

where C_i^0 is baseline commercial floor space, O_i^{free} is the office space that public agencies leave, and O_i^{new} is the office space the state built for the program. The parameter σ is the elasticity of commercial floor space with respect to commercial rent. With $\sigma = 0$ the commercial floor space is fixed, which represents full zoning.

The government finances public payroll and office costs with a common tax on labor income. The private wage bill is $W^P = \sum_j w_j H_{Mj}^P$, the public wage bill $W^G = \sum_j w_j \bar{H}_{Mj}^G$, and the public office bill $C^G = \sum_j q_j \bar{L}_j^G$. The balanced budget implies

$$t(W^P + W^G) = W^G + C^G, \qquad t = \frac{W^G + C^G}{W^P + W^G}. \tag{14}$$

Public wages are taxed on the same basis as private wages. The office bill values both leased and state-owned premises at their commercial rental equivalent. Rents accrue to absentee landlords and are not returned to resident households. The budget covers the modeled administration sector. The office bill, the public wage bill, and the tax base change with equilibrium rents and wages.

8.7 Equilibrium

An equilibrium makes worker choices, private factor demand, assigned public employment, floor-space use, spillovers, and the government budget mutually consistent. Labor supplied to each workplace equals the public jobs assigned there plus firms' demand, and workplace

and residence totals equal the national mass. Private firms, households, and public offices use no more floor space than each location has, and the tax rate lies between zero and one, which requires the public office bill to be smaller than the private wage bill. Expected utility adjusts endogenously with the allocation of the fixed national workforce.

Total employment is fixed nationally.

9 Model Estimation

I quantify the model for the 2,055 municipalities in the policy state and 3,676 locations outside the policy state. These are the commuting buffer, the matched control municipalities with their counties, and the rest of Germany as one location.

Baseline employment by workplace and residence municipality comes from the Federal Employment Agency's employment records for 2014, the year before the first relocation (Section 3). Public employment is employment in public administration, defense, and social security (WZ 2008 division 84). All other industries define private employment.[14] Residential and office rents for 2014 come from the municipal indices of Ahlfeldt, Heblich, and Seidel (2023) and Ahlfeldt, Heblich, Seidel, and Yin (2026), land K_i is developable land in CORINE 2012, and road travel times in minutes come from OSRM routing between municipalities. CORINE Land Cover is the European inventory of land cover and land use, classified from satellite images. The commuting time within a municipality is the mean distance between two points in a disk of the municipality's area, at 30 km per hour with a circuity factor of 1.25. I rescale the workplace counts to the national residence total, so that $\sum_j H_{Mj} = \sum_i H_{Ri} = \bar{H}$.

9.1 Workplace Choice

I take the production and consumption shares, the private productivity elasticity, the amenity elasticity, and the spatial decay rates from Ahlfeldt, Redding, et al. (2015). The commuting decay comes from a gravity equation for bilateral commuting flows between municipalities in 2014 from the Federal Employment Agency, for employees subject to social security who live in the policy state. Flows from residence i to workplace j are

$$N_{ij} = \exp\left(\mu_i + \chi_j - \kappa\epsilon\,\tau_{ij} + \rho\,\hat{v}_{ij}\right) u_{ij},$$

where μ_i and χ_j are residence and workplace fixed effects and $\hat{v}_{ij}$ is a control function for travel time. It is the residual from a regression of τ_{ij} on straight-line distance with residence and workplace fixed effects. I estimate the equation by Poisson pseudo-maximum likelihood, which gives $\kappa\epsilon = 0.0786$ per minute (standard error 0.0059).

Every extra minute of commuting lowers the number of commuters by 7.6%, so the flow halves every 8.8 minutes. A resident of Furth in the Landkreis Landshut is 13.6 minutes from Landshut and 54.1 minutes from Munich. All else equal, commuting to Munich is $\exp(-0.0786 \times 40.5) = 4.1\%$ of the flow to Landshut.

[14] Of the 10,742 municipalities in the 2014 employment data, I exclude 53 without developable land in the CORINE 2012 land-cover data and 52 without private workplace employment.

In a commuting inversion, I choose the Fréchet shape ϵ to match the cross-municipality variance of log private workplace wages in 2014. This gives $\epsilon = 5.156$ and $\kappa = 0.0152$ per minute. The tax rate that balances the baseline government budget is 5.4967%. Table 5 reports the parameters.

9.2 Residential Choice and Public-to-Private Spillovers

I estimate the amenity elasticity η by two-step GMM. The estimate is $\eta = -0.871$ (standard error 0.243). Appendix G describes the estimation. The amenity elasticity is negative, so amenities fall where resident density rises. The value in Ahlfeldt, Redding, et al. (2015) is 0.155. A 10% rise in nearby resident density lowers amenities by 8.0%, while it would raise them by 1.5% with the value of Ahlfeldt, Redding, et al. (ibid.).[15]

I estimate λ_{PG} by indirect inference (Ahlfeldt, Baum-Snow, et al. 2023). For each candidate value, I recover the baseline fundamentals, solve a zero-shock equilibrium that must reproduce the baseline, solve the relocation counterfactual, and compute model-implied long differences in the transformation of the reduced form, $g_j^o(\lambda_{PG}) = \log\left(1 + E_j^{o,\text{cf}}\right) - \log\left(1 + E_j^{o,0}\right)$.

I apply the triple difference to the model exactly as to the data, with the same weights. For each receiving matched set, it takes the change in the treated town, minus the entropy-balanced change in its controls outside the policy state, minus the trend in the policy state from reference towns at least 30 minutes from every site. The target is the 30 minute triple difference for workplace employment in receiving towns, 3.75 (standard error 1.75), on 40 matched sets. I hold $\lambda_{PP} = 0.071$ fixed. The estimate is $\widehat{\lambda}_{PG} = 0.0375$ (standard error about 0.017). The standard error is the standard error of the target divided by the slope of the model moment in λ_{PG}. The model moment rises from 2.98 at $\lambda_{PG} = 0.03$ to 4.03 at $\lambda_{PG} = 0.04$, so the slope is about 105.

The estimate measures the contribution of the government to agglomeration. A 10% rise in nearby public employment density raises private productivity by about 0.36%, holding private density and fundamentals fixed.

9.3 Calibrated Parameters

9.4 Model Inversion

Given the parameters, I invert the model to recover productivity and amenity fundamentals that rationalize the baseline. The inversion matches resident employment and total workplace employment exactly through a residence factor and the workplace factor E_j. Relative to its geometric mean, the workplace factor lies between 0.94 and 1.57 for 98% of locations. Wages are the observed private workplace wages in 2014, and every job at a workplace earns this wage. Wages and rents recover private productivity and factor demand. Public office requirements and housing demand then complete the effective floor-space stock. Appendix G gives the inversion equations and validation checks.

[15] With purchase prices from Ahlfeldt, Heblich, and Seidel (2023) instead of rents, $\eta = -0.401$ (standard error 0.195).

Table 5. Model parameters

Symbol	Description	Value	Source
λ_{PG}	Public-to-private agglomeration elasticity	0.0375	Indirect inference, triple difference
η	Amenity elasticity with respect to resident density	-0.871	Two-step GMM
$\kappa\epsilon$	Commuting decay per minute	0.0786	CF-IV-PPML, commuting flows 2014
ϵ	Fréchet shape	5.156	Variance of log private workplace wages 2014
κ	Commuting cost per minute	0.0152	$\kappa\epsilon/\epsilon$
λ_{PP}	Private-to-private agglomeration elasticity	0.071	Ahlfeldt, Redding, et al. (2015)
δ_A	Productivity decay per minute (cutoff 120 minutes)	0.3617	Ahlfeldt, Redding, et al. (2015)
δ_B	Amenity decay per minute (cutoff 60 minutes)	0.7595	Ahlfeldt, Redding, et al. (2015)
σ	Elasticity of commercial floor space with respect to commercial rent	5	Fit of commercial rents
α	Labor share in private production	0.80	Ahlfeldt, Redding, et al. (2015)
β	Consumption expenditure share	0.75	Ahlfeldt, Redding, et al. (2015)
γ_G	Public office intensity relative to private	0.256	State-built offices relative to Munich private floor space
t^0	Baseline labor-income tax rate	0.054967	Balanced government budget

Notes: Residential floor space is fixed at baseline. Travel times are in minutes, so decay parameters are per minute. Standard errors are 0.017 for λ_{PG}, 0.243 for η, and 0.0059 for $\kappa\epsilon$.

9.5 Model Fit

Table 6 compares the model with the triple-difference outcomes for the receiving towns. All seven untargeted outcomes are inside the 90% interval. Private workplace employment in the receiving towns rises by 3.19 log points in the model's triple difference.

Table 6. Model and triple difference, receiving towns

Outcome	Data	Model
Workplace employment (target)	3.75 (1.75)**	+3.76
Employment density	+2.27 (0.66)***	+1.41
Residence employment	−0.71 (0.58)	+0.08
Local employment	+1.84 (1.31)	+3.52
Income	+0.74 (0.60)	+0.14
Residential rent	−0.30 (1.12)	+0.21
Purchase price	+2.65 (2.67)	+0.21
Commercial rent	+0.15 (0.84)	+0.68

Notes: 30 minute triple difference, 40 matched sets, $\widehat{\lambda}_{PG} = 0.0375$. Standard errors in parentheses. ** $p < 0.05$, *** $p < 0.01$.

10 Quantifying the Effects of Relocating Public Jobs

I re-solve for wages, residential and commercial rents, workplace and residence choices, floor-space use, productivity, amenities, and the tax rate. Productivity and amenity fundamentals, taste scales, land, travel times, and the national worker mass remain at their baseline values. Residential floor space is fixed, and commercial floor space responds to commercial rent. The comparison describes the difference between the relocation equilibrium and the 2014 baseline equilibrium without the relocations.

10.1 Welfare and Output

I measure welfare by the expected utility of workers, $\bar{U}$ in equation (5). The relocations raise welfare by 0.0045%. Table 7 reports private workplace employment and private output. Private workplace employment rises by 7,596 jobs, or 2.27%, in the receiving towns. Total employment is fixed nationally. Private output in Germany falls by 375 million euros, or 0.010%, and private output in the policy state by 203 million euros, or 0.059%. The HHI of municipal output shares in the policy state falls from 896.4 to 890.3, so output in the policy state becomes less concentrated.

10.2 Channels

The relocations change welfare through six channels. Wages respond in every local labor market. Residential rents respond to the new distribution of residents. Amenities respond to resident density, and with η negative they fall where density rises. The tax rate responds

Table 7. Private workplace employment and private output

Region	Jobs	Jobs (%)	Output (m euros)	Output (%)
Receiving towns	+7,596	+2.27	+435	+2.65
Other municipalities in the policy state	−5,669	−0.10	−637	−0.19
Rest of Germany	−1,927	−0.007	−172	−0.005
Germany	0	0	−375	−0.010

Notes: Changes between the equilibrium with all relocations up to 2024 and the equilibrium without them, $\widehat{\lambda}_{PG} = 0.0375$.

to the public wage bill and the public office bill relative to the tax base. Commercial floor space responds to commercial rent with elasticity σ. The public-to-private agglomeration effect λ_{PG} raises private productivity where public jobs arrive.

Table 8 decomposes the change in welfare by channel. The contributions of wages, residential rents, amenities, and the tax rate are first-order shares of the welfare change. The contributions of commercial floor space and of the public-to-private agglomeration effect compare the full equilibrium with the equilibrium in which $\sigma = 0$ or $\lambda_{PG} = 0$. Wages contribute -0.0010 percentage points, residential rents $+0.0031$, amenities 0.0000, and the tax rate $+0.0021$. The tax rate falls from 5.4967% to 5.4948%. The first-order total is $+0.0042$ percentage points, against the exact change of $+0.0045$. Commercial floor space contributes $+0.0013$ percentage points and the public-to-private agglomeration effect $+0.0027$. Without the public-to-private agglomeration effect, private workplace employment in the receiving towns falls by 2,300 jobs, or 0.69%, instead of rising by 7,596 jobs. With fixed commercial floor space, it rises by 2,673 jobs, or 0.80%.

Table 8. Welfare by channel

Channel	Change (pp)
Wages	−0.0010
Residential rents	+0.0031
Amenities	0.0000
Tax rate	+0.0021
Total, first order	+0.0042
Total, exact	+0.0045
Commercial floor space	+0.0013
Public-to-private agglomeration effect (λ_{PG})	+0.0027

Notes: Changes in welfare in percentage points, $\widehat{\lambda}_{PG} = 0.0375$. Wages, residential rents, amenities, and the tax rate are first-order shares. Commercial floor space and the public-to-private agglomeration effect compare the full equilibrium with the equilibrium in which $\sigma = 0$ or $\lambda_{PG} = 0$.

11 Allocation of Public Jobs

I evaluate the allocation of public jobs with two counterfactuals. Both use the one-pool model at $\widehat{\lambda}_{PG} = 0.0375$.

11.1 Leaving Out One Receiving Town

The first counterfactual leaves out one receiving town at a time. The public jobs relocated to that town are not relocated, and all other relocations take place as planned. For each of the 39 receiving towns with relocated public jobs by 2024, the contribution to welfare is the welfare change of the actual allocation minus the welfare change without that town. A receiving town is more welfare improving the larger its contribution. The 39 contributions add up to 0.00454 percentage points, almost the full welfare change of 0.0045%. Figure 5 ranks the 39 receiving towns. The most welfare improving receiving town is Gunzenhausen, with 230 public jobs and a contribution of 0.00090 percentage points. Per public job, Obernburg a. Main contributes the most. Two receiving towns lower welfare. Kaufbeuren, with 186 public jobs, contributes -0.00061 percentage points, and Kulmbach, with 20 public jobs, contributes -0.00002 percentage points.

11.2 Swapping the Least Welfare Improving Receiving Towns

The second counterfactual replaces the two least welfare improving receiving towns, Kaufbeuren and Kulmbach, with the nearest untreated eligible town by travel time. A town is eligible if it meets the *Heimatstrategie*'s eligibility criteria. Untreated towns received no relocation in any program. There are 61 untreated eligible towns, with 826,639 inhabitants in 2014. Peißenberg, 50.5 minutes away, replaces Kaufbeuren, and Burgkunstadt, 18.8 minutes away, replaces Kulmbach. Each replacement town takes over all relocations of the receiving town it replaces, with the same public jobs, the same office space per job, and the same state-built office floor space. Figure 6 shows the towns switched out and the towns switched in. With the swap, welfare rises by 0.00505% instead of 0.00455%, so the swap adds 0.00050 percentage points, or 11%. Private output in Germany falls by 397 million euros instead of 375 million euros. Private workplace employment rises by 1,027 jobs, or 27.7%, in Peißenberg and by 260 jobs, or 6.7%, in Burgkunstadt.

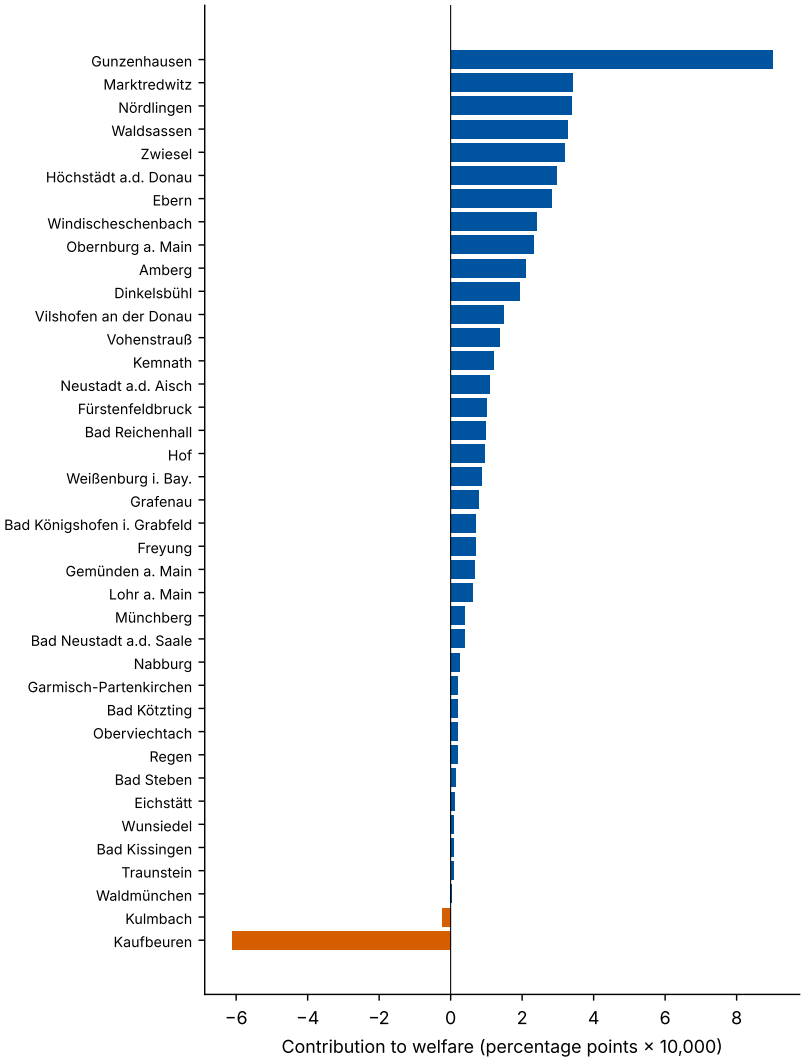


Figure 5. Contribution of each receiving town to welfare. The contribution is the welfare change of the actual allocation minus the welfare change when the public jobs relocated to that town by 2024 are not relocated, in percentage points times 10,000. 39 receiving towns, one-pool model, $\widehat{\lambda}_{PG} = 0.0375$.

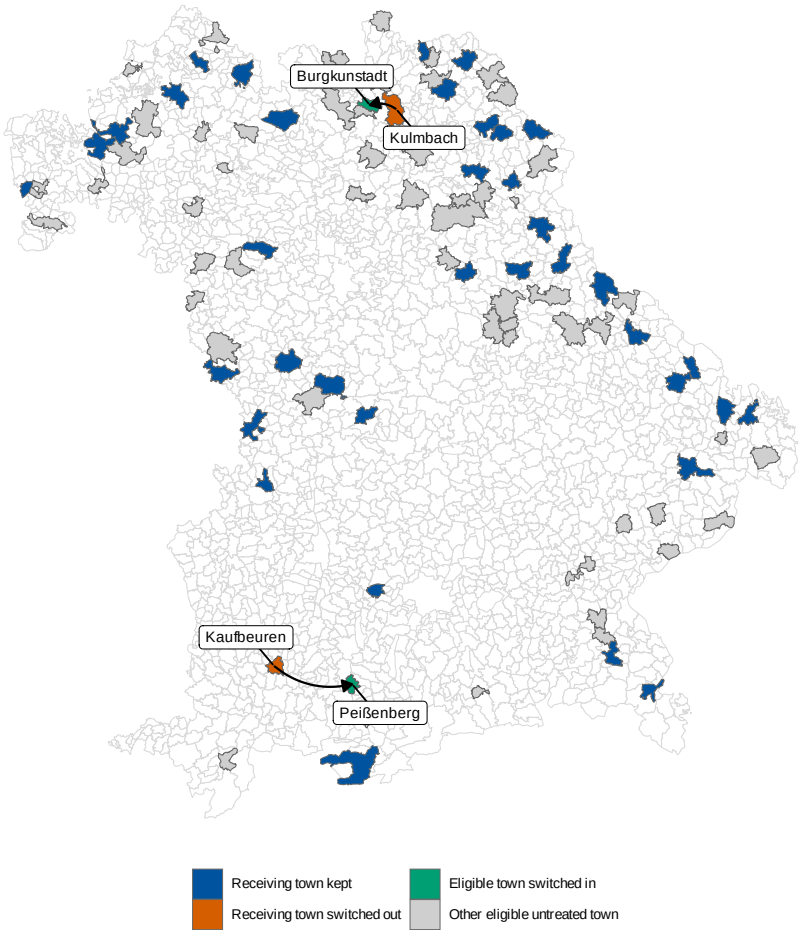


Figure 6. Receiving towns switched out and untreated eligible towns switched in. Kaufbeuren is replaced by Peißenberg and Kulmbach by Burgkunstadt, the nearest untreated eligible towns by travel time. Gray municipalities are the other untreated eligible towns. One-pool model, $\widehat{\lambda}_{PG} = 0.0375$.

12 Conclusion

Can governments narrow regional disparities in economic opportunities by moving public sector jobs from thriving to lagging regions? Unlike conventional place-based policies that subsidize private firms, public employment relocations allow governments to redistribute their own economic activity. Whether these relocations stimulate private employment in receiving regions without harming the regions that lose public jobs is therefore a central policy question.

I study an ongoing program in Bavaria, Germany, that is relocating 5,196 public sector jobs and 1,170 apprenticeships between 2015 and 2030. Using linked employer-employee records, I follow the original agencies' workforce and examine employment adjustments in both receiving and sending municipalities. I construct comparison groups outside Bavaria using the policy's eligibility criteria and estimate the relocations' effects through entropy balancing and synthetic controls. I then use a quantitative spatial model to examine the underlying mechanisms and the aggregate consequences of reallocating public employment.

My first finding is that **public sector jobs move, but incumbent workers largely do not**. Fewer than 2% of the original workforce change residence to follow their jobs, while about 10% work at the new locations without moving. Many remain employed near their previous residence, often in public administration, while others leave regular employment, largely through retirement. Receiving agencies recruit almost entirely outside the original workforce. Their employees are younger, more often women, more educated, and more frequently employed in specialist and expert occupations, yet earn less per day. As the baby-boom generation retires, the policy allows the state to shift recruitment from large cities to smaller labor markets, where uniform public pay is comparatively attractive. Rather than relocating its existing workforce, the government changes where the next generation of public employees is hired.

My second finding is that **private employment increases in receiving locations without corresponding losses in sending locations**. The estimates imply about 1.7 additional private jobs per relocated public job in receiving municipalities. Private employment builds up slowly, with significant gains five to six years after the first relocation. Local employment also increases, while in-commuting does not, suggesting that existing and new residents benefit from the additional jobs. Population and housing prices respond with a similar lag. Residential rents rise by 5 to 7% at longer horizons, compared with private wage gains of 1 to 2.5%, consistent with part of the benefits being capitalized into land values. Employment effects are concentrated in receiving municipalities, while population gains extend to neighboring towns. Removing Bavaria-specific trends strengthens the estimated workplace employment gains, although the evidence on residence employment is more sensitive to this adjustment.

Sending locations do not experience the expected decline in private employment. In Munich, the largest sending location, employment density increases by 3.6 workers per 100 inhabitants relative to its synthetic control, or 5.3 after removing Bavaria-specific trends. Most relocated agencies occupied offices in the city center, where commercial space is scarce and private productivity is high. Their departure may therefore release

space for more productive private activity, offsetting the loss of public employment.

Matching the model to the employment response in the reduced-form yields an estimated public-to-private productivity elasticity of 0.0375. This elasticity is the contribution of the government to agglomeration. For a receiving municipality, a 10% increase in surrounding public employment density, weighted by proximity, raises private firms' productivity by about 0.36%, holding other determinants fixed.

These findings raise several questions for future research. Previous studies of public employment relocations have largely focused on place-level outcomes, often interpreting local employment growth through the arrival and spending of public employees. My worker-level evidence challenges this interpretation for relocations of individual agencies. Establishing whether incumbent workers move, who replaces them, and how accompanying public investment affects local firms would help distinguish the mechanisms through which relocation programs stimulate private activity. In Bavaria, stronger manufacturing employment responses in locations receiving newly constructed offices suggest a possible role for government investment beyond the relocated payroll, although the causal contribution of this investment remains unresolved.

A second set of questions concerns the government's own workforce and the services it provides. If relocations change who works in public administration, do they also change the quality and cost of public service delivery? Can relocating recruitment to smaller labor markets help governments replace retiring public employees without increasing wage expenditure? These questions are particularly relevant as governments face the retirement of the baby-boom generation. The answers may also depend on the functions being relocated. Back-office administration, local public services, and federal government agencies may generate different spillovers and face different location constraints.

Finally, the relocation of public offices raises questions about land use within cities. Do new public agencies shift the location of economic activity and the emergence of city centers in receiving municipalities? What happens to prime urban locations when public agencies leave, and how much of the released space is subsequently occupied by private firms? Understanding these intraurban adjustments would help explain why relocating public employment can benefit both the regions that gain public jobs and those that lose them.

The location of public employment determines more than the geographical distribution of the public wage bill. It also determines where governments recruit workers, how public offices compete with private firms for space, and where public employment generates productivity spillovers. These margins are central to understanding when public sector relocations can reduce regional disparities without imposing large aggregate efficiency costs.

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Appendix

Appendix A documents institutions and costs. Appendix B describes the data and worker samples. Appendix C reports additional worker results. Appendix D gives the empirical specifications and diagnostics. Appendix E reports additional place-level results. Appendix F reports robustness checks. Appendix G derives the model and reports its quantification on all municipalities.

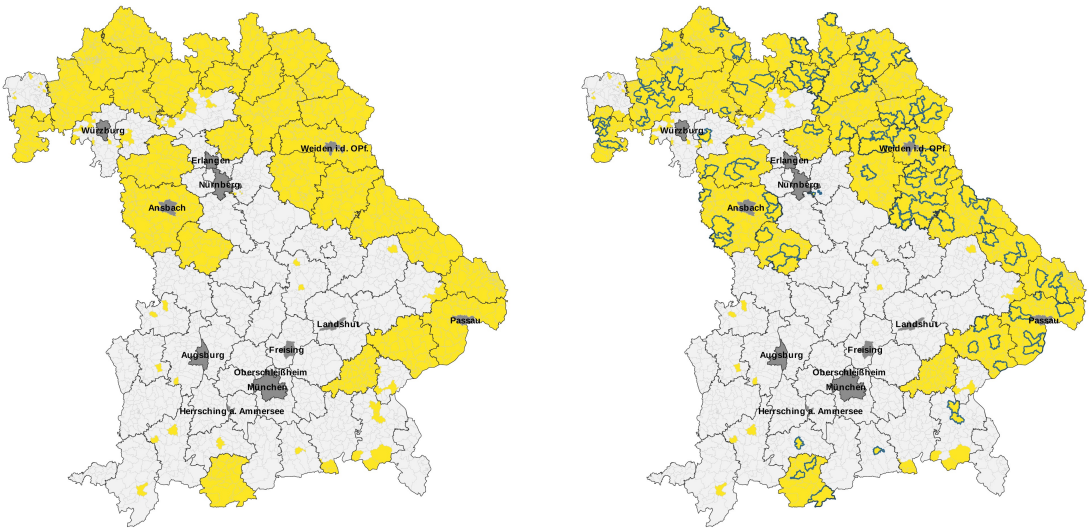
Appendix A: Institutional Setting

Geographical Targeting

Weight	Indicator	District reference period
30%	Projected population change	2010–2030
30%	Unemployment rate	2007–2011
10%	Employment density	2007–2011
20%	Disposable household income per resident	2005–2009
10%	Net migration of ages 18–29 per 1,000 in that age group	2006–2010

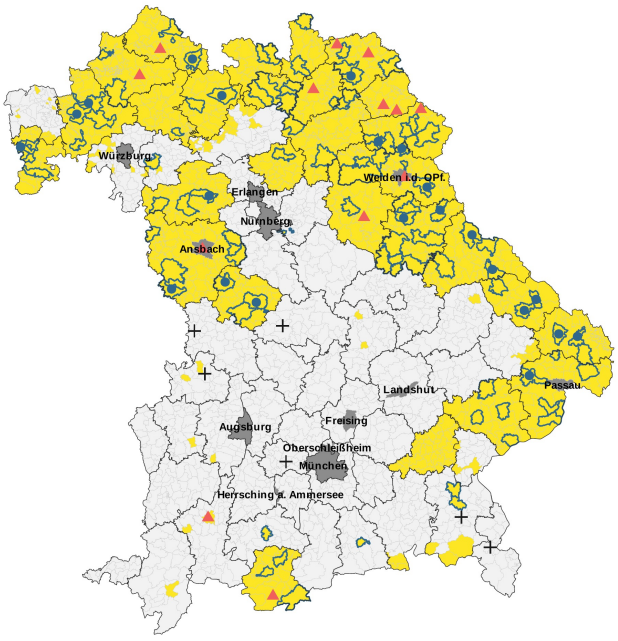
Table 9. Composition of the structural index. Weights and periods refer to the district index underlying the 2014 expansion of eligibility. The Bavarian mean is 100 and the expanded threshold is 90. Municipal qualification uses a comparable index with municipal proxies, including taxable income. Source: Bavarian Ministry of Finance (2015) and StMWi (2014).

The local reconstruction identifies 829 municipalities through the district index and 392 through the municipal index. Their union contains 885 of Bavaria's 2,056 municipalities, with overlap counted once, and the official list contains 886. The reconstruction uses an index value of 90 or below. The government describes eligibility as below 90, and published scores are rounded to one decimal place. Service-center designations and ministerial site selection operate in addition to these index-based eligibility criteria. Figure 7 illustrates these geographical selection layers.



(a) Municipal eligibility

(b) Service-center designations



(c) Receiving and sending locations

Figure 7. Geographical targeting and site selection. Panel (a) marks municipalities meeting the index-based eligibility criteria in yellow. Panel (b) outlines eligible service centers in teal. Panel (c) adds receiving municipalities. Blue circles mark service centers, red triangles other municipalities, and black crosses municipalities outside the index-defined eligible set. Sending locations appear in gray throughout.

Program Boundaries and Position Counts

The apprenticeships include public-service positions organized as degree programs. Participants receive full remuneration during their training, so these positions also bring wage income to receiving places (Freistaat Bayern 2026, Art. 75).

Phase	Administrative designation	Coverage in this paper
I	*Regionalisierung von Verwaltung*	Included
I	*Strukturkonzept – Chancen im ganzen Land*	Included
II	*Behördenverlagerung Bayern 2030*	Included; implementation under way
Other measures	Individual projects outside phases I and II	Not included

Table 10. The two phases of the relocation program. The empirical sample covers projects with an observed opening within the outcome-data window. Source: Bavarian Ministry of Finance (2025) and Kaltenhauser (2023).

Phase	Intended jobs	Apprenticeships
I	2,511	730
II	2,685	440
Total	5,196	1,170

Table 11. Intended final staffing across both phases. Source: Bavarian Ministry of Finance (2026, pp. 6, 16). These are the government's final staffing figures, not current employment or the number of incumbents who move. Apprenticeships include remunerated public-service degree programs.

The intended jobs equal 1.668% of the pre-policy direct state workforce (5,196/311,580). Including apprenticeships gives 2.043% (6,366/311,580). Section 2 reports the jobs-only ratio to keep the two categories distinct.

Public Employment and Pay

Contractual employees are paid under the collective agreement for state-government employees which Bavaria negotiates jointly with the other participating German states. Wage setting is thus coordinated nationally rather than determined by individual agencies or municipalities. Civil-service pay is set by Bavarian law. Both systems use schedules in which the job determines the pay grade and experience determines progression within it, much as grades and steps organize pay in the US federal General Schedule (TdL 2025; Freistaat Bayern 2026; OPM 2026).

Appendix Table 12 gives the corresponding civil-service grades and pay ranges, distinguishing career level from experience.[16]

[16]A public-sector wage premium also depends on the private-sector alternative. Using German survey

Germany's June 2024 public-service workforce comprised approximately 527,000 federal employees, 2.67 million state employees, 1.80 million municipal employees, and 381,000 employees of social insurance institutions (Destatis 2025; Destatis 2026). In the World Bank series, the public wage bill remained close to one-sixth of total public expenditure from 2000 to 2019. Its share declined in 2020 as total spending rose during the pandemic (World Bank 2024).

Bavaria's direct state administration employed 311,580 people in 2014, including 213,090 civil servants and judges. The count includes full-time and part-time staff and excludes municipal employers and legally separate public institutions (Statistical Offices 2014; Bavarian Statistical Office 2014). I use this population to benchmark the policy's intended size.

	Monthly gross base pay (€)	
Grade (published steps)	First step	Last step
E9a (1, 6)	3,520.10	4,490.04
A9 (2, 10)	3,294.99	4,108.12
E13 (1, 6)	4,629.74	6,580.44
A13 (5, 11)	5,247.58	6,168.25

Table 12. Contractual and civil-service pay schedules, effective February 1, 2025. Amounts are full-time monthly base pay at each grade's first and last published steps. The endpoints do not represent equal experience across employment statuses. Annual special payments and other compensation are excluded. Source: LfF (2025b) and LfF (2025a).

Bavarian budget law pairs E9 with A9 and E13 with A13 for the purpose of comparing budgeted posts. This is not a rule assigning employees to grades (Freistaat Bayern 2024, Art. 6a). E9a covers administrative duties accessible without a university degree. In the non-graduate civil-service career, workers normally enter at A6 or A7 and reach A9 through promotion. A9 can also be an entry grade for the degree-based career track. E13 and A13 illustrate master's-level work rather than all university-qualified employment (TdL 2025; Freistaat Bayern 2026).

A non-graduate who reaches A9 through promotion will already have accumulated service. The first published A9 step therefore does not provide an equal-experience comparison with an E9a entrant. Civil servants and contractual employees also face different pension and insurance arrangements, so gross base pay alone does not determine their relative take-home pay (Freistaat Bayern 1992). A net-pay comparison would require a specified career history, tax situation, and health-insurance arrangement. For the policy, the common feature is that a given grade and experience step carries the same scheduled base pay across Bavarian municipalities.

Reported category	Amount (€)
Budgeted temporary staffing costs, 2016–2022	16,623,849.00
Construction expenditure, four sites	74,923,320.64
Renovation expenditure	3,708,003.37
Standardized moving-cost estimates, 2015–2021	450,713.00
Standardized rental-cost estimates, 2015–2022	18,188,015.00
Sum of reported categories	113,893,901.01

Table 13. Reported cost categories for completed relocation projects. Source: Kaltenhauser (2023, pp. 13–14), response dated May 23, 2023. The sum combines actual construction and renovation spending, budgeted costs of temporary posts, and standardized estimates. Categories cover different periods. It is neither a measure of total cash expenditure nor the full program's lifetime net cost.

Implementation Costs

For three tax-office relocations, the same report provides construction spending and the number of jobs established by completion. Höchstädt an der Donau accounts for €9,712,273.52 and 53 jobs, Waldmünchen for €4,358,424.71 and 21 jobs, and Bad Kötzting for €2,691,643.64 and 20 jobs (Kaltenhauser 2023, pp. 8, 13). Summing spending and jobs across these sites gives €16,762,341.87/94, or €178,323 per public job, rounded to €180,000. This construction-only ratio excludes other transition costs, savings at origins, and the value of the resulting assets. It does not measure the fiscal cost per additional private job.

Regional investment subsidies under the German GRW program provide a further benchmark. Using regional variation in subsidy rates, Grunau et al. (2025) estimate a cost of about €25,000 per additional job. Their causal employment denominator differs from the public positions accommodated by the three relocation projects.

For Northvolt's planned battery plant in Heide, the European Commission approved €700 million in direct grants and €202 million in guarantees in January 2024 (European Commission 2024). The government announced 3,000 direct jobs (German Federal Government 2024). Dividing the grants by planned direct employment gives €233,333 per job, rounded to €230,000. This calculation excludes guarantees, whose face value is not a grant expenditure, and projected indirect jobs.

For Intel's planned semiconductor plant in Magdeburg, the federal government proposed €9.9 billion in support in June 2023, subject to European Commission approval, alongside about 3,000 direct jobs (German Federal Government 2023). This implies €3.3 million per planned direct job, excluding temporary construction and projected indirect employment. Intel canceled the project in July 2025 (Intel 2025). This ratio therefore describes the original subsidy offer, not a realized cost per job.

Both firm-specific benchmarks measure announced support per planned direct job, not a causal cost per additional job. All amounts are in nominal euros at their respective source dates. A cost-effectiveness comparison would require net policy costs and additional

data, Bonaccolto-Töpfer et al. (2022) find a wage advantage for women but a pay penalty for men after accounting for worker characteristics and selection. These estimates refer to Germany as a whole.

employment measured over comparable horizons.

Regional Background

Bavaria lies in southeastern Germany, bordering Austria and the Czech Republic. Its 13.25 million inhabitants in 2024 made it Germany's second most populous state, with a population comparable to Pennsylvania's (Bavarian Statistical Office 2025a; U.S. Census Bureau 2025). It is also Germany's largest state by area (Bavarian Statistical Office 2025b).

Selection and Implementation

Ministries assessed whether potential destinations offered suitable premises and infrastructure for the agencies they proposed to relocate. They selected sites in coordination with the finance ministry. The cabinet approved the program, and the responsible ministries implemented it (Kaltenhauser 2023).

Worker Mobility Provisions

Workers can seek reassignment, while those who follow an agency can receive training and support for moving or commuting, including a conditional €3,000 mobility payment. Telework is available where duties permit (Bavarian Ministry of Finance 2021). In practice, two work-from-home days per week are common.

Appendix B: Data

Relocation Records

The project records distinguish intended staffing, reported implementation dates, and construction milestones. Source links document the evidence behind each entry. A program entry may represent an entire agency, a branch, or part of an existing organization. Neither an agency's continued presence at its origin nor the absence of a separately named destination in the register establishes that no employment was relocated.

Implementation often unfolds over several years. An agency may open a branch before moving into its permanent building, while staffing grows toward the announced target. I therefore distinguish the start of operations from construction milestones and intended final staffing. I use the official state government sources to establish implementation dates. The program totals in the preceding section describe the intended scale of program rollout phases I and II.

Structured News Search

I conducted a structured German-language news search in Nexis Uni using the search string

> **Behördenverlagerung OR Behördenumzug OR "Umzug von Behörden" OR "Umzug vom Amt" OR "Amtsumzug"**.

I restricted the search period to articles published from 1991 onward and limited the sample to news articles, excluding private-company publications, individual webpages, and legal texts.

The query yielded 1,531 results. I identified news reports on 22 federal and 87 state agencies between 2005 and 2032. In total, the reports cover 13,395 relocated jobs. The largest number of relocations occurs in 2019, with 5,843 jobs, followed by 2030, with 2,300 jobs, and 2021, with 1,070 jobs. Table 14 reports the number of relocated jobs and apprenticeships by destination state, and Table 15 lists all sending and destination cities.

The search identifies both single-agency relocations and broader relocation programs. The three main programs are the federal program for coal regions, the Bavarian *Heimatstrategie*, and the Saxon *Lausitzprogramm 2038*. I use this search to document the broader institutional context of public employment reallocation in Germany.

State	N of Relocated Jobs	N of Relocated Study Places
Bavaria	9,681	1,730
Brandenburg	510	
Hesse	100	
Saxony	2,214	
Saxony-Anhalt	850	
Thuringia	40	

Table 14. Number of public-sector jobs and apprenticeships relocated to each state according to the Nexis Uni news search.

Sending Cities	Destination Cities
Ammersee	Amberg, Ansbach, Augsburg
Berlin, Bonn	Bad Kissingen, Bad Königshofen im Grabfeld,
Bad Kissingen	Bad Kötzting, Bad Neustadt an der Saale, Bad Reichenhall,
	Bad Steben, Bautzen, Berlin, Bischofswerda, Borna
	Cottbus
Dresden	Dessau, Dinkelsbühl, Dresden
Erlangen, Eschborn, Essen	Ebern, Eichstätt
	Freital, Freyung, Fürstenfeldbruck, Fürth
Großenhain	Garmisch-Partenkirchen, Gemünden am Main, Gera,
	Görlitz, Grafenau, Gunzenhausen
Herrsching a. Ammersee	Halle (Saale), Höchstädt a.d.Donau, Hof
	Ingolstadt
	Kaufbeuren, Kemnath, Kitzingen, Kronach, Kulmbach
	Lauterbach, Leipzig, Lohr am Main
München	Marktredwitz, Meißen, Merseburg, Mittenwald
	Mühldorf a. Inn, Münchberg
Nürnberg	Nabburg, Neustadt an der Aisch, Nördlingen, Nürnberg
	Obernburg am Main, Oberviechtach
	Pegnitz
Regensburg	Regen, Rosenheim
Starnberg	Schwandorf, Schweinfurt
	Tirschenreuth, Traunstein
	Vilshofen an der Donau, Vohenstrauß
Würzburg	Waldkirchen, Waldmünchen, Waldsassen, Weiden in der Oberpfalz
	Weißenburg i. Bay., Weißwasser, Windischeschenbach, Wunsiedel
	Zittau, Zwiesel

Table 15. Sending and destination cities identified in the Nexis Uni news search, listed alphabetically. Some cities, such as Berlin and Dresden, appear both as sending and destination cities.

Public-Agency Register

I reconcile the January 2014, March 2023, and June 2026 editions to follow agencies across changes in names, identifiers, and addresses. Comparing these records with the policy histories helps distinguish a move to another municipality from an address correction or an organizational change. It also reveals why a partial relocation may leave the parent agency at its original address.

I reconcile the 2014, 2023, and 2026 register editions first by agency identifier. Among unmatched records, I use normalized agency names that occur only once in each edition and then normalized addresses that occur only once in each edition. Normalization removes differences in capitalization, punctuation, and spacing. I review location changes against agency and program sources, distinguishing changes within a municipality from moves between municipalities. Shared addresses and renamed units require particular care. The register records administrative units and locations. It is not an employment census. Its coverage includes state agencies and schools, as well as other institutions with state personnel. These cross-year links remain separate from the establishment identifiers in the IAB data.

IAB descriptive samples and candidate establishments

Table 16 describes employment growth and the administration share in receiving and sending municipalities. Workplace employment grew by 6.6% in receiving municipalities and 13.5% in sending municipalities between 2014 and 2024. Employment among residents grew by 11.1% and 15.9%, respectively.

	Receiving municipalities		Sending municipalities	
	2014	2024	2014	2024
Workplace employment (2014 = 100)	100	106.6	100	113.5
Administration workplace employment (2014 = 100)	100	120.7	100	121.5
Other-industry workplace employment (2014 = 100)	100	105.8	100	113.1
Employed residents (2014 = 100)	100	111.1	100	115.9
Administration share of workplace employment (%)	5.01	5.51	5.10	5.61
Establishments	778	716	12,187	11,707
Municipalities	40	40	9	9

Table 16. Employment growth and composition in treated municipalities. Employment indices set the corresponding 2014 mean to 100. Administration shares are unweighted mean municipal percentages. Establishments are unweighted municipal means. Administration denotes WZ 2008 division 84.

Goldschmidt and Schmieder (2017) trace outsourced jobs through clustered flows of workers who move together between establishment identifiers in German social-security records. I build on their idea of following jobs through establishment and worker identifiers in the Integrated Employment Biographies of the IAB (IAB FDZ 2026; IAB 2017). I first

select public administration establishments[17] at the workplace postcodes of the program's origins and destinations.

Table 16 reports the treated municipalities in the samples used for the municipal analysis. The comparison uses controls outside Bavaria. Treatment dates come from the official year where available and otherwise the recorded completion year. The sample retains *Regionalisierung von Verwaltung* projects with a treatment date by 2024 and excludes projects that are still being planned. The receiving sample contains 40 municipalities and the sending sample nine. The table reports municipal means, rather than pooled worker totals.

For the worker analysis, workplace postcodes identify public-administration establishments near program origins and destinations. An establishment is classified by its identifier and postcode. A postcode can appear among both origins and destinations and is retained in both sets. Establishments observed in both 2010 and 2013 provide the baseline distribution of employment changes over that interval. I calculate its standard deviation separately for the origin and destination groups. Among continuing establishments, the change from 2014 to 2024 must be at least twice the destination-group standard deviation to qualify as a candidate expansion, or at most minus the origin-group standard deviation to qualify as a candidate contraction. Establishments observed only in 2024 qualify as candidate openings, and those observed only in 2014 as candidate closures. These rules define the establishments used in the descriptive worker comparisons.

The source cohort consists of workers employed at candidate sending establishments in 2014. Residential movers and commuters are distinguished among those employed at candidate receiving establishments in 2024 using changes in recorded residence. Employees at candidate receiving establishments who were not in the source cohort are additional employees relative to that cohort, not necessarily new labor-market entrants. Workers no longer observed in regular employment in 2024 are very likely retirees.

Model Employment Inputs

Workplace and residence counts by municipality distinguish public administration, defense, and social security (WZ 2008 division 84) from all other industries, which define private employment. They supply the baseline cross-section for the model, while the annual panel supplies the empirical changes. Because civil servants are outside these records, baseline public employment in the model covers employees subject to social-security contributions.

Table 17 reports the administration shares of municipal employment in 2014. Administration accounts for an average of 6.2% of workplace employment across German municipalities and 5.0% in the policy state. Weighted by employment, the shares are 4.9% and 4.2%. Workplace and residence measures describe different sides of local labor markets and provide the baseline for the model.

[17] WZ 84 is division 84 of the German classification of economic activities (WZ 2008), public administration and defense and compulsory social security.

	Germany		Bavaria	
	Mean	SD	Mean	SD
Administration share of workplace employment (%)	6.2	8.1	5.0	4.3
Administration share of employed residents (%)	6.1	2.3	4.8	1.3
Aggregate share of workplace employment (%)	4.9		4.2	
Municipalities	10,637		2,055	

Table 17. Administration shares of municipal employment in 2014. Unweighted means and sample standard deviations across the municipalities of the model. Sector shares are percentages calculated within each municipality. The aggregate share divides administration employment by total workplace employment of the group. Administration comprises public administration, defense, and compulsory social security (WZ 2008 division 84). Employment subject to social-security contributions. Civil servants are not covered. 1,131 municipalities, 104 of them in Bavaria, have no administration workplace employment.

Travel Times and Commuting

The model's commuting decay comes from flows of private employees between municipalities in the 2014 IAB records. Federal Employment Agency flows of Bavarian residents provide an untargeted check. For the model, the travel-time matrix describes car journeys between municipalities, in minutes.

Rents and Floor Space

The floor area of the office buildings the state constructed for the program calibrates the office space of public jobs.

I take the corresponding municipal office-rent measures for 2014.

I take the municipal residential rent measures for 2014, the baseline year of the model. These inputs describe the housing costs households face in the model and allow location choices to depend on more than wages and commuting.

Worker Sample Definitions

- *Incumbents* are all regular employees of closed or shrinking origin public sector agencies in 2014, 2,352 workers in total.
- *Movers* are incumbents who work at an opened or expanding destination public sector agency in 2024 and have changed their municipality of residence.
- *Commuters* are incumbents who work at an opened or expanding destination public sector agency in 2024 and still live in the same municipality as before the relocation.
- *Stayers* are incumbents in regular employment outside these public sector agencies in 2024 who still live in the same municipality.

- *Incumbents no longer observed* are not in regular employment in 2024, because they exited the social security system (civil service or retired).
- *New employees* are workers at opened or expanding destination public sector agencies in 2024 who were not incumbents.

Appendix C: Worker Adjustment and Public Capacity

Worker Adjustment

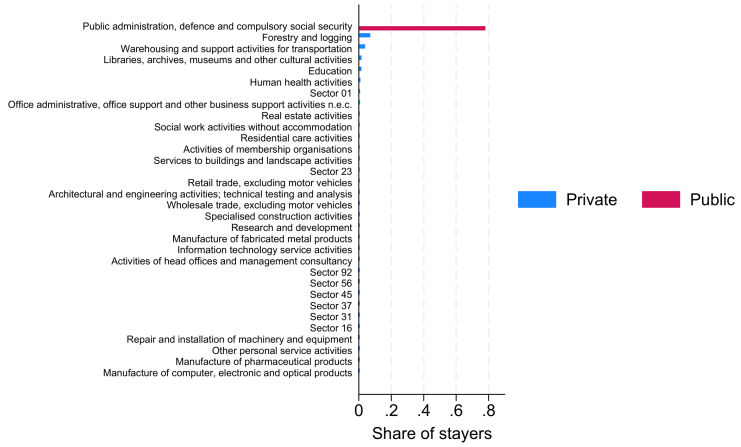
In Danish firm relocations, wages rise by about 0.15% per extra kilometer of commuting three years after the move (Mulalic et al. 2014), but the uniform German public sector pay schedule rules this out.

The staff of closed public sector agencies were older than the local workforce, with a mean age of 47.3 years against 41.8 in the sending municipalities (Table 18). More than a quarter of them (28%) were aged 55 or older, against 16% locally, so they reached retirement age while the program ran from 2015 to 2030.

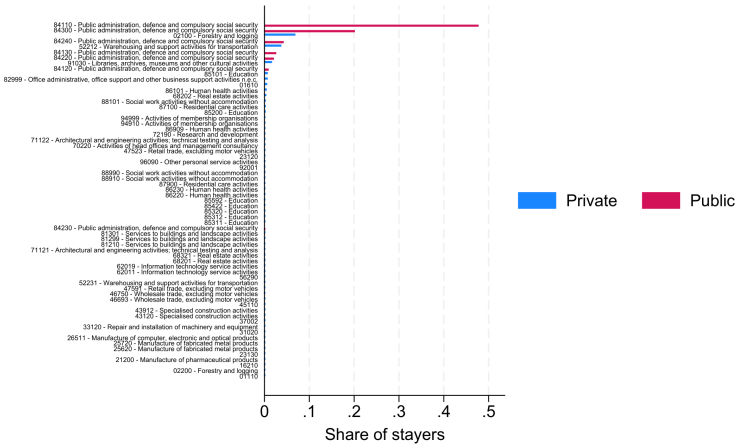
Women value commuting time about 20% more than men (Le Barbanchon et al. 2021), so public jobs in small towns may reach a female labor pool with high commuting costs.

In other relocation programs like in Denmark, workers do not move (Rigsrevisionen 2018).

The figures below show the sectors of stayers in 2024 and of new employees in 2014, and the female share and educational attainment of closed and opened agencies against their municipalities' 2014 averages.

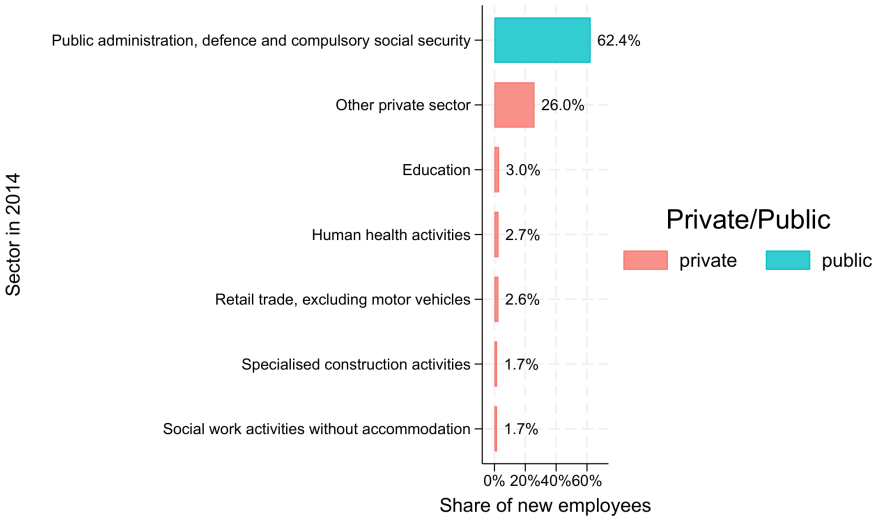


(a) Broad industries

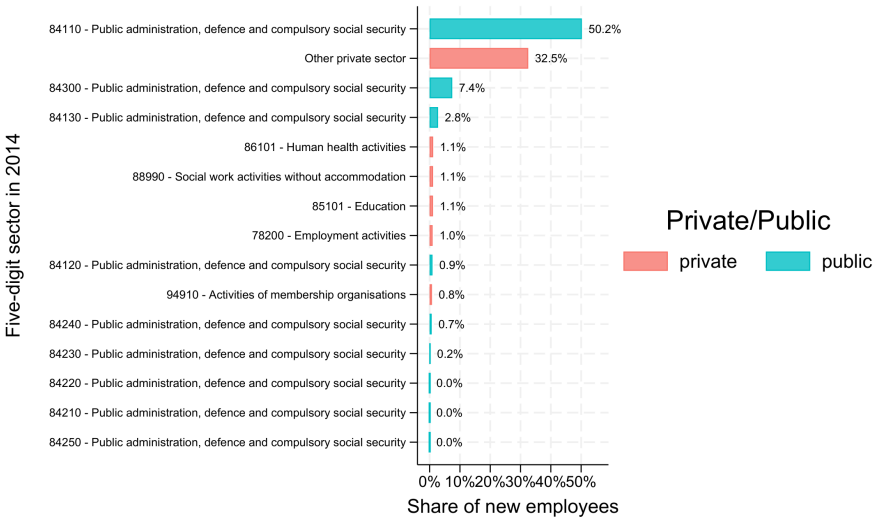


(b) Detailed activities

Figure 8. Employment sectors of stayers in 2024. Shares among original sending workers who retain their municipality of residence, work outside receiving establishments, and have a recorded sector in 2024.



(a) Broad industries



(b) Detailed activities

Figure 9. New hires' employment sectors before the program. Shares among workers at receiving establishments in 2024 who were outside the original sending workforce and have a recorded sector in 2014.

Public Capacity

Table 18. Closed public sector agencies and sending municipalities

	Closed public sector agencies (2014)	Sending municipalities (2014 avg.)	Closed − sending
Mean worker wage (gross EUR/day)	108.7	108.1	+0.6
Share women	0.48	0.50	−0.02
Mean education (1–3)	2.16	2.18	−0.02
Mean age (years)	47.3	41.8	+5.5
Share under 35	0.15	0.32	−0.17
Share 55+	0.28	0.16	+0.12
Share foreign	0.01	0.13	−0.12
Share helper	0.09	0.14	−0.05
Share skilled	0.71	0.53	+0.18
Share specialist or expert	0.20	0.33	−0.13

Notes: Closed public sector agencies are public administration establishments (WZ 84) at sending postcodes that exist in 2014 but not in 2024. Their values pool all their regular employees in 2014. Sending municipalities are unweighted averages of 2014 municipal values across all sectors. Wages are deflated gross daily wages. Education is coded 1 for no vocational qualification, 2 for vocational training, and 3 for an academic degree. Occupation shares use the requirement level of the KldB 2010 occupation among workers with a valid code. Differences are computed from the rounded values.

Table 19. Opened public sector agencies and receiving municipalities

	Opened public sector agencies (2024)	Receiving municipalities (2014 avg.)	Opened − receiving
Mean worker wage (gross EUR/day)	103.5	93.4	+10.1
Share women	0.64	0.48	+0.16
Mean education (1–3)	2.30	2.05	+0.25
Mean age (years)	44.3	42.6	+1.7
Share under 35	0.26	0.29	−0.03
Share 55+	0.27	0.18	+0.09
Share foreign	0.04	0.08	−0.04
Share helper	0.10	0.16	−0.06
Share skilled	0.58	0.62	−0.04
Share specialist or expert	0.32	0.21	+0.11

Notes: Opened public sector agencies are public administration establishments (WZ 84) at receiving postcodes that exist in 2024 but not in 2014. Their values pool all their regular employees in 2024. The other definitions follow Table 18.

Table 20. Opened public sector agencies and closed public sector agencies

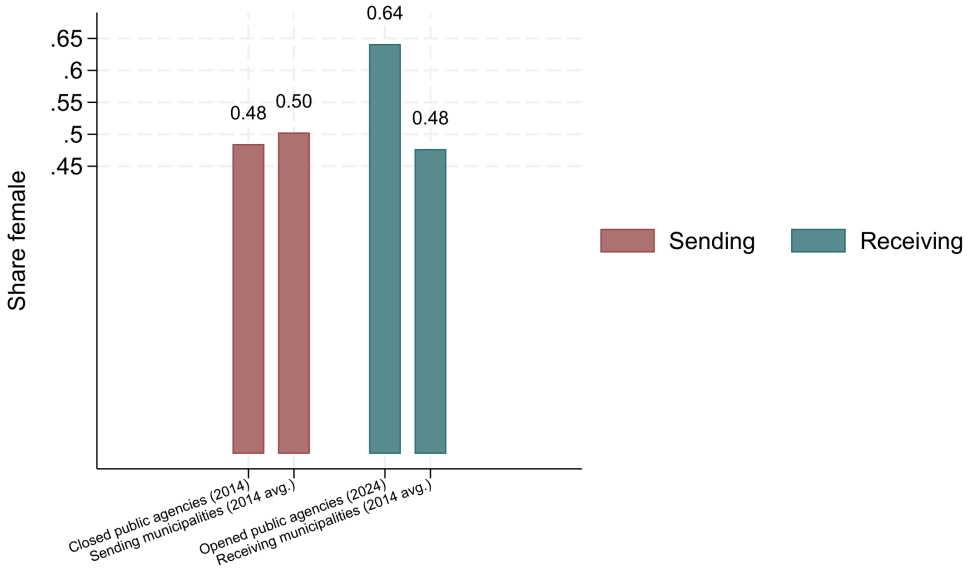
	Opened public sector agencies (2024)	Closed public sector agencies (2014)	Opened − closed
Mean worker wage (gross EUR/day)	103.5	108.7	−5.2
Share women	0.64	0.48	+0.16
Mean education (1–3)	2.30	2.16	+0.14
Mean age (years)	44.3	47.3	−3.0
Share under 35	0.26	0.15	+0.11
Share 55+	0.27	0.28	−0.01
Share foreign	0.04	0.01	+0.03
Share helper	0.10	0.09	+0.01
Share skilled	0.58	0.71	−0.13
Share specialist or expert	0.32	0.20	+0.12

Notes: Definitions follow Tables 18 and 19.

The agency-profile comparisons in Figures 10 and 11 use closed establishments in 2014 and opened establishments in 2024. The worker classification also includes substantial contractions and expansions, as defined above.

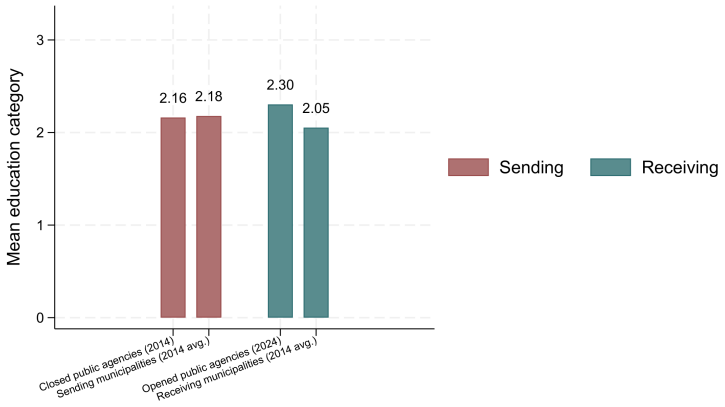
Experience and Pay

Under § 16 of the German public sector pay schedule, a hire without relevant experience starts at step 1. Progressing in the scale happens through more years of experience and reaching step 6, for example, takes 15 years.

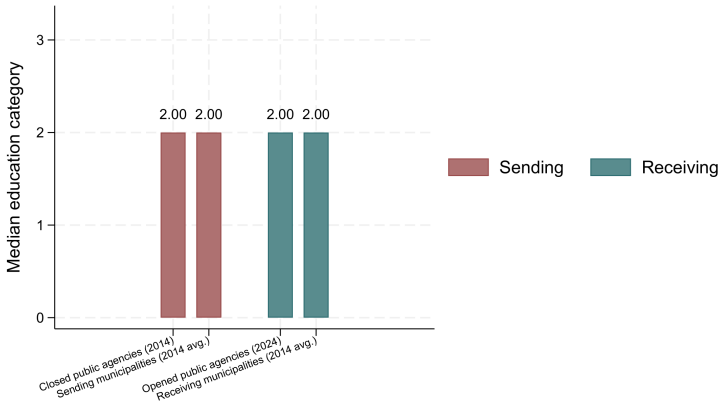


(a) Female employment shares

Figure 10. Female employment shares at sending and receiving agencies. Closed sending agencies in 2014 and newly opened receiving agencies in 2024 are compared with their respective municipalities' 2014 averages.



(a) Mean educational attainment



(b) Median educational attainment

Figure 11. Educational attainment at sending and receiving agencies. The panels compare closed sending agencies in 2014 and newly opened receiving agencies in 2024 with their municipalities' 2014 averages. Categories distinguish no formal vocational qualification, vocational training, and tertiary education.

Appendix D: Empirical Methodology

Policy Rule and Reconstruction

Districts are counties, and a county-free city with fewer than 100,000 inhabitants is merged with its surrounding county. The policy state has 96 counties and 79 districts. In 2014, 28 of the 79 districts had an index below 90 and covered 829 municipalities, and 57 further municipalities qualified through their own index. In total, 886 of the 2,056 municipalities of the policy state were eligible.

Table 21 lists the five indicators of the municipal index and of the district index. For a municipality i, the composite index is

$$I_i = 0.3\, I_i^{F} + 0.3\, I_i^{U} + 0.1\, I_i^{D} + 0.2\, I_i^{Y} + 0.1\, I_i^{M}. \tag{15}$$

With x_i the raw indicator of municipality i, x_{P} its value for the policy state as a whole and $x_{\min}$ its lowest value among the units of the policy state, the indicators are scaled as

$$\begin{aligned} I_i^{F} &= 100\,\frac{x_i^{F} - x_{\min}^{F}}{x_{\mathrm{P}}^{F} - x_{\min}^{F}}, \qquad I_i^{U} = 100\,\frac{x_{\mathrm{P}}^{U}}{x_i^{U}}, \qquad I_i^{D} = 100\,\frac{x_i^{D}}{x_{\mathrm{P}}^{D}}, \\ I_i^{Y} &= 100\,\frac{x_i^{Y}}{x_{\mathrm{P}}^{Y}}, \qquad I_i^{M} = 100\,\frac{x_i^{M} - x_{\min}^{M}}{x_{\mathrm{P}}^{M} - x_{\min}^{M}}. \end{aligned} \tag{16}$$

Unemployment enters inverted, so a lower index always means a weaker location. The policy state describes this scaling only in words.

Indicator	Weight	Municipal index	District index
Population forecast	0.3	Percentage change of the population 2009 to 2021	Percentage change of the population 2010 to 2030
Unemployment (inverted)	0.3	Unemployed per 100 employees at residence plus unemployed, 30 June 2012	Unemployment rate, mean 2007 to 2011
Employment density	0.1	Employees at workplace per 100 inhabitants, 2012	Employees at workplace per 1,000 inhabitants, 2007 to 2011
Income	0.2	Income per taxpayer, 2007	Disposable income per inhabitant, 2005 to 2009
Net migration of young adults	0.1	Net migration of 18 to 29 year olds per 1,000 of that age, 2011	Net migration of 18 to 29 year olds per 1,000 of that age, mean 2006 to 2010

Table 21. Indicators of the policy rule. Employees are employees subject to social security contributions. Income per taxpayer is the total amount of income divided by the number of taxpayers. District values are computed from the summed counts of their counties. Source: Bavarian Ministry of Finance (2015) and StMWi (2014).

I compute every indicator for all municipalities and districts in Germany with the definitions and years of the policy rule and with the values of the policy state as the reference. Unemployed and employees come from the Federal Employment Agency, population and migration from the regional statistics of the statistical offices, and income

per taxpayer from the income tax statistics of 2007. Each district value is built from the summed counts of its counties and never from averages of rates.

The population forecast term requires its own construction, because no forecast with the method of the policy state exists outside the policy state. For districts, I project the population from 2010 to 2030 with a constant annual growth rate equal to the natural balance plus net migration, averaged over 2006 to 2010. On the 96 counties of the policy state, this projection has a correlation of 0.94 with the official forecast. Constant rates overstate the spread of the official forecast, so I map the projection onto the official scale with the line fitted in the policy state, $x^F = -1.30 + 0.714\,\tilde{x}^F$, where $\tilde{x}^F$ is my own projection. The municipal forecast from 2009 to 2021 is no longer published, even for the policy state. I therefore back out the official forecast term of each municipality in the policy state from its official composite index and the four other indicators. I then predict the forecast term for all municipalities in Germany from information available in 2009. The predictors are the municipal population growth from 2008 to 2009, the county population growth from 2005 to 2009, the calibrated county forecast, the shares of residents aged 65 and over and of women aged 20 to 39, and log population. In a tenfold cross-validation on the municipalities of the policy state, the rank correlation of the predicted with the official forecast term is 0.76. Within the policy state, I use the official forecast term.

Measured against the policy state, 82.4% of West German and 99.8% of East German municipalities would be eligible, almost all of them through their district.

Central Place Prediction

A classifier trained on the 2,056 municipalities of the policy state predicts the official central place status from log population in 2014, log employment density in 2012, the population within 30 minutes' drive and the own share of it, the number of larger municipalities within 30 minutes and the drive time to the nearest one, and eligibility under the policy rule. Its cross-validated area under the curve is 0.983.

Cells and Donor Selection

The route to eligibility has four categories. A municipality qualifies through its own index and its district, through its district only, through its own index only, or not at all.

I also exclude municipalities named in other relocation programs, municipalities with suppressed data or boundary changes, the Tesla site in Grünheide, eight sites of announced large industrial plants, and municipalities that hosted refugee reception centers, identified by an increase of the working-age population in 2015 or 2016 above 3% of its 2014 level.

Entropy Balancing

For cell c, with donors $\mathcal{D}_c$ and receiving locations $\mathcal{T}_c$, the weights solve

$$\begin{aligned} \min_{\{w_j\}_{j \in \mathcal{D}_c}} \quad & \sum_{j \in \mathcal{D}_c} w_j \log \frac{w_j}{q_j} \\ \text{subject to} \quad & \sum_{j \in \mathcal{D}_c} w_j X_j = \bar{X}_{\mathcal{T}_c}, \qquad \sum_{j \in \mathcal{D}_c} w_j = 1, \qquad w_j \geq 0. \end{aligned} \tag{17}$$

The vector X_j contains the five indicators of donor j, and $\bar{X}_{\mathcal{T}_c}$ contains their means among receiving locations in the cell. The base weights are $q_j = 1/|\mathcal{D}_c|$, so entropy balancing changes equal donor weights as little as possible while meeting the constraints. Every donor in a cell receives a positive weight. The comparison outcome is $\bar{Y}_{C,t} = \sum_{j \in \mathcal{D}_c} w_j Y_{jt}$.

Table 22 reports the cells. The 40 receiving locations fall into seven cells. Five cells contain between 156 and 2,417 donors. Their effective number of donors, $1/\sum_j w_j^2$, ranges from 9.6 to 261, and the largest single weight ranges from 0.02 to 0.24. In total, 4,174 donors carry a positive weight for at least one receiving location.

Route to eligibility	Central place	Receiving locations	Donors	Effective donors	Largest weight
Own index and district	Yes	18	664	37.1	0.09
District only	Yes	13	225	23.6	0.11
Not eligible	Yes	5	156	9.6	0.24
Own index and district	No	1	2,417	261.0	0.02
Not eligible	No	1	710	34.3	0.12
Own index only	Yes	1	1	1.0	1.00
District only	No	1	1	1.0	1.00
Total		40	4,174		

Table 22. Cells of route to eligibility under the policy rule and central place status. Donors are municipalities outside the policy state in the same cell. The effective number of donors is $1/\sum_j w_j^2$. Kaufbeuren (own index only, central place) and Bad Steben (district only, not a central place) each have one control, the donor in their cell closest by Mahalanobis distance. The total counts every donor once.

Two exceptions apply. First, six receiving locations do not meet the policy rule. These are Nördlingen, Traunstein, Höchstädt an der Donau, Bad Reichenhall, Eichstätt and Fürstenfeldbruck. They are compared with donors that would not have been eligible either. Second, Kaufbeuren and Bad Steben lie outside the range of the donors in their cells, so no weights meet the constraints in Equation 17. Each of them gets one control, the donor in its cell with the smallest Mahalanobis distance

$$d_{ij}^2 = (X_i - X_j)' S^{-1} (X_i - X_j), \tag{18}$$

where S is the covariance matrix of the five indicators.

The covariance adjustment accounts for the different scales and correlations of the matching variables.

Balance

Variable	Receiving	Weighted controls	SMD
Population forecast term	76.30	77.23	−0.03
Unemployed per 100, 2012	5.42	5.93	−0.15
Employment density, 2012	49.18	49.52	−0.02
Income per taxpayer, 2007	29,334	29,467	−0.02
Net migration of young adults, 2011	−5.32	−4.12	−0.02
Not used in the balancing			
Log population, 2014	9.41	9.26	+0.10
Change in log employment density, 2011 to 2013	0.02	0.04	−0.09
Change in log population, 2011 to 2013	−0.00	−0.00	+0.01
Change in unemployed per 100, 2009 to 2012	−2.35	−2.12	−0.15
Change in log income per taxpayer, 2010 to 2013	0.11	0.11	−0.04
Change in net migration of young adults, 2011 to 2013	5.25	−0.55	+0.07
Log workplace employment in the year before the first relocation	8.77	8.62	+0.07

Table 23. Balance after entropy balancing within cells of route to eligibility under the policy rule and central place status, with controls outside the policy state. Entries are means of receiving locations and their weighted controls and standardized mean differences.

Sending Locations and Synthetic Controls

Table 24 compares each sending location with the municipality of the policy state closest in population among those never named as a receiving or sending location. Munich has 1.43 million inhabitants, and the closest such municipality is Bamberg with 71,952 inhabitants, 5% of Munich's population. The closest municipality has 14% of the population of Nürnberg, 26% of Augsburg, 58% of Würzburg and 68% of Erlangen.

Sending location	Population 2014	Rank in the policy state	Closest municipality not in the programs	Population 2014	Ratio
Munich	1,429,584	1	Bamberg	71,952	0.05
Nürnberg	501,072	2	Bamberg	71,952	0.14
Augsburg	281,111	3	Bamberg	71,952	0.26
Würzburg	124,219	6	Bamberg	71,952	0.58
Erlangen	106,423	8	Bamberg	71,952	0.68
Landshut	67,509	12	Aschaffenburg	68,167	1.01
Freising	45,857	20	Dachau	45,985	1.00
Oberschleißheim	11,456	182	Vilsbiburg	11,411	1.00
Herrsching	10,206	215	Dießen am Ammersee	10,203	1.00

Table 24. Sending locations and the municipality of the policy state closest in 2014 population among the municipalities never named as receiving or sending locations in the relocation programs. Ratio is the population of the closest municipality divided by the population of the sending location.

For sending location i with first relocation year G_i, I define $\widetilde{Y}_{jt} = Y_{jt} - Y_{j,G_i-1}$ and

choose weights that solve

$$\begin{aligned} \min_{\{\omega_{ij}\}_{j\in\mathcal{D}}} \quad & \sum_{t=2008}^{G_i-2} \left(\widetilde{Y}_{it} - \sum_{j\in\mathcal{D}} \omega_{ij} \widetilde{Y}_{jt} \right)^2 \\ \text{subject to} \quad & \sum_{j\in\mathcal{D}} \omega_{ij} = 1, \qquad \omega_{ij} \geq 0. \end{aligned} \tag{19}$$

The donor pool $\mathcal{D}$ contains the 5,164 donors described above that are observed in every year of the outcome. It contains between 4,763 and 5,164 donors depending on the outcome, and 4,775 donors for workplace employment. The fit uses only the outcome itself and is repeated separately for each outcome, giving the synthetic outcome $\bar{Y}_{C(i),t} = \sum_{j\in\mathcal{D}} \omega_{ij} Y_{jt}$. For tax income, the reference is the latest available year before the first relocation.

Because many weightings reproduce the path exactly, the weights spread across many donors. Munich's synthetic control has an effective number of 4,443 donors, and no donor carries a weight above 0.0008. The largest single weight of any sending location is 0.0104 in Freising, which has 639 effective donors. No single donor drives these synthetic controls. Across all outcomes, the fit before the first relocation is exact except for commercial rents in Oberschleißheim, with a root mean squared prediction error of up to 1.07, and purchase prices in Munich, with 0.25.

Sending location	First relocation	Fit years	Donors	Effective donors	Largest weight
Munich	2015	2008 to 2013	4,775	4,443	0.0008
Freising	2015	2008 to 2013	4,775	639	0.0104
Herrsching	2015	2008 to 2013	4,775	4,151	0.0017
Oberschleißheim	2016	2008 to 2014	4,775	3,165	0.0015
Landshut	2016	2008 to 2014	4,775	4,022	0.0012
Erlangen	2016	2008 to 2014	4,775	4,344	0.0008
Augsburg	2016	2008 to 2014	4,775	4,645	0.0005
Nürnberg	2017	2008 to 2015	4,775	4,401	0.0007
Würzburg	2020	2008 to 2018	4,775	4,457	0.0007

Table 25. Synthetic controls of the sending locations for workplace employment. Fit years are the years whose change relative to the year before the first relocation enters Equation 19. The effective number of donors is $1/\sum_j \omega_{ij}^2$. The root mean squared prediction error over the fit years is below 0.00001 for every sending location.

Matching and comparison groups

The sample contains 40 receiving locations.[18]

Gunzenhausen illustrates how the cell determines the comparison group. It received 230 positions from Munich for the State Office for Schools and was first treated in 2016.

[18] Kaufbeuren and Bad Steben lie outside the donor range in their cells and get one control each selected by Mahalanobis distance.

It is a central place in a district eligible under the policy's eligibility criteria, and twelve other receiving locations share this cell. Its comparison group contains 225 donors outside the policy state, with an effective number of donors of $1/\sum_j w_j^2 = 23.6$ and a largest weight of 0.11. Table 23 in Appendix D reports balance after entropy balancing. The largest standardized mean difference among the five index levels is 0.15, for unemployed per 100.

Each sending location receives its own synthetic control because Munich and the other sending locations have no close match among single locations outside the policy state. The sample contains Munich, Freising, Herrsching, Oberschleißheim, Landshut, Erlangen, Augsburg, Nürnberg and Würzburg. Passau is excluded because its region also contains receiving locations, while Ansbach and Weiden are excluded because they switch between sending and receiving. Donors are locations outside the policy state observed in every year, with about 4,700 to 5,160 donors depending on the outcome. I report Munich separately and pool the other eight sending locations. Munich accounts for 1,082 of the 1,561 positions sent by these nine locations.

Munich's first relocation is in 2015, so its workplace employment weights are fitted on 2008 to 2013 relative to 2014. Its 4,775 donors have an effective number of about 4,440, and the largest weight is 0.0008.

I choose comparison municipalities outside Bavaria to limit contamination by policy spillovers within the state. Both the donors for entropy balancing and the donors for synthetic controls lie outside the policy state. Even municipalities that neither receive nor send an agency may be affected as workers, firms, and expenditure adjust across nearby locations. As an additional precaution, I exclude municipalities in the 20 non-Bavarian counties with the strongest commuting links to Bavaria.[19]

Matching on pre-policy levels addresses observed differences relevant to this comparison.

Figure 12 shows the top and bottom 20 non-Bavarian counties by their share of commuters to Bavaria in 2013. The top-20 group defines the additional exclusion. The bottom-20 group is shown for comparison and does not define the donor pool.

Table 23 reports the means of receiving locations and their weighted controls and the standardized mean differences. Entropy balancing brings receiving locations within 0.15 standard deviations of their weighted controls on every index level. The pre-relocation changes and the size of the local labor market were not part of the balancing, yet they are also within 0.15 standard deviations. For sending locations, the event-study figures show the fit of their synthetic controls before the first relocation.

Long differences

Long differences show how far each outcome has moved since 2014 by each end year from 2015 to 2025. For each end year, I compare the change in the treated location with the change in its comparison group. I report receiving locations together and separately for early cohorts, first treated in 2015 or 2016, and late cohorts, first treated in 2017 or later. For sending locations, I report Munich alone and the other eight sending locations

[19] The exclusion uses the counties identified by their shares of commuters to Bavaria in 2013. Appendix D shows their geography and documents the matching procedure.

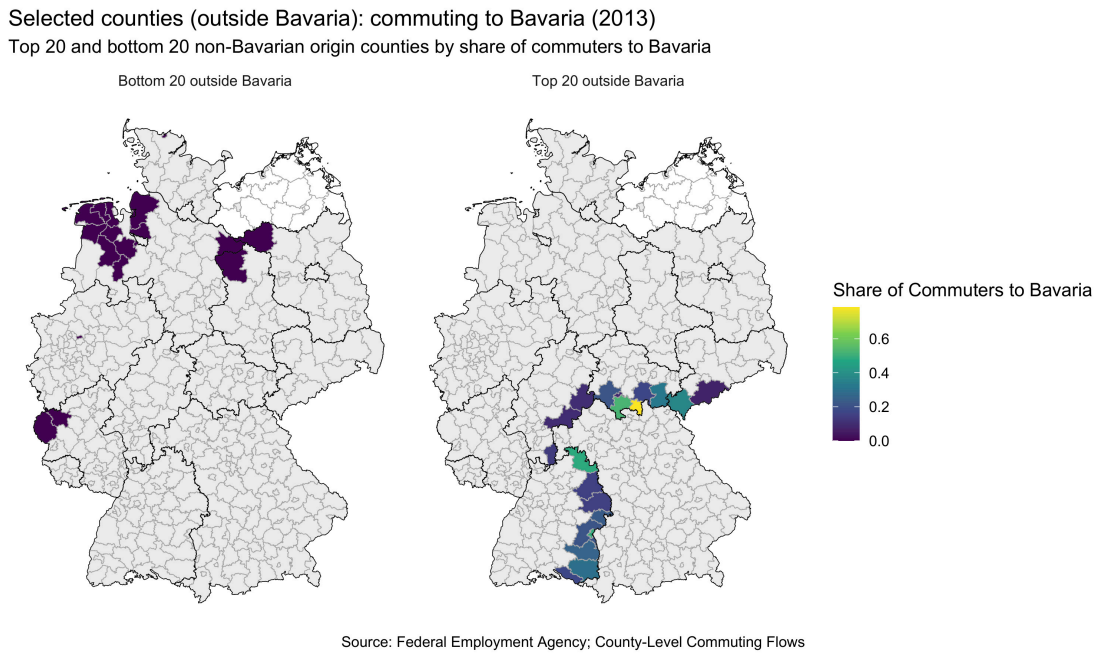


Figure 12. Commuting links used to restrict the comparison pool. Counties outside Bavaria with the lowest and highest shares of commuters to Bavaria in 2013. Municipalities in the top-20 counties are excluded from the main control pool, in addition to municipalities within Bavaria. Source: Federal Employment Agency county commuting flows.

pooled. Population, tax income, employment, rents and purchase prices enter as 100 times the log, so β_{kt} approximates a percentage change. The unemployment rate and employment density enter in percentage points, and net migration of young adults per 1,000 inhabitants.

Causal interpretation requires parallel trends and no anticipation. The parallel trends assumption requires that, absent the public-sector relocation, treated Bavarian municipalities would have followed the same trajectory as matched control municipalities in local labor-market and population outcomes. The no-anticipation assumption requires that outcomes were not affected by the impending treatment in pre-treatment periods. The announcement in 2015 and the staggered implementation of relocations from 2015 to 2025 could introduce anticipation effects. To assess this possibility, I estimate event-study specifications, which do not reveal significant pre-treatment effects. This absence of anticipation effects is consistent with private-sector firms waiting until local demand changes materialize and with public employees relocating only when new agencies open.

Long differences measure the cumulative effect of the policy in calendar time. For each end year t from 2015 to 2025, I compare the change since 2014 in a treated location with the change in its comparison group,

$$\begin{aligned} \Delta_{skt} &= \left(Y_{i(s)kt} - Y_{i(s)k,2014}\right) - \left(\bar{Y}_{C(s)kt} - \bar{Y}_{C(s)k,2014}\right), \\ \hat{\beta}_{kt} &= \frac{1}{G_t} \sum_{s\,:\,G_s \le t} \Delta_{skt}. \end{aligned} \tag{20}$$

Here k indexes outcomes, $\bar{Y}_{C(s)kt}$ is the weighted comparison outcome, and G_t counts the matched sets first treated in or before t. Receiving locations use entropy balancing weights and sending locations use their synthetic controls. The weights already condition on the policy rule for receiving locations, so the estimator includes no further covariates. A matched set enters once its first relocation has occurred by the end year.

For end year 2019, the receiving estimate uses the 31 matched sets first treated from 2015 to 2018. Residence employment in these receiving locations grew about 2.3% more between 2014 and 2019 than in their comparison groups, significant at the 10% level. For Munich, workplace employment grew about 5% more than in its synthetic control between 2014 and 2019. The workplace employment long difference reaches 11.6 in 2025 and is not statistically significant.

The early cohorts contain 19 locations first treated in 2015 or 2016, and the late cohorts contain 21 locations first treated in 2017 or later. Outcomes enter in the units described above. For receiving locations, $\widehat{\mathrm{se}}(\hat{\beta}_{kt}) = \mathrm{sd}_s(\Delta_{skt})/\sqrt{G_t}$, with critical values from the t distribution with $G_t - 1$ degrees of freedom. Munich uses placebo standard deviations and the other sending locations use variation across locations as described above.

Treatment cohorts and repeated relocations

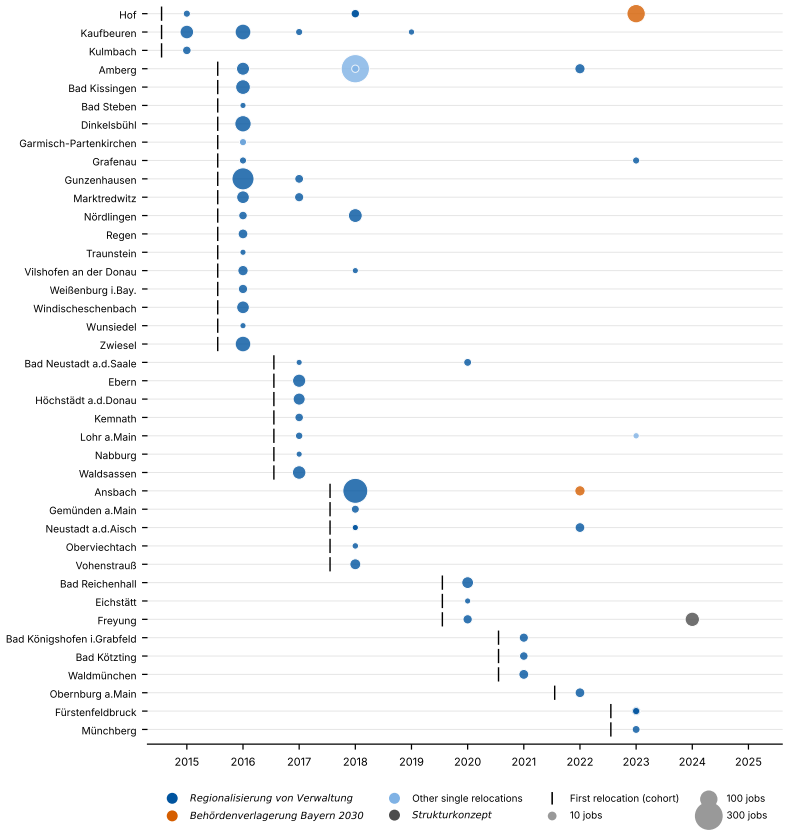
Treatment begins in the year of the first recorded relocation into a receiving municipality or out of a sending municipality. I use the official treatment year where it is available and the recorded completion year otherwise. Some municipalities experience further

relocations, so first treatment marks the start of exposure rather than the completion of every project. Figure 13 in Appendix D shows the timing of individual projects by municipality and program, and Table 26 reports first- and last-treatment cohorts in the matched sample. The distinction is especially relevant for places receiving several agencies over time.

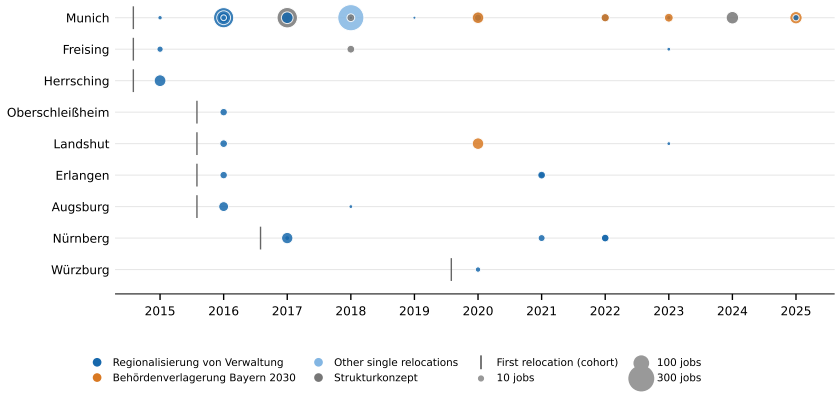
The baseline treatment year is the first recorded relocation for a municipality among the projects included in the analysis. The last-treatment year is the latest such date. Both use the official treatment year when available and otherwise the recorded completion year. Repeated dates at a location therefore describe further exposure to the policy, rather than additional independent treated municipalities. Table 26 gives the first- and last-treatment distributions underlying Figure 13.

	Receiving		Sending	
Year	First	Last	First	Last
2015	3	1	3	1
2016	16	10	4	1
2017	7	7	1	0
2018	5	5	0	1
2019	0	1	0	0
2020	3	3	1	1
2021	3	3	0	1
2022	1	4	0	1
2023	2	5	0	2
2024	0	1	0	0
2025	0	0	0	1
Municipalities	40	40	9	9

Table 26. First and last treatment years in the matched sample. Each municipality appears once in each timing distribution. First and last refer to the earliest and latest recorded treatment among projects from 2015 to 2025, using the official year when available and the completion year otherwise.



(a) Receiving locations



(b) Sending locations

Figure 13. Timing of relocation projects for the 40 receiving and 9 sending municipalities in the matched sample, 2015 to 2025. Each circle is one project, placed at its official treatment year or, where that is missing, its completion year. Circle area is proportional to the relocated jobs and apprenticeships. Colors show the program. The vertical bar marks the first relocation, which defines the cohort. Receiving rows show projects arriving in the municipality. Sending rows show projects leaving it.

Event-time aggregation and inference

For Gunzenhausen, $G_s = 2016$, so event time zero is 2016 and the reference year is 2015. For Munich, $G_s = 2015$, so event time four is 2019.

The average treatment effect on the treated averages each matched set's gaps over its observed event times from zero to eight and then across matched sets. With G matched sets, it is

$$\widehat{\text{ATT}} = \frac{1}{G}\sum_{s}\frac{1}{|\mathcal{L}_s|}\sum_{\ell\in\mathcal{L}_s} D_{s\ell}, \qquad \mathcal{L}_s = \{\ell \in \{0,\dots,8\} \mid G_s + \ell \text{ is observed}\}. \tag{21}$$

Munich's workplace employment gaps from event time zero to eight are 0.64, 2.1, 3.3, 4.02, 5.04, 5.4, 4.91, 6.32 and 8.75. Their mean is 4.5, with a placebo standard error of 3.01. For receiving locations, the total population gap eight years after the first relocation is 1.75, significant at the 10% level, averaged over the 26 matched sets observed at that event time. The average treatment effect is 0.56, significant at the 5% level, and averages each matched set over its own observed years before averaging over all 40 matched sets.

I measure uncertainty for Munich using 58 placebo synthetic controls. Each donor in Munich's 2014 population band or either adjacent band is treated as if it had sent jobs in 2015 and receives its own synthetic control from the other donors. At event time ℓ, the standard error is $\widehat{\text{se}}_\ell = \text{sd}_p(D^p_\ell)$, with critical values from the t distribution with 57 degrees of freedom. Four years after the first relocation, Munich's workplace employment gap is 5.04 with a standard error of 3.67, giving a 90% confidence interval from negative 1.1 to 11.18. For the other eight sending locations, I average their gaps and use $\widehat{\text{se}}_\ell = \text{sd}_i(D_{i\ell})/\sqrt{n_\ell}$, with $n_\ell - 1$ degrees of freedom, where n_ℓ is the number of locations observed at that event time. Their average workplace employment effect is negative 1 with a standard error of 0.71.

I report event times from five years before to eight years after the first relocation. Receiving estimates require at least five matched sets at each event time, while Munich is shown throughout this window and the other sending estimates show the number of contributing locations below each point. Outcomes enter in the same units as in the long differences. For receiving locations, the standard error is $\widehat{\text{se}}(\hat{\theta}_\ell) = \text{sd}_s(D_{s\ell})/\sqrt{G_\ell}$, with critical values from the t distribution with $G_\ell - 1$ degrees of freedom. Munich uses placebo inference, and the other sending locations use variation across locations as described above.

Each matched set contributes to an event time only where the treated municipality and its controls are observed. The composition of the event-study coefficients can therefore vary across event times, and the figures report the number of contributing matched sets below each coefficient. Late cohorts contribute few years after treatment, so the coefficients at long horizons rest mainly on early cohorts. For receiving locations, standard errors use the variation across the 40 matched sets, divided by the square root of the number contributing to the estimate, with critical values from the t distribution with $G - 1$ degrees of freedom. For Munich, standard errors are the standard deviations of the gaps from 58 placebo synthetic controls, with 57 degrees of freedom. Each placebo is a donor in Munich's 2014 population band or either adjacent band, treated as if it had

sent jobs in 2015 and assigned its own synthetic control. For the other eight sending locations, standard errors are the standard deviations of their gaps divided by the square root of the number of contributing locations, with $n-1$ degrees of freedom. The number of locations falls at later event times because Würzburg's first relocation is in 2020.

Stacked difference-in-differences

The stacked difference-in-differences condenses the event study into one number per outcome. It compares each treated location with its own weighted controls in the same calendar year, from five years before to eight years after the first relocation.

I stack each matched set s, containing its treated location and weighted controls j, over the window $G_s - 5 \leq t \leq G_s + 8$ and estimate by weighted least squares

$$Y_{jst} = \alpha_{js} + \lambda_{st} + \beta \mathbf{1}\{j = i(s)\}\mathbf{1}\{t \geq G_s\} + \varepsilon_{jst}. \tag{22}$$

The fixed effects α_{js} are specific to each location within each matched set, while λ_{st} are specific to each matched set and calendar year. Each treated location is therefore compared only with its own controls in the same calendar year. For receiving locations, the treated location has weight one and the controls carry their entropy balancing weights. Standard errors are clustered by matched set, with critical values from the t distribution with $G-1$ degrees of freedom.

The stacked estimate compares the mean outcome gap after the first relocation with its mean over the five preceding years, while the average treatment effect compares it with the single preceding year. This difference in the reference period allows the two estimates to differ. For receiving locations, workplace employment in the public data has a stacked estimate of 2.04 with a standard error of 1.69, based on 40 matched sets and 246,240 observations. The corresponding average treatment effect is 0.93 with a standard error of 1.25.

For a sending location with one synthetic control, the stacked estimate is

$$\hat{\beta} = \frac{1}{9}\sum_{\ell=0}^{8} D_{s\ell} - \frac{1}{5}\sum_{\ell=-5}^{-1} D_{s\ell}. \tag{23}$$

For Munich, the second term is zero because its synthetic control matches the outcome path before the first relocation. The stacked workplace employment estimate therefore equals its average treatment effect of 4.5.

Appendix E: Additional Place-Level Results

The long differences measure changes relative to 2014. Figures 15, 17, 19, 23, 27 and 39 report policy targets, employment and factor prices. Figures 29, 31, 33 and 35 report human capital.

Policy Evaluation of the 'Heimatstrategie'

Total population rises significantly by 1% in year six and 1.8% in year eight after the relocation. Working-age population rises every year, with significant increases of 0.6% in year one and 3.6% in year eight. This slow growth fits the gradual staffing of the agencies. After three years, 37 agencies had started with 483 employees and 170 students, against 2,225 planned positions and 930 apprenticeships. Many agencies began with small teams in interim offices. Population may therefore follow the staffing of the agencies rather than their announcement.

Munich sends 1,082 positions, only 0.14% of its 2014 workplace employment of 776,480, while the city grows by about 30,000 residents a year. The relocation may be too small to show up against this growth.

In the other sending locations, the unemployment rate rises a little in later years, by 0.4 to 0.9 percentage points from year four, with an average increase of 0.33 percentage points. These locations lose few positions relative to their size. Herrsching is the exception, sending 71 positions, or 2.2% of its workplace employment, while the other sending locations lose less than 0.5%.

Public jobs pay more than private jobs in these towns. Mean gross pay in 2014 was 31.9 thousand euros in the public sector and 26 thousand euros in the private sector. The first income gains may therefore come from better-paid public jobs held by residents, before newcomers arrive. In Munich, tax income rises by 2.42% on average and by 4.4% in year six, while other sending locations experience an average decline of 0.99%. However, neither average effect is significant.

In receiving locations, the significant increases in employment density grow from 0.9 workers per 100 inhabitants in year three to 2.1 in year eight. Density rises when private jobs start to rise, which may reflect agencies approaching full staff. Munich also becomes denser in jobs, with employment density rising by 1.2 workers per 100 inhabitants in year one and by a significant 7.1 in year eight.

In receiving locations, the significant increase in net migration of young adults is already 10.5 per 1,000 in the year of the first relocation and reaches 13.9 in year two. Young adults may respond quickly because they are more mobile and because the program also moves training and apprenticeships, including 930 apprenticeships in the 2015 concept.

Munich gains jobs, its residents increasingly work locally, and its rents rise, yet fewer young adults move in. Housing costs may price young adults out of Munich. Rents and housing shortages were reasons for moving jobs out, but the relocation does not appear to relieve these pressures.

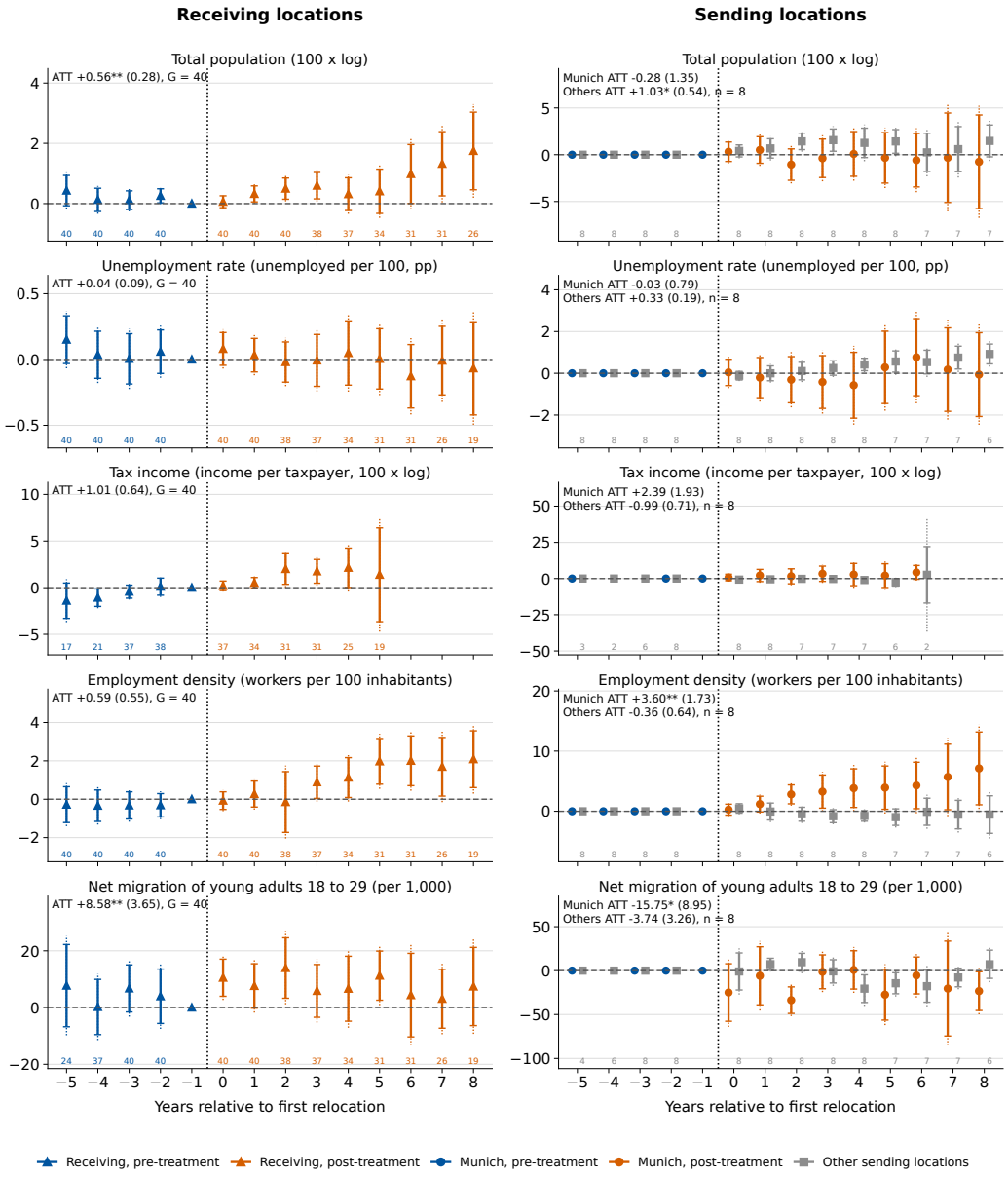


Figure 14. Policy targets before and after the first relocation. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations are compared with synthetic controls, with Munich shown separately from the other eight sending locations. The average effect covers event times zero to eight. Log outcomes are multiplied by 100. Unemployment effects are in percentage points, employment density in workers per 100 inhabitants, and net migration of young adults in people per 1,000. Because relocations occur in different years, the small numbers below the estimates show how many treated locations contribute at each event time.

In receiving locations, tax income rises significantly by 2% in year two and 2.1% in year four. The data end in 2021.[20]

[20] Municipal income per taxpayer comes from the income tax statistics (Lohn- und Einkommensteuerstatistik). These statistics are complete only once all tax assessments for a year are final, which takes about three to four years. Thus, 2021 is the latest published year.

In receiving locations, total population rises significantly by 0.95 log points and working-age population by 2.0 log points by 2024. Tax income rises significantly by 2.9 log points in 2019. Employment density rises significantly by 1.3 to 1.7 workers per 100 inhabitants in 2020 to 2022. In Munich, working-age population rises significantly by 7.4 log points by 2024.

Private-to-Public Employment Switches

Switches from private to public employment rise significantly by about 26% (Figure 16 in Appendix E). The estimate in worker counts is about eight additional switches per receiving location and year, compared with about 22 switches in 2014. Workers switching from the private to the public sector are not a main margin of adjustment to the relocation.

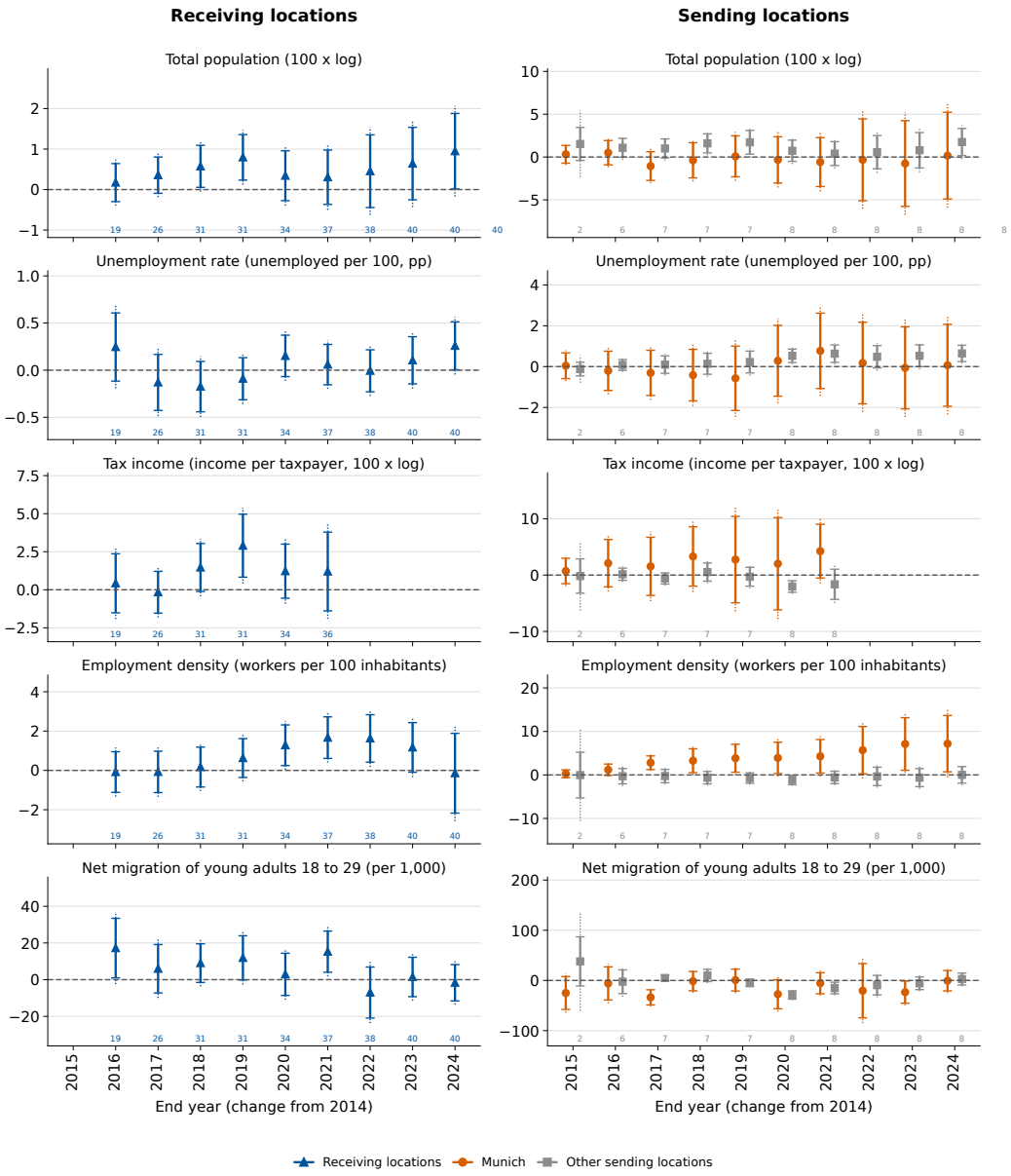


Figure 15. Policy targets, long differences from 2014. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations are compared with synthetic controls, with Munich shown separately from the other eight sending locations. Log outcomes are multiplied by 100. Unemployment effects are in percentage points, employment density in workers per 100 inhabitants, and net migration of young adults in people per 1,000.

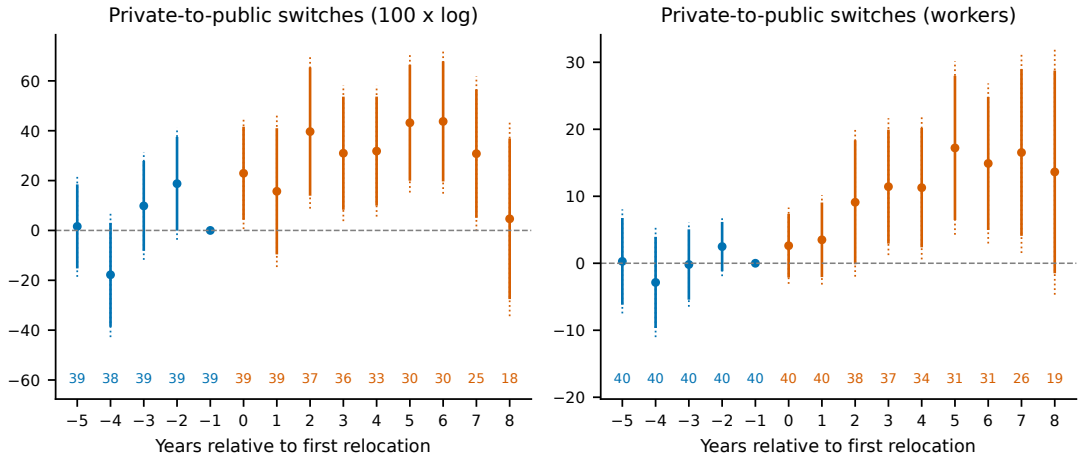


Figure 16. Switches from private to public employment in receiving locations. Event studies relative to the first relocation, comparing receiving locations with entropy balancing controls outside the policy state. The left panel reports log outcomes multiplied by 100 and the right panel reports worker counts. Solid bars show 90% confidence intervals and dotted extensions show 95% confidence intervals. Because relocations occur in different years, the small numbers below the estimates show how many treated locations contribute at each event time.

Early vs. Late Treated Cohorts

Early cohorts drive the policy effects.[21] Three results stand out. First, the 19 locations first treated in 2015 or 2016 gain population, young adults, jobs and rents, while the 21 late-treated locations show almost no change at the same horizons. Second, sending locations do not lose. Munich keeps growing, and the other sending locations gain residents and out-commuters. Third, prices follow people. Rents rise only in early cohorts and mostly where the state built a new office.

Early cohorts gain residents and young adults. Total population rises by 0.96% on average, working-age population by 1.90%, net migration of young adults by 14.1 per 1,000 and employment density by 1.36 workers per 100 inhabitants. These increases are statistically significant. In late cohorts, total population rises by 0.21%, working-age population by 0.39% and net migration of young adults by 3.6 per 1,000, while employment density falls by 0.11 workers per 100 inhabitants. None of these changes is significant. Early cohorts already gain population in years one to four, with significant increases of 0.5 to 1.2%, so longer exposure alone does not explain the difference.

The unemployment rate does not change in either group. Tax income rises by 1.45% in early cohorts and 0.55% in late cohorts, although neither average effect is significant.

Early receiving locations may draw young adults from locations treated later. Late cohorts lose young adults three and five years after their first relocation, with declines of 10.2 and 9.6 per 1,000, neither significant. Differences in treatment timing could therefore create competition for mobile residents.

Employment and commuting also rise mainly in early cohorts. Residence employment rises by 2.49% on average, workplace employment by 3.36%, local employment by 5.42%, in-commuters by 2.57% and out-commuters by 2.87%. These increases are statistically significant. Late cohorts are close to zero on all five margins. Private workplace employment rises significantly by 2.99% in early cohorts and private employment among workers with tertiary education by 5.82%. Late cohorts gain public workplace employment and knowledge-intensive business services, but not total private employment. The manufacturing results in Section 6.2 show the same concentration in early cohorts.

The timing of the later relocations may help explain these differences. Nine of the 21 late-treated locations were first treated in 2020 to 2023, during and after the pandemic. Their agencies are younger and may be further from full staff. If effects take about five years to develop, these locations have not yet reached that horizon in the data. Rents also rise only in early cohorts, with a significant average increase of 3.90%, compared with a decline of 1.90% in late cohorts, which is not significant.

Early cohorts gain young adults in the calendar years when late cohorts lose them. Net migration of young adults in early cohorts rises significantly by 20.9 per 1,000 in 2019 and 26.4 in 2021. Tax income in early cohorts rises significantly by 3.0 log points in 2019. Their local employment rises by 0.8 log points in 2017 and by a significant 8.8 log points in 2025. Across receiving locations, local private employment rises significantly by 3.9 log points by 2024. The additional manufacturing estimates show significant gains of 4.67 log points in early cohorts in 2019 and 4.86 in 2020.

[21] The second phase remains to be evaluated as it develops through 2030.

Working-Age Population

Working-age population (including individuals that are 15 to 64 years old) is not an explicit policy target, but it is an implicit one, because the policy targets local labor markets. Its growth begins in the first year after the relocation and reaches significant increases of 2.5% seven years and 3.6% eight years after the relocation. These gains underlie the average increase reported above.

Effects on the Local Labor Markets

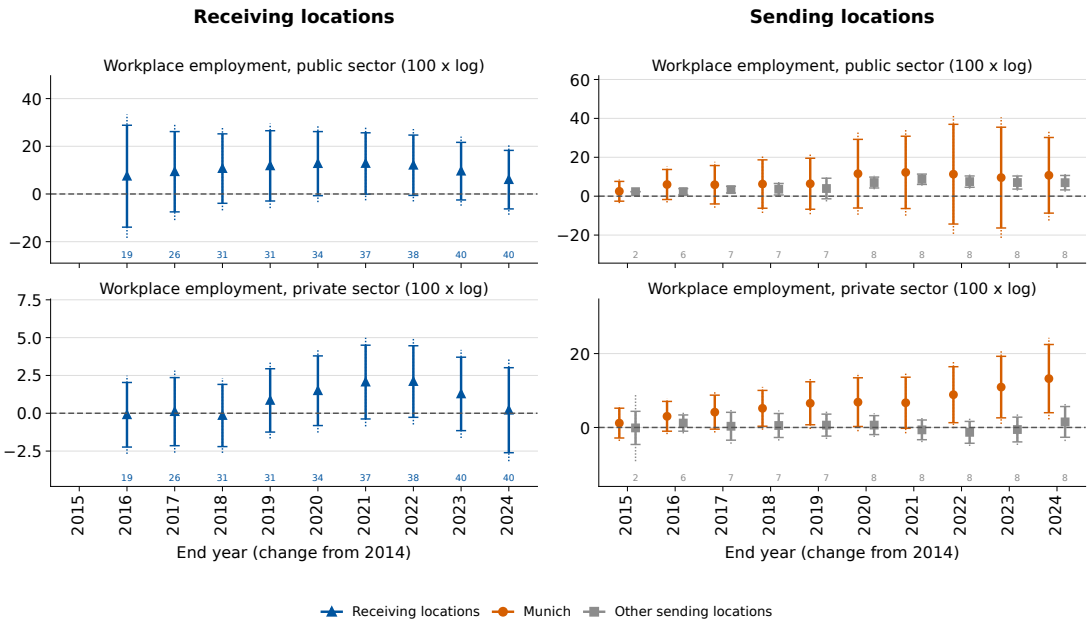


Figure 17. Public and private workplace employment. Long differences from 2014. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations, where shown, are compared with synthetic controls, with Munich reported separately from the other eight sending locations. Log outcomes are multiplied by 100.

Residence Employment

Residence employment grows less than local employment, which tells us whether co-movers, family members who work somewhere else, move along. Eight years after the relocation, residence employment is 3.1% higher, compared with 5.4% for local employment, with ATTs of +1.4% and +2.1%, respectively, while the long differences for early cohorts show increases of 4.6% and 8.8% by 2025, respectively. Out-commuting follows the same pattern. It rises by 0.6% to 1.4% in the year of the relocation and one and three years after the relocation, and by 2.6% eight years after the relocation.

Local Employment and Commuting

In receiving locations, the increase in local employment grows from 2.1% in year three to a significant 5.4% in year eight. Local public employment rises significantly by 5.67%. Across sectors, local employment rises significantly by 2.1%.

In receiving locations, out-commuting rises significantly by 0.9% in the year of the first relocation.

In Munich, local employment rises significantly by 9.9%, with significant increases of 11.2% in local private employment and 18.6% in local public employment. Residence employment rises significantly by 5.81% and out-commuting by 9.44%, while the increase in in-commuting of 4.05% is not significant.

In other sending locations, people stay but commute out. Residence employment rises significantly by 4.13% and out-commuters by 8.31%, while in-commuters fall significantly by 3.32%. Workplace employment falls by 1%, but the estimate is not significant.

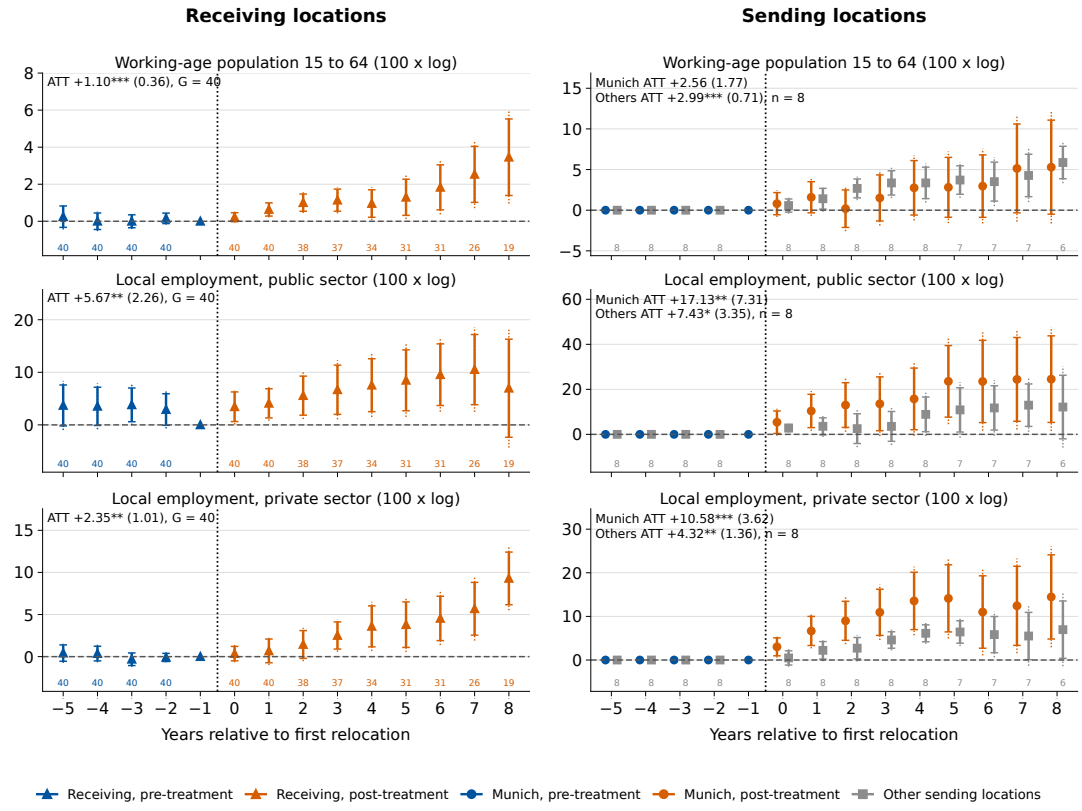


Figure 18. Working-age population and local employment. Event studies relative to the first relocation. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations, where shown, are compared with synthetic controls, with Munich reported separately from the other eight sending locations. Log outcomes are multiplied by 100. Because relocations occur in different years, the small numbers below the estimates show how many treated locations contribute at each event time.

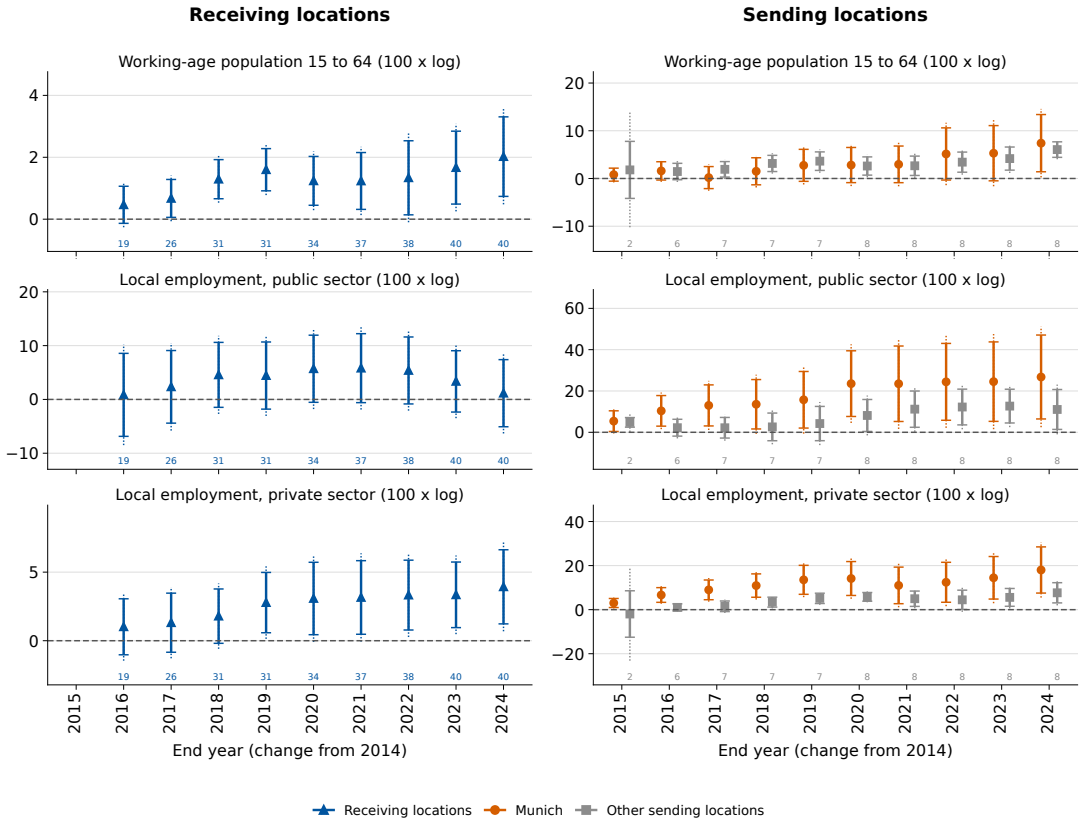


Figure 19. Working-age population and local employment. Long differences from 2014. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations, where shown, are compared with synthetic controls, with Munich reported separately from the other eight sending locations. Log outcomes are multiplied by 100.

Additional Outcomes

In-commuting hardly moves in receiving locations while local employment rises, so the new local jobs seem to go to residents rather than commuters. In the event study, in-commuting barely changes for four years after the relocation and rises to about +4% eight years after the relocation (not significant). Five years before the relocation, in-commuting is slightly positive (+2.9%, only 14 sets). In the long differences, in-commuting is +3.0% by 2025 in early cohorts and −1.3% in late cohorts (both not significant). As a share of working-age population, in-commuting in late cohorts rises by 3.2 percentage points in 2020, 3.9 percentage points in 2021 and 3.8 percentage points in 2022. In sending locations, in-commuting is about zero throughout the event study and −2.4% by 2025 in the long differences (not significant). As a share of working-age population, in-commuting falls by 3.2 percentage points in 2023 and 3.1 percentage points by 2024.

Figure 20 shows the event studies and Figure 21 the long differences for the additional outcomes.

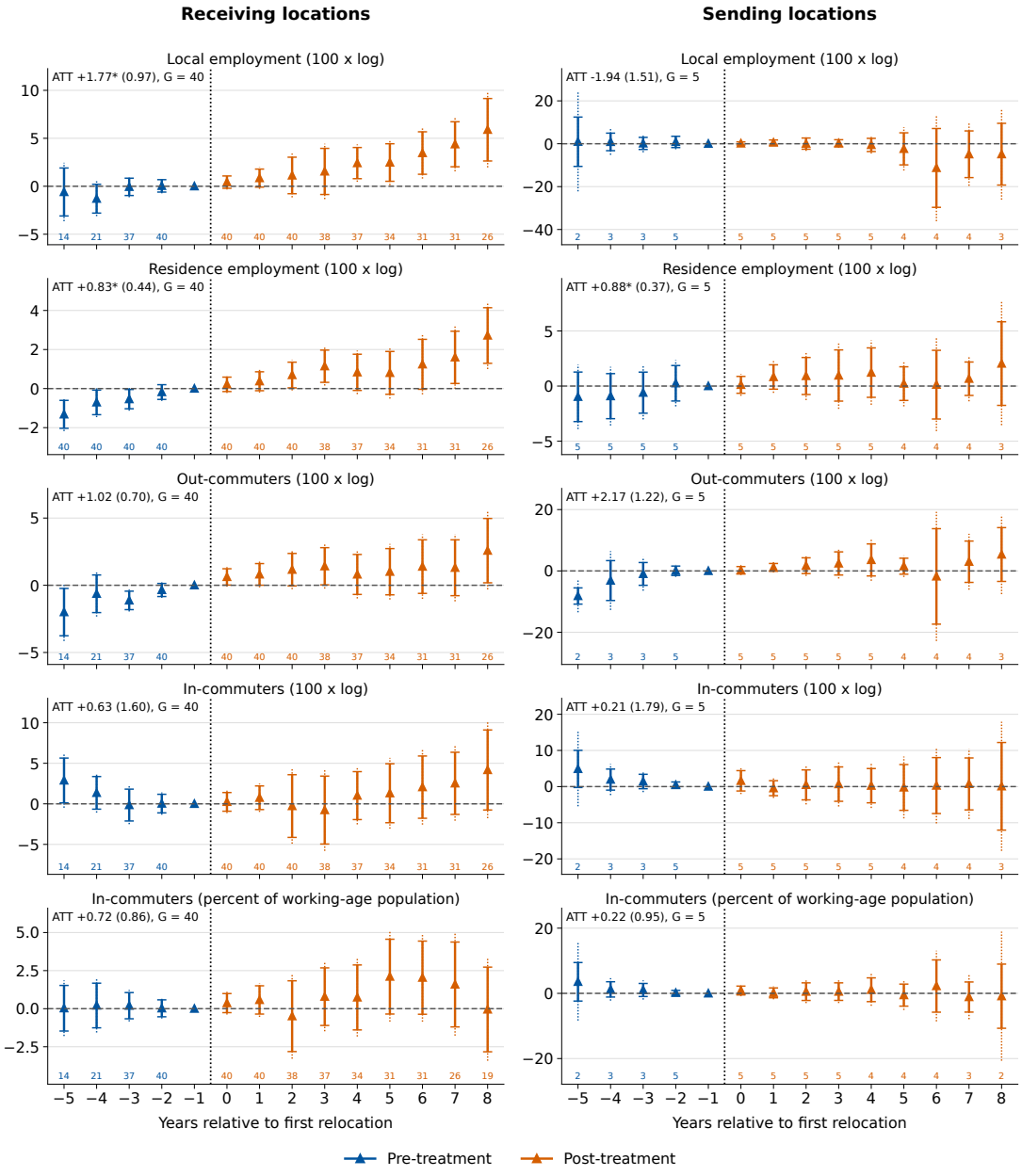


Figure 20. Additional outcomes, event studies. Event study estimates relative to the year before the relocation. The number of sets is shown below each estimate. Each point is the mean over matched sets of the treated location minus the mean of its three matched control locations outside the policy state. Solid bars show 90% confidence intervals and dotted extensions show 95% confidence intervals.

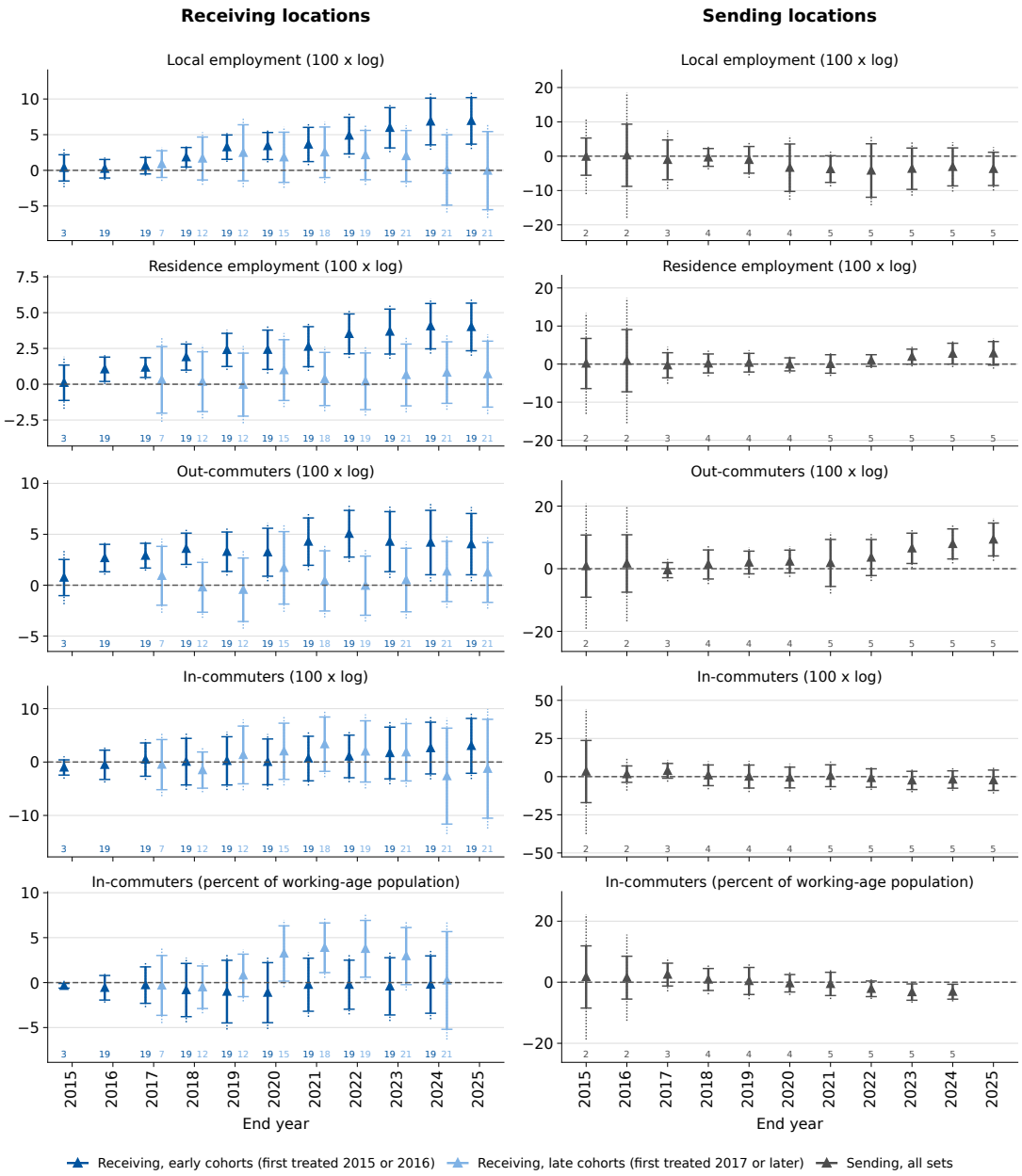


Figure 21. Additional outcomes, long differences. Long differences relative to 2014 by end year. Receiving locations are split into early cohorts (2015 to 2016) and late cohorts (2017 and later). Each point is the mean over matched sets of the treated location minus the mean of its three matched control locations outside the policy state. Solid bars show 90% confidence intervals and dotted extensions show 95% confidence intervals.

Manufacturing Employment

In receiving locations, early cohorts drive the rise in manufacturing employment, with a significant average increase of 12.1%, compared with a decline of 1.4% in late cohorts, which is not significant.

Large plant openings after the relocation in some receiving locations may help explain this rise in manufacturing employment, but it is unclear whether these openings are directly linked to the agencies. Some cases could be related. The LfA Development Bank, which funds firms, moved to Hof in 2015. LAMILUX, a family-owned manufacturer of daylight systems for buildings and fiber-reinforced plastic panels, has been ramping up there since 2023 and could in principle benefit from LfA loans. The Bavarian Competence Centre for Nutrition, relocated to Kulmbach in 2015, works on food and nutrition, while Ireks, a manufacturer that opened a warehouse there shortly after, makes food ingredients.[22] The State Office for Weights and Measures, which moved to Bad Reichenhall, oversees industrial measuring and weighing, and Reber packs food by weight, making it a firm the office regulates. Other cases show no obvious overlap. Forchheim and Wunsiedel receive BayernLab IT centers, while the manufacturers that opened there were Siemens Healthineers and a sawmill. Figures 24 and 25 in Appendix E document the firm events.

State investment could also affect manufacturing by signaling a lasting state commitment. In most relocations, the state invested in infrastructure and new offices. Manufacturers may have read these investments as a long-term signal. A plant is a large investment that is hard to reverse, while a state building or new infrastructure commits public money to a town for decades. This commitment could lower the risk manufacturers perceive in the town's future services, accessibility and workforce. The firm's product need not be related to the agency for this signal to matter.

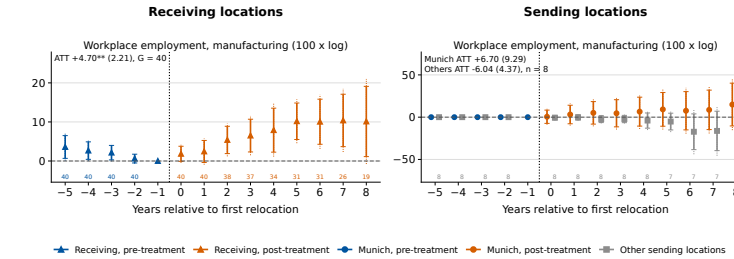
I examine whether the announcement alone affects manufacturing employment, whether state investment in new offices drives the effects, and whether manufacturing employment also rises in neighboring towns, indicating a broader increase rather than a signal specific to the towns where the state invests. Manufacturing employment does not rise in response to the announcement alone, before the agency moves to the receiving location. Manufacturing employment rises significantly after an agency opens, by 5.02% in year three and 8.72% in year six.

Between announcement and opening, the significant difference in manufacturing employment growth is 7% in favor of new-building locations. This gap reflects a significant decline of 5.59% in rented-office locations, while employment remains flat in new-building locations, with an insignificant increase of 1.03%. After an agency opens, manufacturing employment grows by 10.9% more in new-building locations. The state built new offices in 25 of the 33 locations observed between announcement and opening, and in 30 of the 40 locations overall. Only eight locations rented offices in the announcement window. The comparison therefore rests on few rented-office locations, and the state may have

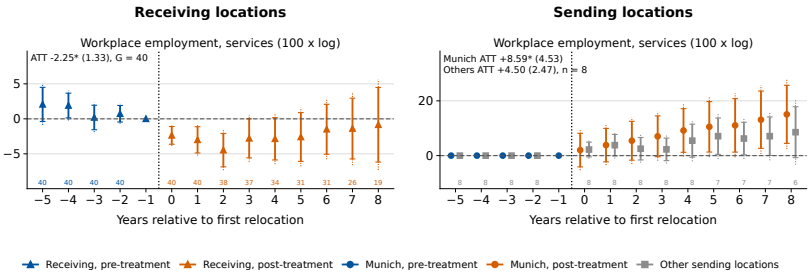
[22] Ireks has been based in Kulmbach for a long time, and the opening is a warehouse. The relationship could therefore run the other way, with the agency placed in Kulmbach because Ireks was already there.

rented where local industry was weaker or a quick solution was needed. Even so, the larger gains where the state builds provide suggestive evidence for this interpretation rather than establishing a causal signaling effect. The signal may come from committing investment to a town rather than announcing a relocation alone.

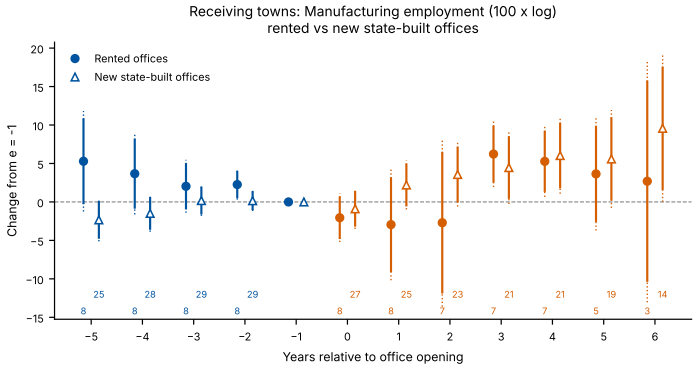
Neighboring towns show no significant manufacturing employment gains. In untreated towns within 15 minutes of a receiving location, the average increase is 0.68% and is not significant, while the manufacturing employment share falls by 0.59 percentage points. This pattern supports a signal about the towns where the state invests rather than a broader regional boom.



(a) Manufacturing workplace employment



(b) Services workplace employment



Set gap (treated town minus entropy-balanced controls) regressed on the new-building dummy per event time; HC1, t(G-2). 90% CI solid, 95% CI dotted. Numbers = sets per group (lower row rented, upper row new building). Manufacturing employment: BA workplace employment in sectors B-F (mining, manufacturing, energy, water, construction) as proxy. Data end in 2022.

(c) Manufacturing employment by office type

Figure 22. Manufacturing and services workplace employment. The upper two panels report event studies relative to the first relocation. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations, where shown, are compared with synthetic controls, with Munich reported separately from the other eight sending locations. Log outcomes are multiplied by 100. The lower panel shows manufacturing employment in receiving locations with rented offices and new state-built offices, relative to the year before office opening. Because relocations occur in different years, the small numbers below the estimates show how many treated locations contribute at each event time.

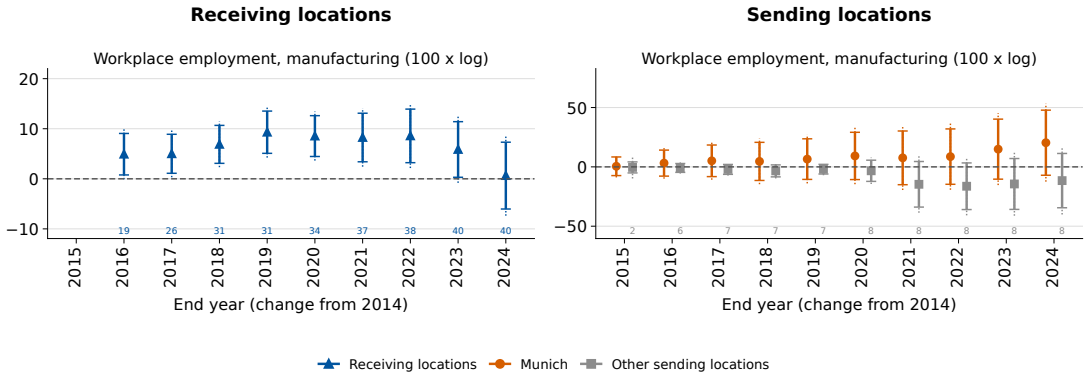
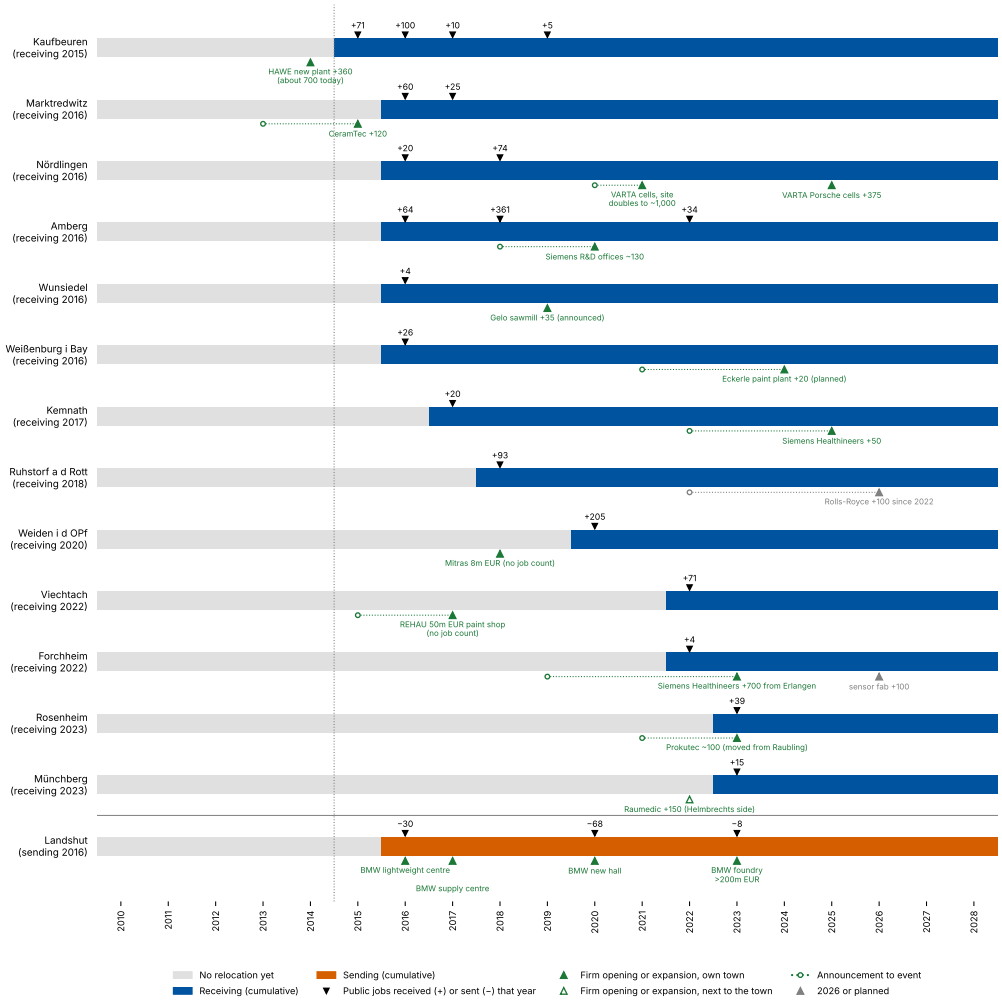


Figure 23. Manufacturing workplace employment. Long differences from 2014. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations, where shown, are compared with synthetic controls, with Munich reported separately from the other eight sending locations. Log outcomes are multiplied by 100.

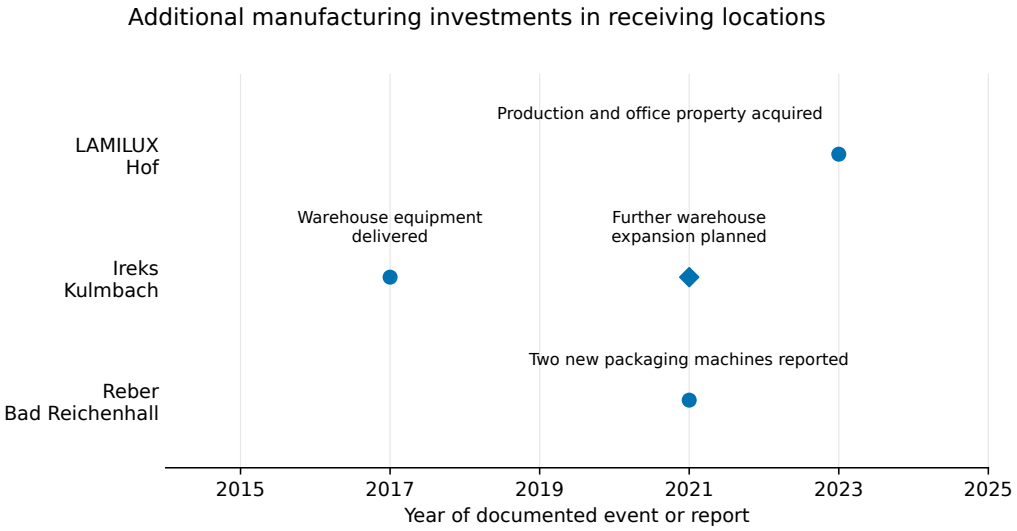
Relocation towns with manufacturing firm openings or expansions: public jobs and firm events

Receiving towns above the line, sending town below.



Public jobs: relocation file 2026_11_01 (jobs plus study places), receiving year = official treatment year, else completion year; sending = sheet sending_flows (projects with several source towns split equally).
Firm events: web search 3 Nov 2026, sources in Obsidian note "2026-11-03 Firm openings in relocation towns". Job numbers are those reported in the press.
HAWE Kaufbeuren opened 19 Sep 2014 with 360 jobs, built for 750. CeramTec Marktredwitz: 80m EUR, by 2015. VARTA Nördlingen: new building opened June 2021, site jobs doubled to about 1,000; Porsche car cells from about April 2025 (+375). Siemens Healthineers Forchheim: 350m EUR campus announced Feb 2019, X-ray tube production from 2023, about 700 jobs moved from Erlangen; sensor fab opened Sep 2026.
REHAU Viechtach: plant has about 1,050 staff. Raumedic hall is on Helmbrechts land next to Münchberg. Wunsiedel Gelo sawmill: announcement Nov 2019, opening year not found.

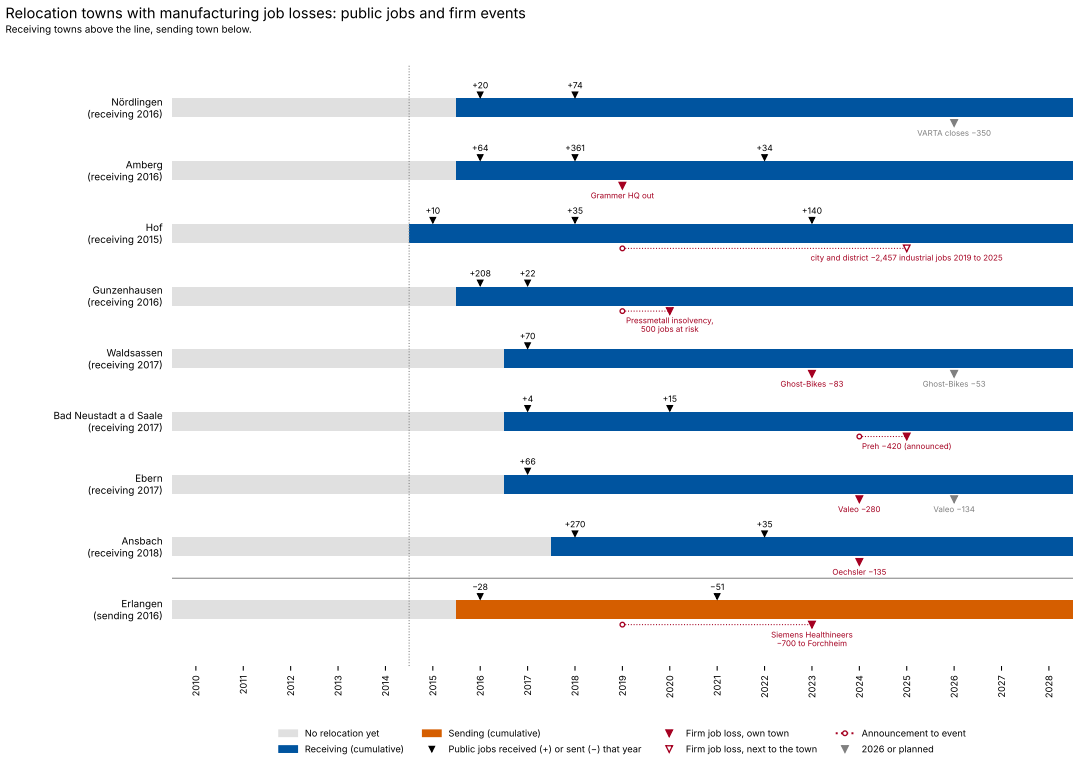
Figure 24. Manufacturing plant openings and expansions in relocation towns. The timeline records public jobs received or sent and documented firm events. Announcements and planned events are distinguished from completed events. The cases include Nördlingen, Forchheim and Wunsiedel.



Sources. IHK Oberfranken, May/June 2024. AM Logistic Solutions, July 2021.
Schubert, Paul Reber case study with a January 2021 statement.

The diamond marks planned completion. Reber is dated by the report, not an opening date.
No job counts are inferred. These events do not establish a link to an agency relocation.

Figure 24. Manufacturing plant openings and expansions, continued. Additional documented investments cover LAMILUX in Hof, Ireks in Kulmbach and Reber in Bad Reichenhall. LAMILUX's property acquisition is reported by IHK Oberfranken. Ireks's warehouse equipment delivery and planned expansion are reported by AM Logistic Solutions. Reber's packaging investment is documented by Schubert. The dates distinguish acquisitions, deliveries, plans and reports. These events do not establish a link to agency relocations, and no job counts are inferred.



Public jobs: relocation file 2026_11_01 (jobs plus study places), receiving year = official treatment year, else completion year; sending = sheet sending_flows (projects with several source towns split equally).
Firm events: web search 3 Nov 2026, sources in Obsidian note "2026-11-03 Firm openings in relocation towns". Job numbers are those reported in the press.
VARTA Nördlingen plant closes Oct 2026 (−350). Grammer moved its headquarters from Amberg to Ursensollen in Dec 2019 (about 80 jobs stay in Amberg). Hof: Frankenpost 4 May 2026, city plus district.
Pressmetall Gunzenhausen: insolvency Dec 2019. Preh Bad Neustadt: 420 cuts announced June 2024. Siemens Healthineers: about 700 X-ray jobs moved from Erlangen to Forchheim, production there from 2023.

Figure 25. Manufacturing closures and employment losses in relocation towns. The timeline records the firm events and public jobs received or sent. Reported losses, jobs at risk and announced reductions retain their original distinctions.

Services Employment

In receiving locations, services employment declines significantly by 4.4% in year two, before the decline narrows to 0.9% by year eight.

Food and beverage firms grow significantly by 4.26%. In Munich, services rise significantly by 8.97% and business services by 22.5%, although the latter estimate is not significant. Business services rise significantly by 18.1% in other sending locations.

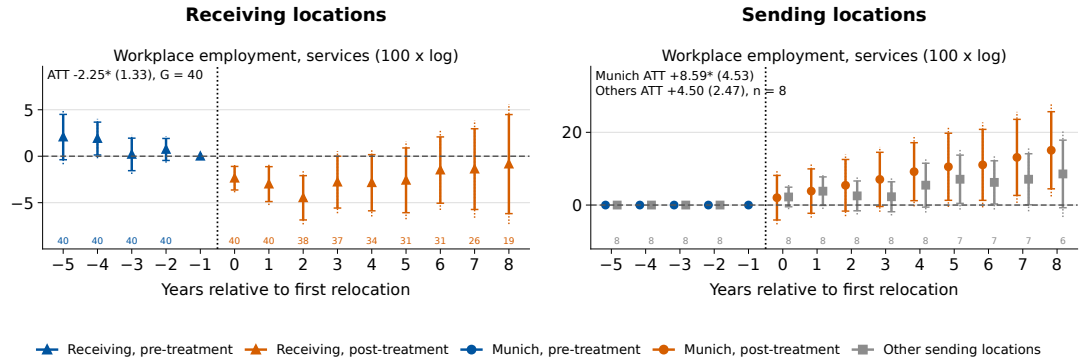


Figure 26. Services workplace employment. Event studies relative to the first relocation. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations, where shown, are compared with synthetic controls, with Munich reported separately from the other eight sending locations. Log outcomes are multiplied by 100. Because relocations occur in different years, the small numbers below the estimates show how many treated locations contribute at each event time.

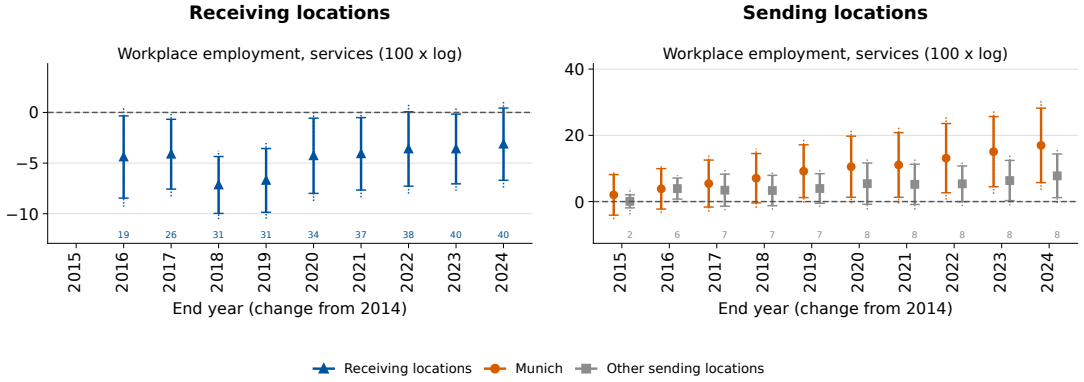


Figure 27. Services workplace employment. Long differences from 2014. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations, where shown, are compared with synthetic controls, with Munich reported separately from the other eight sending locations. Log outcomes are multiplied by 100.

Human Capital

Private employment among workers with tertiary education rises significantly by 4.76% on average, with significant increases growing steadily from 1.3% in the year of the first relocation to 11.9% in year eight. I also observe an occupational requirement scale distinguishing helper jobs at level one, skilled jobs at level two, specialist jobs at level three and expert jobs at level four. High requirement level combines specialist and expert jobs at levels three and four.

Public employment among workers with tertiary education rises by 17.4% on average and by about 39% in year eight. In Munich, private employment at a high requirement level rises significantly by 11.3% and private employment among workers with tertiary education by 11.4%.

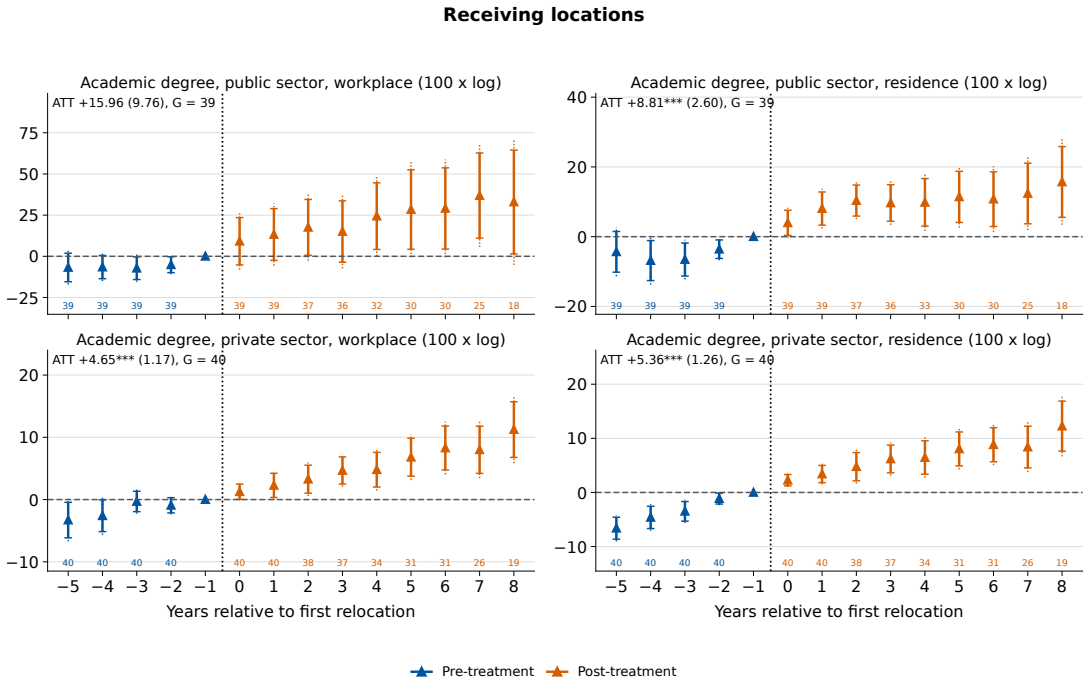


Figure 28. Employment by academic education in receiving locations. Event studies relative to the first relocation. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations, where shown, are compared with synthetic controls, with Munich reported separately from the other eight sending locations. Log outcomes are multiplied by 100. Because relocations occur in different years, the small numbers below the estimates show how many treated locations contribute at each event time.

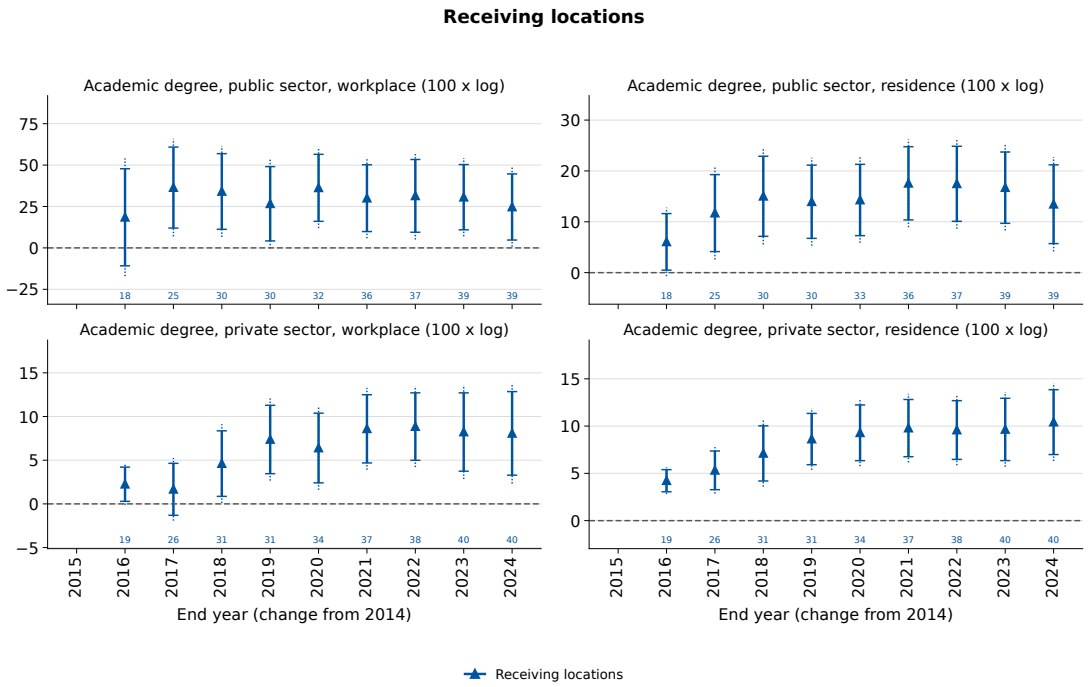


Figure 29. Employment by academic education in receiving locations. Long differences from 2014. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations, where shown, are compared with synthetic controls, with Munich reported separately from the other eight sending locations. Log outcomes are multiplied by 100.

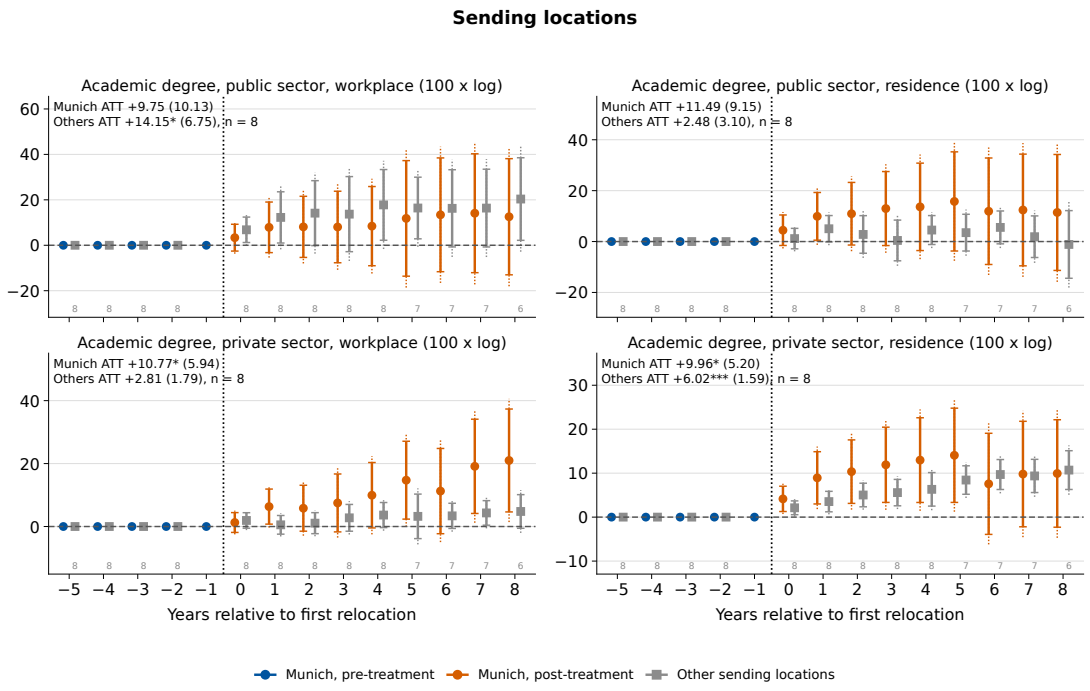


Figure 30. Employment by academic education in sending locations. Event studies relative to the first relocation. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations, where shown, are compared with synthetic controls, with Munich reported separately from the other eight sending locations. Log outcomes are multiplied by 100. Because relocations occur in different years, the small numbers below the estimates show how many treated locations contribute at each event time.

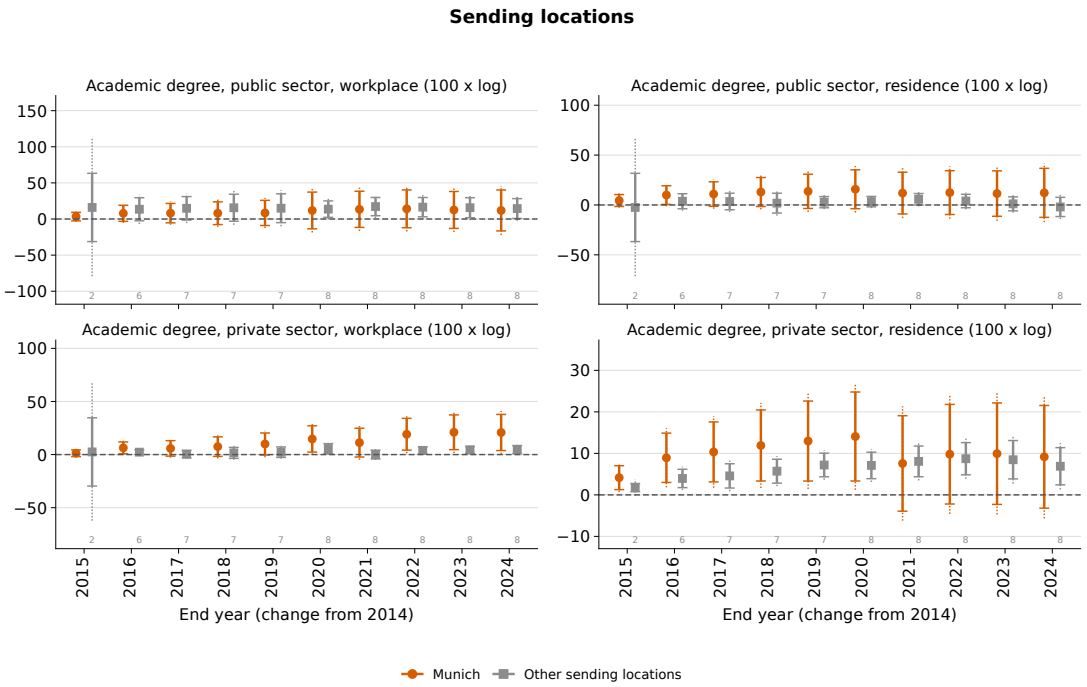


Figure 31. Employment by academic education in sending locations. Long differences from 2014. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations, where shown, are compared with synthetic controls, with Munich reported separately from the other eight sending locations. Log outcomes are multiplied by 100.

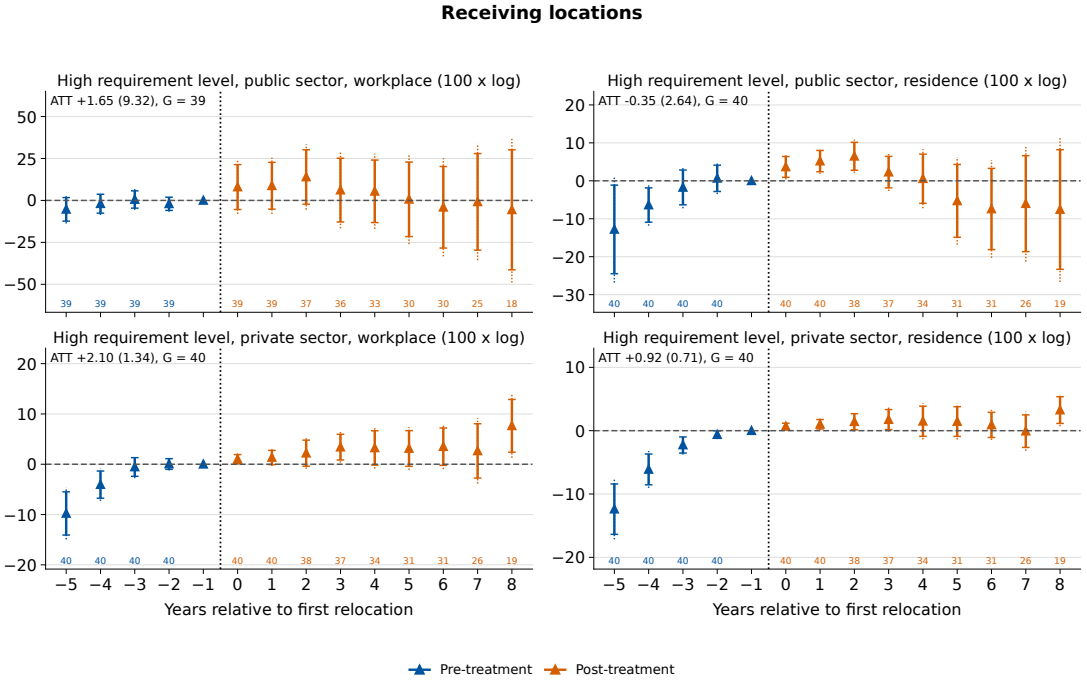


Figure 32. Employment by requirement level in receiving locations. Event studies relative to the first relocation. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations, where shown, are compared with synthetic controls, with Munich reported separately from the other eight sending locations. Log outcomes are multiplied by 100. Because relocations occur in different years, the small numbers below the estimates show how many treated locations contribute at each event time.

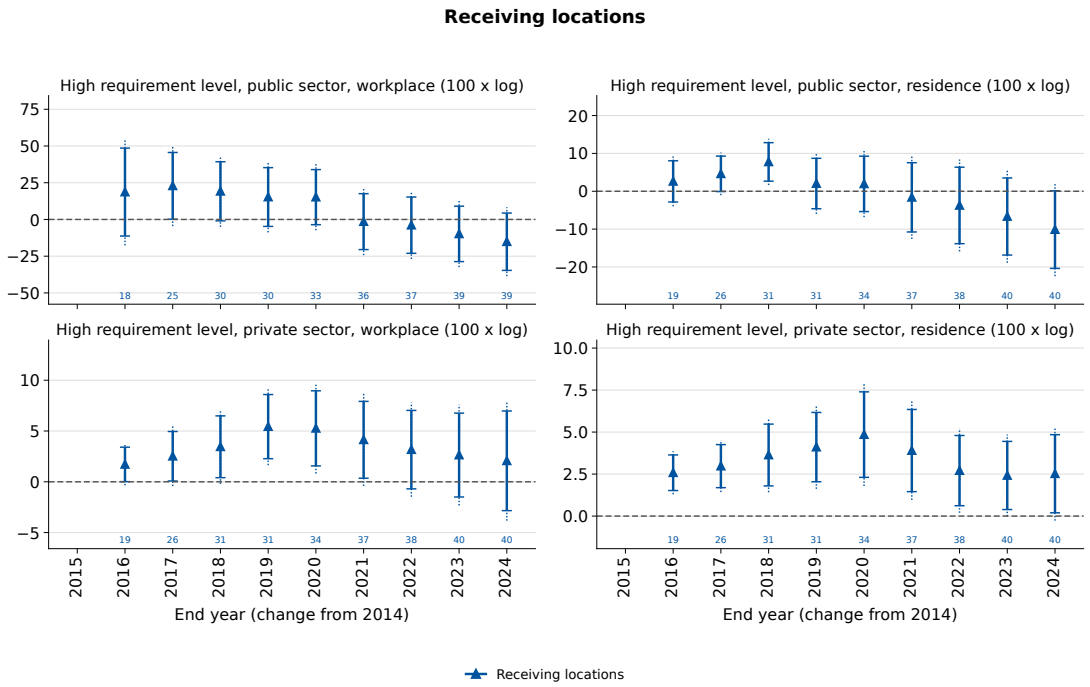


Figure 33. Employment by requirement level in receiving locations. Long differences from 2014. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations, where shown, are compared with synthetic controls, with Munich reported separately from the other eight sending locations. Log outcomes are multiplied by 100.

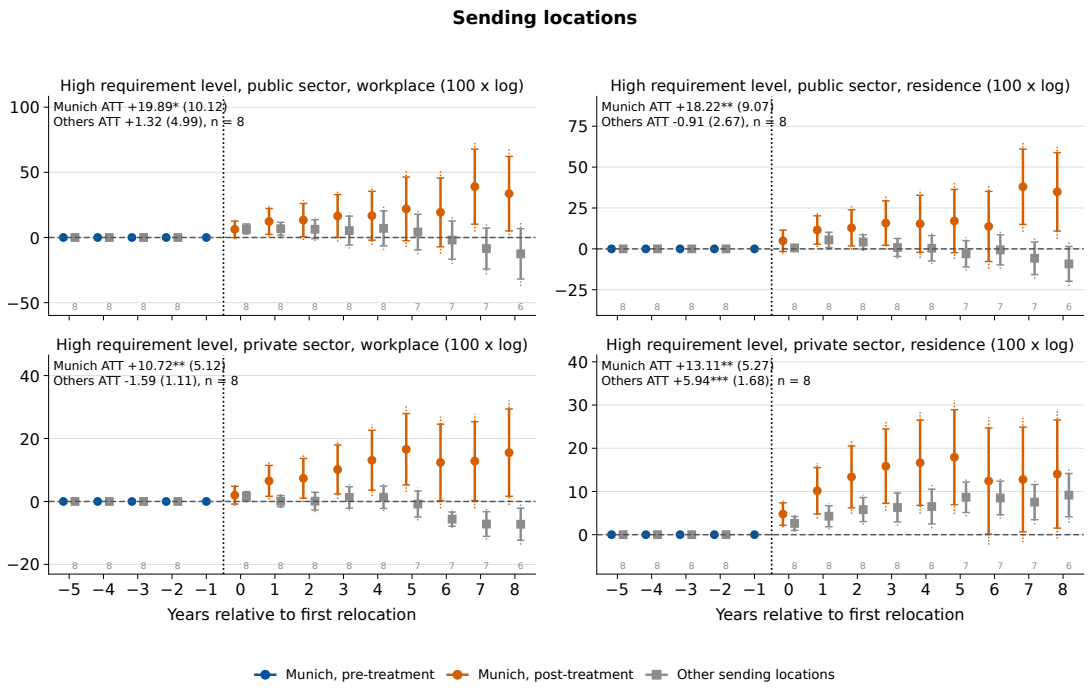


Figure 34. Employment by requirement level in sending locations. Event studies relative to the first relocation. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations, where shown, are compared with synthetic controls, with Munich reported separately from the other eight sending locations. Log outcomes are multiplied by 100. Because relocations occur in different years, the small numbers below the estimates show how many treated locations contribute at each event time.

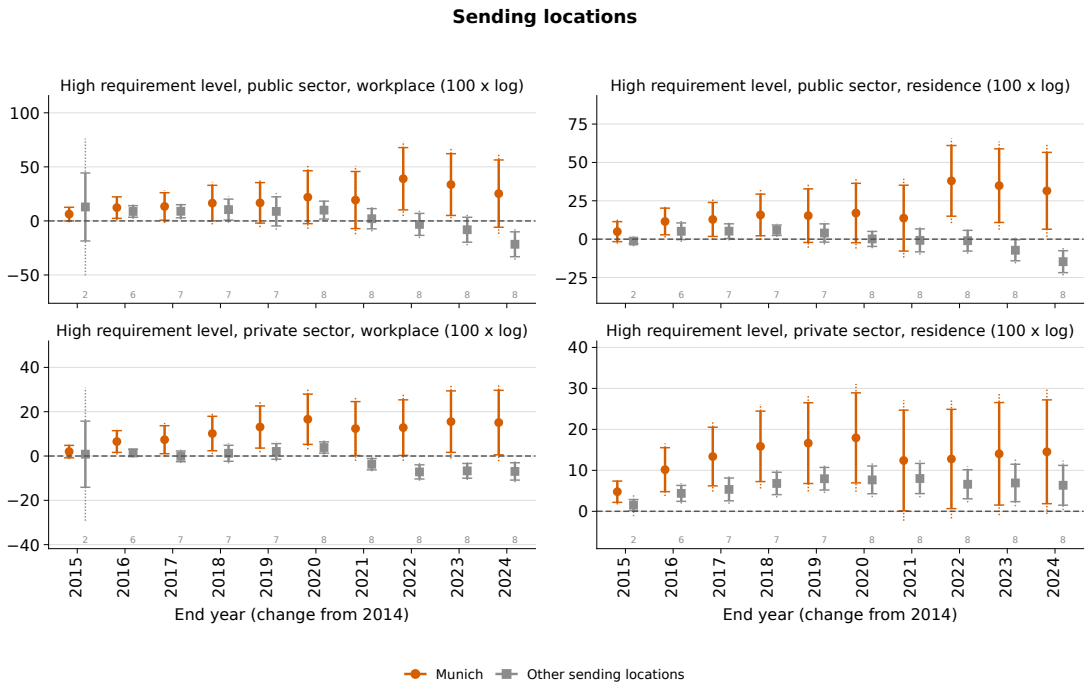


Figure 35. Employment by requirement level in sending locations. Long differences from 2014. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations, where shown, are compared with synthetic controls, with Munich reported separately from the other eight sending locations. Log outcomes are multiplied by 100.

Effects on Factor Prices

Rents rise where the state builds a new office. Rents rise significantly by 3.38% in these locations and fall by 0.65% elsewhere. The difference of 4.05% is statistically significant. Rents move in the same years as total population and private jobs, five to seven years after the first relocation. Housing demand may come from newcomers, while new office buildings may make a location more attractive or signal that the agency will stay.

Commercial Rents

Commercial rents rise in receiving locations five years after the first relocation, in the same years as residential rents. The increase is 2.2% in year four and a significant 4.7% in year five. The average increase of 0.8% is not significant. Munich's average increase is 10.29% and is not significant. The average increase in other sending locations is 2.41% and is not significant.

Munich's private wages fall by 1.35%, but the estimate is not significant. In other sending locations, private wages fall significantly by 3.32% on average, with the decline growing steadily to 6.9% in year eight.

Munich's rents rise far above its synthetic control, with a significant average increase of 29.6%. Purchase prices rise by 23.7%, although this estimate is not significant. Moving 0.14% of Munich's jobs may be too small to offset pressure in a city gaining about 30,000 residents a year. In other sending locations, rents rise significantly by 5.2% and purchase prices by 19.5%.

Rents and Purchase Prices

The long differences for rents and purchase prices end in 2023. In early cohorts, residential rents rise by 3.7% in 2022 and 5.0% by 2023. Purchase prices are +4.4% by 2023, with +10.2% in 2017 and +9.5% in 2022. Commercial rents are +4.6% in 2021. In late cohorts, residential rents are +1.9% (not significant), purchase prices +6.8% by 2023, and commercial rents +4.4% in 2017, +4.6% in 2021 and +4.3% in 2022. In sending locations, rents and purchase prices show no clear pattern in the event study, but commercial rents are higher in some years, by 15.5% four years, +1.4% six years and +6.1% seven years after the relocation (4 sets or fewer). In the long differences, residential rents are −2.7% and purchase prices +1.2% by 2023 (not significant). Purchase prices rise by 6.4% in 2018 and fall by 4.9% in 2020 and 9.4% in 2021, and commercial rents rise by 17.1% in 2019 and 5.4% in 2021.

Residential rents remain flat for five years, then rise significantly by 5% in year six and 7.4% in year seven (Figure 38 in Appendix E). The average increase is 0.81%. Purchase prices rise by 1.46% on average. Their significant increases reach 7.4% in year five and 7.5% in year six.

Figure 36 shows the event studies and Figure 37 the long differences for rents and purchase prices.

Residential rents in receiving locations rise significantly by 3.5 log points in 2019 and 4.4 in 2023. In Munich, the significant gap in residential rents relative to its synthetic

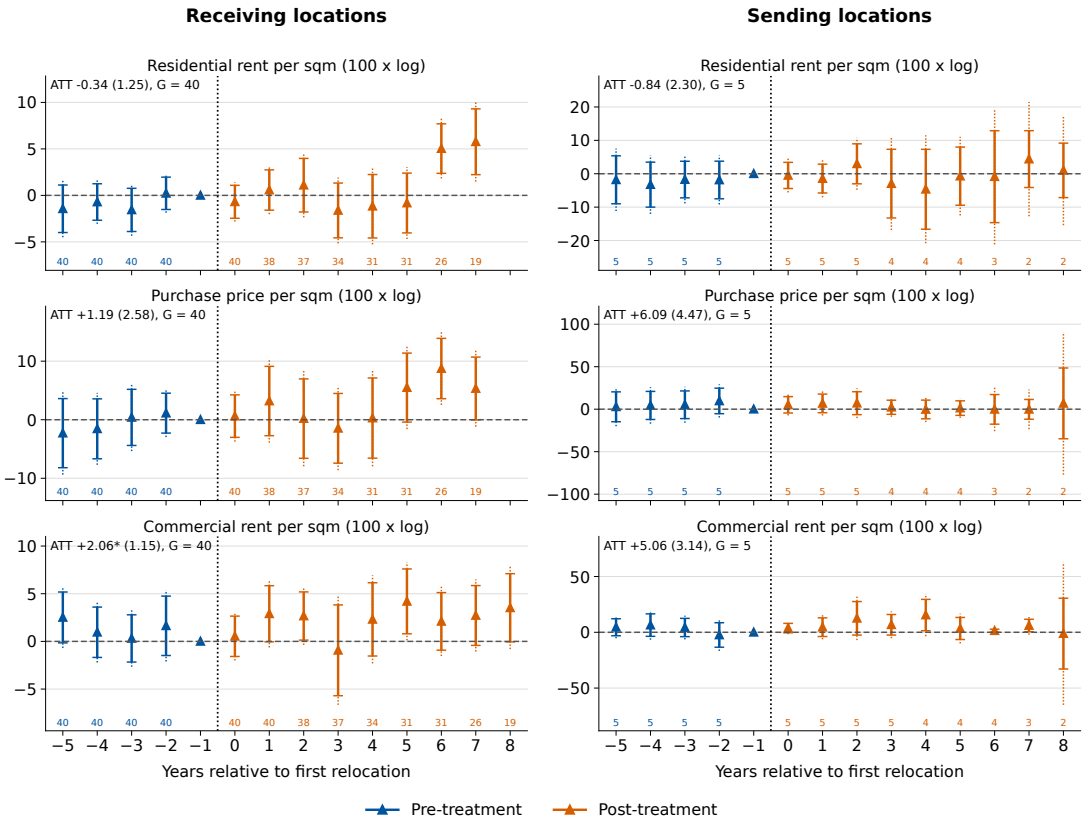


Figure 36. Rents and purchase prices, event studies. Event study estimates relative to the year before the relocation. The number of sets is shown below each estimate. Each point is the mean over matched sets of the treated location minus the mean of its three matched control locations outside the policy state. Solid bars show 90% confidence intervals and dotted extensions show 95% confidence intervals.

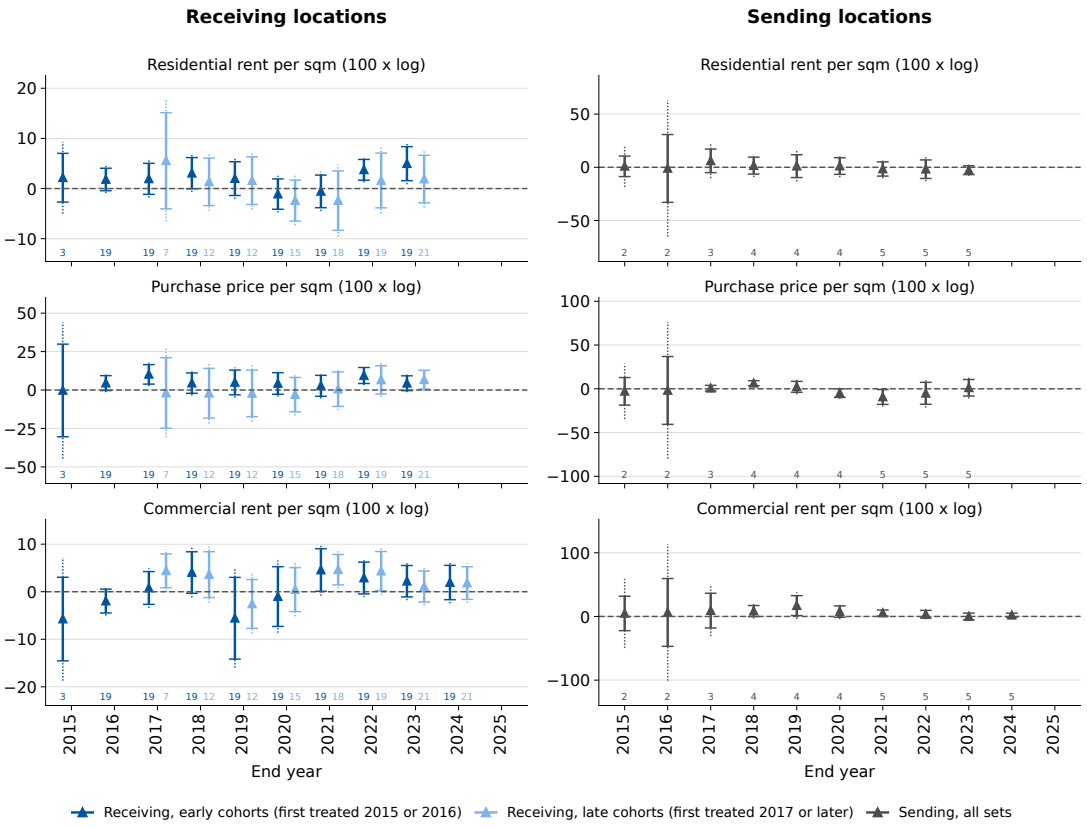


Figure 37. Rents and purchase prices, long differences. Long differences relative to 2014 by end year. Receiving locations are split into early cohorts (2015 to 2016) and late cohorts (2017 and later). Each point is the mean over matched sets of the treated location minus the mean of its three matched control locations outside the policy state. Solid bars show 90% confidence intervals and dotted extensions show 95% confidence intervals.

control reaches 42.5 log points in 2019 and narrows to 15.9 by 2023. Munich's commercial rents rise significantly by 14.8 log points in 2018.

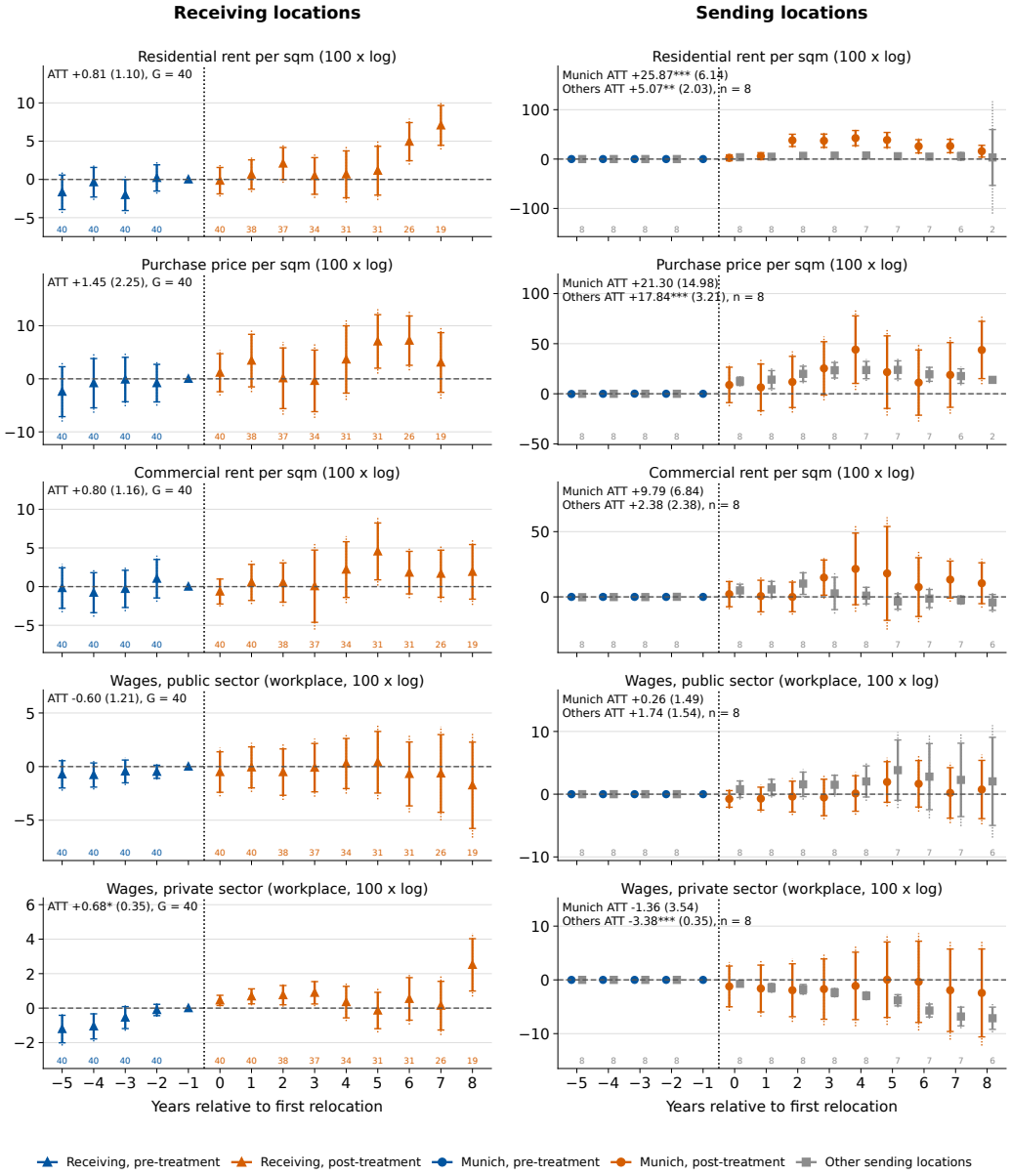


Figure 38. Rents, purchase prices and wages before and after the first relocation. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations are compared with synthetic controls, with Munich shown separately from the other eight sending locations. Log outcomes are multiplied by 100. Because relocations occur in different years, the small numbers below the estimates show how many treated locations contribute at each event time.

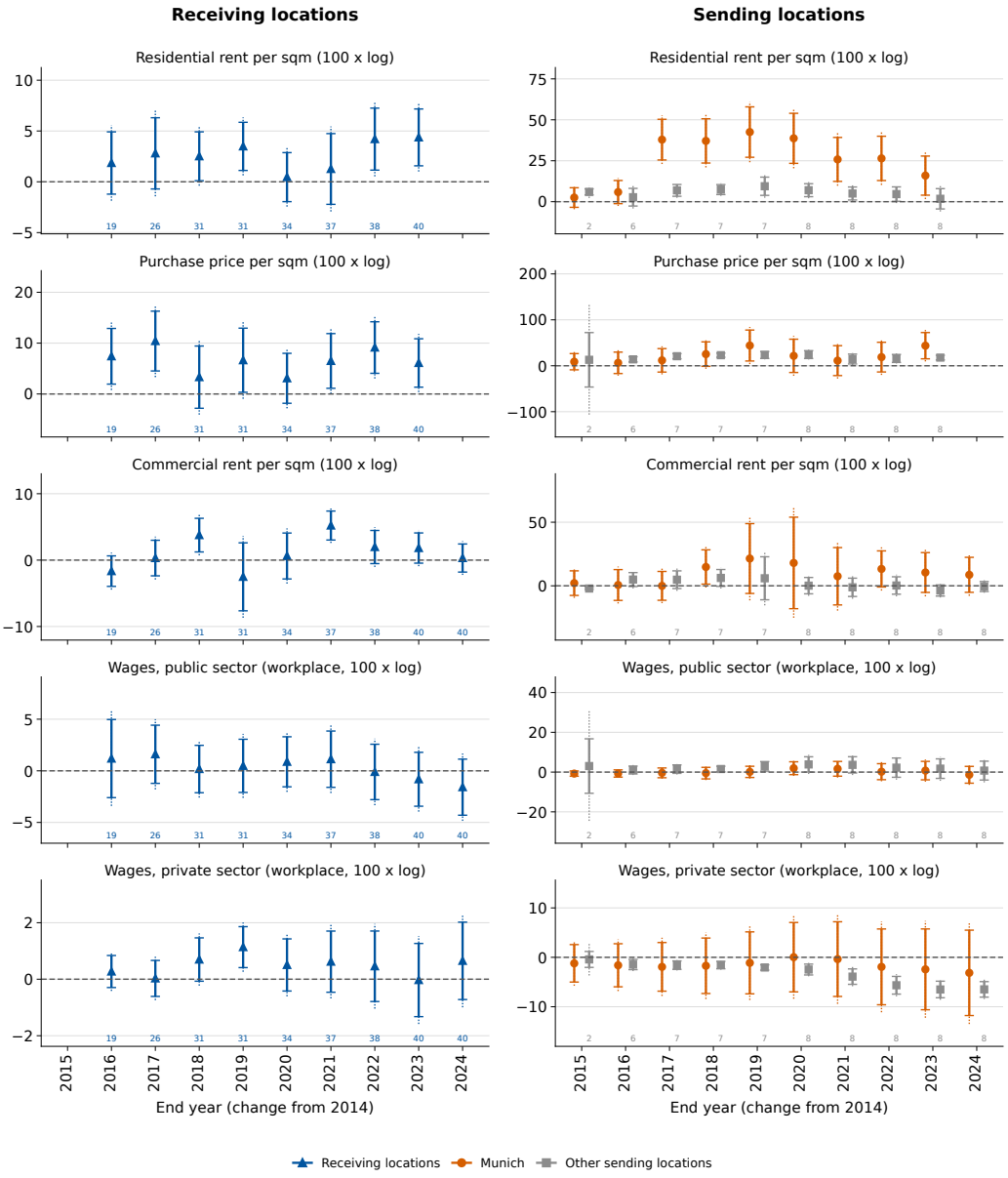


Figure 39. Rents, purchase prices and wages, long differences from 2014. Receiving locations are compared with entropy balancing controls outside the policy state. Sending locations are compared with synthetic controls, with Munich shown separately from the other eight sending locations. Log outcomes are multiplied by 100.

Appendix F Robustness Checks

Spatial Spillovers

Faggio (2019) finds that the employment effects of the 2004 Lyons Review in the United Kingdom are highly localized. I test whether this also holds here and, if effects reach neighboring towns, at which distance they dissipate. This distance determines which untreated towns in the policy state can serve as reference towns for the next check. Localized employment effects would also corroborate the manufacturing signaling result.

Faggio studies rings of 0 to 1, 1 to 2 and 2 to 3 kilometers around relocation sites and finds no effect beyond 3 kilometers. She also finds displacement as private businesses move closer to the sites, with negative effects in the two outer rings. My narrowest band covers 0 to 10 minutes by car and therefore extends over a larger area.

I use the joint model of spatial spillovers described by Butts (2024). The direct effects, effects on neighboring towns and year effect of the policy state are estimated in one regression. This separates changes near relocation sites from changes shared across the policy state. The model includes town fixed effects and year effects by policy cell, where cells combine policy eligibility category and central place status. The sample includes all 2,056 municipalities in the policy state, whether eligible or not, and 4,076 donor towns outside it. Sending towns are excluded as units in this first specification. Appendix F gives the formal specification.

Exposure sums relocated positions, including apprenticeships, at receiving sites within bands of 0 to 10, 10 to 20 and 20 to 30 minutes by car. Effects on neighboring towns are measured per 100 relocated positions. Bands around sending sites enter as control variables. I also extend the bands to 90 minutes and vary the reference cutoff from 0 to 90 minutes. Following Clarke (2017), I widen the bands to identify the distance at which the outer effects disappear. Inference uses standard errors clustered by district region, Conley standard errors with cutoffs of 30, 45 and 60 minutes, and randomization inference that moves receiving sites to untreated towns in the same policy cell.

Employment effects are highly localized. With standard errors clustered by district region, no band within 30 minutes shows a statistically significant effect on any outcome, including the band closest to receiving sites (Appendix Table 27). Population rises by 0.2% per 100 relocated positions in towns 10 to 20 minutes away and by 0.1% in towns 20 to 30 minutes away. These two effects are statistically significant with Conley standard errors, while employment and commuting effects remain insignificant in every band within 30 minutes. See Appendix F for details in Table 28 and Figure 41. Figure 40 shows the direct effect of the main analysis in the center and the effects on towns 0 to 10, 10 to 20 and 20 to 30 minutes away in the rings around it, for receiving locations, Munich and the other sending locations. Significance in the rings uses Conley standard errors with a 60 minute cutoff.

The data do not identify a distance of dissipation. The cutoff selected using district region clusters is zero minutes for all six outcomes.

With bands extending to 90 minutes, population does not rise significantly beyond 30 minutes. Residence employment, in-commuters and out-commuters decline significantly

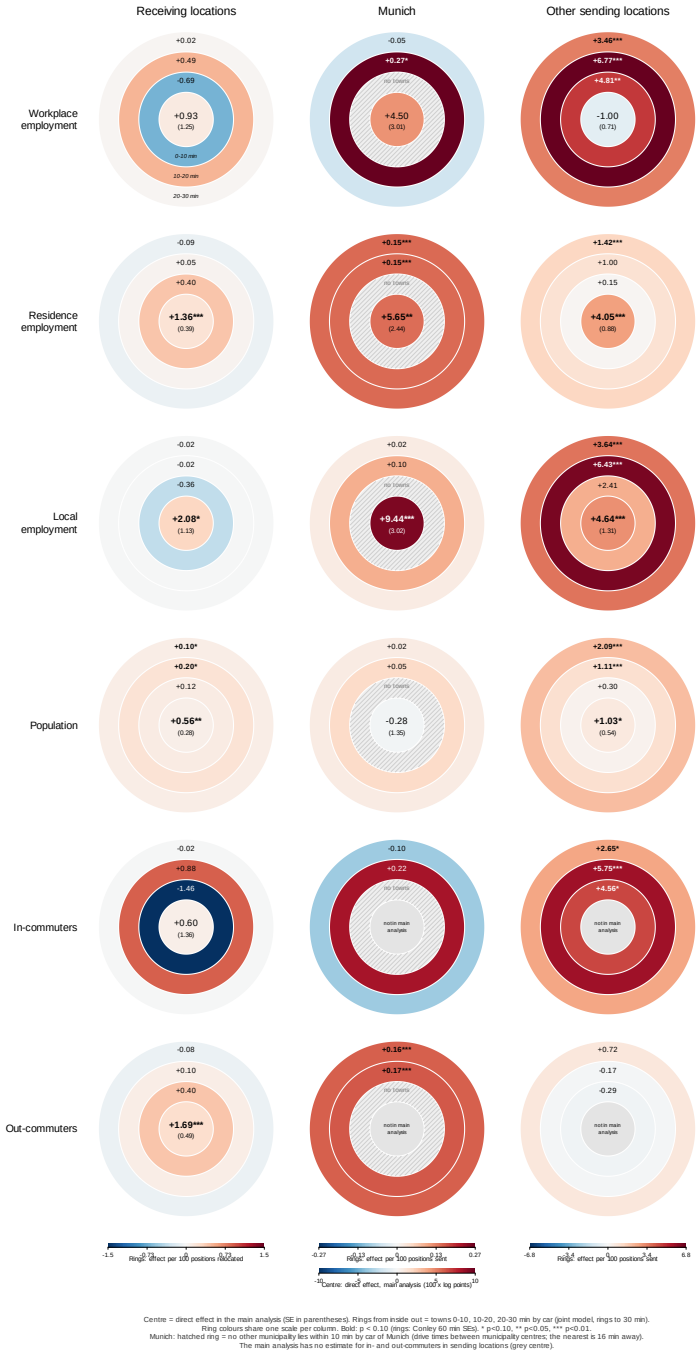
in the band from 60 to 90 minutes when standard errors are clustered by district region (Appendix Tables 29 and 30).

The joint model estimates direct effects, exposure to receiving and sending sites, and the year effect of the policy state in one regression

$$y_{it} = \alpha_i + \lambda_{c(i)t} + \delta_t \mathrm{BY}_i + \sum_e \tau_e D_{it}^e + \sum_b \sum_e \beta_{be} R_{it}^{be} + \sum_b \sum_e \gamma_{be} S_{it}^{be} + \varepsilon_{it}. \tag{24}$$

The town fixed effect is α_i, $\lambda_{c(i)t}$ gives year effects by policy cell, and BY_i indicates towns in the policy state. Policy cells combine policy eligibility category and central place status. The term D_{it}^e marks the event year of the 40 receiving locations. Event years run from five years before to eight years after the first relocation, omitting the year before it. Exposure R_{it}^{be} sums relocated positions, including apprenticeships, across the 53 receiving sites in driving band b at event time e, divided by 100. Exposure S_{it}^{be} does the same for positions sent from the 13 sending sites. The model does not include county or district region by year fixed effects.

The main specification uses bands of 0 to 10, 10 to 20 and 20 to 30 minutes. The extended specification adds bands of 30 to 45, 45 to 60 and 60 to 90 minutes. A separate specification replaces exposure to receiving sites by driving distance with exposure based on commuting before relocation. The regional net effect adds the direct effect and the ring effects, weighted by employment.



Centre = direct effect in the main analysis (SE in parentheses). Rings from inside out = towns 0-10, 10-20, 20-30 min by car (joint model, rings to 30 min).
Ring colours share one scale per column. Bold: p < 0.10 (rings: Conley 60 min SEs). * p<0.10, ** p<0.05, *** p<0.01.
Munich: hatched ring = no other municipality lies within 10 min by car of Munich (drive times between municipality centres; the nearest is 16 min away).
The main analysis has no estimate for in- and out-commuters in sending locations (grey centre).

Figure 40. Direct effects and effects on towns by driving distance

The center shows the direct effect in the main analysis, with the standard error in parentheses. The rings show, from the inside out, the effects on towns 0 to 10, 10 to 20 and 20 to 30 minutes away by car per 100 positions relocated or sent, from the joint model. Bold marks significance at the 10% level, using Conley standard errors with a 60 minute cutoff in the rings. No other municipality lies within 10 minutes of Munich. Gray centers have no estimate in the main analysis. Appendix Tables 27 and 32 report the ring effects with standard errors clustered by district region.

Table 27. Effects on neighboring towns by driving distance

	0 to 10 min	10 to 20 min	20 to 30 min
Workplace employment	−0.69	+0.49	+0.02
	(2.06)	(0.56)	(0.25)
Residence employment	+0.40	+0.05	−0.09
	(0.43)	(0.23)	(0.11)
Local employment	−0.36	−0.02	−0.02
	(1.17)	(0.46)	(0.24)
Population	+0.12	+0.20	+0.10
	(0.18)	(0.16)	(0.08)
In-commuters	−1.46	+0.88	−0.02
	(2.19)	(0.73)	(0.31)
Out-commuters	+0.40	+0.10	−0.08
	(0.42)	(0.24)	(0.10)

The joint model includes bands through 30 minutes. Entries are averages over event years zero to eight, measured in 100 times log per 100 relocated positions, including apprenticeships. Exposure sums positions across 53 receiving sites from all programs, rather than only the 40 receiving locations in the main analysis. Standard errors clustered by district region are in parentheses. Bold marks significance at the 10% level.

Table 28. Standard errors of effects on neighboring towns

Outcome	Ring	Estimate	District region	Conley 30 min	Conley 45 min	Conley 60 min
Workplace employment	0 to 10	−0.69	2.06	1.94	1.96	1.97
	10 to 20	+0.49	0.56	0.57	0.58	0.58
	20 to 30	+0.02	0.25	0.27	0.27	0.28
	45 to 60 †	−0.14	0.23	0.19	0.20	0.20
	60 to 90 †	−0.10	0.09	0.09	0.09	0.09
Residence employment	0 to 10	+0.40	0.43	0.38	0.38	0.38
	10 to 20	+0.05	0.23	0.14	0.14	0.15
	20 to 30	−0.09	0.11	0.08	0.08	0.08
	45 to 60 †	−0.08	0.08	0.06	0.06	0.06
	60 to 90 †	−0.17	**0.04*****	**0.03*****	**0.03*****	**0.04*****
Local employment	0 to 10	−0.36	1.17	1.07	1.08	1.10
	10 to 20	−0.02	0.46	0.40	0.39	0.38
	20 to 30	−0.02	0.24	0.25	0.24	0.24
	45 to 60 †	−0.06	0.23	0.18	0.19	0.19
	60 to 90 †	−0.13	0.10	0.09	0.10	0.11
Population	0 to 10	+0.12	0.18	0.19	0.19	0.18
	10 to 20	+0.20	0.16	**0.10****	**0.10***	**0.11***
	20 to 30	+0.10	0.08	**0.06***	**0.06***	**0.06***
	45 to 60 †	+0.02	0.06	0.04	0.04	0.04
	60 to 90 †	+0.01	0.02	0.02	0.02	0.02
In-commuters	0 to 10	−1.46	2.19	2.09	2.11	2.11
	10 to 20	+0.88	0.73	0.67	0.64	0.61
	20 to 30	−0.02	0.31	0.34	0.34	0.34
	45 to 60 †	−0.24	0.22	0.23	0.23	0.23
	60 to 90 †	−0.20	**0.11***	**0.12***	0.12	0.13
Out-commuters	0 to 10	+0.40	0.42	0.37	0.38	0.38
	10 to 20	+0.10	0.24	0.15	0.16	0.16
	20 to 30	−0.08	0.10	0.09	0.09	0.10
	45 to 60 †	−0.12	**0.07***	**0.06***	**0.07***	0.07
	60 to 90 †	−0.20	**0.05*****	**0.04*****	**0.04*****	**0.05*****

Effects are measured in 100 times log per 100 relocated positions, including apprenticeships. Bands through 30 minutes use the joint model with bands to 30 minutes. Bands marked with a dagger use the model with bands to 90 minutes. Stars refer to the normal approximation with each standard error. Bold marks significance at the 10% level.

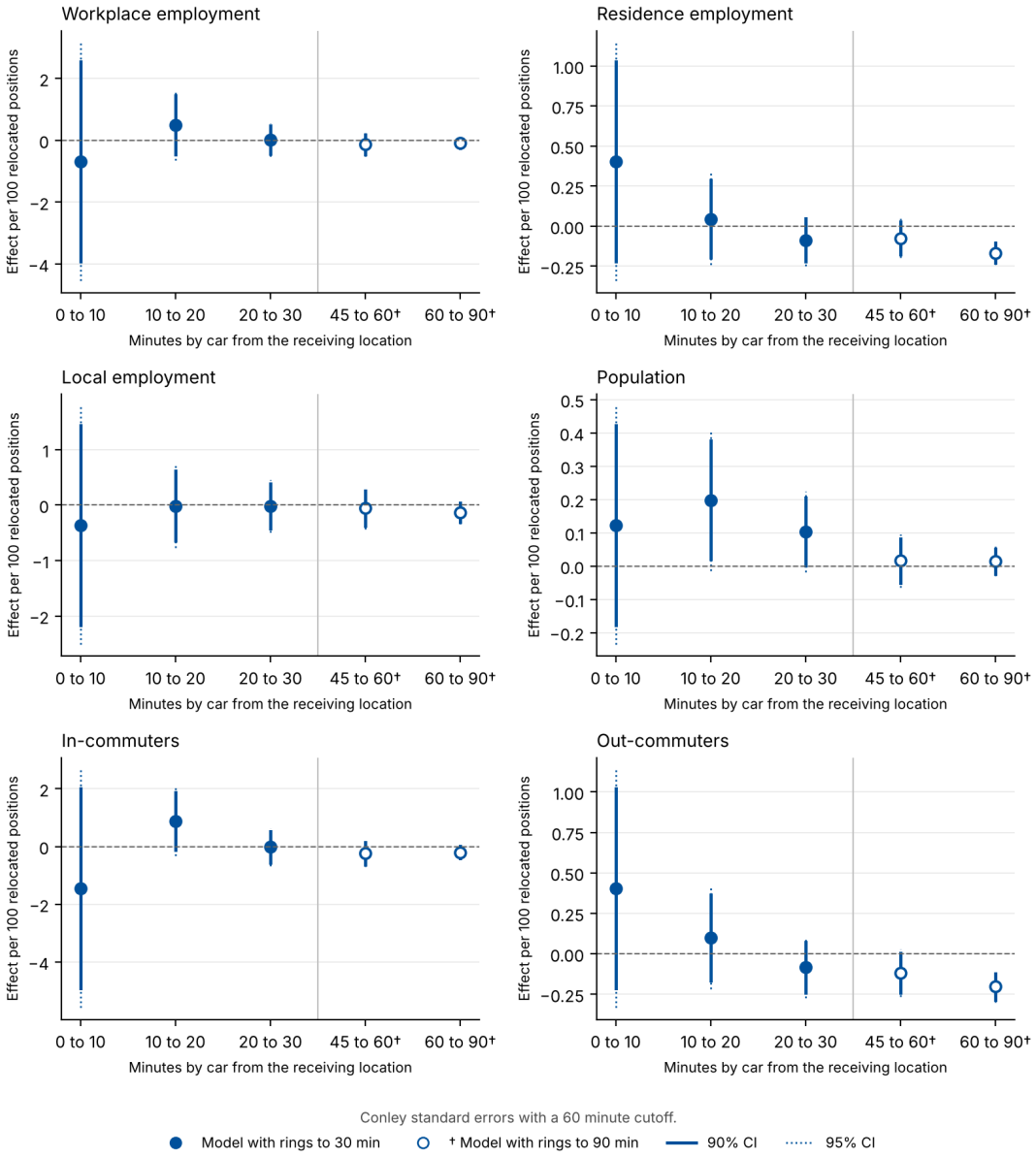


Figure 41. Effects on neighboring towns with Conley confidence intervals
Filled markers use the joint model with bands to 30 minutes. Hollow markers use bands to 90 minutes. Bars show 90% solid and 95% dotted confidence intervals using Conley standard errors with a 60 minute cutoff. Effects are measured in 100 times log per 100 positions.

Table 29. Effects on neighboring towns with driving bands to 90 minutes

Outcome	0 to 10 min	10 to 20 min	20 to 30 min	30 to 45 min	45 to 60 min	60 to 90 min
Workplace employment	−0.83	+0.43	+0.04	+0.23	−0.14	−0.10
	(2.11)	(0.61)	(0.26)	(0.27)	(0.23)	(0.09)
Residence employment	+0.36	+0.06	−0.01	+0.05	−0.08	**−0.17*****
	(0.39)	(0.24)	(0.11)	(0.07)	(0.08)	(0.04)
Local employment	−0.40	−0.04	+0.00	+0.19	−0.06	−0.13
	(1.14)	(0.48)	(0.25)	(0.22)	(0.23)	(0.10)
Population	+0.13	+0.17	+0.07	+0.04	+0.02	+0.01
	(0.19)	(0.17)	(0.08)	(0.06)	(0.06)	(0.02)
In-commuters	−1.61	+0.82	−0.01	+0.15	−0.24	**−0.20***
	(2.22)	(0.76)	(0.32)	(0.31)	(0.22)	(0.11)
Out-commuters	+0.33	+0.09	−0.04	+0.07	**−0.12***	**−0.20*****
	(0.37)	(0.24)	(0.10)	(0.07)	(0.07)	(0.05)

Effects are measured in 100 times log per 100 relocated positions, including apprenticeships. Entries are averages over event years zero to eight. Standard errors clustered by district region are in parentheses. Bold marks significance at the 10% level.

Table 30. Conley standard errors with driving bands to 90 minutes

Outcome	Ring	Estimate	District region	Conley 30 min	Conley 45 min	Conley 60 min
Workplace employment	0 to 10	−0.83	2.11	1.96	1.98	1.99
	10 to 20	+0.43	0.61	0.59	0.59	0.59
	20 to 30	+0.04	0.26	0.30	0.30	0.30
	30 to 45	+0.23	0.27	0.23	0.23	0.23
	45 to 60	−0.14	0.23	0.19	0.20	0.20
	60 to 90	−0.10	0.09	0.09	0.09	0.09
Residence employment	0 to 10	+0.36	0.39	0.35	0.36	0.36
	10 to 20	+0.06	0.24	0.14	0.15	0.15
	20 to 30	−0.01	0.11	0.08	0.09	0.09
	30 to 45	+0.05	0.07	0.06	0.06	0.07
	45 to 60	−0.08	0.08	0.06	0.06	0.06
	60 to 90	−0.17	**0.04****	**0.03****	**0.03****	**0.04****
Local employment	0 to 10	−0.40	1.14	1.07	1.08	1.10
	10 to 20	−0.04	0.48	0.42	0.41	0.41
	20 to 30	+0.00	0.25	0.27	0.26	0.27
	30 to 45	+0.19	0.22	0.20	0.21	0.22
	45 to 60	−0.06	0.23	0.18	0.19	0.19
	60 to 90	−0.13	0.10	0.09	0.10	0.11
Population	0 to 10	+0.13	0.19	0.19	0.19	0.19
	10 to 20	+0.17	0.17	**0.10***	0.11	0.11
	20 to 30	+0.07	0.08	0.06	0.06	0.07
	30 to 45	+0.04	0.06	0.05	0.05	0.05
	45 to 60	+0.02	0.06	0.04	0.04	0.04
	60 to 90	+0.01	0.02	0.02	0.02	0.02
In-commuters	0 to 10	−1.61	2.22	2.07	2.09	2.09
	10 to 20	+0.82	0.76	0.69	0.66	0.63
	20 to 30	−0.01	0.32	0.37	0.37	0.37
	30 to 45	+0.15	0.31	0.29	0.29	0.30
	45 to 60	−0.24	0.22	0.23	0.23	0.23
	60 to 90	−0.20	**0.11***	**0.12***	0.12	0.13
Out-commuters	0 to 10	+0.33	0.37	0.35	0.36	0.35
	10 to 20	+0.09	0.24	0.16	0.16	0.16
	20 to 30	−0.04	0.10	0.09	0.10	0.10
	30 to 45	+0.07	0.07	0.07	0.08	0.08
	45 to 60	−0.12	**0.07***	**0.06***	**0.07***	0.07
	60 to 90	−0.20	**0.05*****	**0.04*****	**0.04*****	**0.05*****

Effects are measured in 100 times log per 100 relocated positions, including apprenticeships. Stars refer to the normal approximation with each standard error. Bold marks significance at the 10% level.

Exposure by Commuting

Driving time treats towns at the same distance alike. Commuting exposure instead measures which towns supplied each receiving site's workers before relocation. I use each town's share of a site's workforce in June 2014 to allocate the site's relocated positions. Exposure is the expected percentage gain in residence employment if the new positions are filled in the same way. A town that supplied 10% of a site's workers would receive 10% of its new positions. I replace the driving bands around receiving sites with commuting exposure by event time and retain the bands around sending sites.

Commuting exposure has no statistically significant effect on any outcome with standard errors clustered by district region (Table 31). A one percentage point expected gain is associated with a 0.35% increase in residence employment. If positions were filled exactly as before relocation, this coefficient would be one. The estimate is too imprecise to distinguish zero from one. Population rises by 0.86% per percentage point of expected gain, significant with Conley standard errors using a 60 minute cutoff. Commuting exposure thus gives the same pattern as driving distance, with no significant employment spillovers and a population gain that depends on the standard errors used.

Table 31. Commuting exposure around receiving sites

Outcome	Commuting exposure	Conley 60 min SE	Direct effect in this model
Workplace employment	+0.26	2.74	−0.72
	(2.47)		(1.03)
Residence employment	+0.35	0.73	+0.62
	(0.98)		(0.59)
Local employment	+0.57	2.16	−0.67
	(2.31)		(1.09)
Population	+0.86	**0.51***	**+0.62***
	(0.76)		(0.36)
In-commuters	+2.19	3.29	−1.61
	(3.23)		(1.28)
Out-commuters	+0.49	0.82	**+1.77****
	(1.04)		(0.83)

The joint model replaces driving bands around receiving sites with commuting exposure. Effects in the exposure column are measured in 100 times log per percentage point of expected gain in residence employment. Direct effects are measured in 100 times log. Standard errors clustered by district region are in parentheses. The Conley column uses a 60 minute cutoff. Stars in that column use the normal approximation. Bold marks significance at the 10% level.

Rings Around Sending Sites

I next examine the bands around sending sites in the joint model, separating Munich's bands from those around the other sending sites. Bands around receiving sites remain in the model. As in the main analysis, I exclude Passau, Ansbach and Weiden. Passau

lies in a district region with receiving locations, while Ansbach and Weiden switch from sending to receiving. Nuremberg is not a sending unit here because it is a net receiving site in the file of all relocations.

Towns 10 to 20 minutes from Munich gain 0.27% in workplace employment and 0.15% in residence employment per 100 positions sent. Both effects are significant with standard errors clustered by district region (Appendix Table 32). With Conley standard errors, the workplace employment gain in towns 10 to 20 minutes from Munich is significant only at the 10% level with a 60 minute cutoff and not significant with cutoffs of 30 or 45 minutes, while the residence employment gain in these towns remains significant (Figure 40 and Appendix Table 34). There is no estimate for Munich's 0 to 10 minute band because it contains no other town in the driving-time data.

With bands extending to 90 minutes, residence employment rises significantly in every band around Munich. The data therefore do not identify a distance at which these effects disappear within 90 minutes (Appendix Table 33). Around the other sending locations, workplace employment rises significantly within 30 minutes and population rises significantly at 10 to 30 minutes. Figure 42 in Appendix F shows the effects around Munich and the other sending locations with Conley confidence intervals.

Table 32. Effects on towns around sending sites by driving distance

	Munich		Other sending locations		
	10 to 20 min	20 to 30 min	0 to 10 min	10 to 20 min	20 to 30 min
Workplace employment	**+0.27*****	−0.05	**+4.81****	**+6.77*****	**+3.46*****
	(0.08)	(0.07)	(2.19)	(1.72)	(1.04)
Residence employment	**+0.15*****	**+0.15*****	+0.15	+1.00	**+1.42****
	(0.02)	(0.03)	(1.20)	(1.02)	(0.56)
Local employment	+0.10	+0.02	+2.41	**+6.43*****	**+3.64*****
	(0.07)	(0.07)	(3.44)	(1.62)	(1.33)
Population	+0.05	+0.02	+0.30	**+1.11****	**+2.09*****
	(0.04)	(0.02)	(0.82)	(0.53)	(0.43)
In-commuters	**+0.22*****	−0.10	**+4.56****	**+5.75*****	**+2.65****
	(0.08)	(0.08)	(2.10)	(1.87)	(1.09)
Out-commuters	**+0.17*****	**+0.16*****	−0.29	−0.17	+0.72
	(0.03)	(0.04)	(1.22)	(1.08)	(0.57)

Effects are averages over event years zero to eight, measured in 100 times log per 100 positions sent. The joint model includes bands through 30 minutes. There is no estimate for Munich's 0 to 10 minute band because it contains no other town in the driving-time data. Standard errors clustered by district region are in parentheses. Bold marks significance at the 10% level.

Table 33. Effects on towns around Munich with driving bands to 90 minutes

Outcome	10 to 20	20 to 30	30 to 45	45 to 60	60 to 90
Workplace employment	**+0.29****	−0.02	+0.08	+0.14	**+0.10****
	(0.12)	(0.10)	(0.11)	(0.09)	(0.05)
Residence employment	**+0.22*****	**+0.22*****	**+0.14*****	**+0.13*****	**+0.09*****
	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)
Local employment	+0.05	−0.01	+0.14	+0.10	**+0.09***
	(0.11)	(0.12)	(0.12)	(0.07)	(0.05)
Population	+0.01	−0.02	−0.01	+0.03	**+0.03****
	(0.04)	(0.03)	(0.02)	(0.02)	(0.01)
In-commuters	+0.22	−0.09	+0.04	+0.16	**+0.11***
	(0.14)	(0.11)	(0.13)	(0.10)	(0.06)
Out-commuters	**+0.27*****	**+0.27*****	**+0.16*****	**+0.17*****	**+0.12*****
	(0.05)	(0.05)	(0.04)	(0.04)	(0.03)

Effects are measured in 100 times log per 100 positions sent. Standard errors clustered by district region are in parentheses. Bold marks significance at the 10% level.

Table 34. Standard errors of Munich's ring effects

Outcome	Ring	District region	Conley 30	Conley 45	Conley 60
Workplace employment	10 to 20	0.078	0.184	0.171	0.164
Workplace employment	20 to 30	0.068	0.067	0.061	0.060
Residence employment	10 to 20	0.023	0.052	0.049	0.047
Residence employment	20 to 30	0.030	0.030	0.029	0.028
Local employment	10 to 20	0.068	0.143	0.128	0.113
Local employment	20 to 30	0.074	0.081	0.078	0.073
Population	10 to 20	0.035	0.038	0.037	0.035
Population	20 to 30	0.018	0.019	0.019	0.019
In-commuters	10 to 20	0.078	0.191	0.167	0.153
In-commuters	20 to 30	0.076	0.082	0.075	0.070
Out-commuters	10 to 20	0.031	0.046	0.043	0.042
Out-commuters	20 to 30	0.035	0.039	0.040	0.038

The joint model includes driving bands through 30 minutes. Columns compare standard errors clustered by district region with Conley standard errors using cutoffs of 30, 45 and 60 minutes.

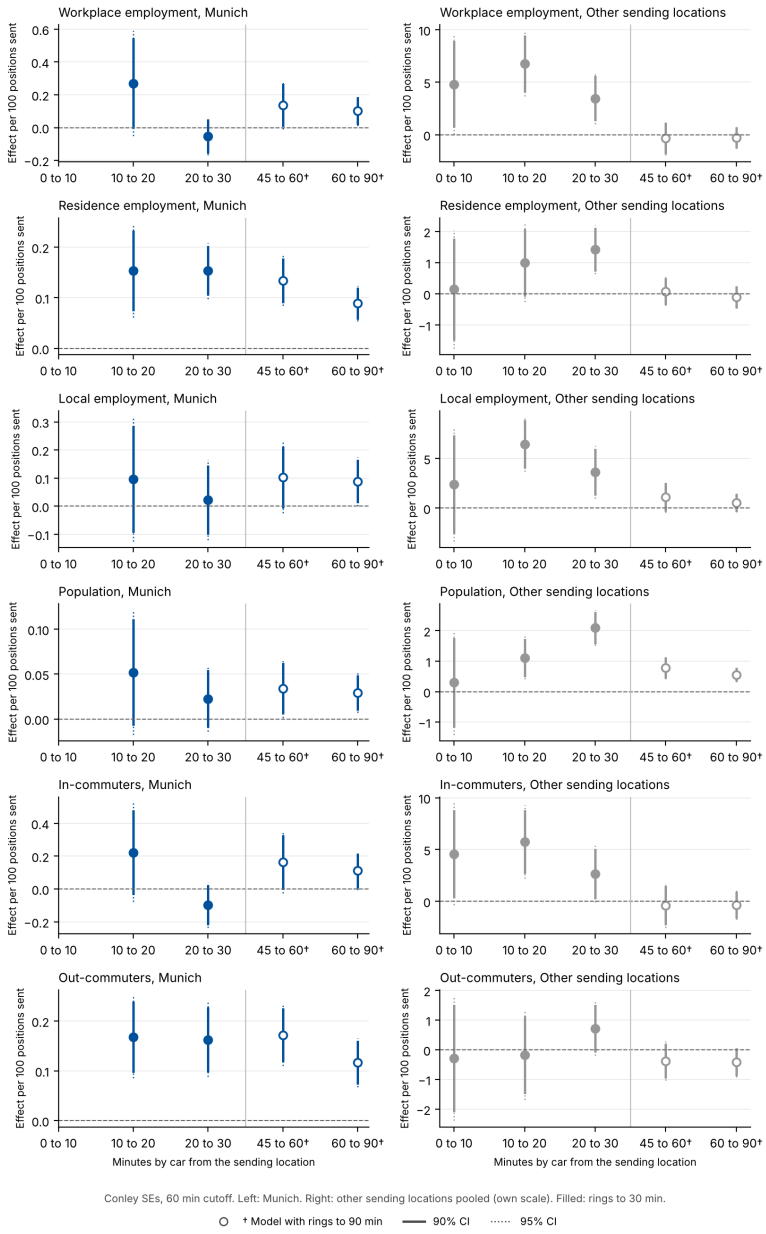


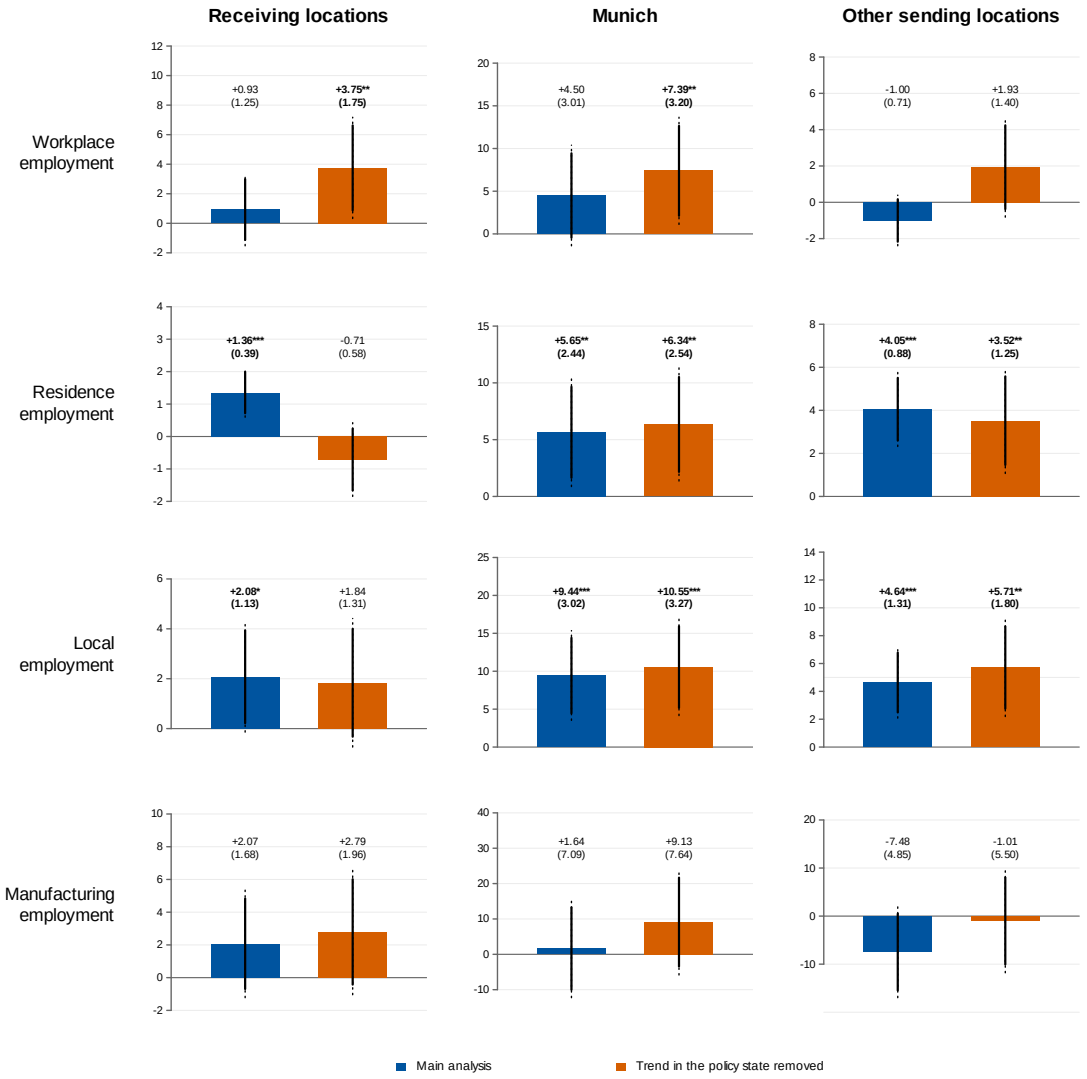
Figure 42. Effects on towns around sending locations with Conley confidence intervals
Munich and the other sending locations are shown on separate scales. Filled markers use bands to 30 minutes and hollow markers use bands to 90 minutes. There is no estimate for Munich's 0 to 10 minute band because it contains no other town in the driving-time data. Bars show 90% solid and 95% dotted confidence intervals using Conley standard errors with a 60 minute cutoff. Effects are measured in 100 times log per 100 positions.

Removing the Trend in the Policy State

The reference group contains 432 towns at least 30 minutes from every relocation site, using the conservative buffer from the spillover check to exclude towns exposed to the population spillover detected in the previous section.

For each year from 2008 to 2025, I measure the gap between these reference towns and the cell's controls outside the policy state.

If these towns lose workplace employment relative to their controls outside the policy state, the refitted synthetic control falls and Munich's estimated gain increases.



ATT over event years 0 to 8, 100 x log points (standard error). Bold: p < 0.10. Solid lines: 90% CI. Dotted: 95% CI. * p<0.10, ** p<0.05, *** p<0.01.

Figure 43. Employment in receiving and sending locations before and after removing the trend in the policy state

Blue bars show the main analysis and orange bars the estimates after removing the trend in the policy state. For receiving locations (left), the triple difference is estimated by imputation, with reference towns at least 30 minutes from every relocation site, entropy balanced within policy cells using the smallest feasible tolerance, and standard errors clustered by set. For Munich (middle) and the eight other sending locations pooled (right), the synthetic control is estimated again after subtracting the trend measured in seven untreated towns in the policy state with at least 20,000 inhabitants in 2014, at least 30 minutes from every relocation site. Entries are averages over event years zero to eight, measured in 100 times log. The numbers above each bar give the estimate with its standard error in parentheses, in bold when significant at the 10% level. Solid lines show 90% and dotted lines 95% confidence intervals. Each panel has its own scale.

For receiving locations, I estimate the triple difference by imputation. Let n_c be the number of treated sets in policy cell c and let N be the total number of sets. The trend in the policy state is the gap between untreated reference towns and controls outside the policy state, averaged across cells

$$\Delta_t^{\mathrm{BY}} = \sum_c \frac{n_c}{N} \left(\sum_{r \in \mathcal{R}_c} v_{rc} Y_{rt} - \sum_{j \in \mathcal{D}_c} w_{jc} Y_{jt} \right). \tag{25}$$

Reference towns $\mathcal{R}_c$ are not relocation sites in any program and lie at least 30 minutes from every relocation site. Their entropy balancing weights v_{rc} match the five index levels of the cell's treated towns. The set $\mathcal{D}_c$ contains the cell's controls outside the policy state, with weights w_{jc}. Where exact balance is infeasible, I use the smallest feasible tolerance of 0.25, 0.5 or 1 standard deviation.

For receiving location i in matched set $s(i)$, let $\overline{Y}^{\mathrm{out}}_{s(i)t}$ denote its weighted controls outside the policy state. Its counterfactual combines these controls, the trend in the policy state and a town constant fitted over the five years before relocation

$$\widehat{Y}_{it}(0) = \overline{Y}^{\mathrm{out}}_{s(i)t} + \Delta_t^{\mathrm{BY}} + \mu_i, \tag{26}$$

$$\mu_i = \frac{1}{5} \sum_{e=-5}^{-1} \left(Y_{i,G_i+e} - \overline{Y}^{\mathrm{out}}_{s(i),G_i+e} - \Delta^{\mathrm{BY}}_{G_i+e} \right). \tag{27}$$

Here, G_i is the first relocation year. The effect is the difference between the observed outcome and this counterfactual, averaged over event years zero to eight. Standard errors are clustered by set.

For sending locations, the reference group consists of untreated towns in the policy state with at least 20,000 inhabitants in 2014, at least 30 minutes from every relocation site. Controls outside the policy state are entropy balanced to these towns on the five index levels and log population in 2014. I subtract the resulting trend for large towns from each sending location's outcome series and estimate its synthetic control again.

Appendix G: Quantitative Model

This appendix derives the worker choices and baseline inversion for the model in Section 8, documents the quantification, and reports validation checks. It documents how wages, productivity, amenities, office requirements, housing demand, and the public budget combine to rationalize the baseline economy.

Data preparation. The baseline uses employment subject to social-security contributions in 2014 by workplace and residence municipality from the IAB employment records. Public employment is employment in public administration, defense, and compulsory social security (WZ 2008 division 84). All other industries define private employment. These are industry classifications, and civil servants are not covered. The model has 5,731 locations. These are the 2,055 municipalities in the policy state, 3,675 municipalities outside the policy state, and the rest of Germany as one location. Residents equal total jobs nationally, 36,450,961. Wages are the private workplace wages per worker in the 2014 employment records. Road travel times τ_{ij} are car travel times in minutes between municipalities from OSRM routing, with residence first and workplace second in worker choices. The commuting time within municipality i is the mean distance between two points in a disk of the municipality's area, $\frac{128}{45\pi}\sqrt{\text{area}_i/\pi}$ km, at 30 km per hour and with a circuity factor of 1.25. In the productivity and amenity kernels, the first index is the recipient and the second the source of density, and the travel time within a municipality is zero. Travel links across the policy-state boundary remain in the model.

Commuting parameters. A gravity equation for bilateral commuting flows between municipalities in 2014 from the Federal Employment Agency, for employees subject to social security who live in the policy state, identifies $\kappa\epsilon = 0.0786$ per minute (standard error 0.0059) on 35,169 municipality pairs. It is estimated by Poisson pseudo-maximum likelihood with residence and workplace fixed effects and a control function for travel time. I choose the Fréchet shape ϵ to match the cross-municipality variance of log private workplace wages in 2014 across the 5,731 locations. This gives $\widehat{\epsilon} = 5.156$ and $\widehat{\kappa} = \widehat{\kappa\epsilon}/\widehat{\epsilon} = 0.0152$ per minute.

Estimation of the amenity elasticity. I estimate the amenity elasticity η by two-step GMM on the county version of the same one-pool model. The model inversion covers 412 counties, and the moments use the 96 counties in the policy state, with the change between the Census 2011 and the Census 2022 and rents from Ahlfeldt, Heblich, and Seidel (2023). In the county model, $\epsilon = 7.153$. The estimate is $\eta = -0.871$ (standard error 0.243). The p-value of the J test is 0.25 and the first-stage F statistic is 3.6. I use this estimate in the municipal model.

Choice probabilities. Independence of the Fréchet draws gives the distribution of the best alternative,

$$\Pr\left(\max_{i,j} u_{ijo} \leq u\right) = \exp\left(-\Phi u^{-\epsilon}\right).$$

This distribution yields the probabilities and expected utility in equations (4) and (5). Expectations exist for $\epsilon > 1$. Bilateral flows satisfy

$$\sum_{i,j} N_{ij} = \bar{H}, \qquad \sum_i H_{Ri} = \sum_j H_{Mj} = \bar{H}, \qquad H^P_{Mj} = H_{Mj} - \bar{H}^G_{Mj}. \tag{28}$$

Probabilities sum to one over all residence–workplace pairs. Public jobs are a fixed slice of the jobs at each workplace, so the workplace margin preserves the government's allocation of public jobs.

Residential inversion. With $D_{ij} = \exp(-\kappa\epsilon\tau_{ij})$, the inversion recovers a residence factor Z_i and a workplace factor y_j from resident employment and total workplace employment,

$$Z_i^0 = \frac{H^0_{Ri}}{\sum_j D_{ij} y_j^0}, \qquad y_j^0 = \frac{H^0_{Mj}}{\sum_i D_{ij} Z_i^0}, \tag{29}$$

where H^0_{Mj} counts private and public jobs. The iteration converges in 1,038 steps to a largest absolute log gap of 10^{-12}, so resident employment and total workplace employment are matched exactly. Wages are data, and the workplace factor is $E_j = y_j^0/(w_j^0)^\epsilon$. Relative to its geometric mean, E_j lies between 0.94 at the 1st percentile and 1.57 at the 99th percentile. The common tax cancels from the employment probabilities. The recovered residential index gives $B_i^0 = (Q_i^{R,0})^{1-\beta}(Z_i^0)^{1/\epsilon}$.

Production and space inversion. Wages and rents recover private productivity from zero profits and private floor space from factor payments.

$$A_j^{P,0} = \left(\frac{w_j^0}{\alpha}\right)^\alpha \left(\frac{q_j^0}{1-\alpha}\right)^{1-\alpha}, \qquad L_j^{P,0} = \frac{1-\alpha}{\alpha}\frac{w_j^0 H^{P,0}_{Mj}}{q_j^0}. \tag{30}$$

Equation (12) gives baseline public office space. Payroll and office costs determine t^0, and bilateral flows allocate gross earnings to residences. Housing demand gives $L_i^{R,0} = (1-\beta)v_i^0/Q_i^{R,0}$. The effective stock is $L_i = L_i^{P,0} + \bar{L}_i^{G,0} + L_i^{R,0}$. Residential floor space stays fixed at $L_i^{R,0}$, and commercial floor space responds to commercial rent as in equation (13), with $\sigma = 5$.

Fundamentals. For each candidate public elasticity λ, I remove the baseline density components.

$$a_j(\lambda) = \frac{A_j^{P,0}}{(\Upsilon_j^{P,0})^{\lambda_{PP}}(\Upsilon_j^{G,0})^{\lambda}}, \qquad b_i = \frac{B_i^0}{(\Omega_i^0)^\eta}. \tag{31}$$

Every candidate therefore rationalizes the same baseline productivity. Fundamentals remain fixed within each comparison of the baseline and relocation equilibria. For computation, kernel weights beyond 120 minutes (productivity) and 60 minutes (amenities) are set to zero, and the fundamentals are inverted with the truncated kernels, so the baseline is reproduced exactly.

Baseline fiscal identity. Summing income across residences gives $\sum_i I_i = W^P + W^G$. Combining disposable income with the public budget gives $\sum_i v_i = W^P - C^G$. Every job earns the local wage, and the office rule implies

$$C^{G,0} = \gamma_G \frac{1-\alpha}{\alpha} W^{G,0}, \qquad t^0 = \left(1 + \gamma_G \frac{1-\alpha}{\alpha}\right) \frac{W^{G,0}}{W^{P,0} + W^{G,0}}. \tag{32}$$

With $\gamma_G = 0.256$ and $\alpha = 0.80$, the baseline office bill equals 6.40% of public payroll. The national wage bill is 3,084.2 billion euros. Public payroll is 159.3 billion euros, or 5.166% of the national wage bill, and the public office bill is 10.19 billion euros, or 0.330%. The baseline tax rate is $t^0 = 5.4967\%$. Public jobs are 4.913% of all jobs.

Counterfactual accounting. Every public job earns the wage of its workplace, so the public wage bill at workplace j after the relocation is $w_j \bar{H}^G_{Mj}$ at the counterfactual wage. New state offices completed by 2024 add their office requirement to both public occupancy and the commercial floor space of the destination. Offices accommodated in the existing stock use commercial floor space that would otherwise be available to private firms. Welfare follows from the expected-utility formula.

$$\Delta \log \bar{U} = \epsilon^{-1} \log\left(\Phi^{\mathrm{cf}} / \Phi^0\right).$$

The tax factors inside Φ incorporate the change in disposable income.

Solution. For each candidate elasticity, I invert the baseline, solve a zero-shock equilibrium, solve the relocation counterfactual, and apply the triple difference of Section 9 to the model outcomes. The solver iterates on wages, rents, residents, and jobs with a damping factor of 0.2, and residents are damped like wages and rents. Wages clear each local labor market with Newton steps. The solver stops when the largest absolute log change is below 10^{-10}. The zero-shock solution reproduces the baseline with a largest absolute log gap of 2×10^{-13}.

Model fit. The inversion matches resident employment, total workplace employment, wages, and residential rents exactly. Section 9 compares the model with the triple-difference outcomes for the receiving towns. In the policy state, 16.2% of residents work in their own municipality in the model, against 46.7% in the data. The data share is workplace employment in 2014 minus in-commuters, divided by residents, for 2,045 municipalities. Across these municipalities, the model and data shares correlate at 0.73. Model commuting flows correlate at 0.412 in logs with the Federal Employment Agency flows on 34,075 municipality pairs.