The Wage Effects of Local Public Goods[*]

Kentaro Asai[†] Bernhard Schmidpeter[‡]

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Abstract

We analyze the impact of local public goods on workers' earnings by exploiting quasi-random transfer shocks generated by revisions to official population counts under Austria's fiscal equalization scheme. Positive transfer shocks increase municipal spending, leading to an expansion of local public goods, such as below-school-age childcare. Workers in more exposed municipalities experience persistent declines in earnings. These declines occur both within the same employer and within the same municipality, consistent with workers accepting lower pay in exchange for improved local amenities. Heterogeneity by gender and parental status suggests that childcare is an important regional amenity for women: earnings decline within the original region, especially among mothers with kindergarten-age children. In contrast, earnings losses among men are concentrated within the original employer, pointing to a firm- or workplace-specific mechanism rather than the effects of region-wide public goods. We find no evidence that changes in labor supply account for these effects. Overall, our findings provide evidence that local public goods can lower wages through compensating differentials even in the absence of worker reallocation.

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[†]Vienna University of Economics and Business. Email: kentaro.asai@wu.ac.at

[‡]Vienna University of Economics and Business, and IZA Institute of Labor Economics. Email: bernhard.schmidpeter@wu.ac.at

1 Introduction

Local governments, such as municipalities, are important providers of public goods and services. They operate childcare facilities, maintain local infrastructure, provide social services, invest in health and education, and support local economic activity. Accordingly, the impacts of local government spending and the underlying decision-making process have been the subject of extensive debate (e.g. Knight, 2002; Suárez Serrato and Wingender, 2016b; Agrawal et al., 2024; Helm and Stuhler, 2024a). Yet surprisingly little is known about how the provision of local public goods affects individual workers. If local spending raises labor demand or worker productivity, workers may benefit through higher earnings. If it instead improves the amenities associated with living or working in a municipality, workers may be willing to accept lower wages in exchange for those amenities. In that case, part of the surplus generated by public goods may accrue not to workers, but to firms through lower wage growth or to landowners through capitalization. Distinguishing between these mechanisms requires following workers across employers and locations and separating earnings effects due to amenity capitalization from those driven by conventional labor-supply responses.

In this paper, we provide new empirical evidence on how exogenous increases in municipal fiscal capacity affect the labor-market outcomes of incumbent workers. We ask whether and how additional municipal revenues affect workers' earnings, for example through increased local attractiveness and labor supply, or through compensating wage differentials. We exploit a 2008 reform of Austria's municipal fiscal equalization system that generated municipality-level changes in transfers from the federal government. The reform replaced the decennial-census population counts used to allocate transfers with register-based counts produced by the national statistical office. This change revised the population measures relevant for fiscal transfers. Following Suárez Serrato and Wingender (2016b); Helm and Stuhler (2024a), we exploit the discrepancy between the adjusted 2008 population count and the most recent pre-reform count as plausibly exogenous variation. The discrepancy reflects corrections to the population measure used for fiscal allocation rather than ordinary population growth. The resulting shock varies substantially within federal states and regions, and is not systematically correlated with pre-reform trends in employment, labor force participation, unemployment, or wages.

We first show that a one-percentage-point increase in the population-count shock raised fiscal transfers by approximately one percent from 2009 onward. On average, municipalities subsequently increased spending in several functional categories, including education, social welfare and housing, health, and economic promotion. As a concrete measure of local amenities, we also observe that the shock increased the number of pre-school childcare facilities, such a nurseries and kindergarten. These results combined show that the population-count shock increased local fiscal capacity and led municipalities to expand public services.

We then estimate the effect of the shock on workers using administrative employer-employee linked data and a sample of prime-age private-sector workers who were attached to the labor market before the reform. We find that workers in municipalities receiving larger fiscal shocks experienced *lower* cumulative earnings in the six years following the shock. The effect is absent before the reform and emerges gradually as municipal spending and public-good provision in-

crease. To put the magnitude into perspective, moving from the 25th to the 75th percentile of the shock distribution reduces cumulative earnings over the following six years by 1.64 percent of pre-reform annual earnings. While the magnitude is modest relative to shocks that directly reduce labor demand, such as plant closures and trade shocks (e.g. Couch and Placzek, 2010; Autor et al., 2014), we consider it economically meaningful given that the fiscal shock operates indirectly through local public goods. The sign of the earnings effect is opposite to that predicted by a standard local multiplier, under which higher municipal transfers stimulate local economic activity and raise workers' earnings (Nakamura and Steinsson, 2014; Corbi et al., 2019).

Decomposition of the earnings effects points toward a compensating wage differential mechanism. We find that the earnings losses are driven at the same employer or at the same municipality, while changing the workplace to other municipalities partially offset these losses. This is consistent with the idea that public-good improvements change the surplus available in local employment relationships. Firms in treated municipalities can retain workers at lower earnings growth when those workers value the amenities attached to the local labor market, while workers with lower valuations can partially offset the loss by working elsewhere.

Heterogeneity by gender and family structure provides further evidence. Average earnings losses are similar for men and women, but the underlying channels differ: men's earnings fall primarily at the same employer, while women's fall primarily at the same municipality. This pattern points to municipality-wide amenities, such as childcare, whose value is tied to local availability rather than to a specific employer.[1] Consistent with this, women's earnings losses are concentrated among mothers with kindergarten-age children. Municipal childcare may thus tie mothers' earnings more closely to the local availability of care, allowing employers to retain them at lower wage growth. However, we highlight that this need not imply lower welfare overall, since public goods can raise utility through non-pecuniary amenities even as earnings fall.

In the classic Rosen-Roback framework, more attractive locations attract more workers, who can be compensated with better local amenities rather than higher pay (Rosen, 1979; Roback, 1982). In our setting, however, we find no significant effects on the total number of workers, the unemployment rate, or the female employment share in a municipality. The absence of these responses suggests that the wage decline we document is not driven by worker reallocation, indicating that compensating differentials can arise even without spatial sorting.

The paper contributes to the literature on spatial equilibrium, local amenities, and compensating wage differentials. A central insight of this literature is that wages, housing costs, and amenities jointly determine spatial equilibrium (Rosen, 1979; Roback, 1982; Gyourko and Tracy, 1991). Although public goods are important components of the local amenities that individuals value (Albouy et al., 2020; Albouy and Farahani, 2017), direct quasi-experimental evidence on how local public-good provision affects workers' earnings remains scarce.[2] We

[1] We provide evidence that municipalities with larger shocks expanded public transportation, a public good that tends to benefit specific firms.

[2] Existing quasi-experimental work generally try to quantify the value of local amenities (rather than directly studying wages and earnings) and focus on specific dimensions of amenities, such as pollution and air quality (e.g. Bayer et al., 2009; Gao et al., 2023; Khanna et al., 2025), public safety and urban parks (Albouy et al., 2020), urban trans-

provide such evidence by linking an exogenous municipal revenue shock to both public-good provision and worker earnings. The negative earnings response is consistent with the prediction that consumption amenities can be capitalized into lower wages when they increase the value of local employment. At the same time, our worker-level design shows that this capitalization can occur even without large short-run changes in aggregate labor supply, complementing work showing that the incidence of place-based shocks depends on mobility costs, commuting possibilities, and other frictions that shape spatial equilibrium (Diamond, 2016; Monte et al., 2018; Allen and Arkolakis, 2023; Kahn and Tracy, 2024). Moreover, we document substantial heterogeneity in earnings responses across workers, potentially reflecting differences in preferences for local amenities that are not captured by the standard Rosen–Roback framework.

Second, it contributes to the literature on intergovernmental transfers and local public finance, with a classical question on whether grants crowd out local fiscal effort or instead "stick where they hit" (Knight, 2002; Cascio et al., 2013). More recent work uses quasi-experimental variation in grants and transfers to estimate local spending multipliers (Nakamura and Steinsson, 2014; Suárez Serrato and Wingender, 2016a; Corbi et al., 2019; Helm and Stuhler, 2024b). Much of this literature focuses on government budgets, employment, or aggregate income, and is silent on how these aggregate effects are distributed across individual workers. We complement this literature by following individual workers before and after a fiscal shock, showing that the same transfers that expand local spending and public-good provision can simultaneously reduce the earnings of incumbent workers

Third, the paper contributes to research on the labor-market effects of family policies and childcare. Existing work has examined how childcare affects maternal labor supply (e.g. Baker et al., 2008; Bauernschuster and Schlotter, 2015), child outcomes (e.g. Havnes and Mogstad, 2011), and gender inequality (e.g. Kleven et al., 2019). We study a different margin: who captures the surplus generated by local childcare provision. The particularly strong negative earnings effects for mothers of kindergarten- and primary-school-aged children indicate that firms can partly capture the surplus generated for mothers; more generally, it shows that public goods can have heterogeneous wage incidence depending on workers' household circumstances and likely amenity valuations.

The findings also have broader implications for the evaluation of place-based policies and public spending (Busso et al., 2013; Kline and Moretti, 2014; Neumark and Simpson, 2015). Local public goods may make municipalities better places to live and work, but the distribution of those gains depends on labor-market and spatial-equilibrium forces. If public spending raises productivity, workers may benefit through higher wages. If it raises the value of local amenities, workers may benefit in utility terms while receiving lower nominal earnings. Our results suggest that at least part of the return to place-based spending can be translated into lower wage growth rather than distributed as broad-based earnings gains, implying that evaluations focused on aggregate income or employment effects may miss this cost to incumbent workers.

portation infrastructure (e.g. Tyndall, 2021; Severen, 2023), city shape (Harari, 2020), cultural amenities (Falck et al., 2011) and changes in local public administration (Becker et al., 2021; Bøje-Kovács et al., 2026).

The rest of the paper proceeds as follows. Section 2 describes Austria's fiscal equalization system and the 2008 reform. Section 3 introduces the municipality- and worker-level data. Section 4 presents the empirical strategy. Section 5 documents the effect of the shock on municipal transfers, spending, public employment, and childcare provision. Section 6 estimates the worker-level earnings effects and decomposes them by employer and municipality. Section 7 evaluates alternative mechanisms, including aggregate labor-supply responses.

2 Fiscal Equalization Scheme and Census Shock

2.1 The Austrian Financial Equalization Pact

Austrian municipalities have access to different revenue sources. For example, they levy a payroll tax (*Kommunalsteuer*) of 3% on gross wages paid by local firms and collect fees on local services, such as water, waste, and building permits.

One of the largest revenue streams for municipalities comes through the fiscal equalization scheme (Finanzausgleich, FAG). The FAG governs which level of government, federal, state, or municipal, bears the cost of providing public services, and which level has the right to levy what type of taxes. It also governs how revenues collected by the federal government, for example through federal taxes, are redistributed between the federal government, federal states, and municipalities. The goal is to provide a fair re-distribution of income to ensure that provided public services, such as education, health, social welfare, and childcare are sufficiently funded. Some services are financed simultaneously by two or more entities. For example, roads can either be maintained by the federal government (e.g. highways), federal states (e.g. Bundesstrassen), or municipalities.

The redistribution follows a two-step approach. First, tax revenues are allocated among the federal government, the federal states, and the municipalities (*vertikaler FAG*). In 2023, 54% of overall tax revenues remained with the federal government, while 30% was allocated to the federal states and the remaining 16% to the municipalities.[3] Afterward, the allocated revenues are redistributed horizontally among the different federal states and municipalities (*horizontaler FAG*). One important determinant of the transfer amount a municipality receives is its population size, where larger municipalities receive higher transfers.[4]

2.2 2008 Transfer Reform

The structure of the FAG is frequently re-negotiated between the different levels of government, typically every four to six years. In this paper, we exploit a 2008 reform of the FAG that changed how municipality population is measured. Prior to the reform, municipality population figures were determined using decennial census counts, with the most recent pre-reform census conducted in 2001. Consequently, regardless of whether a municipality's population

[3] See here for an overview over the FAG. Note that the split can change everytime the FAG is re-negotiated.

[4] To obtain the exact revenue stream for each municipality, the population size is multiplied by a population-weighted gradient (*abgestufte Bevölkerungsschlüssel*), to account for the fact that larger municipalities also face larger costs of providing public services. The municipalities fall into four different gradient categories (up to 10,000, between 10,001 and 20,000, 20,001 to 50,000, and above 50,000 inhabitants) and the exact size of the multipliers changes during renegotiation. We ignore the multipliers in our work, noting that we implicitly control for municipality fixed effects in our regressions and very few municipalities changed their gradient threshold during our observation period.

was actually shrinking or expanding, its revenues remain proportional to its last decennial population count.

With the reform coming into effect on January 1, 2008, the determination of municipality population was shifted from decennial census counts to yearly register-based counts provided by Statistik Austria on October 31 of each year. These counts are based on a cleaned version of the central population register (Zentrales Melderegister, ZMR), from which Statistik Austria removes outdated and erroneous entries and supplements it with registrations confirmed through a cross-validation procedure. For example, each entry is cross-checked against other administrative databases, including the tax register, school registration statistics, and the family allowance register, and only individuals appearing in at least one of these sources are counted as confirmed residents (Statistik Austria, 2009).

The register-based counts used as the basis for transfer calculations typically lag their legal implementation by two years; for example, transfers in 2025 were based on the population counts from 2023. However, during the first two years of the reform (2009 and 2010), the counts published in October 2008 were used (Städtebund, 2008). This institutional setting allows us to use the switch from census-based to register-based population counts in 2008 and the associated projection error as a shock to municipality finances, similar to Suárez Serrato and Wingender (2016b) and Helm and Stuhler (2024a).

An important additional feature of the FAG 2008 reform was that the corresponding revenue gains for municipalities were tied to municipalities and states taking on additional responsibilities in child and after-school care (WuG, 2007). These are two public goods we can directly measure in our municipality data; see Section 5.[5]

2.3 The Census Shock

While the announcement of the reform in October 2007 and the publication of the adjusted register-based counts in October 2008 by Statistik Austria left very little room for anticipation, municipalities may have tracked their population sizes through the ZMR to infer any potential changes to their budgets. Therefore, instead of directly using the original change in population between the 2001 census and the 2008 register-based counts as the shock, we use the population measured at the beginning of 2007, the year preceding the policy change. We then define the census shock for municipality m as

$$\text{shock}_m = log(Pop_{m,2008}^{StatAustria}) - log(Pop_{m,2007}^{ZMR}) \tag{1}$$

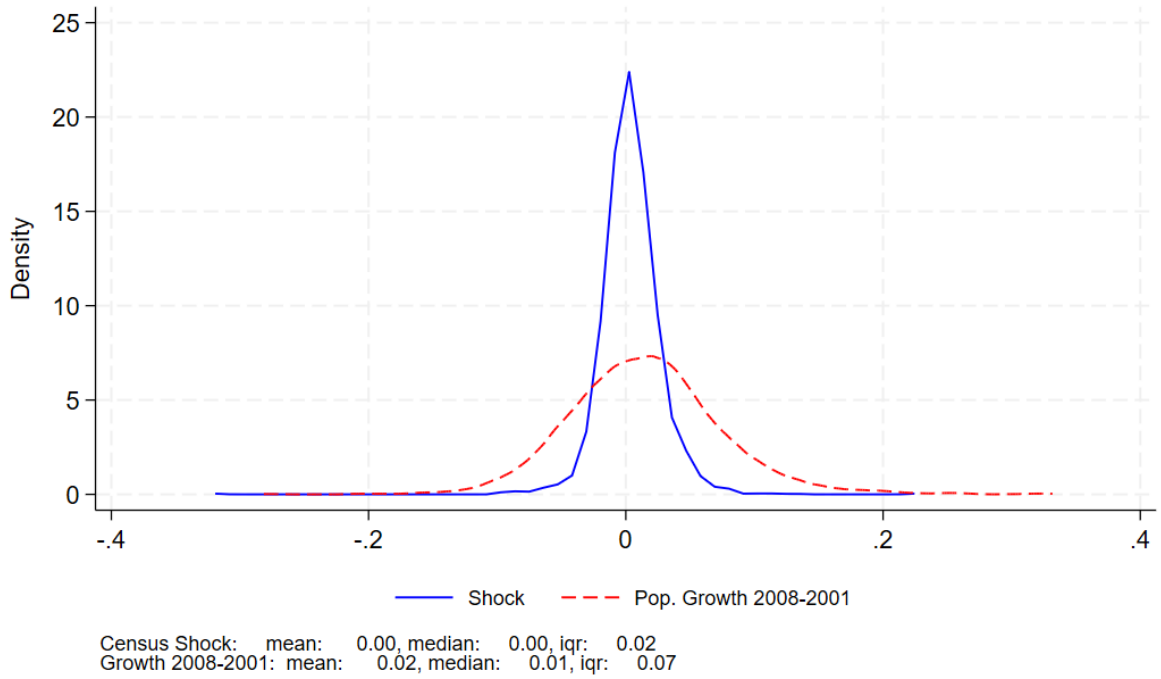
where $Pop_{m,2008}^{StatAustria}$ is the adjusted population size provided by Statistik Austria in October 2008 and $Pop_{m,2007}^{ZMR}$ is the most recent population in the ZMR. This definition reflects the forecast of a municipal decision-maker, based on the fact that municipal populations are persistent at an annual frequency: large swings in population size over a single year are rare for the vast majority of municipalities, making the most recently observed count a natural benchmark for the population municipalities would expect in the absence the reform. Any deviation from

[5] In addition, the agreement required states and municipalities to take on additional responsibilities in elderly care as well. However, our data do not allow us to investigate this channel.

this forecast therefore reflects a correction to the population count rather than genuine demographic change, as we show below.

Figure 1 shows the distribution of the census shock from Equation (1), together with the original population growth rate between 2001 and 2008. Our census shock is tightly distributed around zero. The 25th percentile is -0.009 and the 75th percentile is 0.0155. This distribution suggests that our shock mimics corrections in population counts rather than pure population growth. More importantly, as we show in the next section, the census shock is uncorrelated with local economic trends. In contrast, the distribution of the original shock (i.e., the discrepancy between the 2001 census population and the Statistik Austria population count in October 2008) is right skewed and exhibits substantially larger variation. Municipalities at the 25th percentile experienced a shrinking population of -0.023 and municipalities at the 75th percentile grew by 0.0481. This suggests that it reflects a combination of actual population growth and deviations in population counts.

Figure 1: Distribution of Census Shock

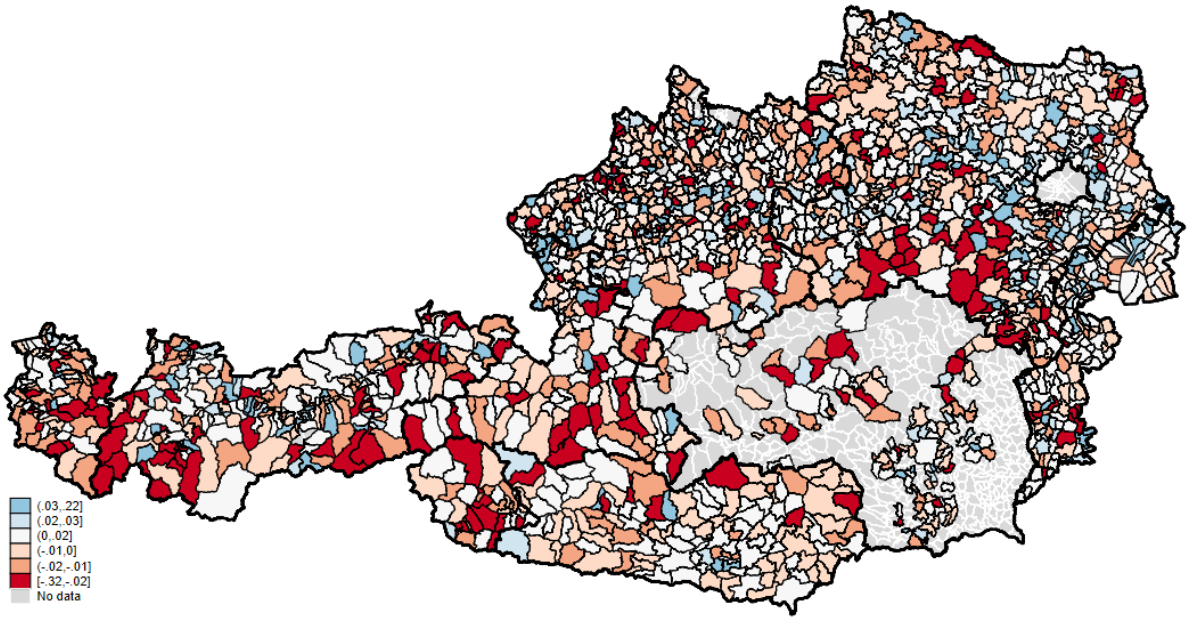


Notes: The figure plots kernel density functions of the Census Shock, defined as the log deviation between the Statistik Austria population counts and the 2007 ZMR population (see Equation (1)) alongside the original population growth rates for 2001–2008.

Figure 2 shows the regional distribution of the shock. Darker blue (red) colors indicate positive (negative) shocks.[6] The shock varies substantially within federal state (thick lines) and across municipalities.

[6] We exclude Vienna as Statistik Austria population counts are not available for Viennese districts and therefore the effect cannot be identified in our empirical model with state fixed effects. Many municipalities in Styria either did not report population counts or reported them only partially.

Figure 2: Geographic Distribution of Census Shock



Notes: The figure shows the geographic distribution of the census shock by municipality, defined as the log deviation between the Statistik Austria population counts and the ZMR count in 2007 (see Equation (1)). Darker blue (red) colors indicate above (below) zero growth. Data are not available for gray shaded municipalities.

One might worry that our census shock is related to a region-wide factors that could also explain workers' labor market outcomes. We investigate this possibility by examining the extent to which the census shock varies across nearby municipalities within the same region (Bezirk) or federal state.

We find that the census shock varies substantially within geographic regions. Regressing the census shock on region (Bezirke) fixed effects and federal-state fixed effects respectively, we find that state indicators explain only 2%, while the finer Bezirke indicator explain only 16%.[7] These results suggest that the census shock captures idiosyncratic factors that do not reflect the economic conditions of a local labor market as a whole.

2.4 Balancing Tests

We also investigate how the census shock is related to local economic and demographic conditions before the changes to the FAG came into effect in 2008. Figure 3 reports pre-trend estimates. The left panel reports correlation coefficients between the census shock and long-term growth rates from 2003 to 2007. The right panel reports the corresponding correlations for short-term growth rates from 2006 to 2007, immediately before the policy change.

Our balancing tests do not suggest significant pre-trends in changes in the log size of the local labor force. Nor do they suggest that our census shock is associated with differential changes in the participation rates of men and women in either the long- or short-run esti-

[7] The explanatory power of Bezirke is similar to that of commuting zone and districts indicators in Helm and Stuhler (2024a) and slightly higher than that of the MSA effects in Suárez Serrato and Wingender (2016b). Note however that Bezirke in Austria are measured at a very fine geographic level in a relatively small country.

mates; see the top of the figure. We also find no evidence that employment is correlated with our shock. These results provide further evidence that our census shock indeed reflects idiosyncratic deviations between the ZMR and Statistik Austria counts rather than general population growth.

The lower half of the figure shows the correlation coefficients between the census shock and economic conditions in the municipalities. We find no strong evidence that our census shock is correlated with changes in the average wage level in a municipality, either overall or when wages are analyzed separately by gender. Although some of the long-term correlations are marginally significant, their magnitudes are small. We find no evidence of imbalance in the short term. Furthermore, the census shock is not correlated with the municipal unemployment rate. This suggests that municipalities with relatively stronger labor markets did not experience systematically different shocks.

3 Data and Descriptives

3.1 Municipality-level Data

Our key municipality-level data come from Statistics Austria, Austria's federal statistical office.[8] For annual population measures, we use register-based population counts from the first quarter of each calendar year, as well as the population count as of October 31, 2008. We also use population counts from the 2001 Population Census.

Regarding fiscal data, Statistics Austria publishes information on both revenues and expenditures at the municipality level.[9] The fiscal data used in this paper are based on the accounting and budgeting rules established in 1997, the *Voranschlags- und Rechnungsabschlussverordnung 1997* (VRV 1997), which applied to municipalities and federal states during our sample period. The data contain information on different types of municipal revenues, including fiscal-equalization revenues from shared federal taxes, which are allocated largely according to population-based formulas and account for roughly 40–45% of current municipal revenues.

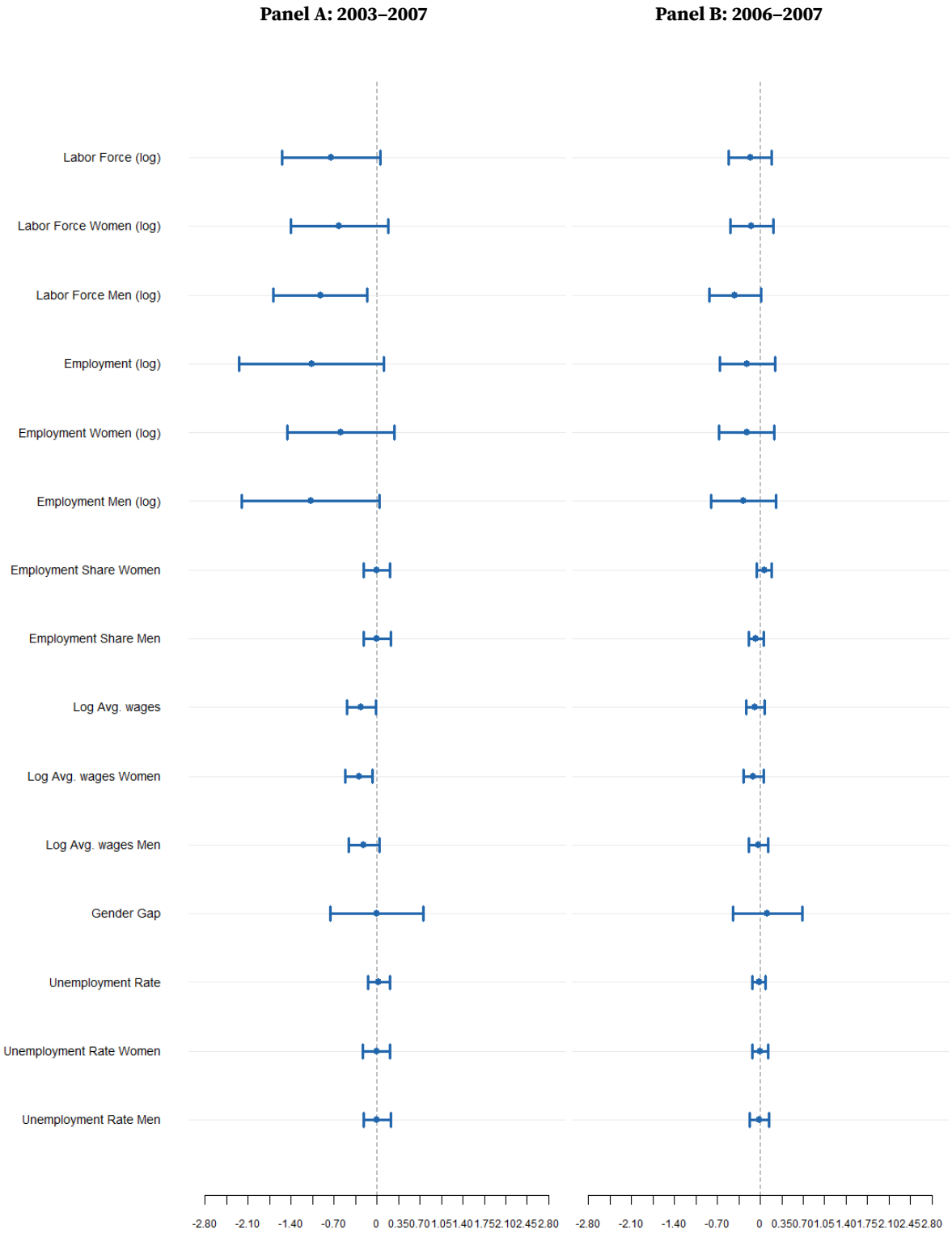
We also use expenditures by functional category, reported at the most aggregated level, to capture local public services and amenities provided by municipalities. Although the functional classification of expenditures was governed by the same accounting rule (VRV 1997), accounting practices and the assignment of expenditures to functions were not fully harmonized across municipalities and federal states during our study period. We therefore interpret results based on these data with caution. Fiscal years correspond to calendar years in Austria.

In addition, we use the number of childcare facilities in each municipality as a concrete measure of local amenity and service provision. Similarly, we also use information on local public employment, including the number of municipal public servants and other municipal staff employed in each municipality.

In our sample, we keep municipalities other than Vienna that have no missing information during 2003 and 2013 and can be consistently matched to municipalities recorded in the Census. As a result, the final sample consists of a balanced panel of 1,831 municipalities.

[8] Statistik Austria: https://www.statistik.at/datenbanken/statcube-statistische-datenbank
[9] More details on the data are provided in Appendix Section A.

Figure 3: Census Shock and Pre-trends



Notes: The figure shows correlation coefficients between the census shock and pre-treatment growth rates in municipal demographic and economic outcomes and controlling for state fixed effects. Panel A reports correlations with growth rates over 2003–2007. Panel B reports correlations with growth rates over 2006–2007. Horizontal bars indicate 95% confidence intervals based on standard errors clustered at the municipality level.

3.2 Worker-level Data

Linked employer-employee data: Our worker-level analysis is based on the Austrian Social Security Data Base (ASSD). The data consist of administrative records used to verify pension claims and are structured as a matched employer-employee dataset. For each private-sector employee, we observe daily information on employment, unemployment, and parental leave taking. We also observe socioeconomic characteristics, such as age, education, broad occupation, experience, tenure, and earnings, with earnings reported annually and separately for each employer. The data allow us to construct firm-level variables, such as firm size and pay (Zweimüller et al., 2009).[10]

We use the municipality identifier available in the firm data to assign the census shock to a worker. This means that we measure exposure at the workplace rather than the place of residence. Due to a change in the reporting practices, the municipality identifier is only observed for firms only until 2013. We therefore restrict our sample to the period ending in 2013.

Sample: We focus our analysis on prime-age workers born between 1952 and 1984, who were therefore between 24 and 56 years old at the time of the census shock. We choose the lower age bound of 24 to capture individuals who have likely finished their education. We set the upper bound at 56 to avoid including older workers who may transition into early retirement in response to the shock.

We concentrate on a sample of workers with high labor force attached to the labor market similar, following, for example, Autor et al. (2014). We exclude all individuals who were not employed full-time during the three years preceding the census shock in 2008. While we do not observe a full-time indicator in the ASSD, we proxy for it by excluding all workers who earned less than $14,000$ Euro/ per year at baseline. This lower bound approximately corresponds to the (deflated) lowest full-time collective bargaining wage in Nimczik et al. (2024). In addition, we impose the restriction that the worker have been employed at least 360 days. Finally, we exclude workers employed in agriculture, quarrying and mining, temporary help agencies, private households, and extraterritorial organization at baseline, as these sectors are either very small (agriculture, quarrying and mining) or feature employment relationships that differ fundamentally from the private-sector norm (temporary help agencies, private households, and extraterritorial companies).

We consider the employing firm in 2007 as the main (baseline) firm and use the corresponding municipality as a worker's baseline local labor market. All worker-level baseline variables, such as earnings, are calculated over the three years spanning 2005–2007. Firm characteristics are measured at baseline. Table 1 shows characteristics of our estimation samples.

[10] A limitations of the data is the lack of information on contracted or actual working hours.

Table 1: Worker Sample Characteristics

	All	Men (60%)	Women (40%)
Census Shock			
$Shock_m$	0.007	0.007	0.007
	(0.014)	(0.014)	(0.014)
P75,P25 interval	[0.015,0.001]	[0.015,-0.000]	[0.015,0.002]
P90,P10 interval	[0.021,-0.008]	[0.022,-0.009]	[0.021,-0.006]
Worker Characteristics			
Avg. yearly earnings (euro)	34,661.31	38,756.18	28,554.03
	(11,993.830)	(11,246.015)	(10,366.519)
Total tenure in main firm (days)	3,009	3,006	3,013
	(2,325)	(2,342)	(2,300)
Firm Characteristics			
Log firm size	4.857	4.826	4.904
	(2.254)	(2.096)	(2.470)
Log daily firm wage	4.444	4.515	4.338
	(0.282)	(0.248)	(0.296)
Service	0.433	0.328	0.588
Construction	0.060	0.085	0.023
Manufacturing	0.304	0.397	0.165
Others	0.203	0.189	0.224
State			
Burgenland	0.035	0.032	0.039
Carinthia	0.084	0.080	0.090
Lower Austria	0.235	0.233	0.236
Upper Austria	0.276	0.288	0.259
Salzburg	0.101	0.098	0.105
Styria	0.101	0.094	0.113
Tyrol	0.110	0.112	0.107
Voralberg	0.058	0.062	0.051
Individuals	908,998	544,150	364,848

Notes: The table summarizes the characteristics of the estimation samples in 2007 (baseline). The overall sample includes all workers born between 1952 and 1984 who were full-time employed in all the three years preceding the census shock in 2008. All estimates are means; where applicable, standard errors are in parentheses below.

Outcomes: Our first outcome of interest is cumulative earnings, which captures medium-run effects of the census shock on workers' careers. Let Earn_{it} represents the annual earnings of individual i in year t. We define:

$$E_{it} \equiv \begin{cases} \frac{\sum_{r=2008}^{t} \text{Earn}_{ir}}{\frac{1}{3}\sum_{r=2005}^{2007} \text{Earn}_{ir}} & 2008 \leq t \leq 2013 \\ \frac{\text{Earn}_{ir}}{\frac{1}{3}\sum_{r=2005}^{2007} \text{Earn}_{ir}} & 2003 \leq t \leq 2007 \end{cases}.$$

We normalize post-shock cumulative earnings by average earnings during the pre-shock period, following Autor et al. (2014). This standardization allows us to interpret the effect of the census shock on the evolution of earnings as a percentage of average annual pre-shock earnings. It also has the advantage of being well-defined when a worker's earnings in a given period are zero, thereby capturing both the extensive and intensive margins.[11]

For duration outcomes, such as days employed and days unemployed, we use an analogous definition with a common denominator based on pre-shock employment:

$$D_{it} \equiv \begin{cases} \frac{\sum_{r=2008}^{t} \text{Duration}_{ir}}{\frac{1}{3}\sum_{r=2005}^{2007} \text{Employed Days}_{ir}} & 2008 \leq t \leq 2013 \\ \frac{\text{Duration}_{ir}}{\frac{1}{3}\sum_{r=2005}^{2007} \text{Employed Days}_{ir}} & 2003 \leq t \leq 2007 \end{cases}.$$

where Duration_{it} denotes the number of days that individual i spends in a particular labor market status in year t. Note that, for workers with strong labor force attachment, the denominator corresponds approximately to a full working year.

4 Estimation Strategy

Our estimation strategy is a difference-in-differences, comparing the evolution of outcomes across municipalities with different fiscal revenue shocks before and after the implementation of the change in the fiscal equalization scheme.

Municipality level: For the municipality level analysis, we estimate the following equation:

$$y_{mt} = \sum_{t \neq 2007} \gamma_t \left(Shock_m \times \mathbf{1}[t = k] \right) + X_{mt}\Gamma + \mu_m + \sigma_{st} + \varepsilon_{imt} \tag{2}$$

where y_{mt} denotes the outcome for municipality m in year t, $Shock_m$ is the population gap shock for municipality m, $\mathbf{1}[t = k]$ are year dummies with $t = 2007$ as the omitted reference period[12], X_{mt} are a vector of municipality-level time-varying controls, μ_m are municipality fixed effects, σ_{st} are state-by-year fixed effects, and ε_{mt} is the error term. Unless stated otherwise, outcomes are expressed in logs.

Our coefficients of interest are γ_k, the difference-in-differences estimators capturing the effects of the sudden change in fiscal revenue on the outcome. μ_m controls for time-invariant

[11] We do not use cumulative outcomes in the nominator in the pre-shock period as such an approach leads mechanically to a constant in the last pre-shock year.

[12] We normalize the event-study coefficients to 2007 because the reform was approved by Parliament in December 2007. Although the reform affected fiscal transfers for the first time for the 2009 budget year (based on the population as of October 31, 2008), using 2007 as the reference year allows us to capture any early adjustments in 2008 due to anticipation.

municipality characteristics, while σ_{st} absorbs all time-varying state-level variation, so that the effect of the population shock is estimated from within-region, across-municipality comparisons. Regarding the control variables, we include the log population growth of each municipality from 2001 (the last census before the reform) to 2007 interacted with year dummies to control for the influence of underlying population trends. We cluster standard errors at the municipality level. In the baseline specification, we weight municipalities equally so that our estimates capture effects for a typical municipality and are less sensitive to the large dispersion in municipality sizes. The results are, however, qualitatively similar when we weight observations by the log number of pre-reform residents or workers.

Worker level: We estimate the following event study specification for a worker i who was employed at baseline in municipality m

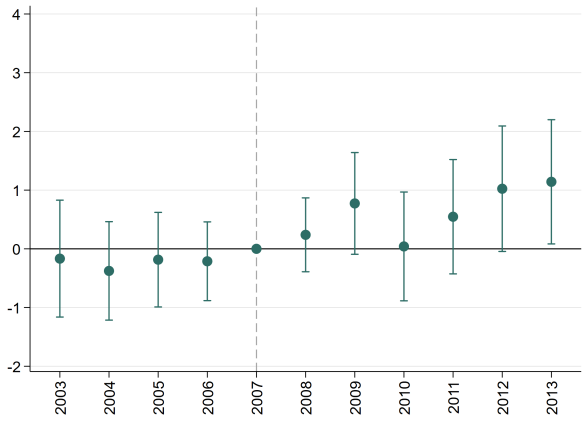
$$y_{imt} = \sum_{\tau \neq 2007} \beta_\tau \left(shock_m \times \mathbf{1}[t = \tau] \right) + \Theta X_{imt} + \alpha_i + \sigma_{st} + \eta_{imt} \tag{3}$$

where X_{imt} is a vector that includes baseline worker, firm, and regional characteristics interacted with time fixed effects, to allow their effects to vary flexibly over time. On the worker side, these characteristics include baseline wage and tenure. On the firm side, these characteristics include firm size and average wage. At the municipality level, these characteristics include 2001–2007 log changes in the ZMR population as well as baseline unemployment rate year interactions. The pre-reform population trends approximate any long-term municipality spending plans. The local unemployment rate captures labor market tightness which can directly affect workers' labor market outcomes. α_i and μ_m are worker and municipality fixed effects. σ_{st} are state$\times$ year fixed effects capturing state specific shocks, such as investment cycles. We cluster standard errors at the municipality level.

5 Impact of Census Shock on Fiscal Transfers and Expenditures

Fiscal transfer. We first examine whether the shock to population counts affected municipal fiscal capacity. Figure 4 presents estimates from Equation 2, showing the effect of the population shock on the log of fiscal transfers received by municipalities under the fiscal equalization scheme. The coefficients become positive beginning in fiscal year 2009, when the new population counts were incorporated into the municipal fiscal distribution formula.[13] The estimates imply that a one-percentage-point increase in population count is associated with an approximately 1% increase in fiscal transfers received by the municipality.[14] Importantly, the coefficients prior to 2009 are close to zero, indicating that our population shock variable predicts fiscal growth only from 2009 onward, and not before.

Figure 4: First Stage Effects on Fiscal Transfer



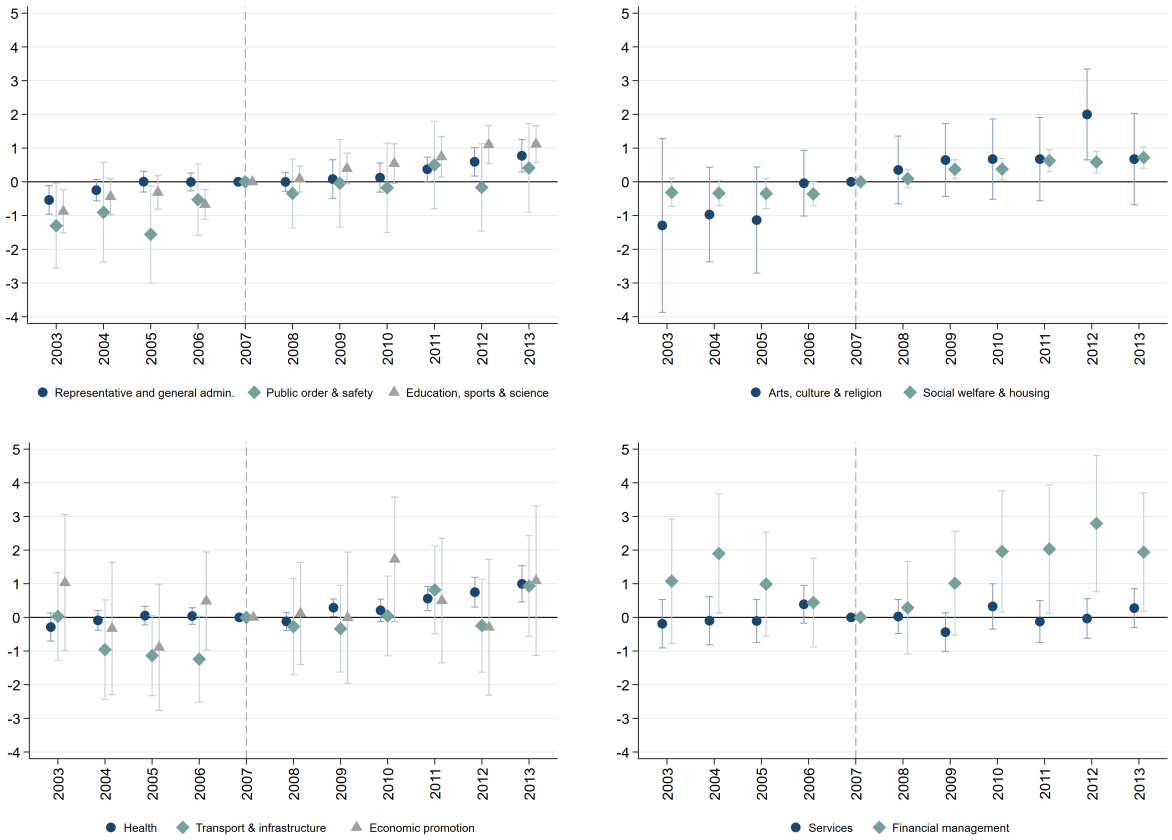
Notes: The figure shows the estimated coefficients $\hat{\gamma}_t$ from Equation (2), with 95% confidence intervals. Standard errors are clustered at the municipality level. The outcome is the log of fiscal transfer received by municipality through fiscal equalization.

Expenditure by functional category. Figure 5 displays the effects of the population shock on expenditure by functional category.[15] Although the effects are clearer for some categories than for others, we observe positive post-reform coefficients for expenditures on "Education, sports and science", "Social welfare and housing", "Health", and "Economic promotion".[16] The results are unclear for "Transport and infrastructure" and "Arts, culture and religion", partly because of large pre-reform coefficients. We do not find any significant increase in general services expenditure. Overall, the results show that the increase in the population count affected the provision of amenities and local public goods, at least in certain domains.

[13] For 2010, the coefficient is close to zero. This is likely related to recession-induced temporary changes in fiscal redistribution following the financial crisis.
[14] This amounts to an increase of roughly 200 euros per capita (Appendix Figure D.1).
[15] Appendix Figure B.1 provides the share of each functional category in total expenditure.
[16] We also observe an increase for the "Financial management" category, but this category captures fiscal and treasury-related items rather than municipal service provision. Expenditures in this category may reflect debt service, intergovernmental levies or transfers, reserve and budget-balancing transactions, or other general financial obligations.

Childcare provision. Figure 6 shows the effects of changes in fiscal revenue on the absolute number of different types of childcare facilities.[17] The results show clear effects for some categories. Beginning in 2009, the year in which fiscal revenue changed, the number of nurseries and kindergartens increased significantly. By contrast, the number of after-school facilities and mixed-type childcare facilities, which account for a relatively small share of childcare facilities, did not respond. These results constitute a concrete example of amenity supply that responded to changes in fiscal revenue.

Figure 5: Effects on Expenditure by Function

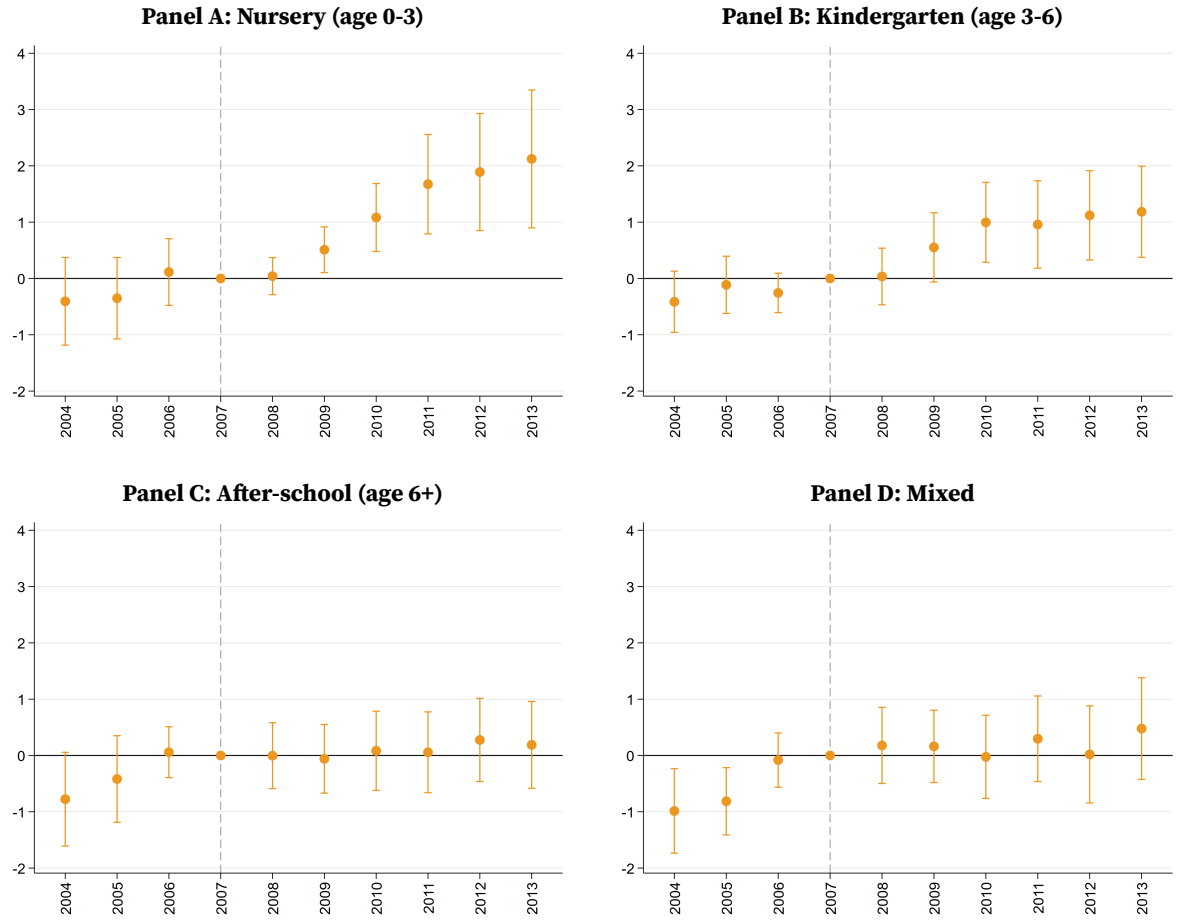


Notes: The figure shows the estimated coefficients on the fiscal spending by municipalities by its function. The categorization of each spending was not harmonized across municipalities during the studied period, and therefore it must be taken as suggestive evidence.

In addition, we show in Appendix Figure D.2 that the additional transfers led to a gradual increase in the number of contract employees and staff, while having no effect on the number of civil servants. Moreover, Appendix **??** shows a positive correlation between fiscal transfers and the future provision of public transportation during the 2023–2025 period.

[17] We do not log-transform these outcomes because many observations are zero.

Figure 6: Effects on Childcare Provision



Notes: The figures show the effects of fiscal revenue on the absolute number of different types of childcare facilities.

6 Worker-level Impact

6.1 Overall Effects

Panel A of Figure 7 shows our event-study estimates of the impact of the census-shocks on workers' cumulative earnings. Two patterns emerge from the figure. First, prior to the announcement of the reform and the census shocks, we do not see any impact on workers' earnings. Our estimates are all very small and not statistically significant at conventional levels.

Second, after the census shock, our estimates indicate a gradual but significant decline in workers' earnings, with no sign of recovery. The slow build-up of the effect is consistent with our estimates for municipality transfers and expenditures: we do not find significant impacts on earnings in the first few years after the census shock when municipalities started to increase expenditure and expand their provision of public goods. Workers' earnings begin to decline only after expenditures and public-good provision have increased significantly.

To interpret the magnitude of the effects, we compare an individual working at the 75th percentile of the change in the population size due to the census shock (1.5 percentage points; see Table 1) with an individual at the 25th percentile (0.1 percentage points). Over the following

six years, the implied differential reduction in cumulative earnings for the worker at the 75th percentile is 1.64% ($-1.17 \times (1.5 - 0.1)$) of initial annual earnings.

While these effects may seem modest, note that our census shock, unlike other shocks such as import competition (Autor et al., 2014), is unlikely to come from a direct labor demand channel. Rather, our findings likely operate through an alternative mechanism, whereby firms indirectly benefit from increased public-goods provision via compensating wage differentials. As municipalities provide more public goods after the shock, firms can lower wages and wage growth while retaining workers who place a high valuation on these public goods and losing those who place a lower valuation. As a consequence, the negative earnings effects should be concentrated in the same-employer and same-municipality components while we should observe a positive effect in the different-region component.[18]

Panel B of Figure 7 shows the results from an additive decomposition of changes in earnings. There are clear negative, albeit noisy, effects on for workers who remain with the same employer (-1.97, blue) and for those who remain in the same municipality (-1.09, red). However, more exposed workers recover part of their earnings through moving municipalities, which is associated with a positive impact on earnings (1.36, green).[19] These patterns suggest that firms in more exposed municipalities profit from the provision of public goods. While our decomposition estimates are noisy in the pooled sample, we show in the next section that men and women face different compensating wage differentials and therefore are affected differently depending on the type of public goods provided.

6.2 Effects by Gender

Figure 8 shows the impact of the census shocks on workers' earnings and their decomposition by gender. Panels A and B present the overall impact for men and women respectively. Interestingly, both genders are affected by the census shocks in a very similar way, both in terms of dynamics and magnitude. Therefore, firms' ability to apply compensating wage differentials when more public goods are provided does not depend on the gender of the workers.

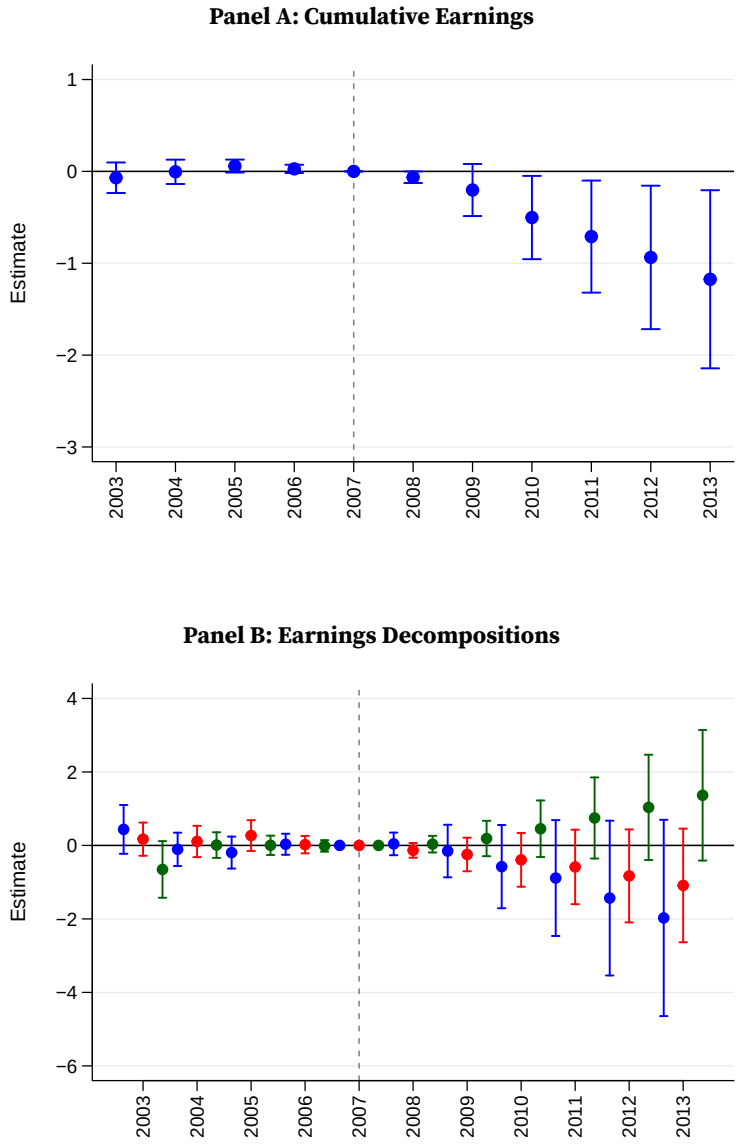
However, the decomposition results in Panels C and D show that the underlying channels are highly gender-specific. More exposed male workers experience a significant and large reduction at the same employer, while the same-municipality component is small and not statistically significant at any conventional level. However, men can recover part of their earnings reduction by moving to different municipalities. In contrast, the earnings loss of women is driven by the same-municipality component while the other two channels are close to zero.

The decomposition results suggest that firms can benefit from region-wide public goods, such as childcare provision, to apply compensating differentials for women. We develop and test this interpretation directly in the next section below. In contrast, the compensating differentials for men are concentrated at the same employer. This suggests compensating differ-

[18] Alternatively, higher public amenities in an area can attract more workers, reducing wages in turn, such as in Rosen (1979) and Roback (1982). In Section 7 we show that in our setting the census shock is not related to higher labor supply.

[19] The remaining 0.52 needed for the components to sum to the baseline effect is due to workers for whom we do not observe a municipality identifier. We omit this group from the figure for expositional purposes, noting that the corresponding effects are smaller in magnitude and mostly not statistically significant.

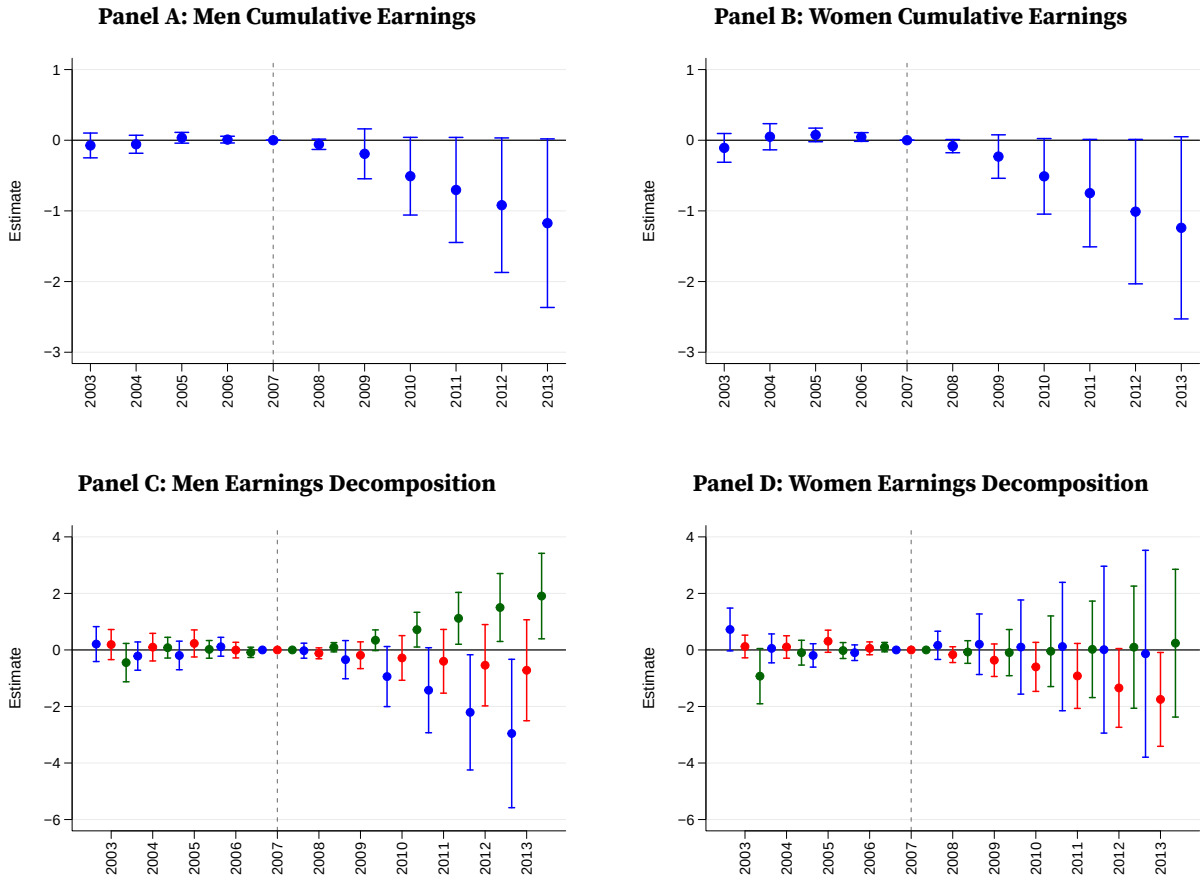
Figure 7: Impact of Census Shock on Cumulative Earnings



Notes: The figure shows the estimated coefficients $\hat{\beta}_t$ from Equation (3), with 95% confidence intervals. Standard errors are clustered on the municipality level m. The outcome variable in Panel A is cumulative yearly earnings, divided by average earnings from 2005 to 2007. Panel B shows the decomposition of the census shock into earnings effects due to the same employer (blue), the same municipality, but not same employer (red), and different region (green). The three components in Panel B sum to the baseline effects together with a residual NA category (workers with a missing municipality identifier), which is omitted from the figure. The sample used includes all workers aged 26 to 56 at the time of the census shock and who were full-time employed in the three years preceding it.

entials for men operate through a firm-specific channel rather than a region-wide one, such as public transport access. While we lack expenditure data that would allow us to identify this specific amenity directly, we show in Appendix C that our census shock is positively correlated with growth in public transportation services over subsequent years, particularly during night hours. Growth at these hours plausibly extends reachability to workers on non-standard shifts who would otherwise depend on private means of transport, such as a car.

Figure 8: Impact of Census Shock on Cumulative Earnings by Gender



Notes: The figure shows the estimated coefficients $\hat{\beta}_t$ from Equation (3), with 95% confidence intervals. Standard errors are clustered on the municipality level m. The outcome variables in Panels A and B are cumulative yearly earnings, divided by average earnings from 2005 to 2007 for men and women respectively. Panels C and D show the decomposition of the census shock into earnings effects due to the same employer (blue), the same region, but not same employer (red), and different region (green) for men and women respectively. The three components in Panel C and D sum to the baseline effects together with a residual NA category (workers with a missing municipality identifier), which is omitted from the figure. The sample used includes all workers aged 26 to 56 at the time of the census shock and who were full-time employed in the three years preceding it.

6.3 Effects by Value for Public Goods: Childcare

As discussed in Section 2, revenue gains tied to the FAG 2008 reform were linked to increased provision of childcare. Our results presented in Section 5 confirmed that the census shock is associated with childcare expansion. Childcare is naturally a region-wide public good: its benefits accrue to any employer located in a treated municipality, rather than to a single firm. Our data allow us to test this region-wide explanation directly, since we observe both childcare expansion at the municipality level and whether workers have children, along with their ages.

We use the age of the youngest child at baseline to split the sample by likely valuation of publicly provided childcare: ages 0–2, 3–5, 6–10, 11–14, older than 14, or no children at baseline.

We choose the first age interval, 0–2, to correspond to the period of job-protected parental leave, an important milestone for parents intending to return to the labor market (Laffers and Schmidpeter, 2025). Parents, most often mothers, can take leave while retaining the right to

return to their previous employer in the same or a similar position.[20] Since most parents are still on leave during this period and have not yet returned to work, we would not expect a strong impact of childcare provision on our municipality component at this age range.

The second threshold, 3–5, coincides with the end of job-protected leave and the usual age at which children in Austria enter kindergarten, while the third threshold, 6–10, reflects primary-school age. For both age ranges, publicly provided childcare is likely to be most valuable for mothers, and we would therefore expect the largest impact on our municipality component to be concentrated here.

The remaining thresholds are 11–14 and older than 14, corresponding to lower and upper secondary school, respectively. At these age ranges, school days are generally longer and children require less direct supervision by parents.[21] Having access to childcare should therefore be less important for compensating wage differentials.

Figure 9 shows estimates six years after the census shock from event-studies run separately by the age of the youngest child at baseline. The upper part (Panels A and B) show the impact on cumulative earnings, and the lower part (Panels C and D) show the results of our decomposition, with men shown on the left and women on the right.

Panel A shows that earnings effects for men are of comparable magnitude across all child-age ranges. In contrast, results for mothers show a clear age gradient; see Panel B. Consistent with our prediction, we find no significant effect when the youngest child is under 2, an age range in which most parents are still on parental leave and have not yet returned to work. Effects become large and significant when the youngest child in the family is between 3 and 10 years old, the age range when childcare and after-school care are likely to be most valuable to mothers. To put these effects into context, we compare a mother working in a municipality at the 75th percentile of the change in population size due to the census shock (1.5 percentage points; see Table 1) with a mother at the 25th percentile (0.2 percentage points). The implied differential reduction in cumulative earnings over the following six years for the mother at the 75th percentile, relative to the mother at the 25th percentile, is 13.11% ($= -10.09 \times (1.5 - 0.2)$) of initial annual earnings when the child is of kindergarten age, and a still-substantial 3.07% when the child is of primary-school age ($= -2.36 \times (1.5 - 0.2)$).

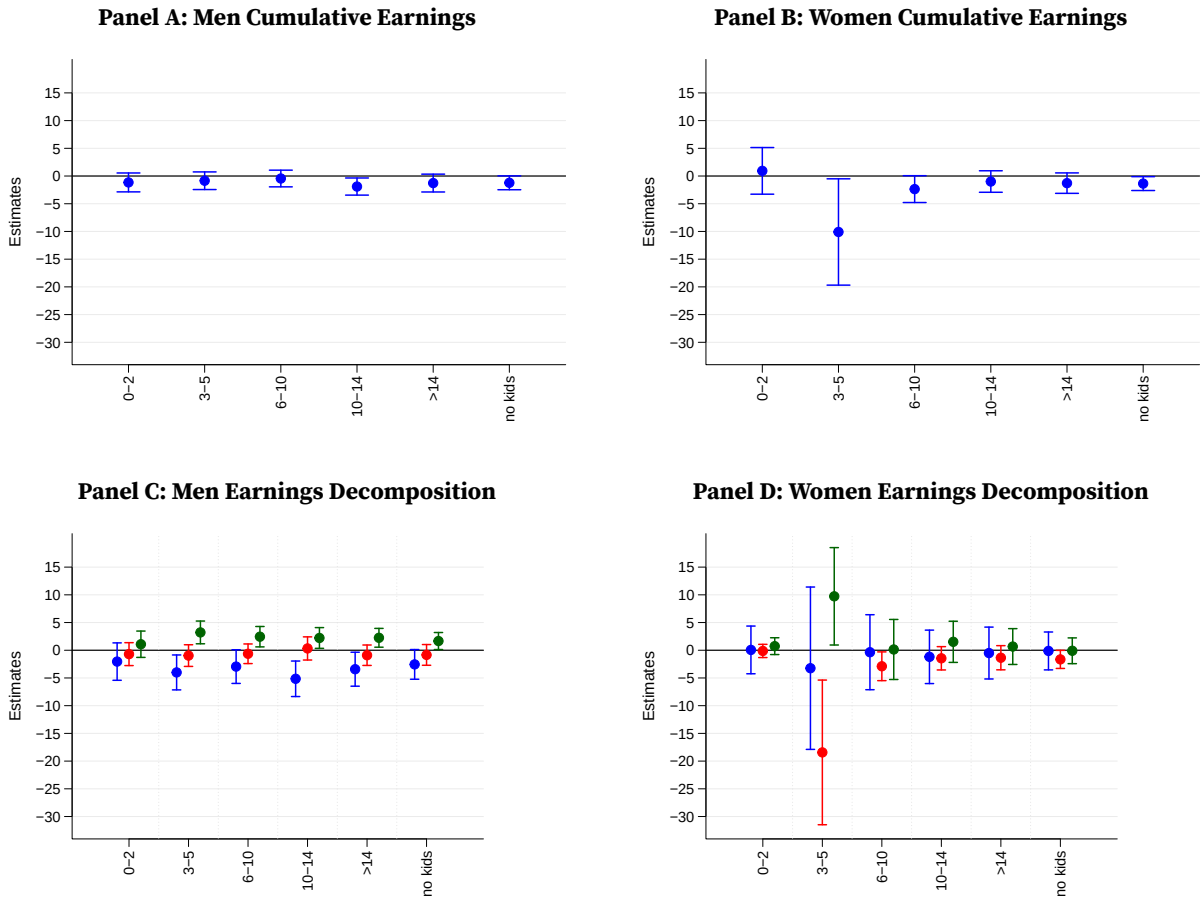
For men, the same-employer component negatively affects cumulative earnings across almost all child-age ranges, with a similar magnitude throughout. In contrast, for women, we find large and significantly negative effects of the same-municipality component for mothers of children aged 3–5 and 6–10; these effects become small and insignificant for mothers of older children. This pattern supports our conjecture that firms capitalize on region-wide pub-

[20] There have been various changes in the benefit duration in Austria over the past decades. At our baseline period, parents were eligible for 30 months of benefits, extended to 36 months if the second partner took 6 months of leave. Parents of children born after January 1, 2008 could choose from a menu of durations, ranging from 12 months with higher benefits to 36 months with lower benefits. Regardless of the chosen duration, job-protected leave is fixed to 24 months after childbirth.

[21] A typical school day in primary school runs from 8 a.m. to 1 or 2 p.m. while it can extend to 4 p.m. in secondary school.

lic goods as compensating differentials for women, while the male effect operates through a firm-specific channel largely unrelated to child age.[22]

Figure 9: Impact of Census Shock on Cumulative Earnings by Age of Child



Notes: The figure shows the estimated coefficients $\hat{\beta}_t$ from Equation (3) for the year 2013 and separate event-studies splitting the sample by the age of the youngest child at baseline, with 95% confidence intervals. Standard errors are clustered on the municipality level m. The outcome variables in Panels A and B are cumulative yearly earnings, divided by average earnings from 2005 to 2007 for men and women respectively. Panels C and D show the decomposition of the census shock into earnings effects due to the same employer (blue), the same region, but not same employer (red), and different region (green) for men and women respectively. The three components in Panel C and D sum to the baseline effects together with a residual NA category (workers with a missing municipality identifier), which is omitted from the figure. The sample used includes all workers aged 26 to 56 at the time of the census shock and who were full-time employed in the three years preceding it.

7 Alternative Mechanism: Increase in Aggregated Labor Supply

An alternative explanation for our results is that the census shock operates through a Roback-Rosen-style spatial-equilibrium channel (Rosen, 1979; Roback, 1982): higher local amenities attract more workers into the municipality, and it is this increase in labor supply that pushes

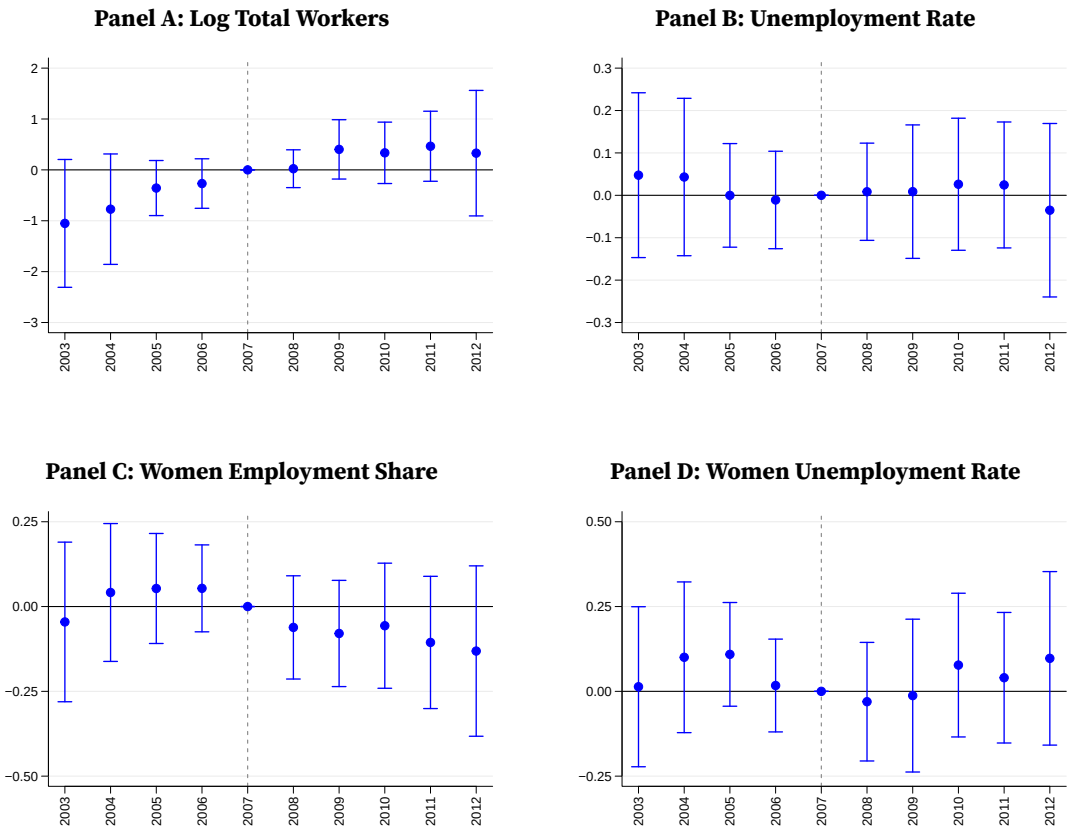
[22] We additionally find a large, significant, and positive effect of the other-municipality component for mothers of kindergarten-aged children. We conjecture this reflects the salience of kindergarten provision specifically, which was the most visible and publicly debated component of the FAG-linked childcare expansion and renewed discussions about the Barcelona targets, an EU-level policy target to improve childcare for children below school age: firms in treated municipalities may be able to use this salience to compress wages for mothers who remain, while firms elsewhere may need to offer a premium to attract those who move away.

wage growth down. This alternative predicts that municipalities with larger census shocks should see an inflow of workers. We test this by examining how the census shock relates to the log total number of workers and to labor supply more generally. Figure 10 shows the results.

We do not find evidence of such a channel. Municipalities with larger census shocks neither see a stronger increase in the log total number of workers nor a change in the local unemployment rate (Panels A and B). Splitting labor supply by gender, we find no meaningful effect of the census shock on the female employment share (Panel C), nor evidence that gender-specific changes in the unemployment rate drive our results.

Overall, we interpret these results as evidence against the Roback-Rosen inflow channel: the census shock does not generate the increase in local labor supply that this alternative mechanism predicts, supporting our preferred interpretation that the wage decline operates through firms' appropriation of amenity gains through lower wages for incumbent workers rather than through population inflows.

Figure 10: Impact of Census Shock on Cumulative Earnings (unweighted)



Notes: The figure shows the estimated coefficients $\hat{\gamma}_t$ from Equation (2), with 95% confidence intervals. Standard errors are clustered on the municipality level m. The outcome variable in Panel A is log total number of employed workers in a municipality, and in Panel B the municipality unemployment rate. Panels C use female employment as a share of total municipality employment and Panel D the female unemployment rate.

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Online Appendix

Contents

Appendix A Data Appendix **1**

Appendix B Descriptive Tables and Figures **1**

Appendix C Census Shock and Provision of Public Transportation **1**

Appendix D Supplementary Tables and Figures **4**

Appendix A Data Appendix

Fiscal Data

We use annual municipal finance data from Statistik Austria's municipal finance statistics, available through STATcube (https://www.statistik.at/datenbanken/statcube-statistische-datenbank), for the years 2003–2013. The data are based on municipalities' annual closed accounts and form part of the public-sector finance statistics. For municipalities, the data are transmitted electronically through the municipal household-data interface: municipalities report their accounts to the responsible regional authority, which then forwards the collected data to Statistik Austria.

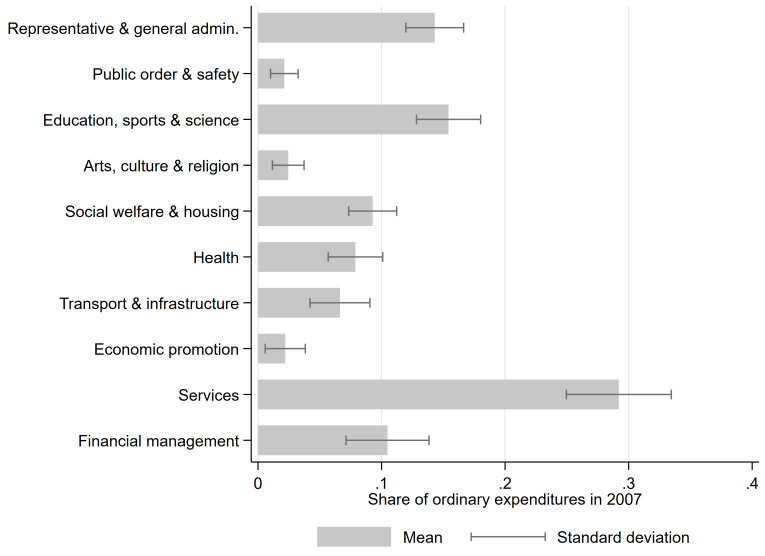
During our sample period, municipal accounts were prepared under the *Voranschlags- und Rechnungsabschlussverordnung 1997* (VRV 1997), the Austrian regulation governing the structure of municipal budgets and annual accounts. VRV 1997 classified revenues and expenditures along two main dimensions. A functional classification assigned budget items to municipal tasks or policy areas, such as administration, education, social services, health, infrastructure, public services, and financial administration. An economic classification identified the type of transaction, such as personnel expenditure, purchases of goods and services, transfers, capital expenditure, or debt-related transactions. Our expenditure outcomes use the functional classification of municipal spending, reported in STATcube at the level of broad functional groups and sections.

We also use municipal fiscal-equalization revenue. This variable captures revenues received by municipalities through the fiscal equalization scheme, in particular municipal revenue shares from shared federal taxes. These revenues are recorded in municipal accounts under the consistent accounting framework, but their allocation is determined by the applicable Fiscal Equalization Acts.

Although municipalities reported under a common accounting framework during 2003–2013, accounting practices and the assignment of revenues and expenditures to categories were not fully harmonized in practice across municipalities and regional states (Länder). The major harmonization reform took effect from 2020, when municipalities began preparing accounts under VRV 2015.

Appendix B Descriptive Tables and Figures

Figure B.1: Share of Expenditure by Functional Classification



Notes: The figure presents the cross-municipality average and standard deviation of share of municipal expenditure by each functional category in 2007.

Appendix C Census Shock and Provision of Public Transportation

In our main analysis, we conjecture that the negative earnings effects for men are tied to public goods which benefits specific firms, as indicated by the large same-employer component of our decompo-

sition. In this section, we provide additional evidence and show that our census shock is related to expansion of public transportation in subsequent periods. However, we want to stress that we interpret the results in this section as indicative rather than causal, given data limitations and their impact on identification assumptions.

Our fiscal data, described in Section 3, do not contain information on the provision of public transportation within a municipality. The decision how much of public transportation to provide, such as frequency , weekends, and night services, is normally made by the federal state and the municipality, together with the regional providers of public transportation.

We obtained the yearly schedule of all transport associations (Verkehrsverbünde) in Austria for the years 2023 and 2024 from Mobilitätsverbünde Österreich.[23] The data describe scheduled trips, stops, and service calendars, which we use to construct the average numbers of overall, weekday, and weekend departures, as well as scheduled nighttime departures, defined as scheduled departures after 8 p.m. and before 6 a.m. We also calculate the share of scheduled weekend and nighttime trips, which provides a measure of service availability at times outside standard working hours. The data also contain the geographic information of stops in latitude and longitude format, which we use to assign each stop to a municipality.

Given the limited data availability, we are not able to run event-study estimates with municipality fixed effects. Instead, we run separate cross-sectional regressions of the outcome of interest in 2023 and in 2024 on our census shock, controlling for baseline (2007) log population, log number of workers, average log wage, share of women, unemployment rate, and the change in population between 2001 and 2007. We include these variables to capture potential differences in demand for public transportation, such as the number of potential customers. We additionally include state and transport association fixed effects and cluster standard errors at the municipality level. We stress again that we interpret the coefficients on our census shock as suggestive evidence rather than as causal estimates.

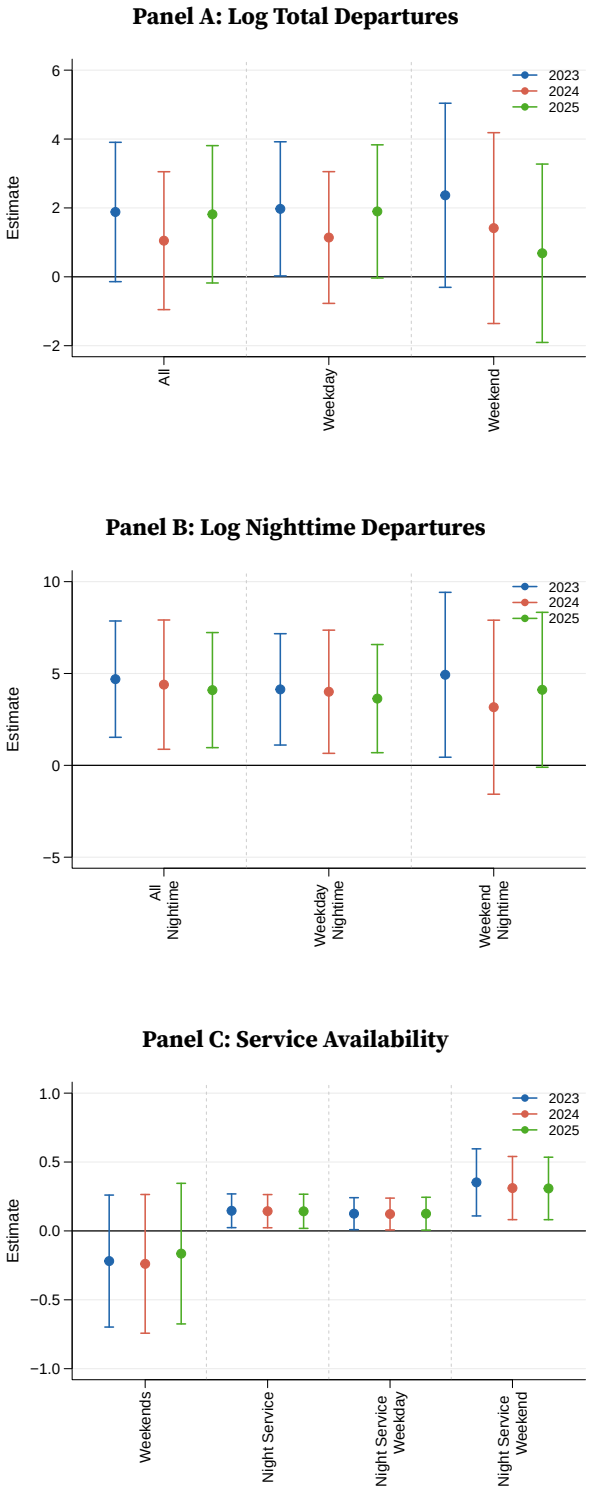
Figure C.1 shows the coefficient on the census shock, together with 95% confidence intervals, for the outcome years 2023 and 2024. Panel A shows estimates for the log number of overall scheduled departures, split by weekday and weekend. Panel B shows the same split, but restricted to nighttime hours. Panel C shows estimates for service availability, defined as the share of departures occurring on weekends and during nighttime hours, respectively.

As Panel A shows, our census shock is positively correlated with a general expansion in services, although some coefficients are noisily estimated. However, as Panel B shows, most of this expansion occurs during nighttime hours, with large and precisely estimated coefficients. This pattern is mirrored in service availability, shown in Panel C: estimates are large and statistically significant for nighttime services generally, and particularly at weekends. Coefficients for 2023 and 2024 are very similar throughout. Overall, these service expansions plausibly extend reachability to workers on non-standard shifts who would otherwise depend on private means of transport.[24]

[23] Mobilitätsverbünde Österreich provides mobility data of all transport associations in Austria in GTFS format, with data available from 2022 onward. We exclude 2022, as public transportation schedules were still affected by the aftermath of the Covid pandemic.

[24] We do not claim that service expansion, such as at nighttime, benefits only workers. It may also benefit other individuals, such as leisure travelers and late-night party goers. However, firms could benefit from such expansions insofar as they function as compensating amenities, reducing the wage premium needed to attract or retain workers on non-standard shifts.

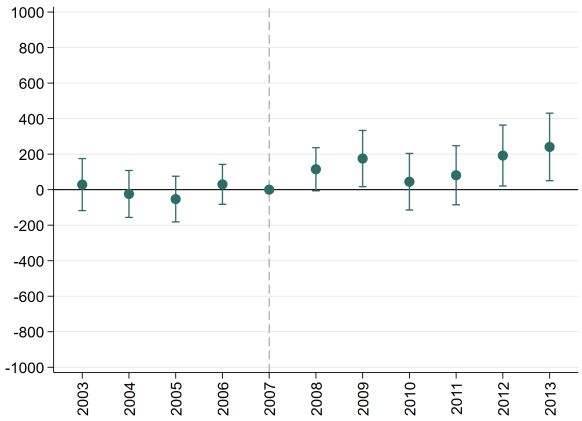
Figure C.1: Impact of Census Shock on Public Transportation Provision



Notes: The figure shows the estimated coefficients on the census shock, defined as the log deviation between actual and predicted 2008 municipal population (see Section 2 for details), from separate cross-sectional regressions run for outcome years 2023 and 2024, with 95% confidence intervals. The outcome variables are: the log number of overall, weekday, and weekend scheduled departures (Panel A); the same split, restricted to nighttime hours, defined as departures after 8 p.m. or before 6 a.m. (Panel B); and service availability, defined as the share of departures occurring on weekends and during nighttime hours, respectively (Panel C). All regressions include baseline (2007) log population, log number of workers, average log wage, share of women, unemployment rate, and the change in population between 2001 and 2007, as well as state and transport association fixed effects. Standard errors are clustered at the municipality level.

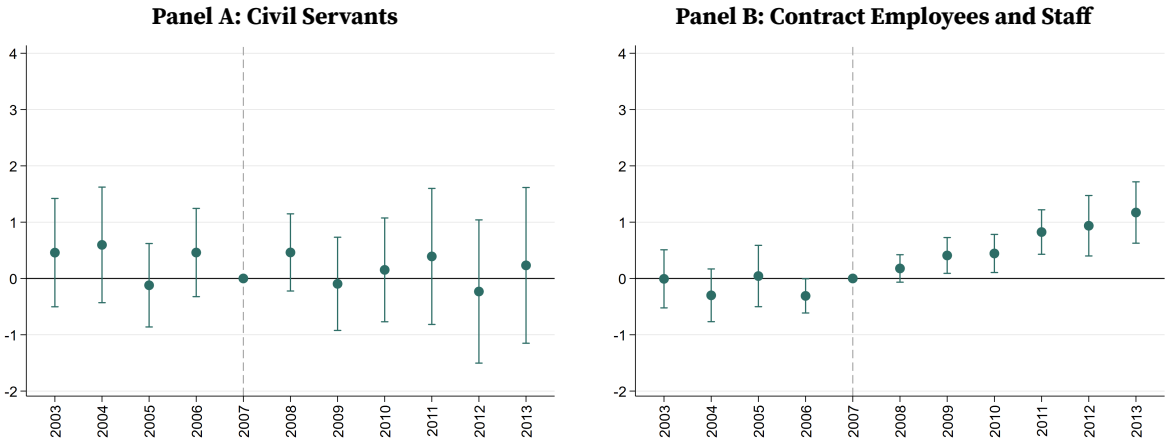
Appendix D Supplementary Tables and Figures

Figure D.1: First Stage Effects on Fiscal Transfer (per capita)



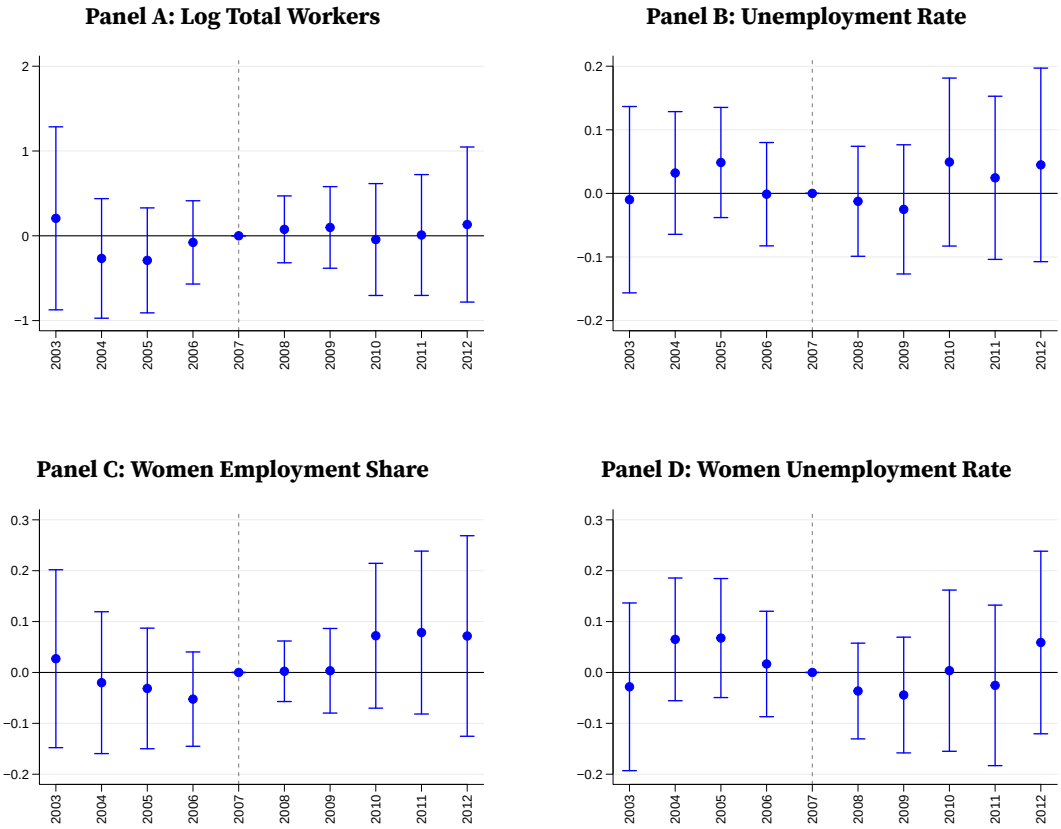
Notes: The figure shows the estimated coefficients $\hat{\beta}_t$ from Equation (3), with 95% confidence intervals. Standard errors are clustered at the municipality level. The outcome is fiscal transfer per capita (in euro) received by municipality through fiscal equalization. The population in the denominator is the population in 2007.

Figure D.2: Effects on Public Employment



Notes: The figures show the effects of the fiscal revenue on log number of civil servants and log number of contract employees and staff. Civil servants hold a public-law service relationship based on formal appointment, whereas contract employees are employed under a contractual service relationship.

Figure D.3: Impact of Census Shock on Cumulative Earnings (weighted)



Notes: The figure shows the estimated coefficients $\hat{\gamma}_t$ from Equation (2), with 95% confidence intervals. Regressions are weighted by the total number of workers in a municipality at baseline. Standard errors are clustered on the municipality level m. The outcome variable in Panel A is log total number of employed workers in a municipality, and in Panel B the municipality unemployment rate. Panels C use female employment as a share of total municipality employment and Panel C the female unemployment rate.