Fiscal Transfers to Local Governments and the Distribution of Economic Activity *

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Abstract

We study the economic effects of transfers to local governments using a reform of the Finnish municipal grant system as a source of exogenous variation. We find that higher grants lead to lower municipal taxes and fees, and higher public spending. These changes in local fiscal policy lead to an increase in private sector jobs. Our estimates imply a cost per job of €33,000. The increase in jobs is paired with a reduction in commuting to other municipalities. The effect on migration seems small, suggesting grants bring local benefits without drastically affecting where households choose to live.

Key words: Local government grants, Internal migration, Labor markets, Commuting, Fiscal equalization, Regional policy
JEL codes: R23, R28, H72

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1 Introduction

Fiscal transfers from national to subnational governments aim to reduce disparities in service provision and tax capacity across regions. Local governments can use these transfers to improve public services or to reduce taxes and fees. These policy changes may in turn affect location choices of households and firms, and boost the local economy. Indeed, a growing body of evidence demonstrates that local spending decisions can generate large multiplier effects on income and employment (Acconcia et al., 2014; Adelino et al., 2017). Similarly, state and local tax policy has been found to affect employment, wages and business location decisions (Albouy, 2009; Duranton et al., 2011; Fuest et al., 2018; Giroud and Rauh, 2019; Suárez Serrato and Zidar, 2016). These findings suggests the effects of fiscal transfers go beyond reducing fiscal disparities and may alter the distribution of economic activity.

In OECD countries, subnational governments account for 39.5% of public expenditure, and on average 39.8% of their revenue comes from intergovernmental transfers.[1] Despite their prevalence, the effects of fiscal equalization on the spatial distribution of economic activity remain underexplored (Agrawal et al., 2024). By contrast, the spatial impacts of other geographically targeted interventions—such as infrastructure investment, enterprise zones, and related place-based policies—have been studied more extensively. According to Austin et al. (2018), the strongest case for spatial targeting rests not on traditional efficiency (agglomeration) or equity (insurance) arguments, but on market failures best addressed locally. In particular, supporting high-distress areas may yield higher marginal returns (Bartik, 2020). Because most equalization formulas inherently direct resources toward fiscally weaker jurisdictions, unconditional transfers share this targeting feature. Whether they can serve as an effective tool to support distressed places, however, remains an open question.

In this paper, we examine the impact of fiscal transfers on the spatial distribution of economic activity. First, we analyze how municipalities adjust their fiscal policies in response to a major grant reform. Next, we explore the effects of these changes on the number of local jobs, employment and income. Finally, we analyze how migration and commuting patterns respond.

We study Finland, where municipalities have large spending responsibilities, especially in education, health and social services. Finnish municipalities have significant tax autonomy too, collecting more than half of their revenue through local taxes. Central government transfers account for a fifth of municipalities' revenues and amounted to €1,540 per capita or about 4% of GDP in 2019.

The key challenge to studying the effects of fiscal transfers to local authorities is

[1] Source: "Subnational government structure and finance", OECD Regional Statistics database, https://doi.org/10.1787/05fb4b56-en (accessed on 12 December 2024).

the two-way causality between local economic performance and transfers. The amount of transfers local governments receive tends to be formula-based and depends on local socioeconomic conditions. To address this, we exploit a reform of the transfer system in 2015, which drastically simplified the grant formula. Our empirical strategy compares municipalities that gained more financing in the reform (winners) to those that lost it (losers) over time. On average, winners' grants increased in the reform by over €300 per resident, compared to losers. Pre-reform trends of grants and key outcomes in these groups were highly similar, indicating that post-reform developments can be causally interpreted as the impact of grant changes.

We find first that the grant reform resulted in significant changes in local fiscal policy, including changes in tax rates, service fees and overall spending. These findings speak to a large literature on intergovernmental grants.[2] Standard collective choice theory suggests that public funds are fungible and should be allocated in line with the marginal willingness to pay for public goods, so that unconditional grants are spent similarly to private income (Bradford and Oates, 1971a,b). Empirically, however, grants tend to be spent rather than returned through tax cuts—a pattern known as the "flypaper effect."

In our case, increased grant revenue still raises spending, but the effect is not as strong as in much of the prior literature. This is true even in comparison to studies in closely related institutional contexts such as Sweden and Switzerland, as well as Finland, where sizable flypaper effects have been documented (Dahlberg et al., 2008; Köthenbürger and Loumeau, 2023; Lundqvist, 2015; Saastamoinen and Kortelainen, 2020). Some early evidence of flypaper effects has been attributed to limitations in research design (Becker, 1996; Knight, 2002). More recent work often exploits narrow discontinuities or kinks in grant formulas, which generate credible causal estimates but may be less externally valid and less salient to voters. By contrast, the reform we study was structural, wide-ranging, and likely well known, increasing the chance that voters perceived the revenue as general and durable. This interpretation is consistent with the traditional misperception explanation for the flypaper effect (e.g. Filimon et al., 1982; Courant et al., 1979; Oates, 1979; Turnbull, 1992; Hines and Thaler, 1995).

It is useful to compare our findings to those of Lutz (2010), who shows nearly one-for-one tax offsets following a school finance reform under direct democracy. In contrast, fiscal policy in Finnish municipalities is decided by elected representatives. Furthermore, rather than relying on property taxation, Finnish municipalities depend predominantly on income taxation, collecting around half of total income tax revenue. When spending is financed by distortionary taxation, additional grant revenue is more likely to be spent (e.g. Hamilton, 1986; Dahlby, 2011; Aragon, 2013; Vegh and Vuletin, 2016). The combination of representative democracy and distortionary taxation may help explain the more limited

[2]See, for example, recent reviews by Agrawal et al. (2024), Clemens and Veuger (2023), and Lago et al. (2024). For the flypaper effect specifically, see Inman (2008) and Gamkhar and Shah (2007).

tax offsets we observe compared to Lutz (2010).

Another factor may be the broader economic environment. Some of the strongest flypaper effects in the literature appear in poorer or more fiscally stressed jurisdictions or periods (e.g. Dahlberg et al., 2008; Lundqvist, 2015; Leduc and Wilson, 2017; Clemens et al., 2024b). Our reform took place in a stable macroeconomic period, which may have allowed municipalities to credibly cut taxes or fees without jeopardizing services.

Finally, the permanence of the grant change may play a role. Temporary or unexpected grant shocks are often associated with high spending propensities and little evidence of smoothing through tax or debt policy, frequently linked to political opportunism (e.g. Berset and Schelker, 2020; Clemens et al., 2024b; Leduc and Wilson, 2017; Shoag, 2010; Singhal, 2008). In contrast, persistent changes tend to generate more balanced adjustments in both spending and taxation (Helm and Stuhler, 2024; Leisibach et al., 2023; Lutz, 2010; Serrato and Wingender, 2014). This could be the result of mental accounting (Hines and Thaler, 1995; Becker et al., 2020). Voters may treat temporary transfers differently, especially if they target a specific need (e.g. pandemic fiscal relief).

In summary, we attribute our relatively mild flypaper effect to the grant reform being broad-based, well known, and permanent, and to it having taken place in a stable economic environment without binding service constraints or urgent fiscal needs.

We next examine how the grant reform affected the spatial distribution of economic activity. Intergovernmental grants are primarily intended to equalize fiscal capacity and support public services, but—because most formulas direct resources toward fiscally weaker or economically lagging areas—they share important features with place-based policies. At the same time, they are typically less programmatic and more general-purpose, resembling fiscal stimulus policies, though the latter are usually transitory. These overlapping characteristics suggest that studying intergovernmental grants can inform both literatures.

To our knowledge, we are the first to provide quasi-experimental evidence of how unconditional transfers affect local economic activity through adjustments in local tax and spend policy.[3] We find that the reform increased the number of private sector jobs, with no discernible effect on the overall employment rate or migration. Instead, more residents worked locally rather than commuting out of their municipality, indicating a reallocation of labor within local labor markets.

Place-based policies are geographically targeted interventions designed to improve economic performance or social conditions in specific locations. Unlike people-based policies, they aim to raise the welfare of residents in targeted areas by altering the local environment—often through public investment, regulatory relief, or fiscal incentives.[4]

[3]Note however that the impact of grants on location decisions has been explored in quantitative theory contributions by Albouy (2012) and Henkel et al. (2021), more recently.

[4]See Neumark and Simpson (2015) and Austin et al. (2018) for overviews of the economic rationales and empirical evidence on place-based policy. Other reviews include Bartik (2020) on job creation,

Traditional skepticism toward place-based policies centers on potential efficiency losses from mobility distortions: if workers and firms relocate to treated areas to capture local transfers, this may misallocate labor and capital, reduce aggregate productivity, and lower overall welfare (Glaeser and Gottlieb, 2008; Neumark and Simpson, 2015). More recent work softens this view, noting that households are often imperfectly mobile (Austin et al., 2018). In such cases, people may respond to local decline not by moving, but by "voicing" their discontent politically, potentially contributing to polarization and instability (Rodríguez-Pose, 2018; Guriev and Papaioannou, 2022).

Our results are consistent with this newer perspective. We find no evidence of population movement in response to the grant reform, suggesting either substantial mobility frictions or geographically limited awareness of the reform's fiscal effects (though not so limited as to produce a large flypaper effect). Instead, we observe a shift toward more residents working locally rather than commuting out, indicating a reallocation of labor within local markets. These effects do not involve the relocation patterns traditionally cited as efficiency losses in the place-based policy debate.[5]

At the same time, our results offer only little support for what some now view as the strongest rationale for place-based policies over people-based ones: spatially heterogeneous employment elasticities (Austin et al., 2018; Bartik, 2020). The grant reform did not lift people out of unemployment and thus did not mitigate fiscal externalities associated with joblessness, such as foregone tax revenue, higher social spending, or broader social costs related to crime, educational outcomes, and life satisfaction.

Finally, there is an ongoing debate about how to design place-based policies. Should they be tailored to local conditions (Austin et al., 2018) or should the funding be more flexible to allow for many uses attuned to local needs (Bartik, 2020)? Our results suggest that general-purpose grants can indeed stimulate local economies, though more tailored policies may be needed to reduce unemployment.

Our findings are also relevant in light of a growing literature using spatial variation to determine fiscal multiplier effects. Due to the lack of (high-quality) local output data, this literature tends to focus on employment effects. Reviewing recent work, Chodorow-Reich (2019) finds cost-per-job estimates ranging from $25,000 to $125,000, with a preferred estimate of $50,000. Chodorow-Reich (2019) converts this to an output multiplier of 2. We estimate a cost-per-job of €33.000, which translates to a multiplier effect of 1.97 to 2.56, depending on parameterization of the same conversion (see Online Appendix A).

Our estimated multiplier is relatively large. A key reason is the permanence of the

Ehrlich and Overman (2020) on EU Cohesion Policy, and Corinth and Feldman (2024) on Opportunity Zones. An emerging strand of the literature examines place-based redistribution targeting residents directly (Gaubert et al., 2021; Austin et al., 2018).

[5]Recent theory also highlights conditions under which place-based targeting can increase efficiency, for example by relieving congestion in high-wage cities (Fajgelbaum and Gaubert, 2020; Henkel et al., 2021).

grant reform. Unlike transitory stimulus, which may lead to more cautious responses, a durable increase in revenue can justify sustained spending and tax cuts, which in turn may boost demand and private investment. In this regard, our findings align with studies of persistent funding shocks, such as Adelino et al. (2017) and Serrato and Wingender (2016), who report cost-per-job estimates of $20,000 and $30,000 respectively—comparable to our estimate of €33,000. However, this may also imply that our multiplier is not nationally representative. For the treated municipalities and their residents, the reform was largely externally financed and therefore unlikely to trigger a Ricardian response—one that would be sizable given the persistence of the reform.

Separately, our estimate may not fully capture economic effects occurring beyond the treated municipalities. Persistent transfers may influence the location decisions of households or firms, leading to negative spillovers in untreated areas and upward bias in local estimates. However, most studies of subnational multipliers find positive spillovers, as treated areas stimulate demand in surrounding localities through consumption or supply chain linkages (e.g. Dupor and McCrory, 2018; Auerbach et al., 2020; Shoag, 2010). In our case, we find no evidence of population movement or firm relocation, we only observe changes in commuting patterns.

The broader fiscal multiplier literature also highlights the importance of state dependence. In her review, Ramey (2019) concludes that while evidence for higher spending multipliers during periods of economic slack is fragile, there is more consistent support for larger effects when monetary policy is constrained—such as at the zero lower bound. She also finds that tax multipliers are uniformly pro-cyclical, with stronger effects during expansions than in downturns. This is particularly relevant in our setting, where the grant reform affected not only spending but also local taxation and fees. Finland was experiencing modest growth following a period of post-crisis stagnation, and nominal interest rates were close to zero. Taken together, these conditions suggest that state dependence may help explain the relatively large multiplier effect we observe.

Finally, most local multiplier studies focus solely on spending. Our estimate captures the combined effect of spending increases, tax cuts, fee reductions, and lower deficits, so it cannot be easily compared. Nevertheless, our results suggest that broader subnational fiscal responses may matter when assessing the overall impact of stimulus funding.

The rest of this paper is organized as follows. In Section 2, we present the institutional setting, including the transfer system and local public finance in Finland more broadly. In Section 3, we discuss our data and show descriptive statistics. In Section 4, we present the identification strategy and estimation methodology. In Section 5, we first analyze the impact of grants on local fiscal policy and then estimate the effect on the local economy and inter-municipality migration. In Section 5.5, we provide some robustness checks. Finally, Section 6 concludes.

2 Institutional setting and the reform

2.1 Local public finance in Finland

Finland is a unitary but highly decentralized country with a single tier of local governance consisting of approximately 300 municipal authorities.[6] Municipalities are responsible for providing a wide range of services, including most notably health and social services[7], primary education and children's daycare. Most municipal services are mandated by the law, but municipalities can independently decide how they provide the services. Municipalities can organize the services themselves, through joint municipal authorities or purchase them from other municipalities or the private sector. Municipalities do not have legislative power, but they implement certain regulations, for example, related to zoning. Municipal services account for a large share of public spending in Finland. In 2014, total operational spending of municipalities was about 40 billion euros, which was almost as much as central government spending and about 20% relative to GDP.

Table 1 shows a breakdown of the spending categories and income sources of Finnish municipalities in 2014.[8] Health and social services accounted for over half of total spending, while schooling and daycare represented one fourth. Minor spending categories were culture and recreation, public utility services (such as road infrastructure and water and waste management) as well as administration and internal services (such as general administration and IT services).

Municipalities obtained half of their income from tax revenues. The majority of tax revenue stemmed from a residence-based flat tax on labor income net of certain deductions.[9] Smaller sources of tax revenue were property taxes and a share of the national corporate income tax. Income and property tax rates are set locally, but corporate income taxes are levied at the national level. Central government transfers represented about one-fifth of municipal income. In 2014, these transfers totaled 8.2 billion euros. This revenue is not earmarked. Municipalities can freely spend the transfers, as long as they offer the basic services mandated by the law. Remaining income was derived from various service fees, sales revenues and subsidies (other than central government transfers).

[6]The autonomous region of Åland is an exception. Moreover, in 2023, after our period of analysis, a new middle tier of regional authorities was created, but they operate entirely with central government financing and have limited autonomy.

[7]Since 2023, health and social services are no longer organized by municipalities but by larger regional authorities. This paper studies a time period during which municipalities were still responsible for providing those services.

[8]Spending covers only operational spending, net of internal revenues. Investments, loan repayments and financial expenses are excluded. For completeness, borrowing, sales of assets and financial revenue are excluded from income sources.

[9]Finland has a dual income tax system, where capital income and labor income are taxed separately. The local income tax does not apply to capital income.

Table 1: Operational spending and income sources of Finnish municipalities in 2014

Operational spending		Income sources	
Health and social services	56.2%	Income tax	47.2%
Schooling and daycare	26.4%	Property tax	3.8%
Public utility services	6.0%	Share of national corporate tax	3.9%
Culture and recreation	5.3%	Central government transfers	21.3%
General administration	3.6%	Fees, sales revenue and subsidies	23.8%
Other spending	2.5%		

Notes: The table describes the categories of operational spending and income sources of Finnish municipalities in 2014. All municipalities except those located in Åland Island are included. Shares are calculated based on aggregated spending and revenue of the municipalities.

2.2 The central government transfer system and the reform of 2015

Central government transfers are allocated to municipalities based on a formula that aims to capture their spending needs and tax capacity. First, the imputed costs of providing the services are calculated with a formula that considers differences in attributes reflecting service needs (e.g., age structure and relative morbidity of the population) and attributes reflecting the costs of providing the services (e.g., population density). These costs are then aggregated to determine the total imputed costs of the municipality. Each municipality is responsible for a fixed amount of financing (3,520.93 euros per resident in 2014), and the central government covers any remaining imputed costs. Because the transfers are based on imputed costs, municipalities have no incentives to increase their spending in order to obtain more government financing.

The cost-based compensation is complemented with a tax-base equalization system to take into account differences in municipalities' ability to collect tax revenue. Equalization is based on imputed tax revenues that are calculated by multiplying municipality's income tax base by the national average municipal income tax rate, and adding municipality's share of the national corporate tax revenue. These imputed revenues are compared to the equalization limit (the national average or slightly below). Imputed revenues per resident below (above) the equalization limit increase (decrease) transfers received from the central government. Since imputed revenues are based on average municipal tax rates, municipalities have no incentives to cut their taxes in order to receive more financing.

Transfers are recalculated every year as grant determinants change. Grants for year t are based on grant determinants measured in year $t-2$. Thus, changes in grant determinants affect grants with a two-year lag. In addition, the central government may change the share of municipal services it pledges to finance. However, year-to-year changes are usually small, because municipalities' demographic and economic conditions change sluggishly and total government financing has remained quite stable during the

study period (years 2010–2019).

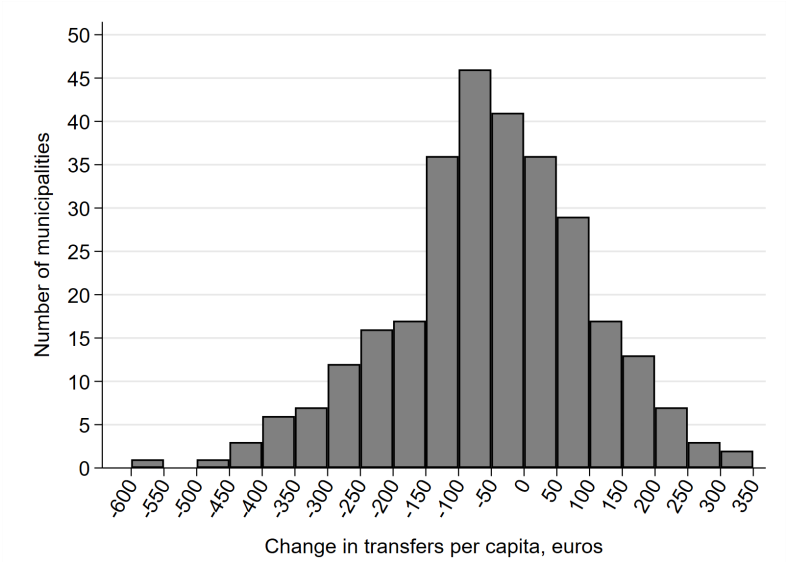
In 2015, the central government transfer system was reformed. The objective of the reform was to simplify the system and update it to better reflect differences in service needs and production costs. Some components that determined cost-based compensation were removed, while others were added, and the calculation methods for imputed costs were adjusted. Appendix B discusses the reform in more detail and describes how the weights of different municipality characteristics changed. On aggregate, the reform was revenue neutral.

Although total central government financing remained unchanged, it lead to significant redistribution of funds between municipalities. About one-third of municipalities received more central government financing, while two-thirds lost. The annual amount of transfers redistributed from the losing municipalities to the winning ones was about €220 million. To avoid too drastic changes in municipal finances, the reform included a transition period from 2015 to 2019, during which transfers were gradually adjusted. In 2015, the maximum change was capped to €50 per resident, followed by €100 in 2016, €180 in 2017, €260 in 2018 and €380 in 2019. However, by 2017, most municipalities had already experienced the full change in grant revenue.

2.3 Descriptive analysis of the reform

To study the reform, we examine imputed changes in grants. These are the reform-induced changes in grants, calculated by applying pre-reform values of the grant determinants to both the old and the new grant formula, and taking the difference. Actual changes in grants are endogenous to changes in the demographic and economic characteristics of each municipality. Imputed changes in grants only capture variation in revenue due to adjustments in the allocation formula. Figure 1 shows the distribution of imputed grant changes. Many municipalities were almost unaffected by the transfer reform, but others saw large changes in their grant revenue.

Figure 1: Distribution of imputed transfer changes



Notes: The figure shows the distribution of imputed grant changes due to the grant reform in 2015. The imputed change is calculated keeping characteristics of the municipality fixed at 2014 level.

In Table 2, we zoom in on the most affected municipalities, defining winners as those municipalities gaining over €50 per resident, and losers as those losing over €50 per resident. Our identification approach discussed in Section 4 is based on the comparison of these groups. There are roughly twice as many losers as winner municipalities. Before the reform, winner municipalities received slightly less grant revenue per resident than loser municipalities. However, the difference is small and the median was in fact higher for winner municipalities. Overall, the distribution of grant revenue is quite similar between the two groups. Following the reform, loser municipalities lost on average €172 per resident while winners gained slightly less, €132 per resident on average.

Table 2: Summary statistics of central government transfers

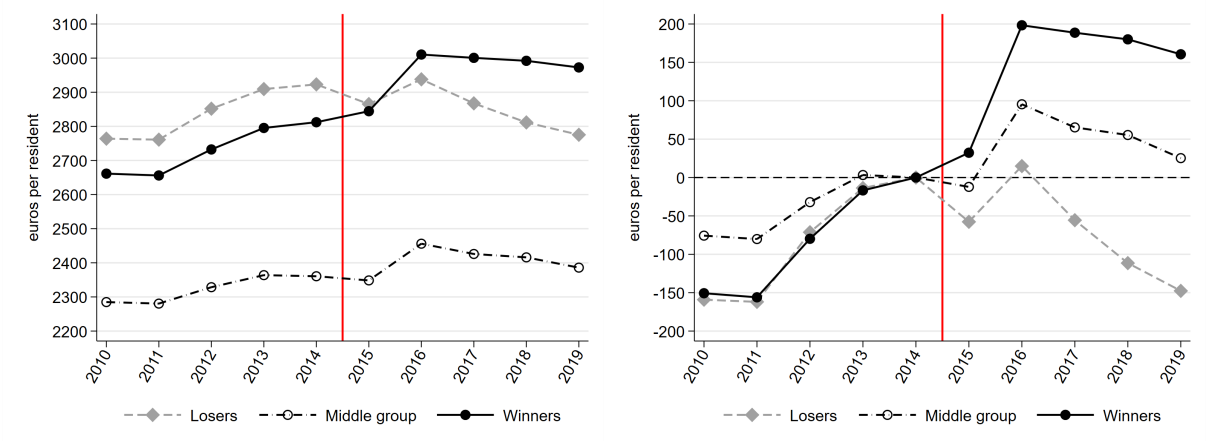
	N	mean	p25	p50	p75	min	max	sd
Transfers per resident (€) in 2014								
Winner municipalities	63	2812	2080	3064	3406	447	5315	1004
Loser municipalities	135	2923	2172	2885	3740	78	5447	1086
Imputed transfer change per resident (€)								
Winner municipalities	63	132	79	114	170	51	340	67
Loser municipalities	135	-172	-235	-136	-95	-592	-51	101

Notes: The table describes grants in winner municipalities that gained more than €50 per resident in the grant reform of 2015, and loser that lost more than €50 per resident. The top panel shows summary statistics for actual grants per resident in 2014. The bottom panel shows summary statistics for imputed grant changes due to the reform, keeping municipality characteristics constant at 2014 level.

Figure 2 (left panel) shows that the losers received somewhat higher grant revenue per resident than the winners until the reform. After the reform, the winners bypassed the

losers. The unaffected ("middle") group, which experienced grant changes of less than 50 euros per capita, had significantly lower grant revenue throughout the period. Moreover, the right panel of the figure shows that winner and loser municipalities were on the same growth trajectory until the reform. The middle group had a flatter pre-reform trend than the winner and losers, indicating that post-reform development of grant revenue in the middle group would not be a valid counterfactual for the other groups. These observations motivate our identification approach, which compares winners to losers, omitting unaffected municipalities.

Figure 2: Means of received central governments per resident



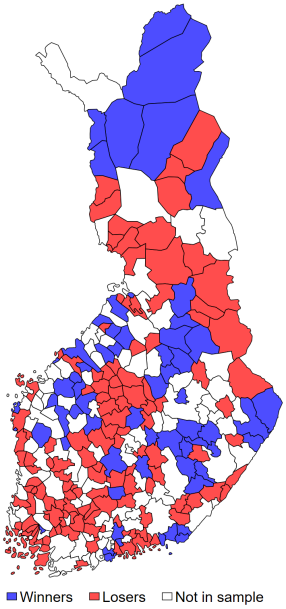
(a) Level **(b)** Relative to year 2014

Notes: The figure shows the development of central government grants in levels (left panel) and relative to year 2014 (right panel). Winner municipalities gained more than €50 per resident in the grant reform of 2015, loser municipalities lost more than €50 per resident, and the middle group gained or lost less than €50.

Finally, Figure 3 demonstrates that there is no clear spatial pattern in winning or losing grant revenue, except that winner (blue) and loser (red) municipalities tend to be clustered together. The municipalities that are not in the sample belong either to the middle group or are excluded due to having gone through a municipality merger during the panel[10].

[10]Municipality merger means that two or more independent municipalities join to become one municipal authority.

Figure 3: Winner and loser municipalities of the 2015 reform



Notes: The map shows in blue winner municipalities that gained more than €50 per resident in the grant reform of 2015, and in red loser municipalities that lost more than €50 per resident. White indicates the middle group that gained or lost less than €50 as well as some municipalities omitted due to mergers.

3 Data and Descriptive statistics

3.1 Data

We use detailed municipality-level data on local public finances, economic and demographic indicators, and domestic migration.[11] Data on central government transfers and their imputed changes due to the 2015 reform are provided by the Association of Finnish Cities and Municipalities (Kuntaliitto) and the Ministry of Finance Finland. All data are publicly available.

Our period of analysis spans the years 2010–2019. The transfer reform took place at the beginning of 2015, so we consider 2010–2014 as pre-reform and 2015–2019 as post-reform. We do not analyze later years, as the effects of the transfer reform would be difficult to disentangle from the COVID-19 shock, particularly because the central government temporarily raised grants during the pandemic.

3.2 Descriptive statistics

Table 3 shows pre-reform means (reference year 2014) of the key variables of interest (sections 1, 2 and 3 of the table) and also some other descriptive statistics (section 4

[11]The data are mainly provided by Statistics Finland. Other data sources are: National Land Survey of Finland, Finnish Tax Administration, and the Legal Register Centre. All the data can be freely downloaded from the web.

and 5 of the table). As before, we divide the sample into three groups according to the imputed grant change per capita.

In terms of municipal finances, winners and losers were very similar before the reform. On top of receiving similar amounts of grant revenue, they also set similar tax rates and spent similar amounts. The main difference between winners and losers is that losers' net long-term borrowing per resident was clearly lower than that of the winners in year 2014. However, net borrowing fluctuates a lot year-by-year. The group of unaffected municipalities spent significantly less than either winner or loser municipalities, and their finances relied more on tax revenues than grants.

With regard to labor market outcomes, winners and losers are still similar, but not to the same extent. The winner group had considerably more jobs per resident than the loser municipalities. This is apparent for both the private and public sector. However, the share of out-commuters (share of employed individuals working outside of their residence municipality) was much higher in the loser group. More frequent commuting to other municipalities in the loser group means that the employment rates between winners and losers hardly differed, despite the difference in the number of local jobs.[12] Compared to both winners and losers, unaffected municipalities had a higher employment rate and income per capita, contributing to their higher (lower) tax (grant) revenues.

With regard to internal migration, the average net migration rate is negative in all three groups, but least negative in the middle group, which includes more large cities than the other groups. This helps explain the more positive labor market and municipal finance situation in the middle group. Comparing winners to losers, we see that they have similar out-migration rates. However, winner municipalities have lower in-migration rates on average, resulting in a larger negative net migration rate.

The middle group of municipalities has younger and more highly educated population than other groups. Winners and loser municipalities tend to be more similar, though loser municipalities have slightly fewer working-age residents. The largest difference between winners and losers, relatively speaking, relates to the share of residents with a foreign native language (other than Finnish, Swedish, or Sámi languages). The significance of this component in the grant formula was considerably increased in the 2015 reform.

Cumulative population growth and cumulative natural population growth of the unaffected municipalities differed from the winners and the losers in pre-reform period (years 2010–2014). As the population density and the share of residents living in the built-up area suggest, this difference relates to unaffected municipalities including more large cities than the other groups. In these respects, the winner and loser municipalities are quite similar to each other.

[12]The difference in the share of out-commuters is one of the reasons why the winners won and the losers lost. The reform introduced a new component in the grant formula that awarded municipalities for having a high number of jobs per employed resident.

As the winners and losers were oppositely affected by the transfer reform, it is natural to see some differences in the socioeconomic features that correlate with the grant determinants. However, it is reassuring that these differences are modest, as it implies the outcomes of interest are more likely to develop similarly in the absence of the reform, which is what we find in Section 5.

Table 3: Means of dependent variables and other municipal characteristics in year 2014

	Winners	Losers	Middle
1. Municipal finances			
Central government transfers per resident (€)	2812	2923	2361
Municipal income tax rate (%)	20.72	20.31	20.59
General property tax rate (%)	1.01	0.91	1.01
Tax revenue per resident (€)	3138	3068	3270
Collected service fees per resident (€)	293	236	269
Operating expenses per resident (€)	6484	6324	6029
Annual contribution margin per resident (€)	97	96	89
Net long-term borrowing per resident (€)	74	29	93
2. Labor market outcomes			
Jobs per 1,000 residents	356	305	335
Private sector jobs per 1,000 residents	247	215	238
Municipal sector jobs per 1,000 residents	95	84	87
Employment rate of 20- to 64-year-old individuals (%)	67.5	68.4	69.6
Share of out-commuters (%)	31.4	42.3	39.2
Mean disposable cash income per resident (€)	19029	19334	19616
3. Migration			
In-migration rate (%)	4.18	4.34	4.50
Out-migration rate (%)	4.78	4.73	4.77
Net migration rate (%)	-0.60	-0.39	-0.27
Log of population	8.97	8.51	9.31
4. Socioeconomic characteristics			
Population share of individuals under 15 years old (%)	15.6	15.9	16.9
Population share of 15–64-year-old individuals (%)	59.5	58.4	60.4
Population share of individuals over 64 years old (%)	24.9	25.7	22.7
Share of individuals aged 15 or older with a tertiary degree (%)	21.2	21.3	23.5
Share of residents with foreign native language (%)	2.9	1.9	2.7
Share of households living in rental apartments	22.4	18.8	21.6
Cumulative population change in 2010–2014 (%)	-2.8	-2.7	-0.8
Cumulative natural population growth in 2010–2014 (%)	-1.4	-1.8	-0.3
Cumulative net inter-municipality migration in 2010–2014 (%)	-2.7	-1.7	-1.4
5. Geographical characteristics			
Log of population density	2.31	2.30	2.97
Log of land area	6.66	6.21	6.34
Mean latitude of municipality's centroid	63.25	62.29	62.41
Mean longitude of municipality's centroid	25.94	24.80	24.93
Share of residents living in the built-up area (%)	62.5	58.1	70.2

Notes: Winner municipalities are defined as those municipalities seeing an increase in imputed transfer revenue over €50 per capita while loser municipalities are those that lost over €50 per capita and the middle group lies in between.

4 Empirical strategy

4.1 Identification approach

To determine the effect of a change in grant revenue, we would ideally have a set of municipalities unaffected by the reform but otherwise identical to the affected municipalities. The reform of the municipal transfer system did leave a set of municipalities mostly unaffected. However, as the previous section demonstrated, these municipalities are different from winner and loser municipalities in several key aspects, most notably in their population density and demographic composition. These differences also show up in different trends and feed into many of the outcomes we are interested in.

Affected municipalities are divided into two groups - winners and losers - that are highly similar. As Figure 2 showed, these municipalities followed exactly the same trend in grant revenue before the reform. This similarity extends beyond grant revenue to many variables of interest, both in levels and in trends, as the next section will demonstrate. The pre-reform trends suggest that one group could have been a counterfactual for the other, had they not been treated.

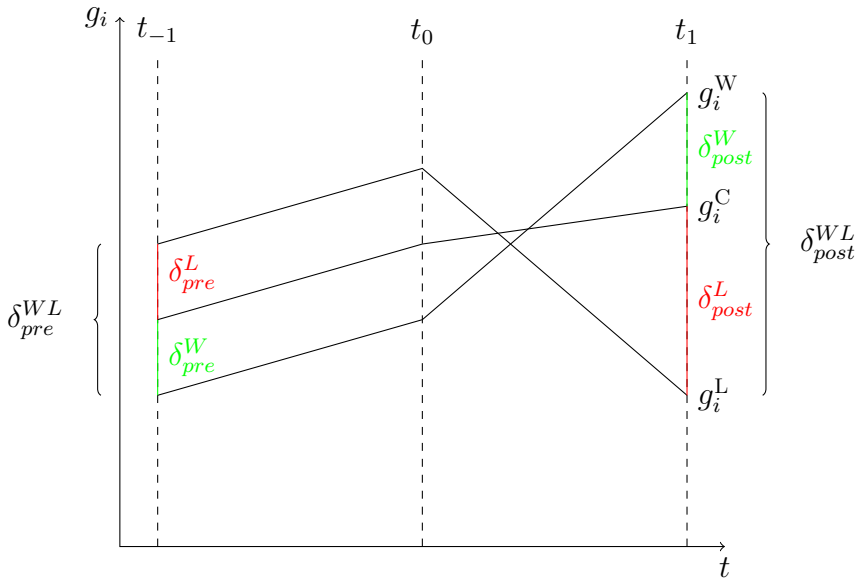
Since these municipalities are so similar, we argue that they would share the same counterfactual. In such a case, it is easy to show that comparing winners to losers produces an estimate that aggregates the average treatment effect on the winners and on the losers. This is illustrated in Figure 4, where we draw the evolution of average grant revenue for winners, losers and their shared counterfactual. In a difference-in-differences framework, the average treatment effect on the winners is identified by first taking the difference between the winners and the counterfactual in the post-reform year t_1, this is δ^W_{post}, and then subtracting the difference in the pre-reform year t_0, δ^W_{pre}. Similarly, the effect on the losers is the difference between δ^L_{post} and δ^L_{pre}.

In reality, we do not observe a counterfactual. However, if winners and losers share the same (unobserved) counterfactual, comparing them in a difference-in-differences framework provides an estimate equal to:

$$TE^{DiD} = \delta^{WL}_{post} - \delta^{WL}_{pre} \quad = (\delta^W_{post} - \delta^L_{post}) - (\delta^W_{pre} - \delta^L_{pre}) = (\delta^W_{post} - \delta^W_{pre}) - (\delta^L_{post} - \delta^L_{pre}),$$

which demonstrates that the effect we estimate is the difference between the treatment effect on the winners and the treatment effect on the losers. Since the effects tend to have opposite signs, this difference captures the aggregate effect of the reform.

Figure 4: Estimation of grant effect



Note: g_i^{W}, g_i^{L} and g_i^{C} represent respectively the evolution of average grants for the winners, losers and a shared counterfactual. δ^W_{pre} and δ^L_{pre} represent the winners' and losers' differences in grants relative to the counterfactual in the pre-reform period, while δ^W_{post} and δ^L_{post} and represent their post-reform differences. Finally, δ^{WL}_{post} and δ^{WL}_{pre} represent the post and pre-reform differences comparing winners to losers.

This effect can be interpreted as the total absolute change in outcome variables following the reform or the effect on the winners relative to the losers. While this estimate is not informative of how a given group of municipalities (winners or losers) responded to the reform, it does reveal whether the reform had local effects and the extent of those effects.

Formally, suppose there are two doses, d_w for winners and d_l for losers, then under no anticipation and parallel trends, we can interpret the difference in observed outcome paths as the difference in ATTs:[13]

$$\begin{aligned}\mathbb{E}[\Delta Y \mid D = d_w] - \mathbb{E}[\Delta Y \mid D = d_l] &= (\mathbb{E}[\Delta Y \mid D = d_w] - \mathbb{E}[\Delta Y \mid D = 0]) \\ &\quad - (\mathbb{E}[\Delta Y \mid D = d_l] - \mathbb{E}[\Delta Y \mid D = 0]) \\ &= ATT(d_w \mid d_w) - ATT(d_l \mid d_l)\end{aligned}$$

The last equation holds because of the standard parallel trends assumption, applied to both dose groups.[14] Specifically:

$$\mathbb{E}[\Delta Y(0) \mid D = d_w] = \mathbb{E}[\Delta Y \mid D = 0] = \mathbb{E}[\Delta Y(0) \mid D = d_l]$$

[13] See remark 3.1 in Callaway et al. (2024).

[14] See also A.1 and A.2 in the proofs of theorems 3.1 resp. 3.2 in appendix A.1 of Callaway et al. (2024).

In other words, we assume they have on average the same untreated potential outcome paths.

However, Callaway et al. (2024)[15] argue this difference in average ATTs for winners and losers mixes a causal effect with selection bias. As demonstrated below and applied to our case, this selection bias reflects a concern that winners may respond differently to the losing dose than losers:

$$\begin{aligned}
ATT(d_w \mid d_w) - ATT(d_l \mid d_l) &= \mathbb{E}[Y_{t=2}(d_w) - Y_{t=2}(0) \mid D = d_w] - \mathbb{E}[Y_{t=2}(d_l) - Y_{t=2}(0) \mid D = d_l] \\
&= \mathbb{E}[Y_{t=2}(d_w) - Y_{t=2}(d_l) \mid D = d_w] \\
&\quad + \mathbb{E}[Y_{t=2}(d_l) - Y_{t=2}(0) \mid D = d_w] - \mathbb{E}[Y_{t=2}(d_l) - Y_{t=2}(0) \mid D = d_l] \\
&= \underbrace{\mathbb{E}[Y_{t=2}(d_w) - Y_{t=2}(d_l) \mid D = d_w]}_{\text{causal effect}} + \underbrace{(ATT(d_l \mid d_w) - ATT(d_l \mid d_l))}_{\text{selection bias}}
\end{aligned}$$

The causal response reflects the effect of going from dose d_l to d_w for dose group d_w. To retain only this causal effect, we need to assume that dose group d_w would respond in the same way to dose d_l as dose group d_l does. This is what Callaway et al. (2024) refer to as the strong parallel trends assumption.[16]

In practice, we compare winners to losers across a wider range of doses and assume this assumption holds on average for winners and losers, once we remove the middle group of municipalities. Specifically, winners are defined as municipalities that received a net positive shock of at least 50 euros per capita, i.e. $D \in \mathcal{D}_w$, where $\mathcal{D}_w = \{D\colon D \geq 50\}$ and losers are defined as those that lost at least 50 euros per capita, i.e. $D \in \mathcal{D}_l$, where $\mathcal{D}_l = \{D\colon D \leq -50\}$.

Under the assumption that both groups share the same potential untreated outcome, the difference in average outcome paths can be interpreted as the difference in average ATT between winners and losers across the doses:

$$\begin{aligned}
\mathbb{E}[\Delta Y \mid D \in \mathcal{D}_w] - \mathbb{E}[\Delta Y \mid D \in \mathcal{D}_l] &= (\mathbb{E}[\Delta Y \mid D \in \mathcal{D}_w] - \mathbb{E}[\Delta Y(0) \mid D \in \mathcal{D}_w]) \\
&\quad - (\mathbb{E}[\Delta Y \mid D \in \mathcal{D}_l] - \mathbb{E}[\Delta Y(0) \mid D \in \mathcal{D}_l]) \\
&= \mathbb{E}[ATT(d \mid d) \mid D \in \mathcal{D}_w] - \mathbb{E}[ATT(d \mid d) \mid D \in \mathcal{D}_l] \\
&= ATT_w^{loc} - ATT_l^{loc},
\end{aligned}$$

where ATT^{loc} is notation borrowed from Callaway et al. (2024), representing an ag-

[15] See Theorem 3.2.

[16] According to the strong parallel trends assumption the average evolution of outcomes for the entire treatment population if they experienced dose d, is equal to the path of outcomes that dose group d actually experienced. For all $d \in \mathcal{D}$, $\mathbb{E}[\Delta Y(d) \mid D > 0] = \mathbb{E}[\Delta Y(d) \mid D = d]$. This further implies that $ATT(d \mid d) = (ATE(d)$

gregate level effect reflecting how the grant change affected our outcomes of interest on average.

It is important to reiterate that even when the average evolution of untreated potential outcomes is the same, comparison between these groups are biased when treatment effects for the same dose would vary between the two groups. Note that this does not imply we would expect a symmetric response between winning and losing, just that winners would on average respond the same way to losing as losers. We argue this is the case based on their similarity (not just in trends but in levels), which follows naturally from the main aim of the reform: to simplify the grant formula. The reform collapsed numerous collinear service-specific indicators into a smaller set of summary variables. To a large extent, the variation in imputed grant changes reflects idiosyncratic differences between the dropped parameters and the retained, summary parameter. These differences are unlikely to relate to unobserved trends that correlate with the outcomes of interest. Note also that the imputed grant changes are relatively small but large enough in absolute terms to be economically meaningful. Finally, by conditioning on the retained grant parameters, we can further relax the strong parallel trends assumption, following the suggestion by Callaway et al. (2024).

In sum, we estimate a relative treatment effect that needs to be interpreted accordingly, as the effect of gaining grant revenue relative to losing grant revenue. By assuming linearity, we can discuss our estimates in terms of euro per capita. In particular, we use the linearity assumption in Section 5 to calculate the cost per job or cost per migrant. However, the estimates we will present in Section 5 are purely the relative treatment effects of winners against losers, without assuming that the effect of marginal grant changes is linear. In Section 5.5, we formally adopt the linearity assumption to estimate euro per capita effects of the grant reform in a continuous difference-in-differences framework.

4.2 Estimation method

We compare winner and loser municipalities with dynamic difference-in-differences regressions. The winning group of municipalities are defined as those whose imputed grant change per resident, $\hat{g}_i$, exceeds €50 and loser municipalities are those that lost more than €50 per capita, as in equation 1.

$$winner_i = \begin{cases} 0, & \text{if } \hat{g}_i < \text{€-50} \\ 1, & \text{if } \hat{g}_i > \text{€50} \end{cases} \tag{1}$$

To identify the effect of winning grant revenue relative to losing grant revenue, we estimate the following equation:

$$Y_{it} = \sum_{t \neq 2014} \beta_t * winner_i * D_{it} + \delta_t + \eta_i + \epsilon_{it}, \tag{2}$$

where Y_{it} represents the dependent variable of interest for municipality i in year t. The winner dummy is interacted with year dummies D_{it}, and the coefficients of interest are the DiD estimates β_t. They are estimates of the relative treatment effect, the difference in the dependent variable between the winner and loser municipalities relative to the omitted base year 2014. δ_t are year fixed effects and η_i municipality fixed effects. The standard errors are clustered at the municipality-level.

As with all differences-in-differences settings, we assume that in the absence of the reform, the values of the dependent variables in the winner and loser municipalities would have developed similarly. We find support for this parallel trend assumption in the pre-reform estimates of β_t.

In addition to yearly estimates β_t, we present DiD estimates that combine estimates for years 2018 and 2019 to get a more precise estimate of the impact of grant shocks after the transition period has ended.

When we examine effects on migration patterns, we try to account for the bilateral nature of migration, as a robustness check. Following Borusyak et al. (2022), we control for the average imputed grant change in other municipalities, weighted by migration intensity. This method is explained in more detail in Section C.5.

Finally, since the grant reform affected municipalities to varying extents, it is natural to consider a continuous difference-in-differences approach. In Section 5.5, we present estimates from a two-way fixed effects regression. Because the observed change in grants is endogenous to changes in the socioeconomic features of a municipality, we use imputed grant changes and estimate the following equation:

$$Y_{it} = \sum_{t \neq 2014} \beta_t^c * \hat{g}_i + \delta_t + \eta_i + \epsilon_{it}, \tag{3}$$

where δ_t represents time fixed effects, η_i are unit fixed effects, and $\hat{g}_i$ is the imputed grant change of municipality i in the 2015 reform. In the regression, we scale the grant change by dividing it by 100. The interpretation of the coefficient β_t^c is therefore that a €100 increase in grants in the 2015 reform changes the dependent variable by β_t^c in year t.

Under the assumptions of strong parallel trends *and* linearity, this two-way fixed effects estimate can be interpreted as a global average causal response to a marginal increase in dose ($ACRT(d)$). However, Callaway et al. (2024) demonstrate that without strong parallel trends, β_t^c include selection bias, while the assumption of any other functional form introduces weighting issues such that β_t^c reflect a non-intuitive aggregation of treatment effects across doses. We therefore urge readers to take these estimates with a grain of salt. We use the binary treatment specification as our main approach because

the estimates have a meaningful interpretation even without the linearity assumption.

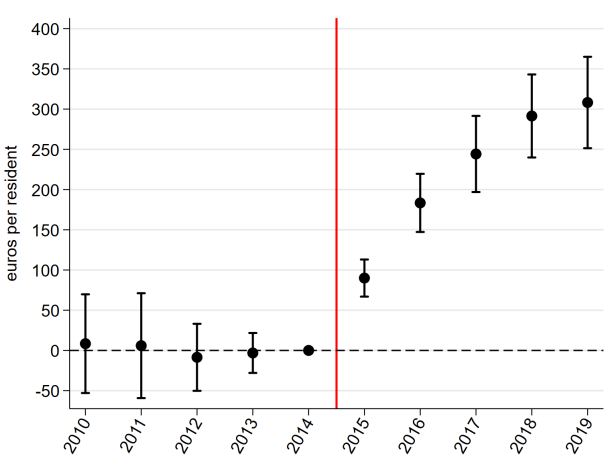
5 Results

5.1 Central government transfers received

In Figure 5, we analyze the evolution of actual central government transfers received by winner and loser municipalities. In the left panel, we plot the estimates relative to the omitted base year 2014, for both the loser and winner municipalities. Municipality fixed effects are included as controls. In the right panel, we show the estimates for the difference between the winner and loser municipalities relative to year 2014, that is, the interaction terms between the winner dummy and year dummies in Equation 2. Municipality and year fixed effects are included as controls. The figures show the estimated coefficients and their 95% confidence intervals. We present similar event-study-like graphs for all outcome variables. All monetary outcome variables are adjusted for inflation and measured in year 2014 euros.

Before the reform, central government transfers to winners and losers increased at the same pace (left panel). The general trend reflects increasing demand for public services, mainly driven by an aging population. Immediately after the reform, the grants received by the winners increased compared to the losers (right panel). This difference widened gradually because the reform was implemented with a transition period. From 2018 to 2019 there was only a minor change, which indicates that the reform had largely taken full effect on actual grants by 2018. In 2019, the difference between the transfers received by the winners and losers was slightly over €300 per resident, on average. Note that the mean imputed grant change in the reform was €132 per resident for the winners and -€172 per resident for the losers. The difference in imputed grant changes, €304, is therefore very close to the DiD estimate for year 2019. Before the reform, in 2014, the mean of transfers received was about €2,800–2,900 per resident in both groups, which means that a €300 difference is relatively large.

Figure 5: Central government transfers per resident: winner municipalities compared to loser municipalities



5.2 Municipality responses

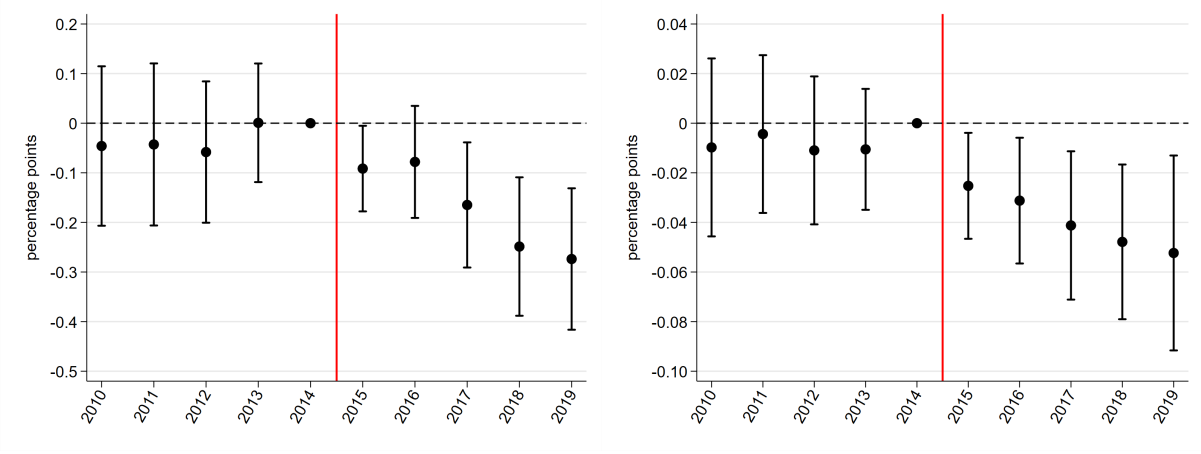
In Figures 6-8, we analyze how municipalities adjusted their fiscal policy in response to changes in central government transfers.

Figure 6a shows that municipal income tax rates of winners and losers followed the same trend before the reform. After 2014, tax rates in winning municipalities increased at a slower rate than those of losers. In 2018 and 2019, the municipal income tax rates of the winners had decreased by about 0.3 percentage points compared to the losers. The difference is statistically significant, but relatively small given the pre-reform mean of about 20.7% (in the winner group). The median taxable income (after deductions) was 23,071 euros in 2014.[17] Thus, a 0.3 percentage point lower tax rate implies a 69-euro increase in disposable income. This median income, however, is conditional on having taxable income and not everyone has it. So, the 69-euro increase in taxable income would concern the median taxpayer, not the median resident.

In Figure 6b, the general property tax rates follow qualitatively the same development as the income tax rates. The general property tax is levied on land and most non-residential buildings. The property tax rates of winners and losers developed similarly until 2014, but started to diverge in 2015, when the grant reform was enacted. In 2019, the general property tax rates of the winners had decreased by about 0.06 percentage points compared to the losers. This is quite sizable an effect, as the pre-reform mean is about 1%.

[17]Source: Statistics on income taxation, Finnish Tax Administration.

Figure 6: Tax rates: winner municipalities compared to loser municipalities



(a) Income tax rate **(b)** General property tax rate

Given the divergence of winners' and losers' tax rates, it is unsurprising that also total tax revenues diverged after the reform (Figure 7a). The tax revenues per resident in the winner group decreased by some €50–100 compared to the losers after the reform. The difference in tax revenues stops growing after 2017, potentially due to tax rates stabilizing and changes in tax bases, which we explore in the following subsections. The decrease in tax revenue was driven by income tax revenue (see Figure C.1 in the Appendix) indicating that residents rather than firms were the main direct beneficiaries of the tax cuts.

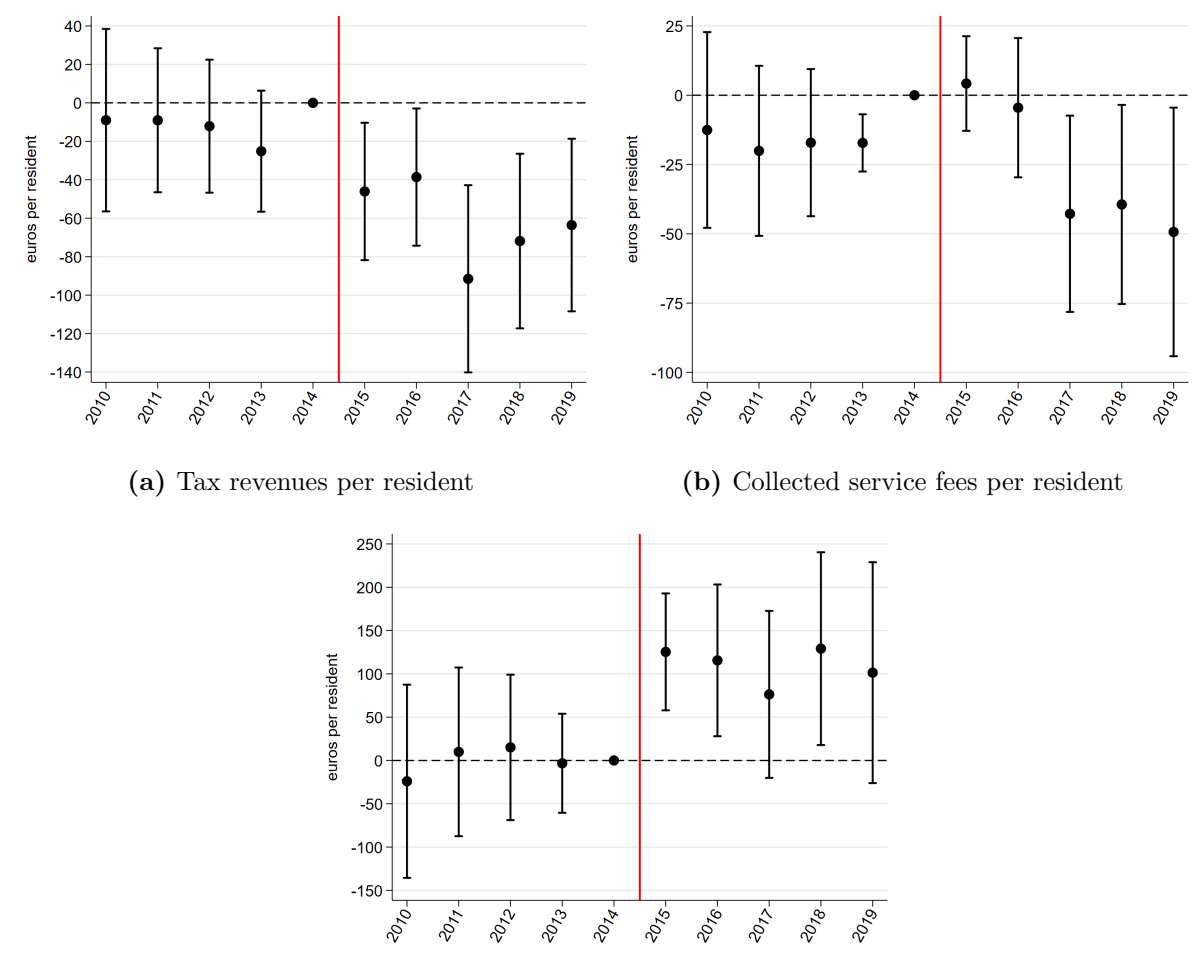
Next, we look at service fees. Most municipal services are free of charge for residents, but some service fees are collected, e.g., for health care services, daycare for children, garbage collection and the issuance of building permits. These fees are relatively unimportant in terms of revenue and they cover only a small share of the costs of the services, but along with local tax rates the service fees are a source of own revenue, which the municipality can easily adjust. Based on Figure 7b, the service fees collected by the winners decreased by some €40–60 per resident compared to the losers after the reform. The pre-reform mean of service fees per resident was €236 in the loser group and €293 in the winner group. Therefore, the decrease in the service fee revenues of the winners is relatively large. There was a small but significant change in the difference between the groups in the year prior to the reform, but otherwise the pre-reform trends were similar.

Based on the estimates shown in Figures 7a and 7b, the tax revenues and the service fee collections of the winner municipalities decreased by approximately €100–150 per resident, after the reform, compared to the losers. The central government transfers of the winners increased by about €300 per resident compared to losers, on average. This would imply that up to half of the grant shock was channeled to private consumption through lower taxes (mainly income taxes) and fees collected from residents and firms.

The effect of the reform on municipal spending is shown in Figure 7c. Operational expenses are net of sales revenue, which municipalities earn by providing services to

other municipalities or to public and private entities. These services are typically sold at production cost, meaning sales revenues match expenses not directed to local residents. The difference between the winners and losers was very stable until the reform, but after the reform, the winners' expenses increased by roughly €100 per resident compared to the losers. The difference remained quite stable over time, although the confidence intervals are relatively wide. Figure C.2 in the Appendix shows that the increase in spending in the first two postreform years was driven by health and social services. However, that effect faded away, but the increase in the spending on services related to education, daycare, culture and recreation was more sustained.

Figure 7: Revenues and spending: winner municipalities compared to loser municipalities



(a) Tax revenues per resident

(b) Collected service fees per resident

(c) Total operational spending per resident

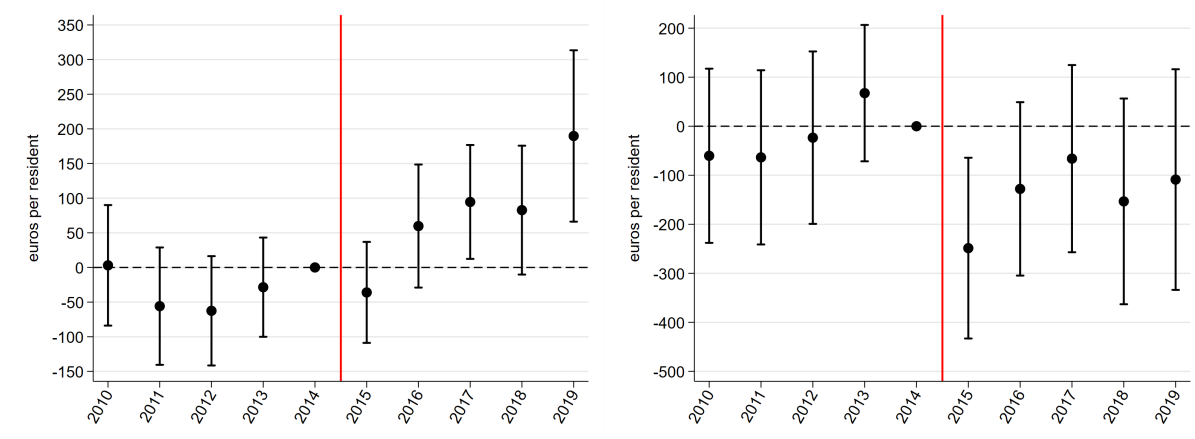
In addition to increased spending and reduced revenues, the winner municipalities seem to have used part of the increase in government transfers to run surpluses (or to cut deficits). In Figure 8a, the annual contribution margin[18] of the winners increased by

[18]The annual contribution margin measures the difference between operational expenses and municipal revenues (service fees and other operational revenues, tax revenue and government transfers). The variable is calculated before depreciation, amortization and extraordinary items. There are no exact

approximately €100 per resident compared to the losers after the reform. In 2019, the difference increased further but this may have been caused partly by one-off changes in a rather volatile measure. Nevertheless, there is a clear improvement in the budget balance of winners compared to losers. Better financial health may improve the attractiveness of the municipality by, e.g., reducing need for future tax increases or facilitating investment.

Finnish municipalities can borrow to finance their investments or meet acute revenue needs. Figure 8b shows the development of net long-term borrowing (issuance of new long-term debt less repayment of old ones). Before the reform, there were no significant differences in borrowing patterns between winners and losers, although confidence intervals are relatively wide. Nevertheless, following the reform, the net borrowing of the winners decreases significantly compared to the losers, by about €100 to €200 per resident. This follows directly from the positive effect on the annual contribution margin (Figure 8a), which reduces the need for borrowing. Borrowing is a fast means of responding to income shocks, and might be favored especially during negative shocks to avoid too sudden spending cuts or tax increases.

Figure 8: Budget balance: winner municipalities compared to loser municipalities



(a) Annual contribution margin per resident

(b) Net long-term borrowing per resident

In Table 4, we summarize the results of this subsection by presenting the means of the DiD estimates for years 2018 and 2019. To respond to the transfer reform, municipalities employed a wide range of policy levers, adjusting revenue collection, spending and borrowing in roughly equal measures. Overall, the effect of the transfer reform on the winner municipalities compared to the losers is economically significant in each of these categories, at over a €100 per resident. In relative terms, we estimated the strongest effect on services fees (roughly 17 % reduction) and local property taxation (roughly 5%

regulations on how large surpluses municipalities should run, but in principle, they should be high enough to cover depreciation and investment in the long run, so as to prevent municipalities from continuously accumulating debt. In practice, however, contribution margins are often quite low, or even negative such that debt is used to finance consumption.

reduction).[19] Next, we examine how economic activity changed in response to the local policy changes triggered by the grant reform.

Table 4: Pooled effect (years 2018-2019) on municipal finances: winner municipalities compared to loser municipalities

	Coefficient	Standard error	Pre-reform mean
Central government transfers received (€/resident)	300	27	2888
Income tax rate (p.p.)	-0.26	0.07	20.44
General property tax rate (p.p.)	-0.050	0.017	0.94
Tax revenue (€/resident)	-68	22	3090
Collected service fees (€/resident)	-44	19	254
Total operational spending (€/resident)	115	58	6375
Annual contribution margin (€/resident)	136	49	96
Net long-term borrowing (€/resident)	-131	97	44

Notes: This table reports the equally weighted linear combination of the DiD estimates for years 2018 and 2019 that were estimated in Figures 5-8b. Each regression has 1980 observations: a ten-year balanced panel of 198 municipalities. 63 municipalities belong to the winner group and 135 belong to the loser group. All the regression include municipality and year fixed effects. Standard errors are clustered at the municipality level.

5.3 Labor market

The previous section showed that municipalities responded to the transfer reform by adjusting both revenue collection and spending. These changes may affect consumption as well as labor demand. As argued by e.g. (Serrato and Wingender, 2014), public spending may also affect the local labor supply as it can attract new residents. This channel is examined in the next section on migration.

Figure 9a shows the development of the number of jobs per 1,000 residents. The trends in the winner and loser groups were close to parallel until the reform, but then diverged, with the number of jobs in the winner group clearly increasing compared to the loser group. In 2019, the number of jobs per 1,000 residents had increased by about 9 in the winner municipalities compared to the losers. The pre-reform mean of jobs in loser and winner municipalities was 305 and 356, respectively. The effect is therefore relatively modest but not negligible.

Given the estimated difference in grant change of roughly €300 per resident, a back-of-the-envelope calculation suggests a cost-per-job of around $€300,000/9 = 33,000$. This corresponds closely to the estimate reported by Serrato and Wingender (2016), $30,000, but falls below the preferred estimate of $50,000 reported in Chodorow-Reich (2019)'s review of spending multipliers. Interestingly, Räsänen and Mäkelä (2021) estimate a cost-per-job of €55,000, using exogenous regional variation in Finnish defense and immigration

[19]Note that the effect on net borrowing and contribution margin cannot be related to their means, because these variables can take negative values.

related spending. Potentially, our lower cost estimate is due to funding being directed to its best use, not just through varying changes in public spending but also through tax cuts.

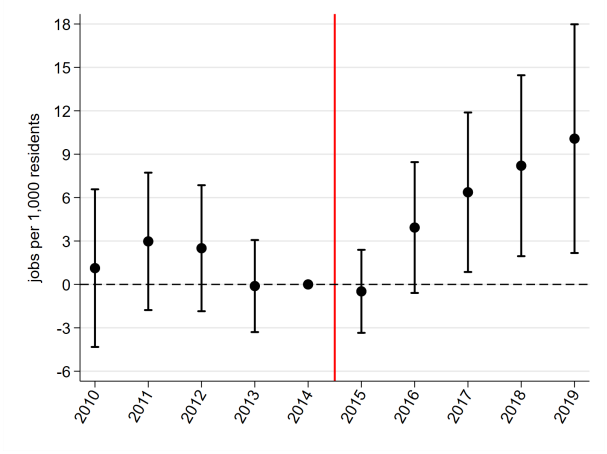
In Figures 9b and 9c we show that the positive effect on jobs can almost entirely be explained by an increase in private sector jobs. The number of private sector jobs per 1,000 residents had increased by about 9 in 2019 in the winner group compared to the losers (Figure 9b). The mean of the variable in 2014 was 215 in the loser group and 247 in the winner group, so the relative increase is about 4%. On the other hand, the number of municipal sector jobs (see Figure 9c) did not develop differently after the reform[20]. This contrasts sharply with the findings of Räsänen and Mäkelä (2021), who report employment effects only in the public sector.

The strong effect on the number of private sector jobs may seem surprising as the reform did not generate strong direct incentives for firms to move to winner municipalities, other than a small change in property taxes. The likely mechanisms behind the increase in private sector jobs are: 1) There was a local demand boost through increased private demand due to tax cuts and fee cuts. The fact that the grant changes were permanent likely contributed towards tax cuts and fee cuts increasing consumption rather than saving. 2) The increase in municipal operating expenditure increased the demand for private firms through procurement from the private sector.[21] 3) The improvement in annual contribution margin can be used for investment, which also increases demand for private firms.

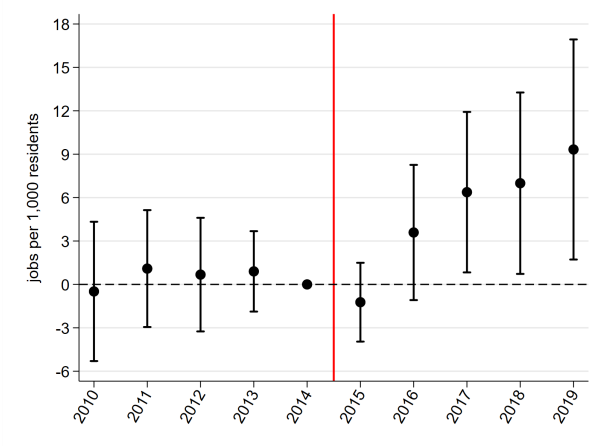
[20]Municipal sector jobs are part of the public sector. Rest of public sector jobs are state-employed but we exclude them, because it is unlikely that municipality-level economic shocks would affect them, and the state is a relatively small employer with jobs heavily concentrated in the capital.

[21]In 2015, roughly 30% (€13 bn) of operating expenses went to private firms, and €3.9 bn of municipal investment spending went to private firms. At the margin, the share of spending going to private firms is likely even larger. These numbers suggest that increases in municipal operational spending and contribution margin boost demand for private firms significantly.

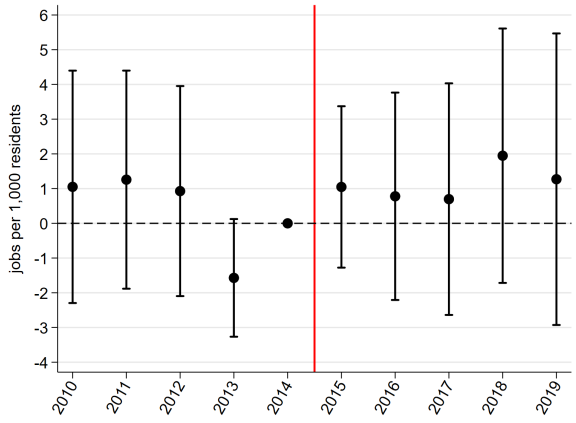
Figure 9: Number of jobs: winner municipalities compared to loser municipalities



(a) Total number of jobs per 1,000 residents



(b) Private sector jobs per 1,000 residents



(c) Municipal sector jobs per 1,000 residents

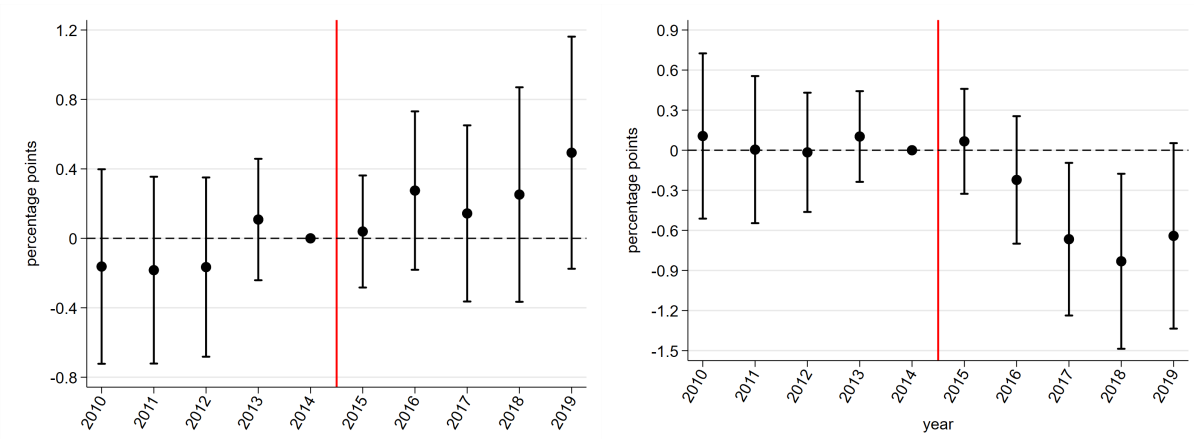
To determine the extent to which this increase in the number of jobs supported the employment of local residents, we next examine the impact on the employment rate, defined as the ratio of working-age employed persons to the working-age population (ages 20–64). Figure 10a shows that there is no clear effect on the employment rate, although there might be a small increase in 2019. Since the pre-reform employment rate averaged roughly 68%, the largest point estimate of around 0.4 percentage points would be relatively small.

The post-reform increase in jobs without a simultaneous increase in employment rate can only be reconciled through increased net-migration, or a change in commuting patterns. In Figure 10b, we therefore estimate the effect on the share of a municipality's employed population working outside of the municipality of residence. We find that this share decreased in the winner group compared to the loser group by about 0.5–1 percentage points after the reform. The pre-reform mean in winner (loser) municipalities was 31% (42%). The next section analyzes migration responses.

Finally, Figure C.3 shows the estimated effects on real disposable income per resident

(in 2014 euros). Disposable income includes earned income, entrepreneurial income, capital income and net transfers. The point estimate is positive in 2015, but other post-reform coefficients are close to zero. A potential explanation for a weak effect on disposable income is that reductions in service fees and property taxes do not affect disposable monetary income. However, the confidence bands are wide, and we can not rule out economically significant effects on disposable income.

Figure 10: Employment, commuting and incomes: winner municipalities compared to loser municipalities



(a) Employment rate

(b) Share of out-commuters

Table 5 summarizes the main findings of this subsection. While we find a significant increase in the number of jobs, mostly in the private sector, the employment rate remained unaffected. This seems to be due to more residents taking up work in their home municipality rather than commuting out. While residents did not see a clear increase in income, it is likely that they saved commuting time.

Table 5: Pooled effect (years 2018-2019) on labor market outcomes: winner municipalities compared to loser municipalities

	Coefficient	Standard error	Pre-reform mean
Jobs per 1,000 residents	9.14	3.49	321
Private sector jobs per 1,000 residents	8.16	3.40	226
Municipal sector jobs per 1,000 residents	1.61	1.90	88
Employment rate (%)	0.37	0.31	68.1
Share of out-commuters (%)	-0.74	0.33	38.9
Mean disposable cash income per resident (€)	-21	74	19237

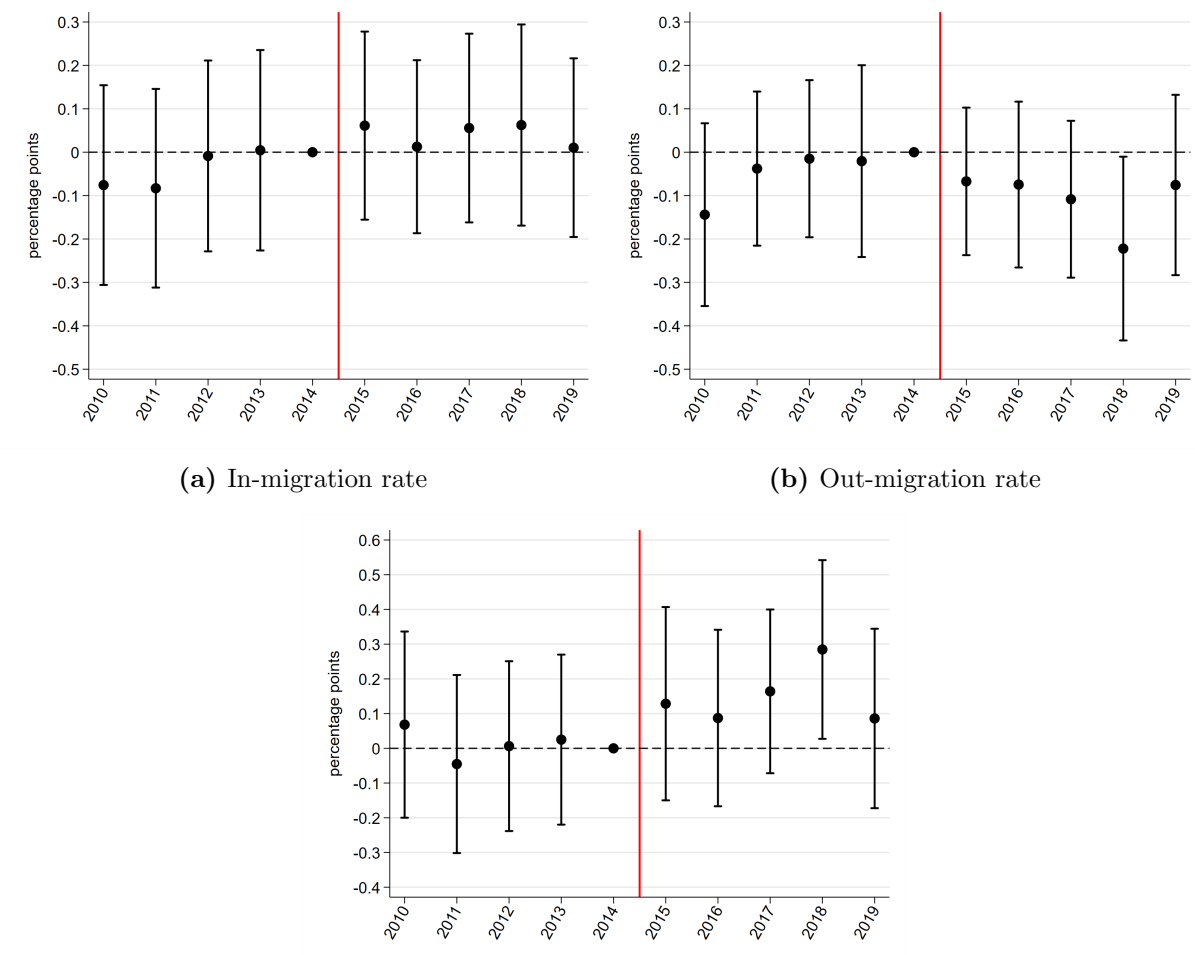
Notes: This table reports the equally weighted linear combination of the DiD estimates for years 2018 and 2019 that were estimated in Figures 9a-C.3. Each regression has 1980 observations: a ten-year balanced panel of 198 municipalities. 63 municipalities belong to the winner group and 135 belong to the loser group. All the regression include municipality and year fixed effects. Standard errors are clustered at the municipality level.

5.4 Migration

The tax cuts and spending increases as well as the creation of jobs can help municipalities attract new residents or prevent existing residents from moving out.

Figures 11a-11c show dynamic DiD estimates for the in-migration, out-migration and net migration rate. Overall, pre-reform trends for winners and losers are very similar. After the reform, DiD estimates for in-migration (Figure 11a) are positive but close to zero and insignificant. For out-migration (Figure 11b), the estimates are negative after the reform, but only the estimate for 2018 differs significantly from zero. The estimated 0.2 percentage point decrease in out-migration rate in 2018, corresponds to a roughly 4% decrease in relative terms. The post-reform estimates for net migration rate (Figure 11c) are positive, but only the estimate for 2018 is statistically significant.

Figure 11: Migration rates: winner municipalities compared to loser municipalities



(a) In-migration rate

(b) Out-migration rate

(c) Net migration rate

In Appendix Figure C.7, we repeat the analysis on migration responses using the log of the number of movers as the outcome. The effect on net migration is estimated using the log of the ratio of in-migration to out-migration, because net migration can

take negative values. The results are similar to the those obtained with migration rates, but there are no statistically significant point estimates when we use logged migration. Finally, we estimate the effect on total population in Figure C.4 and find a zero effect. The widest 95% confidence interval suggests a relative population change between -1.5% and 1.5% after five years. While this is not a precise zero, there is very little evidence of an effect on population.

Table 6 summarizes our estimates of the impact of transfers on migration and population. The positive pooled estimate for the net migration rate (0.185 percentage points) aligns with winners attracting migrants compared to losers, although the confidence band includes zero. To assess the economic significance of the net migration estimate, we calculate the implied cost of one migrant. A 0.185 percentage point increase in the net-migration rate corresponds to 1.85 new residents per 1,000 residents. Since winners gained €300 in grants per resident compared to losers, this implies that the cost of attracting one additional migrant per year is very high, over €160,000. Our estimates suggest the reform had little if any impact on migration patterns. This finding is robust to different ways of looking at migration.

Table 6: Pooled effect (years 2018-2019) on migration: winner municipalities compared to loser municipalities

	Coefficient	Standard error	Pre-reform mean
In-migration rate (%)	0.037	0.101	4.29
Out-migration rate (%)	-0.149	0.089	4.74
Net migration rate (%)	0.185	0.113	-0.46
Log of in-migration	-0.002	0.026	5.47
Log of out-migration	-0.026	0.020	5.59
Log of in-migration/out-migration	0.025	0.025	-0.12
Log of population	-0.000	0.007	8.66

Notes: This table reports the equally weighted linear combination of the DiD estimates for years 2018 and 2019 that were estimated in Figures 11a-C.4 and C.7. Each regression has 1980 observations: a ten-year balanced panel of 198 municipalities. 63 municipalities belong to the winner group and 135 belong to the loser group. All the regression include municipality and year fixed effects. Standard errors are clustered at the municipality level.

Accounting for bilateral nature of migration

In Appendix C.5 we present a version of the model that tries to account for the bilateral nature of migration. Moving decisions are influenced not just by changes in the municipality of origin but also by changes in the set of potential destinations. Not only did the transfer reform affect many municipalities simultaneously, but its impact

was also spatially correlated, as Figure 3 shows. This spatial correlation is relevant as households are more likely to move to nearby municipalities than to distant ones. Recent work by Borusyak et al. (2022) demonstrates that the correlated nature of most economic shocks, if ignored, may lead to underestimation of migration responses to those shocks. Following their work, we control for the average imputed grant change in other municipalities, weighted by a measure of migration intensity, which we interact with time dummies. The findings reported in Figure C.6 are very similar to those in Figure 11. For completeness, Figure C.8 in Appendix C.6 shows the results using logs of migration and controlling for transfer changes in other municipalities. These results are very similar to those in Figure C.7, where controls are not used.

The role of housing supply

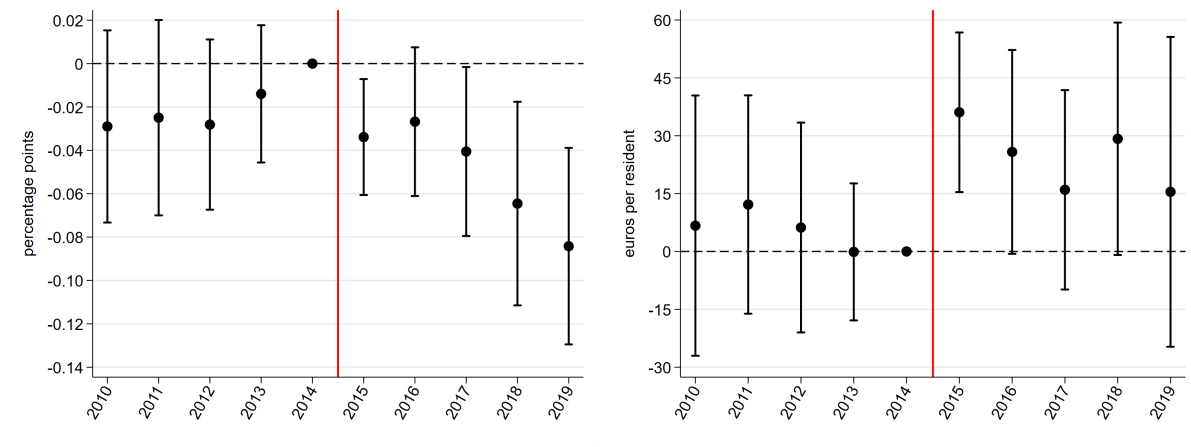
Restrictions on adjustments of housing supply could be a potential explanation for muted migration responses. If housing supply is inelastic, increased housing demand due to lower taxes and fees, and improved services in the winner municipalities, would push up housing prices without supply responding to accommodate new migrants.[22] However, data by Statistics Finland on vacancy rates in 2017 suggests that housing supply is not an important impediment to mobility in our data. The median share of housing units that were not permanently occupied was 14.9% in the winner group and 14.7% in the loser group. In Figure C.5, we use mean apartment price per square meter and median prices of single family homes (for a subset of municipalities with at least 10 transactions in each year) as additional outcomes and do not find clear evidence of strong capitalization of the grant shock.

5.5 Continuous treatment

In the main specifications, we compared the development of municipalities that received additional central government financing of more than €50 per resident to those that lost more than €50 in a binary fashion. Using the same sample, we now estimate a continuous treatment two-way fixed effects parameter, based on Equation 3. We apply this approach to our main outcomes; Figure 12 presents estimates for local fiscal policy (income tax rate and operational spending), Figure 13 for the labor market (number of jobs and share of out-commuters) and Figure 14 for migration rates. Our findings remain robust to a continuous treatment specification. While there is a clear effect on fiscal policy, jobs and commuting patterns, the evidence for changes in migration patterns is weak.

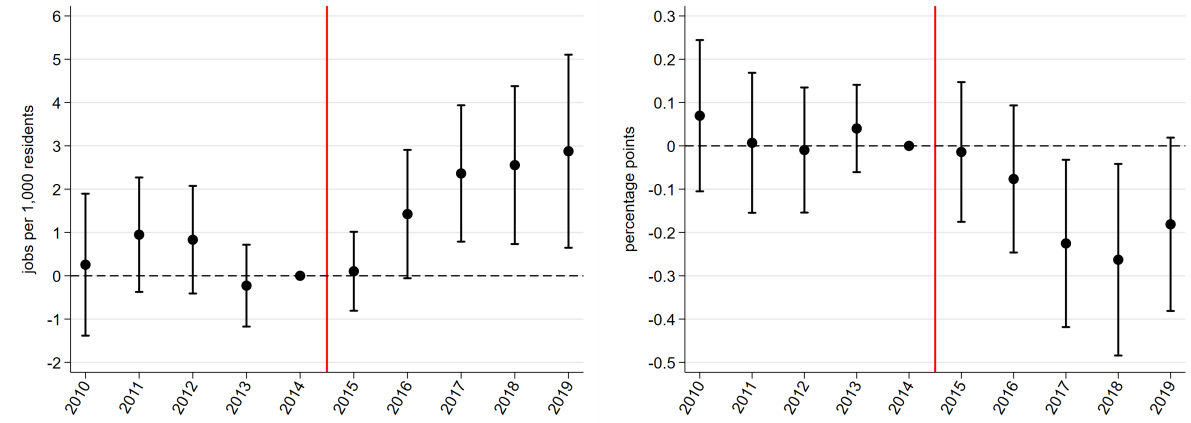
[22] Hilber et al. (2011) find evidence of stronger capitalization of central government grants into housing prices in more supply-constrained areas.

Figure 12: Municipal finances: effect of an imputed €100 increase in transfers per resident



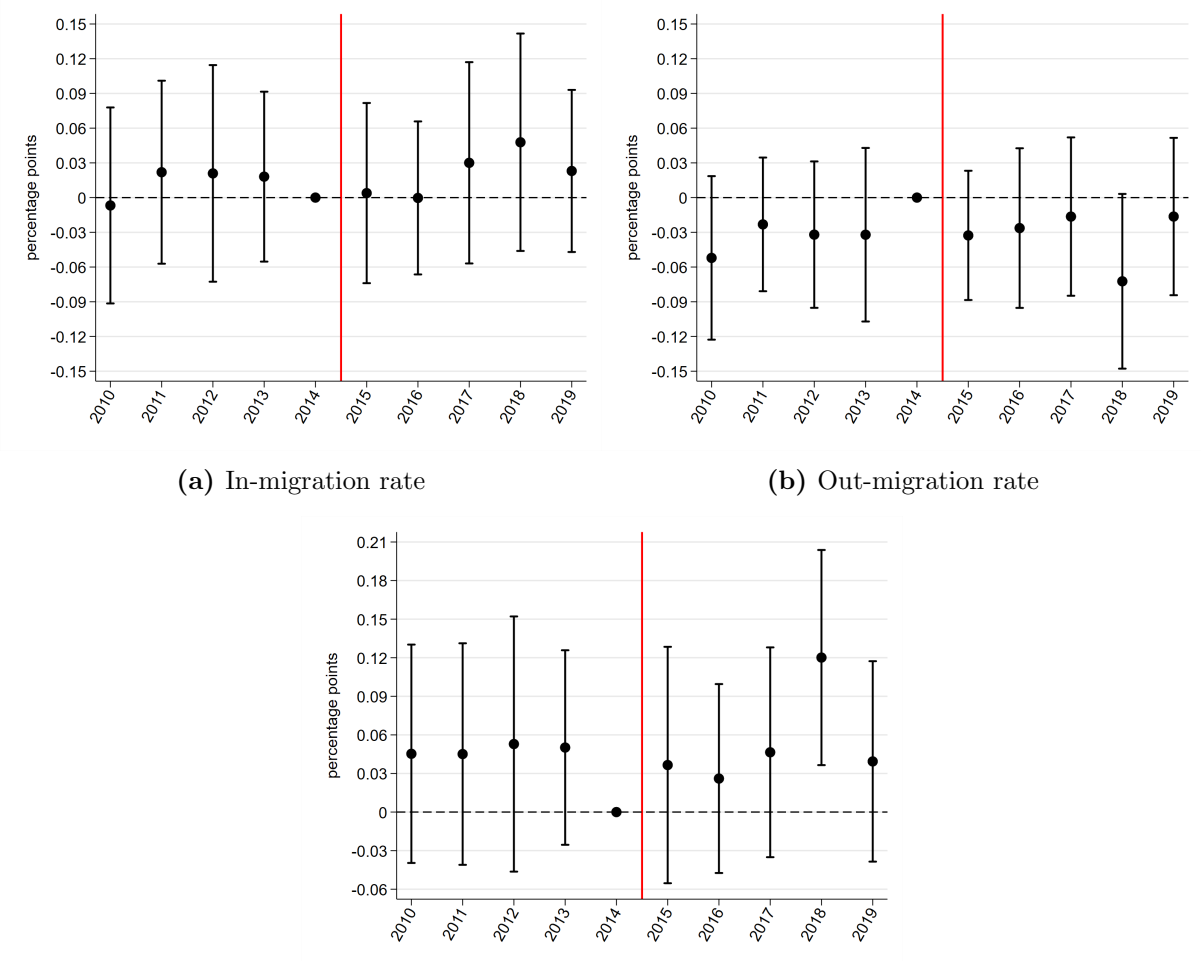
(a) Income tax rate **(b)** Total operational spending per resident

Figure 13: Labor market: effect of an imputed €100 increase in transfers per resident



(a) Number of jobs per 1,000 residents **(b)** Share of out-commuters

Figure 14: Migration rates: effect of an imputed €100 increase in transfers per resident



(a) In-migration rate

(b) Out-migration rate

(c) Net migration rate

6 Conclusions

The 2015 Finnish reform of central government transfers to municipalities had significant effects on local public finance. In response to the revenue shock, municipalities adjusted a wide range of policy levers. Relative to those that lost revenue, municipalities which gained grants lowered local income and property taxes as well as service fees, raised public spending and reduced net borrowing. Each of these adjustment channels represented about one third of the additional €300 per resident that winners of the reform received relative to the losers.

These local policy changes likely had real economic effects, either by boosting local (private) consumption or increasing labor demand. Indeed, we find that the number of jobs in winner municipalities increased compared to loser municipalities. Importantly, these were private sector jobs, suggesting that the effect worked through local fiscal multipliers. A back-of-the-envelope calculation suggests a cost-per-job of €33,000, closely

aligned with recent estimates in the spending multiplier literature, see e.g. Serrato and Wingender (2016) for the U.S. and Gabriel et al. (2023) for the Eurozone. It bears reminding however that spending changes represented only one third of the fiscal adjustments of the reform.

Although the number of local jobs increased, employment rates remained largely unaffected. Instead, the job growth coincided with a reduction in commuting to other municipalities. This suggests that, beyond the direct benefits of reduced taxes, lower fees and increased public spending, the residents of winner municipalities also benefited in terms of reduced commuting times.

Despite these changes in fiscal policy and job creation, our results suggest that the reform's effects on domestic migration were limited. This may be due to the lack of a clear effect on disposable income, implying only weak incentives to move apart from the direct benefits generated by fiscal policy changes. Additionally, some fiscal policy changes may not have been very salient to potential migrants. Comparing the quality and cost of public services across municipalities is challenging, and the benefits from reduced borrowing may only materialize in the future. Nevertheless, the absence of a clear effect on migration suggests that the efficiency effects of the grant reform are relatively small.

We find that national spending in the form of a block grant to local government generates jobs to a similar extent as explicit national spending relating e.g. to defense or highways. While other studies have used grant shocks to identify local spending multipliers, they have either examined cases where local government is entirely dependent on grants or ignored endogenous changes in local fiscal policy.

Although our study speaks more to the local policy response to a revenue than a spending shock, it demonstrates that the effects of local policy choice cannot be overlooked. Not only do we find a response in local income taxation, we also find changes in property taxation and fees - more common ways of financing local governments across the world. Alongside changes in spending, these many fiscal adjustments contribute to a strong job creation effect. In fact, previous work estimating fiscal multipliers in Finland, using regional variation in national defense and immigration spending, has found cost-per-job estimates that are significantly higher at €55,000 (Räsänen and Mäkelä, 2021).

As suggested by Chodorow-Reich et al. (2012), the fungibility of grants can be a main reason for finding relatively low cost-per-job estimates, as it allows government to direct the funds to their best use. This aligns with Bartik (2020)'s argument to use flexible block grants to help distressed areas as opposed to more programmatic place-based policies.

However, two concerns merit further attention in this debate. The first relates to moral hazard. An extensive literature has studied the design of transfer systems in relation to revenue collection, grant dependency, bailout-expectations, etc. (see e.g. Lago et al., 2024). The design of the Finnish transfer system based on tax capacity rather than

tax revenue and spending needs rather than actual spending, helps mitigate these risks.

The second concern relates to the extent to which the distressed benefit from the expansion of grant revenue. The current place-based policy literature places great emphasis on the social costs of non-employment and therefore the importance of fighting joblessness (Austin et al., 2018; Bartik, 2020). Although we find the number of jobs grow, we measure no clear effect on the employment rate. Nevertheless, our evidence does suggest the benefits go towards local residents. The finding that the migration response to the tax and spend changes are minimal at best, suggests also that from a redistributive perspective, there is potential for spatially targeted transfers to improve on place-blind policies (Gaubert et al., 2021).

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Online Appendix

A Local fiscal Multiplier

Subject to certain conditions, Chodorow-Reich (2019) argues that cross-sectional multipliers can be considered a lower bound for national multiplier estimates in a zero lower bound setting, i.e. when monetary policy is accommodating of fiscal stimulus. Restricting the set of papers in his review to those studying transitory shocks, and making a small adjustment to account for the spending being financed outside the treated areas, he lands on a multiplier effect of 1.7.

To convert our cost-per-job to an output multiplier, we follow the approach proposed by Chodorow-Reich (2019). For a change in spending of ΔG_t, define the output multiplier as $\beta^Y_{t+h} = \frac{\Delta Y_{t+h}}{\Delta G_t}$ and the employment multiplier $\beta^E_{t+h} = \frac{\Delta E_{t+h}}{\Delta G_t}$. Let $e_{t+h} = \frac{\Delta E_{t+h}}{E_t}$ denote the percent change in employment caused by spending and $y_{t+h} = \frac{\Delta Y_{t+h}}{Y_t}$ the percent change in output, and $g_t = \frac{\Delta G_t}{Y_t}$ the change in spending relative to output. It follows that $e_{t+h} = \beta^E_h \frac{Y_t}{E_t} g_t$. Now assume a production function of the form $Y_t = A(H_t E_t)^{1-\xi}$, which links output (Y_t) to hours worked per employee (H_t), employment (E_t) and productivity (A), without capital adjustment in the short run. Following previous notation, $n_t = \frac{\Delta N_{t+h}}{N_t}$. Then the output multiplier β^Y_h relates to the employment multiplier β^E_h through the following equation:

$$\beta^Y_h = \frac{y_{t+h}}{g_t} = \frac{y_{t+h}}{e_{t+h}} \frac{e_{t+h}}{g_t} \approx (1-\xi)(1+\chi) \cdot \frac{Y}{E} \cdot \beta^E_h,$$

where ξ denotes the capital share in production, χ is the elasticity of hours worked per employee with respect to total employment (n_t/e_t), and Y/E is output per employed worker.

To parameterize the equation, Chodorow-Reich (2019) assumes a standard value of 1/3 for ξ and a value of 0.5 for χ, based on Elsby et al. (2010). For Brazil, Corbi et al. (2019) uses 0.12 to parameterize χ. Based on these parameters, our output multiplier would range between 1.95 and 2.61, given a cost-per-job of €33.000 and GDP per worker €82014. Labor adjustments along the intensive margin are generally thought to be more important in Europe but Van Rens (2012) shows it is relatively unimportant both in Europe and the US.

Note finally that in a discussion of the ARRA spending multiplier in Chodorow-Reich (2019), Ramey (2019) argues the denominator (ΔG_t) should be corrected to include any additional local and state spending triggered by federal spending. Since we observe a wide range of local fiscal policy changes, we simply use the amount of transfers gained.

Our multiplier effect is relatively large. There are several reasons for this and why it may exceed what would be a nationally representative multiplier effect. First and fore-

most, we do not study a transitory fiscal shock but a relatively permanent shift in funds. Indeed, our findings are more in line with other work that has studied persistent funding changes, namely Adelino et al. (2017) and Serrato and Wingender (2016), who estimate a cost-per-job of $20.000 resp $30.000.[1] Beyond the expectation that a persistent change in funds is more likely to spur firm investment and yield a larger multiplier, persistency matters for two reasons. First, the tax burden of these funds are borne (also) by residents outside the affected areas. In line with Nakamura and Steinsson (2014) and Farhi and Werning (2016), Chodorow-Reich (2019) argues that outside-financed spending - when transitory - generates only marginally higher multiplier effects than deficit-financed spending. However, a persistent increase in funds is likely to trigger much stronger Ricardian effects and our multiplier would need to be adjusted downwards considerably more in order to be nationally representative.[2] Secondly, a persistent change in funds is more likely to affect household and firm location decisions, which would generate negative spillover effects and lead us to overestimate a nationally representative multiplier effect. While we find no evidence of changes in location patterns, commuting patterns are affected. At the same time, household consumption and firm input purchases may occur outside the affected areas, which would bias the nationally representative multiplier downwards.

Spillover effects have been examined in several studies on local fiscal multipliers. While the evidence is mixed, it predominantly points towards positive effects. Dupor and McCrory (2018) study the effect of ARRA on local economic activity and identify strong positive spillover effects on the wage bill and employment within the local labor market. Auerbach et al. (2020) finds strong positive spillover effects of US Department of Defense contracts on GDP and labor earnings in neighboring cities (core-based statistical areas). Shoag (2010) finds strong positive effects on income in economically-linked counties of spending triggered by windfalls in pension plan returns. He reports little evidence of a migration response however. Clemens et al. (2024a) examine cross-state spillovers of pandemic-era federal aid, finding only small effects on public employment, weak evidence of private employment effects and virtually no effects on income or output, possibly due to interrupted supply chains and limited opportunities for travel or cross-border shopping in the context of the pandemic. Acconcia et al. (2014) find no significant spillovers of spending cuts in Italian provinces triggered by a law combating political corruption (mafia infiltration). Studying local effects of persistent federal spending changes due to census-based population revisions, Serrato and Wingender (2016) find negative but economically

[1] Adelino et al. (2017) look at the employment effects of spending changes driven by exogenous upgrades in U.S. municipal bond ratings. Serrato and Wingender (2016) use spending changes triggered by census-based population count revisions, similar to Gordon (2004)

[2] Clemens and Miran (2012) are one of the few to study subnational variation in spending that is not due to windfalls but rather due to institutional rules on budget deficits, effectively finding substantially lower multiplier effects.

small and statistically insignificant spillover effects on employment and income. In a follow-up paper however, Serrato and Wingender (2014) show the spending changes cause shifts in population. Finally, Corbi et al. (2019) study local fiscal multipliers exploiting discontinuities in the allocation mechanism of federal transfers in Brazil. They find small negative effects on employment and earnings in surrounding municipalities.

A second reason for large multiplier effects may be state dependence. A growing literature is studying whether fiscal multipliers vary depending on circumstances. One strand of literature has studied whether multipliers are stronger in periods of economics slack, when fiscal expansions is likely to crowd out private consumption or investment (see e.g. Auerbach and Gorodnichenko (2012); Auerbach et al. (2022); Jo and Zubairy (2025); Owyang et al. (2013); Ramey and Zubairy (2018) for work looking at periods of slack and Brückner and Tuladhar (2014); Nakamura and Steinsson (2014); Serrato and Wingender (2016) for work studying places experiencing more slack). Another has studied multipliers when monetary policy is constrained, e.g. at the zero lower bound, or is otherwise accommodating of fiscal stimulus (see e.g. Christiano et al. (2011); Miyamoto et al. (2018); Ramey and Zubairy (2018); Woodford (2011)). In a review of this work, Ramey (2019) concludes first that the evidence for higher multipliers during recessions is fragile.[3] Secondly, the evidence for higher multipliers during zero lower bound periods is relatively strong. Finally, although the literature studying state dependence on tax multipliers is small, they fairly uniformly find them to be pro-cyclical (see Alesina et al. (2018); Bonam and Konietschke (2020); Demirel (2016); Eskandari (2015); Sims and Wolff (2018). At least the latter two conclusions support our multiplier effect, since we analyze a period in which Finland saw modest growth following some time of post-crisis stagnation, with nominal interest rates at the zero lower bound.

Finally, the fact that we do not study pure (local) spending or tax effects complicates the calculation of a multiplier. Indeed, we found that the change in funds affected spending, revenue raising and deficits in equal measure. Tax multipliers estimated using time series methods are considerably larger than spending multipliers. However, more recent estimates using New Keynesian DSGE models are much smaller and more comparable to spending multipliers (Ramey, 2019). Either way, we urge to take our fiscal multiplier estimate with a grain of salt. The fact remains however, that fiscal stimulus funds are often spent on, or with the help of, state and local government. While the majority of the literature estimating subnational multipliers has only looked at spending, it is likely that other dimensions of state and local public finance are affected by fiscal stimulus. Our work suggest money does not completely stick where it hits, so taking into account subnational tax responses may be important to estimate the overall impact of stimulus

[3]Ramey (2019) argues early findings of state dependence were not robust to small specification changes. However, Berge et al. (2021) suggests conflicting findings can be reconciled by decomposing the business cycle in four stages, where the multiplier effect depends on whether the unemployment rate is rising or decreasing and above or below its trend rate.

funding.

B Grant reform of 2015

The bill for reforming the system of central government transfers to municipalities was given to the parliament in April 2014, it was passed in August, and came into force in 2015. The purpose of the reform was to simplify and update grant allocation rules. The old system included separate complicated formulae for different service classes 1) social services 2) healthcare 3) pre- and primary schools 4) libraries and culture. In addition, the system included a a tax base equalization scheme and a small general component. These formulae included more than 50 parameters for often overlapping grant determinants. The reform replaced the separate formulae for different services with a single formula for all basic services. The parameters of this new formula were based on research on the association of service expenditure and municipality characteristics, but expert views and political interference also played a role.

The new formula had only 19 determinants and parameters, and a tax base equalization scheme. For instance, compensation for the share of population with a native language other than Finnish, Swedish, or Sámi languages was introduced to address the special needs of non-native residents. Additionally, a compensation for workplace self-sufficiency (the ratio of jobs to employed residents) was included to incentivize municipalities to foster local economic development.[4] .The new formula also had fewer discontinuities that lead to changes in grants in the case of municipality mergers.

We do not attempt to fully describe the pre- and post-reform grant allocation rules, but Table B.1 describes the importance of different municipality characteristics in the grant allocation formula before and after the reform. The reform placed more emphasis on tax base equalization, which shifted from a zero-sum redistribution model to a source of net financing for municipalities.[5] Also the morbidity index and some other more defined cost determinants received more weight. At the same time, compensation based on age-group specific imputed costs was cut.

[4]The incentive created by this grant component was similar to all municipalities and very weak. A one percentage point increase in the ratio of jobs to employment would lead to a grant increase of mere €0.64 per capita.

[5]Before the reform, the equalization limit was 91.86 % of mean imputed tax revenue per capita, and grant increases to municipalities with low tax base could be funded by grant decreases from high tax base municipalities. After the reform, the equalization limit was raised to the national average of imputed revenue per capita. This implied that the central government had to cover part of the grant increases to low tax base municipalities.

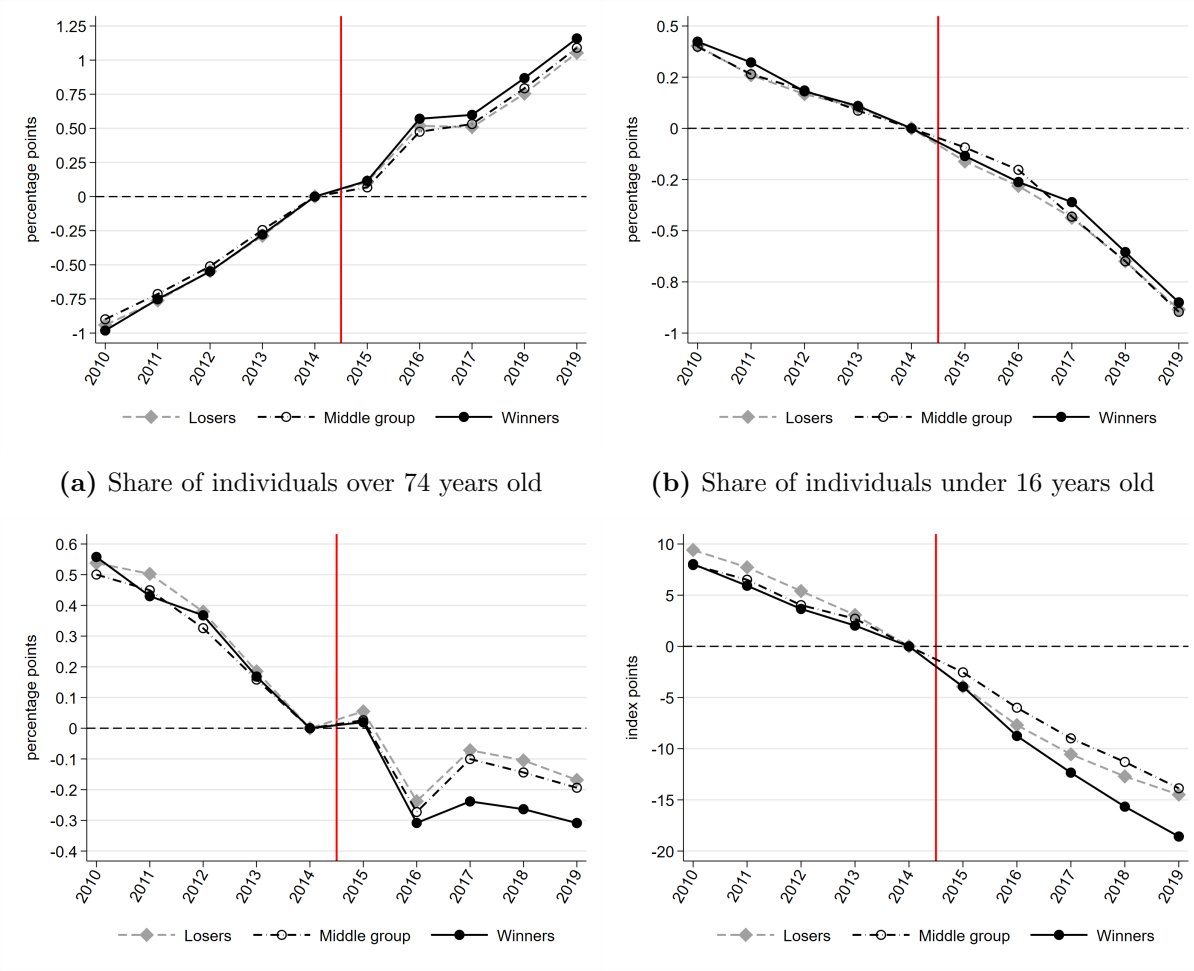
Table B.1: Fiscal importance of grant determinants before and after the reform

	2014 grant system		2015 grant system	
	EUR, million	Share of total	EUR, million	Share of total
Age structure	6252	0.727	4591	0.545
Morbidity index	705	0.082	1552	0.184
Basic compensations	290	0.034		
Unemployment rate	143	0.017	121	0.014
Population density	86	0.010	48	0.006
Child protection	80	0.009		
Remoteness	63	0.007	113	0.013
Built-up area	38	0.004		
Bilinguality	30	0.003	24	0.003
Disability	28	0.003		
Archipelago	17	0.002	4	0.000
Foreign language share	13	0.002	133	0.016
Population change	4	0.000		
Sápmi homeland	4	0.000	4	0.000
Tax base equalization	-50	-0.006	673	0.080
Workplace self-sufficiency			195	0.023
Adults without a post-primary degree			41	0.005
Additions and reductions	892	0.104	927	0.110
Total	**8594**		**8425**	

Notes: The table shows the amount of funding allocated to municipalities based on different municipality characteristics before and after the reform of 2015. Transition financing related to the 2015 reform is not included (€29 million in 2015).

B.1 Development of grant determinants

Figure B.1: Development of grant determinants



(a) Share of individuals over 74 years old

(b) Share of individuals under 16 years old

(c) Share of 16–74-year-old individuals

(d) Morbidity index

Figure B.2: Tax-base equalization per resident



Table B.2: Means of dependent variables and other municipality characteristics in 2014: differences between municipality groups

	W-L	W-M	L-M
1. Municipal finances			
Central government transfers per resident (€)	-111	451	562
	162	178	157
Municipal income tax rate (%)	0.41	0.13	-0.28
	0.12	0.14	0.12
General property tax rate (%)	0.09	-0.01	-0.10
	0.02	0.03	0.02
Tax revenue per resident (€)	70	-132	-202
	79	79	79
Collected service fees per resident (€)	58	25	-33
	25	28	21
Operating expenses per resident (€)	160	455	295
	133	142	124
Annual contribution margin per resident (€)	1	7	6
	36	36	33
Net long-term borrowing per resident (€)	45	-19	-64
	63	78	55
2. Labor market outcomes			
Jobs per 1,000 residents	51	20	-31
	11	12	9
Private sector jobs per 1,000 residents	32	9	-23
	9	10	8
Municipal sector jobs per 1,000 residents	11	8	-3
	3	3	3
Employment rate of 20- to 64-year-old individuals (%)	-0.9	-2.2	-1.2
	0.9	1.0	0.9
Share of out-commuters (%)	-10.9	-7.8	3.1
	2.6	3.0	2.7
Mean disposable cash income per resident (€)	-305	-587	-282
	308	264	313

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Table B.2 – *Continued from previous page*

	W-L	W-M	L-M
3. Migration			
In-migration rate (%)	-0.16	-0.32	-0.16
	0.20	0.21	0.19
Out-migration rate (%)	0.05	0.01	-0.04
	0.16	0.19	0.15
Net migration rate (%)	-0.21	-0.33	-0.12
	0.12	0.12	0.12
Log of population	0.46	-0.34	-0.80
	0.16	0.20	0.15
4. Socioeconomic characteristics			
Population share of individuals under 15 years old (%)	-0.3	-1.3	-1.0
	0.7	0.6	0.6
Population share of 15–64-year-old individuals (%)	1.2	-0.9	-2.0
	0.5	0.6	0.4
Population share of individuals over 64 years old (%)	-0.9	2.1	3.0
	0.9	1.0	0.8
Share of individuals aged 15 or older with a tertiary degree (%)	-0.1	-2.3	-2.2
	1.0	1.0	1.0
Share of residents with foreign native language (%)	1.0	0.2	-0.8
	0.3	0.4	0.3
Share of households living in rental apartments	3.6	0.8	-2.8
	0.9	1.2	0.9
Cumulative population change in 2010–2014 (%)	-0.1	-2.0	-1.9
	0.7	0.8	0.7
Cumulative natural population growth in 2010–2014 (%)	0.5	-1.1	-1.5
	0.5	0.5	0.4
Cumulative net inter-municipality migration in 2010–2014 (%)	-1.0	-1.2	-0.2
	0.4	0.4	0.4
5. Geographical characteristics			
Log of population density	0.01	-0.66	-0.67
	0.22	0.28	0.21
Log of land area	0.45	0.32	-0.13
	0.16	0.19	0.14

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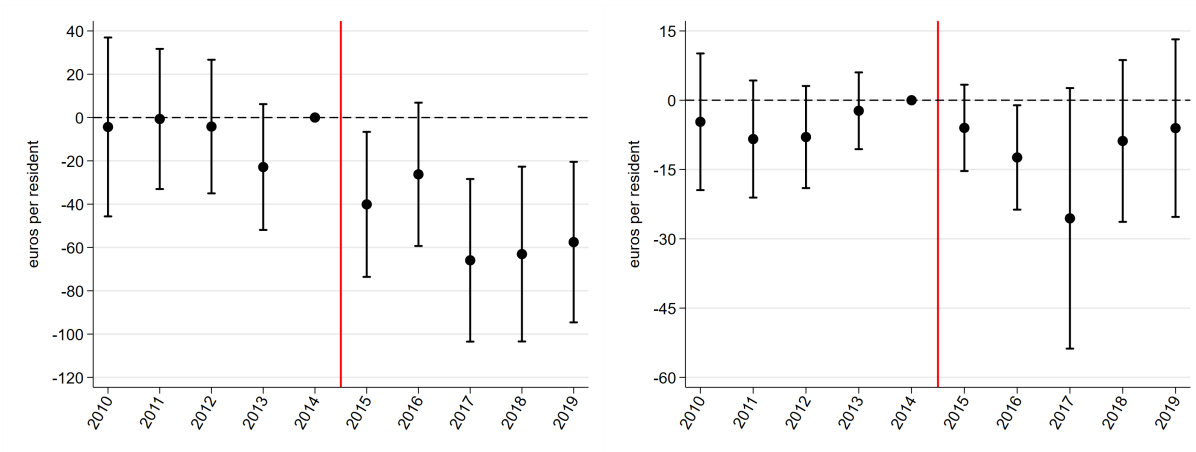
	W-L	W-M	L-M
Mean latitude of municipality's centroid	0.96	0.83	-0.12
	0.29	0.34	0.26
Mean longitude of municipality's centroid	1.14	1.01	-0.13
	0.34	0.38	0.32
Share of residents living in the built-up area (%)	4.4	-7.7	-12.1
	2.8	3.3	2.7

Notes: This table shows the differences in the means of the dependent variables and other municipality characteristics between the three municipality groups and the standard errors of the means. For each variable, the first row tells the difference in the means, and the second row is for the standard error of the difference. Column *W-L* is the difference between winners and losers, *W-M* is the difference between winners and the middle group, and *L-M* is the difference between losers and the middle group. Winner municipalities are defined as those municipalities seeing an increase in imputed transfer revenue over €50 per capita while loser municipalities are those that lost over €50 per capita and the middle group lies in between.

C Additional results

C.1 Breakdown of tax revenue effects

Figure C.1: Income tax revenue and property tax revenue per resident: winner municipalities compared to loser municipalities



(a) Income tax revenue per resident

(b) Property tax revenue per resident

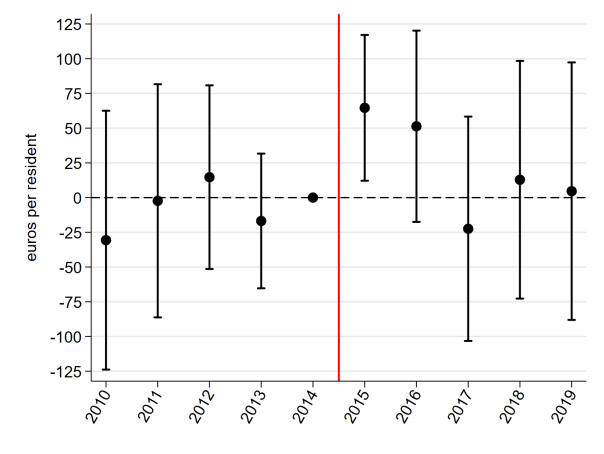
Notes: The mean income tax revenue per resident in 2014 was €2,843 in the winner group and €2,784 in the loser group. The mean property tax revenue per resident in 2014 was €295 in the winner group and €283 in the loser group. Property tax revenue includes here all the different property tax categories.

C.2 Breakdown of spending effects

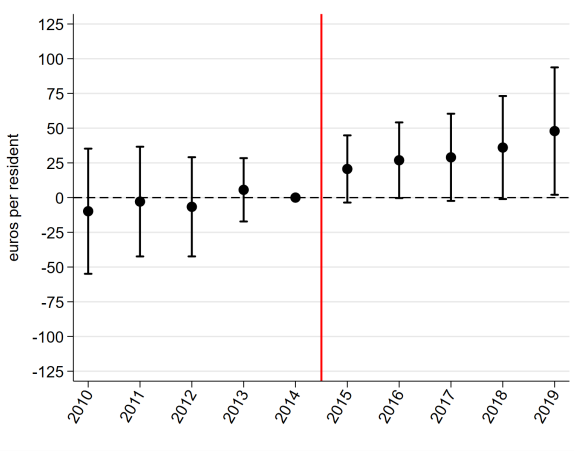
In Figure C.2 we break down the effect on spending into three different categories. Health and social services are the most important spending type, accounting for about 55% of total spending. Education, daycare, culture and recreation were 30% of total spending and other spending (e.g., infrastructure, support services and administration) about 15%.

Spending is calculated net of sales revenue, because those revenues correspond to expenses provided for other municipalities or other entities and do not represent services provided for the residents. Eight municipalities (four in both the winner and loser group) from the region of Kainuu are left out of the regressions, because their cost allocation to different categories was erroneous in years 2010–2012. This is why the point estimates in Figure C.2 do not sum up to the point estimates of total operating expenses (Figure 7c), where the omitted municipalities are included. Also, Figure 7c uses operating expenses from the income statement, which slightly differs from total expenses aggregated from the activity-specific accounts.

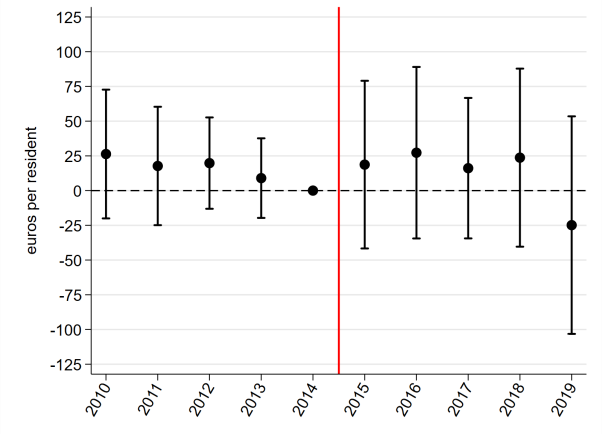
Figure C.2: Operating expenses per resident by activity type



(a) Health and social services



(b) Education, daycare, culture and recreation



(c) Other spending

Notes: The mean spending on health and social services in 2014 was €3,892 in the winner group and €3,750 in the loser group. The mean spending on education, daycare, culture and recreation in 2014 was €1,905 in the winner group and €1,839 in the loser group. The mean of other spending (e.g., administration and infrastructure) in 2014 was €994 in the winner group and €958 in the loser group. Spending by activity is net of sales revenue. Eight municipalities (four in both the winner and loser group) from the region of Kainuu are left out of the regressions, because their cost allocation to different categories was erroneous in years 2010–2012.

C.3 Disposable income and population

Figure C.3: Mean disposable income per resident

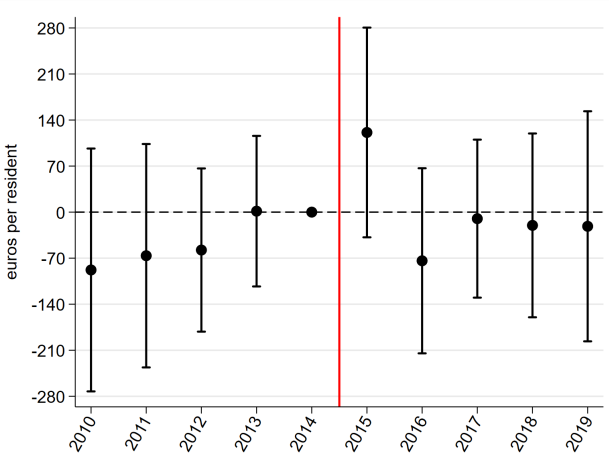
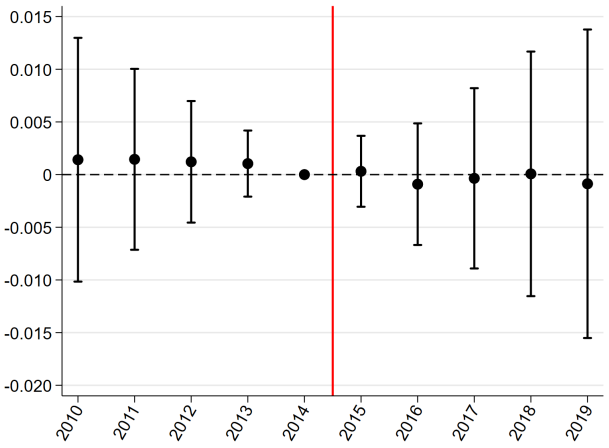


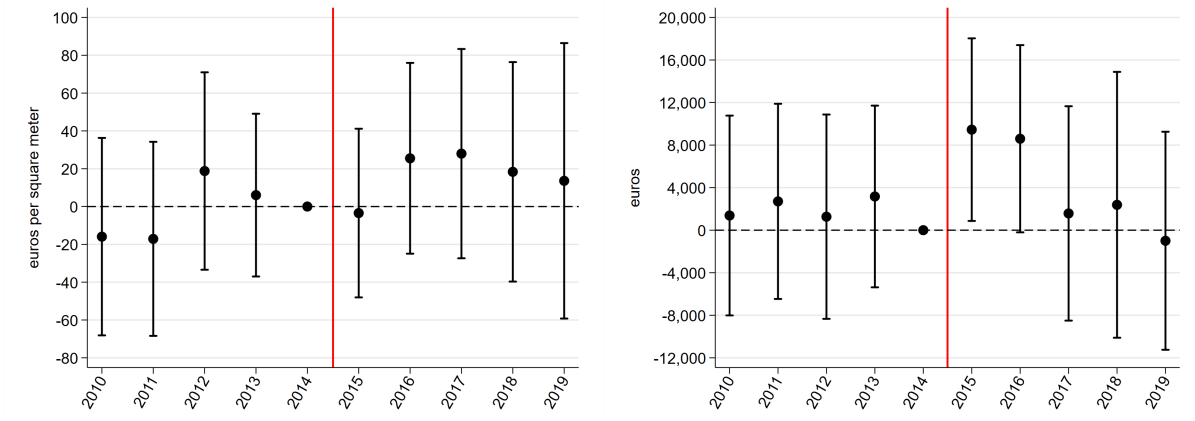
Figure C.4: Logarithm of population: winner municipalities compared to loser municipalities



C.4 Housing prices

Figure C.5 shows DiD estimates for the effect on housing prices. Prices of apartments and single family homes are analyzed separately, because they come from different data sources. Panel (a) shows the development of mean apartment price per square meter (by Statistics Finland). Panel (b) shows median single family house price (by National land registry). We use median price of the whole property because statistics on price per square meter at municipality level is not available at municipality level. Observations based on less than ten transactions are omitted.

Figure C.5: Housing prices of winners compared to losers



(a) Mean apartment price per square meter

(b) Median price of single family homes

Notes: The outcome is mean apartment price per square meter published by Statistics Finland and median single family house prices by the National land survey. The sample includes only municipalities with at least 10 transactions in the observation year. Mean apartment price per square meter in 2014 was €1,332 in the winner group and €1,302 in the loser group. Mean of median single family home prices was €149,483 in the winner group and €153,396 in the loser group.

The DiD estimates for the effect on apartment prices per square meter (Fig. C.5a are positive but close to zero. For median single family house price (Fig. C.5b), the estimates are positive in 2015 and 2016 but close to zero in later years. Standard errors seem large in both figures. To analyze whether strong capitalization of the grant shock is likely, we perform a simple calculation comparing the upper bounds of the confidence intervals of the price effect estimates to the size of the grant shock.

Starting from apartment price results in Figure C.5a, the upper bound of the confidence band of the DiD estimate in 2019 is roughly €100 per square meter. Mean housing space per resident in Finland is approximately 40 square meters. Thus the upper bound corresponds to a €4,000 increase in housing value per person. Relating this estimate to the difference in grant shocks per person (€300) gives that the implied discount rate consistent with full capitalization would be 7.5%. Turning to single family housing prices in panel (b), the upper bound of the confidence band in 2019 is roughly €9,000. As-

suming 2.5 persons per single family house means an increase of €3,600 per person in single family house values. The discount rate consistent with full capitalization of the €300 grant shock would be 8.3%. These discount rates seem overly large to be true, especially in the period of low interest rates we study. Thus, the results suggest less than full capitalization, but it is evident that we cannot rule out economically significant effects on housing prices.

C.5 Accounting for the bilateral nature of migration

To account for the bilateral nature of migration, we draw on recent work by Borusyak et al. (2022) by controlling for the average imputed grant change in other municipalities, weighted by migration intensity, $\overline{G_i}$, which we interact with time dummies.

Specifically, we estimate:

$$Y_{it} = \sum_{t \neq 2014} \beta_t * winner_i * D_{it} + \sum_{t \neq 2014} \gamma_t * \overline{G_i} * D_{it} + \delta_t + \eta_i + \epsilon_{it}, \tag{4}$$

where $\overline{G_i}$ is calculated as follows:

$$\overline{G_i} = \sum_{k \neq i} \frac{(M_{ik} + M_{ki})}{\sum_{k \neq i} M_{ik} + M_{ki}} * \hat{g_k},$$

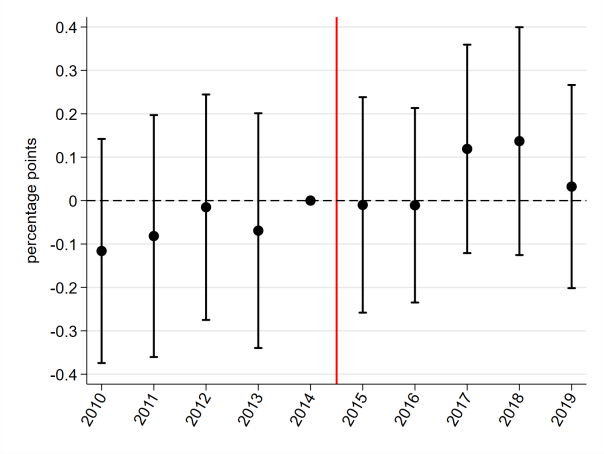
where M_{ik} represents the pre-reform migration flow from municipality i to municipality k, M_{ki} is the pre-reform migration flow from municipality k to municipality i, and $\hat{g_k}$ is the imputed grant change of municipality k.[6] Thus, the weight municipality k's grant change receives, reflects the share of pre-reform migration between i and k relative to total inwards and outwards migration of i. This means that more weight is put on municipalities that were important migration destinations or origins for residents in municipality i prior to the reform.

The overall mean weighted transfer change in other municipalities is €35 per resident for the winners and -€18 per resident for the losers. In other words, there is indeed a positive correlation between winner status and grant changes in relevant migration destinations and origins. This means that the relative attractiveness of the winner municipalities is exaggerated without the control variable.

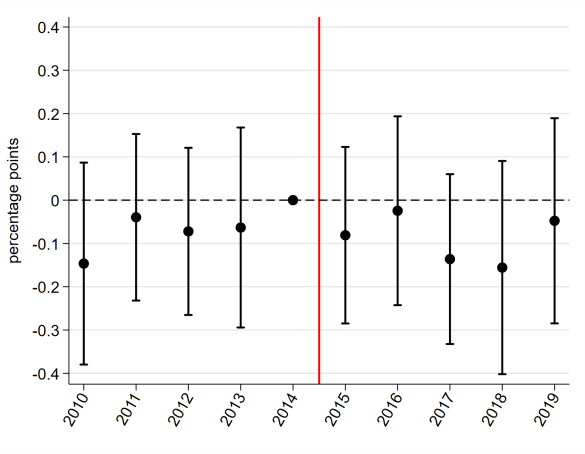
Nevertheless, when we account for grant changes in other municipalities, the estimated effects on migration rates change only slightly, as shown in Figure C.6. The DiD estimates for neither the in-migration rate nor the out-migration rate show significant effects. However, in 2017 and 2018, the two rates diverge so that the point estimates regarding the net migration rate are almost statistically significant for those two years. The estimates are, however, somewhat sensitive to the specification as the significance disappears when we use logs of migration, as shown Figure C.8.

[6] In this formula, we also include those municipalities excluded from the analysis whose imputed grant change fell within the range of -50 to 50 euros per resident.

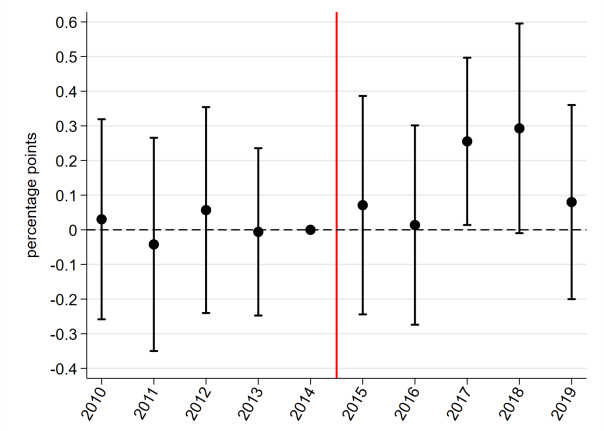
Figure C.6: Migration rates controlling for transfer changes in other municipalities: winner municipalities compared to loser municipalities



(a) In-migration rate



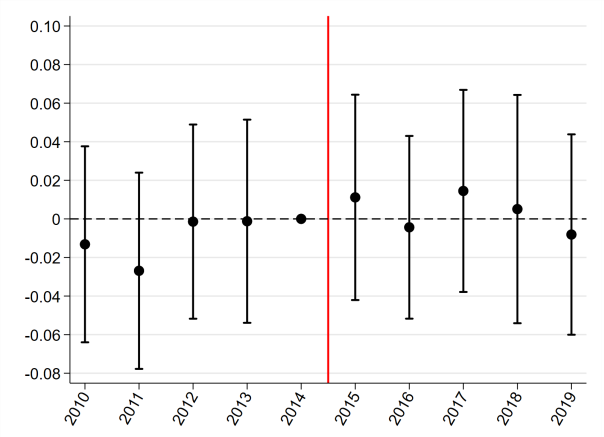
(b) Out-migration rate



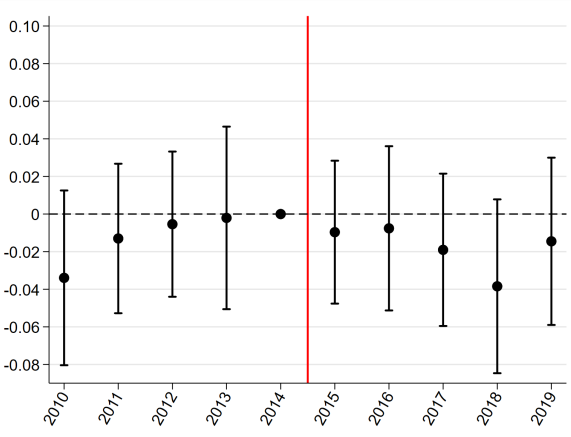
(c) Net migration rate

C.6 Logarithm of migration

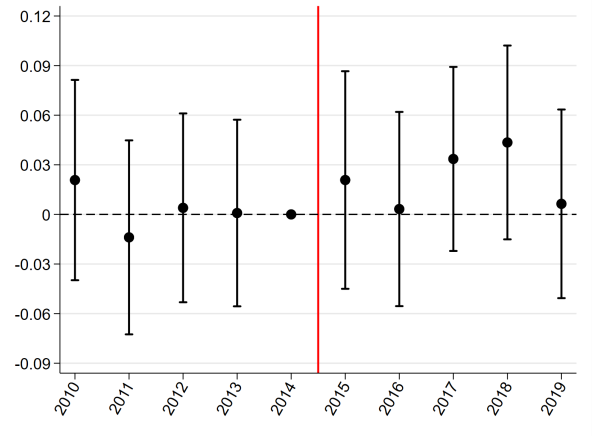
Figure C.7: Log of migration: winner municipalities compared to loser municipalities



(a) Log of in-migration

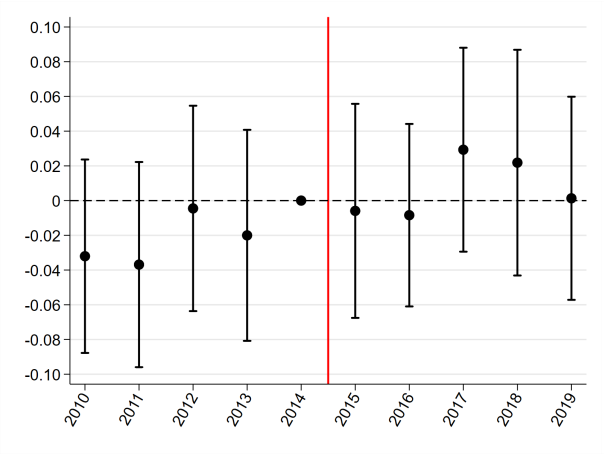


(b) Log of out-migration

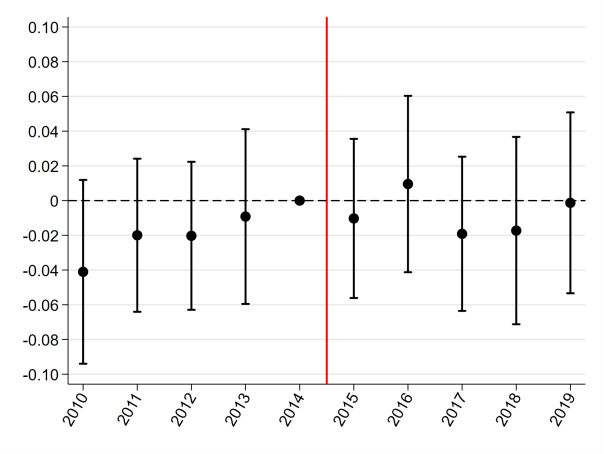


(c) Log of in-migration to out-migration

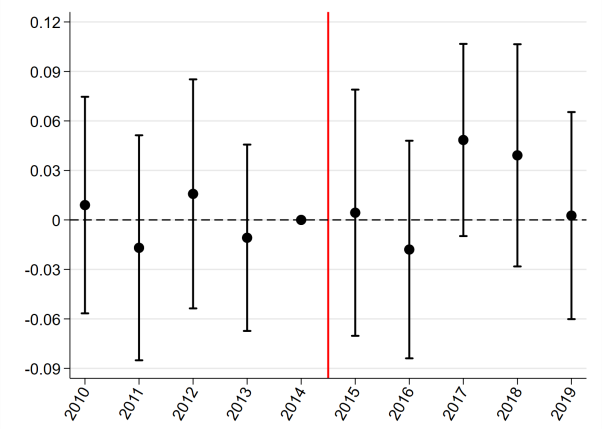
Figure C.8: Log of migration controlling for transfer changes in other municipalities: winner municipalities compared to loser municipalities



(a) Log of in-migration



(b) Log of out-migration



(c) Log of in-migration to out-migration

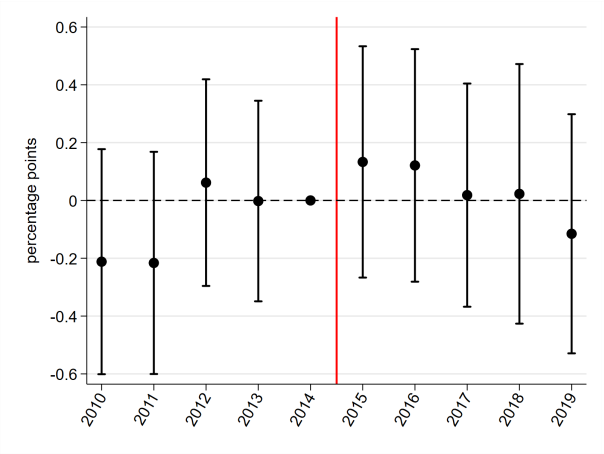
C.7 Migration response in prime-working-aged population

So far, we have considered the migration response in the entire population, but some groups of individuals might be very unresponsive to changes in local public services or taxes. Young adults are very mobile, but their moving decisions are related to leaving their parents and/or going to study. Moreover, their incomes are often such low that they pay only little local taxes. Retirees, instead, are generally very immobile, and unlikely to move because of small changes in municipal taxes and spending. Total population also includes underage children that do not make moving decision themselves. For these reasons, we estimate the effects of the reform on the migration rates of individuals in the prime working age (25–54-year-old population). At this age, people usually have families and might put more weight on public services and amenities in the municipality, and they are also often net tax payers. These individuals, who usually consume only little costly public health and social services, are also the ones that municipalities usually would like to temp to move in or prevent from moving out.

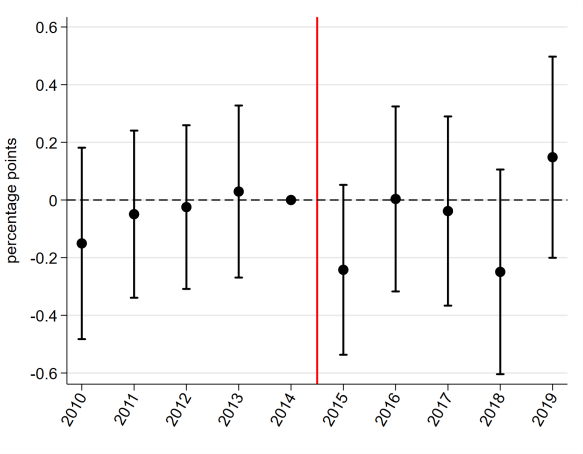
The mean migration rates of prime-working-aged individuals were larger than in the total population before the reform. The mean in-migration rate in 2014 was 5.40 % in the winner group and 5.78 % in the loser group. The mean out-migration rate was 5.49 % in the winner municipalities and 5.45 % in the loser municipalities. However, the pre-reform difference in the levels between the winners and the losers was similar to the aggregate migration rates. Losers had a slightly larger mean in-migration rate, and the mean out-migration rates were equal.

In Figure C.9 we see that the DiD estimates considering the 25–54-year-old population are very similar to the ones estimated with total population. Point estimates fluctuate around zero and are statistically insignificant. With aggregate population, the DiD estimate for out-migration (net migration) in year 2018 was significantly negative (positive), but now even that effect disappears. Given that the prime-working-aged are likely to be more responsive to municipal finances than the population on aggregate, our conclusions remain robust: changes in central government transfers to municipalities did not affect migration, or affected only in an economically insignificant magnitude.

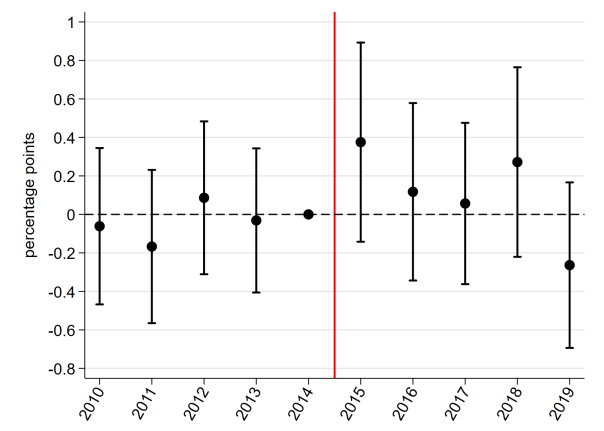
Figure C.9: Migration rates of 25–54 year old individuals: winner municipalities compared to loser municipalities



(a) In-migration rate



(b) Out-migration rate



(c) Net migration rate

C.8 Allowing for deviations from parallel trends assumption

Table C.1: Pooled effect (years 2018-2019) on municipal finances: confidence intervals robust to violation of parallel trends

	Original		M = 0.5	
	lb	ub	lb	ub
Central government transfers received (€/resident)	247	352	182	420
Income tax rate (p.p.)	-0.398	-0.124	-0.621	0.129
General property tax rate (p.p.)	-0.084	-0.016	-0.133	0.023
Tax revenue (€/resident)	-110	-25	-204	47
Collected service fees (€/resident)	-82	-7	-121	33
Total operational spending (€/resident)	3	228	-92	323
Annual contribution margin (€/resident)	41	231	-212	440
Net long-term borrowing (€/resident)	-321	59	-687	399

Notes: This table reports the 95% confidence intervals of the equally weighted linear combination of the DiD estimates for years 2018 and 2019 that were estimated in Table 4. Column *Original* presents the original confidence intervals. The confidence intervals in column *M = 0.5* are estimated using the method for robust inference and sensitivity analysis for differences-in-differences and event study designs developed by Rambachan and Roth (2023), and allowing the slope of the prereform trends to change no more than 0.5.

Table C.2: Pooled effect (years 2018-2019) on labor market outcomes: confidence intervals robust to violation of parallel trends

	Original		M = 0.5	
	lb	ub	lb	ub
Jobs per 1,000 residents	2.31	15.97	-4.41	23.89
Private sector jobs per 1,000 residents	1.49	14.83	-2.23	19.84
Municipal sector jobs per 1,000 residents	-2.12	5.34	-8.93	12.68
Employment rate (%)	-0.24	0.99	-1.23	1.79
Share of out-commuters (%)	-1.38	-0.10	-1.86	0.43
Mean disposable cash income per resident (€)	-166	124	-361	339

Notes: This table reports the 95% confidence intervals of the equally weighted linear combination of the DiD estimates for years 2018 and 2019 that were estimated in Table 5. Column *Original* presents the original confidence intervals. The confidence intervals in column *M = 0.5* are estimated using the method for robust inference and sensitivity analysis for differences-in-differences and event study designs developed by Rambachan and Roth (2023), and allowing the slope of the prereform trends to change no more than 0.5.

Table C.3: Pooled effect (years 2018-2019) on migration: confidence intervals robust to violation of parallel trends

	Original		M = 0.5	
	lb	ub	lb	ub
In-migration rate (%)	-0.161	0.234	-0.538	0.642
Out-migration rate (%)	-0.324	0.026	-0.807	0.448
Net migration rate (%)	-0.036	0.407	-0.547	0.958
Log of in-migration	-0.052	0.049	-0.153	0.160
Log of out-migration	-0.066	0.013	-0.161	0.095
Log of in-migration/out-migration	-0.025	0.075	-0.160	0.219
Log of population	-0.013	0.013	-0.019	0.017

Notes: This table reports the 95% confidence intervals of the equally weighted linear combination of the DiD estimates for years 2018 and 2019 that were estimated in Table 6. Column *Original* presents the original confidence intervals. The confidence intervals in column *M = 0.5* are estimated using the method for robust inference and sensitivity analysis for differences-in-differences and event study designs developed by Rambachan and Roth (2023), and allowing the slope of the pre-reform trends to change no more than 0.5.

C.9 Standard errors clustered at the district level

Table C.4: Pooled effect (years 2018-2019) on municipal finances: standard errors clustered at the district level

	Coefficient	Standard error	Pre-reform mean
Central government transfers received (€/resident)	300	28	2888
Income tax rate (p.p.)	-0.26	0.07	20.44
General property tax rate (p.p.)	-0.050	0.016	0.94
Tax revenue (€/resident)	-68	22	3090
Collected service fees (€/resident)	-44	22	254
Total operational spending (€/resident)	115	59	6375
Annual contribution margin (€/resident)	136	58	96
Net long-term borrowing (€/resident)	-131	101	44

Notes: This table reports the equally weighted linear combination of the DiD estimates for years 2018 and 2019 that were estimated in Figures 5-8b. Each regression has 1980 observations: a ten-year balanced panel of 198 municipalities. 63 municipalities belong to the winner group and 135 belong to the loser group. All the regression include municipality and year fixed effects. There are 66 districts, meaning on average 3 municipalities and 30 observations per cluster.

Table C.5: Pooled effect (years 2018-2019) on labor market outcomes: standard errors clustered at the district level

	Coefficient	Standard error	Pre-reform mean
Jobs per 1,000 residents	9.14	4.09	321
Private sector jobs per 1,000 residents	8.16	3.88	226
Municipal sector jobs per 1,000 residents	1.61	2.19	88
Employment rate (%)	0.37	0.39	68.1
Share of out-commuters (%)	-0.74	0.36	38.9
Mean disposable cash income per resident (€)	-21	76	19237

Notes: This table reports the equally weighted linear combination of the DiD estimates for years 2018 and 2019 that were estimated in Figures 9a-C.3. Each regression has 1980 observations: a ten-year balanced panel of 198 municipalities. 63 municipalities belong to the winner group and 135 belong to the loser group. All the regression include municipality and year fixed effects. Standard errors are clustered at the district level. There are 66 districts, meaning on average 3 municipalities and 30 observations per cluster.

Table C.6: Pooled effect (years 2018-2019) on migration: standard errors clustered at the district level

	Coefficient	Standard error	Pre-reform mean
In-migration rate (%)	0.037	0.110	4.29
Out-migration rate (%)	-0.149	0.087	4.74
Net migration rate (%)	0.185	0.121	-0.46
Log of in-migration	-0.002	0.029	5.47
Log of out-migration	-0.026	0.020	5.59
Log of in-migration/out-migration	0.025	0.027	-0.12
Log of population	-0.000	0.008	8.66

Notes: This table reports the equally weighted linear combination of the DiD estimates for years 2018 and 2019 that were estimated in Figures 11a-C.4 and C.7. Each regression has 1980 observations: a ten-year balanced panel of 198 municipalities. 63 municipalities belong to the winner group and 135 belong to the loser group. All the regression include municipality and year fixed effects. Standard errors are clustered at the district level. There are 66 districts, meaning on average 3 municipalities and 30 observations per cluster.