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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, sub-national governments account for 39.5% of public expenditure and 39.8% of their revenue comes from intergovernmental transfers.[1] In spite of their prevalence and importance, the implications of fiscal equalization for the spatial distribution of economic activity have not received much attention (Agrawal et al., 2024).

The spatial economic impact of some more implicit equalization policies such as infrastructure investment, enterprise zones and other place-based policies, have been more widely studied. This body of literature has demonstrated that support for lagging regions is likely to impair efficient location choices, contributing to a general skepticism surrounding place-based policy among economists. However, growing political discontent in lagging areas has sparked renewed interest in place-based policy, specifically place-based redistribution (Guriev and Papaioannou, 2022). Still, according to Austin et al. (2018), the strongest justification for spatial targeting lies not in the traditional efficiency (agglomeration-based) or equity (insurance-based) arguments, but in market failures that are best addressed at the local level. In other words, there may be higher marginal returns to tackling social distress in high-distress areas (Bartik, 2020). Such targeting is an inherent feature of most equalization formulas. Whether unconditional transfers are an effective tool to support distressed places remains an open question however.

In this paper, we set out to 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.

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

We study the case of 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 €1800 per capita or over 4% of GDP in 2020.

The key challenge to studying the effects of fiscal transfers to local authorities is the two-way causality between local economic performance and transfers. The amount of transfers local governments receive is formula-based and depends on local socio-economic conditions. To address this issue, we exploit a reform of the transfer system in 2015, which drastically changed 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. In response, we see the number of private sector jobs increase. However, our findings indicate that the effects on the employment rate and migration patterns were small. Rather, we see more residents work locally instead of commuting out of their municipality to work. Overall, this indicates the indirect benefits of fiscal transfers are quite localized and raise little efficiency concerns through migration responses. While Boadway (2006); Glaeser and Gottlieb (2008) have noted that policy-induced shifts in economic activity may harm overall agglomeration economies, the shifts we identify seem to take place within a commuting zone, suggesting they are unlikely to matter for productivity.

These findings are relevant for the literature on intergovernmental grants, place-based policy and local fiscal multipliers.

First, a large literature in fiscal federalism is dedicated to studying the role, design and incentive effects of intergovernmental grants.[2] To determine the welfare implication of grants, empirical research has mostly examined their effect on local government behavior. Less studied is their effect on the spatial distribution of economic activity (Agrawal et al., 2024).[3] Yet a wide range of studies has

[2]The literature on fiscal federalism was recently reviewed by Agrawal et al. (2024). Core insights on intergovernmental grants specifically are summarized by Clemens and Veuger (2023). Finally, Lago et al. (2024) provide a comprehensive survey of the effects of intergovernmental grants on a wide range of outcomes relevant to the fiscal federalism literature, including tax effort, tax competition, crowding out, the flypaper effect, government size and accountability.

[3]It is worth noting a large literature draws identifying variation from fiscal equalization reforms to study the impact of educational spending on student outcomes, see e.g. Biasi (2023);

demonstrated the effects of fiscal transfers on local fiscal policy and this in turn is likely to impact economic activity. 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.

Previous quantitative theory work suggests that equalization transfers cause locational inefficiency. Albouy (2012) builds a federal model to study the impact of fiscal transfers on household location decisions. Simulating the transfer system in Canada, he concludes that fiscal equalization leads to inefficient location choices and loss of output, and even undermines equity. Calibrating a general equilibrium model for Germany, Henkel et al. (2021) also conclude that fiscal equalization distorts location choices. They find that national output per capita declines as economic activity is diverted away from high-productivity cities but welfare is still higher from preventing overcongestion. Our quasi-experimental estimates suggest that distortions to location choices are quite muted. While we find a shift in economic activity (jobs), it seemingly takes place within local labor markets. Commuting patterns change, but migration responses to grants seem very small. Our preferred estimate for the migration response indicates that attracting one additional migrant would require a grant increase of €160,000. In comparison, the cost of one additional local job is only €33,000.

Secondly, the study of intergovernmental grants relates closely to research on place-based policies.[4] The efficiency rationale for place-based policy is rooted in locational externalities, agglomeration economies and congestion, which imply that a decentralized spatial equilibrium may not be optimal.[5] Similarly, the efficiency rationale for intergovernmental transfers lies in the spillovers created by local (distortionary) taxation and spending decisions, which imply that decentralized governance is suboptimal.[6]

In practice, equity considerations may be a bigger motivator for intergovernmental transfers and place-based policy. However, efforts to reduce disparities in public goods provision and employment may also be efficiency enhancing. For

Hoxby (2001); Hyman (2017); Jackson et al. (2015); Litschig and Morrison (2013)

[4]See Neumark and Simpson (2015) and Austin et al. (2018) for an overview of the economic rationales for place-based policy and review of the empirical literature. In other reviews, Bartik (2020) surveys the literature looking at job creation, Ehrlich and Overman (2020) take a closer look at Cohesion Policy in Europe while Corinth and Feldman (2024) reviews Opportunity Zones. Finally, it is worth noting an emerging literature focusing on place-based redistribution targeting neither firms nor local governments, but residents, cf. Austin et al. (2018); Gaubert et al. (2021).

[5]As pointed out by Glaeser and Gottlieb (2008); Austin et al. (2018) this does not justify place-based intervention as long as the relative strength and spatial heterogeneity of these externalities remain elusive, which they argue is still the case.

[6]As apparent from reviews by Lago et al. (2024) and others, the strength of these externalities is also heterogeneous and context-dependent, which similarly complicates the design of intergovernmental transfer systems.

example, public investment may have diminishing returns and may therefore be more effective in areas with low levels of public goods. Similarly, employment elasticities may be higher in places with few jobs. Joblessness also creates fiscal externalities through reduced tax revenue and increased social spending as well as social externalities, relating to crime, educational outcomes and life satisfaction (Austin et al., 2018; Bartik, 2020).

While the literature has provided evidence that place-based policies create jobs, the findings are very heterogeneous and sensitive to both the program and location. Studies have also raised efficiency concerns related to migration, spillovers and capitalization of benefits into property prices. Although the same concerns apply to intergovernmental transfers, the lack of research on grants examining their real effects warrants a closer look.

Ultimately, there are significant differences between intergovernmental transfers and other place-based policies. First, revenue flows from intergovernmental grants tend to be significantly stronger. In Finland, fiscal transfers from the central government to local governments are more than 20 times as large as EU Cohesion policy transfers. Secondly, most place-based policies are focused on improving the business environment, either through tax cuts and subsidies or through infrastructure investment. Furthermore, place-based policies also tend to be more programmatic, project-based and involve more steering from supra-local authorities. Intergovernmental transfers allow local governments to adjust the levers they see fit to support their residents.

Indeed, many types of (local) policies can support employment. In reviewing the role and impact of place-based policy on job creation, Bartik (2020) notes that no single type of intervention is the best or can be the best for all places. Similarly, Austin et al. (2018) write that the most compelling case for place-based policies is the diversity in regional economies that makes one-size-fits-all interventions woefully inappropriate. While Austin et al. (2018) argue for tailoring of federal policies to local conditions, Bartik (2020) advocates for a more "flexible block grant with many allowable uses, which can then be attuned to local needs" - much like intergovernmental transfers.

Our findings suggest that the literature should reconsider the potential of intergovernmental grants. In spite of the differences, we find an impact on jobs in line with the place-based policy literature. However, as is quite common in this literature, we find suggestive evidence of spillover effects. While migration patterns don't change significantly, commuting patterns do. Nevertheless, since commuting usually takes place within the same local labor market, aggregate productivity changes might be small.

Finally, our work is related to a recent literature providing quasi-experimental estimates of fiscal multipliers using shocks in local or regional (cross-sectional) spending (see e.g. Acconcia et al., 2014; Adelino et al., 2017; Clemens and Miran, 2012; Cohen et al., 2011; Corbi et al., 2019; Fishback and Kachanovskaya, 2015; Gabriel et al., 2023; Nakamura and Steinsson, 2014; Räsänen and Mäkelä, 2021; Serrato and Wingender, 2016; Shoag, 2013). In a review of this literature, Chodorow-Reich (2019) finds cost-per-job estimates ranging from \$25,000 to \$125,000, with a preferred estimate of \$50,000 and an average multiplier of 1.8.[7] Spillover effects seem quite limited and multipliers tend to be higher in depressed areas. For example, Serrato and Wingender (2016) find that counties with lower income and employment growth are more impacted by changes in government spending. Similarly, Corbi et al. (2019) finds stronger effects in municipalities with below median income and lack of access to private credit. Finally, Adelino et al. (2017) finds that more financially constrained municipalities see higher local income multipliers.

In response to the transfer reform, we find that municipalities utilize a wide range of policy levers, adjusting revenue collection, spending and borrowing in roughly equal measure. While the resulting effects on local mean income and the employment rate are small, our cost-per-job estimate of €33,000 corresponds closely to that of Serrato and Wingender (2016) and falls within the range of estimates reported in the spending multiplier literature, albeit more on the lower end of the spectrum. For Finland specifically, our estimate is lower than the spending multiplier reported by Räsänen and Mäkelä (2021), using regional variation in defense and immigration spending. The fungibility of grant revenue, as expressed through the mix of local policy adjustments, may be a contributing factor. The weaker effects on income and employment rate may in turn stem from the observation that fewer residents commute to work in other municipalities, gaining utility through time savings rather than higher income. Finally, although our findings are more informative about a revenue shock than the typical spending shocks studied in this literature as well as the the place-based policy literature, they suggest the endogenous response of local government cannot be overlooked.

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. In Section 5.5, we provide some robustness checks. Finally, Section 6 concludes.

[7]For an earlier review of studies on spending multipliers, including estimates from the macroeconomics literature, see Ramey (2011).

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.[8] Municipalities are responsible for providing a wide range of services, including most notably health and social services[9], 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% of GDP.

Figure 1 shows a breakdown of the spending categories and income sources for Finnish municipalities in 2014.[10] 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.[11] 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

[8]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.

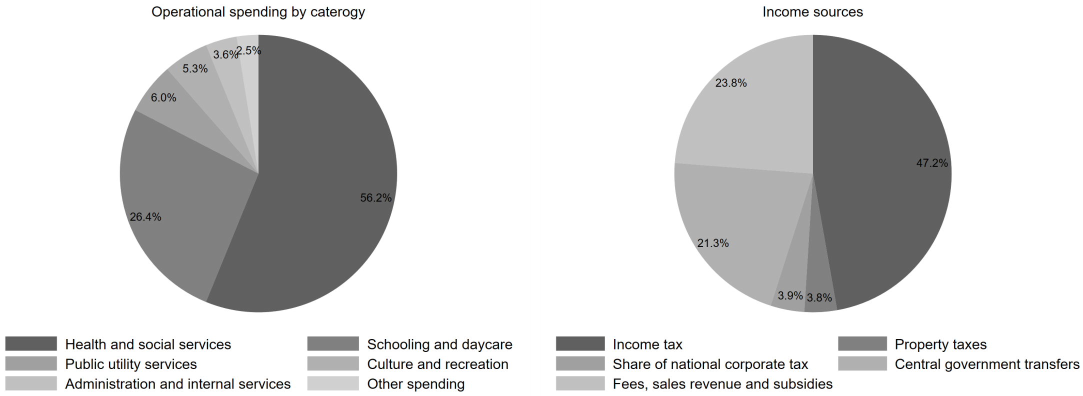
[9]Since 2023, health and social services are no longer organized by municipalities but by larger regional authorities. This paper studies a time period during which municipalities were still responsible for providing those services.

[10]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.

[11]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.

subsidies (other than central government transfers).

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



2.2 The central government transfer system

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 sickness of the population) and attributes reflecting the costs of providing the services (e.g., population density). For example, in 2015, every 7 to 12-year-old resident of a municipality was considered to cost 7,269.02 euros while each resident whose native language was neither Finnish or Swedish was considered to incur an additional 1,893.80 euros in costs. 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), and the central government covers any remaining imputed costs. For instance, when the imputed costs total 5,000 euros per resident, a municipality would receive 1,479.1 euros per resident in central government transfers. Because the transfers are based on imputed costs, municipalities have no incentive 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 national average. Imputed revenues per resident below (above) the national average 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).

2.3 The 2015 reform of central government transfer system

In 2015, the central government transfer system was reformed. The objective of the reform was to simplify the system, take into account previously under-represented cost determinants, and incentivize municipalities to develop their local economy. Some components that determined cost-based compensation were removed, while others were added, and the calculation methods for imputed costs were adjusted. For instance, compensation for the share of population with a native language other than Finnish or Swedish 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. The reform also placed more emphasis on tax revenue equalization, which shifted from a zero-sum redistribution model to a source of net financing for municipalities. Compensation based on age-group specific imputed costs was cut, while more defined cost determinants received more weight. Appendix A describes in more detail how the weights of different municipality characteristics changed in the reform.

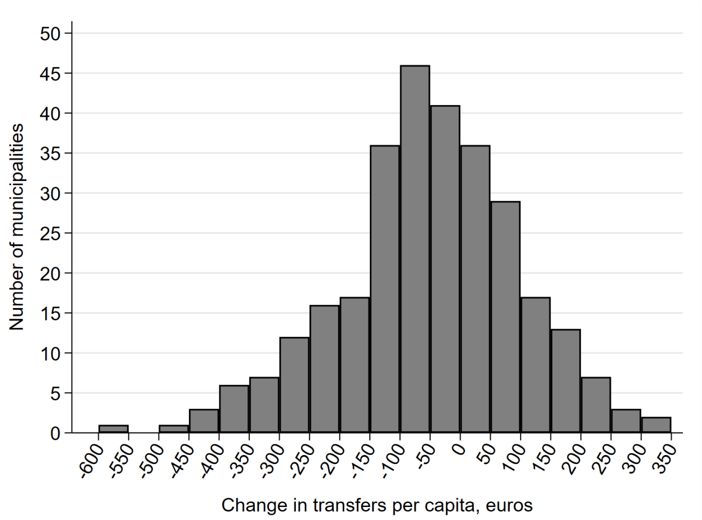
The 2015 reform was revenue neutral on aggregate; total central government financing remained unchanged, but funds were redistributed between municipalities. About one-third of municipalities received more central government financing, while two-thirds lost. The annual amount of redistributed transfers 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.

2.4 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 2 shows the distribution of imputed grant changes. As can be observed in the figure, many municipalities were almost unaffected by the transfer reform. Many others however, saw large changes in their grant revenue.

Figure 2: 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 1, 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 is 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.

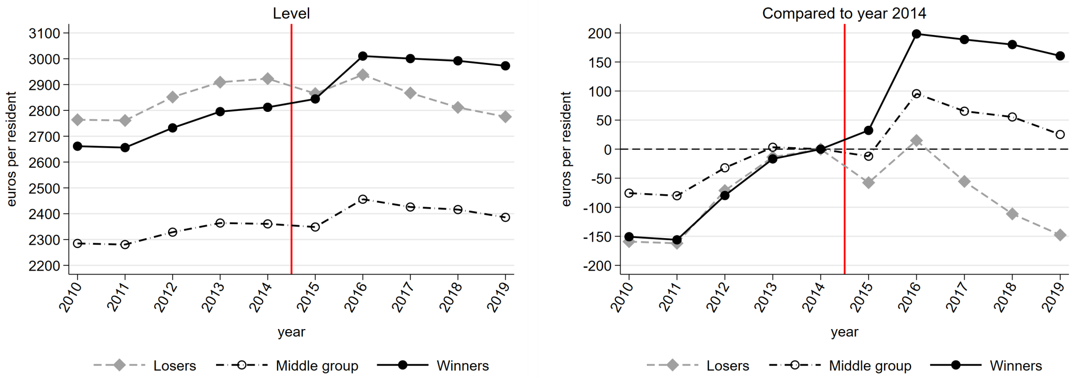
Table 1: Summary statistics of central government transfers

	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.

Tracing the evolution of grants per resident in Figure 3 (left panel) reveals that until the reform, the losers received somewhat higher grant revenue than the winners. 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 virtually 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.

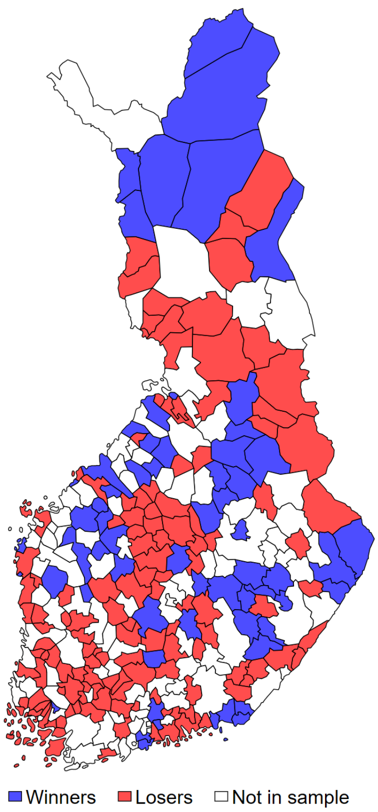
Figure 3: Means of received central governments per resident



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 4 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.

Figure 4: 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.[12] Data on central government transfers and their imputed changes due to the 2015 reform are provided by the Association of Finnish Municipalities (Kuntaliitto) and the Ministry of Finance. All data is 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–

[12]The data are provided by Statistics Finland and can be freely downloaded from the web.

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. The unit of observation is a municipality.

3.2 Descriptive statistics

Table 2 shows pre-reform means (reference year 2014) of the key variables of interest. As before, we divide the sample in 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. Despite this, employment rates hardly differed between the two groups, probably because a larger share of residents in loser municipalities commuted to other municipalities.[13] 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.

[13] The difference in commuter share 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.

Table 2: Means of dependent variables in year 2014

	Winners	Losers	Middle
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
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 population (%)	67.5	68.4	69.6
Share of employed working outside of municipality (%)	31.4	42.3	39.2
Mean disposable cash income per resident (€)	19,029	19,334	19,616
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

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.

Table 3 tabulates means of demographic and geographical municipal characteristics. We find that 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 or Swedish, the official languages in Finland). Also this component was added into the grant formula following the 2015 reform.

Cumulative population growth and 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.

As the winners and losers were oppositely affected by the transfer reform, it is natural to see some differences in the socio-economic features that correlate with the grant determinants. It is reassuring however 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 other municipal characteristics in year 2014

	Winners	Losers	Middle
Socio-economic 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
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 3 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, 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 5, 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 d^W_{post}, and then subtracting the difference in the pre-reform year t_0, d^W_{pre}. Similarly, the effect on the losers is the difference between d^L_{post} and d^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:

$$TE^{DiD} = d^{WL}_{post} - d^{WL}_{pre}. \tag{1}$$

As can be observed from Figure 5, this is equivalent to:

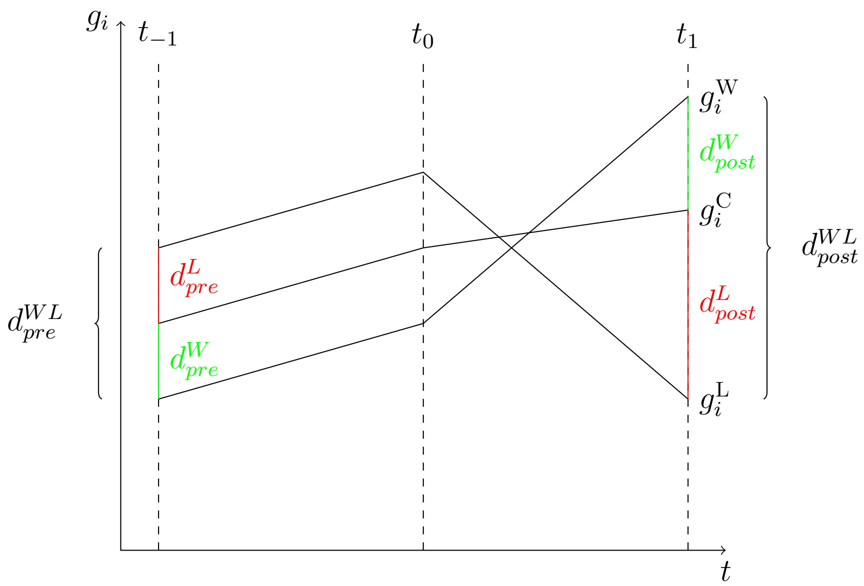
$$(d^W_{post} - d^L_{post}) - (d^W_{pre} - d^L_{pre}). \tag{2}$$

This can in turn be rewritten as follows;

$$(d^W_{post} - d^W_{pre}) - (d^L_{post} - d^L_{pre}), \tag{3}$$

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 5: 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. d_{pre}^{W} and d_{pre}^{L} represent the winners' and losers' differences in grants relative to the counterfactual in the pre-reform period, while d_{post}^{W} and d_{post}^{L} and represent their post-reform differences. Finally, d_{post}^{WL} and d_{pre}^{WL} represent the post and pre-reform differences comparing winners to losers.

This effect can be interpreted as the total absolute change in variables such as grants, tax rates and spending 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.

The main identifying assumption is that winner and loser municipalities share the same counterfactual. This we cannot prove, much like a common trend assumption cannot be proven. However, as for the common trend assumption, we can show that these municipalities follow similar trends before treatment, i.e. before the reform. This is very clear from the event-study-like plots we show below. Going even further, we can demonstrate that the municipalities are not just similar in trends but also in levels, such that we could take a simple difference following a random assignment logic. However, in what follows we allow for time-invariant differences between the two groups and opt for difference-in-differences estimation.

Interpretation and external validity

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. While we believe this to be a meaningful causal effect, under the additional assumption of symmetry, we can scale our estimate to be interpreted as the average treatment effect on winner (or loser) municipalities.

Recent work by Helm and Stuhler (2024) has examined to what extent local governments respond symmetrically to positive and negative budget shocks. While collective choice models predict symmetric adjustments, existing empirical work has found evidence of asymmetric responses. This has been interpreted as a bias towards fiscal expansion. However, Helm and Stuhler (2024) argue asymmetry may arise in the short-term due to sluggish adjustments (or bad research design).

To identify the causal effect of grant changes, Helm and Stuhler (2024) exploit Census revisions of population counts which trigger adjustments in the fiscal equalization scheme, similar to Serrato and Wingender (2016). They find indeed that municipal budgets adjust with a lag and even more so, that these adjustments are symmetric in the long run. Specifically, they find that spending and investment patterns adjust within 4 years. The tax response on the other hand can take a decade or longer.

In contrast to the slow tax response of German municipalities, our results in Section 5.2 show that the winner and loser municipalities in the Finnish reform adjusted their municipal finances rather quickly, with post-reform differences stabilizing towards the end of the panel (2015–2019). Therefore, our results are likely to capture most of municipalities' responses to the reform. Additionally, for most outcomes, the post-reform trajectories of the winners and losers seem to be split rather symmetrically around the extrapolated pre-reform trend, making it reasonable to assume a symmetric response.

Assuming symmetry, halving our estimates roughly represents the effect of the reform on the winning (or losing) municipality. Since the winning group of municipalities won on average slightly less grant revenue than the losing municipalities lost, halving the estimate is not exactly correct. To precisely determine the effect on the winner or loser, we need to scale the estimate proportional to the amount of grants allocated to the winner or loser. This in turn requires the assumption of linearity.[14] Given our estimation method, this assumption does not impose any additional restriction.

For ease of interpretation then, we fully extend the linearity assumption in

[14]Suppose, for example, that the winners on average gained €100 per capita, while the losers lost €200 per capita. Then we would first need to assume that the effect of losing €200 per capita is twice the effect of losing €100, i.e. the effect is linear. Secondly, we need to assume that the effect of losing €100 is the same as winning €100, i.e. symmetric, in order to claim that the effect of winning €100 per capita equals one third of the estimated effect of winning €300 per capita grant relative to the losers. In reality, winners gained on average €132 per capita and losers lost on average €172 per capita.

Section 5.5 to discuss our estimates in terms of euro per capita effects. However, the estimates we will present in Section 5 are purely the relative treatment effects of winners against losers, without assuming symmetry of the effects of losses and gains, or that the effect of marginal grant changes is linear.

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 4.

$$winner_i = \begin{cases} 0, & \text{if } \hat{g}_i < \text{€-50} \\ 1, & \text{if } \hat{g}_i > \text{€}50 \end{cases} \tag{4}$$

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

$$Y_{it} = \sum_{t \neq 2014} \beta_t * winner_i * D_{it} + \delta_t + \eta_i + \epsilon_{it}, \tag{5}$$

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 5.4.

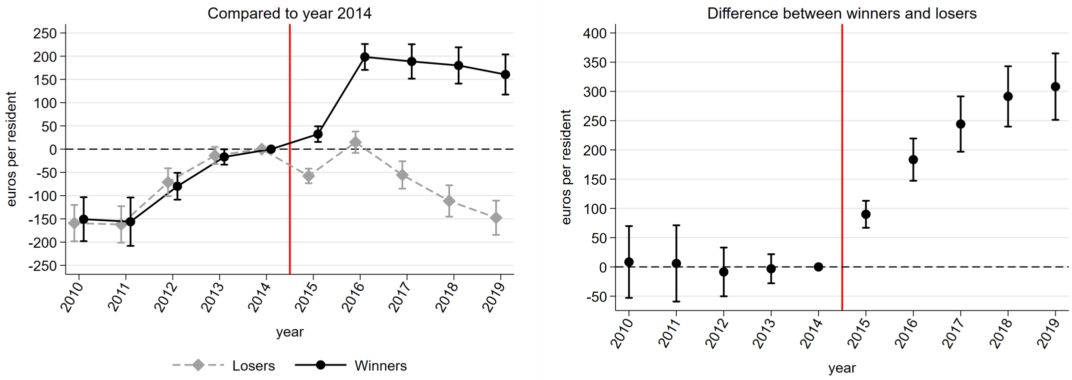
5 Results

5.1 Central government transfers received

In Figure 6, 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 5. 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 6: Development of central governments per resident



Notes: The figure illustrates the effect of the reform on central government grants per resident. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

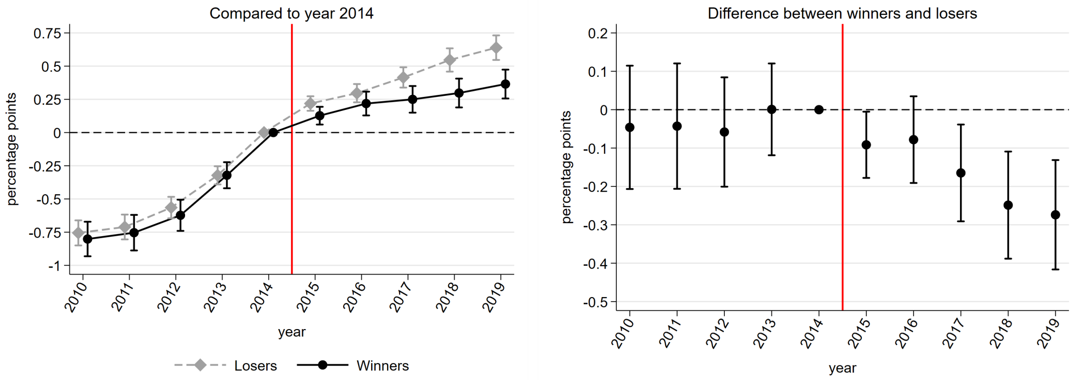
5.2 Municipality responses

In Figures 7-13, we analyze how municipalities adjusted their fiscal policy in response to changes in central government transfers.

Figure 7 shows that municipal income tax rates of winners and losers followed the same upward 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 21,420 euros in 2014. Thus, a 0.3%-point lower tax rate implies a 65 euro increase in disposable income. This median income, however, is conditional on having taxable income and not everyone has it. So, the 65-euro increase in taxable income would concern the median taxpayer, not the median resident.

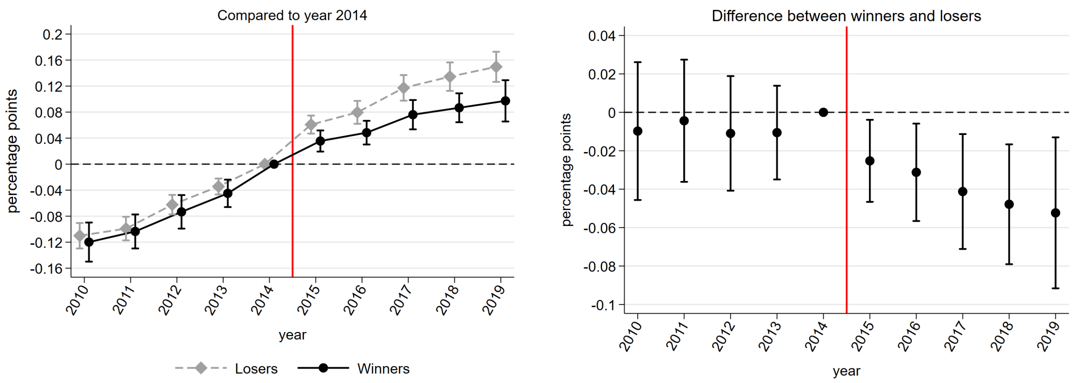
In Figure 8, the general property tax rates follow qualitatively the same development as the income tax rates. The general property tax is levied on land and most non-residential buildings. The property tax rates of winners and losers developed similarly until 2014, but their trends started to diverge 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%.

Figure 7: Development of municipal income tax rates



Notes: The figure illustrates the effect of the reform on municipal income tax rates. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

Figure 8: Development of general property tax rates



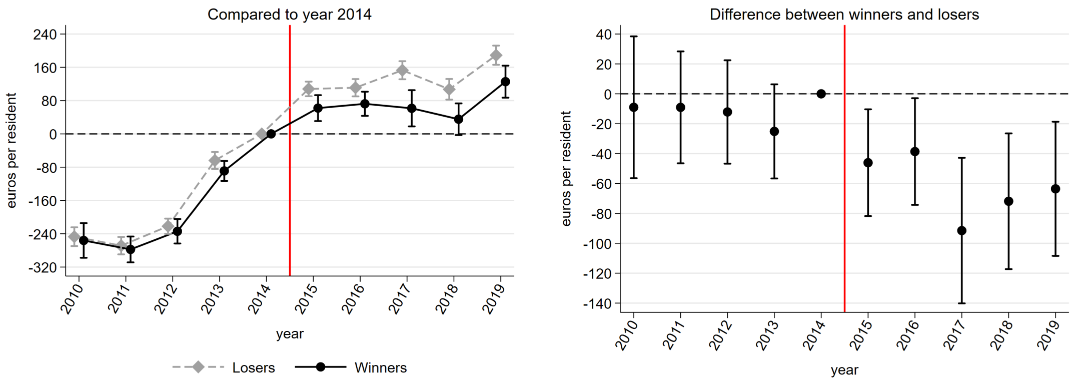
Notes: The figure illustrates the effect of the reform on the general property tax rate. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

Given the divergence of winners' and losers' tax rates, it is unsurprising that also total tax revenues diverged after the reform (Figure 9). 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.

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 10, 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.

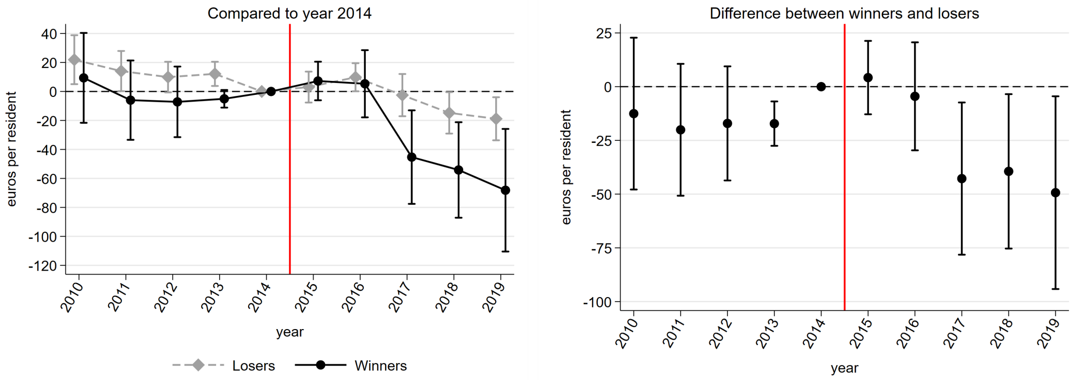
Based on the estimates shown in Figures 9 and 10, the tax revenues and the service fee collections of the winner municipalities decreased by approximately €100–150 per resident, after the reform, compared to the losers. The central government transfers of the winners increased by about €300 per resident compared to losers, on average. This would imply that up to half of the grant shock was channeled to private consumption through lower taxes and fees collected from residents and firms.

Figure 9: Development of tax revenue per resident



Notes: The figure illustrates the effect of the reform on tax revenue per resident. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

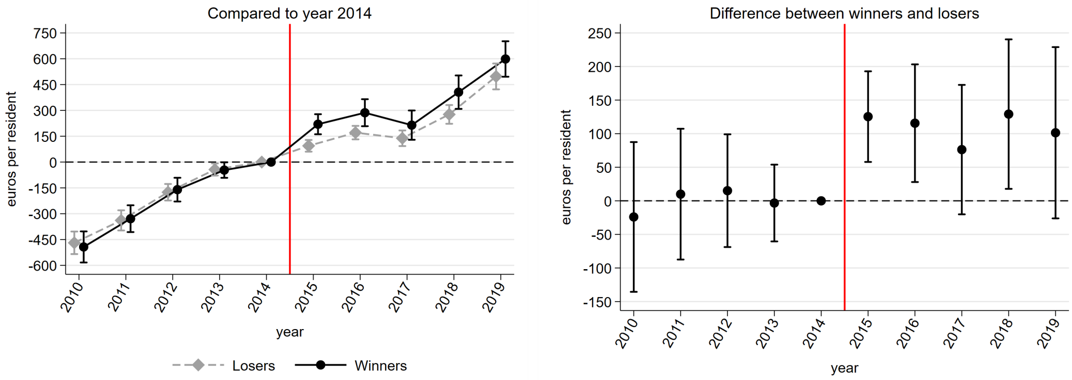
Figure 10: Development of collected service fees per resident



Notes: The figure illustrates the effect of the reform on revenue from service fees. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

The effect of the reform on municipal spending is shown in Figure 11. 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. In both groups, total spending increased throughout the period of analysis (in real terms), reflecting an overall increase in the demand for public services, mostly due to aging population. The difference between the winners and losers was very stable until the reform, but afterwards, the winners' expenses increased by roughly €100 per resident compared to the losers. The difference is quite stable over time, although the confidence intervals are relatively wide.

Figure 11: Development of total operational spending per resident



Notes: The figure illustrates the effect of the reform on operational expenditure per resident. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

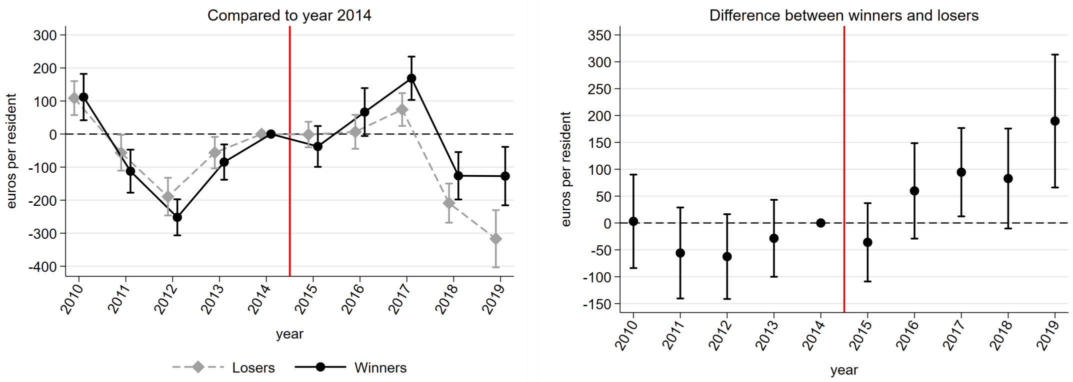
In addition to increased spending and reduced revenues, the winner municipalities seem to have used part of the increase in government transfers to run surpluses (or to cut deficits). In Figure 12, the annual contribution margin[15] of the winners increased by approximately €100 per resident compared 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 13 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 12), which reduces the

[15]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 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.

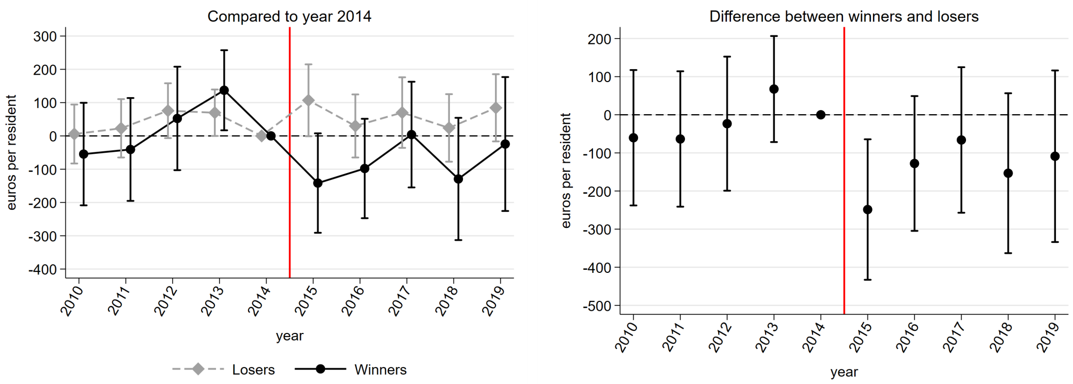
need for borrowing. Borrowing is a fast means of responding to income shocks, and might be favored especially during negative shocks to avoid too sudden spending cuts or tax increases.

Figure 12: Development of the annual contribution margin



Notes: The figure illustrates the effect of the reform on the annual contribution margin per resident. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

Figure 13: Development of net long-term borrowing per resident



Notes: The figure illustrates the effect of the reform on net long-term borrowing per resident. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

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% reduction).[16] 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 6-13. 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. * denotes significance at p<0.10, ** p<0.05, *** p<0.01.

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.

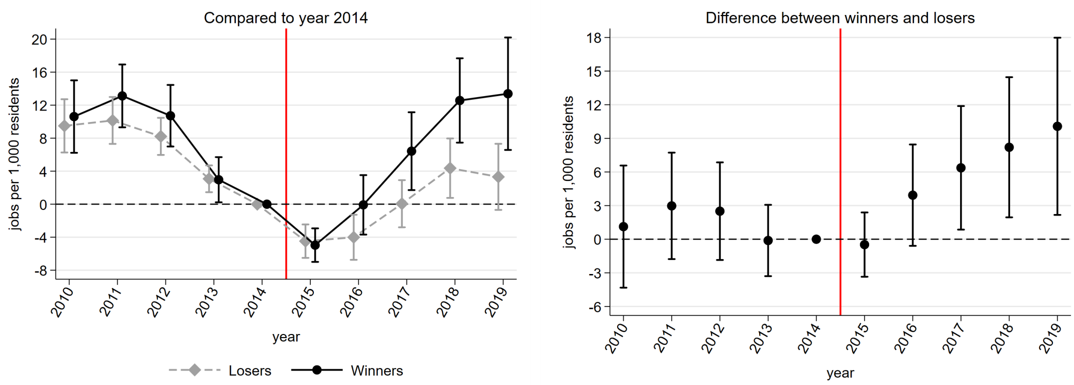
Figure 14 shows that the number of jobs per 1,000 residents decreased in both the winner and the loser municipalities before the reform. This is consistent with economic recovery following a prolonged recession in the aftermath of the 2007–2008 financial crisis and the European sovereign debt crisis. The trends in the winner and loser groups were close to parallel until the reform, but then diverged,

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

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 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.

Figure 14: Development of the number of jobs per 1,000 residents



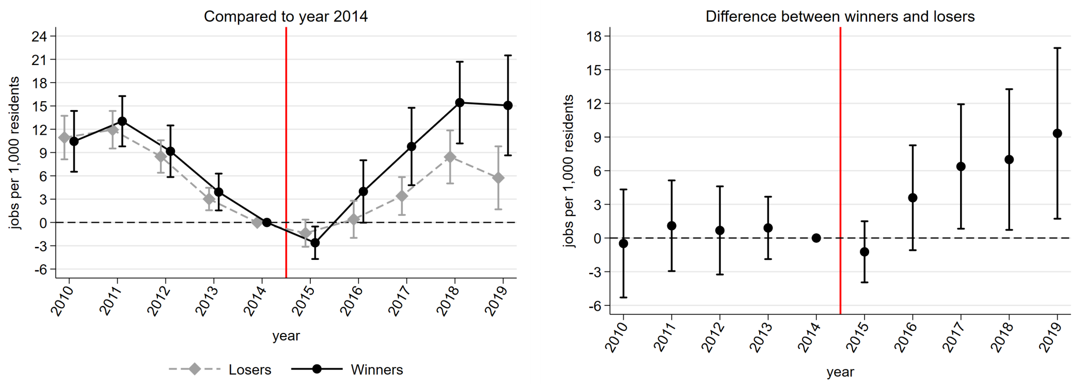
Notes: The figure illustrates the effect of the reform on the number of jobs per 1,000 residents. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

In Figures 15 and 16 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 15). 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 16) did not develop differently after the reform[17]. This contrasts sharply with the findings

[17]Municipal sector jobs are part of the public sector. Rest of public sector jobs are state-

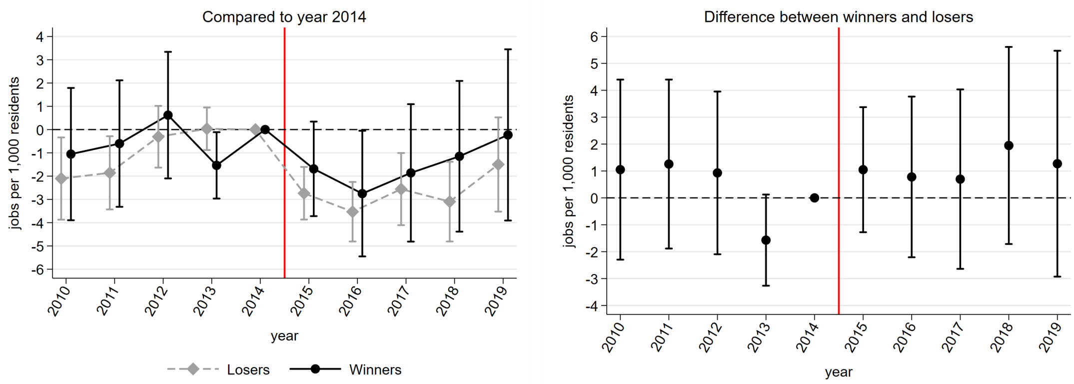
of Räsänen and Mäkelä (2021), who report employment effects only in the public sector.

Figure 15: Development of the number of private sector jobs per 1,000 residents



Notes: The figure illustrates the effect of the reform on the number of private sector jobs per 1,000 residents. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

Figure 16: Development of the number of municipal sector jobs per 1,000 residents



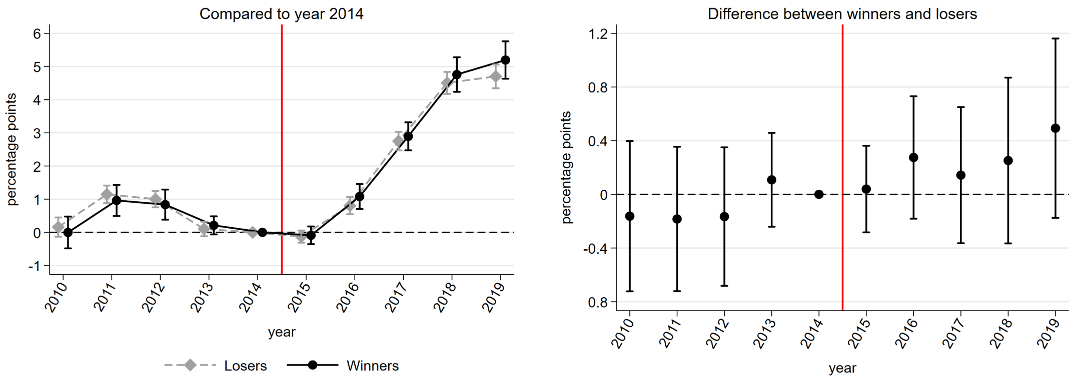
Notes: The figure illustrates the effect of the reform on the number of municipal sector jobs per 1,000 residents. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

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.

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 17 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.

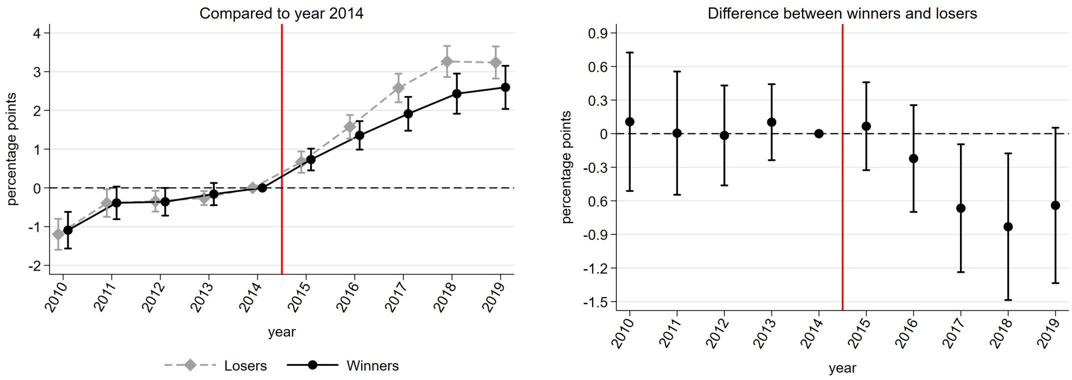
The post-reform increase in jobs without a simultaneous increase in employment rate can only be reconciled through increased net-migration, or a change in commuting patterns. In Figure 18, we therefore estimate the effect on the share of a municipality's employed population working outside of the municipality 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.

Figure 17: Development of the employment rate



Notes: The figure illustrates the effect of the reform on the employment rate. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

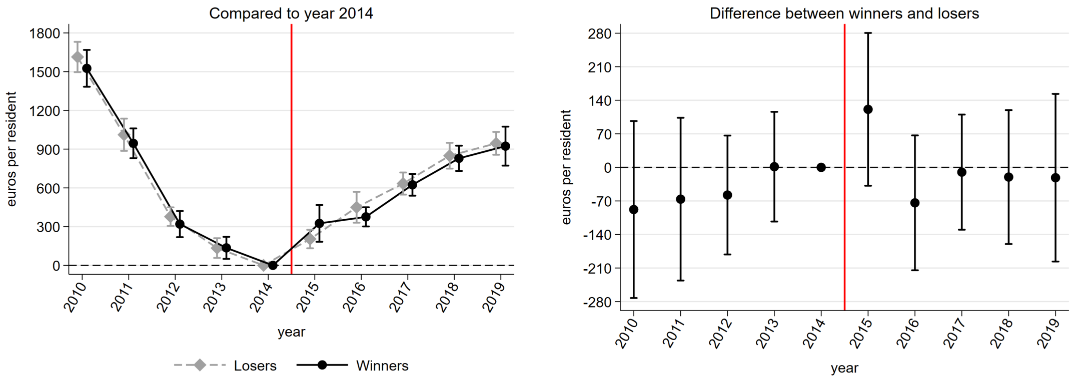
Figure 18: Development of the share of out-commuters



Notes: The figure illustrates the effect of the reform on the share of workers commuting to other municipalities. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

Finally, Figure 19 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. Throughout the analysis period, winner and loser municipalities followed the same u-shaped income pattern, consistent with the post-crisis recovery. Besides an income shock in 2015, post-reform coefficients are close to zero and statistically insignificant. A likely explanation for a weak effect on disposable income is that reductions in service fees and property taxes do not affect disposable monetary income. Also, mean income is calculated over all residents, including those who do not receive (taxable) income (also children), meaning that the observed small decrease in winners' local income tax rates might have had only a minor effect on mean disposable income, buried under noise..

Figure 19: Development of mean disposable income per resident



Notes: The figure illustrates the effect of the reform on disposable monetary income per resident, defined as gross income net of transfers paid and received. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

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 significant 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

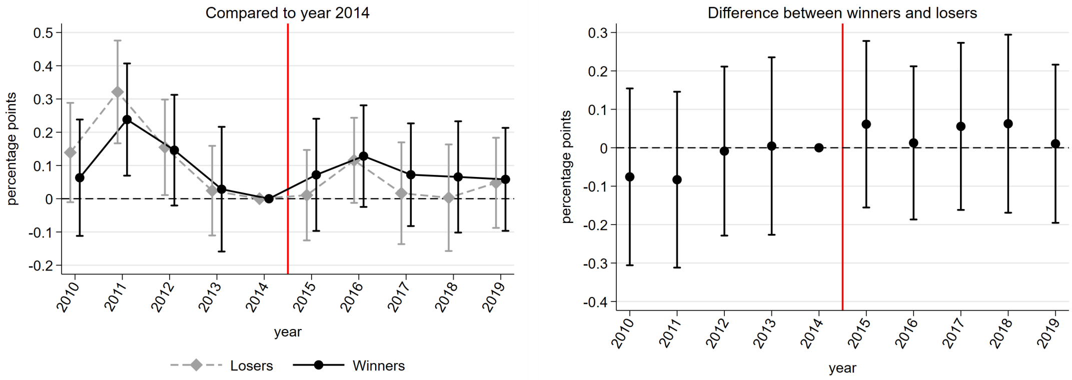
Notes: This table reports the equally weighted linear combination of the DiD estimates for years 2018 and 2019 that were estimated in Figures 14-19. Each regression has 1980 observations: a ten-year balanced panel of 198 municipalities. 63 municipalities belong to the winner group and 135 belong to the loser group. All the regression include municipality and year fixed effects. Standard errors are clustered at the municipality level. * denotes significance at p<0.10, ** p<0.05, *** p<0.01.

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.

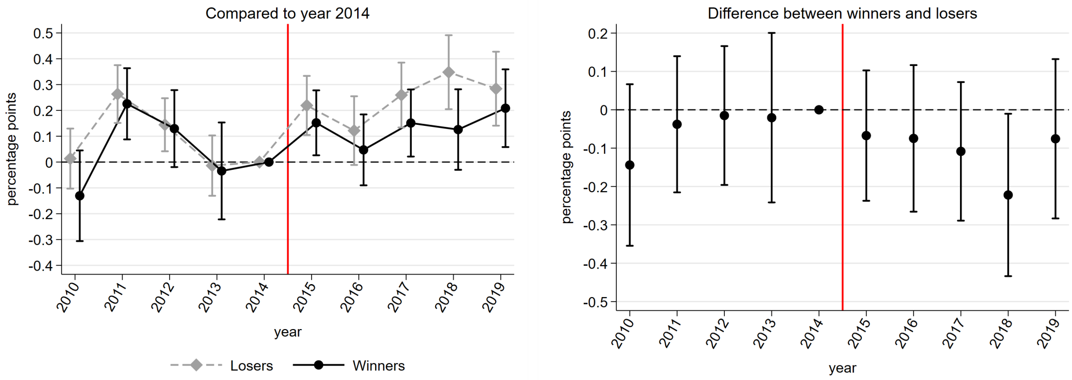
Figures 20-22 show dynamic DiD estimates for the in-migration, out-migration and net migration rate. Overall, pre-reform trends for winners and losers are very similar, and the DiD coefficients are close to zero for the pre-reform period. After the reform, DiD estimates for in-migration (Figure 20) are positive but close to zero and insignificant. For out-migration (Figure 21), 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 22) are positive, but only the estimate for 2018 is statistically significant.

Figure 20: Development of in-migration rates



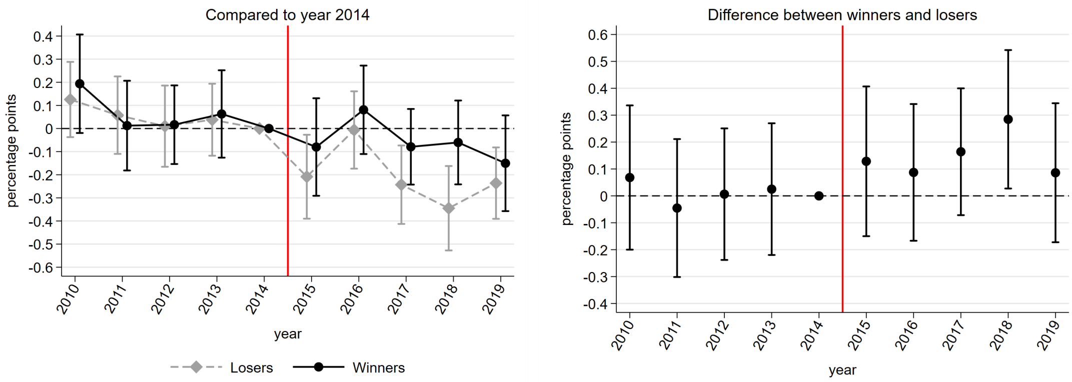
Notes: The figure illustrates the effect of the reform on the in-migration rate. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

Figure 21: Development of out-migration rates



Notes: The figure illustrates the effect of the reform on the out-migration rate. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

Figure 22: Development of net migration rates



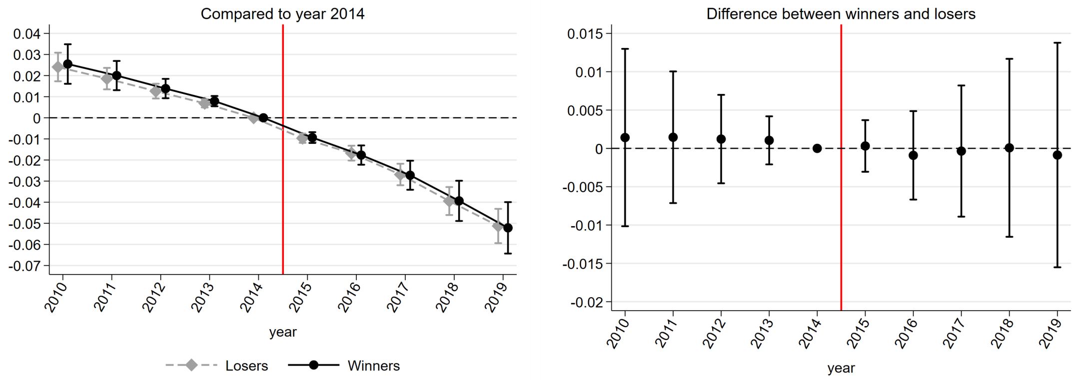
Notes: The figure illustrates the effect of the reform on the net migration rate. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

In Figure B.1, 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 23 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 1000 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.

Figure 23: Development of the logarithm of population



Notes: The figure illustrates the effect of the reform on the logarithm of population. The left panel shows estimates of group-specific year effects for winner and loser municipalities compared to 2014, and the right panel shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

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 20-23 and B.1. 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. * denotes significance at p<0.10, ** p<0.05, *** p<0.01.

Accounting for the bilateral nature of migration

Next we perform a robustness check 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 4 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 migration intensity, $\overline{G_i}$, which we interact with time dummies.

Specifically, we estimate:

$$Y_{it} = \sum_{t \neq 2014} \beta_t * winner_i * D_{it} + \sum_{t \neq 2014} \gamma_t * \overline{G_i} * D_{it} + \delta_t + \eta_i + \epsilon_{it}, \qquad (6)$$

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

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

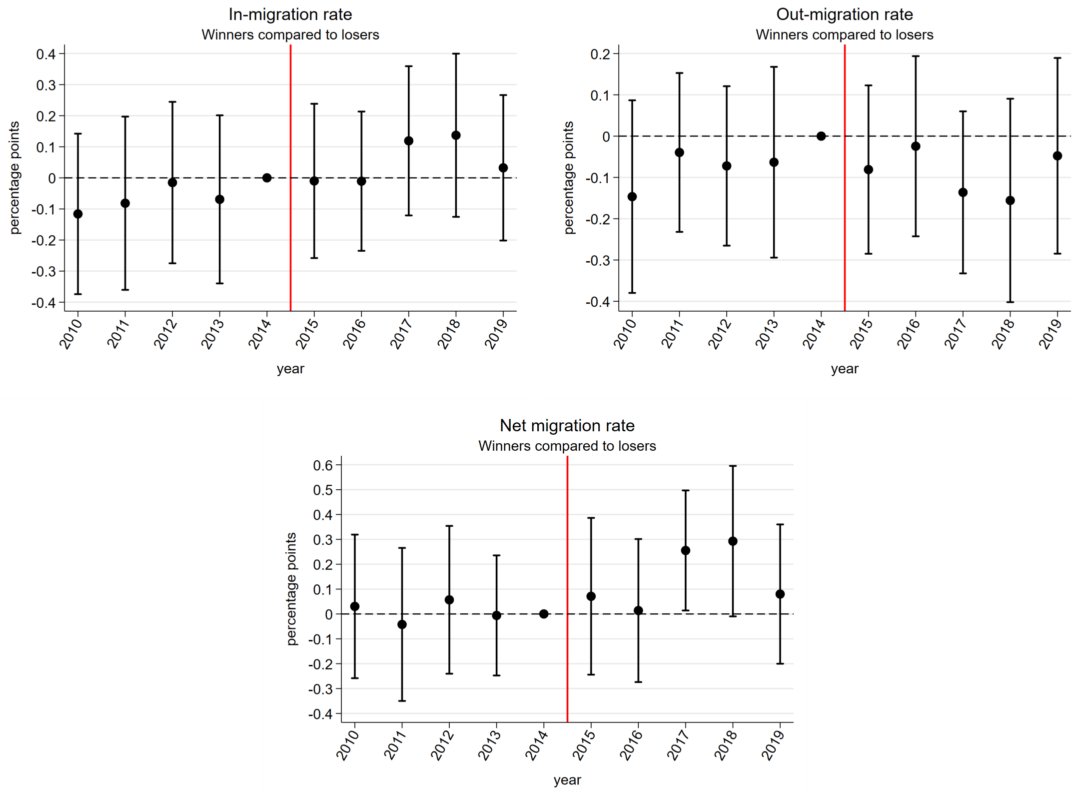
where M_{ik} represents the pre-reform migration flow from municipality i to municipality k, M_{ki} is the pre-reform migration flow from municipality k to municipality i, and $\hat{g_k}$ is the imputed grant change of municipality k.[18] Thus, the weight municipality k's grant change receives, reflects 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 24. 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 B.2.

[18]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 24: Migration rates controlling for transfer changes in other municipalities



Notes: The figure illustrates the effect of the reform on migration rates. The figure shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

5.5 Continuous treatment

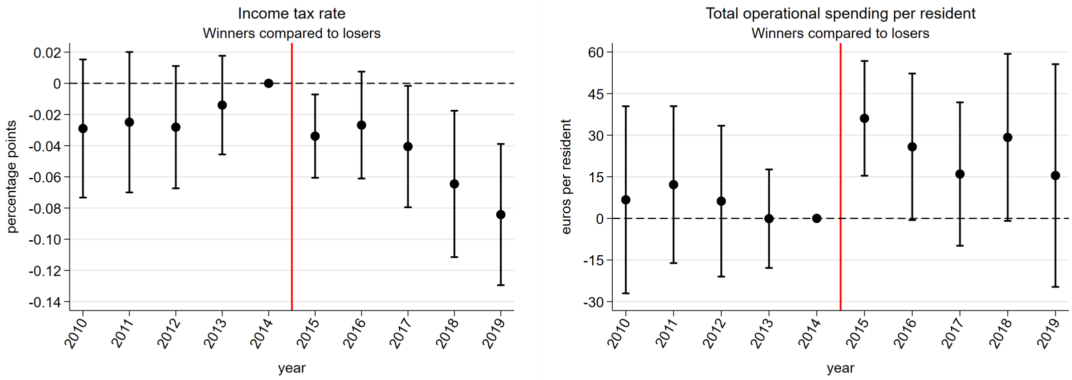
In the main specifications, we compared the development of municipalities that received additional central government financing of more than €50 per resident to those that lost more than €50 in a binary fashion. Using the same sample, we now perform a robustness check, in which we use a continuous measure of grant changes. Because the observed change in grants is endogenous to changes in the socio-economic features of a municipality, we use imputed grant changes and estimate the following equation:

$$Y_{it} = \sum_{t \neq 2014} \beta_t * \hat{g}_i * D_{it} + \delta_t + \eta_i + \epsilon_{it}, \tag{7}$$

in which $\hat{g_k}$ 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 is therefore that a €100 increase in grants in the 2015 reform changes the dependent variable by β_t in year t.

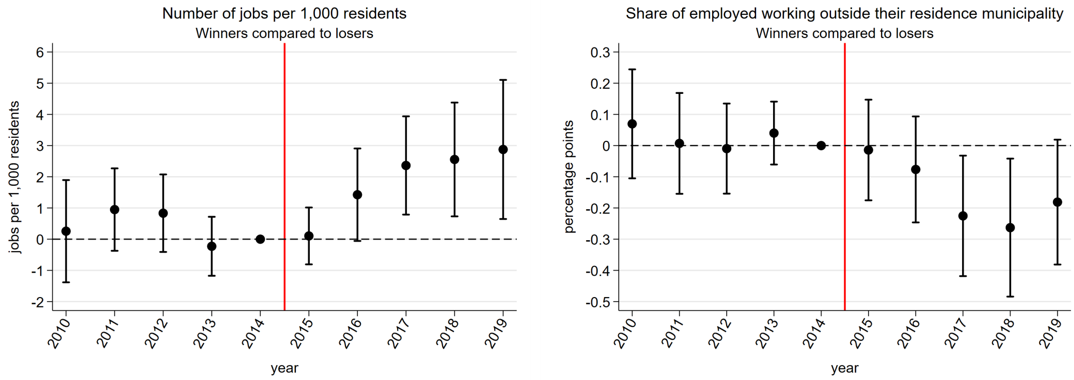
We apply this approach to our main outcomes; Figure 25 presents estimates for local fiscal policy (income tax rate and operational spending), Figure 26 for the labor market (number of jobs and share of out-commuters) and Figure 27 for migration rates. Our findings remain robust to a continuous treatment specification. While there is a clear effect on fiscal policy, jobs and commuting patterns, the evidence for changes in migration patterns is weak.

Figure 25: Municipal finances: effect of an imputed €100 increase in transfers per resident



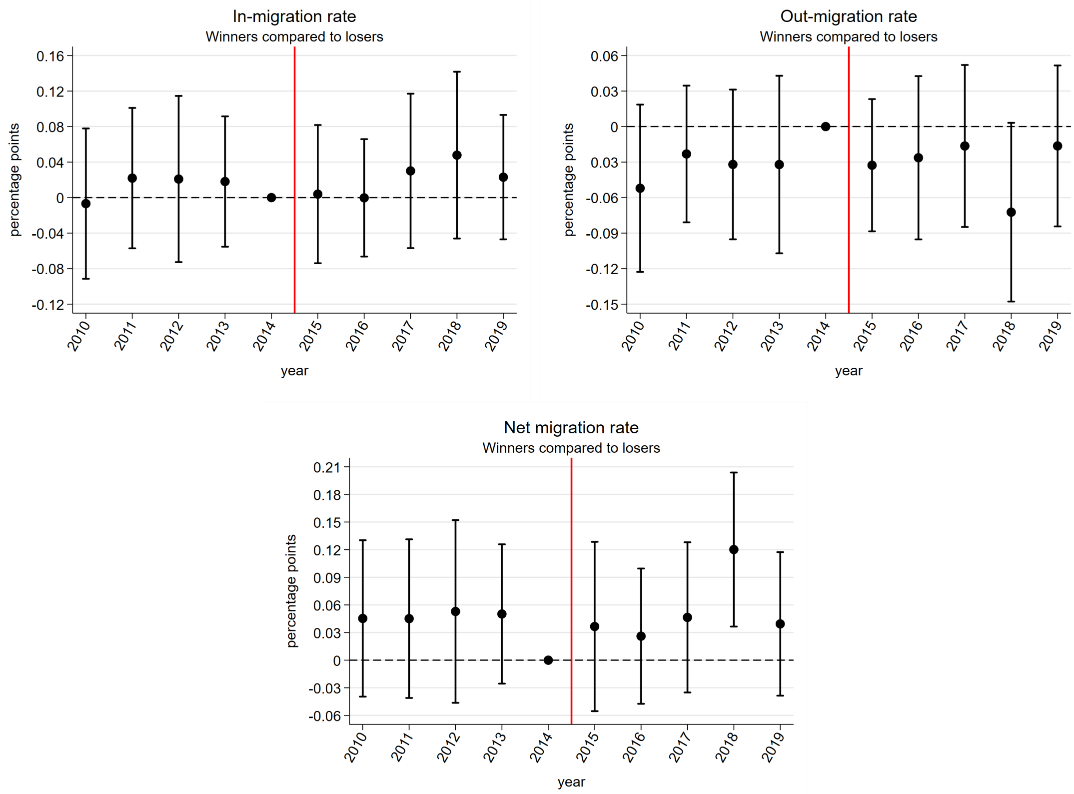
Notes: The figure illustrates the effect of the reform on the municipal income tax rate and operational spending per resident. The figure shows the estimated effect of an imputed €100 increase in grants per capita compared to 2014. The whiskers indicate 95% confidence intervals.

Figure 26: Labor market: effect of an imputed €100 increase in transfers per resident



Notes: The figure illustrates the effect of the reform on the number of jobs per 1,000 residents and the share of workers commuting to other municipalities. The figure shows the estimated effect of an imputed €100 increase in grants per capita compared to 2014. The whiskers indicate 95% confidence intervals.

Figure 27: Migration rates: effect of an imputed €100 increase in transfers per resident



Notes: The figure illustrates the effect of the reform on migration rates. The figure shows the estimated effect of an imputed €100 increase in grants per capita compared to 2014. The whiskers indicate 95% confidence intervals.

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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Appendix

A Grant reform

Table A.1 describes the importance of different municipality characteristics in the grant allocation formula before and after the reform of 2015.

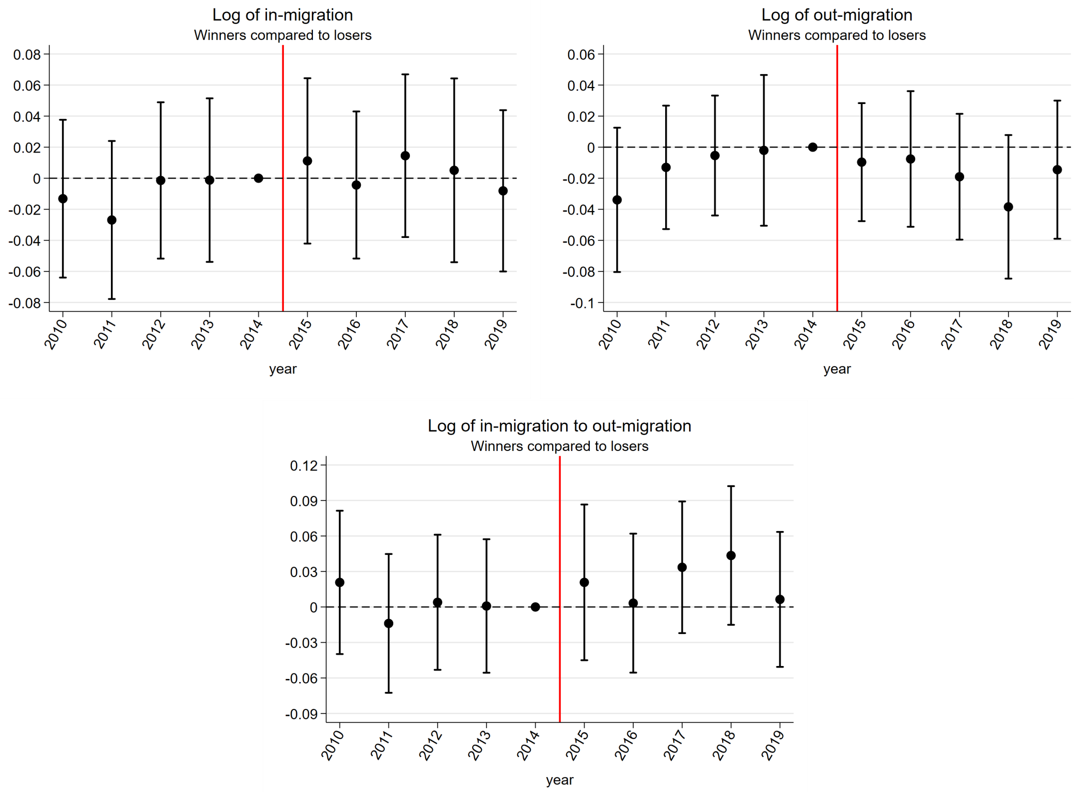
Table A.1: Fiscal importance grant determinants before and after the reform

	2014 grant system		2015 grant system	
	EUR, million	Share of total	EUR, million	Share of total
Age structure	6252	0.728	4587	0.545
Sickness index	705	0.082	1551	0.184
Unemployment rate	143	0.017	121	0.014
Disability	28	0.003		
Child protection	80	0.009		
Bilinguality	30	0.003	48	0.006
Foreign languange	13	0.002	133	0.016
Population density	86	0.01	48	0.006
Archipelago	17	0.002	4	0
Sapmi homeland	4	0	4	0
Basic compensations	290	0.034		
Population change	4	0		
Urban area	36	0.004		
Remoteness	63	0.007	113	0.013
Tax base equalization	-50	-0.006	673	0.08
Education background			41	0.005
Workplace self-sufficiency			195	0.023
Additions/reductions	892	0.104	927	0.11
Total	8592		8425	

B Additional results

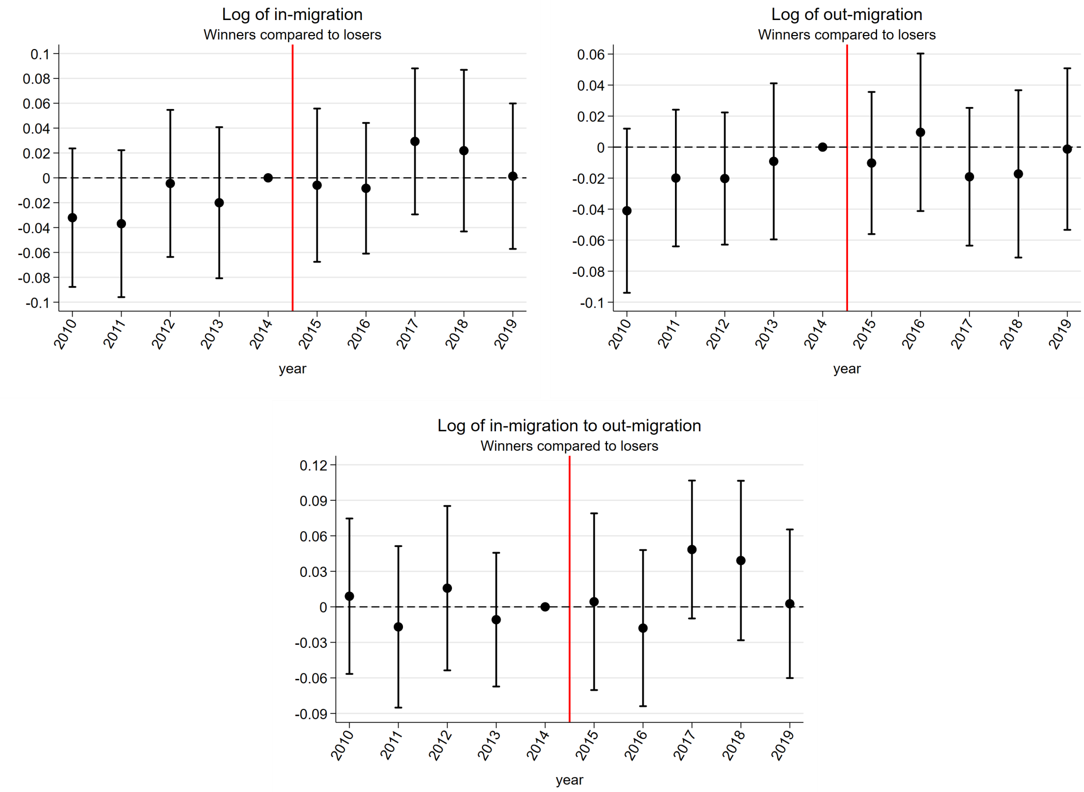
B.1 Logarithm of migration

Figure B.1: Log of migration



Notes: The figure illustrates the effect of the reform on logarithms of migration. The figure shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

Figure B.2: Log of migration controlling for transfer changes in other municipalities



Notes: The figure illustrates the effect of the reform on logarithms of migration. The figure shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.

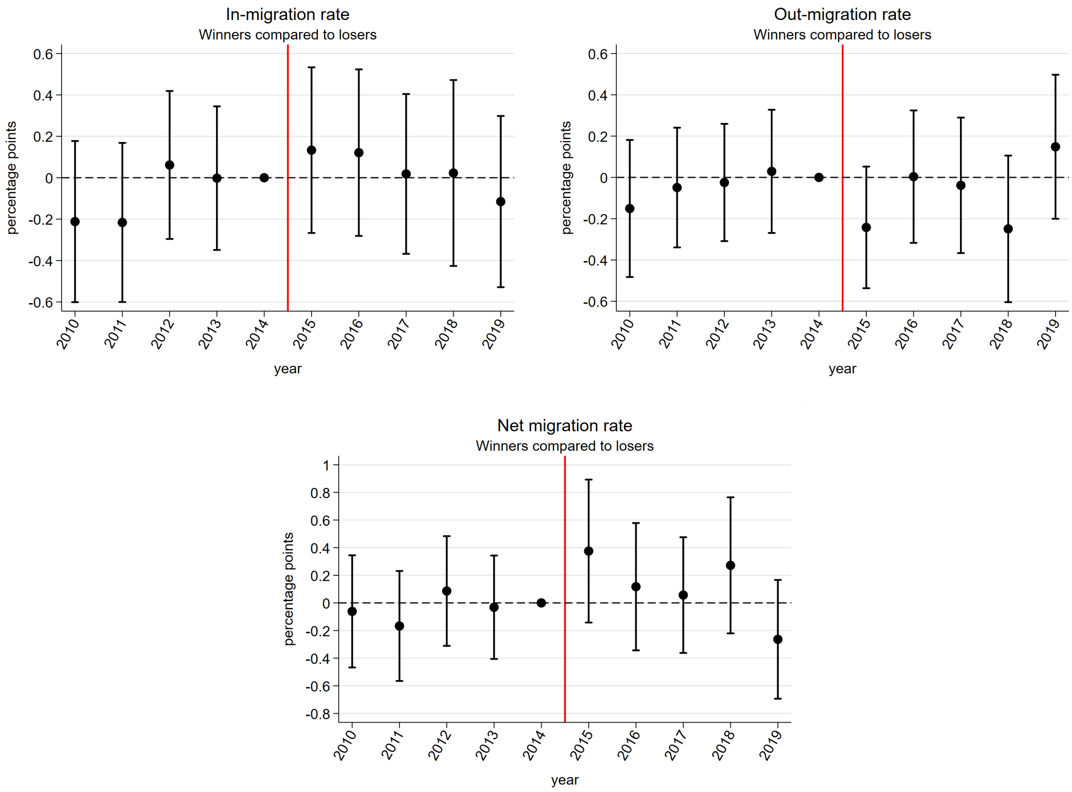
B.2 Migration response in prime-working-aged population

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 B.3 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 B.3: Migration rates of 25–54-year-old individuals



Notes: The figure illustrates the effect of the reform on migration rates of 25-54-year-old individuals. The figure shows estimates for the difference between winners and losers compared to 2014. The whiskers indicate 95% confidence intervals.