Local Democratization and Public Finance: Lessons from the 1870 Municipal Reform in Cities of the Russian Empire*

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Abstract

This paper examines the influence of subnational democratization on fiscal outcomes and public goods provision, taking advantage of an understudied large-scale institutional innovation: the introduction of elected self-government in the cities of the Russian Empire in the 1870s and 1880s. Applying a staggered difference-in-differences design to original city-level panel datasets, I find that the reform had a large positive impact: it increased revenues and expenditures by 20–50%, enabling an expanded allocation of funds to education, healthcare, fire protection, and other areas, and significantly increased the number of primary schools. The introduction of self-governance was less consequential in cities with a greater share of merchants in the population, apparently because they were able to increase public finances in accordance with their preferences in the pre-reform period. It was also seemingly more beneficial for towns with greater religious heterogeneity and a relatively smaller Christian Orthodox population. Disaggregation of revenues and expenditures suggests that the outcomes were shaped by the preferences of both the central and provincial authorities and local communities. These results demonstrate that in an authoritarian setting, a decentralization reform bringing even limited autonomy and representation at the local level can empower new social groups and considerably improve public finance.

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Introduction

The impact of democratization on public spending, taxation, and provision of public goods has been a subject of a voluminous empirical literature, which has mostly found a positive, or at least non-negative association between democracy and redistribution, as well as other salient social, economic, and institutional outcomes (Gerring, Knutsen and Berge 2022). However, scholars have faced serious obstacles to finding valid identification strategies (Acemoglu et al. 2015), especially in cross-country research, which seem to prevail, and now they increasingly turn to subnational studies of democratization.

This project takes advantage of a large-scale institutional innovation: the introduction of elective municipal government in the cities of the Russian Empire–a large authoritarian state on the European periphery–in the 1870s. This is a case of virtually identical formal democratic institutions being imposed on a large variety of localities by means of a centralized top-down reform; such uniform treatment is obviously unattainable in country-level studies. As of 1870, there were about 7.3 million urban dwellers in the European part of the Russian Empire, which was characterized by high socio-economic and ethno-religious diversity.

The reform involved a transition of more than six hundred urban settlements from a largely bureaucratic to a limited all-estate representative rule with the electorate extended to about one tenth of a city population. Despite a curtailed franchise, which privileged commercial and industrial elites, the transformation of municipal administration relieved bureaucratic and police control and expanded fiscal and administrative autonomy of urban communities. Newly created elected city councils (*dumas*) came to decide on revenues, expenditures, and other local affairs. The democratizing potential of the reform, which created a new "civic public sphere," stood out as a big concern for the Tsarist regime (Nardova 1984, Brower 1990).

The fact that the reform was implemented in a gradual fashion, with some cities being granted new representative institutions earlier than others, helps resolve causal identification issues and gain novel insights on the effects of the introduction of self-governance on municipal revenues and expenditures. Most of the existing quantitative research on historical subna-

tional representative institutions either deals with causally remote outcomes, such as population growth over long periods (De Long and Shleifer 1993, Stasavage 2014, Stasavage 2019, Wahl 2019), or employs cross-sectional data (Hollenbach 2021; Charasz 2022). My project overcomes these limitations employing a quasi-experimental difference-in-differences identification strategy in a manner similar to recent studies of the impact of historical municipal reforms on spending and taxation in advanced democracies (Chapman 2018, 2024, 2023; Paulsen 2022). It resonates with a recent stream of studies using related methods in oder to explore the impact of local self-governance reforms in the 20th or early 21st centuries (e. g., Cascio and Washington 2014, Fujiwara 2015, Martinez-Bravo et al. 2012, Carreri, Payson and Thompson 2022, Beazer and Reuter 2022, Pierskalla 2022).

This study bears a direct relationship to a well-established literature on political and fiscal decentralization, which is expected to bring multiple benefits, such as mitigating the problems of informational asymmetry and heterogeneous preferences and harnessing inter-jurisdictional competition, as well as ensuring greater accountability of local policymakers, which, however can be offset by local elite capture (see, e. g., Mookherjee 2015, Enikolopov and Zhuravskaya 2007, Fan, Lin and Treisman 2009, Seabright 1996).

Utilizing previously untapped data (Zverinskii and Otsolig 1887), I build city-level panels of fiscal data for 1840–1884 (period-averaged) and 1870–1884 (yearly) and compare city revenues and expenditures before and after the reform, applying a staggered difference-in-differences design. I use a panoply of recently developed DiD estimators and a senstitivity test in order to ensure the robustness of my findings to violations of the parallel trends assumption and treatment effect heterogeneity. My companion project reveals a non-random timing of reform adoption by different cities, but, as I demonstrate, earlier and later adopters were unlikely to follow diverging trajectories without the reform.

According to my findings, the introduction of self-government increased city revenues and expenditures by about 20–50% in 1870–84, and this increment, in line with the central government's intentions, was attained not only (or predominantly) by increased taxation, but by a more efficient exploitation of municipal property and the opening of municipal enterprises. It

proves that even in settings with limited tax capacity, democratization can entail fiscal expansion by improving management of public assets.

Despite an objectively increased autonomy, the composition of expenditures depended on the bargaining between elected city authorities and provincial and central officials and agencies. With mixed success, the state promoted security-related spending on the military, police, prisons, and fire-fighters, whereas municipalities initiated significant expansion of education, heathcare, charities, and public contruction spending. In addition to analyzing fiscal outcomes, I also find a long-term reform impact on the actual number of primary schools.

I also examine heterogeneous reform effects, depending on socio-economic and other structural factors. It turns out that cities with a higher share of merchants in the population experienced a lower post-reform growth of public revenues and expenditures, probably because merchants had mobilized more resources and kept higher spending, meeting their needs, already before the reform. At the same time, cities with higher religious heterogeneity and a lower share of Christian Orthodox, which had often been in a disadvantaged position before the reform, seemed to derive relatively greater benefits in terms of fiscal expansion. I don't find a significant relationship between the level of industrialization and increased spending on education and healthcare (Hollenbach 2021), arguably because a class of industrialists in need of a higher-quality workforce had not yet matured in the Russian Empire, which was on the verge of an industrial "take-off." The merchants seemed to be primarily interested in the protection of their property by means of inreasing expenditures on police and firefighters.

These results are consistent with historical studies noting that instead of commercial classes and nascent industrialists, progressive-minded municipal activists, largely consisting of middle-class educated professionals, often took the lead advocating greater fiscal expansion and public goods provision (Brower 1990, Popov 2006; see also Hillis 2013 for the case of pro-spending agenda overlapping with inter-ethnic tensions). Overall, my findings demonstrate that even in an authoritarian and underdeveloped setting, a democratic transformation of local governance, bringing greater accountability and representation and unleashing the potential of civil society, can be remarkably successful.

The state of urban society in Imperial Russia and its political and economic repercussions have been recently brought into the spotlight by social scientists (Lankina 2021, Lankina and Libman 2021). Charasz 2022 considers the 1869 reform in Congress Poland (a part of the Russian Empire), which deprived most previously urban localities of their city status. "Degraded" cities received elected self-governance, but their public goods provision and administrative performance stayed relatively depressed because of the domination of landowning elites. I examine the introduction of self-government in the core European territories of the Russian Empire, contrasting them to remaining Polish cities, which were denied representative institutions.

The establishment of urban self-governance has attracted much less attention in the academic literature and public discourse than the creation of more rural-oriented provincial and district self-governance (*zemstvos*) and other liberal "Great Reforms," implemented during the rule of Alexander II, but given the concentration of industry, education, science, and culture in urban areas, it must have been no less consequential for the country's development. A pioneering monograph Nardova 1984 is focused on electoral procedures, the scope of franchise, and the social profile of deputies and mayors, but mostly neglects the reform's impact in the fiscal sphere, which lied at the heart of municipal government activities. Reform outcomes receive more attention in an insightful study Brower 1990, which offers a panoramic view of the evolution of urban life in the Russian empire in the second half of the 19th century. Pre-revolutionary statisticians and economists accumulated a large body of data on city budgets (e. g., Zverinskii and Otsolig 1887) and analyzed different aspects of self-government (Semenov 1901, Mikhailovskii 1908), but a comprehensive assessment of the reform's influence on fiscal indicators and development is still lacking. This project aims to fill this gap and contribute to a growing body of work on democratization and political and fiscal decentralization.

The paper is structured as follows: Section 1 provides a historical account of the motifs of the 1870 municipal reform in the Russian Empire (Subsection 1.1) and its implementation (Subsection 1.2); it also formulates my theoretical expectations and discusses arguments from the relevant literature on subnational democratization and decentralization (Subsection 1.3). Section 2 contains a description of the dataset, and Section 3 explains the empirical design. Sec-

tion 4 reports the baseline empirical results of the panel data difference-in-differences analysis with robustness checks (Subsection 4.1), followed by synthetic DiD estimates (Subsection 4.2), the analysis of interactions effects (Subsection 4.3), and the dynamics of different components of revenues and expenditures (Subsection 4.4). The empirical analysis is followed by the conclusion.

1 Historical Context and Hypotheses

1.1 City Governance before the 1870 Municipal Reform

The 1870 municipal reform was part of a broader program of administrative decentralization and extension of local self-governance, which was undertaken by the new emperor Alexander II as a reaction to a humiliating defeat in the Crimean War (1853–1856), waged against the Ottoman Empire, England, and France (Starr 1972). In order to promote urban development, the reformers aimed to increase the capacity of municipal administration by making it more representative, accountable to local communities, and commanding access to more financial resources (Nardova 1984).

The 1870 reform was not the first attempt to democratize municipal government in the Russian Empire, but it is commonly believed that previous institutions, introduced by Catherine II's 1785 city statute (*Zhalovannaya gramota*; see PSZR 1830, pp. 358–384), lacked real autonomy and rested on an obsolete model of estate-based representation (Nardova 1984, Brower 1990). The 1785 statute defined a category of "city dwellers" (*gorodskie obyvateli*), entitled to take part in local self-governance. The city dwellers were divided into six groups: "real city dwellers" (*nastoyashchie gorodskie obyvateli*), who owned some kind of immovable property (houses, other buildings or land) in a given city; merchants of the three guilds; members of artisan guilds; foreign subjects; honorable citizens; and common townspeople (*posadskie liudi*)–permanent residents engaged in trade and industry, but not falling into other categories. Every three years, city dwellers must have elected a "common city duma", which, in its turn,

elected a six-deputy duma (one deputy from each group). The latter should have regularly convened in order to decide on local affairs, being chaired by an elected city mayor (*gorodskoi glava*). The sphere of duma responsibilities was defined in relatively broad terms, and included provision of subsistence to city dwellers; keeping peace and order without meddling in the work of the police; promoting trade; maintaining public buildings, market squares and warehouses; increasing revenues and providing public services; adjudicating guild-related disputes.

Although Catherine II's legislation envisaged a kind of representative self-government, its practical functioning faced severe obstacles. It suffered from the lack of autonomy from provincial and imperial authorities and administrative overcentralization, which required a sanction of the Ministry of Interior to resolve local affairs. Tsarist administrators effectively saw the municipal institutions as means of control over and "part of the authoritarian ordering of the Russian city," rather than authentic self-governance (Brower 1990, p. 10). For instance, although there was a considerable demand for greater education provision on the part of urban middle classes and merchant elites, and the authorities seemingly urged them to sponsor it, benevolent inititatives faced absurd bureaucratic hurdles (Kupriyanov 2007).

Towns were typically having hard time raising funds sufficient to cover administrative costs and the provision of public goods. The law of 1785 prescribed the following revenue sources: income from urban properties,[1] alcohol licensing and customs duties, administrative fees and fines, as well as the escheat of *meshchane*, which were complemented by state subsidies. From the early 19th century, the Ministry of Interior tried to boost and regularize city finances by allowing the collection of additional fees and duties and systematizing budget planning and implementation. However, it was done in a piecemeal fashion by issuing special statutes for about 150 cities in different years (Khoz. Dep. MVD 1877, pp. 159–160). At the same time, local surveys discovered that the main potential sources of income–urban public properties, including arable land and pastures–were often neglected and lacked proper evaluation and exploitation. Urban land was arbitrarily and inapporpriately used by private individuals without

[1] *Obrochnyye stat'yi*, such as, for instance, mills, fisheries, and toll roads; more generally, municipal properties, like land or buildings, rented out at auctions or directly managed by the municipality.

commensurate or any compensation (Khoz. Dep. MVD 1877, pp. 161–162).

The malfunctioning of self-administration institutions impeded the expansion of revenues and expenditures and urban development in the Russian Empire. Whatever rudimentary representation existed, it had limited impact: voter and deputy absenteism was rampant (Tiurin 2007), and in practice, cities were often run by executive officials of city chancelleries, rather than inexperienced and unmotivated duma members (Nardova 1984, p. 14), who did not receive any remuneration for their service.

1.2 Design and Implementation of the 1870 Reform

The deficiencies of the existing structure of urban government, which produced "a sense of crisis in local and provincial governance" (Brower 1990, p. 96), stipulated the need for a fundamental reform. The first experiment with a more autonomous and modernized municipal self-government, with property-, rather than estate-based franchise and a separation of legislative and executive authorities, was launched in 1846 in Saint Petersburg. In 1862–1863, similar institutions were established in Moscow and Odessa (Nardova 1994, p. 182). The Ministry of Internal Affairs started to work on an all-encompassing municipal reform project in 1862, and, after eight years of deliberation, the final draft of the City Statute (*Gorodovoye Polozheniie*) was passed by the State Council and signed by Alexander II in June 1870.

According to the provisions of the 1870 law, municipal government consisted of two main bodies. The city legislature (*duma*), elected every four years, assigned members of the city board (*uprava*) vested with executive powers. Both were chaired by the city mayor (*gorodskoi golova*), elected by the duma for a one-year term, who could veto duma decisions and was accountable to the provincial governor (Tiurin 2007, p. 35). The role assigned to the mayor was criticized for violating the separation of the executive and legislative branches and restricting the autonomy of urban self-government by facilitating state interference into municipal affairs (Nardova 1984, pp. 39–40). Moreover, a special provincial municipal commission (*gubernskoye po gorodskim delam prisutstvie*) had a right to cancel contested rulings of city dumas, although its decisions

could be appealed in the Senate.

The eligible voters were male subjects of the Russian Empire who were at least twenty-five years old and owned taxable real estate or paid licensing fees for commercial or industrial activities (Khoz. Dep. MVD 1870, pp. 20–21).[2] These rules ensured a heavily restricted franchise. According to available calculations for 46 big cities (mostly provincial centers), the percentage of urban dwellers granted voting rights ranged from 2.9% to 10.6% (5.6% on average) of a city population (Nardova 1984, pp. 60-62). These figures can be roughtly doubled to get a sense of the proportion relative to the total male population. Smaller cities tended to have a larger percentage of eligible voters.

The lawmakers adopted a three-curia electoral system from the Prussian Municipal Code, which tied the weights of individual votes to tax contributions to the city budget (Nardova 1994, p. 185). All the voters were sorted with respect to the amount of taxes they paid and then divided into three electoral assemblies, the members of each paying one third of the total sum of taxes. Each assembly elected one third of duma deputies (Khoz. Dep. MVD 1870, p. 29). As a result, a small number of the richest citizens falling into the top two curiae enjoyed disproportionate influence. For instance, during the first municipal duma elections in Saint Petersburg (in 1872), with the population of about 660,000 people, the first curia consisted of 224, the second curia – of 887, and the third curia – of 17,479 voters. Each curia selected 84 deputies. Limited representation by design was further exacerbated by low voter turnout (Nardova 1984, pp. 52–53).

Nevertheless, the sheer enlargement of the size of representative bodies was a great improvement in comparison to the pre-reform period. With a few exceptions, earlier dumas had at most 6 members, elected through some obscure procedures, whereas after 1870, in the smallest towns with less than 300 voters, dumas should have consisted of 30 deputies, and in larger towns (except for capitals) their number must have been increased up to 72 (Khoz. Dep. MVD 1870, p. 51–52). Regarding dumas' composition, the reform somewhat diluted the monopoly of merchants, adding more representatives of urban middle classes, civil servants, and nobility.

[2]Eligible voters, acting as proxies, could cast a second vote on behalf of females and younger individuals.

The top panel of Figure A1, based on information from *pamyatnyye knizhki* (provincial yearbooks) for a sample of 42 cities[3] in pre-reform years, shows that the total number of merchants exceeded all other categories combined.[4] After the reform (the bottom-left panel), we can observe a relatively greater presence of other groups in dumas of the same cities.[5] In more than half of the cities in my sample, merchants made up a minority of deputies with a known social estate (the bottom-right panel).

The city statute limited the scope of local self-government to economic (*khoziastvennye*) and social matters, such as maintenance of public buildings, provision of lighting, heating, water supply and sewery systems, education, healthcare, charity, promotion of trade and industry, and issuing of various regulations addressing local affairs. The 1870 law allowed city dumas to levy taxes on immovable property (already practiced by a number of cities before the reform), commercial and industrial enterprises, pubs, shops, inns, and horse-drawn transport. The tax on immovable property could not exceed 10% of the income derived from it or, if the income was unknown, 1% of its value. As will be shown, the unified and augmented taxation practices contributed to fiscal growth, but most of the revenue expansion was associated with a more profitable exploitation of public assets, such as land, buildings, financial capital, and profits of public banks.

The new legislation (Khoz. Dep. MVD 1870) contained a list of "obligatory" city expenditures, which must have been performed in the first place: administrative expenses; maintenance of public buildings, streets, canals, plumbing, and sewer pipes; debt payments; obligatory funding of army, prisons, police, and firefighters, centrally controlled educational and charity institutions (see also Brower 1990, pp. 131–132). Defined this way, obligatory expenditures could constite the lion's share of city budgets. What was left after they were met could be directed to other needs, in particular, municipal education and healthcare. However, the exact

[3]Mostly provincial capitals and other large cities, but also some district centers. This is the same sample of cities as those included in Alabin and Konovalov 1889.

[4]*Meshchane*, or burghers, constituted the bulk of urban middle classes, whereas honored citizens (*pochetnyye grazhdane*), which enjoyed a noble-like status, included distinguished merchants, industrialists, and professionals. Magistrates were a kind of elected commercial courts, mostly resolving disputes between merchants.

[5]The "other" category mostly included peasants or professionals identified by their occupation or educational degree.

amount and proportions of funds accruing to each of these categories was not prescribed by the law, but had to be determined by *dumas*, interacting and negotiating with state officials (see examples in Tiurin 2007). The actual composition and size of the budget depended on the preferences of deputies and urban dwellers they represented, which is supported by my findings provided below.

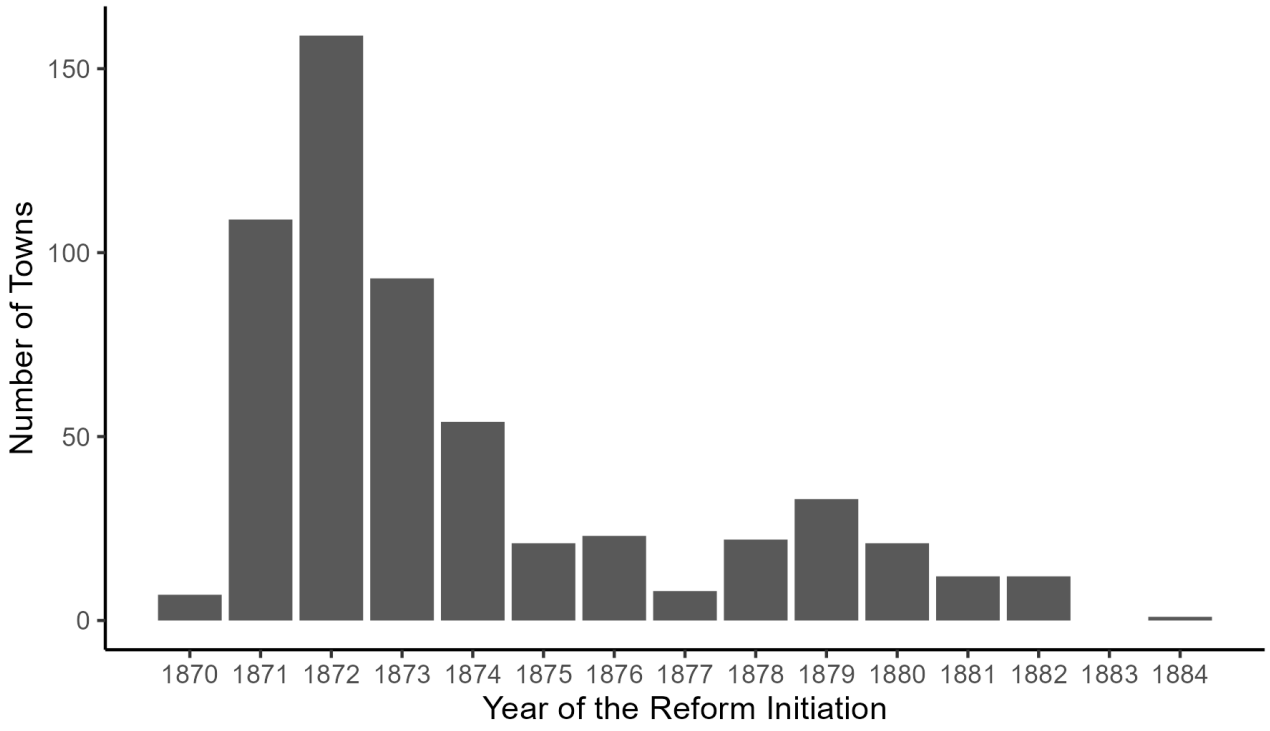

Figure 1: The number of towns that received municipal self-government, by year.

To sum up, although the new municipal institutions were based on a restricted franchise and subjected to state control, they heralded a transition from a mostly bureaucratic to a limited representative rule and constituted a big step forward towards the democratization of local self-government and the development of civil society (Brower 1990). Urban communities received a greater autonomy in making decisions and capacity to implement them. The following areas were particularly important:

– Introduction of all-estate representative legislative and executive elected bodies functioning on a regular basis and governing local economic and social affairs under state oversight. The franchise was based on immovable property ownership and tax payments.

– Unification, regularization, and broadening of sources of city revenues.

– Delineation of the structure of city expenditures.

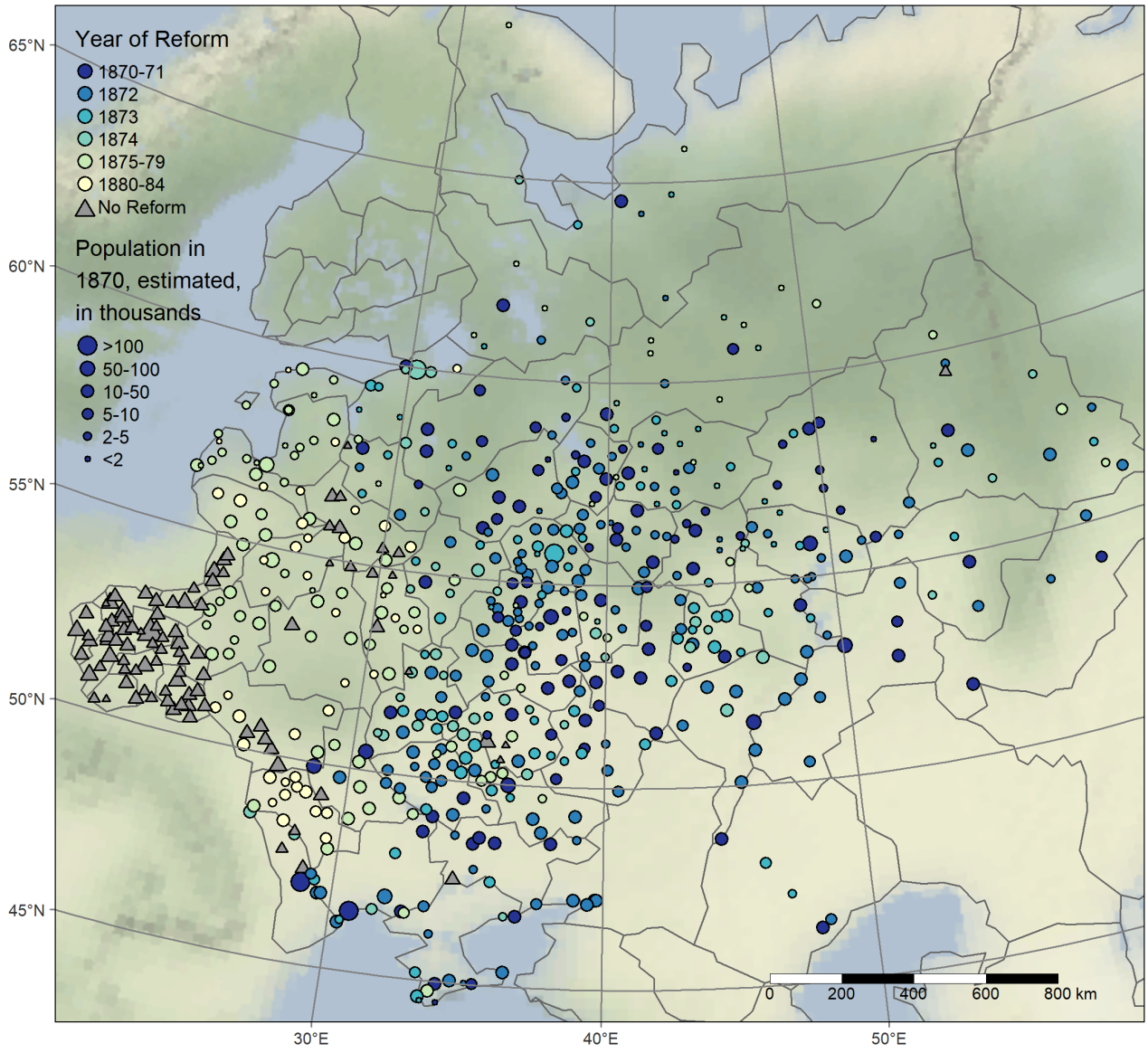

Figure 2: The introduction of city self-government in the European part of the Russian Empire. An Albers equal-area conic projection. Map created by the author.

The Tsar's edict issued in 1870 immediately instituted self-government in 41 provincial capitals and 4 major ports of European Russia and Siberia. The remaining towns must have received new institutions "in the near future, in accordance with local circumstances, at the discretion of the Minister of Internal Affairs" (PSZR 1874, p. 821). In practice, the authorities

followed a formal procedure: urban societies must have voted for a petition, which was then approved by the governor and the Minister. It gave some say to local communities with respect to the timing of the reform, but the central and provincial authorities were the instigators of the process and kept tight control over it. The cities of European Russia and Eastern Ukraine were covered by the reform by the mid-1870s. In accordance with special laws, the reform was launched in Transcaucasian towns in 1874, in Ukrainian and Belarusian towns that used to be parts the Polish-Lithuanian Commonwealth–in 1875, and in the cities of Baltic provinces–in 1877 (see Figures 2 and 1). The reform was never spread to Congress Poland (Weeks 2008, Ch. 8), Finland, and Central Asian provinces. By 1892, self-government was instituted in 692 out of 707 cities in the regions covered by the 1870 statute and related legal acts (Nardova 1984, p. 51).

Thus, the creation of elective municipal self-governance in the cities of the Russian Empire was a gradually unrolling centralized, top-down process, based on a unified insitutional design, which makes it a suitable case to test the consequences of democratization. On the downside, the timing of reform in different localities was not random. My companion project shows that self-governace was introduced earlier in cities with already high revenues per capita, which signaled their capacity to raise more revenues, and a lower share of potentially disloyal non-Orthodox minorities, which posed a greater political threat to the central government, seeking to contain their empowerment. The authorities were particularly distrustful of Poles and Jews (Weeks 2008). It could happen that cities with different starting characteristics would have followed different trajectories even in the absence of the reform. My identification strategy, explained below, addresses this problem.

1.3 Effects of Democratization and the Introduction of Self-Governance

Democratization, more specifically, the expansion of franchise to poorer citizens is theoretically associated with redistribution (Meltzer and Richard 1983). There is a considerable empirical support of a positive, or at least non-negative effect of democracy on the equality of incomes

and opportunitiues, as well as various salient social, economic, and institutional outcomes (Gerring, Knutsen and Berge 2022). Nevertheless, its effects on redistribution and economic growth remain contested, and scholars have encountered difficulties with finding appropriate identification strategies (Acemoglu et al. 2015).

An extensive literature discusses potential benefits of political and fiscal decentralization. Classical explanations have stated that local population has informational advantage regarding its needs; local decision-making addresses preference heterogeneity across different communities; greater autonomy can spur beneficial competition between jurisdictions (Mookherjee 2015, Enikolopov and Zhuravskaya 2007). Later studies pay more attention to a positive impact of local democratization in terms of a greater accountability of local governments (Seabright 1996) because of improved transparency, voters' better informedness, coordination, and more adequate credit and blame attribution (Mookherjee 2015, Fan, Lin and Treisman 2009). On the other hand, local governments can be more susceptible to elite capture (Blanchard and Shleifer 2001), which needs to be compensated by a greater political centralization at the national level (Riker 1964, Enikolopov and Zhuravskaya 2007).

The 1870 reform considered in this paper was in the first place a case of political decentralization that created new local representative institutions rather than just redistributed powers between municipalities and the central and provincial governments within the confines of existing institutions. It introduced a greater expenditure autonomy, whereas in terms of revenue decentralization, it extended the ability of municipalities to impose new taxes rather than just redistributed revenue streams between the center (or province centers) and municipalties.

Studying democratization at the subnational level can help overcome difficulties with identification and the lack of observations. Recent work on urban development and municipal reforms provides a nuanced view on the consequences of the liberalization of self-governance and sometimes brings contradictory results. For example, Stasavage 2014 finds that the population of medieval autonomous cities only initially rose faster than in non-autonomous cities, because the capture of autonomous institutions by the merchant oligarchy led to stagnation. Likewise, the inclusion of the members of craft guilds, protecting their privileges, in city councils depressed

populational growth (Stasavage 2019; see also Wahl 2019). Chapman 2018, 2024 show that the extension of franchise in the cities of England and Wales in the 19th century first lead to greater public spending–when the median voter shifted from the richest to the middle class,–but after reaching a 50% threshold, it was associated with lower spending, because the poor prefer basic private consumption to public spending.[6] Relatedly, Paulsen 2022 finds that the introduction of proportional representation, more accommodative of poorer voters, in Norwegian municipalities in the early 20th century led to higher income taxation and poor relief.

Turning to more recent cases, Martinez-Bravo et al. 2012 find that the introduction of village-level elections in China from the late 1980s boosted tax collection and public goods provision due to improved incentives for local leaders. On the other hand, Pierskalla 2022 reveal greater promotion of educated civil servants, but not much gain in term of public goods provision after the introduction of national and municipal elections in Indonesia in 1999. Turning to the contemporary Russian autocracy, Beazer and Reuter 2022 demonstrate that non-elected mayors, effectively accountable to the central government rather than local citizens, pay less attention to the improvement of housing conditions.

Technically, the municipal reform in the Russian Empire did not expand franchise, but it defined the pool of voters and created more representative and accountable elective bodies with expanded powers, which itself must have been important for governing municipal affairs. Therefore, my study is more in line with the work evaluating the impact of granting fiscal autonomy and transitioning from appointed to elected administration. Despite possible oligarchic capture, urban elites can play a positive role in the functioning of self-governance institutions. Bourgeiosie, including middle and upper middle classes, can be thought of as the main beneficiaries of a limited democratization, which brings more secure property rights, imposes less constraints on markets (Moore 1966, Ansell and Samuels 2014), and facilitates investment into more productive human capital (Beramendi, Dincecco and Rogers 2019). Under restricted franchise, one can observe elite-driven fiscal spending on "capital-enhancing" public goods, like

[6] Chapman 2023 also shows that the 1894 democratization of rural councils, governing provision of social insurance, improved redistribution in more unequal places, although the effect is moderated by the presence of powerful landed elites.

education, healthcare or security (Suryanarayan 2023, p. 6, Beramendi, Dincecco and Rogers 2019, Chapman 2018). For instance, despite limited franchise in the 19th century Prussian cities (which served as the model for urban elections in the Russian Empire), industrial elites controlling local self-government favored higher taxation and public spending on education, that is, investing in skill formation and increasing their capital gains (Hollenbach 2021).

Thus, my main hypothesis tested below is that the introduction of self-governance has a positive effect on urban revenues and expenditures, which is in line with most of the existing democratization and decentraliation literature. It is also consistent with the expectations and evaluations of Russian authorities themselves, who conducted decentralization reforms in order to improve urban management and support local development. Tangible improvements in these areas were reported in official documents.

My analysis also tackles the issues of accountability, the influence of local elites and state officials or agencies, and populational heterogeneity. I test whether the impact of the reform can depend on the level of industrialization and the share of merchants, who in the pre-reform era were "the sole active public force in most towns," but later were often seen as a conservative force by progressive urban activists (Brower 1990, pp. 98, 112). Of course, in the context of a limited franchise, populational shares need not reflect the political influence of a given social group, but it is still possible that more numerous merchants would exert a greater pressure on self-governance, both through formal and informal channels, or have their preferred candidates elected.

I also explore the role of ethnoreligious diversity, which is often considered to have a negative impact on the provision of public goods due to diverging preferences, in-group bias, or greater opportunities for cooperation and distributional conflict between different groups (e.g., Alesina, Baqir and Easterly 1999, Habyarimana et al. 2007).

Despite an expansion of local autonomy, the government still sought to control the reform implementation and influence the fiscal outcomes, as explained above. Considering different categories of expenditures, I examine to what extent the state was successful in the promotion of its priorities or had to take into account the preferences of local elites and other citizens.

2 Data

For this project, I created a novel dataset, which includes information on the time of the establishment of self-governance institutions in each city and municipal revenues and expenditures, collected by statisticians of the Ministry of Internal Affairs in order to evaluate the results of the 1870 municipal reform (Zverinskii and Otsolig 1887). The summary statistics of the variables is reported in Table A1.

I compiled a panel of revenues and expenditures for 608 cities aggregated over primarily five-year intervals: 1840–43, 1844–48, 1849–53, 1858–64, 1870–74, 1875–79, and 1880–84 (unfortunately, the averages for 1854–57 and 1865–69 are not available), and a yearly panel of revenues and expenditures in 1870–84 for 731 city (including Polish cities). After the exclusion of towns with incomplete information and merging some units, the final sample includes 510 cities for the period-averaged panel and 677 cities for the yearly panel; both panels are balanced.[7] For the five-year panel, I define aggregated treatment cohorts – towns with self-governance established in 1870–74, 1875–79, 1880–84, and without self-governance as of 1884. The fiscal indicators are expressed in nominal terms: unfortunately there is no data on sub-national price dynamics, but I compensate for it adding province-year fixed effects.

In order to obtain per capita fiscal indicators, I collected data on the number of city dwellers in 1840, 1847, 1856, 1863, 1870, 1885, and 1897 (for varying samples of towns)[8] and applied linear interpolation in order to get period-averaged and yearly panels covering the periods from 1840 to 1884 and from 1870 to 1884, correspondingly. I use two measures of time-varying economic shocks–proximity to railroads and temperature deviations–as additional covariates. I geocoded all the cities in the sample and used their geographic location in order to find distance to the nearest railroad; the data on the time of construction and location of railroads, available for each year, was collected for Kofanov 2020. The weather shocks were calculated as the

[7] In the baseline estimates, I keep the panels balanced by dropping cities with missing variable values in certain time periods. These are three cities with missing expenditure values for the period-averaged panel and 30 cities with missing population numbers in the yearly panel. However, keeping these cities in the sample and estimating unbalanced panales brings virtually identical results.

[8] A list of sources can be found in Nurutdinov 2019, pp. 154–155.

period-averaged deviations of yearly temperatures from the 1820–1884 mean for the five-year panel, and yearly deviations for the 1820-1869 mean for the yearly panel. For each town, I used temperatures reported for the nearest weather station in a given year, derived from the monthly data provided by Global Historical Climatology Network (Menne et al. 2018).

The data on social composition of cities in 1861 comes from MVD 1863, and on the pre-reform religious composition–from Semenov et al. 1863–1885. A time-varying indicator of proximity to railroads is used in the panel estimates below. City-level industrial employment in 1868 (excluding alcohol and sugar industries, concentrated in the countryside) was calculated on the basis of MVD 1872.

Furthermore, I descriptively compare the 1880–84 composition of revenues and expenditures coming from (Zverinskii and Otsolig 1887) with the 1862 data taken from a pre-reform description of 545 cities published in MVD 1863. In order to estimate a causal effect of the reform on different categories of revenues and expenditures, I also put together a two-period panel based on a more detailed information compiled by the Ministry of Internal Affairs from the two available volumes of Khoz. Dep. MVD 1882, 1887–for 1871 and 1878 (the sample also excludes Polish cities).[9]

3 Empirical Design

In order to estimate the impact of the municipal reform on urban finances, I apply a staggered difference-in-differences (DiD) design, which is made feasible by a gradual implementation of the reform. The baseline empirical model looks as follows:

$$Y_{it} = \alpha_i + \mu_{pt} + \beta T_{it} + \boldsymbol{\gamma} X_{it} + \epsilon_{it},$$

where Y_{it} is the logarithm of nominal per capita revenues or expenditures in city i in year or

[9]The reason behind this separation is that the 1880–1884 period data are provided in Zverinskii and Otsolig 1887 in the averaged form and almost all the cities in our sample had received self-government by 1884, which leaves little useful variation. At the same time, 1862 data come from a source with a somewhat different classification, which makes impossible a fully disaggregated comparison.

multi-year period t; T_{it} is the treatment variable equal to 0 in the periods before the introduction of self-government in a given city and 1 afterwards. This is a "static" design, in which the estimated effect does not vary over time. X_{it} is an optional vector of time-varying controls; α_i is a city fixed effect, and μ_{pt} is a province-year fixed effect. Given that the budget indicators are expressed in logarithms, whereas serious intra-province divergence of price trends is unlikely and relative prices are captured by town fixed effects, this model goes a long way towards absorbing local price levels and movements and enables us to interpret the results as reflecting the impact on real revenues and expenditures. In any case, according to all the available estimates, inflation was low and could not explain the observed evolution of urban finances (see Appendix C).

In addition to the baseline model, I use a dynamic, "event study" setup, which helps evaluate pre-trends and allows for a temporal heterogeneity of treatment effects:

$$Y_{it} = \alpha_i + \mu_{pt} + \sum_{\substack{\tau=-\mathrm{T}_1 \\ \tau\neq -1}}^{\mathrm{T}_2} \beta_\tau \mathbb{1}[t - g_i = \tau] + \boldsymbol{\gamma} X_{it} + \epsilon_{it},$$

where g_i is the period in which city i receives treatment, and τ stands for the difference between g_i and the calendar time t. For the period-averaged panel, lags and leads defined by τ range from -6 to 2, and for the yearly panel, the number of lags and leads is limited to eight by means of endpoint binning whenever it is feasible. The results are similar if we use fully disaggregated data. I exclude the coefficient for $\tau = -1$, which corresponds to the immediate pre-treatment period, taken as the reference point. The period-averaged panel, covering three decades preceding the reform, allows for a long-term comparison of pre-treatment trends, whereas the yearly panel reveals the short-term impact of the reform. In the period-averaged panel, treatment counterfactuals are primarily represented by not-yet-treated units; the yearly panel adds about one hundred never-treated Polish cities.

I exclude the cities that received self-governance in 1870 from the yearly panel: always-treated observation do not help to identify the effect of the treatment and can only introduce additional bias. In addition to the total revenues and expenditures, I also provide estimates of the reform impact on their components, using a two-period panel, comparing cities in 1871

and 1878. As of 1878, more than 22% of 471 towns in the final sample (which does not contain Polish cities) still remained untreated, thus constituting an appropriate control group. I also estimate the effect of the introduction of self-governance on the number of primary schools.

The recent years has seen a spectacular development of DiD methods in the econometric and political science literature (Roth et al. 2023, De Chaisemartin and d'Haultfoeuille 2023). One of the central problems is the issue of heterogeneous treatment effects, under which a commonly used TWFE panel estimate can be a non-convex combination of unit treatment effects, which can make it severely biased and even take a wrong sign. Another, even more important problem is the validity of the parallel trends assumption, that is, parallel trajectories of average outcomes of treated and control units in the absence of treatment (Chiu et al. 2024). In the context of a "static" staggered DiD, the effect estimate can be biased by "forbidden comparisons" between already treated cohorts if treatment effects are heterogeneous over time (Goodman-Bacon 2021). Event studies sideline this issue, but if cohort-specific effects are heterogeneous, which is a common occcurrence, estimated coefficients on leads and lags get "contaminated by treatment effects from other periods" (Sun and Abraham 2021, p. 183), problematizing the assessment of the parallel trends assumption. Treatment effect heterogeneity problem is resolved by applying appropriate estimators, but parallel trends validity is not directly testable and has to be assumed with more or less plausibility.

In order to check the robustness of my findings to effect heterogeneity, I apply a recently proposed method of effect decomposition (Goodman-Bacon 2021) and alternative DiD estimators. A regression-based method developed in Sun and Abraham 2021 uses the last-to-be-treated cohort along with never-treated units as the control group. Another method, described in Callaway and Sant'Anna (2021), non-parametrically estimates ATT (average treatment for the treated) effects for each treated cohort and period by comparing these cohorts with not-yet-treated or never-treated units and enables effect aggregation by cohorts and relative and calendar periods. In addition, I provide results for the imputed estimator of Borusyak, Jaravel and Spiess 2024 and the "multiple DiD" estimator of De Chaisemartin and d'Haultfoeuille 2020.

The validity of the parallel trends assumption can be a greater challenge to the identification

strategy used in this paper. As explained above, the order in which the treatment was assigned in different localities in the course of the 1870 reform was not random, which could entail divergent parallel trends between treated and control cohorts. In order to alleviate this concern, I analyse placebo pre-treatment coefficients from event studies, check the sensitivity of my estimates to potential violations of parallel trends (Rambachan and Roth 2023), and apply the synthetic DiD estimator (Arkhangelsky et al. 2021), which constructs a more credible counterfactual to treated cohorts in the post-treatment period.

4 Empirical Results

4.1 Difference-in-Differences Analysis: Main Results

Before proceeding to statistical estimates, on Figure 3, I plot trajectories of mean log per capita revenues and expenditures for the cohorts defined by five-year treatment periods of the period-averaged panel. A town falls in a given cohort if it received self-government in one of the corresponding years. It can be seen that cohorts that were treated earlier consisted of already richer cities.

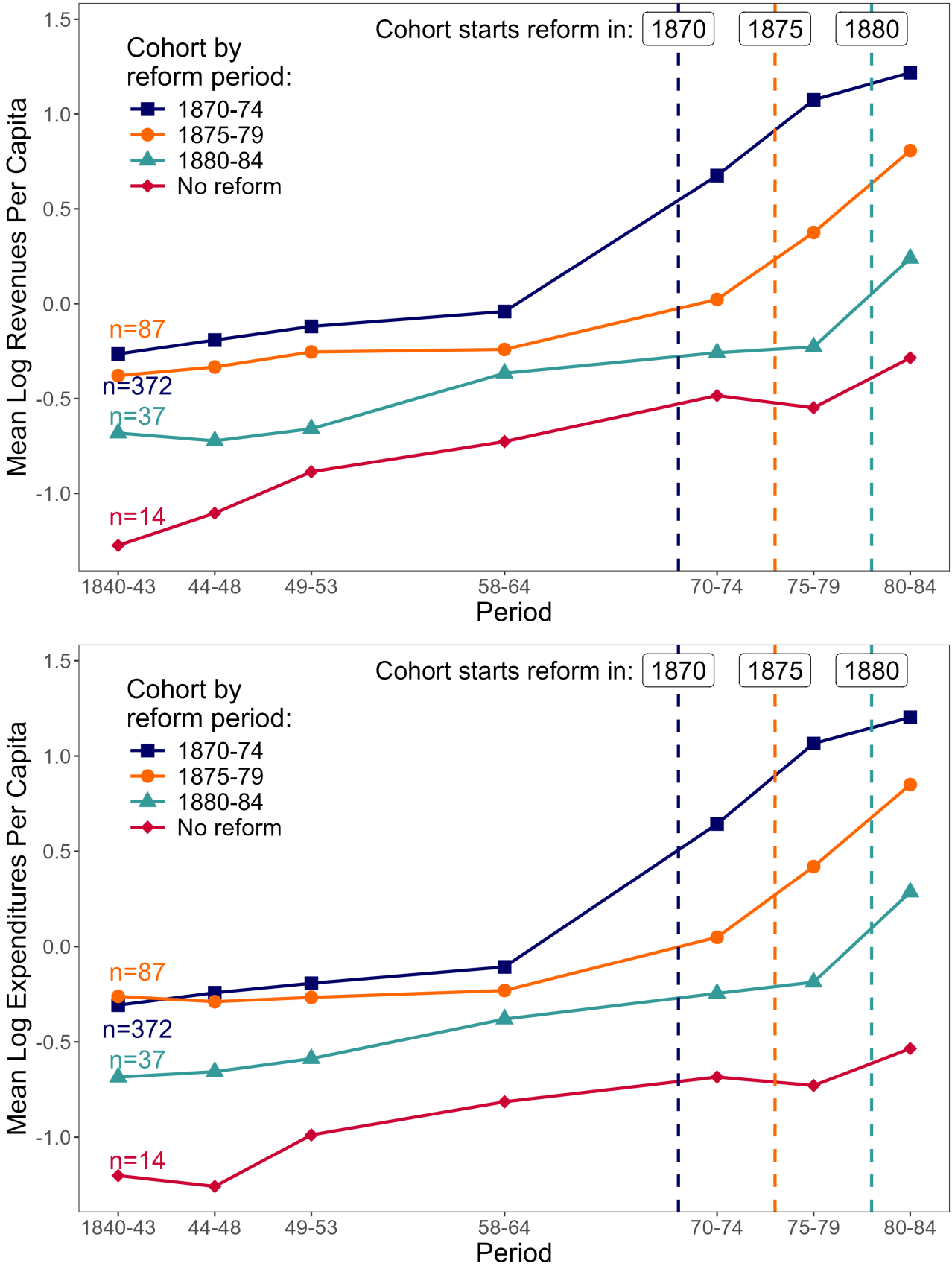

Figure 3: Logarthims of mean nominal revenues (top) and expenditures (bottom) per capita by cohort and period.

The first cohort includes core European Russia provinces, whereas other cohorts are made up of cities from imperial provinces lying further west. Earlier treated cohorts also contain a larger number of units. Nevertheless, as can be inferred from Figure 3, the pre-reform dynamics of cohort revenues and expenditures look quite similar, which is consistent with the parallel trends assumption.

If the reform had a tangible impact on city budgets of a given cohort, we should observe a sharp increase in revenues and expenditures in the period right after a cohort line crosses the dashed vertical line of the same color. The data match this expectation: for example, both revenues and expenditures for the 1870–74 cohort considerably rose in the period of the reform implementation and continued their growth in the next periods; other treated cohorts undergo similar evolution. Moreover, after crossing corresponding vertical dashed lines, cohort revenues or expenditures run virtually parallel. Judging by the first cohort, treated in 1870–74, after the first two periods, the fiscal expansion slows down.

Table 1 reports baseline DiD estimates of the reform effect for the five-year panel. After the introduction of self-governance, revenues and expenditures increased by about 20–30% (Models 2, 3, 6, and 7). The effect is almost twice as large in two-way fixed effects regressions (Models 1 and 5) than in the baseline specifications including province-year fixed effects. As we can see from specifications with controls, a higher population is usually associated with a smaller increment of per capita revenues and expenditures, which probably reflects their returns to scale, for instance, known for state administration (Gehlbach 2008). As expected, proximity to railroads could help in boosting city budgets. At the same time, temperature deviations are positively associated with the fiscal indicators. Models 4 and 8 present an even more demanding specification, complementing city and province-year effects with city-level linear time trends: the effect of the reform is lowered, but still remains relatively large and statistically significant at the 99% level. The DiD results for the yearly panel (Table 2) confirm these findings. The static effect on revenues and expenditures is somewhat smaller in most cases, but close in magnitude.

In addition, Appendix C breaks down the samples into geographic regions treated in different

periods and shows a positive effect of the reform in each of them, as well as the relevance of using Polish cities as a control cohort, despite Congress Poland's geographically peripheral position and detachment from the core Imperial provinces.

Table 1: Effect of 1870 Municipal Reform: Difference-in-Differences, 5-Year City-Level Panel

	Log revenues pc				Log expenditures pc			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reform_t	0.45^{***}	0.24^{***}	0.29^{***}	0.18^{***}	0.53^{***}	0.26^{***}	0.31^{***}	0.21^{***}
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Log city population_t			-0.62^{***}	-0.85^{***}			-0.59^{***}	-0.83^{***}
			(0.06)	(0.06)			(0.06)	(0.06)
Log distance to railroad_t			-0.02^{*}	-0.03^{***}			-0.02^{**}	-0.03^{**}
			(0.01)	(0.01)			(0.01)	(0.01)
$\text{Temperature deviation}_t$			0.03^{*}	0.02^{*}			0.02	0.02^{*}
			(0.01)	(0.01)			(0.01)	(0.01)
Num. obs.	3570	3570	3570	3570	3549	3549	3549	3549
Num. towns	510	510	510	510	507	507	507	507
R^2	0.85	0.90	0.92	0.96	0.85	0.90	0.91	0.95
Adj. R^2	0.83	0.87	0.89	0.93	0.82	0.87	0.89	0.93
City FEs	✓	✓	✓	✓	✓	✓	✓	✓
Period FEs	✓				✓			
Province × Period FEs		✓	✓	✓		✓	✓	✓
Linear city trends				✓				✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Standard errors clustered at the city level are in parentheses.

Table 2: Effect of 1870 Municipal Reform: Difference-in-Differences, Yearly City-Level Panel

	Log revenues pc				Log expenditures pc			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reform_t	0.33^{***}	0.25^{***}	0.23^{***}	0.20^{***}	0.34^{***}	0.25^{***}	0.23^{***}	0.19^{***}
	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)
Log city population_{t-1}			-0.71^{***}	-0.56^{**}			-0.66^{***}	-0.11
			(0.08)	(0.24)			(0.10)	(0.22)
Log distance to railroad_{t-1}			-0.02^{***}	-0.02^{**}			-0.01^{*}	-0.01
			(0.01)	(0.01)			(0.01)	(0.01)
$\text{Temperature deviation}_{t-1}$			0.01	0.00			0.01	0.01
			(0.01)	(0.01)			(0.01)	(0.01)
Num. obs.	10155	10155	9705	9705	10155	10155	9705	9705
Num. towns	677	677	647	647	677	677	647	647
R^2	0.88	0.90	0.91	0.93	0.88	0.90	0.91	0.93
Adj. R^2	0.87	0.88	0.89	0.91	0.87	0.88	0.89	0.91
City FEs	✓	✓	✓	✓	✓	✓	✓	✓
Year FEs	✓				✓			
Province × Year FEs		✓	✓	✓		✓	✓	✓
Linear city trends				✓				✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Standard errors clustered at the city level are in parentheses.

In order to check whether the estimates are seriously biased by comparisons between treated cohorts, I apply Goodman-Bacon's decompositon (Goodman-Bacon 2021), currently available for two-way fixed effects models, to Models 1 and 5 from Table 1. It turns out that the inappropriate comparison of later vs. earlier treated cohorts has an average weight of 0.22 (out of 1) and still makes a positive contribution to the estimate (the effect is 0.11 for revenues and 0.12 for expenditures). The comparisons of treated with never-treated and not-yet-treated units enter with similar weights and have similar effect sizes (around 0.6 for both revenues and expenditures), which justifies the inclusion of both of them in the control group.

Furthermore, I conduct the "event study" analysis, evaluating both dynamic treatment effects and placebo pre-treatment coefficients for treated groups, which addresses potential effect heterogeneity and checks the validity of the parallel trends assumption. Figure 4 plots event study coefficients from the regular TWFE model (top row), and methods proposed by Sun and Abraham 2021 (middle row) and Callaway and Sant'Anna 2021 (bottom row) for the five-year panel dataset.[10] These results lend credibility to the parallel trends assumption: coefficients on treatment leads are always statistically indistinguishable from zero, and even if they seemingly move in a certain direction in the pre-treatment period, the trends are actually opposite to a permanent positive shift after the beginning of the reform implementation. The effect of the reform seemed to propagate gradually, and by the second post-treatment period it surpassed the "static" DiD estimate from Table 1.

The event study results for yearly panels are plotted on Figure 5. Most of the cities were treated in the first five years (see Figure 1), and as we move backward, we have to rely on much smaller treated cohorts in order to estimate lead coefficients, which can produce biased results. Therefore, for TWFE and Sun and Abraham's method, I limit the number of both pre-trends and post-treatment coefficients, applying endpoint binning, which assumes constant effects after eight periods forward and backward in time (De Chaisemartin and d'Haultfoeuille 2023, p. 72). For Callaway and Sant'Anna's methods, I provide year-averaged cohort average

[10]The method of Callaway and Sant'Anna 2021, as implemented in R package `did`, allows to include only time-invariant controls, therefore this specification is omitted in the bottom panel of Figure 4.

treatment effects for eight treatment leads and lags.[11] In order to avoid visual cluttering, I do not show treatment coefficients for specifications with controls, which deliver similar results.

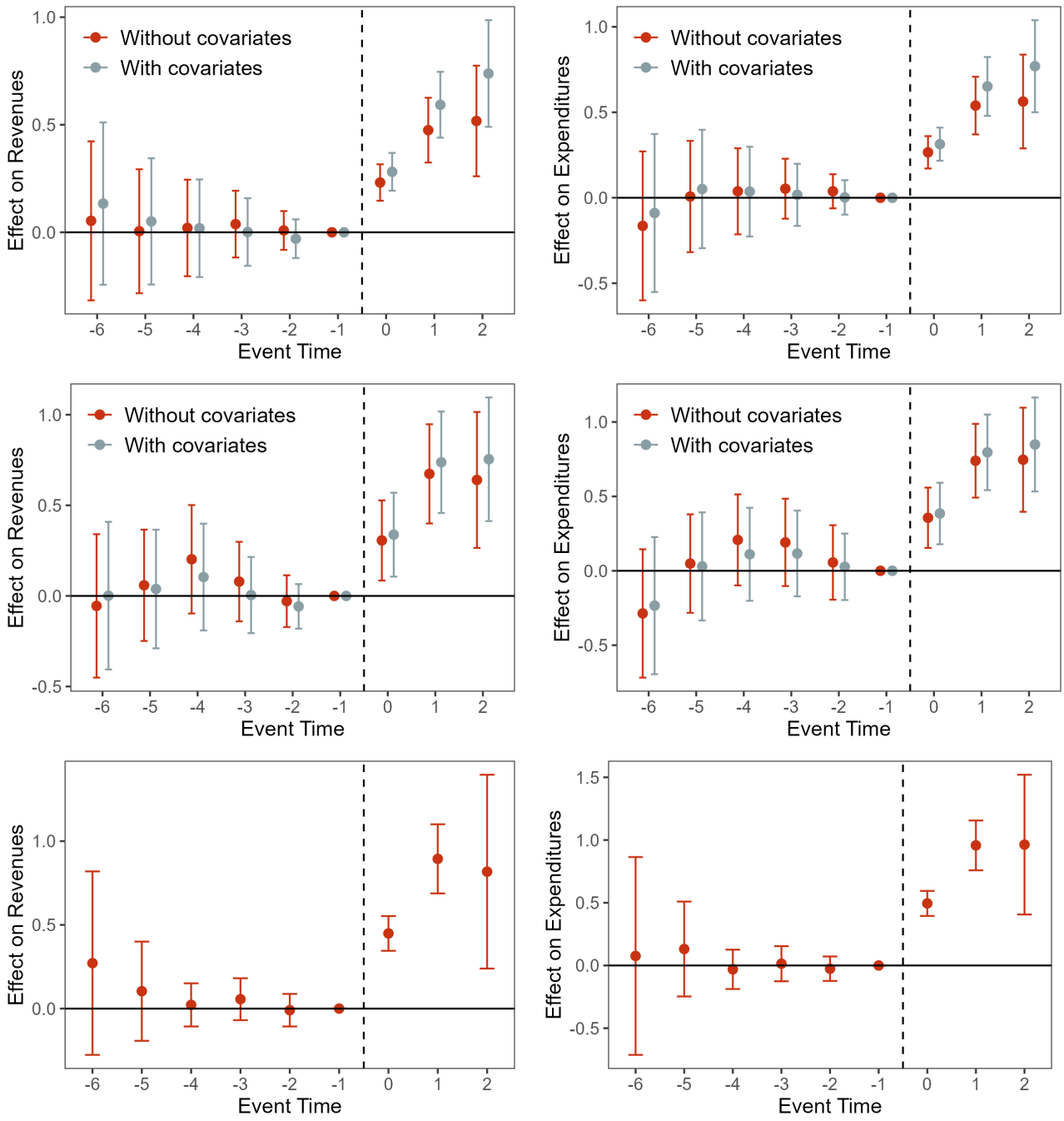

Figure 4: Event study coefficients for the 5-year panel. Top row: baseline TWFE with province-year FE; middle row: the method of Sun and Abraham 2021; bottom row: the method of Callaway and Sant'Anna 2021.

[11]The sample to which I apply Callaway and Sant'Anna's method excludes the cohort that was treated in 1884 because the algorithm returns a warning that it is too small.

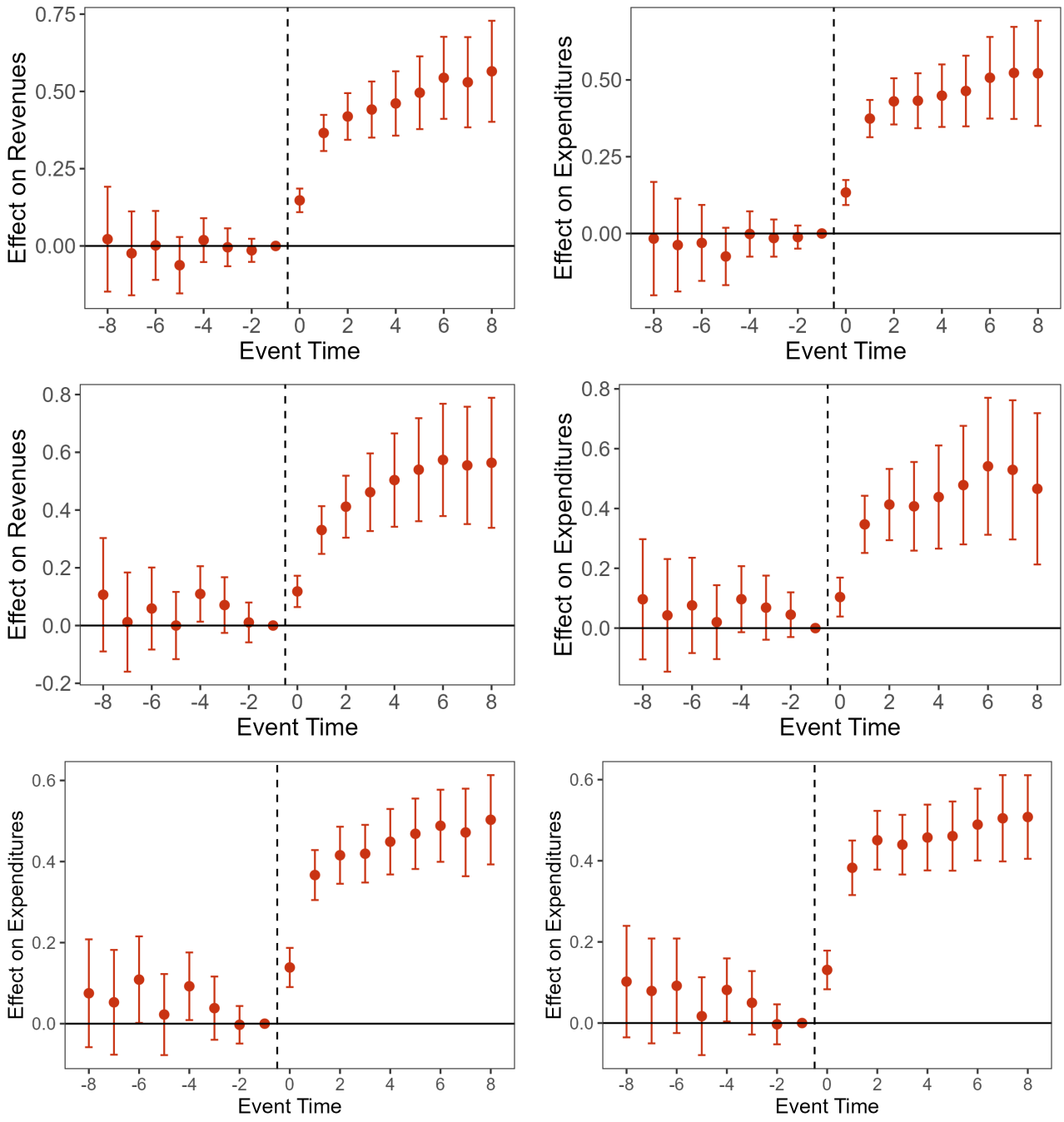

Figure 5: Event study coefficients for the yearly panel. Top row: TWFE, middle row: the method of Sun and Abraham 2021; bottom row: the method of Callaway and Sant'Anna 2021.

Similar to the five-year panel estimates, the reform produces a long-term expansion of revenues and expenditures of 40–50%. It is telling that for both revenues and expenditures, the effect in the first period is moderate, but it once again jumps in the second period and slowly rises afterwards up to a point. Indeed, the self-government institutions could be introduced at

different parts of a year, and it could take some time to conduct the elections, convene a city *duma*, assemble a pro-spending coalition, and approve and implement the budget. Many towns could just fail to go through all the steps in the first year of treatment. Appendix B, containing estimates of a significant positive effect of the reform on the number of schools, shows that fiscal expansion was actually accompanied by a greater public goods provision.

In addition, Figure A3 in Appendix A contains event study results (without controls) for two other effect heterogeneity-robust estimators: the imputed estimator of Borusyak, Jaravel and Spiess 2024 and the "multiple DiD" estimator of De Chaisemartin and d'Haultfoeuille 2020. They are quite in line with the estimates obtained by the application of other methods. The pre-trend coefficients as per Borusyak, Jaravel and Spiess 2024 are calculated using only non-treated observations and independently of the treatment effect estimates, which are based on imputed counterfactuals; the reference group includes periods preceding the earliest pre-trend. All the pre-trend placebo coefficients are insignificant. The Stata realization of the De Chaisemartin and d'Haultfoeuille 2020 method allows the calculation of only seven "placebo" leads for the given sample, which are provided on the figure. It can be seen that although some of them are positive and significant, and sometimes quite large, especially in the case of revenues (the bottom right panel) they reveal nothing like a discernible positive pre-trend.

The estimates provided in this section show a large and statistically significant effect of the reform, but occasional pre-trend irregularities can cast some doubt on the validity of the parallel trends assumption and the robustness of the effect estimate to its violation. For instance, as far as the Callaway and Sant'Anna's method is concerned, the p-value for the pre-test of parallel trends assumption is less than 0.05 for the 5-year revenues panel, but is about 0.19 for expenditures, and close to 1 for the yearly panels. The p-values for the joint nullity of pre-trends are about 0.29 for estimates per Borusyak, Jaravel and Spiess 2024, but close to zero if we use the De Chaisemartin and d'Haultfoeuille 2020 method.

Rambachan and Roth 2023 suggest a robust confidence set ("relative magnitudes") approach, measuring sensitivity of estimates to a potential violation of the parallel trends assumption, bounding post-treatment period-to-period deviations by $\bar{M}$, the maximum absolute

deviation in the pre-treatment period. Figure A2 shows the robust confidence sets computed for estimates of TWFE, Sun and Abrakham's and Callaway and Sant'Anna's methods without controls; the left column is for revenues, the right one–for expenditures.

It turns out that in the TWFE models, the reform effect is highly robust: it loses significance at $\bar{M} \approx 2$. However, for the other two methods, the estimates look considerably more sensitive, with $\bar{M} \approx 0.5$. The latter threshold is apparently lower than a "natural" benchmark of $\bar{M} = 1$, suggested by Rambachan and Roth 2023 (p. 2563), but it seems to be quite common in empirical studies. In a recent meta-analysis (Chiu et al. 2024), in 74% (27 out of 37) of replicated studies published in top political science journals the confidence set included 0 at $\bar{M} = 0.5$. It should also be stressed that we are dealing with an average effect across all the post-treatment periods, and the violation of parallel trends is computed as accumulated across consecutive periods, which leads to a widening of the confidence set as more periods are included (Rambachan and Roth 2023, pp. 2584–2585).

In addition, the placebo coefficients preceding the treatment period often fluctuate and show a downward trend, which abruptly switches to an increase in the post-treatment period; after an initial rise, the effect is gradually reaching a plateau. A systematic post-treatment violation of the parallel trends assumption eliminating the treatment effect seems to be unlikely.

Overall, the estimates of the treatment effect of the reform seem to be moderately sensitive to the violation of the parallel trends identification assumption. In the next subsection, I employ a different approach to this issue, applying a synthetic difference-in-differences method, which can produce more credible counterfactuals.

4.2 Synthetic Difference-in-Differences

In this subsection, I utilize the synthetic difference-in-differences estimator (Arkhangelsky et al. 2021), which does not just rely on the assumption of parallel pre-treatment trends between treated and control units, but effectively enforces it by applying a proper weighting scheme to control units, thus creating a better counterfactual for the post-treatment period.

A consistent synthetic DiD estimate of the average causal effect of exposure for a balanced panel with N units and T time periods is found from a weighted TWFE regression model (Arkhangelsky et al. 2021, p. 4091):

$$(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \underset{\tau,\mu,\alpha,\beta}{\arg\min} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} \left(Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau\right)^2 \hat{\omega}_i^{sdid} \hat{\lambda}_t^{sdid} \right\}, \tag{1}$$

where $\hat{\omega}_i^{sdid}$ are pre-selected unit weights that approximately match pre-treatment trends of treated and non-treated units, and $\hat{\lambda}_t^{sdid}$ are time weights that balance the pre- and post-treatment trajectories of unexposed units. W_{it} is an indicator equal to 1 if unit i is treated in period t and equal to 0 otherwise.

The method was originally designed for "block" treatment assignment, when all the treated units receive treatment at the same period and stay exposed afterwards. One way to adjust synthetic DiD to a staggered treatment adoption setting, which is described in Arkhangelsky et al. 2021 (pp. 4114–4115) and implemented in the Stata package `sdid` (Clarke et al. 2023, pp. 8–13), is to find a weighted sum of cohort-wise effect estimates that match treated units from a given cohort with never-treated units, with weights equal to each cohort's share in the total number of post-treatment unit-periods.

Table 3: SDID Estimates

	Full Sample		Western Krai & Poland	
	Revenues	Expenditures	Revenues	Expenditures
	(1)	(2)	(3)	(4)
Reform_t	0.403***	0.399***	0.520***	0.537***
	(0.030)	(0.033)	(0.044)	(0.046)
Num. obs	10,155	10,155	3,150	3,150

* p<0.10, ** p<0.05, *** p<0.01. Clustered bootstrap standard errors in parentheses. Estimates performed in Stata package `sdid`.

The averaged treatment effects calculated in this fashion are shown in Table 3. The models do not include controls or province-year fixed effects, which might be redundant given the essence of the method. The estimates are very precisely estimated and in terms of the magni-

tude, close to the upper range of the DiD coefficients provided above, which fully confirms my conclusions regarding a large positive impact of the reform. The never-treated Polish towns might constitute a better counterfactual for geographically proximate Western Krai cohorts (see also Appendix C), therefore, I provide estimates for this subsample in Models 3 and 4.

Figure A4 shows the trajectories of selected treated and synthesized control cohorts, which first received self-governance in 1874, 1875, 1876, and 1877. In each of the panels, green polygons show period weights $\hat{\lambda}_t^{sdid}$, and the lower figure plots calculated effects for each treated group, that are sized in accordance to the weights $\hat{\omega}_i^{sdid}$ assigned to them. The vertical lines mark the first treatment period. On average, treatment and weighted control groups indeed follow similar trajectories, and unit weights are quite evenly assigned across observations. Table A2 shows a full disaggregation into cohort-wise effects, which are almost always positive and significant.[12]

4.3 Interaction Effects

This section considers a possible moderating role of socio-economic indicators that are expected to shape the functioning of self-governance. In Tables 4 and 5, I evaluate the interactions between the treatment variable and the populational share of various socio-economic classes, measured in 1861, namely, merchants of all the three guilds combined, which included both large and small licensed traders; *meshchane*–the bulk of urban dwellers, including petty bourgeoisie and professionals, who constituted the backbone of urban social life (Lankina 2021); and the hereditary nobility.

The only significant interaction in Tables 4 and 5 is with the share of merchants (Models 1 and 2)–and it is somewhat counterintuitively negative. Cities with a higher share of merchants experienced a lower positive impact of the introduction of self-governance: it would be lower by about one quarter in a city with the average proportion of merchants (0.09).[13]

[12] In accordance with the Tsar's decree, the reform in the Western Krai must have started in 1875, but there are three Western Krai cities that, according to the data, received self-governance prior to that. Thus, the first three effects for Models 3 and 4 are based on only one treated unit each, which could explain the appearance of a negative sign. Anyway, it must have little influence on the final weighted average.

[13] Except for a handful of cities with a large share of merchants (I drop one of them, exceeding 50%, from the sample), this share does not reach values making the effect of the reform negative.

Table 4: Effect on Revenues Depending on the Shares of Social Groups (Yearly Panel)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reform_t	0.31^{***}	0.29^{***}	0.27^{***}	0.32^{***}	0.30^{***}	0.22^{***}	0.21^{***}
	(0.03)	(0.03)	(0.03)	(0.07)	(0.06)	(0.03)	(0.03)
$\text{Reform}_t \times$ Share Merchants	-0.84^{***}	-0.78^{***}	-0.31				
	(0.24)	(0.23)	(0.23)				
$\text{Reform}_t \times$ Share *Meshchane*				-0.13	-0.09		
				(0.10)	(0.08)		
$\text{Reform}_t \times$ Share Nobility						1.17	1.67
						(1.07)	(1.16)
$\text{Reform}_t \times$ Log revenues pc, 1870			-0.21^{***}		-0.22^{***}		-0.22^{***}
			(0.03)		(0.03)		(0.03)
Log city population_{t-1}		-0.72^{***}	-0.68^{***}		-0.67^{***}		-0.68^{***}
		(0.08)	(0.08)		(0.08)		(0.08)
Log distance to railroad_{t-1}		-0.02^{**}	-0.02^{**}		-0.02^{**}		-0.02^{***}
		(0.01)	(0.01)		(0.01)		(0.01)
$\text{Temperature deviation}_{t-1}$		0.01	0.01		0.01		0.01
		(0.01)	(0.01)		(0.01)		(0.01)
Num. obs.	8,145	8,145	8,145	8,145	8,145	8,145	8,145
Num. towns	543	543	543	543	543	543	543
R^2	0.90	0.90	0.91	0.90	0.91	0.90	0.91
Adj. R^2	0.88	0.89	0.89	0.88	0.89	0.88	0.89
City FEs	✓	✓	✓	✓	✓	✓	
Province × Year FEs	✓	✓	✓	✓	✓	✓	

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Standard errors clustered at the city level are in parentheses. The sample excludes towns with the share of merchants exceeding 0.5.

Table 5: Effect on Expenditures Depending on the Shares of Social Groups (Yearly Panel)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reform_t	0.31^{***}	0.29^{***}	0.25^{***}	0.31^{***}	0.27^{***}	0.23^{***}	0.20^{***}
	(0.04)	(0.03)	(0.03)	(0.09)	(0.07)	(0.04)	(0.03)
$\text{Reform}_t \times$ Share Merchants	-0.89^{***}	-0.83^{***}	-0.37^{*}				
	(0.22)	(0.21)	(0.20)				
$\text{Reform}_t \times$ Share *Meshchane*				-0.11	-0.08		
				(0.13)	(0.09)		
$\text{Reform}_t \times$ Share Nobility						0.51	1.48
						(1.19)	(1.26)
$\text{Reform}_t \times$ Log expenditures pc, 1870			-0.22^{***}		-0.23^{***}		-0.24^{***}
			(0.03)		(0.03)		(0.03)
Log city population_{t-1}		-0.68^{***}	-0.62^{***}		-0.62^{***}		-0.62^{***}
		(0.11)	(0.10)		(0.10)		(0.10)
Log distance to railroad_{t-1}		-0.01^{*}	-0.01^{*}		-0.01^{*}		-0.01^{*}
		(0.01)	(0.01)		(0.01)		(0.01)
$\text{Temperature deviation}_{t-1}$		0.01	0.02^{*}		0.02^{*}		0.02^{*}
		(0.01)	(0.01)		(0.01)		(0.01)
Num. obs.	8,145	8,145	8,145	8,145	8,145	8,145	8,145
Num. towns	543	543	543	543	543	543	543
R^2	0.90	0.90	0.91	0.90	0.91	0.90	0.91
Adj. R^2	0.88	0.89	0.89	0.88	0.89	0.88	0.89
City FEs	✓	✓	✓	✓	✓	✓	
Province × Year FEs	✓	✓	✓	✓	✓	✓	

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Standard errors clustered at the city level are in parentheses. The sample excludes towns with the share of merchants exceeding 0.5.

However, it could be the case that the share of merchants itself could affect the size of the budget. In models excluding town fixed effects (not shown here), cities with a higher share of merchants indeed had higher revenues and spending per capita before the reform. If we directly control for the initial level of per capita revenues and expenditures interacted with the reform period (Model 3), the interaction with the share of merchants is more than halved and no longer significant at the 95% level.

It seems that when it was sufficiently strong in numerical terms, the merchant community had incentives and capacity to lobby and sponsor provision of public goods even with defunct self-governance institutions that existed before 1870. In that case, they might have felt less need in further expansion of city budgets, which had already been aligned with their interests. This finding resonates with historical accounts showing that active and rich merchants could play a distinguished role in urban governance and development, even investing their own funds, prior to the reform (Savelyev 1885). As discussed above, merchants retained strong presence in self-governance institutions after the reform. Overall, granting representation was beneficial for towns with different social composition, but more so with previously weak trade and commercial classes.

Tables 6 and 7 explore interactions of the reform treatment with industrialization and religious heterogeneity (to a considerable extent overlapping with ethnic heterogeneity). Industrial elites can be interested in public goods provision as long as it brings them more skilled and healthier workforce, thus, greater industrial employment may entail higher public spending (Hollenbach 2021). However, in our case, there is only a weakly negative interaction of the reform with industrialization, operationalized by the logarithm of the number of industrial workers per capita in 1868 (Models 1 and 2).

Table 6: Effect on Revenues Depending on Industrialization and Diversity (Yearly Panel).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reform_t	0.22^{***}	0.21^{***}	0.20^{***}	0.27^{***}	0.19^{***}	0.21^{***}	0.40^{***}	0.35^{***}
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.07)	(0.07)
$\text{Reform}_t \times$ Log workers pc, 1868	-0.01	-0.01^{*}						
	(0.01)	(0.01)						
$\text{Reform}_t \times$ Religious Polarization, 1860s			0.10^{*}	-0.01				
			(0.06)	(0.06)				
$\text{Reform}_t \times$ Religious Fractionalization, 1860s					0.20^{**}	0.15		
					(0.10)	(0.09)		
$\text{Reform}_t \times$ Share Orthodox, 1860s							-0.20^{**}	-0.14^{*}
							(0.09)	(0.08)
$\text{Reform}_t \times$ Log revenues pc, 1870		-0.22^{***}		-0.25^{***}		-0.22^{***}		-0.22^{***}
		(0.03)		(0.03)		(0.03)		(0.03)
Log city population_{t-1}		-0.68^{***}				-0.69^{***}		-0.70^{***}
		(0.08)				(0.09)		(0.09)
Log distance to railroad_{t-1}		-0.02^{**}		-0.02^{**}		-0.02^{**}		-0.02^{**}
		(0.01)		(0.01)		(0.01)		(0.01)
$\text{Temperature deviation}_{t-1}$		0.01		0.01		0.01		0.01
		(0.01)		(0.01)		(0.01)		(0.01)
Num. obs.	8,145	8,145	7,470	7,470	7,470	7,470	7,470	7,470
Num. towns	543	543	498	498	498	498	498	498
R^2	0.90	0.91	0.90	0.91	0.90	0.91	0.90	0.91
Adj. R^2	0.88	0.89	0.88	0.89	0.88	0.89	0.89	0.89
City FEs	✓	✓	✓	✓	✓	✓	✓	✓
Province × Year FEs	✓	✓	✓	✓	✓	✓	✓	✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Standard errors clustered at the city level are in parentheses.

Table 7: Effect on Expenditures Depending on Industrialization and Diversity (Yearly Panel).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reform_t	0.22^{***}	0.18^{***}	0.20^{***}	0.22^{***}	0.19^{***}	0.17^{***}	0.41^{***}	0.35^{***}
	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.08)	(0.08)
$\text{Reform}_t \times$ Log workers pc, 1868	-0.01	-0.01^{**}						
	(0.01)	(0.01)						
$\text{Reform}_t \times$ Religious Polarization, 1860s			0.10	0.02				
			(0.07)	(0.06)				
$\text{Reform}_t \times$ Religious Fractionalization, 1860s					0.21^{*}	0.19^{*}		
					(0.11)	(0.10)		
$\text{Reform}_t \times$ Share Orthodox, 1860s							-0.23^{**}	-0.18^{**}
							(0.09)	(0.09)
$\text{Reform}_t \times$ Log expenditures pc, 1870		-0.24^{***}		-0.27^{***}		-0.24^{***}		-0.23^{***}
		(0.03)		(0.04)		(0.03)		(0.03)
Log city population_{t-1}		-0.62^{***}				-0.71^{***}		-0.71^{***}
		(0.10)				(0.10)		(0.10)
Log distance to railroad_{t-1}		-0.01		-0.01		-0.01		-0.01
		(0.01)		(0.01)		(0.01)		(0.01)
$\text{Temperature deviation}_{t-1}$		0.02^{*}		0.01		0.01		0.01
		(0.01)		(0.01)		(0.01)		(0.01)
Num. obs.	8,145	8,145	7,470	7,470	7,470	7,470	7,470	7,470
Num. towns	543	543	498	498	498	498	498	498
R^2	0.90	0.91	0.90	0.91	0.90	0.91	0.90	0.91
Adj. R^2	0.88	0.89	0.88	0.89	0.88	0.89	0.88	0.89
City FEs	✓	✓	✓	✓	✓	✓	✓	✓
Province × Year FEs	✓	✓	✓	✓	✓	✓	✓	✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Standard errors clustered at the city level are in parentheses.

At the same time, greater religious fractionalization, and in particular, a lower share of Orthodox Christian, predominant in core Russian territories, seem to amplify the positive impact of the reform. In accordance with the region-level findings provided in Appendix C, more diverse peripheral territories experienced greater relative gains from the reform, and interestingly, this result holds even if we control for the initial per capita levels of fiscal indicators. Unfortunately, I don't have information on the distribution of reform gains between different ethnicities. It should be noted that a group's "legibility" to the state and access to public goods considerably varied in the Russian Empire, often leaving minorities in a disadvantaged position (Charnysh 2022). Urban politics in diverse regions were sometimes fraught with inter-group conflict and distrust (e. g., between Christian Orthodox and Jews; see Hillis 2013). On the other hand, as Charnysh 2019 argues, more heterogeneous communities, facing coordination issues, may benefit more from state institutions providing third-party enforcement and boosting taxation and public goods provision. Formal self-governance institutions might have been helpful in terms of bringing representatives of different ethnic and religious groups together and enforcing their cooperation.

4.4 Composition of Revenues and Expenditures

4.4.1 Comparison of 1862 and 1880–84

The reform of urban self-government was accompanied by an impressive expansion of revenues and expenditures, but what elements of city budgets were responsible for it? I start with a descriptive comparison of urban budgets in 1862 and 1880-84. Unfortunately, there are no local price indices avaiable, so that I used the index of Saint Petersburg consumer prices from Mironov 2012 to get approximate estimates of city budgets composition in real terms. The index is provided in averages over decade intervals. I deflate the 1862 revenues and expenditures by the average price level for 1851–1860 and 1861–1870, and the fiscal indicators for 1880–84 – by the averaged means for 1871–1880 and 1881–1890. I take 1851–1860 as the baseline period.

Table 8: City Revenues Before and After the Reform, Rubles per Capita in 1851–60 SPb Prices.

year	Measure	Public Property	Trade Licensing	Real Estate Tax	Indirect Taxes	Other Regular	Extra-ordinary
1862	mean	0.51	0.15	0.04	0.08	0.09	0.29
	median	0.40	0.11	0	0.05	0.03	0.14
1880–84	mean	1.16	0.46	0.15	0.10	0.16	0.78
	median	0.82	0.37	0.10	0.07	0.06	0.58

Note: Calculated for 545 cities of European Russia and Western Krai.

It is evident from Table 8 and the top panel of Figure 6 that all categories of revenues increased, some of them dramatically. The average per capita income from public property – the main source of revenues – rose more than two-fold, the license fees collected from commercial and industrial enterprises more than tripled. As of 1862, a minority of cities taxed real estate and other immovable property, but it became common after the reform. "Extraordinary revenues", which increased almost threefold, included the receipt of arrears, income from city endowments (*gorodskie kapitaly*), profits of public banks, which were allowed to be established by cities, and Treasury subsidies.

Table 9: City Expenditures Before and After the Reform, Rubles per Capita in 1851–60 SPb Prices.

year	Measure	Administra-tion, Police, Firefighters	Public Infra-structure	Edu-cation	Health-care and Charity	Mili-tary	Debt Service	Other
1862	mean	0.54	0.18	0.05	0.02	0.03	0.08	0.05
	median	0.49	0.13	0.03	0	0.01	0	0.03
1880–84	mean	0.96	0.28	0.26	0.11	0.23	0.10	0.83
	median	0.87	0.14	0.19	0.06	0.13	0	0.62

Note: Calculated for 542 cities of European Russia and Western Krai.

The most important takeaway is that although taxation intensified, the largest contribution to the expansion of urban finances came from a more efficient management of public assets, both physical and financial, in accordance with the central government's goals. More specifically, it included profitable leasing out of public land and buildings (Zverinskii and Otsolig

1887, foreword) or increased profits of public enterprises and banks (Tiurin 2007, p. 183).[14] Democratization is typically associated with redistribution via taxation of income or asset holdings, but the example of the Russian Empire demonstrates that in settings with a relatively low tax capacity, revenues generated by public property can serve as a valuable substitute. Greater autonomy based on representative institutions improves governance practices, in particular, rationalizes market-based exploitation of public property.

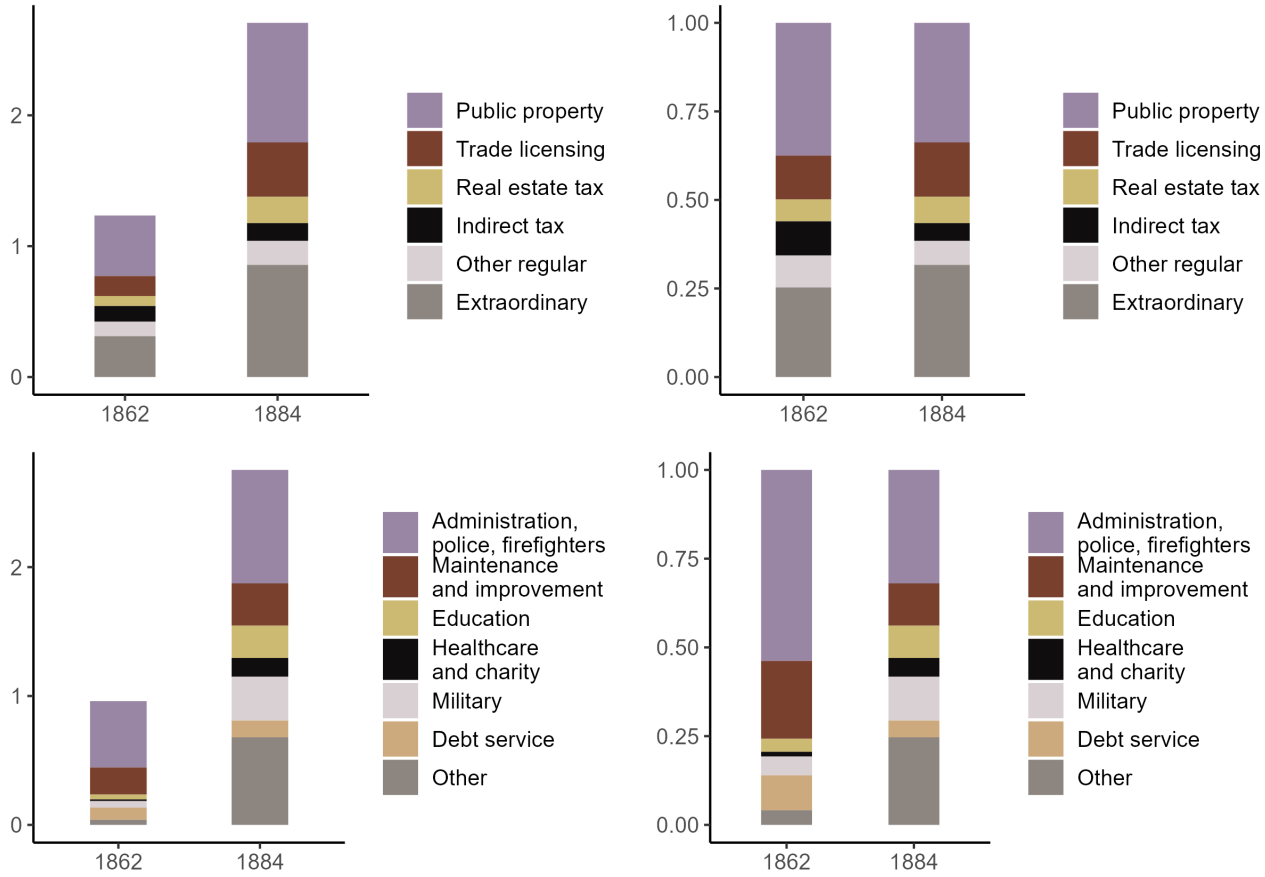

Figure 6: Composition of revenues (top row) and expenditures (bottom row) before and after the reform. Figures in the left columns are expressed in rubles per capita in 1851–1860 Saint Petersburg prices, in the right column – shares of total revenues and expenditures.

How did cities spend their growing income? The classifications of expenditures is not uniform across the two periods: for instance, in 1862, funding of police and fire protection are not shown as a separate articles. The available literature attests that cities were responsible for these services before the reform as well, and they must have been wholly or partly included

[14]Banking profits along with the capital income were part of extraordinary revenues.

in the administrative expenses.[15] Therefore I combine administration, police, and firefighting expenditures in Table 9. The funding of these three categories taken together almost doubled, but their share in total expenditures somewhat decreased (see the bottom panel of Figure 6). These services performed the central tasks of order, security, and property protection. To sum up, post-reform cities spent much more on basic public goods, which must have had beneficial impact on urban development.

4.4.2 Difference-in-Differences Estimates, 1871 and 1878

Next, I proceed to a causal difference-in-differences analysis of the reform impact on different components of revenues and expenditures, measured in 1871 and 1878. Instead of plain logarithms, I apply inverse hyperbolic sine transformation because some categories in some cities take zero values. On Figure 7, I plot regular TWFE estimates from models with time-varying controls and with and without province-year fixed effects. The solid horizontal lines separate estimates for aggregate revenues and expenditures from their components. In alternative specifications, I use shares of different revenues and expenditure components as dependent variables (Figure D).

We can observe a positive and significant effect on total (both regular and extraordinary) revenues and total expenditures of the same magnitude as in the 1870–1884 panel estimates provided above. The inclusion of province-year effects usually brings estimates that are less precise and different in size, but largely consistent with plain TWFE results. There is a considerable positive impact on tax collection, both property and indirect (taxation of pubs and shops, transportation, legal fees, etc.), income derived from the exploitation, leasing, and sales of public property, and profits of city banks. Interestingly, cities also tended to receive greater subsidies from the treasury and *zemstovs* (all-estate bodies of provincial and district self-governance) after the reform. Although exploitation of municipal property was still a major source of fiscal income, its budget share decreased as a result of the reform, whereas the share of property tax

[15]Part of these expenses could also enter the maintenance of public buildings where police and firefighter offices were located.

increased (Figure D).

Regarding expenditures, it would be important to know to what extent the local society participating in self-governance and the state were able to promote their priorities. As was mentioned above, the 1870 city statute prescribed a range of "obligatory" expenditures that must have been satisfied by municipalities in the first place, but did not set any quantitive limits, which created a room for bargaining between urban self-governance and state executive. Sometimes, most of the expenditures could be categorized as "obligatory," but their exact amounts depended on the discretion of local dumas and city mayors.

Nevertheless, it is possible to highlight the areas that were of greater immediate importance to the tsarist authorities. One of them was police: the state felt the need to reinforce the policing of cities before 1870 and launched a comprehensive police reform, mostly affecting provincial and state capital, in the 1860s (Brower 1990, pp. 189–190). Furthemore, the rise of domestic terrorism made the authorities press for "the expansion of the municipal police forces" by the late 1870s (Brower 1990, p. 131). It was common for state officials to make requests to city dumas regarding expanded police funding (Tiurin 2007, p. 131). Another important kind of expenditures, which had been the bone of contention between municipalities and state authorities long before the reform (Nardova 1984), was funding military garrisons located in towns (Brower 1990, p. 131).

The focus on the police and the military reflects the preeminent importance of order and security among the domestic goals of the tsarist government, manifesting itself in other administrative spheres (Gokmen and Kofanov 2019). They were among the main components of obligatory expenditures, the growth of which in the 1870s and 1880s provoked complaints of municipalities (Brower 1990, p. 131). Cities had no control over the way this money was spent, since the municipal police was directly subordinated to the Ministry of Interior (Mikhailovskii 1908, p. 49), and the military–to the Ministry of War. A similar situation was with fire brigades, which, in their turn, were controlled by the police (Mikhailovskii 1908, p. 49), and prisons.

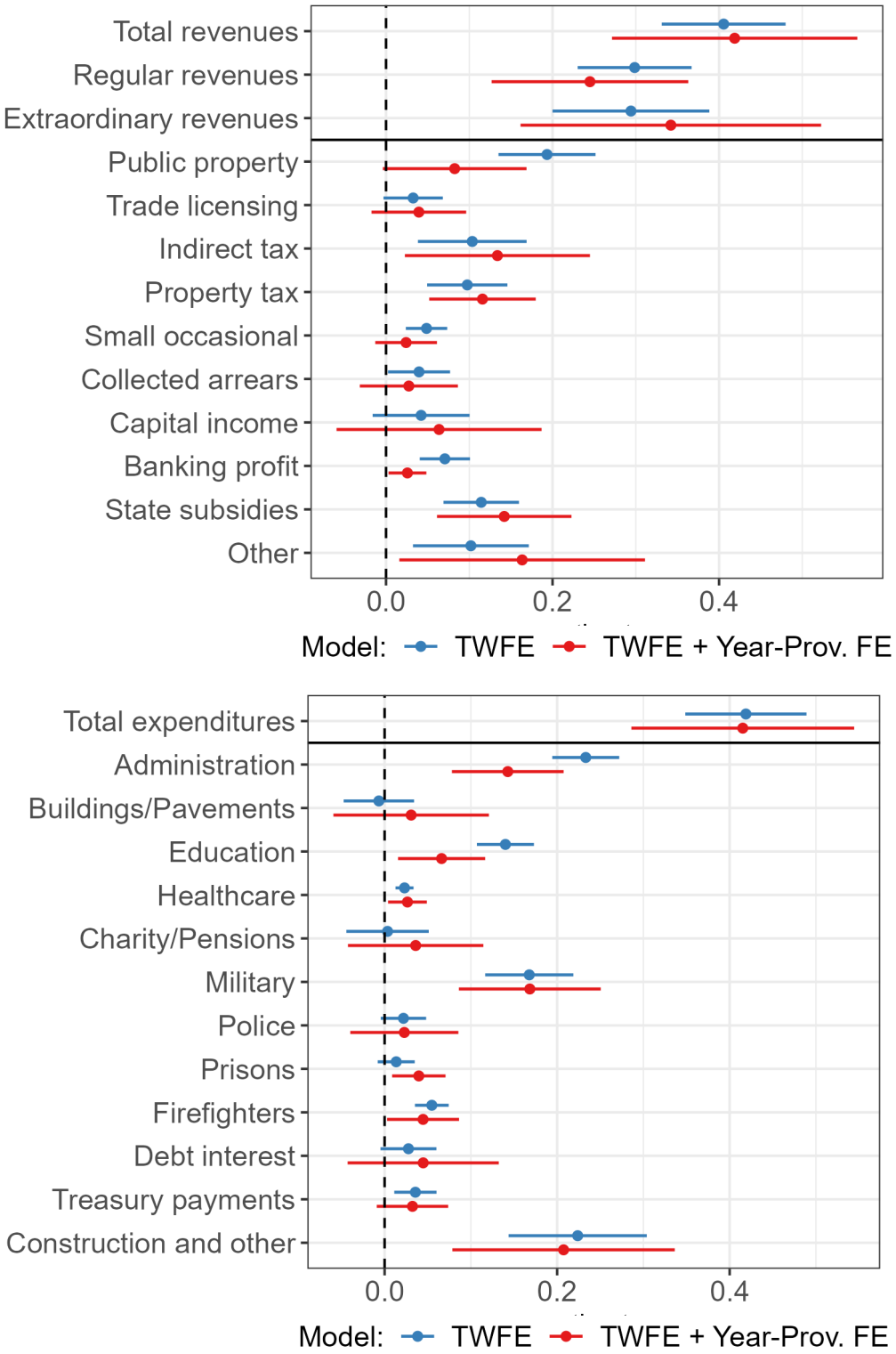

Figure 7: The effect of the reform on per capita revenue and expenditure components (subjected to inverse hyperbolic sine transformation). Regression coefficients for the two-period TWFE panel (1871 and 1878). *Top panel:* revenues components; *bottom panel:* expenditures components. Horizontal solid lines separate aggregate indicators. Regular revenues include income from public property, trade licensing, indirect and property taxes, small and occassional income; extraordinary revenues consist of collected arrears, capital income, banking profits, state (treasury and *zemstvo*) subsidies, and other revenues.

Among other obligatory expenditures, the legislation listed the funding of municipal administration and the maintenance of public buildings and infrastructure. At the same time, spending on education, healthcare, and charity institutions was obligatory only if they belonged to central government agencies; any additional expenses in this sphere were at the full discretion of municipal authorities.[16]

To sum up, we can tentatively expect that spending on police, military, prisons, and firefighters must have been more aligned with state preferences, whereas expenditures on education, healthcare, and charities were to a larger extent driven by the initiative of municipalities.

Figure 7 makes evident that the introduction of self-governance had significant impact on all the categories of expenditures, in particular, administrative and military,[17] but also education, healthcare, firefighters, and prisons. Notably, the reform-induced expansion of police funding is small and statistically insignificant. Moreover, the share of police spending in budgets shrinked, but of the military expenditures–rose (Figure 7). Although on average, cities did not start to spend more on the maintenace of existing public buildings and pavements (and also streetlights), the construction of new buildings and infrastructure and other expenditures increased considerably. It can be concluded that the reform allowed the urban governance to tap on a wide variety of tax and non-tax sources of revenues and maintain at least partial autonomy with respect to spending.

The last emprical exercise provided in this subsection evaluates the interaction effect of industrialization and populational shares of merchants and Orthodox Christians (Figure 8), using TWFE with province-year fixed effects. The interactions are insignificant for most categories, even if the coefficient signs for aggregate revenues and expenditures mostly coincide with the estimates for the 1870–1884 panel. A smaller sample size, entailing underpowered interaction

[16] According to Pisar'kova 2010 (p. 340), in Moscow, "non-obligatory" expenditures included healthcare, education, charity and improvement of public spaces and infrastructure (*blagoustroistvo*). Before the reform, virtually all the hospitals, schools, charities, public buildings, water pipes, parks, and gardens belonged to ministries and other central government agencies (Pisar'kova 2010, p. 338). By 1890s, obligatory expenditures in Moscow primarily consisted of police (50%), fire protection (15–20%), prisons, military garrisons, and courts funding. Between 1873 and 1901, the total share of obligatory expenditures decreased from 60.1 to 16.5%.

[17] The transition to mass conspription in the army, which was finished in 1874, could have also required greater military expenditures.

estimates, might be one of the reasons behind that.

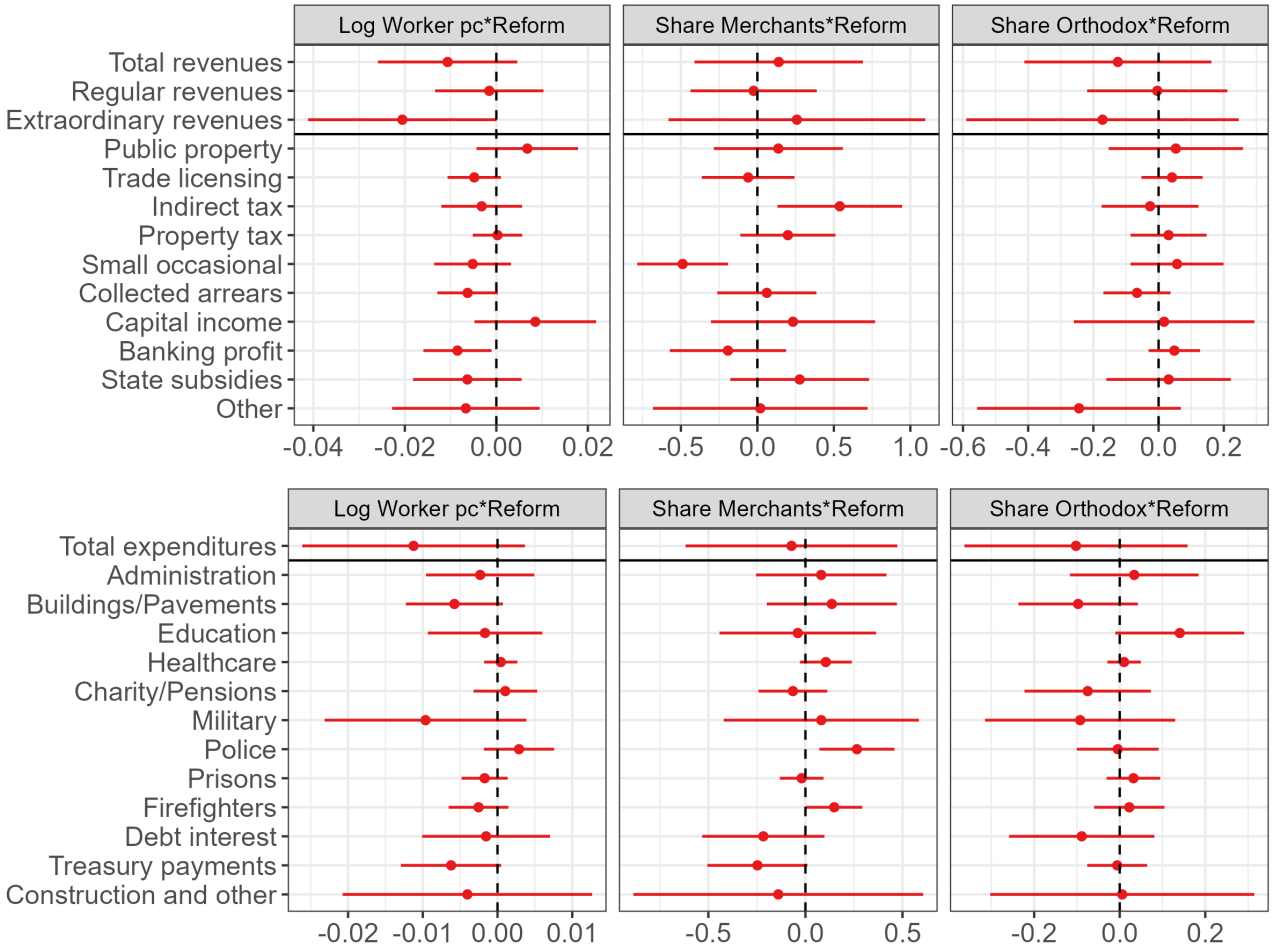

Figure 8: Interaction regression coefficients for the two-period TWFE panel regressions (1871 and 1878). Dependent variables: *top panel:* revenue components per capita; *bottom panel:* expenditure components per capita; inverse hyperbolic sine transformation. Horizontal solid lines separate aggregate indicators.

We do not see a positive and significant interaction of education and healthcare with industrialization. Cities with a higher share of merchants saw a stronger impact on the revenues from indirect taxes, which probably reflected a greater scope of commercial activities, and, to a lesser extent, property taxes. They also favored greater spending on police and firefighters. Thus, we do not observe an influence of industrialists, who could take advantage of new representative institutions in order to cultivate more skilled and healthier labor force. As demonstrated above, merchants might have approached a desired level of revenues and expenditures before the reform, and subsequently, they showed interest mostly in the protection of their property. This evidence is consistent with a more important role of progressive-minded municipal activists in

promoting public goods spending (Brower 1990, Popov 2006).

Also, in cities with a higher share of Orthodox, more was spent on education as a result of the reform. On the one hand, it might have happened because the state- or municipality-sponsored education must have been provided in the Russian language, making non-Russian and non-Orthodox minorities less interested in funding it. On the other hand, many non-Orthodox groups, like Catholics, Protestants, and Jews were on average already more literate than Orthodox, which could limit their demand for the expansion of education.[18] These results provide additional evidence of the influence of local preferences on fiscal policies.

5 Conclusion

This study represents the first attempt at a rigorous quantitative assessment of an undeservedly neglected large-scale subnational democratization episode: the 1870 reform of urban government in the Russian Empire. It demonstrates that even in an unfavorable institutional and economic setting of a backward authoritarian state on the European periphery, a decentralization reform granting western-style representative institutions to municipal communities can empower the local civil society and bring tangible developmental benefits.

My analysis discovers a strong positive impact of the introduction of municipal self-government, even if based on a limited franchise, on city revenues and expenditures. Cities considerably increased their spending on all kinds of public goods, and the necessary funds were obtained not so much by taxation as by a raised profitability of public property, whereas the structure of expenditures was apparently determined by the preferences of both the state and local communities. Cities could derive greater or smaller benefits from the reform depending on their social and religious composition. The financing of the provision of vital public goods, such as

[18]It should also be noted that part of educational institutions, especially those serving certain ethno-religious minorities, were directly sponsored by corresponding communities rather than the city budget. For instance, Vilno, mostly populated by Catholics and Jews, had a considerable number of schools, but reported zero municipal educational expenditures both in 1871 and 1878. In this case, previous community expenditures do not seem to have been substituted by spending from the municipal budget. Some regular municipal schools also used to be directly funded by the local society (MVD 1863, Kupriyanov 2007), but judging by data in MVD 1863, this kind of public fundraising was rare.

education and healthcare, could be driven by ideological, rather than purely economic considerations. As illustrated by the positive effect of the reform on the number of schools, increased spending was converted to actual provision of public goods and developmental outcomes. These findings are in line with the sholarship suggesting a beneficial influence of decentralization due to greater accountability and other mechanisms, and they also complement empirical studies of contemporary municipal reforms (Martinez-Bravo et al. 2012, Pierskalla 2022, Beazer and Reuter 2022), discovering mostly favorable impact of elected self-governance offices and institutions on economic outcomes.

Further research is needed to uncover the mechanisms responsible for the reforms's success. Although it is hard to obtain detailed evidence on every city in the sample, It might be possible to conduct a deeper study of several dozens larger cities (described, for instance, in Nardova 1984 and Alabin and Konovalov 1889) to better understand the role of franchise, social composition of municipal representative bodies, and their functioning before and after the reform. Case studies of the proceedings of newly created legislative and executive bodies in selected cities would also be helpful. In particular, we need to learn more about the process of bargaining with state officials over the composition of revenues and expenditures and the influence of different socio-economic groups represented in self-government.

Studying the 1870 reform is not only illuminating from the perspective of the consequences of democratization, but it also opens a new avenue for understanding its causes. The central authorities designed and implemented the reform seeking to modernize the country and boost local development and security, and they achieved a remarkable success. At the same time, they acted strategically with respect to the pace of granting self-governance to different localities. My ongoing companion project finds that the pace of reform implementation was positively associated with the fiscal capacity of affected cities, and negatively–with the presence of non-Christian Orthodox religious minorities, which posed a threat to the Tsarist regime. The evidence contained in this study shows that it must have relatively hampered the development of municipalities that were denied self-governance.

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Appendices

A Main Empirics

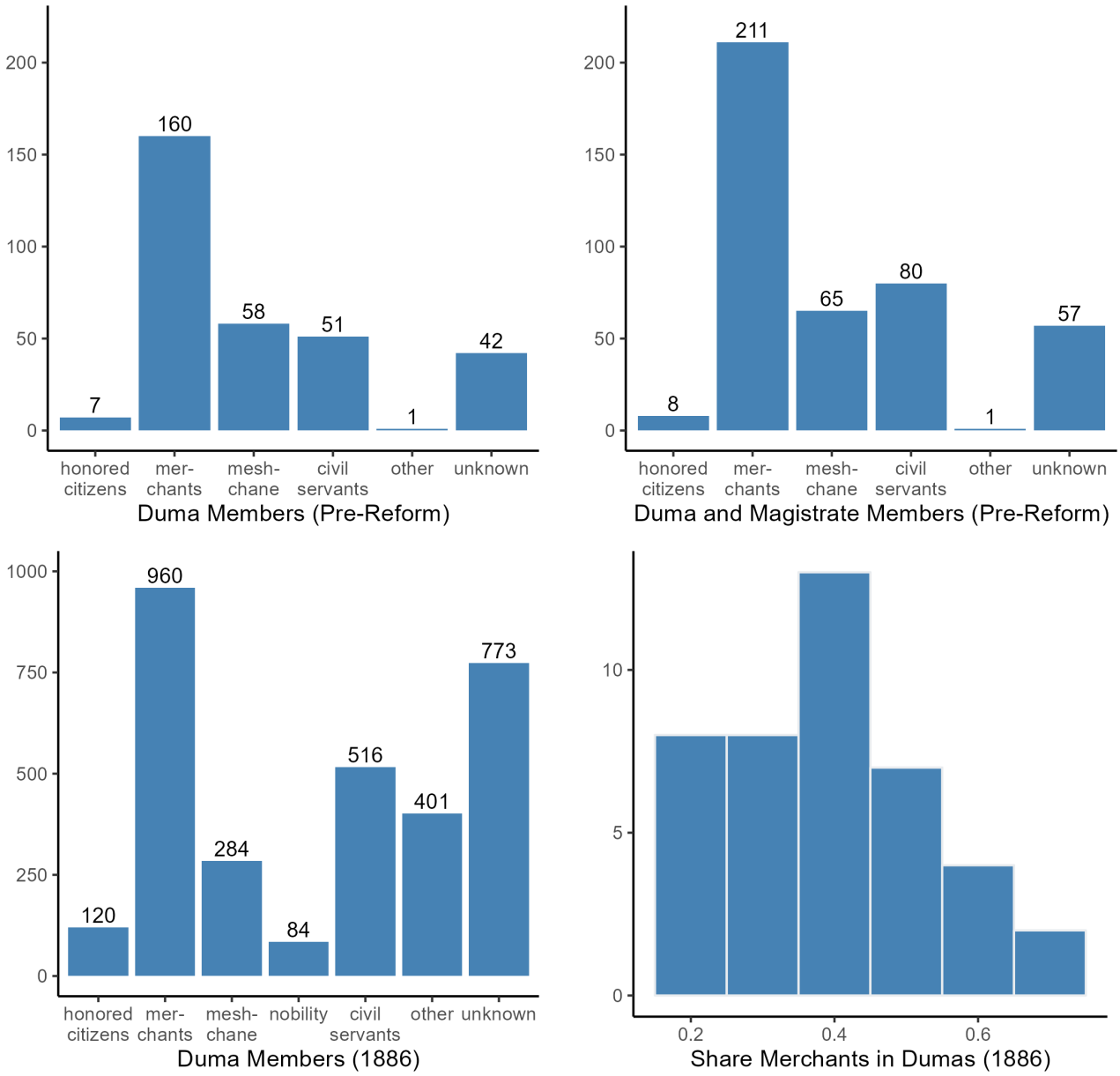


Figure A1: Estate composition of duma and serf-governance members before (top panel, sample of 42 cities) and after (bottom panel, samples of 48 and 42 cities) the reform (in 1886). Sources: pre-reform composition: provincial *pamyatnyye knizhki* for various pre-reform years; post-reform: Alabin and Konovalov 1889; author's calculations.

Table A1: Variables Summary

Variables	N	Mean	St. Dev.	Min	Max
5-Year Periods Panel (1840–1884)					
Log revenues pc	3,570	0.20	0.90	−3.22	4.49
Log expenditures pc	3,567	0.17	0.90	−3.30	3.75
Reform (1/0)	3,570	0.37	0.48	0	1
Log population	3,570	8.56	0.93	5.82	11.94
Log distance to railroad	3,570	4.48	2.48	−5.50	7.59
Temperature deviation	3,570	0.06	0.78	−2.23	4.43
Yearly Panel (1870–1884)					
Log revenues pc	10,155	0.61	0.85	−2.73	4.95
Log expenditures pc	10,155	0.55	0.87	−3.31	4.26
Reform (1/0)	10,155	0.60	0.49	0	1
Log population$_{t-1}$	10,155	8.78	0.94	6.25	13.00
Log distance to railroad$_{t-1}$	10,155	2.12	2.15	−5.50	7.11
Temperature deviation$_{t-1}$	10,155	0.06	1.41	−4.00	5.72
Schools Panel (1861–1884)					
Number of all primary schools	16,920	2.40	3.16	0	82
Number of public primary schools	16,872	1.13	1.55	0	27
Reform (1/0)	16,920	0.36	0.48	0	1
Log city population$_{t-1}$	16,137	8.66	0.91	5.87	12.02
Log distance to railroad$_{t-1}$	16,920	2.88	2.30	−6.28	7.39
Temperature deviation$_{t-1}$	16,920	0.10	1.36	−4.02	5.72
Time-Invariant Variables					
Share merchants, 1861	543	0.09	0.08	0.00	0.48
Share *meshchane*, 1861	543	0.57	0.21	0.00	0.97
Share nobility, 1861	543	0.02	0.02	0.00	0.18
Log industrial workers pc, 1868	543	−3.32	2.97	−10.02	0.00
Religious polarization, 1860s	498	0.32	0.35	0.00	1.00
Religious fractionalization, 1860s	498	0.19	0.23	0.00	0.71
Share Orthodox, 1860s	498	0.81	0.28	0.01	1.00

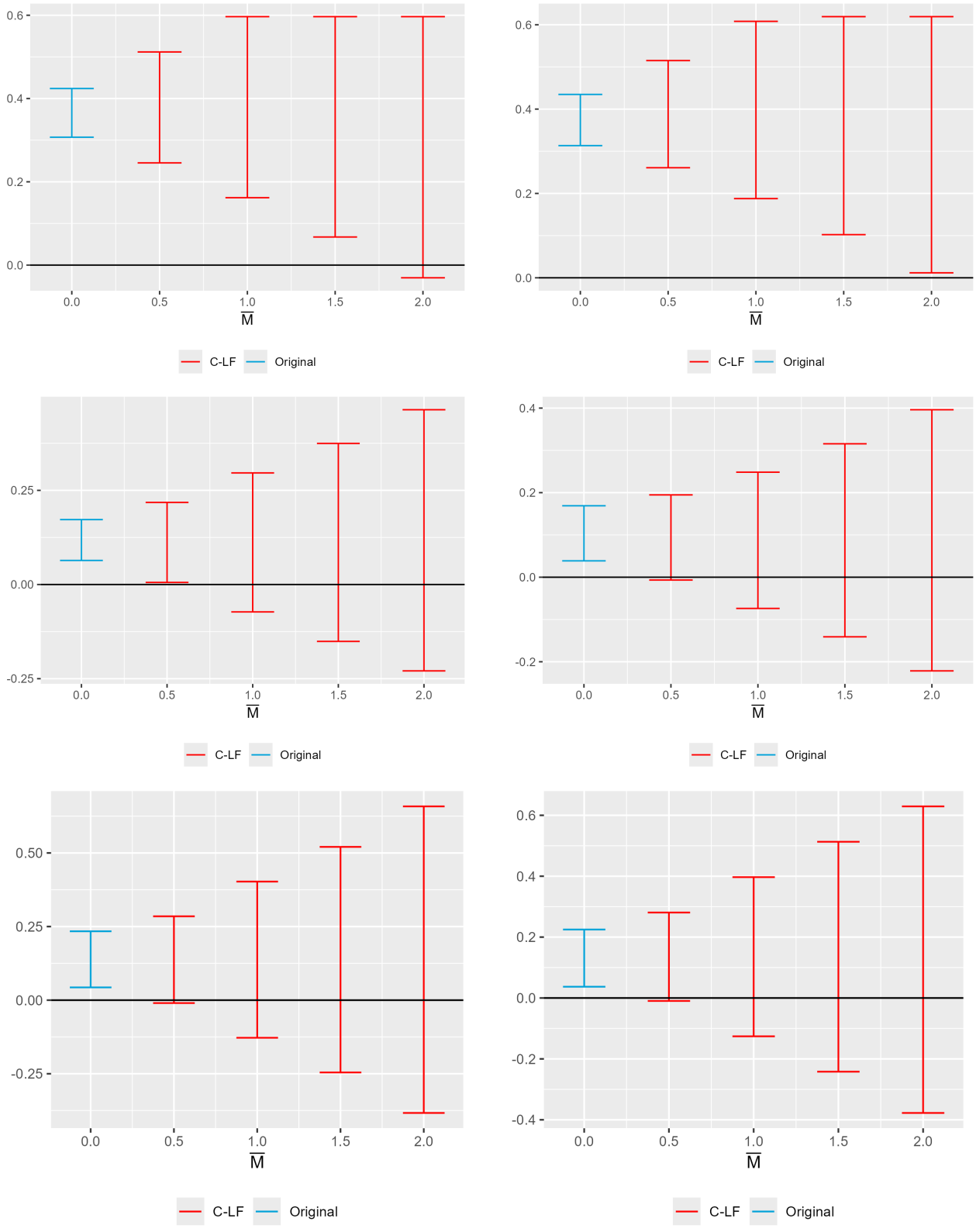


Figure A2: Robust confidence sets ("bounding relative magnitudes") for DiD estimates of the effect of reform on revenues (left column) and expenditures (right column) at different levels of $\bar{M}$ (Rambachan and Roth 2023). Top row: TWFE, middle row: Sun and Abraham 2021; bottom row: Callaway and Sant'Anna 2021 (town and year FE); no controls are added.

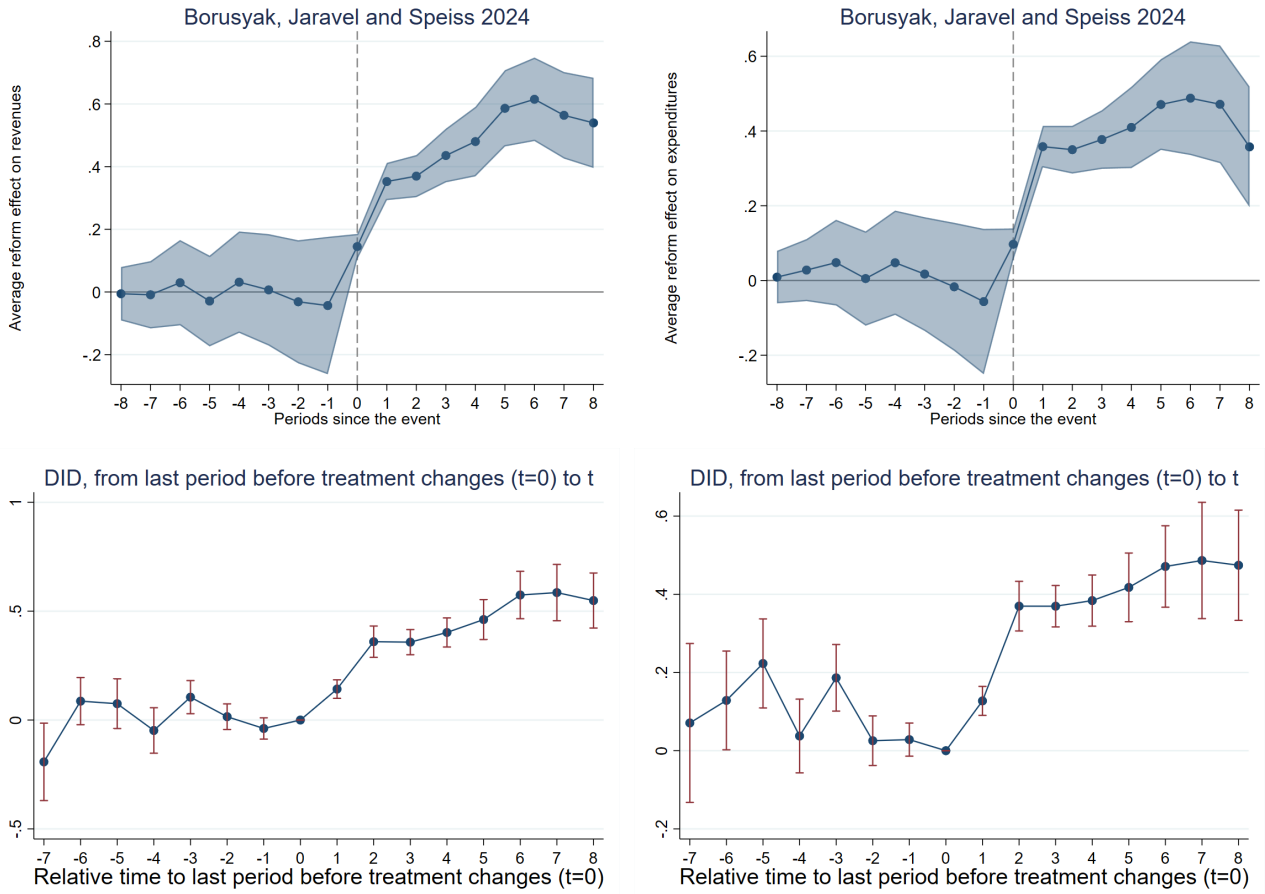
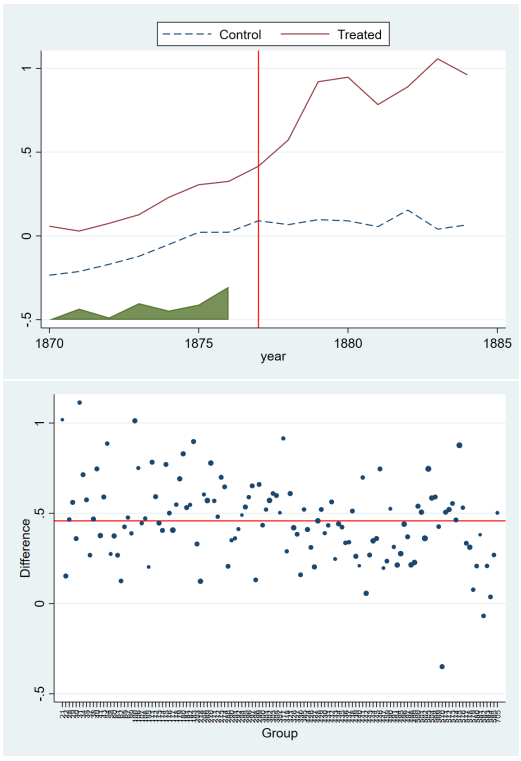


Figure A3: Event study coefficients for Borusyak, Jaravel and Spiess 2024 (top row; implemented using Stata modules `did_imputation` and `event_plot`) and De Chaisemartin and d'Haultfoeuille 2020 (bottom; implemented using stata module `did_multiplegt_dyn`). Left column shows effect on revenues, right–on expenditures. The models include town and province-year fixed effects (province-level "non-parametric trends" in case of De Chaisemartin and d'Haultfoeuille 2020's method); no controls are added.

a) 1874 cohort

b) 1875 cohort

c) 1876 cohort



d) 1877 cohort

Figure A4: Synthetic DiD estimates of the impact of the reform on revenues for selected cohorts that received self-governance in a given year (Model 1 in Table 3). The top figure in each panel shows the trajectories of the treated and synthetic control groups. The bottom figure plots unit-specific effects sized by weights assigned to them.

Table A2: Cohort Effects of Reform from SDID Estimatees

	Tau	Std.Err.	Time
	Model 1: Revenues		
1	.3440134	.0604789	1871
2	.3823678	.0460752	1872
3	.4012575	.0456686	1873
4	.4799034	.0562713	1874
5	.4282342	.0908745	1875
6	.6160164	.082791	1876
7	.4579295	.1354237	1877
8	.5586336	.0958725	1878
9	.4863095	.0810026	1879
10	.3595739	.0602112	1880
11	.5253183	.1027418	1881
12	.4730223	.1737203	1882
13	.1438762	.0604227	1884
	Model 2: Expenditures		
1	.3551225	.0493691	1871
2	.361924	.0478111	1872
3	.405423	.0523788	1873
4	.4639215	.0613318	1874
5	.4042734	.0856545	1875
6	.5783973	.0721401	1876
7	.4381503	.1641255	1877
8	.5707033	.0833154	1878
9	.5130536	.0865717	1879
10	.4257744	.0575733	1880
11	.4962279	.1388626	1881
12	.5667636	.177225	1882
13	-.0786086	.1046048	1884
	Model 3: Revenues, only Poland and Western Krai		
1	.3104838	.0542398	1871
2	-.1339506	.0281161	1872
3	.6302729	.044507	1873
4	.5966136	.1010701	1876
5	.7768214	.1299064	1877
6	.6083748	.1413306	1878
7	.5091002	.1057424	1879
8	.4115912	.078397	1880
9	.5437295	.117636	1881
10	.4908642	.1525025	1882
11	.1424742	.0539019	1884
	Model 4: Expenditures, only Poland and Western Krai		
1	.2176371	.0386249	1871
2	-.2905953	.031253	1872
3	.7739191	.0363859	1873
4	.6069607	.0897456	1876
5	.8867896	.1670404	1877
6	.5830907	.1266021	1878
7	.5204779	.1061318	1879
8	.494044	.0887256	1880
9	.5112721	.1263438	1881
10	.576188	.1564722	1882
11	-.1208002	.0997146	1884

Cohort effects for weighted SDID estimates from Table 3.

B Reform Effect on the Number of Schools

The reform's impact in the fiscal sphere must have been converted into the actual provision of public goods and services. Here I use the data on the number of primary schools and the dates of their establishment as of 1894 (Falbork and Charnoluskii 1900), which enable me to construct a panel dataset for the number of primary schools in a city for each year between 1861 and 1884. There are several potential limitations of this approach. First of all, the authors of the publication took a snapshot of schools that operated in 1894 and did not account for schools that had existed and been closed before that year. It can be reasonalbly assumed that school closures were rare and could not introduce large systemic bias in the estimates. Second, the sample does not include "unorganized" elementary religious schools serving different confessions. Third, as mentioned in the main text, some schools were funded by local communities rather than municipalities. However, if public fundraising was later substituted by budgetary funds, it is more likely to lead to a downward slope in the estimates.

With all these caveats in mind, we can perform a statistical analysis. The dependent variable is the number of schools, not normalized by the population, in a given city-year. I consider two versions of the variable. One is the aggregate number of schools established by state agencies, towns, religious bodies, companies and private individuals (not all of them were necessarily financed from city budgets). The second includes only "community" (*obshchestvennye*) schools, established by cities, *zemstvos* or peasant self-governance institutions.

The results are provided in Table B1. The TWFE-estimated effect of the reform on the number of schools is significant and relatively large for all schools (increase by at least 10% of the mean value), but loses significance if we add controls or use more demanding specifications for community schools. Nevertheless, if we consider event study results for TWFE and the Sun and Abraham 2021 method, provided on Figure B1, in both cases we find large and positive long-term dynamic effects, gradually accumulating over years, although pre-trends reflect potential deviations from the parallel trends assumption.[19] The results seem to be most robust if we apply

[19] I apply endpoint binning to placebo lead effects past the fifteenth year before the treatment.

Sun and Abraham's method to community schools (the bottom-right panel), which might have been more directly affected by the reform. The observed patterns makes sense because the opening of a new school in a city was a relatively rare event, at best happening once in several years, which required considerable preparatory work, so that we expect not a sharp rise in the total number of schools right after the reform, but rather a smooth upward trajectory.

Table B1: The Effect of the Reform on the Number of Primary Schools (1861–1884).

	All Schools			Community Schools		
	(1)	(2)	(3)	(4)	(5)	(6)
Reform_t	0.43^{***}	0.34^{***}	0.20^{*}	0.21^{***}	-0.01	-0.07
	(0.07)	(0.10)	(0.11)	(0.04)	(0.05)	(0.05)
$\text{Log city population}_{t-1}$			1.47^{***}			0.68^{***}
			(0.45)			(0.23)
$\text{Log distance to railroad}_{t-1}$			-0.14^{***}			-0.06^{***}
			(0.04)			(0.02)
$\text{Temperature deviation}_{t-1}$			-0.02			0.01
			(0.05)			(0.03)
Num. obs.	16920	16920	15072	16872	16872	15048
Num. towns	705	705	628	703	703	627
R^2	0.84	0.86	0.87	0.81	0.83	0.84
Adj. R^2	0.83	0.84	0.84	0.80	0.81	0.81
City FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓			✓		
Province × Year FEs		✓	✓		✓	✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Standard errors clustered at the city level are in parentheses.

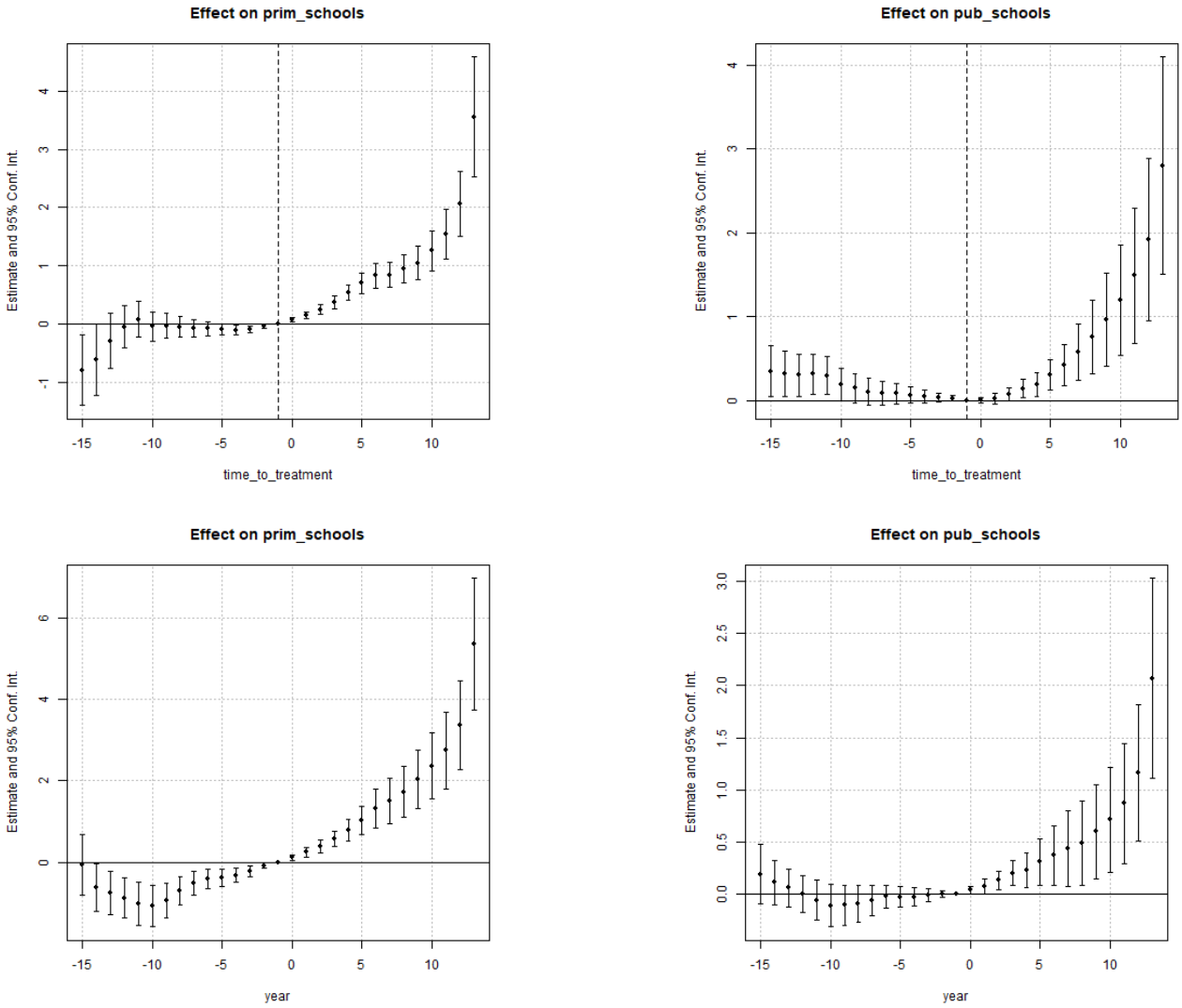


Figure B1: Event studies for schools, based on estimating Models 2 and 5 from Table B1 with dynamic effects. Top row: TWFE version; bottom row: Sun and Abraham 2021 method version. Right column: all primary schools; left column: only "community" schools.

C Regional Variation

The decentralization reform brought positive results in all the regions where it was implemented, although to a various degree. Figure C1 reveals telling differences between the dynamics of revenues and expenditures in different parts of the Russian Empire in 1870–1884. The richest towns were in the Baltic provinces, and they experienced a steep rise simultaneously with the introduction of self-governance institutions in 1878–1881. Between 1870 and 1884, their revenues rose by 74% and expenditures by 65%. Cities of European Russia were less well-off in nominal per capita terms, but still in a much better situation than the Western periphery. They underwent a particularly rapid growth of city budgets in the first half of the 1870s, right after the introduction of self-governance, and reached a plateau by the mid-1880s, when the cumulative effect of the reform must have been fully realized. Overall, in 1870–1884, their per capita revenues increased by 79%, and expenditures by 87%.

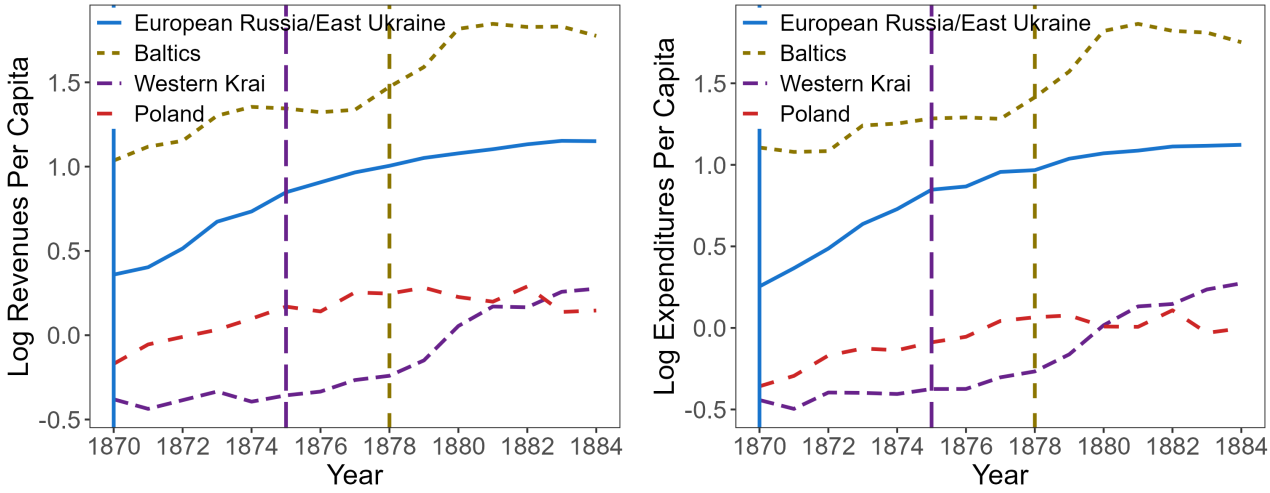


Figure C1: Dynamics of mean nominal revenues (left) and expenditures (right) per capita in European Russia (including Eastern Ukraine and Crimea according to categorizations adopted at that period), three Baltic provinces, and two parts of the former Polish-Lithunian Commonwealth: "Western Krai" (central Ukraine, Belarus, Lithuania), and Congress Poland provinces.

The towns of ten Polish provinces, which were denied representative urban institutions, saw some growth until the mid-1870s, but then stagnated for a decade. As a result, the gap between them and Russian and Baltic provinces widened. The starkest turnaround occurred within the former Polish-Lithuanian Commonwealth: compare "Western *(Zapadnyi) Krai*",

which included central Ukrainian, Belarusian, and Lithuanian provinces (known as "Kresy" in contemporary Poland) and the ten provinces of Congress Poland. The reform in Western Krai was launched in 1875, implemented in most of the cities after 1877, and was followed by a steep rise of the fiscal indicators. Between 1870 and 1884, nominal per capita revenues in this region on average rose by 66%, and expenditures by 72%, vs. 32% and 36% in Poland. Having started with the smallest budgets, Western Krai provinces outstripped Congress Poland by the early 1880s.

These diverging trajectories are unlikely to be explained by relative price movements across regions. Unfortunately, there is no price index for the Russian Empire at that period, but, judging by available evidence, the inflation must have been low and its impact very modest. For instance, an index of consumer prices calculated for Saint-Petersburg grew by only 6% between 1871 and 1890 (Mironov 2012, p. 416, author's calculations). If we consider the yearly prices of rye, available for every province (Mironov 1985), except for the Polish ones, for which I take the price on the Warsaw market, their change in 1870–1884 varied between 12 and 27% across all regions, and was highest in Warsaw. Price levels were quite similar: ranging from 68 to 73 *kopecks* per *pud* in 1870 and 77 to 96 *kopecks* per *pud* in 1884. If anything, accounting for relative inflation seems to put Polish provinces into an even more disadvantaged position.

As a robustness check of my results, placed in the main text, I consider separately former Polish-Lithuanian Commonwealth, consisting of Poland and Western Krai; Western Krai alone; Baltics; and European Russia/Eastern Ukraine. Given that the first part encompasses geographically proximate territories, formerly belonging to the same state, Polish cities should constitute a better counterfactual for the treated cities of Western Krai. For other regions, the control groups would primarily consist of not-yet-treated units. The results displayed in Table C1 reveal significant and positive reform effects for all the regions, with the exception of Baltics, where the effect is positive, but imprecisely estimated, which is not surprising given a small number of cities in the subsample. Remarkably, the ATT effect in Western Krai stays the same regardless of whether we include Polish cities as part of the control group or not. The effect in European Russian and Baltic provinces is considerably lower than in former Poland-

Lithuania and also lies below the full-sample effect, but is still substantial. The estimated effect may depend on the choice of control cohorts and regional specifics, but it always remains sizeable and significant. There is also an obvious tendency of a relatively higher fiscal expansion in the regions with smaller budgets in the pre-reform period.

Table C1: Effect of 1870 Municipal Reform on Revenues and Expenditures by Region.

	Poland & Western Krai		Western Krai		Baltics		European Russia	
	Log Rev. pc (1)	Log Exp. pc (2)	Log Rev. pc (3)	Log Exp. pc (4)	Log Rev. pc (5)	Log Exp. pc (6)	Log Rev. pc (7)	Log Exp. pc (8)
Reform_t	0.38^{***}	0.41^{***}	0.38^{***}	0.41^{***}	0.10	0.11	0.18^{***}	0.17^{***}
	(0.05)	(0.05)	(0.05)	(0.05)	(0.07)	(0.09)	(0.03)	(0.03)
$\text{Log city population}_{t-1}$	-0.72^{***}	-0.65^{***}	-0.74^{***}	-0.69^{***}	-1.17^{***}	-1.28^{***}	-0.70^{***}	-0.66^{***}
	(0.16)	(0.16)	(0.18)	(0.18)	(0.25)	(0.25)	(0.10)	(0.13)
$\text{Log distance to railroad}_{t-1}$	−0.01	−0.01	−0.00	−0.01	−0.03	−0.03	-0.02^{***}	-0.02^{*}
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)
$\text{Temperature deviation}_{t-1}$	−0.02	−0.01	−0.02	−0.01	0.08^{*}	0.11^{**}	0.01	0.02
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.01)	(0.01)
Num. obs.	2700	2700	1680	1680	240	240	6765	6765
Num. towns	180	180	112	112	16	16	451	451
R^2	0.87	0.86	0.86	0.87	0.82	0.81	0.88	0.88
Adj. R^2	0.84	0.83	0.84	0.85	0.78	0.76	0.86	0.86
City FEs	✓	✓	✓	✓	✓	✓	✓	✓
Province × Year FEs	✓	✓	✓	✓	✓	✓	✓	✓

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Standard errors clustered at the city level are in parentheses.

D Composition of Revenues and Expenditures

Table D1: Variables Summary: Revenue and Expenditure Components in 1871 and 1878. Inverse Hyperbolic Sine Transformation

Statistic	N	Mean	St. Dev.	Min	Max
Revenues:					
Total revenues	942	1.31	0.68	0.07	4.75
Regular revenues	942	1.07	0.60	0.05	4.68
Extraordinary revenues	942	0.48	0.52	0.00	3.39
Public property	942	0.65	0.53	0.00	4.09
Trade licensing	942	0.30	0.23	0.00	1.97
Indirect tax	942	0.23	0.30	0.00	3.55
Property tax	942	0.09	0.17	0.00	1.86
Small occasional	942	0.06	0.14	0.00	1.79
Collected arrears	942	0.12	0.19	0.00	1.71
Capital income	942	0.14	0.32	0.00	2.94
Banking profit	942	0.06	0.18	0.00	1.89
State subsidies	942	0.07	0.20	0.00	1.89
Other revenues	942	0.15	0.33	0.00	2.97
Expenditures:					
Total expenditures	934	1.26	0.66	0.05	4.64
Administration	934	0.45	0.32	0.00	2.66
Buildings/Pavements	934	0.14	0.21	0.00	2.33
Education	934	0.14	0.23	0.00	2.98
Healthcare	934	0.03	0.06	0.00	0.58
Charity/Pensions	934	0.07	0.18	0.00	2.26
Military	934	0.12	0.25	0.00	1.87
Police	934	0.17	0.17	0.00	2.15
Prisons	934	0.07	0.10	0.00	0.81
Firefighters	934	0.18	0.17	0.00	1.18
Debt interest	934	0.04	0.17	0.00	2.33
Treasury payments	934	0.07	0.15	0.00	1.93
Construction and other	934	0.38	0.48	0.00	3.34

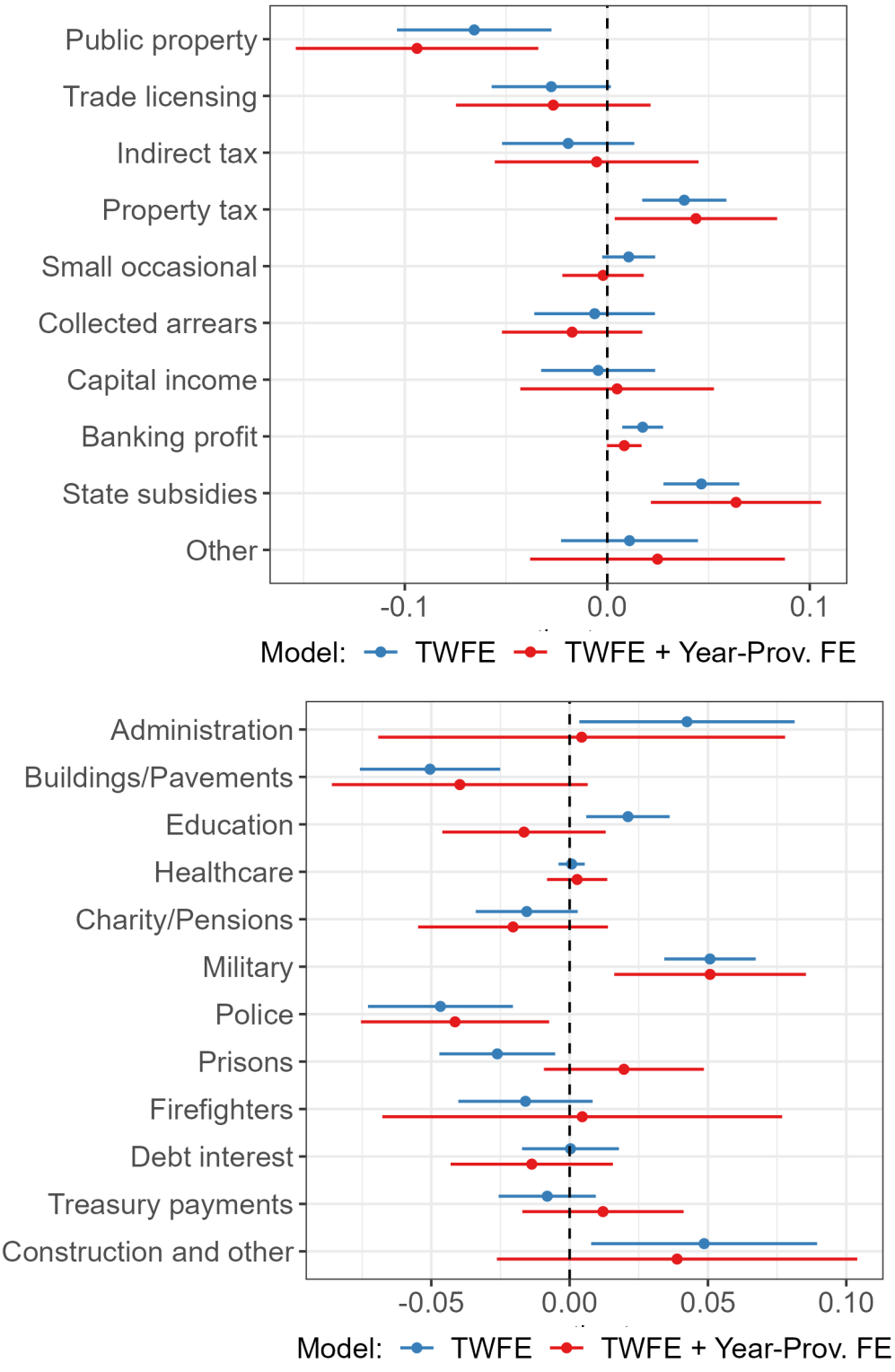


Figure D1: The effect of the reform on the shares of revenue and expenditure components. Regression coefficients for the two-period TWFE panel (1871 and 1878). *Top panel:* shares of revenue components; *bottom panel:* shares of expenditure components.