Losing Capital Status: Does it Matter for a City's Development?

Marta Korczak [1]

[1]Department of Economics, European University Institute

October 27, 2025

Link to the most recent version

Abstract

How do changes in the administrative hierarchy of cities impact their development? This paper focuses on the loss of regional capital status, using the context of the 1999 administrative reform in Poland. Exploiting variation in administrative status, I compare former regional capitals to control cities to construct a causal estimate of the loss of capital status. I find that treated cities experienced a persistent decline in both public and private sector activity, female employment, fertility, and local public good provision, despite receiving higher central government transfers relative to control cities. These results are consistent with a simple theoretical model that shows path-dependence in labor markets and *sticky feet* in migration: a decrease in administrative capacity does not lead to immediate adjustments in employment and migration. The findings highlight that administrative status is crucial for city-level development and that the loss of such status has negative consequences, even when accompanied by increased fiscal autonomy.[1]

JEL Codes: H70, H73, H75, J21, J45, O18

[1]I gratefully acknowledge the financial support from the Early Stage Research Grant of the European University Institute. I would like to thank the seminar participants at the European University Institute, SciencesPo, George Mason University, and during IMD Days in Warsaw. I am grateful to Marian Moszoro, Alessandro Tarozzi, Florian Oswald, Cesar Martinelli, David K. Levine, Bo Honoré, Andrea Ichino, Christina Hauser, Javier Viviens, and Filipe Caires for their helpful comments and advice.

1 Introduction

The administrative status of a city matters. It determines both the size of the public workforce and the sphere of governmental influence on the local economy. While it is well documented what the effects of *gaining* capital status are, not much is known about *losing* it (Chambru et al. 2022, Becker et al. 2021). Intuitively, the effects should not be symmetrical, as the cost of transitioning from the specialized administrative workforce to the private sector might be more costly than the other way around, and the in-built administrative infrastructure might become obsolete instead of being reused for other purposes. Conversely, a city can potentially thrive even after losing administrative functions if it accommodates a large portion of the educated population. The reforms that merge regions and reduce the number of capital cities usually serve to improve the management of public finances, or are an attempt to cut administrative spending. While this kind of reform might be net beneficial on a macro scale (e.g., faster development of large infrastructure projects), it is unclear what the effects are for the municipalities that are no longer the seats of regional power. So far, the effects of losing capital status on socio-economic development have not been explored causally in the economic literature. This paper represents the first attempt to do so, exploiting the context of an administrative reform that took place in Poland in 1999.

The reform was introduced just 10 years after the fall of communism in 1989, so the context I study is still an economy in transition with developing democratic institutions. During the reform, Poland reduced the number of its first-tier administrative regions (*voivodeships*) from 49 to 16. Additionally, this reform created approximately 300 second-tier regions (*poviats*), which I refer to hereafter as "counties". Among municipalities that lost their status as capitals, most became "city-counties", along with another 20 municipalities that had not previously had any administrative functions: such city-counties have an integrated municipal and county administration. Becoming a city-county for an ex-capital reduced the loss of fiscal resources associated with losing a regional capital status. Another important aspect is that before 1999, the self-governance of regions had been virtually non-existent, as regional governors had been representatives of the central government from Warsaw, and they had performed duties as assigned from the Polish capital. In 1999, regions got more autonomy, and Poland also introduced local elections. Therefore, paradoxically, although ex-capitals lost the regional capital status, with a new city-county status, they might have acquired more autonomy (but over a much smaller area than the region they previously officially governed).

The main motivation of the reform was the preparation of Poland to join the EU. As the EU funds were to be administered largely at the regional level, maintaining 49 small regions was deemed inefficient. Larger regions would be better suited to accommodate larger infrastructure projects, thereby accelerating economic development. Additionally, a three-tier administrative hierarchy would resemble the system used by EU countries of similar size, such as Italy, France, and Spain. Furthermore, the floods in Central Europe in 1997 accentuated the perceived inefficacy of disaster management within the system of many small first-tier regions.

To guide the empirical analysis, I outline a simple theoretical model with two cities, a labour market, both private and public sectors, and migration. Administrative capacity, determined exogenously, influences productivity in each sector and consequently employment. Available jobs can change as a result of a negative shock to administrative capacity, leading to either

unemployment or emigration.

In the empirical strategy, I use an event-study design with heterogeneous group-time effects as in Callaway and Sant'Anna (2021). To estimate the effect of losing capital status, I compare ex-capital cities to city-county municipalities (those with integrated municipal and county administration). The combined effect of losing capital status and gaining city-county status is deduced from a comparison of former capital cities to county seats. I also compare city-counties to county seats to estimate the effect of becoming a city-county versus only a county seat. While I rely on the parallel trends assumption, I test its sensitivity using the approach developed by Rambachan and Roth (2023).

Between 1999 and 2003, the ex-capitals received larger transfers from the central government compared to control cities. On average, they received 250 PLN per capita per year (approximately 77 USD), which was around 15% of the monthly average salary in Poland in 1999.[2] On the other hand, the municipality's own revenues decreased by 100 PLN (34% of pre-treatment mean). The additional revenues for ex-capitals were not spent on investment, but rather allocated to salaries in the public sector. Although part of this effect is mechanical: since 1999, salaries in the local public sector have been paid from municipal/county accounts, rather than from the central government. In the labour market, losing capital status was a particularly negative shock for women: between 1999 and 2003, the relative yearly decline in female employment rate oscillated between 3 and 5 p.p. points. The effect for men was also negative, but smaller in magnitude and not statistically significant. I hypothesize that most of the women who lost their employment had worked in public administration or in private firms providing services to the administration. Interestingly, around three years after the reform, compared to city-counties, ex-capitals experienced a significant and persistent fall in births per woman of working age. Potentially, the worsening of the labour market, particularly for women, discouraged many of them from having more children. I observe a delayed negative effect on the migration balance; however, this result is not statistically significant.

This paper contributes to several strands of the literature in urban and public economics. First, it adds to the work exploring the impact of institutional changes on local economic development. Prior studies have focused primarily on the benefits of *gaining* capital or administrative status - such as attracting population, increasing employment, investment, or amenities (e.g., Chambru et al. 2022, Dascher 2000, Becker et al. 2021) - but the effects of *losing* such status remain underexplored. Chambru et al. (2022) investigate the 1790 reform in France in which, due to an exogenous shock, a set of municipalities gained a local capital status. They find that 100 years after the reform, these capitals are 40% more populated than other, comparable cities. Becker et al. (2021), find that Bonn, after gaining capital status in Germany, experienced a substantial increase in public employment. This paper contributes by offering the first causal estimates of the economic impact of *losing* capital status, a relatively rare but highly consequential institutional downgrade. Second, the paper relates to the literature on political decentralization. Administrative reforms, especially those altering the territorial hierarchy (e.g., merging or abolishing subnational units), reshape public finance, governance, and local labor markets (Enikolopov and Zhuravskaya 2007, Tricaud 2019, Martinez-Vazquez et al. 2017, Jin 2023, Foa 2022, Breuillé et al. 2018). In a survey on literature about fiscal decentralization, Martinez-Vazquez et al. (2017) point out that the overall net effect on the economies is positive, while Enikolopov and Zhuravskaya

[2]Control cities: city-counties; treated: ex-capitals that have become city-counties.

(2007), using a panel of 75 countries, find that appointing local politicians instead of electing them does not enhance fiscal decentralization. While I do not focus on local elections in this study, an important aspect of the reform was the introduction of county and regional offices. Importantly, state capacity matters - both in Chambru et al. 2022 and Foa (2022), a more autonomous region does better economically if it has had a higher degree of historically accumulated state capacity. I am contributing to this literature by showing the effects of losing capital status in the context of fiscal decentralization and *de facto* gaining larger fiscal autonomy. Third, this study connects to the literature on reallocation or changes in local public employment. In the context of Italian municipalities in the 2000s, Auricchio et al. (2020) find that exogenous reductions in local public employment stimulated the growth of private jobs. In contrast, Faggio (2019) in studying relocation of 25,000 civil service jobs in the UK in 2004, finds that where public employment increased, there were positive spillovers to the private sector, although highly localised. Similar results were found in Berlin after moving the capital from Bonn in 1999 (Faggio et al. 2018). I add to this literature by showing the labour market effect of lowering the administrative status of a municipality.

The results from the literature indicate an ambiguous potential impact of losing capital status, accompanied by increased self-governance. On the one hand, given past strong administrative capacity, increased autonomy may lead to positive effects. On the other hand, retrenchment in public employment may have a negative impact on cities.

The rest of the paper is organised as follows. In the next section, I explain the context of the reform, which was preceded by heated discussions in the Parliament, as well as street protests. Section three outlines a theoretical framework that explains the main mechanism of the reform. Section four describes the data used and summary statistics, as well as outlines the empirical strategy. Section five presents results, followed by the conclusion in section six.

2 Context

In 1999, the number of first-level administrative regions in Poland (*voivodeships*) was reduced from 49 to 16. The reform not only changed the first-tier structure of administrative regions but also introduced second-tier administrative units (called "counties" hereafter). The capital of a county can have a status of either "city-county" or county seat, with the former bringing more powers to the local administration. Specifically, the main difference between a city-county and a county-seat is that in the former a mayor and a city council rule over the city *and* county, whereas in the latter the county had a separate administration from the municipality. The smallest administrative units, "communes", were preserved from before the reform. Since 1999, a municipality can be a regional capital ("always-capitals", 18 municipalities), a city-county (66), a county seat (248), or a commune seat (2,479). Importantly, 28 of the 31 former capitals became city-counties, along with 20 municipalities that had not previously served as a regional capital.[3] Figure 1 shows the administrative hierarchy of Polish cities from before and after the reform. For demonstration purposes, I introduce labels for each group of cities (Level 1, Level 2, ..., Level 6), abbreviated as L1, L2, ..., L6. If a city serves as a regional capital (L1), it is also the seat of a city-county and a commune. If a city is

[3]Ex-capitals: Ciechanów, Piła, and Sieradz have not become city-counties. Wałbrzych had held a city-county status until 2002 and has regained it since 2012.

a city-county, it is also the seat of a commune (L2 and L4). However, if a city is a county seat, it can only be a commune seat (L3 and L5). A city can also be a commune seat only (L6). Our primary group of interest is L2 - ex-capitals, which have become city-counties. The comparison groups are L4 (commune seats which have become city-counties) and L5 (commune seats which have become county seats).

Before 1999 a city could be:	Since 1999 a city can be:	#	Labels	Labels abbreviation
Regional capital + Commune seat	Regional capital + City-county + Commune seat	18	Level-1 city	**L1**
	City-county + Commune seat	28	Level-2 city	**L2**
	County seat + Commune seat	3	Level-3 city	**L3**
Commune seat	City-county + Commune seat	20	Level-4 city	**L4**
	County seat + Commune seat	314	Level-5 city	**L5**
	Commune seat	2774	Level-6 city	**L6**

Figure 1: Administrative hierarchy of Polish cities before and after the reform.

Regarding the regions, as shown in Figure 2, the reform closely followed the administrative division prior to 1975. The notable exception was the Koszalińskie region, situated between the present-day Zachodniopomorskie and Pomorskie regions on the Baltic coast. The regional capitals after 1999 were mostly the same as before 1975. In Figure 2, they are depicted in the middle map with gray points.

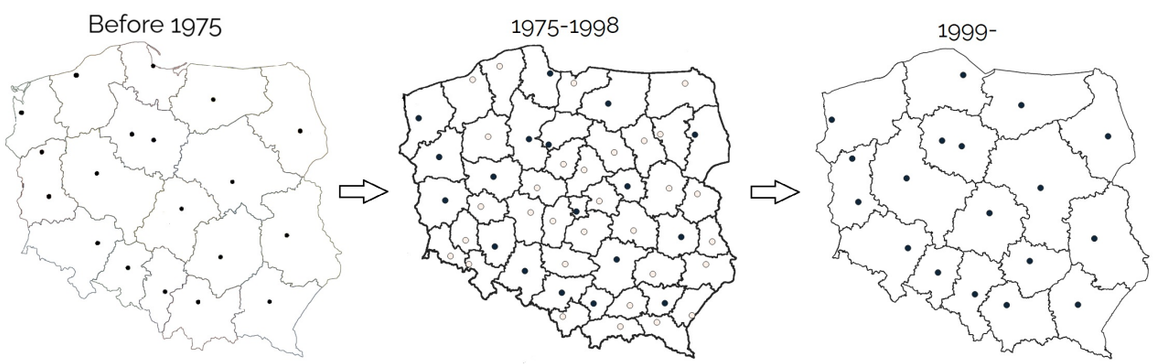


Figure 2: Administrative map of Poland over time

The creation of 49 regions in 1975 was a calculated political decision made by the communist nomenclature. Edward Gierek, the head of the communist party at the time, had been a prominent politician in the Silesia region before taking the highest office in Warsaw. He did not want another powerful communist politician to emerge from the local structures. Therefore, he divided the country into 49 small regions and sent a representative of the communist party from Warsaw to each of them. After the fall of the communist regime, between 1989 and 1998, these 49 regions were still ruled by the government representatives, and therefore, there was very little self-governance.

Figure 3 shows the map of counties in Poland with highlighted city-counties (together with the area of the county). Most of the city-counties are located in Silesia, a region situated in the southwest of Poland. Silesia is predominantly a mining region, with relatively high population density. The other municipalities that were upgraded from commune seat to a city-county status are Gdynia and Sopot (located near Gdańsk, an "always-capital"), Świnoujście (a port city close to the German border), and Grudziądz, located in the northern part of Poland. The main criterion for a municipality (which had not been a regional capital before 1999) to become a city-county was having a population of at least 100,000. Because Silesia

is a highly urbanized region with a large population, most of the city-counties are located there. Only Świnoujście received city-county status due to its strategic location as a port city bordering Germany. Potentially, spillovers from losing capital status might impact control municipalities (city-counties or county seats). Among the outcomes I study, the most likely channels of spillovers are migration and the labor market. Nevertheless, it is far more likely for unemployed people living in ex-capitals to seek employment in always-capitals (where they were often transferred), rather than in city counties.

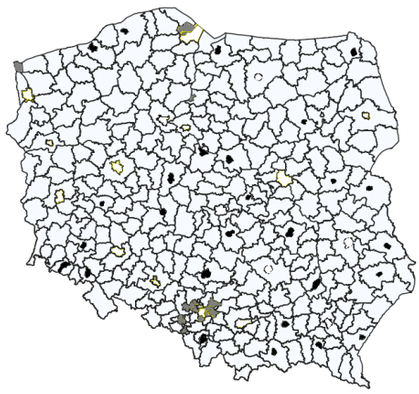


Figure 3: Map of counties in Poland: ex-capitals+city counties are in black, city counties are in gray, always-capitals are in light yellow

It was not necessarily the size that determined which municipalities remained regional capitals. For instance, Bielsko-Biała, one of the ex-capitals, is larger in population than seven cities that remained regional capitals.[4] The choice mostly followed the administrative division from before 1975, but political influences also played a significant role. For instance, Aleksander Kwaśniewski, the President of Poland at the time, with veto power over any legislation, promised the inhabitants of Kielce city a status of capital as a political favor. Additionally, two regions have two capitals, a result of a compromise with local politicians (Lubuskie region, with Gorzów Wielkopolski and Zielona Góra, and Kujawsko-Pomorskie region, with Toruń and Bydgoszcz).

2.1 Timeline of the Reform

The economic crisis brought about by the prolonged inefficiency of the communist system prompted the first discussions about changing the administrative landscape among Polish geographers and urbanists in the 1980s. In 1990, the communes gained power in self-governance: the first elections in democratic Poland were held locally, for commune councils and municipal mayors. In 1993, the government introduced a reform that elevated the status of communes to counties. However, in the same year, it lost the elections to the opposition, and the topic of the administration reform vanished from the public debate until 1997, when it won the elections again. At the beginning of 1998, the ruling coalition presented the first project of administrative reform, which aimed to change the regions in Poland. During the

[4]Also, an ex-capital, Częstochowa, and Radom are bigger than two of the remaining capitals. Finally, Kalisz is larger than Gorzów Wielkopolski.

parliamentary sessions between April and June, politicians discussed dividing Poland into 27, 17, 16, 15, 14, or 12 regions. Finally, in July 1998, the coalition and opposition reached a compromise on the project for 16 regions. The final project, which involved 16 regions and a three-tier administrative division of Poland, was voted on in the Parliament during the night of July 25-26, and the President signed it on July 27, 1998. The first local elections for all tiers — communal, county, and regional governments — were held in October 1998. Since January 1, 1999, Poland has had 16 regions, 373 counties, and 2489 communes.

The heated debate among politicians over the reform reflected the lack of consensus in society. Many municipalities at risk of losing their capital (especially in the context of the most debated idea of division into 12 regions) were genuinely worried about their future. In some cases, citizens took to the streets to protest against the potential loss of capital regions. However, according to a survey conducted in all regional capitals before 1999, not every former capital was afraid to lose its status. For instance, the majority of the population of Słupsk, an ex-capital of the słupskie region, wanted to be part of the gdańskie region after 1998.[5]

Given the lack of consensus among politicians, intense public debate, and even protests in some municipalities, it was difficult to conclude in mid-1998 what the reform would ultimately look like, or even whether it would be implemented. In one of the testimonies presented in the book "Miasto Archipelag" (eng. "City Archipelag") by Filip Springer, an ex-employee of the Regional Office in Łomża (one of the ex-capitals) complains about the lack of preparation to implement the administrative reform: *There was a disagreement about everything (in the last months of 1998). We learned about the reform process around half a year before its introduction, but it was too late. In my opinion, nothing had been prepared for this administrative reform apart from the legislation in Warsaw.* While this is just one testimony, it strongly suggests that the local administration had great difficulty anticipating the effects of the reform prior to its introduction.

2.2 Institutional Differences

Table 1 summarizes changes in administrative functions for the municipalities, according to the legislation from October 13, 1998.[6] Prior to the reform, regional capitals were headed by centrally appointed governors (*voivodes*), who held full administrative authority. These cities had regional offices and sector-specific branches (e.g., police, fire, education), all of which were under state control. Municipalities that later became city-counties typically hosted subordinate district offices. After the reform, new regional capitals hosted both the *voivode*, responsible for state tasks, and a newly elected regional *marshal*, heading the self-government. This introduced a dual structure: regional offices (state) and marshal offices (self-government). Public services were mostly subordinated to the voivode, though some (e.g., employment, road traffic) fell under the marshal's authority. Therefore, we can characterize voivodes as "state's managers", responsible for tasks assigned by the central government, while marshals as "regional executives", deciding together with the regional parliaments about the economic development in their respective regions.

Former capitals that lost status saw their regional offices downgraded to delegations. Many

[5] Please see Appendix A for the details.

[6] Link to the legislation, in Polish

institutions not explicitly listed in the reform law became institutionally orphaned. Staff and assets were partially transferred to the new capitals, weakening local administrative capacity. However, those municipalities that became city-counties, including ex-capitals, gained county-level powers, with unified governance and access to the county tax stream. County-seat cities, by contrast, maintained separate municipal and county administrations. City-counties assumed full responsibility for public services (e.g., secondary schools, healthcare), while county-seat cities remained limited to commune-level services.

Feature	**Old Regional Capitals (pre-1999)**	**New Regional Capitals (post-1999, Level 1)**	**City-Counties (Level 2 and 4) / County Seats (Level 3 and 5); post-1999**
Administrative Status & Governance Structure	Seat of regional administration; governed under centrally-appointed voivodes	Seat of regional administration; governed under centrally-appointed voivodes, and regional self-government (marshal)	City-counties: control over municipality and county; County seats: separate municipal and county government
Offices Present	Regional Office (voivode) + sectoral state branches (e.g., fire, education, police)	Regional Office (voivode) + Marshall Office (regional gov); sectoral services mostly under voivode	City-counties: absorbed district offices; County seats: separate city and county offices coexist
Staff Transfers	N/A (fully centralized state staffing)	Split between the voivode's and the marshal's offices	City-counties: absorbed staff from former district offices; Ex-capitals: partial transfer to new capitals or local units
Revenue Sources	State-financed with limited local discretion	Regional budget (under marshal) + state oversight (voivode)	City-counties: receive both municipal and county tax shares; County seats: municipal government receives only commune-level funds
Public Services	Central government delivered services via regional branches	Split: voivode oversees services like police and fire; marshal handles regional roads, culture, planning	City-counties: responsible for both municipal- and county-level services County seats: county-level services under separate county administration

Table 1: Institutional differences across municipality types before and after the 1999 reform

De facto, former capitals that have become city-counties have gained greater autonomy than before. However, the jurisdiction present in the ex-capital ruled over a much smaller area (county vs. region). Furthermore, decisions about regional infrastructure and higher-level public services (e.g., specialist hospitals) were being made in the remaining regional capitals, and therefore, the population in ex-capitals often felt it was a downgrade. For instance, according to a former mayor of the Bialskopodlaski county (one year after the city of Biała Podlaska lost its capital status): *I admit that our area receives fewer financial transfers than before. We have less money to invest in infrastructure, education, and healthcare, all of which are important sectors. After the region's liquidation, we lost a great deal. We are afraid that the larger regions might not meet our needs. We would like to know the role of Biała Podlaska among other municipalities in the newly created region.*[7] The biggest winners

[7]"Miasto Archipelag", Filip Springer, 2008. Own translation from the original in Polish.

of the reform: remaining regional capitals, not only gained governance over a much larger region than before, but also, since 2004, were the destination of the EU transfers. Therefore, comparing them to the ex-capitals would be misleading. To elicit the impact of losing capital status, I compare ex-capitals (group of municipalities labeled L2 in Figure 1) to city-counties (labeled L4). Before 1999, the former had a larger "administrative capacity" than the latter, but since 1999, this has been equal. In the following section, I outline a simple theoretical model that takes that as a main mechanism driving the labor market and migration outcomes between two hypothetical cities. The definition of the equilibrium, as well as proofs for its existence and uniqueness, can be found in Appendix B.

3 Theoretical Framework

To build intuition behind the main mechanisms of the reform, I present a simple model with endogenous migration and labor market outcomes, including employment in the private and public sectors, as well as unemployment. There are three periods $t = 1, 2, 3$ and two cities, indexed by $i \in \{1, 2\}$. In each period, a continuum of myopic individuals choose between working in the public or private sector, depending on their preferences. However, in periods two and/or three, they might not find employment. They can migrate in the third period, but it is costly, so they might choose to remain unemployed in their home city. Administrative capacity affects productivity in both sectors, but more in the public sector. The capacity changes exogenously in the second period, and as a result, employment allocation might change, triggering unemployment and/or migration.

3.1 Population and Output

Let N_{it} denote the total population in city i in period t. The population in each city can be divided into:

$$N_{it} = N_{p,it} + N_{g,it} + N_{u,it} = 1 \tag{1}$$

where: $N_{p,it}$ are employed in private sector, $N_{g,it}$ are employed in public sector, and $N_{u,it}$: are unemployed. Unemployment might occur only in the second and third period ($N_{u,i1} = 0;\ N_{u,i2}, N_{u,i3} \geq 0)\ \forall_i$.
Output in each city and period is divided between the private P_{it} and the public sector G_{it}:

$$Y_{it} = P_{it} + G_{it} \tag{2}$$

$$P_{it} = A \cdot \text{adm}_{it}^{\beta} \cdot N_{p,it}, \quad G_{it} = \text{adm}_{it}^{\alpha} \cdot N_{g,it} \tag{3}$$

where adm_{it} is administrative capacity, A is the productivity parameter in the private sector, and $\alpha > \beta > 0$ are elasticities of the administrative capacity. The latter can be interpreted as the quality of local institutions shaping the business climate or the efficiency in the production of public goods.

3.2 Preferences and Sectoral Choice

The sector-specific consumption is given by equations:

$$c^p = (1 - \tau)A \cdot \text{adm}^{\beta} + \frac{\tau P}{N}, \quad c^g = \text{adm}^{\alpha} + \frac{\tau P}{N} \tag{4}$$

where τ are taxes from the private sector output P, distributed equally among all agents. The functional form of utility from consumption in either sector is of constant relative risk aversion (CRRA):

$$u(c) = \frac{c^{1-\sigma}}{1-\sigma}, \quad \text{with } \sigma > 0 \tag{5}$$

Agents are heterogeneous with respect to the parameter ϕ, drawn from a uniform distribution: $\phi \sim U(0,1)$. It proxies the weight an agent puts on the public good, while $1-\phi$ is the weight on the private good. I assume that an agent chooses the public sector if:

$$(1-\phi)u(c^p) < \phi u(c^g) \tag{6}$$

Therefore, the indifference cutoff is derived to be:

$$\phi^* = \frac{1}{\frac{u(c^g)}{u(c^p)} + 1} \tag{7}$$

We can thus characterise the share θ^* choosing the public sector as:

$$\theta^* = \mathbb{P}[\phi \geq \phi^*] = 1 - \frac{1}{\frac{u(c^g)}{u(c^p)} + 1} \tag{8}$$

And the welfare of each agent is:

$$U = max\{(1-\phi)u(c^p), \phi u(c^g)\} \tag{9}$$

3.3 Timeline

Period 1

I assume that the administrative capacity in each city adm_{i1} is exogenous and known by everybody. Agents draw their parameter ϕ and choose the sector in which to work. There is no unemployment: $N_{u,i1} = 0, \ \forall_i$. The resulting labor allocation to both sectors in city $i = \{1,2\}$ is given by $N_{g,i1} = \theta_i^* N_i, \quad N_{p,i1} = (1-\theta_i^*)N_i$. Also, there is no migration.

Period 2

At the beginning of period 2, there is a shock to the administrative capacity in both cities: $\text{adm}_{i2} \neq \text{adm}_{i1}$. Importantly, in this period, the number of available jobs in both sectors and cities is fixed at the level of employment from period one, scaled by the change in administrative capacity:

$$\bar{N}_{g,i2} = N_{g,i1} \cdot \frac{\text{adm}_{i2}}{\text{adm}_{i1}}, \quad \bar{N}_{p,i2} = N_{p,i1} \cdot \frac{\text{adm}_{i2}}{\text{adm}_{i1}} \tag{10}$$

As the administrative capacity changes, agents might want to change the sectors (according to the decision rule defined in (6)). However, if demand in either sector exceeds the supply, unemployment occurs. I assume that in this period, agents cannot get employed in the less preferred sector and cannot migrate. Therefore, the employment in both sectors and cities in the second period is given by:

$$N_{g,i2} = \min\left\{\bar{N}_{g,i2}, \theta_i N_i\right\}, \quad N_{p,i2} = \min\left\{\bar{N}_{p,i2}, (1-\theta_i)N_i\right\} \tag{11}$$

And unemployment:

$$N_{u,i2} = N_i - N_{g,i2} - N_{p,i2} \tag{12}$$

with the utility of the unemployed assumed to be zero.

Period 3

Now, migration is allowed: unemployment agents from period 2 can seek employment in another city, but they face migration costs c. They may migrate from city i to city $j \neq i$ only if:

$$\text{available jobs in } j > \text{job demand in } j$$

When they migrate, they are placed into open positions (public or private, depending on an agent's preferences). Before they decide to migrate, they can also find, if available, employment in the less preferred sector. They can also remain unemployed. Their utility in each case would be:

$$\begin{aligned} \text{With migration: } U &= max\{(1-\phi)u(c^p), \phi u(c^g)\} - c \\ \text{Without, working in less preferred sector: } U &= min\{(1-\phi)u(c^p), \phi u(c^g)\} \\ \text{Unemployed: } U &= 0 \end{aligned} \tag{13}$$

Therefore, the decision of the unemployed whether to migrate or not depends on the relationship between migration costs and the difference in utility from working in different sectors (provided that the preferred job is available in another city and the less preferred job is available in the hometown). An agent would then decide to migrate if the difference in utility from working in both sectors is larger than the cost of migrating, according to:

$$max\{(1-\phi)u(c^p), \phi u(c^g)\} - min\{(1-\phi)u(c^p), \phi u(c^g)\} > c \tag{14}$$

Also, if there is no available job in the hometown, the utility of working in a less/more preferred job elsewhere should be larger than the cost of migrating:

$$\begin{aligned} (1-\phi)u(c^p) &> c \\ \phi u(c^g) &> c \end{aligned} \tag{15}$$

3.4 Main Takeaways

I outline the main results and related testable hypotheses for the empirical part here.

Proposition 1: Administrative capacity and unemployment. *A negative shock to administrative capacity ($\Delta adm_{it} < 0$) decreases employment in both the public and private sectors. If migration is costly or incomplete, total employment in the city falls, and local unemployment necessarily increases.*

Sketch of proof. From equations (3) in the model, sectoral outputs are given by

$$P_{it} = A \cdot \text{adm}_{it}^{\beta} N_{p,it}, \quad G_{it} = \text{adm}_{it}^{\alpha} N_{g,it}, \quad \alpha > \beta > 0.$$

A decline in administrative capacity (adm_{it}) reduces productivity in both sectors. In equilibrium, the demand for labor in each sector is proportional to its marginal product:

$$N^*_{p,it} = f_p(\text{adm}_{it}), \qquad N^*_{g,it} = f_g(\text{adm}_{it}),$$

where $f'_p(\cdot), f'_g(\cdot) > 0$. Hence, $\Delta\text{adm}_{it} < 0 \Rightarrow \Delta N_{p,it} < 0,\ \Delta N_{g,it} < 0$.

Total employment satisfies

$$N_{it} = N_{p,it} + N_{g,it} + N_{u,it} = 1.$$

Differentiating with respect to adm_{it} gives

$$\frac{dN_u}{d(\text{adm})} = -\left(\frac{dN_p}{d(\text{adm})} + \frac{dN_g}{d(\text{adm})}\right) < 0.$$

If migration is limited ($M \approx 0$), then the fall in N_p and N_g cannot be fully absorbed by out-migration, implying $\Delta N_u > 0$. Only if migration perfectly clears the local labor market ($\Delta M = -\Delta N_u$) will unemployment remain unchanged.

□

H1: A permanent decline in administrative capacity (such as losing regional capital status) increases local unemployment by reducing both public and private sector employment. The effect should be strongest where the public sector initially represented a large share of local employment and migration costs are high.

Proposition 2: Administrative capacity and local revenues. *A decline in administrative capacity reduces local own-source revenues by lowering productivity and employment in the private sector, which is the sole source of local taxation in the model.*

Sketch of proof. Let local own-source revenues be defined as:

$$T_{it}^{\text{own}} = \tau_p \cdot P_{it} = \tau_p \cdot A \cdot \text{adm}_{it}^{\beta} N_{p,it}, \quad \tau_p > 0,\ \beta > 0.$$

A drop in administrative capacity ($\text{adm}_{it} \downarrow$) reduces both the productivity of private labor and the demand for private labor:

$$\Rightarrow P_{it} \downarrow, \quad N_{p,it} \downarrow .$$

It follows that:

$$\frac{dT_{it}^{\text{own}}}{d\text{adm}_{it}} = \tau_p \cdot A \cdot \left(\beta \cdot \text{adm}_{it}^{\beta-1} N_{p,it} + \text{adm}_{it}^{\beta} \cdot \frac{dN_{p,it}}{d\text{adm}_{it}}\right) > 0,$$

so a negative shock to administrative capacity reduces T_{it}^{own}, holding all else constant.

Since public employment does not generate own revenue, and migration is limited, the municipality faces a structural erosion of its fiscal base.

□

H2: A decline in administrative capacity lowers local own-source revenues by reducing private sector productivity and employment, which constitute the taxable base. The effect should be strongest in municipalities where private sector activity is closely tied to administrative infrastructure or demand.

Remark 1: Migration occurs when gains>costs *Unemployed agents in city i will migrate to city $j \neq i$ if: i) their preferred sector has vacancies in city j, and ii) their utility gain from migrating exceeds cost c:*

$$\Delta U(\phi) = \max\left\{(1-\phi)u\left(c_j^p\right), \phi u\left(c_j^g\right)\right\} - \min\left\{(1-\phi)u\left(c_i^p\right), \phi u\left(c_i^g\right)\right\} > c$$

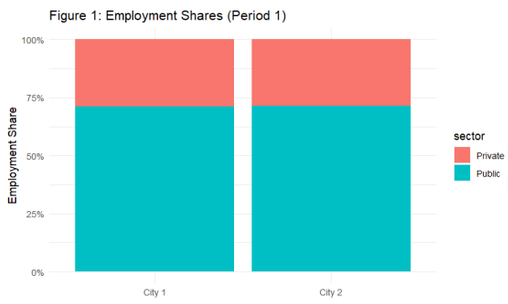
The related testable hypothesis would be:
H3: Migration flows respond positively to the availability of preferred jobs and the utility differential between origin and destination.

Given the studied context, I expect a decline in employment in the former capital cities. If migration is too costly, ex-capital cities should experience larger unemployment rates, despite having similar administrative capacities to city-counties. Furthermore, in the context I study, there is a group of municipalities that have undergone an administrative upgrade (always-capitals). Potentially, laid-off workers in ex-capitals could have emigrated to these municipalities (creating spillover effects). Hence, always-capitals would not be a reasonable control group.

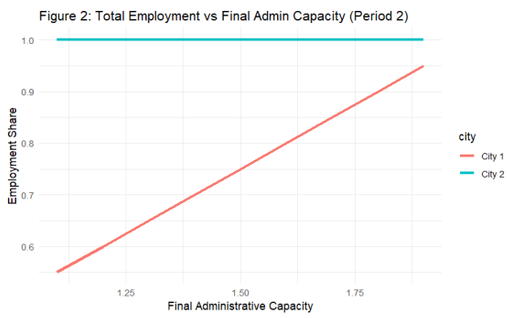
In the next section, I calibrate this model to simulate an example that maps the studied context and provides further intuition for the mechanisms at play.

3.5 Example

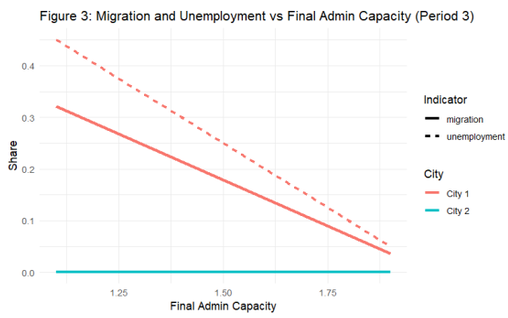
In this example, city 1 has higher administrative capacity than city 2 in the first period ($adm_{11} = 2,\ adm_{21} = 1$), while in the second period their capacities are equalised at value $adm_{i2} \in (1, 2)$. We can think of city 1 as our treated ex-capital, while city 2 is a control city-county.



(a) Period 1



(b) Period 2



(c) Period 3

Figure 4: Simulated outcomes with the following parameters and initial values: $adm_{1,1} = 2, adm_{2,1} = 1, A = 10, \alpha = 0.6, \beta = 0.5, \tau = 0.3, \sigma = 2, \phi \sim U(0, 1)$

In the first period (a), everybody finds employment, with a similar share of people working in the public sector in both cities. In the second period (b), the lower the final administrative capacity is, the smaller the employment share is in city 1. Despite both cities sharing the same administrative capacity, there are no unemployed individuals in city 2, which is consistent with Proposition 1. Finally, when the migration is allowed in the third period

(c), the lower the final administrative capacity, the larger the outflow of people from city 1. The unemployment rate remains larger than the migration share for all values of the final administrative capacity. Migration is limited by both the costs and the availability of public sector jobs in the second city. In turn, nobody is unemployed, nor do they migrate from the city 2.

In the empirical part, I do not focus solely on employment or migration. I hypothesize that the reform affected the labour market according to the mechanisms outlined above, although the development of a city also depends on the evolution of public goods, firms, and sound municipal finance.

4 Empirical Framework

Building on the theoretical model's insights, the empirical analysis examines a broad set of outcomes and employs a robust identification strategy to estimate the causal impact of losing regional capital status. This section describes the data, presents summary statistics, and outlines the empirical strategy.

4.1 Data

I assemble an annual panel of municipal-level data from Statistics Poland's Local Data Bank. The event-study analysis uses data from 1995 to 2008, while the difference-in-differences analyses focus on 1995–2003 to avoid the confounding effects of Poland's 2004 EU accession. Observations prior to 1995 are omitted to sidestep distortions from the 1990 "Balcerowicz Plan" and the early-1990s transition recession (a period of radical market reforms in Poland, often called "shock therapy").[8] For municipalities classified as rural-urban, only the urban portion is included. Additionally, several historical series (e.g., unemployment, sectoral employment, land use, and healthcare resources) were digitized from archival records; these data will be incorporated in future analyses but are not utilized in the current draft. All variables are expressed in per capita terms (or per working-age population for labor market measures).

I examine outcomes across five broad domains, chosen to capture the multifaceted impacts of the reform:

- **Municipal finances**: Local own-source revenues, central government transfers, and expenditures on public-sector salaries and investments.
- **Labor market**: Employment rates (overall and female) and the size of the working-age population.
- **Economic activity**: Number of public-sector firms and private-sector firms, as well as household electricity usage (an indicator of economic activity).
- **Demographics and migration**: Population changes, including net migration, along with demographic indicators such as birth and marriage rates (per relevant population).

[8] Named after the Polish minister and economist Leszek Balcerowicz.

- **Local public goods**: Composite indices of public service provision in three sectors – education, health/family services, and public transport. Each index is constructed as a z-score (standardized) average of several per-capita indicators in its category. For example, the education index aggregates the number of kindergarten places, library books, and public educational institutions; the health/family index includes the number of crèches, doctors, dentists, and public health institutions; and the transport index includes the number of bus lines and public transport firms.[9]

4.2 Summary Statistics

Table 2 presents summary statistics for key outcomes before (1995–1998) and after (1999–2003) the reform, separately for the three main groups of municipalities in the analysis: *ex-capitals* (Level 2 cities, see Figure 1), *city-counties* (Level 4), and *county seats* (Level 5).

Several notable patterns emerge. First, there is a clear upward shift in municipal revenues across all city groups following the 1999 decentralization. Central government transfers per capita increased dramatically everywhere – for example, in ex-capital cities, the mean annual transfer rose from approximately 307 PLN before the reform to 1,257 PLN after – reflecting the new fiscal arrangements that accompanied the administrative reorganization. Own-source revenues also grew, and importantly, ex-capitals and city-counties exhibited very similar fiscal profiles prior to the reform (apart from slightly higher own revenues in city-counties), consistent with Poland's highly centralized pre-1999 system. In terms of expenditures, all groups experienced significantly larger growth in salary spending than in investment spending, with ex-capitals in particular quadrupling their per-capita spending on public salaries (from 197 PLN to 854 PLN) while only modestly increasing their investment outlays. This suggests that a substantial portion of the increased transfers was absorbed by administrative payroll expenses rather than development projects.

Labor market trends indicate widespread deterioration around the time of the reform. Employment rates fell significantly from the late 1990s to the early 2000s in all groups (e.g., overall employment in ex-capitals dropped from around 51% to 42%), largely due to a nationwide economic slowdown and restructuring (Poland's unemployment rate approached 20% in the early 2000s). Female employment declined in parallel with the overall rate, and by the early 2000s, women's employment was particularly low in the city-counties group (around 31%, reflecting the industrial downturn in some of those cities). Despite these job losses, the working-age population remained relatively stable across the board, likely due to the large cohort born in the early 1980s entering the labor force. All groups experienced net population outflows (negative net migration) in the post-reform period, a pattern partly attributable to the rise in suburbanization and out-migration during this time. Notably, even before 1999, the city-counties were already experiencing a net population loss (perhaps due to the decline of local industries, such as coal mining, in several of those cities).

Indicators of economic activity display some resilience: the density of both public and private firms (per 1,000 people) actually increased from pre- to post-reform in all groups. This could reflect a combination of business formation and population decline, which mechanically increases the number of firms per capita.

[9] Primary and secondary school data are excluded due to an education reform in 1999–2000 that introduced new middle schools and restructured school staffing, making pre-/post-reform school counts non-comparable.

Table 2: Summary statistics; pre- and post-reform

	Ex-capitals (L2)		City-counties (L4)		County seats (L5)	
	1995–98	1999–03	1995–98	1999–03	1995–98	1999–03
(per capita unless noted)						
Municipal finance						
Own revenues (PLN)	293.6	787.9	326.9	952.0	283.3	662.9
	(89.6)	(199.3)	(153.9)	(289.0)	(106.6)	(230.7)
Central transfers (PLN)	307.4	1257.2	347.1	907.3	304.8	524.5
	(103.5)	(214.1)	(229.9)	(225.8)	(137.4)	(106.4)
Investment (PLN)	182.0	257.4	186.7	276.6	180.2	192.6
	(92.4)	(120.5)	(242.4)	(182.5)	(108.9)	(124.3)
Salaries (PLN)	196.8	853.9	267.3	655.0	158.4	420.2
	(101.9)	(191.7)	(92.8)	(130.7)	(77.5)	(122.1)
Labor market & demographics						
Employment rate (%)	51.0	42.2	43.0	34.7	46.5	38.9
	(9.8)	(8.8)	(8.8)	(7.4)	(19.1)	(9.5)
Female employment (%)	49.6	41.5	36.0	30.8	45.7	39.5
	(7.1)	(6.5)	(7.4)	(6.2)	(9.8)	(9.0)
Net migration (abs.)	37.3	-162.1	-252.9	-412.5	-14.4	-68.4
	(220.0)	(213.6)	(517.0)	(518.2)	(100.7)	(120.0)
Working-age pop. ('000s)	67.4	68.6	87.6	86.3	22.6	22.4
	(34.3)	(34.3)	(45.2)	(44.1)	(12.6)	(12.4)
Firms & population						
Public firms / 1000 pop.	1.9	3.0	1.1	2.9	1.8	3.9
	(0.6)	(1.7)	(0.6)	(2.1)	(3.2)	(3.0)
Private firms / 1000 pop.	81.8	102.8	64.6	83.2	68.6	94.2
	(14.6)	(16.8)	(21.3)	(27.3)	(19.4)	(18.9)
Population ('000s)	100.7	99.3	128.4	123.4	32.6	31.8
	(51.4)	(50.1)	(65.9)	(63.1)	(18.5)	(17.9)
Public goods indices (std. scores)						
Education index	-0.05	-0.13	-0.38	-0.37	-0.01	0.02
	(0.36)	(0.44)	(0.25)	(0.26)	(0.55)	(0.58)
Health/Family index	0.16	-0.13	-0.46	-0.70	0.02	—
	(0.38)	(0.39)	(0.35)	(0.24)	(0.50)	
Transport index	0.23	0.00	-0.14	-0.12	0.19	0.17
	(0.58)	(0.33)	(0.34)	(0.33)	(0.72)	(0.65)
Observations (N)	112	139	80	100	592	768

Notes: Values are means with standard deviations in parentheses on the line below. County seats' post-reform Health/Family index unavailable (em-dash).

Finally, the local public goods indices suggest that ex-capital cities experienced the largest declines in public service provision after losing their administrative status. The education index of ex-capitals fell from about –0.05 to –0.13 (standard deviations relative to the national mean), and their transport index showed a notable drop (though ex-capitals still had the highest transport provision among the groups). The health/family services index declined post-reform for both ex-capitals and city-counties, with the sharpest drop in city-counties (from –0.46 to –0.70). These declines in public goods are consistent with fiscal constraints

or administrative disruptions in the affected cities.

In summary, three takeaways emerge from the descriptive statistics. First, losing regional capital status was not associated with a reduction in central government support to local budgets – in fact, transfers to ex-capitals grew substantially after the reform. Second, the late-1990s downturn meant that local labor market conditions were worsening throughout Poland, independent of the reform. Third, the ex-capital cities saw the greatest average declines in local public goods provision following the reform (relative to other cities). These patterns underscore the importance of a careful empirical strategy: while ex-capitals did worse in certain dimensions post-reform, one must account for common shocks and trends. I next turn to the identification strategy, which will formally isolate the causal effects of losing capital status from general trends.

4.3 Empirical Strategy

4.3.1 Overview and Identification

The 1999 administrative reform reorganized Poland's territorial governance and created variation in cities' administrative status. My identification strategy exploits this variation by comparing cities that lost their status to appropriate control groups. In broad terms, the goal is to isolate two treatment effects of interest: (i) the effect of losing regional capital status (holding constant the attainment of city-county status), and (ii) the combined effect of losing capital status *and simultaneously gaining* city-county status (relative to cities that only gained a lower-tier county seat status).

To clarify the treatment definitions, I categorize municipalities into three relevant groups (see Figure 1 for the full hierarchy):

- L2 (N=28) – Ex-capitals: cities that lost regional capital status in 1999 but became city-counties (thus retaining an intermediate administrative role).
- L4 (N=20) – New city-counties: cities that became city-counties in 1999 but were never regional capitals before (pure gain of city-county status).
- L5 (N=187) – County seats: cities that became county seats (poviats) in 1999, a lower status in the administrative hierarchy than city-county status.

Using these groups, I construct difference-in-differences comparisons to identify the treatment effects. Following the framework of Roller and Steinberg (2023), the average treatment effect of losing capital status (conditional on city-county status) for the treated cities can be defined as:

$$\begin{aligned} ATT_I = \delta_t(x) = \\ E\left[Y_1 \mid X = x, L2 = 1\right] - E\left[Y_0 \mid X = x, L2 = 1\right] \\ - \left\{E\left[Y_1 \mid X = x, L4 = 1\right] - E\left[Y_0 \mid X = x, L4 = 1\right]\right\} \end{aligned} \tag{16}$$

Likewise, the average treatment effect of losing capital status and gaining city-county status

is captured by comparing ex-capitals to never-capital county seats:

$$\begin{aligned} ATT_{II} = \gamma_t(x) = \\ E\left[Y_1 \mid X = x, L2 = 1\right] - E\left[Y_0 \mid X = x, L2 = 1\right] \\ - \left\{E\left[Y_1 \mid X = x, L5 = 1\right] - E\left[Y_0 \mid X = x, L5 = 1\right]\right\} \end{aligned} \tag{17}$$

In these expressions, Y_1 and Y_0 denote potential outcomes with and without the treatment, X represents covariates, and the expectations are taken over the distribution of X in the treated group. Under standard assumptions – no interference between units (SUTVA), no treatment anticipation, conditional exogeneity of treatment assignment, and parallel pre-treatment trends – equations (16) and (17) identify the causal impact of losing status (alone) and losing status plus gaining city-county status, respectively. Intuitively, the first comparison (ex-capitals vs. city-counties) isolates the effect of losing regional capital functions, net of any general benefits of being a city-county, since both groups (L2 and L4) received city-county status. The second comparison (ex-capitals vs. county seats) captures the joint effect of status loss combined with the upgrade to city-county status, relative to the more modest upgrade of becoming a county seat.

Each of these difference-in-differences comparisons relies on the parallel trends identifying assumption: in the absence of the reform, the treatment and control groups would have evolved along similar trajectories. I also assume there were no significant spillovers (e.g., an ex-capital's decline did not directly affect neighboring control cities) and that there was no anticipation of the reform's exact timing. To bolster these assumptions, I exclude the always-capital cities (18 municipalities that retained capital status in 1999, denoted L1) from the analysis of status loss. Those cities gained substantial new administrative powers after 1999 (e.g., regional infrastructure planning authority), meaning their post-1999 trends are not a valid counterfactual for ex-capitals. Moreover, the selection of which cities remained regional capitals was partly political, undermining any as-if random assignment, and many civil servants from ex-capitals relocated to the new regional capitals, potentially creating spillover effects. By focusing on ex-capitals (L2) and comparing them to similar non-capital cities (L4 or L5), the analysis aims to isolate the net impact of losing the administrative status (and the concurrent changes in autonomy and prestige that came with city-county status). Finally, given the same assumptions as in ATT_I and ATT_{II}, I should obtain the average effect of becoming a city-county on the treated municipalities, if I compare group L4 to L5 (city-counties and county seats).

4.3.2 Event-Study Design

I begin by estimating an event-study specification to visualize the dynamic effects of the reform and to test for parallel pre-trends between treated and control cities. Specifically, I estimate the following two-way fixed effects model around the time of treatment:

$$Y_{it} = \alpha_i + \delta_t + \sum_{k \neq -1} \beta_k \cdot \mathbb{1}\{\text{event\_time}_{it} = k\} + \varepsilon_{it} \tag{18}$$

where Y_{it} is the outcome for city i in year t, α_i are municipality fixed effects, δ_t are year fixed effects, and $\mathbb{1}event\_time_{it} = k$ is an indicator for being k periods from the reform (so, for example, $k = 0$ in 1999 for treated units). The coefficient β_k thus represents the average outcome difference between treated and control cities k years relative to the reform, with

$k = -1$ (the year before the reform) as the omitted baseline. Standard errors are clustered spatially: municipalities within a 50 km radius are grouped into the same cluster.[10] Under the parallel trends assumption, the pre-treatment coefficients β_k for $k < 0$ should be close to zero (no systematic differences in trends before 1999). The post-treatment coefficients β_k trace out the dynamic treatment effects – in the first year after the reform ($k = 0$), two years after ($k = 1$), and so forth.

I implement the event-study for each of the key treatment comparisons described above. In the first comparison, the "treatment" is defined as losing regional capital status conditional on becoming a city-county (L2 vs. L4), corresponding to the causal effect β_k that should estimate ATT from equation (16) at event time k. In the second comparison, the treatment is losing capital status and gaining city-county status (L2 vs. L5), so β_k in that specification estimates the effect defined by equation (17). For completeness, I also examine a third comparison (L4 vs. L5) where the treatment is gaining city-county status (without prior capital status), which isolates the effect of the city-county designation itself (Appendix C). These event-study estimates enable a visual assessment of whether treated and control cities followed similar trends before 1999 and how their paths diverged after the reform.

4.3.3 Robust Inference under Relaxed Parallel Trends Assumption

While the event-study approach can indicate whether pre-trends are parallel, it relies on the assumption that there are no differences in trends prior to treatment. To assess the robustness of the results to potential violations of parallel trends, I apply the "Honest DiD" methodology proposed by Rambachan and Roth (2023). This approach constructs partial identification bounds for the treatment effects by allowing for some divergence in pre-trends between treated and control units.

In the honest DiD framework, let δ_t represent a possible deviation from parallel trends in period t (i.e., the difference in treated–control trends that is not due to treatment). The conventional DiD assumption is $\delta_t = 0$ for all post-treatment periods. Instead, Rambachan and Roth's method allows δ_t to be nonzero but constrains these deviations to lie within a plausible set Δ chosen by the researcher (based on observed pre-period data). For example, one can impose a bounded difference restriction on consecutive period deviations:

$$|\delta_t - \delta_{t-1}| \leq M, \quad \forall t \geq g, \tag{19}$$

which limits how quickly the treated–control trend difference can change after treatment, with M calibrated to the maximum change observed in the pre-treatment period. More generally, the identified set for the true event-study coefficients can be written as:

$$\beta_t^{\text{Honest}} \in [\widehat{\beta}_t - \delta_t^-,\ \widehat{\beta}_t + \delta_t^+], \quad \delta_t \in \Delta, \tag{20}$$

as in equation (19), where $\hat{\beta}_t$ is the ordinary event-study estimate from (18) and $\underline{\delta}_t, ; \overline{\delta}_t$ are the researcher-imposed bounds on trend deviations (derived from set Δ). By varying the magnitude of allowed deviation M (e.g., permitting post-1999 trends to differ by up to 0.5, 1, or 2 times the largest pre-1999 trend fluctuation), we obtain "honest" confidence intervals for the treatment effect. These intervals remain valid even if the parallel trend assumption is moderately violated, as long as any post-treatment divergence in trends is no

[10] Estimations using clusters with different distances are to be added in the subsequent drafts of this paper.

more severe than the worst-case divergence observed before the reform. I report results from this robustness check for key outcomes in Appendix E. Some of the findings change under reasonable relaxations of the parallel trends assumption (e.g., allowing post-treatment trend shifts up to 1× the pre-trend differences). This is the main reason I employ covariate-adjusted DiD (Callaway and Sant'Anna 2021).

4.3.4 Two-Way Fixed Effects

To summarize the reform's overall impact in a single estimate, I also estimate a standard difference-in-differences regression with two-way fixed effects (TWFE). This involves a simpler specification:

$$Y_{it} = \alpha_i + \delta_t + \beta D_{it} + \varepsilon_{it} \tag{21}$$

where D_{it} is an indicator equal to 1 if municipality i has lost capital status by time t (and 0 otherwise). Here, β is intended to capture the average treatment effect of the reform over the post-1999 period (in practice, I focus on the 1999–2003 window for this aggregated analysis). Standard errors are again spatially clustered at a distance of 50 km. While convenient, the TWFE estimator rests on the assumption that the treatment effect is homogeneous across all treated cities and time periods. If treatment effects are heterogeneous (which is likely in this context), the TWFE estimate β will represent a weighted average of various group-specific effects, and those weights can even be negative if some groups experience later treatment or different magnitudes (Goodman-Bacon 2021). Moreover, any violation of the parallel trends assumption would bias β. Given these concerns, the TWFE results are interpreted with caution. I use them primarily as a baseline summary, supplementing them with more flexible estimators that allow for treatment effect heterogeneity.

4.3.5 Difference-in-Differences with Multiple Time Periods (CS-DiD)

Finally, I implement the recent difference-in-differences estimator of Callaway and Sant'Anna (2021), which is well-suited to settings with staggered treatment timing or heterogeneous effects over time. In our setting, the reform treatment occurs in a single year (1999) for all treated units, so timing is not staggered; however, the Callaway–Sant'Anna group-time average treatment effect framework provides a robustness check that does not rely on TWFE's homogeneity assumption. This approach estimates separate treatment effects by group and time and then aggregates them.

Formally, let $G_i = g$ indicate that municipality i is first treated in year g (for ex-capitals, $g = 1999$) and let $C_i = 0$ denote a never-treated unit (here, the control cities). Define $\text{ATT}_{g,t} = E[Y_{it}(1) - Y_{it}(0) \mid G_i = g]$ as the average treatment effect for units first treated in year g at time t. In our case, with a single treatment cohort, $\text{ATT}_{1999,t}$ is the effect for ex-capitals at time t, and we compare them to the never-treated controls. The Callaway–Sant'Anna estimator for $\text{ATT}_{g,t}$ is constructed using an inverse probability weighting (IPW) and outcome regression approach (doubly robust). Intuitively, it re-weights control units to match the treated units' covariate distribution and then computes differences in outcomes.

The doubly robust estimator of the group-time ATT (for the treated group g in period t)

can be written as:

$$\widehat{\text{ATT}}_{g,t}^{DR} = \frac{1}{N_g} \sum_{i:G_i=g} (Y_{it} - \widehat{\mu}_0(i,t)) - \frac{1}{N_0} \sum_{i:C_i=0} (Y_{it} - \widehat{\mu}_0(i,t)) \tag{22}$$

where $\hat{\mu}_0(i,t)$ is the predicted outcome for unit i at time t in the absence of treatment (estimated from a regression on control units in the pre-treatment period). In this formulation without covariates, the difference in brackets $Y_{it} - \hat{\mu}_0(i,t)$ represents the outcome surprise relative to the no-treatment prediction, and averaging those surprises for treated vs. control yields an estimate of the treatment effect. When incorporating covariates X_i, the estimator adjusts the control group using propensity score weights $\hat{w}_i(X_i)$ to ensure comparability:

$$\widehat{\text{ATT}}_{g,t}^{DR} = \frac{1}{N_g} \sum_{i:G_i=g} [Y_{it} - \widehat{\mu}_0(X_i,t)] - \frac{1}{N_0} \sum_{i:C_i=0} \widehat{w}_i(X_i) \cdot [Y_{it} - \widehat{\mu}_0(X_i,t)] \tag{23}$$

Here $\hat{\mu}_0(X_i,t)$ denotes the predicted untreated outcome as a function of covariates (again estimated from the never-treated units), and $\hat{w}_i(X_i)$ are inverse probability weights derived from a propensity score model for being an ex-capital. The inclusion of covariates in this CS-DiD estimator can improve balance and account for any observable differences between treated and control cities, thereby strengthening the credibility of the parallel trends assumption. I report covariate-adjusted CS-DiD results alongside the traditional TWFE estimates in the results section.

With the data, descriptive patterns, and empirical strategy in place, the analysis now turns to the results. The next section presents the event-study graphs, "honest" confidence intervals, and difference-in-differences estimates that quantify the impact of losing capital status on city development.

5 Results

Losing regional capital status induced persistent adverse effects across multiple domains of city development. Ex-capital cities did receive substantially higher central government transfers after the reform than comparable cities, but these funds were largely absorbed by public-sector salaries rather than productive investment. At the same time, ex-capitals saw a decline in own-source revenues, indicating a weaker local tax base. Consistent with a contraction in administrative employment, ex-capitals experienced significant declines in local employment – particularly among women – yet little evidence of offsetting out-migration. The number of active firms (both private businesses and public institutions) in ex-capitals fell relative to control cities, signaling a broader economic downturn. Finally, a few years after the reform, demographic indicators worsened: birth rates and marriage rates declined in the affected cities, suggesting that economic uncertainty had translated into delayed family formation and population stagnation.

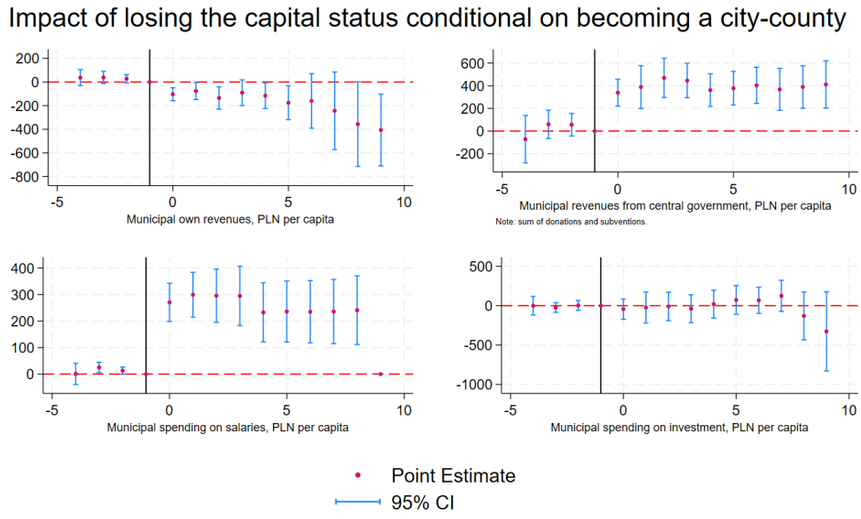
The subsections below detail these results by outcome category. I begin with municipal finance, the immediate channel through which the reform impacted cities' resources. Next, I assess labor market outcomes and overall economic activity, which are central to understanding the reform's impact on local economies. I then examine migration and demographic responses, which may have been indirectly influenced by the labor market shock. Finally, I consider local public goods provision as a gauge of municipalities' capacity to invest in services after the reform.

Methodological note: For most outcomes, I employ an event-study difference-in-differences design (with covariates and municipality and year fixed effects) as outlined by Callaway and Sant'Anna (2021). For fiscal outcomes, I present a traditional event-study according to (18), with a window of 1995-2008. For all other outcomes, the window is 1995–2003 to avoid conflating the reform effect with the post-2004 structural changes. These dynamic estimates are adjusted for covariates to improve balance in pre-trends (as described in the empirical framework). I present "honest" confidence intervals following Rambachan and Roth (2023) to assess sensitivity to potential pre-trend violations in Appendix E. In addition to the event-study graphs, I report aggregate difference-in-differences estimates for the 1999–2003 period in tabular form. These include both a conventional two-way fixed effects (TWFE) specification and the covariate-adjusted difference-in-differences estimator from Callaway and Sant'Anna (2021). In the main text, I focus on two key treatment–control comparisons: (1) ex-capitals (L2) vs. city-counties (L4), which isolates the effect of losing regional capital status conditional on gaining city-county status (i.e., the impact of status loss beyond the general decentralization that affected all city-counties); and (2) ex-capitals (L2) vs. county seats (L5), which captures the combined effect of losing capital status and gaining city-county status, relative to the more modest administrative upgrade of becoming a county seat. The identifying assumption is that, absent the reform, the treated and control groups would have followed parallel trends. Always-capital cities (L1, which retained their status and gained additional powers) are excluded from these comparisons, since their post-1999 trajectories are not a valid counterfactual. For completeness, I compare outcomes in surrounding areas (neighboring towns and villages) of treated vs. control cities – though these are not the primary focus of the analysis (Appendix F).

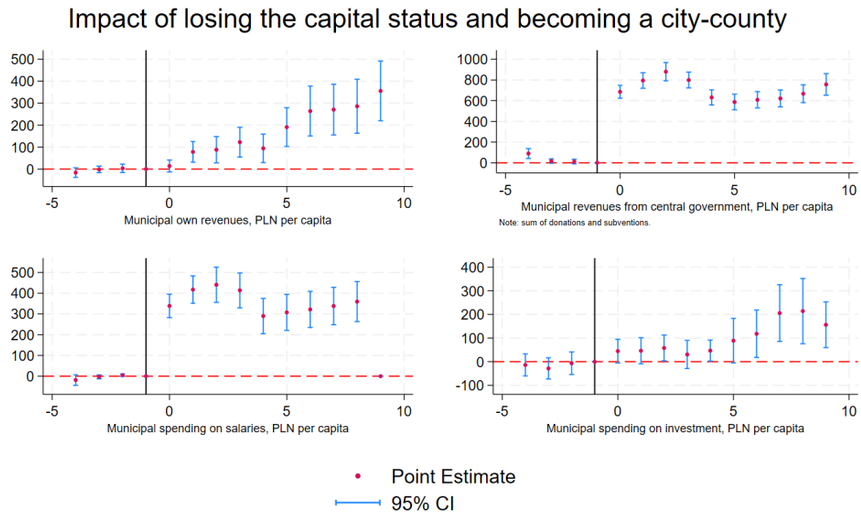
5.1 Municipal Finance

Because the 1999 reform was a decentralization of government, its most direct impact was on municipal finances: more funds were devolved to local budgets, and new expenditure responsibilities were assigned to cities based on their post-reform status. Figures 5a and 5b present event-study estimates for key fiscal variables, using ex-capital cities as the treated group. Panel 5a (ex-capitals vs. city-counties, L2 vs. L4) shows that ex-capital municipalities experienced a sizable drop in own-source revenues after losing capital status. The gap is around 100 PLN per capita on average in the post-1999 years, meaning ex-capitals collected significantly less local revenue than the city-counties (for context, 100 PLN is roughly 34% of the pre-reform mean of per-capita own revenue; see Table 2). This decline suggests a relatively weaker economic and tax base in cities that lost their administrative status. Conversely, central government transfers to ex-capitals rose dramatically: ex-capitals received on the order of 400 PLN more per capita in transfers each year compared to city-counties through 2003–2004 (about 130% of the pre-reform mean). On the expenditure side, most of these additional funds were allocated to public-sector salaries. Ex-capitals spent roughly 300 PLN more per capita on administrative salaries post-reform relative to city-counties (approximately 158% of the baseline mean). By contrast, there was no discernible increase in investment spending in ex-capitals. In sum, Panel 5a indicates that although ex-capitals were compensated with much higher transfers from Warsaw, they suffered a loss in their own revenues, and the additional funds were largely absorbed by salary expenditures rather than development projects. This outcome can be partially explained by the fact that, before 1999, personnel costs for regional administration were borne by the central government, and

after the reform, many of those administrative offices (and their employees) remained in the ex-capital cities, with their salaries now paid out of local budgets. But the control cities have also experienced an increase in administrative functions, which should have led to similar increases in salary spending.



(a) Event study: ex-capitals (Level 2 cities, treated) vs. city-counties (Level 4 cities, control)



(b) Event study: ex-capitals (Level 2 cities, treated) vs. county seats (Level 5 cities, control)

Figure 5

Panel 5b (ex-capitals vs. county seats, L2 vs. L5) shows the combined effect of losing capital status and gaining city-county status. Ex-capitals ended up with higher own revenues than county seats, and they also received much higher transfers, on the order of 800 PLN per capita more, which is nearly half of an average monthly salary in 1999. Once again, most of this windfall appears to have gone into maintaining the public payroll: expenditures on salaries by ex-capitals increased after 1999 (relative to county seats), whereas investment spending showed at most a slight and statistically insignificant rise. In other words, even when given greater fiscal autonomy and extra central funding, the former capitals did not significantly increase their development expenditures in the early post-reform years.

To assess the robustness of these fiscal results, Table 3 reports "honest DiD" confidence intervals for the main finance outcomes in the ex-capital vs. city-county comparison. This analysis (following Rambachan and Roth 2023) relaxes the parallel trends assumption by

allowing for certain bounded pre-trend differences. Reassuringly, the negative impact on own-source revenue in ex-capitals remains statistically significant so long as any post-1999 trend deviations are not more than 0.5 times as large as the biggest pre-1999 trend change (M = 0.5). The increase in central transfers to ex-capitals is somewhat less robust – the confidence interval widens under mild trend violations. The rise in salary expenditures for ex-capital cities is very robust: even allowing for a worst-case violation (M = 1), the lower bound remains above zero, confirming a positive treatment effect on payroll spending. As expected from the graphs, there is no robust effect on investment spending – even the original unadjusted estimate was near zero, and under any allowance for pre-trend differences, the confidence interval comfortably spans zero.

Table 3: "Honest DiD": Impact of losing the capital status conditional on becoming a city-county

	Municipal Own Revenue		**Central Government Revenues**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−1054.60	−131.42	1286.67	2795.96
0	−1050.36	−135.66	1293.61	2789.02
0.5	−1336.22	−17.15	−19.61	3667.44
1	−1695.73	243.00	−1551.24	4994.76
2	−2494.23	986.72	−4662.38	8068.81
	Municipal Spending on Salaries		**Municipal Spending on Investment**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	886.98	1829.23	−817.47	850.35
0	891.31	1824.90	−809.80	842.68
0.5	694.02	1992.23	−1073.02	1297.65
1	421.63	2253.19	−1347.32	1772.39
2	−263.79	2924.65	−2133.92	2837.03

Notes: Ex-capital cities are treated, city-counties are controls. Honest DiD bounds reported for the sensitivity parameter M. "Original" denotes baseline specification. All values in PLN per capita.

Table 4 summarizes the average municipal finance impacts over 1999–2003 for both comparisons (ex-capitals (L2) vs. city-counties (L4), and ex-capitals (L2) vs. county seats (L5)). Several patterns stand out. First, central transfers emerge as the most responsive fiscal category, with large and highly significant increases for ex-capitals. In the TWFE specification, ex-capitals received about 390 PLN per capita more in annual transfers than city-counties, and the CS-DiD estimate is about 437 PLN – both significant. Compared to county seats, ex-capitals got an even larger boost (around +730 to +820 PLN per capita, depending on specification). This underscores a compensatory role of the central government: the reform redirected substantial funds to the cities that lost their regional status. Own-source revenues, by contrast, declined in ex-capitals relative to city-counties by roughly 100–135 PLN per capita (significant at the 5% level in the CS-DiD), which corresponds to about 6–8% of an average monthly wage in 1999. In the ex-capital vs. county seat comparison, own revenues for ex-capitals were actually higher (by 77 PLN in TWFE), but that difference disappears in CS-DiD estimate (approx. –16 PLN, and not significant) – indicating that the apparent revenue advantage of ex-capitals over small county seats was driven by their larger economic size rather than the treatment.

Table 4: Municipal finance outcomes (1999–2003)

	Ex-capitals (L2) vs. City-counties (L4)		Ex-capitals (L2) vs. County seats (L5)	
Outcome (PLN per capita)	TWFE	CS-DiD	TWFE	CS-DiD
Own revenues	-136***	-98.3**	77.2**	-15.9
	(48.0)	(46.6)	(58.2)	(23.6)
Central transfers	390***	437.1***	729***	820.5***
	(40.4)	(99.0)	(36.8)	(51.1)
Salaries	275***	270.7***	390***	418.8***
	(40.6)	(43.6)	(34.3)	(37.3)
Investment	-12.3	15.0	59.2***	80.0
	(54.8)	(118.9)	(17.7)	(55.1)
Observations	628	628	2,030	2,030

Notes: Estimates from Two-Way Fixed Effects (TWFE) and Callaway–Sant'Anna Difference-in-Differences (CS-DiD). Standard errors clustered at 50 km radius. *, **, *** denote $p < 0.1$, $p < 0.05$, $p < 0.01$.

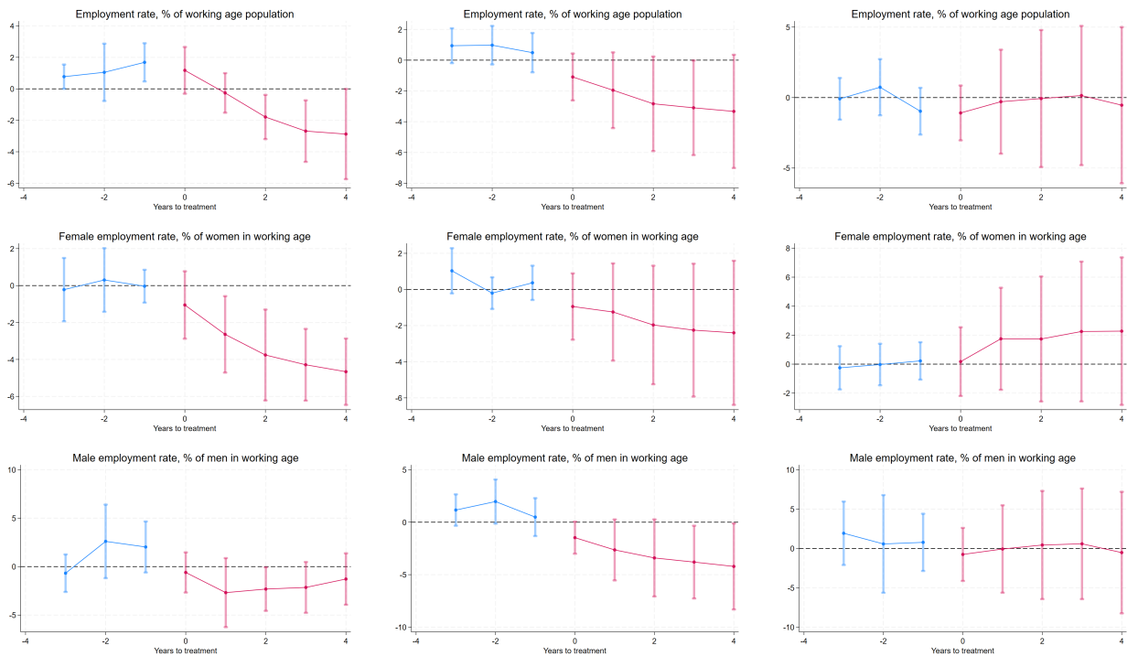
Turning to expenditures, salary spending in ex-capital cities increased markedly in the post-reform period. Table 4 shows that spending on salaries is approximately +270–275 PLN per capita higher in ex-capitals compared to city-counties, and roughly +400 PLN per capita higher relative to county seats, all of which are significant. These figures mirror what we saw in the event studies: former capitals used a considerable portion of their budgets to pay public employees. Meanwhile, investment spending remained relatively unchanged. The point estimates for investment are slightly positive in the ex-capital vs. county seat comparison (TWFE +59 PLN, significant at 1%, but the CS-DiD is a statistically insignificant +80 PLN), and effectively zero or negative for ex-capitals vs. city-counties. Overall, the fiscal evidence suggests that losing capital status left cities with a weaker autonomous revenue base, while the increased intergovernmental transfers were largely spent on recurrent costs.

Having examined the budgetary shock, I next investigate how the reform affected local labor markets.

5.2 Labor Market

Figure 6 presents the event-study estimates for municipal employment rates, disaggregated by gender, in the years surrounding the reform. These estimates use the Callaway and Sant'Anna 2021 approach with covariate adjustments chosen to balance pre-trends for each outcome (see Table 5 notes for details – for example, the female employment analysis controls for the number of firms in female-dominated industries). Panel 6a (ex-capitals vs. city-counties, L2 vs. L4) reveals a clear decline in employment in ex-capitals concentrated among women. Starting about two years after the reform, the female employment share in ex-capital cities drops by roughly 4 percentage points relative to the control group, and this gap persists thereafter. The effect on the male employment share in Panel 6a is negligible and not statistically significant. Panel 6b (ex-capitals vs. county seats), which reflects the combined effect of losing capital status and gaining city-county status, also shows a

negative post-reform impact on employment rates – though in this case the decline in female employment is smaller and not statistically significant, while the male employment rate in ex-capitals does dip significantly below that of county-seat cities in the third year after the reform. Finally, Panel 6c (city-counties vs. county seats, L4 vs L5) indicates that becoming a city-county by itself had little to no effect on local employment. The employment trends for city-counties versus county seats are nearly flat; if anything, city-county status may have a slight positive (but insignificant) impact on female employment. Taken together, the Figure 6 results suggest that the negative labor market outcomes were driven by the loss of regional capital functions. Women's employment was particularly sensitive to the reform, suggesting that many jobs eliminated in ex-capital cities were disproportionately held by women.



(a) Impact of losing capital status cond. on becoming a city-county

(b) Impact of losing capital status and becoming a city-county

(c) Impact of becoming a city-county

Figure 6: Results from event-study according to Callaway and Sant'Anna 2021. Each panel (a–c) shows four event-study plots: total, female, and male employment shares.

Table 5 quantifies these employment effects over the 1999–2003 period. Several insights emerge. First, examining ex-capitals (L2) versus city-counties (L4) (the left panel of Table 5), the overall employment rate in ex-capitals shows a larger decline once we account for heterogeneity. The TWFE estimate for the employment rate is a small and insignificant –0.26 percentage points, but the covariate-adjusted CS-DiD estimate is –1.29** (significant at 5%). In other words, after controlling for pre-reform differences (such as firms per capita), ex-capitals saw about a 1.3 percentage point lower employment rate on average than they would have if they had not lost capital status. The female employment rate exhibits a substantial drop in ex-capitals: –2.82*** under TWFE and –3.28*** under CS-DiD. The fact that the adjusted estimate is even more negative (and remains highly significant) confirms a strong and robust negative effect on women's employment in the treated cities. By contrast, the male employment rate shows an interesting reversal. The TWFE suggests a positive effect (+2.16**), implying that men's employment initially rose in ex-capitals relative to city-counties – likely a misleading result driven by other trends. Once we include additional controls that account for local industry shocks (specifically, controls for the decline of mining activity, which was concentrated in certain regions like Silesia, as well as the number of

mining firms and recent mine closures), the CS-DiD estimate for men becomes -1.81 and is not statistically significant. In short, any apparent gain in male employment was an artifact of unrelated local factors, and after adjustment, the data are consistent with a modest decline in men's employment as well (though smaller and noisier than the effect on women).

Table 5: Impact of losing capital status on employment outcomes (1999–2003)

	Ex-capitals (L2) vs. City-counties (L4)		Ex-capitals (L2) vs. County seats (L5)	
Outcome	TWFE	CS-DiD	TWFE	CS-DiD
Employment rate (%)	-0.26	-1.29**	-2.22***	-2.46*
	(0.77)	(0.60)	(0.54)	(1.33)
Female employment (%)	-2.82***	-3.28***	-1.78***	-1.76
	(0.72)	(0.97)	(0.54)	(1.50)
Male employment (%)	2.16**	-1.81	-2.67***	-3.10**
	(1.06)	(1.01)	(0.63)	(1.51)
Working-age pop. ('000s)	2.60**	1.77**	1.59***	2.35***
	(1.09)	(0.67)	(0.26)	(0.60)
Observations	628	628	2,030	2,030

Notes: Estimates from Two-Way Fixed Effects (TWFE) and Callaway–Sant'Anna Difference-in-Differences (CS-DiD). Standard errors in parentheses, clustered at 50 km. *, **, *** denote $p < 0.1$, $p < 0.05$, $p < 0.01$. CS-DiD covariates differ by outcome: (i) Employment rate and working-age population: firms per capita; (ii) Female employment: firms in female-intensive sectors (hospitality, health, education, services, admin); (iii) Male employment: firms per capita, mining firms, coal mine closures, Silesia region FE. For Ex-capitals vs. County seats comparisons, population is additionally included as a control.

Turning to the ex-capital (L2) vs. county seat (L5) comparison (right panel of Table 5), we see broadly similar outcomes. The employment rate in ex-capitals was about 2.2–2.5 percentage points lower than in county seats on average after the reform (–2.22*** in TWFE, –2.46* in CS-DiD). The female employment rate also fell (by around –1.8 p.p. in both specifications), although in the adjusted CS-DiD, it is not statistically significant at conventional levels ($p \approx 0.20$). The male employment rate in ex-capitals is estimated to have dropped by roughly 2.7–3.1 p.p. relative to county seats, a sizable effect that is significant in both specifications (–2.67*** TWFE; –3.10** CS-DiD).

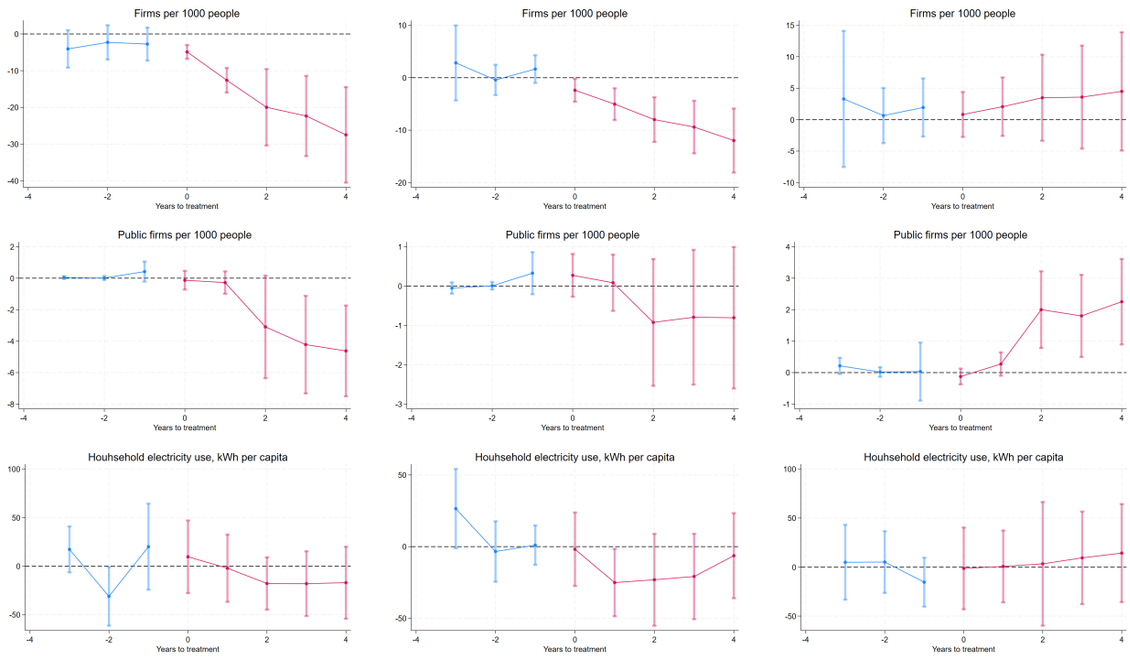
Overall, the labor market evidence suggests a substantial negative impact of the reform on employment, particularly affecting women. Across specifications, female employment in cities that lost capital status declined by on the order of 3 percentage points relative to the control cities. Given that an average ex-capital had about 32,800 women of working age, a 3% decline in the female employment rate implies approximately 1,000–1,250 fewer women employed in each treated city. Men's employment also tended to decline in ex-capitals (once we account for external shocks), but by roughly half as much as women's in percentage-point terms. These gender-differentiated effects are consistent with the nature of the shock: many of the jobs eliminated by the loss of administrative status were in public administration and related services, sectors where female employment was relatively high. The fact that male employment was somewhat more resilient – and that some men likely left to find jobs

elsewhere, as discussed below – meant that the burden of local job loss fell disproportionately on women.

5.3 Economic Activity

Beyond formal employment, we can ask whether the reform dampened overall economic dynamism in the affected cities. To this end, I examine the number of firms per capita (both total firms and the subset of publicly owned firms) as indicators of local business activity and public-sector presence, as well as household electricity use per capita as a proxy for consumer demand. Figure 7 displays the event-study results for these outcomes (following Callaway and Sant'Anna 2021). Panel 7a (ex-capitals vs. city-counties, L2 vs. L4) indicates that losing capital status led to a decline in the density of firms in the treated cities. After 1999, ex-capitals have fewer firms per 1,000 residents than the control cities, and this gap becomes significant a couple of years into the post period. The effect on public-sector firms in Panel 7a is delayed: there is little immediate change, but by the third year after the reform ex-capitals show a significant drop in the number of public firms per capita as well. These public firms include local government agencies and state-owned enterprises, so this suggests some of those either closed or relocated following the status loss (likely reflecting the shutdown of regional administrative units or associated institutions). Panel 7b (ex-capitals vs. county seats, L2 vs. L5) shows a similar overall pattern: ex-capitals experienced a relative decline in total firms per capita in the post-reform years. However, Panel 7c (city-counties vs. county seats, L4 vs. L5) helps to disentangle the effects – it shows that becoming a city-county, in the absence of prior capital status, did not reduce firm activity. In fact, city-counties appear to see an increase in public-sector firms over time (the middle line in Panel 7c trends upward from the second year onward), presumably because these cities gained new administrative offices and public institutions as part of their upgraded status. Meanwhile, the total number of firms in city-counties versus county seats (top line of Panel 7c) is relatively flat or even slightly positive. This comparison reinforces the interpretation that the negative outcomes in ex-capital cities were driven by the loss of capital functions, rather than by the general administrative changes that some cities underwent.

For household electricity consumption, the event studies show at most a modest effect. In Panel 7a (ex-capitals vs. city-counties, L2 vs L4), the electricity use in ex-capitals is consistently a bit lower post-1999, but the difference is not statistically significant at conventional levels. In Panel 7b (ex-capitals vs. county seats, L2 vs. L5), there is a significant drop in electricity usage in ex-capitals around the second year after the reform, suggesting a short-term contraction in local demand or living standards relative to the county-seat cities. However, that effect does not persist, and importantly, Panel 7c shows no effect of city-county status on electricity use. This implies that the dip in Panel 7b was likely due to the loss of capital status (perhaps a temporary shock to consumer confidence or spending in ex-capitals), rather than anything related to the administrative reorganization for other cities.



(a) Impact of losing capital status cond. on becoming a city-county

(b) Impact of losing capital status and becoming a city-county

(c) Impact of becoming a city-county

Figure 7: Event study results according to Callaway and Sant'Anna 2021. Each panel (a–c) shows event-study results for the total number of firms, firms in the public sector, and electricity usage.

These results are consistent with the theoretical framework: a reduction in administrative capacity tends to depress economic activity in both private and public sectors, with the private sector bearing the brunt of the decline. Unfortunately, I do not yet have direct data on the number of jobs in the private vs. public sectors of these cities, so we cannot pinpoint exactly how the employment losses were distributed. However, the firm counts suggest that the loss of capital status led to a broad-based downturn in local business formation and retention. Even though some public offices remained in ex-capitals (and ex-capitals themselves became city-counties), the overall number of public institutions in those cities still fell, indicating that many agencies likely closed or moved to the new regional capitals (L1, always-capitals). Meanwhile, city-counties that were not previously capitals (L4) actually gained public institutions, highlighting a reallocation of administrative functions toward those cities. In summary, the reform not only directly reduced public-sector activity in the ex-capital cities but also indirectly dampened private-sector dynamism, leaving the treated cities with fewer businesses.

Table 6 presents the difference-in-differences estimates for economic activity outcomes, confirming the patterns observed in the graphs. Focusing first on total firms per capita: in the ex-capital vs. city-county comparison, the TWFE estimate is small and statistically insignificant (+1.46 firms per 1,000 people), whereas the covariate-adjusted CS-DiD estimate is –17.46*. Thus, once we account for observable confounders (including population size and region fixed effects), we estimate that an ex-capital ended up with about 17 fewer firms per 1,000 residents, on average, than a comparable city-county that did not lose capital status. The difference between TWFE and CS-DiD here suggests that unadjusted methods understated the true impact – likely because certain ex-capitals had positive pre-trends in firm growth that made the TWFE estimate biased upward.

Table 6: Economic activity outomes (1999–2003)

	Ex-capitals (L2) vs. City-counties (L4)		Ex-capitals (L2) vs. County seats (L5)	
Outcome	TWFE	CS-DiD	TWFE	CS-DiD
Firms per 1000 people	1.46	-17.46***	-5.90***	-7.36***
	(3.58)	(3.75)	(1.81)	(1.95)
Public firms per 1000 people	-0.74*	-2.47**	-1.08***	-0.43
	(0.37)	(1.00)	(0.31)	(0.56)
Electricity use (kWh per capita)	-20.83*	-9.14	-23.32***	-15.44
	(11.20)	(14.73)	(6.42)	(12.51)
Observations	628	628	2,030	2,030

Notes: Estimates from Two-Way Fixed Effects (TWFE) and Callaway–Sant'Anna Difference-in-Differences (CS-DiD). Standard errors in parentheses, clustered at 50 km. *, **, *** denote $p < 0.1$, $p < 0.05$, $p < 0.01$. CS-DiD covariates differ by outcome: (i) Firms per 1000 people: working-age population; (ii) Public firms: municipal expenditure, population, Silesia region FE (where relevant); (iii) Electricity use: employment rate, population, Silesia region FE.

A similar story holds for public-sector firms per capita. Ex-capitals have about –0.74* fewer public institutions per 1,000 people under TWFE, but the CS-DiD finds –2.47** fewer, a more substantial decline. In the ex-capital vs. county-seat comparison (L2 vs. L5), we also see significant negative effects: roughly 6–7 fewer firms per 1,000 people in ex-capitals compared to county seats (both TWFE and CS-DiD are highly significant). This indicates that without the mitigating influence of city-county status, the treated cities would likely have seen even larger losses in business activity. In other words, the act of becoming a city-county did not impede firm growth and may have modestly fostered it (likely by attracting some public-sector activity and related services).

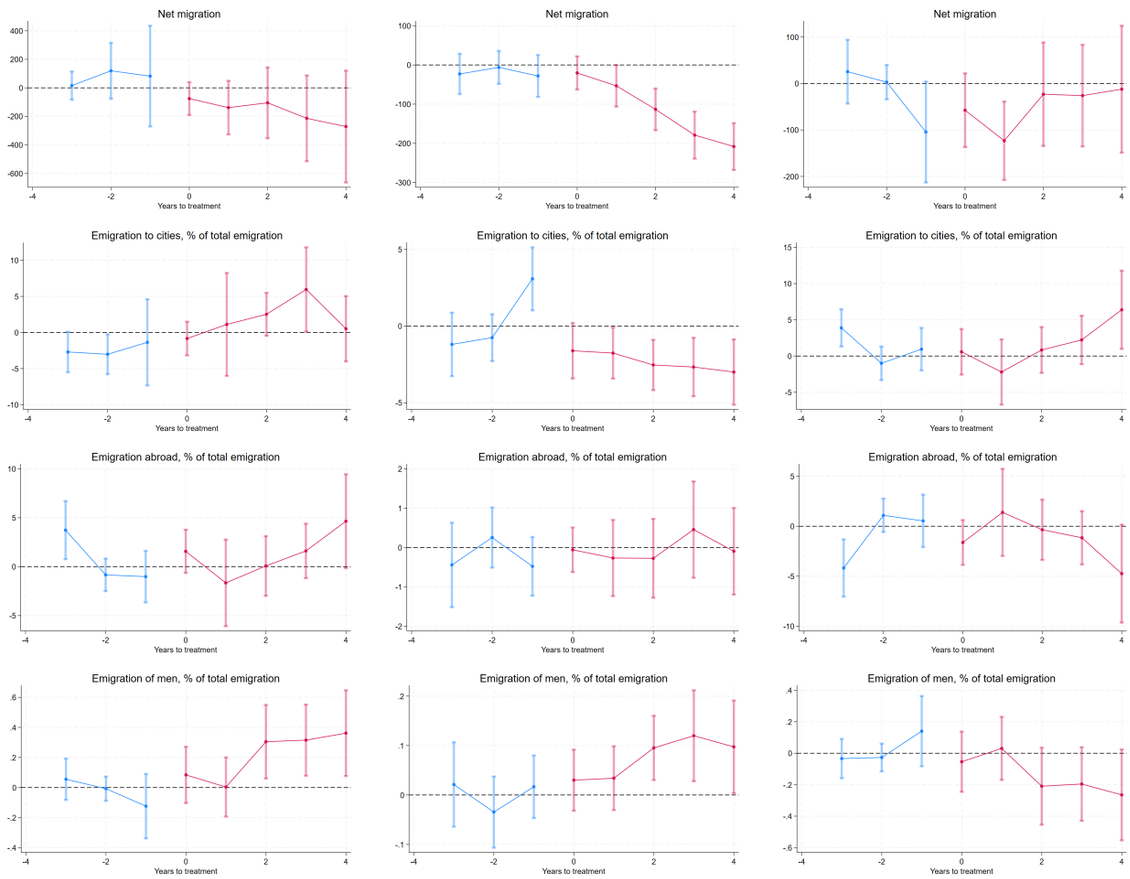
When we examine public firms in Table 6, we find that ex-capitals ended up with fewer public institutions than either of the control groups. However, city-counties gained public institutions relative to county seats. This highlights that administrative functions were being redistributed: the loss in ex-capital cities is partly offset by gains in other cities. For electricity usage, the table shows a negative coefficient for ex-capitals vs county seats (–23.3*** kWh per capita under TWFE), but the adjusted estimate is smaller and not significant (–15.4). Against city-counties, the electricity effect is negative but not significant in either specification. Thus, any impact of reform on household electricity consumption was at most temporary or marginal.

In summary, cities that lost capital status experienced a notable contraction in local economic activity, as evidenced by fewer firms and lower consumption, whereas cities that gained some administrative role did not see such declines. The results also suggest that the city-county status conferred some economic benefits (or avoided worse outcomes): ex-capitals, having become city-counties, still lost firms, but the loss would likely have been even steeper in the absence of that city-county status (since city-counties generally fared better than pure county seats). Conversely, city-counties benefited from the establishment of new public-sector entities, partially offsetting the centralization of administration in the new regional capitals.

Having established the economic fallout within ex-capitals, a natural question is whether residents responded by moving away from these cities. We turn next to the patterns of migration.

5.4 Migration

Figure 8 investigates whether the reform led to population outflows from the affected cities. Panel 8a (ex-capitals vs. city-counties, L2 vs. L4) shows no significant change in net migration for ex-capitals relative to the control cities. In other words, there is no evidence that losing capital status triggered an immediate mass exodus of residents when compared to similar cities. Panel 8b (ex-capitals vs. county seats, L2 vs. L5) does indicate a relative decline in net migration for ex-capitals – the line dips below zero – but the effect is fairly small and only mildly significant. This suggests that ex-capitals might have lost slightly more people (or attracted fewer new residents) than the smaller county-seat towns after 1999, although the difference is not dramatic. Panel 8c (city-counties vs. county seats, L4 vs. L5) is inconclusive due to non-parallel pre-trends: the migration rates for these two groups were on different trajectories even before 1999, so we cannot confidently interpret any post-1999 divergence. Overall, the event-study evidence suggests that the reform did not induce large-scale out-migration from the former capital cities, at least in the short to medium term.



(a) Impact of losing capital status cond. on becoming a city-county

(b) Impact of losing capital status and becoming a city-county

(c) Impact of becoming a city-county

Figure 8: Event-study results according to Callaway and Sant'Anna 2021. Each panel (a–c) shows event-study estimates for total migration, emigration share, emigration abroad, and male emigration.

One potential form of migration could have been targeted moves by displaced public servants – for instance, if many regional government employees from the former capitals were reassigned to public jobs in the cities that became the new regional capitals. If that had happened, we would expect to see a noticeable increase in out-migration from former capitals to other urban areas. However, we do not observe such a pattern. Panel 8a shows no spike in emigration to other cities (the data include migration destinations, and there is no significant outflow from ex-capitals to the remaining capitals or other cities). Similarly, international migration did not increase; there is no significant change in emigration abroad from former capitals relative to controls in any comparison. The only notable demographic movement visible in Figure 8 is a slight gender imbalance: in Panels 8a and 8b, the male migration rate appears a bit more negative than the female migration rate (i.e., men were somewhat more likely to leave ex-capitals than women were). This aligns with the idea that men might have found it easier to relocate for jobs while women, who probably had skills tied to public administration, might have been less mobile. However, crucially, even the male out-migration is not a significant effect in absolute terms.

Table 7 confirms that losing capital status had essentially no significant impact on net migration when aggregated over the 1999–2003 period. The net migration balance (in absolute numbers of people per year) for ex-capitals is slightly negative relative to controls in some specifications (e.g., –161 in the CS-DiD for ex-capitals vs city-counties, and –115** for ex-capitals vs county seats). We also see no increase in migration to other cities: the share of the population out-migrating to urban areas is essentially unchanged in ex-capitals (the CS-DiD coefficients are small and insignificant for ex-capitals vs city-counties, and even negative –2.31*** for ex-capitals vs county seats, suggesting ex-capitals, if anything, had lower emigration to other cities than the small county seats). Emigration abroad remains very low (on the order of 0.5–1% of the population annually) and shows no significant differences between ex-capitals and either of the control groups. The only consistent difference in migration is in the gender breakdown: male out-migration from former capitals was slightly higher than female out-migration. For example, in ex-capitals versus city-counties, the male emigration share is about 0.21 percentage points higher for ex-capitals (CS-DiD), whereas the female emigration share shows essentially no difference. In ex-capitals vs county seats, both men and women from ex-capitals were somewhat more likely to leave than those from county-seat towns, but the male gap (+0.08**) is significant. These results imply that men were more geographically mobile in response to the reform shock, albeit the differences are quite small in percentage terms.

Why do we not see a larger migratory response? The theoretical framework suggests that if migration is costly or if outside opportunities are limited, people will tend to stay even when local conditions worsen – leading instead to higher local unemployment (which is exactly what we found). The period I study (late 1990s to early 2000s) was a time of relatively high nationwide unemployment, so leaving an ex-capital for another city might not have substantially improved one's job prospects. Moreover, many displaced public-sector workers (especially women) may have had skills and family ties that made moving difficult. It appears that, rather than moving away, most people remained in the ex-capital cities despite the economic slowdown.

Table 7: Migration outcomes (1999–2003)

	Ex-capitals (L2) vs. City-counties (L4)		Ex-capitals (L2) vs. County seats (L5)	
Outcome	TWFE	CS-DiD	TWFE	CS-DiD
Net migration (absolute)	-46.64	-161.0	-151.1***	-114.9***
	(41.21)	(119.6)	(25.43)	(23.01)
Emigration to cities (%)	-1.04	1.86	-0.65	-2.31***
	(0.94)	(1.76)	(0.64)	(0.72)
Emigration abroad (%)	1.00	1.25	-0.39	-0.05
	(0.73)	(1.52)	(0.34)	(0.37)
Male emigration (%)	0.13***	0.21*	0.08**	0.08**
	(0.05)	(0.11)	(0.03)	(0.03)
Observations	628	628	2,030	2,030

Notes: Estimates from Two-Way Fixed Effects (TWFE) and Callaway–Sant'Anna Difference-in-Differences (CS-DiD). Standard errors in parentheses, clustered at 50 km. *, **, *** denote $p < 0.1$, $p < 0.05$, $p < 0.01$. CS-DiD covariates differ by outcome: (i) Net migration: firms per capita, distance to new capital; (ii) Emigration to cities: distance to new capital; (iii) Emigration abroad: no covariates; (iv) Male emigration: no covariates.

The fact that some men did leave at the margin could also help explain the earlier finding that women's employment took a bigger hit: if a portion of the male workforce out-migrated in search of work, the remaining pool of job-seekers in ex-capitals became disproportionately female. In any case, the bottom line is that the loss of capital status did not trigger a population collapse or major exodus from the affected cities during the period we observe.

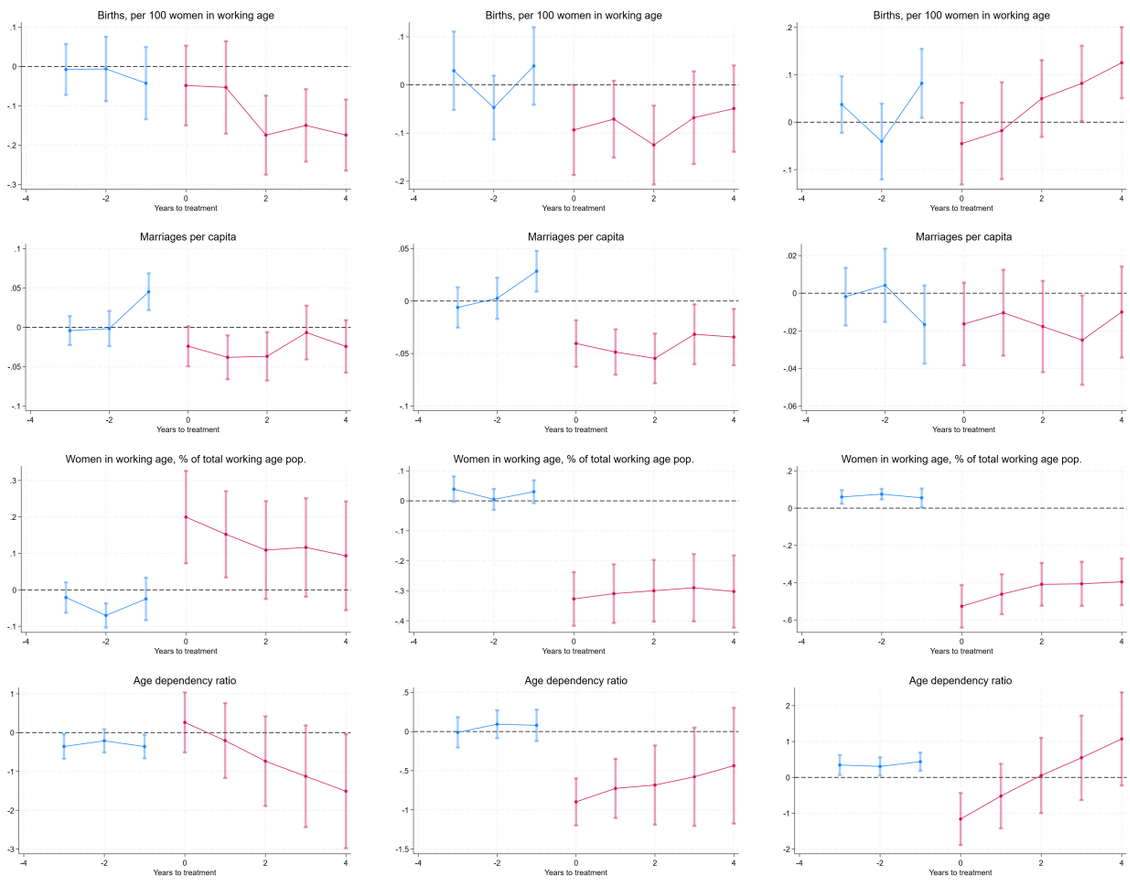
5.5 Demographics

The adverse labor market impacts in ex-capitals were accompanied by changes in demographic trends, particularly those related to family formation. If people lost jobs or faced worse economic prospects, they might postpone having children or getting married, and over a longer horizon, this could alter the age structure of the population. We explore these outcomes by looking at birth rates, marriage rates, the gender composition of the working-age population, and the age dependency ratio (youth and elderly population relative to the working-age population).

Figure 9a (ex-capitals vs. city-counties, L2 vs. L4) shows a clear decline in fertility in the ex-capital cities following the reform. Roughly three years after 1999, the number of births per 100 women of childbearing age in ex-capitals falls significantly below that of the control cities. This timing coincides with the cumulative impact of the economic downturn on household decisions, suggesting that many families in ex-capital cities chose to have fewer children (or delay childbirth) in the face of reduced employment and income prospects. We also observe a decline in marriages in ex-capitals relative to city-counties after the reform. The marriage rate (per 1,000 population) drops in the treated cities, which could reflect a combination of economic insecurity and changes in the local marriage market. As noted

earlier, ex-capitals saw a slight excess of women remaining (since men were more likely to leave or find jobs elsewhere), which may have disrupted traditional family formation patterns. In line with that, Panel 9a shows that the share of women in the working-age population of ex-capitals actually increased relative to city-counties. This implies that, proportionally, there were fewer men of working age in ex-capital cities post-reform (consistent with male out-migration and possibly higher male job-loss rates). Such a demographic imbalance can itself contribute to lower marriage rates and fertility, creating a feedback loop.

Figure 9b (ex-capitals vs. county seats, L2 vs. L5) reveals a similar drop in births in ex-capitals compared to the smaller towns. The fertility rate in ex-capital cities declines significantly in the post-reform years, mirroring the result from Panel 9a. An interesting difference, however, is seen in the gender composition: ex-capitals ended up with a lower share of working-age women relative to county seats (the coefficients in Panel 9b for the female share are negative). At first glance, this seems contradictory to Panel 9a, but the discrepancy is explained by the effect of city-county status. Panel 9c (city-counties vs. county seats) shows that gaining city-county status is associated with a decline in the female percentage of the working-age population. In other words, city-counties tended to retain or attract relatively more men (perhaps due to generally better employment prospects that kept men from leaving).



(a) Impact of losing capital status cond. on becoming a city-county

(b) Impact of losing capital status and becoming a city-county

(c) Impact of becoming a city-county

Figure 9: Event-study according to Callaway and Sant'Anna 2021. Each panel (a–c) shows event-study estimates for births, marriages, women's productivity, and the age-dependency ratio.

Turning to the aggregate demographic impacts, Table 8 reports difference-in-differences estimates for births, marriages, the female working-age share, and the age dependency ratio. Consistent with the graphs, fertility declined significantly in ex-capitals. In the ex-capitals

vs. city-county comparison, ex-capitals saw about 0.12 fewer births per 100 women (CS-DiD, significant at 1%). Against county seats, the decline was on the order of 0.07–0.08 fewer births per 100 women (significant at the 5% level in CS-DiD).

Table 8: Demographic outcomes (1999–2003)

	Ex-capitals (L2) vs. City-counties (L4)		Ex-capitals (L2) vs. County seats (L5)	
Outcome	TWFE	CS-DiD	TWFE	CS-DiD
Births per 100 women	-0.16***	-0.12***	-0.07*	-0.08**
	(0.04)	(0.04)	(0.03)	(0.04)
Marriages per capita	0.01	-0.03*	-0.02**	-0.04***
	(0.01)	(0.01)	(0.01)	(0.01)
Women of working age (%)	0.08	0.13**	-0.27***	-0.31***
	(0.06)	(0.06)	(0.05)	(0.05)
Age dependency ratio (%)	-1.13	-0.66	-0.56	-0.66**
	(0.77)	(0.57)	(0.36)	(0.25)
Observations	628	628	2,030	2,030

Notes: Estimates from Two-Way Fixed Effects (TWFE) and Callaway–Sant'Anna Difference-in-Differences (CS-DiD). Standard errors in parentheses, clustered at 50 km. *, **, *** denote $p < 0.1$, $p < 0.05$, $p < 0.01$. CS-DiD covariates: (i) Births, marriages: population; (ii) Women in working age: population; (iii) Age dependency ratio: population. The age dependency ratio is the share of the population below the age of 18 and above 64 to the rest of the population.

The marriage rate also declines in ex-capital cities, though this effect is more pronounced after adjusting for covariates. In the raw TWFE, ex-capitals vs city-counties show basically no change (+0.01, n.s.), but the CS-DiD indicates a slight decrease in marriages (–0.03* per capita). For ex-capitals vs county seats, both TWFE and CS-DiD confirm a drop in marriages (around –0.02** to –0.04*** per capita). Thus, it appears that the reform not only reduced fertility but also discouraged marriages, likely due to economic strain and possibly a mismatch between male and female populations.

Looking at the gender composition, Table 8 shows that ex-capitals had a higher fraction of women in their working-age population than city-counties did (+0.13** percentage points in CS-DiD), reflecting the relative loss of men. Conversely, ex-capitals ended up with a lower female share than county seats (–0.31*** in CS-DiD), consistent with the idea that their city-county status helped them retain men relative to those smaller towns. Finally, the age dependency ratio (the share of children and seniors in the working-age population) tended to decline in ex-capital cities, meaning the population became more weighted toward working-age adults. The coefficients are negative across the board in Table 8 (for example, –0.66 in the ex-capitals vs city-county CS-DiD, significant at 5%). Despite fewer births, the former capital cities did not experience a disproportionate aging of their populations in the immediate aftermath of the reform.

In summary, the reform's demographic impact on ex-capital cities was to lower fertility and marriage rates, and to slightly alter the population structure. These cities witnessed fewer new births and marriages during the post-reform years, likely a consequence of economic

uncertainty. They also observed a mild reshuffling of their population: a proportionally higher number of working-age adults (especially women) remained, while there were somewhat fewer children being born and possibly fewer men in the prime working ages (due to migration). These demographic shifts underscore that the consequences of losing capital status extend beyond economics into the social fabric of the city.

5.6 Public Goods

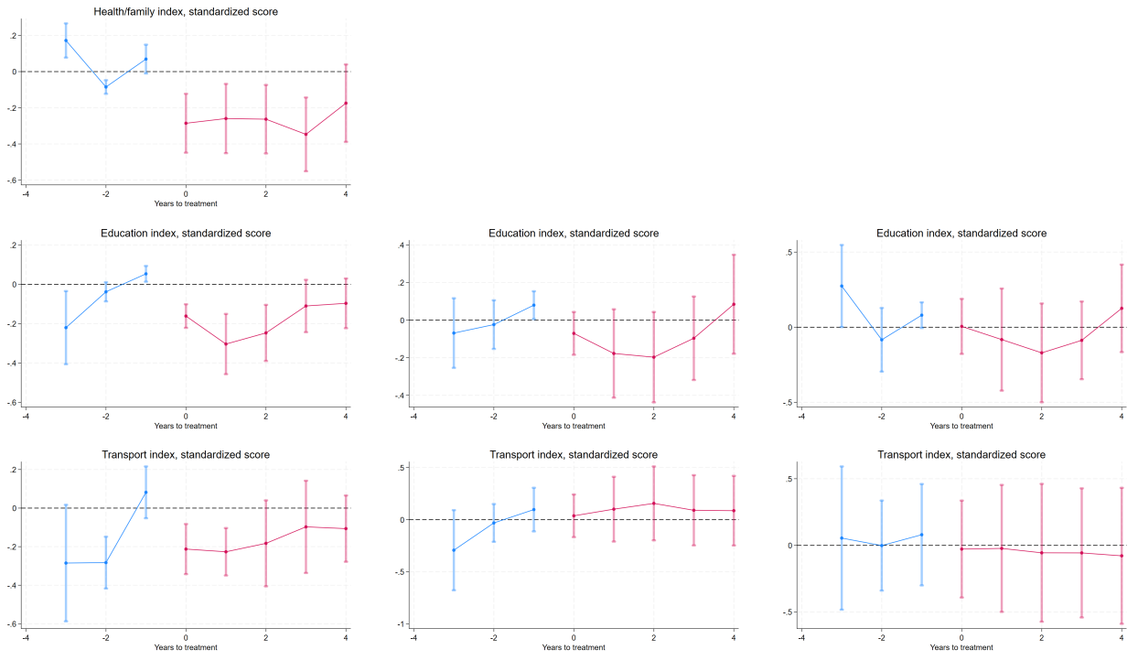
Finally, I examine the impact of the reform on the provision of local public goods and services. I construct three simple indices to capture different dimensions of public goods: an education index, a health and family services index, and a public transport index. Each index is the standardized (z-score) average of a set of per-capita variables in that category. For example, the education index combines metrics such as the number of kindergarten places, library books, and public educational institutions per capita. The health/family index includes the availability of childcare facilities (crèches), the number of doctors and dentists, and the number of public healthcare institutions per capita. The transport index consists of the number of bus lines and public transit enterprises per capita. These indices provide a summary measure of the quantity of public services or infrastructure accessible to residents.[11]

Figure 10a (ex-capitals vs. city-counties, L2 vs L4) suggests that losing capital status had a broadly negative effect on local public goods provision. Ex-capital cities saw declines in all three indices relative to the control cities. The education index in ex-capitals trended downward post-1999, meaning fewer educational resources per capita (e.g., reductions in kindergarten slots or schools) compared to city-counties. The health and family services index also declined in ex-capital cities relative to controls. The public transport index shows a sharp drop in ex-capitals after the reform – indicating a deterioration in transit services (such as cuts to bus lines or public transport companies). However, it is worth noting that the transport index in particular exhibits some divergence in pre-trends (ex-capitals were on an upward trajectory relative to city-counties before 1999, which then reversed), so we should be cautious in attributing the entire post-1999 gap to the reform without reservation. Panels 10b and 10c confirm that gaining city-county status alone did not significantly improve or worsen public goods outcomes. In Panel 10b (ex-capitals vs. county seats, L2 vs. L5), the education index in ex-capitals is a bit lower after the reform (consistent with Panel 10a), and the transport index shows no clear difference (the CS-DiD stays around zero). Panel 10c (city-counties vs. county seats, L2 vs. L5) shows very little change in any index – the lines hover around zero, indicating no systematic effect of just becoming a city-county on these public goods indicators. These comparisons reinforce that the notable declines in public goods were specific to the cities that lost their regional capital functions.

The interpretation is that, despite receiving higher transfers from the central government, ex-capitals did not channel those funds into maintaining or expanding local public services. This aligns with our earlier finding that ex-capitals did not boost investment spending: essentially, the extra money was used to cover administrative operating costs (salaries), and perhaps to replace some lost revenue, rather than to invest in schools, healthcare, or transit. As a result, the data indicate a relative stagnation or decline in public amenities in former

[11] Data for some indicators were not available for all city types – in particular, the health/family index could not be computed for county-seat towns due to missing data on certain services in those smaller municipalities.

capitals after 1999. By contrast, the control cities – whether city-counties or county seats – did not experience such declines, and in some cases even modestly improved their public goods (for instance, city-counties managed to roughly keep pace with or exceed county seats in these indices).



(a) Ex-capitals vs. city-counties **(b)** Ex-capitals vs. county seats **(c)** City-counties vs. county seats

Figure 10: CS-DiD results for public goods indices under different comparison groups. Each panel (a–c) shows event-study estimates for health/family, education, and transport indices.

Table 9: Public goods outcomes (1999–2003)

	Ex-capitals (L2) vs. City-counties (L4)		Ex-capitals (L2) vs. County seats (L5)	
Outcome (std. score)	TWFE	CS-DiD	TWFE	CS-DiD
Education index	-0.09	-0.18***	-0.11***	-0.09
	(0.06)	(0.06)	(0.03)	(0.09)
Healthcare / family index	-0.02	-0.27***	–	–
	(0.08)	(0.09)		
Public transport index	-0.27***	-0.17**	-0.24***	0.09
	(0.08)	(0.08)	(0.08)	(0.14)
Observations	628	628	2,030	2,030

Notes: Estimates from Two-Way Fixed Effects (TWFE) and Callaway–Sant'Anna Difference-in-Differences (CS-DiD). Standard errors in parentheses, clustered at 50 km. *, **, *** denote $p < 0.1$, $p < 0.05$, $p < 0.01$. CS-DiD covariates: (i) Education and healthcare indices: municipal revenues per capita, population; (ii) Transport index: municipal investment per capita, population, Silesia region FE. Data for the healthcare/family index is unavailable for county seats.

Table 9 presents the aggregate impacts on the public goods indices for 1999–2003. Consistent with the graphical evidence, the ex-capital vs. city-county comparison (L2 vs. L4) yields negative treatment effects across all indices. For education, ex-capitals are about –0.18*** standard deviations below city-counties (CS-DiD estimate) on average after the reform. For

health/family services, the effect is –0.27*** (a sizable decline, though, as noted, we lack a county-seat benchmark for this index). Public transport shows a TWFE impact of –0.27*** and a CS-DiD of –0.17**, indicating a significant drop in transport provision in ex-capitals relative to city-counties. In the ex-capital vs. county-seat comparison (L2 vs. L5), the point estimates are also mostly negative, though the statistical significance is weaker once covariates are included. For example, the education index shows –0.11*** under TWFE but only –0.09 (not significant) under CS-DiD; the transport index is –0.24*** in TWFE but a small +0.09 (not significant) in CS-DiD. These discrepancies suggest that some of the measured declines in ex-capitals, when compared to the smaller towns, may be driven by underlying differences or non-parallel trends rather than the treatment itself. Indeed, as mentioned, the transport index suffered from pre-trend violations, which likely explain why the adjusted estimate is null, even though the raw difference was large – the ex-capitals were on a different trajectory prior to 1999.

In broad terms, all signs point to a reduction in local public goods provision in the cities that lost capital status, consistent with their fiscal challenges and lack of new investment. Residents of former capitals experienced relative declines in educational, healthcare, and transportation services in the early 2000s compared to residents of the control cities. However, these public goods results should be interpreted with caution. The presence of non-parallel trends (especially for the transport index) means we cannot be entirely sure these declines were caused by the reform rather than other contemporaneous factors. While the qualitative pattern – ex-capital cities lagging behind in service provision – fits the overall narrative of the reform's impact, the causal evidence for public goods is less definitive than for outcomes such as employment or fiscal revenues.

6 Conclusion

The results provide robust evidence that losing capital status induced a negative shift in socioeconomic outcomes. First, consistent with the theoretical model's predictions, the loss of administrative capacity led to a decline in employment and activity in the public and private sectors, with particularly strong effects on women's labor force participation. This likely reflects the concentration of women in public administration jobs and services supporting the public sector. These results suggest that institutional downgrades trigger a gendered reallocation in local labor markets.

Second, I find that while central government transfers to ex-capitals relatively increased after the reform, they only slightly offset the loss of own-source revenue, and they did not translate into higher investment spending. This suggests that fiscal compensation either was insufficient to maintain prior levels of administrative activity or was mostly spent on salaries. The negative impact of losing capital status on public goods supports the finding of no significant changes in investment.

Third, there is evidence of demographic decline in the affected cities: the birth rate, as a ratio of women of working age, has fallen significantly, along with the marriage rate. Migration effects, by contrast, were limited and relevant for men, consistent with the model's assumption of costly and selective mobility.

Overall, these findings confirm that administrative status operates as a productive asset with

real economic and demographic consequences. Cities that lost status experienced sustained declines in fiscal capacity, employment, and demographic vitality, despite receiving partial fiscal compensation and undergoing formal city-county upgrades. These results have broader implications for how institutional status shapes spatial inequality. Reforms that downgrade administrative functions in secondary cities should consider not only direct fiscal transfers but also restructuring local economies to enhance their competitiveness. In the theoretical framework, the loss of administrative capacity was associated with welfare declines in both public and private sectors, which is consistent with the empirical results. To restore efficiency, local authorities could have invested in, e.g., increasing the productivity of the private sector.

Future research could investigate whether administrative downgrades lead to political disaffection or shifts in voting behavior, or whether different forms of compensation—such as relocating public agencies—can mitigate the effects of status loss.

7 References

Auricchio, Marta, Emanuele Ciani, Alberto Dalmazzo, and Guido de Blasio. 2020. "Life after public employment retrenchment: evidence from Italian municipalities
[Redistributive public employment]." *Journal of Economic Geography* 20 (3): 733–782. 10.1093/JEG/LBZ013.

Becker, Sascha O., Stephan Heblich, and Daniel M. Sturm. 2021. "The impact of public employment: Evidence from Bonn." *Journal of Urban Economics* 122 103291. 10.1016/J.JUE.2020.103291.

Breuillé, Marie Laure, Pascale Duran-Vigneron, and Anne Laure Samson. 2018. "Inter-municipal cooperation and local taxation." *Journal of Urban Economics* 107 47–64. 10.1016/J.JUE.2018.08.001.

Callaway, Brantly, and Pedro H.C. Sant'Anna. 2021. "Difference-in-Differences with multiple time periods." *Journal of Econometrics* 225 (2): 200–230. 10.1016/J.JECONOM.2020.12.001.

Chambru, Cédric, Emeric Henry, and Benjamin Marx. 2022. "The Dynamic Consequences of State-Building: Evidence from the French Revolution." *SSRN Electronic Journal*. 10.2139/SSRN.4046820.

Dascher, Kristof. 2000. "Are Politics and Geography Related?: Evidence from a Cross-Section of Capital Cities." *Public Choice 2000 105:3* 105 (3): 373–392. 10.1023/A:1005151611464.

Enikolopov, Ruben, and Ekaterina Zhuravskaya. 2007. "Decentralization and political institutions." *Journal of Public Economics* 91 (11-12): 2261–2290. 10.1016/J.JPUBECO.2007.02.006.

Faggio, Giulia. 2019. "Relocation of public sector workers: Evaluating a place-based policy." *Journal of Urban Economics* 111 53–75. 10.1016/J.JUE.2019.03.001.

Faggio, Giulia, Teresa Schlüter, and Philipp vom Berge. 2018. "Interaction of Public and Private Employment: Evidence from a German Government Move." *SERC Discussion Papers*, https://ideas.repec.org/p/cep/sercdp/0229.htmlhttps://ideas.repec.org//p/cep/sercdp/0229.html.

Foa, Roberto Stefan. 2022. "Decentralization, historical state capacity and public goods provision in Post-Soviet Russia." *World Development* 152 105807. 10.1016/J.WORLDDEV.2021.105807.

Goodman-Bacon, Andrew. 2021. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics* 225 (2): 254–277. 10.1016/J.JECONOM.2021.03.014.

Jin, Haizhen. 2023. "Effects of decentralization on firm performance: Evidence from Chinese county-level quasi-experiments." *Economic Modelling* 119 106116. 10.1016/J.ECONMOD.2022.106116.

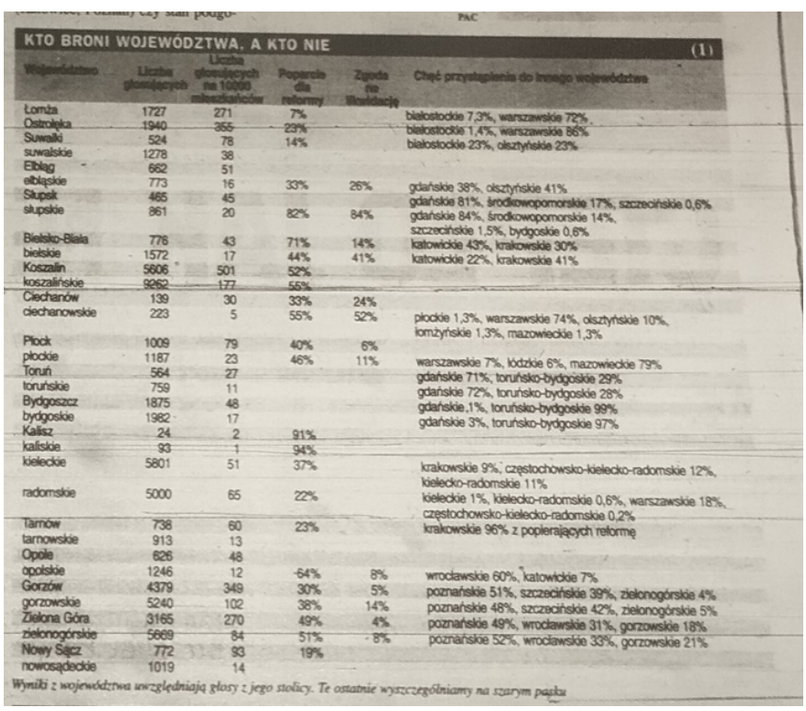
Martinez-Vazquez, Jorge, Santiago Lago-Peñas, and Agnese Sacchi. 2017. "THE IMPACT OF FISCAL DECENTRALIZATION: A SURVEY." *Journal of Economic Surveys* 31 (4): 1095–1129. 10.1111/JOES.12182.

Rambachan, Ashesh, and Jonathan Roth. 2023. "A More Credible Approach to Parallel Trends." *Review of Economic Studies* 90 2555–2591. 10.1093/restud/rdad018.

Roller, Marcus, and Daniel Steinberg. 2023. "Differences-in-Differences with multiple Treatments under Control."

Tricaud, Clemence. 2019. "Better Alone? Evidence on the Costs of Intermunicipal Cooperation."

A Context



KTO BRONI WOJEWÓDZTWA, A KTO NIE (1)

Województwo	Liczba głosujących	Liczba głosujących na 10000 mieszkańców	Poparcie dla reformy	Zgoda na likwidację	Chęć przystąpienia do innego województwa
Łomża	1727	271	7%		białostockie 7,3%, warszawskie 72%
Ostrołęka	1940	355	23%		białostockie 1,4%, warszawskie 86%
Suwałki	524	78	14%		białostockie 23%, olsztyńskie 23%
suwalskie	1278	38			
Elbląg	662	51			
elbląskie	773	16	33%	26%	gdańskie 38%, olsztyńskie 41%
Słupsk	465	45			gdańskie 81%, środkowopomorskie 17%, szczecińskie 0,6%
słupskie	861	20	82%	84%	gdańskie 84%, środkowopomorskie 14%, szczecińskie 1,5%, bydgoskie 0,6%
Bielsko-Biała	776	43	71%	14%	katowickie 43%, krakowskie 30%
bielskie	1572	17	44%	41%	katowickie 22%, krakowskie 41%
Koszalin	5606	501	52%		
koszalińskie	9262	177	55%		
Ciechanów	139	30	33%	24%	
ciechanowskie	223	5	55%	52%	płockie 1,3%, warszawskie 74%, olsztyńskie 10%, łomżyńskie 1,3%, mazowieckie 1,3%
Płock	1009	79	40%	6%	
płockie	1187	23	46%	11%	warszawskie 7%, łódzkie 6%, mazowieckie 79%
Toruń	564	27			gdańskie 71%, toruńsko-bydgoskie 29%
toruńskie	759	11			gdańskie 72%, toruńsko-bydgoskie 28%
Bydgoszcz	1875	48			gdańskie,1%, toruńsko-bydgoskie 99%
bydgoskie	1982	17			gdańskie 3%, toruńsko-bydgoskie 97%
Kalisz	24	2	91%		
kaliskie	93	1	94%		
kieleckie	5801	51	37%		krakowskie 9%, częstochowsko-kielecko-radomskie 12%, kielecko-radomskie 11%
radomskie	5000	65	22%		kieleckie 1%, kielecko-radomskie 0,6%, warszawskie 18%, częstochowsko-kielecko-radomskie 0,2%
Tarnów	738	60	23%		krakowskie 96% z popierających reformę
tarnowskie	913	13			
Opole	626	48			
opolskie	1246	12	64%	8%	wrocławskie 60%, katowickie 7%
Gorzów	4379	349	30%	5%	poznańskie 51%, szczecińskie 39%, zielonogórskie 4%
gorzowskie	5240	102	38%	14%	poznańskie 48%, szczecińskie 42%, zielonogórskie 5%
Zielona Góra	3165	270	49%	4%	poznańskie 49%, wrocławskie 31%, gorzowskie 18%
zielonogórskie	5669	84	51%	8%	poznańskie 52%, wrocławskie 33%, gorzowskie 21%
Nowy Sącz	772	93	19%		
nowosądeckie	1019	14			

Wyniki z województwa uwzględniają głosy z jego stolicy. Te ostatnie wyszczególniamy na szarym pasku

Figure 11: Results of the survey on preferences for remaining in a regional capital, Rzeczpospolita newspaper, March 1998

B Theoretical framework

Equilibrium Definition

An **equilibrium** in this economy is a sequence of allocations $\{\theta_{it}, N_{g,it}, N_{p,it}, N_{u,it}, M_{i3}\}$ for each city $i \in \{1, 2\}$ and period $t = 1, 2, 3$ such that:

1. Agents choose their sector according to preferences and expected utility:
$$\theta_{it} = \mathbb{P}\left[\phi \geq \phi^*_{it}\right], \quad \text{where } \phi^*_{it} = \frac{1}{\frac{u(c^g_{it})}{u(c^p_{it})} + 1}$$
2. Employment in period 1 is frictionless:
$$N_{g,i1} = \theta_{i1}, \quad N_{p,i1} = 1 - \theta_{i1}, \quad N_{u,i1} = 0$$
3. In period 2, employment is capped by available jobs, derived from administrative capacity:
$$\bar{N}_{g,i2} = N_{g,i1} \cdot \frac{\text{adm}_{i2}}{\text{adm}_{i1}}, \quad \bar{N}_{p,i2} = N_{p,i1} \cdot \frac{\text{adm}_{i2}}{\text{adm}_{i1}}$$
$$N_{g,i2} = \min\{\bar{N}_{g,i2}, \theta_{i2}\}, \quad N_{p,i2} = \min\{\bar{N}_{p,i2}, 1 - \theta_{i2}\}$$
$$N_{u,i2} = 1 - N_{g,i2} - N_{p,i2}$$
4. In period 3, migration occurs if the utility gain from moving exceeds the migration cost c, and there are open positions in the target city:
$$M_{i3} = \int_{\phi \in \mathcal{M}_{i3}} d\phi, \quad \text{where } \mathcal{M}_{i3} = \{\phi \in \mathcal{U}_{i2} : \Delta U(\phi) > c\}$$
5. All markets clear and agents are matched to jobs based on availability and preferences.

Existence of Equilibrium

An equilibrium exists under the assumptions that:

1. The utility function $u(c)$ is continuous, strictly increasing, and concave (CRRA).
2. The public-good weight ϕ is drawn from a continuous distribution $U[0, 1]$.
3. Administrative capacity adm_{it} is finite and strictly positive for all i, t.

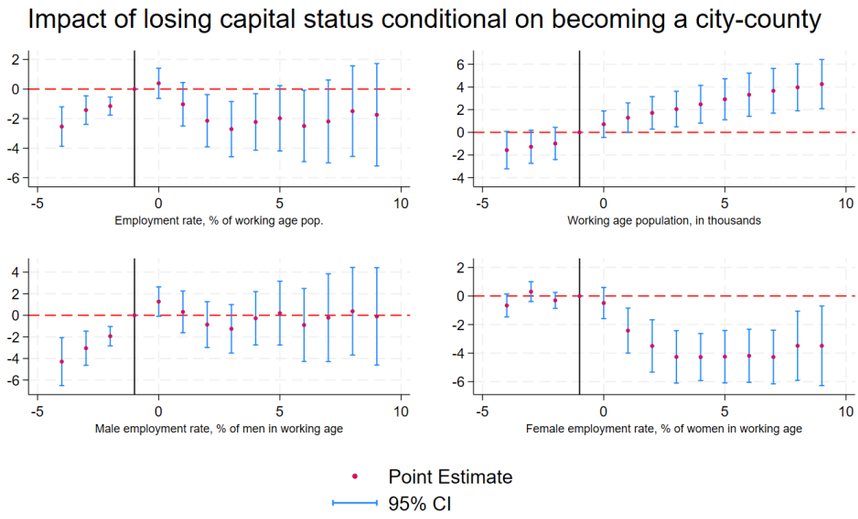
Sketch of Proof. Given the continuous and strictly increasing nature of $u(c)$ and the CRRA functional form, the indifference cutoff ϕ^*_{it} is a continuous function of sectoral consumption. Since consumption depends on the number of workers in each sector, and that in turn depends on θ_{it}, we are solving a fixed point problem:
$$\theta_{it} = 1 - \frac{1}{\frac{u(c^g_{it}(\theta_{it}))}{u(c^p_{it}(\theta_{it}))} + 1}$$
This function maps $[0, 1] \to [0, 1]$ and is continuous. By Brouwer's fixed point theorem, a solution exists for each city-period pair. □

Uniqueness of Equilibrium

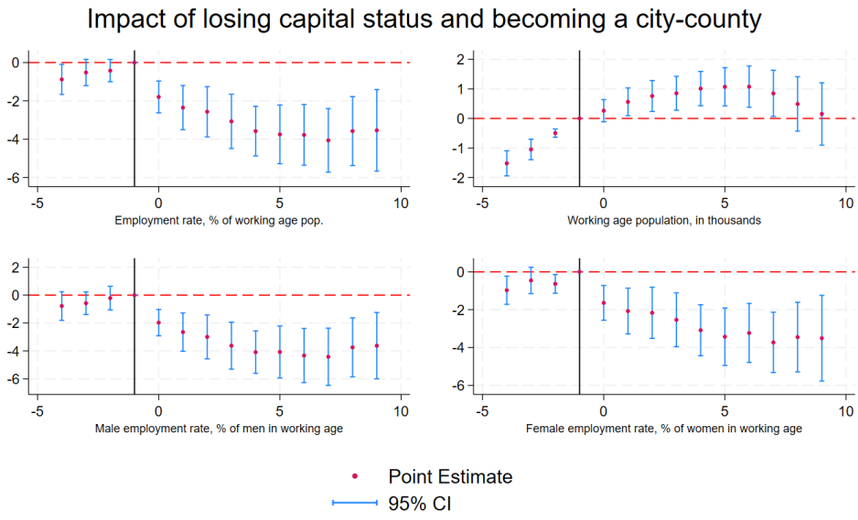
If the utility functions are strictly concave and the difference in sectoral productivity is sufficiently large, the equilibrium share θ_{it} is unique in each city and period.

Sketch of Proof. Given strict concavity of the utility functions, the indifference cutoff ϕ^*_{it} is strictly monotonic in θ_{it}. The right-hand side of the fixed point equation defines a strictly monotonic function in θ_{it}. A strictly monotonic continuous function can have at most one fixed point. Thus, the solution θ_{it} is unique. The uniqueness of migration and unemployment in period 3 follows from the monotonicity of utility gain in ϕ and capacity constraints. □

C Event-studies

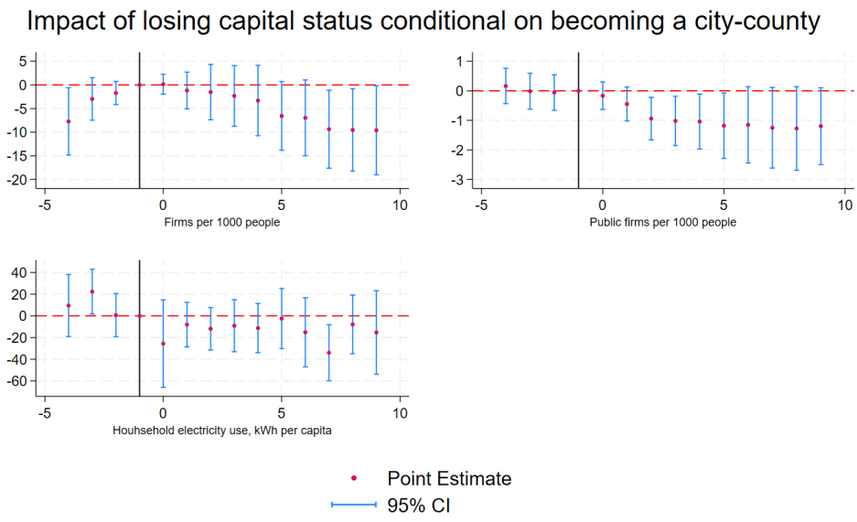


(a) Event study: ex-capitals (treated) vs. city-counties (control)

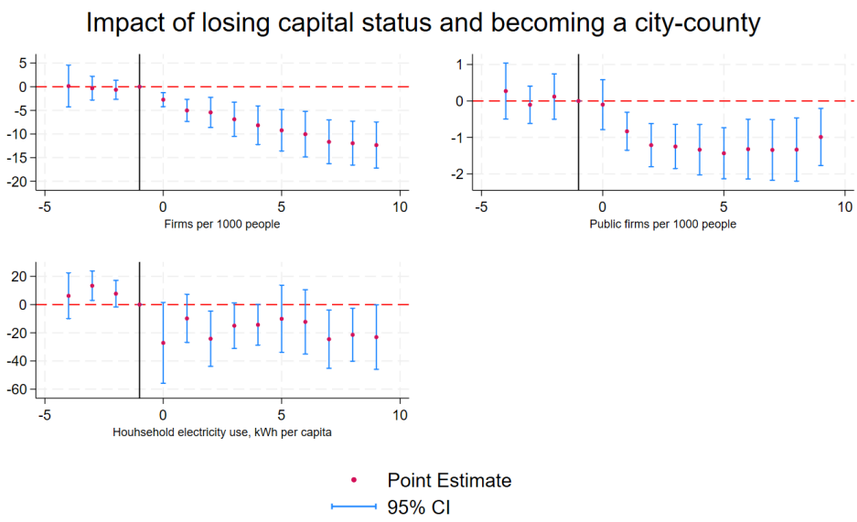


(b) Event study: city-counties (treated) vs. county seats (control)

Figure 12



(a) Event study: ex-capitals (treated) vs. city-counties (control)



(b) Event study: city-counties (treated) vs. county seats (control)

Figure 13

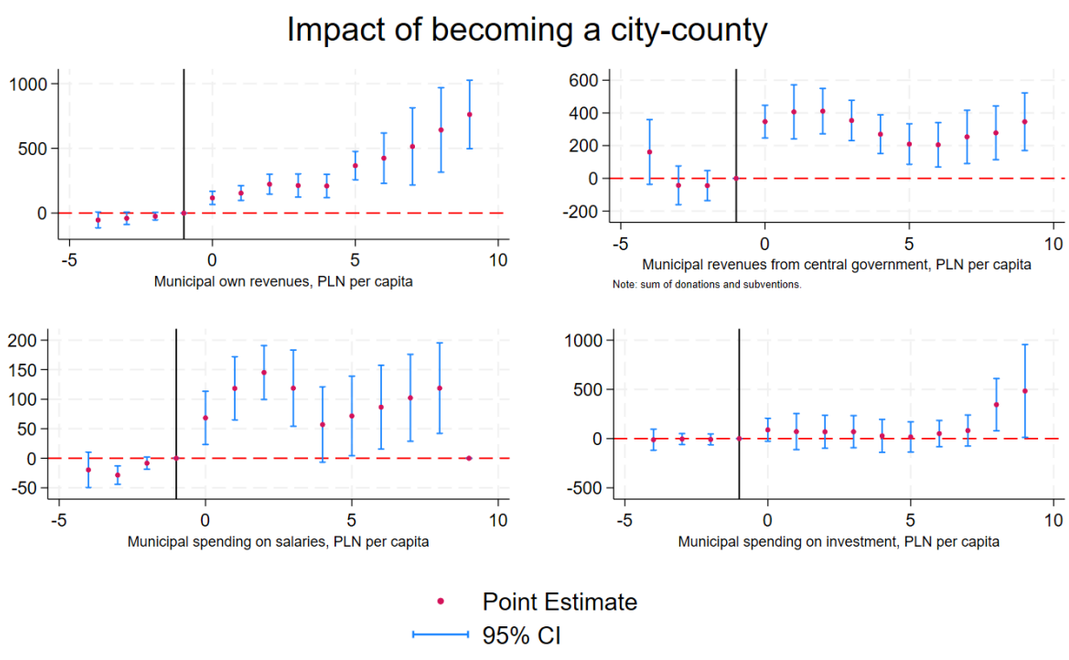


Figure 14: Event study: city-counties (treated) vs. county seats (control)

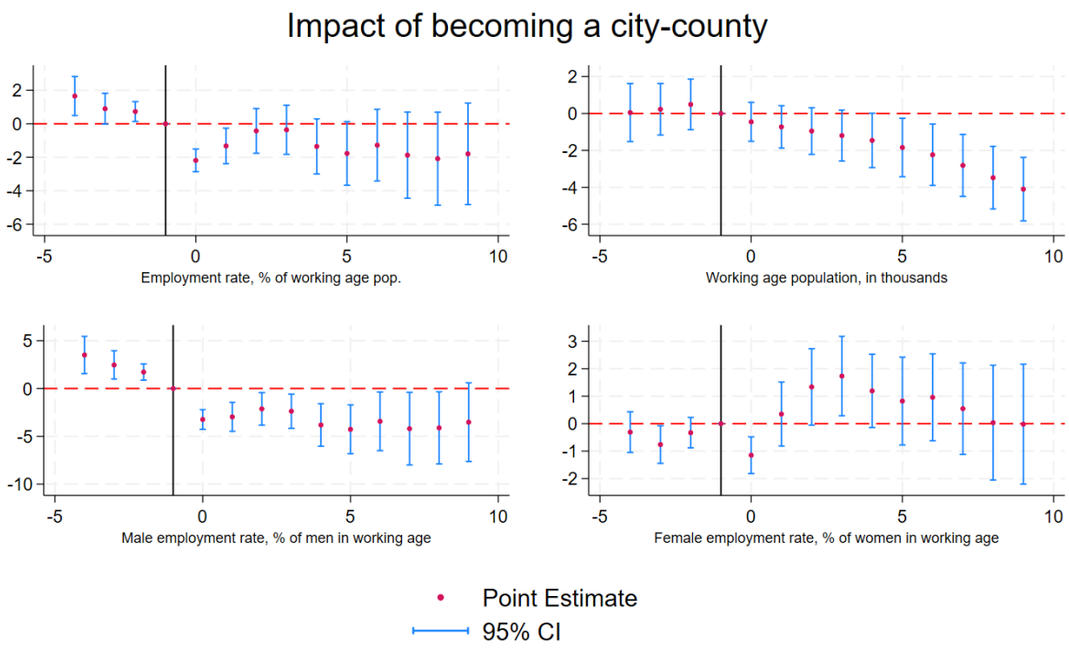


Figure 15: Event study: city-counties (treated) vs. county seats (control)

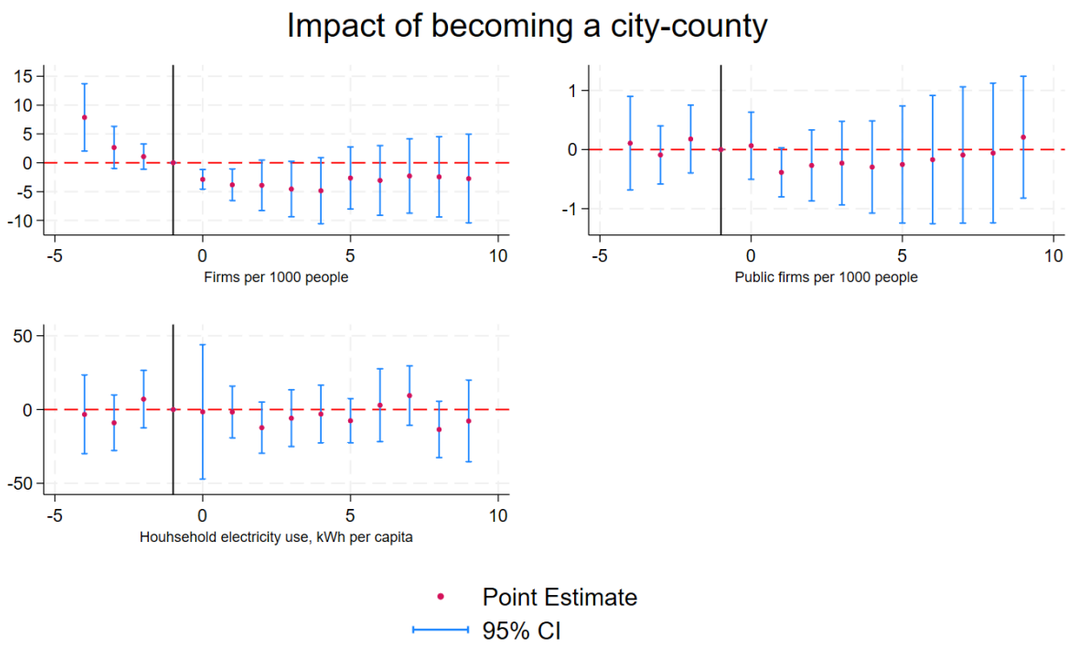
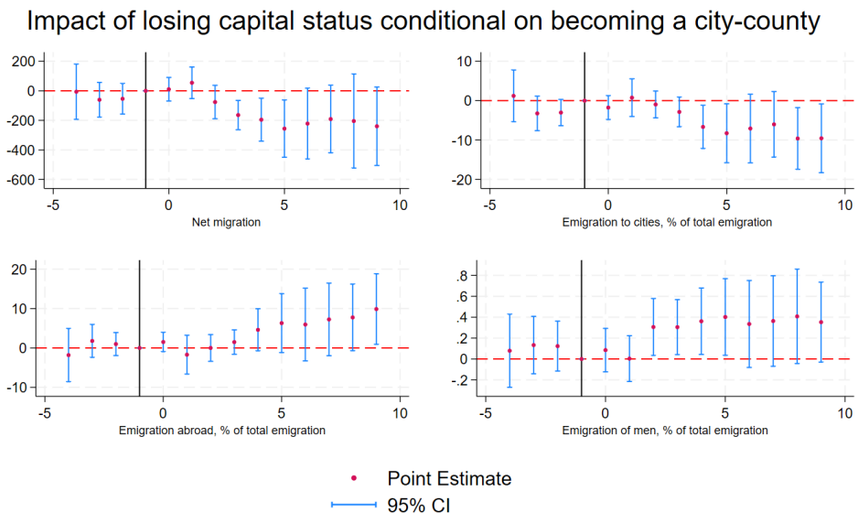
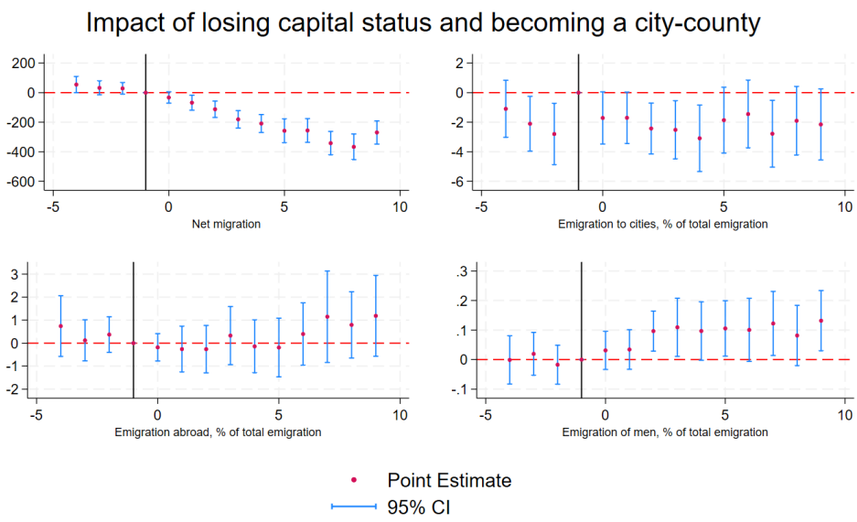


Figure 16: Event study: city-counties (treated) vs. county seats (control)



(a) Event study: ex-capitals (treated) vs. city-counties (control)



(b) Event study: city-counties (treated) vs. county seats (control)

Figure 17

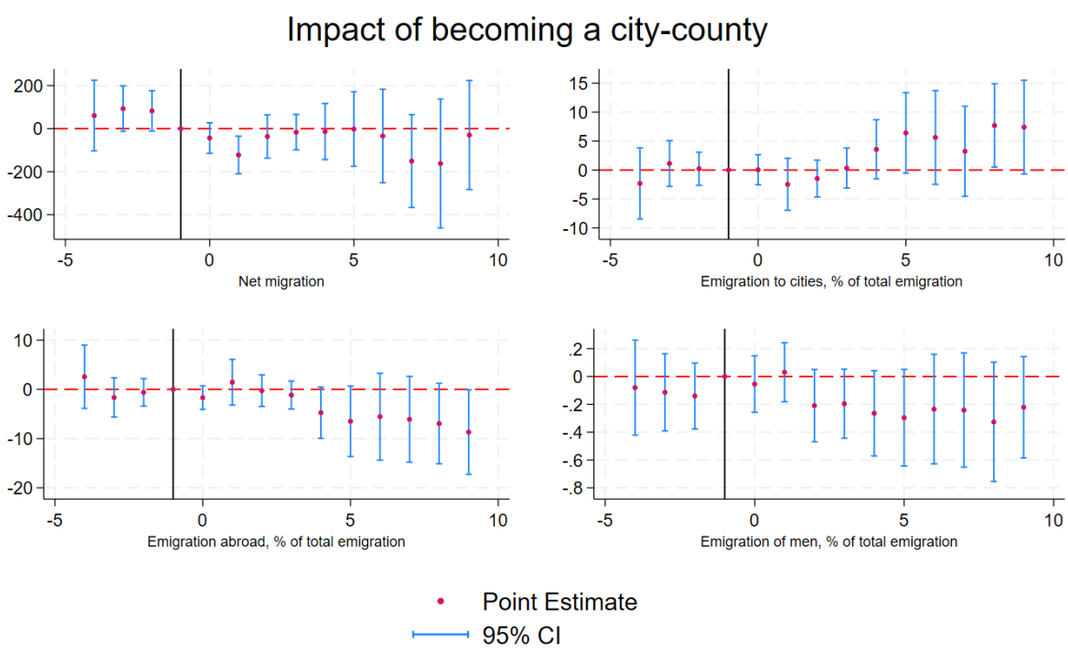


Figure 18: Event study: city-counties (treated) vs. county seats (controls)

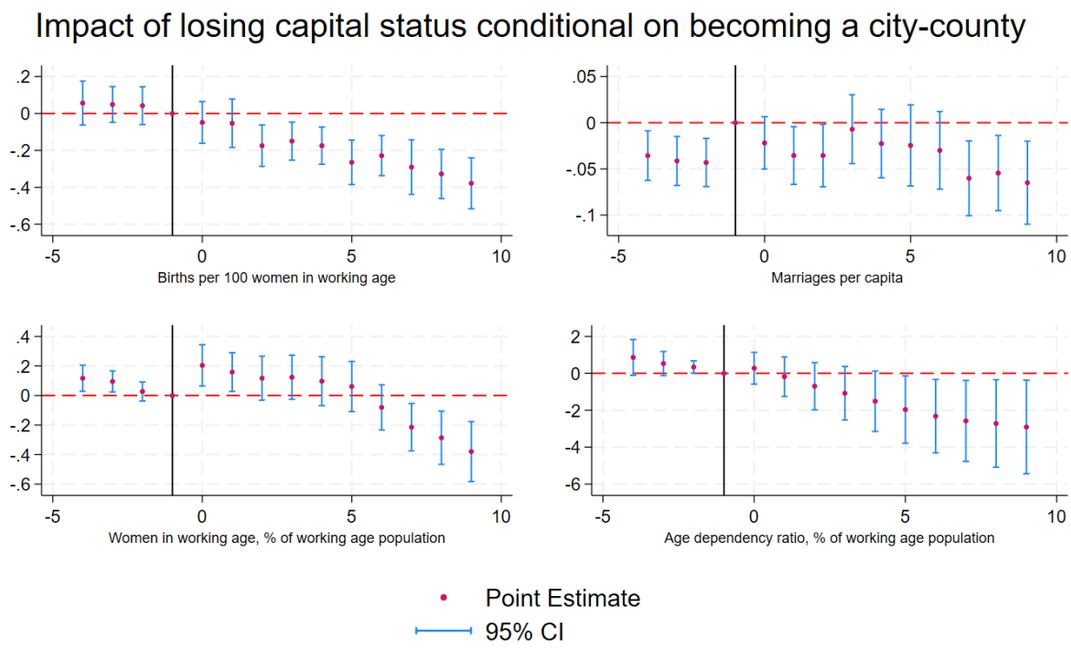


Figure 19: Event study: ex-capitals (treated) vs. city-counties (control)

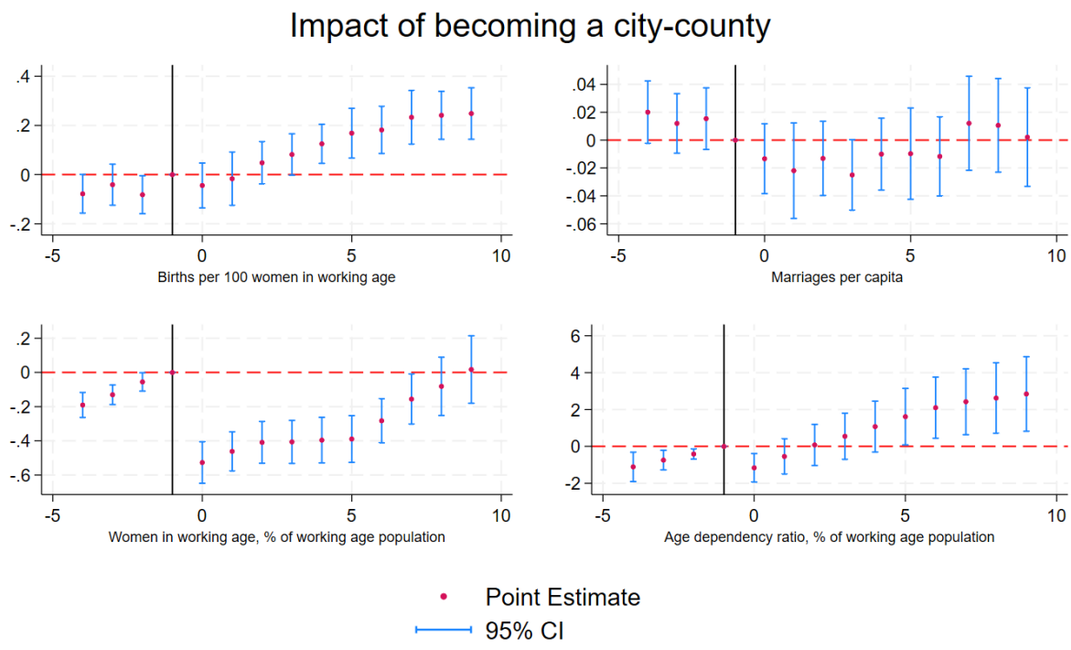


Figure 20: Event study: city-counties (treated) vs. county seats (controls)

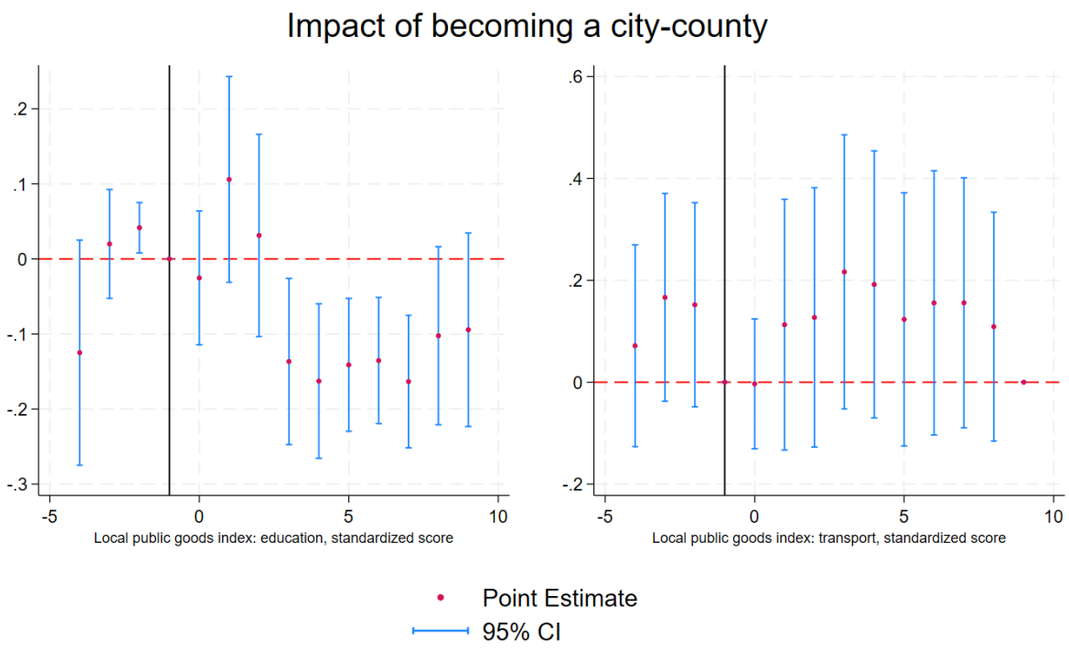


Figure 21: Event study: city-counties (treated) vs. county-seats (controls)

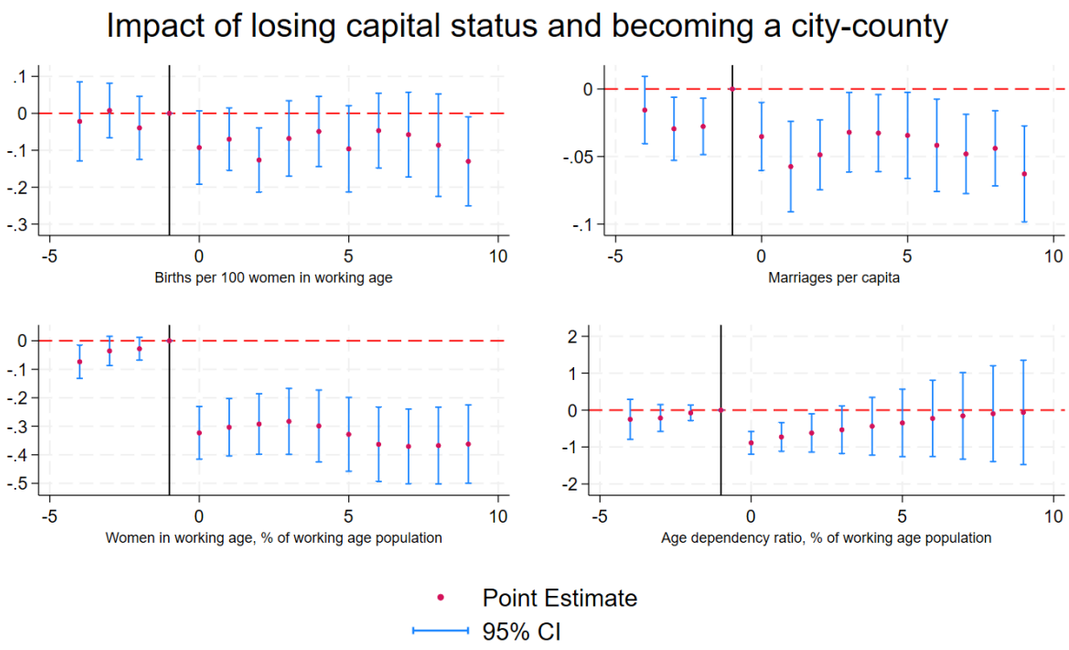
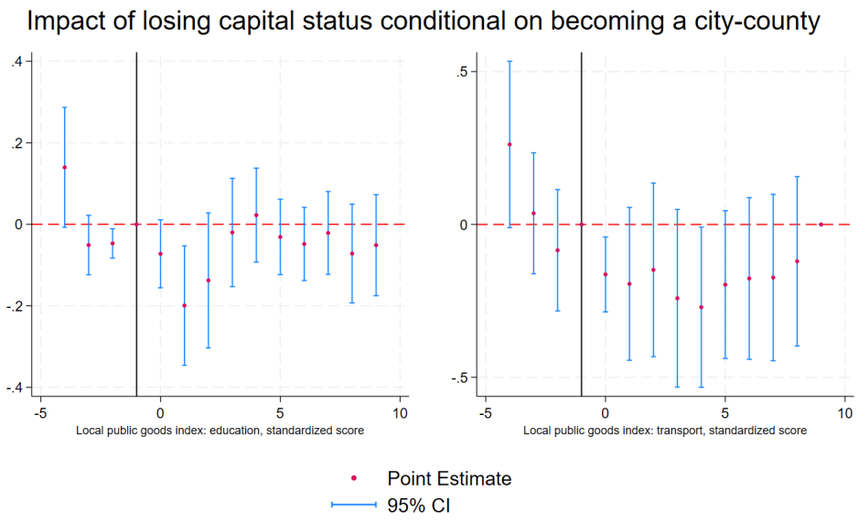
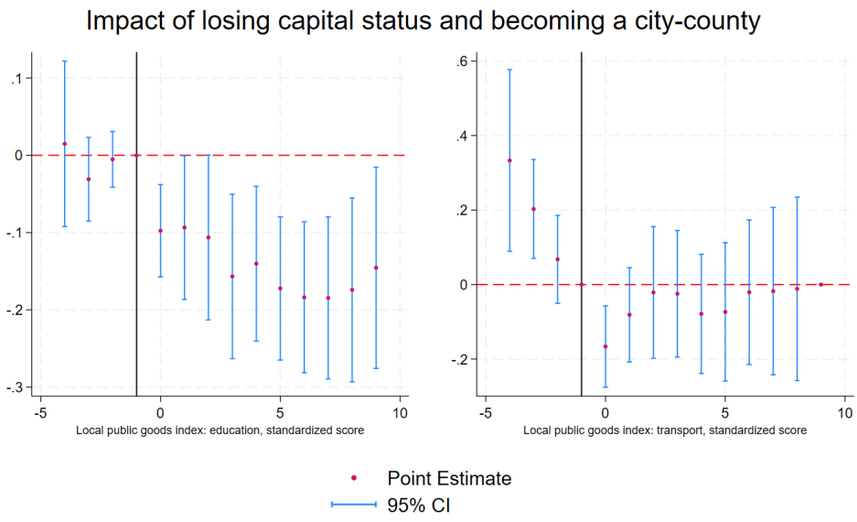


Figure 22: Event-study: ex-capitals (treated) vs. county seats (controls)



(a) Event study: ex-capitals (treated) vs. city-counties (control)



(b) Event study: city-counties (treated) vs. county seats (control)

Figure 23

D All results

Table 10: Results: municipal finance, per capita in PLN

	Ex-capitals vs City-counties		Ex-capitals vs County seats		City-counties vs County seats	
	TWFE	*CS-DiD*	*TWFE*	*CS-DiD*	*TWFE*	*CS-DiD*
Own revenues	−135.87	−98.26	77.20	−15.93	213.07	48.59
SE	48.02	46.56	58.16	23.56	41.31	77.59
t-stat/z-stat	−2.83	−2.11	2.94	−0.27	5.16	0.63
P-val	0.01	0.04	0.00	0.78	0.00	0.53
95% CI	[-233.57, -38.18]	[-189.52, -6.99]	[25.15, 129.26]	[-129.91, 98.06]	[131.00, 294.95]	[-103.48, 200.65]
Central transfers	389.64	437.14	728.88	820.46	339.25	450.87
SE	40.40	99.03	36.78	51.08	23.17	85.57
t-stat/z-stat	9.65	4.41	19.82	16.06	14.64	5.27
P-val	0.00	0.00	0.00	0.00	0.00	0.00
95% CI	[307.45, 471.83]	[243.05, 631.23]	[656, 801.77]	[720.35, 920.58]	[294.32, 385.18]	[283.15, 618.60]
Spending on salaries	274.67	270.68	390.27	418.80	115.60	153.95
SE	40.61	43.58	34.32	37.33	21.09	30.79
t-stat/z-stat	6.76	6.21	11.37	11.22	5.48	5.00
P-val	0.00	0.00	0.00	0.00	0.00	0.00
95% CI	[192.04, 357.29]	[185.27, 356.08]	[322.27, 458.26]	[345.64, 491.95]	[73.80, 157.41]	[93.60, 214.30]
Spending on investment	−12.32	14.98	59.22	80.01	71.54	96.88
SE	54.81	118.86	17.74	55.13	52.01	99.30
t-stat/z-stat	−0.22	0.13	3.34	1.45	1.38	0.98
P-val	0.82	0.90	0.00	0.15	0.17	0.33
95% CI	[-123.83, 99.20]	[-217.99, 247.95]	[24.07, 94.38]	[-28.04, 188.07]	[-31.54, 174.62]	[-97.74, 291.50]

Notes: Standard errors clustered at 50 km radius. TWFE = Two-Way Fixed Effects; CS-DiD = Callaway–Sant'Anna Difference-in-Differences. CS-DiD estimation controls for population.

Table 11: Employment outcomes across municipal comparisons

	Ex-capitals vs City-counties		Ex-capitals vs County seats		City-counties vs County seats	
	TWFE	*CS-DiD*	*TWFE*	*CS-DiD*	*TWFE*	*CS-DiD*
Employment rate, %	−0.26	−1.29	−2.22	−2.46	−1.95	−0.37
SE	0.77	0.60	0.54	1.33	0.65	2.04
t-stat/z-stat	−0.34	−2.13	−4.14	−1.85	−3.00	−0.18
P-val	0.73	0.03	0.00	0.07	0.00	0.86
95% CI	[-1.83, 1.30]	[-2.47, -0.11]	[-3.28, -1.16]	[-5.07, 0.15]	[-3.24, -0.66]	[-4.37, 3.63]
Covariates	-	Firms per capita	-	Firms per capita; Population	-	Firms per capita; Population
Working age pop. (thousands)	2.60	1.77	1.59	2.35	−1.15	0.39
SE	1.09	0.67	0.26	0.60	1.05	0.94
t-stat/z-stat	2.39	2.64	6.24	3.89	−1.10	0.41
P-val	0.02	0.01	0.00	0.00	0.28	0.68
95% CI	[0.39, 4.82]	[0.46, 3.08]	[1.09, 2.10]	[1.17, 3.53]	[-3.23, 0.92]	[-1.46, 2.24]
Covariates	-	Firms	-	Firms	-	Firms
Female employment rate, %	−2.82	−3.28	−1.78	−1.76	1.04	1.64
SE	0.72	0.97	0.54	1.50	0.60	1.92
t-stat/z-stat	−3.94	−3.38	−3.28	−1.17	1.74	0.85
P-val	0.00	0.00	0.00	0.24	0.09	0.39
95% CI	[-4.28, -1.37]	[-5.18, -1.38]	[-2.86, -0.71]	[-4.70, -1.18]	[-0.15, 2.23]	[-2.13, 5.40]
Covariates	-	Firms per capita in hospitality, health education, services and adm. sectors	-	Firms per capita in hospitality, health education, services and adm. sectors Population	-	Firms per capita in hospitality, health education, services and adm. sectors Population
Male employment rate, %	2.16	−1.81	−2.67	−3.10	−4.83	−0.07
SE	1.06	1.01	0.63	1.51	0.95	2.94
t-stat/z-stat	2.04	−1.79	−4.21	−2.05	−5.06	−0.02
P-val	0.05	0.07	0.00	0.04	0.00	0.98
95% CI	[0.01, 4.31]	[-3.79, 0.17]	[-3.93, -1.42]	[-6.07, -0.14]	[-6.72, -2.94]	[-5.82, 5.69]
Covariates	-	Firms per capita; Mining firms per capita; Years of local coalmine closure; Silesia region	-	Firms per capita; Population	-	Firms per capita; Mining firms per capita; Years of local coalmine closure; Silesia region population

Notes: Standard errors clustered at 50 km radius. TWFE = Two-Way Fixed Effects; CS-DiD = Callaway–Sant'Anna Difference-in-Differences. Employment rate is a share of employed working age population (18-64) in %, female employment rate is a share of women in working age (18-59) in %, male employment rate is a share of men in working age (18-64) in %

Table 12: Firms and household electricity use across municipal comparisons

	Ex-capitals vs City-counties		Ex-capitals vs County seats		City-counties vs County seats	
	TWFE	*CS-DiD*	*TWFE*	*CS-DiD*	*TWFE*	*CS-DiD*
Firms per 1000 people	1.46	−17.46	−5.90	−7.36	−7.36	2.89
SE	3.58	3.75	1.81	1.95	2.97	3.11
t-stat/z-stat	0.41	−4.66	−3.26	−3.77	−2.48	0.93
P-val	0.69	0.00	0.00	0.00	0.01	0.35
95% CI	[-5.83, 8.75]	[-24.80, -10.12]	[-9.50, -2.31]	[-11.19, -3.54]	[-13.26, -1.47]	[-3.21, 8.99]
Covariates	-	Working age pop.; Silesia region	-	Working age population	-	Working age pop.; Silesia region
Public firms per 1000 people	−0.74	−2.47	−1.08	−0.43	−0.33	1.24
SE	0.37	1.00	0.31	0.56	0.26	0.39
t-stat/z-stat	−1.99	−2.48	−3.53	−0.78	−1.27	3.18
P-val	0.06	0.01	0.00	0.44	0.21	0.00
95% CI	[-1.51, 0.02]	[-4.42, -0.51]	[-1.69, -0.47]	[-1.52, 0.65]	[-0.86, 0.19]	[0.48, 2.00]
Covariates	-	Municipal exp.; Population; Silesia region	-	Municipal exp.; Population	-	Municipal exp.; Population; Silesia region
Electricity use (kWh/capita)	−20.83	−9.14	−23.32	−15.44	−2.49	5.23
SE	11.20	14.73	6.42	12.51	9.42	20.36
t-stat/z-stat	−1.86	−0.62	−3.63	−1.23	−0.26	0.26
P-val	0.07	0.53	0.00	0.22	0.79	0.80
95% CI	[-43.62, 1.95]	[-38.00, 19.72]	[-36.05, -10.60]	[-39.96, 9.08]	[-21.15, 16.17]	[-34.67, 45.14]
Covariates	-	Employment rate; Population; Silesia region	-	Employment rate; Population	-	Employment rate; Population; Silesia region

Notes: Standard errors clustered at 50 km radius. TWFE = Two-Way Fixed Effects; CS-DiD = Callaway–Sant'Anna Difference-in-Differences.

Table 13: Migration outcomes across municipal comparisons

	Ex-capitals vs City-counties		Ex-capitals vs County seats		City-counties vs County seats	
	TWFE	*CS-DiD*	*TWFE*	*CS-DiD*	*TWFE*	*CS-DiD*
Net migration (abs.)	−46.64	−161.00	−151.05	−114.89	−104.41	−48.50
SE	41.21	119.57	25.43	23.01	31.53	45.37
t-stat/z-stat	−1.13	−1.35	−5.94	−4.99	−3.31	−1.07
P-val	0.27	0.18	0.00	0.00	0.00	0.29
95% CI	[-130.49, 37.21]	[-395.37, 73.35]	[-201.43, -100.66]	[-160.00, -69.79]	[-166.92, -41.90]	[-137.42, 40.42]
Covariates	-	Firms per capita Distance to new capital	-	Firms per capita Distance to new capital	-	Firms per capita Distance to new capital
Emigration to cities (%)	−1.04	1.86	−0.65	−2.31	0.38	1.56
SE	0.94	1.76	0.64	0.72	0.80	1.63
t-stat/z-stat	−1.11	1.05	−1.01	−3.22	0.48	0.96
P-val	0.28	0.29	0.31	0.00	0.63	0.34
95% CI	[-2.94, 0.86]	[-1.60, 5.31]	[-1.93, 0.62]	[-3.72, -0.91]	[-1.20, 1.97]	[-1.64, 4.78]
Covariates	-	Distance to new capital	-	Distance to new capital	-	Distance to new capital
Emigration abroad (%)	1.00	1.25	−0.39	−0.05	−1.39	−1.30
SE	0.73	1.52	0.34	0.37	0.67	1.52
t-stat/z-stat	1.37	0.82	−1.16	−0.13	−2.09	−0.85
P-val	0.18	0.41	0.25	0.90	0.04	0.39
95% CI	[-0.49, 2.49]	[-1.74, 4.24]	[-1.06, 0.28]	[-0.78, 0.67]	[-2.71, -0.07]	[-4.28, 1.69]
Covariates	-	-	-	-	-	-
Male emigration (%)	0.13	0.21	0.08	0.08	0.53	−0.23
SE	0.05	0.11	0.03	0.03	0.32	0.83
t-stat/z-stat	2.82	1.91	2.41	2.50	1.66	−0.27
P-val	0.01	0.06	0.02	0.01	0.10	0.79
95% CI	[0.04, 0.23]	[-0.01, 0.43]	[0.01, 0.14]	[0.02, 0.13]	[-0.01, 1.15]	[-1.86, 1.41]
Covariates	-	-	-	-	-	-

Notes: Standard errors clustered at 50 km radius. TWFE = Two-Way Fixed Effects; CS-DiD = Callaway–Sant'Anna Difference-in-Differences.

Table 14: Demographic outcomes across municipal comparisons

	Ex-capitals vs City-counties		Ex-capitals vs County seats		City-counties vs County seats	
	TWFE	*CS-DiD*	*TWFE*	*CS-DiD*	*TWFE*	*CS-DiD*
Births per 100 women	−0.16	−0.12	−0.07	−0.08	0.09	0.04
SE	0.04	0.04	0.03	0.04	0.04	0.04
t-stat/z-stat	−3.55	−2.78	−1.98	−2.25	2.42	1.08
P-val	0.00	0.01	0.05	0.02	0.02	0.28
95% CI	[-0.25, -0.07]	[-0.20, -0.04]	[-0.14, 0.00]	[-0.15, -0.01]	[0.02, 0.16]	[-0.03, 0.11]
Marriages per capita	0.01	−0.03	−0.02	−0.04	−0.03	−0.02
SE	0.01	0.01	0.01	0.01	0.01	0.01
t-stat/z-stat	0.55	−1.90	−2.51	−3.80	−3.06	−1.57
P-val	0.58	0.06	0.01	0.00	0.00	0.12
95% CI	[-0.02, 0.03]	[-0.05, 0.00]	[-0.04, -0.004]	[-0.06, -0.02]	[-0.04, -0.01]	[-0.04, 0.00]
Women in working age (%)	0.08	0.13	−0.27	−0.31	−0.35	−0.44
SE	0.06	0.06	0.05	0.05	0.06	0.06
t-stat/z-stat	1.17	2.11	−5.42	−6.03	−6.15	−7.91
P-val	0.25	0.03	0.00	0.00	0.00	0.00
95% CI	[-0.06, 0.21]	[0.01, 0.26]	[-0.37, -0.17]	[-0.41, -0.21]	[-0.46, -0.23]	[-0.55, -0.33]
Age dependency ratio (%)	−1.13	−0.66	−0.56	−0.66	0.57	0.00
SE	0.77	0.57	0.36	0.25	0.68	0.52
t-stat/z-stat	−1.47	−1.16	−1.55	−2.66	0.84	0.00
P-val	0.15	0.25	0.13	0.01	0.41	1.00
95% CI	[-2.69, 0.44]	[-1.78, 0.46]	[-1.28, 0.16]	[-1.15, -0.17]	[-0.76, 1.91]	[-1.02, 1.02]

Notes: Standard errors clustered at 50 km radius. TWFE = Two-Way Fixed Effects; CS-DiD = Callaway–Sant'Anna Difference-in-Differences. Age dependency ratio is the share of people aged 0-17 and >65 to the working age population, in %.

Table 15: Local public goods

	Ex-capitals vs City-counties		Ex-capitals vs County seats		City-counties vs County seats	
	TWFE	*CS-DiD*	*TWFE*	*CS-DiD*	*TWFE*	*CS-DiD*
Education index	−0.09	−0.18	−0.11	−0.09	−0.02	−0.04
SE	0.06	0.06	0.03	0.09	0.06	0.12
t-stat	−1.41	−3.19	−3.42	−0.97	−0.36	−0.36
P-val	0.17	0.00	0.00	0.33	0.72	0.72
95% CI	[-0.22, 0.04]	[-0.30, -0.07]	[-0.18, -0.05]	[-0.28, 0.09]	[-0.14, 0.10]	[-0.27, 0.19]
Covariates	-	Municipal revenue per capita; Population	-	Municipal revenue per capita; Population	-	Municipal revenue per capita; Population
Healthcare/family index	−0.02	−0.27				
SE	0.08	0.09				
t-stat	−0.29	−3.04				
P-val	0.77	0.00				
95% CI	[-0.04, -0.01]	[-0.44, -0.09]				
Covariates	-	Municipal revenue per capita; Population				
Public transport index	−0.27	−0.17	−0.24	0.09	0.03	−0.05
SE	0.08	0.08	0.08	0.14	0.06	0.21
t-stat/z-stat	−3.33	−2.18	−2.96	0.65	0.49	−0.23
P-val	0.00	0.03	0.00	0.52	0.63	0.81
95% CI	[-0.43, -0.10]	[-0.31, -0.02]	[-0.40, -0.08]	[-0.19, 0.38]	[-0.10, 0.16]	[-0.46, 0.36]
Covariates	-	Population; Municipal investment per capita; Silesia region	-	Population; Municipal investment per capita	-	Population; Municipal investment per capita; Silesia region

Notes: Standard errors clustered at 50 km radius. TWFE = Two-Way Fixed Effects; CS-DiD = Callaway–Sant'Anna Difference-in-Differences.

E Robustness of parallel trends

Municipal finance

Table 16: "Honest DiD": Impact of losing the capital status and becoming a city-county

	Municipal Own Revenue		**Central Government Revenues**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	268.88	879.46	3317.83	4068.87
0	271.69	876.65	3321.29	4065.42
0.5	196.52	997.00	2620.08	4512.54
1	76.93	1158.17	1816.68	5244.93
2	−282.81	1544.94	174.27	6863.54
	Municipal Spending on Salaries		**Municipal Spending on Investment**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	1478.83	2258.98	22.34	518.58
0	1482.42	2255.39	24.63	516.30
0.5	1348.31	2348.51	−277.74	721.44
1	1182.02	2488.37	−689.84	1116.10
2	753.55	2904.28	−1543.21	1962.59

Notes: Ex-capitals are treated, county seats are controls. Honest DiD bounds reported for sensitivity parameter M. "Original" denotes the baseline specification. All values are PLN per capita.

Table 17: "Honest DiD": Impact of becoming a city-county

	Municipal Own Revenue		**Central Government Revenues**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	794.34	1540.02	1008.54	2295.54
0	797.77	1536.59	1014.45	2289.63
0.5	684.33	1793.68	−366.25	4205.30
1	445.32	2114.36	−2116.46	6249.42
2	−216.55	2814.04	−6211.22	10 379.11
	Municipal Spending on Salaries		**Municipal Spending on Investment**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	259.10	762.50	−541.64	1049.69
0	261.41	760.19	−534.33	1042.38
0.5	99.69	916.47	−901.08	1310.82
1	−61.15	1077.01	−1257.03	1566.91
2	−458.26	1409.97	−2069.16	2124.32

Notes: City-counties are treated, county seats are controls. Honest DiD bounds reported for sensitivity parameter M. "Original" denotes the baseline specification. All values are PLN per capita.

Labor market

Table 18: "Honest DiD": Impact of losing capital status conditional on becoming a city-county

	Employment rate, % of working-age population		**Working-age population, thousands**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−18.56	−1.62	3.00	17.87
0	−18.48	−1.69	3.07	17.80
0.5	−27.51	7.14	−2.98	31.52
1	−38.64	18.68	−17.97	47.19
2	−62.31	43.35	−49.42	79.16
	Male employment rate, % of men in working age		**Female employment rate, % of women in working age**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−12.56	8.81	−26.61	−10.83
0	−12.46	8.71	−26.54	−10.90
0.5	−27.47	22.23	−32.69	−3.86
1	−45.94	39.98	−40.89	4.98
2	−84.64	78.21	−59.20	23.74

Notes: Ex-capitals are treated, city-counties are controls. Honest DiD bounds reported for sensitivity parameter M. "Original" denotes the baseline specification.

Table 19: "Honest DiD": Impact of losing capital status and becoming a city-county

	Employment rate, % of working-age population		**Working-age population, thousands**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−21.57	−9.07	1.57	6.94
0	−21.51	−9.12	1.60	6.92
0.5	−25.71	−6.13	−2.97	11.30
1	−31.53	−1.35	−7.84	16.65
2	−44.47	11.24	−19.01	27.97
	Male employment rate, % of men in working age		**Female employment rate, % of women in working age**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−24.94	−9.93	−19.61	−6.97
0	−24.87	−10.00	−19.55	−7.02
0.5	−28.63	−6.08	−24.73	−2.82
1	−34.45	−0.09	−31.60	3.54
2	−47.53	13.54	−46.51	18.26

Notes: Ex-capital cities are treated, county seats are controls. Honest DiD bounds reported for the sensitivity parameter M. "Original" denotes the baseline specification.

Table 20: "Honest DiD": Impact of becoming a city-county

	Employment rate, % of working-age population		Working-age population, thousands	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−12.17	1.70	−12.92	0.56
0	−12.10	1.64	−12.86	0.50
0.5	−17.70	7.55	−23.22	3.90
1	−25.86	15.89	−35.21	14.32
2	−43.86	34.14	−59.40	38.52
	Male employment rate, % of men in working age		**Female employment rate, % of women in working age**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−24.74	−6.37	−0.81	11.67
0	−24.66	−6.46	−0.75	11.61
0.5	−37.45	6.75	−4.30	15.56
1	−53.98	23.58	−10.57	21.86
2	−89.37	58.83	−24.63	35.95

Notes: City-counties are treated, county seats are controls. Honest DiD bounds are reported for sensitivity parameter M; "Original" denotes the baseline specification. Bounds reflect identification under deviations from parallel trends of magnitude M.

Economic Activity

Table 21: "Honest DiD": Impact of losing capital status conditional on becoming a city-county

	Firms per 1,000 people		Public firms per 1,000 people	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−44.19	14.34	−8.25	−1.00
0	−43.92	14.07	−8.22	−1.03
0.5	−72.81	61.70	−9.73	0.63
1	−123.42	117.26	−13.32	4.23
2	−236.18	231.94	−21.19	12.11
	Household electricity use, kWh per capita			
M	**Lower bound**	**Upper bound**		
Original	−139.79	53.94		
0	−138.90	53.05		
0.5	−375.98	230.20		
1	−651.97	497.71		
2	−1213.27	1055.18		

Notes: Ex-capital cities are treated, city-counties are controls. Honest DiD bounds reported for sensitivity parameter M; "Original" denotes the baseline specification. Bounds reflect identification under deviations from parallel trends of magnitude M.

Table 22: "Honest DiD": Impact of losing capital status and becoming a city county

	Firms per 1,000 people		Public firms per 1,000 people	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−51.54	−17.86	−8.95	−3.17
0	−51.38	−18.01	−8.92	−3.19
0.5	−64.58	−9.06	−13.60	1.29
1	−86.09	11.97	−20.10	7.77
2	−131.64	57.27	−33.54	21.18
	Household electricity use, kWh per capita			
M	**Lower bound**	**Upper bound**		
Original	−149.44	2.47		
0	−148.74	1.77		
0.5	−264.45	79.12		
1	−415.10	223.53		
2	−723.69	530.34		

Notes: Ex-capitals are treated, county seats are controls. Honest DiD bounds are reported for the sensitivity parameter M; "Original" denotes the baseline specification. Bounds reflect identification under deviations from parallel trends of magnitude M.

Table 23: "Honest DiD": Impact of becoming a city-county

	Firms per 1,000 people		Public firms per 1,000 people	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−42.13	2.58	−4.41	1.55
0	−41.93	2.38	−4.38	1.52
0.5	−93.34	35.07	−7.56	4.77
1	−150.73	88.48	−12.63	9.91
2	−267.33	203.91	−23.41	20.63
	Household electricity use, kWh per capita			
M	**Lower bound**	**Upper bound**		
Original	−102.84	41.72		
0	−102.17	41.06		
0.5	−239.30	234.88		
1	−456.74	456.37		
2	−903.69	906.07		

Notes: City-counties are treated, county seats are controls. Honest DiD bounds are reported for sensitivity parameter M; "Original" denotes the baseline specification. Bounds reflect identification under deviations from parallel trends of magnitude M.

Migration

Table 24: "Honest DiD": Impact of losing capital status conditional on becoming a city-county

	Net migration		Emigration to cities, % of total emigration	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−1152.21	−122.27	−38.38	2.40
0	−1147.48	−127.00	−38.19	2.21
0.5	−2003.39	638.21	−75.76	47.51
1	−3013.40	1617.37	−118.97	98.95
2	−5056.32	3602.60	−223.54	204.67
	Emigration abroad, % of total emigration		**Emigration of men, % of total emigration**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−0.10	0.31	0.02	2.73
0	−0.10	0.31	0.04	2.72
0.5	−0.50	0.53	−0.79	4.82
1	−0.96	0.96	−2.34	7.19
2	−1.89	1.88	−7.05	11.91

Notes: Ex-capital cities are treated, city-counties are controls. Honest DiD bounds are reported for sensitivity parameter M; "Original" denotes the baseline specification. Bounds reflect identification under deviations from parallel trends of magnitude M.

Table 25: "Honest DiD": Impact of losing capital status and becoming a city-county

	Net migration		Emigration to cities, % of total emigration	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−1084.62	−571.82	−19.15	−4.06
0	−1082.26	−574.17	−19.08	−4.13
0.5	−1398.13	−269.65	−49.16	21.24
1	−1850.56	157.50	−82.45	54.24
2	−2790.01	1063.88	−149.99	121.95
	Emigration abroad, % of total emigration		**Emigration of men, % of total emigration**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−0.05	0.04	0.10	0.78
0	−0.05	0.04	0.10	0.78
0.5	−0.12	0.13	−0.35	1.28
1	−0.23	0.24	−1.06	1.99
2	−0.46	0.48	−2.52	3.46

Notes: Ex-capital cities are treated, county seats are controls. Honest DiD bounds are reported for sensitivity parameter M; "Original" denotes the baseline specification. Bounds reflect identification under deviations from parallel trends of magnitude M.

Table 26: "Honest DiD": Impact of becoming a city-county

	Net migration		Emigration to cities, % of total emigration	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−660.11	278.16	−13.27	26.04
0	−655.80	273.85	−13.09	25.85
0.5	−1192.43	1351.80	−50.04	49.21
1	−2333.23	2542.65	−93.33	89.95
2	−4724.55	4939.31	−181.90	177.64
	Emigration abroad, % of total emigration		**Emigration of men, % of total emigration**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−0.31	0.09	−2.27	0.40
0	−0.31	0.09	−2.25	0.38
0.5	−0.57	0.53	−4.52	1.01
1	−1.03	1.03	−7.04	3.10
2	−2.03	2.04	−12.07	8.13

Notes: City-counties are treated, county seats are controls. Honest DiD bounds are reported for sensitivity parameter M; "Original" denotes the baseline specification. Bounds reflect identification under deviations from parallel trends of magnitude M.

Demographics

Table 27: "Honest DiD": Impact of losing capital status on fertility

	Births per 100 women in working age	
M	**Lower bound**	**Upper bound**
Original	−1.28	−0.36
0	−1.27	−0.36
0.5	−1.71	0.35
1	−2.59	1.26
2	−4.44	3.12

Notes: Ex-capital cities are treated, city-counties are controls. Honest DiD bounds are reported for sensitivity parameter M; "Original" denotes the baseline specification. Bounds reflect identification under deviations from parallel trends of magnitude M.

Table 28: 'Honest DiD": Impact of losing capital status conditional on becoming a city-county

	Births per 100 women in working age		Marriages per capita	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−1.28	−0.36	−0.28	0.03
0	−1.27	−0.36	−0.28	0.03
0.5	−1.71	0.35	−0.71	0.32
1	−2.59	1.26	−1.18	0.77
2	−4.44	3.12	−2.14	1.72
	Women of working age, % of total working-age population		**Age dependency ratio, % of working-age population**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−0.15	1.26	−12.38	1.52
0	−0.15	1.26	−12.32	1.46
0.5	−0.55	1.77	−15.95	2.73
1	−1.08	2.39	−19.81	4.84
2	−2.38	3.77	−28.57	11.70

Notes: Ex-capital cities are treated, city-counties are controls. Honest DiD bounds are reported for sensitivity parameter M; "Original" denotes the baseline specification. Bounds reflect identification under deviations from parallel trends of magnitude M.

Table 29: "Honest DiD": Impact of losing capital status and becoming a city-county

	Births per 100 women in working age		Marriages per capita	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−0.81	−0.01	−0.33	−0.08
0	−0.81	−0.01	−0.33	−0.08
0.5	−1.42	0.49	−0.62	0.11
1	−2.21	1.23	−0.95	0.43
2	−3.85	2.81	−1.62	1.10
	Women in working age, % of total working-age population		**Age dependency ratio, % of working-age population**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−2.05	−0.96	−5.82	0.49
0	−2.05	−0.96	−5.79	0.46
0.5	−2.41	−0.72	−6.35	2.09
1	−2.88	−0.32	−6.95	4.02
2	−3.94	0.70	−10.26	8.36

Notes: Ex-capital cities are treated, county seats are controls. Honest DiD bounds are reported for sensitivity parameter M; "Original" denotes the baseline specification. Bounds reflect identification under deviations from parallel trends of magnitude M.

Table 30: "Honest DiD": Impact of becoming a city-county

	Births per 100 women in working age		**Marriages per capita**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	0.02	0.79	−0.20	0.04
0	0.03	0.79	−0.20	0.04
0.5	−0.87	1.49	−0.32	0.24
1	−1.93	2.53	−0.55	0.49
2	−4.10	4.70	−1.05	0.99
	Women in working age, % of total working-age population		**Age dependency ratio, % of working-age population**	
M	**Lower bound**	**Upper bound**	**Lower bound**	**Upper bound**
Original	−2.66	−1.46	−3.37	8.90
0	−2.66	−1.47	−3.31	8.84
0.5	−3.19	−0.97	−4.93	12.83
1	−3.84	−0.34	−7.22	17.03
2	−5.26	1.05	−14.59	26.28

Notes: City-counties are treated, county seats are controls. Honest DiD bounds are reported for sensitivity parameter M; "Original" denotes the baseline specification. Bounds reflect identification under deviations from parallel trends of magnitude M.

Public Goods

Table 31: "Honest DiD": All comparisons.

	Impact of losing capital status conditional on becoming a city-county				**Impact of losing capital status and becoming a city-county**			
M	Education index (std. score)		Transport index (std. score)		Education index (std. score)		Transport index (std. score)	
	Lower	Upper	Lower	Upper	Lower	Upper	Lower	Upper
Original	−0.92	0.19	−2.24	0.14	−1.08	−0.26	−0.99	0.43
0	−0.92	0.19	−2.23	0.13	−1.08	−0.26	−0.98	0.43
0.5	−2.68	1.91	−4.60	2.06	−1.58	0.41	−2.91	2.20
1	−4.79	4.00	−7.52	4.89	−2.42	1.28	−5.29	4.58
2	−9.10	8.32	−13.46	10.83	−4.20	3.06	−10.15	9.43
	Impact of becoming a city-county							
M	Education index (std. score)		Transport index (std. score)					
	Lower	Upper	Lower	Upper				
Original	−0.81	0.20	−0.39	1.93				
0	−0.81	0.20	−0.38	1.92				
0.5	−2.25	1.71	−1.19	3.96				
1	−4.09	3.54	−3.37	6.30				
2	−7.82	7.27	−8.05	11.04				

Notes: Honest DiD bounds are reported for sensitivity parameter M; "Original" denotes the baseline specification. Bounds are presented for standardized education and transport indices under three different municipal status change scenarios.

Table 32: "Honest DiD": Impact of losing capital status on local public goods

M	Impact of losing capital status conditional on becoming a city-county				Impact of losing capital status and becoming a city-county			
	Education index (std. score)		Transport index (std. score)		Education index (std. score)		Transport index (std. score)	
	Lower	Upper	Lower	Upper	Lower	Upper	Lower	Upper
Original	−0.92	0.19	−2.24	0.14	−1.08	−0.26	−0.99	0.43
0	−0.92	0.19	−2.23	0.13	−1.08	−0.26	−0.98	0.43
0.5	−2.68	1.91	−4.60	2.06	−1.58	0.41	−2.91	2.20
1	−4.79	4.00	−7.52	4.89	−2.42	1.28	−5.29	4.58
2	−9.10	8.32	−13.46	10.83	−4.20	3.06	−10.15	9.43

Notes: Honest DiD bounds are reported for sensitivity parameter M; "Original" denotes the baseline specification. Bounds are presented for standardized education and transport indices under two different municipal status change scenarios.

F Net migration in neighbouring towns and villages

The analysed period (late 1990s and early 2000s) was a time of growing suburbanization. This might be due to the growing wealth of the middle class or congestion in cities. Figure 24 presents the event study for villages within a 20km radius from ex-capitals (treated) and from city-counties (control).

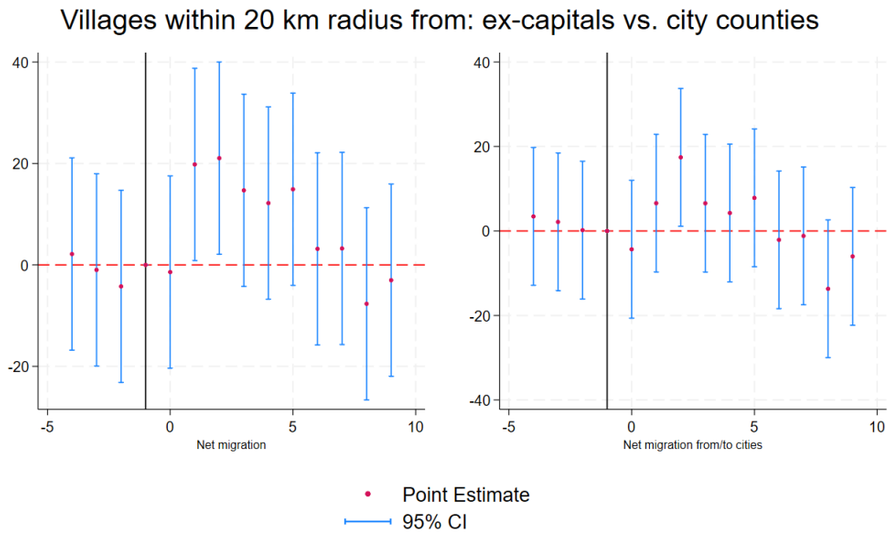


Figure 24: Event study: migration

While the net migration to villages close to the ex-capital was positive in the second and third year as compared to city-counties, the confidence intervals are as large as close to zero and 40. Also, only in the third year following the treatment, the net migration from cities becomes positive.

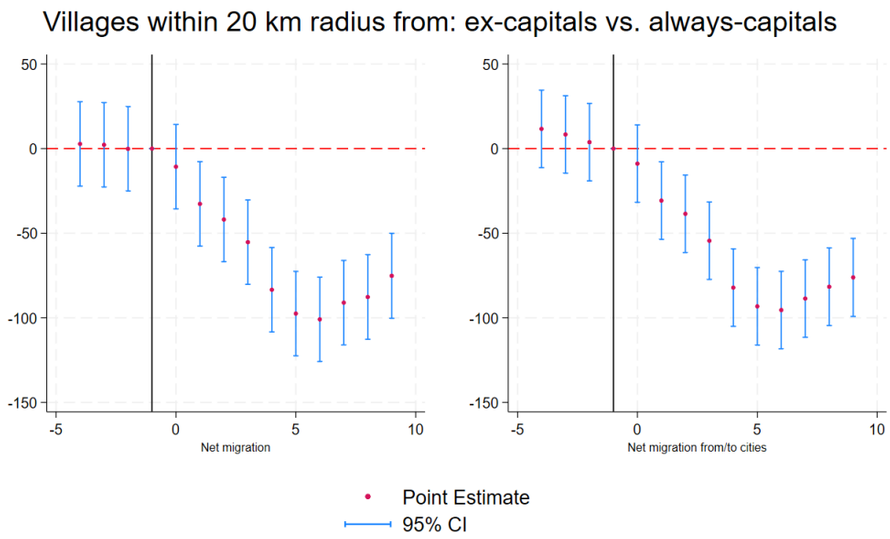


Figure 25: Event study: migration

On the other hand, if we compare villages close to ex-capitals (treated) with those from always-capitals (control), we can see that the latter have experienced a much pronounced suburbanization process, as also net migration from/to cities mirrors the overall migration balance (Figure 25).