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

Marta Korczak[1]

[1]Department of Economics, European University Institute

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Abstract

How do changes in the administrative hierarchy of cities impact their development? This paper focuses on losing a regional capital status, using the context of the 1999 administrative reform in Poland. Exploiting variation in administrative statuses, I compare ex-capitals to newly created "city-counties" and county seats to construct a causal estimate of losing capital status. I find that ex-capital cities experienced a persistent decline in public sector activity, female labor force participation, fertility, and local public good provision, despite receiving higher central government transfers. These results are consistent with a simple theoretical model where a fall in administrative capacity induces sectoral employment reallocation and delayed migration responses. The findings highlight that administrative status is important for city-level development and that the loss of such status has negative consequences, even when paired with increased fiscal autonomy.[1]

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

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1 Introduction

Administrative status of a city matters. It both determines the size of the public workforce and the sphere of 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 transition from the specialized administrative workforce to the private sector might be more costly than the other way round, and the in-built administrative infrastructure might become obsolete. On the other hand, a city can thrive without 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 is the first attempt to do so, exploiting the context of an administrative reform that took place in Poland in 1999. Given that municipalities that lost regional capital status received substantial transfers from the central government, the outcomes of interest are not driven by the decrease in municipal budgets.

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. As a result, 31 municipalities lost the status of regional capital (I denote them as *ex-capitals*).[2] Additionally, this reform created about 300 second-tier regions (*poviats*) which I refer to hereafter as “counties". Among municipalities that lost the status of capital, 28 became city-counties, together with another 20 municipalities that had not had any functions before the reform (city-counties have an integrated municipal and county administration). Becoming a city-county for an ex-capital meant that the loss of fiscal resources associated the losing a regional capital status was not so pronounced. Another important aspect is that before 1999, the self-governance of regions was virtually non-existent, as regional governors were representatives of the central government from Warsaw, and they were performing 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 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, keeping 49 small regions would have been inefficient. Larger regions would accommodate larger infrastructure projects and thus speed up economic development. Also, a three-tier administrative hierarchy would resemble the system used by EU countries of similar size: 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.

[2] In two newly created regions, two municipalities became capitals: In the Lubuskie region, Gorzów Wielkopolski and Zielona Góra; and in Kujawsko-Pomorskie, Toruń and Bydgoszcz.

In order to guide the empirical analysis, I outline a simple theoretical model with two cities, a labor market, and migration. In the empirical strategy, I use an event-study design and the difference-in-difference method with heterogeneous group-time effects (Callaway and Sant'Anna 2021). I compare ex-capital cities (treated) to city-counties (control) or county seats (control) (the latter have separate municipal and county administration). I also compare city-counties (treated) to county seats (control) to estimate the effect of becoming a city-county as compared to 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).

On the one hand, between 1999 and 2003, compared to control cities, the ex-capitals received larger transfers from the central government: on average, 250 PLN per capita per year (approx. 77 USD as of 2000), which was around 15% of a monthly average salary in Poland in 1999.[3] On the other hand, the municipality's own revenues decreased by 100 PLN (34% of pre-treatment mean), suggesting the worsening of the local economic situation, as they also include taxes collected from local businesses. The additional revenues for ex-capitals were not spent on investment, but rather on salaries in the public sector, although part of this effect is mechanical: since 1999, salaries in the local public sector have been paid from the municipal/county accounts, rather than from the central government. In the labor market, losing capital status was a particularly negative shock for women: between 1999 and 2003, the relative yearly decline in female employment rate (as % of women of working age) oscillated between 3 and 5 p.p. points. The effect for men was also negative, but smaller in magnitude and not significant. I hypothesize that most of the women who lost employment had worked in public administration, as public firms per capita (including institutions), also fell relatively to city-counties, with no effect for the private sector. 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, worsening of the labor market, particularly for women, discouraged many of them from enlarging their families.

This paper contributes to several strands of the literature in urban economics and public administration. 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) explore 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 (2007), using a panel of 75 countries, find that appointing local politicians instead of electing

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

them does not enhance fiscal decentralization. While I do not focus on local elections in this study, an important aspect of the reform was its introduction to 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. On the other hand, 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 localized. 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 labor market effect of lowering the administrative status of a municipality.

The results from the literature show an ambiguous potential impact of losing capital status. 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 affect the cities negatively. The rest of the paper is organized 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 (I outline details in a section: Institutional Differences). 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 could be a regional capital (there are 18 of them), a city county (66), a county seat (248), or a commune seat (2479). Importantly, 28 of 31 ex-capitals became city-counties, together with 20 municipalities that had not been a regional capital before.[4] Figure 1 shows the administrative hierarchy of Polish cities from before and after the reform. Importantly, if a city is a regional capital, it is also a city-county and commune seat. If a city is a city-county, it is also a commune seat. However, if a city is a county seat, it can only be a commune seat. A city can also be a commune seat only.

[4]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.

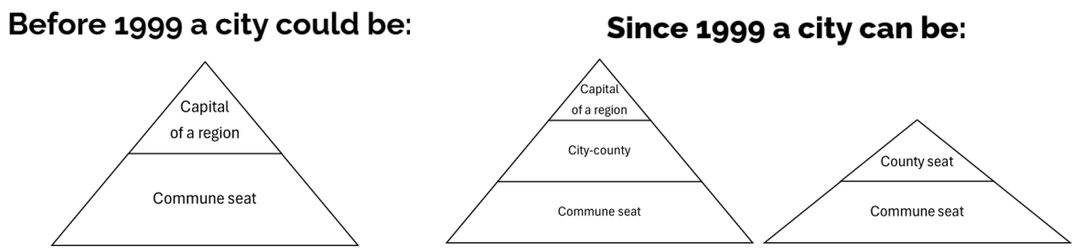


Figure 1: Administrative hierarchy of Polish cities

Regarding the regions, as can be seen from Figure 2, the reform followed closely the administrative division from before 1975. The notable exception was the Koszalińskie region, which had been located between the nowadays Zachodniopomorskie and Pomorskie regions at the Baltic coast. The regional capitals after 1999 were mostly the same as before 1975. Between 1975 and 1999, there were 49 regional capitals, and in 1999, 31 of them lost this status.[5] In Figure 2, they are depicted in the middle map with gray points.

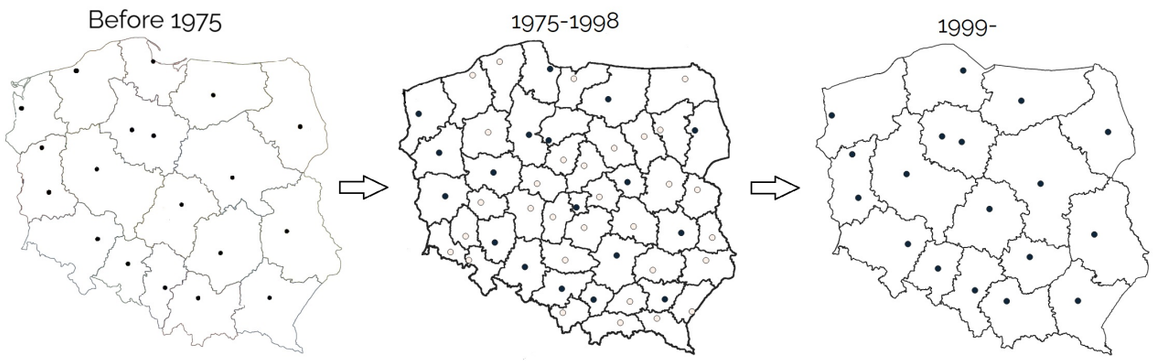


Figure 2: Administrative map of Poland over time

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, the region located in the south-west of Poland. Silesia is predominantly a mining region, with relatively high population density. Apart from ex-capitals, the other municipalities that were upgraded to a city-county status are Gdynia and Sopot (located close to 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 to become a city-county for the remaining municipalities 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 got a city-county status because of 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 channel of spillovers would be 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.

[5]There are two regions, each with two municipalities holding the capital status.

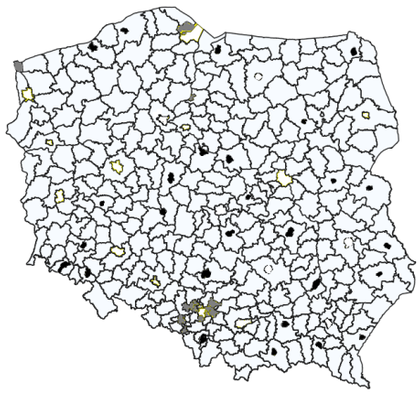


Figure 3: Map of counties in Poland: ex-capitals+city counties are in black, city counties are in gray, always-capitals are in light yellow

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.[6] 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. Also, two regions have two capitals, which is a result of a compromise to local politicians (Lubuskie region with Gorzów Wielkopolski and Zielona Góra; and Kujawsko-Pomorskie region with Toruń and Bydgoszcz).

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 local, powerful communist to emerge. Therefore, he divided the country into 49 small regions and sent to each of them a representative of the communist party from Warsaw. 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. The status of the regional capital was mainly a matter of prestige, but many investments (housing, roads, etc.) were undertaken in proximity to these capitals.

2.1 Timeline of the reform

The economic crisis brought by the prolonged inefficiency of the communist system prompted the first discussions about changing the administrative landscape between the Polish geographers and urbanists in the 1980s. The discussion became more public and was discussed in the press after the communist regime fell. In 1990, the communes gained power in self-governance: the first elections in a democratic Poland were local, for commune councils and municipal mayors. In 1993, the government at the time presented a reform introducing a higher-ranked region to communes: counties. However, in the same year, it lost the elections

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

to the opposition, and the topic of the administration reform vanished from the public debate until 1997, when it won the elections again. In January 1998, the ruling coalition at the time presented the first project of administrative reform, which would change the regions in Poland. The initial project, put into Parliament vote in March 1998, assumed 12 regions (the final version had 16). The Senate suggested adding three regions to the twelve, but the president vetoed the proposition. Nevertheless, the ruling coalition did not stop working on the reform: in the parliamentary sessions between April and June, politicians were discussing dividing Poland into 49, 27, 17, 16, 15, 14, or 13 regions. Finally, the Parliament and Senate agreed on 15 regions during the night between 18 and 19 June 1999. However, the President (supported by the opposition party) announced that he would veto again the project of 15 regions and proposed dividing Poland into 17 regions. Finally, in July 1998, the coalition and opposition compromised on the project of 16 regions. The final project of 16 regions and the three-tier administrative division of Poland was voted on in the Parliament during the night between July 25 and 26, and the President signed it on July 27, 1998. The first local elections to all tiers: communal, county, and regional governments were organized 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 protested against the potential loss of capital regions.

However, according to the survey conducted in all capitals before 1999, not every ex-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.[7] Given the lack of consensus among politicians, very heated public debate, and even protests in some municipalities, it was difficult to conclude in mid-1998 what the reform would finally 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 it was very difficult for the local administration to anticipate 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.[8] Prior to the reform, regional capitals hosted centrally appointed governors (*voivodes*), who held full administrative authority. These cities contained regional offices and sector-specific branches (e.g., police, fire, education), all 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

[7] Please see Appendix A for the details.

[8] Link to the legislation, in Polish

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. Former capitals that lost status saw their regional offices downgraded to delegations. Many 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. Municipalities that became city-counties gained both municipal and county-level powers, with unified governance and access to both tax streams. 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)**	**City-Counties / County Seats (post-1999)**
Administrative Status	Seat of regional administration; full state authority under a regional governor (voivode)	Seat of both central gov (voivode) and regional self-government (marshal)	City-counties: gained county-level powers; County seats: separate county gov
Governance Structure	Centralized: appointed governor with broad administrative control	Dual governance: voivode (state tasks) + marshal (elected by regional assembly)	City-counties: unified council + mayor for both levels; County seats: separate councils for city and county
Offices Present	Regional Office + 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 voivode's and marshal's offices; some central staff transferred to new capitals	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 gov receives only communne-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 a full range of services (schools, health, transport); County seats: some services under county gov

Table 1: Institutional Differences Across Municipality Types Before and After the 1999 Reform

De facto, ex-capitals that became city-counties have gained larger 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 more high-level public services (e.g., specialist hospitals) were taking place 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 county Bialskopodlaski (one year after the loss of capital status by the Biała Podlaska city): *I admit that our area gets less financial transfers than before. We have*

less money to invest in infrastructure, education, and healthcare, all the important sectors. After the liquidation of the region, we lost a lot. 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.[9] The biggest winners of the reform: remaining regional capitals, not only gained autonomy over a much larger region, but also, since 2004, were the place where all the EU transfers went to be further distributed. Therefore, comparing them to the ex-capitals would be misleading. To elicit the impact of losing capital status, I compare ex-capitals to city-counties: before 1999, the former had larger "administrative capacity" than the latter, but since 1999, it 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 the existence and the uniqueness of the equilibrium, can be found in Appendix B.

3 Theoretical framework

To build intuition behind the main mechanisms of the reform, I show a simple model with endogenous migration and labor market outcomes: employment in private and public sectors, and 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.

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

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 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. 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 characterize 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 function of each agent:

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

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 which sector to work in. 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 $N_{g,i1} = \theta_i^* N_i, \quad N_{p,i1} = (1-\theta_i^*)N_i$. 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 decision rule in (6). However, if demand in either sector is larger than the supply, there is unemployment. 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. Their utility in both cases would be:

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

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

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

3.4 Main takeaways

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

Proposition 1: Unemployment is increasing in magnitude of shock *Let the difference $\Delta adm_i = adm_{i2} - adm_{i1}$ be large and negative. Then the share of unemployed agents in city i in period 2 increases:*

$$\frac{dN_{u,i2}}{d\Delta \text{adm}_i} < 0$$

Sketch of a proof: Given fixed job slots (scaled by $\text{adm}_{i2}/\text{adm}_{i1}$), but revised preferences (θ_{i2}) based on new consumption values, demand may exceed supply in either sector. Larger shocks lead to greater mismatch $\rightarrow$ more unemployment.

The related testable hypothesis would be:
H1: The larger the decline in the administrative capacity, the higher the unemployment rate.

Proposition 2: Path dependence in sectoral employment *Consider two cities $i = 1, 2$ with initial administrative capacities adm_{i1} such that $adm_{11} > adm_{21}$. Suppose that in period 2, administrative capacities change exogenously and converge in period 3: $adm_{13} = adm_{23}$. Then, even after convergence of administrative capacity in period 3, the share of workers employed in the public sector θ_{i3} may remain higher in the initially higher-capacity city (City 1), i.e.,*

$$\theta_{13} > \theta_{23}$$

under the following conditions: i) Migration cost c is strictly positive; ii) Agents are heterogeneous in public good preferences and face job slot constraints; iii) Agents can take up less preferred jobs only in period 3.

Sketch of a proof: In period 1, higher administrative capacity in City 1 leads to higher public employment θ_{11} via higher productivity of public jobs. In period 2, job availability scales down proportionally to the change in administrative capacity. If $\text{adm}_{12} < \text{adm}_{11}$, City 1 experiences unemployment due to excess demand for public jobs. In period 3, even though administrative capacities equalize, the stock of unemployed agents, prior allocations, and migration costs prevent full reallocation across cities. Moreover, path dependence arises from (i) initially larger public sector orientation in City 1, and (ii) frictions in switching sectors or cities. Thus, the public employment share in City 1 remains persistently higher in period 3.

The related testable hypothesis would be:
H2: Cities with initially higher administrative capacity maintain higher public employment shares even after convergence in capacity.

Remark 1: Migration occurs when gains>costs *Unemployment 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:*

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

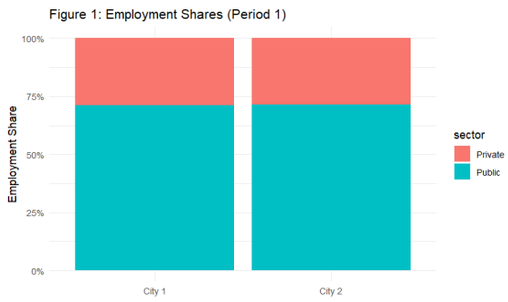
The related testable hypothesis would be:
H3: Migration flows respond positively to the availability of preferred jobs and the utility differential between origin and destination.

Given the studied context, I expect a fall in employment in the ex-capital cities. However, the employment share would still be larger in the public sector than in the private, relative to control cities. Unless migration is too costly, ex-capitals should experience larger unemployment rates, despite having similar administrative capacity to city-counties. In the next section, I calibrate this model to simulate an example that would map the studied context and provide further intuition for the mechanisms at play.

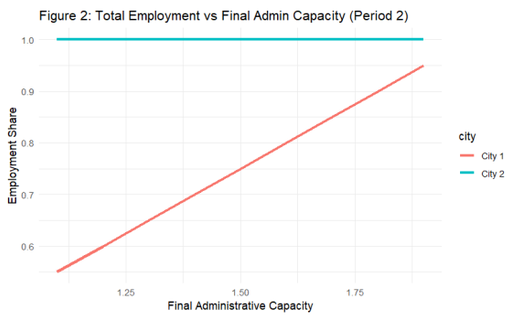
3.5 Example

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

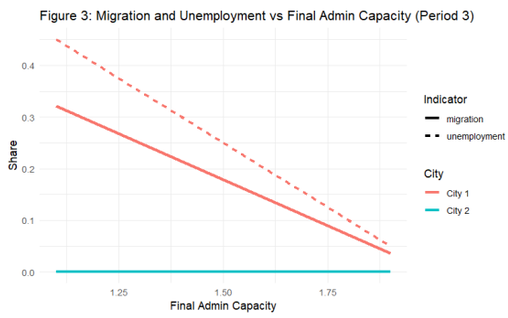
In the first period (a), everybody finds employment, with a similar share of people working in the public sector in both cities. In the second period (b), the lower the final administrative capacity is, the smaller the employment share is in city 1. Despite both cities sharing the same administrative capacity, nobody is unemployed in city 2, which is consistent with Proposition 1. Finally, when the migration is allowed in the third period (c), the migration is declining with the final administrative capacity. The unemployment rate remains larger than the migration share for all values of the final administrative capacity. Migration is limited by the costs as well as the availability of public sector jobs in the second city. In turn, nobody is unemployed nor migrates from the second city.



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

4 Empirical framework

4.1 Data

I use official data from Statistics Poland - Local Data Bank. I use yearly data at the municipal level. If municipalities are rural-urban, I take into account only the urban part. In the event study, I use data from 1995 to 2008, and in estimating aggregated outcomes, I use data until 2003 in order to avoid the confounding effect of joining the European Union in 2004. I drop

observations from before 1995 due to the confounding impact of the “Balcerowicz plan"[10] and the subsequent economic crisis in Poland at the beginning of the 1990s. Part of the data is newly digitized from the archives of Statistics Poland (unemployment, employment by sectors, arable and non-arable land, hospital beds, and number of nurses, access to sewage and mains gas, use of water and mains gas, use of tourist accommodation), but will be used in subsequent drafts of this paper.

I select four distinct groups of municipalities:

1. Treated: ex-capitals who have become city counties (28)[11]
2. Control 1: City-counties (20)
3. Control 2: County seats (187)
4. Municipalities which remained capitals, “always-capitals": only taken into account in the analysis of outcomes for neighbouring towns or villages.

Also, I select villages located within a 20 km radius and municipalities located within a 30 km radius from the treated municipalities, from new city counties, and from always-capitals. I present several comparisons: between treated municipalities and city-counties; between treated municipalities and county seats; and between city-counties and county seats. Additionally, in order to investigate the migration patterns after the reform in the surrounding area of treated municipalities, I compare villages and towns located in proximity to treated municipalities and city-counties, and always capitals.

This is the before-and-after analysis, where a treatment is the binary variable taking the value of one for observations from 1999. In most comparisons, the treated group is ex-capital municipalities, with the exception of comparison between city-counties and county seat (in this case, city-counties are treated), and neighbouring areas. All variables listed here are in per capita terms or per working-age population, depending on the context. I estimate the effect of losing capital status on the following outcomes: Firstly, I describe municipal finance: revenues collected locally and transfers from the central government, expenditures on salaries and investment. Secondly, I describe the condition of the labour market before and after the treatment: employment for men and women, and the working-age population. Thirdly, I study the economic conditions: the number of firms in the public and private sectors, and the household use of electricity. I also show results for demographic variables and migration.

Finally, I construct a local public good index separately for education, healthcare and family, and public transport. The education index consists of the number of places in kindergartens, the number of books, and the number of public educational firms/institutions.[12] The healthcare and family index consists of the number of creches, doctors, dentists, and public healthcare firms/institutions. The transport index consists of bus lines and public transport firms. All indices are z-scores of the simple averages of the respective variables in per capita terms.

[10] Also termed “Shock Therapy", was a set of reforms to rapidly transform the Polish economy from centrally planned to a capitalist market. Named after the Polish minister and economist Leszek Balcerowicz.

[11] Wałbrzych was a city county, but in 2002 it changed its status to county seat. Since 2012, it has again become a city-county. It is included in the event study but dropped in the subsequent analysis.

[12] I do not use data on primary schools or high schools due to another reform in 1999/2000, which introduced secondary schools in Poland and reshuffled employment in all levels of schooling.

4.2 Summary statistics

I present summary statistics for the main variables used in the econometric analysis.

Table 2: Summary statistics: municipal finance, before (1995-1998) and after the treatment (1999-2008)

Group	Own Rev.	Central Rev.	Expend.	Investment	Salaries
	Per capita in current PLN				
Always-capitals, post	1420.73	1006.17	2557.27	478.21	792.35
	(606.93)	(177.53)	(695.58)	(279.45)	(142.64)
	N=160	N=160	N=160	N=160	N=144
Always-capitals, pre	357.75	319.19	1100.77	225.29	265.41
	(127.49)	(91.02)	(367.68)	(126.97)	(87.20)
	N=64	N=64	N=64	N=64	N=64
City-counties, post	1359.44	965.60	2410.25	419.73	714.04
	(666.46)	(269.80)	(864.97)	(454.60)	(160.34)
	N=200	N=200	N=200	N=200	N=180
City-counties, pre	326.87	347.12	1079.85	186.70	267.31
	(153.94)	(229.87)	(393.29)	(242.37)	(92.82)
	N=80	N=80	N=80	N=80	N=80
County seat, post	903.49	632.51	1591.47	272.79	487.15
	(378.59)	(177.40)	(521.09)	(194.48)	(139.20)
	N=1493	N=1450	N=1484	N=1484	N=1329
County seat, pre	283.25	304.84	761.66	180.20	158.38
	(106.62)	(137.36)	(286.91)	(108.88)	(77.50)
	N=592	N=580	N=592	N=592	N=592
Treated municipalities, post	1094.54	1235.05	2454.17	376.09	851.22
	(450.12)	(322.75)	(682.65)	(260.49)	(245.10)
	N=304	N=300	N=304	N=304	N=274
Treated municipalities, pre	290.56	305.51	858.15	189.34	189.97
	(88.26)	(106.48)	(310.41)	(98.35)	(100.95)
	N=124	N=120	N=124	N=124	N=124

Across all groups, there is a clear upward trend in public finances from the pre- to post-treatment period: a direct result of the decentralization in 1999 (Table 2). Due to the reform, a large bulk of public services have become a responsibility of local authorities. Treated municipalities (ex-capitals) show particularly strong growth in central transfers, increasing from 306 PLN to 1235 PLN, suggesting targeted support mechanisms. As a comparison, the average monthly salary in Poland in 1999 amounted to 1706.74 PLN. Regarding expenditures, we can notice a much stronger growth of salaries than investment for all groups, but in particular for treated municipalities: more than fourfold, from 190 PLN per capita to 851 PLN per capita. Meanwhile, the increase of municipal spending on investment was larger for new city-counties than ex-capitals. We can also notice that all groups of municipalities were not so different from each other before 1999 (except own municipal income), reflecting the fact that Poland pre-reform was a very centralized state with little autonomy for municipalities, regardless of their status.

In the labor market, a decline in employment rates (both total and female) is observed in all groups over time (Table 3). This is due to the economic crisis in Poland: at the beginning of

the 2000s, the unemployment rate reached 20%. The job loss was triggered by the crisis in Russia in 1998, and massive layoffs of privatized companies, which were no longer obliged to keep employment at the level of 1989. In the econometric analysis, I control for the number of firms to capture the potential differential effect of the crisis at the time. We can also see a negative migration balance post-treatment for all groups of municipalities, reflecting the opening of borders after the EU accession in 2004. Interestingly, in city counties, emigration also picked up before 1999, which might be due to the falling coalmine industry in many of these cities. Meanwhile, the working-age population remained relatively stable: it even slightly increased for most of the groups (except county seats). This might be due to the demographic boom cohort from the early 1980s, who started to enter the labor market.

Table 3: Summary statistics: employment and demographics, before (1995-1998) and after (1999-2008) the treatment

Group	Employment	Female Emp.	Net Migration	Working Age Pop.
	% of working age pop.			*In thousands*
Always-capitals, post	47.74	46.87	-461.39	257.19
	(9.05)	(7.33)	(870.50)	(146.69)
	N=155	N=155	N=150	N=160
Always-capitals, pre	54.72	52.49	233.50	252.57
	(8.55)	(6.34)	(586.12)	(145.15)
	N=60	N=60	N=60	N=64
City-counties, post	34.33	30.58	-451.28	85.41
	(7.70)	(6.46)	(458.41)	(43.45)
	N=200	N=200	N=200	N=200
City-counties, pre	43.00	35.96	-252.91	87.63
	(8.81)	(7.38)	(517.01)	(45.23)
	N=80	N=80	N=80	N=80
County seat, post	38.89	39.32	-91.97	22.55
	(9.76)	(9.38)	(138.33)	(12.34)
	N=1453	N=1443	N=1470	N=1463
County seat, pre	46.49	45.70	-14.37	22.57
	(19.13)	(9.79)	(100.69)	(12.65)
	N=583	N=572	N=638	N=589
Treated municipalities, post	41.82	41.19	-263.51	65.39
	(8.71)	(6.63)	(249.05)	(33.66)
	N=304	N=304	N=310	N=304
Treated municipalities, pre	50.95	49.93	47.87	64.57
	(9.35)	(6.88)	(214.45)	(33.92)
	N=122	N=120	N=124	N=124

Differences in economic activity are also evident (Table 4). Always-capitals and city-counties report higher numbers of private firms per thousand people, reaching over 120 and 88, respectively, in the post period, compared to 104 in treated municipalities and 98 in county seats. Interestingly, public firms' density increases for all groups, including treated municipalities which lost capital status. However, before the reform, the ex-capitals had the largest number of public firms per capita, and post-reform, they have the smallest number, reflecting a decrease in overall administrative capacity.

While working-age population increased for most cities, overall population decreased post-reform everywhere. This might reflect the growing emigration abroad due to joining the EU

and steadily falling birth rate over the years considered (1999-2008).

Table 4: Summary statistics: firms, population, and distance to Warsaw, before (1995-1998) and after the treatment (1999-2008).

Group	Public Firms	Private Firms	Population	Distance to Warsaw
	Per thousand persons		*In thousands*	*In km*
Always-capitals post	3.82	124.13	369.08	253.73
	(1.77)	(15.28)	(213.14)	(83.46)
	N=153	N=153	N=160	N=150
Always-capitals pre	1.61	93.66	373.87	253.73
	(0.69)	(15.23)	(218.07)	(83.88)
	N=60	N=60	N=64	N=60
City-counties post	3.63	87.89	121.86	285.12
	(2.46)	(29.44)	(62.19)	(50.05)
	N=200	N=200	N=200	N=200
City-counties pre	1.13	64.61	128.42	285.12
	(0.59)	(21.34)	(65.89)	(50.24)
	N=80	N=80	N=80	N=80
County seat post	4.57	98.30	32.14	221.44
	(2.99)	(20.84)	(17.64)	(100.55)
	N=1475	N=1475	N=1495	N=1469
County seat pre	1.76	68.56	32.57	221.98
	(3.18)	(19.39)	(18.50)	(99.88)
	N=627	N=627	N=636	N=640
Treated municipalities post	3.39	103.87	93.95	219.30
	(1.81)	(17.22)	(49.02)	(98.13)
	N=300	N=300	N=304	N=300
Treated municipalities pre	1.93	81.74	96.40	219.30
	(0.62)	(14.28)	(50.80)	(98.37)
	N=121	N=121	N=124	N=120

Looking at local public goods indices, the treated municipalities registered the biggest decline post-reform (Table 5). The largest fall was in the health/family index, although ex-capitals still maintained a higher position than city-counties post-reform. While the transport index also fell dramatically for the ex-capitals, it remained top of the rank after the treatment.

Table 5: Summary statistics: education, health and family, and transport indices, before and after the treatment

Group	Education	Health/Family	Transport
	Standardized scores		
Always-capitals, post	-0.39	0.39	-0.31
	(0.44)	(0.50)	(0.23)
	N=150	N=105	N=135
Always-capitals, pre	-0.37	0.36	-0.12
	(0.29)	(0.43)	(0.28)
	N=60	N=60	N=60
City-counties, post	-0.38	-0.72	-0.15
	(0.26)	(0.24)	(0.33)
	N=200	N=126	N=180
City-counties, pre	-0.38	-0.46	-0.14
	(0.25)	(0.35)	(0.34)
	N=80	N=74	N=80
Treated municipalities, post	-0.11	-0.14	0.00
	(0.43)	(0.39)	(0.35)
	N=300	N=174	N=274
Treated municipalities, pre	-0.04	0.15	0.25
	(0.34)	(0.37)	(0.58)
	N=120	N=105	N=124

The summary statistics are just a snapshot of local economic and fiscal conditions before and after the reform, but they give a few important takeaways:

- Losing capital status was *not* associated with smaller transfers from Warsaw to municipal budgets
- The labor market was deteriorating in all regions in Poland around the time of the reform
- The number of public firms increased everywhere, but the least in ex-capitals
- Treated municipalities registered a decline in all local public good indices, contrary to the control group of city-counties (no change in education) and always-capitals (improvement in health/family index).

The next section offers an overview of the empirical strategy, followed by the results.

4.3 Empirical strategy

4.3.1 Overview and identification

The administration reform implemented in 1999 involved a reorganization of territorial governance, affecting cities in different ways depending on their administrative status prior to the reform. I focus on three main treatment types stemming from the reform:

- Remaining a regional capital (Cap) – cities that retained capital status and gained additional administrative powers (always-capitals);
- Becoming a city-county (CC) – cities granted broader local self-governance;
- Losing regional capital status (ExCap) – former capitals that lost regional status but often became city-counties (ex-capitals).

I distinguish several groups of municipalities that received different treatments:

- Cap+CC (N=18): Cities that remained regional capitals and became city-counties.
- CC+ExCap (N=28): Cities that lost capital status but became city-counties.
- ExCap only (N=3): Former capitals that did not gain city-county status.
- CC only (N=20): Cities that became city-counties but were never capitals.
- Control (N=240): County seats that did not experience any change in status.

Following Roller and Steinberg (2023), the average treatment effect of losing capital status on the treated municipalities that gained a city-county status is:

$$\begin{aligned} ATET^{ExCap|CC=1}_{t|ExCap=1,CC=1} = \delta_t^{1,1}(x) = \\ E\left[Y_1 \mid X=x, ExCap=1, CC=1\right] - E\left[Y_0 \mid X=x, ExCap=1, CC=1\right] \\ -\left\{E\left[Y_1 \mid X=x, ExCap=1, CC=0\right] - E\left[Y_0 \mid X=x, ExCap=1, CC=0\right]\right\} \end{aligned} \tag{15}$$

If the Stable Unit Treatment-Value, no anticipation, exogeneity, and common trends assumptions hold.
This comparison isolates the effect of losing capital status conditional on receiving city-county status, capturing the impact of status loss over and above the general institutional and fiscal changes associated with city-county designation.
In turn, the average treatment effect of losing capital status and gaining city-county status on the treated municipalities is:

$$\begin{aligned} ATET^{ExCap,CC}_{t|ExCap=1,CC=1} = \gamma_t^{1,1}(x) = \\ E\left[Y_1 \mid X = x, ExCap = 1, CC = 1\right] - E\left[Y_0 \mid X = x, ExCap = 1, CC = 1\right] \\ - \left\{E\left[Y_1 \mid X = x, ExCap = 0, CC = 0\right] - E\left[Y_0 \mid X = x, ExCap = 0, CC = 0\right]\right\} \end{aligned} \tag{16}$$

If the Stable Unit Treatment-Value, no anticipation, exogeneity, and common trends assumptions hold.
In this specification, we compare ex-capitals that became city-counties to municipalities that became county seats but were never regional capitals. This comparison captures the combined effect of losing regional capital status and gaining city-county status, relative to gaining county seat status alone.

In both comparisons, the identifying assumption is that, in the absence of losing capital status, both groups would have followed parallel trends in municipal outcomes. Furthermore, there is no anticipation of treatment and no spillovers of treatment to control units. I exclude always-capitals in identifying causal effects of losing capital status because, as explained in the Context section, remaining capitals got a lot of powers compared to the pre-reform period (e.g., decisions regarding infrastructure projects in a region). While becoming a city-county may have increased local autonomy, the loss of capital status implied a decline in prestige and a decline in the administrative hierarchy of local institutions. I am interested in the net effect of these transitions.

To check visually the parallel trends, I begin with a traditional event study design. Coefficients on pre-reform outcomes close to zero suggest parallel trends between treated and control municipalities. I complement the event-study analysis with the "honest Difference-in-Difference" method developed by Rambachan and Roth (2023). Rather than assuming exact parallel trends, this method allows for bounded deviations post-treatment, calibrated by observed pre-treatment trends. The treatment effect becomes partially identified under researcher-specified restrictions (e.g. the maximum post-treatment trend bias is at most M times the observed pre-trend), and uniformly valid confidence intervals are constructed. Thanks to these "honest" intervals, I can assess how strong the parallel trends assumption must be to support my findings.

To investigate the aggregate effect of the treatment on the period 1999-2003, I proceed with a Two-Way Fixed Effect model (TWFE). The main assumption of a TWFE approach is a constant treatment effect across treated units and periods. This might likely not hold, and thus I complement the analysis with the Difference-in-Difference method (DiD) with group-time effects a la Callaway and Sant'Anna (2021).

4.3.2 Event study design

The event study design examines the *dynamic effects* of the 1999 administrative reform and assesses the *validity of the parallel trends assumption.* This approach estimates how the outcomes of treated municipalities evolved relative to control municipalities, before and after the reform, while controlling for unit and time fixed effects. I estimate the following specification:

$$Y_{it} = \alpha_i + \delta_t + \sum_{k \neq -1} \beta_k \cdot \mathbb{1}\{\text{event_time}_{it} = k\} + \varepsilon_{it} \tag{17}$$

where:

- Y_{it} is the outcome of interest for municipality i in year t,
- α_i are municipality fixed effects and δ_t are year fixed effects
- $\text{event_time}_{it} = k$ is an indicator for k periods relative to treatment
- The omitted category is $k = -1$, the year immediately before treatment.

I cluster standard errors using spatial clusters defined by a fixed-radius rule: municipalities within a given distance (25km, 50km, 75km, 100km) are assigned to the same cluster. Here I show the results for the 50km cluster, while different distances are shown in the Appendix.

Assuming parallel trends and no anticipation holds, the estimated β_k can be interpreted as the *average treatment effect k years after treatment*, relative to the baseline year. In the first comparison, the treatment is losing capital status, conditional on both comparison groups becoming city-counties. In that case, β_k would be an estimation of the ATET effect from (15). In the second comparison, the treatment is defined as losing capital status and gaining city-county status, relative to municipalities that gained county seat status only. Finally, in the third comparison, the treatment is gaining city-county status, relative to county-seats. This would correspond to the ATET effect from (16).

4.3.3 Robust inference under relaxed parallel trends assumption

To assess the robustness of the estimates, I implement the method proposed by Rambachan and Roth (2023), which provides valid inference when the parallel trends assumption may be violated.

Let δ_t denote the bias from a violation of the parallel trends assumption in period t. The conventional DiD estimator assumes $\delta_t = 0$ for all $t \geq g$, where g is the treatment onset. In contrast, the honest DiD approach allows for nonzero violations, constrained within a plausible set Δ defined by the researcher.

Specifically, the treatment effect at time t is partially identified as:

$$\tau_t^{\text{Honest}} \in [\widehat{\tau}_t - \delta_t^-,\ \widehat{\tau}_t + \delta_t^+], \quad \delta_t \in \Delta, \tag{18}$$

where $\widehat{\tau}_t$ is the conventional event-study estimate (corresponding to β_k from (17)), and δ_t reflects potential deviations in trends. The set Δ typically restricts the magnitude and smoothness of δ_t based on observed pre-treatment trends.

One common restriction is the *bounded derivatives* assumption, which limits the slope of trend violations:

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

where M is calibrated using the maximum first difference in estimated pre-treatment coefficients.

This approach yields *honest confidence intervals* that remain valid even when exact parallel trends do not hold, under the assumption that post-treatment trend deviations are no larger than those observed pre-treatment.

4.3.4 Two-Way Fixed Effects

In order to show the aggregate effects of the reform, we estimate the following equation:

$$Y_{it} = \alpha_i + \delta_t + \tau D_{it} + \varepsilon_{it} \tag{20}$$

where Y_{it} denotes the outcome of interest for municipality i in year t, α_i are unit fixed effects, and δ_t are time fixed effects. The parameter τ captures the average treatment effect. Standard errors are clustered at the 50km radius. TWFE assumes a constant treatment effect across treated municipalities and years. If they vary across groups or over time (which is common in practice), then TWFE does not estimate the average treatment effect (ATE). Instead, it gives a weighted average of group-time-specific effects, and, in some cases, the weights can be negative. In this context, there is a risk of negative weights as the panel is unbalanced, effects can be heterogeneous, and group sizes unequal (Goodman-Bacon 2021). Additionally, in case of the lack of parallel trends, the TWFE estimates are likely biased. Given the aforementioned difficulties, I also employed difference-and-difference method a la Callaway and Sant'Anna (2021).

4.3.5 Difference-in-Differences with Multiple Time Periods

The method developed by Callaway and Sant'Anna (2021) (CS-DiD) accommodates multiple time periods, while allowing treatment effects to vary and over time. Specifically, the approach estimates group-time average treatment effects ($\text{ATT}_{g,t}$), defined as the average treatment effect for units first treated in period g, evaluated at time t. In our case, treatment is sharp and therefore there are only two groups: treated and never-treated, but the effects can be heterogeneous with respect to time. Estimation proceeds using doubly robust DiD estimation based on inverse probability weighting and ordinary least squares.
Let Y_{it} denote the outcome for municipality i in year t, and let $G_i = g$ indicate that unit i is first treated in period g. Let $C_i = 0$ indicate that unit i is never treated. The ATT is defined as:

$$\text{ATT}_{g,t} = \mathbb{E}[Y_{it}(1) - Y_{it}(0) \mid G_i = g] \tag{21}$$

The doubly robust estimator is then given by:

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

where $\widehat{\mu}_0(i,t)$ is the predicted untreated outcome obtained from a regression of Y_{it} on unit and time fixed effects using only never-treated units in the pre-treatment period.

With covariates, the group-time average treatment effect is given by:

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

where $\widehat{\mu}_0(X_i,t)$ is the predicted untreated outcome from a regression of Y_{it} on covariates X_i using only never-treated units, and $\widehat{w}_i(X_i)$ are inverse probability weights estimated from a propensity score model for treatment assignment.

In the next section, I present event-study graphs, *honest confidence intervals*, the TWFE, and CS-DiD results. I put part of the event-study graphs according to Callaway and Sant'Anna (2021) in the Appendix.

5 Results

Summary of main results. Ex-capital cities received much larger transfers from the central government than city-counties and county seats, but spent them mostly on salaries rather than investment. In the labor market, the treatment of losing capital status precipitated a fall in employment, especially among women, but it did not cause an immediate increase in emigration. In the economy, ex-capital cities registered a steeper fall of public firms than private ones. Furthermore, around three years after the reform, ex-capitals experienced a relative fall in births.

Results are divided between five sections, showing estimates for municipal finance, labor market, economic activity, migration, demographics, and local public goods. I start with municipal finance outcomes, which are the main channel through which the treatment of losing capital status worked. I follow by describing the outcomes in the labor market, which, together with the economic situation of municipalities, are of the main interest in this analysis. Later, I describe migration and demographics outcomes, where changes might be partly assigned to the impact of the treatment in the labor market. Finally, I present results for the local public good indices, which mirror the fiscal soundness of municipal budgets.

Each section starts with the event study design graphs, giving the intuition about the existing pre-trends. Importantly, I show the results for 1995 to 2008, while the second part shows the aggregated results until 2003. In the event study design, I want to show the period post-joining the European Union in 2004, relative to the 1999 administration reform. The year of joining the EU would be denoted as period 4 in the graphs. *Honest confidence intervals* according to Rambachan and Roth (2023) would complement the event study as a sensitivity analysis.

The second part, estimates from simple TWFE and covariate-adjusted DiD a la Callaway-Sant'Anna, are contained in tables. The rest of the results can be found in the Appendix, as indicated in the text. I show three comparisons:

1. ex-capitals (treated) vs. city counties (control). It shows the impact of status loss

over and above the general institutional and fiscal changes associated with city-county designation. The identifying assumption is that, in the absence of losing capital status, both groups would have followed parallel trends in municipal outcomes.

2. ex-capitals (treated) vs. county seats (control). The effect of losing capital status and gaining city-county status, relative to municipalities that gained county seat status only.

In the section on migration, I also show comparisons between villages and towns in proximity of ex-capitals, city-counties, and always-capitals: the latter being the capitals which have not lost the status of capital and, in the 1999 reform, got additional powers.

5.1 Municipal finance

The main channel through which this reform operated was municipal finance: given the decentralization, more funds went to municipal budgets. The changes in administrative powers also brought some responsibilities to cities, depending on their status. Figures 5, 6, and 7 show the results of the event study for the years 1995-2008, with fixed year and municipality effects. In Figure 5, showing the comparison between ex-capitals (treated) and city counties (control); we can see that the municipal own revenues in PLN per capita were lower in treated municipalities by an average of 100 PLN per capita post-1999 relative to city counties, suggesting relatively weaker economic conditions in places that lost capital status (100 PLN corresponded to 34% of the pre-treatment mean, see Table 2). Conversely, these municipalities experienced much larger relative inflows of funds from the central government, averaging 400 PLN per capita until joining the EU in 2004 (400 PLN corresponded to 131% of the pre-treatment mean). Regarding the expenditure, while parallel trends might not exactly hold, municipal spending on salaries in ex-capital cities was relatively much higher post-1999, averaging 300 PLN per capita (158% of the pre-treatment mean). This, in large part, reflects the changes in public administration: before, salaries for employees of regional administration were paid from the central government, while after 1998 they were paid from municipal budgets. Given that some part of administrative offices have remained in ex-capitals (apart from city-county administration), such as the office for environmental matters, the increase in spending on salaries might reflect this. Interestingly, it seems that most of the inflow from Warsaw was spent on salaries, as there is no change in spending on investment post-reform in treated municipalities.

Figure 6 shows the comparison between ex-capitals and county seats, reflecting the combined effect of losing capital status and gaining the city-county status, the latter being higher in administrative hierarchy than the county seat. The effect shows the relative increase in municipal own and central revenues, the latter being on average 800 PLN per capita, which is almost half of the average monthly salary in 1999. It seems that most of this money was spent on salaries, as there is only a slight and not significant increase from zero in spending on investment post-1998. Finally, Figure 7 shows the comparison between city counties (treated) and county seats (control). Consistent with the results in Figure 6, gaining the status of city-county was linked to an increase in own revenues (although in the latter, the parallel trends do not seem to be satisfied). Also, the increase in revenues from the central government is similar to that for ex-capitals in Figure 5 and smaller than that for ex-capitals

in Figure 6, suggesting that *ex-capitals received more funds from the central government following the reform*. There is no effect on investment.

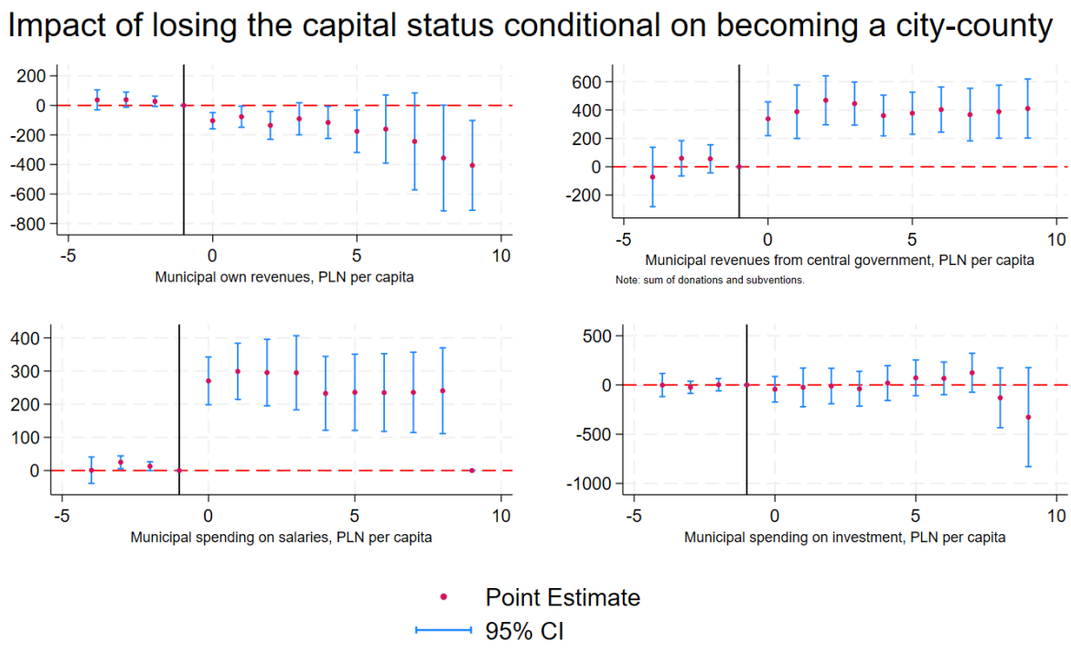


Figure 5: Event study: ex-capitals (treated) vs. city-counties (control)

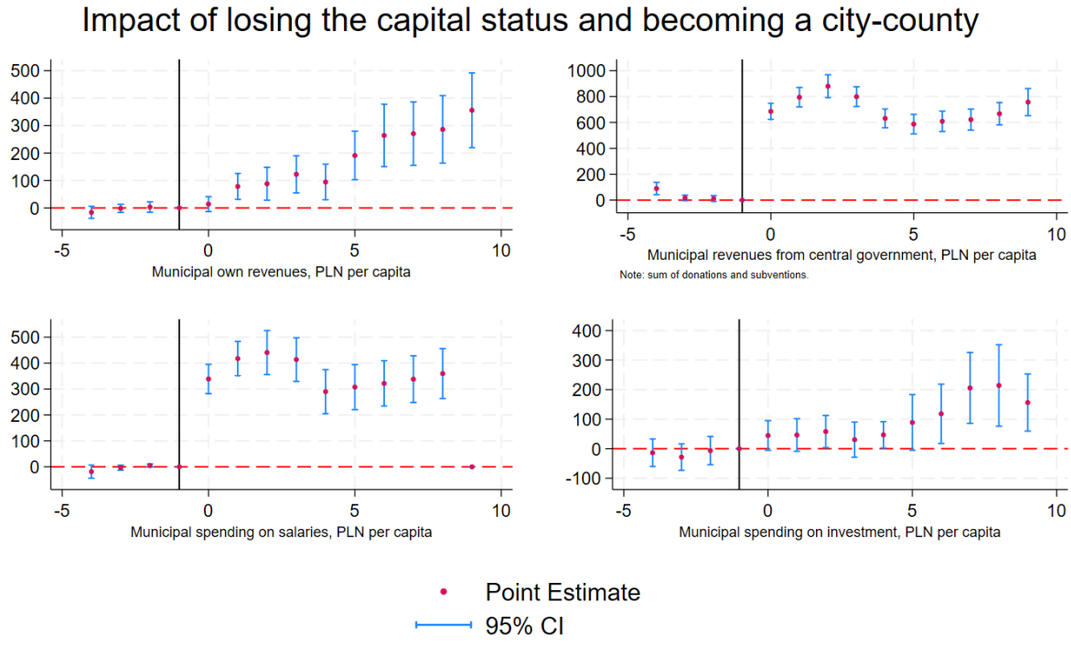


Figure 6: Event study: ex-capitals (treated) vs. county seats (control)

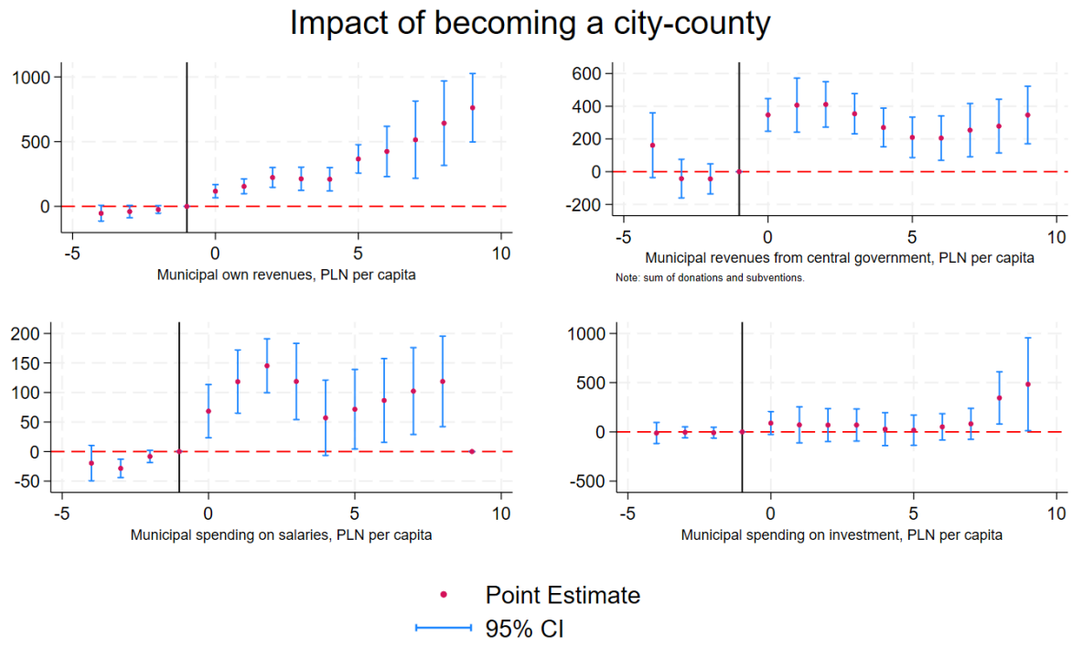


Figure 7: Event study: city-counties (treated) vs. county seats (control)

Relaxing parallel trend assumption In Table 6 I show "honest" difference-in-difference bounds for the first comparison (Figure 5), describing the impact of losing capital status conditional on becoming a city-county (Honest DiD bounds for comparisons presented in Figures 6 and 7 are in Appendix C). Importantly, the 95% confidence intervals presented in Table 6 concern the aggregate outcomes over the period 1999-2003, and not 1999-2008 as in the event studies above, calculated as a simple overage of β_k coefficients from (17):

$$\tau = \frac{1}{5}\sum_{k=0}^{4}\beta_k$$

Parameter M denotes the deviations from the parallel trends before the treatment. "Original" is the standard confidence interval from the event study regression assuming perfect parallel trends. $M = 0$ assumes no trend violations post-treatment, but with the more conservative inference method. It allows some violations of parallel trends before the treatment and is constructed according to the conditional sensitivity model (Rambachan and Roth 2023). $M \in \{0.5, 1, 2\}$ allows for the worst-case change in outcome trends post-treatment to be $M\times$ as large as the biggest observed pre-trend shift (largest change between adjacent pre-treatment event-time coefficients).

We can see in Table 6 that the impact of losing capital status conditional on becoming a city-county on the municipal own revenue is negative and robust to $0.5\times$ violations of parallel trend. The same magnitude of violation for the central government revenues increases the confidence interval to be negative, but to a relatively small degree. The outcomes for municipal spending on salaries are quite robust, as even for $M = 1$ the bounds do not include zero, and the effect is positive. As could be predicted by inspecting Figure 5, there is no effect on municipal investment regardless of the magnitude of M.

Table 6: "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.

According to Table 7 across all comparisons, central transfers emerge as the most responsive fiscal category, with large, statistically significant increases in per capita terms. For example, ex-capitals received an additional 390–437 PLN per capita relative to city-counties, and even higher transfers compared to county seats (729–820 PLN), suggesting a compensatory role of central government funding post-reform. Nevertheless, it appears to be the least robust outcome according to more conservative confidence intervals (Table 6).

Table 7: 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.

In contrast, own revenues declined in ex-capitals relative to city-counties (by 98–136 PLN). These are substantial amounts given that the average monthly salary in Poland in 1999 was 1706.74 PLN. However, as can be seen in Figure 33 in Appendix D showing event-study design according to Callaway and Sant'Anna (2021), there seems to be a falling trend in own revenues for ex-capitals compared to city counties starting before 1999. City-counties, in turn, exhibit significantly higher own revenues than county seats only under TWFE. Spending on salaries mirrors the pattern in central transfers, with large and significant increases in treated municipalities, consistent across specifications. Investment spending, however, appears far less responsive and more volatile, with point estimates generally imprecise and

confidence intervals spanning zero. Overall, while central support and recurrent expenditures (like salaries) increased in treated municipalities, own revenue capacity did not follow uniformly.

5.2 Labor market

In Figure 8 in the ex-capitals vs. city counties comparison, the employment rate begins to decline two years after treatment, but the trends are not parallel. This is driven by male employment, as the trend is fairly similar for males, but not for females. Employment rate among women fell following the reform by more than 4 percentage points by 2002. For the working-age population, the pre-treatment period shows a mild upward drift, which continues after 1999. The ex-capitals vs. county seats comparison, as shown in Figure 9, displays decreases in the employment rate for both males and females following the reform, although parallel trends are not fully satisfied. In Figure 10, in the city-counties vs. county seats comparison, the employment rate drops in 1999, but then recovers. However, the fall post-reform might be a continuation of a trend pre-1999.

Overall, the event studies provide mixed evidence: while certain comparisons (especially describing the impact of losing capital status conditional on becoming a city-county) show patterns consistent with a negative treatment effect, there are violations of parallel trends. The CS-DiD specification presented in Table 10 balances pre-trends using labor market indicators relevant for given groups and variables.

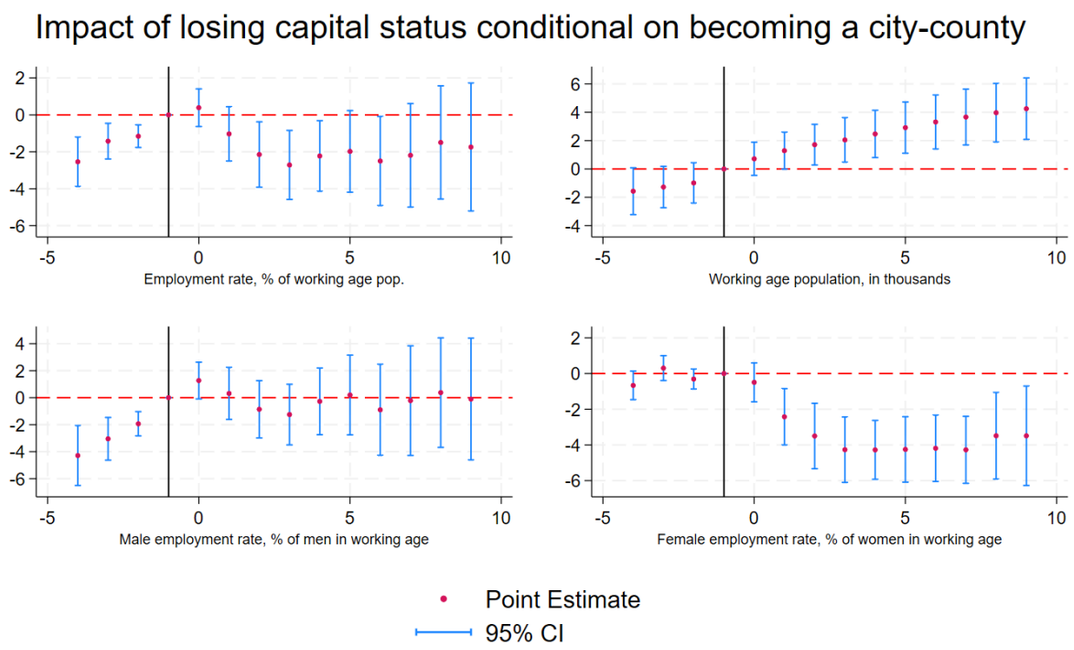


Figure 8: Event study: ex-capitals (treated) vs. city-counties (control)

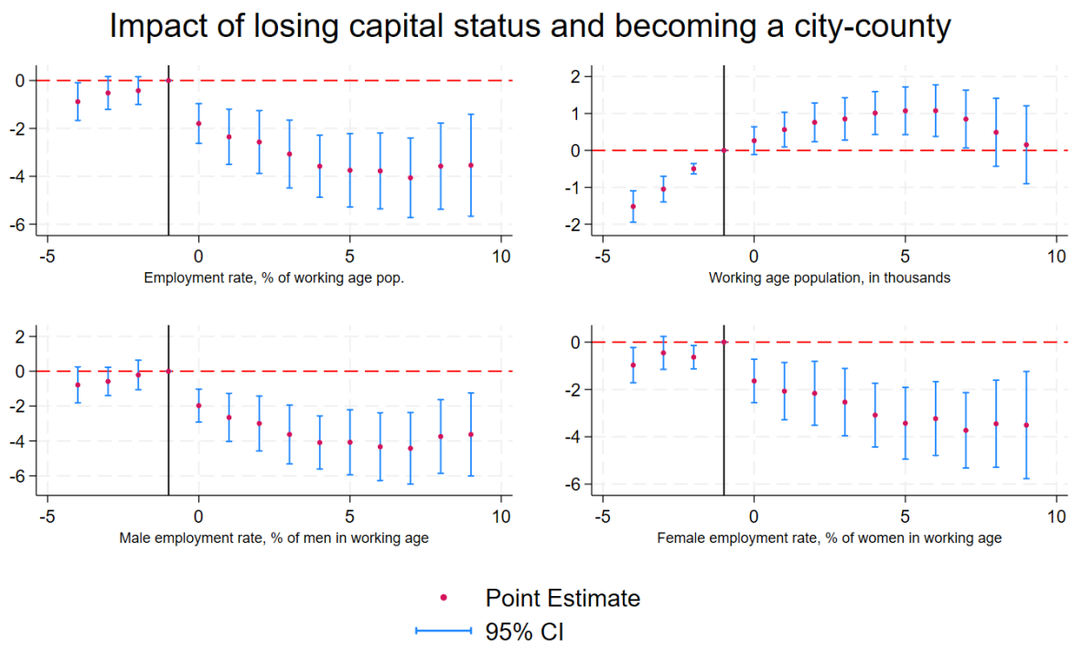


Figure 9: Event study: ex-capitals (treated) vs. county seats (control)

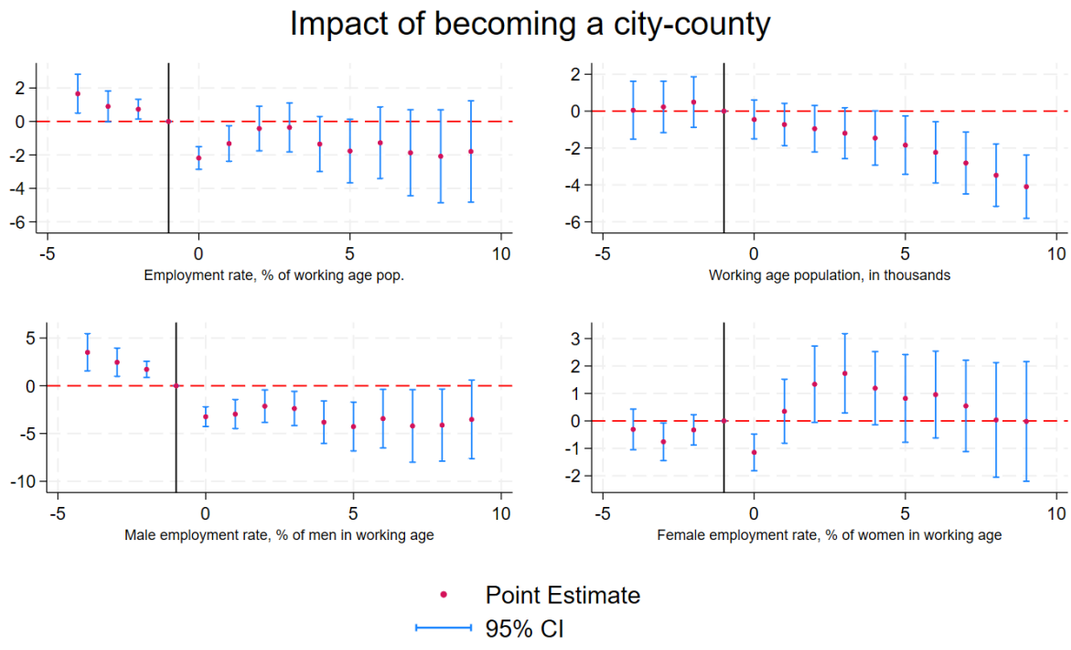


Figure 10: Event study: city-counties (treated) vs. county seats (control)

Relaxing parallel trend assumption According to Table 8, in comparison of ex-capital cities with city-counties, the most robust result is for the female employment rate. The negative effect persists when the violation of parallel trend is magnified by 0.5. While the effect for the male employment rate is null, the overall employment rate and working-age population outcomes are not robust to even the slight violations of the parallel trends.[13] According to Table 9, the impact of losing capital status *and* becoming a city county on the overall employment rate is negative and quite robust - even with the violation of parallel trends of $M = 1$, the impact is negative. The effect is also negative and robust for female and male employment rates. It seems that losing capital status prevails over the hypothetical

[13]Honest DiD estimates for the comparison of city-counties and county-seats are in Appendix C.

positive effect of becoming a city-county. However, as can be seen from Figure 10, becoming a city-county also has a negative effect on employment, contrary to the intuition. It is mostly driven by the falling mining industry in the Silesia region, where most city-counties are located, which I discuss below.

Table 8: "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 9: "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 10 compares employment outcomes across municipalities. If I use in comparison county seats, I adjust regressions additionally by population to put a larger weight on municipalities of similar size: the group of county seats consists of more than 100 municipalities, and a large part of them is relatively small.

Zooming in on the employment rate and the first column, adjusting the regression with firms per capita makes the effect of the reform stronger and significant. However, as can be seen in Figure 36 in Appendix D, parallel trends are not satisfied. The effect is stronger when we compare ex-capitals to county seats, although it might be a continuation of negative trends

from before the reform (Figure 37 in Appendix D). While the TWFE estimate in the third column is negative and statistically significant, after adjusting the regression for firms per capita and population, the effect of reform is null.

Table 10: 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 %

Results for the female employment rate suggest that the reform had a particular, negative effect on the situation of women in affected labor markets. In the first column, while adjusting the regression with the number of firms in traditionally female sectors (hospitality, health, education, services, and administration), the effect becomes larger than in the standard TWFE. Additionally, the event study graph with the group-time estimates (Figure 11) suggests parallel trends hold:

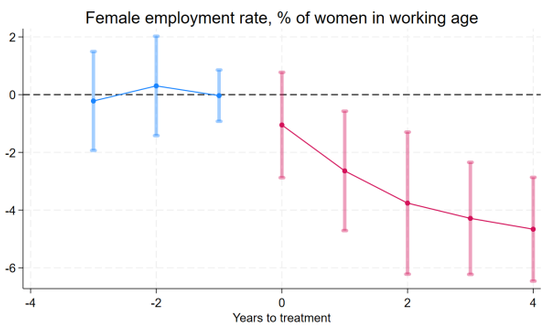


Figure 11: Ex-capitals vs. city-counties: CS-DiD estimates

The negative effect can also be seen in the second column of Table 10, but the effect becomes insignificant in the CS-DiD specification. Interestingly, in the comparison of city-counties and county seats, the coefficients are positive (but become insignificant once adjusted for the

number of firms and population). These estimates suggest that the effect of losing capital status, conditional on becoming a city-county, was negative and significant for the female employment rate. On average, over the period of 1999-2003, the employment among women declined by 3.28 p.p. compared to the city-counties.

Regarding male employment, once adjusted for firms per capita, mining firms per capita, years of local coal mine closure, and fixed effect of being located in the Silesia region (dominated by the mining industry), the effect of reform flips sign from positive and significant to negative and significant. Many coal mines in Poland in the 1990s and 2000s were closed, and the majority of them were located in the Silesia region. After accounting for the worsening economic conditions in this industry, the CS-DiD estimator can isolate the effect of reform, which is negative, but almost half as strong as the effect for women. Additionally, the trends before the reform are not ideally parallel (Figure 12), even after balancing on covariates:

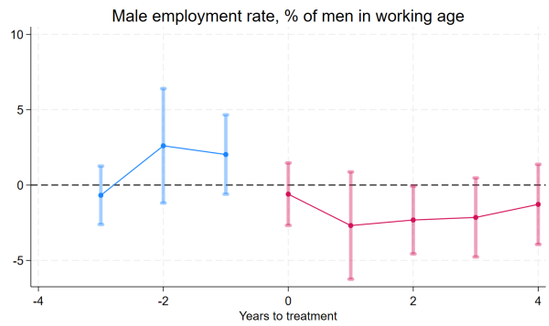


Figure 12: Ex-capitals vs. city-counties: CS-DiD estimates

The effect on the male employment rate is stronger in the comparison of ex-capitals with county seats, but the results are very sensitive to parallel trend assumption (Table 9), which can be seen visually in Figure 37 in Appendix D.[14] Interestingly, in the third column of Table 10, once adjusted for population, number of firms per capita, and mining industry, the effect is null.

Overall, estimates in Table 10 suggest that the reform had a strong and negative effect on employment, especially for women. According to CS-DiD estimate, the decline amounted to 3.28%. Given that, on average, in ex-capitals, there were 32,808 women of working age, this corresponds to the job loss for approximately 1250 women in the years 1999-2003 in a city that lost capital status.

5.3 Economic activity

In this section, I present outcomes for firms per capita, public firms per capita, and household electricity use. The first two outcomes serve as an indicator of local economic conditions. Public firms also include public institutions, and therefore, this indicator gives insight into the size of the public sector. Finally, household electricity use is a proxy of consumers' situation and overall demand.

[14]In the comparison of ex-capitals with county seats, I do not adjust for variables describing the mining industry, as the majority of county seats were not affected by bankrupt coalmines.

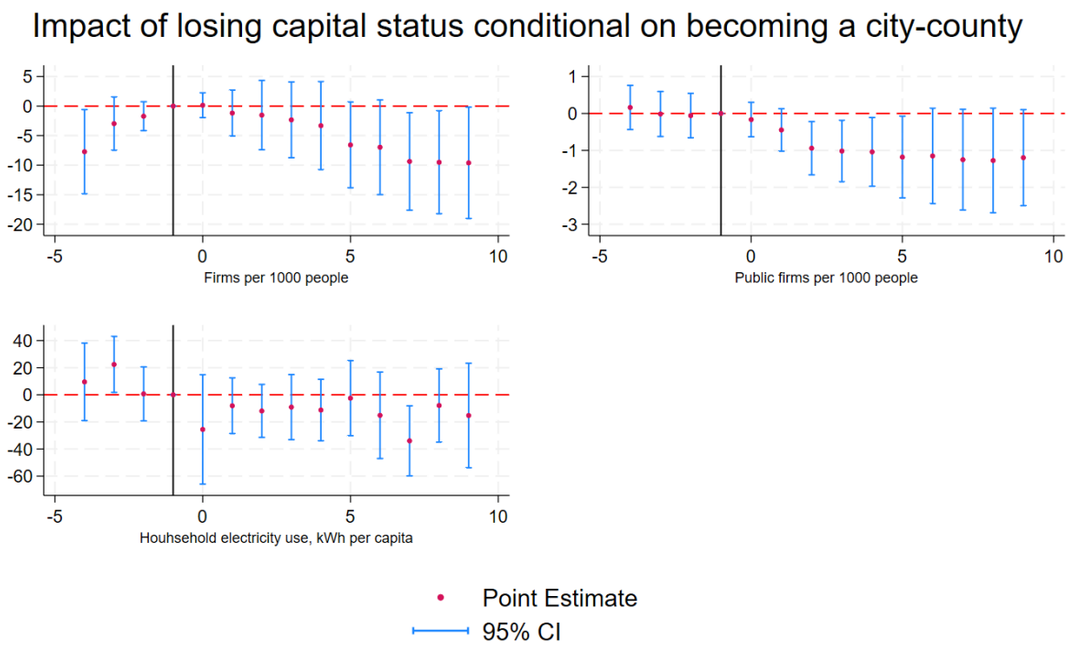


Figure 13: Event study: ex-capitals (treated) vs. city-counties (control)

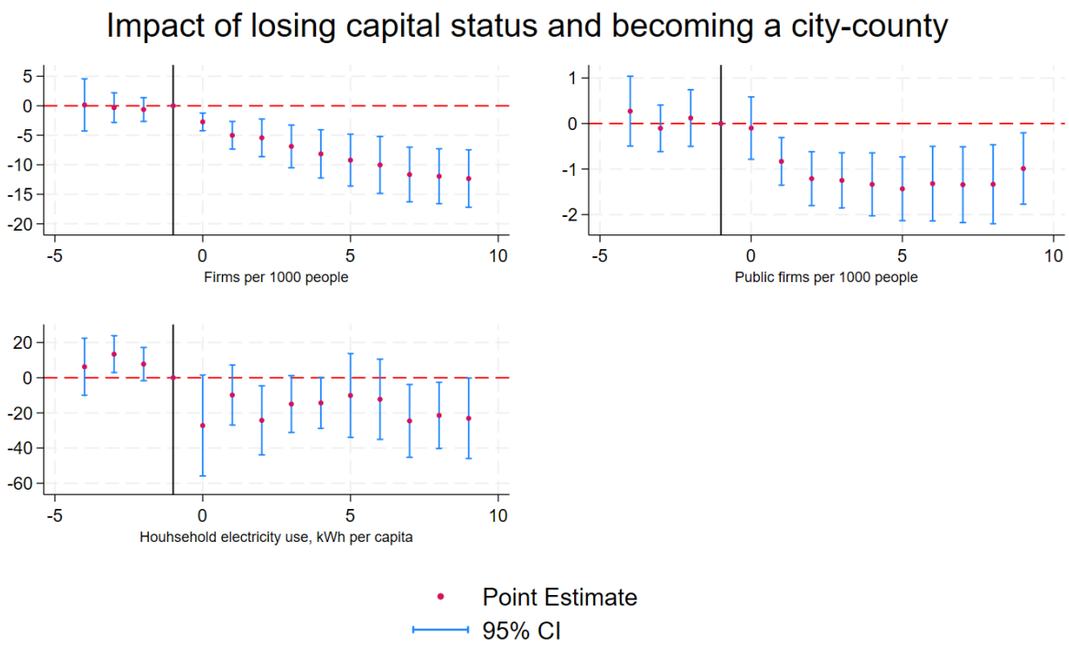


Figure 14: Event study: ex-capitals (treated) vs. county seats (control)

According to the event study in Figure 13, losing capital status did not translate into a fall in firms compared to city-counties. However, the number of public firms per capita fell relatively to city-counties, suggesting administration cuts. In this comparison, there is no effect on electricity use by households. However, if we compare ex-capitals to county seats in Figure 14, we can see a substantial decline in firms per capita after the treatment, as well as public firms. There is also a fall in household electricity use, although with dubious trends before 1999.

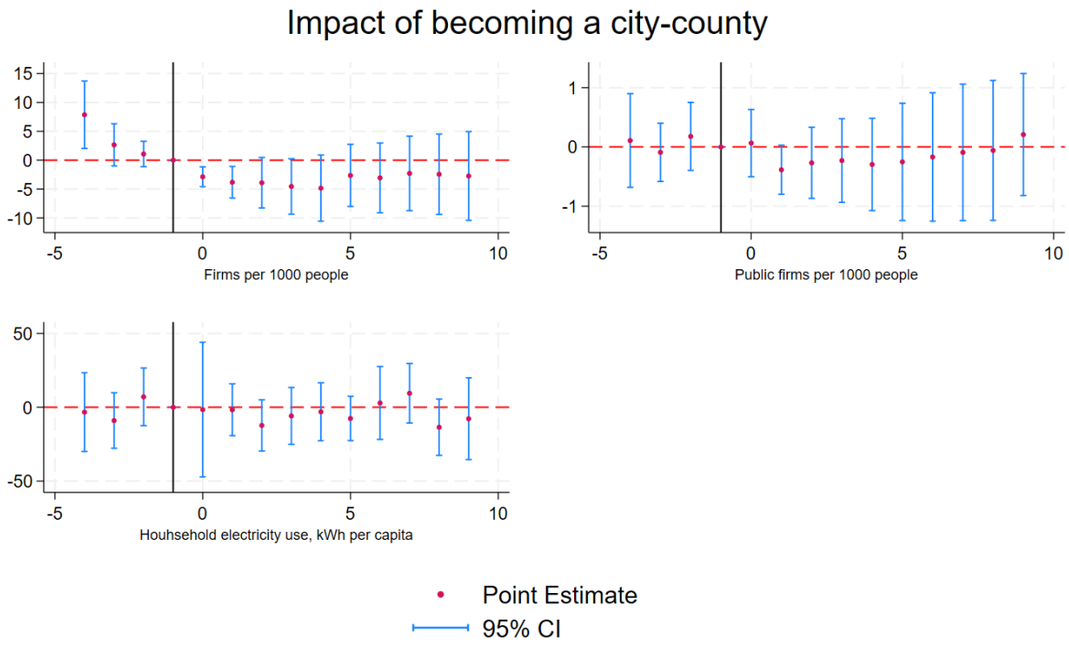


Figure 15: Event study: city-counties (treated) vs. county seats (control)

Comparing city counties with county seats in Figure 15, we can see a fall in firms per capita post-reform, although it might be a continuation of a trend from before 1999. There is a relative fall in the number of public firms in 2000, although the effect is not strong and diminishes one year later. From the demand side, there is no effect on the use of electricity.

Relaxing parallel trend assumption. As we can see from Table 11, the impact of losing capital status conditional on becoming a city-county on the number of public firms per 1,000 people does not seem robust if the trend deviation was up to 0.5× the largest pre-treatment trend. According to Table 22 in Appendix C, showing bounds to the comparison of ex-capitals to county seats, the outcome for public firms is not robust either.

Table 11: "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 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 12 shows TWFE and CS-DiD estimates for all three comparisons of municipalities. Comparing ex-capitals both to city-counties and county-seats leads to a negative and statistically significant coefficient, reaching even -17.46 firms per capita if we compare ex-capitals to city-counties and adjust for the fixed effect of being in the Silesia region (although the latter is sensitive to the parallel trend assumption, see Figure 39 in Appendix D). The adjusted regression comparing city-counties and county seats shows a positive and insignificant effect of reform on firms per capita. Given that the coefficient is statistically significant and negative in comparison with ex-capitals and county seats (with parallel trends, see Figure 16 showing event study according to Callaway and Sant'Anna 2021), these results suggest that losing capital status had a negative effect on the number of firms per capita, and without city-county status, the fall could have been even steeper.

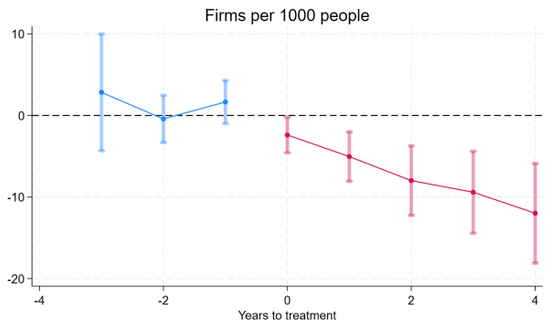


Figure 16: Ex-capitals vs. County seats

The middle panel of results in Table 12 shows the effect on the relative number of public firms adjusted by population. Interestingly, the adjusted regression shows that, on average, 2.47 fewer firms per 1000 people in ex-capitals compared to city counties, a credible conclusion given the lack of pretrends on Figure 17:

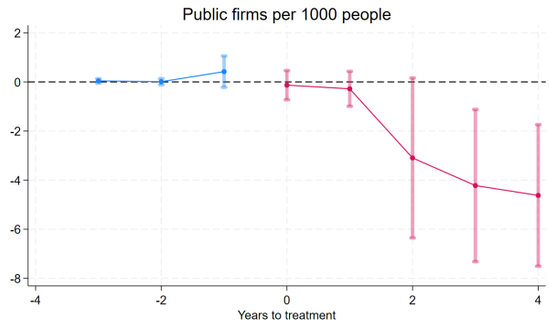


Figure 17: Ex-capitals vs. city-counties

There is also a null effect when compared to county seats (the coefficient is -0.43 and insignificant). The CS-DiD regression also shows a positive impact of getting an administrative upgrade on the number of public firms in the comparison of city-counties and county seats. Regarding the use of electricity, the adjusted regressions show insignificant results: in terms of energy use, there was no impact of the reform. Overall, results from the event study in Figures 13, 14, and 15 and in Table 12 suggest that losing capital status had a stronger negative effect on the size of public administration than the positive effect of becoming a city-county.

5.4 Migration

According to the event study in Figure 18, ex-capitals did not experience an immediate outflow of population as compared to city counties. The coefficients on net migration become negative and statistically significant in 2002, and register a dip in 2004, the year of joining the EU. However, compared to county seats (Figure 19), there is a negative effect on migration following the reform, although not steep. A comparison of net migration between city counties and county seats is inconclusive, as pre-trends do not appear to be parallel.

If administration employees from ex-capitals had been transferred to another capital post-reform, we should observe a significantly higher emigration to cities. However, according to Figure 18, the event-study does not show a positive effect, and in Figure 19, there is no evidence of immediate exodus to other cities. There is neither a significant outflow of population abroad. For ex-capitals, as compared to city-counties (Figure 19), it slightly increases at the time of joining the European Union.

Despite the fact that women were mostly touched by the negative labor shock of losing capital status, according to Figures 18 and 19, men were on average more likely to migrate than women.

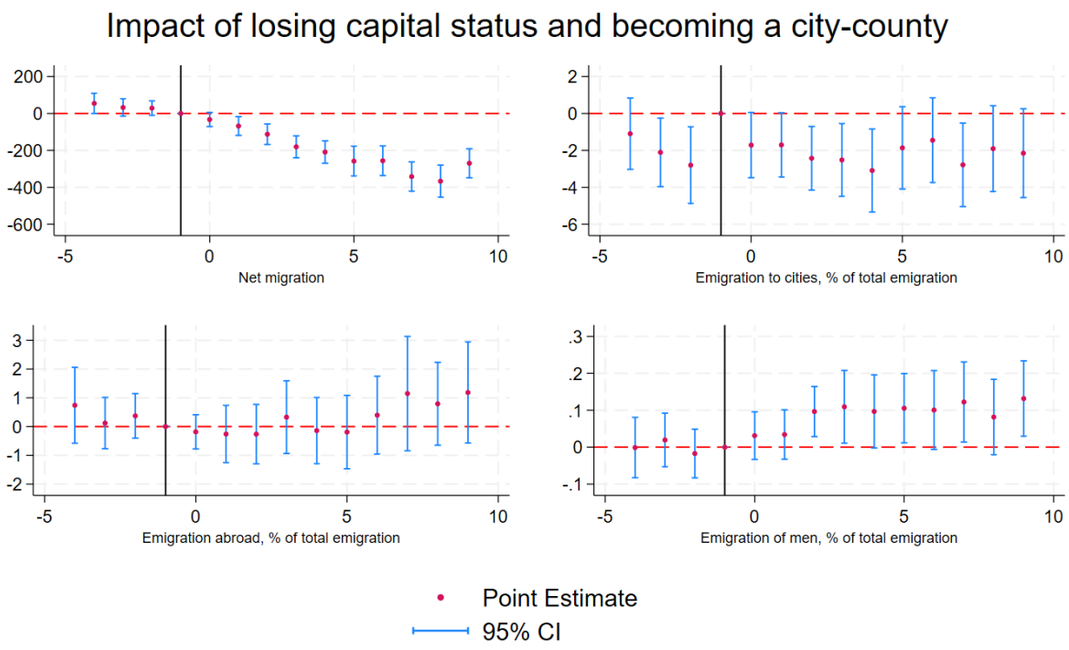


Figure 19: Event study: ex-capitals (treated) vs. county-seats (control)

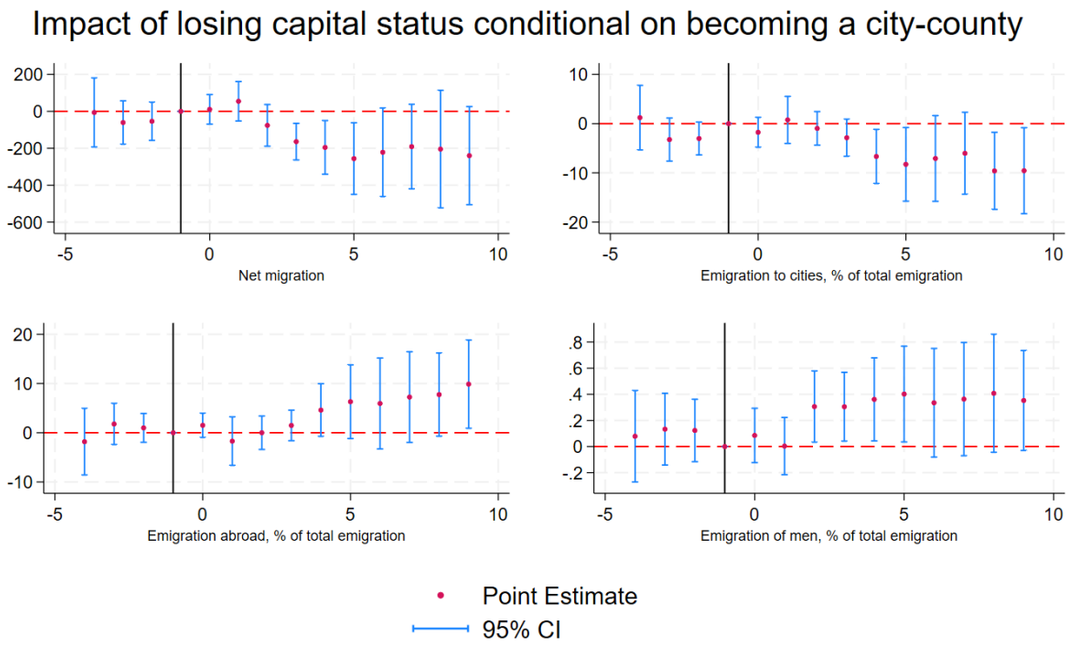


Figure 18: Event study: ex-capitals (treated) vs. city-counties (control)

If we compare city counties and county seats (Figure 20), we can see different patterns in migration. From the event study, it seems that women were more likely to migrate from city-counties than males, and rather to another Polish city than abroad.

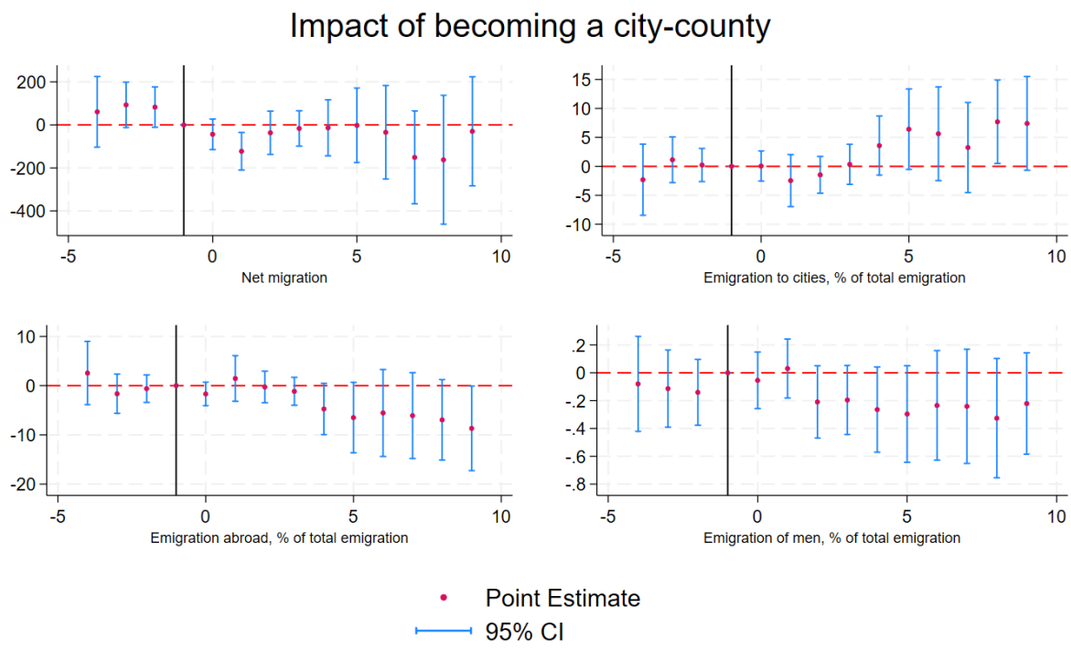


Figure 20: Event study: city-counties (treated) vs. county seats (controls)

Relaxing parallel trend assumption. Table 13 shows confidence intervals when a parallel trend assumption is relaxed for the comparison of ex-capitals to city-counties. In the case of migration outcomes, outcomes are sensitive to this assumption, as for all the outcomes, the bounds contain zero once $M > 0$.

Table 13: "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.

Results for net migration in Table 14 show that there is no effect of losing capital status on net migration even after adjusting for firms per capita and distance to a remaining regional capital.[15]

[15]If a municipality is located relatively close to a remaining regional capital, people could look for work there without moving out of their hometowns.

Table 14: 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.

However, the combined effect of losing capital status and gaining a city-county status, as compared to municipalities only with the county seat status, is negative and significant. Also, adjusted trends appear to be parallel (Figure 21). However, there is no steep decline after the reform, suggesting the confounding effect of joining the EU:

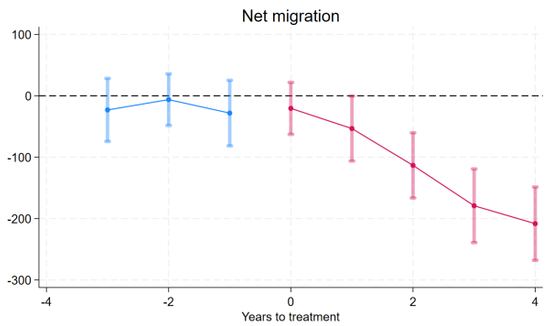


Figure 21: Ex-capitals vs. county seats

The results in Table 14 for emigration to cities support hypotheses from the event-study. Also, there is no significant emigration abroad. Male emigration seems to be larger than female emigration in ex-capitals, while not in the comparison between city-counties and county seats. However, these results should be taken with caution, as they are not robust to the violation of the pre-trends (Table 13).

5.4.1 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 22 presents the event study for villages within a 20km radius from ex-capitals (treated) and from city-counties (control).

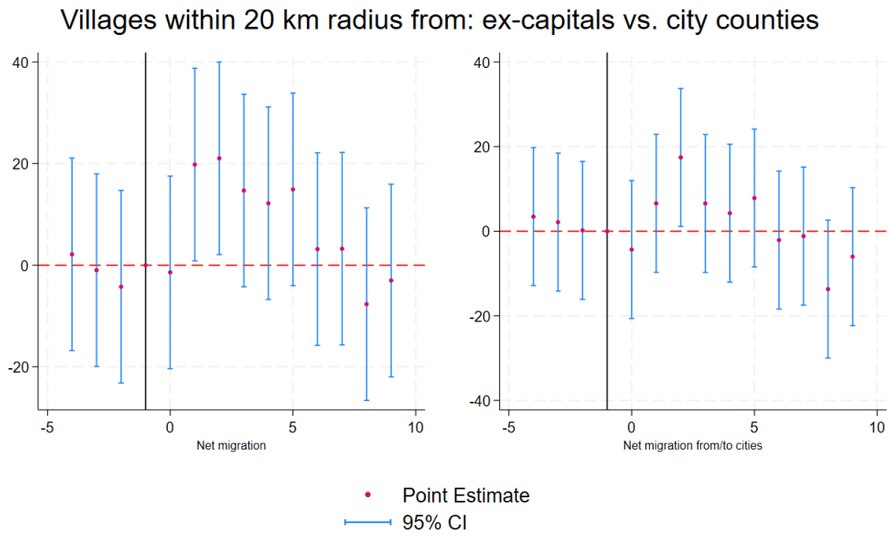


Figure 22: Event study: migration

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.

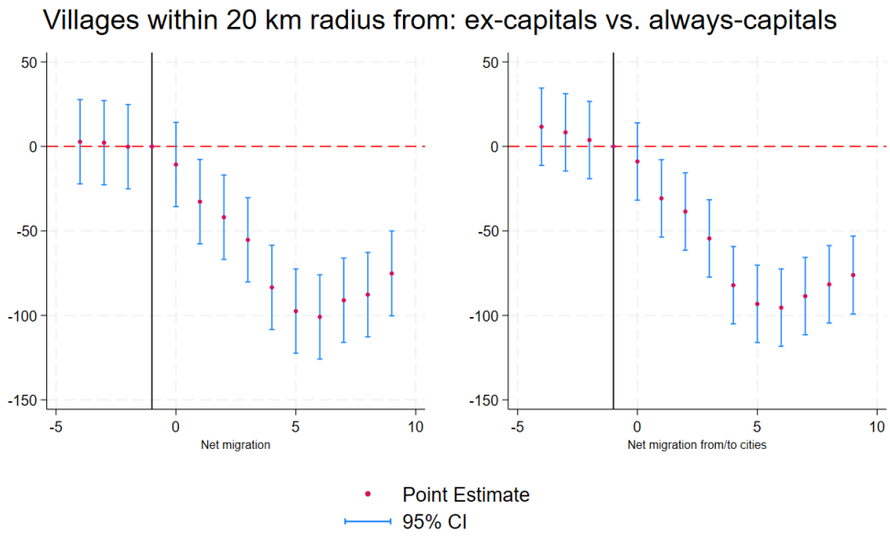


Figure 23: Event study: migration

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.

5.5 Demographics

Losing a job can also alter decisions towards forming a family. In this section, we focus on outcomes such as births, marriages, and the age dependency ratio to get an intuition about demographic changes following the reform in 1999.

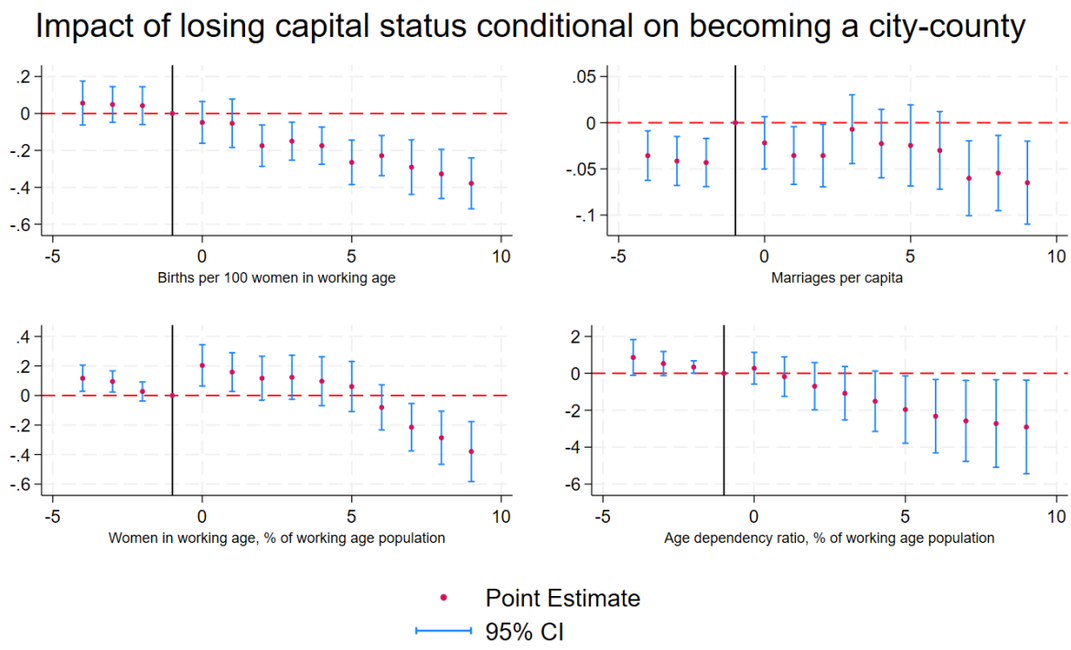


Figure 24: Event study: ex-capitals (treated) vs. city-counties (control)

Figure 24 shows an event study comparing ex-capitals with city counties. We can see that in the third year following the reform, there were significantly fewer births as a share of women of working age, suggesting that worse employment outcomes for women translated into decisions not to expand families. We can also see that there is a relative increase in the share of women in the working-age population, but parallel trends might not hold. However, this result is consistent with the outflow of men, as seen in the results on migration.

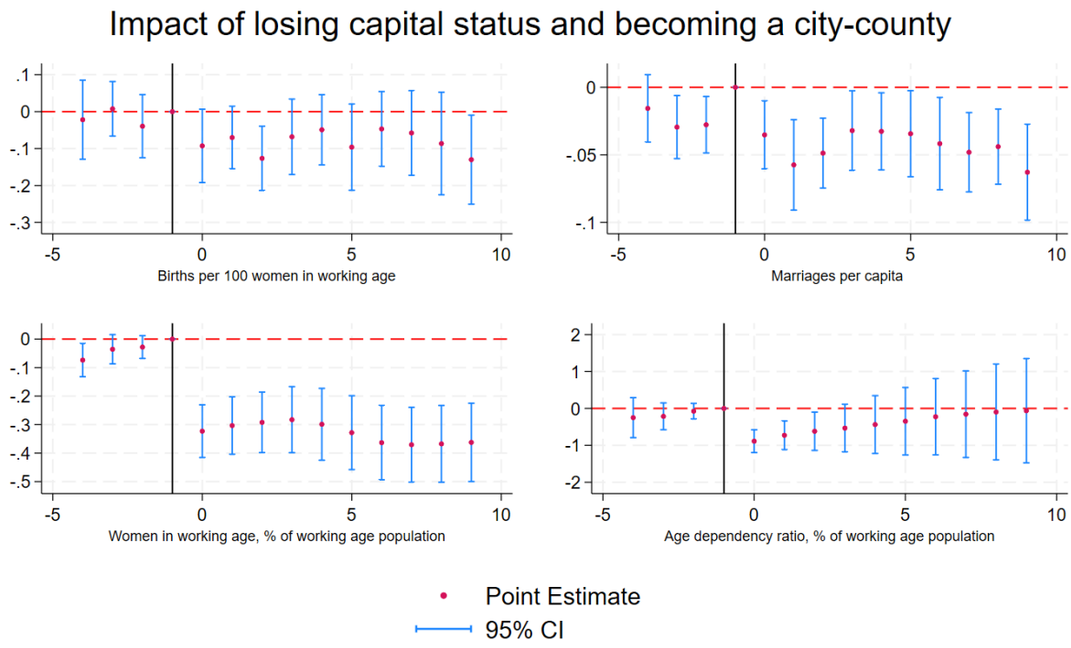


Figure 25: Event-study: ex-capitals (treated) vs. county seats (controls)

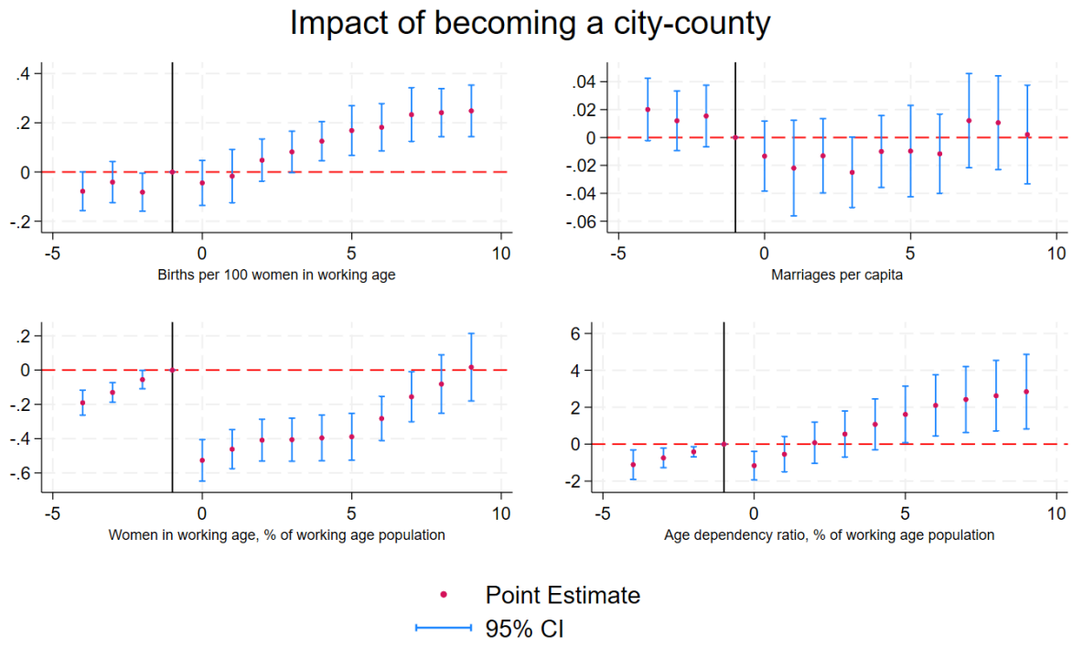


Figure 26: Event study: city-counties (treated) vs. county seats (controls)

If we compare ex-capitals to county seats in Figure 25, we can also see a significant fall in births following the reform. Interestingly, there is also a relative fall in the share of women in the working age population, a falling age dependency ratio, suggesting a growing inactive population in the working age.

Relaxing parallel trend assumption The bounds of the impact on losing capital status, conditional on becoming a city-county, on the births per 100 women of working age are between -1.71 and 0.35, suggesting weak robustness of the results. The effect for the rest of the outcomes is not significantly different from zero even when assuming parallel trends.[16]

Table 15: "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.

[16] Table 24 and Table 25 for the remaining comparisons are in Appendix C.

Table 16: 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 %.

The top panel in Table 16 confirms a negative effect of reform on fertility. Also, the event study according to Callaway and Sant'Anna (2021) (Figure 27) suggests a relative fall in fertility in the third year in ex-capitals following the reform:

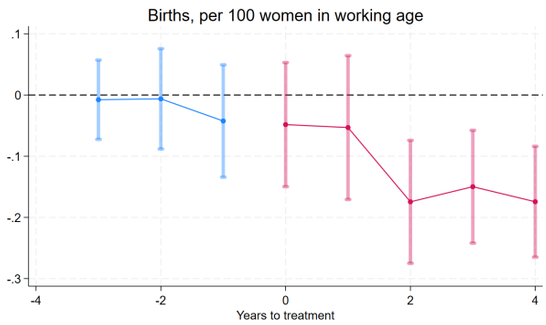


Figure 27: Ex-capitals vs. city counties

While coefficients on marriages are mostly negative, the pre-trends in the relevant event studies a la Callaway and Sant'Anna (2021) (Figures 45, 46, and 47 in Appendix D) are not parallel, invalidating the inference. Depending on the comparison, the share of women in the working age is negative or positive, suggesting that an imbalance between the number of men and women might not have been the principal reason behind the fall in births.

5.6 Public Goods

Finally, I show results for the local public goods. I construct three simple indices, describing: education, health/family, and public transport. The education index consists of the number of places in kindergartens, the number of books, and the number of public educational firms/institutions. The healthcare and family index consists of the number of creches, doctors, dentists, and public healthcare firms/institutions. The transport index consists of bus lines and public transport firms. All indices are z-scores of the simple averages of the respective variables in per capita terms.

According to the event study presented in Figure 28, ex-capitals reported relatively worse local public goods following the reform, although it might be the continuation of the trend from before the treatment. There is no impact on healthcare and family institutions, relative city-counties. Local public transport shows a negative and persistent effect, but without parallel trends.

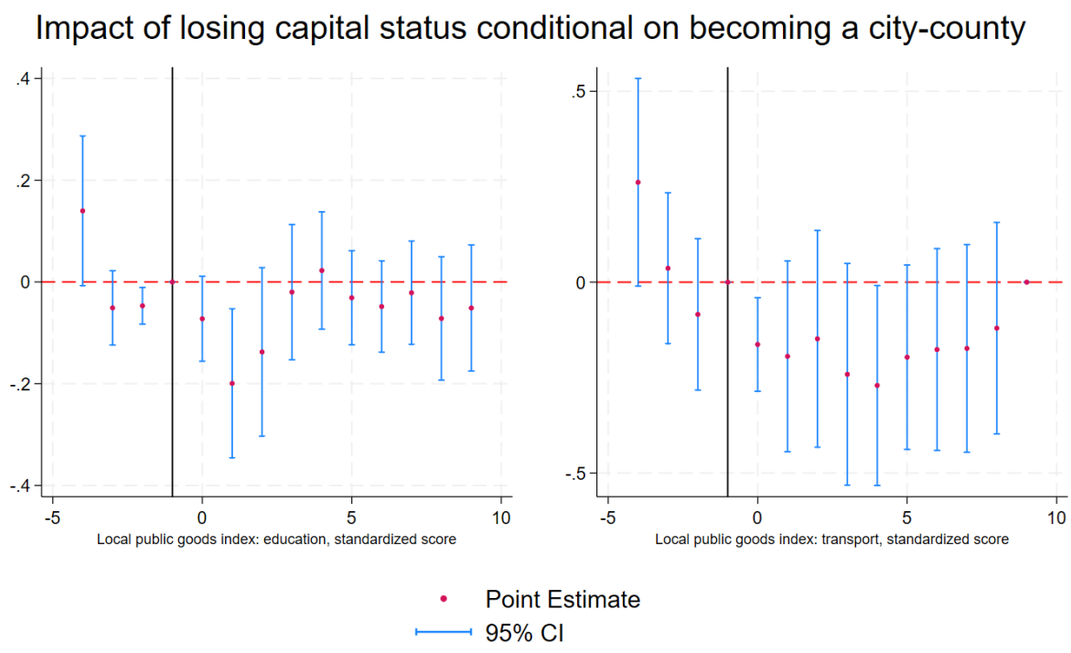


Figure 28: Event study: ex-capitals (treated) vs. city-counties (controls)

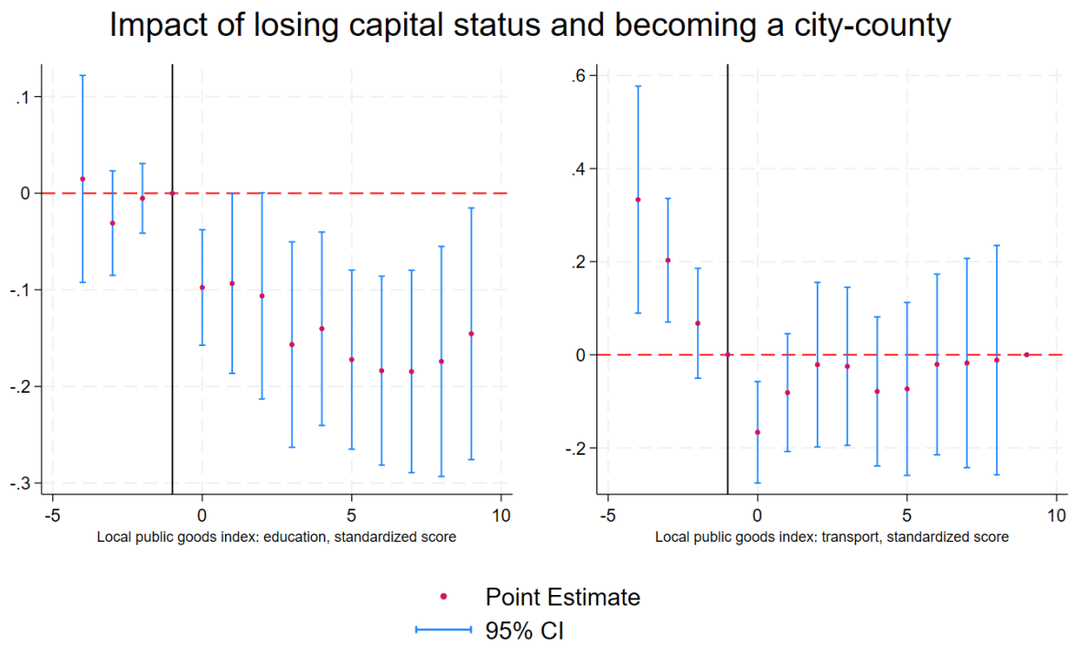


Figure 29: Event study: ex-capitals (treated) vs. county-seats (controls)

In Figure 29, in the comparisons with the county seats, there are no results for the health/family index due to the lack of data. In the comparison of ex-capitals with the county seats, there is a persistent fall in the education index. The local public transport index shows visible pre-trends. In the comparison of city-counties to county seats (Figure 30) the education index falls at the time of joining the European Union, suggesting a diverging distribution of

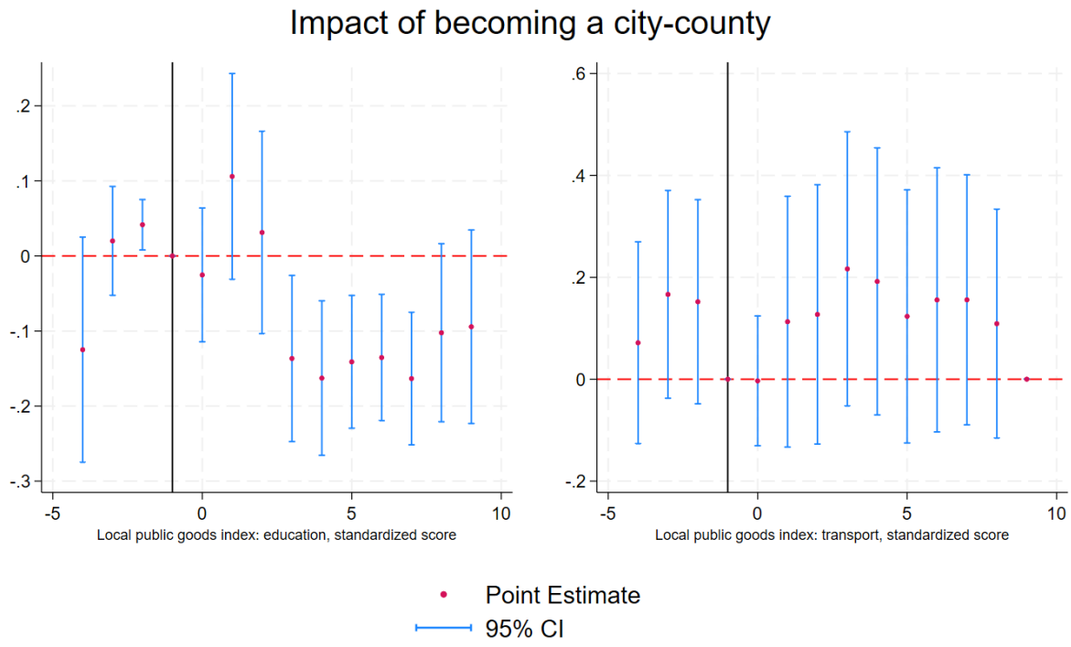


Figure 30: Event study: city-counties (treated) vs. county-seats (controls)

EU funds. As the coefficients for the education index following the treatment in Figure 30 are positive or not different from zero, the negative effects in Figures 29 and 30 suggest a strong impact of losing capital status on the degradation of the education institutions.

Relaxing parallel trend assumption Table 17 shows confidence intervals for education and transport indices for all comparisons. Sensitivity to the parallel trend violations for all outcomes and comparisons is a serious issue, as no aggregate coefficient is significantly below or above zero in any comparison.

Table 17: "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 18: 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.

After accounting for heterogeneous effect over time in the CS-DiD estimator, the effect of reform on the education index is negative and significant, as compared to city-counties (Table 18). However, the trends before 1999 are not parallel (Figure 48 in Appendix D). There is also a negative impact on the healthcare/family index, but trends also do not seem to be parallel, as shown in Figure 31:

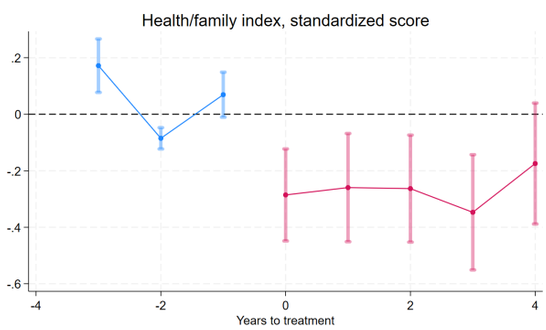


Figure 31: CS-DiD estimates: ex-capitals vs. city counties.

There is a negative effect on the local public transport index, but without parallel trends (Figure 48).

Overall, while the results for the local public goods indices suggest an overall negative impact of the reform, the lack of parallel trends undermines the validity of these estimates as causal.

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, with particularly strong effects on women's labor force participation. This likely reflects the concentration of women in public administration jobs, and supports the idea that institutional downgrades trigger 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.

Third, there is evidence of demographic decline in the affected cities: birth rates as a ratio of women of working age fell significantly. Migration effects, by contrast, were limited and slow to materialize, 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 faced sustained losses in fiscal capacity, employment, and demographic vitality, despite partial fiscal compensation and 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 the long-run erosion of local labor markets and demographic stability.

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

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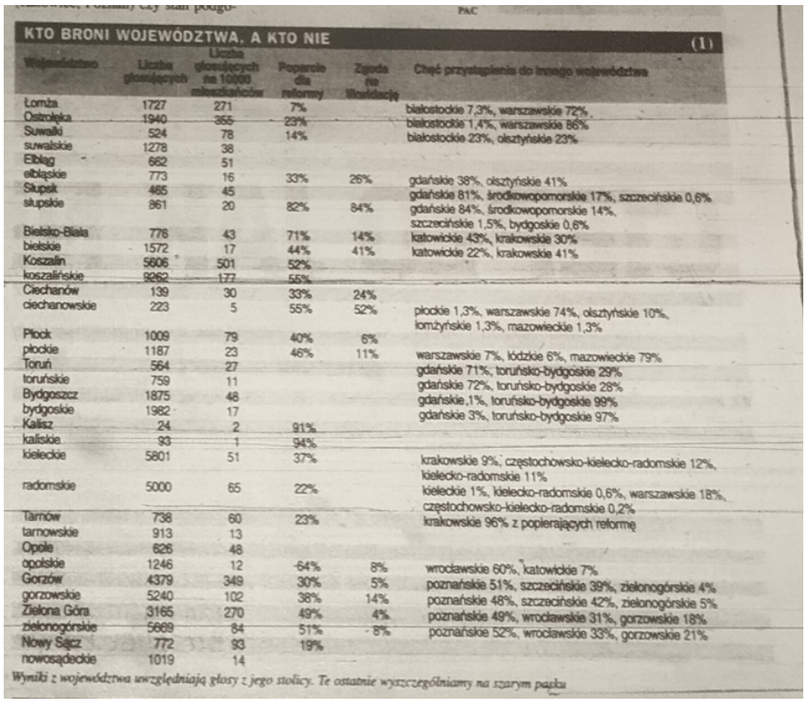
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A



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

B

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

Municipal finance

Table 19: "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 20: "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 21: "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 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 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 25: "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 26: "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 27: "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.

D

Municipal finance

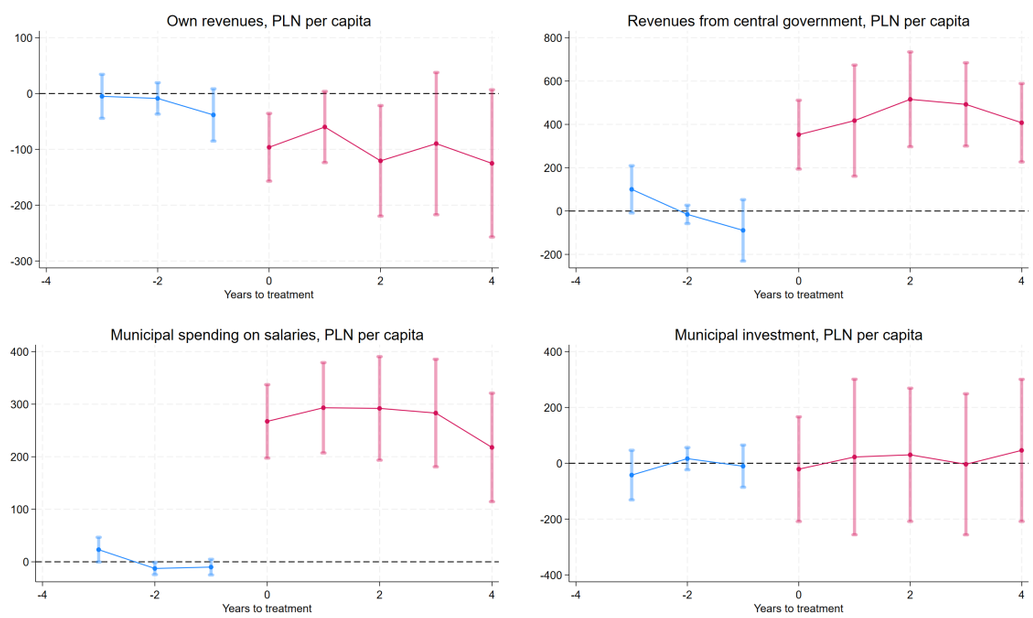


Figure 33: CS-DiD: ex-capitals vs. city counties

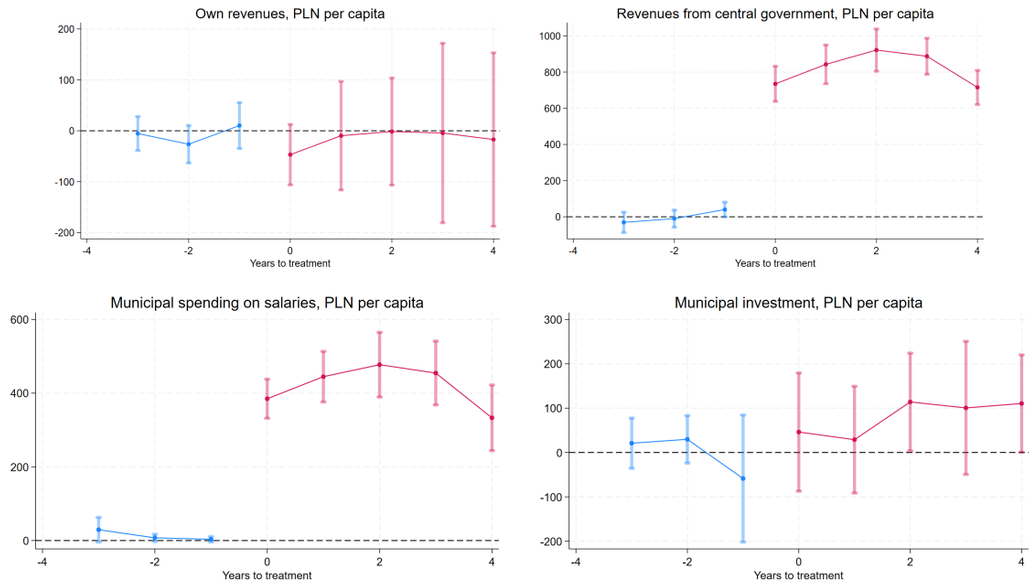


Figure 34: CS-DiD: ex-capitals vs. county seats

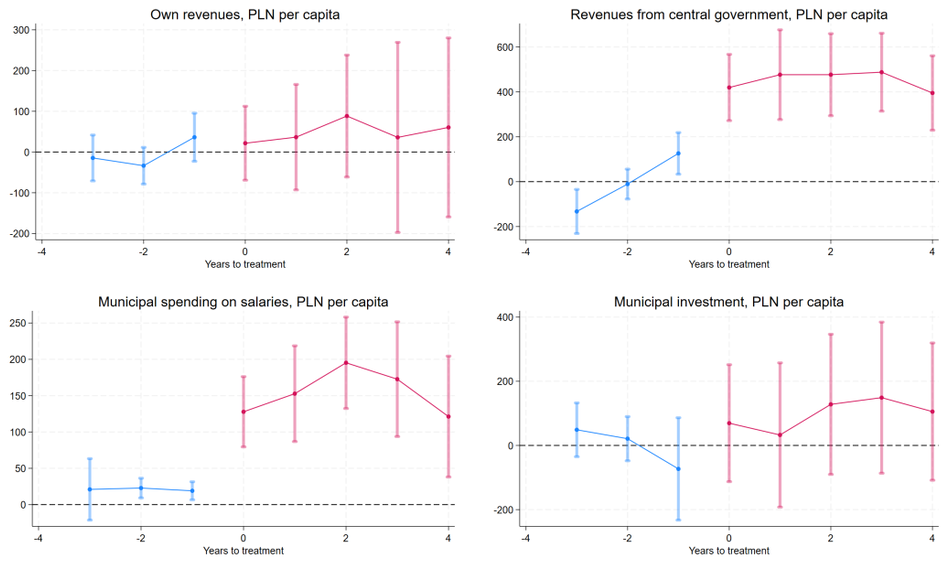


Figure 35: CS-DiD: city-counties vs. county seats

Labor market

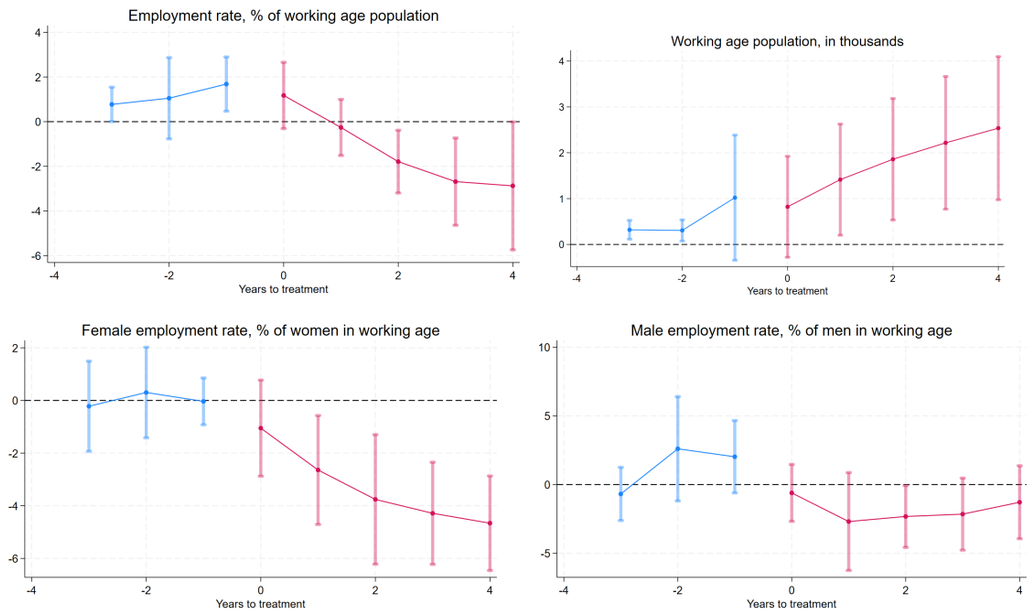


Figure 36: CS-DiD: ex-capitals vs. city-counties

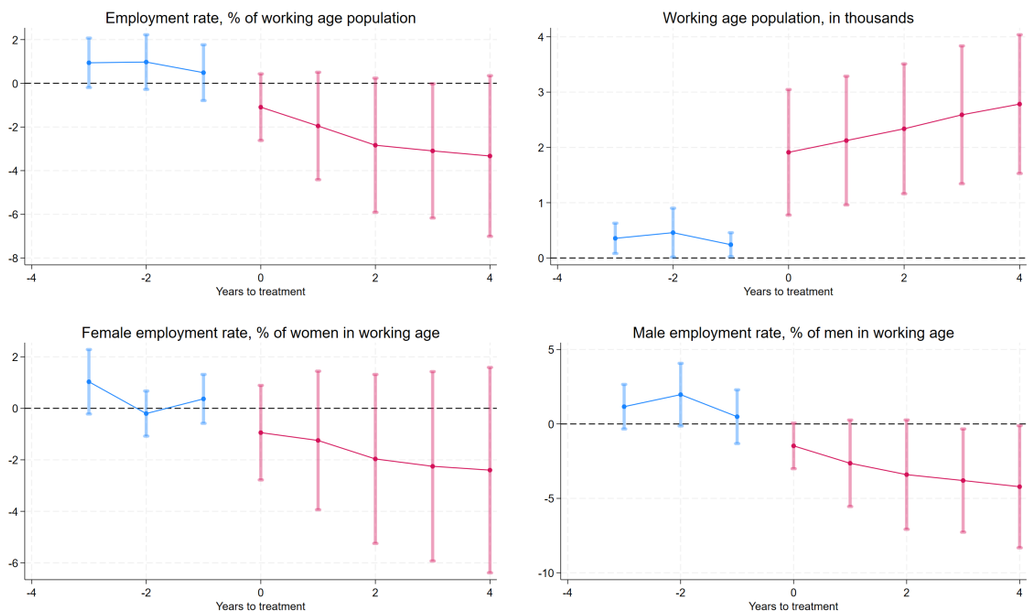


Figure 37: CS-DiD: ex-capitals vs. county seats

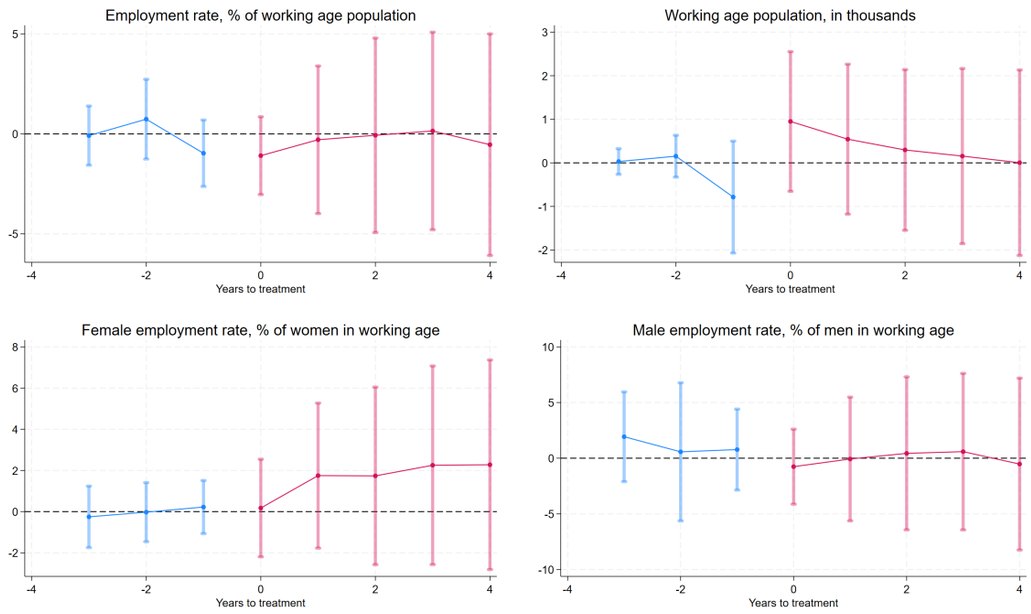


Figure 38: CS-DiD: city counties vs. county seats

Economic Activity

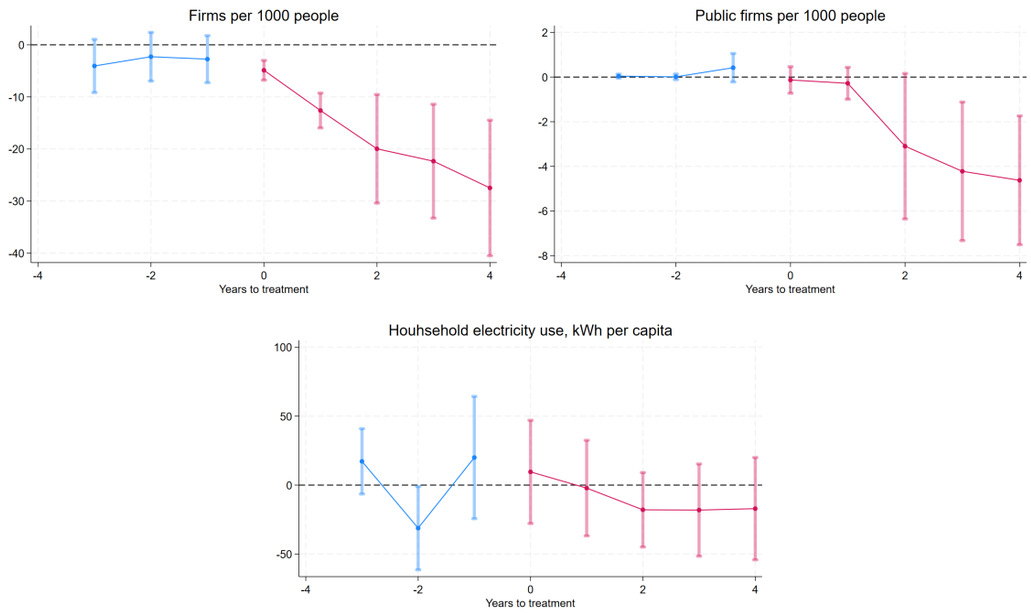


Figure 39: CS-DiD: ex-capitals vs. city-counties

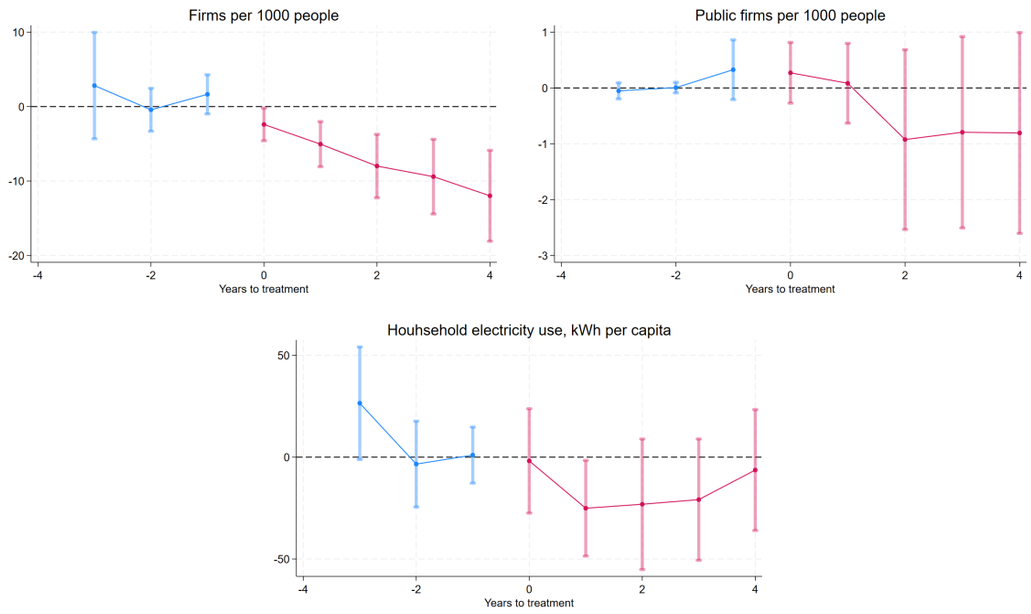


Figure 40: CS-DiD: ex-capitals vs. county seats

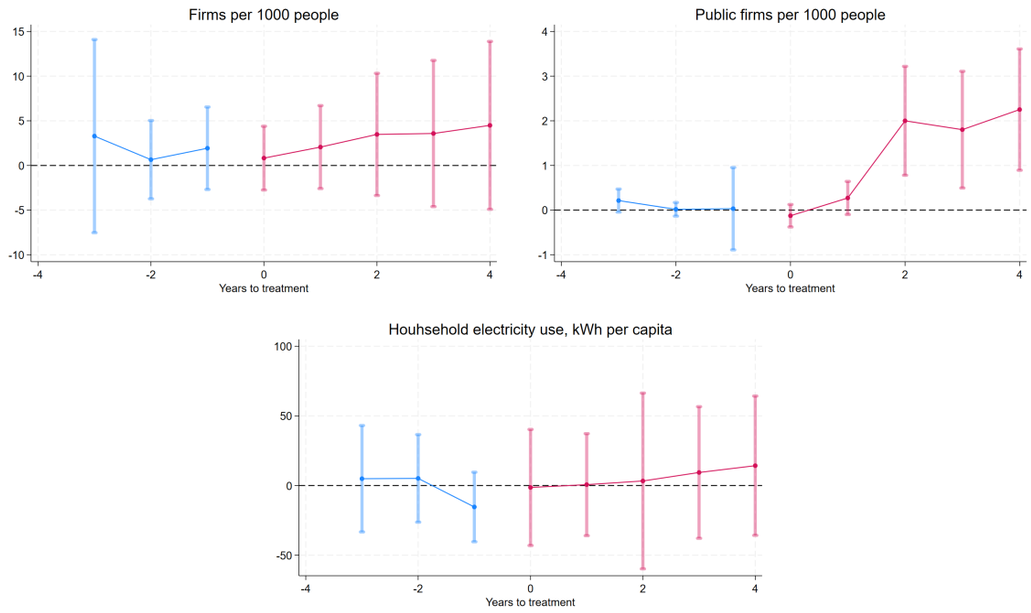


Figure 41: CS-DiD: city-counties vs. county seats

Migration

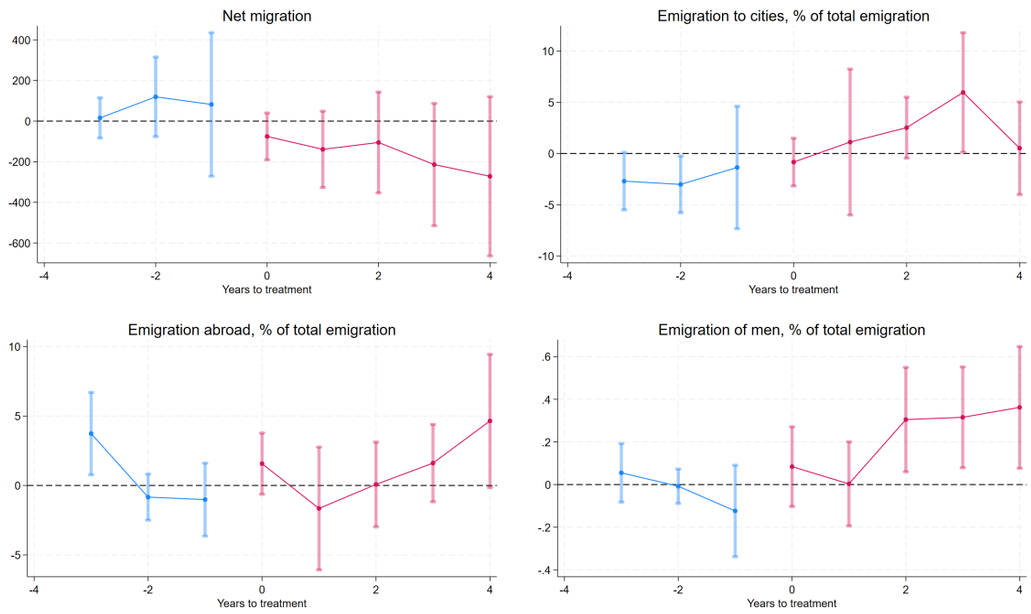


Figure 42: CS-DiD: ex-capitals vs. city-counties

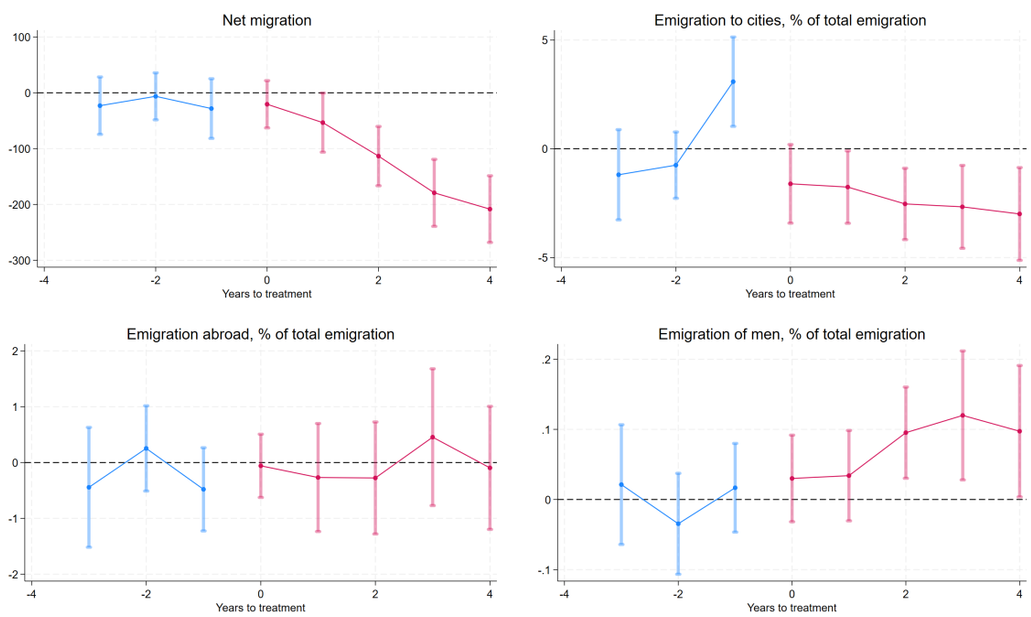


Figure 43: CS-DiD: ex-capitals vs. county seats

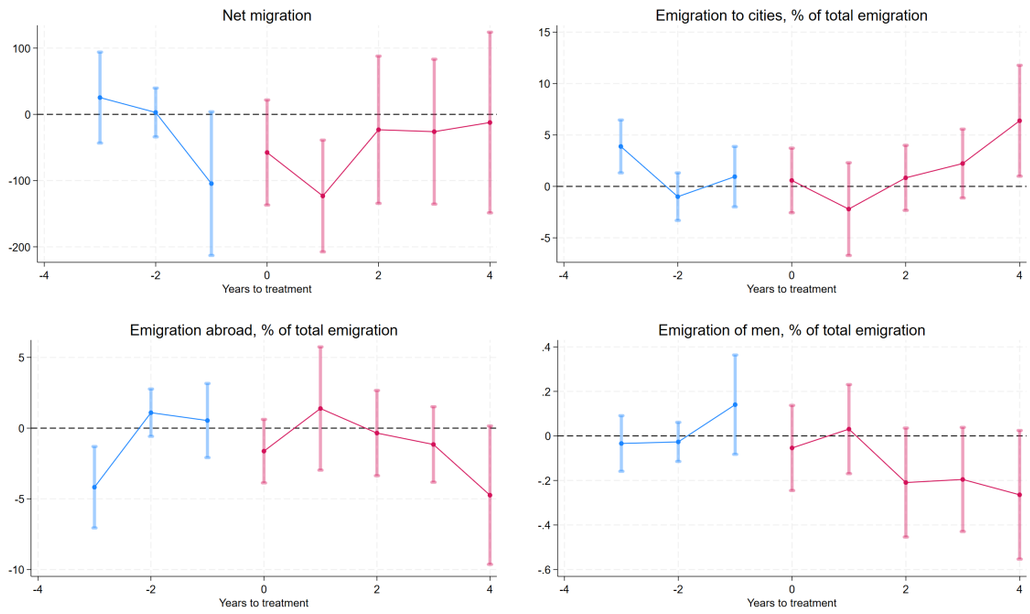


Figure 44: CS-DiD: city-counties vs. county seats

Demographics

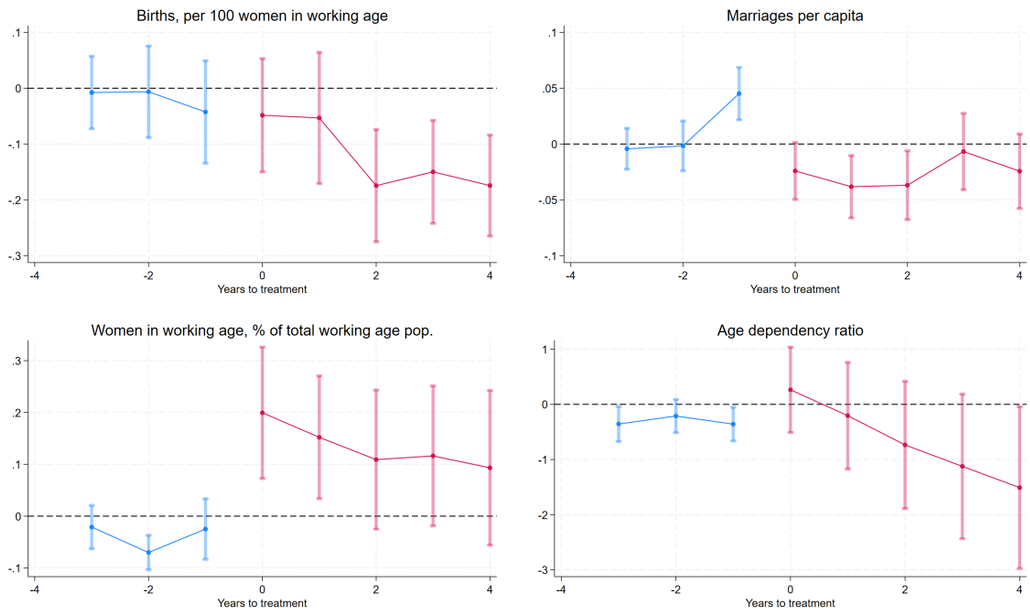


Figure 45: CS-DiD: ex-capitals vs. city-counties

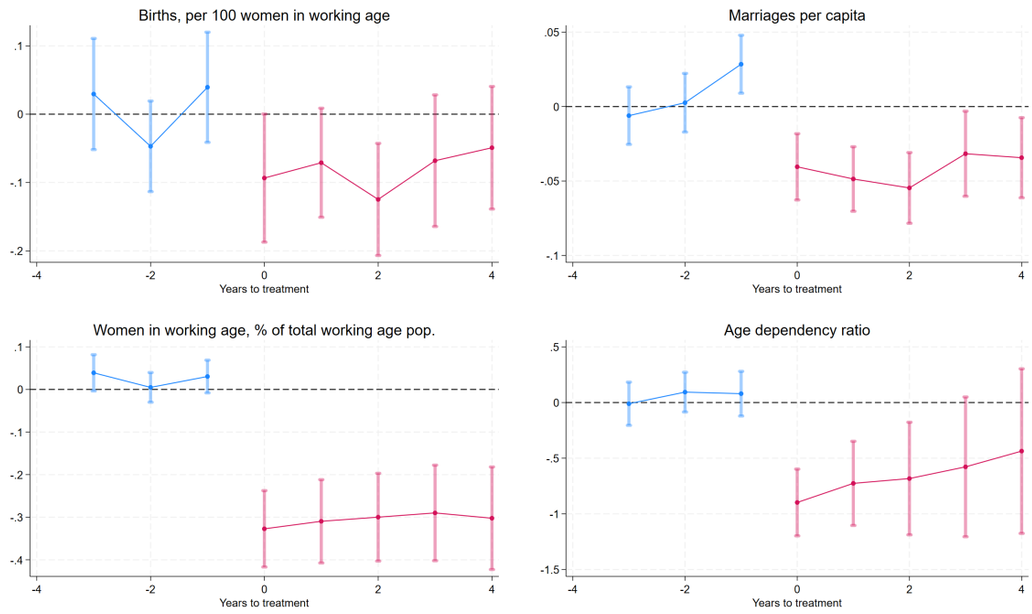


Figure 46: CS-DiD: ex-capitals vs. county seats

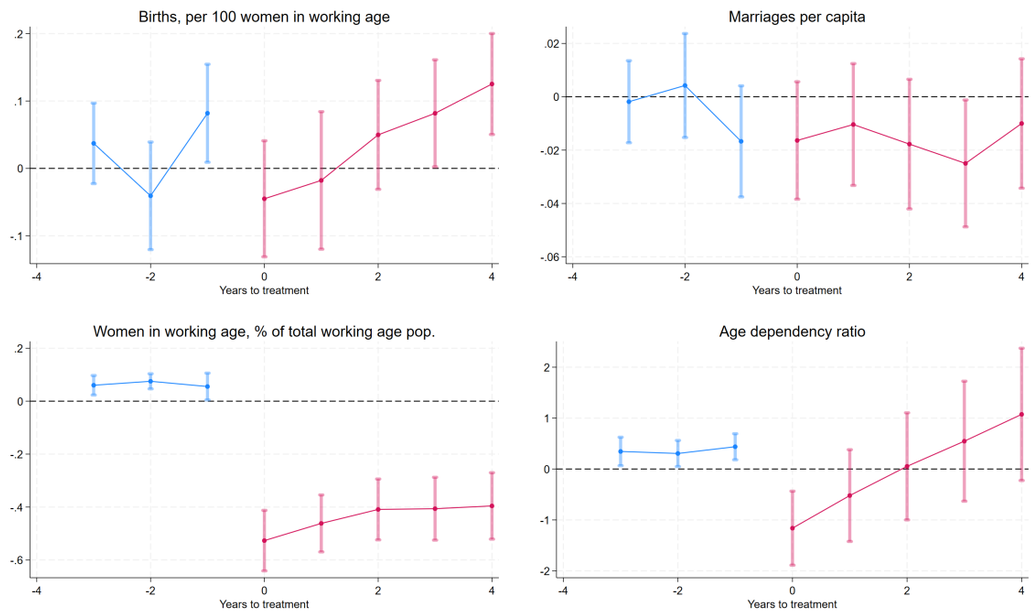


Figure 47: CS-DiD: city counties vs. county seats

Public goods

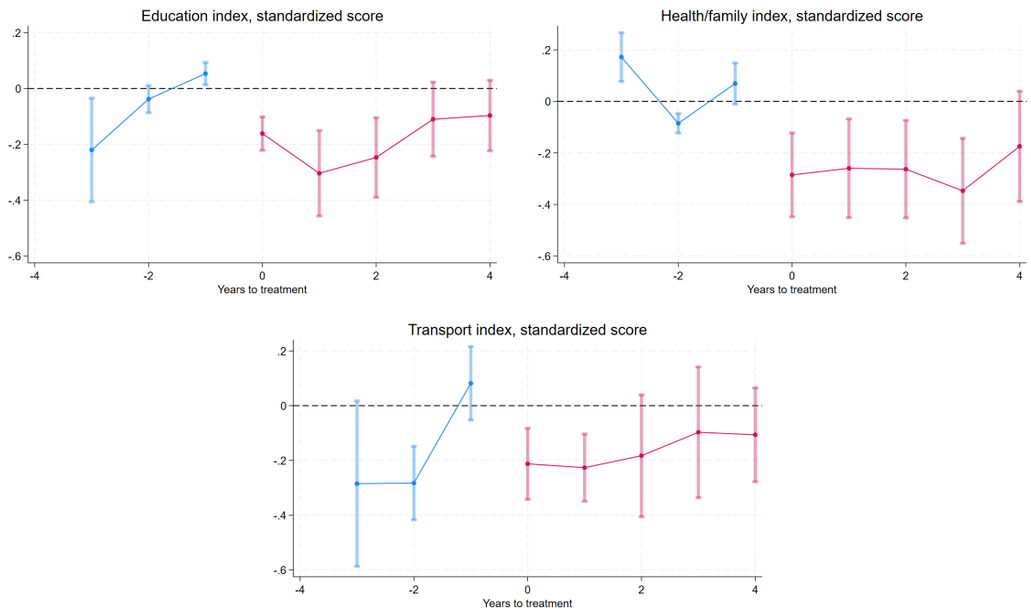


Figure 48: CS-DiD: ex-capitals vs city-counties

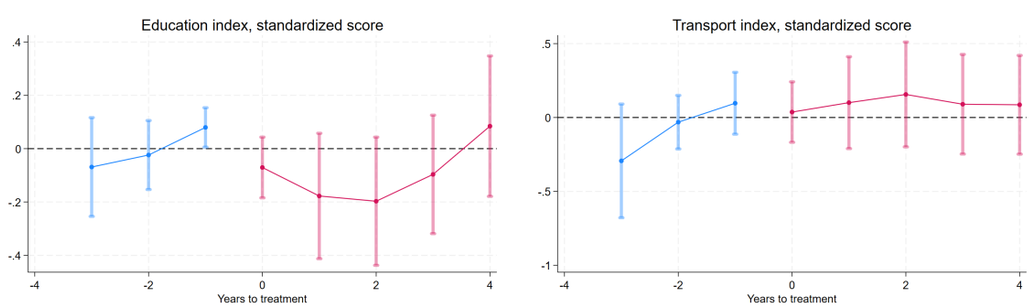


Figure 49: CS-DiD: ex-capitals vs county seats

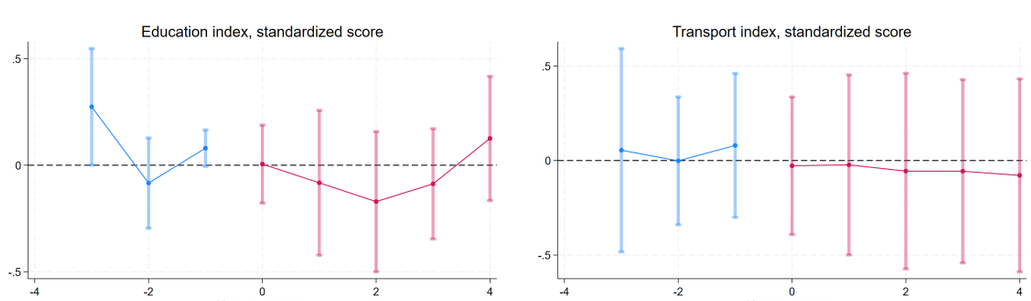


Figure 50: CS-DiD: city counties vs county seats