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Abstract

The paper estimates the political connection premium for Italian cities tracked during the second half of the 1900s, when the role of the state in the economy was very widespread. It leverages the peculiar features of the gridlocked political landscape in place between the end of World War II and the fall of the Berlin wall, during which most influential politicians remained in charge for a very long time. We compare connected cities – small areas surrounding birthplaces of both prime ministers and leaders of the parties in power – with very similar, but unconnected municipalities, and find that politically connected cities gained a population premium of 8% over 40 years. When the connection ends, the difference in growth rate fades away. We document that birthplaces of powerful politicians benefited from both infrastructure investments and the location of plants by state-owned enterprises. Not surprisingly, the connection favored industrialization, raised employment and wages, but crowded out private entrepreneurship. Finally, our empirical evidence indicates that agglomeration economies in treated municipalities were not higher, thus suggesting that, if anything, place-based interventions linked to political connections have not been output-enhancing from a nationwide point of view.

Keywords: political connections, city growth.

JEL codes: H50, R11, R12

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Non-technical summary

Political economy literature has recently documented that politicians do favor some targeted places by means of pork barrel transfers, asymmetric public good provision (e.g. infrastructures), location of state-owned enterprises (SOEs). However, much less is known about the consequences of regional favoritism in terms of economic growth. This paper documents the effects of regional favoritism in a developed country, providing also evidence about the persistence of the effects and on the spatial allocative optimality. We exploit the peculiar Italian institutional background between the end of WWII and the fall of the Berlin wall. At the beginning of this period, known in the journalistic and political jargon as First Republic, a completely new political system emerged: the end of the Fascist dictatorship led its way to a political system grounded on five political parties sharing an anti-communist stance and ruling for all the period. The stability of such a scenario was essentially based on Cold War and on the fear that the Italian Communist Party, the strongest among Western countries, could win regular elections and take power. Then, in the first part of the nineties, this system suddenly collapsed. The fall of the Berlin wall made the communist threat obsolete, mitigating the political pressures to keep the ruling parties in power. Shortly after, a massive judicial investigation into political corruption of the governing parties induced a sharp change in the political élites.

Against this historical background, we investigate the impact of political connections on population growth at the city level. We select population as dependent variable because it reflects economic growth in small areas and has the advantage of being available and consistently measured over a long estimation window. Then, we define the set of powerful politicians as prime ministers and leaders of the five parties in power between 1948 and 1992 and investigate whether municipalities in their areas of birth experienced stronger population growth over the forty-year period of the First Republic, with respect to untreated municipalities with similar characteristics at the beginning of the period. Our findings indicate the existence of a sizeable connection premium, equal to 8% over 40 years (18% of the standard deviation of the dependent variable). The abrupt end of the treatment period in the first part of the nineties allows us to study persistence in the following 20 years, from 1991 to 2011. The difference in population growth between treated and control municipalities fades until it disappears, while the difference in levels of population accumulated in the previous forty years remains stable. This suggests that the benefits provided by the political connections are not permanent, even though the levels of population may be sluggish in reverting to their original path, probably because the capital stock created may depreciate quite slowly over time. This conclusion is reinforced by the existence of structural changes in the local

economies of connected municipalities. We find that treated municipalities experienced improved economic prosperity at the end of the period: at the sunset of the First Republic, they showed greater industrialization (but not in the high-tech sectors), higher wages and employment rates. The downside was a toll on some private sector activity: the share of entrepreneurs out of the total number of workers was lower. Interestingly, after the connection ended, these structural differences slowly disappeared. Also, when investigating the mechanisms behind such results, we find that connected areas disproportionally benefitted from the post-WWII development of the transport network and that state-owned enterprises were more likely to be located in their neighborhoods.

After documenting the local advantages deriving from political connections, we complete the overall picture by turning to nationwide allocative considerations. Having favored some areas at the expense of others does not necessarily point to economic inefficiency. For example, politicians might have better inside information about the existence of higher agglomeration economies in their hometowns. At the same time, the blocked political system and the fact that politicians were destined to remain in power for long time might have favored forward-looking political choices. Under these conditions, moving population and economic activity to connected places would have brought to higher aggregate output. To check for this possibility, we test whether connected cities displayed higher agglomeration economies and find that this not the case.

1 Introduction

Between 1956 and 1964, in the midst of the economic boom, the Italian government built the most important infrastructure of the country, i.e. the highway connecting the city of Milan, the main economic center in the North, to the capital city, Rome, in the Central part of Italy. The most convenient route passed through Siena, a middle-sized city between Florence and Rome. According to an anecdotal story, the then Prime Minister Amintore Fanfani, born near Arezzo, a city 70-km east of Siena, decided to have the highway pass through Arezzo rather than Siena and drew this deviation of the route with his red pencil on a map. With the so called "Fanfani bend" (Figure 1), he was able to kill two birds with one stone: rewarding his electoral feud of Arezzo and inflicting a blow to Siena, that was governed by the main opposition group, the Communist party.

Aside from such suggestive anecdotes, some papers have recently documented that politicians do favor some targeted places by means of pork barrel transfers, asymmetrically public good provisions (e.g. infrastructures), location of state-owned enterprises (SOEs). This has been shown for both democracies and autocracies, as well as for countries with different degrees of economic development (Kahn et al., 2020; Carozzi and Repetto, 2016; Do et al., 2017; Fiva and Halse, 2016; Gehring and Schneider, 2018; Gonschorek et al., 2018). However, much less is known about the consequences of regional favoritism in terms of economic growth. Two notable exceptions are Hodler and Raschky (2014), who look at a large sample of both developed and developing countries, and Asher and Novosad (2017), who focus on India: both papers show that the positive bias of politicians towards their hometowns translates into greater economic development for beneficiaries. Nevertheless, two further relevant research questions remain unanswered. First, whether the growth in gains persists after the end of the connection and, if not, whether there is a reversal in the level of economic fortune. This is important to understand whether the greater benefits from regional favoritism can permanently change the development trends of such regions. Second, what are the allocative distortions induced by hometown bias. Indeed, while all the existing literature assumes that favoritism implies some form of misallocation, it might also be the case that politicians have private information on local development potential (e.g. in terms of higher agglomeration economies) of their birthplaces, a case in which targeting public intervention to birthplaces would be output-enhancing even at a national level.

This paper complements the existing literature documenting the effects of regional favoritism in a developed country and providing evidence regarding the persistence of the benefits from connections and the allocative bias induced. To do so, we exploit the peculiar Italian institutional background between the end of WWII and the fall of the Berlin wall. At the beginning of this

period, known in journalistic and political jargon as the First Republic, a completely new political system emerged: the end of the Fascist dictatorship led the way to a political system grounded on five political parties sharing an anti-communist stance and ruling for the whole period. The stability of such a scenario was essentially based on the Cold War and on the fear that the Italian Communist Party, the strongest among Western countries, could win regular elections and come into power. Then, during the first part of the nineties, this system suddenly collapsed. The fall of the Berlin wall made the Communist threat obsolete, mitigating the political pressures to keep the ruling parties in power. Shortly after, a massive judicial investigation into political corruption of the governing parties induced a sharp change in the political élites.

Against this historical background, we investigate the impact of political connections on population growth at city level. We select population as the dependent variable because it reflects economic growth in small areas and has the advantage of being available and consistently measured over a long estimation window.[1] Then, we define the set of powerful politicians as prime ministers and leaders of the five parties in power between 1948 and 1992 and investigate whether municipalities in their areas of birth experienced stronger population growth over the forty-year period of the First Republic with respect to untreated municipalities with similar characteristics at the beginning of the period. Particularly, we focus on small- and medium-sized cities, to avoid potential confounders influencing population growth in large metropolitan areas, such as rural-urban migration, human capital spillovers, etc., and regress changes in population between 1951 and 1991 against a treatment dummy and a vector of municipal characteristics observed in 1951. To provide a correct comparison between treated and control municipalities, we exclude from the control group the municipalities with the lowest predicted probability of receiving the treatment according to past population growth, sectoral composition of the local economy, geographic, demographic and socio-economic variables. Reverse causality issues are limited by the peculiar historical facts leading to the onset of the First Republic. Indeed, before the fall of the Fascist dictatorship it was impossible to forecast the economic boom of the '50s and '60s and was even more difficult to forecast which political parties would have gained momentum.

Our findings indicate the existence of a sizeable connection premium, equal to 8% over 40 years (18% of the standard deviation of the dependent variable). The impact is larger in municipalities that are connected to politicians with longer duration as elected members of Parliament and closer to the municipalities of birth of these politicians. A number of sensitivity checks regarding the selection of the sample, the clustering of standard errors, the potential role of outliers, and the estimation

[1] Ideally, one would like to observe the impact on city-level GDP but, unfortunately, this is not available as in almost any country. On the other hand, population is a well celebrated proxy for local economic growth (e.g. Roback, 1982; Hanlon and Heblich, 2020).

method reassure as to the robustness of our core result. Our second finding is about persistence. The abrupt end of the treatment period in the first part of the nineties allows us to study persistence in the following 20 years, from 1991 to 2011. The difference in population growth between treated and control municipalities fades until it disappears, while the difference in levels of population accumulated in the previous forty years remains stable. This suggests that the benefits provided by the political connections are not permanent, even though the levels of population may be sluggish in reverting to their original path, probably because the capital stock created may depreciate quite slowly over time. This conclusion is reinforced by the existence of structural changes in the local economies of connected municipalities. We find that treated municipalities experienced improved economic prosperity at the end of the period: at the sunset of the First Republic, they showed greater industrialization (but not in the high-tech sectors), higher wages and employment rates. The downside was a toll on some private sector activity: the share of entrepreneurs out of the total number of workers was lower. Interestingly, after the connection ended, these structural differences slowly disappeared. Also, when investigating the mechanisms behind such results, we find that connected areas disproportionally benefitted from the post-WWII development of the transport network and that state-owned enterprises were more likely to be located in their neighborhoods. Finally, after documenting the *local* advantages deriving from political connections, we complete the overall picture by turning to *nationwide* allocative considerations. Having favored some areas at the expense of others does not necessarily point to economic inefficiency. For example, politicians might have better inside information about the existence of higher agglomeration economies in their hometowns. At the same time, the blocked political system and the fact that politicians were destined to remain in power for a long time might have favored forward-looking political choices. Under these conditions, moving population and economic activity to connected places would have led to higher *aggregate* output. To check for this possibility, we test whether connected cities displayed higher agglomeration economies. We find that this is not the case.

The remainder of the paper is structured as follows. The next section provides a brief overview of the related literature, while the political landscape of the First Republic is described in Section 3. Section 4 provides the building blocks of our empirical analysis. In Section 5 we present the empirical results on the connection premium and on its persistence. Further evidence on other economic outcomes and on the underlying mechanisms is shown in Section 6. Section 7 illustrates that the benefits from connections were localized and did not contribute to nationwide economic growth. Finally, Section 4.3 provides some concluding remarks.

2 Literature review

Our paper is mainly related to the literature on the impact of political connections at city level. The two nearest papers are Hodler and Raschky (2014) and Asher and Novosad (2017). The former studies a large global panel dataset with more than 38,000 subnational regions in the 1992-2019 period and find that the hometowns of political leaders experience systematically higher nighttime light intensity than other locations. However, their results are fully driven by countries in Asia and Africa. The latter focuses on more than 4,000 legislative constituencies in India between 1990 and 2015 and show that political connections favor higher private sector employment, higher share prices of firms, and increased nighttime lighting.[2]

We add to the existing studies in several ways. First, we show that the growth premium holds also in an advanced economy. Second, our results are based on a longer time span, so that the result is more likely to capture the steady state spatial equilibrium. Third, the special features of the First Republic allow us to perform a persistence analysis that, to the best of our knowledge, is totally new. Fourth, this is the first paper exploring the inefficiency of the politically biased spatial allocation of resources across cities.

This paper is also related, to a lesser extent, to those papers documenting larger payoffs in terms of public spending, infrastructures, SOEs, etc. for cities with political connections. This has been shown for a variety of countries, featured with different degrees of economic and institutional development: Italy, (Carozzi and Repetto, 2016; Golden and Picci, 2008), European countries (Gehring and Schneider, 2018), Norway (Fiva and Halse, 2016), China (Kahn et al., 2020; Chen et al., 2017), Vietnam (Do et al., 2017), Indonesia (Gonschorek et al., 2018). Our additional contribution to these works consists in providing new evidence that political connections induce the construction of more infrastructures and increase the likelihood of SOE allocation.

Finally, we also speak to two other streams of the literature. First, a fundamental issue in urban economics and in economic geography is examining why the spatial distribution of economic activity is uneven. While natural advantages and agglomeration economies are important explanations, historical shocks with long-lasting effects may also play a relevant role (Rosenthal and Ross, 2015; Schumann, 2014). In this respect, we emphasize that political favoritism towards places of birth is an important historical determinant, since political decisions such as the route of a highway or the location of a state enterprise have profound consequences on the location choices of companies and households. Second, our findings are also of interest for the political economy literature, which mainly analyzed the consequences of political connections at the firm- (e.g. Knight, 2006; Fisman,

[2] A formally similar but conceptually very different line of research deals with political favoritism stemming from ethnic proximity (e.g. Burgess et al., 2015; Dickens, 2018; Dreher et al., 2019).

2001; Faccio, 2006; Khwaja and Mian, 2005; Claessens et al., 2008; Cingano and Pinotti, 2013) or household-level (Gagliarducci and Manacorda, 2020). We point out that analyzing the consequences of direct links at the firm or household level is not enough to capture the whole effect of political connections on economic outcomes: the impact of non-rival public goods (e.g. highways) and of private goods with local spatial spillovers (e.g. SOEs) calls for an aggregate area-level analysis.

3 The political landscape

The Italian political era referred to as First Republic spanned the period between the first general elections of the newborn Republic in 1948 and the first years of the nineties. It provides us with a very favorable setting to investigate the long-term effects of political connections. After having experienced the Fascist dictatorship, with the end of WWII the country designed its new republican democratic institutions with the election of a Constituent Assembly in 1946, who wrote the new constitution. The first Parliamentary elections were held in 1948. From then on and for the next 40 years, the political system featured a high degree of stability, with five parties in power at the national level. The Christian Democratic party (*Democrazia Cristiana* – DC), whose vote share was about 37% over the 1953-1992 period, was the hub of the system. The other four minor parties were the Italian Socialist Party (*Partito Socialista Italiano* – PSI, 11.9%), the Italian Social Democratic Party (*Partito Social-Democratico Italiano* – PSDI, 4.2%), the Italian Liberal Party (*Partito Repubblicano Italiano* – PLI, 3.4%), and the Italian Republican Party (*Partito Repubblicano Italiano* – PRI, 2.8%). Political stability was favored by the existence of the Cold War and the strength of the Italian Communist Party (*Partito Comunista Italiano* – PCI, 26.8%), the largest Communist party in Western advanced countries. Supported by the US, there was a tacit agreement to prevent the PCI from running the national government (the so called *conventio ad excludendum* – agreement for exclusion). The electoral system in place, which featured proportional representation, was also pivotal to reduce party turnover. Political stability continued up to 1992, when the Communist threat had vanished and a massive judicial investigation into political corruption of the ruling parties resulted in the demise of the First Republic and the disappearance of the five governing political parties (together with their main representatives).

The stability at the party-level was also associated with a very high number of governments (45 between 1948 and 1992) but also with a high degree of permanency for the politicians who held top positions. In most cases, new governments were just a reshuffling of the same politicians to different ministries. The most striking case was that of Giulio Andreotti. He was already a member of the 1946 Constituent Assembly and, without interruption, an elected member of Parliament up to 1991 (subsequently, he was appointed senator for life by the President of the Republic). During his

career, Andreotti was Prime Minister (7 times), Minister of Defense (8), Minister of External Affairs (5), Minister of State-owned enterprises (3), Minister of Finance, Minister of Economic Planning, Minister of Industry (2 times each), Minister of the Treasury, Minister of Interior, Minister of Culture, Minister of European affairs (1 time each). In journalistic jargon, the system was called "partitocrazia" (meaning "party politics", in other words that most of the power was in political parties' hands), with a lucky few in charge permanently.

At the same time, during the First Republic, the role of the public sector increased significantly. According to Franco (1993), the public expenditures share of GDP went up from 29% in 1960 to 54% in 1990 (net of debt-service obligations, from 28% to 44%). With a growing public sector, the influence of politicians was likely to grow. Chronicles referring to that period are full of anecdotal stories about pork-barrel politics.

Most of them refer to infrastructures, where the utilization of government funds for projects designed to benefit local residents can be easily traceable. The "Fanfani bend" example does not stand alone. Another highway going from Rovigo to Trento crossing Vicenza (in the North-East of the country) was locally named "PiRuBi", from the surnames of three DC ministers that lobbied for it: Flaminio Piccoli, Mariano Rumor e Antonio Bisaglia, respectively from Trento, Vicenza and Rovigo. Ciani et al. (2020) build on the fact that the final path of the Salerno-Reggio Calabria highway (in the southernmost part of the Italian peninsula) was chosen to pass through Cosenza, the birthplace of two very influential politicians (Giacomo Mancini, PSI, and Riccardo Misasi, DC), while two competing coastal routes had been discarded.[3]

Local favoritism was not limited to roads. Another channel were state-owned firms, which were a distinctive feature of the Italian post-WWII development process (see, for instance, Morck and Steier, 2005; Castronovo, 1995). In 1947, the Mechanical Industry Fund was created, later transformed into EFIM, a public financial holding that managed shareholdings and the financing of manufacturing firms. The structuring of public intervention in the economy continued with the creation of a system of monetary incentives targeted to underdeveloped areas (*Cassa del Mezzogiorno*) in 1950, and the National Hydrocarbons Authority (ENI, 1953, with the task of coordinating the

[3]Italy's roads network provides many additional examples. At the beginning of the seventies, the construction of the Cassia bis road, near Rome was supposed to serve the private villa of Giovanni Leone, an important DC politician, next President of the Republic in the 1971-1978 period. The exact path of the highway connecting the Abruzzo region (Central Italy) to Rome was the reason for a strong dispute between Remo Gaspari and Lorenzo Natali (both from the DC), as both of them wanted the infrastructure to connect their local constituency, respectively Chieti and L'Aquila. The highway connecting Naples to Bari (in the South of Italy) was re-directed towards Avellino (leaving Benevento out of the path) because of the pressure of Fiorentino Sullo, obviously from Avellino. The highway from Genoa to Gravellona Toce in Piedmont (North-West of the country) was named by local people the "Nicolazzi highway", from the name of the PSDI secretary who lobbied for building this infrastructure (and an exit next to his small hometown of Gattico).

State's interventions in the oil industry). Moreover, the Institute for Industrial Reconstruction (IRI, *Istituto per la ricostruzione industriale*), founded in 1933 under Fascism, became more and more pivotal to public intervention in the Italian economy. For instance, it was involved in the development of the steel industry, the telephone network and the construction of the first and most important highway (including the "Fanfani bend"). In 1980, the IRI group consisted of about 1,000 companies with more than 500,000 employees. To coordinate the state ownership of the firms a special Ministry of State Holdings was established (in 1956), which collected all the duties and the assignments previously attributed in this sector to other ministries and government bodies. In sum, postwar Italian politicians opted to allocate capital via discretionary industrial policies, rather than through decentralized market-based mechanisms. Not surprisingly, the SOEs were potentially a gold mine for influential politicians in need of transferring resources towards their preferred places. For example, COVEI (COmponenti VEtrari Italiani) and VEM (VEtrerie del Mediterraneo), two firms that operated in the glass sector and belonged to EFIM, were located in Cosenza, hometown of the PSI party's secretary (Giacomo Mancini) in the period 1970-1972. Italtractor (state-owned tractor producer) had a plant in Potenza, connected to Emilio Colombo, DC Prime Minister from 1970 to 1972. Figure 2 shows the map of the Italian SOE establishments between 1948 and 1991, including the municipalities within a 10-km radius. It is easy to notice how relevant such a state-driven industrial policy has been and how important it was for local authorities to attract such establishments in their electoral basin to foster local employment and welfare.

In the early 1990s, the rising public debt and taxes made industrial policies unsustainable so that a sweeping privatization program greatly changed the picture. On April 15 1993, the abrogative referendum of the Ministry of State Holdings obtained a large consensus and, in a couple of years, many state-owned enterprises were privatized. Overall, historical facts document that during the First Republic there were many conditions leading to political favoritism: a gridlocked political system with very limited turnover in political leadership and a relevant role of the State in the economy. Anecdotal evidence supports the hypothesis that influential politicians were able to channel public resources towards their cities of birth, for example, by means of the transport network and the location of SOEs.

4 Empirical framework

In this Section, we first describe how we exploit the institutional background outlined above to derive our empirical strategy (Subsection 4.1). Then, we define connected cities (Subsection 4.2) and select the control group to maximize comparability between treated and control units (Subsection 4.3). Data and descriptive statistics are presented in Subsection 4.4. Finally, the regression model

is illustrated in Subsection 4.5.

4.1 Timing

As outlined in Section 3, the First Republic, spanning from 1948 to 1992, was a very well-defined political era, which markedly differed from the preceding and the subsequent ones. In our empirical context, no municipality is treated before 1948. From then on, some cities are treated while other comparable ones are in the control group. Since the decadal Censuses, which are our main source of data, are run in the years whose last digit is "1" (see below), we consider 1951, the nearest to 1948, as the last pre-treatment year. Then, connected municipalities receive the treatment up to 1991 (the closest to 1992, the year of the collapse of the First Republic). This setting would naturally call for a difference-in-differences empirical design with periods given by decades. However, it is somehow arbitrary to define the exact year in which the treatment switches on. Given the structure of the political power during the First Republic, it might be that our politicians were very influential also before and after their office. As discussed above, even though for some periods politicians were not officially prime ministers or party leaders, they were likely to manage public resources and to affect decisions in any case. These considerations translate into our preferred empirical strategy in which we compare population dynamics between treated and control units over the whole of the First Republic.

Interestingly, there are two census waves that took place after the collapse of the First Republic, in 2001 and 2011. This allows us to evaluate whether the impact detected for the treatment period survives after its end.

4.2 Defining politically connected cities

Our starting point is defining the most powerful political representatives. Hodler and Raschky (2014) define connected cities as the towns in which prime ministers (or similar) were born. However, focusing on Italy, we must also acknowledge the importance of the five ruling parties. Indeed, as discussed in Section 3, the parties in power played a pivotal role in allowing the stability of the governments in the First Republic. Thus, we consider both the prime ministers and the leaders of the five parties in power between 1948 and 1992. Overall, in the period considered there were 18 prime ministers and 46 party leaders, corresponding to 57 influential politicians.[4] After excluding 2 politicians born abroad, we end up with 55 influential politicians. Then, we remove from the sample those politicians coming from large cities (above 200,000 inhabitants in 1951) and their surrounding areas (within a 20-km radius). Indeed, in defining the treated and the control units we

[4]Seven politicians were both prime ministers and party leaders.

prefer to avoid considering metropolis, for which there are too many potential confounding factors.[5] Finally, we label as politically connected the 33 distinct municipalities of birth of the remaining politicians. Table 1 displays the names of these politicians and lists the corresponding birthplaces. In a robustness check, we find that our results are robust to the exclusion of other larger cities, those above the 99th percentile of the population (about 50,000 inhabitants).[6]

To account for the possibility of geographical spillovers of the treatment to neighboring municipalities, we assign the same treatment status to all the municipalities within a 10-km radius from the birthplaces.[7] Indeed, gains accruing from the construction of an infrastructure or the opening of a new plant of a SOE are likely to benefit not only the target municipality but also all the surrounding municipalities. We will show that our results are robust to perturbation of this cutoff.

While we are very confident that all the influential politicians according to our definition were powerful, we cannot rule out the possibility that others, such as members of the Parliament, regional governors or important ministers who never became prime ministers or party leaders, were powerful as well. In our view, a clear-cut and undisputable definition of powerful politician does not exist, and, hence, the assignment of the treatment will always have some degree of measurement error. A clear advantage of our conservative definition is that the bias potentially embedded in it is a type-II error, that is we mistakenly consider as not treated some places being treated. Under these circumstances, our estimates will be a lower bound of the true effect.

Finally, it should be noted that the abrupt change of regime between the fall of the Fascist dictatorship and the onset of the First Republic prevents the possibility of municipal selection into the treatment, i.e. municipalities with higher growth potential before 1951 were more likely to affirm one of their inhabitants as prime minister or political leader. Indeed, as shown in Table 1, more than 90% of our influential politicians were born before the '30s, when the chance of predicting future (1951-1991) local growth rates in small- and- medium-sized municipalities was basically null given the forthcoming World War II. In the next subsection, as further reassurance regarding selection into the treatment we also check the balance of population growth rates before 1951 between treated and control municipalities.

[5]For instance, large cities experienced strong population growth in the period considered due to rural-urban migration. Thus, including them in the treatment group would lead us to overestimate the impact of political connections on local population growth. Moreover, since wealthy urban environments with higher levels of human and social capital may be more likely to affirm their political representatives with respect to less educated peripheral areas, their inclusion may lead to a reverse causality bias. Some scholars have focused on favoritism towards national capitals or other larger cities (Ades and Glaeser, 1995; Davis and Henderson, 2003). However, our empirical choice is motivated by the fact the we are not interested in the favoritism per se, but in its economic consequences, for which the political bias effect is likely to be blurred by too many confounders.

[6]In 1951, at the beginning of the First Republic, the median municipal size was 2,590 inhabitants.

[7]To put this empirical choice in perspective, 10 km is about the average radius of a local labor market in 2011.

4.3 Designing a proper control group

Similarly to what we do for the treated units, in designing a control group of municipalities, we preliminarily exclude from the sample the cities with more than 200,000 inhabitants in 1951. Also, we exclude the municipalities within a 20-km radius from all the big cities in our sample, since in the period considered they might have benefited from the agglomeration economies triggered by their neighboring metropolitan areas. In addition, to account for possible spillovers from treated to control units, we exclude all the municipalities in a radius between 10 and 20 kilometers from the politicians' municipalities of birth (as explained above, municipalities within a 10-km radius are part of the treated group).

Finally, to credibly identify the effect of political connections on population growth, treated and control municipalities at the beginning of the First Republic should be similar in terms of their observable characteristics. If this is not the case, we cannot rule out the possibility of spurious correlations between political connections and population growth. To check for this, Columns (1)-(3) of Table 2 show the balancing of these characteristics between treated and control municipalities at the beginning of the period (see below for data description).[8] Particularly, we consider the logarithm of municipal population at the beginning of the period in 1951, the past (1936-1951) population growth rate, geographic variables, sectoral composition of the local economy, the demographic composition of the population, the electoral turnout (as a proxy of social capital), the vote shares for the five ruling parties in power and for the Communist party at the beginning of the period. It turns out that treated municipalities show (i) higher past population growth; (ii) lower surface; (iii) lower altitude; (iv) lower probability to be located in Southern Italy (where on average the quality of institutions is lower); (v) larger share of workers in manufacturing; (vi) higher labor market participation; (vii) more human capital; (viii) larger turnout at general elections; (ix) larger vote share for the Communist party.

To address the balancing requirement, we perform a logit regression of the treatment against the vector of municipal characteristics displayed in Table 2, and drop from the sample the municipalities in the control group with the lowest predicted probability of being treated. After removing 26% of municipalities in the control group, all the main characteristics are balanced (Table 2, Columns (4)-(6)): this is central for the causal interpretation of our estimates. A similar procedure is adopted by Kline and Moretti (2014a). Note that the balancing condition on the past population growth reassures that other unobserved variables, such as political connections in the pre-1951 years, are balanced as well. On the political side, the balancing of the vote shares for both the five ruling

[8] To ensure comparability across treated and control units, we drop from the sample municipalities with missing observations in control variables (approx. 300 municipalities).

parties and the Communist party is crucial to rule out the possibility of systematic correlations between connected politicians and local political preferences (e.g. politicians of the five ruling parties coming from areas with stronger preference for their parties or connections endogenously emerging to contrast the local Communist threat).

4.4 Data and descriptive statistics

Our empirical analysis draws from a number of data sources. Data on party leaders and prime ministers, as well as their birthplaces, have been hand-collected from Wikipedia and double-checked with other online resources.

Population data at municipal level come from national censuses, carried out by the Italian Statistical Institute (Istat). Censuses take place every 10 years on years ending in "1", with the only exception of the 1941 census that took place in 1936. The closest year to the beginning of the First Republic is 1951, while the closest year to its end is 1991. The municipal participation rate, the share of college educated and the old-to-young population ratio are also taken from population censuses.[9] Data on geographical characteristics are drawn from the Statistical Atlas of Municipalities provided by Istat, as well as information on municipal altitude and area.[10] The geographical coordinates to compute the distance between municipalities are also provided by Istat. The share of workers in manufacturing, construction and service sectors are taken from the Istat Census of Manufacturing and Services. From the latter Census we also draw data on the share of employers and self-employed workers, as well as municipal plant density and workers per plant in 1991 and in 2011. Data on voter turnout at general elections at the baseline (in 1953) are drawn from the electoral archive of the Italian Ministry of the Interior. Information about their tenure as members of Parliament are taken from a database of the "Fondazione Rodolfo de Benedetti".

Then, we exploit some additional variables to evaluate the local benefits of political connections and their persistence after 1991. The share of workers in agriculture, manufacturing and services, the share of workers in high tech industries, the employment and unemployment rates for 1991 and 2011 all come from "8000 Census", a national data provider of municipal data managed by Istat. Data on per capita wages in 1991 and 2011 come from the National Security Database. Data on roads and railways at the province level come from the Statistical Atlas of Infrastructures, managed by Istat, while data on plants of state-owned enterprises come from a unique dataset built by the authors as follows. We start from the list of all Italian main privatizations from 1985, made available at Privatizationbarometer. For each firm, we hand collect the location of the headquarters

[9] Participation rates, shares of college educated and old-to-young population ratios are available from 1971 on. Thus, we imputed 1971 values to 1951 and 1961.

[10] Municipal surface is available from 1971 on. So, we imputed 1971 values to 1951 and 1961.

and the plants from Internet. Then, this information is complemented with data taken from (i) Amatori (2013), (ii) a paper carried out by Mediobanca (an Italian leading investment bank), and (iii) a list of the companies belonging to the EFIM group provided by the Italian Parliament.[11]

Finally, for the structural analysis in Section 7, we use 1951-1991 average per capita wages at the provincial level provided by "Istituto Tagliacarne". After deflating the nominal values to account for inflation, we simply assign provincial values to all the corresponding municipalities. We also compute population density as the ratio between municipal population and surface. Main descriptive statistics are shown in Table 3.

4.5 Estimating equation

Our core empirical exercise studies whether politically connected cities experienced higher population growth between 1951 and 1991. The baseline regression is:

$$\ln Pop_{m,1991} - \ln Pop_{m,1951} = a_0 + a_1 Treated_m + a_2 Controls_m + \varepsilon_m \tag{1}$$

where the dependent variable is the log-difference between population in 1991 and in 1951, and the subscript m denotes municipalities. $Treated$ is a dummy variable equal to 1 for treated units (as defined above) and 0 otherwise. a_1 is the coefficient of interest. Among the controls, we include all the variables listed in Table 2: the logarithm of municipal population at the beginning of the period, the 1936-1951 population growth rate, geographic variables, sectoral composition of the local economy, the demographic composition of the population, the electoral turnout and the vote shares, all measured in 1951.[12] ε_m is the usual error term. Standard errors are robust to heteroskedasticity and clustered at municipal level. Estimating the model in first difference ensures that idiosyncratic municipality-level features are differentiated away, while the balancing of the pre-treatment population growth guarantees that, before the treatment, treated and control units were on similar development paths.

5 Main results

We start by presenting our baseline results on population (Subsection 5.1); then, we provide a full-fledged robustness analysis (Subsection 5.2), while persistence is analyzed in Subsection 5.3.

[11] References to data sources: privatization barometer: `www.privatizationbarometer.com`; Mediobanca paper: `https://www.mbres.it/sites/default/files/resources/download_it/rs_priv_testo.pdf`. The list of EFIM companies is available upon request from the authors.

[12] Electoral data refers to 1953.

5.1 Baseline results

Results on the estimation of Equation (1) are reported in Table 4. Column (1) displays the estimated coefficient of interest without adding additional controls. The statistically significant coefficient of 0.088 suggests that on average, during the 40 years of the First Republic, the population of connected municipalities grew 9% more than the population of control cities. Columns (2) to (6) show the key estimates after adding alternatively the city-level characteristics described in Table 2. Finally, in Column (7) we add jointly all the covariates. The coefficient of interest is rather stable across the specifications and amounts to 0.081 in the most demanding one: the connection premium roughly equals 8% (18% of the standard deviation of the dependent variable). We choose Column (7) as our baseline specification.

To analyze how the end-of-period 8% difference materializes over time, we run a complete event study by exploiting the time dimension and introducing time dummies interacted with the treatment variable. The first year in which connected municipalities are treated is 1961, while we exclude 1951 to avoid perfect collinearity. We estimate the following equation:

$$\begin{aligned} \ln Pop_{m,t} = \beta_0 + \sum_{t=1901}^{1936} \beta_{1,t} Treated_m D_t + \sum_{t=1961}^{1991} \beta_{1,t} Treated_m D_t + \\ + \beta_2 Controls_{m,t} + T_t + \varphi_m + \nu_{m,t} \end{aligned} \tag{2}$$

where t is equal to Census years; T_t and φ_m denote year and municipality fixed effects, respectively; $Controls_{m,t}$ include all the control variables in Table 2 measured at 1951 interacted for year dummies while $\nu_{m,t}$ is the usual error term. As shown in the Figure 3, which reports the estimated $\beta_{1,t}$s, treated municipalities do not display any different trend before the treatment. On the contrary, the effect is positive and statistically significant in 1961 and keeps growing afterwards, reaching its maximum in 1991 (0.088, fully in line with the baseline estimates). These results also suggest that our core estimate is robust to an alternative estimation strategy, based on a difference-in-differences setting.

Table 5 looks at the heterogeneous effects. In Column (1), we augment our baseline specification by adding an interaction between the treatment term and a dummy variable for municipalities in the South. In this area, favoritism may be stronger because of weaker institutions and lower social capital (Putnam, 1993). It turns out that this is the case: the estimated effect is around 7% in the Centre-North, 12% in the South. In Column (2) we analyze what happens when moving from the birthplace of influential politicians outwards of the 10-km-radius circle of treated units. We define two indicators for, respectively, municipalities above (farther) and below (closer) the median distance from the center of the treated area. The magnitude for the closer ones is twice that of

the farther ones. The last two columns investigate the moderating role of a politician's salience, proxied by the years spent as member of Parliament (Column 3), or by the number of Google citations (Column 4): taken together, the two pieces of evidence suggest that the more powerful the politician, the larger the connection premium.

5.2 Robustness checks

The first six rows of Table 6 report sensitivity checks for some empirical choices. We start by analyzing the robustness of our findings with respect to the use of the 200,000-inhabitant threshold to remove big cities from the sample: we drop all the municipalities above the 99th percentile of the 1951 municipal population distribution (along with their neighboring municipalities within a 10-km radius). This is equivalent to decreasing the threshold to 50,000 inhabitants. We find that the impact increases to 11%, while remaining highly significant (Row 1). Row (2) shows that our core result is confirmed after trimming the dependent variable at the 1-99 percentiles to check whether outliers drive the point estimate. In Row (3) we rerun our regression after cutting 30% of the control municipalities with the lowest predicted probability of being treated (it was 26% at the baseline).[13] The core result nicely survives. Row (4) provides robustness with respect to the distance to hometowns that we use to label municipalities as treated. In this experiment, we consider as treated those municipalities within a 15-km radius (instead of 10 km) and we exclude from the sample municipalities between 15 and 30 kilometers (10-20 km in the baseline). Again, results are in line with the main estimates. The next two exercises are about the definition of the most powerful politicians. In Row (5), we assume they are only prime ministers, while municipalities connected to party leaders are excluded from the sample. This way, our definition of connection is in line with that in Hodler and Raschky (2014)'s paper. The estimated premium is statistically significant and slightly higher with respect to the baseline. The specular exercise is shown in Row (6), in which only cities connected to party leaders are treated while those linked to prime ministers are out of the sample. Again, we detect a connection premium. Thus, in our sample, prime ministers are not the only politicians that are able to give rise to benefits for their hometowns.

To account for the potential spatial correlation of the error terms, Rows (7) and (8) report alternative clustering procedures for the standard errors, respectively clustering at the province-level and considering municipalities within a 10-km radius. In both cases, the precision of our estimates diminishes significantly (the standard error goes from 0.019 to 0.037 and 0.028, respectively), while remaining within the conventional limits. Finally, we adopt different estimation methods. In

[13] We also try with a sample in which 20% of the municipalities with the lowest predicted probability of being treated are trimmed. Results were still there. However, note that in the baseline estimates, we choose the threshold of 26% because it is the minimal threshold at which municipal characteristics are balanced.

this way, we can show the robustness of our core result to alternative weighting strategies of the control units. Particularly, we first consider propensity score matching, that weights observations in the control group according to the probability of receiving the treatment given the observable characteristics Rosenbaum and Rubin (1983). Then, we consider the Oaxaca-Blinder estimator. As shown by Kline (2011), this estimator is simply a propensity score reweighting estimator with superior properties with respect to the propensity score matching in terms of robustness to the assumptions underlying the estimation (see the original paper for further details). The coefficient estimated with the first method is presented in Row (9), while the second one is presented in Row (10). Both regressions show coefficients fully in line with our core result, suggesting that the selected municipalities in the control group are sufficiently similar to the treated ones and that our OLS estimates provide a good approximation regardless of which control municipalities are given more weight.

Our last concern is about the possibility that our estimates may be driven by some specific municipalities. To test for this, we run a simulation exercise in which at each draw we exclude from the sample 5 birthplace cities randomly chosen (along with their neighboring municipalities) and re-estimate the model. We repeat this procedure 1,000 times. The kernel density of the estimated coefficients is presented in Figure 4. The vertical dashed line indicates the average of the estimated coefficients, which is equal to 0.081 (standard error = 0.021), the same point estimate we have in Table 4 (Column 6). The mass under the kernel density curve is concentrated around the mean value and does not present unwarranted peaks in other parts of the distribution. This suggests that the effect of political connections comes quite evenly from all the treated municipalities.

5.3 Persistence

A nice feature of our setting is that we can observe local economies for 20 years after the collapse of the First Republic, from 1991 to 2011. In Table 7, which is analogous to Table 4, we re-estimate Equation (1) with the dependent variable equal to $\ln(Pop_{m,2011}) - \ln(Pop_{m,1991})$. The connection premium vanishes in all the specifications, even if we control for political connections related to the new political élite that came into power after 1992 (Column 8).

Extending the event study to 2011 confirms that the connection premium earned during the First Republic stabilizes after 1991. This event study confirms that the differential speed of local growth for treated units gradually fades away (Figure 5).

6 Further results

In this Section, we explore how political connections shape structural changes (Subsection 6.1) and provide some evidence on transmission channels (Subsection 6.2).

6.1 Structural change

Thus far, we have shown that connected municipalities earn a sizeable connection premium in terms of population. We now investigate the specific changes induced by the political connections on the local economies, using as depend variables a number of other economic outcomes. Namely, we run a number of regressions of the following type:

$$y_m = \gamma_0 + \gamma_1 Treated_m + \gamma_2 Controls_m + \xi_m \tag{3}$$

where y_m is the outcome of interest measured in 1991, at the end of the treatment period, or in 2011, that is 20 years later, to see whether some persistence in the same outcome is at work. $Controls_m$, measured in 1951, and $Treated_m$ are as above, while ξ_m is the error term. Note that, differently from Equation (1), the lack of available data on the dependent variable referring to 1951 (or to previous years) prevents us from systematically controlling for its beginning-of-period value or for its past dynamics. This might undermine a neat causal interpretation of the estimated γ_1. However, the resulting conditioned correlations are suggestive anyway, thanks to the balancing properties of the observable characteristics at 1951 shown in Table 2 that account for a non-negligible part of unobserved heterogeneity. Results for the estimates of γ_1 are reported in Table 8, in which the dependent variable is measured in 1991 (Column 1) or in 2011 (Column 3). At the end of the First Republic (Column 1), the birthplaces of influential politicians and their surroundings featured higher levels of economic development compared with their unconnected counterparts. The structure of the local economies was tilted more towards manufacturing (but not towards high tech sectors), to the detriment of agriculture, while the weight of the service sector did not differ from that prevailing in the control municipalities. The density of industrial plants was higher. Firms were larger, workers received higher wages and a greater number of local residents were employed. On the other hand, the share of private entrepreneurs out of total workers was smaller, compared to the group of control units.

Very interestingly, Column (3) shows that, 20 years after the treatment is turned off, most of these advantages have disappeared (wages and plant size) or are significantly fewer: the degree of industrialization has halved, as well as the employment rate, and unemployment is now higher. Higher plant density persists, probably because the capital stock is sticky, but the average plant size

is now undistinguishable between treated and control units. On the other hand, the displacement effect on private entrepreneurship is still in place. The evidence of a glorious past has lost intensity.

Overall, the evidence in Table 8 is fully consistent with theoretical insights from Acemoglu et al. (2006). They develop a model showing that at an earlier stage of development, when a country is far away from the technology frontier, government interventions can be useful to overcome market failures. However, as the economy approaches the world technology frontier, new market-friendlier policies and institutions are needed, while direct public intervention becomes a burden for sustaining further development. Our findings are also consistent with research carried out by economic historians, who have argued that the pervasive public intervention in the Italian economy was able to give the big push (which was more intense in treated areas) while the country was lagging behind. At the same time, that development model was unfit after the nineties, in a new scenario featured by globalization, the information and communication revolution, the adoption of the Euro (Calligaris et al., 2016).

6.2 Mechanisms

In this subsection, we complete the picture by presenting some descriptive evidence on transmission channels. As depicted in Section 3, anecdotal evidence suggests that our results might mainly reflect two types of advantages provided by political connections: infrastructure investments and localization of state-run firms. We now provide some descriptive evidence in this regard. In Table 9, we estimate variations of Equation (3) with different dependent variables. Column (1) shows that the length of highways, national, regional and provincial roads per square kilometer measured in 1996 (first year of available data) was significantly greater in connected areas. Column (2) provides similar evidence with reference to railway density. We also use our data on the plants of state-owned enterprises to investigate the probability of having a SOE in the neighborhood of treated municipalities. The dependent variable in Column (3) is a dummy variable that takes on the value of 1 if the municipality is within a 10-kilometer radius from a SOE. The experiment of Column (4) is similar to that of Column (3) but excludes utility companies, for which there may be less discretion in the spatial allocation. This evidence strongly suggests that the anecdotal stories depicted in Section 3 also have a factual base.[14] Overall, the connection premium in terms of infrastructures and SOEs is fully consistent with the industrialization results in Table 8. Results in Tables 8 and 9, taken together, support the idea that politicians can manipulate economic

[14]Our results are consistent with Golden and Picci (2008), who studied Italian provinces during the same period. They argue that in the context of openlist proportional representation, the governing parties were not able to discipline their own members sufficiently to target the parties' areas of core electoral strength, rather than powerful politicians' areas of reference. Not surprisingly, the paper documents that the geographical distribution of public investments depended on connection with powerful politicians.

variables. In our case study, politicians seem to have pushed SOEs to engage in over employment and above-market wages, probably in order to have greater political support (Shleifer and Vishny, 1994). To further investigate this issue, Figure 6 shows an event study depicting the differential increase in the logarithm of employment in manufacturing in treated and control municipalities over time. Unfortunately, data on employment in manufacturing are only available as of 1951 and thus it is not possible to check for parallel trends before that. Again, we control for all the variables measured at 1951 listed in Table 2 interacted for year fixed-effects. Omitting 1951 to avoid perfect collinearity, the figure shows a dramatic increase in the level of employment in manufacturing in treated municipalities. The figure also shows that employment in manufacturing grew up to the '80s and stabilized afterwards.

7 General equilibrium effects

While during the First Republic powerful politicians generated benefits for connected cities, the nationwide effect remains unclear. If the connection simply reallocates resources across space, the overall impact might be null, or even negative if, for instance, agglomeration externalities spurred by infrastructures and SOEs were higher in unconnected cities. To address this crucial point, we borrow from Kline and Moretti (2014a), who propose a spatial equilibrium model to empirically test whether allocating resources to a given area is optimal from a nationwide perspective. To our aim, the interesting features of their model are the nature of agglomeration forces, working through externalities on productivity, and the minimal data requirements.[15]

Italian municipalities are modelled as small open economies with perfectly mobile capital and labor over the 10-year Census horizon. Municipalities are price takers on capital, labor, and output markets. Workers have also homogeneous tastes so that utility is equalized across municipalities. In each municipality, output is obtained combining labor, capital and a fixed factor by means of a Cobb-Douglas technology with constant returns to scale multiplied by a shifter that represents the total factor productivity (TFP). Political connections have a double effect on local TFP: (i) a direct one (e.g. a new road), and (ii) an indirect one through agglomeration economies. Indeed, the increase in TFP generated by the direct effect translates into higher local wages that, in turn, attract additional workers so obtaining a second-round gain in productivity via greater worker density, i.e. agglomeration.[16] If the marginal productivity of labor is equalized across municipalities, it is

[15] In other models the main source of agglomeration are agglomeration economies in consumption (Kline and Moretti, 2014b): increases in local income induce greater demand for goods and thus more economic development. However, such models are better suited to explain agglomeration externalities in the non-tradeable sector. Since in our context the main source of agglomeration are infrastructures and the diversion of SOEs in the manufacturing sector, we prefer to rely on a model encompassing externalities on the production side.

[16] Greater worker density is traditionally associated with gains in productivity because it allows for positive spillover

possible to show that moving labor from municipality i to municipality j raises total (nationwide) output if and only if the agglomeration elasticity (i.e. the elasticity of local productivity with respect to the local density) is larger in j (see Kline and Moretti, 2014a for further details). Thus, if the agglomeration elasticity is constant across municipalities, there is no aggregate gain from reallocating a worker from one community to another. In order to bring this result to the data, Kline and Moretti (2014a) use a dynamic panel approach. We adapt their model to our empirical framework and subdivide the sample in splines according to the distribution of the logarithm of municipal density in 1951. In this way, we can ascertain whether the elasticity of agglomeration is constant over different density levels. Then, we derive the following estimating equation, which tests whether agglomeration elasticity varies with population density:

$$\begin{aligned}\ln Pop_{m,t} - \ln Pop_{m,t-1} = {} & \delta_0(\ln w_{m,t} - \ln w_{m,t-1}) + \\ & + \delta_1 g_1\Big(\ln \frac{Pop_{m,t-1}}{R_m} - \ln \frac{Pop_{m,t-2}}{R_m}\Big) + \\ & + \delta_2 g_2\Big(\ln \frac{Pop_{m,t-1}}{R_m} - \ln \frac{Pop_{m,t-2}}{R_m}\Big) + \\ & + \delta_3 g_3\Big(\ln \frac{Pop_{m,t-1}}{R_m} - \ln \frac{Pop_{m,t-2}}{R_m}\Big) + \\ & + \delta_4 Treated_m + \delta_5 Controls_{m,1951} + T_t + \omega_{m,t}\end{aligned} \tag{4}$$

where w are wages, proxied by per capita GDP at the province level (see Section 4.4), R is the municipality area, *Controls* include all the control variables listed in Table 2 measured at 1951, the logarithm of municipal density in 1951 and provincial time trends, T are time fixed effects, ω is the error term, and the rest of the notation is as above. The key feature of Equation (4) is the presence of the $g_i(.), i = 1, 2, 3$, which are spline functions defined as follows:

$$g_k(x) = \begin{cases} \min\left(\ln x, \ln q_1\right) & if\ k = 1 \\ \min\left(\ln x - \ln q_{k-1}, \ln q_k - \ln q_{k-1}\right) 1[x > q_{k-1}] & if\ k > 1 \end{cases} \tag{5}$$

where q_k are spline knots.[17] The parameters of interest are the spline coefficients δ_1, δ_2 and δ_3 that capture the indirect effect of the connection at "low", "medium" and "high" density, respectively. In terms of the theoretical predictions of the model, if these three parameters are statistically indistinguishable from each other, there is no evidence that the political connections are output-improving at the nationwide level. If they are not, we will check whether treated units are disproportionally

effects, such as better employer-employee matches, positive externalities from human capital, etc.

[17] Following Kline and Moretti (2014a), we choose knots according to the percentiles in the 1951 logarithm of municipal density distribution that makes the variation in the first difference of each spline component over our sample period approximately the same. We have $q_1 = 4.2, q_2 = 5.2, q_3 = \infty$, corresponding respectively to the 18th and 62nd percentiles of the 1951 distribution of the logarithm of municipal population density.

represented in the most or in the least productive interval. The model is estimated on the panel of 4,839 municipalities belonging to our sample, observed in four decadal intervals: 1951-1961, 1961-1971, 1971-1981, 1981-1991. Since wages are endogenous to population dynamics, we also follow Kline and Moretti (2014a) in calibrating the coefficient δ_0 to -1.5. Nevertheless, we check the sensitivity of our results to perturbations of this parameter.

Before moving to the results, we must acknowledge that our estimates of the elasticities of agglomeration may be curbed by serial correlation bias. In other words, since agglomeration induced by public intervention at some point in time spontaneously generates additional agglomeration in subsequent periods, we may mistakenly attribute the serial correlation in observations to agglomeration forces. This problem may be partially addressed clustering standard errors at municipal level. However, to properly account for it, Kline and Moretti (2014a) derive an instrumental variable strategy based on lags of population density: the change in population density between periods $t-2$ and $t-3$ should be correlated to the change in population density between $t-1$ and $t-2$, but current population changes should not be correlated with population density twenty years before. Unfortunately, in our context we incur in a weak instrument problem and this IV strategy is not viable, probably because of the sharp changes in municipal population induced by the economic boom of the '50s and the '60s.

Results are recorded in Table 10. In the first three columns we change the set of controls, while the last two columns test whether results are robust to perturbations of the calibrated elasticity of population with respect to wages. Particularly, in Column (1) we add the control variables measured at 1951 listed in Table 2, in Column (2) we add the logarithm of population density at 1951, and in Column (3) we add provincial time trends. In the most demanding specification and with the preferred calibrated parameter for wages of -1.5, the elasticity of agglomeration ranges between 0.353 in high density municipalities and 0.365 in medium density municipalities, suggesting that a 10% increase in population density in the previous decade increases the observed population by 3-4%. More importantly, in all the specifications we cannot reject the null of equal elasticities of agglomeration across density levels. This means that nationwide output is basically insensitive to which municipalities receive the transfers in terms of population density. Thus, targeting public resources to specific areas would not have produced an increase in national output with respect to alternative targeting schemes.

To better disentangle the elasticities of agglomeration between treated and control municipalities, in our last exercise we slightly move from the theoretically grounded regression in (4) and modify it to test *directly* whether connected cities show higher agglomeration elasticity. Namely,

we estimate the following regression:

$$\begin{aligned}\ln Pop_{m,t} - \ln Pop_{m,t-1} = \theta_0 \ln w_{m,t} - \ln w_{m,t-1} + \theta_1 Treated_m + \\ + \theta_2 \ln \Big(\frac{Pop_{m,t-1}}{R_m} - \ln \frac{Pop_{m,t-2}}{R_m} \Big) + \\ + \theta_3 \ln \Big(\frac{Pop_{m,t-1}}{R_m} - \ln \frac{Pop_{m,t-2}}{R_m} \Big) Treated_m + \\ + \theta_4 Controls_{m,1951} + T_t + u_{m,t}\end{aligned} \tag{6}$$

where the notation is the same used above, except for u which is the error term. The coefficient θ_1 captures the direct effect of political connections, θ_2 is the agglomeration elasticity, while θ_3 is the additional effect of political connections and population density. In practice, this regression is equivalent to a test for equal means between elasticities of agglomeration in treated and control municipalities, conditional on control variables at the beginning of the period. The coefficient of interest is θ_3, the interaction term between the treatment and the average elasticity of agglomeration over density levels: if positive, it means that targeting politicians' birthplaces increases nationwide output; if negative, it means that allocating resources according to politicians' birthplaces decreases aggregate output. The model is estimated on the sample of Equation (4). Table 11 presents the results. In all the specifications additional controls are included, as well as decade fixed effects. As in the previous table, the first three columns show the results for different specifications of controls, while the last two provide estimates with alternative calibrated coefficients for the elasticity θ_0. In all the specifications, the interaction term between population density and treatment turns out to be negative, even though imprecisely estimated. Thus, if anything, state-driven investments in connected municipalities had a negative impact on aggregate productivity and total output, suggesting suboptimal localization decisions.

8 Conclusions

In this paper, we leverage the peculiar features of the Italian First Republic (years 1948-1992) to document the local benefits of political connections. At the end of the period, municipalities connected to powerful politicians show a population premium equal to 8 per cent with respect to control municipalities with similar characteristics at the beginning of the period. Indeed, political connections brought advantages for local residents in terms of a larger manufacturing sector, higher employment and wages, even though they displaced private initiatives. Particularly, the politicians of the First Republic managed to locate in their territories of reference important nodes of the transport system and parts of the state industries. However, these benefits did not persist: after the end of the First Republic, the differential population growth rate faded away and con-

nected municipalities began to regress to the levels of socio-economic development of the control municipalities. After 20 years, few signs of the glorious past are still visible.

The lack of persistence in population growth may suggest an inefficient allocation of resources from a nationwide point of view. To test this hypothesis, we borrow from Kline and Moretti (2014a) and examine the agglomeration elasticities of Italian municipalities in the period under examination. Our econometric tests suggest that agglomeration economies were not higher in connected cities compared to control units, so the idea that politicians had directed resources to the territories in a far-sighted way can be discarded.

We believe that this study, grounded in the relatively recent Italian history, have more general and important implications for the future. In recent years, in several Western countries there has been a renewed attention to the issues of public intervention in the economy, with the idea that the state should resume a role of direction abandoning the more neutral role of regulator. Our results suggest caution: in a period of predominant state intervention, the benefits were transient and asymmetrically distributed over the territory. Also, we are not aware of the costs of such benefits. Given its inability to generate self-sustaining growth, this system became financially unviable in the long run and the rising public debt was one of the reasons for its abandonment. Finally, the displacement of private initiative may pose problems for the subsequent transition to more market-oriented institutions, since the lack of an appropriate entrepreneurial class may hamper future economic growth.

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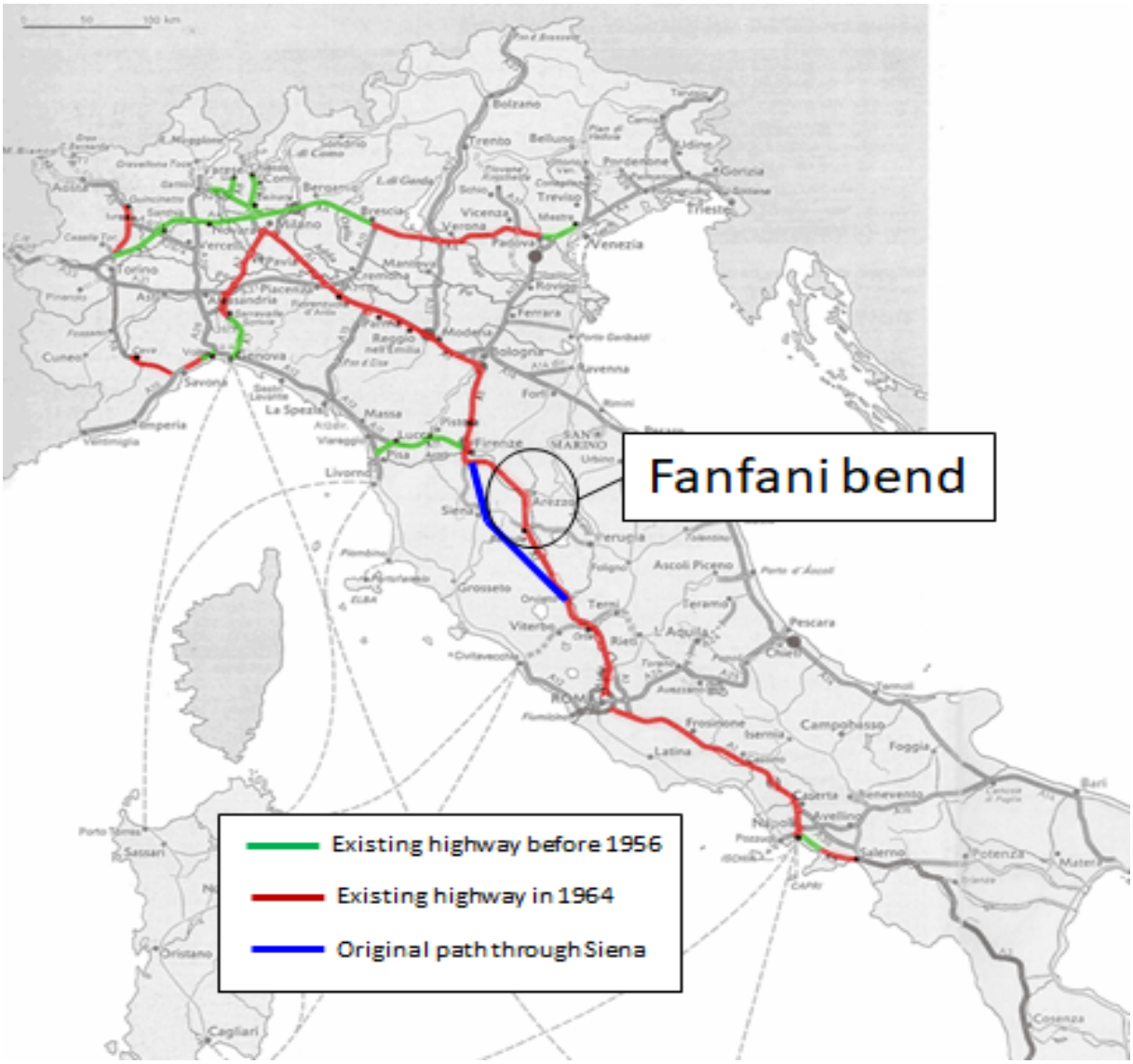
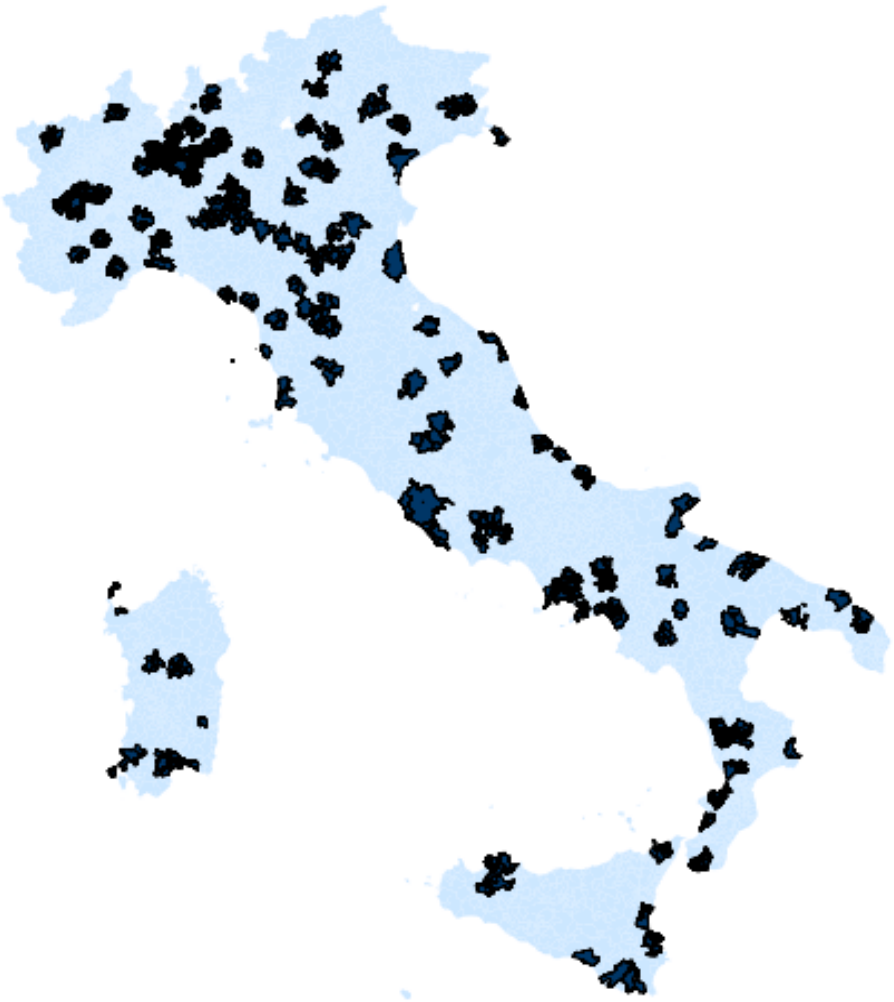
Figure 1: Path of the main Italian highway and the "Fanfani bend"



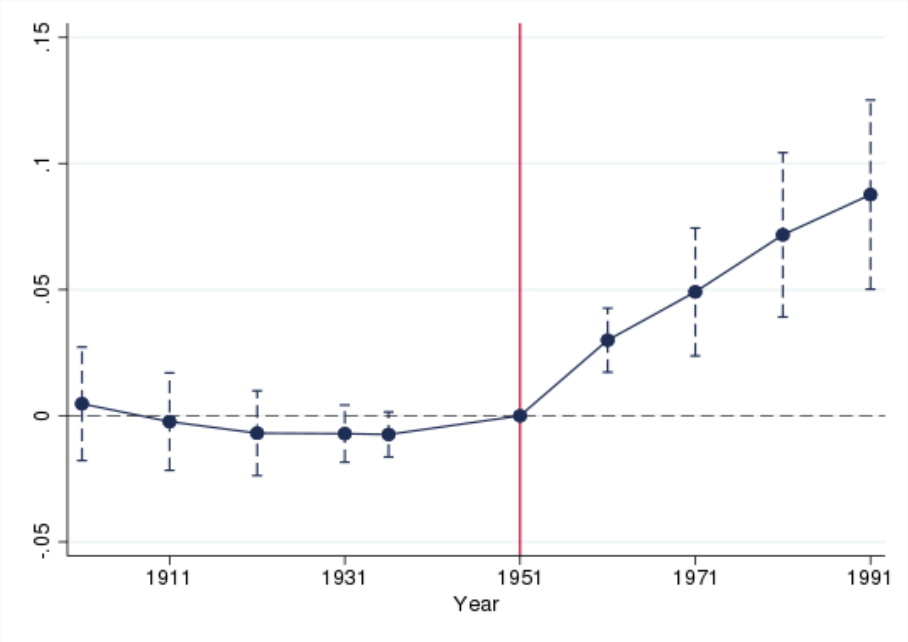
Notes - The highway connecting the city of Milan to Rome was built between 1956 and 1964. The green lines on the map show the existing highways before its construction in 1956. The red lines on the map show the existing highways at the end of that period, in 1964. The blue line shows the planned route through Siena.
Source: Authors' archival research.

Figure 2: Municipalities with state-owned enterprises and their neighboring municipalities in a 10-km radius



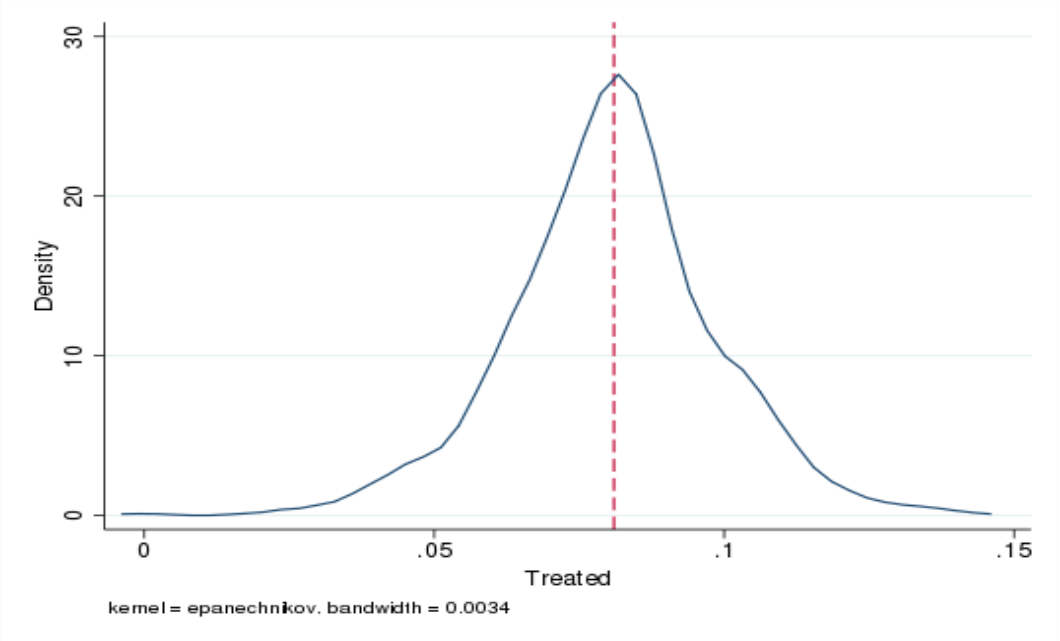
Notes - Dark areas depict municipalities with state-owned enterprises and all their neighboring municipalities in a 10-km radius.
Source: Authors' archival research.

Figure 3: Event study



Notes - Logarithm of municipal population regressed on the interaction between treated municipalities and year dummies, year fixed effects, municipal fixed effects, year dummies interacted with all the control variables at 1951 listed in Table 2. The reference year is 1951. Treated municipalities are the municipalities of birth of party leaders and prime ministers (listed in Table 1) and all their neighboring municipalities in a 10-km radius. Control municipalities are defined as discussed in Section 4.3. Standard errors are robust and clustered at municipal level. 95 percent confidence intervals.
Source: ISTAT, Population census - years 1901, 1911, 1921, 1931, 1936, 1951, 1961, 1971, 1981, 1991; ISTAT, Census of Manufacturing and Services - year 1951; ISTAT, Statistical Atlas of Municipalities - year 1951; Italian Ministry of Interior, Electoral Archives - year 1953.

Figure 4: Simulation exercise

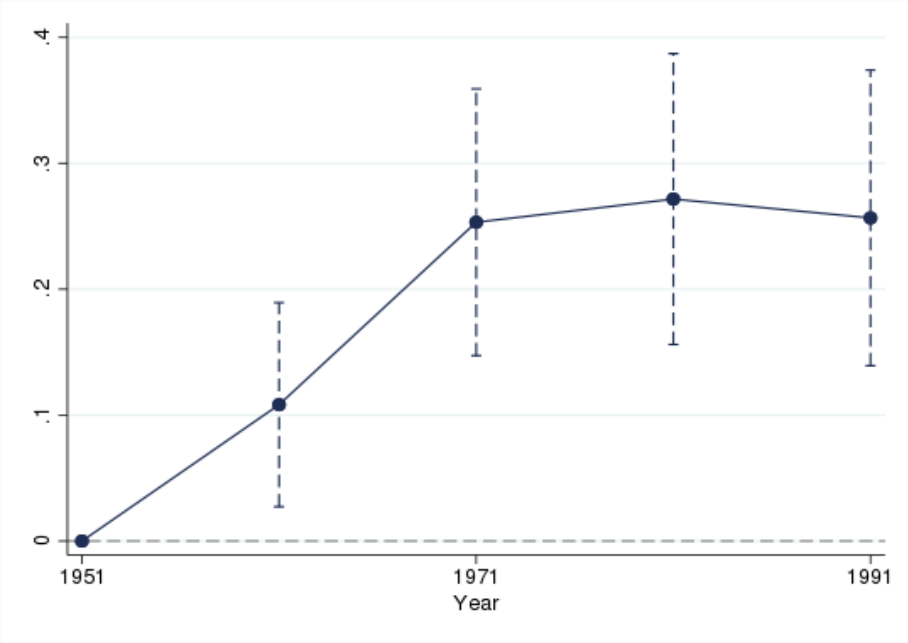


Notes - Simulation of 1,000 treatment effects after removing 5 treated municipalities at random (and their neighboring municipalities in a 10-km radius). Treated municipalities are the municipalities of birth of party leaders and prime ministers (listed in Table 1) and all their neighboring municipalities in a 10-km radius. Control municipalities are defined as discussed in Section 4.3. The vertical line corresponds to the mean value of 0.081. The mean of the standard errors is 0.021. Standard errors are robust and clustered at municipal level.
Source: ISTAT, Population census - years 1951, 1991; ISTAT, Census of Manufacturing and Services - year 1951; ISTAT, Statistical Atlas of Municipalities - year 1951; Italian Ministry of Interior, Electoral Archives - year 1953.

Figure 5: Event study - persistence

Notes - Logarithm of municipal population regressed on the interaction between treated municipalities and year dummies, year fixed effects, municipal fixed effects, year dummies interacted with all the control variables at 1951 listed in Table 2. The reference year is 1951. Treated municipalities are the municipalities of birth of party leaders and prime ministers (listed in Table 1) and all their neighboring municipalities in a 10-km radius. Control municipalities are defined as discussed in Section 4.3. Standard errors are robust and clustered at municipal level. 95 percent confidence intervals.
Source: ISTAT, Population census - years 1901, 1911, 1921, 1931, 1936, 1951, 1961, 1971, 1981, 1991, 2001, 2011; ISTAT, Census of Manufacturing and Services - year 1951; ISTAT, Statistical Atlas of Municipalities - year 1951; Italian Ministry of Interior, Electoral Archives - year 1953.

Figure 6: Event study - logarithm of manufacturing employment



Notes - Logarithm of manufacturing employment regressed on the interaction between treated municipalities and year dummies, year fixed effects, municipal fixed effects, year dummies interacted with all the control variables at 1951 listed in Table 2. The reference year is 1951. Treated municipalities are the municipalities of birth of party leaders and prime ministers (listed in Table 1) and all their neighboring municipalities in a 10-km radius. Control municipalities are defined as discussed in Section 4.3. Standard errors are robust and clustered at municipal level. 95 percent confidence intervals.
Source: ISTAT, Population census - years 1951, 1961, 1971, 1981, 1991; ISTAT, Census of Manufacturing and Services - year 1951; ISTAT, Statistical Atlas of Municipalities - year 1951; Italian Ministry of Interior, Electoral Archives - year 1953.

Table 1: List of connected municipalities

Name	Party	Role	Year birth	Municipality	Province	Region	Area
Altissimo Renato	PLI	Party leader	1940	Portogruaro	Venezia	Veneto	NE
Biasini Oddo	PRI	Party leader	1917	Cesena	Forlí-Cesena	Emilia-R.	NE
Biondi Alfredo	PLI	Party leader	1928	Pisa	Pisa	Toscana	CE
Cappi Giuseppe	DC	Party leader	1883	Castelverde	Cremona	Lombardia	NW
Cariglia Antonio	PSDI	Party leader	1924	Vieste	Foggia	Puglia	SO
Chiostergi Giuseppe	PRI	Party leader	1889	Senigallia	Ancona	Marche	CE
Colombo Emilio	DC	Prime minis.	1920	Potenza	Potenza	Basilicata	SO
Cossiga Francesco	DC	Prime minis.	1928	Sassari	Sassari	Sardegna	SO
De Gasperi Alcide	DC	Both	1881	Pieve Tesino	Trento	Trentino-A A.	NE
De Mita Ciriaco	DC	Both	1928	Nusco	Avellino	Campania	SO
Fanfani Amintore	DC	Both	1908	Pieve Santo Stefano	Arezzo	Toscana	CE
Forlani Arnaldo	DC	Both	1925	Pesaro	Pesaro-Urbino	Marche	CE
Gonella Guido	DC	Party leader	1905	Verona	Verona	Veneto	NE
Goria Giovanni	DC	Prime minis.	1943	Asti	Asti	Piemonte	NW
Jacometti Alberto	PSI	Party leader	1902	San Pietro Mosezzo	Novara	Piemonte	NW
Mancini Giacomo	PSI	Party leader	1916	Cosenza	Cosenza	Calabria	SO
Mondolfo Ugo Guido	PSDI	Party leader	1875	Senigallia	Ancona	Marche	CE
Moro Aldo	DC	Both	1916	Maglie	Lecce	Puglia	SO
Nenni Pietro	PSI	Party leader	1891	Faenza	Ravenna	Emilia-R.	NE
Nicolazzi Franco	PSDI	Party leader	1924	Gattico	Novara	Piemonte	NW
Orlandi Flavio	PSDI	Party leader	1921	Canino	Viterbo	Lazio	CE
Pella Giuseppe	DC	Prime minis.	1902	Valdengo	Biella	Piemonte	NW
Piccioni Attilio	DC	Party leader	1892	Poggio Bustone	Rieti	Lazio	CE
Reale Oronzo	PRI	Party leader	1902	Lecce	Lecce	Puglia	SO
Romita Giuseppe	PSDI	Party leader	1887	Tortona	Alessandria	Piemonte	NW
Rumor Mariano	DC	Both	1915	Vicenza	Vicenza	Veneto	NE
Scelba Mario	DC	Prime minis.	1901	Caltagirone	Catania	Sicilia	SO
Segni Antonio	DC	Prime minis.	1891	Sassari	Sassari	Sardegna	SO
Simonini Alberto	PSDI	Party leader	1896	Reggio nell'Emilia	Reggio nell'Emilia	Emilia-R.	NE
Sommovigo Amedeo	PRI	Party leader	1891	La Spezia	La Spezia	Liguria	NO
Tambroni Fernando	DC	Prime minis.	1901	Ascoli Piceno	Ascoli Piceno	Marche	CE
Tanassi Mario	PSI	Party leader	1916	Ururi	Campobasso	Molise	SO
	PSDI	Party leader					
Terrana Emanuele	PRI	Party leader	1923	Ardore	Reggio di Calabria	Calabria	SO
Vigorelli Ezio	PSDI	Party leader	1892	Lecco	Lecco	Lombardia	NW
Villabruna Bruno	PLI	Party leader	1884	Santa Giustina	Belluno	Veneto	NE
Zaccagnini Benigno	DC	Party leader	1912	Faenza	Ravenna	Emilia-R.	NE
Zoli Adone	DC	Prime minister	1887	Cesena	Forlí-Cesena	Emilia-R.	NE

Notes - Political parties: *DC* - Christian Democratic Party; *PSI* - Socialist Party; *PRI* - Republican Party; *PSDI* - Social-democratic Party; *PLI* - Liberal Party. List of Italian regions included in each geographic area: *North-West (NW)* includes Valle d'Aosta, Piemonte, Liguria and Lombardia; *North-East (NE)* includes Trentino-Alto Adige, Veneto, Friuli-Venezia Giulia and Emilia-Romagna; *Centre (CE)* includes Toscana, Marche, Umbria and Lazio; *South (SO)* includes Abruzzo, Molise, Campania, Puglia, Basilicata, Calabria, Sicilia and Sardegna.

Table 2: Sample characteristics - municipal level, year 1951

	Whole sample			Trimmed sample		
	Treated	Control	P-value	Treated	Control	P-value
	(1)	(2)	(3)	(4)	(5)	(6)
Log population	7.93	7.91	0.785	7.92	7.90	0.763
Pop. growth 1936-51	0.06	0.05	0.066	0.06	0.06	0.307
South	0.26	0.34	0.004	0.26	0.28	0.631
Altitude	0.21	0.23	0.089	0.21	0.21	0.785
Area	32.3	40.6	0.003	32.3	30.4	0.302
Municipal size	1.99	2.01	0.689	1.99	2.02	0.573
Share of workers in manufacturing	0.50	0.46	0.000	0.50	0.50	0.378
Share of workers in construction	0.07	0.07	0.212	0.07	0.06	0.660
Share of workers in private services	0.43	0.46	0.000	0.43	0.44	0.305
Labor market participation	0.49	0.48	0.042	0.49	0.49	0.719
Share of college educated	0.01	0.01	0.000	0.01	0.01	0.113
Ratio pop. over 65 / pop. under 15	0.74	0.72	0.394	0.74	0.73	0.690
Turnout at national elections	0.94	0.92	0.000	0.94	0.94	0.626
Share votes 5 parties in power	0.68	0.68	0.512	0.68	0.69	0.231
Share votes Communist party	0.19	0.17	0.004	0.19	0.19	0.691
Observations	357	6,058		357	4,482	

Notes - Average characteristcs of treated and control municipalities. Treated municipalities are the municipalities of birth of party leaders and prime ministers (listed in Table 1) and all their neighboring municipalities in a 10-km radius. Control municipalities are defined as discussed in Section 4.3. The trimmed sample is obtained after dropping the 26% of control municipalities with the lowest predicted probability of receiving the treatment. *Log population* is the logarithm of municipal population. *Population growth 1936-1951* is the difference between the logarithm of population in 1951 and 1936. *South* is a dummy variable equal to 1 for municipalities in Southern Italy. Southern regions are: Abruzzo, Molise, Campania, Basilicata, Puglia, Calabria, Sicilia and Sardegna. *Altitude* is a continuous variable obtained projecting the difference between the maximum and minimum altitude of the municipality on municipal surface. *Area* is the surface of the municipality in squared kilometers. *Municipal size* ranges between 1 and 3 and refers to the terciles of population distribution. *Share of workers in manufacturing, Share of workers in construction, Share of workers in private services* are the shares of workers in manufacturing, construction and private services. We do not have information on workers in the public sector at 1951. *Labor market participation* is the share of workers out of municipal population. *Share of college educated* is the share of population with college education. *Ratio pop. over 65/pop. under 15* is the ratio between municipal population over 65 years old and municipal population below 15 years old. *Turnout at national elections* is the share of voters at national elections (for lower chamber) in 1953. *Share votes 5 parties in power* is the sum of the share of votes of the 5 parties in power at national elections (for lower chamber) in 1953: the Christian Democratic party, the Italian Socialist party, the Italian Social Democratic party, the Italian Liberal party, and the Italian Republican party. *Share votes Communist party* is the share of votes of the Italian Communist party at national elections (for lower chamber) in 1953.

Table 3: Descriptive statistics

Variable	Year	Unit	Mean	Std. Dev.	Observations	Source
Baseline results and controls at 1951 - Tables 4 to 9						
Population	1951	Ln(units)	7.910	0.990	4,839	Istat
Population growth	1951-1991	Ln(units)	-0.105	0.460	4,839	Istat
Population growth	1936-1951	Ln(units)	0.056	0.132	4,839	Istat
Treated	1951	Dummy	0.074	0.261	4,839	Wikipedia
Municipal size 1	1951	Dummy	0.317	0.465	4,839	Istat
Municipal size 2	1951	Dummy	0.349	0.477	4,839	Istat
Municipal size 3	1951	Dummy	0.334	0.472	4,839	Istat
South	1951	Dummy	0.274	0.446	4,839	Istat
Altimetry	1951	Km	0.206	0.198	4,839	Istat
Surface	1951	Sq-Km	30.53	33.24	4,839	Istat
Workers in manufacturing	1951	Shares	0.497	0.177	4,839	Istat
Workers in construction	1951	Shares	0.064	0.094	4,839	Istat
Workers in services	1951	Shares	0.438	0.167	4,839	Istat
Participation rate	1951	Shares	0.491	0.062	4,839	Istat
College education	1951	Shares	0.007	0.005	4,839	Istat
Ratio old/young population	1951	Units	0.730	0.543	4,839	Istat
Voters' turnout	1953	Shares	0.938	0.043	4,839	Ministry of Interior
Votes parties in power	1953	Shares	0.691	0.148	4,839	Ministry of Interior
Votes Communist party	1953	Shares	0.191	0.135	4,839	Ministry of Interior
Persistence analysis - Table 7						
Population growth	1991-2011	Ln(units)	0.047	0.205	4,839	Istat
Politicians II republic	1991	Dummy	0.028	0.166	4,839	Wikipedia
Economic structure in 1991 and 2011 - Table 8						
Workers in agriculture	1991	Shares	0.138	0.120	4,839	Istat
	2011	Shares	0.088	0.083	4,839	Istat
Workers in manufacturing	1991	Shares	0.395	0.142	4,839	Istat
	2011	Shares	0.318	0.109	4,839	Istat
Workers in services	1991	Shares	0.467	0.124	4,839	Istat
	2011	Shares	0.593	0.101	4,839	Istat
Workers in high tech	1991	Shares	0.020	0.062	4,697	Istat and OECD
and high human capital	2011	Shares	0.017	0.067	4,697	Istat and OECD
Plants per sq-km	1991	Ln(units)	2.153	1.185	4,839	Istat
	2011	Ln(units)	2.286	1.371	4,839	Istat
Workers per plant	1991	Ln(units)	1.160	0.410	4,839	Istat
	2011	Ln(units)	1.059	0.406	4,839	Istat
Per capita wages	1991	Ln(euro)	8.817	1.597	4,488	Inps
	2011	Ln(euro)	9.918	1.321	4,488	Inps
Employment rate	1991	Shares	0.417	0.084	4,839	Istat
	2011	Shares	0.456	0.075	4,839	Istat
Unemployment rate	1991	Shares	0.148	0.115	4,839	Istat
	2011	Shares	0.097	0.057	4,839	Istat
Share enterpreneurs	1991	Shares	0.280	0.082	4,839	Istat
	2001	Shares	0.258	0.067	4,839	Istat
Mechanisms - Table 9						
Provincial road density	1996	ln(km/sq-km)	0.601	0.169	4,839	Istat
Provincial railway density	1996	ln(km/sq-km)	0.086	0.053	4,839	Istat
10-km from SOE	1991	Dummy	0.145	0.353	4,839	Archival sources
10-km from SOE no-utilities	1991	Dummy	0.137	0.344	4,839	Archival sources
Structural analysis - Tables 10 to 11						
Wages	1951-1991	Ln(euro)	4.541	2.881	24,195	Istituto Tagliacarne
Population density	1951-1991	Ln(units)	4.837	1.019	24,195	Istat

Notes - Among the data sources, *Istat* is the Italian National Institute for Statistics and *Inps* is the National Institute for Social Security. The share of workers in services has some missing observations because the Census for Manufacturing and Services did not survey the public sector in 1951. Turnout, share of votes for the parties in power and share of votes for the Communist party refers to the national elections for the lower chamber.

Table 4: Baseline results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.088***	0.076***	0.096***	0.083***	0.089***	0.091***	0.081***
	(0.023)	(0.021)	(0.023)	(0.022)	(0.020)	(0.023)	(0.019)
Observations	4,839	4,839	4,839	4,839	4,839	4,839	4,839
Impact on sd dep. var. (%)	19.1	16.5	20.9	18.0	19.3	19.8	17.6
Log population	No	Yes	No	No	No	No	Yes
Population growth 1936-51	No	Yes	No	No	No	No	Yes
Geographic controls	No	No	Yes	No	No	No	Yes
Sectoral composition	No	No	No	Yes	No	No	Yes
Demographic controls	No	No	No	No	Yes	No	Yes
Voting behavior	No	No	No	No	No	Yes	Yes

Notes - The dependent variable is the log-difference between municipal population in 1991 and 1951. The standard deviation of the dependent variable is 0.460. *Treated* refers to the municipalities of birth of party leaders and prime ministers (listed in Table 1) and all their neighboring municipalities in a 10-km radius. Control municipalities are defined as discussed in Section 4.3. Control variables are listed in Table 2 and are measured in 1951. *Geographic controls* include municipal altitude, area and size and a dummy variable for municipalities in the South. *Sectoral composition* includes the share of workers in manufacturing and construction and labor market participation. *Demographic controls* include the share of population with college education and the ratio between people above 65 and below 15 years old. *Voting behavior* includes the turnout, the share of votes to the 5 parties in power and the share of votes to the Communist party at the 1953 national elections for the lower chamber. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors (in parenthesis) are robust and clustered at municipal level.

Table 5: Interactions

	North vs. South	Distance	Years in Parliament	Google citations
	(1)	(2)	(3)	(4)
Treated*North	0.066***			
	(0.022)			
Treated*South	0.122***			
	(0.037)			
Treated*Farther		0.051**		
		(0.024)		
Treated*Closer		0.111***		
		(0.027)		
Treated*More years			0.082***	
			(0.019)	
Treated*Less years			0.046	
			(0.059)	
Treated*More citations				0.083***
				(0.019)
Treated*Less citations				0.034
				(0.059)
Observations	4,839	4,839	4,839	4,839
Controls at 1951	Yes	Yes	Yes	Yes

Notes - The dependent variable is the log-difference between municipal population in 1991 and 1951. The standard deviation of the dependent variable is 0.460. *Treated*North* refers to the municipalities of birth of party leaders and prime ministers (listed in Table 1) and all their neighboring municipalities in a 10-km radius in the North of Italy. *Treated*South* is the interaction between treated and municipalities in the South. *Treated*Farther* and *Treated*Closer* are defined according to the median distance from treated municipalities. *Treated*More years* and *Treated*Less years* are defined according to the median number of years in Parliament of the connected politicians. Finally, *Treated*More citations* and *Treated*Less citations* are defined according to the median number of Google citations of the connected politicians. Control municipalities are defined as discussed in Section 4.3. Control variables are listed in Table 2 and are measured in 1951. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors (in parenthesis) are robust and clustered at municipal level.

Table 6: Robustness checks

Row	Robustness	Treated	Observations	Impact on std. dev. dep. var. (%)
(1)	Without cities > 99th perc.	0.108***	3,735	24.4
		(0.024)		
(2)	Trimming of dep. variable	0.080***	4,788	18.4
		(0.018)		
(3)	Cut sample <30th perc.	0.080***	4,597	17.4
		(0.019)		
(4)	15 km treatment	0.060***	4,326	12.9
		(0.013)		
(5)	Only prime ministers	0.105***	4,631	22.8
		(0.030)		
(6)	Only party leaders	0.065***	4,759	14.1
		(0.019)		
(7)	Cluster at province	0.081**	4,839	17.6
		(0.037)		
(8)	Cluster at 10-km radius	0.081***	4,839	17.6
		(0.028)		
(9)	PS Matching (kernel)	0.087***	4,839	18.9
		(0.023)		
(10)	Oaxaca-Blinder	0.082***	4,839	17.8
		(0.023)		
	Controls at 1951	Yes		

Notes - The dependent variable is the log-difference between municipal population in 1991 and 1951. In Row (2) the dependent variable is trimmed at percentiles 1 and 99. In Rows (1)-(3) and (7)-(10), *Treated* refers to the municipalities of birth of party leaders and prime ministers (listed in Table 1) and all their neighboring municipalities in a 10-km radius. In Row (4) *Treated* refers to the municipalities of birth of party leaders and prime ministers (listed in Table 1) and all their neighboring municipalities in a 15-km radius. In this regression, the municipalities between 15 and 30 kilometers from the municipality of birth of a connected politician are removed from the sample. In Row (5) *Treated* refers to the municipalities of birth of prime ministers and all their neighboring municipalities in a 10-km radius. In this regression, the municipalities of birth of party leaders and their neighboring municipalities in a 10-km radius are removed from the sample. In Row (6) *Treated* refers to the municipalities of birth of party leaders and all their neighboring municipalities in a 10-km radius. In this regression, the municipalities of birth of prime ministers and their neighboring municipalities in a 10-km radius are removed from the sample. The propensity score matching in Row (9) is run adopting a kernel algorithm. Control municipalities are defined as discussed in Section 4.3. Control variables are listed in Table 2 and are measured in 1951. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors (in parenthesis) are robust and clustered at municipal level.

Table 7: Persistence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	0.009	0.008	0.010	0.009	0.012	0.014	0.012	0.011
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Politicians 2nd Republic								0.012
								(0.015)
Observations	4,839	4,839	4,839	4,839	4,839	4,839	4,839	4,839
Impact on sd dep. var. (%)	4.4	3.9	4.9	4.4	5.8	6.8	5.8	5.4
Log population	No	Yes	No	No	No	No	Yes	Yes
Population growth 1936-51	No	Yes	No	No	No	No	Yes	Yes
Geographic controls	No	No	Yes	No	No	No	Yes	Yes
Sectoral composition	No	No	No	Yes	No	No	Yes	Yes
Demographic controls	No	No	No	No	Yes	No	Yes	Yes
Voting behavior	No	No	No	No	No	Yes	Yes	Yes

Notes - The dependent variable is the log-difference between municipal population in 2011 and 1991. The standard deviation of the dependent variable is 0.205. *Treated* refers to the municipalities of birth of party leaders and prime ministers (listed in Table 1) and all their neighboring municipalities in a 10-km radius. Control municipalities are defined as discussed in Section 4.3. Control variables are listed in Table 2 and are measured in 1951. *Geographic controls* include municipal altitude, area and size and a dummy variable for municipalities in the South. *Sectoral composition* includes the share of workers in manufacturing and construction and labor market participation. *Demographic controls* include the share of population with college education and the ratio between people above 65 and below 15 years old. *Voting behavior* includes the turnout, the share of votes to the 5 parties in power and the share of votes to the Communist party at the 1953 national elections for the lower chamber. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors (in parenthesis) are robust and clustered at municipal level.

Table 8: Economic structure in 1991 and 2011

Dependent variable	1991	Impact on std. dev. dep. var. 1991 (%)	2011	Impact on std. dev. dep. var. 2011 (%)	Observations
	(1)	(2)	(3)	(4)	(5)
Share workers agriculture	-0.037***	30.8	-0.019***	22.9	4,839
	(0.005)		(0.004)		
Share workers manufacturing	0.032***	22.5	0.012***	11.0	4,839
	(0.006)		(0.004)		
Share workers services	0.004	3.2	0.007	6.9	4,839
	(0.005)		(0.005)		
Share workers high tech	-0.001	1.6	-0.001	1.5	4,697
	(0.003)		(0.003)		
Plants' density	0.250***	21.1	0.287***	20.9	4,839
	(0.046)		(0.067)		
Workers per plant	0.067***	16.3	0.008	2.0	4,839
	(0.021)		(0.020)		
Per capita wages	0.214***	13.4	0.089	6.7	4,488
	(0.073)		(0.062)		
Employment rate	0.010***	11.9	0.006**	8.0	4,839
	(0.002)		(0.002)		
Unemployment rate	-0.005**	4.3	0.003*	5.3	4,839
	(0.003)		(0.002)		
Share enterpreneurs	-0.033***	40.2	-0.021***	31.3	4,839
	(0.003)		(0.003)		
Controls at 1951	Yes		Yes		

Notes - Each row reports the regression coefficients of a different dependent variable on treated municipalities and a set of control variables. Column (1) reports the regression results measuring the dependent variables in 1991, while Column (3) reports the regression results measuring the dependent variables in 2011. Treated municipalities are the municipalities of birth of party leaders and prime ministers (listed in Table 1) and all their neighboring municipalities in a 10-km radius. Control municipalities are defined as discussed in Section 4.3. The *Share of workers in agriculture, manufacturing and services* range between 0 and 1. *Share workers high tech* refers to the share of workers in high technology and high human capital industries, as defined by OECD. Since not all the municipalities have workers in high technology industries, there are some missing observations. Thus, we restrict the sample to muincipalities with high technology industries both in 1991 and 2011. *Plants' density* is the logarithm of the number of plants per squared kilometer. *Workers per plant* is the logarithm of the number of workers per plant in the municipality. *Per capita wages* is the logarithm of total monthly wages divided by the population of the corresponding year. These data come from the National Social Security Institute Database (INPS) collecting data on wages for the universe of employees. Because of difficulties in data cleaning, we ended up with some missing observations. Thus, we restrict the sample to municipalities with nonmissing data both in 1991 and 2011. *Employment rate* and *Unemployment rate* range between 0 and 1. Finally, *Share enterpreneurs* is the share of enterpreneurs out of total workers. This value is not available for 2011, and the coefficient in Column (3) is based on a regression using data at 2001. The standard deviations of the dependent variables are reported in Table 3. Columns (2) and (4) report the percentage impact of the estimated coefficients on the standard deviations of the dependent variables. All the regressions include control variables measured in 1951. Control variables are listed in Table 2. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors (in parenthesis) are robust and clustered at municipal level.

Table 9: Mechanisms

	Road density (1)	Railway density (2)	10-km from SOE (3)	10-km SOE (no utilities) (4)
Treated	0.055***	0.018***	0.152***	0.141***
	(0.008)	(0.003)	(0.024)	(0.024)
Observations	4,839	4,839	4,839	4,839
Impact on sd dep. var. (%)	32.5	34.0	43.1	41.0
Controls at 1951	Yes	Yes	Yes	Yes

Notes - The dependent variable in Column (1) is the length of highways, national, regional and provincial roads per square kilometer in 1996. This variable is not available at municipal level and is constructed at provincial level. The dependent variable in Column (2) is the length of railways per square kilometer in 1996. This variable is not available at municipal level and is constructed at provincial level. The dependent variable in Column (3) is a dummy variable equal to 1 if the municipality is in a 10-kilometer radius from a state-owned enterprise (SOE). The dependent variable in Column (4) is similar to Column (3) but excludes utility companies. The standard deviations of the dependent variables are reported in Table 3. *Treated* refers to the municipalities of birth of party leaders and prime ministers (listed in Table 1) and all their neighboring municipalities in a 10-km radius. Control municipalities are defined as discussed in Section 4.3. Control variables are listed in Table 2 and are measured in 1951. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors (in parenthesis) are robust and clustered at municipal level.

Table 10: Structural estimates of agglomeration function

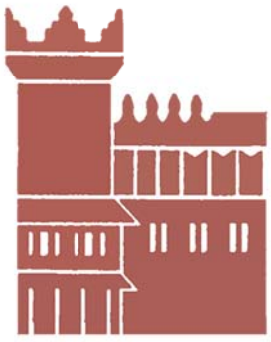
	(1)	(2)	(3)	(4)	(5)
Low density	0.265***	0.262***	0.361***	0.374***	0.347***
	(0.028)	(0.028)	(0.028)	(0.028)	(0.029)
Medium density	0.301***	0.302***	0.365***	0.399***	0.330***
	(0.020)	(0.020)	(0.019)	(0.017)	(0.021)
High density	0.287***	0.301***	0.353***	0.386***	0.319***
	(0.021)	(0.022)	(0.018)	(0.015)	(0.021)
Treated	0.026***	0.027***	0.029***	0.021***	0.036***
	(0.005)	(0.005)	(0.005)	(0.004)	(0.006)
P-value equal slopes	0.473	0.375	0.870	0.545	0.696
Observations	19,356	19,356	19,356	19,356	19,356
Wage coefficient	1.5	1.5	1.5	1	2
Decade fixed effects	Yes	Yes	Yes	Yes	Yes
Controls at 1951	Yes	Yes	Yes	Yes	Yes
Density at 1951	No	Yes	Yes	Yes	Yes
Provincial time trends	No	No	Yes	Yes	Yes

Notes - The dependent variable is the first difference in the logarithm of municipal population. *Low density* refers to municipalities below the 18th percentile of the distribution of the logaritmic municipal density in 1951, *Medium density* refers to municipalities between the 18th and the 62nd percentile of the distribution of the logaritmic municipal density in 1951 and *High density* refers to municipalities above the 62nd percentile of the distribution of logaritmic municipal density in 1951. *Treated* refers to the municipalities of birth of party leaders and prime ministers (listed in Table 1) and all their neighboring municipalities in a 10-km radius. Control municipalities are defined as discussed in Section 4.3. Control variables are listed in Table 2 and are measured in 1951. The wage coefficient is calibrated at 1.5 in Columns (1)-(3), at 1 in Column (4) and at 2 in Column (5). The 4,839 municipalities in the sample are observed for 4 periods: 1991-1981; 1981-1971; 1971-1961; 1961-1951. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors (in parenthesis) are robust and clustered at municipal level.

Table 11: Structural estimates of agglomeration function - treated vs control municipalities

	(1)	(2)	(3)	(4)	(5)
Log density	0.289***	0.293***	0.362***	0.389***	0.335***
	(0.016)	(0.016)	(0.016)	(0.015)	(0.018)
Treated	0.026***	0.027***	0.029***	0.021***	0.037***
	(0.005)	(0.005)	(0.005)	(0.004)	(0.006)
Treated*Log density	-0.043	-0.044	-0.033	-0.024	-0.043
	(0.034)	(0.034)	(0.034)	(0.028)	(0.041)
Observations	19,356	19,356	19,356	19,356	19,356
Wage coefficient	1.5	1.5	1.5	1	2
Decade fixed effects	Yes	Yes	Yes	Yes	Yes
Controls at 1951	Yes	Yes	Yes	Yes	Yes
Density at 1951	No	Yes	Yes	Yes	Yes
Provincial time trends	No	No	Yes	Yes	Yes

Notes - The dependent variable is the first difference in the logarithm of municipal population. *Log density* is the first difference in the logarithm of municipal population divided by municipal surface and lagged one decade. *Treated* refers to the municipalities of birth of party leaders and prime ministers (listed in Table 1) and all their neighboring municipalities in a 10-km radius. Control municipalities are defined as discussed in Section 4.3. *Treated*Log density* is an interaction variable. Control variables are listed in Table 2 and are measured in 1951. The wage coefficient is calibrated at 1.5 in Columns (1)-(3), at 1 in Column (4) and at 2 in Column (5). The 4,839 municipalities in the sample are observed for 4 periods: 1991-1981; 1981-1971; 1971-1961; 1961-1951. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors (in parenthesis) are robust and clustered at municipal level.



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