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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 focus on population, a well-celebrated proxy of local development in the long run, and compare connected cities – small areas surrounding birthplaces of both prime ministers and leaders of the parties in power – with unconnected municipalities that show, thanks to a propensity score matching procedure, very similar baseline characteristics, including lagged outcome. Our results indicate that politically connected cities gained a population premium of 7.4% between 1951 and 1991. When the connection ends, the difference in growth rate fades away. We also document that birthplaces of powerful politicians benefit from infrastructure investments, other ordinary and special-purpose public expenditures, and the location of plants by state-owned enterprises. The political connection favors industrialization, and raises employment and wages, but crowds out private entrepreneurship. The paper also illustrates that local communities repay the benefits gained through voting. Finally, it turns out 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 over a fourty-year time span, providing also evidence about (i) the structural change of the treated local economies, (ii) the electoral payoffs for the connecting politicians/parties, (iii) the persistence of the effect after the end of the treatment, (iv) the inefficiency of the politically biased spatial allocation of resources across cities. 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 7.4% over 40 years (17% of the standard deviation of the dependent variable). When looking at the mechanisms behind this result, we find that connected areas disproportionally benefited from the post-WWII development of the transport network and from higher transfers from the central government. Also, state-owned enterprises were more likely to be located near to the connected cities. As a result, treated municipalities experienced a structural change of the local economy: at the sunset of the First Republic, connected cities show a higher degree of industrial-

ization, higher wages, higher employment but a lower share of enterpreneurs. We then look at the electoral payoffs for such interventions and we find that on average in treated municipalities the parties of the connecting politicians increased their shares of votes at the expenses of the other five parties in power. The abrupt end of the treatment in the first part of the nineties also allows us to study the evolution of the connection premium in the following 20 years, from 1991 to 2011. We find that the difference in population growth between treated and control municipalities disappears in the long run, suggesting that political connections did not induce self-sustaining growth. 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 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

The benefits from political connections accruing to firms and/or individuals have been long recognized as a potential source of distortion of the free market functioning (Knight, 2006; Fisman, 2001; Faccio, 2006; Khwaja and Mian, 2005; Claessens et al., 2008; Cingano and Pinotti, 2013; Gagliarducci and Manacorda, 2020). Nevertheless, these connections can influence, through spillover effects, other economic agents than those directly involved. For instance, local public good provision (e.g. infrastructures), which is partially non-rival and non-excludable in nature, may benefit a larger number of agents than those originally targeted. To the same extent, firm subsidies may trickle down to other firms through input-output linkages or to individuals through changes in the labor demand. Thus, if these spillovers have a relevant spatial nature (as in the case of local public good provision), analyzing the impact of political connections at the aggregate local level may be more appropriate than focusing on single agents. In this perspective, some papers have recently documented that politicians do favor some targeted places by means of pork barrel transfers. This has been shown for both democracies and autocracies, as well as for countries with different degrees of economic development (Baskaran and da Fonseca, 2021; 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 on regional growth and several questions are still underexplored: do connections spur local economic growth? If so, what is the underlying mechanism? Does favoritism change the structure of the targeted local economy? Is there an electoral gain for politicians? Does the politicians' aid shape the structure of the targeted local economy? Are possible gains persistent when the connection switches off? Is the connection welfare-enhancing from a spatial general equilibrium point of view?

This paper contributes to answering these questions by offering new evidence based on Italian data. To do so, we exploit the peculiar Italian institutional background between the end of the World War II (WWII) and the fall of the Berlin wall, known in the journalistic and political jargon as First Republic: a political system grounded on five political parties sharing an anti-communist stance. Indeed, after the end of the Fascist dictatorship, Italy was designed as a Parliamentary Republic with a proportional electoral system. The first democratic elections in 1948 led to a new political class that stayed in power for over 40 years. 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 focus on population 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. We focus on small- and medium-sized cities for at least three reasons: (i) in large metropolitan areas potential confounders correlated both to the treatment and to the outcome (e.g. human capital spillovers) are likely to be at work, (ii) almost all large cities are treated, and (iii) their growth depends on a substantial number of factors so that the detectability of the connection effect might be very difficult. Then, we regress the log difference in population between 1951 and 1991 against a treatment dummy and a vector of municipal characteristics observed in 1951. To provide a credible comparison between treated and control municipalities, we exclude from the control group the municipalities with the lowest predicted probability of receiving the treatment estimated by means of a propensity score. In this way, we make sure that treated and control units have the same baseline observable characteristics in terms of geographic, demographic, socio-economic and political variables, sectoral composition of the local economy and, notably, past population growth. Time invariant omitted variables at the municipality level are implicitely controlled for by estimating the model in first difference. Reverse causality, i.e. the possibility that fast growing municipalities were more likely to be the birthplaces of a political leader in power or to place one of its inhabitant as an influential leader, seems unlikely for at least three reasons. First, all but two (out of 37) relevant politicians were born before 1930, when World War II, the end of the Fascist dictatorship and the following economic boom were impossible to forecast. Second, we select the control group of municipalities to balance pre-treatment population trends, to avoid comparing municipalities with very different ex-ante population growth potential. Third, the focus on small- and medium-sized municipalities should also prevent this possibility: the median municipality in our sample has roughly 2,600 inhabitants and municipalities of such a limited size should not be able to influence political dynamics at national level.

Our findings indicate the existence of a positive and sizeable connection premium, equal to 7.4%, 17% of the standard deviation of the dependent variable. The estimated impact is larger for municipalities located in the South of the country – the lagging area featured with weaker

institutions – and for those connected to more powerful politicians (proxied by the duration of their office as elected members of Parliament or through their Google citations). Many sensitivity checks regarding the selection of the sample, the clustering of standard errors, and the identification strategy reassure as to the robustness of our core result. This is confirmed even if we exploit the staggered treatment adoption based on the timing of the politician's rise to power.

Around this core finding, we use variations of the same empirical setting to provide some additional pieces of evidence. The first result is about the mechanisms behind the connection premium: we find that connected areas disproportionally benefited from the post-WWII development of the transport network, and that state-owned enterprises were more likely to be located near to the connected cities; we also document that being linked to an influential politician led to higher transfers from the centeral government, both for local government operating expenses and for national programs aiming at some regional redistribution of resources. Second, we find that higher population growth in treated municipalities was coupled with the structural change of the local economy: at the sunset of the First Republic, connected cities show a higher degree of industrialization (but not in the high-tech sectors), higher wages and higher employment rates. The downside was a toll on entrepreneurship: the share of entrepreneurs out of the total number of workers is lower in connected places. Third, we move to analyze the politicians' payoff: it turns out that over the whole period of the First Republic, in treated cities the party of the connecting politician gained votes at the expenses of the other parties in power, while no effect is detected with respect to the share of the Communist party, whose electoral consensus was strongly driven by ideological reasons. Fourth, we study persistence: the abrupt end of the treatment in the first part of the nineties allows us to study the evolution of the connection premium in the following 20 years, from 1991 to 2011. We find that the difference in population growth between treated and control municipalities disappears in the long run, suggesting that political connections did not induce self-sustaining growth. Finally, we turn 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 or specific input shortages in their hometowns. Under these conditions, moving economic activity towards connected places would lead to higher aggregate output, providing a rationale for place-based policies (Bartik, 2020). Again, the similarity in observable characteristics between treated and control municipalities suggests that this is unlikely to be the case. Building on Kline and Moretti (2014a)'s setting, we also test for the presence higher agglomeration economies in treated municipalities and 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 links with 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 together with the sensitivity analysis. Section 6 is devoted to ancillary results: it provides evidence on (i) the mechanisms behind our results, (ii) the structural change linked to the connection, (iii) the electoral payoffs for the influential politicians, (iv) the lack of persistence and (v) the nationwide general equilibrium considerations. Finally, some concluding remarks are shown in Section 7.

2 Links with the related literature

Our paper is mainly related to the literature on the impact of political connections at the city level. The 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 1992-2009 period and find that the hometowns of political leaders systematically experience higher nighttime light intensity than other locations; this result is 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 shows that political connections favor higher private sector employment, higher share prices of firms, and increased nighttime lighting.[1]

We add to the existing studies in several ways. We show that the growth premium holds also in an advanced economy, and we do so on the basis of a longer time span, so that our result is more likely to capture the steady state spatial equilibrium. Moreover, this is also the first paper showing results on (i) the structural change of the treated local economies, (ii) the electoral payoffs for the connecting politicians/parties, (iii) the persistence of the effect after the end of the treatment, (iv) 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), Germany (Baskaran and da Fonseca, 2021), 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), and 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 public transfers and SOE allocation.

[1] 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).

Finally, we also speak to the economic geography literature dealing with 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.

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 the 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 was characterized by a high degree of stability as to which parties were in power. 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 Liberale 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 competition between the five ruling parties on the one hand and the Communist Party on the other hand was marked by strong ideological contrasts: the five governing parties shared a liberal-democratic approach and promoted a market economy, although balanced by a large role for the State; on the contrary, the Communist party was still tied to collectivist forms of economic organization. The electoral system in force was a proportional one with open lists. Proportionality guaranteed that, given the underlying voters' political preferences, the five non-communist parties always succeeded in remaining in power by forming coalition governments, while the existence of open lists incentivized politicians to maximize the number of preferences received in their own electoral districts. Political stability continued up to the first years of the nineties,

when the Cold War ended, 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).

During the First Republic, the parliamentary system (in which a coalition of parties with a majority in the parliament forms the government) and the continuous bargaining among the five parties led to a very high number of governments (45 between 1948 and 1992). However, since the parties in the ruling coalition were still the same, this large government turnover was associated 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* ("partycracy", meaning 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%). Over the same period, the number of state-owned companies ranged roughly from 15% to 20% of the total number of listed companies (Aganin and Volpin, 2007).

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, which were strongly needed in that historical period and, at the same time, had well-identified beneficiaries. A notable example is the so called "Fanfani bend". Between 1956 and 1964, in the midst of its 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 the country. According to the original project, the route should have passed through Siena, a middle-sized city between Florence and Rome but 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 (Figure 1). This way, he was able to kill two birds with one stone: rewarding his electoral turf of Arezzo and inflicting a blow to Siena, that was governed by the Communist party. 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. (2022) 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.[2]

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 the National Hydrocarbons Authority (ENI, 1953, with the task of coordinating the 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.[3] 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 significantly 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). 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 municipalities (including other municipalities within a

[2]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).

[3]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.

10-km radius) with SOE establishments during the First Republic.

Public ownership of industrial enterprises was accompanied by programs designed to achieve special goals. For instance, the Plan INA-Casa, inspired by the British "Beveridge Plan", was implemented over the period 1949-1963, and aimed at providing good quality housing to working class people. A crucial feature of this plan was that its territorial distribution was uneven, with national politicians who played an important role in selecting where to build the new housing settlements (Carmignani et al., 2021). Another notable public intervention was a large place-based policy, which was launched under the auspices of the World Bank. The program was managed by a state-owned agency (*Cassa per il Mezzogiorno*, created in 1950) and conveyed large amounts of financial resources (predominantly but not exclusively) towards backward areas of Southern Italy. Interestingly, the program was initially led by a technocratic steering committee, but over the years the management shifted in the hands of politicians (Felice and Lepore, 2017), thus representing another channel potentially available for transferring resources to local communities.

In the early 1990s, the rising public debt and the increasing fiscal pressure 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 few years, many state-owned enterprises were privatized. With the start of the European regional policy, the *Cassa per il Mezzogiorno* lost its central role (it was definitively abolished in 1992). 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.

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 differs 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", 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). Given the structure of the political power during the First Republic, it is somehow arbitrary to define the exact year in which the treatment switches on because 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 empirical strategy in which we compare population dynamics between treated and control municipalities over the whole of the First Republic. Nevertheless, in an extensive robustness check we show that even if we adopt a staggered difference-in-differences (DID) estimation strategy, which exploits different cohort-specific starting points for the treatment, we obtain similar results. Finally, we exploit the two census waves in 2001 and 2011 to evaluate whether the impact detected for the treatment period survives after the end of the First Republic.

4.2 Preliminary sample construction and treatment definition

We preliminarily exclude from the sample large cities (above 200,000 inhabitants in 1951) for the following reasons. First, in large metropolitan areas potential confounders correlated both to the treatment and to the outcome (e.g. human capital spillovers) are likely to be at work.[4] Second, the vast majority of large cities are the birthtown of an influential politicians. Thus, since almost all of them are treated it is not possible to find compelling comparison municipalities for them. Finally, since growth in large cities depends on a substantial number of factors, the treatment is probably not intense enough relative to the size of the city to have a considerable effect on growth. We also drop municipalities within a 20-km radius from these big cities, since in the period under examination they might have benefited from the agglomeration economies triggered by their neighboring metropolitan areas.[5]

Connected cities are defined as the municipalities of birth of prime ministers and/or leaders of the five parties in power between 1948 and 1992. Differently from Hodler and Raschky (2014),

[4]For instance, 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 that we are not interested in favoritism per se, but in its economic consequences, for which the political bias effect is likely to be blurred by too many confounders.

[5]Further preliminary sample selections are as follows. Municipalities with missing observations in control variables are dropped from the sample (approximately 300 municipalities). We also drop municipalities with values of the population growth rate between 1951 and 1991, our main dependent variable of interest, below the first and above the 99^{th} percentile of its distribution in 1951. This way, we avoid including outlier observations whose growth is likely to stem from mergers among cities.

who consider prime ministers (or similar) only, we also include party leaders because of the pivotal role of parties in power discussed in Section 3. Overall, there are 18 prime ministers and 46 party leaders, corresponding to 57 influential politicians.[6] After excluding two politicians born abroad, as well as those born in cities with over 200,000 inhabitants or in their surrounding areas (within a 20-km radius), we are left with 37 powerful politicians and 33 distinct municipalities of birth. Table 1 displays the names of these politicians and lists the corresponding birthplaces. 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.

Our operationalization of connected city deserves some more comments. First, we clarify why politicians should direct their favors right back to their birthplaces. While personal and parochial motives is an immediate answer (Baskaran and da Fonseca, 2021), another possibility is rooted in the electoral rules: as stated above, under the proportional system with open lists, only the politicians scoring the highest number of personal votes were elected in the Parliament; thus, powerful politicians had strong incentives to reward their core voters and to find ways to retain a large number of preference votes in their areas of influence (hometowns and the surrounding municipalities are usually included in the voting districts in which politicians ran). Second, 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 minister or party leader, 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. If this is the case, we might incur in a type-II error and our estimate will be a lower bound of the true effect.[8] 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 makes very unlikely the possibility of municipal selection into the treatment, i.e. municipalities with higher growth potential before 1951 were more likely to place 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,

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

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

[8] A further potential source of measurement error, translating into a downward bias, may occur if a politician was born in a different municipality with respect to the actual municipality of residence.

given the forthcoming WWII. It was also almost impossible to predict which political parties would have gained momentum. 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.

4.3 Designing a proper control group

The design of a proper control group comes in two steps. The first one addresses concerns about spatial spillovers. As stated in the previous subsection, municipalities in a 10-km radius from the birthplaces are treated; needless to say, we cannot fully rule out the possibility that the spillovers' range is slightly larger than 10 km: in such a case, we would erroneously classify treated units as controls. Hence, we exclude from the sample municipalities in a radius between 10 and 20 kilometers from the politicians' municipalities of birth.

Second, treated and control municipalities must have similar observable characteristics at the beginning of the First Republic in order to credibly identify the effect of political connections on future population growth. 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). 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 parties in power and for the Communist party at the beginning of the period. It turns out that the two samples are not well balanced. For example, treated municipalities are less likely to be located in Southern Italy, have a higher population density, etc.

To address the balancing requirement, we follow Kline and Moretti (2014a) and 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 30% of municipalities in the control group, all the main characteristics are balanced at the baseline (Table 2, Columns (4)-(6)). It is worth noting that the balancing condition on the past population growth reassures about parallel trends before the start of the treatment and 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 parties and the Communist party is central to rule out the possibility of systematic correlations between connected politicians and local political preferences (e.g. connections endogenously

emerging to contrast the local communist threat). Figure 3 shows the map of the municipalities excluded from the sample (in white) and distinguishes between treated (in green) and control (in orange) municipalities.

4.4 Data and descriptive statistics

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

Population data at municipal level come from various waves of the Population Census, 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, and the last one is 2011. The municipal surface, the participation rate, the share of college educated, and the old-to-young population ratio are also taken from population censuses and are available up to 2011. Data on geographical characteristics are drawn from the Statistical Atlas of Municipalities provided by Istat, as well as information on municipal altitude and area.[9] The geographical coordinates to compute the distance between municipalities are also provided by Istat. Data on employment by sector, and on number of plants at the municipality level come from various waves of the Istat Census of Manufacturing and Services (some variables are available only from 1971 on). Data on voter turnout at general elections and party vote shares at the baseline (in 1953) and at the end of the period (in 1987) are drawn from the electoral archive of the Italian Ministry of the Interior. Information about politicians' tenure as members of Parliament are taken from a database of the "Fondazione Rodolfo de Benedetti". Data on per capita wages in 1991 and 2011 at provincial level are drawn 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 novel dataset we built for this project as follows. We start from the list of all Italian main privatizations from 1985, made available at Privatizationbarometer. This information is complemented with data taken from (i) Amatori (2013), (ii) a paper prepared by Mediobanca (an Italian leading investment bank), (iii) a list of the companies belonging to the EFIM group provided by the Italian Parliament, and (iv) the "Imita.db", a dataset on Italian firms managed by the University of Siena.[10] From these

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

[10] 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`; Imita.db

sources we have a long list of state-owned firms; then, for each firm, we hand-collect the location of the headquarters and the plants from Internet in order to attach a SOE label to municipalities. Data on the Plan INA-Casa program come from the archives of the Ministry of Labor, while those relative to the *Cassa per il Mezzogiorno* program are drawn from the "Archive of Local Economic Development" managed by the Ministry of Culture. Data on municipal budgets come from the archives of the Minister of the Interior.

Finally, for the structural analysis in Subsection 6.5, 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. 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. We start from the equation:

$$ln(pop)_{m,t} = \lambda_m + a_0 Post_t + a_1 Treated_m Post_t + \eta_{m,t} \tag{1}$$

where the dependent variable is the logarithm of population at municipality-year level. $t = 1951$, 1991 and $Post$ is a dummy equal to 1 if $t = 1991$ and 0 if $t = 1951$. $Treated_m$ is a dummy variable equal to 1 for treated units (as defined above) and 0 otherwise while a_1 is the coefficient of interest. λ_m are municipality fixed effects and $\eta_{m,t}$ is the usual error term. As explained above, we prefer not to include years in between (1961, 1971, and 1981) because we cannot exactly define the exact year of start of the treatment. After first-differencing Equation (1) we obtain our estimating equation:

$$dln(pop)_{m,1951-1991} = \alpha_0 + \alpha_1 Treated_m + \varepsilon_m \tag{2}$$

where the parameter α_1 in Equation (2) causally measures the connection premium in terms of log population. In the baseline estimates we also control for municipality-specific trends in the observable characteristics at 1951 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. This should account for time-varying omitted factors. First differencing the logarithm of population we control for time-invariant municipality level features. Standard errors are obtained from a spatial HAC variance estimator based on the technique of Conley (1999) using

dataset: `http://imitadb.unisi.it/iscrizione.asp`. A full description of the Imita.db dataset is in Vasta and Giannetti (2006). The list of EFIM companies is available from the authors upon request.

a 10-kilometer bandwidth. The estimation is run on the propensity score-based sample described in Section 4.3 and the main econometric challenges related to the estimation of this equation have already been discussed in Sections 4.2 and 4.3.

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).

5.1 Baseline results

Results on the estimation of Equation (2) are reported in Table 4. Column (1) displays the estimated coefficient of interest without adding controls. The statistically significant coefficient of 0.082 suggests that on average, during the 40 years of the First Republic, the population of connected municipalities grew roughly 8% 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 0.074 in the most demanding and preferred one (17% of the standard deviation of the dependent variable).

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). In addition, this area has disproportionately benefited from a substantial spending program devoted to it (the *Cassa per il Mezzogiorno* program, see Section 3). The estimated effect is around 5.4% in the Centre-North and 12.9% in the South, suggesting a stronger effect in the South. Then, in Column (2) we analyze what happens when moving from the birthplace of influential politicians outwards of the 10-km-radius 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 almost double than 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 effect is concentrated among the most influential politicians. Overall, some heterogeneity in the impact of political connections at local level is detectable, even though the difference between the estimated coefficients is never statistically significant.

5.2 Robustness checks

We start by ruling out the possibility that our estimates are 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 0.073 (standard error = 0.028), a similar point estimate to the one we have in Table 4 (Column 7). 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.

The first six rows of Table 6 report sensitivity checks for the main empirical choices. In Row (1) we analyze the robustness with respect to the 200,000-inhabitant threshold used to remove big cities from the sample: we drop all the municipalities above the 99^{th} 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 around 50,000 inhabitants.[11] We find that the impact increases to 9.3%, while remaining highly significant. Row (2) 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 as safety belt (10-20 km in the baseline). Again, results are in line with the main estimates. In Row (3) we rerun our regression without cutting 30% of the control municipalities with the lowest predicted probability of receiving the treatment. Instead, we keep all the control observations in the sample and we weight them according to the propensity score of receiving the treatment. The point estimate is still very similar to the baseline. The next exercises are about the definition of the most powerful politicians. In Row (4) we exclude from the sample the municipalities connected to politicians that were below 40 years old or above 80 years old in 1971, i.e. too young or too old to be powerful throughout the whole period we consider. Interestingly, our key estimate is larger (10.5%). In Row (5), we regard as powerful the prime ministers only and exclude from the sample the municipalities connected to party leaders. 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 can detect a connection premium. Thus, in our context, prime ministers are not the only politicians

[11] In 1951, the median municipal size was 2,590 inhabitants.

that are able to give rise to benefits for their hometowns.

To account for the potential spatial correlation of the error terms, Rows (7), (8) and (9) report alternative clustering procedures for the standard errors, respectively clustering at the province-level or applying the Conley (1999) technique using a 5- and a 15-kilometer bandwidth. In all cases, the precision of our estimates slightly changes (the standard error goes from 0.026 to 0.033, 0.020 and 0.027, respectively), while remaining within the conventional limits. Finally, we adopt different estimation methods. In 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. The coefficient estimated with the first method is presented in Row (10), while the second one is presented in Row (11). 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 estimates provide a good approximation irrespective of which control municipalities are given more weight.

Our last robustness exercise is concerned with the estimation strategy but, differently from the last two tests in Table 6, we explicitly take into account the panel structure of the data and exploit the staggered treatment adoption. As stated above, this is not our preferred approach because establishing the exact year after which a politician is powerful is necessarily arbitrary. Nevertheless, we implement four potential empirical choices to define the treatment date: (i) the year in which the connecting politician becomes prime minister or party leader; (ii) the year in which the connecting politician is 40 years old; (iii) the year in which the connecting politician is 50 years old; (iv) the year in which the connecting politician becomes a member of Parliament. In all cases, we round the initial year with the nearest year ending in "1". Figure 5 shows the resulting distribution of the beginning of the treatment across different operationalizations. In most cases the treatment starts in 1961 and the density mass is generally negligible in 1981 and in 1991, reinforcing the idea that in 1991 we can see long run consequences of connections. Notice that we assume that the treatment turns on without turning off. This is because, differently from most of the literature, we do not look at fairly responsive outcomes in the short term such as current spending, but we are interested in long-run local growth. This choice is also consistent with further results of the paper showing that the transmission mechanisms also consist of infrastructures and large state-owned firms, two types of assets that remain in their original place even if the connecting politicians lose

power. In this robustness test we use all the data in the 1901-1991 period.[12]

Panel A of Table 7 shows the two-way fixed effect results. Namely, we regress the logarithm of population on municipality and year fixed effects, all the control variables listed in Table 2 interacted with a time trend (analogously to our preferred specification in Table 4), and *Treated*, which is now a dummy variable equal to 1 for treated units in years from 1961/1971/1981/1991 on (depending on the treatment cohort, see Figure 5), and 0 otherwise. It turns out that the connection premium is always statistically significant and ranges between 6.7% and 7.1%, not far from our preferred point estimate of 7.4%.

Recent advances in the econometric literature on staggered-adoption DiD designs suggest that estimates in Panel A might be biased because they depend, in part, on the "forbidden" comparison between units switching to treatment from 1971 on and units already treated in previous periods (De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021). In particular, De Chaisemartin and d'Haultfoeuille (2020) show that the OLS estimation of two-way fixed effect models may lead to inconsistent estimation of the average treatment effect, since the coefficient of interest is equal to a weighted sum of the average treatment effects in each cohort of treated municipalities by year of treatment implementation. Moreover, they also show that the weights of the weighted sum of average treatment effects may be negative, which would threaten the correct identification of the average treatment effect in the presence of heterogeneous treatment. As a first step to assess whether these issues are relevant in our analysis, in Panel B of Table 7 we stick to the same estimation framework but exclude from the sample all the observations relative to treated units after the first period of treatment. Doing so, the treated units cannot play the role of additional controls, and cannot contribute to generating negative weights. The estimated effect is smaller in magnitude but precisely estimated and ranges between 4.3% and 5.6%. Finally, to properly address the econometric issues highlighted above, in Panel C we apply the estimator proposed by De Chaisemartin and d'Haultfoeuille (2020), which is robust to negative-weighting issues. Results are largely comparable to those in Panel B. Reassuringly, in all columns we fail to find negative weights for each average treatment effect. All in all, results in Table 7 suggest that even if we resort to a staggered DID approach, we can detect a conservative connection premium equal to 4-6%. Figure 6 plots the event study coefficients for the De Chaisemartin and d'Haultfoeuille (2020)'s estimator. In 3 out of 4 cases we fail to reject the hypothesis that all the coefficients related to the pre-treatment periods are jointly equal to 0. Moreover, the connection has an effect on city size that is increasing over time, consistently with the mechanisms highlighted in the next section.

[12] In 1936, the fascist regime started a quinquennial census program but the World War II prevented the unfolding of the program after the first wave.

6 Further results

In this section, we present some additional results to provide a comprehensive picture of the mechanisms and the consequences generated by political connections. Subsection 6.1 shows some transmission channels; then, Subsection 6.2 illustrates how political connections shape local structural change; Subsection 6.3 is devoted to analyzing electoral payoffs; Subsection 6.4 explores whether the connection premium survives the end of the connections. Finally, some spatial general equilibrium considerations are included in Subsection 6.5.

6.1 Mechanisms

The anecdotal evidence suggests that our results might reflect different types of advantages provided by political connections: infrastructure investments, localization of state-run firms and public transfers (see Section 3). We now provide some descriptive evidence in this regard by running variations of Equation (2) in which we change the dependent variable. Note that in these regressions, differently from Equation (2), the dependent variable is measured during or around the end of the First Republic because we have no data on the beginning-of-period values. The lack of available data on the dependent variable referring to 1951 (or similar) prevents us from implicitly control for unobserved time invariant shocks and might undermine a neat causal interpretation of the estimated coefficient of interest. However, we think that the resulting conditioned correlations are still suggestive, because of the balancing properties of the observable characteristics in 1951 shown in Table 2 that account for a non-negligible part of unobserved heterogeneity. Results are reported in Table 8.

Column (1) shows that the length of highways, national, regional and provincial roads per square kilometer measured in 1996 (first year of available data) is 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. Our results suggest that connected areas have a much higher probability of hosting publicly owned industrial establishments.

In Column (4) we consider the transfers municipalities receive from the central State to meet their ordinary operating expenses for administrative activities and the provision of local public services. The availability of data begins in 1998, but the mechanism of "historical spending" introduced at the end of the 1970s (Law 43 of February 27, 1978), that is the circumstance whereby

the allocation of resources by the State to local authorities takes place on the basis of the spending sustained in the previous year increased by a fixed percentage, allows us to use the available information to approximate the benefits received in previous years. In other words, current spending has memory of any positive drifts occurred in the past. It turns out that treated municipalities receive larger ordinary transfers. Next, we look at government spending for regional convergence. Column (5) shows that the probability of receiving financing through the *Cassa per il Mezzogiorno* program (see Section 3 for further details) was significantly higher for connected municipalities. Column (6) shows that a bias in favor of municipalities with an influential politician also exists for the INA-Casa plan, although in this case the coefficient lacks statistical significance.[13] Overall, these pieces of evidence strongly suggest that the anecdotal stories depicted in Section 3 have also a factual base.[14]

6.2 Structural change

We estimate other variations of Equation (2) with different dependent variables measured at the end of the period, in 1991, to investigate whether political connections induce structural changes to the treated local economies. Again (and unfortunately), beginning-of-period values for the dependent variables are not available. Results are reported in Table 9.

At the end of the First Republic, the birthplaces of influential politicians and their surroundings feature higher levels of economic development compared with their unconnected counterparts. The structure of the local economies is tilted more towards manufacturing (but not towards high tech sectors), to the detriment of agriculture, while the weight of the service sector does not differ from that prevailing in the control municipalities. The density of industrial plants is higher. Firms are larger, workers receive higher wages, and a larger share of local residents is employed. On the other hand, the share of private entrepreneurs out of total workers is smaller, compared to the group of control units.[15] Overall, these results are broadly consistent with the idea that in connected cities local development benefits from publicly planned industrialization policies (based on large firms), more than by the initiative of private agents.

[13] The INA-Casa plan was implemented during the period 1949-63 but data at our disposal relate only to the first phase (1949-56). This may explain the low statistical significance of our estimates.

[14] Golden and Picci (2008) study Italian provinces during the same period and document that the geographical distribution of public investments is correlated to connections to powerful politicians.

[15] This effect could also work through the allocation of talented workers who, given the incentives of the economic environment, may prefer entering the political system and looking for a position as a politician rather than starting a new private business. See for instance Alesina et al. (2001).

6.3 Electoral payoffs

If politicians can manipulate economic variables, engaging in over employment and above-market wages, does it bring to greater political support (Shleifer and Vishny, 1994)? This subsection focuses on this point. In Table 10, we adapt Equation (2) by using the change in the vote share for the five ruling parties between 1953 and 1987 as dependent variable and find no gain stemming from the preferential treatment (Column 1). On the other hand, Column (2) testifies that we do not find any negative impact on the Communist Party. The political landscape of the time helps to rationalize these results. The electoral competition took place within the prevailing rigid ideological fences. For instance, those who were in favor of the Communist Party had very strong opinions on the need to reorganize economic activity on a collectivist basis and would have hardly decided to vote for a candidate of the ruling parties, even if that candidate had transferred important public resources to the community. Our guess is that, within a blocked political system, party competition was *within* ruling parties.

To test for this conjecture, we use as dependent variable the 1953-1987 change of an index of party specialization at the municipality level built as follows:

$$\frac{1}{5}\sum_{p=1}^{5}\frac{|s_{p,m,1987}-s_{p,Italy,1987}|}{s_{p,Italy,1987}}-\frac{1}{5}\sum_{p=1}^{5}\frac{|s_{p,m,1953}-s_{p,Italy,1953}|}{s_{p,Italy,1953}}$$

where $s_{p,m,t}$ is the ruling party p vote share in municipality m in year t computed with respect to total votes of the five parties in power and $s_{p,Italy,t}$ is the corresponding share at the national level. The absolute difference between municipal and national party shares are normalized by their national share to account for the large differences in party size and averaged across the five parties. The index provides a measure of the mobility in party choices throughout the period of interest. If voters reward the connecting politician's party, one should observe an increase in the index over the First Republic period in the treated municipalities. This is indeed what we find in Column (3), in which the estimated impact equals around 16% of the standard deviation of the dependent variable (and is statistically significant at 10%). Finally, Column (4) goes deeper into the issue of reallocation of votes within ruling parties. We focus our attention on the 351 treated municipalities and consider the vote shares for the 5 parties in power in a municipality*party panel dataset, obtaining a sample of 1,755 observations. For each municipality, we define the treatment as a dummy variable equal to 1 if the connected politician belongs to that party and 0 otherwise. The dependent variable is the change between 1953 and 1987 in the shares of votes at the municipality-party level (the share is computed with respect to the sum of five parties' votes) and, in addition to the usual set of municipality-level control variables measured at the beginning of the period, we

also control for party fixed effects. The reported coefficient is positive and statistically significant, suggesting that the parties of the connected politicians gained the larger vote shares in the observed period. The average electoral gain was very large: 5.5%, almost half of the standard deviation in the dependent variable. Thus, connected politicians managed to extract large electoral payoffs in their birthplaces at the expense of the other governing parties.

6.4 Persistence

An interesting 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 Panel A of Table 11, which is analogous to Table 4, we re-estimate Equation (2) with the dependent variable equal to the log difference of municipal population between 1991 and 2011 ($dln(pop)_{m,1991-2011}$). We provide estimates with time varying controls measured in 1991.[16] In both cases, 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 (Columns 6 and 7), suggesting no persistence of differential growth rates from the First Republic political connections.

Panel B of Table 11 presents the results of a diff-in-diff exercise with three periods: 1936-1951, 1951-1991, 1991-2011. The dependent variable is the log difference in municipal population in each period. In all the reported specifications, we control for municipality and period fixed effects and interact the control variables measured in 1951 with a time trend. The regressors of interest are $Treated * \mathbb{1}[t = 1951 - 1991]$ and $Treated * \mathbb{1}[t = 1991 - 2011]$, where $\mathbb{1}[\cdot]$ is the indicator function. The former regressor captures the effect of political connections on population growth during the First Republic while the latter measures the same effect in the subsequent 20 years. Results are consistent with those shown in Table 4 and in Table 11, Panel A. The estimated coefficients for the First Republic are positive, statistically significant and very similar to our core estimates in Table 4 (0.071 in the most demanding specification vs 0.074). On the other hand, the interaction between the treatment and the indicator for the period after 1991 is always small in magnitude and never significant, suggesting a lack of persistence in the connection premium after the end of the First Republic.

Interestingly, Table A.2 in Appendix shows that, 20 years after the treatment is turned off, most of the advantages in terms of economic structure we observe in Table 9 for 1991 disappears (degree of industrialization, wages and plant size) or are significantly fewer (e.g. the employment rate almost halves). Higher plant density persists, probably because the capital stock is sticky,

[16] Table A.1 in the Appendix, instead, reports the usual coefficients adding the control variables measured in 1951, as in Table 4.

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, this evidence suggests that the local economic growth induced by political connections was not self-sustaining.

6.5 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, the benefits from discretionary spending were higher in unconnected cities. The balancing between treated and control municipalities of many relevant characteristics at the beginning of the period (see Table 2) suggests that treated units were not, from an ex-ante perspective, better suited to be targeted by discretionary spending. For example, they were not featured with better quality of institutions (as proxied by human capital and voter turnout). Nevertheless, if politicians had private information about the existence of greater agglomeration externalities in their birthplaces – these agglomeration externalities being uncorrelated to al thel balanced variables in Table 2 – targeting the connected municipalities might have been beneficial for the country as a whole (Bartik, 2020). We now address this point following the methodology of Kline and Moretti (2014a).

Kline and Moretti (2014a) propose an estimable 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.[17] 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.[18] If the marginal productivity of labor is equalized across municipalities, it is 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,

[17] 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.

[18] Greater worker density is traditionally associated with gains in productivity because it allows for positive spillover effects, such as better employer-employee matches, positive externalities from human capital, etc.

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 Appendix B, we replicate the empirical analysis by Kline and Moretti (2014a) and show that, similarly to what they find for the United States, the elasticities of agglomeration for the Italian municipalities in our sample are constant over different density levels. This suggests no aggregate advantage from targeting some particular municipalities (see Table B.1).

Nevertheless, to provide a further test about the absence of stronger agglomeration economies in treated municipalities, in our last exercise we slightly move from the theoretically grounded regression adopted by Kline and Moretti and modify it to test *directly* whether connected cities show higher agglomeration elasticity. Namely, we estimate the following regression:

$$\begin{aligned} \ln Pop_{m,t} - \ln Pop_{m,t-1} = \theta_0(\ln w_{m,t} - \ln w_{m,t-1}) + \theta_1 Treated_m + \\ + \theta_2 \ln\Big(\frac{Pop_{m,t-1}}{R_m} - \ln\frac{Pop_{m,t-2}}{R_m}\Big) + \\ + \theta_3 \ln\Big(\frac{Pop_{m,t-1}}{R_m} - \ln\frac{Pop_{m,t-2}}{R_m}\Big) Treated_m + \\ + \theta_4 Controls_{m,1951} + \lambda_t + u_{m,t} \end{aligned} \tag{3}$$

where w are wages, proxied by per capita GDP at the province level, R is the municipal area, $Controls$ include all the control variables listed in Table 2 measured in 1951 and different time trends for municipalities located in the South, λs are time fixed effects, and u 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 on population growth induced by the interaction between 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: 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 in first differences on the panel of 4,515 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 θ_0 to -1.5. We check the sensitivity of our results to perturbations of this parameter.

Table 12 presents the results of the estimation of Equation (3). In Column (1) we add the usual set of controls listed in Table 2, while in Column (2) we add different time trends for municipalities located in the South. In Columns (3) and (4) we provide estimates of the most demanding specification with alternative calibrated coefficients for the elasticity θ_0. In all the specifications,

the estimated elasticities of agglomeration (in the first row) are around 0.3, suggesting that an increase in population density of 10% in the prior decade increases the observed population by 3%. This result is fairly in line with the preferred estimates by Kline and Moretti (2014a). The coefficient for the *Treated* variable captures the direct effect of the policy, i.e. the decadal population growth induced by political connections. Finally, 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. However, we do not have the statistical power to reject the hypothesis of a null effect.

Still, since agglomeration induced by public intervention at some point in time spontaneously generates additional agglomeration in subsequent periods, our estimates of the elasticities of agglomeration may be curbed by a serial correlation bias. In other words, we may mistakenly attribute the serial correlation in the direct effect to agglomeration forces. To properly account for it, Table B.2 in the Appendix provides some additional results. The first three columns of the table presents the 2SLS estimates instrumenting the lagged population density growth with a second lag in population density. This restricts our sample to the only two decades 1971-1981 and 1981-1991, but the correlation between the instrumented and the instrumental variables is sufficiently large, as suggested by the Kleibergen-Paap F-statistic. The reported 2SLS estimates for the elasticity of agglomeration are smaller than those presented in Table 12, suggesting a positive serial correlation bias. However, even after accounting for it, treated municipalities do not show stronger agglomeration economies with respect to control municipalities. To provide some further evidence on this issue, the last three columns of Table B.2 adopt a different estimation strategy: rather than exploiting the decadal variation in population growth, we consider the whole period of interest, i.e. between 1951 and 1991, as in the reduced-form analysis of Section 5. Indeed, the serial correlation bias should be much less an issue in a very long, one-period setting; moreover, we have already shown that the assignment of the treatment was unrelated to population growth in the previous period, i.e between 1936 and 1951. Thus, an instrumental variable is not necessarily needed in this context. Even though the reported results are quite sensitive to the calibration of the wage parameter, the interaction coefficient between lagged population density growth and political connections is once again negative and not statistically significant, reinforcing our conclusion about the absence of aggregate advantages in targeting connected municipalities from a nationwide point of view.

7 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. Municipalities connected to powerful politicians show a population premium equal to 7.4% over 40 years. The connecting politicians manage to locate in their territories of reference important nodes of the transport system and parts of the state industries and increased the likelihood of these territories of receiving public transfers. Not surprisingly, treated cities benefit from a larger manufacturing sector, higher employment and wages, even though they suffer from lower entrepreneurship. Powerful politicians have their own return: in treated cities, their party shows an electoral gain at the expenses of the other parties in power. Our results also suggest that the connection premium does not persist: the population growth rate differential fades away after the end of the First Republic, as well as some of the other observed advantages in economic outcomes. Finally, we find 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, while grounded in the relatively recent Italian history, have also more general and important implications for the future of economic policies. 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 coming with 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. In addition, 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. On a final note, consider that the structure of the political system may also foster or discourage politicians' exploitative behavior. In the Italian First Republic, the proportional system with open lists gave to politicians a strong incentive to favor their electoral feuds at the expense of other places. Countries with similar political arrangements may also consider corrective measures to prevent such distortionary behaviors.

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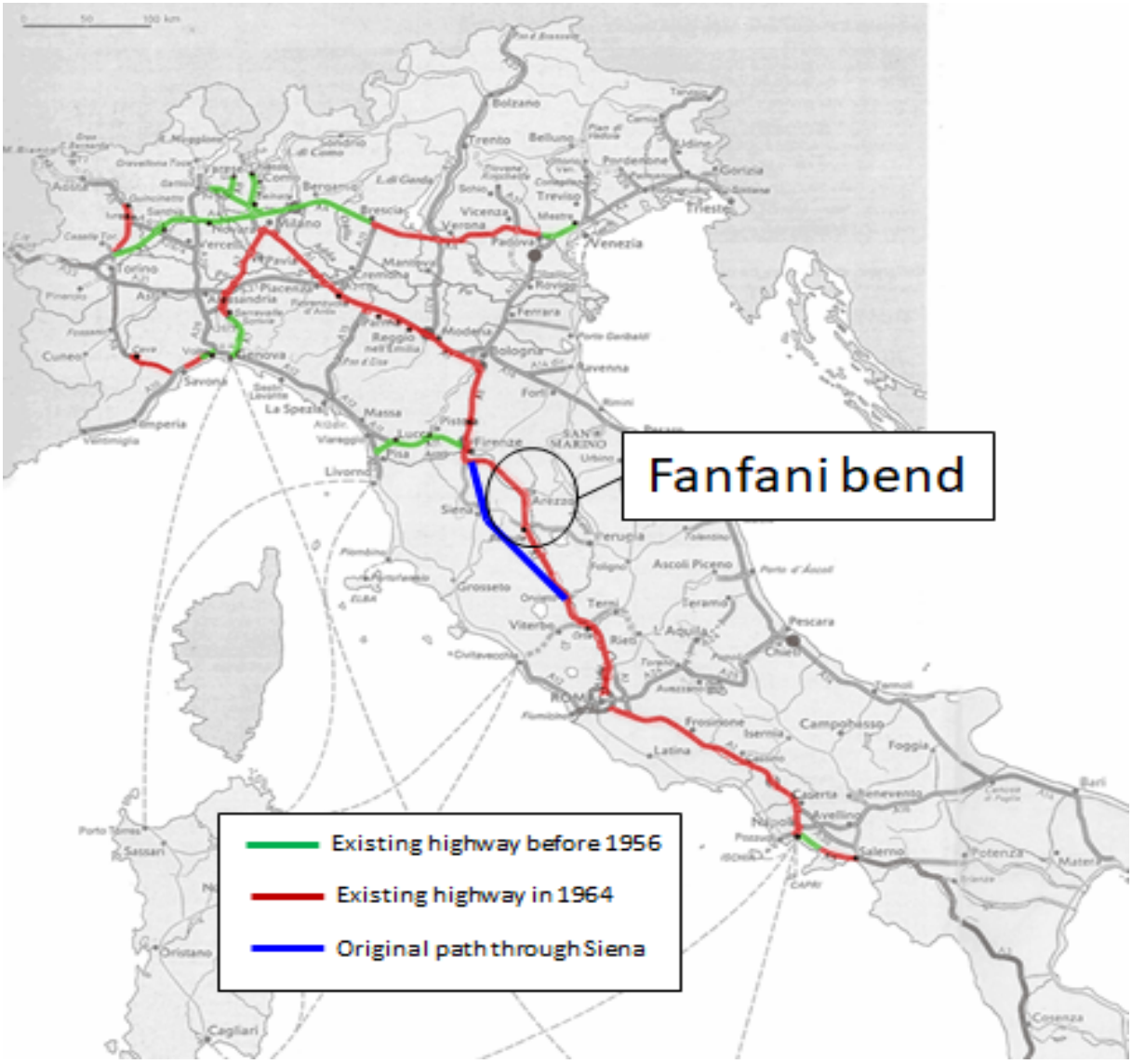
Figure 1: Path of the main Italian highway and the "Fanfani bend"

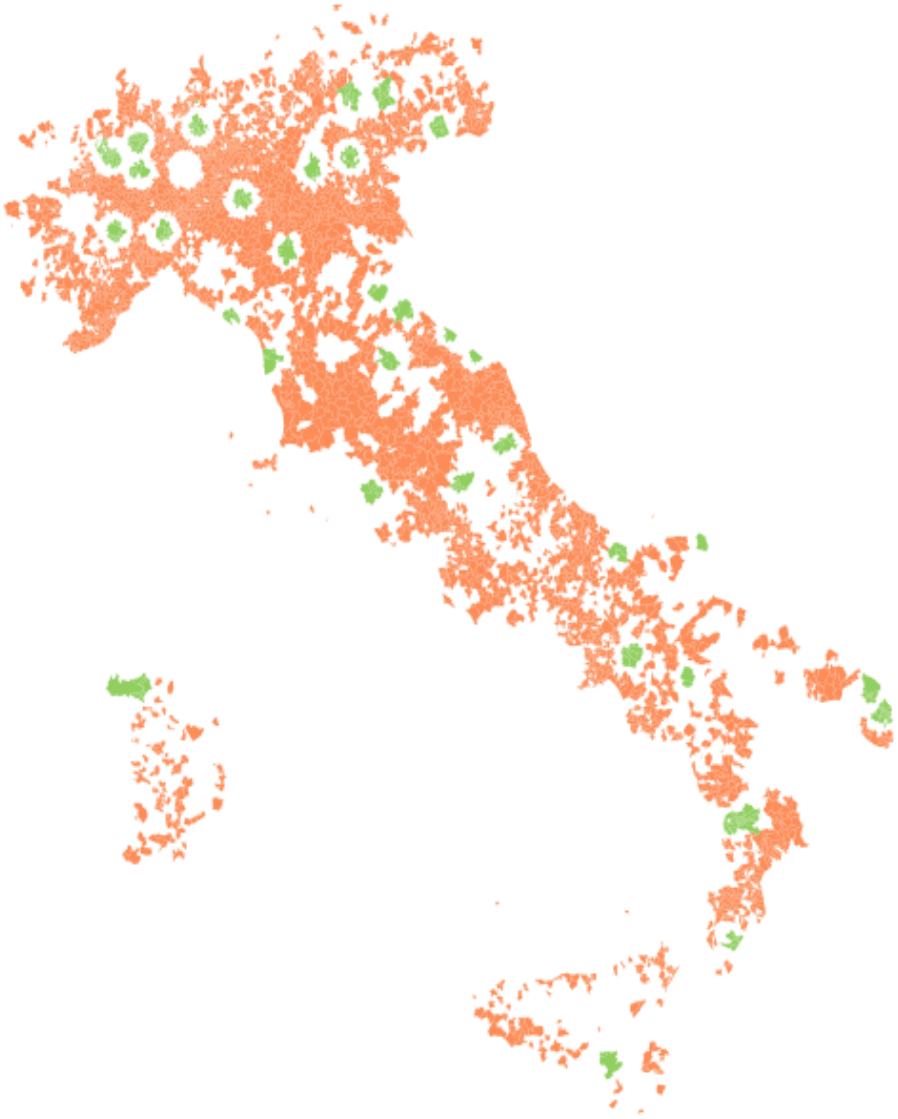


Notes - The highway connecting the city of Milan to Rome was built between 1956 and 1964. The green lines on the map show the existing highways before its construction in 1956. The red lines on the map show the existing highways at the end of that period, in 1964. The blue line shows the planned route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

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

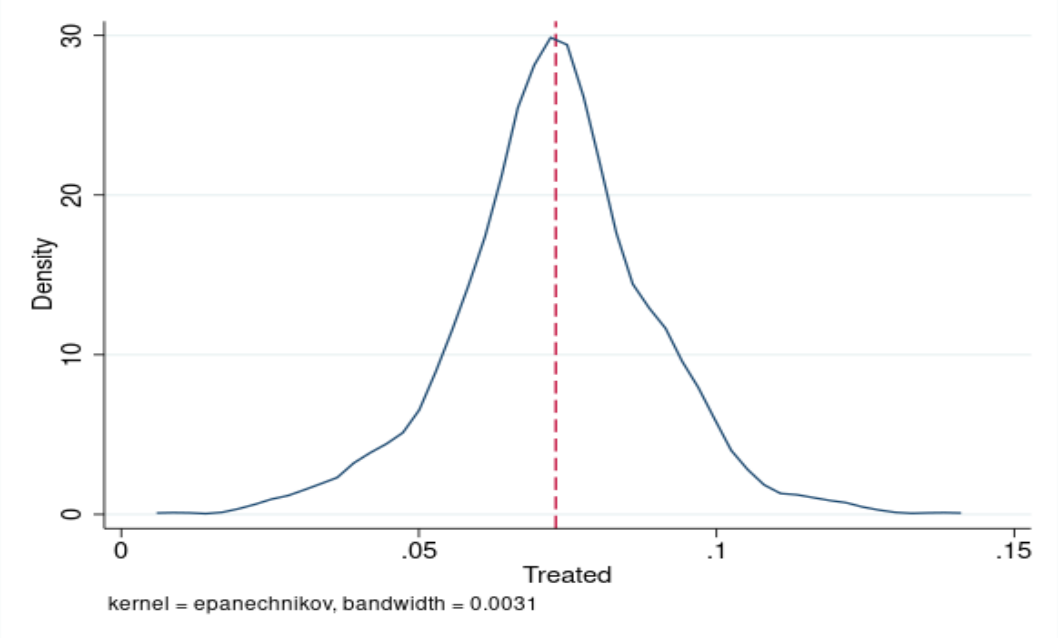
Notes - Dark areas depict municipalities with state-owned enterprises and all their neighboring municipalities in a 10-km radius.

Figure 3: Municipalities in treated and control groups and municipalities excluded from the sample



Notes - Green areas depict treated municipalities, orange areas control municipalities and white areas excluded municipalities.

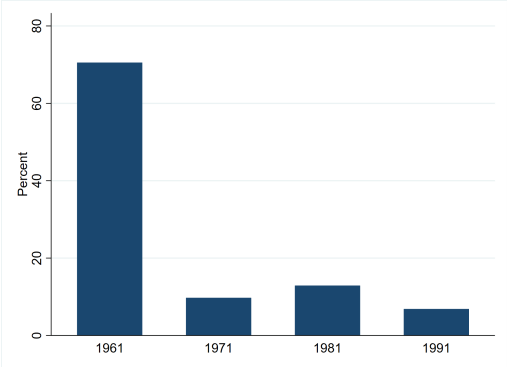
Figure 4: Simulation exercise



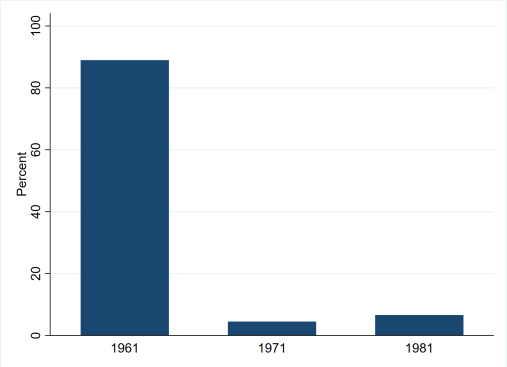
Notes - Simulation of 1,000 treatment effects after removing 5 municipalities of birth of connected policiticians at random (and their neighboring municipalities in a 10-km radius). Treated municipalities are the municipalities of birth of party leaders and prime ministers (listed in Table 1) and all 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 0.073. The mean of the standard errors is 0.028. Standard errors are obtained from a spatial HAC variance estimator based on the technique of Conley (1999) using a 10-kilometer bandwidth.

Figure 5: Distribution of the year of first treatment

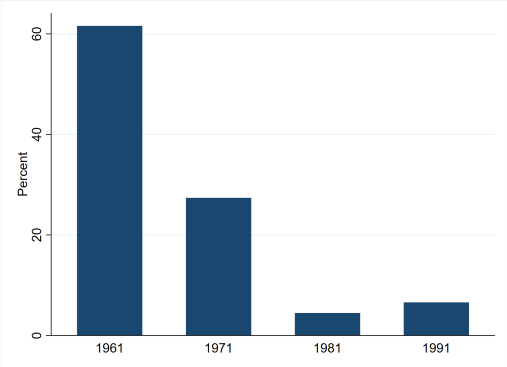
(a) First time PM/PL



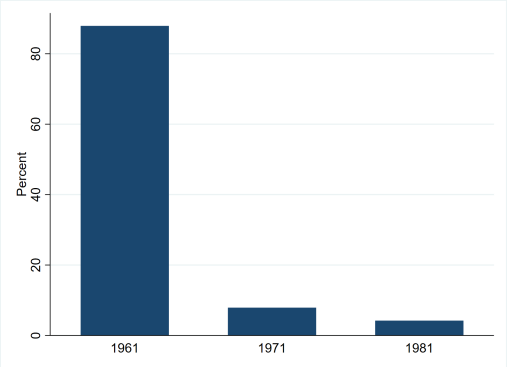
(b) Aged 40



(c) Aged 50



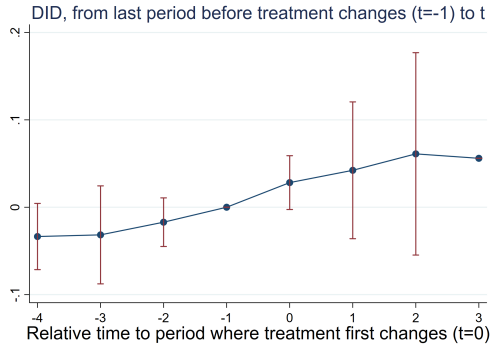
(d) First time in Parliament



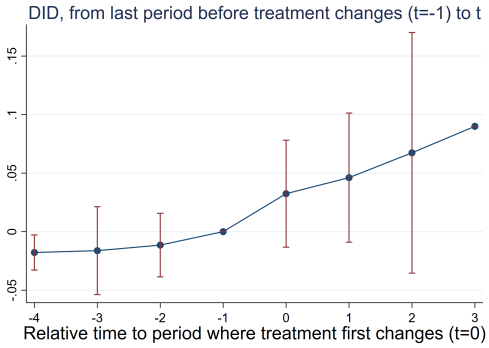
Notes - Municipalities and dates of birth of connected politicians are listed in Table 1.

Figure 6: Event study according to the De Chaisemartin and d'Haultfoeuille (2020)'s estimator

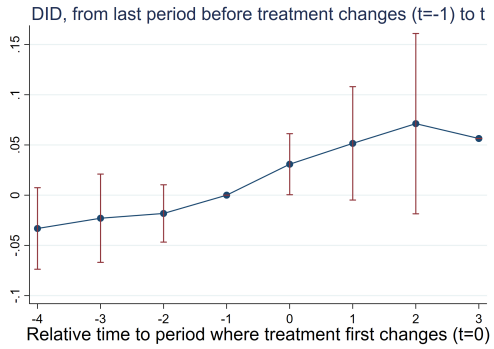
(a) First time PM/PL



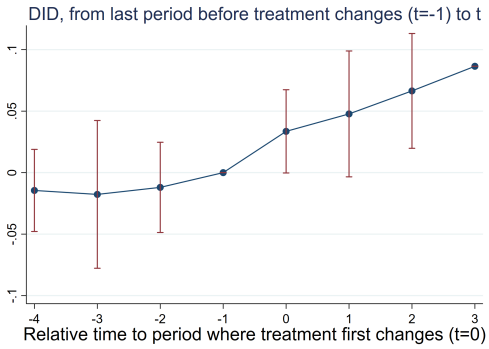
(b) Aged 40



(c) Aged 50



(d) First time in Parliament



Notes - P-values for the joint test that all the coefficients related to the pre-treatment periods are equal to 0: 0.236 in Panel A, 0.066 in Panel B, 0.286 in Panel C, 0.748 in Panel D.

Table 1: List of connected politicians

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 PSDI	Party leader Party leader	1916	Ururi	Campobasso	Molise	SO
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 (1)	Control (2)	P-value (3)	Treated (4)	Control (5)	P-value (6)
Log population	7.93	7.92	0.874	7.93	7.91	0.675
Pop. growth 1936-51	0.06	0.05	0.066	0.06	0.05	0.274
South	0.26	0.34	0.003	0.26	0.26	0.918
Slope	0.21	0.23	0.155	0.21	0.21	0.812
Surface	32.6	40.6	0.004	32.6	30.2	0.204
Density	227.9	174.5	0.001	227.9	203.7	0.167
Size	2.00	2.02	0.605	2.00	2.00	0.995
Share of workers in manufacturing	0.50	0.46	0.000	0.50	0.50	0.839
Share of workers in construction	0.07	0.07	0.262	0.07	0.07	0.701
Share of workers in private services	0.43	0.46	0.000	0.43	0.43	0.662
Log employed	5.44	5.28	0.037	5.44	5.36	0.315
Labor market participation	0.49	0.48	0.051	0.49	0.49	0.598
Share of college educated	0.01	0.01	0.000	0.01	0.01	0.139
Ratio pop. over 65 / pop. under 15	0.75	0.71	0.131	0.75	0.73	0.634
Turnout at national elections	0.94	0.93	0.000	0.94	0.94	0.430
Share votes 5 parties in power	0.68	0.68	0.648	0.68	0.69	0.221
Share votes Communist party	0.20	0.17	0.002	0.20	0.19	0.867
Observations	351	5,949		351	4,164	

Notes - Average characteristcs of treated and control municipalities in 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. The trimmed sample is obtained after dropping the 30% 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 decadal growth rate of population between 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. *Slope* is a continuous variable obtained projecting the difference between the maximum and minimum altitude of the municipality on municipal surface. *Surface* is the surface of the municipality measured in square kilometers. *Density* is the ratio between municipal population and surface. *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, respectively. *Log employed* is the logarithm of total employment. *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 during the First Republic 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 for the Italian Communist party at national elections (for lower chamber) in 1953.

Table 3: Descriptive statistics

Variable	Year	Unit	Mean	Std. Dev.	Obs.	Source
Panel A - Baseline results and controls at 1951						
Population growth	1951-1991	Log difference	-0.112	0.431	4,515	Istat
Population growth	1936-1951	Log difference	0.055	0.13	4,515	Istat
Treated	1951	Dummy	0.078	0.268	4,515	Wikipedia
Population	1951	Ln(units)	7.909	1.009	4,515	Istat
South	1951	Dummy	0.260	0.439	4,515	Istat
Slope	1951	Km	0.208	0.202	4,515	Istat
Surface	1951	Sq-Km	30.3	33.9	4,515	Istat
Density	1951	Units/Sq-Km	205.6	315.2	4,515	Istat
Municipal size 1	1951	Dummy	0.336	0.472	4,515	Istat
Municipal size 2	1951	Dummy	0.332	0.471	4,515	Istat
Municipal size 3	1951	Dummy	0.332	0.471	4,515	Istat
Workers in manufacturing	1951	Shares	0.503	0.178	4,515	Istat
Workers in construction	1951	Shares	0.065	0.096	4,515	Istat
Workers in services	1951	Shares	0.432	0.167	4,515	Istat
Employment	1951	Ln(units)	5.365	1.374	4,515	Istat
Participation rate	1951	Shares	0.491	0.062	4,515	Istat
College education	1951	Shares	0.007	0.005	4,515	Istat
Ratio old/young population	1951	Units	0.734	0.514	4,515	Istat
Voters' turnout	1953	Shares	0.939	0.042	4,515	Ministry of Interior
Votes parties in power	1953	Shares	0.689	0.149	4,515	Ministry of Interior
Votes Communist party	1953	Shares	0.195	0.136	4,515	Ministry of Interior
Panel B - Mechanisms (Table 8)						
Provincial road density	1996	Ln(km/sq-km)	0.601	0.168	4,515	Istat
Provincial railway density	1996	Ln(km/sq-km)	0.086	0.053	4,515	Istat
10-km from SOE	1991	Dummy	0.206	0.404	4,515	Archival sources
Governamental transfers	1998	Ln(euro)	13.15	1.197	4,295	Ministry of Interior
Municipal budget	1998	Ln(euro)	14.74	1.189	4,389	Ministry of Interior
Cassa per il Mezzogiorno	1991	Dummy	0.248	0.432	4,515	Ministry of Culture
INA-Casa plan	1991	Dummy	0.523	0.500	4,515	Ministry of Labor
Panel C - Economic structure in 1991 and 2011 (Tables 9 and A.2)						
Workers in agriculture	1991	Shares	0.137	0.120	4,515	Istat
	2011	Shares	0.087	0.081	4,515	Istat
Workers in manufacturing	1991	Shares	0.398	0.142	4,515	Istat
	2011	Shares	0.321	0.108	4,515	Istat
Workers in services	1991	Shares	0.466	0.123	4,515	Istat
	2011	Shares	0.593	0.100	4,515	Istat
Workers in high tech & human capital	1991	Shares	0.020	0.064	4,387	Istat & OECD
	2011	Shares	0.017	0.067	4,387	Istat & OECD
Plants' density per sq-km	1991	Ln(units)	2.153	1.176	4,515	Istat
	2011	Ln(units)	2.29	1.35	4,515	Istat
Workers per plant	1991	Ln(units)	1.161	0.409	4,515	Istat
	2011	Ln(units)	1.058	0.409	4,515	Istat
Per capita wages	1991	Ln(euro)	8.896	1.591	4,178	Inps
	2011	Ln(euro)	9.983	1.328	4,178	Inps
Employment rate	1991	Shares	0.418	0.083	4,515	Istat
	2011	Shares	0.457	0.074	4,515	Istat
Share enterpreneurs	1991	Shares	0.279	0.081	4,515	Istat
	2001	Shares	0.257	0.067	4,515	Istat

Notes - Istat is the Italian National Institute for Statistics, *Inps* is the National Institute for Social Security. *Voters' turnout, Votes parties in power, Votes Communist party* refer to the national elections for the lower chamber. See the notes of Table 2 and of the other cited tables for further details on the construction of the listed variables.

Table 3: Descriptive statistics - cont'd

Variable	Year	Unit	Mean	Std. Dev.	Obs.	Source
Panel D - Electoral results (Table 10)						
Votes parties in power	1953-1987	Diff. in shares	-0.070	0.149	4,515	Ministry of Interior
Votes Communist party	1953-1987	Diff. in shares	0.054	0.083	4,515	Ministry of Interior
Party mobility index	1953-1987	Diff. in units	-0.140	0.461	4,515	Ministry of Interior
Votes parties in power (treated only)	1953-1987	Diff. in shares	0.000	0.112	1,755	Ministry of Interior
Party connection (treated only)	1953-1987	Dummy	0.207	0.406	1,755	Ministry of Interior
Panel E - Persistence analysis and controls at 1991 (Tables 11 and A.1)						
Population growth	1991-2011	Log difference	0.048	0.202	4,515	Istat
Politicians II republic	1991	Dummy	0.027	0.161	4,515	Wikipedia
Population	1991	Ln(units)	7.796	1.215	4,515	Istat
Slope	1991	Km	0.208	0.202	4,515	Istat
Surface	1991	Sq-Km	30.3	33.6	4,515	Istat
Density	1991	Units/Sq-Km	242.8	404.1	4,515	Istat
Municipal size 1	1991	Dummy	0.331	0.471	4,515	Istat
Municipal size 2	1991	Dummy	0.339	0.473	4,515	Istat
Municipal size 3	1991	Dummy	0.329	0.470	4,515	Istat
Workers in manufacturing	1991	Shares	0.381	0.219	4,515	Istat
Workers in construction	1991	Shares	0.168	0.117	4,515	Istat
Workers in services	1991	Shares	0.451	0.183	4,515	Istat
Employment	1991	Ln(units)	6.047	1.560	4,515	Istat
Participation rate	1991	Shares	0.415	0.046	4,515	Istat
College education	1991	Shares	0.018	0.012	4,515	Istat
Ratio old/young population	1991	Units	1.483	1.139	4,515	Istat
Voters' turnout	1987	Shares	0.888	0.086	4,515	Ministry of Interior
Votes parties in power	1987	Shares	0.619	0.136	4,515	Ministry of Interior
Votes Communist party	1987	Shares	0.248	0.134	4,515	Ministry of Interior
Pane F - Analysis of aggomeration economies (Tables 12, B.1 and B.2)						
Wages	1951-1991	Ln(euro)	4.549	2.881	22,575	Istituto Tagliacarne
Population density	1951-1991	Ln(units)	4.845	1.013	22,575	Istat

Notes - Istat is the Italian National Institute for Statistics. *Votes parties in power (treated only)* and *Party connection (treated only)* refer to a different sample where the 351 treated municipalities are expanded over the 5 parties in power. Electoral variables refer to the votes received by parties at the national elections for the lower chamber in 1953 and 1987. The sample for the analysis of agglomeration economies (variables *Wages* and *Population density*) is a panel spanning over 5 decades. See the notes of the cited tables for further details on the construction of the listed variables.

Table 4: Baseline results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.082**	0.069**	0.079**	0.074**	0.084**	0.087**	0.074***
	(0.037)	(0.033)	(0.034)	(0.032)	(0.033)	(0.036)	(0.026)
Observations	4,515	4,515	4,515	4,515	4,515	4,515	4,515
Impact on sd dep. var. (%)	19.0	16.0	18.3	17.2	19.5	20.2	17.2
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. *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 slope, surface, density, size and a dummy variable for municipalities in the South. *Sectoral composition* includes the share of workers in manufacturing and construction, employment levels 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 electoral 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 obtained from a spatial HAC variance estimator based on the technique of Conley (1999) using a 10-kilometer bandwidth.

Table 5: Interactions

	North vs. South	Distance	Years in Parliament	Google citations
	(1)	(2)	(3)	(4)
Treated*North	0.054*			
	(0.031)			
Treated*South	0.129***			
	(0.039)			
Treated*Farther		0.053**		
		(0.029)		
Treated*Closer		0.094***		
		(0.032)		
Treated*More years			0.075***	
			(0.027)	
Treated*Less years			0.050	
			(0.052)	
Treated*More citations				0.075***
				(0.026)
Treated*Less citations				0.051
				(0.057)
Observations	4,515	4,515	4,515	4,515
P-value equal means	0.138	0.194	0.660	0.695
Controls at 1951	Yes	Yes	Yes	Yes

Notes - The dependent variable is the log-difference between municipal population in 1991 and 1951. *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 the treatment 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 obtained from a spatial HAC variance estimator based on the technique of Conley (1999) using a 10-kilometer bandwidth.

Table 6: Robustness checks

Row	Robustness	Treated	Observations	Impact on std. dev. dep. var. (%)
(1)	Without cities $> 99^{th}$ perc.	0.093***	3,545	21.0
		(0.036)		
(2)	15 km treatment	0.060***	4,135	12.9
		(0.021)		
(3)	PS weight trimmed obs.	0.066**	6,300	15.4
		(0.029)		
(4)	W/out old & young	0.105***	4,428	24.2
		(0.029)		
(5)	Only prime ministers	0.078**	4,309	18.1
		(0.039)		
(6)	Only party leaders	0.071***	4,437	16.5
		(0.027)		
(7)	Cluster at province	0.074**	4,515	17.2
		(0.033)		
(8)	Cluster at 5-km radius	0.074***	4,515	17.2
		(0.020)		
(9)	Cluster at 15-km radius	0.074***	4,515	17.2
		(0.027)		
(10)	PS Matching (kernel)	0.080***	4,515	18.6
		(0.022)		
(11)	Oaxaca-Blinder	0.075**	4,515	17.4
		(0.036)		
	Controls at 1951	Yes		

Notes - The dependent variable is the log-difference between municipal population in 1991 and 1951. For the robustness check in Row (1), all the municipalities above the 99^{th} percentile of population are removed from the sample. In Rows (1), (3) and (7)-(11), *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 (2) *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 (3) we consider the untrimmed sample in Table 2 and weight the observations according to the propensity score of receiving the treatment. In Row (4) *Treated* refers to the municipalities of birth of party leaders and prime ministers above the age of 40 and below the age of 80 in 1971 and all their neighboring municipalities in a 10-km radius. In this regression, the municipalities of birth of party leaders and prime ministers below 40 and above 80 and their neighboring municipalities in a 10-km radius 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 (10) is run adopting a kernel algorithm. In all the robustness checks, 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 obtained from a spatial HAC variance estimator based on the technique of Conley (1999) using a 10-kilometer bandwidth.

Table 7: Staggered DID design

	(1)	(2)	(3)	(4)
		Panel A - OLS		
Treated	0.070***	0.067***	0.071***	0.067***
	(0.015)	(0.014)	(0.014)	(0.014)
Observations	44,785	44,785	44,785	44,785
Impact on sd dep. var. (%)	6.8	6.5	6.9	6.5
	Panel B - 1st period after switching			
Treated	0.056***	0.043***	0.052***	0.043***
	(0.009)	(0.009)	(0.009)	(0.009)
Observations	43,937	43,798	43,936	43,791
Impact on sd dep. var. (%)	5.4	4.1	5.0	4.2
	Panel C - DCDH (2020)'s estimator			
Treated	0.045*	0.058**	0.051**	0.058***
	(0.024)	(0.025)	(0.021)	(0.014)
Observations	43,937	43,798	43,936	43,791
Impact on sd dep. var. (%)	4.4	5.6	5.0	5.6
Treatment starting from:	First time PM/ PL	Aged 40	Aged 50	First time in Parliament

Notes - The dependent variable is the logarithm of population in the 1901-1991 period (i.e, years 1901, 1911, 1921, 1931, 1936, 1951, 1961, 1971, 1981, 1991). *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 and is a dummy variable equal to 1 for treated units in years from 1961/1971/1981/1991 on (depending on the treatment cohort, see Figure 5) and 0 otherwise. Control municipalities are defined as discussed in Section 4.3. The estimation method is OLS in Panel A and B, and the De Chaisemartin and d'Haultfoeuille (2020)'s estimator in Panel C. In Panel B observations related to the treated units after the first period of treatment are dropped from the sample. In all the estimates control variables include municipality and year fixed effects and all the variables listed in Table 2 interacted with time trends. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors (in parenthesis) are clustered at the municipality level.

Table 8: Mechanisms

	Road density (1)	Railway density (2)	10-km from SOE (3)	Govt. transfers (4)	*Cassa per il Mezzogiorno* (5)	INA-Casa Plan (6)
Treated	0.052***	0.017**	0.257***	0.031**	0.092***	0.004
	(0.017)	(0.008)	(0.065)	(0.013)	(0.035)	(0.025)
Observations	4,515	4,515	4,515	4,292	4,515	4,515
Impact on sd dep. var. (%)	31.0	32.1	63.6	2.6	21.3	0.8
Controls at 1951	Yes	Yes	Yes	Yes	Yes	Yes
Municipal budget size	No	No	No	Yes	No	No

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 the logarithm of the overall transfers received by the municipalities from the government in 1998. *Municipal budget size* controls for the logarithm of total municipal revenues in 1998. The dependent variable in Column (5) is a dummy equal to 1 for the municipalities that received transfers from the program "Cassa per il Mezzogiorno". This program was managed by the central government and was meant to provide public transfers to Southern municipalities and to some selected municipalities in the Centre-North to favor economic convergence with the more advanced municipalities in the North of the country. The dependent variable in Column (6) is a dummy equal to 1 for the municipalities that received transfers from the "INA-Casa" plan, a social housing program that took place between the '50s and the '60s. 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 obtained from a spatial HAC variance estimator based on the technique of Conley (1999) using a 10-kilometer bandwidth.

Table 9: Economic structure in 1991

Dependent variable	Treated	Observations	Impact on std. dev. dep. var. (%)
	(1)	(2)	(3)
Share workers agriculture	-0.036***	4,515	30.0
	(0.008)		
Share workers manufacturing	0.033***	4,515	23.2
	(0.011)		
Share workers services	0.002	4,515	1.6
	(0.009)		
Share workers high tech	-0.001	4,387	1.6
	(0.003)		
Plants' density	0.224***	4,515	19.0
	(0.053)		
Workers per plant	0.061**	4,515	14.9
	(0.025)		
Per capita wages	0.203**	4,178	12.8
	(0.090)		
Employment rate	0.011***	4,515	13.3
	(0.004)		
Share enterpreneurs	-0.032***	4,515	39.5
	(0.005)		
Controls at 1951	Yes		

Notes - Each row refers to a different dependent variable measured in 1991. 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. *Plants' density* is the logarithm of the number of plants per square 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) and refer to the universe of employees. Because of difficulties in data cleaning, we ended up with some missing observations. *Employment rate* ranges between 0 and 1. Finally, *Share enterpreneurs* is the share of enterpreneurs out of total workers. The standard deviations of the dependent variables are reported in Table 3. All the regressions include the control variables listed in Table 2 and measured in 1951. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors (in parenthesis) are obtained from a spatial HAC variance estimator based on the technique of Conley (1999) using a 10-kilometer bandwidth.

Table 10: Electoral results

	Share parties in power (1)	Share Commun. party (2)	Party mobility index (3)	Share parties in power (4)
Treated	0.003	0.004	0.072*	0.055***
	(0.011)	(0.006)	(0.040)	(0.018)
Observations	4,515	4,515	4,515	1,755
Impact on sd dep. var. (%)	2.0	4.8	15.6	49.1
Controls at 1951	Yes	Yes	Yes	Yes
Party-in-power FE	No	No	No	Yes

Notes - The dependent variable in Column (1) is the change in the vote share for the 5 parties in power between 1953 and 1987 at the national elections for the lower chamber. The dependent variable in Column (2) is the change in the vote share for the Communist party (PCI) between 1953 and 1987 at the national elections for the lower chamber. The dependent variable in Column (3) is the change between 1953 and 1987 of a mobility index accounting for the deviation of the municipal share of votes of each of the 5 parties in power from their national average vote share, normalized by their national vote share and averaged across the 5 parties in power to obtain a synthetic municipal index. The dependent variable in Column (4) is the change in the vote shares for each of the 5 parties in power between 1953 and 1987 at the national elections for the lower chamber out of the sum of votes for the 5 parties only. The standard deviations of the dependent variables are reported in Table 3. In Columns (1) to (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. In Column (4) the sample is restricted to treated municipalities and is expanded to separately account for the 5 parties in power. In this regression, *Treated* refers to a dummy equal to one for the party of the connected politician. The control group is made of the 4 remaining unconnected parties by municipality. In all the regressions, control variables are those listed in Table 2, with the exception of the shares of votes for the 5 parties in power and for the Communist party (which are nevertheless balanced at the baseline), and are measured in 1951. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors (in parenthesis) are obtained from a spatial HAC variance estimator based on the technique of Conley (1999) using a 10-kilometer bandwidth.

Table 11: Persistence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A - control variables at 1991						
Treated	0.009	0.005	0.009	-0.010	0.004	0.010	0.002
	(0.016)	(0.015)	(0.016)	(0.016)	(0.015)	(0.016)	(0.015)
Observations	4,515	4,515	4,515	4,515	4,515	4,515	4,515
Impact on sd dep. var. (%)	4.5	2.5	4.5	5.0	2.0	5.0	1.0
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
Connections 2nd Republic	No	No	No	No	No	Yes	Yes
	Panel B - differerence in differences						
Treated*Period 1951-1991	0.074***	0.072***	0.073***	0.068***	0.075***	0.080***	0.071***
	(0.020)	(0.019)	(0.020)	(0.018)	(0.018)	(0.019)	(0.017)
Treated*Period 1991-2011	0.001	0.002	0.005	0.003	0.002	0.008	0.007
	(0.011)	(0.011)	(0.010)	(0.011)	(0.011)	(0.011)	(0.010)
Observations	13,545	13,545	13,545	13,545	13,545	13,545	13,545
Impact 1951-1991 on sd dep. var. (%)	25.1	24.4	24.7	23.1	25.4	27.1	24.1
Impact 1991-2021 on sd dep. var. (%)	0.3	0.7	1.7	1.0	0.7	2.7	2.4
Time trends log population 1951	No	Yes	No	No	No	No	Yes
Time trends geography 1951	No	No	Yes	No	No	No	Yes
Time t. sectoral composition 1951	No	No	No	Yes	No	No	Yes
Time trends demography 1951	No	No	No	No	Yes	No	Yes
Time t. voting behavior 1951	No	No	No	No	No	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes - In Panel A, the dependent variable is the log-difference between municipal population in 2011 and 1991. *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. *Geographic controls* include municipal slope, surface, density, size and a dummy variable for municipalities in the South. *Sectoral composition* includes the share of workers in manufacturing and construction, employment levels 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. *Connections 2nd Republic* is a dummy equal to 1 for all the municipalities in a 10-km radius from the municipalities of birth of prime ministers and party leaders in power between 1992 and 2011. Control variables are measured in 1991. In Panel B, the dependent variable is the compound decadal growth rate of municipal population in each of the three observed periods: 1936-1951, 1951-1991, 1991-2011. The standard deviation of the dependent variable is 0.295. *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 measured in 1951 and interacted with a time trend. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors (in parenthesis) are obtained from a spatial HAC variance estimator based on the technique of Conley (1999) using a 10-kilometer bandwidth in Panel A and are robust to heteroskedasticity and clustered at municipal level in Panel B.

Table 12: Agglomeration economies - treated vs control municipalities

	(1)	(2)	(3)	(4)
Log density	0.288***	0.292***	0.338***	0.246***
	(0.022)	(0.022)	(0.019)	(0.026)
Treated	0.026***	0.026***	0.021***	0.031***
	(0.008)	(0.008)	(0.006)	(0.010)
Treated*Log density	-0.052	-0.058	-0.034	-0.081
	(0.049)	(0.050)	(0.039)	(0.062)
Observations	18,060	18,060	18,060	18,060
Wage coefficient	1.5	1.5	1	2
Decade fixed effects	Yes	Yes	Yes	Yes
Controls at 1951	Yes	Yes	Yes	Yes
South time trends	No	Yes	Yes	Yes

Notes - The dependent variable is the first difference in the logarithm of municipal population. *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)-(2), at 1 in Column (3) and at 2 in Column (4). The 4,515 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 obtained from a spatial HAC variance estimator based on the technique of Conley (1999) using a 10-kilometer bandwidth.

ONLINE APPENDIX NOT FOR PUBLICATION

A Additional results on persistence

Table A.1: Persistence - control variables at 1951

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.009	0.008	0.011	0.008	0.011	0.014	0.013
	(0.016)	(0.016)	(0.016)	(0.016)	(0.015)	(0.016)	(0.014)
Observations	4,515	4,515	4,515	4,515	4,515	4,515	4,515
Impact on sd dep. var. (%)	4.5	4.0	5.4	4.0	5.4	6.9	6.4
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
Connections 2nd Republic	No	No	No	No	No	Yes	Yes

Notes - The dependent variable is the log-difference between municipal population in 2011 and 1991. *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 slope, surface, density, size and a dummy variable for municipalities in the South. *Sectoral composition* includes the share of workers in manufacturing and construction, employment levels 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. *Connections 2nd Republic* is a dummy equal to 1 for all the municipalities in a 10-km radius from the municipalities of birth of prime ministers and party leaders in power between 1992 and 2011. Control variables are measured in 1951. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors (in parenthesis) are obtained from a spatial HAC variance estimator based on the technique of Conley (1999) using a 10-kilometer bandwidth.

Table A.2: Economic structure in 2011

Dependent variable	Treated	Observations	Impact on std. dev. dep. var. (%)
	(1)	(2)	(3)
Share workers agriculture	-0.018**	4,515	22.2
	(0.007)		
Share workers manufacturing	0.012	4,515	11.1
	(0.008)		
Share workers services	0.005	4,515	5.0
	(0.009)		
Share workers high tech	-0.000	4,387	0
	(0.003)		
Plants' density	0.261***	4,515	19.3
	(0.070)		
Workers per plant	0.015	4,515	3.7
	(0.024)		
Per capita wages	0.092	4,178	6.9
	(0.066)		
Employment rate	0.007*	4,515	9.5
	(0.004)		
Share enterpreneurs	-0.021***	4,515	31.3
	(0.004)		
Controls at 1951	Yes		

Notes - Each row refers to a different dependent variable measured 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. 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 square 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) refer to 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* ranges 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 (1) is based on a regression using data at 2001. The standard deviations of the dependent variables are reported in Table 3. All the regressions include the control variables listed in Table 2 and measured in 1951. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors (in parenthesis) are obtained from a spatial HAC variance estimator based on the technique of Conley (1999) using a 10-kilometer bandwidth.

B Application of Kline and Moretti (2014a)'s model

A thorough application of the analysis by Kline and Moretti (2014a) is beyond the scope of this paper, but checking the linearity of agglomeration economies across different density levels can be useful to assess the potential gains from workers' reallocation. Within their framework, 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 also have 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).

Notice that since we are focusing on small- and medium-sized municipalities, we expect the elasticities of agglomeration to be fairly stable over density levels. To provide such a test, we first subdivide the municipalities of our sample in splines according to the distribution of the logarithm of municipal density in 1951. Then, we derive the following estimating equation, which tests whether the agglomeration elasticity varies with population density:

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

In this equation, w are wages proxied by per capita GDP at the province level, R is the municipal area, $Controls$ include all the control variables listed in Table 2 measured at 1951 and time trends for municipalities in the South, λs are time fixed effects, ω is the error term, and the rest of the notation is as in the main text. The key feature of Equation (B.1) is the presence of the $g_k(\cdot), k = 1, 2, 3$, which are spline functions defined as follows:

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

where q_k are spline knots.[19] 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

[19] 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 $q_1 = 4.2$, $q_2 = 5.1$, $q_3 = \infty$, corresponding respectively to the 19^{th} and 62^{nd} percentiles of the 1951 distribution of the logarithm of municipal population density.

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 represented in the most or in the least productive intervals. The model is estimated on the panel of 4,515 municipalities in our sample, observed over four decades, between 1951 and 1991. Since wages are endogenous to population dynamics, we also follow Kline and Moretti (2014a) in calibrating the coefficient δ_0 to -1.5.

Results are recorded in Table B.1. In the first two 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 and in Column (2) we add time trends for municipalities in the South. In the most demanding specification and with the preferred calibrated parameter for wages of -1.5, the elasticity of agglomeration ranges between 0.249 in high density municipalities and 0.307 in medium density municipalities, suggesting that a 10% increase in population density in the previous decade increases the observed population by roughly 3%. 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.

Table B.1: Estimates of agglomeration function

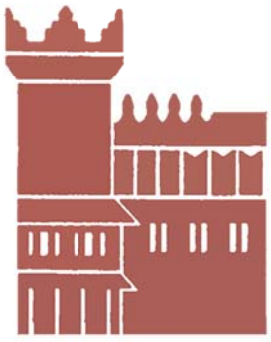
	(1)	(2)	(3)	(4)
Low density	0.270***	0.282***	0.319***	0.245***
	(0.035)	(0.036)	(0.031)	(0.041)
Medium density	0.306***	0.318***	0.362***	0.273***
	(0.024)	(0.024)	(0.020)	(0.029)
High density	0.262***	0.249***	0.314***	0.183***
	(0.032)	(0.033)	(0.024)	(0.043)
Treated	0.273***	0.261***	0.322***	0.201***
	(0.031)	(0.032)	(0.023)	(0.042)
Observations	18,060	18,060	18,060	18,060
Wage coefficient	1.5	1.5	1	2
P-value equal slopes	0.483	0.242	0.152	0.305
Decade fixed effects	Yes	Yes	Yes	Yes
Controls at 1951	Yes	Yes	Yes	Yes
South time trends	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 19^{th} percentile of the distribution of the logaritmic municipal density in 1951, *Medium density* refers to municipalities between the 19^{th} and the 62^{nd} percentile of the distribution of the logaritmic municipal density in 1951 and *High density* refers to municipalities above the 62^{nd} 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)-(2), at 1 in Column (3) and at 2 in Column (4). The 4,515 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 obtained from a spatial HAC variance estimator based on the technique of Conley (1999) using a 10-kilometer bandwidth.

Table B.2: Agglomeration economies - treated vs control municipalities

	2SLS - from 1981 on			**OLS - long difference**		
	(1)	(2)	(3)	(4)	(5)	(6)
Log density	0.136***	0.230***	0.041	0.168*	0.316***	0.019
	(0.036)	(0.028)	(0.045)	(0.093)	(0.095)	(0.106)
Treated	0.025***	0.019***	0.031***	0.130***	0.107***	0.153***
	(0.006)	(0.005)	(0.007)	(0.039)	(0.032)	(0.047)
Treated*Log density	0.017	-0.043	0.077	-0.132	-0.005	-0.260
	(0.074)	(0.060)	(0.088)	(0.291)	(0.249)	(0.345)
Observations	9,030	9,030	9,030	4,515	4,515	4,515
Wage coefficient	1.5	1	2	1.5	1	2
KP F statistic	1,080	1,080	1,080			
Decade fixed effects	Yes	Yes	Yes	No	No	No
Controls at 1951	Yes	Yes	Yes	Yes	Yes	Yes
South time trends	Yes	Yes	Yes	No	No	No

Notes - The dependent variable in Columns (1)-(3) is the first difference in the logarithm of municipal population. The 4,515 municipalities in the sample are observed for 2 periods: 1991-1981 and 1981-1971. Lagged municipal density growth is instrumented with a second lag in municipal density growth. The dependent variable in Columns (4)-(6) is the long difference between 1951 and 1991 in the logarithm of municipal population. Lagged municipal density growth refers to the period 1936-1951 and is not instrumented. In all the specifications, *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) and (4), at 1 in Columns (2) and (5) and at 2 in Columns (3) and (6). Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors (in parenthesis) are robust to heteroskedasticity and clustered at municipal level in Columns (1)-(3). They are obtained from a spatial HAC variance estimator based on the technique of Conley (1999) using a 10-kilometer bandwidth in Columns (4)-(6).



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