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The Effects of Fiscal Decentralization on Publicly Provided Services and Labor Markets^{*}

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Abstract

This paper studies how fiscal decentralization affects local services. It exploits a 1993 reform that increased the fiscal autonomy of Italian municipalities by introducing a local property tax. The identification leverages cross-municipal variation in the degree of decentralization stemming from Allied bombings during WWII. Decentralization reduced overall spending, but expanded municipal services, such as nursery schools. These effects are larger in areas with higher political competition. The paper also investigates how these changes in local services affected the labor markets. Decentralization, likely through expanded nursery schools, increased female labor supply, reducing the gender gap in employment.

JEL Classification: H71, H75, J20, H77, I21.

Keywords: fiscal decentralization, female labor supply, child care, nursery schools, local property tax.

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1 Introduction

Fiscal autonomy of local governments has recently become a global phenomenon. Traditionally a feature of Western European and North-American countries, fiscal decentralization is now on the rise in most developing countries ([Dillinger, 1994](#); [Bird and Vaillancourt, 2006](#); [Ahmad and Brosio, 2006](#)).¹ In spite of its growing importance, it is still not clear how fiscal decentralization affects the provision of publicly provided services. On this issue, economic theory provides conflicting answers. On the one hand, fiscal decentralization can increase the accountability of local politicians, leading to reduced waste and more effective spending. It can also raise efficiency by fostering competition between local governments. On the other hand, interest groups might be able to lobby local politicians more easily than central policy makers. Therefore, increased local autonomy could lead to more corruption and inequality. Moreover, heightened competition between local governments could induce politicians to lower spending for residential services in order to decrease taxes and attract mobile capital.

In situations in which economic theory provides ambiguous hypotheses, it is important to produce quantitative analysis of real-life reforms. Any empirical analysis of the consequences of fiscal decentralization, however, faces substantial challenges. First, the transfer of fiscal authority to local governments is often a gradual process. Without sharp increases in fiscal autonomy, it is hard to isolate the effect of fiscal decentralization from other secular trends. Second, fiscal reforms tend to be large-scale policies that affect all local governments simultaneously. Therefore, the analysis often has to rely on cross-country comparisons that are not well-suited to properly control for all confounding factors. Third, by changing publicly provided services, fiscal decentralization can have consequences that go beyond the initial effects on public spending. It might, for example, affect labor supply by inducing local politicians to offer more or less welfare support. A comprehensive analysis of fiscal decentralization, therefore, requires a combination of different data, ranging from balance sheets of local governments to labor market outcomes.

This paper studies a 1993 Italian reform that has advantageous characteristics with respect to these common challenges. The revenue structure of Italian municipalities changed substantially and abruptly. One year after the policy, local revenue streams (local taxes and service fees) increased by more than 50 percent and replaced government transfers as the major source of municipal revenues. Moreover, the identification strategy exploits plausibly exogenous variation across cities in the degree of fiscal decentralization. In other words, the analysis leverages both longitudinal and cross-sectional changes at the municipal level to

¹ [Arzaghi and Henderson \(2005\)](#) estimate that only countries in the Middle East and North Africa have not joined the trend towards more local fiscal independence. All other countries, instead, have experienced dramatic pushes towards the decentralization of fiscal responsibilities starting from the mid 1980s.

isolate the effect of the policy. Finally, this paper tracks the effects of fiscal decentralization first on publicly provided services and then on the labor markets. For this purpose, the analysis combines data from yearly municipal balance sheets between 1990 and 2010 with data from the decennial population and industry censuses between 1981 and 2011. Moreover, it also uses administrative data from Social Security to estimate heterogeneous effects on labor supply within each affected municipality.

In 1993, a reform introduced a local property tax (LPT) to finance the 8,092 Italian municipalities. Local governments could set their preferred tax rate within a pre-established range and kept the total amount paid by their residents. Property owners paid the LPT separately from any other tax and directly to their municipality. The new system represented a drastic change in the financing of local services. Under the old system, residents paid a personal income tax to the central government, which then redistributed part of its revenues to municipalities to fund their expenses. Because the law was designed to increase fiscal autonomy and leave the size of municipal budgets intact, revenues from the LPT replaced government transfers *lira for lira*.

Our preferred specification leverages variation in the average age of buildings.² The individual LPT liability is computed on the so-called cadastral value, a bureaucratic assessment that is assigned by an independent agency to each property at the time of its construction. Due to the way in which it is assigned (at the time of construction with rare updates), the cadastral value and, consequently, the LPT base are negatively correlated with the age of buildings. All else equal, older properties have significantly lower LPT liability. As a plausibly exogenous variation in the average age of constructions, we use municipal-level data on Allied bombings during WWII. Specifically, we compare outcomes in municipalities adjacent to cities bombed by Allies during WWII and in other similar municipalities.³ This identification strategy is based on three important features. First, municipalities near bombed cities had a younger stock of buildings in 1991 (or any other census year between 1981 and 2011), compared with control cities. Due to imprecise air attacks and the use of carpet bombings, they experienced damages during WWII. Second, municipalities near bombed cities were hit by mistake or random chance. In other words, they were not selected

² In its simplest form, the empirical analysis could compare outcomes in municipalities differentially exposed to decentralization, measured by the post-reform increase in revenues from local taxes, before and after the introduction of the LPT. However, a municipality with larger and more upscale real estate would raise more revenues through the LPT for any level of the tax rate. Therefore, this specification would compare more and less affluent cities, potentially confounding the effect of the 1993 reform.

³ As a control group, the baseline specifications use municipalities adjacent to non-bombed cities that are matched to bombed locations based on their characteristics in the 1991 census. The results, however, are robust to different control groups. We can use, for example, the second layer of municipalities around bombed locations. This alternative specification ensures that both treated and control municipalities are close to bombed cities.

into receiving air attacks based on observed or unobserved characteristics that could possibly interact with the 1993 policy. Third, the initial higher degree of decentralization in near-bombed municipalities—measured by the post-LPT increase in the share of revenues from local taxes—was permanent. Local constraints in adjusting LPT revenues (an upper bound to the LPT rate and limitations in widening the tax base) prevented control cities from increasing their degree of fiscal autonomy to the levels observed in near-bombed locations.

The paper addresses two main research questions. First, what is the impact of fiscal decentralization on municipal spending and publicly provided goods? Second, if we observe a change in local services, what is the resulting effect on local labor markets? Italian municipalities provide a vast array of services to their residents. Some of them might have direct consequences on labor demand and supply. This is the case, for example, of public nursery schools, professional training courses, welfare programs, and initiatives to support local businesses.⁴

In near-bombed municipalities, we find three main changes to public services after the LPT. First, administrators shrank the size of the government, reducing both spending and revenues. They also rebalanced their spending in favor of revenue-generating services, reducing the budget for their internal administrative processes.⁵ Second, there is no evidence of a reduction in quality of public services, even for those services that did not experience increased funding. These findings are consistent with the hypothesis that decentralization induced local politicians to cut waste and increase efficiency. Third, access to many local services, especially those that experienced higher funding, increased. Near-bombed municipalities, for example, had a 12 percent higher probability of having programs in support of local business and a 9 percent higher probability of having at least one public nursery school.

Considering its importance for the labor supply of women, we further analyze the municipal provision of nursery schools. Italian municipalities can decide to offer subsidized nursery schools for children between six months and three years old. When available, public nurseries are substantially cheaper than their private alternatives. Demand often surpasses available capacity, and admission is means tested using a combination of household income and wealth. We find that near-bombed municipalities dedicated a larger share of their budget to nursery schools (+18 percent) and had more public nursery schools (+20 percent), relative to control cities. After the LPT, the number of pupils in nursery schools increased by an additional 24 percent in near-bombed locations.

⁴ However, this is not the case of education beyond nursery schools. All other schools, from kindergartens to most universities, are under the direct control of the central government.

⁵ [Dye and McGuire \(1997\)](#) find similar effects (decrease in spending for administrative tasks) of caps to local property taxes.

Next, we document how expanded access to local services is associated with an increase in employment. This effect is concentrated among women. In near-bombed municipalities, female employment increased by 14 percent, while the number of economically inactive women decreased by 7 percent. The overall result was an 8 percent reduction in the preexisting gender gap in employment. These findings are highly indicative about the importance of nursery schools on female labor supply, but cannot isolate the role of a specific service. We therefore dig deeper within municipalities using Social Security data. Specifically, we estimate triple interactions in which we compare women in different age groups, across treated and control locations, before and after the LPT. The hypothesis is that most local services might equally benefit women in all age groups, while subsidized nursery schools should be more helpful to younger working women. These specifications confirm that labor supply increased the most among women below 35 years old. Moreover, the data suggest that fiscal decentralization lowered the reservation wage of younger women. In addition to being more likely to participate into the labor market, they became more likely to hold positions with lower wages and more flexible hours. The opportunity cost of working might have decreased if more women could send their kids to cheaper public nurseries, instead of relying on expensive private schools.

Finally, we analyze the motivations that might have induced administrators of near-bombed municipalities to expand the provision of local services after the LPT. We find that higher political competition, measured by closer electoral races, is associated with more local services and a higher increase in female employment. Other channels, such as competition between municipalities (measured by the number of adjacent cities) or local preferences towards publicly provided services (measured by the share of low-income residents), have a smaller and less precise predictive power.⁶

This paper contributes to two main strands of the literature. First, there is an extensive theoretical and empirical literature on the effects of fiscal decentralization on local politicians and public goods ([Ahmad and Brosio \(2006\)](#) for a survey).⁷ There is also a much smaller literature on the relationship between fiscal institutions and labor markets ([Huther and Shah \(1998\)](#) for an overview). In most of these studies, the quantitative analysis relies on cross-country correlations.⁸ This paper proposes a novel identification strategy that leverages both longitudinal and cross-sectional variation. Moreover, it attempts to track the effects of

⁶ The results in this paragraph should only be regarded as suggestive evidence, because our setting does not offer quasi-experimental variation in these dimensions.

⁷ Section 2 provides more details on this strand of the literature.

⁸ There is a notable exception. [Hatfield and Kosec \(2013\)](#) study the effect of federal competition (measured by the number of county governments in a metropolitan area) on economic growth. To achieve identification, they instrument the number of local governments with the length of local streams. We complement their findings by addressing a different driver of economic development, fiscal decentralization.

fiscal decentralization on both publicly provided services and labor markets within the same empirical setting.

Second, this paper emphasizes the importance of subsidized child care on female employment (Blau and Currie (2006) for a survey). The literature on this topic suggests that the success of public policies in increasing female labor supply depends on the existing market for child care. In countries with near universal access to affordable child care, expansion of subsidized pre-school does not increase maternal employment (Havnes and Mogstad, 2011), as it mostly moves children across different types of child care arrangements. In other countries, instead, an expansion of affordable child care can have large and positive effects on female labor supply (Gelbach, 2002; Carta and Rizzica, 2018). This paper complements these findings by showing the importance of fiscal institutions in shaping the provision of public child care.

The rest of the paper is organized as follows. Section 2 discusses the relationship between fiscal decentralization and municipal spending. Section 3 outlines the policy change and the data. Section 4 discusses the empirical strategy. Section 5 shows the main results. Section 6 concludes.

2 Fiscal Decentralization, Public Services, and Labor Markets

The literature on fiscal decentralization is incredibly vast and dates back several decades (Musgrave, 1959; Oates, 1972).⁹ In this section, we intend to discuss the main channels through which fiscal decentralization might affect the provision of local services. At the end, we discuss plausible consequences for local labor markets.

A group of theoretical and empirical papers concludes that fiscal decentralization can improve local services. Local taxes, for example, can raise the accountability of local administrators because they make it easier for residents to monitor their elected officials (Fisman and Gatti, 2002; de Mello and Barenstein, 2001). This increased monitoring can happen through different channels. First, decentralization can increase the saliency of local taxes, allowing residents to more accurately assess how much they pay for local services. Second, in the presence of a fiscal deficit, a decentralized system would force administrators to raise more funds directly from their residents, instead of asking higher levels of government for more resources. Raising local tax rates without improving the quality of services could be a signal of bad management. In addition to increased accountability, fiscal decentralization can raise the level of competition between municipalities, leading to a more efficient provision

⁹ Ahmad and Brosio (2006) provide a comprehensive description of relevant contributions and recent developments in this field.

of publicly provided services (Seabright, 1996; Hatfield and Kosec, 2013).¹⁰ Finally, it is possible that local politicians have better information on local preferences towards public services, compared with the central government (Hayek, 1945).

Several papers, however, question the effectiveness of these positive mechanisms. Local taxes, for example, might not be able to increase the accountability of local politicians if existing political competition is not sufficiently high (Albornoz and Cabrales, 2013). Similarly, compared with central policy makers, local administrators might be more easily influenceable by local elites (Oates, 1993; Bardhan and Mookherjee, 2000). Decentralization could therefore increase the level of corruption. Other papers question whether increased competition between municipalities can positively affect local services. For example, in order to be able to “vote with their feet,” individuals need to observe the quantity and quality of publicly provided services in other municipalities (Besley and Case, 1995). Moreover, the cost of moving should be smaller than its benefits. Therefore, sparsely populated areas might not benefit from decentralization. Furthermore, competition between municipalities can become a race to the bottom, in which administrators decrease the local tax rates and the level of residential services in order to attract mobile capital (Zodrow and Mieszkowski, 1986). Finally, in line with the original decentralization theorem by Oates (1972), decentralized autonomy is not recommended for services with significant spillovers across localities and economies of scale (Prud’homme, 1995; Calabrese, Epple and Romano, 2012). In these instances, fiscal decentralization might decrease efficiency and raise inequality between geographical areas (Fernández and Rogerson, 1998).

Due to the existence of conflicting hypotheses, the relationship between fiscal decentralization and publicly provided services is ultimately an empirical issue. Although the literature often limits its analysis to the effects of decentralization on municipal spending, it is plausible to assume that changes in public services might have consequences that extend into the local labor markets. Local governments, in fact, provide a wide array of important services to both their residents and local businesses, such as subsidized child care, job training, welfare supports, programs of local business development. This paper proposes to track the effect of fiscal decentralization first on the provision of local services and then on labor supply and demand.

¹⁰This idea is incorporated into the famous Tiebout model, in which individuals can “vote with their feet” (Tiebout, 1956).

3 Institutional Details and Data

3.1 The Introduction of the Local Property Tax

Between 1992 and 1993, two laws drastically increased the fiscal independence of Italian municipalities.¹¹ As an initial test, the central government established a one-time local property tax (LPT) to be collected only in 1992. The tax revenues went to municipalities, even though local governments could not choose the tax rate. Starting in 1993, the LPT became permanent. At this time, local governments could set their preferred tax rate between 0.4 percent and 0.7 percent (mean 0.57 percent between 1993 and 2010; Table 1, panel A).

The reform had the explicit goal to transition the public sector towards fiscal decentralization. Each euro (Italian lira until 2002) earned through the LPT replaced a euro of government transfers. In the average city, revenues from local taxes increased from €149 per resident in 1990 to €280 per resident in 1994 (Figure 1).¹² Over the same period, transfers from other levels of government decreased from €564 per resident in 1990 to €438 per resident in 1994.¹³ When combined with revenues from municipal services, payments from local taxes became the main source of revenues by 1994 (30 percent of all revenues; Figure A1). LPT was undoubtedly the driving force behind this shift. From 1998 onwards, the first year in which more detailed data on balance sheets are available, LPT revenues alone accounted for 57 percent of total revenues from local taxes (Table 1, panel A).

The implementation of the LPT had one peculiarity that is important to discuss. The tax base was computed using the so-called cadastral value, not the market value of real estate. The Cadastre is an Italian institution that dates back to at least the Middle Ages.¹⁴ Its main function is to keep a register of all real estate in a municipality, recording the characteristics of each property and the identity of the owners. Within these registers, the cadastral value measures the ability of each property to generate real estate income. It is used to compute the value of property for fiscal purposes, such as the determination of estate tax liability. The cadastral value is a function of size, quality, type of property (single-family home, apartment), and location. It is assigned to a property only at the time of construction or after a major renovation. In short, the individual liability for LPT depends on a bureaucratic assessment that is seldom updated and not necessarily aligned with market values. We will

¹¹*Decreto legge* 299/1991 and *decreto legislativo* 504/1992.

¹²The uptick starting in 2002 is due to the introduction of a municipal surcharge on the personal income tax. The decrease starting in 2007 is due to the cancellation of LPT for homeowners on their main residence. The analysis presents robustness checks in which the sample ends before these changes.

¹³Over the same period, total revenues decreased by only 4.6 percent.

¹⁴Akerberg and Botticini (2002), for example, use data from the Florentine Cadastre in 1427.

further discuss the determinants of cadastral values in Section 4 because they are important for identification.

Finally, it is important to note that the introduction of LPT represented a very salient change for the average resident. Under the pre-1993 system, individuals financed the provision of local public services through their personal income tax (PIT). The central government received all PIT revenues and transferred part of them to municipalities in order to fund their functions. This process made it impossible for residents to compute what share of their PIT bill financed their local services. Similarly, it was difficult to hold local politicians accountable for ineffective management of financial resources. A municipality facing a fiscal deficit would have asked the central government, not its residents, for additional money. Under the post-1993 system, individuals paid the LPT separately from their PIT, often twice a year (June and December), and directly to their municipality. The average resident paid €300 for her main residence and €335 for any other property, although there were substantial geographic differences (Table 1, panel A). An individual owning her main residence and a second property would have faced a yearly LPT liability equal to €635, 1.9 percent of the average household disposable income in 1991 (Banca D'Italia, 1993).¹⁵

In short, the implementation of the LPT was salient to residents due to its average amount and its payment method (separated from other taxes). In addition, LPT could have increased the accountability of local politicians by linking directly tax payments to local services and tying a larger share of municipal revenues to local sources. Under the new system, a fiscally irresponsible mayor would have been forced to increase LPT rates or other fees, considering that local taxes and services became the major source of revenues.

3.2 Data

The empirical analysis leverages data on the 8,092 Italian municipalities from four main sources: municipal balance sheets, population and industry censuses, social security, and Allied bombings during WWII.

First, we constructed a panel dataset with yearly financial information about each municipality. Some key variables, such as total revenues and spending, are available every year from 1990 to 2010. The balance sheets, however, become more detailed from 1998 onwards. The post-98 data describe what type of services municipalities provided to their residents (Table A1, panel A). The average city spent 40 percent of its budget just for running the local government and for delivering administrative services, such as vital records and the electoral office. It spent 19 percent for public health, which includes sanitation, waste and

¹⁵Other contemporaneous local taxes and fees, such as the PIT surcharge and a waste disposal fee, were equal to at most 0.6 percent of disposable income (Baldini et al., 2005).

water management, public housing, and city planning, 9 percent for local transportation, and 4 percent for a municipal police force. Two other important areas of spending are education and welfare (10 percent each). In regards to education, municipalities only offer auxiliary services (busing and lunches) for local public schools that are managed by higher levels of government. Therefore, municipal officials have very little opportunities to improve the quality of education of local schools. Spending for welfare services, instead, can have a much larger influence on local households. Cities offer nursery schools for children between six months and three years old, retirement homes, social services, and aid to residents in need. Most of these benefits are mean-tested, and often demand surpasses supply.¹⁶ Considering the breadth of interventions administered by municipalities, it is plausible to assume that a more efficient public administration could have important effects on the local labor markets. In addition to improving amenities and overall quality of life, some publicly provided services can have direct consequences on labor supply. Expanding the provision of subsidized public nursery schools, for example, can induce more women to either enter the labor market or not abandon it after pregnancy.

Second, we linked data from both population and industry censuses from 1981 to 2011 (Table 1, panel C). The resulting dataset has two pre-LPT (1981 and 1991) and two post-LPT observations (2001 and 2011). We use this data to study how participation into the labor market changed after the introduction of the LPT. Moreover, we can analyze how decentralization affected the number and type of firms operating in each city. Simple means reveal that the number of employed individuals in the average municipality increased by 13 percent after the LPT. This change is much larger among women, whose employment level increased by 35 percent over the same period. In the rest of the empirical analysis, we will use a more sophisticated specification to study whether decentralization contributed to these trends.

Third, we leverage administrative data provided by the Italian Social Security Institute (INPS) covering every year between 1987 and 2011. This dataset consists of information on all employees of private-sector, non-agricultural, firms with at least one salaried worker. We use this dataset to study the effect of fiscal decentralization on different types of employees within a municipality. To this end, the observations are aggregated at the level of age, gender, municipality of residence, and year.¹⁷ In this dataset, we observe workers entering

¹⁶Other minor areas of spending are culture (2 percent; theaters, museums, libraries), sport (1.6 percent; stadiums are usually owned by the municipality), activities in support of local economic development (0.5 percent), and auxiliary services for the judicial system (0.1 percent).

¹⁷This aggregation has two purposes. First, it aggregates the data at the same level of variation used by the empirical analysis in Section 5.5. Second, it shrinks the size of the initial worker-level dataset (with more than 150 million observations), allowing us to perform the analysis on the INPS servers.

the labor market for the first time or re-entering it after a break in employment.¹⁸ For each combination of gender, age, municipality and calendar year, the average number of new entrants is equal to 0.53 individuals and the average number of re-entrants is equal to 0.54 individuals (Table 1, panel C). Moreover, the data report the details of the labor contracts. In the sample, the median wage is equal to €13,180 and the median number of days worked during the year is equal to 226. Conditional on gender, age, municipality and year, the average number of individuals working outside their province of residence is 2.31.

Fourth, we exploit Allied bombings during WWII as a shock to cadastral values and, therefore, to the tax base used for the computation of the LPT. Data on Allied bombings in Italy come from the Theater History of Operations Reports (T.H.O.R.; Lt Col Robertson, Burr and Barth, 2013) compiled by the Air Force Research Institute. For each Allied air strike executed in Italy during WWII, this database lists the location, the date, the type of target, and the amount of explosives. As explained in Bianchi and Giorcelli (2018), we leverage a drastic change in military strategy that followed the Armistice of Cassibile, signed by Italy and Allied forces on September 3, 1943. After Italy’s surrender, Allies used air strikes to hit the German military units that controlled most of the country and to help their ground troops break German fortifications. As a result, this sequence of bombings did not systematically target the richest or largest cities in Italy, as it happened before the armistice, but was mostly driven by war events and the movements of the German army. This fact can be seen in the distribution of these bombings across Italian municipalities (Table 2, panel A). The heaviest bombings followed the land battles that characterized the successful Allied invasion of Italy, as well as the progressive retreat of the German army toward the Austrian border.¹⁹ In Section 4.1, we will further discuss how this variation can be used to isolate the effect of fiscal decentralization after 1993.

4 Empirical Strategy

4.1 The Relationship between Cadastral Values and Age of Buildings

The empirical analysis intends to compare municipalities that were differentially exposed to the introduction of the LPT. Ideally, it could be possible to study how the provision of municipal services and labor market outcomes changed across cities in which the share

¹⁸Absence from the INPS dataset could coincide with an unemployment spell or a period of employment outside privately owned firms (self-employment or public sector). The data do not allow us to distinguish between these different scenarios.

¹⁹Most bombings tend to be clustered around the Gustav and Gothic lines, two of the major German entrenchments. They also tend to follow the gradual retreat of German troops towards Austria.

of revenues from local taxes increased differently after 1993. This methodology, however, might not be able to isolate the effect of fiscal decentralization. Municipalities in which revenues from local taxes increased more, in fact, likely had a larger tax base, more expensive buildings, a more developed local economy, and richer residents. Any change that we observe after 1993 might therefore be correlated with these preexisting differences, rather than being directly driven by the LPT. In order to isolate the effect of fiscal decentralization, we need to find a source of variation in LPT revenues that is plausibly exogenous with respect to other drivers of public services.

The individual LPT liability was the result of the following formula: $\text{LPT paid} = \text{cadastral value} \times \text{multiplier} \times \text{tax rate}$. Out of these three components, only the cadastral values are a suitable source of exogenous variation. In fact, they vary across geographical areas. In 2013, the first year in which municipal-level data are available, the average cadastral value was equal to €351 and had a standard deviation of €150 (Table A2). Moreover, mayors did not have any way to directly affect them. Cadastral values were assigned to buildings only at the time of constructions or after major renovations by the cadastre itself, which is a national agency under the control of the *Agenzia delle Entrate* (the Italian counterpart of the US IRS or the UK HM Revenue and Customs). The other two components of the LPT formula do not share these features. The multiplier was set by the national government and was constant across municipalities. The tax rate, instead, was under the direct control of local administrators, even though it was restricted to be between 0.4 percent and 0.7 percent. As a result, any cross-municipality variation in the tax rate would be endogenous and possibly a symptom of other underlying differences.

After assessing the relevance of cadastral values for the empirical analysis, we first want to establish an important fact about them. Cadastral values are negatively correlated with the the age of buildings. Specifically, a 1 standard deviation (σ) increase in the share of buildings constructed before WWII decreases the average cadastral value by €69 or 20 percent from the mean (Table A2, panel A). This correlation holds after controlling for other municipal-level characteristics, such as the average building size, the average building quality, geography, demography, and local economy. By being correlated with lower cadastral values, older buildings are also associated with a lower exposure to fiscal decentralization after 1993. A 1 σ increase in the share of pre-WWII buildings, in fact, decreases the post-LPT share of revenues from local taxes by 2.5 percentage points or 27 percent from the mean (Table A2, panel B). Moreover, the age of buildings does not have the same negative relationship with the market value of real estate. We show this finding by using median rental value per m² of residential properties in larger cities between 2002 and 2010. The correlation between building age and rental value is positive, albeit small and not robust to the inclusion of other

municipal characteristics (Table A2, panel C).

The fact that cadastral values are negatively correlated with building age is not surprising. As already discussed, the cadastral values are assigned to buildings at the time of construction (Agenzia delle Entrate, 2013). After this initial assessment, they are never re-evaluated on a case by case basis, unless a property undergoes a major renovation that affects its overall size or number of rooms. As the prices of real estate increased by 3.5 times between 1950 and 2012 (real values; Cannari, D'Alessio and Vecchi, 2016), the cadastral values of older buildings remained essentially untouched over the decades. The rare attempts to retroactively increase cadastral values were not successful because these reassessments were based on very coarse geographical areas within a municipality (*zone catastali*). These cadastral zones tend to group diverse neighborhoods with different real estate markets and are usually unable to properly isolate the confined historical areas with older buildings (Guerrieri, Festa and Ghirardo, 2014; Messina and Savegnago, 2016). Additionally, compared with postwar buildings, prewar constructions had a higher probability to be considered part of the Italian cultural heritage by 1993 and be subjected to regulations that limited the possibility to perform major transformations.²⁰ As a consequence, it was more difficult to renovate these prewar buildings in a way that would have generated a re-evaluation of their cadastral values.

So far, the analysis showed that older buildings are associated with lower cadastral values and therefore lower exposure to fiscal decentralization after 1993, even though they are not associated with large differences in the rental value of property. Our empirical strategy exploits this source of variation by using Allied bombings during WWII. Bombing in Italy can be divided into two periods: before and after the Italian armistice with the Allied forces.²¹ During the first phase of the war, the Allies relied on strategic bombing. They targeted rich and populous city, as well as the major industrial factories, to damage the war production and weaken the morale of the Italian population.²² On September 3, 1943, Italy signed the Armistice of Cassibile with the Allied forces (McGaw Smyth, 1948). The armistice had a sequence of important short-term effects. First, Italy ceased to be a direct enemy of the Allies. Second, the Italian army disbanded and the German military took control of the majority of the country. Third, the Allied invasion of Italy, started in Sicily in July 1943, gained momentum and created an active warfront between the German army

²⁰Buildings that are at least 50 years old can be considered historic. Postwar buildings could become historic only in 1996. The most recent law on the protection of the national heritage is *decreto legislativo* 42 of 01/22/2004.

²¹Bianchi and Giorcelli (2018) provides a longer historical section and more empirical evidence on this fact.

²²TNA CAB 65/6/50, War Cabinet conclusion, 27 April 1940. TNA AIR 20/5304, Note by C.A.S., 29 April 1940.

from the north and the Allied forces from the south. From this moment, the Allies used tactical bombings as a tool to win the ground battles against the German troops (Baldoli and Knapp, 2012). During this phase, the selection of targets was not based anymore on the economic conditions of different areas, but was driven by the location of the land battles, the movement of German units, and impromptu opportunities to hit the enemy (Bianchi and Giorcelli, 2018, Table 4).

Based on these historical events, the empirical strategy could compare cities bombed after the armistice to non-bombed municipalities. To obtain a sample with an equal number of treated and control locations, we match bombed municipalities to other cities using propensity score matching and a nearest neighbor algorithm.²³ In the resulting subsample, the share of pre-WWII buildings is 4 percentage points lower in bombed locations and the post-LPT change in revenues from local taxes is 1.4 percentage points higher (Table A3, panel A, column 1). This second effect is large in magnitude (0.18σ) and confirms the tight relationship between age of buildings and post-LPT exposure to fiscal decentralization. In spite of these differences in the exposure to the reform, other observable characteristics are generally balanced between bombed and matched locations. Out of 34 variables from the population and industry censuses, only 4 show differences that are statistically significant at the 5 percent level (Table A3, panel B, column 1).²⁴

In spite of these reassuring statistics, post-armistice bombings might be correlated with unobservable factors that could possibly interact with the 1993 introduction of the LPT. In our empirical strategy, however, we intend to bypass this issue by focusing on non-targeted locations only. Specifically, we exploit the fact that WWII bombings were often not precise and could hit areas around the intended target.²⁵ Panel B of Figure 2 visualizes our preferred sample. Bombed cities are in red and locations matched to them are in dark blue. In our analysis, we compare non-bombed municipalities adjacent to locations hit after the armistice (yellow) to municipalities adjacent to the matched locations (light blue). Relative to the control, near-bombed locations have 3.6 percentage points fewer pre-WWII buildings (Table A3, panel A, column 1). They also experienced a 1.7 percentage points higher change in revenues from local taxes after the LPT. The fact that they were involuntarily hit during

²³The observables we use are few demographic and geographical variables included in the 1991 census: population, area, population density, number of buildings, share of homeowners, share of residents below three years old, and region fixed effects.

²⁴Variables from balance sheets are slightly less balanced (Table A3, panel C, column 1).

²⁵Even though every air attack had a specific target, precise bombing was not always possible due to technological limitations. Especially in the case of night time bombings, which were preferred for the lower probability of being spotted by anti-aircraft artillery units, area bombings were often the only viable option (Kirby and Capey, 1997).

WWII makes them experience a higher degree of fiscal decentralization after 1993, but it is unlikely that selection into bombings biases the results.

4.2 Baseline Specifications

As discussed above, our sample includes municipalities adjacent to bombed cities (from now on, near-bombed municipalities) and those adjacent to the matched cities (near-others municipalities). Our baseline specification is the following:

$$y_{mt} = \alpha_m + \beta_b + \gamma_{rt} + \delta \text{Near bombed}_m \times \text{Post}_t + \varepsilon_{mt}, \quad (1)$$

where y_{mt} is one of many variables describing the provision of municipal services or the local labor market in municipality m and year t . The variable Near bombed_m is 1 for municipalities adjacent to cities bombed by Allied air attacks after the Armistice of Cassibile. The dummy Post_t turns 1 after the introduction of the LPT in 1993. This specification also includes multiple sets of fixed effects to control for nonlinear changes in the outcomes. Municipality fixed effects (α_m) captures permanent differences across cities. Fixed effects for each bombed city (β_b) ensure that we always compare near-bombed and near-others municipalities linked to the same bombed location (through the initial matching process). Finally, region-year or province-year fixed effects (γ_{rt}) control for nonlinear changes in the outcomes within region r or province p . Standard errors are clustered at the level of bombed locations. The empirical analysis tests the robustness of the main findings by augmenting the baseline equation (1) with time-varying municipality characteristics or other nonlinear trends.²⁶

The main assumption behind the baseline specification is that outcomes would have followed the same path in near-bombed and near-others municipalities in the absence of the LPT. Section 4.3 provides evidence in favor of this assumption by estimating linear and nonlinear pre-reform trends. More generally, the empirical strategy is based on the hypothesis that a lower share of prewar buildings and therefore a higher LPT tax base are the drivers behind the observed results on the provision of municipal services and the local labor markets. Is it possible that being located near a bombed city makes treated cities different with respect to other factors? Section 4.4 addresses this concern by describing an alternative specification that relies exclusively on non-bombed cities near a bombed location. Section 4.5 discusses possible endogenous responses to the LPT by local administrators.

²⁶When the dependent variable is available only after the LPT implementation, equation (1) loses the dummy Post_t , the municipality fixed effects α_m , and the fixed effects for bombed locations β_b . The last two components, in fact, would now be collinear with the treatment variable Near bombed_m . However, this specification gains controls for city-level time-varying characteristics, such as population, area size, a dummy for coastal cities, and a dummy for urban cities.

4.3 Pre-LPT Trends

Before the introduction of the LPT, near-bombed and near-others municipalities were similar in terms of observable characteristics. As described above, two characteristics were not balanced: the share of pre-WWII buildings and the post-LPT degree of fiscal decentralization. Out of 46 additional variables observed in 1991, only 2 are statistically different at the 5 percent level (Table A3, panel B and C, column 2). Some coefficients are insignificant, but large in magnitude. Although this fact is not necessarily a problem for identification, we address this issue in several ways. First, we present additional results on a subsample in which near-bombed and near-others municipalities are matched using propensity scores.²⁷ In this case, only 1 variable is statistically different at the 5 percent level (Table A3, panel B and C, column 3). Moreover, most coefficients, including the insignificant ones, are smaller in magnitude. Second, we present robustness checks in which we control for key variables that are not precisely balanced in 1991. Specifically, we control for population changes in different ways and we include nonlinear trends correlated with geographical characteristics (population density, a dummy for rural municipalities, a dummy for coastal cities).

More importantly for our difference-in-differences specification, all outcomes show the existence of parallel trends before the implementation of the LPT (Tables 2 and A4). We first estimate whether linear trends are systematically different between treated and control municipalities by interacting Near bombed_m with a linear time trend. For all dependent variables, the coefficients of this interaction are insignificant and small. Their magnitude is often considerably below 0.01σ . Alternatively, we estimate nonlinear pre-LPT trends by interacting the treatment variable with a dummy for year 1991, as well as year 1992 exclusively for variables extracted from the balance sheets. The excluded year is 1981 for the census variables and 1990 for the data from the balance sheets. The coefficients of the interaction between Near bombed_m and the 1991 dummy are small in magnitude and not statistically different from zero. Even the interactions with the 1992 dummy, the year that saw the implementation of an experimental one-time property tax, do not show significantly different trends before the introduction of the full-fledged LPT.

4.4 Additional Effects of Being Near a Bombed Location?

Our empirical strategy assumes that the variation in the age of buildings is the key difference with respect to the 1993 reform between near-bombed and near-others municipalities. However, it is possible that other observed or unobserved factors interacted with the introduction

²⁷The matching algorithm relies on nearest neighbor matching. The observable characteristics used are simply population, area size, and region fixed effects.

of the LPT. [Bianchi and Giorcelli \(2018\)](#), for example, show that provinces that were hit harder by Allied air attacks during WWII received more reconstruction grants through the Marshall Plan. These grants were important drivers of economic development. Is it possible for faster postwar growth to be the mechanism behind our results?

We do not think that this issue affects our findings for at least two reasons. First, near-bombed and near-others municipalities had similar observable characteristics in 1991 and their local labor markets were on statistically identical trends. As shown by [Bianchi and Giorcelli \(2018\)](#), most of the cross-province differences generated by the Marshall Plan disappeared by 1981. Second, while the unit of observation in [Bianchi and Giorcelli \(2018\)](#) is a province in a given year, the analysis in this paper is at the more disaggregated municipality level. Our specifications can include province-year fixed effects, which should capture any lingering effect of the Marshall Plan in the 1990s.

Furthermore, we can directly address this concern by using an alternative specification. We can compare municipalities adjacent to bombed locations (layer 1 cities) and municipalities adjacent to layer 1 cities (layer 2 cities), before and after the LPT. Layer 1 municipalities are closer to bombed locations and, therefore, have a lower share of pre-WWII buildings and higher exposure to fiscal decentralization after 1993. However, both layer 1 and layer 2 cities are relatively close to bombed locations and should have equally benefitted from the modernization of the transportation network realized through the Marshall Plan. These specifications are the following:

$$y_{mt} = \alpha_m + \beta_b + \gamma_{rt} + \delta \text{Layer } 1_m \times \text{Post}_t + \varepsilon_{mt}. \quad (2)$$

Most variables are unchanged from equation (1). Here, however, the treatment variable $\text{Layer } 1_m$ is 1 for municipalities that are located within 0 km and 10 km of cities bombed by Allies after the Armistice of Cassibile (Figure [A2](#), panel A). The control group comprises cities that are within 10 km and 20 km of bombed locations.²⁸ As shown for the main sample, outcomes in layer 1 and layer 2 cities followed similar linear and nonlinear trends before the implementation of the LPT (Table [A5](#)).

4.5 Endogenous Responses to the LPT

The empirical strategy hinges on the assumption that variations in the LPT base, driven by the age of buildings, translated into permanent differences in the exposure to fiscal decentralization. To show that this assumption is correct, we estimate equation (1) with

²⁸The paper presents robustness checks in which layer 1 and layer 2 have a radius of 15 km, instead of 10 km (Figure [A2](#), panel B).

the share of revenues from local taxes as the dependent variable and year fixed effects in place of the Post_t dummy (Figure 3, panel A). Relative to near-others cities, the share of revenues from local taxes in near-bombed municipalities increases disproportionately in 1993 and remained higher throughout the period under consideration.

In a frictionless environment, this result might not have been possible. In light of their lower cadastral values, administrators of near-others cities could have increased the LPT rates to equalize revenues from local taxes across municipalities. Tax rates, however, were bounded between 0.4 percent and 0.7 percent. As expected, near-others cities had an average LPT rate 0.006 percentage points higher and an LPT rate for homeowners (applicable only to their main residence) 0.01 percentage points higher (Table A6, column 6). These effects are statistically significant, but small in magnitude.

Similarly, local administrators could have attempted to increase the LPT revenues by issuing more building permits and widen the tax base. New buildings had the added benefits of having updated cadastral values. Consistently with this hypothesis, the share of issued building permits was 1.9 percentage points higher in near-others cities from 1998 onwards, when this variable is first available. Although the effect is precisely estimated, it might not have been large enough to compensate the preexisting differences in the LPT base. In fact, the stock of buildings constructed after 1991 does not show any statistically significant variation between near-bombed and near-others municipalities.

In short, local administrators in cities with a lower LPT base attempted to increase LPT revenues through higher tax rates and more building permits. Had they been successful, our empirical strategy would not be able to exploit significant differences in the exposure to fiscal decentralization across municipalities. However, a limited range for the tax rates and the impossibility to construct too many new buildings made the initial cross-city changes in LPT revenues permanent in the period under consideration.

5 Effects of Fiscal Decentralization

5.1 Effects on Municipal Spending

Overall Effects The introduction of the LPT had a direct effect on the balance sheets of Italian municipalities. Compared with near-others cities, near-bombed municipalities experienced a disproportionate increase in the share of revenues from local taxes (+1.2 percentage points) and a decrease in the share of revenues from government transfers (-1.5 percentage points; Table 3, panel A). This effect can be considered the first stage of the policy. The LPT was designed to replace lira for lira government transfers with local taxes.

The main findings suggest that fiscal decentralization led to reduced waste and increased access to local services. First, the overall size of the municipal budget decreased disproportionately in near-bombed cities. Spending per resident decreased by €83, while revenues per resident decreased by €88 (Table 3, panel A). The fact that revenues and spending decreased together implies that public deficit or the probability of having fiscal infractions were unaffected. Moreover, we observe changes in the type of spending for publicly provided services from 1998, when this information becomes available. In near-bombed municipalities, administrators prioritized spending for revenue-generating services. Welfare, education, and police account for 24 percent of total spending and 39 percent of total revenues, while administrative tasks and transportation account for 50 percent of spending and only 14 percent of total revenues (Table A1). Spending for welfare, education, and police was between 0.11 σ and 0.15 σ higher in near-bombed cities after the LPT, while spending for administration and transportation was between 0.12 σ and 0.14 σ lower (Table A7). Overall, these findings are consistent with increased accountability of local administrators. Lower spending and the prioritization of more lucrative services can be a sign of reduced waste and increased efficiency.

Second, as additional evidence supporting the hypothesis of reduced waste, we find that near-bombed cities produced more revenues per public worker, even for services that experienced a decrease in spending. This is the case of administrative workers. Even though the share of spending for administrative tasks was 1.2 percentage points lower in near-bombed cities, revenues per administrative worker were €258 higher (Table 3, panel B).

Third, even though total spending decreased, near-bombed municipalities devoted more resources to local services. The share of spending for local services was 1.2 percentage points higher in near-bombed locations (Table 3, panel B).²⁹ Near-bombed cities were 7.4 percentage points more likely to have programs for local economic development, a 12 percent increase from the mean. Similarly, they were 5.4 percentage points more likely to have at least one public nursery school, a 9 percent difference from the mean. These are two services that can have positive and direct effects on the local labor markets. These findings are consistent with the increased salience of the LPT. In cities whose residents directly funded a larger share of municipal spending, administrators had a stronger incentives to raise the quantity and quality of publicly provided services.

The case of nursery schools In the rest of this section, we focus on the provision of public nursery schools for two reasons. First, nurseries are one of the few local services for which data

²⁹This increase came at the expenses of the the other two main sources of spending: capital investments and debt repayments.

is available before the implementation of the LPT. The number of children enrolled in nursery schools is available at the municipality level in the decennial population census between 1991 and 2011. Second, more importantly, nursery schools are one of the most valuable services provided by municipalities. The availability of affordable preschools is positively correlated with female labor participation both in cross-country (OECD, 2012; Vuri, 2016) and within-country studies (Del Boca, 2002). Increasing female labor participation is an important goal in many developed countries. The European Commission stated that “increasing labour-force participation and raising the employment rate of women is paramount to meeting the Europe 2020 headline target (75% of the population aged 20-64 employed by 2020).”³⁰ This issue is especially urgent in Italy, a country that spends significantly less than the OECD average on families and children (OECD, 2011). In 2018, the share of women above 15 years old active in the labor market was 40 percent. In comparison, female labor force participation was equal to 52 percent among OECD countries, 51 percent in the European Union, and 56 percent in the United States.³¹

In Italy, public nurseries schools were first established in 1971 (*legge* 1044). They accept children between six months old and three years old. After nursery school, children can enroll in kindergarten until they start compulsory schooling at 6 years old. Public kindergartens, however, are managed by higher levels of government and were not directly affected by the introduction of the LPT.³² Although nurseries are subsidized by municipalities, families pay a monthly fee. The share of costs paid by families is at least 50 percent, but each municipality can autonomously decrease the level of subsidization below this ceiling. Municipalities can also choose the fee structure: a flat payment or a tiered system based on household income or wealth. In 2018, a two-parent household with a gross annual income of €44,200 would pay on average €300 a month for a public nursery (Cittadinanzattiva, 2018).³³

Demand for public nurseries vastly exceeds available supply. In 2008, the total capacity of public nurseries was equal to only 12 percent of the population below three years old (Cittadinanzattiva, 2018). For this reason, access to public nurseries has long waiting lists. On average, 27 percent of applicants are not admitted (Cittadinanzattiva, 2011).³⁴ As a

³⁰European Commission (2016), page 1.

³¹ILOSTAT database, data available online at <http://api.worldbank.org/v2/en/indicator/SL.TLF.CACT.FE.ZS?downloadformat=xml>.

³²For decades, nurseries have been considered purely a welfare service towards working women, and not part of the education system. For this reason, they are the only type of schooling provided directly by municipalities, instead of being under the control of the Ministry of Education.

³³Average prices show large regional variation ranging from €472 in the northern region of Trentino to €160 in the southern region of Calabria.

³⁴When demand surpasses capacity, admission is usually means tested. Most municipalities rank applications based on a combination of household income and wealth (Cittadinanzattiva, 2018).

result, many households have to rely on private nurseries. Out of all pupils enrolled in nursery schools in 2016, 48 percent attended private institutions, 39 percent public institutions, and 13 percent private providers affiliated with municipalities.³⁵ In general, private nursery schools are significantly more expensive. Although nationally representative data on private nurseries are not available, anecdotal evidence suggests that the price difference can often be above 100 percent. In the city of Milan, for example, public nursery schools cost between €0 a month for low-wealth households and €465 a month for high-wealth households.³⁶ In the same municipality, private nursery schools can cost between €460 and €800 a month.³⁷

To summarize, public nursery schools are a municipal service that can have important consequences on female labor supply. Access to public nurseries is constrained by limited capacity. As a results, many children enroll in private schools. Their higher costs, however, might prevent a substantial share of households from receiving child care, if they are not granted a spot in public nurseries.

Here, we show that near-bombed municipalities disproportionately expanded their provision of public nursery schools after the LPT. This result reinforces the idea that higher saliency of the costs of local services might have induced local administrators to increase their quantity and quality. We find three main results. First, near-bombed municipalities invested more heavily in nursery schools. Compared with near-others cities, they dedicated a larger share of their budget to nursery schools (+18 percent from mean), were 5.4 percentage points (+9 percent from mean) more likely to have at least one public nursery, and had 0.05 (+20 percent from mean) more public nursery schools (Table 3, panel B).

Second, higher provision translated into higher utilization. In near-bombed municipalities, pupils in nursery schools increased by 2.5 children or 24 percent from the pre-LPT mean (Table 3, panel A). Moreover, fertility increased by an additional 13 percent (column 1) to 20 percent (column 4) from the pre-reform average (see Bauernschuster, Hener and Rainer (2016) for evidence on the impact of public childcare on fertility in Germany).

Third, the results suggest that the increase in attendance did not come from children who would have attended a private nursery school. In 2011, the only year in which this variable is available in the census, the number of pupils attending private nurseries was not statistically or economically different between near-bombed and near-others municipalities (Table 3, panel B). This finding is important because it indicates that the expansion of public

³⁵In addition to running public nurseries, municipalities can outsource the service to private providers. These affiliated institutions apply the prices decided by the municipality for public nursery schools.

³⁶Fee structure available online at http://www.comune.milano.it/wps/portal/ist/it/servizi/educazione/Servizi_0-6_anni/Nidi_Micronidi/Quote_Contributive_+Nidi_+e_+Sezioni+Primavera.

³⁷<https://www.milanolife.it/migliori-asili-nido-privati-milano/>.

nurseries might have allowed some lower-income households to access child care, instead of merely moving children across different types of nursery schools.

5.2 Effects on Local Labor Markets

In this section, we study whether the changes in the provision of public services had effects on local labor markets. There are several services provided by municipalities that could have relevant consequences on both labor demand and supply. Programs for local economic development, a more efficient municipal police, and investments in public health, such as better waste management, could make a municipality more attractive for businesses and increase labor demand. Investments in welfare programs, such as nursery schools, could affect labor supply, although the direction of this relationship might differ across programs.

The data indicate that the participation into the labor market disproportionately increased in near-bombed municipalities after the LPT. The effects are large in magnitude, precisely estimated, and robust to the inclusion of either region-year or province-year fixed effects (Table 4, panel A, columns 1 and 4). In near-bombed cities, employment increased by 89 individuals (+7 percent). Similarly, the economically active population, which includes unemployed residents searching for a job, increased by 128 individuals (+9 percent), while the economically inactive population decreased by 133 individuals (-6 percent).

This increased participation stems predominantly from female residents. Relative to its pre-LPT level, employment increased by 14 percent among women and by just 3 percent among men (Table A8). This result is driven by a stark decrease in the number of stay-at-home women in near-bombed cities (-7 percent; Table 4, panel A). The main consequence was a significant reduction in the gender gap in employment. In near-bombed municipalities, the gender gap decreased by 35 individuals, reducing the pre-LPT level by 8 percent. The existence of substantially larger effects of fiscal decentralization for women is indicative of the important role played by expanded nursery schools. It is important to remember, however, that fiscal decentralization changed municipal spending in other dimensions. The city-level census variables do not allow us to dig deeper into the role of a single public service, such as nursery schools. In Section 5.5, we will estimate triple-difference specifications with INPS data in order to weed out the effect stemming from concurrent changes within a city.

Finally, we can estimate the effects of fiscal decentralization on labor demand. In near-bombed cities, the total number of firms increased between 12 and 20 units (5-9 percent), but the effect is imprecisely estimated. This change stems from the entry of smaller firms with less than three employees, which represents the bulk of the Italian firm stock. In this subsample, the increase is between 10 and 16 percent. In near-bombed cities, better local

services and programs for local economic development might have decreased the costs of running a business, promoting small-scale entrepreneurship. The effect on larger firms with at least 200 employees, instead, is a precisely estimated zero. The benefits generated by better local services were likely not large enough to overcome the costs of moving a large business across municipalities. Moreover, due to their size, large firms might rely less on publicly provided services and more on internal processes.³⁸

The results on population suggest that fiscal decentralization did not induce many people to move across municipalities (Table A8). In near-bombed cities, population increased by an additional 4 percent after the LPT, but this coefficient is not precisely estimated. The increase in foreign residents, however, is precise and large in magnitude. Better welfare services could have attracted more individuals in need of economic support.

We conclude this section with two additional findings. First, year-specific estimates indicate how the trend in labor outcomes sharply changed in near-bombed municipalities after fiscal decentralization (Figures 3 and A3). If we consider stay-at-home women, for example, the data show a lack of differential trends between near-bombed and near-others municipalities between 1981 and 1991 (Figure 3, panel E). After 1991, however, the number of stay-at-home women drastically decreased in near-bombed cities. These findings confirm that the specifications are describing changes in the labor market that started only after the full implementation of the LPT.

Second, we can directly tie fiscal decentralization to the labor markets by estimating the following instrumental variable specifications:

$$y_{mt} = \alpha_m + \beta_b + \gamma_{rt} + \delta \Delta \text{Local taxes}_m \times \text{Post}_t + \varepsilon_{mt}, \quad (3)$$

where $\Delta \text{Local taxes}_m$ measures the change in the share of revenues coming from local taxes between 1990 and 1994 in municipality m . This treatment variable captures the short-term exposure to the LPT in each city and allows us to compute the effect stemming from different degrees of fiscal decentralization. Because $\Delta \text{Local taxes}_m$ could be endogenous, as explained in Section 4.1, we instrument it with the baseline treatment Near bombed_m . All other variables are unchanged from equation (1).

The overall pattern of results from these IV specifications is in line with the previous reduced form estimates. In terms of magnitude, we find that a 1 σ increase in fiscal decentralization, or a 7.8 percent higher share of revenues from local taxes, increased female

³⁸In near-bombed cities, we also observe a larger reallocation of workers from industrial and agricultural firms to the service sector. This result could be due to the fact that local services are more valuable for firms operating in this sector (Table A8).

employment by 65 percent, decreased the number of economically inactive women by 30 percent, and narrowed the gender gap in employment by 48 percent.³⁹

5.3 Analysis of Plausible Mechanisms

Our main analysis indicated that higher exposure to fiscal decentralization is associated with reduced waste, more spending for local services, expanded access to public nursery schools, and higher female labor supply. As discussed in Section 2, there can be several mechanisms that induced local administrators to increase the quantity and quality of municipal services. For example, fiscal autonomy could have increased the accountability of local politicians and the degree of competition between adjacent municipalities. Moreover, it could have transferred the responsibility to provide local services to administrators that are closer to the final users and therefore have a better knowledge about their preferences.

In this section, we provide evidence on the role played by these different mechanisms. However, it is important to note that our findings are only suggestive about the importance of different channels, because we do not have experimental variation in these dimensions. We augment the baseline specifications with three interaction terms. First, we measure the level of political competition by adding Close race_m , a dummy equal to 1 if the average victory margin after 1993 is within 10 percentage points. Alternatively, we can include Runoff_m , the total number of runoff elections after 1993 in municipality m .⁴⁰ Second, we measure the level of municipal competition with Adjacent cities_m , the number of municipalities adjacent to city m . Third, we measure differences in the preferences for public services with $\text{Below } \text{€}15,000_m$, the share of income earners with a yearly taxable income below €15,000. This variable is measured in 2000, the first year in which it is available, and intends to capture cross-municipal differences in the preferences for welfare services. All these variables are interacted with Post_t and Near bombed_t to estimate the change in labor outcomes between near-bombed and near-others municipalities, before and after the introduction of the LPT, and between cities with different levels of political competition, municipal competition, or low-income households.⁴¹

As expected, tougher political competition is correlated with larger treatment effects (Table 5). Relative to similar cities with lower competition, near-bombed locations with close political races show lower probability of fiscal infractions, a larger share of spending destined to public services, higher spending for welfare programs, and more pupils attending nursery

³⁹Year-specific estimates confirm the sharp trend change after the LPT (Figure A4).

⁴⁰We use post-1993 electoral data for two reasons. First, available data is not complete before 1993. Second, law 81 of 03/25/1993 changed municipal elections, introducing for the first time the direct election of mayors. The data is available online at <https://elezionistorico.interno.gov.it/>.

⁴¹Although not reported, these specifications also include the interaction of these new variables with just Post_t . The interaction with Near bombed_t is superfluous due to municipality fixed effects.

schools. These differences in the provision and utilization of public services translated into larger effects on female labor supply. These results hold if we measure political competition with the number of runoff elections, although the triple interactions tend to be less precisely estimated.

In comparison, the effect of municipal competition is smaller in magnitude and seldom statistically significant. For example, a 1 σ increase in the number of adjacent municipalities (2 cities) is associated with an additional 7 percent increase in female employment, while close political races predict an additional 16 percent increase from the baseline (Table 5, panel B, column 1). Similarly, the number of low-income residents does not drive any meaningful change in labor market outcomes.⁴²

5.4 Alternative Samples and Robustness Checks

In this section, we start by using alternative specifications to address two separate concerns. First, as discussed in Section 4.4, the identification strategy assumes that the age of buildings is the only factor affected by WWII bombings that interacts with the introduction of decentralization. But bombings, as well as the post-WWII reconstruction effort, might have changed near-bombed locations in other dimensions. We address this concern by estimating equation (2), in which we compare layer 1 and layer 2 municipalities. Layer 1 cities are within 0 km and 10 km of bombed locations, while layer 2 cities are within 10 km and 20 km. These specifications exploits distance from bombed municipalities among cities that are all close to WWII bombings and likely exposed to similar post-WWII conditions. In this set of regressions, the main findings are unchanged (Table A10 and Figure A5). Compared with layer 2 municipalities, layer 1 cities experienced a 21 percent increase in the number of pupils attending nursery schools, a 6 percent increase in female employment, a 4 percent decrease in the number of economically inactive women, and a 5 percent reduction in the gender gap in employment. In spite of a different sample and treatment variable, the estimated effects of fiscal decentralization are generally within 3 percent of the baseline coefficients.

Second, as discussed in Section 4.3, some variables show preexisting differences between near-bombed and near-others cities that are not zero, even though they are not statistically significant. Although this issue is not necessarily a concern for our difference-in-differences specification, we can repeat the main analysis on a group of municipalities with smaller imbalances in 1991. Specifically, we can estimate the baseline equation (1) on a matched

⁴²This last result is not surprising because fiscal decentralization did not shift the responsibility to provide local services between levels of government. It only changed their sources of funding. Throughout the period under consideration, the provision of municipal services was always under the control of local administrators.

subsample of near-bombed and near-others municipalities. The regressions with province-year fixed effects show treatment effects that are close in magnitude and precision to the baseline estimates (Table A11 and Figure A6).

Moreover, we show that the main results are robust to many variations of the baseline specifications. First, the treatment effects retain their statistical significance if standard errors are clustered at the province level, instead of at the level of bombed municipalities (Table A12, panel A). Second, the results are robust to controlling for population size. We show it by including population as a regressor in the baseline specification (Table A12, panel B).⁴³ Moreover, we estimate regressions in which the dependent variables are expressed as shares of local residents (Table A12, panel C). The share of employed women, and not only their total number, increased disproportionately in near-bombed municipalities after the LPT. Therefore, our main results do not merely capture an increase in the population size, but an actual change in labor supply. Third, the results are robust to controlling for nonlinear trends correlated with geographical characteristics (Table A12, panel D). Specifically, we augment the main specifications with three variables observed in 1991 (population density, a dummy for rural municipalities, a dummy for coastal cities) interacted with year dummies. The resulting treatment effects are not only statistically significant, but even larger in magnitude than the baseline estimates. Similarly, the results are robust if we control for nonlinear trends correlated with the characteristics of the real estate market (average size of buildings and share of high-quality buildings; Table A12, panel E). Fourth, we can control for other law changes that took place between 1993 and 2011 (Table A12, panel F). In particular, municipalities could set a surcharge to the PIT from 2002, which increased their degree of fiscal independence, but lost the LPT revenues from the main residence of homeowners from 2007-2008. Our results are robust if we end the sample in 2001, the last census observation before these law changes. Fifth, we show that our findings hold if we exclude from the sample the five Italian regions with enhanced autonomy and special administrative powers (Valle d'Aosta, Trentino, Friuli, Sicilia, Sardegna; Table A12, panel G). Sixth, we estimate placebo treatments by assigning near-bombed status at random (Table A12, panel H). The resulting coefficients are statistically insignificant and small in magnitude.

5.5 Heterogeneities by Age and Gender

In this section, we analyze the effect of fiscal decentralization on different types of workers. The goal is to draw a more direct tie between the expansion of public nursery schools and the previous findings on female labor participation. The rationale behind the following tests is

⁴³The change in female employment retains its sign and statistical significance, although the magnitude is reduced by 31 percent.

that the probability of having a child below three years old, and therefore eligible to attend a nursery school, is not equal between younger and older women. Data from the Survey of Households' Income and Wealth (SHIW), a survey by the Bank of Italy that is representative of the Italian population, suggest that the vast majority of women with at least one child below three years old is between 25 and 35 years old (Figure A7).⁴⁴ The share of mothers with younger children sharply decreases between 35 and 40 years old and becomes close to zero afterwards. If expanded public nursery schools are one of the major drivers of increased female labor supply, we should observe larger treatment effects among younger women with a higher probability of having a child eligible for nursery school.

We use Social Security data to estimate the following triple interactions on the sample of women working for privately owned firms with at least one salaried employee:

$$y_{amt} = \alpha_m + \beta_b + \gamma_{rt} + \zeta_{at} + \sum_a \delta_0^a \text{Age}_a \times \text{Near bombed}_m \times \text{Post}_t \quad (4)$$

$$+ \sum_a \delta_1^a \text{Age}_a \times \text{Near bombed}_m + \delta_2 \text{Near bombed}_m \times \text{Post}_t + \varepsilon_{amt},$$

where the unit of observation is an age group a living in municipality m in year $t \in [1987, 2011]$.⁴⁵ Age_a is a set of dummies identifying individual ages or age bins. The variable ζ_{at} denotes age-year fixed effects, while all other variables are unchanged from equation (1). The coefficients of interests, δ_0^a , measure the difference in labor market outcomes between younger and older women, between near-bombed and near-others municipalities, before and after the implementation of the LPT. They isolate the effect of nursery schools if they are the only expanded municipal service that differentially affects women of different ages across treated and control municipalities.⁴⁶ The control group is represented by employed women between 50 and 54 years old.

Age-specific triple interactions confirm that the increase in female labor supply was larger among younger women (Figure 4 and Table A13). The number of women entering the labor market for the first time increased the most between 26 and 27 years old. Relative to near-others municipalities and women between 50 and 54 years old, new entrants into the labor market increased by 18 percent (from the pre-LPT mean) per post-LPT year, city, and

⁴⁴These figures use observations collected from the SHIW's waves that are concurrent with the rest of the analysis (1987-1993 in panel A and 1987-2010 in panel B).

⁴⁵Variables measuring re-entry into the labor market, instead of first entry, start in 1989 because the first years are necessary to detect a break in employment.

⁴⁶Another identifying assumption is that women of different age living in near-bombed and near-others municipalities would have experienced the same trends in labor market outcomes in the absence of the policy. The data indicate the lack of differential linear and nonlinear pre-reform trends between younger and older women, between treated and control locations (Figure A8; Table A4, panel B).

age group (Figure 4, panel A). After this peak, the coefficients keep decreasing until they become a precisely estimated zero for 34-year-olds. The number of women re-entering the labor market after a hiatus increased by 17 percent to 25 percent between 30 and 40 years old (Figure 4, panel B). As seen for new entrants, the effect goes down to zero afterwards. In both cases, the coefficients are negative for women between 20 and 24 years old, suggesting that labor supply of these women decreased disproportionately in near-bombed municipalities after the LPT. This effect could suggest that expanded nursery schools might have allowed these women to pursue a postsecondary degree. Education information available in the INPS dataset indicate that the number of women with a university degree is 111 percent higher among 20 to 24 years old living in near-bombed municipalities, relative to near-others cities and women between 50 and 54 years old (Table 4, panel D).⁴⁷

After confirming that labor supply increased more for younger women, we analyze what types of jobs they were more likely to hold. We find that fiscal decentralization decreased median wages of female employees (Figure 4, panel C). This decrease is between €345 and €735 per year (2.4 to 5.2 percent) and is concentrated among women below 35 years old.⁴⁸ In near-bombed municipalities, younger women also became more likely to work fewer days during the year (-4 percent; Figure 4, panel D) and to work outside their province of residence (+23 percent; Figure 4, panel E). Overall, these results suggest that fiscal decentralization decreased the reservation wage of younger women. The cost of participating into the labor force might have decreased because more women could enroll their young kids into the cheaper public nursery schools, instead of relying on their expensive private counterparts. A lower reservation wage increased entry or re-entry into the labor market, while allowing women to accept positions with more flexible hours, lower pay, and higher commuting costs.

These findings are robust to alternative measures of labor supply or to slight modifications to equation (4).⁴⁹ Here, we want to briefly describe three follow-ups to the main results. First, estimating triple differences allows us to control non-parametrically for any change at the city-year level. We replace region-year fixed effects in equation (4) with city-year fixed effects. All the main findings are unaffected by this more demanding specification (Table A14, panel B). Second, we can estimate placebo treatments by including only women above 45 years old. In these specifications, the excluded age category is represented by 60-

⁴⁷It should be noted that education is not complete in the INPS data. It is only available for workers who experienced a substantial change in their labor contract (for example, joining a new firm or receiving a major promotion) after 2005. It is therefore more complete for younger employees.

⁴⁸These findings are robust to using alternative wage measures, such as log median wage (Figure 4, panel F) or median hourly wage (Table A13). Moreover, these results hold if we compute median wages using only new entrants or re-entrants into the labor market (Table A13, panel C).

⁴⁹Table A13 presents the estimated coefficients from all the regressions using INPS data. Table A14 presents robustness checks.

year-olds. The data indicate that the cross-age differences among older women are never statistically different from zero (Table A14, panel C). Third, instead of showing age-specific triple interactions, we can fix the age dimension and estimate treatment effects by calendar year (Figure A9). For most age groups between 25 and 39 years old, the increase in the number of new entrants and re-entrants started between 1995 and 1996, not immediately after the implementation of the LPT. Around the same period, most municipalities in the sample held the first post-LPT elections (Figure A10).⁵⁰ Although only suggestive, this finding is consistent with the main takeaway of Section 5.3: the level of political competition seems to be the most plausible mechanism that induced local administrators to improve municipal services, such as public nurseries.

Finally, we can include observations of male employees by estimating quadruple-difference regressions in which we interact a dummy equal to 1 for women, the age variables, a dummy for near-bombed locations, and a post-reform dummy.⁵¹ Relative to the baseline equation (4), this specification makes it more difficult for other municipal services to bias the effect of public nursery schools. In addition to benefiting younger women more than older women, other services now also have to affect women more than men in order to confound the main treatment effects. The quadruple interactions confirm that younger women in near-bombed municipalities experienced larger increases in labor supply after the LPT, relative to men and older women (Table A15). They were also more likely to hold positions with lower wages, more flexible hours, and higher commuting costs.⁵²

6 Conclusions

This paper studies how fiscal decentralization affects the provision of public services and the local labor markets. It exploits a 1993 Italian reform that introduced an LPT under the direct control of municipalities, simultaneously reducing their revenues from government transfers. Our identification relies on cross-municipal differences in the average age of buildings, which is negatively correlated with the fiscal value of real estate used to compute the LPT base.

⁵⁰The first post-LPT date that had a large number of concurrent municipal elections (1,619) is April 23, 1995.

⁵¹The full specification is $y_{gamt} = \alpha_m + \beta_b + \gamma_{rt} + \zeta_{at} + \kappa_{ga} + \psi_{gt} + \sum_a \delta_0^g \text{Female}_g \times \text{Age}_a \times \text{Near bombed}_m \times \text{Post}_t + \sum_a \delta_1^g \text{Age}_a \times \text{Near bombed}_m \times \text{Post}_t + \delta_2 \text{Female}_g \times \text{Near bombed}_m \times \text{Post}_t + \sum_a \delta_3^g \text{Female}_g \times \text{Age}_a \times \text{Near bombed}_m + \sum_a \delta_4^g \text{Female}_g \times \text{Age}_a \times \text{Post}_t + \delta_5 \text{Near bombed}_m \times \text{Post}_t + \sum_a \delta_6^g \text{Age}_a \times \text{Near bombed}_m + \delta_7 \text{Female}_g \times \text{Near bombed}_m + \varepsilon_{gamt}$, where κ_{ga} are gender-age fixed effects and ψ_{gt} are gender-year fixed effects.

⁵²These effects on the characteristics of the labor contracts are more precisely estimated when they are computed only on new entrants or re-entrants into the labor market (Table A15, panel C).

In municipalities with higher exposure to fiscal decentralization, local politicians reduced waste and increased the quantity of publicly provided services. One of the most important services, subsidized nursery schools for children between six months old and three years old, experienced a 24 percent enrollment increase. In municipalities with higher exposure to fiscal decentralization, female employment increased by 14 percent, reducing the preexisting gender employment gap by 8 percent. These findings are indicative of the importance of subsidized public child care on the decision of women to participate into the labor market. Age- and gender-specific estimates confirm that these effects are larger among younger women (25-35 years old) who were more likely to have kids below three years old: they became more likely to increase their labor supply, as their reservation wage decreased. Finally, we provide suggestive evidence on the channels that induced local politicians to improve local services after the introduction of the LPT. Our findings speak about the importance of high political competition. Other factors either matter less (competition between municipalities) or are not correlated with the main treatment effects (local preferences towards public spending).

These findings can be important for policy makers designing two sets of policies: fiscal decentralizations and labor reforms to increase female employment. The Italian experience suggests that increasing the accountability of local administrators can affect the labor market decisions of the residents through an improvement in local services. There are, however, important open questions for future work. Our results cannot be considered conclusive about the conditions that lead local politicians to improve the provision of services. Addressing this issue would require a setting in which there is quasi-random variation in political competition, geographical competition, and local preferences.

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Figures and Tables

Figure 1: Revenues per Capita from Local Taxes and Government Transfers

Notes: These graphs show the change in the composition of revenues of Italian municipalities. Panel A and B show the average revenues from local taxes and from transfers issued by higher levels of government (provinces, regions, central government) either as 2017 € per resident (panel A) or as a share of total revenues (panel B). Panel C and D show changes in the same variables with respect to 1990. These regressions include municipality fixed effects and cluster the standard errors at the level of provinces. Source: Balance sheets of Italian municipalities, Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>.

Figure 2: Distribution of WWII Bombings Across Italian Municipalities

Notes: Panel A shows the distribution of Allied bombings during WWII that were executed after the Armistice of Cassibile between Italy and Allied forces (September 3, 1944). Panel B shows the municipalities in the main estimating sample. Bombed cities are matched to other non-bombed Italian municipalities using propensity score matching. Then, the analysis compares cities around the bombed municipalities (in yellow) to cities around the matched non-bombed municipalities (in light blue). Source: USAF Theater History of Operations Reports (THOR) Database, available at www.afri.au.af.mil/thor.

Figure 3: Yearly Effects of Fiscal Decentralization on Local Labor Markets

A. Share of revenues from local taxes

B. Gender gap in employment

C. Employed—women

D. Econ. inactive pop.—women

E. Stay-at-home women

F. Firms with < 2 employees

Notes: These graphs show the post-LPT change in cities adjacent to municipalities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities adjacent to cities matched to bombed locations. The omitted year is 1991. The regressions also include city fixed effects, bombed city fixed effects, and region-year fixed effects. Standard errors are clustered at the bombed city level. The vertical bars measure 95% confidence intervals. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>; Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/.

Figure 4: Age Effects, Employees of Privately Owned Firms

Notes: These graphs show triple interactions of age, a dummy equal to 1 for near-bombed locations, and a post-1993 dummy. The sample includes only women. The control group is formed by municipalities adjacent to cities matched to bombed locations. The omitted age group is composed by 50 to 54 years old. The regressions also include the pairwise interactions between the main variables, city fixed effects, bombed city fixed effects, age-year fixed effects, and region-year fixed effects. Standard errors are clustered at the bombed city level. The vertical bars measure 95% confidence intervals. Source: Istituto Nazionale della Previdenza Sociale (INPS).

Table 1: Summary Statistics

	All years				$t \leq 1992$	$t > 1992$
	Mean	Std. Dev.	Obs.	Availability	Mean	Mean
	(1)	(2)	(3)	(4)	(5)	(6)
<u>Panel A: Local property tax</u>						
LPT rate	5.66	0.90	153,420	1993-2010		5.66
LPT rate for homeowners	5.13	0.75	153,231	1993-2010		5.13
Avg. LPT bill	335.48	158.01	153,195	1993-2010		335.48
Avg. LPT bill for homeowners	299.24	125.34	153,010	1993-2010		299.24
<u>Panel B: Balance sheets of Italian municipalities</u>						
Revenues from local taxes						
- Per capita 2017 €	325.92	221.62	158,638	1990-2010	150.57	354.83
- Share of revenues	21.75	13.33	157,000	1990-2010	10.99	23.48
Revenues from gov. transfers						
- Per capita 2017 €	433.68	270.59	158,645	1990-2010	542.40	415.22
- Share of revenues	27.59	13.99	156,998	1990-2010	37.74	25.91
Revenues from local services						
- Per capita 2017 €	201.49	196.67	158,638	1990-2010	151.16	209.35
- Share of revenues	11.62	7.80	157,023	1990-2010	9.48	11.95
Revenues from LPT						
- Per capita 2017 €	196.58	150.29	96,302	1998-2010		196.58
- Share of revenues	12.26	7.84	96,360	1998-2010		12.26
Total revenues (per cap.)	1793.65	1253.30	158,640	1990-2010	1672.68	1815.43
Total spending (per cap.)	1808.21	1261.42	158,606	1990-2010	1670.90	1833.36
<u>Panel C: Census data</u>						
Population	7058.49	42942.79	40,349	1981-2011	6924.17	7259.18
Employed	2564.43	15378.52	32,318	1981-2011	2408.40	2720.10
Econ. active pop.	2728.20	16644.11	40,349	1981-2011	2497.16	3073.40
Econ. inactive pop.	3443.77	21303.60	40,349	1981-2011	3669.12	3107.07
Employed—women	955.80	6224.97	32,314	1981-2011	812.77	1098.46
Econ. active pop.—women	990.47	6497.90	40,345	1981-2011	796.97	1279.52
Econ. inactive pop.—women	2211.91	13878.77	40,345	1981-2011	2393.06	1941.32
Firms	435.30	2814.53	40,440	1981-2011	362.16	544.96
Firms with < 2 employees	342.95	2306.57	40,440	1981-2011	257.45	471.12
Firms with ≥ 200 employees	0.21	3.16	40,440	1981-2011	0.14	0.32
Agricultural workers	10.82	51.03	40,440	1981-2011	11.35	10.02
Manufacturing workers	615.13	3513.21	40,440	1981-2011	665.32	539.90
<u>Panel D: Social Security data</u>						
New entry in labor market	0.53	1.84	1,924,707	1987-2011	0.52	0.54
Re-entry in labor market	0.54	1.25	1,786,169	1989-2011	0.36	0.57
Re-entry in same firm	0.13	0.43	1,786,169	1989-2011	0.12	0.13
Median wage	13,180.84	7,481.33	1,924,707	1987-2011	13,695.04	13,036.72
Median days worked	226.16	91.20	1,924,707	1987-2011	225.28	226.40
Working outside province	2.31	5.90	1,924,707	1987-2011	1.85	2.43
Log median wage	9.26	0.77	1,924,707	1987-2011	9.32	9.25

Notes: This table shows summary statistics for the main variables used in the empirical analysis. Monetary values are expressed in 2017 €. Panel A shows data on the local tax rates and the average LPT bills. The LPT bills are computed starting from average cadastral values observed in 2013 (the first year available). Source: Associazione Nazionale Comuni Italiani (ANCI). Panel B shows data from the balance sheets of Italian municipalities. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>. Panel C shows municipality-level data from the population and industrial censuses. Source: Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/. Panel D shows data from an employer-employee matched database covering all non-agricultural privately owned Italian firms. Source: Istituto Nazionale della Previdenza Sociale, VisitINPS program.

Table 2: Trends Before the LPT Introduction

Panel A: Balance sheets of Italian municipalities							
	Share of revenues for local taxes		Share of revenues from gov. transfers		Revenues per capita		Spending per capita
	(1)	(2)	(3)	(4)	(5)	(6)	(7) (8)
Near bombed x Trend	0.155 (0.107)		0.080 (0.298)		-32.939 (25.826)		-28.110 (25.244)
Near bombed x 1991		0.103 (0.177)		0.305 (0.529)		-53.047 (42.940)	-53.518 (43.967)
Near bombed x 1992		0.308 (0.212)		0.168 (0.595)		-66.765 (51.423)	-57.340 (50.315)
Observations	6,842	6,842	6,840	6,840	7,077	7,077	7,077
R^2	0.862	0.862	0.675	0.675	0.744	0.744	0.748
Dep. var.—mean	11.56	11.56	36.67	36.67	1677.24	1677.24	1674.14
Dep. var.—std. dev.	6.61	6.61	11.99	11.99	1197.95	1197.95	1203.15
F statistic		1.05		0.17		1.08	0.93
P value		0.35		0.85		0.34	0.4

Panel B: Population and industrial census							
	Employed		Econ. inactive pop.		Employed—women		Econ. inactive pop.—women
	(1)	(2)	(3)	(4)	(5)	(6)	(7) (8)
Near bombed x Trend	1.394 (1.319)		1.896 (2.026)		1.132 (0.711)		0.832 (1.434)
Near bombed x 1991		13.940 (13.186)		18.962 (20.262)		11.324 (7.107)	8.318 (14.337)
Observations	4,834	4,834	4,834	4,834	4,832	4,832	4,832
R^2	0.997	0.997	0.997	0.997	0.993	0.993	0.997
Dep. var.—mean	1334.9	1334.9	2258.64	2258.64	443.87	443.87	1402.7
Dep. var.—std. dev.	2064.88	2064.88	3641.75	3641.75	718.84	718.84	2291.54

	Population		Firms		Firms with < 2 employees		Firms with ≥ 200 employees
	(1)	(2)	(3)	(4)	(5)	(6)	(7) (8)
Near bombed x Trend	4.581 (3.910)		0.377 (0.345)		0.120 (0.240)		-0.001 (0.001)
Near bombed x 1991		45.811 (39.101)		3.774 (3.452)		1.196 (2.397)	-0.009 (0.007)
Observations	4,834	4,834	4,848	4,848	4,848	4,848	4,848
R^2	0.997	0.997	0.990	0.990	0.988	0.988	0.896
Dep. var.—mean	3843.27	3843.27	224.48	224.48	162.17	162.17	0.04
Dep. var.—std. dev.	6110.78	6110.78	324.62	324.62	227.18	227.18	0.27

Notes: This table shows pre-reform trends in key city-level variables. Monetary values are expressed in 2017 €. “Near bombed” is 1 for municipalities adjacent to cities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities adjacent to cities matched to bombed locations. Panel A estimates pre-reform trends between 1990 and 1992 using data from balance sheets of Italian municipalities. The F-statistic at the bottom tests for the joint significant of the nonlinear trends in 1991 and 1992. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>. Panel B estimates pre-reform trends between 1981 and 1991 (2 observations per municipality) using data from the population and industrial censuses. Source: Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/. The regressions also include city fixed effects, bombed city fixed effects, and region-year fixed effects. Standard errors clustered at the bombed city level in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Effects of Fiscal Decentralization on Municipal Spending and Services

	Region-year fixed effects			Province-year fixed effects			Mean outcome	Std. Dev.
	Near bombed x Post	Obs.	R^2	Near bombed x Post	Obs.	R^2		
	(1)	(2)	(3)	(4)	(5)	(6)		
<u>Panel A: Dependent variables are available before and after LPT</u>								
Share of rev. from local taxes	1.245*** (0.335)	47,255	0.783	1.314*** (0.361)	47,122	0.801	11.53	6.61
Share of rev. from gov. transfers	-1.493*** (0.406)	47,252	0.663	-1.728*** (0.382)	47,119	0.686	36.69	12.02
Revenues per capita	-87.546** (36.436)	47,707	0.602	-69.801* (38.854)	47,578	0.624	1677.24	1197.95
Spending per capita	-83.440** (37.396)	47,694	0.601	-69.614* (40.401)	47,566	0.624	1674.14	1203.15
Deficit (share of rev.)	0.104 (0.132)	47,234	0.248	0.090 (0.128)	47,102	0.299	3.05	4.14
Pupils in nursery schools	2.475*** (0.746)	7,277	0.879	2.774*** (0.775)	7,259	0.900	10.43	21.23
Births	0.847*** (0.262)	4,848	0.941	1.318*** (0.325)	4,836	0.948	6.52	12.72
<u>Panel B: Dependent variables are available only after LPT</u>								
Has fiscal infraction	-0.006 (0.014)	17,954	0.192	-0.011 (0.014)	17,888	0.243	0.51	0.5
Spending for local services (%)	1.195** (0.502)	28,401	0.266	0.835 (0.524)	28,319	0.327	54.8	16.25
Rev. for admin. tasks per employee	257.568* (139.336)	28,560	0.063	292.717* (149.086)	28,478	0.124	2244.73	3756.45
Has program for local develop.	0.074*** (0.019)	28,430	0.163	0.050** (0.020)	28,347	0.233	0.61	0.49
Has nursery schools	0.054*** (0.017)	28,430	0.222	0.042** (0.017)	28,347	0.296	0.63	0.48
Spending for nursery schools (%)	0.178** (0.082)	28,248	0.283	0.045 (0.069)	28,165	0.387	1.01	2.03
Public nursery schools	0.052*** (0.019)	17,326	0.391	0.005 (0.017)	17,194	0.504	0.26	0.61
Pupils in private nursery schools	0.015 (0.517)	2,403	0.771	0.159 (0.522)	2,397	0.804	11.88	21.18

Notes: This table shows how the provision of public nursery schools changed after the introduction of LPT. Monetary values are expressed in 2017 €. “Near bombed” is 1 for municipalities adjacent to cities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities adjacent to cities matched to bombed locations. Panel A uses dependent variables that are available both before and after LPT. The regressions also include city fixed effects, bombed city fixed effects, and either region-year or province-year fixed effects. Panel B uses dependent variables that are available only between 1998 and 2010. In this case, the treatment variable is just “Near bombed”, not its interaction with “Post”. The regressions also include either region-year or province-year fixed effects, population, area of the municipality, a dummy for coastal cities, and a dummy for urban cities. “Has fiscal infraction” is 1 if municipality is not respecting at least one fiscal benchmark set by the central government (panel 50 of balance sheets). Standard errors clustered at the bombed city level in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>; Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/.

Table 4: Effects of Fiscal Decentralization on Local Labor Markets

	Region-year fixed effects			Province-year fixed effects			Mean outcome	Std. Dev.
	Near bombed x Post	Obs.	R^2	Near bombed x Post	Obs.	R^2		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employed	62.658** (30.388)	9,688	0.980	87.812** (34.822)	9,664	0.983	1334.86	2064.17
Econ. active pop.	91.377** (46.079)	9,688	0.971	128.141** (52.047)	9,664	0.977	1427.74	2214.34
Econ. inactive pop.	-116.047*** (42.667)	9,688	0.981	-133.387*** (47.336)	9,664	0.984	2259.21	3640.77
Gender gap in employment	-39.137*** (12.635)	9,686	0.951	-34.876** (13.473)	9,662	0.957	447.46	713.57
Stay-at-home women	-41.171*** (14.945)	9,688	0.969	-40.785** (16.479)	9,664	0.974	599.49	1162.56
Employed—women	50.870*** (16.145)	9,686	0.964	61.345*** (16.027)	9,662	0.971	443.82	718.55
Econ. active pop.—women	67.765*** (24.907)	9,686	0.943	84.858*** (25.960)	9,662	0.956	480.92	772.7
Econ. inactive pop.—women	-74.593*** (27.056)	9,686	0.981	-83.631*** (30.338)	9,662	0.984	1403.04	2290.9
Firms	11.656 (8.979)	9,701	0.953	20.265** (8.436)	9,677	0.965	224.48	324.62
Firms with < 2 employees	17.228* (9.810)	9,701	0.913	26.623*** (8.994)	9,677	0.933	162.17	227.18
Firms with $\geq$ 200 employees	0.012 (0.018)	9,701	0.681	0.020 (0.017)	9,677	0.701	0.04	0.27

Notes: Monetary values are expressed in 2017 €. “Near bombed” is 1 for municipalities adjacent to cities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities adjacent to cities matched to bombed locations. “Post” is 1 from 1993, when the LPT was introduced. The regressions also include city fixed effects, bombed city fixed effects, and either region-year (column 1) or province-year (column 6) fixed effects. Standard errors clustered at the bombed city level in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/.

Table 5: Political Competition, Municipal Competition, and Local Preferences

	Panel A: Public services									
	Has fiscal infraction		Spending for local services (%)		Spending for welfare (%)		Pupils in nursery schools		Births	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Near bombed x Post	-0.153* (0.085)	-0.137* (0.083)	-0.401 (2.313)	0.081 (2.254)	2.114 (1.597)	1.965 (1.553)	0.056 (4.944)	-2.392 (5.228)	-2.091 (1.740)	-2.167 (1.834)
Near bombed x Post x Close race	-0.128** (0.056)		1.058 (1.854)		2.539*** (0.902)		1.404 (1.697)		1.614* (0.845)	
Near bombed x Post x Runoff		0.002 (0.022)		0.003 (0.510)		0.528 (0.333)		3.658* (1.928)		2.196** (0.871)
Near bombed x Post x Adjacent cities	-0.006 (0.006)	-0.007 (0.005)	-0.155 (0.146)	-0.129 (0.138)	-0.183 (0.116)	-0.170 (0.110)	1.122 (0.712)	0.867 (0.724)	0.420 (0.261)	0.369 (0.274)
Near bombed x Post x Below €15,000	0.002** (0.001)	0.002** (0.001)	0.019 (0.029)	0.012 (0.028)	-0.006 (0.020)	-0.006 (0.019)	-0.079 (0.050)	-0.034 (0.050)	0.006 (0.021)	0.010 (0.021)
Available only after LPT	✓	✓	✓	✓	✓	✓				
Observations	17,063	17,876	26,752	28,300	26,772	28,146	7,250	6,830	4,834	4,554
R ²	0.246	0.246	0.270	0.338	0.388	0.396	0.906	0.908	0.949	0.950
Dep. var.—mean	0.51	0.51	55.8	54.84	9.39	9.20	10.43	10.8	6.52	6.68
Dep. var.—std. dev.	0.50	0.50	15.53	16.23	7.37	7.37	21.23	21.72	12.72	13.04
	Panel B: Local labor markets									
	Employed women		Econ. inactive pop.—women		Stay-at-home women		Gender gap in employment		Firms with < 2 employees	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Near bombed x Post	32.507 (82.667)	-11.228 (83.216)	-144.611 (174.163)	-70.888 (145.529)	-115.331 (107.338)	-73.919 (96.840)	-128.191 (92.723)	-93.584 (82.991)	-9.718 (50.868)	-36.627 (40.059)
Near bombed x Post x Close race	69.985** (30.535)		-116.835* (65.719)		-67.242* (37.048)		-46.977 (28.586)		51.161** (21.188)	
Near bombed x Post x Runoff		11.624 (25.531)		-104.246 (103.262)		-53.609 (51.549)		-74.735* (39.849)		10.266 (18.374)
Near bombed x Post x Adjacent cities	16.215 (10.046)	11.074 (9.976)	-10.740 (20.947)	3.286 (21.192)	-5.683 (13.526)	2.270 (13.418)	-7.761 (10.724)	-0.354 (10.972)	9.381* (5.247)	5.683 (4.679)
Near bombed x Post x Below €15,000	-1.591* (0.957)	-0.570 (0.948)	2.280 (1.835)	0.270 (1.442)	1.819 (1.147)	0.674 (0.925)	2.144** (1.066)	1.187 (0.902)	-0.624 (0.573)	0.059 (0.455)
Available only after LPT										
Observations	9,658	9,098	9,658	9,098	9,660	9,100	9,658	9,098	9,667	9,107
R ²	0.975	0.979	0.984	0.987	0.974	0.978	0.958	0.962	0.941	0.958
Dep. var.—mean	443.41	455.82	1401.99	1446.48	598.92	617.6	446.98	458.24	162.03	165.8
Dep. var.—std. dev.	718.41	734.91	2290.79	2346.28	1162.46	1192.47	713.32	730.63	227.12	232.05

Notes: This table shows heterogeneous effects with respect to the level of political competition. “Near bombed” is 1 for municipalities adjacent to cities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities adjacent to cities matched to bombed locations. “Close race” is a dummy equal to 1 when the closest election had a victory margin within 10 percentage points. “Runoff” is a dummy equal to 1 when the closest election had a runoff. When the dependent variables come from the decennial censuses, “Close race” is 1 if the average victory margin after 1993 is within 10 percentage points and “Runoff” measures the total number of runoffs after 1993. In addition, this table controls for other possible mechanisms through which fiscal federalism could have operated: competition across municipalities and better knowledge of local politicians about local preferences towards local services. “Adjacent cities” is the number of adjacent municipalities. “Below €15,000” is the share of income earners with yearly taxable income below €15,000. Panel A uses dependent variables that describe the provision and utilization of publicly provided local services. Some variables only after LPT. In this case the main regressors do not include the variable “Post” in the interactions. Panel B uses dependent variables that describe the local labor markets. When the dependent variable is available only after LPT, the regressions include province-year fixed effects, population, area of the municipality, a dummy for coastal cities, and a dummy for urban cities. Otherwise, the regressions include city fixed effects, bombed city fixed effects, and province-year fixed effects. Standard errors clustered at the bombed city level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>; Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/.

Online Appendix - Not For Publication

A Additional Figures and Tables

Figure A1: Share of Revenues from Local Taxes and Services

Notes: Panel A and B show the average revenues from local taxes and services and from transfers issued by higher levels of government (provinces, regions, central government) either as 2017 € per resident (panel A) or as a share of total revenues (panel B). Panel C and D show changes in the same variables with respect to 1990. These regressions include municipality fixed effects and cluster the standard errors at the level of provinces. Source: Balance sheets of Italian municipalities, Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>.

Figure A2: Alternative Sample, Layers Around Bombed Locations

Notes: Panel A shows the municipalities hit by Allied tactical air attacks after the Armistice of Cassibile (red), cities within 10km of them (yellow), and cities between 10 and 20km (green). Panel B shows the municipalities hit by Allied tactical air attacks after the Armistice of Cassibile (red), cities within 15km of them (yellow), and cities between 15 and 30km (green). Source: USAF Theater History of Operations Reports (THOR) Database, available at www.afri.au.af.mil/thor.

Figure A3: Effects of Fiscal Decentralization on Labor Markets, More Outcomes

Notes: These graphs show the post-LPT change in cities adjacent to municipalities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities adjacent to cities matched to bombed locations. The regressions also include city fixed effects, bombed city fixed effects, and region-year fixed effects. Standard errors are clustered at the bombed city level. The vertical bars measure 95% confidence intervals. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>; Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/.

Figure A4: Yearly Effects of Fiscal Decentralization, Instrumental Variables

A. Employed—women

B. Gender gap in employment

C. Econ. inactive pop.—women

D. Stay-at-home women

E. Firms with < 2 employees

F. Manufacturing workers

Notes: The coefficients show the effect of a 1 percentage point increase in the share of revenues from local taxes. This variable is instrumented by a dummy that identifies cities adjacent to municipalities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities adjacent to cities matched to bombed locations. The omitted year is 1991. The regressions also include city fixed effects, bombed city fixed effects, and region-year fixed effects. Standard errors are clustered at the bombed city level. The vertical bars measure 95% confidence intervals. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>; Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/.

Figure A5: Effects of Fiscal Decentralization on Labor Markets, Alternative Sample

A. Share of revenues from local taxes

B. Gender gap in employment

C. Employed—women

D. Econ. inactive pop.—women

E. Stay-at-home women

F. Firms with < 2 employees

Notes: These graphs show the post-LPT change in cities within 10km of municipalities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities between 10km and 20km of bombed locations. The regressions also include city fixed effects, bombed city fixed effects, and province-year fixed effects. Standard errors are clustered at the bombed city level. The vertical bars measure 95% confidence intervals. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>; Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/.

Figure A6: Effects of Fiscal Decentralization on Labor Markets, Matching Layer 1

Notes: These graphs show the post-LPT change in cities adjacent to municipalities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities matched to cities adjacent to bombed locations. The regressions also include city fixed effects, bombed city fixed effects, and region-year fixed effects. Standard errors are clustered at the bombed city level. The vertical bars measure 95% confidence intervals. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>; Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/.

Figure A7: Probability of Having a Child Below Three Years Old, SHIW

A. 1987-1993

B. 1987-2010

Notes: These graphs show the probability of having a child below three years old for women working in the private sector (`qualp3==1` and `settp9!=8`). The data comes from sequential waves of the Bank of Italy's Survey of Household and Income Wealth, a representative survey of the Italian population. Panel A restricts the waves around the implementation of the LPT (1987-1989-1991-1993), while panel B shows data from all the waves until 2010 (1987-1989-1991-1993-1995-1997-2000-2002-2004-2006-2008-2010). Source: Bank of Italy's Survey of Household and Income Wealth, available online at <https://www.bancaditalia.it/statistiche/tematiche/indagini-famiglie-imprese/bilanci-famiglie/distribuzione-microdati/index.html>.

Figure A8: Pre-Reform Trends, Employees of Privately Owned Firms

Notes: These graphs show triple interactions of age, a dummy equal to 1 for near-bombed locations, and pre-reform year dummies. The sample includes only women. The control group is formed by municipalities adjacent to cities matched to bombed locations. The omitted age group is composed by 50 to 54 years old. For sake of clarity, the graphs shows the coefficients for only two age bins (30-34 years old and 35-39 years old). Table A4 provides more evidence on the remaining age bins. The regressions also include the pairwise interactions between the main variables, city fixed effects, bombed city fixed effects, age-year fixed effects, and region-year fixed effects. Standard errors are clustered at the bombed city level. The vertical bars measure 95% confidence intervals. Source: Istituto Nazionale della Previdenza Sociale (INPS).

Figure A9: Yearly Effects, Employees of Privately Owned Firms

Notes: These graphs show triple interactions of age bins, a dummy equal to 1 for near-bombed locations, and year dummies. The sample includes only women. The control group is formed by municipalities adjacent to cities matched to bombed locations. For sake of clarity, the graphs shows the coefficients for only four age bins (20-24, 25-29, 30-34, 35-39 years old). The omitted age group is composed by 50 to 54 years old. The regressions also include the pairwise interactions between the main variables, city fixed effects, bombed city fixed effects, age-year fixed effects, and region-year fixed effects. Standard errors are clustered at the bombed city level. The vertical bars measure 95% confidence intervals. Source: Istituto Nazionale della Previdenza Sociale (INPS).

Figure A10: Number of Municipal Elections by Date

Notes: This graph shows the number of municipal elections by date in the estimating sample (near-bombed and near-others municipalities). Data before 1993 might be incomplete. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>.

Table A1: Additional Summary Statistics

	All years				$t \leq 1992$	$t > 1992$
	Mean	Std. Dev.	Obs.	Availability	Mean	Mean
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Balance sheets of Italian municipalities						
<u>Share of total spending for local services</u>						
Administrative tasks	40.29	11.25	95633	1998-2010		
Judicial system	0.09	0.33	95642	1998-2010		
Police	4.34	2.81	95641	1998-2010		
Education	10.09	4.98	95638	1998-2010		
Culture	2.05	1.99	95642	1998-2010		
Sport	1.56	1.43	95642	1998-2010		
Tourism	0.66	1.27	95642	1998-2010		
Transport system	9.15	4.52	95639	1998-2010		
Public health	18.83	7.71	95637	1998-2010		
Welfare	9.88	7.67	95639	1998-2010		
Local econ. dev.	0.53	0.92	95642	1998-2010		
<u>Share of total revenues from local services</u>						
Administrative tasks	13.11	16.28	96001	1998-2010		
Judicial system	0.00	0.01	92157	1998-2010		
Police	9.37	15.18	95999	1998-2010		
Education	16.42	18.20	96022	1998-2010		
Culture	0.42	1.56	96025	1998-2010		
Sport	1.07	2.86	96026	1998-2010		
Tourism	0.25	1.60	96025	1998-2010		
Transport system	0.43	2.32	96026	1998-2010		
Public health	27.14	30.37	96024	1998-2010		
Welfare	12.75	18.16	95282	1998-2010		
<u>Other variables</u>						
Spend. for nursery schools (%)	1.15	2.10	95642	1998-2010		
Rev. from nursery schools (%)	1.55	4.50	92504	1998-2010		
Panel B: Census data						
Foreign residents	235.74	2389.56	24256	1991-2011	44.07	331.44
Pupils in nursery schools	28.14	235.36	24262	1991-2011	20.41	32.00
Births	11.88	70.20	16177	1991-2011	11.97	11.80
Stay-at-home women	1098.36	8010.02	40349	1981-2011	1290.07	811.93
Gender gap in employment	-653.06	3475.06	32314	1981-2011	-783.29	-523.18

Notes: This table shows additional summary statistics for ancillary variables used in the empirical analysis. Panel A shows data from the balance sheets of Italian municipalities. Monetary values are in expressed in 2017 €. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>. Panel B shows data from the population and industrial censuses. Source: Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/.

Table A2: Correlation between Age of Buildings and Effect of the Policy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Dependent variable is average cadastral value in 2013 (first available year)							
Share of pre-WWII buildings	-3.631*** (0.269)	-3.522*** (0.234)	-3.116*** (0.223)	-2.934*** (0.202)	-1.638*** (0.193)	-0.800*** (0.171)	-0.546*** (0.155)
Controls	Region FE	Province FE	(2) + building size	(3) + building qual.	(4) + geography	(5) + demography	(6) + economy
Observations	7,990	7,990	7,990	7,990	7,990	7,987	7,987
R^2	0.412	0.555	0.604	0.615	0.684	0.718	0.767
Dep. var.—mean	351.2	351.2	351.2	351.2	351.2	351.2	351.2
Dep. var.—std. dev.	149.95	149.95	149.95	149.95	149.95	149.98	149.98
Pre-WWII buildings—mean	40.24	40.24	40.24	40.24	40.24	40.24	40.24
Pre-WWII buildings—std. dev.	19.04	19.04	19.04	19.04	19.04	19.04	19.04
Panel B: Dependent variable is change in share of revenues from local taxes between 1990 and 1994							
Share of pre-WWII buildings	-0.132*** (0.011)	-0.136*** (0.010)	-0.141*** (0.011)	-0.135*** (0.010)	-0.098*** (0.011)	-0.061*** (0.010)	-0.052*** (0.010)
Controls	Region FE	Province FE	(2) + building size	(3) + building qual.	(4) + geography	(5) + demography	(6) + economy
Observations	7,185	7,185	7,179	7,179	7,179	7,176	7,176
R^2	0.212	0.260	0.263	0.268	0.301	0.318	0.329
Dep. var.—mean	9.19	9.19	9.19	9.19	9.19	9.19	9.19
Dep. var.—std. dev.	7.94	7.94	7.93	7.93	7.93	7.94	7.94
Pre-WWII buildings—mean	39.97	39.97	39.99	39.99	39.99	39.99	39.99
Pre-WWII buildings—std. dev.	19.04	19.04	19.03	19.03	19.03	19.04	19.04
Panel C: Dependent variable is median rental value per m ² between 2002 and 2010							
Share of pre-WWII buildings	0.054*** (0.008)	0.030*** (0.009)	0.022** (0.010)	0.020** (0.010)	0.035*** (0.009)	-0.013 (0.009)	-0.003 (0.009)
Controls	Year FE	(1) + region FE	(2) + building size	(3) + building qual.	(4) + geography	(5) + demography	(6) + economy
Observations	935	935	935	935	935	935	935
R^2	0.128	0.421	0.435	0.443	0.540	0.618	0.636
Dep. var.—mean	4.43	4.43	4.43	4.43	4.43	4.43	4.43
Dep. var.—std. dev.	2.01	2.01	2.01	2.01	2.01	2.01	2.01
Pre-WWII buildings—mean	21.09	21.09	21.09	21.09	21.09	21.09	21.09
Pre-WWII buildings—std. dev.	9.63	9.63	9.63	9.63	9.63	9.63	9.63

Notes: In panel A, the dependent variable is the average cadastral value in 2013, the first available year. Source: Agenzia del Territorio, Statistiche Catastali. In panel B, the dependent variable is the policy-induced change in fiscal federalism, measured as the change in the share of revenues coming from local taxes between 1990 and 1994. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>. In panel C, the dependent variable is the median rental value for a m² of residential real estate between 2002 and 2010. The database measures market values in multiple areas within a municipality, but only larger cities are included in the sample. Source: Osservatorio del Mercato Immobiliare. Building size is the average number of rooms of residential buildings. Building quality is the share of high-quality residential buildings in the municipality (cadastral classes A1, A7, A8, A9). Geography: size of municipality, dummy for coastal cities, dummy for mountain cities, altitude. Demography: population, share of residents above 65 years old, share of household with 2 or fewer members, share of foreign-born residents, share of women. Economy: share of residents with university degree, share of unemployed, share working in the industrial sector, share working in the service sector. Standard errors clustered at the province level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table A3: Differences in Observable Characteristics Measured in 1991

	Bombed vs. all	Near bombed vs. Near others	Matched adjacent	Mean	Std. dev.		Bombed vs. all	Near bombed vs. Near others	Matched adjacent	Mean	Std. dev.
	(1)	(2)	(3)	(4)	(5)		(1)	(2)	(3)	(4)	(5)
<u>Panel A: Age of buildings and change in fiscal federalism</u>											
Share of pre-WWII buildings	-4.058*** (0.915)	-3.560** (1.548)	-2.872* (1.739)	40.28	19.04	Δ Rev. local tax (94-90)	1.446*** (0.485)	1.652*** (0.485)	1.857*** (0.583)	9.20	7.94
<u>Panel B: Population and industrial census</u>											
Population density	-14.908 (52.015)	-10.829 (33.557)	-10.464 (32.297)	268.9	626.64	Econ. active pop.—women	-33.005 (85.991)	92.548 (64.645)	48.668 (46.610)	931.69	6147.94
Area (km ²)	9.571*** (2.583)	2.261 (2.588)	0.070 (2.419)	37.24	50.71	Econ. inactive pop.—women	-3.033 (228.024)	87.815 (162.094)	44.127 (148.117)	2525.66	15264.21
Rural city	-0.080*** (0.030)	0.012 (0.029)	0.019 (0.035)	0.68	0.47	Employed—men	15.290 (142.353)	152.236 (103.312)	83.891 (82.918)	1583.35	9386.46
Coastal city	0.028* (0.017)	-0.026* (0.014)	-0.020 (0.015)	0.08	0.27	Econ. active pop.—men	11.630 (153.011)	136.681 (110.844)	71.353 (91.620)	1706.58	10204.94
Population	-44.437 (630.905)	326.798 (452.572)	162.100 (396.556)	7024.35	42506.97	Econ. inactive pop.—men	7.324 (135.831)	53.177 (98.131)	30.282 (87.799)	1538.32	8995.93
Population—women	-44.757 (324.575)	162.694 (232.589)	80.402 (202.703)	3614.99	22243.91	Pupils in nursery schools	-1.184 (2.179)	-0.342 (1.760)	-0.448 (1.273)	20.41	120.93
Population—men	0.320 (306.450)	164.103 (220.044)	81.698 (193.907)	3409.35	20265.87	Firms	8.009 (36.247)	34.071 (24.539)	21.968 (20.064)	405.47	2119.63
Share women	0.168* (0.091)	0.125 (0.098)	0.130 (0.109)	50.93	1.59	Firms with < 2 employees	14.298 (24.871)	21.861 (16.075)	15.145 (13.665)	278.6	1350.98
Foreign residents	-11.879** (5.567)	1.021 (2.903)	-1.288 (2.624)	44.07	646.42	Firms with $\geq$ 200 employees	-0.024 (0.025)	0.009 (0.014)	0.002 (0.013)	0.12	1.75
Population > 65 years old	0.235 (92.890)	76.948 (57.665)	62.862 (44.058)	1076.32	6604.42	Agricultural firms	0.381 (0.656)	1.228** (0.538)	1.568** (0.668)	3.81	19.15
Households	-56.552 (216.787)	141.700 (150.010)	83.360 (122.696)	2463.04	15948.85	Agricultural workers	0.171 (1.918)	2.608* (1.500)	2.804* (1.617)	11.02	45.08
Births	-0.388 (1.278)	-0.597 (0.850)	-0.780 (0.909)	11.97	79.63	Manufacturing firms	4.593 (6.480)	8.357 (6.005)	3.523 (5.513)	73.2	314.05
Buildings	-56.242 (214.700)	138.391 (149.549)	79.817 (122.222)	2441.62	15755.99	Manufacturing workers	47.844 (72.660)	119.621* (63.335)	54.142 (54.256)	644.48	2918.2
Employed	-15.523 (220.063)	247.111 (162.559)	134.805 (124.315)	2433.9	14964.2	Retail firms	-0.503 (16.373)	7.392 (10.187)	5.277 (8.796)	170.42	1012.85
Econ. active pop.	-21.190 (237.207)	229.301 (174.046)	120.021 (136.635)	2637.77	16318.2	Retails workers	-45.047 (39.790)	17.434 (27.935)	6.881 (20.662)	408.63	3014.89
Econ. inactive pop.	4.510 (363.484)	141.002 (259.907)	74.409 (235.733)	4063.21	24253.33	Real estate firms	-4.715* (2.622)	0.154 (2.433)	0.699 (2.258)	25.29	227.93
Employed—women	-30.988 (79.068)	94.843 (60.421)	50.914 (42.671)	851	5606.78	Real estate workers	-38.090*** (12.108)	-0.783 (8.149)	-0.107 (5.509)	90.05	1268.12
<u>Panel C: Balance sheets of Italian municipalities</u>											
Rev. from local taxes (%)	1.226*** (0.346)	1.110** (0.455)	0.567 (0.556)	11.26	6.49	Current spending (%)	3.125*** (0.983)	1.487* (0.844)	1.194 (1.019)	52.17	15.21
Rev. from gov. transfers (%)	0.659 (0.648)	0.064 (0.636)	-0.310 (0.714)	36.93	11.96	Capital spending (%)	-3.836*** (1.017)	-1.926* (1.028)	-1.128 (1.176)	32.39	17.69
Rev. from local services (%)	1.284** (0.646)	0.691 (0.512)	0.814 (0.586)	9.68	7.92	Loan payments (%)	0.632* (0.365)	0.054 (0.232)	-0.132 (0.287)	5.99	5.43
Capital revenues (%)	-1.776** (0.774)	-1.330 (0.914)	-1.037 (1.015)	19.51	15.8	Total spending (per cap.)	-249.292*** (52.579)	-96.878 (69.423)	-82.599 (76.114)	1636.84	1222.49
Rev. from loan servicing (%)	-1.291** (0.594)	-0.334 (0.672)	-0.322 (0.794)	12.96	11.87	Deficit (per cap.)	6.569 (4.082)	-1.699 (4.989)	2.289 (6.348)	-10.01	118.43
Total revenues (per cap.)	-238.621*** (53.836)	-93.834 (69.724)	-77.833 (76.794)	1633.69	1217.33	Ratio of rev. and spend.	0.001 (0.002)	-0.001 (0.003)	0.002 (0.004)	0.99	0.05

Notes: This table shows differences in the levels of observables characteristics measured in 1991. All monetary values are expressed in 2017 €. Column 1 compares municipalities hit by Allied bombings after the Armistice of Cassibile to other matched Italian cities. The matching process uses geographical and demographic characteristics measured in 1991 (population, area, population density, number of buildings, share of homeowners, share of residents below 3 years old, and region fixed effects). Column 2 compares municipalities around bombed cities (near bombed) to cities around municipalities matched to bombed locations (near others). Column 3 further matches near-bombed cities to near-others cities using just population and area size in 1991. In panel A, the dependent variables measure the average age of buildings and the policy-induced change in fiscal federalism (the change in the share of revenues coming from local taxes between 1990 and 1994). In panel B, the dependent variables come from the population census and the industrial census of 1991. In panel C, the dependent variables come from balance sheets of Italian municipalities. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>. Regressions also include region fixed effects (column 1) and bombed city fixed effects (columns 2 and 3). Standard errors clustered at the province level (column 1) or at the bombed city level (columns 2 and 3) in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: Trends Before the LPT Introduction, More Variables

Panel A: Population and industrial census								
	Econ. active population		Agricultural firms		Manufacturing firms		Retail firms	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Near bombed x Trend	2.092 (1.618)		-0.065 (0.048)		0.093 (0.095)		0.009 (0.140)	
Near bombed x 1991		20.918 (16.178)		-0.653 (0.477)		0.926 (0.954)		0.093 (1.403)
Observations	4,834	4,834	4,848	4,848	4,848	4,848	4,848	4,848
R^2	0.996	0.996	0.790	0.790	0.988	0.988	0.989	0.989
Dep. var.—mean	1427.63	1427.63	3.09	3.09	49.27	49.27	86.29	86.29
Dep. var.—std. dev.	2215.04	2215.04	10.65	10.65	86.52	86.52	140.65	140.65

Panel B: Social Security data								
	New entry in labor market		Re-entry in labor market		Median wage		Median days worked	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Near bombed x 20-24 x Trend	-0.034* (0.018)		-0.006 (0.011)		-26.566 (61.929)		0.210 (0.779)	
Near bombed x 25-29 x Trend	0.005 (0.005)		0.014 (0.010)		-63.898 (70.786)		-0.974 (0.775)	
Near bombed x 30-34 x Trend	0.001 (0.003)		0.008 (0.008)		-62.627 (74.361)		-0.325 (0.853)	
Near bombed x 35-39 x Trend	-0.002 (0.003)		0.003 (0.007)		-30.757 (68.587)		-0.015 (0.743)	
Near bombed x 40-44 x Trend	0.001 (0.003)		0.002 (0.006)		-121.766* (72.710)		-1.423* (0.735)	
Near bombed x 45-49 x Trend	-0.001 (0.003)		0.005 (0.006)		9.355 (74.414)		-0.230 (0.747)	
Nonlinear trends—Partial F-test		0.70		0.95		0.98		0.93
Observations	370,484	370,484	248,966	248,966	370,484	370,484	370,484	370,484
R^2	0.314	0.314	0.334	0.334	0.272	0.272	0.314	0.314
Dep. var.—mean	0.42	0.42	0.37	0.37	14,109.2	14,109.2	230.28	230.28
Dep. var.—std. dev.	1.75	1.75	0.95	0.95	7,175.81	7,175.81	89.29	89.29

Notes: “Near bombed” is 1 for municipalities adjacent to cities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities adjacent to cities matched to bombed locations. Panel A estimates pre-reform trends between 1981 and 1991 (2 observations per municipality) using data from the population and industrial censuses. Source: Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/. Panel B estimates pre-reform linear and nonlinear trends using Social Security data on female employees of privately owned firms. The pre-reform years span from 1987 to 1992 for all variables, but “Re-entry in labor market (1989-1992).” In the case of nonlinear trends, the table reports the p-values from the partial f-tests on the triple interactions between the age bins, a dummy equal to 1 for near-bombed locations, and pre-reform year dummies. Source: Istituto Nazionale della Previdenza Sociale (INPS). The regressions also include city fixed effects, bombed city fixed effects, region-year fixed effects, and age-year fixed effects (only in Panel B). Standard errors clustered at the bombed city level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table A5: Trends Before the LPT Introduction, Alternative Sample

Panel A: Balance sheets of Italian municipalities							
	Share of revenues for local taxes		Share of revenues from gov. transfers		Revenues per capita		Spending per capita
	(1)	(2)	(3)	(4)	(5)	(6)	(7) (8)
Layer 1 x Trend	0.176** (0.085)		0.017 (0.218)		-8.999 (16.559)		-4.958 (16.597)
Layer 1 x 1991		0.280* (0.143)		0.763* (0.402)		25.175 (32.811)	23.501 (32.669)
Layer 1 x 1992		0.353** (0.169)		0.041 (0.436)		-17.378 (33.152)	-9.399 (33.236)
Observations	10,595	10,595	10,593	10,593	10,857	10,857	10,857
R ²	0.857	0.857	0.685	0.686	0.744	0.744	0.748
Dep. var.—mean	12.99	12.56	36.67	36.65	1509.77	1562.61	1507.49
Dep. var.—std. dev.	6.64	6.65	11.53	11.59	1022.66	1077.38	1024.64
F statistic		2.81		2.54		0.96	0.62
P value		0.06		0.08		0.38	0.54

Panel B: Population and industrial census							
	Employed		Econ. inactive pop.		Employed—women		Econ. inactive pop.—women
	(1)	(2)	(3)	(4)	(5)	(6)	(7) (8)
Layer 1 x Trend	1.144 (1.752)		0.578 (1.670)		0.738 (0.842)		0.266 (1.040)
Layer 1 x 1991		11.437 (17.522)		5.777 (16.696)		7.381 (8.419)	2.659 (10.402)
Observations	7,380	7,380	7,380	7,380	7,380	7,380	7,380
R ²	0.997	0.997	0.996	0.996	0.993	0.993	0.996
Dep. var.—mean	1692.98	1622.53	2645.05	2580.31	573.03	545.9	1648.28
Dep. var.—std. dev.	2481.31	2387.62	4006.84	3910.96	889.69	850.27	2516.45

	Population		Firms		Firms with < 2 employees		Firms with ≥ 200 employees
	(1)	(2)	(3)	(4)	(5)	(6)	(7) (8)
Layer 1 x Trend	3.541 (3.508)		0.049 (0.330)		-0.071 (0.238)		0.000 (0.001)
Layer 1 x 1991		35.408 (35.076)		0.489 (3.304)		-0.714 (2.379)	0.003 (0.008)
Observations	7,380	7,380	7,400	7,400	7,400	7,400	7,400
R ²	0.997	0.997	0.991	0.991	0.987	0.987	0.907
Dep. var.—mean	4598.41	4464.11	277.85	268.29	194.17	189.16	0.07
Dep. var.—std. dev.	6902.07	6708.05	391.92	383.14	263.55	261	0.38

Notes: This table shows pre-reform trends in key city-level variables. Monetary values are expressed in 2017 €. “Layer” is 1 for municipalities that are located within 10km of cities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities located between 10km and 20 km of bombed locations. Panel A estimates pre-reform trends between 1990 and 1992 using data from balance sheets of Italian municipalities. The F-statistic at the bottom tests for the joint significant of the nonlinear trends in 1991 and 1992. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>. Panel B estimates pre-reform trends between 1981 and 1991 (2 observations per municipality) using data from the population and industrial censuses. Source: Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/. The regressions also include city fixed effects, bombed city fixed effects, and province-year fixed effects. Standard errors clustered at the bombed city level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Did Mayors Respond to the LPT Introduction?

	Region-year fixed effects			Province-year fixed effects			Mean outcome	Std. Dev.
	Near bombed	Obs.	R^2	Near bombed	Obs.	R^2		
	(1)	(2)	(3)	(6)	(7)	(8)	(9)	(10)
LPT rate	-0.049 (0.033)	28,536	0.320	-0.056* (0.033)	28,454	0.387	5.62	0.88
LPT rate for homeowners	-0.062** (0.031)	28,526	0.200	-0.097*** (0.030)	28,444	0.273	5.15	0.74
Tax benefits for homeowners	-0.001 (0.018)	28,579	0.174	0.030 (0.019)	28,497	0.244	0.31	0.46
Share of issued building permits	-1.369** (0.611)	16,159	0.094	-1.879*** (0.617)	16,098	0.148	82.75	19.89
Buildings built after 1991	11.944 (16.479)	9,595	0.659	8.788 (17.032)	9,571	0.695	385.59	619.1

Notes: This table shows differences in LPT tax rates and rate of construction of new buildings, using variables from balance sheets that are available between 1998 and 2010. The number of buildings constructed after 1991, instead, comes from the population censuses. “Near bombed” is 1 for municipalities adjacent to cities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities adjacent to cities matched to bombed locations. The regressions also include region-year (column 1) or province-year (column 4) fixed effects, as well as controls for population, area of the municipality, a dummy for coastal cities, and a dummy for urban cities. Standard errors clustered at the bombed city level in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>.

Table A7: Effects on Municipal Spending, Additional Results

	Region-year fixed effects			Province-year fixed effects			Mean outcome	Std. Dev.
	Near bombed	Obs.	R^2	Near bombed	Obs.	R^2		
	(1)	(2)	(3)	(4)	(5)	(6)		
	Share of total spending for local services							
Administrative tasks	-1.210*** (0.442)	28,244	0.318	-1.539*** (0.475)	28,161	0.382	41.28	11.02
Judicial system	-0.003 (0.010)	28,248	0.080	-0.009 (0.010)	28,165	0.129	0.06	0.25
Police	0.355*** (0.126)	28,248	0.209	0.432*** (0.133)	28,165	0.271	4.26	2.91
Education	0.342 (0.256)	28,246	0.198	0.563** (0.248)	28,163	0.312	10.23	5.06
Culture	0.119 (0.073)	28,248	0.219	0.135* (0.071)	28,165	0.286	1.89	1.93
Sport	0.196*** (0.061)	28,248	0.150	0.188*** (0.062)	28,165	0.218	1.54	1.45
Tourism	-0.062 (0.055)	28,248	0.096	-0.100* (0.060)	28,165	0.153	0.66	1.27
Transport system	-0.534** (0.206)	28,247	0.212	-0.563*** (0.203)	28,164	0.303	9.54	4.58
Public health	0.063 (0.325)	28,246	0.250	0.487 (0.302)	28,163	0.395	18.75	7.4
Welfare	0.922*** (0.269)	28,248	0.321	0.809*** (0.255)	28,165	0.386	9.19	7.37
Local econ. development	0.094*** (0.031)	28,248	0.098	0.045 (0.035)	28,165	0.169	0.45	0.86

Notes: This table shows differences in spending for publicly provided services, using variables from balance sheets that are available between 1998 and 2010. “Near bombed” is 1 for municipalities adjacent to cities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities adjacent to cities matched to bombed locations. The regressions also include region-year (column 1) or province-year (column 4) fixed effects, as well as controls for population, area of the municipality, a dummy for coastal cities, and a dummy for urban cities. Standard errors clustered at the bombed city level in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>.

Table A8: Effects of Fiscal Decentralization on Local Labor Markets, More Variables

	Region-year fixed effects			Province-year fixed effects			Mean outcome	Std. Dev.
	Near bombed x Post	Obs.	R^2	Near bombed x Post	Obs.	R^2		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employed—men	11.734 (16.768)	9,686	0.983	26.469 (20.980)	9,662	0.985	891.28	1367.29
Econ. active pop.—men	23.570 (23.029)	9,686	0.979	43.287 (27.797)	9,662	0.982	947.09	1468.19
Econ. inactive pop.—men	-41.514*** (15.866)	9,686	0.980	-49.758*** (17.273)	9,662	0.983	856.68	1354.1
Population	107.995 (73.271)	9,688	0.985	172.895* (94.433)	9,664	0.987	3844.21	6109.21
Population—women	56.768 (37.503)	9,688	0.985	89.376* (48.245)	9,664	0.987	1959.16	3117.21
Population—men	51.227 (35.911)	9,688	0.985	83.519* (46.300)	9,664	0.987	1885.04	2993.32
Foreign residents	49.582*** (13.541)	7,271	0.662	50.568*** (12.251)	7,253	0.713	18.64	40.89
Migration balance	0.153 (1.092)	7,278	0.398	1.254 (1.062)	7,260	0.486	1.66	18.34
Positive migration	-0.016 (0.023)	7,278	0.384	0.003 (0.026)	7,260	0.405	.43	.49
Commuters (2011)	61.243** (26.310)	2,403	0.939	36.297 (27.226)	2,397	0.956	1365.05	2062.01
Agricultural workers	-2.909*** (0.971)	9,701	0.678	-1.903 (1.336)	9,677	0.707	8.52	40.63
Manufacturing workers	-42.745** (18.574)	9,701	0.932	-52.522*** (19.588)	9,677	0.937	419.71	914.11

Notes: Monetary values are expressed in 2017 €. “Near bombed” is 1 for municipalities adjacent to cities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities adjacent to cities matched to bombed locations. “Post” is 1 from 1993, when the LPT was introduced. The regressions also include city fixed effects, bombed city fixed effects, and either region-year (column 1) or province-year (column 4) fixed effects. Standard errors clustered at the bombed city level in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>; Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/.

Table A9: Effects of Fiscal Decentralization, Instrumental Variables

	Region-year fixed effects				Province-year fixed effects				Mean outcome	Std. Dev.	Mean treatment	Std. Dev.
	Δ Local taxes x Post	Obs.	R^2	F stat.	Δ Local taxes x Post	Obs.	R^2	F stat.				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)				
Employed	47.282** (21.656)	8,750	0.979	12.97	61.765** (27.161)	8,726	0.978	14.46	1363.89	2124.2	9.04	7.76
Econ. active pop.	67.628** (31.424)	8,750	0.968	12.97	90.361** (35.464)	8,726	0.967	14.46	1457.32	2274.97	9.04	7.76
Econ. inactive pop.	-85.337** (35.569)	8,750	0.975	12.97	-81.507** (37.340)	8,726	0.978	14.46	2292.02	3661.23	9.04	7.76
Gender gap in employment	-27.944** (11.712)	8,750	0.931	12.97	-20.692* (10.859)	8,726	0.945	14.46	455.47	720.13	9.04	7.76
Stay-at-home women	-29.407** (12.940)	8,750	0.961	12.97	-22.865* (12.693)	8,726	0.968	14.46	607.65	1159.16	9.04	7.76
Employed—women	37.613*** (12.461)	8,750	0.952	12.97	41.229*** (12.115)	8,726	0.954	14.46	454.21	743.21	9.04	7.76
Econ. active pop.—women	49.550*** (18.119)	8,750	0.929	12.97	57.654*** (18.611)	8,726	0.930	14.46	491.72	798.39	9.04	7.76
Econ. inactive pop.—women	-54.151** (22.538)	8,750	0.975	12.97	-50.320** (23.713)	8,726	0.978	14.46	1422.59	2300.03	9.04	7.76
Pupils in nursery schools	1.498** (0.707)	6,567	0.863	12.61	1.512** (0.651)	6,549	0.881	11.9	10.59	21.46	9.04	7.76
Births	0.481* (0.263)	4,378	0.924	12.29	0.797** (0.378)	4,366	0.902	11.61	6.55	12.71	9.04	7.76
Firms with < 2 employees	11.822* (6.738)	8,755	0.911	13.25	16.937*** (6.238)	8,731	0.917	14.79	165.33	230.04	9.04	7.76
Agricultural workers	-2.114** (1.003)	8,755	0.646	13.25	-1.222 (1.041)	8,731	0.702	14.79	8.93	42.01	9.04	7.76
Manufacturing workers	-33.342** (16.317)	8,755	0.917	13.25	-34.620** (16.182)	8,731	0.921	14.79	430.23	946.5	9.04	7.76

Notes: “ Δ Local taxes” measures the change in the share of revenues from local taxes measured between 1990 and 1994. “Post” is 1 from 1993, when the LPT was introduced. Their interaction is instrumented using the interaction between “Near bombed” and “Post”. “Near bombed” is 1 for municipalities adjacent to cities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities adjacent to cities matched to bombed locations. The regressions also include city fixed effects, bombed city fixed effects, and either region-year (column 1) or province-year (column 5) fixed effects. Standard errors clustered at the bombed city level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>; Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/.

Table A10: Effects of Fiscal Decentralization on Labor Markets, Alternative Sample

	Layer 1 is within (0km,10km] of bombed location					Layer 1 is within (0km,15km] of bombed locations				
	Layer 1 x Post	Obs.	R^2	Mean outcome	Std. Dev.	Layer 1 x Post	Obs.	R^2	Mean outcome	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<u>Panel A: Balance sheets of Italian municipalities</u>										
Share of rev. from local taxes	1.041*** (0.204)	72,010	0.787	13.3	6.65	1.035*** (0.214)	84,158	0.792	13.3	6.65
Share of rev. from gov. transfers	-0.246 (0.279)	72,011	0.697	36.56	11.46	-0.824*** (0.301)	84,157	0.689	36.56	11.46
<u>Panel B: Population and industrial census</u>										
Employed	46.980** (22.751)	14,786	0.984	1692.74	2480.6	91.770*** (29.241)	17,290	0.984	1622.37	2387.05
Econ. active pop.	71.449** (28.360)	14,786	0.978	1785.58	2623.54	122.134*** (42.130)	17,290	0.978	1716.16	2530.12
Econ. inactive pop.	-79.680** (33.953)	14,786	0.980	2645.33	4005.83	-95.030*** (36.495)	17,290	0.981	2580.6	3910.15
Gender gap in employment	-17.838 (11.846)	14,786	0.942	546.97	791.34	-24.100** (11.748)	17,288	0.945	531.02	773.96
Stay-at-home women	-28.980** (12.824)	14,786	0.966	719.48	1246.09	-35.748** (14.496)	17,290	0.967	698.83	1218.95
Employed—women	32.409** (13.502)	14,786	0.972	572.88	889.42	58.007*** (14.160)	17,288	0.971	545.8	850.05
Econ. active pop.—women	46.241*** (17.117)	14,786	0.960	612.88	948.13	75.438*** (20.862)	17,288	0.959	585.86	907.95
Econ. inactive pop.—women	-52.368** (21.950)	14,786	0.979	1648.44	2515.8	-60.479** (23.872)	17,288	0.980	1606.51	2455.07
Pupils in nursery school	1.561** (0.722)	11,102	0.909	13.02	25.22	2.628*** (0.706)	12,980	0.907	12.48	24.25
Births	0.360* (0.212)	7,398	0.949	7.56	13.74	0.367 (0.233)	8,650	0.950	7.39	13.49
Firms with < 2 employees	15.317* (8.964)	14,803	0.920	194.17	263.55	32.217*** (7.955)	17,307	0.921	189.16	261

Notes: “Layer” is 1 for municipalities that are located within 10km (column 1) or within 15km (column 6) of cities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities located between 10km and 20 km (column 1) or between 15km and 30km (column 6) of bombed locations. “Post” is 1 from 1993, when the LPT was introduced. The regressions also include city fixed effects, bombed city fixed effects, and province-year fixed effects. Standard errors clustered at the bombed city level in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>; Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/.

Table A11: Effects of Fiscal Decentralization on Labor Markets, Matching Layer 1

	Region-year fixed effects			Province-year fixed effects			Mean outcome	Std. Dev.
	Near bombed x Post	Obs.	R^2	Near bombed x Post	Obs.	R^2		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<u>Panel A: Balance sheets of Italian municipalities</u>								
Share of rev. from local taxes	1.366*** (0.373)	35,284	0.782	1.568*** (0.381)	34,987	0.800	11.81	6.63
Share of rev. from gov. transfers	-1.713*** (0.467)	35,281	0.659	-2.222*** (0.451)	34,984	0.685	36.16	11.85
<u>Panel B: Population and industrial census</u>								
Employed	28.722 (28.284)	7,233	0.983	62.206** (28.425)	7,173	0.986	1240.86	1764.88
Econ. active pop.	23.577 (43.053)	7,233	0.975	83.613** (41.690)	7,173	0.981	1319.44	1886.87
Econ. inactive pop.	-38.830 (35.142)	7,233	0.983	-97.219** (39.906)	7,173	0.986	2047.64	3125.82
Gender gap in employment	-15.528 (11.014)	7,233	0.957	-22.072* (12.255)	7,173	0.962	412.11	618.5
Stay-at-home women	-11.667 (13.422)	7,233	0.972	-26.996* (14.785)	7,173	0.976	539.68	994.22
Employed—women	22.125 (16.021)	7,233	0.962	42.139*** (15.443)	7,173	0.969	414.38	609.67
Econ. active pop.—women	19.045 (24.075)	7,233	0.943	54.155** (22.943)	7,173	0.956	446.26	653.36
Econ. inactive pop.—women	-22.884 (22.122)	7,233	0.983	-58.774** (25.702)	7,173	0.986	1273.05	1965.85
Pupils in nursery school	1.704** (0.748)	5,427	0.874	2.324*** (0.699)	5,382	0.897	9.31	17.94
Births	0.907*** (0.289)	3,618	0.939	1.400*** (0.326)	3,588	0.949	5.89	10.78
Firms with < 2 employees	-3.370 (9.569)	7,235	0.911	13.854 (8.795)	7,175	0.929	150.04	196.71

Notes: “Near bombed” is 1 for municipalities adjacent to cities bombed by Allied tactical air attacks during WWII. The control group is formed by non-bombed municipalities matched to cities adjacent to bombed municipalities using population and area size in 1991. The regressions also include city fixed effects, bombed city fixed effects, and either region-year (column 1) or province-year (column 4) fixed effects. Standard errors clustered at the bombed city level in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Italian Minister of the Interior, available online at <https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4>; Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/.

Table A12: Robustness checks

	Local taxes (% rev.) (1)	Gender gap in employment (2)	Employed (women) (3)	Econ. active pop.(women) (4)	Econ. inactive pop. (women) (5)	Pupils in nursery schools (6)
Panel A: Standard errors clustered at the province level						
Near bombed x Post	1.245*** (0.365)	-39.137** (16.269)	50.870*** (16.913)	67.765*** (24.258)	-74.593** (28.948)	2.475*** (0.859)
Observations	47,255	9,686	9,686	9,686	9,686	7,277
R ²	0.783	0.951	0.964	0.943	0.981	0.879
Panel B: Controls for population						
Near bombed x Post	1.117** (0.478)	-45.440*** (13.010)	35.101*** (9.619)	44.143*** (14.674)	-86.514*** (28.591)	1.659*** (0.548)
Observations	4,218	9,686	9,686	9,686	9,686	7,271
R ²	0.845	0.956	0.983	0.975	0.983	0.923
Panel C: Dependent variables are shares of residents (col. 3-6)						
Near bombed x Post			10.020*** (3.031)	10.938*** (3.233)	-0.722** (0.367)	1.839*** (0.662)
Observations			9,686	9,686	9,686	7,228
R ²			0.406	0.414	0.911	0.672
Panel D: Controls for nonlinear trends correlated with geographical characteristics						
Near bombed x Post	1.064*** (0.270)	-44.837*** (16.838)	56.851*** (14.554)	79.929*** (23.508)	-91.753*** (29.877)	2.725*** (0.765)
Observations	46,682	9,588	9,588	9,588	9,588	7,197
R ²	0.795	0.955	0.974	0.958	0.983	0.903
Panel E: Controls for nonlinear trends correlated with real estate market						
Near bombed x Post	0.941*** (0.296)	-47.542** (18.833)	50.059*** (16.130)	66.927*** (24.325)	-90.478*** (34.127)	2.275*** (0.833)
Observations	47,112	9,677	9,677	9,677	9,677	7,268
R ²	0.794	0.952	0.966	0.946	0.981	0.886
Panel F: End sample in 2001 before introduction of other local taxes						
Near bombed x Post	1.074*** (0.299)	-28.636*** (10.220)	37.790*** (12.056)	52.387** (20.853)	-71.211*** (26.827)	1.152** (0.491)
Observations	27,148	7,260	7,260	7,260	7,260	4,846
R ²	0.805	0.971	0.980	0.959	0.985	0.927
Panel G: Exclude special administrative regions						
Near bombed x Post	1.372*** (0.374)	-41.506*** (14.358)	39.981** (17.364)	55.611** (26.719)	-82.560*** (29.939)	2.258*** (0.795)
Observations	40,333	8,238	8,238	8,238	8,238	6,184
R ²	0.759	0.946	0.968	0.951	0.980	0.885
Panel H: Placebo reform						
Near bombed x Post	0.105 (0.229)	-1.144 (12.338)	6.485 (10.701)	9.352 (16.942)	-8.787 (24.480)	0.551 (0.635)
Observations	47,153	9,686	9,686	9,686	9,686	7,277
R ²	0.783	0.951	0.963	0.942	0.981	0.879

Notes: In panel B, regressions include population as a control. In panel D, regressions include geographical variables (population density, a dummy for rural municipalities, a dummy for coastal cities) interacted with year fixed effects. In panel E, regressions include variables describing the real estate market (average size of residential buildings, share of high-quality buildings as defined in Table A2) interacted with year fixed effects. Panel G excludes from the sample five special administrative regions (Valle d'Aosta, Trentino, Friuli, Sicilia, and Sardegna) that enjoy more autonomy. Panel H shows results of placebo tests in which the treatment variable “Near bombed” is assigned at random. *** p<0.01, ** p<0.05, * p<0.1.

Table A13: Effects of Fiscal Decentralization, Employees of Privately Owned Firms

	Near bombed x Post x 20-24	Near bombed x Post x 25-29	Near bombed x Post x 30-34	Near bombed x Post x 35-39	Near bombed x Post x 40-44	Near bombed x Post x 45-49	Mean outcome	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Entry and re-entry into the labor market								
New entry into the labor market	-0.098 (0.068)	0.060** (0.030)	0.023 (0.019)	0.002 (0.014)	-0.001 (0.010)	0.001 (0.008)	0.42	1.75
Re-entry into the labor market	-0.076*** (0.028)	0.005 (0.025)	0.079*** (0.030)	0.078*** (0.029)	0.036* (0.021)	0.016 (0.015)	0.37	0.95
Entry into a new firm	-0.310* (0.161)	0.256** (0.104)	0.344*** (0.106)	0.246*** (0.085)	0.085 (0.059)	0.100** (0.046)	2.12	4.54
Re-entry into the same firm	-0.020 (0.013)	-0.009 (0.009)	0.020*** (0.008)	0.019** (0.008)	0.017 (0.011)	0.016 (0.010)	0.13	0.43
Panel B: Characteristics of labor contracts								
Median wage	-674.523*** (240.946)	-555.762** (225.370)	-549.956** (222.869)	-315.301 (251.048)	82.953 (235.214)	165.886 (204.297)	14,109.22	7,175.81
Log median wage	-0.060*** (0.023)	-0.047** (0.020)	-0.044** (0.021)	-0.027 (0.025)	0.016 (0.020)	0.025 (0.018)	9.36	0.73
Median hourly wage	-0.135** (0.068)	-0.090 (0.066)	-0.125* (0.068)	-0.070 (0.073)	-0.032 (0.067)	-0.054 (0.058)	8.47	2.21
Median days worked	-7.532*** (2.756)	-6.191** (2.423)	-4.622** (2.341)	-1.368 (2.430)	1.731 (2.147)	3.804* (1.961)	230.28	89.29
Working outside province of res.	0.199 (0.293)	0.405*** (0.139)	0.373*** (0.126)	0.224** (0.104)	0.151** (0.069)	0.097* (0.050)	1.93	4.67
Panel C: Characteristics of labor contracts for entrants or re-entrants								
Median wage	-415.907** (182.300)	-447.323** (173.111)	-260.712 (188.708)	-459.764** (184.682)	-112.014 (213.472)	192.761 (208.595)	7,028.32	5,808.04
Log median wage	-0.080*** (0.030)	-0.084*** (0.029)	-0.033 (0.030)	-0.072** (0.033)	-0.029 (0.031)	0.047 (0.032)	8.48	1
Median hourly wage	-0.107 (0.094)	-0.121 (0.095)	-0.084 (0.104)	-0.101 (0.106)	-0.025 (0.109)	-0.037 (0.115)	8.12	2.96
Median days worked	-5.246** (2.397)	-6.102*** (2.320)	-3.045 (2.434)	-6.014** (2.363)	-2.543 (2.484)	2.622 (2.537)	119.82	80.26
Working outside province of res.	0.082 (0.113)	0.112 (0.076)	0.102 (0.064)	0.043 (0.052)	0.021 (0.045)	0.026 (0.040)	0.88	1.84
Panel D: Highest completed education								
High school	0.281** (0.133)	0.539*** (0.184)	0.574*** (0.190)	0.422*** (0.145)	0.232** (0.093)	0.117** (0.049)	0.84	2.33
University degree	0.123*** (0.033)	0.191*** (0.064)	0.142** (0.059)	0.068* (0.036)	0.025 (0.017)	0.011 (0.008)	0.11	0.46
Post-university degree	0.021*** (0.007)	0.015* (0.008)	0.018** (0.008)	0.014** (0.007)	0.010** (0.005)	0.007** (0.003)	0.03	0.21

Notes: Monetary values are expressed in 2017 €. The sample includes only women. “Near bombed” is 1 for municipalities adjacent to cities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities adjacent to cities matched to bombed locations. “Post” is 1 from 1993, when the LPT was introduced. The excluded age category is composed by 50 to 54 years old. The regressions also include the pairwise interactions between the main treatment variables, city fixed effects, bombed city fixed effects, region-year fixed effects, and age-year fixed effects. Standard errors clustered at the bombed city level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Source: Istituto Nazionale della Previdenza Sociale (INPS).

Table A14: Robustness checks, Employees of Privately Owned Firms

	New entry in labor market (1)	Re-entry in labor market (2)	Median wage (3)	Median days worked (4)	Working outside prov. (5)	Log median wage (6)
Panel A: Standard errors clustered at the province level						
Near bombed x Post x 20-24	-0.098 (0.083)	-0.076** (0.031)	-647.523** (262.109)	-7.532** (3.159)	0.199 (0.416)	-0.060** (0.026)
Near bombed x Post x 25-29	0.060* (0.034)	0.005 (0.022)	-555.762** (222.210)	-6.191** (2.685)	0.405** (0.169)	-0.047** (0.022)
Near bombed x Post x 30-34	0.023 (0.018)	0.079** (0.030)	-549.956** (232.850)	-4.622* (2.503)	0.373** (0.152)	-0.044** (0.022)
Near bombed x Post x 35-39	0.002 (0.014)	0.078*** (0.029)	-315.301 (250.072)	-1.368 (2.977)	0.224* (0.134)	-0.027 (0.026)
Near bombed x Post x 40-44	-0.001 (0.009)	0.036 (0.025)	82.953 (223.908)	1.731 (2.774)	0.151* (0.080)	0.016 (0.022)
Near bombed x Post x 45-49	0.001 (0.007)	0.016 (0.014)	165.886 (230.417)	3.804 (2.613)	0.097** (0.048)	0.025 (0.021)
Observations	1,674,914	1,553,397	1,674,914	1,674,914	1,674,914	1,674,914
R^2	0.360	0.420	0.259	0.307	0.680	0.253
Panel B: Controls for city-year fixed effects						
Near bombed x Post x 20-24	-0.096 (0.073)	-0.071*** (0.025)	-599.304** (243.029)	-6.631** (2.788)	0.180 (0.256)	-0.053** (0.023)
Near bombed x Post x 25-29	0.061* (0.033)	0.009 (0.027)	-507.602** (226.134)	-5.203** (2.428)	0.379*** (0.141)	-0.039* (0.020)
Near bombed x Post x 30-34	0.023 (0.022)	0.085** (0.034)	-504.257** (222.406)	-3.767 (2.321)	0.350** (0.164)	-0.037* (0.021)
Near bombed x Post x 35-39	0.003 (0.016)	0.083*** (0.032)	-266.296 (250.235)	-0.523 (2.370)	0.205 (0.137)	-0.021 (0.024)
Near bombed x Post x 40-44	0.001 (0.011)	0.041* (0.024)	114.472 (234.327)	2.311 (2.126)	0.139 (0.085)	0.021 (0.020)
Near bombed x Post x 45-49	0.001 (0.008)	0.017 (0.015)	200.146 (201.271)	4.325** (1.943)	0.080 (0.060)	0.029 (0.018)
Observations	1,674,751	1,553,249	1,674,751	1,674,751	1,674,751	1,674,751
R^2	0.383	0.472	0.291	0.345	0.784	0.293
Panel C: Placebo effects						
Near bombed x Post x 45-49	0.004 (0.008)	0.002 (0.024)	-18.761 (395.865)	-3.157 (4.567)	0.218 (0.135)	-0.037 (0.039)
Near bombed x Post x 50-54	0.002 (0.007)	-0.012 (0.019)	-176.973 (388.864)	-6.900 (4.496)	0.144 (0.123)	-0.062 (0.039)
Near bombed x Post x 55-59	0.003 (0.007)	-0.020 (0.015)	-120.489 (331.457)	-2.451 (4.403)	0.064 (0.081)	-0.032 (0.037)
Observations	583,260	545,856	583,260	583,260	583,260	583,260
R^2	0.089	0.240	0.197	0.232	0.533	0.195

Notes: Monetary values are expressed in 2017 €. All panels include only women. In panel A, regressions cluster standard errors at the province level. In panel B, regressions replace the region-year fixed effects with city-year fixed effects. In panel C, regressions estimate placebo treatment effects including only women above 45 years old. In this case, the excluded age category is composed by 60 years old. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Istituto Nazionale della Previdenza Sociale (INPS).

Table A15: Quadruple Interactions, Employees of Privately Owned Firms

	Near bombed x Post x 20-24 x Female	Near bombed x Post x 25-29 x Female	Near bombed x Post x 30-34 x Female	Near bombed x Post x 35-39 x Female	Near bombed x Post x 40-44 x Female	Near bombed x Post x 45-49 x Female	Mean outcome	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Entry and re-entry into the labor market								
New entry into the labor market	-0.023 (0.047)	-0.025 (0.028)	-0.045** (0.018)	-0.040*** (0.014)	-0.030*** (0.011)	-0.014* (0.008)	0.42	1.77
Re-entry into the labor market	0.763*** (0.066)	0.329*** (0.077)	0.286*** (0.079)	0.244*** (0.072)	0.241*** (0.061)	0.272*** (0.052)	0.51	1.41
Entry into a new firm	1.903*** (0.240)	2.006*** (0.237)	1.413*** (0.229)	1.090*** (0.217)	1.171*** (0.184)	1.286*** (0.165)	2.88	5.96
Re-entry into the same firm	0.221*** (0.016)	0.107*** (0.017)	0.116*** (0.020)	0.097*** (0.019)	0.103*** (0.019)	0.112*** (0.019)	0.19	0.61
Panel B: Characteristics of labor contracts								
Median wage	-136.579 (251.504)	-109.162 (244.162)	-277.240 (266.432)	-67.837 (308.954)	117.056 (293.903)	69.088 (245.525)	16,985.83	8,323.13
Log median wage	-0.031 (0.023)	-0.038* (0.021)	-0.043* (0.024)	-0.036 (0.027)	0.000 (0.023)	0.013 (0.021)	9.56	0.70
Median hourly wage	0.007 (0.075)	0.043 (0.075)	-0.037 (0.080)	0.065 (0.086)	0.006 (0.087)	-0.030 (0.072)	9.51	2.48
Median days worked	-4.827 (3.067)	-4.348 (2.696)	-4.138 (2.974)	-2.577 (3.183)	0.382 (2.762)	2.869 (2.475)	236.89	86.71
Working outside province of res.	0.317 (0.205)	0.193 (0.151)	-0.129 (0.148)	-0.256* (0.152)	-0.227* (0.131)	-0.135* (0.081)	2.92	6.08
Panel C: Characteristics of labor contracts for entrants or re-entrants								
Median wage	-458.156** (205.382)	-400.060** (208.934)	-275.805 (221.527)	-413.388* (237.803)	41.553 (254.105)	29.760 (252.279)	8,736.74	7,091.48
Log median wage	-0.095*** (0.032)	-0.093*** (0.032)	-0.058* (0.033)	-0.104*** (0.037)	-0.053 (0.037)	0.007 (0.037)	8.72	0.97
Median hourly wage	0.065 (0.120)	0.059 (0.124)	0.085 (0.131)	0.198 (0.134)	0.222 (0.140)	0.118 (0.137)	9.31	3.5
Median days worked	-7.364*** (2.558)	-7.464*** (2.523)	-4.847* (2.700)	-8.625*** (2.814)	-3.798 (2.584)	-0.727 (2.855)	121.6	79.07
Working outside province of res.	0.066 (0.123)	-0.054 (0.106)	-0.151 (0.100)	-0.156 (0.095)	-0.150* (0.078)	-0.031 (0.066)	1.27	2.41
Panel D: Highest completed education								
High school	0.151* (0.084)	0.214*** (0.079)	0.167** (0.076)	0.121** (0.061)	0.049 (0.050)	0.002 (0.034)	0.94	2.45
University degree	0.067*** (0.025)	0.083** (0.037)	0.024 (0.029)	0.000 (0.018)	0.004 (0.013)	-0.001 (0.007)	0.12	0.48
Post-university degree	0.000 (0.009)	-0.002 (0.008)	0.005 (0.007)	0.003 (0.006)	0.006 (0.005)	0.004 (0.003)	0.03	0.21

Notes: Monetary values are expressed in 2017 €. “Near bombed” is 1 for municipalities adjacent to cities bombed by Allied tactical air attacks during WWII. The control group is formed by municipalities adjacent to cities matched to bombed locations. “Post” is 1 from 1993, when the LPT was introduced. The excluded age category is composed by 50 to 54 years old. The regressions also include the triple and pairwise interactions between the main variables, as well as fixed effects for city, bombed city, age-year, gender-age, gender-year, and region-year. Standard errors clustered at the bombed city level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Source: Istituto Nazionale della Previdenza Sociale (INPS).