

The Effects of Fiscal Decentralization on Local Labor Markets*

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Abstract

Fiscal decentralization can affect local services and labor markets, but the direction of this effect is theoretically ambiguous. This paper studies a 1993 reform that quickly increased the fiscal autonomy of Italian municipality through the introduction of a local property tax. The identification exploits cross-municipal variation in the age of buildings stemming from Allied bombings during WWII. Decentralization increased female labor supply by 14 percent, reducing the gender gap in employment. Expanded access to subsidized public nursery schools might be responsible for these results. The changes on public services and labor outcomes are larger in areas with higher political competition.

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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. [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 increases in fiscal federalism starting from the mid 1980s. This shift in fiscal institutions has been generally welcomed as an improvement over the old centralized system. In 2003, for example, the World Bank stated that fiscal decentralization improved public services, urban management, and social inclusion by giving new legitimacy and authority to local governments ([World Bank, 2003](#)). In spite of its growing importance, it is still not clear how fiscal decentralization affects labor markets.

On this issue, economic theory provides conflicting answers. On the one hand, fiscal decentralization can increase the accountability of local politicians, making their actions more transparent and assuaging the principal–agent problem between voters and elected officials. 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 at a minimum in order to decrease taxes and attract mobile capital.

In any situation in which economic theory provides ambiguous hypotheses, it would be important to produce quantitative analysis of real-life reforms. Any empirical analysis of fiscal decentralization, however, faces substantial challenges. First, the transfer of fiscal authority to local governments is often a gradual process that happens over several years as a result of multiple law changes. 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. This paper studies an 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 isolate the effect the the policy.

In 1993, a reform introduced a local property tax (LPT) to finance the 8,092 Italian municipalities. Italians paid the LPT separately from any other tax and directly to their municipality. Local governments could set their preferred tax rate within a pre-established range and kept the total amount paid by their residents. The new system represented a drastic change in the financing of local services. Under the old status quo, in fact, 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 not to increase the size of municipal budgets, revenues from the LPT replaced government transfers euro for euro. As a result, the municipal share of revenues from government transfers dropped from nearly 40 percent to less than 30 percent in just a year, making local taxes and service fees the major revenue stream.

We assembled a panel database at the municipal level, using data from the decennial population and industry censuses between 1981 and 2011 and from yearly municipal balance sheets between 1990 and 2010. We also use worker-level data from Social Security to estimate heterogeneous effects of fiscal decentralization within each city. In its simplest form, the empirical analysis could compare outcomes in municipalities differentially exposed to fiscal federalism, 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 with other drivers of labor market outcomes.

Our preferred specification, instead, leverages variation in the average age of buildings that is not correlated with other characteristics of real estate, such as size or quality of the finishes. The individual LPT liability is computed on the so-called cadastral value, a bureaucratic value that is assigned by an independent agency to each property at the time of its construction. The cadastral value is used exclusive for fiscal purposes, is updated only in case of major renovations, and is generally not aligned with market values. 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. 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 other Italian cities. Due to imprecise air attacks and the use of carpet bombings, they experienced damages during WWII. Second, municipalities near bombed cities were not explicitly targeted by Allied bombings, but were hit by mistake or random chance. In other words, they were not selected into receiving air attacks based on observed or unobserved characteristics that could possibly interact with the 1993 policy. Third, the initial cross-city differences in the exposure to the policy were permanent, due to local constraints in freely adjusting LPT revenues (for example, the upper bound to the LPT rate). This fact ensures that municipalities near bombed cities received a higher shares of revenues from local taxes throughout the post-LPT period under consideration, preventing control cities from increasing their degree of fiscal autonomy. As an apt control group, the baseline specifications use municipalities adjacent to cities matched to bombed locations based on their characteristics in the 1991 census. The results, however, are robust to different control groups.¹

Italian municipalities provide a vast array of services to their residents. Some of them might have direct consequences on local labor demand and supply. It is the case, for example, of public nursery schools, professional training courses, welfare programs, and initiatives to support local businesses. It is not the case of education, if we exclude nurseries. All other schools, from kindergartens to most universities, are under the direct control of the central government through the Ministry of Education. Municipalities provide only auxiliary services to local schools, such as bussing and lunch, that might not significantly influence education outcomes.

In near-bombed municipalities, we find three main changes to public services after the LPT. First, administrators shrank the size of the government, reducing simultaneously spending and revenues. They also rebalanced their spending in favor of revenue-generating services, such as welfare interventions, reducing the budget for their internal administrative processes. Second, there is no evidence of a reduction in quality of public services, even in those areas that did not experienced increased funding. These findings are consistent with the hypothesis that higher exposure to fiscal autonomy induced local politicians to cut waste and increase efficiency. Third, more spending in some areas translated into a higher quantity of related services. 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 a public nursery school.

Considering its importance for the labor supply of women, we further analyze the public provision of child care. Italian municipalities can decide to offer subsidized nursery schools

¹ As a control, for example, we can use the second layer of municipalities around bombed locations.

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). Consequently, the number of pupils in all nursery schools (public and private) was 24 percent higher. However, we do not observe any significant difference between treatment and control cities in the number of pupils attending private nursery schools in 2011, suggesting that the disparity in total enrollment might stem from children who would have not attended a more expensive private school.

Expanded access to local services is associated with an increase in employment. This effect, however, is concentrated among women. Female employment increased by 14 percent, while economically inactive women decreased by 7 percent. The overall result was an 8 percent reduction in the preexisting gender gap in employment. These findings using municipal-level data are highly indicative about the importance of subsidized child care on female labor supply, but cannot isolate the role of a specific public good. We dig deeper within municipalities using worker-level observations from Social Security. 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 child care should be more helpful for younger working women.

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 or local preferences towards public goods, have a smaller and less precise predictive power. These results, however, should only be regarded as suggestive evidence, because our setting does not offer quasi-experimental variation in these dimensions.

This paper contributes to two main strands of the literature. First, there is an extensive theoretical and empirical literature on the effects on fiscal decentralization on local politicians ([Ahmad and Brosio \(2006\)](#) for a survey) and labor markets ([Huther and Shah \(1998\)](#) for an overview). In these studies, the quantitative analysis often relies on cross-country correlations. There is a notable exception. [Hatfield and Kosec \(2013\)](#) study the effect of federal competition on economic growth. To achieve identification, they instrument the number of local governments with the length on local streams. We complement their findings by addressing a different driver of economic development, fiscal decentralization. More generally,

this paper contributes to this strand of the literature by proposing a novel identification strategy that leverages both longitudinal and cross-sectional variation. Moreover, it is able to analyze heterogeneous effects of fiscal federalism on different types of workers within the same local labor market.

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](#)). It only 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 child care. Moreover, it is one of the few studies to focus on pre-kindergarten care for children below three years old.

The rest of the paper is organized as follows. [Section 2](#) discusses the relationship between fiscal decentralization and labor market outcomes. [Section 3](#) outlines the policy change. [Section 4](#) describes the data. [Section 5](#) discusses the empirical strategy. [Section 6](#) shows the main results and several robustness checks. [Section 7](#) concludes.

2 The Relationship Between Fiscal Decentralization and Labor Markets

The literature on fiscal decentralization is incredibly vast and dates back several decades ([Musgrave, 1959](#); [Oates, 1972](#)).² In this section, we intend to discuss the main channels through which fiscal federalism might affect the provision of public goods and, therefore, the local labor markets.

A group of theoretical and empirical papers concludes that fiscal federalism can improve local services. If this hypothesis is true, it is plausible to observe beneficial consequences on labor supply and demand. 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. 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](#)). In

² [Ahmad and Brosio \(2006\)](#) provide a comprehensive description of relevant contributions and recent developments in this field.

this sense, fiscal federalism is able to alleviate the principal–agent problem between voters and mayor by making the management of local funds more transparent. This effect can happen through different, and not mutually exclusive channels. First, decentralization can increase the saliency of local taxes, allowing residents to more accurately assess the amount of government spending (Puviani, 1897). 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 for more resources to higher levels of government. Raising local tax rates without improving the quality of services could have a high political cost and would be a straightforward signal of bad management. In addition to increased accountability, fiscal federalism can raise the level of competition between municipalities, leading to a more efficient provision of public goods (Seabright, 1996; Hatfield and Kosec, 2013).³ 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, either question the effectiveness of these channels or outright hypothesize a negative relationship between fiscal federalism and public goods. If this hypothesis is true, we should observe either zero or negative effects of fiscal federalism on labor supply and demand. Fiscal federalism, 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). In the resulting equilibrium, fiscal federalism could 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 public goods 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 fiscal federalism. Moreover, competition between municipalities can become a race to 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 federalism

³ This idea is incorporated into the famous Tiebout model, in which individuals can “vote with their feet” (Tiebout, 1956).

and the labor markets is ultimately an empirical issue. In this paper, we propose a novel identification strategy to isolate this effect on the quantity of local services, labor supply, and labor demand.

3 The Introduction of the Local Property Tax

Between 1992 and 1993, two laws drastically increased the fiscal independence of Italian municipalities. 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; Table 1, panel A).

The reform had the explicit goal to transition the public sector towards fiscal federalism. Each euro (Italian lira until 2002) earned through the LPT replaced a euro of government transfers. As a result, the LPT substantially changed the source of revenues of Italian municipalities. In the average city, revenues from local taxes increase from €149 per resident in 1990 to €280 per resident in 1994, an 88 percent increase (Figure 1).⁴ Over the same period, transfers from other levels of government decreased from €564 per resident in 1990 to €438 per resident in 1994, a 22 percent decrease. 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 revenues from local taxes (Table 1, panel A).

The implementation of the LPT had one peculiarity that is important to discuss. The main component of the tax base was not the market value of real estate, but the so-called cadastral value. The Cadastre is an Italian institution that dates back to at least the Middle Ages.⁵ 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 owner. 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. The location component is the average real estate value

⁴ The 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 first residence. The analysis presents a robustness checks in which the sample ends before these changes.

⁵ [Akerberg and Botticini \(2002\)](#), for example, use data from the Florentine Cadastre in 1427.

within a cadastral zone, which should represent a homogeneous property market. In practice, cadastral zones are often too wide, pooling together areas with very different characteristics. The cadastral value 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 further discuss the determinants of cadastral values in Section 5 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 goods 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 (standard deviations are €125 and €158, respectively; 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).

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 fees for local public goods, considering that local taxes and services became the major source of revenues.

4 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 is particularly useful to describe what type of services municipalities provided to their residents (Table A1, panel A). The average city spent 40 percent of its budget for administrative services, such as vital records and the electoral office, and just for running the local government. It spent 19 percent for public health, which includes sanitation, waste and 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 (bussing 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.⁶ 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 public goods 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 fiscal autonomy contributed to these trends.

Third, we leverage confidential administrative data provided by the Italian Social Security

⁶ Other 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).

Institute (INPS). This dataset consists of annual matched employer-employee records for all private-sector, non-agricultural, firms with at least one salaried employee. These observations are available between xx and xx. We use this dataset to analyze more precisely the effect of fiscal federalism on employed workers. Moreover, the availability of worker-level information allows us to perform heterogeneity analysis, for example by estimating the effect of the LPT on workers of different age, living in different areas, or working in different industries.

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 first at the Gustav line and then at the Gothic line. 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. In Section 5.1, we will further discuss how this variation can be used to isolate the effect of fiscal federalism after 1993.

5 Empirical Strategy

5.1 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 public goods and labor market outcomes changed across cities in which the share 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 in labor supply 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 and labor market outcomes.

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. 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, in fact, 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, a way to increase all cadastral values indistinctly, 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 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 average building size, geography, demography, and local economy. By being correlated with lower cadastral values, older buildings are also associated with a lower exposure to fiscal federalism 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 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 reevaluated 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 the reassessment was 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](#)). There are more specific reasons behind the negative correlation between cadastral values and the share of pre-WWII buildings. In Italy, the massive postwar reconstruction effort often involved the use of cheap materials and unskilled labor. The result is that many of these postwar buildings started requiring major renovations from the end of the 1980s ([Dragotto and Inda, 2007](#)). These renovations were often substantial enough to trigger a reevaluation of cadastral values. 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.⁷ As a consequence, it was more difficult to renovate these prewar buildings in a way that would have generated a reevaluation of their cadastral values. In short, the cadastral process is likely responsible for the negative correlation between cadastral values and age of buildings. The differences in construction between prewar and postwar buildings can further reinforce this negative association.

So far, the analysis showed that older buildings are associated with lower cadastral values and therefore lower exposure to fiscal federalism 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. As described in further details by [Bianchi and Giorelli \(2018\)](#), Italy declared war on France and Great Britain on June 10, 1940 and was first bombed by Allied air forces on the night of June 11, 1940 ([Overy and Wheatcroft, 1989](#)). Bombing in Italy can be divided into two periods: before and after the Italian armistice with the Allied forces. 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.⁸ On September 3, 1943, Italy signed the Armistice of Cassibile with the Allied forces ([McGaw Smyth, 1948](#)). The armistice, made public on September 8, 1943, had a sequence of important short-term effects. First, Italy ceased to be a direct

⁷ Buildings 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 d. lgs. 42 of 01/22/2004.

⁸ TNA CAB 65/6/50, War Cabinet conclusion, 27 April 1940. TNA AIR 20/5304, Note by C.A.S., 29 April 1940.

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 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 other similar 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.⁹ In the resulting subsample, the share of pre-WWII buildings is 4 percentage points lower in bombed locations (Table A3, panel A, column 1). As a result, the post-LPT change in revenues from local taxes is 1.4 percentage points higher. This effects is large in magnitude (0.18σ) and confirms the tight relationship between age of buildings and post-LPT exposure to fiscal federalism. In spite of these differences in the effects of 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). Variables from balance sheets are slightly less balanced (Table A3, panel C, column 1).

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 other words, the fact itself of being selected for a air attack can be a concern. 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.¹⁰ 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-

⁹ The 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.

¹⁰ Even 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).

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

5.2 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 public goods 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 from 1993, when the LPT was introduced. Then, this specification 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). They control for permanent differences across bombed areas. 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.

When 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.

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 introduction of fiscal federalism. Section 5.3 estimates 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 public goods 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 5.4 describes an alternative specification that relies exclusively on non-bombed cities near a bombed location. Section 5.5 discusses possible endogenous responses to the LPT by local administrators.

5.3 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 federalism. Out of 46 additional variables observed in 1991, however, only 2 are statistically different at the 5 percent level (Table A3, panel B and C, column 2). Some coefficients are insignificant, but substantial 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.¹¹ 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 the 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

¹¹The matching algorithm relies on nearest neighbor matching. The observable characteristics used are simply population, area size, and region fixed effects.

saw the implementation of an experimental one-time property tax, do not show significantly different trends before the introduction of the full-fledged LPT.

5.4 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 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 federalism 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. The 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). As shown for the main sample, labor market outcomes in layer 1 and layer 2 cities

followed similar linear and nonlinear trends before the implementation of the LPT (Table A5).

5.5 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 federalism. To show that this assumption is correct, we estimate equation (1) with 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 0.06 percentage points higher and an LPT rate for homeowners (applicable only to their main residence) 0.1 percentage points higher (Table A6, column 6). These effects are statistically significant, but small in magnitude. They suggest that administrators of municipalities with lower tax bases intended to increase their LPT revenues, but the restrictions on the tax rates could have been binding.

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 pick up significant differences in the exposure to fiscal federalism 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.

6 Effects of Fiscal Federalism

6.1 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 persisted throughout the period under consideration (Figure 3, panel A) and can be considered the first stage of the policy. The LPT was designed to replace euro for euro government transfers with local taxes.

What consequences on the provision of public goods should we expect to observe? If successful, fiscal federalism could increase the accountability of local politicians. In our empirical setting, accountability should increase more in municipalities that relied more extensively on local taxes after the LPT (near-bombed cities). If mayors of these municipalities do not manage resources properly, they have to fund their mistakes by raising a larger share of revenues from local sources. Similarly, fiscal federalism made the costs of local public services more salient to local residents. Tax salience should have increased more in near-bombed cities, considering that their residents were more likely to live in newer buildings and had a higher LPT liability for otherwise similar properties (higher cadastral values).

The main findings are consistent with these hypotheses. 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 public goods 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 0.11σ and 0.15σ higher in near-bombed cities after the LPT, while spending for administration and transportation was between 0.12σ and 0.14σ 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 the consequence 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 in areas that experienced

a decrease in spending. It is the case of administrative workers. Even though the share of spending financing 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 public goods. The share of spending for local services was 1.2 percentage points higher in near-bombed locations (Table 3, panel B).¹² 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 types of services that can have positive and direct effects on the local labor markets. These findings are consistent with the increased salience of the LPT. Now that residents pay directly for local public goods, administrators have a stronger incentives to raise their quantity and quality.

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 public goods for which data is available before the implementation of the LPT. In fact, 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 correlational analysis (OECD, 2012) and in experimental studies (Vuri, 2016; Paes de Barros et al., 2013). 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).”¹³ 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.¹⁴ 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. Even in France,

¹²This increase came at the expenses of the the other two main sources of spending: capital investments and debt repayments.

¹³European Commission (2016), page 1.

¹⁴ILOSTAT database, data available online at <http://api.worldbank.org/v2/en/indicator/SL.TLF.CACT.FE.ZS?downloadformat=xml>.

a country that shares many cultural traits with Italy, female labor force participation was much higher at 50 percent.

In Italy, public nurseries schools were first established in 1971 (law 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.¹⁵ 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).¹⁶

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).¹⁷ As a 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.¹⁸ 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.¹⁹ In the same municipality, private nursery schools can cost between €460 and €800 a month.²⁰

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

¹⁵For 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.

¹⁶Average prices show large regional variation ranging from €472 in the northern region of Trentino to €160 in the southern region of Calabria.

¹⁷When demand surpasses capacity, admission is usually means tested. Most municipalities rank applications based on a combination of household income and wealth (Cittadinanzattiva, 2018).

¹⁸In addition to run public nurseries, municipalities can outsource the service to private providers. These affiliated institutions apply the prices decided by the municipality for public nursery schools.

¹⁹Fee 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.

²⁰<https://www.milanolife.it/migliori-asili-nido-privati-milano/>.

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 expanded their provision of public nursery schools after the LPT. This result reinforce 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.

Third, the results suggest that the increase in attendance did not come from children who would have attended a private nursery school. From 1998 onwards, when this variable is first available, the number of pupils attending private nurseries is 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 nurseries might have allowed some lower-income households to access child care, instead of merely moving children across different types of nursery schools.

6.2 Effects on Local Labor Markets

In this section, we study whether the changes in the provision of public services had effects on the local labor markets. There are several public goods 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 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: 41 fewer individuals or 7 percent decrease from the pre-LPT mean (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 6.3, we will estimate triple-difference specifications with worker-level 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. It might be the case, for example, of waste management. Therefore, these firms could be less responsive to changes in the quality or quantity of local public goods.²¹

The results on population suggest that fiscal federalism 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

²¹In 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).

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 federalism (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 the exposure to fiscal federalism 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 5.1, 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 federalism, or a 7.8 percent higher share of revenues from local taxes, increased female employment by 65 percent, decreased economically inactive women by 30 percent, and narrowed the gender gap in employment by 48 percent.²²

6.3 Worker-Level Results

6.4 Analysis of Plausible Mechanisms

Our main analysis indicated that higher exposure to fiscal federalism is associated with expanded access to public nursery schools and higher female labor supply. As discussed in Section 2, there can be several mechanisms that induced the local administrators to increase the quantity and quality of local public goods. 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

²²Year-specific estimates confirm the sharp trend change after the LPT (Figure A4).

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 .²³ 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.²⁴

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 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. This result is not surprising because fiscal federalism did not change the responsibility to provide local services, only their sources of funding. In fact,

²³We 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/>.

²⁴Although 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.

the provision of public goods was already under the control of local administrators in the pre-LPT system.

6.5 Alternative Samples and Robustness Checks

In this section, we first use alternative specifications to address two separate concerns.

First, as discussed in Section 5.4, 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. A possible concern, for example, is the higher exposure to the Marshall Plan (Bianchi and Giorcelli, 2018). 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 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 federalism are generally within 3 percent of the baseline coefficients.

Second, as discussed in Section 5.3, 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 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 to 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). The change in female employment retains its sign and statistical significance, although the magnitude is reduced by 31 percent. 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. Fourth, we can control for other changes to fiscal federalism that took place between 1993 and 2011 (Table A12, panel E). 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 2007-2008.²⁵ 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 F). Sixth, we estimate placebo treatments by assigning near-bombed status at random (Table A12, panel G). The resulting coefficients are statistically insignificant and small in magnitude.

7 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.

In municipalities with higher exposure to fiscal decentralization, local politicians reduced waste and increased the quantity of public goods. 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. Under the new decentralized system, the increased transparency of municipal spending might have induced local politicians to raise the quantity and quality of public services. Improved public goods had consequences on labor supply, especially among women. In municipalities with higher exposure to fiscal decentralization,

²⁵We do not observe any negative consequence of the post-2008 decrease in fiscal autonomy on local services or labor outcomes. It is possible that the 2011 census is too close to this last policy change. More plausibly, it would have been too costly politically for local administrators to cut existing public goods.

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. We use worker-level data from Social Security to estimate triple and quadruple differences and isolate the effect of nursery schools from other concurrent changes in municipal services. 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 goods).

These findings can be important for policy makers designing two sets of policies: fiscal decentralizations and labor reforms to increase female employment. In both cases, there are important open questions for future work. In the case of fiscal federalism, 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. In the case of child care, our analysis has limited information on the effects of public nurseries on the enrolled children. Future work should study how pedagogical changes in nursery schools affect future education outcomes, as well as labor market performance.

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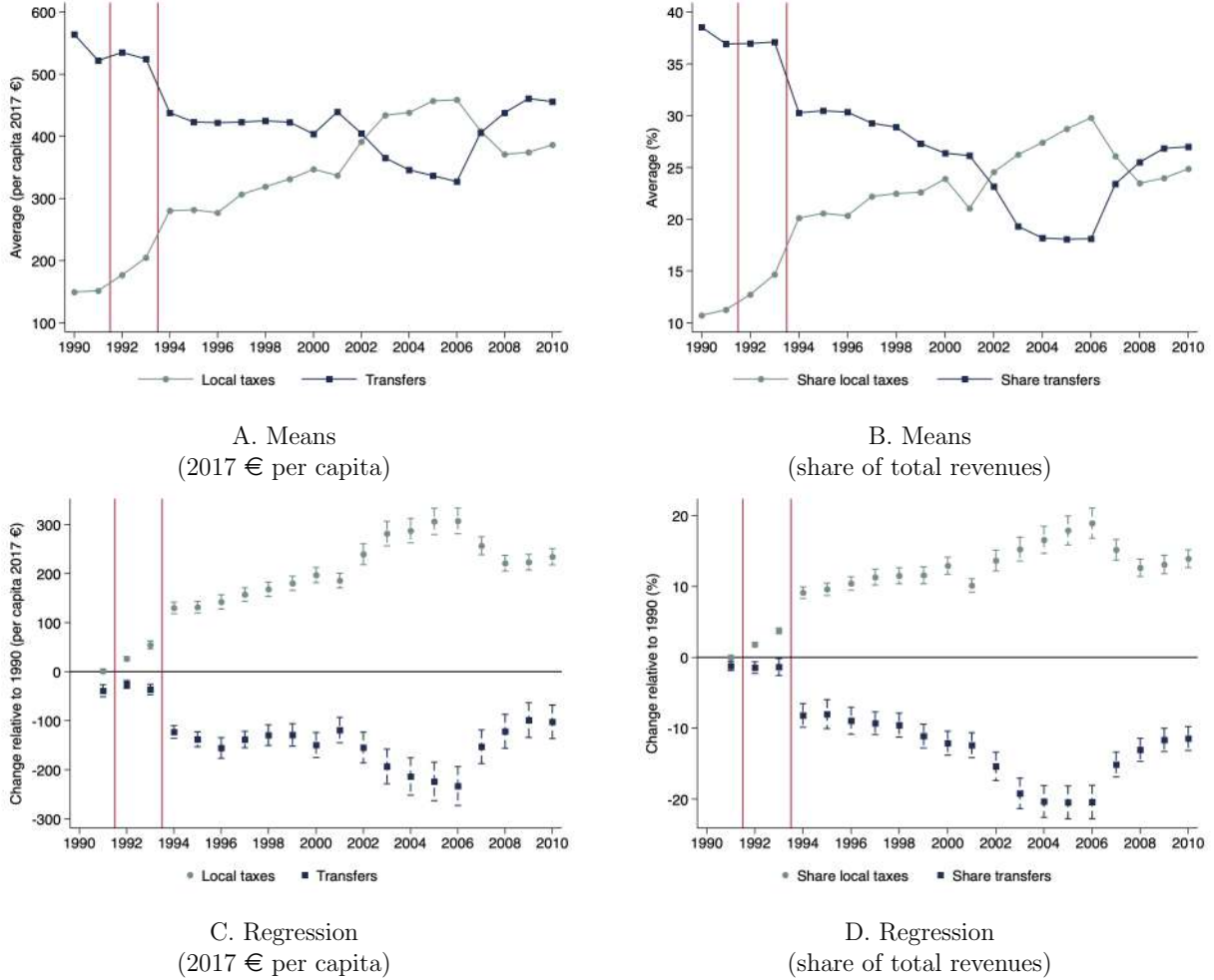
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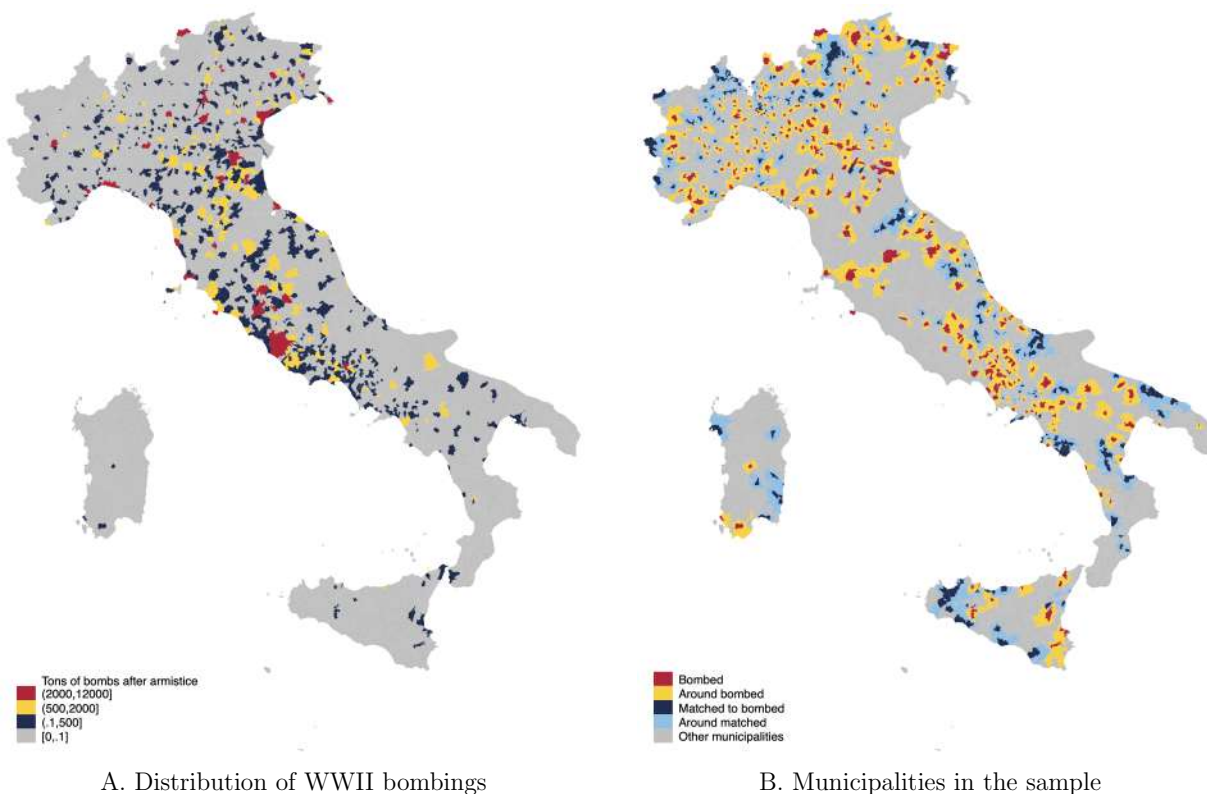
Figures and Tables

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



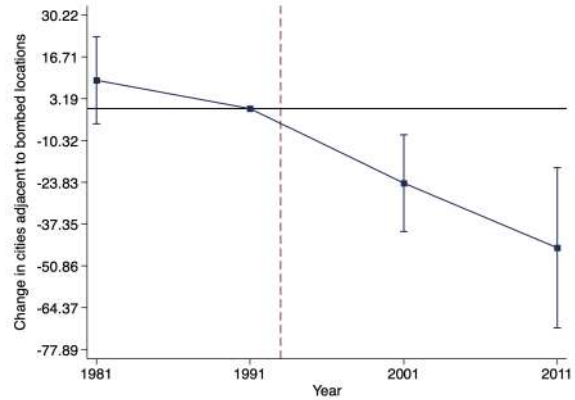
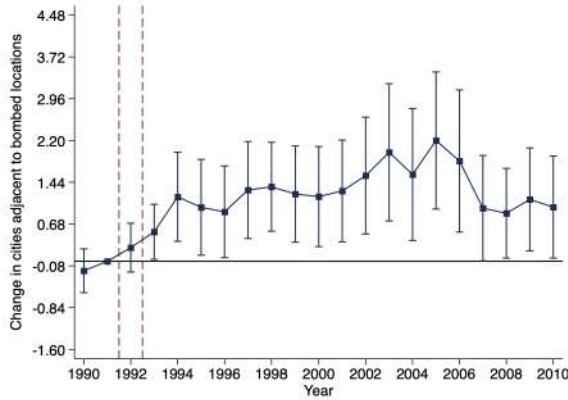
Notes: These graphs show the change in the composition of revenues of Italian municipalities. Panel A and B show the average revenues from local taxes and from transfers issued by higher levels of government (provinces, regions, central government) either as 2017 € per resident (panel A) or as a share 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

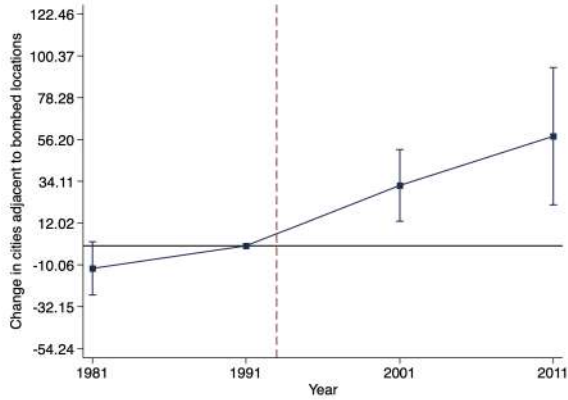


Notes: Panel A shows the distribution of Allied bombings during WWII that were executed after the Armistice of Cassibile between Italy and Allied forces (September 3, 1944). Panel B shows the municipalities in the main estimating sample. Bombed cities are matched to other non-bombed Italian municipalities 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.

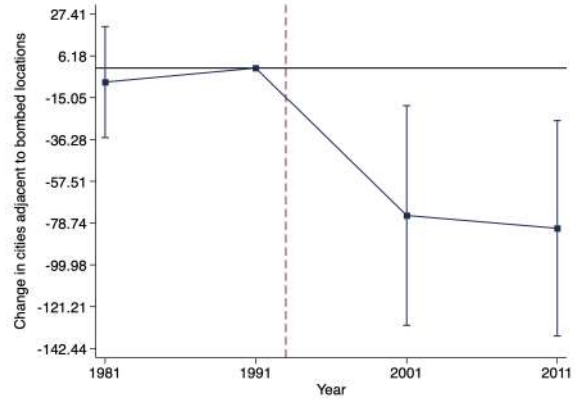
Figure 3: Yearly Effects of Fiscal Decentralization on Local Labor Markets



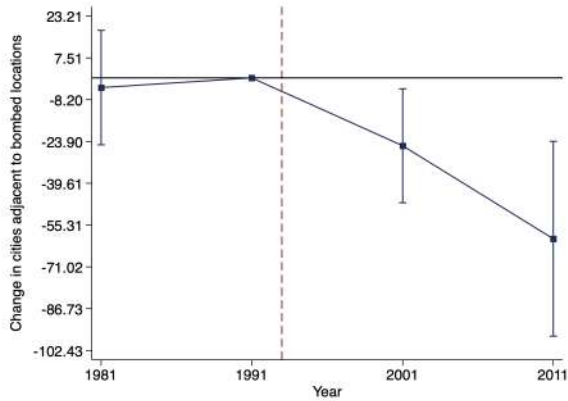
A. Share of revenues from local taxes



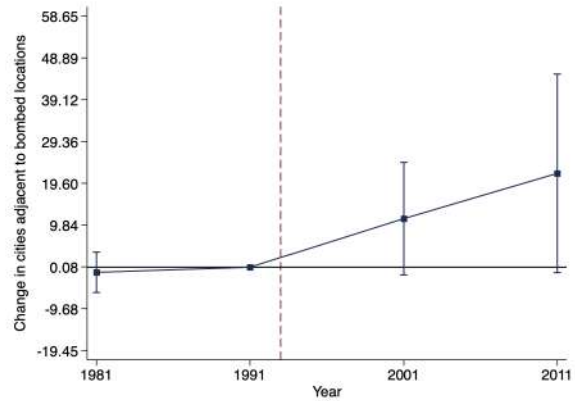
B. Gender gap in employment



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/.

Table 1: Summary Statistics

| | All years | | | | $t \leq 1992$ | $t > 1992$ |
|--|-------------|------------------|-------------|---------------------|---------------|-------------|
| | Mean (1) | Std. Dev. (2) | Obs. (3) | Availability (4) | Mean (5) | Mean (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 | 158638 | 1990-2010 | 150.57 | 354.83 |
| - Share of revenues | 21.75 | 13.33 | 157000 | 1990-2010 | 10.99 | 23.48 |
| Revenues from gov. transfers | | | | | | |
| - Per capita 2017 € | 433.68 | 270.59 | 158645 | 1990-2010 | 542.40 | 415.22 |
| - Share of revenues | 27.59 | 13.99 | 156998 | 1990-2010 | 37.74 | 25.91 |
| Revenues from local services | | | | | | |
| - Per capita 2017 € | 201.49 | 196.67 | 158638 | 1990-2010 | 151.16 | 209.35 |
| - Share of revenues | 11.62 | 7.80 | 157023 | 1990-2010 | 9.48 | 11.95 |
| Revenues from LPT | | | | | | |
| - Per capita 2017 € | 196.58 | 150.29 | 96302 | 1998-2010 | | 196.58 |
| - Share of revenues | 12.26 | 7.84 | 96360 | 1998-2010 | | 12.26 |
| Total revenues (per cap.) | 1793.65 | 1253.30 | 158640 | 1990-2010 | 1672.68 | 1815.43 |
| Total spending (per cap.) | 1808.21 | 1261.42 | 158606 | 1990-2010 | 1670.90 | 1833.36 |
| <u>Panel C: Census data</u> | | | | | | |
| Population | 7058.49 | 42942.79 | 40349 | 1981-2011 | 6924.17 | 7259.18 |
| Employed | 2564.43 | 15378.52 | 32318 | 1981-2011 | 2408.40 | 2720.10 |
| Econ. active pop. | 2728.20 | 16644.11 | 40349 | 1981-2011 | 2497.16 | 3073.40 |
| Econ. inactive pop. | 3443.77 | 21303.60 | 40349 | 1981-2011 | 3669.12 | 3107.07 |
| Employed—women | 955.80 | 6224.97 | 32314 | 1981-2011 | 812.77 | 1098.46 |
| Econ. active pop.—women | 990.47 | 6497.90 | 40345 | 1981-2011 | 796.97 | 1279.52 |
| Econ. inactive pop.—women | 2211.91 | 13878.77 | 40345 | 1981-2011 | 2393.06 | 1941.32 |
| Firms | 435.30 | 2814.53 | 40440 | 1981-2011 | 362.16 | 544.96 |
| Firms with < 2 employees | 342.95 | 2306.57 | 40440 | 1981-2011 | 257.45 | 471.12 |
| Firms with ≥ 200 employees | 0.21 | 3.16 | 40440 | 1981-2011 | 0.14 | 0.32 |
| Agricultural workers | 10.82 | 51.03 | 40440 | 1981-2011 | 11.35 | 10.02 |
| Manufacturing workers | 615.13 | 3513.21 | 40440 | 1981-2011 | 665.32 | 539.90 |
| <u>Panel D: Social Security data</u> | | | | | | |

Notes: This table shows summary statistics for the main variables used in the empirical analysis. 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. Monetary values are expressed in 2017 €. 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. 33

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 | 7,077 |
| R ² | 0.862 | 0.862 | 0.675 | 0.675 | 0.744 | 0.744 | 0.748 | 0.748 |
| Dep. var.—mean | 11.56 | 11.56 | 36.67 | 36.67 | 1677.24 | 1677.24 | 1674.14 | 1674.14 |
| Dep. var.—std. dev. | 6.61 | 6.61 | 11.99 | 11.99 | 1197.95 | 1197.95 | 1203.15 | 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 | 4,832 |
| R ² | 0.997 | 0.997 | 0.997 | 0.997 | 0.993 | 0.993 | 0.997 | 0.997 |
| Dep. var.—mean | 1334.9 | 1334.9 | 2258.64 | 2258.64 | 443.87 | 443.87 | 1402.7 | 1402.7 |
| Dep. var.—std. dev. | 2064.88 | 2064.88 | 3641.75 | 3641.75 | 718.84 | 718.84 | 2291.54 | 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 | 4,848 |
| R ² | 0.997 | 0.997 | 0.990 | 0.990 | 0.988 | 0.988 | 0.896 | 0.896 |
| Dep. var.—mean | 3843.27 | 3843.27 | 224.48 | 224.48 | 162.17 | 162.17 | 0.04 | 0.04 |
| Dep. var.—std. dev. | 6110.78 | 6110.78 | 324.62 | 324.62 | 227.18 | 227.18 | 0.27 | 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) | | |
| Panel A: Dependent variables are available before and after LPT | | | | | | | | |
| 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 |
| Panel B: Dependent variables are available only after LPT | | | | | | | | |
| 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) | | |
| <u>Panel A: Population and industrial census</u> | | | | | | | | |
| 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 |
| <u>Panel B: Social Security data</u> | | | | | | | | |

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

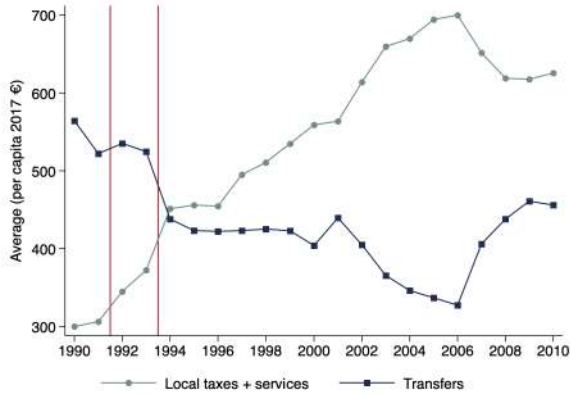
| | Panel A: Public services | | | | | | | | | |
|--------------------------------------|------------------------------|----------|---------------------------------|-----------|--------------------------|----------|---------------------------|----------|--------------------------|----------|
| | Has 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* | -0.137* | -0.401 | 0.081 | 2.114 | 1.965 | 0.056 | -2.392 | -2.091 | -2.167 |
| | (0.085) | (0.083) | (2.313) | (2.254) | (1.597) | (1.553) | (4.944) | (5.228) | (1.740) | (1.834) |
| Near bombed x Post x Close race | -0.128** | | 1.058 | | 2.539*** | | 1.404 | | 1.614* | |
| | (0.056) | | (1.854) | | (0.902) | | (1.697) | | (0.845) | |
| Near bombed x Post x Runoff | | 0.002 | | 0.003 | | 0.528 | | 3.658* | | 2.196** |
| | | (0.022) | | (0.510) | | (0.333) | | (1.928) | | (0.871) |
| Near bombed x Post x Adjacent cities | -0.006 | -0.007 | -0.155 | -0.129 | -0.183 | -0.170 | 1.122 | 0.867 | 0.420 | 0.369 |
| | (0.006) | (0.005) | (0.146) | (0.138) | (0.116) | (0.110) | (0.712) | (0.724) | (0.261) | (0.274) |
| Near bombed x Post x Below €15,000 | 0.002** | 0.002** | 0.019 | 0.012 | -0.006 | -0.006 | -0.079 | -0.034 | 0.006 | 0.010 |
| | (0.001) | (0.001) | (0.029) | (0.028) | (0.020) | (0.019) | (0.050) | (0.050) | (0.021) | (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 | -11.228 | -144.611 | -70.888 | -115.331 | -73.919 | -128.191 | -93.584 | -9.718 | -36.627 |
| | (82.667) | (83.216) | (174.163) | (145.529) | (107.338) | (96.840) | (92.723) | (82.991) | (50.868) | (40.059) |
| Near bombed x Post x Close race | 69.985** | | -116.835* | | -67.242* | | -46.977 | | 51.161** | |
| | (30.535) | | (65.719) | | (37.048) | | (28.586) | | (21.188) | |
| Near bombed x Post x Runoff | | 11.624 | | -104.246 | | -53.609 | | -74.735* | | 10.266 |
| | | (25.531) | | (103.262) | | (51.549) | | (39.849) | | (18.374) |
| Near bombed x Post x Adjacent cities | 16.215 | 11.074 | -10.740 | 3.286 | -5.683 | 2.270 | -7.761 | -0.354 | 9.381* | 5.683 |
| | (10.046) | (9.976) | (20.947) | (21.192) | (13.526) | (13.418) | (10.724) | (10.972) | (5.247) | (4.679) |
| Near bombed x Post x Below €15,000 | -1.591* | -0.570 | 2.280 | 0.270 | 1.819 | 0.674 | 2.144** | 1.187 | -0.624 | 0.059 |
| | (0.957) | (0.948) | (1.835) | (1.442) | (1.147) | (0.925) | (1.066) | (0.902) | (0.573) | (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, close to the bottom decile of the margin distribution. When the dependent variables come from decennial censuses, “Close race” is 1 if the average victory margin after 1993 is within 10 percentage points. “Runoff” is a dummy equal to 1 when the closest election had a runoff. When the dependent variables come from decennial censuses, “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 public goods. “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/.

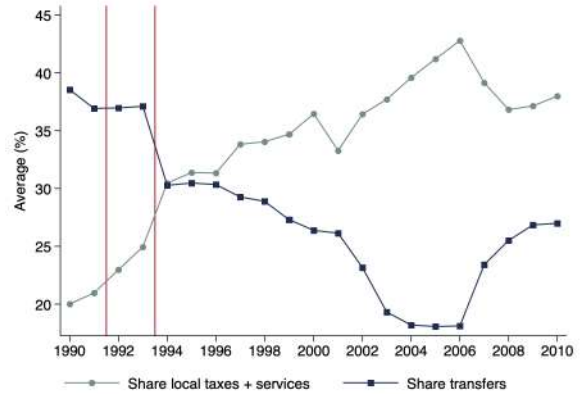
Online Appendix - Not For Publication

A Additional Figures and Tables

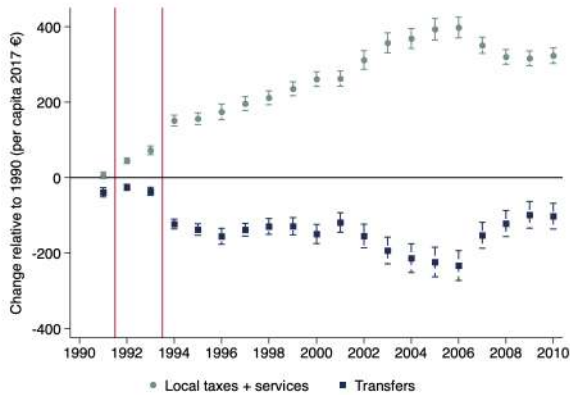
Figure A1: Share of Revenues from Local Taxes and Services



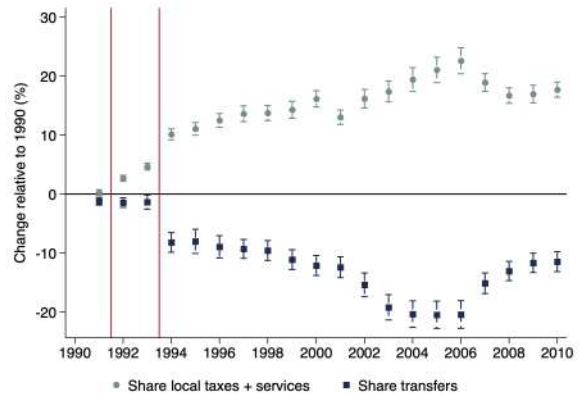
A. Revenues from local taxes and services (2017 € per capita)



B. Revenues from local taxes and services (share of total revenues)



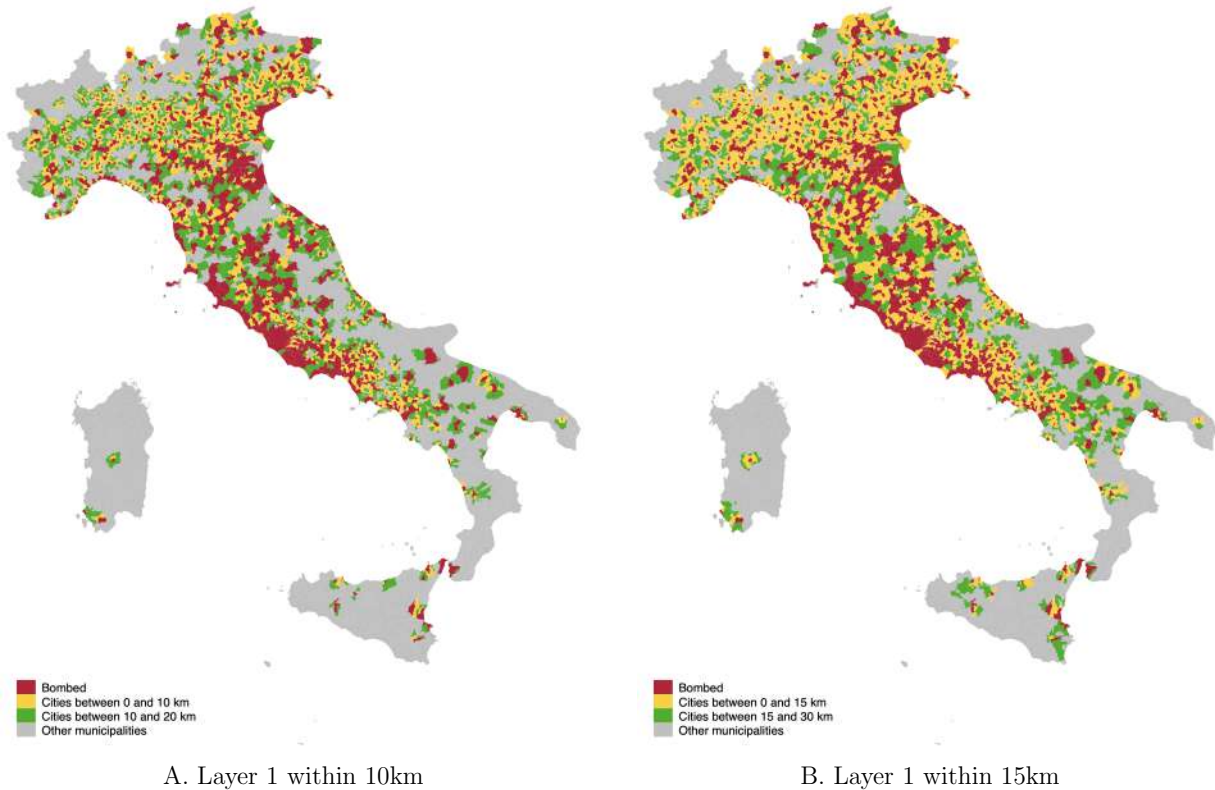
C. Revenues from local taxes and services (2017 € per capita)



D. Revenues from local taxes and services (share of total revenues)

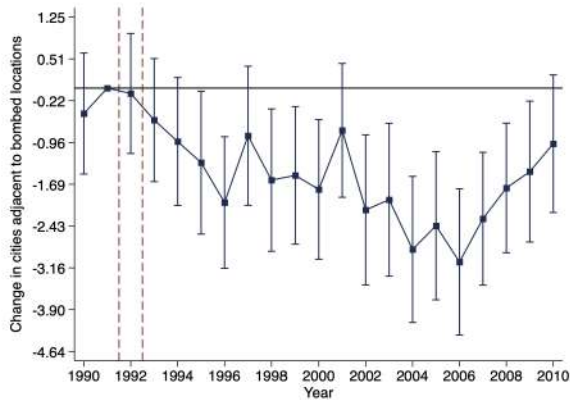
Notes: Panel A and B show the average revenues from local taxes and services and from transfers issued by higher levels of government (provinces, regions, central government) either as 2017 € per resident (panel A) or as a share of total revenues (panel B). Panel C and D show changes in the same variables 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

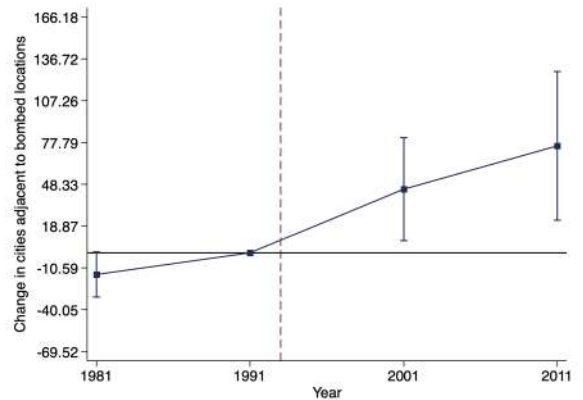


Notes: Panel A shows the municipalities hit by Allied tactical air attacks after the Armistice of Cassibile (red), cities within 10km of them (yellow), and cities between 10 and 20km (green). Panel B shows the municipalities hit by Allied tactical air attacks after the Armistice of Cassibile (red), cities 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.

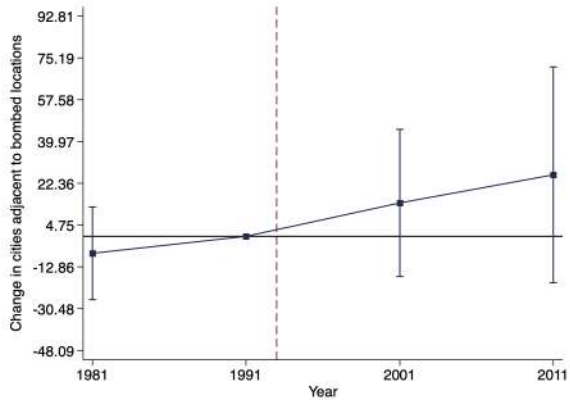
Figure A3: Effects of Fiscal Decentralization on Labor Markets, More Outcomes



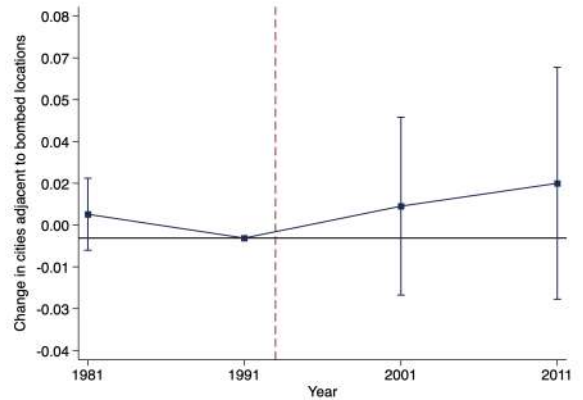
A. Share of revenues from gov. transfers



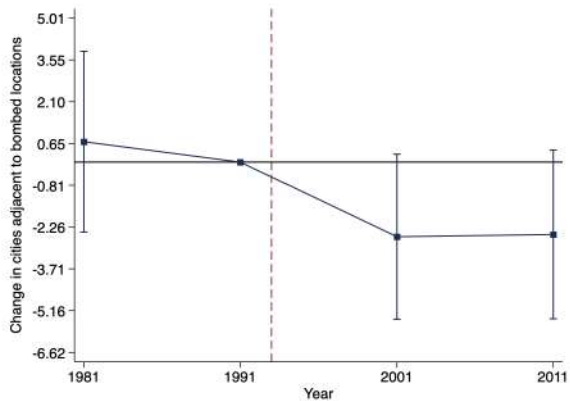
B. Econ. active pop.—women



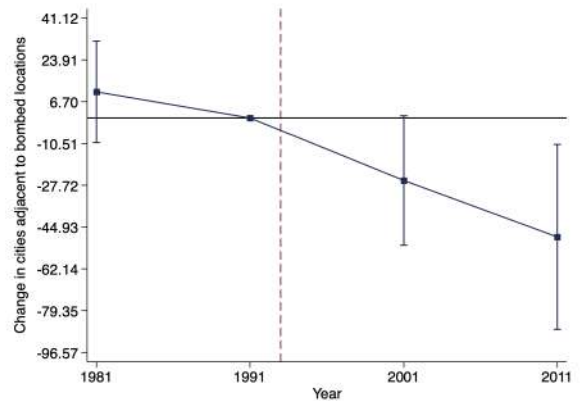
C. Econ. active pop.—men



D. Firms with ≥ 200 employees



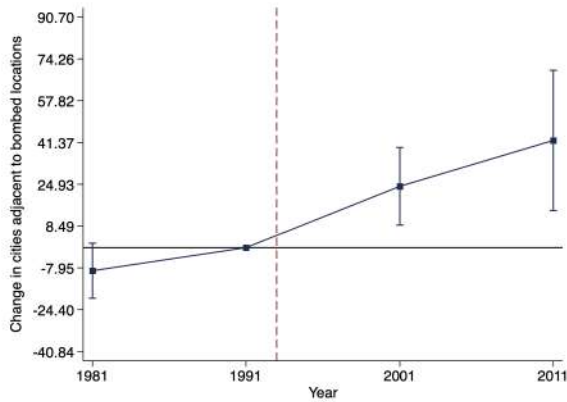
E. Agricultural workers



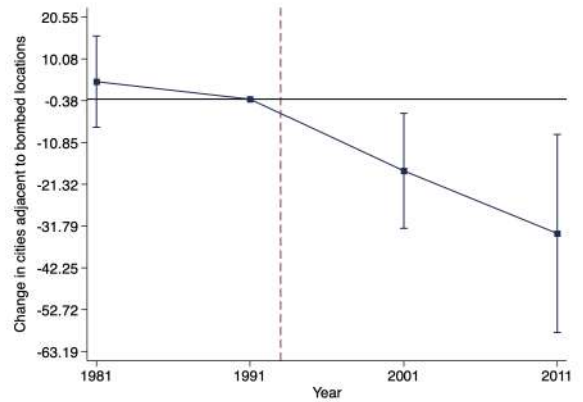
F. Manufacturing workers

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/.

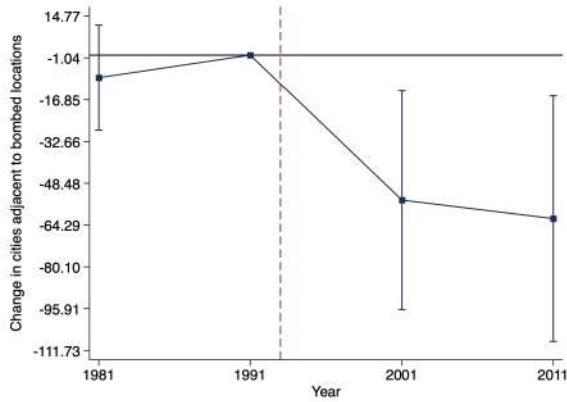
Figure A4: Yearly Effects of Fiscal Decentralization, Instrumental Variables



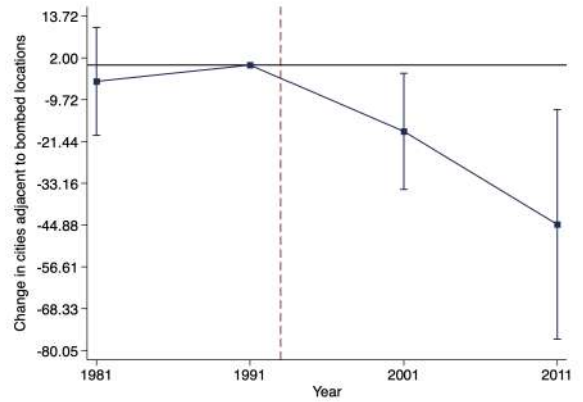
A. Employed—women



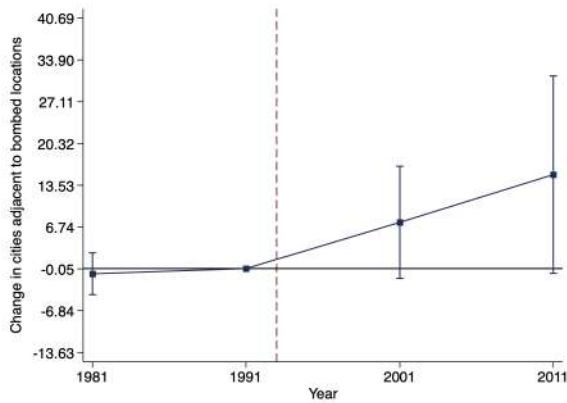
B. Gender gap in employment



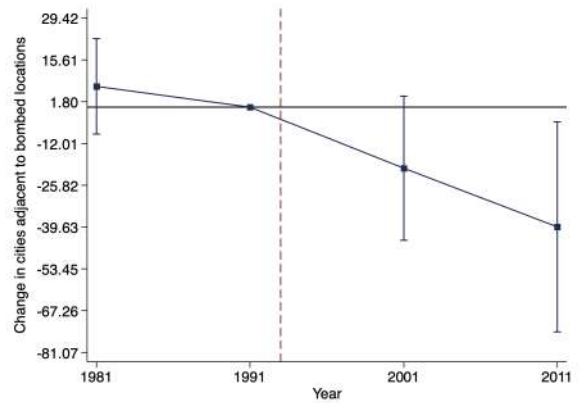
C. Econ. inactive pop.—women



D. Stay-at-home women



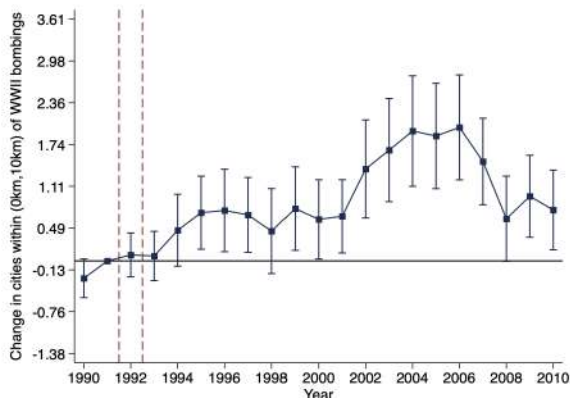
E. Firms with < 2 employees



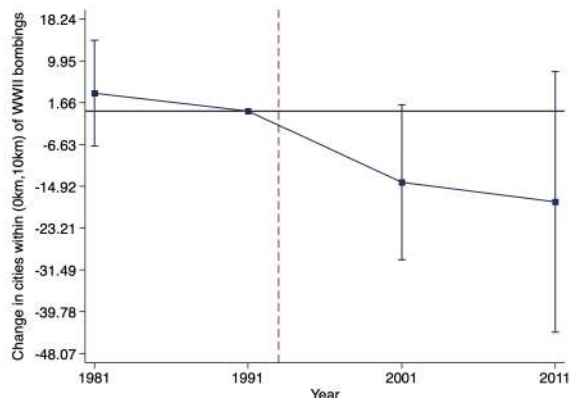
F. Manufacturing workers

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/.

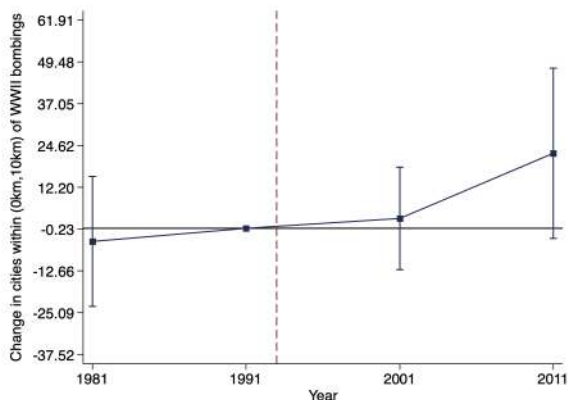
Figure A5: Effects of Fiscal Decentralization on Labor Markets, Alternative Sample



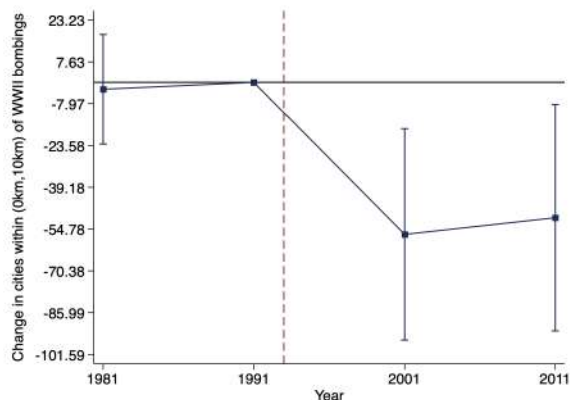
A. Share of revenues from local taxes



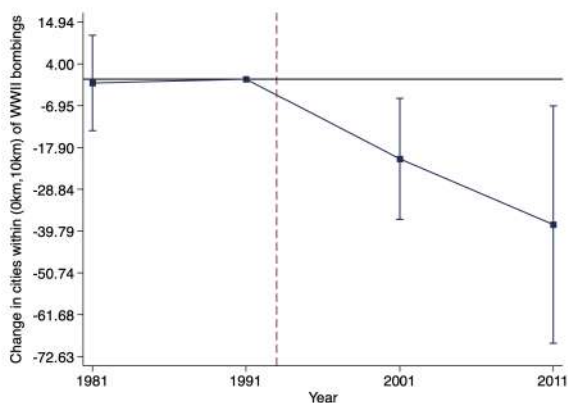
B. Gender gap in employment



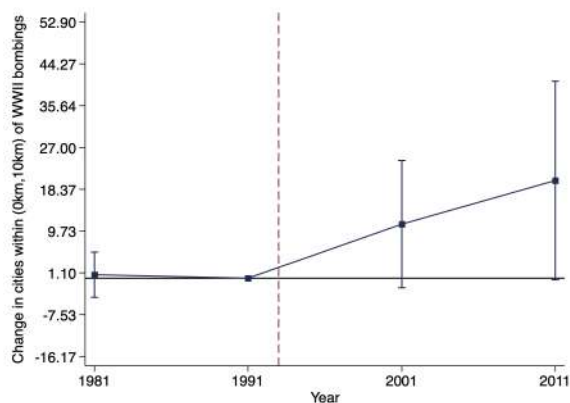
C. Employed—women



D. Econ. inactive pop.—women



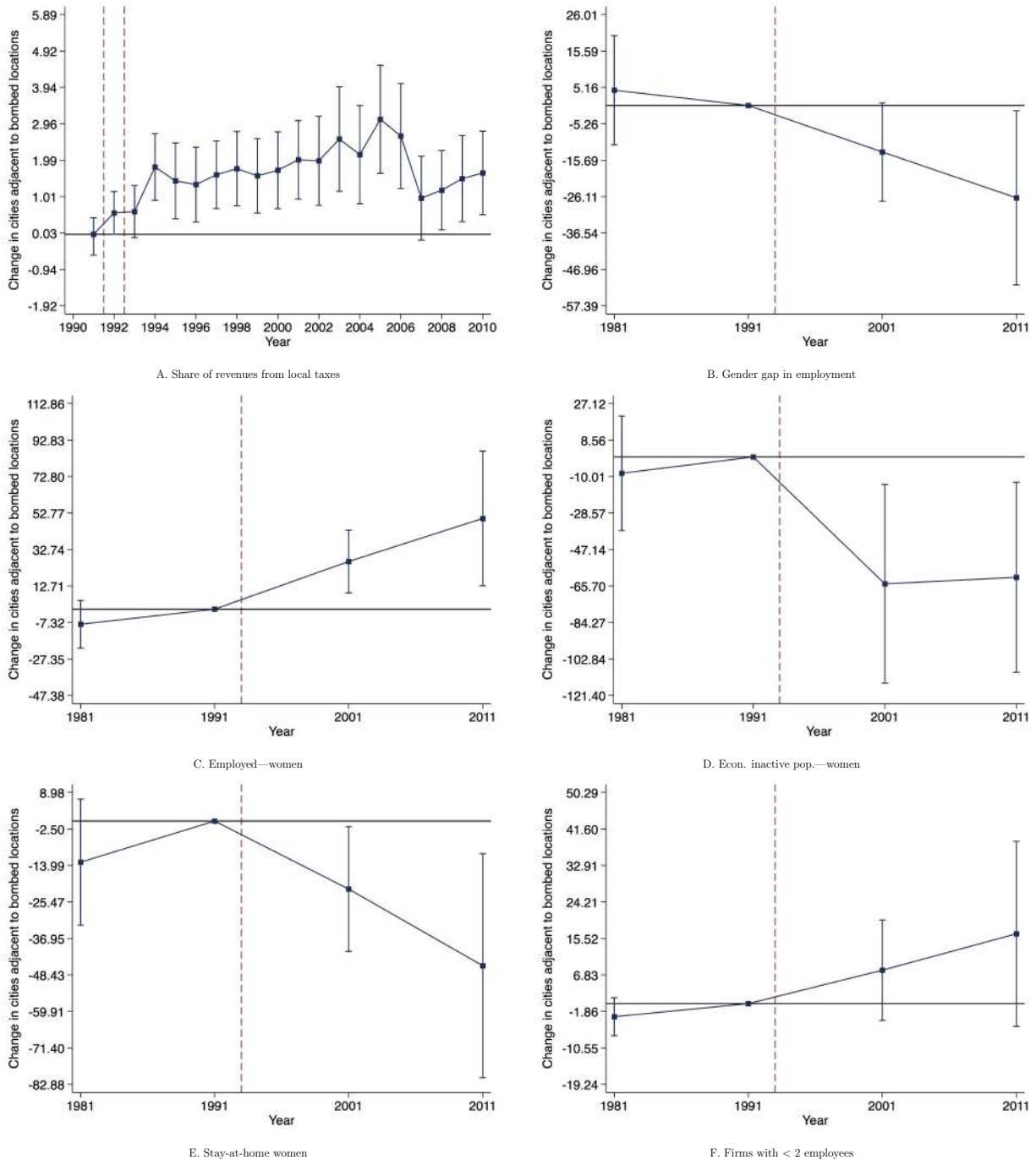
E. Stay-at-home women



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/.

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 |
| Panel C: Social Security data | | | | | | |

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/. Panel C 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 A2: Correlation between Age of Buildings and Effect of the Policy

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|----------------------|----------------------|------------------------|----------------------|----------------------|----------------------|
| 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) | -1.786*** (0.214) | -0.963*** (0.196) | -0.654*** (0.174) |
| Controls | Region FE | Province FE | (2) + building size | (3) + geography | (4) + demography | (5) + economy |
| Observations | 7,990 | 7,990 | 7,990 | 7,990 | 7,987 | 7,987 |
| R^2 | 0.412 | 0.555 | 0.604 | 0.675 | 0.707 | 0.760 |
| Dep. var.—mean | 351.2 | 351.2 | 351.2 | 351.2 | 351.2 | 351.2 |
| Dep. var.—std. dev. | 149.95 | 149.95 | 149.95 | 149.95 | 149.98 | 149.98 |
| Pre-WWII buildings—mean | 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 |
| 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) | -0.102*** (0.012) | -0.065*** (0.011) | -0.055*** (0.010) |
| Controls | Region FE | Province FE | (2) + building size | (3) + geography | (4) + demography | (5) + economy |
| Observations | 7,185 | 7,185 | 7,179 | 7,179 | 7,176 | 7,176 |
| R^2 | 0.212 | 0.260 | 0.263 | 0.298 | 0.315 | 0.328 |
| Dep. var.—mean | 9.19 | 9.19 | 9.19 | 9.19 | 9.19 | 9.19 |
| Dep. var.—std. dev. | 7.94 | 7.94 | 7.93 | 7.93 | 7.94 | 7.94 |
| Pre-WWII buildings—mean | 39.97 | 39.97 | 39.99 | 39.99 | 39.99 | 39.99 |
| Pre-WWII buildings—std. dev. | 19.04 | 19.04 | 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) | 0.013 (0.010) | 0.026*** (0.010) | -0.015 (0.009) | -0.006 (0.009) |
| Controls | Year FE | (1) + region FE | (2) + building size | (3) + geography | (4) + demography | (5) + economy |
| Observations | 935 | 935 | 935 | 935 | 935 | 935 |
| R^2 | 0.128 | 0.421 | 0.477 | 0.553 | 0.620 | 0.641 |
| Dep. var.—mean | 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 |
| Pre-WWII buildings—mean | 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 |

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. 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 (1) | Near bombed vs. Near others (2) | Matched adjacent (3) | Mean (4) | Std. dev. (5) | | Bombed vs. all (1) | Near bombed vs. Near others (2) | Matched adjacent (3) | Mean (4) | Std. dev. (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 | | | | | | | | |
|-------------------------------|-----|-----|-----|-----|-----|-----|-----|-----|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Near bombed x Trend | | | | | | | | |
| Near bombed x 1991 | | | | | | | | |
| Observations | | | | | | | | |
| R^2 | | | | | | | | |
| Dep. var.—mean | | | | | | | | |
| Dep. var.—std. dev. | | | | | | | | |

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 xxx. 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 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 | 10,857 |
| R ² | 0.857 | 0.857 | 0.685 | 0.686 | 0.744 | 0.744 | 0.748 | 0.748 |
| Dep. var.—mean | 12.99 | 12.56 | 36.67 | 36.65 | 1509.77 | 1562.61 | 1507.49 | 1558.83 |
| Dep. var.—std. dev. | 6.64 | 6.65 | 11.53 | 11.59 | 1022.66 | 1077.38 | 1024.64 | 1079.99 |
| 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 | 7,380 |
| R ² | 0.997 | 0.997 | 0.996 | 0.996 | 0.993 | 0.993 | 0.996 | 0.996 |
| Dep. var.—mean | 1692.98 | 1622.53 | 2645.05 | 2580.31 | 573.03 | 545.9 | 1648.28 | 1606.35 |
| Dep. var.—std. dev. | 2481.31 | 2387.62 | 4006.84 | 3910.96 | 889.69 | 850.27 | 2516.45 | 2455.6 |

| | 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 | 7,400 |
| R ² | 0.997 | 0.997 | 0.991 | 0.991 | 0.987 | 0.987 | 0.907 | 0.907 |
| Dep. var.—mean | 4598.41 | 4464.11 | 277.85 | 268.29 | 194.17 | 189.16 | 0.07 | 0.07 |
| Dep. var.—std. dev. | 6902.07 | 6708.05 | 391.92 | 383.14 | 263.55 | 261 | 0.38 | 0.37 |

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 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) | | |
| 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 public goods, 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) |
| <u>Panel A: Population and industrial census</u> | | | | | | | | |
| 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 |
| <u>Panel B: Social Security data</u> | | | | | | | | |

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 | Obs. | R^2 | F | Δ Local taxes | Obs. | R^2 | F | | | | |
| | x Post | | | stat. | x Post | | | stat. | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| <u>Panel A: Population and industrial census</u> | | | | | | | | | | | | |
| 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 |
| <u>Panel B: Social Security data</u> | | | | | | | | | | | | |

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 |
| <u>Panel C: Social Security data</u> | | | | | | | | | | |

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) | | |
| <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 |
| <u>Panel C: Social Security data</u> | | | | | | | | |

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: 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 F: 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 G: 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 |
| Dep. var.—mean | 11.53 | 447.46 | 443.82 | 480.92 | 1403.04 | 10.43 |
| Dep. var.—std. dev. | 6.61 | 713.57 | 718.55 | 772.7 | 2290.9 | 21.23 |

Notes: In panel A, standard errors are clustered at the province level, not at the bombed city level. In panel B, regressions include population as a control. In panel C, the dependent variables are expressed as shares of local residents or share of population below 3 years old in case of pupils in nursery schools. 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, the sample ends in 2001 before the introduction of the municipal surcharge on the personal income tax (2002) and the abolition of LPT on primary residences. Panel F excludes from the sample five special administrative regions (Valle d'Aosta, Trentino, Friuli, Sicilia, and Sardegna) that enjoy more autonomy. Panel G shows results of placebo tests in which the treatment variable “Near bombed” is assigned at random. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.