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The Effects of "ICI" on Publicly Provided Services and Labor Markets

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The Effects of "ICI" on Publicly Provided Services and Labor Markets

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Gli Effetti dell'Introduzione dell'ICI sull'Offerta di Servizi Pubblici e sui Mercati del Lavoro*

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Abstract

Questo articolo studia gli effetti dell'introduzione dell'*Imposta comunale sugli immobili* (ICI) sull'offerta di servizi locali. Introdotta nel 1993, l'ICI ha aumentato l'autonomia fiscale dei comuni italiani, sostituendo i trasferimenti governativi con una tassa di proprietà locale. La nostra strategia di identificazione usa variazioni tra comuni nel livello di decentramento attuato, che deriva dagli effetti dei bombardamenti durante la Seconda Guerra Mondiale. L'ICI, da un lato, ha ridotto le spese locali, e dall'altro ha causato una crescita della spesa per i servizi municipali, come per esempio gli asili nido. Nonostante la maggior parte dei bombardamenti su cui ci focalizziamo sia avvenuta dopo il 1943 e abbia colpito soprattutto il Nord e il Centro del paese, i risultati rimangono validi all'interno di ciascuna macroarea. Gli effetti sono maggiori nei comuni con una più forte competizione politica. L'articolo studia anche come la riforma fiscale abbia influenzato il mercato del lavoro. L'ICI ha aumentato l'offerta di lavoro femminile – probabilmente grazie alla maggiore disponibilità di asili nido – conseguentemente riducendo la differenza in occupazione tra uomini e donne.

Keywords: decentramento fiscale, occupazione femminile, asili nido

The Effects of "ICI" on
Publicly Provided Services and Labor Markets[*]

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Abstract

This paper studies how the introduction of a municipality tax on real estates (ICI) in Italy has affected local services. Introduced in 1993, the ICI increased the fiscal autonomy of Italian municipalities by replacing government transfers with revenues from a local property tax. Our identification leverages cross-municipal variation in the degree of decentralization that stems from bombings during WWII.. ICI reduced local spending but expanded municipal services, such as nursery schools. While we focus on WWII bombing after 1943, that mostly hit the Northern and Central parts of the country, our results hold *within* Italian macroareas. These effects are larger in areas with greater political competition. The paper also investigates how the reform affected labor markets. Decentralization increased female labor supply—probably through expanded availability of nursery schools—thereby reducing the gender gap in employment.

1 Introduction

Traditionally a feature of western European and North American countries, the fiscal autonomy of local governments is now on the rise in most developing economies due to the demand by residents of noncentral areas for more independence (Dillinger, 1994; Arzaghi and Henderson, 2005; Ahmad and Brosio, 2006). Despite its growing importance, the effects of fiscal decentralization on the provision of publicly provided services remain unclear. On the one hand, fiscal decentralization could increase the accountability of local politicians, potentially leading to reduced waste and more effective spending. It could also raise efficiency by fostering competition between local governments to attract new residents. On the other hand, decentralization could make it easier for interest groups to lobby local politicians, potentially increasing corruption and inequality. Moreover, heightened local competition could induce politicians to cut services in order to decrease taxes and attract mobile capital.

In a situation in which economic theory provides competing hypotheses, it is important to produce quantitative analyses of real-life decentralization reforms. However, any empirical analysis faces substantial challenges. First, the transfer of fiscal authority to local governments is often a gradual process. Without sharp increases in fiscal autonomy, it is hard to isolate the effect of fiscal decentralization from the effects of other 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 proper control for all confounding factors.

Moreover, while economic research has focused exclusively on the link between decentralization and government spending, fiscal decentralization can have consequences beyond its initial effects on publicly provided services. A change in the level of public services, such as welfare support, can affect labor supply and demand with far-reaching repercussions on local economies. As a result, a comprehensive analysis of fiscal decentralization requires a combination of different kinds of data, ranging from balance sheets of local governments to labor-market outcomes.

This paper provides new empirical evidence on the effects of fiscal decentralization of the provision of local public services and on the functioning of labor markets. It studies a 1993 reform that increased the fiscal autonomy of Italian municipalities by replacing central government grants with revenues from a newly established local property tax (ICI). As a result, the policy increased the reliance of municipalities on local revenue sources without automatically increasing the overall size of municipal budgets or modifying the preexisting municipal spending responsibilities.[1] Within a year of the policy implementation,

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[1] Revenues from the new local tax replaced intergovernmental transfers one to one. The set of public services that were under municipal responsibility did not change.

local revenue streams (from local taxes and service fees) increased by more than 50 percent relative to 1992 and replaced central government transfers as the major source of municipal revenues. The reform made it much easier for residents to keep local politicians accountable for mismanagement of public funds. Under the pre-ICI system, residents paid a personal income tax (PIT) to the central government, which then redistributed part of those revenues to municipalities to fund their expenses. This process made it impossible for residents to compute how much they had actually paid for their local services. Under the post-ICI system, individuals paid the ICI separately from their PIT and directly to their municipality, creating a direct link between taxes paid and local services.

The first contribution of this paper is to propose an identification strategy that leverages both longitudinal and cross-sectional changes to isolate the effect of the policy on Italian municipalities. Our analysis starts off by showing that older buildings have a lower ICI liability, a fact that stems directly from the formula for computing the ICI bill. As a result, all else equal, municipalities with older properties raised lower revenues from the ICI and had to rely more heavily on central government transfers even after 1993. Then, our analysis uses data on Allied bombings during WWII as a plausibly exogenous shock to the average age of buildings in 1993. Specifically, we use this information to identify municipalities that were not explicitly targeted by Allied air attacks but were hit by mistake due to their proximity to actual targets. Relative to otherwise similar nonbombed locations, these municipalities had newer buildings in 1993 that enabled them to raise more revenues from the ICI and, thus, to have a larger share of their revenues tied to local sources. In other words, they experienced a larger degree of fiscal decentralization after the reform. Our analysis, then, measures the differences in several outcomes between bombed and nonbombed municipalities, as a result of the introduction of the ICI. While we focus on WWII bombing after 1943, that mostly hit the Northern and Central parts of the country, our results hold *within* Italian macroareas.

The second contribution of this paper is to track the effect of fiscal decentralization on both the provision of public services and labor-market outcomes. The paper uses data from yearly municipal balance sheets, decennial population and industry censuses, as well as extensive Social Security databases. Initially, we find that municipalities responded to the ICI in two main ways. First, local administrators shrank the size of the government, reducing both spending and revenues. They also rebalanced their spending in favor of revenue-generating and customer-facing services, reducing the budget for their internal administrative processes. These changes were not mandated by the reform and should be considered endogenous responses.[2] Despite the lower budget, there is no evidence of a reduction in the quality of

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[2] Dye and McGuire (1997) and Gadenne (2017) find similar effects stemming from caps to local property taxes and an increase in tax revenues, respectively.

public services. Overall, these findings suggest that decentralization induced local politicians to cut waste and increase efficiency. Second, access to many local services increased. Here, we further analyze the municipal provision of nursery schools, given how important it can be for female labor supply. We find that municipalities that raised more revenues through the ICI dedicated a larger share of their budget to nursery schools (+18 percent) and had more public nursery schools (+20 percent). In the same cities, the number of pupils in nursery schools increased by an additional 24 percent after the reform.

Next, we document the fact that municipalities that raised more revenues through the ICI experienced a larger increase in female participation in the labor market. In these locations, female employment increased by 14 percent, while the number of economically inactive women decreased by 7 percent. The overall result was an 8 percent reduction in the preexisting gender gap in employment. Although these findings are highly suggestive of the impact of nursery schools on female labor supply, they cannot by themselves isolate the role of a specific public service.

We therefore dig deeper within municipalities using Social Security data. Specifically, we estimate triple interactions in which we compare women in different age groups, across treated and control locations, before and after the ICI. The hypothesis is that most local public services might benefit women in all age groups equally, while subsidized nursery schools should be more helpful to younger working women. The Social Security data allows us to establish three main results. First, these specifications confirm that labor supply increased the most among women under 35. Second, the data suggest that fiscal decentralization lowered the reservation wage of younger women. This result is consistent with the idea that expanded availability of nursery schools decreased the opportunity cost of working for mothers. Third, the availability of yearly observations between 1987 and 2011 allows us to study the dynamics of labor supply. The entry and reentry of women into labor markets started increasing in 1995. This two-year lag relative to the introduction of the ICI is to be expected if the increase in labor supply was indeed a response to improvements in local services. In fact, an expansion of public nursery schools would have required a few years to prepare new infrastructure.

We then use these results to compute the elasticities of local services and labor supply with respect to fiscal decentralization. Relative to control locations, the share of municipal revenues from local taxes increased by 15 percent from the pre-ICI baseline. This implies that a 1-percent increase in the revenue share of local taxes increased the number of nursery schools by 0.6 percent, the number of pupils enrolled in nursery school by 1.6 percent, and the number of economically active women by 0.9 percent. This last effect size is remarkably close to the findings of prior papers on subsidized childcare in Italy or in other countries with similar childcare options (for example, Baker, Gruber, and Milligan, 2008 and Carta

and Rizzica, 2018).

Finally, we analyze the motivations that might have induced administrators to expand the provision of local services after the ICI. We find two main pieces of evidence. First, higher political competition, measured by closer electoral races, is associated with more local services and a greater increase in female employment after the ICI. Second, higher exposure to fiscal decentralization is associated with higher levels of political participation. These findings are consistent with the hypothesis that the introduction of the ICI increased the accountability of local administrators by making it easier for residents to compare the amount of local taxes paid to the quantity and quality of local services received. In areas with high levels of political competition, more effective monitoring induced administrators to cut waste and increase access to many public services. Other factors, such as heightened competition between municipalities or heterogeneous preferences for publicly provided services, have a smaller and less precise explanatory power.

This paper contributes to two main strands of the literature. First, there is an extensive theoretical and empirical literature on the effects of fiscal decentralization on local politicians and public services (see Ahmad and Brosio, 2006 for a survey). There is also a much smaller literature on the relationship between fiscal institutions and labor markets (see Huther and Shah, 1998 for an overview). In most of these studies, the quantitative analysis relies on cross-country correlations.[3] This paper makes two main contributions It proposes a novel identification strategy that leverages both longitudinal and cross-sectional variation. Moreover, it tracks the effects of fiscal decentralization on both publicly provided services and labor markets within the same empirical setting.[4] The results of this comprehensive analysis indicate that local fiscal institutions can have significant effects on individual labor supply.

Second, this paper emphasizes the importance of subsidized childcare for female employment (see 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 childcare. In countries with near-universal access to affordable childcare, expansion of subsidized preschool (usually schools for children between 3 months and 6 years old) does not

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[3] As a notable exception, Hatfield and Kosec (2013) study the effect of federal competition (measured by the number of county governments in a metropolitan area) on economic growth, instrumenting the number of local governments with the length of local rivers. We complement their findings by addressing a different driver of economic development: fiscal decentralization.

[4] Among all possible forms of fiscal decentralization, this paper focuses on a reform that increased the revenue responsibilities of local governments, while keeping their (already decentralized) spending responsibilities fixed. In this context, our findings are consistent with prior work highlighting that tax revenues and intergovernmental grants have different consequences for local spending (Borge and Rattsø, 2008; Gadenne, 2017; Martinez, 2018).

increase maternal employment (Havnes and Mogstad, 2011), since it mostly moves children across different types of childcare arrangements. In other countries, however, the expansion of affordable childcare can have a large and positive effect on female labor supply (Gelbach, 2002; Baker, Gruber, and Milligan, 2008; Carta and Rizzica, 2018). This paper complements these findings by showing the importance of fiscal institutions in shaping the provision of public childcare.

The rest of the paper is organized as follows. Section 2 outlines the policy change and the data. Section 3 discusses the empirical strategy. Section 4 shows the main results. Section 5 concludes.

2 Institutional Details and Data

2.1 The Introduction of the Local Property Tax

Between 1992 and 1993, two laws drastically increased the fiscal independence of Italian municipalities.[5] As an initial test, the central government established a one-time ICI 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 ICI became permanent. At this time, local governments could set their preferred tax rate between 0.4 percent and 0.7 percent. The average rate was 0.57 percent between 1993 and 2010 (Table 1, panel A).

The reform had the explicit goal of transitioning the public sector toward fiscal decentralization. In 1993, each lira earned through the ICI replaced one lira of government transfers.[6] Moreover, implementing the new ICI was not optional for local administrators. Absent a specific municipal ordinance on the new ICI, the tax rate was automatically set at 0.4 percent and the intergovernmental transfers were decreased accordingly. On average, municipal revenues from local taxes increased from €149 per resident in 1990 to €280 per resident in 1994 (Figure 1).[7] Due to the one-to-one replacement between revenues of the new local tax and intergovernmental transfers, the reform did not directly change the overall size of the municipal budgets, but only its composition. Over the same period, transfers from other levels of government decreased from €564 per resident in 1990 to €438 per resident in 1994.[8] When combined with revenues from municipal services, payments from local taxes became the main source of local revenues by 1994 (30 percent of all revenues; Figure A1). The ICI was the driving force behind this shift. From 1998 onwards, the first year in which more

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[5] *Decreto legge* 299/1991 and *decreto legislativo* 504/1992.

[6] We convert all monetary values to 2017 €.

[7] As can be seen in Figure 1, 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 for homeowners of the ICI on their main residence. We present robustness checks in which the sample ends before these changes.

[8] Total revenues decreased by only 4.6 percent.

detailed data from the municipal balance sheets are available, ICI revenues alone accounted for 57 percent of total revenues from local taxes (Table 1, panel B).

One crucial and unusual feature that affected the implementation of the ICI was the fact that the tax base was computed using the so-called *cadastral value* of real estate, not its market value. The cadastre is an Italian institution that dates back to at least the Middle Ages. In its modern form, it is a national agency under the control of the *Agenzia delle Entrate* (the Italian counterpart of the IRS in the US or HM Revenue and Customs in the UK). Its main function is to keep a register of all real estate in a municipality, recording the characteristics of each property and the identity of the owners. Within these registers, the cadastral value measures the ability of each property to generate real-estate income. It is used to compute the value of property for all fiscal purposes, such as the determination of estate-tax liability. The cadastral value is a function of size, quality, type of property, and location. It is assigned to a property only at the time of construction or after a major renovation. In short, the individual ICI liability 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 3, because they are important for identification.

It is important to note that the introduction of the ICI represented a very salient change for residents. As noted earlier, the reform changed how residents funded local services. Under the post-1993 system, individuals started paying the new ICI separately from their PIT, often twice a year (June and December), and directly to their municipality. Residents paid an average of €300 for their main residence and €335 for a second property, if any (Table 1, panel A). This average yearly ICI liability of €635 for owners of one main residence and a second property was equal to 1.9 percent of the average household disposable income in 1991 (Banca D'Italia, 1993).[9] In short, the implementation of the ICI was salient to residents due to both its amount and its separate payment method.[10]

2.2 Data

The empirical analysis leverages data on the 8,092 Italian municipalities from four main sources: municipal balance sheets, population and industry censuses, Social Security databases, and information on Allied bombings during WWII.[11]

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

[10] The 1948 Italian Constitution created a highly centralized government. Therefore, the saliency of the new ICI stemmed also from the fact that it was the first major reform in the history of the Italian republic in which revenue-generating authority was decentralized to local governments.

[11] In the rest of this manuscript, we will use both municipality and city to correspond to the Italian *città*. In other words, they will be umbrella terms that encompass cities, towns, and villages.

First, we constructed a panel dataset with yearly financial information on each municipality. Some key variables, such as total revenues and spending, are available for every year from 1990 to 2010. The balance sheets, however, become more detailed from 1998 onwards. The post-1998 data describe what types of services municipalities provided to their residents (Table A1, panel A). The average city spent 40 percent of its budget just on running the local government and on delivering administrative services, such as vital records and the electoral office. It spent 19 percent on public health, which includes sanitation, waste and water management, public housing, and city planning; 9 percent on local transportation; and 4 percent on a municipal police force.

Two other important areas of spending are education and welfare with 10 percent each.[12] In regard to education, municipalities only offer auxiliary services (i.e. busing and lunches) for local public schools, because these are managed by higher levels of government. Therefore, municipal officials have very few opportunities to improve the quality of education of local schools. In contrast, spending on welfare services can have a much larger influence on local households. Cities offer nursery schools for children between six months and three years old, as well as retirement homes, social services, and aid to residents in need. Most of these benefits are means-tested, and demand often surpasses supply. Considering the breadth of municipal interventions, it is plausible to assume that a more efficient public administration could have important effects on the local labor markets. In addition to improving amenities and overall quality of life, some publicly provided services could have direct consequences on labor supply. Expanding the provision of subsidized public nursery schools, for example, could induce more women to participate in the labor market.

Second, we linked data from both population and industry censuses from 1981 to 2011 (Table 1, panel C). The resulting dataset has two pre-ICI (1981 and 1991) and two post-ICI observations (2001 and 2011). We use these data to study how participation in the labor market changed after the introduction of the ICI. We also analyze how decentralization affected the number and type of firms operating in each city. Using means alone reveal that the number of employed individuals in each municipality increased by an average of 13 percent after the ICI. The increase for women alone, however, is much larger: their employment level increased by 35 percent over the same period.

Third, we leverage administrative data provided by the Italian Social Security Institute (INPS) covering every year between 1987 and 2011. This dataset consists of information on all employees of private-sector nonagricultural firms with at least one salaried worker. We use

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[12]Other minor areas of spending are culture (2 percent, for theaters, museums, libraries), sports (1.6 percent), activities in support of local economic development (0.5 percent), and auxiliary services for the judicial system (0.1 percent).

this dataset to study the effect of fiscal decentralization on different types of employees within a municipality. To this end, the observations are aggregated by age, gender, municipality of residence, and year.[13] In this dataset, we observe workers entering the labor market for the first time or reentering it after a break in employment.[14] For each combination of gender, age, municipality, and calendar year, the average number of new entrants is equal to 0.53 individuals and the average number of reentrants is equal to 0.54 individuals (Table 1, panel C). Moreover, the data report the details of the labor contracts. In the sample, the median annual wage is equal to €13,180 and the median number of days worked during the year is equal to 226. Conditional on gender, age, municipality, and year, the average number of individuals working outside their province of residence is 2.31.

Fourth, we exploit information on Allied bombings during WWII as a shock to cadastral values and, therefore, to the tax base used for the computation of the ICI. Data on Allied bombings in Italy come from the Theater History of Operations Reports (T.H.O.R.; available at www.afri.au.af.mil/thor) 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 (2019), we leverage the shift from strategic to tactical bombing that followed the Armistice of Cassibile, signed by Italy and Allied forces on September 3, 1943 (Table 2, panel A). In Section 3.2, we will further discuss how this variation can be used to isolate the effect of fiscal decentralization after 1993.

3 Empirical Strategy

3.1 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 ICI. Simply examining how the provision of municipal services and labor-market outcomes changed across cities in which the share of revenues from local taxes increased differently after 1993 could be problematic. In fact, municipalities in which revenues from local taxes increased more are likely to have had a larger tax base, more expensive buildings, a more developed local economy, and richer residents. Therefore, we need to find a source of variation in ICI revenues that is plausibly exogenous with respect to other drivers of public services.

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[13]This aggregation has two purposes. First, it aggregates the data at the same level of variation used by the empirical analysis in Section 4.5. Second, it shrinks the size of the initial worker-level dataset (with more than 150 million observations), allowing us to perform the analysis on the INPS servers.

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

To do so, we consider the formula for the individual ICI liability: ICI paid = cadastral value × multiplier × tax rate. Of these three components, only the cadastral values are a suitable source of exogenous variation, because they vary across geographical areas and are not under mayoral control. The other two components do not share these features. The multiplier was set by the national government and was constant across municipalities. In contrast, the tax rate was under the direct control of local administrators, even though it was restricted by law to being 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.

In the Italian context, the 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 (19 percent) decreases the average cadastral value by €69, or 20 percent from the mean (Table A2, panel A). This correlation holds after controlling for other municipal-level characteristics, such as the average building size, the average building quality, geography, demography, and local economy. Since they are correlated with lower cadastral values, older buildings are bound to make a municipality less exposed to fiscal decentralization after 1993. A 1-σ increase in the share of pre-WWII buildings, in fact, decreases the post-ICI share of revenues from local taxes by 2.5 percentage points, or 27 percent from the mean (Table A2, panel B).[15] Moreover, the age of buildings does not have the same negative relationship with the market value of real estate. We show this finding by using the median rental value per squared meter 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 a factor of 3.5 between 1950 and 2012 (real values; Cannari, D'Alessio, and Vecchi, 2016), the cadastral values of older buildings remained essentially untouched over the decades. Additionally, compared with postwar buildings, prewar constructions had a higher probability of being considered part of the Italian cultural heritage by 1993. Therefore, it was more complicated to renovate

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[15]The same correlations can be shown with scatterplots leveraging either all observations in the sample or only the municipalities used to estimate the main specifications (Figure A2).

them in a way that would trigger a reevaluation of their cadastral values.[16]

3.2 A Matching Algorithm to Build the Estimating Sample

So far, our analysis has shown that a larger share of older buildings is associated with lower cadastral values and therefore lower exposure to fiscal decentralization after 1993. A natural variation in the municipality cadastral values comes from Allied bombings during WWII. Bombing in Italy can be divided into two periods: before and after the Italian armistice with the Allied forces in 1943.[17] During the first phase of the war, the Allies relied on strategic bombing. They targeted rich and populous cities, as well as the major industrial factories, to damage war production and weaken the morale of the Italian population. On September 3, 1943, Italy signed the Armistice of Cassibile with the Allied forces. The armistice had a sequence of important short-term effects. First, the Italian army disbanded and the German military took control of the majority of the country. Second, the Allied invasion of Italy, begun in Sicily in July 1943, gained momentum and created an active warfront between the German army in the north and the Allied forces in the south. From this moment on, the Allies used tactical bombings as a tool to win the ground battles against the German troops. During this phase, the selection of targets was based on the location of the land battles, the movement of German units, and impromptu opportunities to hit the enemy (Bianchi and Giorcelli, 2019, Table 4). This is why post-armistice tactical bombings are not correlated with prewar economic conditions. Moreover, post-1943 bombings did not generate permanent differences in population growth across Italian provinces (Bianchi and Giorcelli, 2019).[18]

Based on these historical events, the empirical strategy starts from cities bombed after the armistice. Specifically, we first matched each bombed city to a counterfactual city, using a nearest-neighbor propensity-score matching and imposing a common support between treatment and control. We used the following variables included in the 1991 census: population, area, population density, number of buildings, share of homeowners, share of residents under three years old, and region fixed effects (Table A3). In the resulting subsample of 314 bombed and 314 matched municipalities, the share of pre-WWII buildings is 4 percentage points lower in bombed locations, and the post-ICI change in revenues from local taxes is 1.4 percentage points higher (Table A4, panel A, column 1). This second effect is large in magnitude ($0.18\,\sigma$) and confirms the tight relationship between age of buildings and post-ICI exposure to fiscal

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[16]Buildings that are at least fifty years old can be considered historic (*decreto legislativo* 42/2004).

[17]Bianchi and Giorcelli (2019) provides a longer historical section and more empirical evidence on this fact.

[18]This finding is consistent with prior evidence from Japan (Davis and Weinstein, 2002) and West Germany (Brakman, Garretsen, and Schramm, 2004).

decentralization. Other observable characteristics are generally balanced between bombed and matched locations (Table A4, panel B, column 1).[19]

In spite of these reassuring statistics, we intend to bypass any possible remaining concern about selection by focusing on nontargeted locations only. Specifically, we exploit the fact that WWII bombings were often not precise and could hit areas around the intended target.[20] In our analysis, we compare all 1,384 municipalities adjacent to the 314 locations bombed after the armistice (yellow in panel B of Figure 2) to all 1,058 municipalities adjacent to the 314 locations matched to bombed municipalities (light blue in panel B of Figure 2). Relative to the control, near-bombed locations have 3.6 percentage points fewer pre-WWII buildings (Table A4, panel A, column 1). They also experienced a 1.7 percentage points higher change in revenues from local taxes after the ICI. The fact that they were inadvertently hit during WWII made them experience a higher degree of fiscal decentralization after 1993, but it is unlikely that their unintended bombings are correlated with unobservable confounding factors.

3.3 Baseline Specification

To estimate the effects of fiscal decentralization on publicly provided services, we run the following difference-in-differences specification on the sample of municipalities adjacent to bombed cities (from now on, *bombing-adjacent municipalities*) and those adjacent to the matched cities (*bombing-distant municipalities*):

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

where y_{mt} is one of many variables describing the provision of municipal services, or the local labor market in municipality m and year t. The variable Near bombed$_m$ is 1 for municipalities adjacent to cities bombed by Allied air attacks after the Armistice of Cassibile. The dummy Post$_t$ is 1 after the introduction of the ICI in 1993. Municipality fixed effects (α_m) capture permanent differences across cities. Finally, region-year or province-year fixed effects (γ_{rt}) control for nonlinear changes in the outcomes within region r or province p.[21] In addition to

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[19] Out of the 34 variables from the population and industry censuses, only 4 show differences that are statistically significant at the 5 percent level. Variables from balance sheets are slightly less balanced (Table A4, panel C, column 1).

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

[21] These fixed effects control for other shocks, such as natural disasters or macroeconomic events, that may have differentially affected a region or province over time. One example of such a shock is the devaluation of the lira (Macis and Schivardi, 2016).

capturing confounding trends, these controls ensure that the regressions exploit variation in the age of buildings and, therefore, in the exposure to fiscal decentralization only between treated and control municipalities located in the same region or province, instead of across cities in different geographical areas.[22] Standard errors are clustered at the level of bombed locations.[23] Section 4.6 includes many robustness checks on the baseline specifications. For example, we can show that the main findings are robust to different matching processes between bombed and nonbombed locations.

3.4 Tests of the Identification Assumption

The main assumption behind the baseline specification is that outcomes would have followed the same path in bombing-adjacent and bombing-distant municipalities in the absence of the ICI. While this assumption cannot be tested directly, we document that all outcomes follow a parallel trends before the implementation of the ICI (Tables 2 and A5). 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 0.01 σ. Alternatively, we estimate nonlinear pre-ICI trends by interacting the treatment variable with a dummy for year 1991, as well as a dummy for year 1992 exclusively for variables extracted from the balance sheets. The omitted 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 pre-reform dummies are small in magnitude and not statistically different from zero.

To provide a more robust overview on pre-ICI trends, we repeat the same estimation using the INPS dataset, which has six yearly data points (1987-1992) before the ICI introduction. As discussed in Section 2.2, the data are aggregated at the level of gender, age, municipality, and calendar year. We therefore use the INPS dataset to test the existence of differential linear and nonlinear pre-reform trends between younger and older women and between treated and control locations (Figure A4; Table A5, panel B). For both linear and nonlinear trends, we reject the hypothesis that treated and control municipalities were trending differentially before the ICI. This result holds for all age groups.

Moreover, we show that before the introduction of the ICI, bombing-adjacent and bombing-distant municipalities were similar in terms of observable characteristics, except for the share

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[22]Figure A3 shows the geographical variation of these variables.

[23]When the dependent variable is available only after the ICI implementation, equation (1) loses the dummy Post$_t$ and the municipality fixed effects α_m. 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.

of pre-WWII buildings and the post-ICI degree of fiscal decentralization. Out of 46 additional variables observed in 1991, only two are statistically different at the 5 percent level (Table A4, panels B and C, column 2). Some coefficients are insignificant even though not precisely estimated zeros. 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 bombing-adjacent and bombing-distant municipalities are matched using propensity scores, instead of keeping all bombing-adjacent and bombing-distant municipalities.[24] In this case, only one variable is statistically different at the 5 percent level (Table A4, panels B and C, column 3). Moreover, most coefficients, including the insignificant ones, are smaller in magnitude. Second, we present robustness checks in which we control for key variables that were not perfectly balanced in 1991, such as population changes, and for nonlinear trends correlated with geographical characteristics (population density, a dummy for rural municipalities, and a dummy for coastal cities).

Our empirical strategy hinges on the assumption that variations in the ICI base, driven by the age of buildings, translated into permanent differences in the exposure to fiscal decentralization. To show that this assumption is correct, we estimate equation (1) with the share of revenues from local taxes as the dependent variable and year fixed effects in place of the $Post_t$ dummy (Figure 3, panel A). Relative to bombing-distant cities, the share of revenues from local taxes in bombing-adjacent municipalities increased 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 cities' lower cadastral values, administrators of bombing-distant cities could have increased the ICI rates to equalize revenues from local taxes across municipalities. Tax rates, however, were bounded between 0.4 percent and 0.7 percent. As expected, compared to bombing-adjacent cities, bombing-distant cities had an average ICI rate 0.006 percentage points higher and an ICI rate for main residences 0.01 percentage points higher (Table A6, column 6). These effects are statistically significant but small in magnitude.

Similarly, local administrators could have attempted to increase the ICI revenues by issuing more building permits and widening the tax base. As expected, the share of building permits issued was 1.9 percentage points higher in bombing-distant cities starting in 1998, when this variable first became available.

In short, local administrators in cities with a lower ICI base attempted to increase ICI revenues through higher tax rates and more building permits. Had they been successful, our empirical strategy would not be able to exploit significant differences in the exposure to

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[24]The matching algorithm relies on nearest-neighbor matching and imposes a common support. The observable characteristics used are simply population, area size, and region fixed effects.

fiscal decentralization across municipalities. However, a limited range for the tax rates and the impossibility of constructing too many new buildings made the initial cross-city changes in ICI revenues permanent in the period under consideration.

3.5   Alternative Specification

In this section, we propose an alternative and simpler specification that does not require any matching between Italian locations. We can compare municipalities adjacent to bombed locations (we refer to them as layer-1 cities) and municipalities adjacent to layer-1 cities (layer-2 cities), before and after the ICI. Layer-1 municipalities are closer to bombed locations and, therefore, have a lower share of pre-WWII buildings and higher exposure to fiscal decentralization after 1993. However, both layer-1 and layer-2 cities are relatively close to bombed locations and thus should have benefitted equally from postwar aid, for example Marshall Plan funds intended for the modernization of the transportation network (Bianchi and Giorcelli, 2019). These specifications are the following:

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

Most variables are unchanged from equation (1). Here, however, the treatment variable Layer 1_m is 1 for municipalities that are located within 10 km of cities bombed by the Allies after the Armistice of Cassibile (Figure A5, panel A). The control group comprises cities that are located 10 km to 20 km from bombed locations.[25] As shown for the main sample, outcomes in layer-1 and layer-2 cities had similar average characteristics just before fiscal decentralization (Table A7). Moreover, they followed similar linear and nonlinear trends before the implementation of the ICI (Table A8).

4   Effects of Fiscal Decentralization

4.1   Effects on Municipal Spending

Overall Effects   The introduction of the ICI had a direct effect on the balance sheets of Italian municipalities. Compared with bombing-distant cities, bombing-adjacent 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 is the first stage of the policy.

Our main findings suggest that fiscal decentralization led to reduced waste and increased access to local services. First, the overall size of the municipal budget decreased dispropor-

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[25]For robustness checks in which layer 1 and layer 2 have a radius of 15 km, instead of 10 km, see Figure A5, panel B.

tionately in bombing-adjacent 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 neither the public deficit nor the probability of having fiscal infractions was affected. Moreover, we observe changes in the type of spending for publicly provided services starting in 1998, when this information becomes available. In bombing-adjacent 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 on welfare, education, and police was between 0.11 σ and 0.15 σ higher in bombing-adjacent cities after the ICI, while spending on administration and transportation was between 0.12 σ and 0.14 σ lower (Table A9). Overall, these findings are consistent with increased accountability of local administrators. Lower spending and the prioritization of more lucrative services can be a sign of reduced waste and increased efficiency.

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

Third, even though total spending decreased, bombing-adjacent municipalities devoted more resources to local services. The share of spending for local services was 1.2 percentage points higher in bombing-adjacent locations (Table 3, panel B).[26] Bombing-adjacent cities were 7.4 percentage points more likely to have programs for local economic development, a 12-percent increase from the mean. Similarly, they were 5.4 percentage points more likely to have at least one public nursery school, a 9-percent difference from the mean. These are two services that can have direct positive effects on the local labor markets. These findings are consistent with the increased salience of the ICI. In cities whose residents directly funded a larger share of municipal spending, administrators had stronger incentives to raise the quantity and quality of publicly provided services.

The case of nursery schools   In the rest of this section, we focus on the provision of public nursery schools for two reasons. First, nursery school is one of the few local services for which data are available both before and after the implementation of the ICI (from the

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[26]This increase came at the expense of the other two main sources of spending: capital investments and debt repayments.

1991, 2001, and 2011 censuses). Second, and more importantly, nursery school is one of the most valuable municipal services for residents.

Increasing female labor participation is an important goal in many developed countries.[27] 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 over 15 active in the labor market was 40 percent. In comparison, female labor-force participation was equal to 52 percent among OECD countries, 51 percent in the European Union, and 56 percent in the United States.[28] Among many possible solutions, the availability of affordable nursery school has proved to be positively correlated with female labor participation in both cross-country (OECD, 2012; Vuri, 2016) and within-country studies (Del Boca, 2002).

In Italy, public nursery schools were first established in 1971 (*legge* 1044). They accept children between six months old and three years old. After nursery school, children can enroll in kindergarten until they start compulsory schooling at 6 years old. Public kindergarten, however, is managed by higher levels of government and so was not directly affected by the introduction of the ICI.[29] Although nursery schools are subsidized by municipalities, families pay a monthly fee. The share of costs paid by families must be at least 50 percent and each municipality can autonomously decrease the level of subsidization. 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 school (Cittadinanzattiva, 2018).

Demand for public nursery schools vastly exceeds available supply. In 2008, the total capacity of public nursery schools was equal to only 12 percent of the population below three years old (Cittadinanzattiva, 2018). For this reason, public nursery schools have long waiting lists. On average, 27 percent of applicants (more than 52,000 children in 2008) are not admitted (Cittadinanzattiva, 2011).[30] As a result, many households have to rely on private nursery schools. 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.[31] In general, private nursery schools are significantly more expensive. Al-

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[27]The European Commission stated that "increasing labour-force participation and raising the employment rate of women are paramount to meeting the Europe 2020 headline target (European Commission (2016), p. 1)."

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

[29]For decades, nursery schools have been considered purely a welfare service for 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.

[30]When demand surpasses capacity, admission is usually means-tested (Cittadinanzattiva, 2018).

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

though nationally representative data on private nursery schools are not available, anecdotal evidence suggests that the price difference can often be above 100 percent.[32]

To summarize, public nursery schools are a municipal service that can have important consequences on female labor supply. Access to public nursery schools is constrained by limited capacity. As a result, many parents enroll their children in private nursery schools. Their higher costs, however, might prevent a substantial share of households from being able to afford childcare, if they do not obtain a spot in public nursery schools.

We now show that bombing-adjacent municipalities disproportionately expanded their provision of public nursery schools after the ICI. This result reinforces the idea that the 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, bombing-adjacent municipalities invested more heavily in nursery schools. Compared with bombing-distant 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 school, and had 0.05 (+20 percent from mean) more public nursery schools (Table 3, panel B).

Second, higher provision translated into higher utilization. In bombing-adjacent municipalities, enrollment in nursery schools increased by 2.5 children or 24 percent from the pre-ICI mean (Table 3, panel A). Moreover, fertility increased by an additional 13 percent from the pre-reform average.[33]

Third, the results suggest that the increase in attendance did not come from children who would otherwise have attended a private nursery school. In 2011, the only year in which this variable is available in the census, the number of pupils attending private nursery schools was not statistically or economically different between bombing-adjacent and bombing-distant municipalities (Table 3, panel B). This finding is important because it indicates that the expansion of public nursery schools might have allowed some lower-income households to access childcare, instead of merely moving children across different types of nursery schools.

4.2 Effects on Local Labor Markets

In this section, we study whether the changes in the provision of public services had effects on local labor markets. There are several services provided by municipalities that could

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[32]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 (http://www.comune.milano.it/wps/portal/ist/it/servizi/educazione/Servizi_0-6_anni/Nidi_Micronidi/Quote_Contributive_+Nidi_+e_+Sezioni+Primavera). In the same municipality, private nursery schools cost between €460 and €800 a month (https://www.milanolife.it/migliori-asili-nido-privati-milano/).

[33]See Bauernschuster, Hener, and Rainer (2016) for evidence on the impact of public childcare on fertility in Germany.

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 thus increase labor demand. Investments in welfare programs, such as nursery schools, could also affect labor supply.

The data indicate that participation in the labor market disproportionately increased in bombing-adjacent municipalities after the ICI. 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 bombing-adjacent 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-ICI level, employment increased by 14 percent among women and by just 3 percent among men (Table A10). This result is driven by a stark decrease in the number of stay-at-home women in bombing-adjacent cities (-7 percent; Table 4, panel A). The main consequence was an 8-percent reduction in the gender gap in employment. 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 4.5, we will estimate triple-difference specifications with INPS data in order to weed out the effect stemming from concurrent changes within a city.

Finally, we can estimate the effects of fiscal decentralization on labor demand. In bombing-adjacent cities, the total number of firms increased by 12 to 20 units (5-9 percent), but this effect is imprecisely estimated. This change stems from the entry of smaller firms with fewer than three employees (10-16 percent), which represents the bulk of the Italian firm stock. In bombing-adjacent cities, better local services and programs for local economic development might have decreased the costs of running a business, thereby 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 probably 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.[34]

Fiscal decentralization might not have induced many people to move across munici-palities (Table A10). In bombing-adjacent cities, population increased by an additional 4 percent after the ICI, but this coefficient is not precisely estimated. The increase in foreign residents, however, is precise and large in magnitude. This result is not surprising because foreign residents in Italy are more likely to utilize welfare services. Data from the Survey of Households' Income and Wealth (SHIW), a survey by the Bank of Italy that is representative of the Italian population, indicate that foreign residents earn on average 27 percent less than domestic residents. This correlation is robust to controls for place of residence, age, marital status, household size, gender, and even education. Therefore, this finding indicates that better welfare services could have attracted more individuals in need of economic support.

We conclude this section with two additional findings. First, year-specific estimates indicate how the trend in labor outcomes sharply changed in bombing-adjacent municipalities after fiscal decentralization (Figures 3 and A6). If we consider stay-at-home women, for example, the data show a lack of differential trends between bombing-adjacent and bombing-distant municipalities between 1981 and 1991 (Figure 3, panel E). After 1991, however, the number of stay-at-home women drastically decreased in bombing-adjacent cities. Second, we can directly tie fiscal decentralization to the labor markets by estimating instrumental variable regressions.[35] The overall pattern of results from these IV specifications is in line with the previous reduced-form estimates in terms of both magnitude and dynamics (Table A11 and Figure A7).

4.3 Magnitudes

An ideal test for studying the effects of fiscal decentralization would increase municipal revenues from local taxes, while decreasing revenues from government transfers by the same amount. Similarly, it would decrease the PIT tax owed by each household by the amount owed through the new ICI in order to keep the total taxation constant. From the point of view of municipalities, the Italian reform came close to the ideal experiment. During the first year of implementation, the central government reduced transfers to each municipality to compensate their increased revenues from the ICI. From the point of view of taxpayers,

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[34]In bombing-adjacent 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 A10).

[35]Specifically, we estimate $y_{mt} = \alpha_m + \beta_b + \gamma_{rt} + \delta \Delta \text{Local taxes}_m \times \text{Post}_t + \varepsilon_{mt}$, 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 ICI in each city. Because $\Delta \text{Local taxes}_m$ could be endogenous, as explained in Section 3.1, we instrument it with the baseline treatment Near bombed$_m$. All other variables are unchanged from equation (1).

the introduction of the ICI increased the total tax liability, because the PIT did not decrease accordingly.

However, this negative income shock was unlikely to generate the estimated increase in female labor participation. Although economists have not reached a consensus on the effect size of wealth on labor decisions, "some agreement exists among labor economists that large, permanent changes in real wages induce relatively modest differences in labor supply" (Cesarini et al., 2017; p. 3918). It is therefore implausible for a €300-increase in taxation on the average property to have direct meaningful consequences on labor markets. In contrast, the 1993 reform had large effects on the composition of municipal revenues. The share of revenues from local taxes increased by 1.2 percentage points more in bombing-adjacent municipalities, relative to bombing-distant locations. This difference is large in magnitude, 15 percent of the average share of revenues from local taxes in 1990, and precisely estimated.

We can use this 15-percent increase to estimate the elasticities of local services and labor supply with respect to the degree of fiscal decentralization. A 1-percent increase in the share of revenues from local taxes increased the number of nursery schools by 0.6 percent, spending for nursery schools by 1.2 percent, the number of pupils enrolled in nursery school by 1.6 percent, and spending for local services by 0.15 percent. In the labor market, a 1-percent increase in the share of revenues from local taxes increased total employment by 0.32 percent and the number of economically active women by 0.9 percent.

The magnitude of these effects is consistent with the empirical evidence in the literature on public finance. For example, Gadenne (2017) estimates an elasticity of education spending to revenues from public taxes equal to 0.5. Hatfield and Kosec (2013) find that a 1-percent increase in the number of local governments (a measure of higher local competition) increases income growth per employee by 0.2 percent.[36]

Finally, the magnitude of the changes in labor supply is plausible if the main mechanism is indeed an expansion of public nursery schools. The literature on childcare suggests that an expansion of subsidies can increase the female labor supply when the preexisting availability of affordable childcare options is scarce. For example, Carta and Rizzica (2018) study the rollout of early admission of two-year-old children to subsidized public kindergartens in Italy. The availability of cheap childcare one year before the standard entry age increased female participation in the workforce up to 12.5 percent and female employment up to 12.3 percent. Moreover, it decreased the reservation wage of women by up to 24 percent. Baker,

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[36]Other papers confirm that changes in the sources of revenues of local governments can have substantial effects on local outcomes, even though these papers do not explicitly compute elasticities (Zhuravskaya, 2000; Martinez, 2018).

Gruber, and Milligan (2008) is another relevant example because it is one of the few papers to focus on pre-kindergarten childcare. It studies the introduction of subsidized childcare in Quebec for children up to four years old and finds that it increased maternal employment by 14.5 percent. All these estimates are remarkably close to our results. In the context of the introduction of the ICI, we found that higher exposure to fiscal decentralization, and therefore higher availability of subsidized public nursery schools, increased female participation in the workforce by 14 percent and female employment by 11 percent.

4.4 Analysis of Potential Mechanisms

Several mechanisms may have induced local administrators to increase the quantity and quality of municipal services.[37] For example, fiscal autonomy could have increased the accountability of local politicians and the degree of competition between adjacent municipalities. Moreover, it could have transferred the responsibility to provide local services to administrators who are closer to the final users and therefore have better knowledge about their preferences.[38]

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 along 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 electoral 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.[39] Second, we measure the level of municipal competition with Adjacent cities$_m$, the number of municipalities bordering city m. Third, we measure differences in the residents' preferences for public services with Below €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 is designed 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 bombing-adjacent and bombing-distant municipalities, before and after

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[37]Appendix B includes a longer discussion of the mechanisms studied by the theoretical and empirical literature on fiscal decentralizations.

[38]We do not expect this channel to be important in the Italian context, because the ICI did not shift the responsibility for providing local services between levels of government. It only changed their sources of funding.

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

the introduction of the ICI, and between cities with different levels of political competition, municipal competition, or low-income households.[40]

As expected, tougher political competition is correlated with larger treatment effects (Table A12). Relative to similar cities with lower competition, bombing-adjacent locations with close political races show a lower probability of fiscal infractions, a larger share of spending on 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 a larger effect on female labor supply. These results hold if we measure political competition in terms of 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. Similarly, the number of low-income residents does not drive any meaningful change in labor-market outcomes.

Finally, the fact that higher fiscal decentralization increased the accountability of local politicians is corroborated by data on political participation. To this end, we leverage multiple waves of the European Social Survey (ESS), a cross-national survey of attitudes and behavior established in 2001. Specifically, we correlate several measures of active political participation with either the mean or median difference in the share of municipal revenues from local taxes between 1990 and 1994 in the respondents' region of residence, a measure of short-term exposure to fiscal decentralization. We are forced to aggregate the effect of the policy at the regional level because information on the municipality or province of residence is not available in the ESS data. These specifications also include fixed effects for gender, years of completed education, survey year, citizenship status, and paternal country of birth. Overall, the data indicate that higher exposure to fiscal decentralization is correlated with higher levels of political participation (Table A13). For example, a 1-σ increase in mean revenues from local taxes (+ 2.83 percentage points) is associated with a 3-percent higher probability of being interested in politics and with a 2-percent higher probability of voting.

4.5 Heterogeneities by Age and Gender

In this section, we analyze the effect of fiscal decentralization on different types of workers. The goal is to discover just how direct the tie is between the expansion of public nursery schools and the previous findings on female labor participation. The rationale behind the following tests is that the probability of having a child under three years old, and therefore eligible to attend a nursery school, is not equal between younger and older women. Data from the Bank of Italy's SHIW indicate that the vast majority of women with at least one child

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[40]Although not reported, these specifications also include the interaction of these new variables with just $Post_t$. The interaction with Near bombed$_m$ is superfluous due to municipality fixed effects.

below three years old are between 25 and 35 years old (Figure A10).[41] The share of mothers with younger children sharply decreases between 35 and 40 years of age and becomes close to zero afterwards. If expanded public nursery schools are one of the major drivers of increased female labor supply, we should observe larger treatment effects among younger women, who have a higher probability of having a child eligible for nursery school.

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

$$
\begin{aligned}
y_{amt} \ = \ & \alpha_m + \beta_b + \gamma_{rt} + \zeta_{at} + \sum_a \delta_0^a \text{Age}_a \times \text{Near bombed}_m \times \text{Post}_t \\
& + \sum_a \delta_1^a \text{Age}_a \times \text{Near bombed}_m + \delta_2 \text{Near bombed}_m \times \text{Post}_t + \varepsilon_{amt},
\end{aligned}
\tag{3}
$$

where the unit of observation is an age group a living in municipality m in year $t \in [1987, 2011]$.[42] Age_a is a set of dummies identifying individual ages or age bins. The variable ζ_{at} denotes age-year fixed effects, while all other variables are unchanged from equation (1). The coefficients of interests, δ_0^a, measure the difference in labor-market outcomes between younger and older women, between bombing-adjacent and bombing-distant municipalities, and before and after the implementation of the ICI. They isolate the effect of nursery schools if they are the only expanded municipal service that differentially affects women of different ages across treated and control municipalities. The control group is represented by employed women between 50 and 54 years old.

Age-specific triple interactions confirm that the increase in female labor supply was larger among younger women (Figure 4 and Table A14). The number of women entering the labor market for the first time increased the most for women aged 26 to 27. Relative to bombing-distant municipalities and women between 50 and 54 years old, the number of new entrants into the labor market increased by 18 percent (from the pre-ICI mean) per post-ICI year, city, and age group (Figure 4, panel A). After this peak, the coefficients continued to decrease until they became a precisely estimated zero for 34-year-olds. The number of women reentering the labor market after a hiatus increased by 17 percent to 25 percent between ages 30 and 40 (Figure 4, panel B). As seen for new entrants, the effect goes down to zero afterwards. In both cases, the coefficients are negative for women between 20 and 24 years old, suggesting that the labor supply of these women decreased disproportionately in bombing-adjacent municipalities after the ICI. This effect could suggest that expanded

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

[42]Variables measuring reentry into the labor market, instead of first entry, start in 1989 because the first years are necessary to detect a break in employment.

nursery schools might have allowed these women to pursue a postsecondary degree. The education information available in the INPS dataset indicates that the number of women with a university degree is 111 percent higher among 20- to 24-year-old women living in bombing-adjacent municipalities, relative to bombing-distant cities and women between 50 and 54 years old (Table A14, panel D).[43]

After confirming that labor supply increased more for younger women, we analyze what types of jobs they were more likely to hold. We find that fiscal decentralization slightly decreased median wages of female employees (Figure 4, panel C). This decrease was between €345 and €735 per year (2.4 to 5.2 percent) and was concentrated among women under 35.[44] In bombing-adjacent municipalities, younger women also became slightly more likely to work fewer days during the year (-4 percent; Figure 4, panel D) and more likely to work outside their province of residence (+23 percent; Figure 4, panel E). Overall, these results suggest that fiscal decentralization decreased the reservation wage of younger women. The cost of participating in the labor force might have decreased because more women could enroll their young children in the cheaper public nursery schools, instead of relying on expensive private ones. A lower reservation wage induced women to accept positions with more flexible hours, lower pay, and higher commuting costs.

These findings are robust to alternative measures of labor supply or to slight modifications to equation (3).[45] Here, we want to briefly describe two follow-ups to the main results. First, estimating triple differences allows us to control nonparametrically for any change at the city-year level. We replace region-year fixed effects in equation (3) with city-year fixed effects. All the main findings are unaffected by this more demanding specification (Table A15, panel B). Second, we can estimate placebo treatments by including only women over 45. In these specifications, the excluded age category is represented by 60-year-olds. The data indicate that the cross-age differences among older women are never statistically different from zero (Table A15, panel C).

More importantly, the Social Security data are the only dataset available at the municipal level which has yearly observations between 1987 and 2011. Therefore, we can use them to study the dynamics of labor supply, a task that would be difficult to achieve using only decennial Census data. Specifically, instead of showing age-specific triple interactions, we

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

[44]These findings are robust to using alternative wage measures, such as log median wage (Figure 4, panel F) or median hourly wage (Table A14). Moreover, these results hold if we compute median wages using only new entrants or reentrants into the labor market (Table A14, panel C).

[45]Table A14 presents the estimated coefficients from all the regressions using INPS data. Table A15 presents robustness checks.

can fix the age dimension and estimate treatment effects by calendar year (Figure A11). As discussed in Section 3.4, the results show the lack of differential pre-ICI trends between bombing-adjacent and bombing-distant locations for all age groups. For younger workers, the increase in the number of new entrants and reentrants started between 1995 and 1996, and thus not immediately after the implementation of the ICI. This finding is consistent with our prior expectations for two reasons. First, around the same period, most municipalities in the sample held the first post-ICI elections (Figure A12).[46] Although only suggestive, this finding is consistent with the main takeaway of Section 4.4: the level of political competition seems to be the most plausible mechanism to have induced local administrators to improve municipal services, such as public nursery schools. Second, we should not expect any change in the labor market before the local governments had the time to expand access to local services. In the case of public nursery schools, expanded access required investments in infrastructure, which presumably needed a few years to come to fruition. The graphs also show that the effects peaked between 1999 and 2001 and then either stabilized or decreased. This nonincreasing trend during the last years of the sample is consistent with the fact that some municipalities experienced a decrease in fiscal decentralization after the implementation of the PIT surcharge in 2002 and the cancellation of the ICI on main residences in 2007 (Section 4.6).

Finally, we can include observations of male employees by estimating quadruple-difference regressions in which we interact a dummy equal to 1 for women, the age variables, a dummy for bombing-adjacent locations, and a post-reform dummy.[47] Relative to the base-line equation (3), this specification makes it more difficult for other municipal services to introduce bias to the estimated effect of public nursery schools. In order to confound the main treatment effects, other improved services would now also have to affect women more than men, in addition to benefiting younger women more than older women. The quadruple interactions confirm that the labor supply in bombing-adjacent municipalities experienced a larger increase in younger women after the ICI, relative to men and older women (Table A16). Younger women became also more likely to hold positions with lower wages, more flexible hours, and higher commuting costs.[48]

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[46] The first post-ICI date on which a large number of concurrent municipal elections (1,619) occurred is April 23, 1995.

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

[48] These effects on the characteristics of the labor contracts are more precisely estimated when they are computed only on new entrants or reentrants into the labor market (Table A16, panel C).

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4.6   Alternative Samples and Robustness Checks

In this section, we use alternative specifications to address two separate concerns. First, as discussed in Section 3.5, our identification strategy has assumed that WWII bombings interacted with the introduction of decentralization only through the age of buildings. To test this assumption, we augment the baseline equation (1) by including the amount of aid received by a province through the Marshall Plan interacted with a post-reform dummy.[49] All the main treatment effects are robust to the inclusion of three different specifications of Marshall Plan aid (Table A17).

Alternatively, we can estimate equation (2), in which we compare layer-1 and layer-2 municipalities. Layer-1 cities are within 10 km of bombed locations, while layer-2 cities are between 10 km and 20 km from bombed locations. These specifications exploit distance from bombed municipalities among cities that are fairly close to the sites of WWII bombings and likely to have been exposed to similar post-WWII conditions. In this set of regressions, the main findings are unchanged (Table A18 and Figure A8). 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, and a 5-percent reduction in the gender gap in employment. In spite of a different sample and treatment variable, the estimated effects of fiscal decentralization are generally within 3 percent of the baseline coefficients.

Second, as discussed in Section 3.4, some variables show some small preexisting differences between bombing-adjacent and bombing-distant cities, 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 bombing-adjacent and bombing-distant municipalities. The regressions with province-year fixed effects show treatment effects that are close in magnitude and precision to the baseline estimates (Table A19 and Figure A9).

Moreover, we show that the main results are robust to many variations of the baseline specifications. First, the main findings are fully robust to several changes to the matching process between bombed and nonbombed locations (Table A20). For example, including twenty-four variables in the propensity score matching, instead of the eight listed in Section 3.2, does not modify the results.

Second, 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 A21,

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[49]We cannot use the amount of aid at the city level because none of the municipalities in our sample directly received grants. This fact alone suggests that postwar reconstruction is unlikely related to our findings.

panel A). Similarly, the results hold if we estimate standard errors that are robust to both spatial and serial correlation (Conley, 1999; Table A21, panel B).

Third, the results are robust to controlling for population size. We show this by including population as a regressor in the baseline specification (Table A21, panel C).[50] Moreover, we estimate regressions in which the dependent variables are expressed as shares of local residents (Table A21, panel D). The share of employed women, and not only their total number, increased disproportionately in bombing-adjacent municipalities after the ICI. Therefore, our main results capture not only an increase in the population size, but also an actual change in labor supply.

Fourth, the results are robust to controlling for nonlinear trends correlated with geographical characteristics (Table A21, panel E). Specifically, we augment the main specifications with three variables observed in 1991 (i.e., population density, a dummy for rural municipalities, a dummy for coastal cities) interacted with year dummies. The resulting treatment effects are not only statistically significant but even larger in magnitude than the baseline estimates. Similarly, the results are robust if we control for nonlinear trends correlated with the characteristics of the real-estate market (i.e., average size of buildings and share of high-quality buildings; Table A21, panel F).

Fifth, we can control for other legal changes that took place between 1993 and 2011 (Table A21, panel G). In particular, municipalities could impose a surcharge to the PIT from 2002. On the surface, this reform increased their degree of fiscal independence, but in reality the PIT surcharge was not as transparent a revenue stream as the ICI. For this reason, some municipalities might have had the opportunity to replace ICI revenues with revenues from the new PIT surcharge to reduce monitoring by local residents (Bordignon, Grembi, and Piazza, 2017). In addition, as a result of the laws 244/2007 and 126/2008, municipalities lost the ICI revenues from the main residence of homeowners starting in 2007, significantly reducing their reliance on local taxes. Our results are robust if we end the sample in 2001, the last census year before these law changes.

Sixth, 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 A21, panel H).

Seventh, we estimate placebo treatments by assigning bombing-adjacent status at random (Table A21, panel I). The resulting coefficients are statistically insignificant and small in magnitude.

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

5 Conclusions

This paper studies how fiscal decentralization affects the provision of public services and local labor markets. It exploits a 1993 Italian reform that introduced a local property tax (ICI) under the direct control of municipalities, and simultaneously reduced 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 ICI base.

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

More broadly, our results inform about contexts in which municipalities receive revenue-generating authority to fund the delivery of local public services. The Italian experience suggests that increasing the accountability of local administrators through a higher reliance on transparent local taxes can improve the delivery of local services and, therefore, the labor-market outcomes of the residents. Out of all local services, the central role played by nursery schools is particularly relevant for countries in which access to affordable child care is limited.

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Figures and Tables

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

A. Means
(2017 € per capita)

B. Means
(share of total revenues)

C. Regression
(2017 € per capita)

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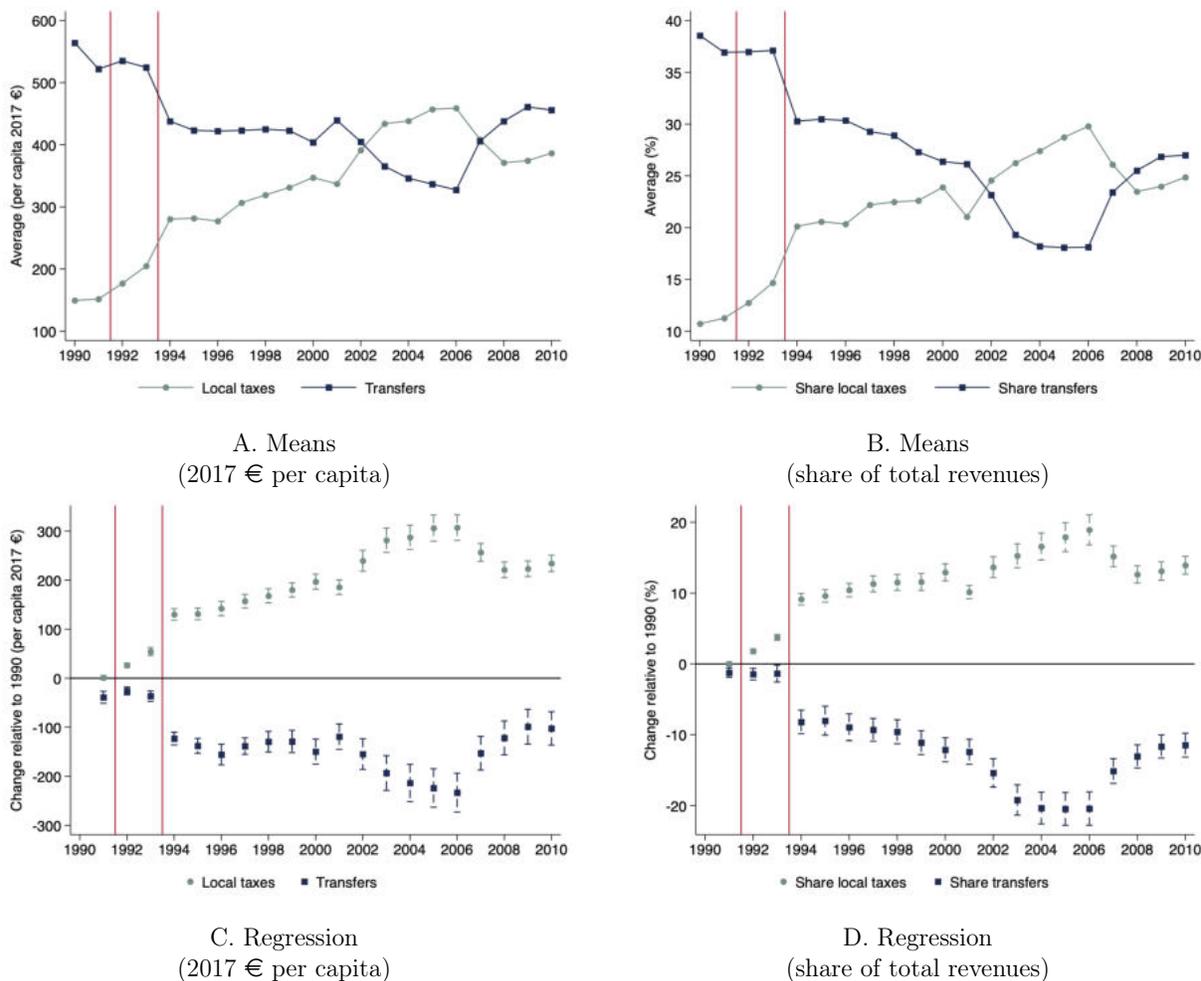
Notes: These graphs show the change in the composition of revenues of Italian municipalities. Panels A and B show the average revenues from local taxes and from transfers issued by higher levels of government (provinces, regions, central government), either as 2017 € per resident (panel A) or as a share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

Tons of bombs after armistice
- (2000,12000]
- (500,2000]
- (.1,500]
- [0,.1]

Bombed
Around bombed
Matched to bombed
Around matched
Other municipalities
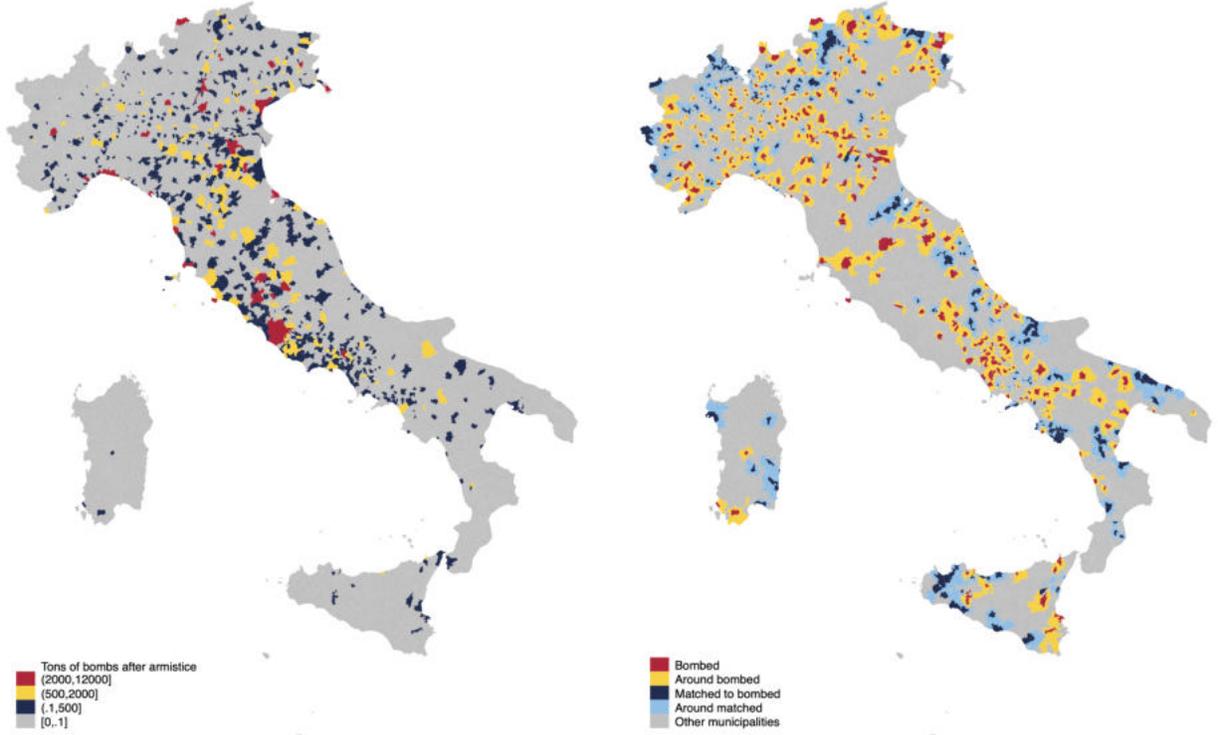
A. Distribution of WWII bombings   B. Municipalities in the sample

Notes: Panel A shows the distribution of Allied bombings during WWII that were executed after the Armistice of Cassibile between Italy and the 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 percent confidence intervals. Source: Italian Minister of the Interior, available online at https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4; Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/.

Figure 4: Age Effects, Employees of Privately Owned Firms
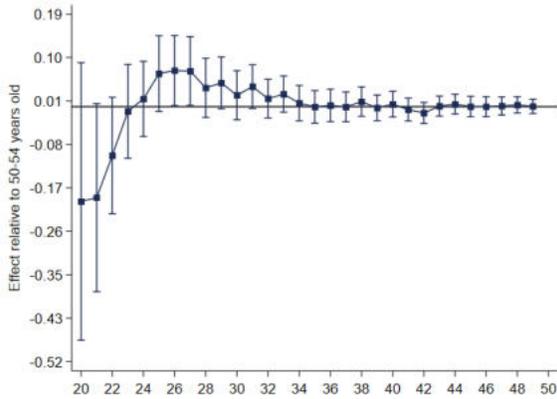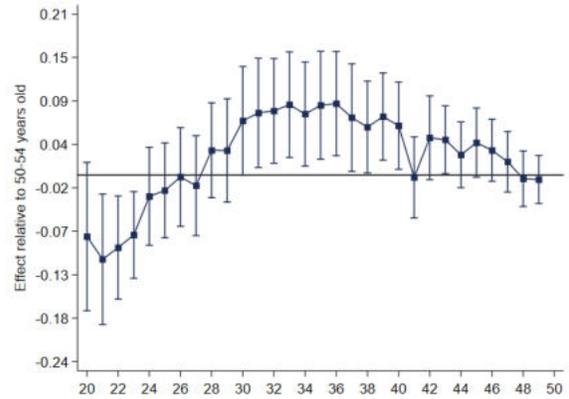
A. New entry in labor market

B. Reentry in labor markets
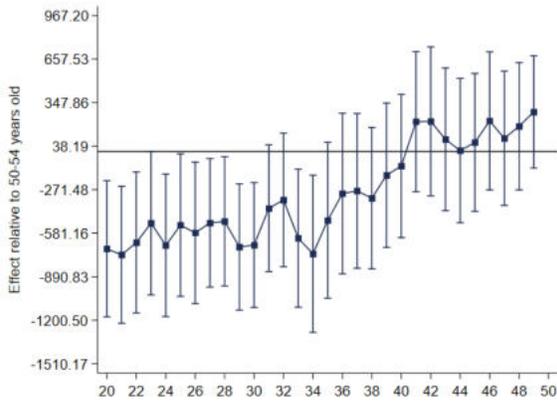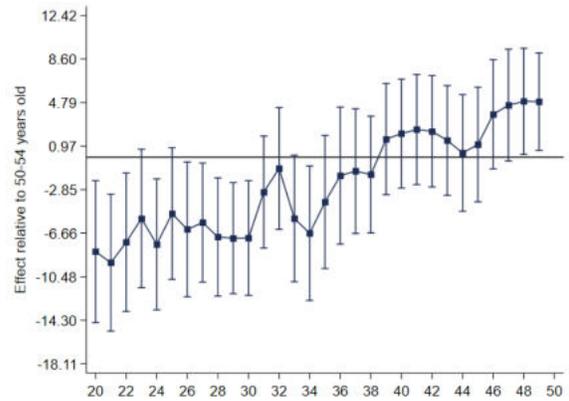
C. Median wage

D. Median days worked
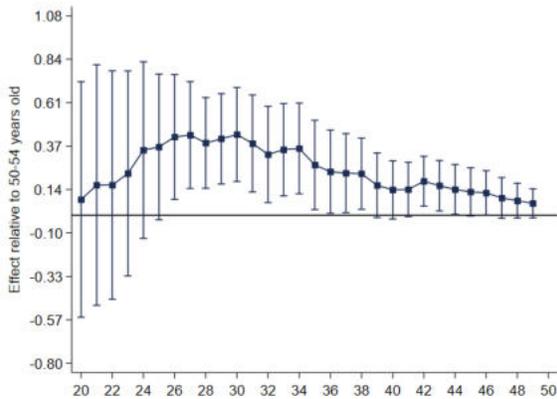
E. Working outside province of residence
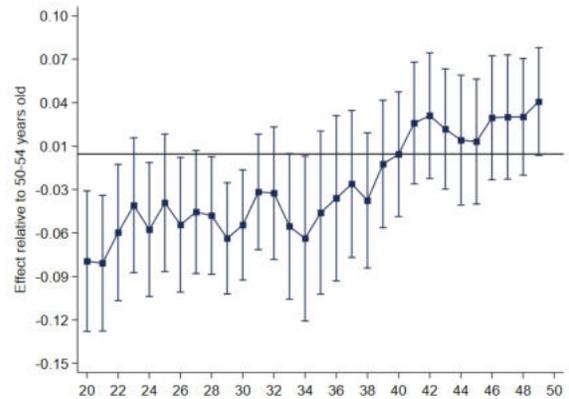
F. Log median wage

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

Table 1: Summary Statistics

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

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

Table 2: Trends Before the LPT Introduction

|	Panel A: Balance sheets of Italian municipalities							
	Share of revenues from 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		0.080		-32.939		-28.110	
	(0.107)		(0.298)		(25.826)		(25.244)	
Near bombed x 1991		0.103		0.305		-53.047		-53.518
		(0.177)		(0.529)		(42.940)		(43.967)
Near bombed x 1992		0.308		0.168		-66.765		-57.340
		(0.212)		(0.595)		(51.423)		(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		1.896		1.132		0.832	
	(1.319)		(2.026)		(0.711)		(1.434)	
Near bombed x 1991		13.940		18.962		11.324		8.318
		(13.186)		(20.262)		(7.107)		(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		0.377		0.120		-0.001	
	(3.910)		(0.345)		(0.240)		(0.001)	
Near bombed x 1991		45.811		3.774		1.196		-0.009
		(39.101)		(3.452)		(2.397)		(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 composed of 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				
	Near bombed x Post	Obs.	R^2	Near bombed x Post	Obs.	R^2	Mean outcome	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Dependent variables are available before and after LPT								
Share of rev. from local taxes	1.245***	47,255	0.783	1.314***	47,122	0.801	11.53	6.61
	(0.335)			(0.361)				
Share of rev. from gov. transfers	-1.493***	47,252	0.663	-1.728***	47,119	0.686	36.69	12.02
	(0.406)			(0.382)				
Revenues per capita	-87.546**	47,707	0.602	-69.801*	47,578	0.624	1677.24	1197.95
	(36.436)			(38.854)				
Spending per capita	-83.440**	47,694	0.601	-69.614*	47,566	0.624	1674.14	1203.15
	(37.396)			(40.401)				
Deficit (share of rev.)	0.104	47,234	0.248	0.090	47,102	0.299	3.05	4.14
	(0.132)			(0.128)				
Pupils in nursery schools	2.475***	7,277	0.879	2.774***	7,259	0.900	10.43	21.23
	(0.746)			(0.775)				
Births	0.847***	4,848	0.941	1.318***	4,836	0.948	6.52	12.72
	(0.262)			(0.325)				
Panel B: Dependent variables are available only after LPT								
Has fiscal infraction	-0.006	17,954	0.192	-0.011	17,888	0.243	0.51	0.5
	(0.014)			(0.014)				
Spending for local services (%)	1.195**	28,401	0.266	0.835	28,319	0.327	54.8	16.25
	(0.502)			(0.524)				
Rev. for admin. tasks per employee	257.568*	28,560	0.063	292.717*	28,478	0.124	2244.73	3756.45
	(139.336)			(149.086)				
Has program for local develop.	0.074***	28,430	0.163	0.050**	28,347	0.233	0.61	0.49
	(0.019)			(0.020)				
Has nursery schools	0.054***	28,430	0.222	0.042**	28,347	0.296	0.63	0.48
	(0.017)			(0.017)				
Spending for nursery schools (%)	0.178**	28,248	0.283	0.045	28,165	0.387	1.01	2.03
	(0.082)			(0.069)				
Public nursery schools	0.052***	17,326	0.391	0.005	17,194	0.504	0.26	0.61
	(0.019)			(0.017)				
Pupils in private nursery schools	0.015	2,403	0.771	0.159	2,397	0.804	11.88	21.18
	(0.517)			(0.522)				

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

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 composed of 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/.

Online Appendix - Not For Publication

A   Additional Figures and Tables

Figure A1: Share of Revenues from Local Taxes and Services
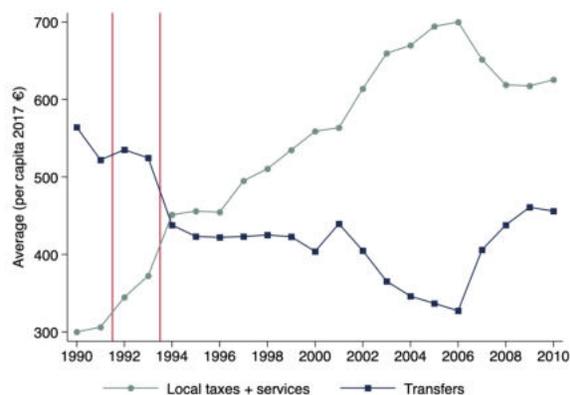
A. Revenues from local taxes and services
(2017 € per capita)
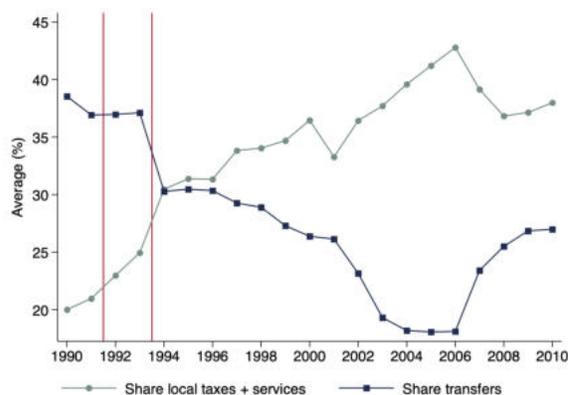
B. Revenues from local taxes and services
(share of total revenues)
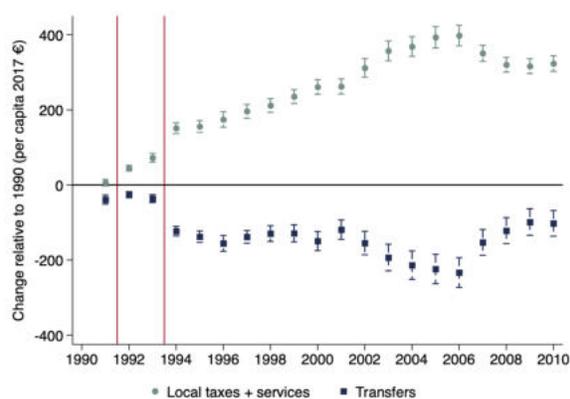
C. Revenues from local taxes and services
(2017 € per capita)
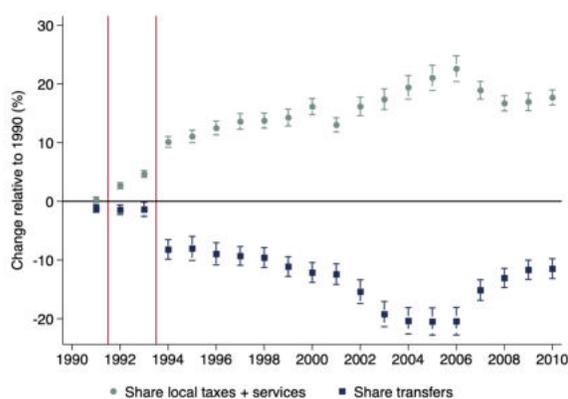
D. Revenues from local taxes and services
(share of total revenues)

Notes: Panels 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). Panels 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: Scatterplots between Age of Buildings and Effects of the Policy

A. Pre-WWII buildings and cadastral values—Whole sample

B. Pre-WWII buildings and cadastral values—Estimating sample

C. Pre-WWII buildings and Δ Rev. local tax (94-90)—Whole sample

D. Pre-WWII buildings and Δ Rev. local tax (94-90)—Estimating sample
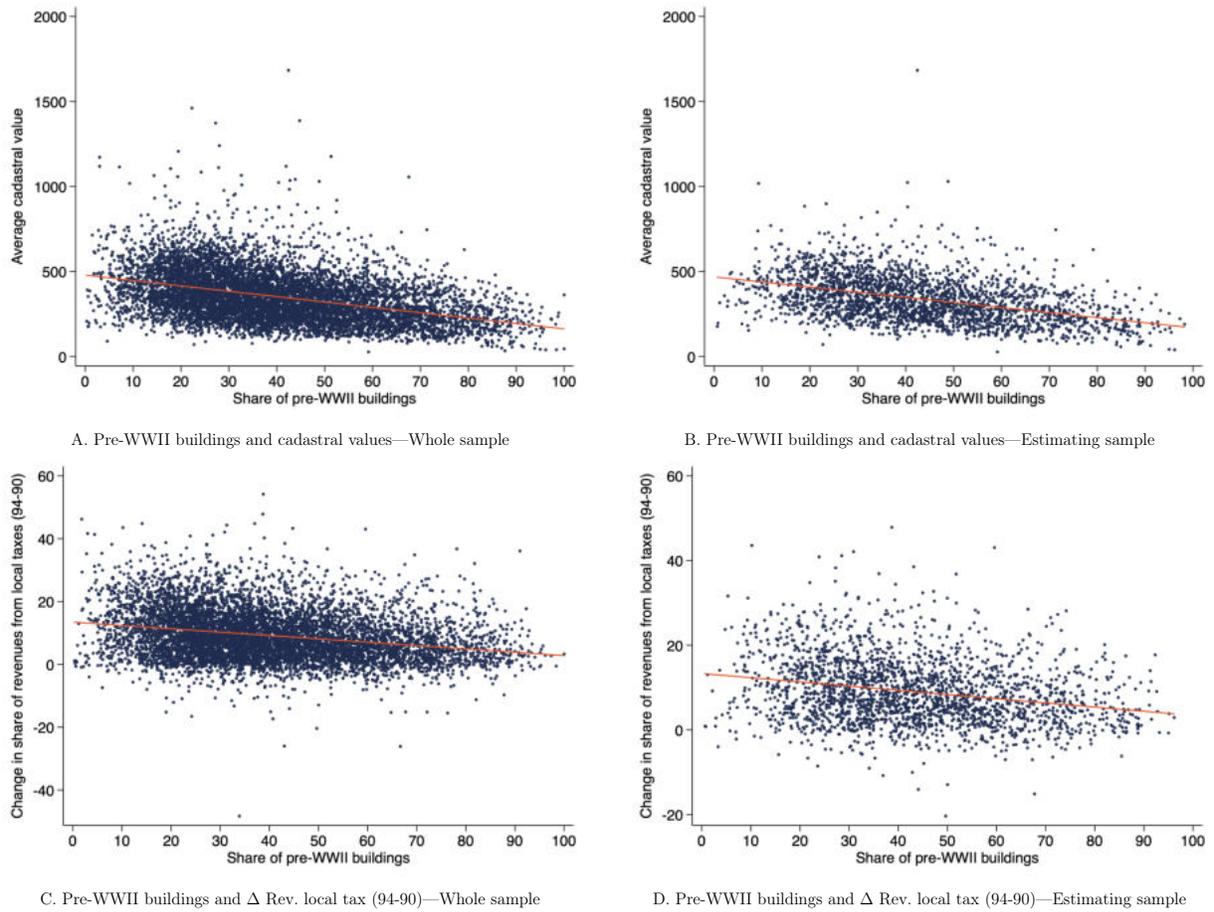
Notes: These graphs show the correlations between the share of pre-WWII buildings and the average cadastral value of buildings (panels A and B) or the difference in the share of revenues from local taxes between 1990 and 1994 (panels C and D). Panel B and D show only the municipalities in the main estimating sample. Source: Italian Minister of the Interior, https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4; Atlante Statistico dei Comuni, http://asc.istat.it/asc_BL/.

Figure A3: Maps of Age of Buildings and Exposure to Fiscal Decentralization
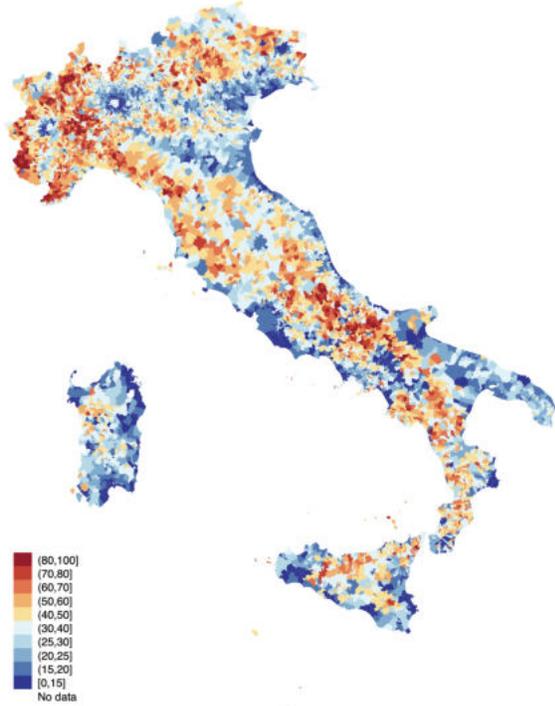
A. Share of pre-WWII buildings
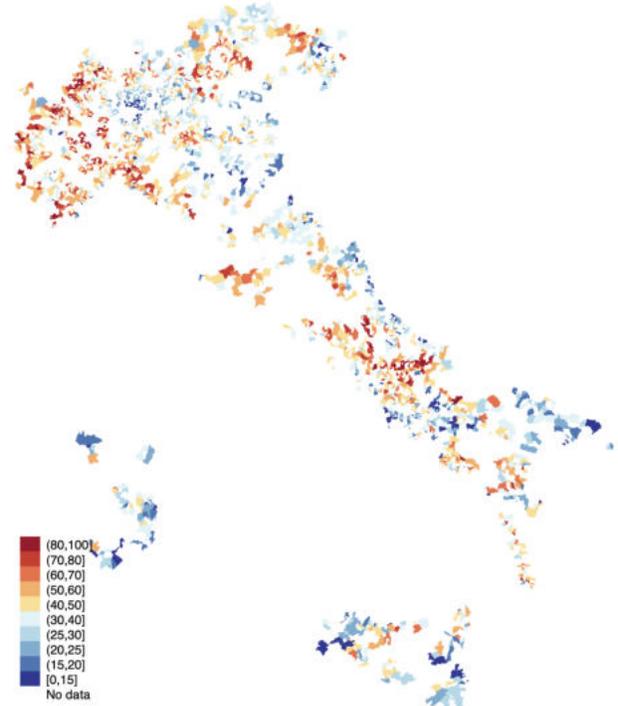
B. Pre-WWII buildings—Estimating sample
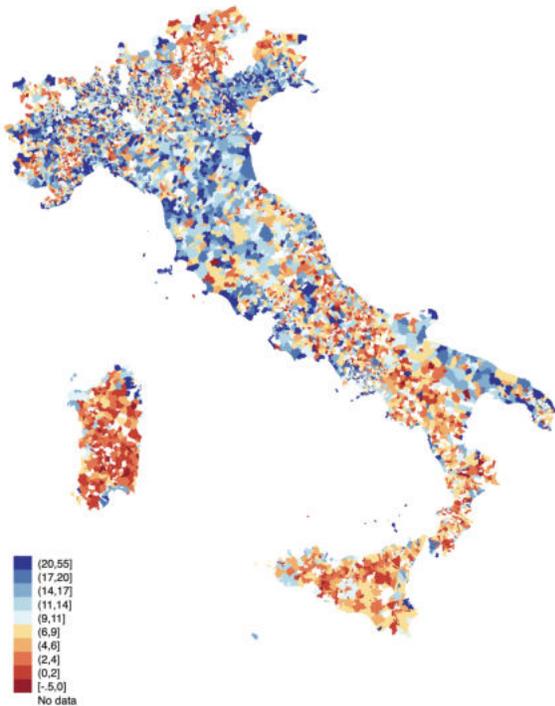
C. Δ Rev. local tax (94-90)
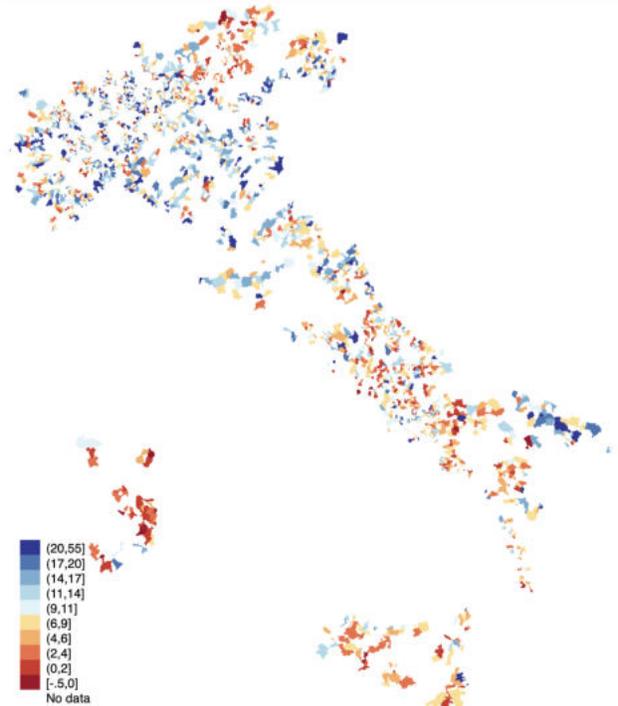
D. Δ Rev. local tax (94-90)—Estimating sample

Notes: These graphs show the geographical distribution of the share of pre-WWII buildings and the difference in the share of revenues from local taxes between 1990 and 1994. Panel B and D show only the municipalities in the main estimating sample. Source: Italian Minister of the Interior, https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4; Atlante Statistico dei Comuni, http://asc.istat.it/asc_BL/.

A3

Figure A4: Pre-Reform Trends, Employees of Privately Owned Firms
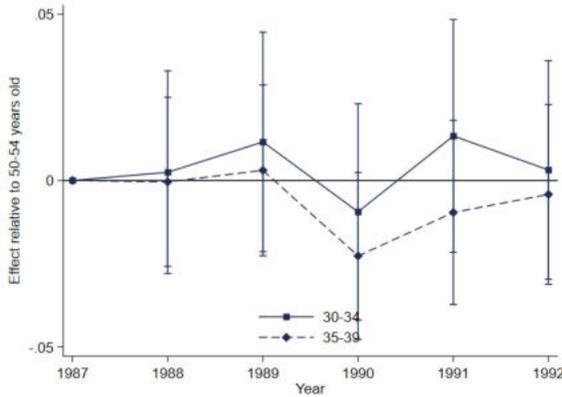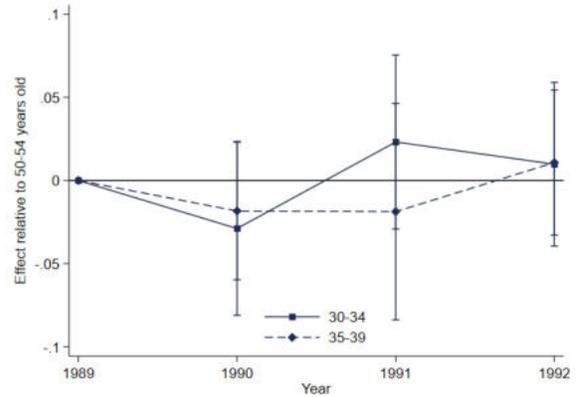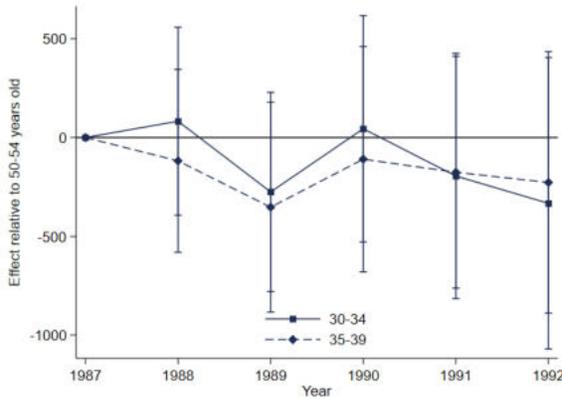
A. New entry in labor market

B. Reentry in labor markets
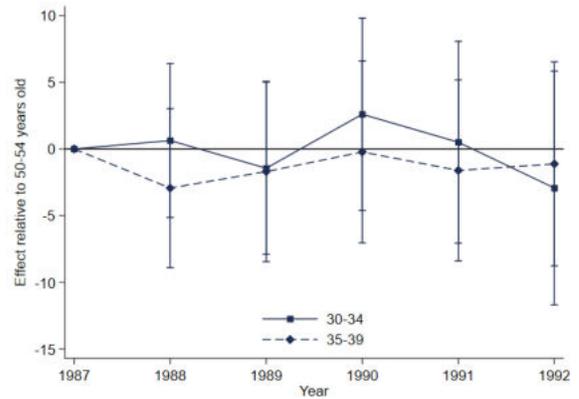
C. Median wage

D. Median days worked
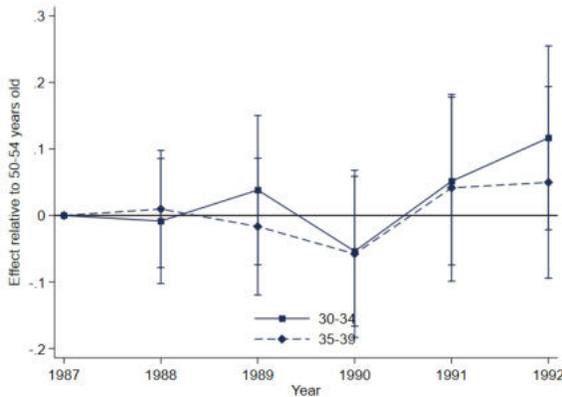
E. Working outside province of residence
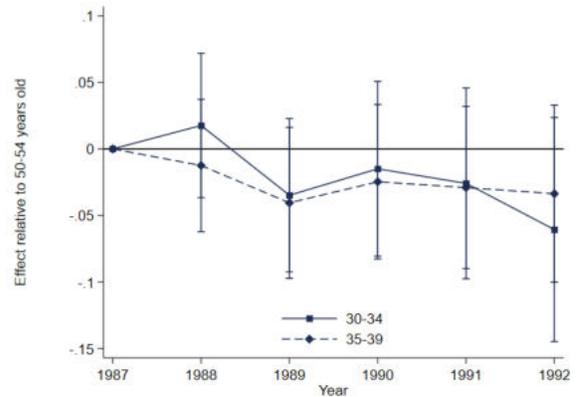
F. Log median wage

Notes: These graphs show triple interactions of age, a dummy equal to 1 for near-bombed locations, and pre-reform year dummies. The sample includes only women. The control group is composed of municipalities adjacent to cities matched to bombed locations. The omitted age group is composed by 50- to 54-year-olds. For sake of clarity, the graphs shows the coefficients for only two age bins (30-34 years old and 35-39 years old). Table A5 provides more evidence on the remaining age bins. The regressions also include the pairwise interactions between the main variables, city fixed effects, bombed-city fixed effects, age-year fixed effects, and region-year fixed effects. Standard errors are clustered at the bombed-city level. The vertical bars measure 95 percent confidence intervals. Source: Istituto Nazionale della Previdenza Sociale (INPS).

Figure A5: Alternative Sample, Layers Around Bombed Locations

Legend (Panel A)
Bombed
Cities between 0 and 10 km
Cities between 10 and 20 km
Other municipalities

Legend (Panel B)
Bombed
Cities between 0 and 15 km
Cities between 15 and 30 km
Other municipalities

A. Layer 1 within 10km   B. Layer 1 within 15km
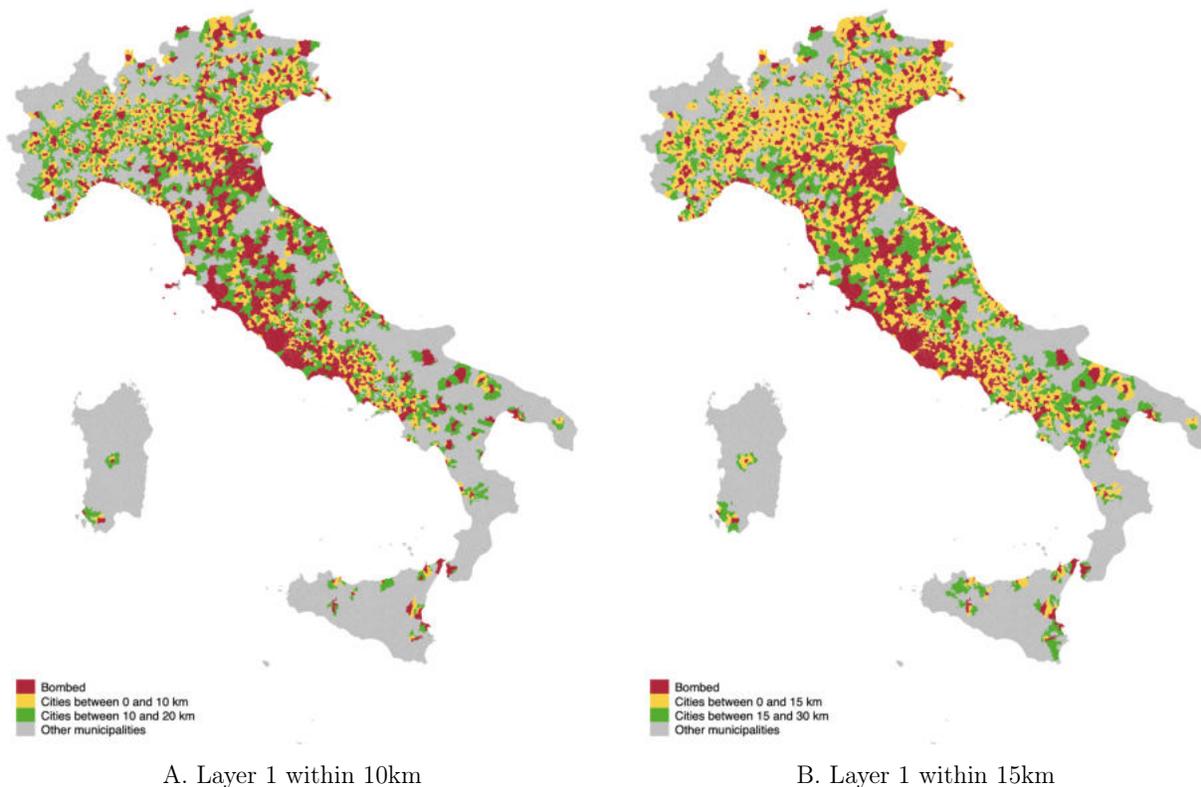
Notes: Panel A shows the municipalities hit by Allied tactical air attacks after the Armistice of Cassibile (red), cities within 10km of them (yellow), and cities between 10 and 20km from them (green). Panel B shows the municipalities hit by Allied tactical air attacks after the Armistice of Cassibile (red), cities within 15km of them (yellow), and cities between 15 and 30km from them (green). Source: USAF Theater History of Operations Reports (THOR) Database, available at www.afri.au.af.mil/thor.

Figure A6: Effects of Fiscal Decentralization on Labor Markets, More Outcomes
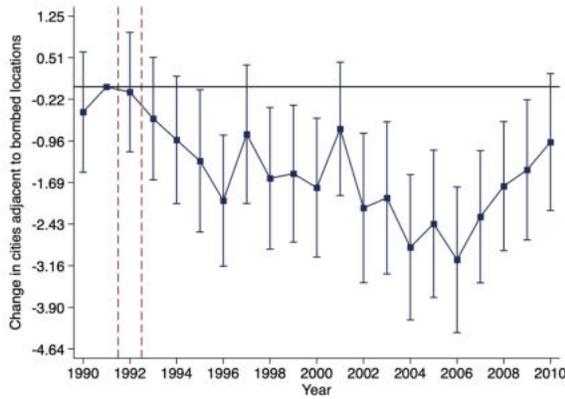
A. Share of revenues from gov. transfers
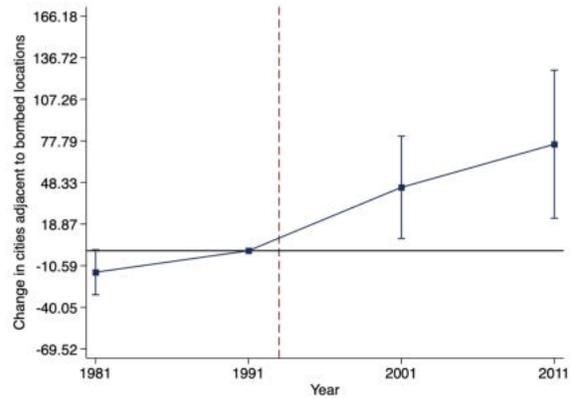
B. Econ. active pop.—women
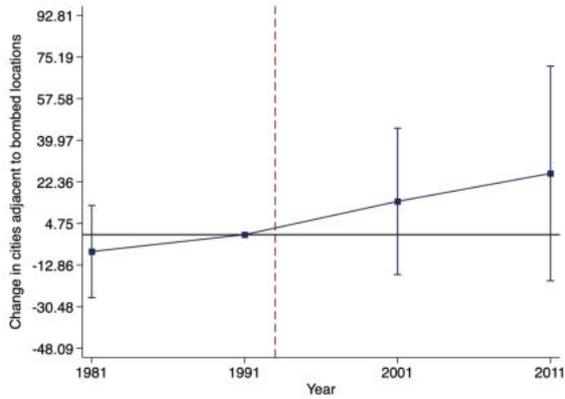
C. Econ. active pop.—men
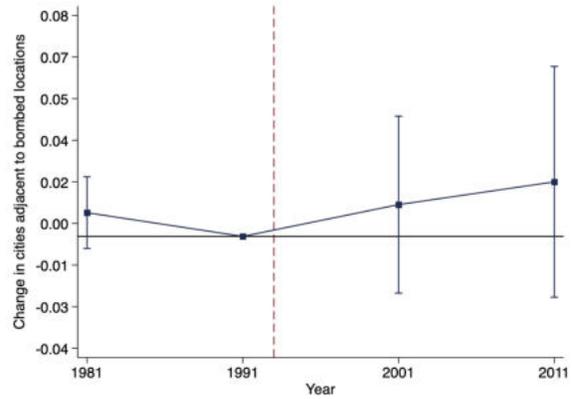
D. Firms with ≥ 200 employees
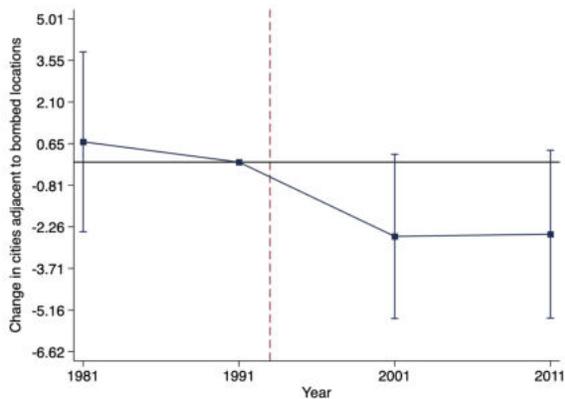
E. Agricultural workers
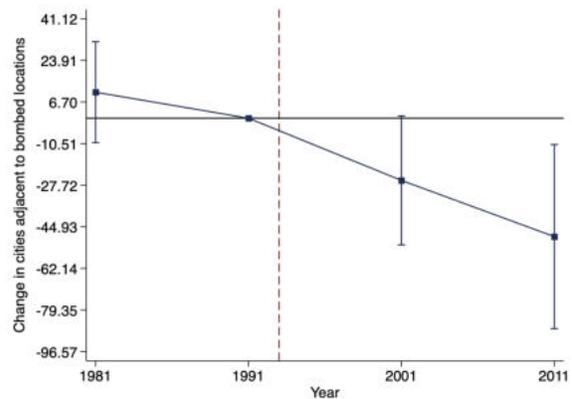
F. Manufacturing workers

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 composed of 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 percent confidence intervals. Source: Italian Minister of the Interior, available online at https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4; Atlante Statistico dei Comuni, Istituto Nazionale di Statistica, available online at http://asc.istat.it/asc_BL/.

Figure A7: 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
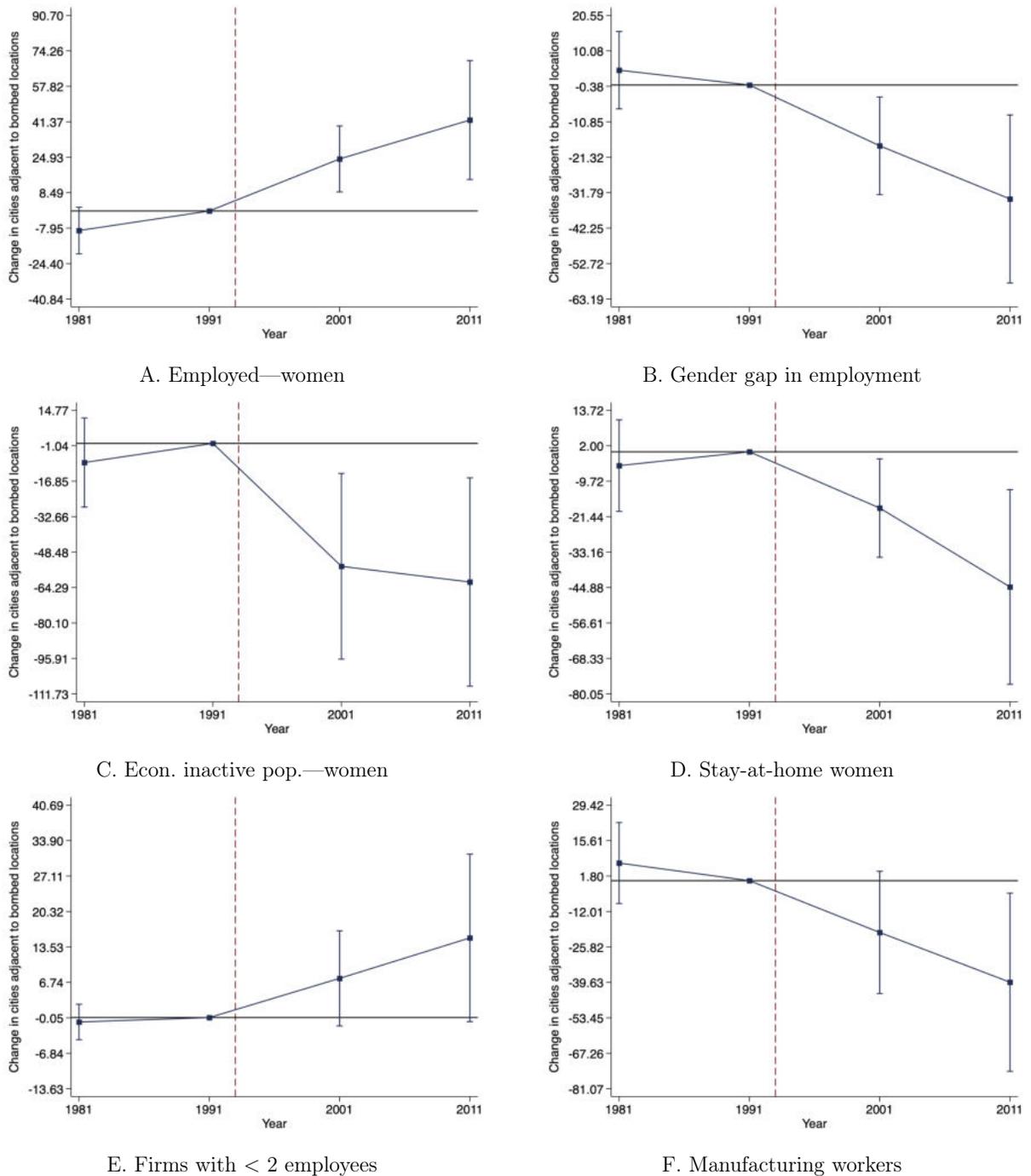
Notes: The coefficients show the effect of a 1 percentage point increase in the share of revenues from local taxes. This variable is instrumented by a dummy that identifies cities adjacent to municipalities bombed by Allied tactical air attacks during WWII. The control group is composed of municipalities 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 percent 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 A8: Effects of Fiscal Decentralization on Labor Markets, Alternative Sample
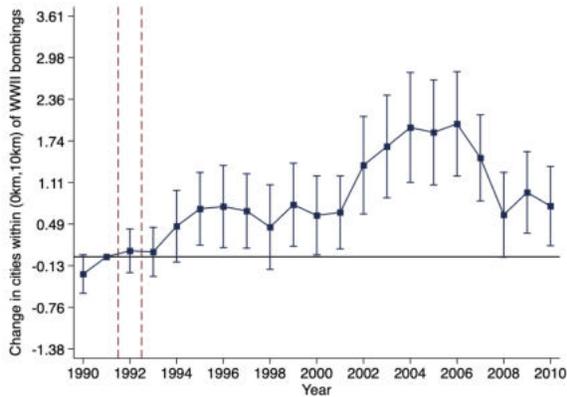
A. Share of revenues from local taxes
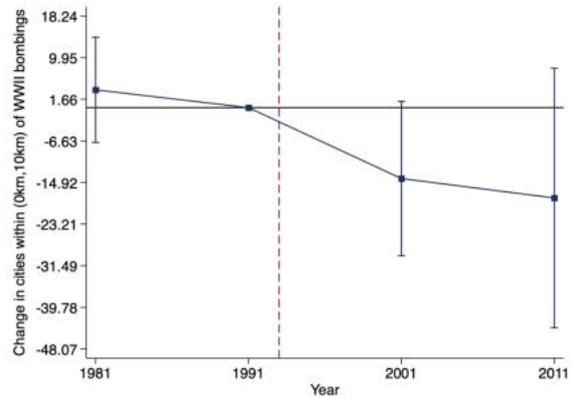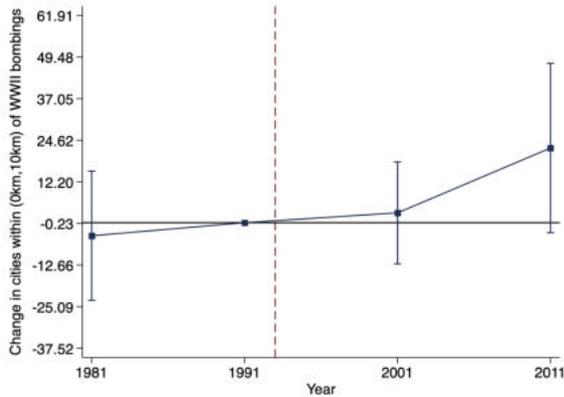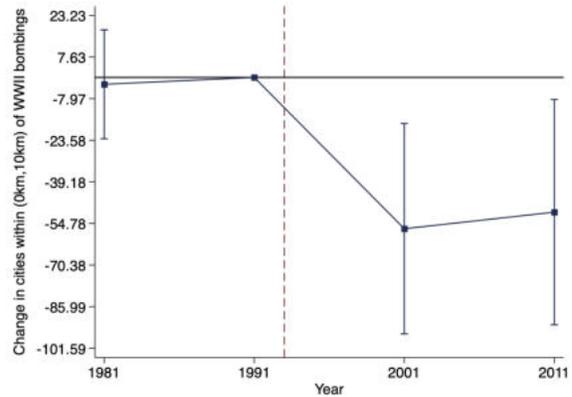
B. Gender gap in employment

C. Employed—women

D. Econ. inactive pop.—women
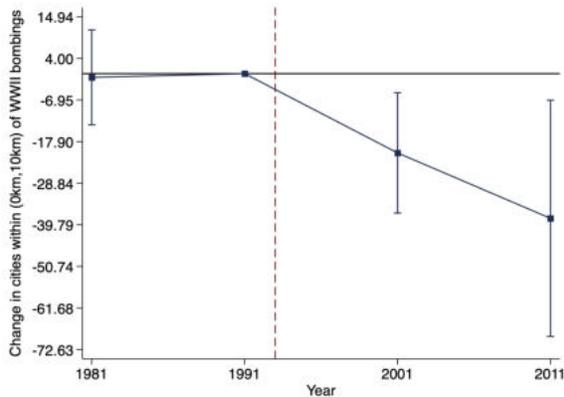
E. Stay-at-home women
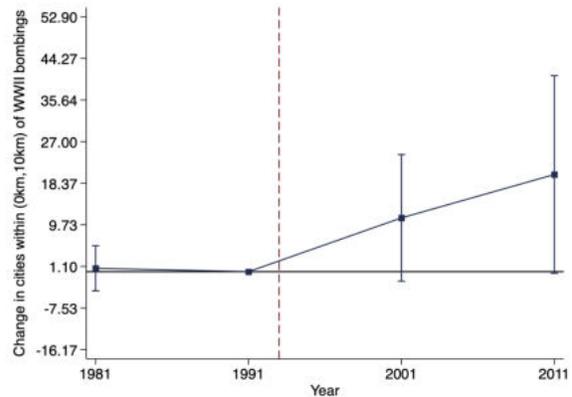
F. Firms with < 2 employees

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 composed of municipalities between 10km and 20km from 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 percent 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 A9: Effects of Fiscal Decentralization on Labor Markets, Matching Layer 1

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 composed of 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 percent 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 A10: Probability of Having a Child Below Three Years Old, SHIW
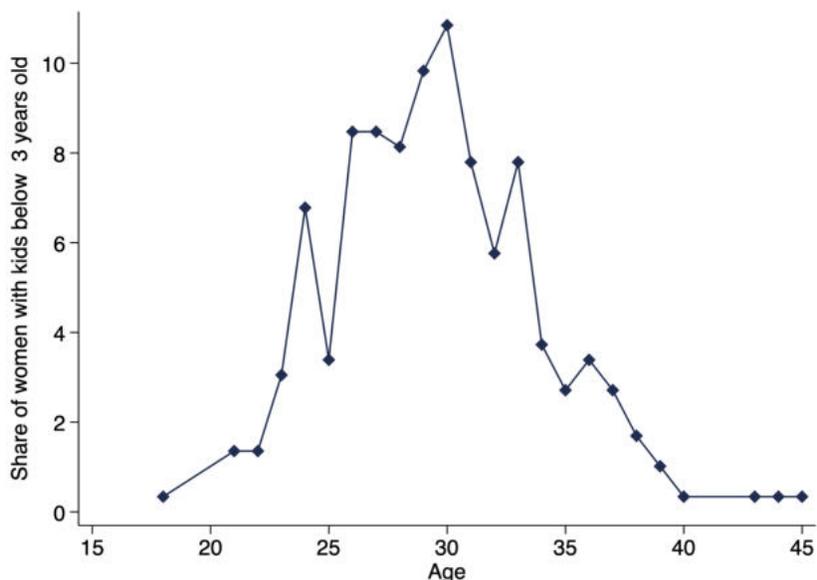
A. 1987-1993
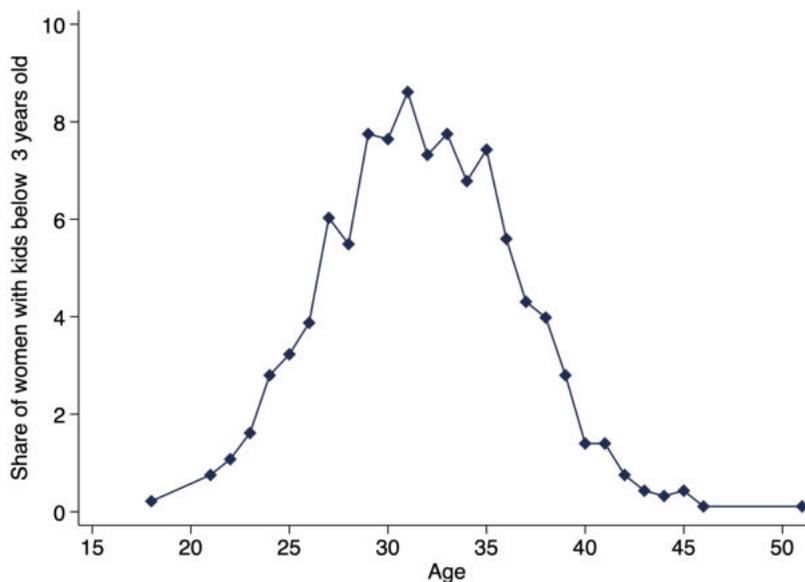
B. 1987-2010

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

Figure A11: Yearly Effects, Employees of Privately Owned Firms
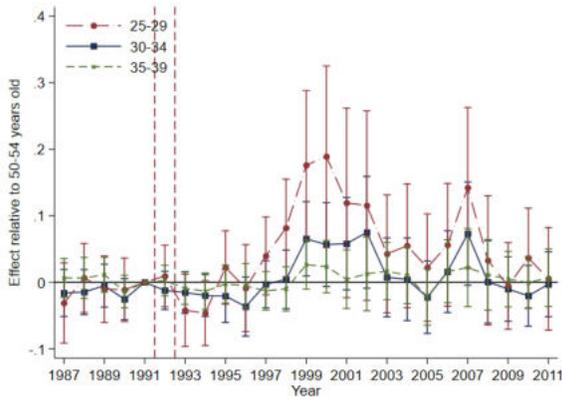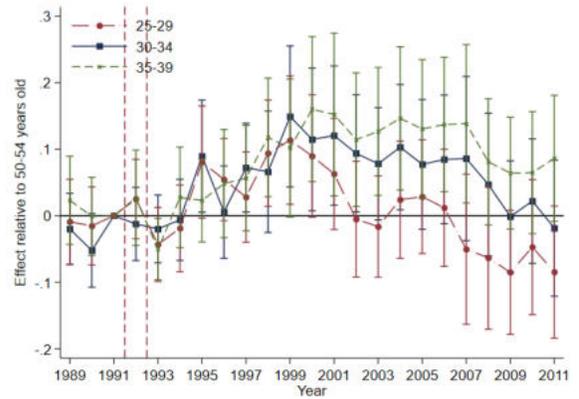
A. New entry in labor market

B. Reentry in labor markets
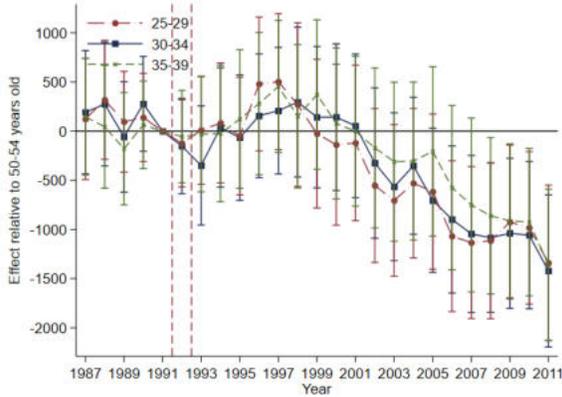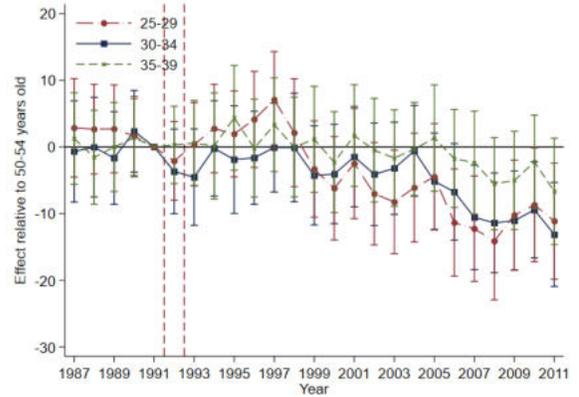
C. Median wage

D. Median days worked
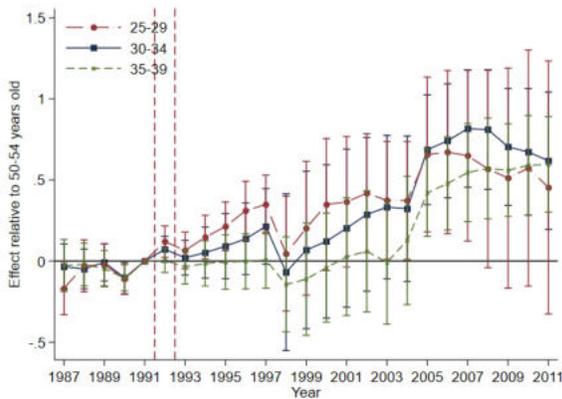
E. Working outside province of residence
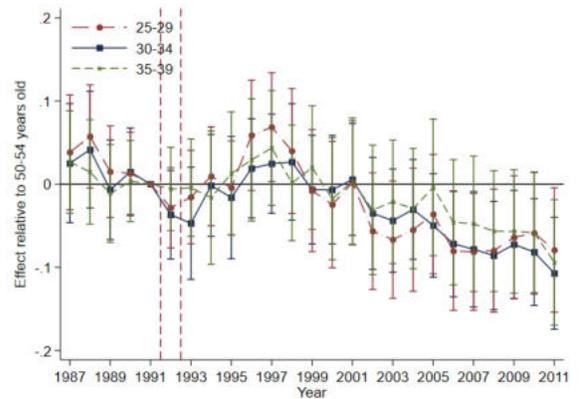
F. Log median wage

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

Figure A12: Number of Municipal Elections by Date
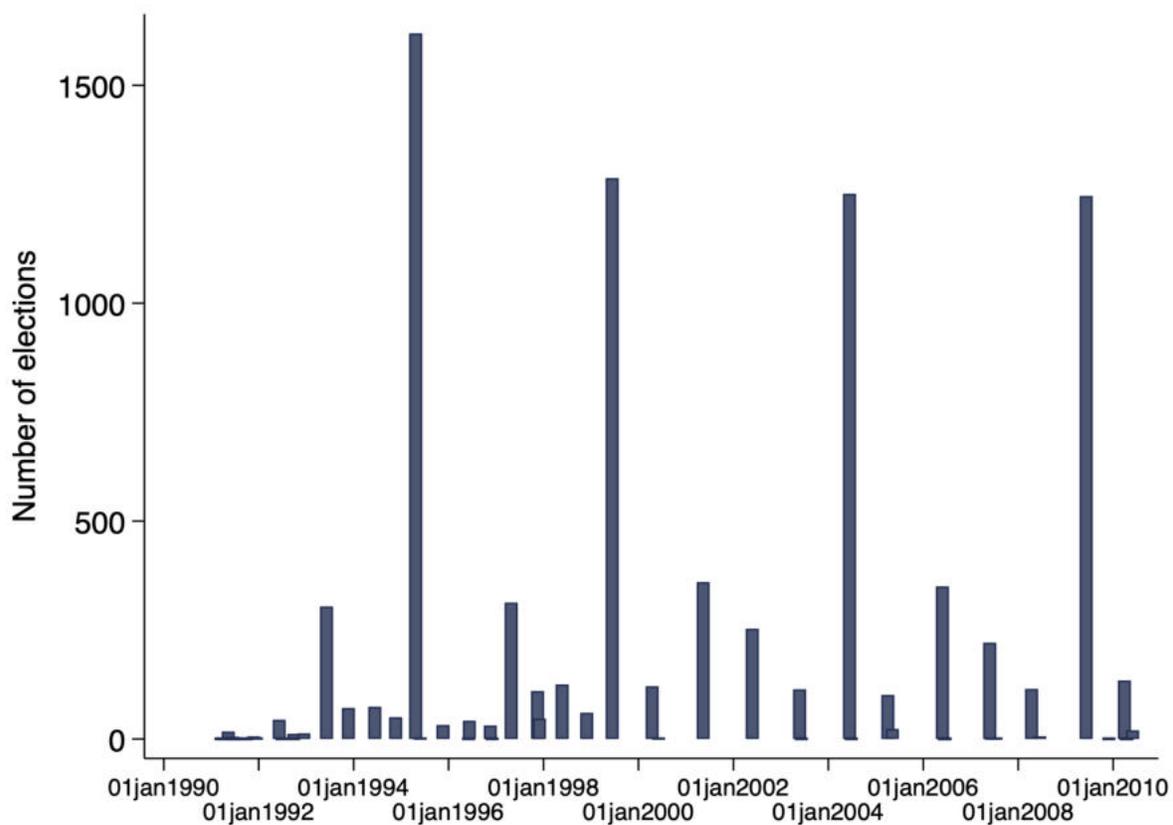
Notes: This graph shows the number of municipal elections by date in the estimating sample (near-bombed and near-others municipalities). Data before 1993 is likely incomplete. Source: Italian Minister of the Interior, available online at https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4.

Table A1: Additional Summary Statistics

	All years				$t \leqq 1992$	$t > 1992$
	Mean	Std. Dev.	Obs.	Availability	Mean	Mean
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Balance sheets of Italian municipalities						
Share of total spending on local services						
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		
Sports	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		
Share of total revenues from local services						
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		
Sports	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		
Other variables						
Spend. on nursery schools (%)	1.15	2.10	95642	1998-2010		
Rev. from nursery schools (%)	1.55	4.50	92504	1998-2010		
Panel B: Census data						
Foreign residents	235.74	2389.56	24256	1991-2011	44.07	331.44
Pupils in nursery schools	28.14	235.36	24262	1991-2011	20.41	32.00
Births	11.88	70.20	16177	1991-2011	11.97	11.80
Stay-at-home women	1098.36	8010.02	40349	1981-2011	1290.07	811.93
Gender gap in employment	-653.06	3475.06	32314	1981-2011	-783.29	-523.18

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

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

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

Notes: In panel A, the dependent variable is the average cadastral value in 2013, the first year in which this information is available. 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 m^2 of residential real estate between 2002 and 2010. The database measures market values in multiple areas within a municipality, but only larger cities are included in the sample. Source: Osservatorio del Mercato Immobiliare. Building size is the average number of rooms of residential buildings. Building quality is the share of high-quality residential buildings in the municipality (cadastral classes A1, A7, A8, A9). Geography: size of municipality, dummy for coastal cities, dummy for mountain cities, altitude. Demography: population, share of residents above 65 years old, share of household with 2 or fewer members, share of foreign-born residents, share of women. Economy: share of residents with university degree, share of unemployed. share working in the industrial sector, share working in the service sector. Standard errors clustered at the province level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table A3: Propensity Score Matching

	Bombed after armistice (1)
Population (per 1000 residents)	0.01337
	(0.04016)
Population^2 (per 1000 residents)	-0.00073***
	(0.00015)
Area (km^2)	0.01867***
	(0.00249)
Area^2	-0.00007***
	(0.00001)
Population density	-0.00021**
	(0.00009)
Number of buildings	0.00020*
	(0.00011)
Share owner-occupied properties	-0.03673***
	(0.00452)
Share of population < 3 years old	-0.14224**
	(0.05818)

Notes: This table shows the coefficients used to match bombed and nonbombed municipalities. Specifically, we match bombed municipalities to other cities using propensity-score matching and a nearest-neighbor algorithm. We also impose a common support between treated and control locations. The caliper is 0.15 and matching is performed without replacement. We used the Stata command `psmatch2` with options "common ties noreplacement descending caliper(0.15)." The matching is based on the following variables from the 1991 census: population, area, population density, number of buildings, share of homeowners, share of residents under three years old. The regression also includes region fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: 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)
					Panel A: Age of buildings and change in fiscal federalism						
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
					Panel B: Population and industrial census						
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 (km²)	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 $\leq$ 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
					Panel C: Balance sheets of Italian municipalities						
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 non-bombed 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 under 3, 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 A5: 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		-0.065		0.093		0.009	
	(1.618)		(0.048)		(0.095)		(0.140)	
Near bombed x 1991		20.918		-0.653		0.926		0.093
		(16.178)		(0.477)		(0.954)		(1.403)
Observations	4,834	4,834	4,848	4,848	4,848	4,848	4,848	4,848
R^2	0.996	0.996	0.790	0.790	0.988	0.988	0.989	0.989
Dep. var.—mean	1427.63	1427.63	3.09	3.09	49.27	49.27	86.29	86.29
Dep. var.—std. dev.	2215.04	2215.04	10.65	10.65	86.52	86.52	140.65	140.65

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

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

Table A6: Did Mayors Respond to the LPT Introduction?

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

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. "Near bombed" is 1 for municipalities adjacent to cities bombed by Allied tactical air attacks during WWII. The control group is composed of 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: Differences in Levels in 1991, Alternative Sample

	Layer 1 vs. layer 2 (1)	Mean (2)	Std. dev. (3)		Layer 1 vs. layer 2 (1)	Mean (2)	Std. dev. (3)
				Panel A: Age of buildings and change in fiscal federalism			
Share of pre-WWII buildings	-3.479*** (0.732)	39	19	Δ Rev. local tax (94-90)	0.821** (0.355)	10	8
				Panel B: Population and industrial census			
Population	452.793* (269.691)	4789	7608	Employed—men	112.348* (63.900)	1172	1782
Population—women	236.189* (138.167)	2446	3901	Econ. active pop.—men	117.932* (69.274)	1238	1899
Population—men	216.604 (131.580)	2343	3707	Econ. inactive pop.—men	91.736 (56.716)	1019	1610
Share women	0.094* (0.050)	51	1	Pupils in nursery schools	0.860 (0.903)	13	27
Foreign residents	0.575 (1.974)	25	47	Firms	31.732* (16.901)	299	432
Population > 65 years old	66.350* (34.174)	692	967	Firms with $\leq$ 2 employees	20.478* (11.190)	202	282
Births	0.569 (0.488)	8	15	Firms with $\geq$ 200 employees	-0.022 (0.017)	0.06	0.35
Buildings	156.213* (87.110)	1631	2509	Employees	71.677 (92.509)	1279	2206
Gender gap in empl.	48.971 (29.595)	539	832	Agricultural workers	-1.992 (1.307)	9	34
Employed—women	63.377* (36.071)	633	1019	Manufacturing workers	23.760 (45.277)	646	1183
Econ. active pop.—women	68.427* (39.534)	682	1094	Retails workers	14.182* (7.189)	129	172
Econ. inactive pop.—women	162.488* (94.179)	1680	2730	Real estate workers	35.487 (21.935)	266	514
				Panel C: Balance sheets of Italian municipalities			
Rev. from local taxes (%)	0.695*** (0.245)	13	6	Rev. from local services (%)	0.087 (0.347)	11	9
Rev. from gov. transfers (%)	0.207 (0.438)	36	11	Capital revenues (%)	-0.468 (0.494)	18	14
Rev. from loan servicing (%)	-0.769* (0.448)	13	12	Current spending (%)	0.947* (0.520)	54	15
Capital spending (%)	-1.193* (0.637)	31	17	Loan payments (%)	0.030 (0.163)	6	5
Rev. from local taxes (cap.)	4.858 (3.565)	160	91	Ratio of rev. and spend.	0.002 (0.002)	1	0.05

Notes: This table shows differences in the levels of observables characteristics measured in 1991. All 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. "Layer 2" is composed of municipalities located between 10km and 20 km from bombed locations. 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. Standard errors clustered at the province level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table A8: 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**		0.017		-8.999		-4.958	
	(0.085)		(0.218)		(16.559)		(16.597)	
Layer 1 x 1991		0.280*		0.763*		25.175		23.501
		(0.143)		(0.402)		(32.811)		(32.669)
Layer 1 x 1992		0.353**		0.041		-17.378		-9.399
		(0.169)		(0.436)		(33.152)		(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		0.578		0.738		0.266	
	(1.752)		(1.670)		(0.842)		(1.040)	
Layer 1 x 1991		11.437		5.777		7.381		2.659
		(17.522)		(16.696)		(8.419)		(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		0.049		-0.071		0.000	
	(3.508)		(0.330)		(0.238)		(0.001)	
Layer 1 x 1991		35.408		0.489		-0.714		0.003
		(35.076)		(3.304)		(2.379)		(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 composed of municipalities located between 10km and 20 km from 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 A9: Effects on Municipal Spending, Additional Results

| | Region-year fixed effects | | | Province-year fixed effects | | | | |
	Near bombed	Obs.	R^2	Near bombed	Obs.	R^2	Mean outcome	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Share of total spending for local services							
Administrative tasks	-1.210***	28,244	0.318	-1.539***	28,161	0.382	41.28	11.02
	(0.442)			(0.475)				
Judicial system	-0.003	28,248	0.080	-0.009	28,165	0.129	0.06	0.25
	(0.010)			(0.010)				
Police	0.355***	28,248	0.209	0.432***	28,165	0.271	4.26	2.91
	(0.126)			(0.133)				
Education	0.342	28,246	0.198	0.563**	28,163	0.312	10.23	5.06
	(0.256)			(0.248)				
Culture	0.119	28,248	0.219	0.135*	28,165	0.286	1.89	1.93
	(0.073)			(0.071)				
Sports	0.196***	28,248	0.150	0.188***	28,165	0.218	1.54	1.45
	(0.061)			(0.062)				
Tourism	-0.062	28,248	0.096	-0.100*	28,165	0.153	0.66	1.27
	(0.055)			(0.060)				
Transport system	-0.534**	28,247	0.212	-0.563***	28,164	0.303	9.54	4.58
	(0.206)			(0.203)				
Public health	0.063	28,246	0.250	0.487	28,163	0.395	18.75	7.4
	(0.325)			(0.302)				
Welfare	0.922***	28,248	0.321	0.809***	28,165	0.386	9.19	7.37
	(0.269)			(0.255)				
Local econ. development	0.094***	28,248	0.098	0.045	28,165	0.169	0.45	0.86
	(0.031)			(0.035)				

Notes: This table shows differences in spending for publicly provided services, using variables from balance sheets that are available between 1998 and 2010. "Near bombed" is 1 for municipalities adjacent to cities bombed by Allied tactical air attacks during WWII. The control group is composed of 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 A10: Effects of Fiscal Decentralization on Local Labor Markets, More Variables

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

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 composed of municipalities adjacent to cities matched to bombed locations. "Post" is 1 starting in 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 A11: Effects of Fiscal Decentralization, Instrumental Variables

	Region-year fixed effects				Province-year fixed effects							
	Δ Local taxes x Post	Obs.	R^2	F stat.	Δ Local taxes x Post	Obs.	R^2	F stat.	Mean outcome	Std. Dev.	Mean treatment	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Employed	47.282** (21.656)	8,750	0.979	12.97	61.765** (27.161)	8,726	0.978	14.46	1363.89	2124.2	9.04	7.76
Econ. active pop.	67.628** (31.424)	8,750	0.968	12.97	90.361** (35.464)	8,726	0.967	14.46	1457.32	2274.97	9.04	7.76
Econ. inactive pop.	-85.337** (35.569)	8,750	0.975	12.97	-81.507** (37.340)	8,726	0.978	14.46	2292.02	3661.23	9.04	7.76
Gender gap in employment	-27.944** (11.712)	8,750	0.931	12.97	-20.692* (10.859)	8,726	0.945	14.46	455.47	720.13	9.04	7.76
Stay-at-home women	-29.407** (12.940)	8,750	0.961	12.97	-22.865* (12.693)	8,726	0.968	14.46	607.65	1159.16	9.04	7.76
Employed—women	37.613*** (12.461)	8,750	0.952	12.97	41.229*** (12.115)	8,726	0.954	14.46	454.21	743.21	9.04	7.76
Econ. active pop.—women	49.550*** (18.119)	8,750	0.929	12.97	57.654*** (18.611)	8,726	0.930	14.46	491.72	798.39	9.04	7.76
Econ. inactive pop.—women	-54.151** (22.538)	8,750	0.975	12.97	-50.320** (23.713)	8,726	0.978	14.46	1422.59	2300.03	9.04	7.76
Pupils in nursery schools	1.498** (0.707)	6,567	0.863	12.61	1.512** (0.651)	6,549	0.881	11.9	10.59	21.46	9.04	7.76
Births	0.481* (0.263)	4,378	0.924	12.29	0.797** (0.378)	4,366	0.902	11.61	6.55	12.71	9.04	7.76
Firms with ≤ 2 employees	11.822* (6.738)	8,755	0.911	13.25	16.937*** (6.238)	8,731	0.917	14.79	165.33	230.04	9.04	7.76
Agricultural workers	-2.114** (1.003)	8,755	0.646	13.25	-1.222 (1.041)	8,731	0.702	14.79	8.93	42.01	9.04	7.76
Manufacturing workers	-33.342** (16.317)	8,755	0.917	13.25	-34.620** (16.182)	8,731	0.921	14.79	430.23	946.5	9.04	7.76

Notes: "Δ Local taxes" measures the change in the share of revenues from local taxes measured between 1990 and 1994. "Post" is 1 starting in 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 composed of 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 A12: Political Competition, Municipal Competition, and Local Preferences

	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 composed of municipalities adjacent to cities matched to bombed locations. "Close race" is a dummy equal to 1 when the closest election had a victory margin within 10 percentage points. "Runoff" is a dummy equal to 1 when the closest election had a runoff. When the dependent variables come from the decennial censuses, "Close race" is 1 if the average victory margin after 1993 is within 10 percentage points and "Runoff" measures the total number of runoffs after 1993. In addition, this table controls for other possible mechanisms through which fiscal federalism could have operated: competition across municipalities and better knowledge of local politicians about local preferences towards local services. "Adjacent cities" is the number of adjacent municipalities. "Below €15,000" is the share of income earners with yearly taxable income below €15,000. Panel A uses dependent variables that describe the provision and utilization of publicly provided local services. Some variables are available 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/.

Table A13: Effects on Political Participation

| | Effects of 1 p.p. increase in Δ Rev. local tax (94-90) | | | | |
| | Mean | Median | Obs. | Mean dep. var. | Std. dev. dep. var. |
	(1)	(2)	(3)	(4)	(5)
High interest in politics	0.011***	0.010***	6,058	0.34	0.47
	(0.003)	(0.003)			
Voted	0.007***	0.007***	6,058	0.75	0.43
	(0.002)	(0.001)			
Shown badges	0.004***	0.004***	6,058	0.08	0.26
	(0.001)	(0.001)			
Signed petition	0.008**	0.008***	6,058	0.16	0.37
	(0.003)	(0.002)			
Local preference	0.015**	0.014**	1,162	0.28	0.45
	(0.006)	(0.006)			
Discuss politics often	0.025***	0.023***	1,162	0.50	0.50
	(0.006)	(0.006)			
Voting important	0.012***	0.011***	1,162	0.81	0.39
	(0.004)	(0.004)			
Participation important	0.005	0.005	1,162	0.32	0.47
	(0.003)	(0.003)			
Mean Δ Rev. local tax (94-90)	9.64	8.90			
Std. dev. Δ Rev. local tax (94-90)	2.83	3.08			

Notes: Data on political participation come from the European Social Survey (ESS), available online at https://www.europeansocialsurvey.org/data/country.html?c=italy. Out of all waves with Italian data (2002, 2004, 2012, 2016, 2018), we drop the 2018 wave because it does not contain information about the respondents' region of residence. The resulting dataset has 6,058 observations. Each cell in columns 1 and 2 shows the main coefficient from a separate regression. Specifically, we regress several measures of political participation (on the left) on either the mean (column 1) or median (column 2) difference in the share of municipal revenues from local taxes between 1990 and 1994 in the respondents' region of residence, a measure of short-term exposure to fiscal decentralization. We need to aggregate the effect of the policy at the regional level because the ESS dataset does not have information on the municipality or province of residence. The regressions also include fixed effects for gender, years of completed education, survey year, citizenship status, and paternal country of birth. *High interest in politics* is 1 for respondents who are very or quite interested in politics (var. *polintr*). *Voted* is 1 for respondents who voted in the last national election (var. *vote*). *Shown badges* is 1 for respondents who worn or displayed a campaign badge/stick in the last 12 months (var. *badge*). *Signed petition* is 1 for respondents who signed a petition in the last 12 months (var. *sgnptit*). *Local preference* is 1 for respondents whose preferred decision level of social welfare policies is regional or local (var. *dclwlfr*). *Discuss politics often* is 1 for respondents who discuss politics/current affairs at least several times a month (var. *discpol*). *Voting important* is 1 for respondents who think that voting in an election has an importance level of at least 6 on a scale from 0 (extremely unimportant) to 10 (extremely important) to be considered a good citizen (var. *impvote*). *Participation important* is 1 for respondents who think that actively participating to politics has an importance level of at least 6 on a scale from 0 (extremely unimportant) to 10 (extremely important) to be considered a good citizen (var. *impapol*). The last four dependent variables are only available in the first ESS wave (2002). Standard errors clustered at the region level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table A14: Effects of Fiscal Decentralization, Employees of Privately Owned Firms

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

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

Table A15: Robustness checks, Employees of Privately Owned Firms

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

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

Table A16: Quadruple Interactions, Employees of Privately Owned Firms

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

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

Table A17: Including Controls for Postwar Reconstruction

	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: Baseline results			
Near bombed x Post	1.245***	-39.137***	50.870***	67.765***	-74.593***	2.475***
	(0.335)	(12.635)	(16.145)	(24.907)	(27.056)	(0.746)
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: Including total Marshall Plan aid			
Near bombed x Post	1.355***	-38.374***	55.252***	81.416***	-93.449***	2.262***
	(0.363)	(14.080)	(18.167)	(30.872)	(35.705)	(0.851)
MP aid x Post	-0.000	-0.000	-0.000	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
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
MP aid—mean	147,004	148,443	148,443	148,443	148,443	148,382
MP aid—std. dev.	100,945	101,813	101,813	101,813	101,813	101,837
		Panel C: Including Marshall Plan aid for reconstruction of public and private buildings				
Near bombed x Post	1.226***	-37.372***	48.528***	64.867***	-72.607***	2.281***
	(0.334)	(12.276)	(15.881)	(24.599)	(26.580)	(0.719)
MP aid x Post	0.000	-0.007	0.009***	0.011**	-0.007	0.001***
	(0.000)	(0.004)	(0.003)	(0.005)	(0.006)	(0.000)
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	0.880
MP aid—mean	2,248	2,493	2,493	2,493	2,493	2,492
MP aid—std. dev.	4,041	4,302	4,302	4,302	4,302	4,302
		Panel D: Including Marshall Plan aid for reconstruction of private buildings				
Near bombed x Post	1.226***	-37.565***	48.731***	65.130***	-72.971***	2.303***
	(0.333)	(12.256)	(15.895)	(24.610)	(26.595)	(0.721)
MP aid x Post	0.000	-0.021	0.028***	0.035**	-0.021	0.002***
	(0.000)	(0.013)	(0.009)	(0.015)	(0.020)	(0.001)
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	0.880
MP aid—mean	689	763	763	763	763	763
MP aid—std. dev.	1,212	1,289	1,289	1,289	1,289	1,289

Notes: In panel B, regressions include the total amount of aid received by a province through the Marshall Plan. Aid is aggregated at the province level because none of the municipalities in the sample (near-bombed and near-others) directly received grants. In panel C, regressions include the amount of aid received by a province through the Marshall Plan to reconstruct public and private buildings. In panel D, regressions include the amount of aid received by a province through the Marshall Plan to reconstruct only private buildings. All measures of Marshall Plan aid are expressed in hundreds of 2010 USD. 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 A18: Effects of Fiscal Decentralization on Labor Markets, Alternative Sample

	Layer 1 is within 10km of bombed location					Layer 1 is within 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)
Panel A: Balance sheets of Italian municipalities										
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
Panel B: Population and industrial census										
Employed	46.980** (22.751)	14,786	0.984	1692.74	2480.6	91.770*** (29.241)	17,290	0.984	1622.37	2387.05
Econ. active pop.	71.449** (28.360)	14,786	0.978	1785.58	2623.54	122.134*** (42.130)	17,290	0.978	1716.16	2530.12
Econ. inactive pop.	-79.680** (33.953)	14,786	0.980	2645.33	4005.83	-95.030*** (36.495)	17,290	0.981	2580.6	3910.15
Gender gap in employment	-17.838 (11.846)	14,786	0.942	546.97	791.34	-24.100** (11.748)	17,288	0.945	531.02	773.96
Stay-at-home women	-28.980** (12.824)	14,786	0.966	719.48	1246.09	-35.748** (14.496)	17,290	0.967	698.83	1218.95
Employed—women	32.409** (13.502)	14,786	0.972	572.88	889.42	58.007*** (14.160)	17,288	0.971	545.8	850.05
Econ. active pop.—women	46.241*** (17.117)	14,786	0.960	612.88	948.13	75.438*** (20.862)	17,288	0.959	585.86	907.95
Econ. inactive pop.—women	-52.368** (21.950)	14,786	0.979	1648.44	2515.8	-60.479** (23.872)	17,288	0.980	1606.51	2455.07
Pupils in nursery school	1.561** (0.722)	11,102	0.909	13.02	25.22	2.628*** (0.706)	12,980	0.907	12.48	24.25
Births	0.360* (0.212)	7,398	0.949	7.56	13.74	0.367 (0.233)	8,650	0.950	7.39	13.49
Firms with ≤ 2 employees	15.317* (8.964)	14,803	0.920	194.17	263.55	32.217*** (7.955)	17,307	0.921	189.16	261

Notes: "Layer" is 1 for municipalities that are located within 10km (column 1) or within 15km (column 6) of cities bombed by Allied tactical air attacks during WWII. The control group is composed of municipalities located between 10km and 20 km (column 1) or between 15km and 30km (column 6) from bombed locations. "Post" is 1 starting in 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 A19: Effects of Fiscal Decentralization on Labor Markets, Matching Layer 1

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

Notes: "Near bombed" is 1 for municipalities adjacent to cities bombed by Allied tactical air attacks during WWII. The control group is composed of 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 A20: Robustness of Matching Process

	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: Matching stratified by Italian macro-regions (NUTS 1)			
			1. Regressions include region-year fixed effects			
Near bombed x Post	1.208***	-34.402**	46.325***	66.152***	-80.572***	2.321***
	(0.363)	(14.252)	(15.961)	(23.986)	(30.761)	(0.686)
Observations	43,478	8,941	8,941	8,941	8,941	6,720
			2. Regressions include province-year fixed effects			
Near bombed x Post	1.110***	-30.098**	50.996***	72.936***	-84.521***	2.008***
	(0.380)	(13.816)	(15.414)	(23.165)	(30.183)	(0.617)
Observations	43,356	8,917	8,917	8,917	8,917	6,702
			Panel B: Matching includes more controls			
			1. Regressions include region-year fixed effects			
Near bombed x Post	1.095***	-27.036**	45.234***	67.177***	-58.362**	2.419***
	(0.312)	(12.693)	(16.776)	(25.371)	(28.898)	(0.738)
Observations	47,222	9,711	9,711	9,711	9,711	7,302
			2. Regressions include province-year fixed effects			
Near bombed x Post	1.059***	-28.164**	58.640***	83.460***	-72.643**	2.646***
	(0.329)	(12.692)	(15.720)	(24.119)	(28.262)	(0.775)
Observations	47,113	9,691	9,691	9,691	9,691	7,287
			Panel C: Matching includes province fixed effects			
			1. Regressions include region-year fixed effects			
Near bombed x Post	1.092***	-40.095**	41.690**	62.663**	-88.558**	1.416
	(0.358)	(18.244)	(18.725)	(28.482)	(35.282)	(0.919)
Observations	39,547	8,121	8,121	8,121	8,121	6,102
			2. Regressions include province-year fixed effects			
Near bombed x Post	0.873***	-44.923**	38.567**	53.661**	-86.793***	1.642*
	(0.329)	(18.922)	(15.591)	(23.135)	(32.426)	(0.837)
Observations	39,435	8,101	8,101	8,101	8,101	6,087

Notes: This table shows the robustness of the main results to variations of the main matching process between bombed and nonbombed locations (Section 3.2). In panel A, bombed and nonbombed locations are matched by first stratifying by Italian macro-regions or Nomenclature of Territorial Units for Statistics (NUTS) 1 (Northeast, Northwest, Center, South, Islands). In other words, the propensity score matching uses the same variables outlined in the main text, but bombed and nonbombed locations are allowed to match only if they are in the same macro-region. In panel B, the matching process uses many more predetermined variables relative to the baseline matching. Specifically, in addition to all the variables listed in the main text, the propensity score matching uses the following municipality-level characteristics from the 1991 census: a dummy for coastal locations, a dummy for rural locations, share of female population, foreign-born population, share of population over 65, births, pupils in nursery schools, economically active residents, number of firms, number of employees, share of agricultural firms, share of manufacturing firms, share of retail firms, share of agricultural workers, share of manufacturing workers, share of retail workers. In panel C, the matching process includes the baseline variables outlined in the main text, but it replaces region fixed effects with more disaggregated province fixed effects. For each of these alternative matching processes, the table shows the result of regressions comparing several economic outcomes between bombing-adjacent and bombing-distant municipalities (Equation (1)). For each matching, we present results of regressions with either regression-year fixed effects or province-year fixed effects. All regressions also include municipality fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A21: Other 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***	-39.137**	50.870***	67.765***	-74.593**	2.475***
	(0.365)	(16.269)	(16.913)	(24.258)	(28.948)	(0.859)
Observations	47,255	9,686	9,686	9,686	9,686	7,277
Panel B: Spatial HAC standard errors						
Near bombed x Post	1.245***	-39.137***	50.870***	67.765***	-74.593***	2.475***
	(0.225)	(9.749)	(9.748)	(13.574)	(13.062)	(0.771)
Observations	47,256	9,687	9,687	9,687	9,687	7,273
Panel C: Controls for population						
Near bombed x Post	1.117**	-45.440***	35.101***	44.143***	-86.514***	1.659***
	(0.478)	(13.010)	(9.619)	(14.674)	(28.591)	(0.548)
Observations	4,218	9,686	9,686	9,686	9,686	7,271
Panel D: Dependent variables are shares of residents (col. 3-6)						
Near bombed x Post			10.020***	10.938***	-0.722**	1.839***
			(3.031)	(3.233)	(0.367)	(0.662)
Observations			9,686	9,686	9,686	7,228
Panel E: Controls for nonlinear trends correlated with geographical characteristics						
Near bombed x Post	1.064***	-44.837***	56.851***	79.929***	-91.753***	2.725***
	(0.270)	(16.838)	(14.554)	(23.508)	(29.877)	(0.765)
Observations	46,682	9,588	9,588	9,588	9,588	7,197
Panel F: Controls for nonlinear trends correlated with real-estate market						
Near bombed x Post	0.941***	-47.542**	50.059***	66.927***	-90.478***	2.275***
	(0.296)	(18.833)	(16.130)	(24.325)	(34.127)	(0.833)
Observations	47,112	9,677	9,677	9,677	9,677	7,268
Panel G: End sample in 2001 before introduction of other local taxes						
Near bombed x Post	1.074***	-28.636***	37.790***	52.387**	-71.211***	1.152**
	(0.299)	(10.220)	(12.056)	(20.853)	(26.827)	(0.491)
Observations	27,148	7,260	7,260	7,260	7,260	4,846
Panel H: Exclude special-administration regions						
Near bombed x Post	1.372***	-41.506***	39.981**	55.611**	-82.560***	2.258***
	(0.374)	(14.358)	(17.364)	(26.719)	(29.939)	(0.795)
Observations	40,333	8,238	8,238	8,238	8,238	6,184
Panel I: Placebo reform						
Near bombed x Post	0.105	-1.144	6.485	9.352	-8.787	0.551
	(0.229)	(12.338)	(10.701)	(16.942)	(24.480)	(0.635)
Observations	47,153	9,686	9,686	9,686	9,686	7,277

Notes: In panel B, the estimates show spatial HAC standard errors (Conley, 1999). Spatial HAC standard errors correct for spatial correlation among municipalities that are within 1,000km of each other and for autocorrelation for up to 20 years. In panel C, regressions include population as a control. In panel E, regressions include geographical variables (population density, a dummy for rural municipalities, a dummy for coastal cities) interacted with year fixed effects. In panel F, regressions include variables describing the real-estate market (average size of residential buildings, share of high-quality buildings as defined in Table A2) interacted with year fixed effects. Panel H excludes from the sample five special-administration regions (Valle d'Aosta, Trentino, Friuli, Sicilia, and Sardegna) that enjoy more autonomy. Panel I 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.

B   Fiscal Decentralization, Public Services, and Labor Markets

The literature on fiscal decentralization is vast and dates back several decades (Musgrave, 1959; Oates, 1972).[51]

One group of theoretical and empirical papers concludes that fiscal decentralization can improve local services. Local taxes, for example, can raise the accountability of local administrators because they make it easier for residents to monitor their elected officials (Fisman and Gatti, 2002; de Mello and Barenstein, 2001). This increased monitoring can happen through different channels. First, decentralization can increase the saliency of local taxes, allowing residents to more accurately assess how much they pay for local services. Second, in the case of a fiscal deficit, a decentralized system would force administrators to raise more funds directly from their residents, instead of asking higher levels of government for more resources. Raising local tax rates without improving the quality of services could be a clear signal of bad management. In addition to increased accountability, fiscal decentralization can raise the level of competition for new residents between municipalities, leading to a more efficient provision of publicly provided services (Seabright, 1996; Hatfield and Kosec, 2013).[52] Finally, local politicians are likely to have better information regarding local preferences towards public services than the central government does (Hayek, 1945).

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

This paper does not intend to test the validity of individual theories. Instead, it contributes to the literature by analyzing a reform that has advantageous features for the identification strategy. Moreover, it is one of the first papers to follow the effect of fiscal decentralization on local labor markets.

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

[52] This idea is incorporated into the Tiebout model, in which individuals can "vote with their feet" (Tiebout, 1956).