The Effects of Fiscal Decentralization on Publicly Provided Services and Labor Markets[*]

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Abstract

This paper studies how fiscal decentralization affects local services. It explores a 1993 reform that 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 differences in the average age of buildings caused by bombings during WWII. Decentralization reduced local spending but expanded municipal services, such as nursery schools. 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.

JEL Classification: H71, H75, J20, H77, I21.
Keywords: fiscal decentralization, female labor supply, childcare, nursery schools, local property tax.

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 (Fisman and Gatti, 2002). It could also raise efficiency by fostering competition between local governments to attract new residents (Hatfield and Kosec, 2013). On the other hand, decentralization could make it easier for interest groups to lobby local politicians, potentially increasing corruption and inequality (Bardhan and Mookherjee, 2000). Moreover, heightened local competition could induce politicians to cut services in order to decrease taxes and attract mobile capital (Zodrow and Mieszkowski, 1986).

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 decentralization of Italian municipalities by replacing central government grants with revenues from a newly established local property tax (LPT). 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, 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-LPT 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-LPT system, individuals paid the LPT 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 LPT liability, a fact that stems directly from the formula for computing the LPT bill. As a result, all else equal, municipalities with older properties raised lower revenues from the LPT 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 LPT 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 LPT.

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 LPT 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

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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.

responses.[2] Despite the lower budget, there is no evidence of a reduction in the quality of 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 LPT 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 LPT experienced a larger increase in female participation in the labor market. In these locations, women's labor-force participation increased by up to 20 percent, leading to a 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 LPT. 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 allow 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 of young children. Third, the availability of yearly observations between 1974 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 LPT 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.

Finally, we analyze the motivations that might have induced administrators to expand the provision of local services after the LPT. We find two main pieces of evidence. First, higher political competition, measured by higher turnover of mayors, is associated with more local services and a greater increase in female employment after the LPT. Second, higher exposure to fiscal decentralization is associated with higher levels of political participation. Other

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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.

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.

Within this literature, prior works have proposed several mechanisms through which fiscal decentralization can improve the delivery of local services. One of the main hypotheses is that fiscal decentralization increases 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 induces administrators to cut waste and increase access to local public services (Fisman and Gatti, 2002). In addition to increased accountability, fiscal decentralization can lead to better public services by raising the level of competition for new residents between municipalities (Seabright, 1996; Hatfield and Kosec, 2013).[5] Finally, local politicians are likely to have better information regarding local preferences towards public services than the central government does (Hayek, 1945). This paper contributes to this branch of the literature by testing the importance of these three mechanisms in the Italian context. Our findings are consistent with the hypothesis that fiscal decentralization can improve local public services through better monitoring, but only when it is paired with a sufficiently high level of political competition.

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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).

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

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 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.[6] As an initial test, the central government established a one-time LPT to be collected only in 1992. The tax revenues went to municipalities, even though local governments could not choose the tax rate. Starting in 1993, the LPT became permanent. At this time, local governments could set their preferred tax rate between 0.4 percent and 0.7 percent. 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 LPT replaced one lira of government transfers.[7] Moreover, implementing the new LPT was not optional for local administrators. Absent a specific municipal ordinance on the new LPT, 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).[8] 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

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

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

[8] 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 LPT on their main residence.

other levels of government decreased from €564 per resident in 1990 to €438 per resident in 1994.[9] 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 LPT was the driving force behind this shift. From 1998 onwards, the first year in which more detailed data from the municipal balance sheets are available, LPT revenues alone accounted for 57 percent of total revenues from local taxes (Table 1, Panel B).[10]

One crucial and unusual feature that affected the implementation of the LPT 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 LPT 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 LPT 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 LPT separately from their PIT, often twice a year (June and December), and directly to their municipality.[11] Owners paid an average of €300 for their main residence and €335 for a second property, if any (Table 1, Panel A). This average yearly LPT 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).[12] In short, the implementation of the LPT was salient to residents due to both its amount and its separate payment method.[13]

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[9] Total revenues decreased by only 4.6 percent.

[10] Although the balance sheets become more detailed after 1998, we have key information of spending and revenues starting from 1990.

[11] The LPT is paid only by property owners. However, renters may pay it indirectly if the LPT is at least partially passed onto tenants through higher rents.

[12] 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).

[13] The 1948 Italian Constitution created a highly centralized government. Therefore, the saliency of the new LPT stemmed also from the fact that it was the first major reform in the history of the Italian republic in

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.[14]

First, we constructed a panel dataset with yearly financial information on each municipality (Table 1, Panel B). 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). 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.[15] 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 1951 to 2011 (Table 1, Panel C). The resulting dataset has five pre-LPT (1951, 1961, 1971, 1981, and 1991) and two post-LPT observations (2001 and 2011). We use these data to study how participation in the labor market changed after the introduction of the LPT. Using means

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which revenue-generating authority was decentralized to local governments.

[14] 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.

[15] 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).

alone reveal that the number of employed women in each municipality increased by an average of 61 percent after the LPT, while the number of employed men stayed fairly constant.

Third, we leverage administrative data provided by the Italian Social Security Institute (INPS) covering every year between 1974 and 2011 (Table 1, Panel D). This dataset consists of information on all employees of private-sector nonagricultural firms with at least one salaried worker. We use this dataset to study the effect of fiscal decentralization on different types of employees within a municipality. To this end, the observations are aggregated by age, gender, municipality of residence, and year.[16] In this dataset, we observe workers entering the labor market for the first time or reentering it after a break in employment.[17] For each combination of gender, age, municipality, and calendar year, the average number of new entrants is equal to 0.52 individuals and the average number of reentrants is equal to 0.54 individuals. 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 225. Conditional on gender, age, municipality, and year, the average number of individuals working outside their province of residence is 2.11.

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 LPT. 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 (Figure 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 LPT. Simply examining how the provision of municipal services and labor-market outcomes changed across cities in which the share of revenues from local

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[16]This aggregation has two purposes. First, it aggregates the data at the same level of variation used by the empirical analysis in Section 4.6. 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.

[17]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.

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 LPT revenues that is plausibly exogenous with respect to other drivers of public services.

To do so, we consider the formula for the individual LPT liability: LPT 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.[18]

In the Italian context, the cadastral values are negatively correlated with the age of buildings. Specifically, a 13-percent increase in the share of buildings constructed before WWII, which is equal to moving from the 25th percentile to the median in the distribution of prewar buildings, decreases the average cadastral value by €47, or 13 percent from the mean (Table A2, Panel A, column 1). 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 13-percent increase in the share of pre-WWII buildings, in fact, decreases the post-LPT per-capita revenues from local taxes by €14, or 11 percent from the mean (Table A2, Panel B, column 1).[19] 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

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[18]In Section 4.3, we will further analyze the endogenous response in the tax rates.

[19]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).

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 them in a way that would trigger a reevaluation of their cadastral values.[20]

3.2 Allied Bombings as a Source of Variation

So far, our analysis has shown that a larger share of older buildings in a municipality is associated with lower cadastral values and therefore lower exposure to fiscal decentralization after 1993. In this section, we show that Allied bombings during WWII generated variation in the cadastral values.

Bombing in Italy can be divided into two periods: before and after the Italian armistice with the Allied forces in 1943. 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.[21] 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.[22]

To be a good source of variation for fiscal decentralization, we should expect post-armistice bombings at the municipal level to (i) be positively related with the increase in revenues from local taxes after 1993 and (ii) to positively affect municipal outcomes but only starting after 1993. The data confirm both hypotheses. First, we regress the share of pre-WWII buildings in 1991 on the inverse hyperbolic sine transformation (IHST) of the tons of

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

[21]All the findings described in this paragraph are included in Bianchi and Giorcelli (2019).

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

post-armistice Allied bombings dropped on each municipality (Table A3, Panel A, columns 1 to 3).[23] As expected, more bombings significantly reduced the share of prewar buildings, therefore reducing the average age of the municipal stock of real estate. This correlation is statistically and economically significant. A threefold increase in the tons of post-armistice bombings, which is equivalent to moving from the 25th percentile to the median or from the median to the 75th percentile in the distribution of bombings, is associated with 4-percentage-points fewer prewar buildings, a 10-percent increase from the mean (column 2). As pointed out in Section 3.1, younger buildings are associated with higher cadastral values and, therefore, a larger stream of revenues from the local property tax after 1993. We further prove this point by regressing the change in per-capita revenues from local taxes between 1990 and 1994 on the IHST of the tons of post-armistice bombings. An increase in bombings—and, therefore, a decrease in the age of buildings—is associated with a significant increase in revenues from local taxes after 1993 (Table A3, Panel A, columns 4 to 6). Specifically, a threefold increase in bombings is associated with a €13-increase in per-capita revenues from local taxes, a 10-percent increase from the mean (column 5).

Second, we show that more WWII bombings are associated with better labor-market outcomes for women and that these improvements materialized only after the introduction of fiscal decentralization. We start with reduced-form specifications, as follows:

$$y_{mt} = \alpha_m + \gamma_{rt} + \sum_t \delta_t \text{IHST post-armistice bombs}_m \times \text{Year}_t + \varepsilon_{mt}, \tag{1}$$

where y_{mt} measures female labor-force participation in municipality m and year t and IHST post-armistice bombs$_m$ is the IHST of post-armistice Allied bombings in municipality m. The variables Year$_t$ are year fixed effects. In some specification, they can be replaced by Post$_t$, which is a single dummy equal to 1 after the introduction of the LPT in 1993. Municipality fixed effects (α_m) capture permanent differences across cities. Finally, γ_{rt} includes several nonlinear trends that are correlated with observable characteristics: year fixed effects are interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities, as well as fixed effects for either region r or province p.[24] Standard errors are clustered at the city level.

If we start from simpler pre-post comparisons, we find that more bombings are associated with more women participating in the labor force and with more women working. Specifically,

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[23]The inverse hyperbolic sine function retains the same interpretation of the log transformation, but is defined for zero values.

[24]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. Two examples of such a shock are the devaluation of the lira (Macis and Schivardi, 2016) and the "Clean Hands" political scandal.

a threefold increase in bombings is associated with 35 more women in the labor force after 1993 (Table A3, Panel B, column 1), a 7-percent increase from the mean, and with 41 more employed women after 1993 (Table A3, Panel C, column 1), a 9-percent increase from the mean. As we will explore in more depth in Section 4.1, our main hypothesis is that these effects are driven by the decision of municipalities to improve local services and add seats to their severely constrained public nursery schools. Consistent with this theory, we observe that an increase in bombings leads to a significant increase in the number of children attending nursery schools: for a threefold increase in bombings, nursery-school attendance increased by 1.5 pupils or by 12.5 percent from the mean (Table A3, Panel D, column 1).

After showing pre-post comparisons, we move to full event studies (Figure A3, Panels A to C). These graphs confirm that women's labor-market outcomes improved more after 1993 in municipalities that received more post-armistice Allied bombings. Moreover, they show that differences in Allied bombings did not significantly affect female labor-force participation *before* 1993. In other words, cross-municipal differences in bombings are associated with different trends in female occupation only after the introduction of fiscal decentralization, which happened forty-eight years after the end of WWII. Therefore, these findings corroborate the hypothesis that different levels of post-armistice bombings were not correlated with other omitted factors that may have affected women's participation in the labor force during the postwar era.

Finally, we modify Equation (1) by replacing IHST post-armistice bombs$_m$ with ΔRev. local tax $(90\text{-}94)_m$, which measures the change in per-capita revenues from local taxes between 1990 and 1994 in municipality m. Moreover, we instrument this new variable with IHST post-armistice bombs$_m$ to keep leveraging cross-municipal differences in post-armistice bombings. This new specification allows us to establish a direct link between the effects of fiscal decentralization on municipal revenues and the changes in the labor market. Consistent with the estimation of Equation (1), municipalities with a larger increase in revenues from local taxes after 1993 experienced more entry of women in the labor market and more pupils in nursery school, although the coefficients tend to be slightly less precise than the reduced-form estimates in Equation (1) (Figure A3, Panels D to F; Table A3, Panels B to D, columns 3 to 6). Moreover, the positive effects start only after the implementation of fiscal decentralization.

3.3 A Matching Algorithm to Build the Estimating Sample

Section 3.2 showed that the Allied post-armistice bombings were indeed connected to the municipal degree of fiscal decentralization after 1993. However, there are several reasons why the specification in Equation (1) can be improved. First, out of 8,092 Italian municipalities in

1991, only 671 were directly targeted by Allied bombings after the Armistice of Cassibile. In other words, Equation (1) creates a control group of nonbombed municipalities that is much larger than the treatment group of bombed municipalities. Due to its larger size, the control group is therefore bound to be more heterogeneous than the treatment group. Second, even though municipalities with different levels of bombings experienced parallel trends in female labor-force participation prior to 1993 (Figure A3, Panels A and B), there is the lingering concern that bombed cities were explicitly targeted by the Allied forces. This fact may imply that targeted cities differed from nontargeted cities based on unobservable characteristics that were possibly connected to the implementation of fiscal decentralization. Third, the main treatment variable in Equation (1) is likely to be affected by measurement error. 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). The consequences on the data are twofold. On the one hand, many nontargeted municipalities that are adjacent to targeted locations were actually bombed, even though they were not included in the official list of targets.[25] On the other hand, targeted municipalities may have been hit by a lower tonnage of explosives, compared with the official numbers collected by the Allied air forces.

We start addressing these points by first matching 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 A4, column 1). However, in Section 4.7, we show that the results are highly robust to alternative implementations of the matching algorithm. In the resulting subsample of 314 bombed and 314 matched municipalities, the share of pre-WWII buildings is 5 percentage points lower in bombed locations, and the post-LPT change in per-capita revenues from local taxes is €4 higher, although the second coefficient is imprecisely estimated (Table A5, Panel A, column 1). Other observable characteristics are generally balanced between bombed and matched locations (Table A5, Panel B, column 1).[26]

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[25]In addition to the reports of military historians, there are many anecdotes about bombings of wrong targets. For example, Savigno was bombed in place of Tolè, who is located just 11km to the south (https://bit.ly/2UALQeQ). Venafro was repeatedly hit during the heavy bombings of Cassino, even though it was already under Allied control (https://bit.ly/36l7iHn). Comeana was mistakenly hit in place of the adjacent city of Prato (https://bit.ly/3hruJ8d). Urbania was hit in place of Poggibonsi, losing 4 percent of its population (https://bit.ly/3qUVjtv). Finally, Allied bombers hit neutral Swiss cities more than 50 times during the war, instead of their intended Italian and German targets (https://bit.ly/3jXUMoZ).

[26]Out of the 34 variables from the population and industry censuses, only two variables are statistically

In spite of these reassuring statistics, we intend to bypass any possible remaining concern about selection into bombings by focusing on nontargeted locations only. As discussed at the beginning of this section, we exploit the fact that WWII bombings were often not precise and could hit areas around the intended target. Specifically, 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).

There are several pieces of evidence indicating that municipalities adjacent to bombed locations were indeed hit by Allied bombings, even though they were not explicitly targeted by Allied air forces and, therefore, were not included in the official reports. First, relative to the control, near-bombed locations have 2.7 percentage points fewer pre-WWII buildings (Table A5, Panel A, column 1). They also experienced a €24 higher change in per-capita revenues from local taxes after the LPT, an 18 percent increase from the mean. Second, the intensity of the Allied bombings in the bombed locations can predict the share of buildings in the adjacent municipalities that were destroyed during WWII. Specifically, a threefold increase in the tons of explosives dropped on a targeted location decreased the share of pre-WWII buildings *in the adjacent municipalities* by up to 6 percent from the mean (Table A6, Panel A). Similarly, it increased the change in per-capita revenues from local taxes after 1993 in adjacent municipalities by €12.6, a 9.5 percent increase from the mean (Table A6, Panel B). Both effects are precisely estimated.

In short, the fact that municipalities adjacent to bombed locations were inadvertently hit during WWII made them experience a higher degree of fiscal decentralization after 1993, but it is unlikely that their unintended bombings were correlated with unobservable confounding factors.

3.4   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*):

$$y_{mt} = \alpha_m + \gamma_{rt} + \sum_t \delta_t \text{Near bombed}_m \times \text{Year}_t + \varepsilon_{mt}, \qquad (2)$$

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different at the 5 percent level. Variables from balance sheets are slightly less balanced (Table A5, Panel C, column 1).

where most variables have already been defined after Equation (1). The main difference is that the treatment variable is now Near bombed$_m$, which is (i) 1 for municipalities adjacent to cities bombed by Allied air attacks after the Armistice of Cassibile and (ii) 0 for municipalities adjacent to cities matched to bombed locations. As discussed in Section 3.2, γ_{rt} includes many nonlinear trends that are correlated with observable characteristics. In addition to 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.[27] Section 4.7 includes many robustness checks on this baseline specifications.

When the dependent variable is available only after the LPT implementation, Equation (2) loses the dummies Year$_t$ 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 characteristics, such as population, altitude, and a dummy for rural cities.

3.5 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 LPT. While this assumption cannot be tested directly, we document that all outcomes followed a parallel trends for several decades before the implementation of the LPT.

We first estimate whether linear trends are systematically different between treated and control municipalities by interacting Near bombed$_m$ with a linear time trend (Table 2, Panel A). For all dependent variables, the coefficients of this interaction are insignificant and small. For example, the number of women in the labor force increased by an additional 0.06 individuals in bombing-adjacent municipalities for each pre-reform year between 1951 and 1991. This estimate is equal to an economically and statistically insignificant 0.01-percent increase from the mean. Similarly, there are not significant and differential linear trends in the number of employed women, the number of men in the labor force, and the number of employed men.

Alternatively, we estimate nonlinear pre-LPT trends by interacting the treatment variable with individual year dummies (Table 2, Panel B). In the case of the variables from the Italian census, we usually have five pre-reform observations starting in 1951 and ending in 1991. In the case of the balance-sheets data, we estimate nonlinear pre-reform trends using three observations from 1990 to 1992. The omitted year is 1991 for the census variables and

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[27]Figure A4 shows the geographical variation of these variables.

1992 for the balance-sheets data. The coefficients of the interaction between Near bombed$_m$ and the pre-reform dummies are small in magnitude and not statistically different from zero. These findings indicate that bombing-adjacent municipalities and bombing-distant municipalities experienced parallel nonlinear trends in the number of women and men in the labor force, as well as the number of women and men with jobs, for several decades before the implementation of the LPT reform (Figure A5). For example, in the case of the number of women in the labor force, the individual coefficients denote differences between treated and control municipalities that are equal to at most 2.7 residents or 0.6 percent of the pre-reform mean.[28]

Next, we repeat the same estimation using the INPS dataset, which has eighteen yearly data points (1974-1992) before the LPT 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 A6; Table A8, Panel B). For both linear and nonlinear trends, we reject the hypothesis that treated and control municipalities were trending differentially before the LPT. This result holds for all age groups.

Finally, we show that before the introduction of the LPT, bombing-adjacent and bombing-distant municipalities were similar in terms of observable characteristics, except for the share of pre-WWII buildings and the post-LPT degree of fiscal decentralization. Out of 46 additional variables observed in 1991, only one is statistically different at the 5 percent level (Table A5, Panels B and C, column 2). In addition to being insignificant, most coefficients are also small in magnitude, indicating that treated and control municipalities were balanced just before the LPT implementation. For example, bombing-adjacent and bombing-distant municipalities differed in the number of women in the labor force by 11.5 residents or 1.1 percent of the 1991 mean, in the number of men in the labor force by 4.3 residents or 0.2 percent of the mean, and in the number of pupils in nursery school by 0.5 children or 2.4 percent of the mean. Moreover, these balancing tests range from labor-market outcomes to demographics, ideology, geography, and municipal balance sheets, indicating that treated and control firms were similar in a wide variety of areas.

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[28]Table A5 shows small and insignificant pre-reform trends for more census variables.

4 Effects of Fiscal Decentralization

4.1 Effects on Municipal Spending

Overall effects Using Equation (2), we show that the introduction of the LPT 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 revenues from local taxes (on average, €12 after 1992; Figure A7, Panel A) and a decrease in the revenues from government transfers (€-19; Figure A7, Panel B). 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 disproportionately in bombing-adjacent cities. Spending per resident decreased by €61, while revenues per resident decreased by €64 (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.

Second, 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 LPT, 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.

Third, 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).

Fourth, 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).[29] 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 LPT. 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 LPT (from the 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.[30] This issue is especially urgent in Italy, a country that spends significantly less than the OECD average on families and children (OECD, 2011). In 2018, the share of women 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.[31] 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 LPT.[32] 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

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

[30]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)."

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

[32]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.

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).[33] 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.[34] In general, private nursery schools are significantly more expensive. Although nationally representative data on private nursery schools are not available, anecdotal evidence suggests that the price difference can often be above 100 percent.[35]

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 LPT. 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-LPT mean (Table 3, Panel A).

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

[34]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.

[35]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/).

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 have relevant consequences on both labor demand and supply. Programs for local economic development, a more efficient municipal police, and investments in public health 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 LPT, but only among women. This is the first piece of evidence that connects expanded access to public nursery schools to better labor-market outcomes.

We establish five main findings. First, female labor-force participation significantly increased in bombing-adjacent municipalities. As we discussed in Section 3.5, bombing-adjacent and bombing-distant municipalities experienced similar trends in women's labor-force participation between 1951 and 1991 (Figure 3, Panel A). All pre-LPT coefficients are small and statistically insignificant. Then, in bombing-adjacent municipalities, female labor-force participation increased by 15 individuals (3.4 percent) in 2001 and by 23 individuals (5.1 percent) in 2011. Both coefficients are statistically different from zero.

Moreover, we can focus exclusively on employment, therefore excluding from the analysis unemployed women in search for a job. The results on female employment closely track the previous findings on labor-market participation. The pre-LPT trends are flat, close to zero, and statistically insignificant (Figure 3, Panel B). After 1991, bombing-adjacent municipalities experienced an increase in female employment by 13 individuals (3.4 percent) in 2001 and by 18 individuals (4.6 percent) in 2011.

Second, there are not systematic differences in male labor-force participation between bombing-adjacent and bombing-distant municipalities. Relative to 1991 and bombing-distant locations, men's labor-force participation in bombing-adjacent municipalities increased by 8 individuals (0.8 percent) in 1951, by 5 individuals (0.4 percent) in 1961, by 10 individuals (0.9 percent) in 1971, by 4 individuals (0.04 percent) in 1981, and by 8 individuals (0.8

percent) in both 2001 and 2011 (Figure 3, Panel C). None of these coefficients is statistically different from zero. Focusing on male employment leads to the same conclusions (Figure 3, Panel D).

The main consequence was a reduction in the preexisting gender gap in labor-force participation (Figure 3, Panel E) and employment (Figure 3, Panel F). For example, in bombing-adjacent municipalities, the gender gap in employment decreased by 10 individuals (1.8 percent) in 2001 and by 21 individuals (3.6 percent) in 2011.

Third, the labor-market effects are much larger in cities with a higher share of young parents, who therefore are more likely to have children in pre-kindergarten age. Specifically, we modify Equation (2) by adding a triple interaction between year fixed effects, the bombing-adjacent dummy, and a new dummy equal to 1 for cities whose share of younger (below 35 years old) residents with children was either above the median, in the top tertile, or in the top quartile (Figure A8, Panels A to C).[36] In bombing-adjacent cities with a share of young parents in the top quartile, women's labor-force participation increased by 58 individuals (12.9 percent) in 2001 and by 88 individuals (19.6 percent) in 2011. Instead, the number of women in the workforce did not significantly increase after the LPT in cities with lower shares of young parents. We find the same results if we consider the share of young single parents (Figure A8, Panels D to F), rather than the share of all young parents.

The existence of significant effects of fiscal decentralization only for younger 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 4.6, we will estimate more triple-difference specifications with INPS data in order to better weed out the effect stemming from other concurrent changes within a city.

Fourth, fiscal decentralization did not induce large movements of people across municipalities. In bombing-adjacent cities, population increased by an additional 27 individuals in 2001 (0.7 percent) and by 34 individuals (0.9 percent) in 2011 after the LPT (Figure A9, Panel A). Both coefficients are statistically insignificant. Moreover, the results are similar if we focus exclusively on women (Figure A9, Panel B) or men (Figure A9, Panel C). However, we establish that the number of foreign residents disproportionately increased in bombing-adjacent municipalities after the LPT (Figure A9, Panel D). This finding can be rationalized by the fact that 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

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[36]To complete the triple-difference specification, we also include the interaction between the new dummy and year fixed effects.

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 result indicates that better welfare services may have attracted more individuals in need of economic support.

Fifth, we do not find large and significant effects of fiscal decentralization on the number of local firms. In bombing-adjacent cities, the total number of firms decreased by 3 units (1.5 percent) in 2001 and by 4 units (1.9 percent) in 2011 (Figure A9, Panel E). This result does not change if we focus on firms with fewer than three employees (Figure A9, Panel F), which represents the bulk of the Italian firm stock.[37] Overall, these findings suggest that the introduction of the LPT did not significantly change the demand for labor. Therefore, the observed increase in female employment may have stemmed from an improvement in local services, for example, easier access to public nursery schools, that eased existing constraints in the supply of labor.

Finally, we can directly tie fiscal decentralization to the labor markets by estimating instrumental variable regressions. In Equation (2), we replace Near bombed$_m$ with ΔRev. local tax $(90\text{-}94)_m$, which measures the change in per-capita revenues from local taxes between 1990 and 1994 in municipality m. Because ΔRev. local tax $(90\text{-}94)_m$ 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 (2). The overall pattern of results from these IV specifications is in line with the previous reduced-form estimates, but the magnitudes tend to be larger (Figure A10). For example, a €49 increase in per-capita revenues from local taxes, which is equivalent to moving from the 25$^{\text{th}}$ percentile to the median, is associated with 31 additional employed women (7.7 percent) in 2001 and 36 additional employed women (9 percent) in 2011.

4.3   Endogenous Responses of Local Administrators

Our empirical strategy hinges on the assumption that variations in the LPT base, driven by the age of buildings, translated into permanent differences in the exposure to fiscal decentralization. The data support this assumption. Relative to bombing-distant cities, the per-capita revenues from local taxes in bombing-adjacent municipalities increased disproportionately in 1993 and remained higher throughout the period under consideration, while the per-capita revenues from government transfers followed the opposite trend (Section 4.1).

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[37]In bombing-adjacent cities, we also observe a larger reallocation of workers from agricultural (Figure A9, Panel G) and manufacturing (Figure A9, Panel H) 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.

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 LPT rates to equalize revenues from local taxes across municipalities. Tax rates, however, were bounded between 0.4 percent and 0.7 percent. Therefore, we should expect tax rates to be higher in bombing-distant municipalities, but this effect should not be sufficient to delete any difference in per-capita revenues from local taxes. As expected, compared to bombing-adjacent cities, bombing-distant cities had an average LPT rate 0.006 percentage points higher and an LPT rate for owner-occupied residences 0.01 percentage points higher (Table A10, column 6). These effects are statistically significant but small in magnitude.

The data also indicate that the upper bound in tax rates was indeed binding for many municipalities. Compared to bombing-adjacent cities, bombing-distant municipalities were 2.8 percentage points more likely to reach the max LPT rate of 0.7 percent. This coefficient represents a sizable 22-percent increase from the mean and is statistically significant. The effect is much smaller if we focus on the tax rates for owner-occupied residences. In this case, bombing-distant municipalities were only 0.9 percentage points more likely to reach the max LPT rate. This finding indicates that, in addition to being constrained by an upper bound to tax rates, local administrators may have perceived that the political cost of imposing the maximum tax rate on owner-occupied residences was too high.

Similarly, local administrators could have attempted to increase the LPT 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. This coefficient is again precisely estimated but small in magnitude.

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

Next, we incorporate the previous findings into our empirical strategy. Specifically, if local administrators were hesitant to set higher tax rates on owner-occupied residences, we should observe larger effects of fiscal decentralization in cities with a lower share of owner-occupied main residencies.[38] Therefore, we modify Equation (2) by adding a triple interaction between year fixed effects, the bombing-adjacent dummy, and a new dummy equal to 1 for

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[38]The special LPT for main residencies is valid only for owner-occupied properties. Therefore, the LPT on rental properties, including renters' primary residences, is paid by the owners at standard rates.

cities whose share of owner-occupied properties in 1991 was in the top quartile. Consistent with our initial hypothesis, the estimation of this triple-difference specification indicates that the increase in women's labor-force participation and employment primarily stemmed from cities with a lower share of owner-occupied buildings (Figure A11).

4.4   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 LPT 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 LPT. From the point of view of taxpayers, the introduction of the LPT increased the total tax liability, because the PIT did not decrease accordingly.

Therefore, a plausible question is whether the observed increase in women's labor-force participation is the result of a negative income effect from higher taxation, rather than stemming from improved local services. 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). However, using Italian data, Giupponi (2019) finds that individuals who lose a substantial portion of survivor benefits (on average, €2000 a year or 32 percent of the survivors' yearly labor income) fully offset this income loss with increases in earnings.

There are several pieces of evidence suggesting that negative income effects were not large after the introduction of the LPT. First, unlike the negative income shock studied by Giupponi (2019), the increase in taxation was small in magnitude. On average, residents owed €300 on their main residence or 0.9 percent of the average household income in 1991. It is plausible to assume that this increase in taxation was sufficient to make the LPT salient, but not enough to induce more people to enter the workforce.

Second, the small magnitude of the income effect is not fully compatible with the observed results on labor supply. In fact, a negative income effect should induce both men and women to increase their earnings. However, we established that the introduction of the LPT increased only women's employment, while the effect on men is a precisely estimated zero.

Third, if effective in increasing labor supply, such a small negative income effect should act mostly on the intensive margin. In other words, a 0.9-percent income loss should primarily

induce people who are already in the workforce to slightly increase their working hours, rather than forcing people to enter the workforce. However, we find that the introduction of the LPT significantly increased women's labor-force participation.

Fourth, while the negative income effects from a higher taxation affects all residents, the benefits from improved access to public nursery schools target only parents with very young children. The triple interactions in Section 4.2 indicated that the labor-market effects are concentrated in municipalities with higher shares of young parents, who are more likely to have pre-kindergarten children. These findings are further corroborated by the analysis on Social Security data in Section 4.6. In short, the pattern of results is not fully consistent with the hypothesis that the labor-market effects are a response to higher taxation.

Next, we estimate the elasticities of local services and labor supply with respect to the degree of fiscal decentralization. The per-capita revenues from local taxes increased by up to €19 more in bombing-adjacent municipalities, relative to bombing-distant locations. This difference is large in magnitude, 12 percent of the average share of revenues from local taxes in 1990, and precisely estimated. A 1-percent increase in the share of revenues from local taxes increased the probability of having nursery schools by 0.7 percent, spending for nursery schools by 1.5 percent, the number of pupils enrolled in nursery school by 1.9 percent, and spending for local services by 0.2 percent. In the labor market, a 1-percent increase in per-capita revenues from local taxes increased women' labor-force participation and employment by 0.4 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.[39]

4.5 Analysis of Potential Mechanisms

Several mechanisms may have induced local administrators to increase the quantity and quality of municipal services.[40] 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

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[39]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).

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

administrators who are closer to the final users and therefore have better knowledge about their preferences.[41]

In this section, we provide evidence on the role played by these different mechanisms.[42] We augment the baseline specifications with three interaction terms. First, we measure the level of political competition by adding Mayoral term$_m$, which measures the average number of terms held by mayors after 1993 in municipality m. Alternatively, we can include Runoff$_m$, the total number of runoff elections after 1993 in municipality m.[43] 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 year fixed effects and Near bombed$_t$ to estimate the change in labor outcomes between bombing-adjacent and bombing-distant municipalities, before and after the introduction of the LPT, and between cities with different levels of political competition, municipal competition, or low-income households.[44]

As expected, tougher political competition is correlated with larger treatment effects. Relative to similar cities with lower competition, bombing-adjacent locations with closer political races show a lower probability of fiscal infractions, a larger share of spending on welfare programs, and more pupils attending nursery schools (Table A11). These differences in the provision and utilization of public services translated into a larger effect on women's labor-force participation and employment (Figure A12). In comparison, the effect of municipal competition is smaller in magnitude and seldom statistically significant. Similarly, the share 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

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

[42] 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.

[43] 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/.

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

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 A12). For example, a €54 increase in mean revenues from local taxes (from 25th percentile to median) is associated with an 8-percent higher probability of being interested in politics and with a 3-percent higher probability of voting.

4.6 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 below three years old are between 25 and 35 years old (Figure A13). 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.

The expansion of subsidized retirement homes is another municipal service who may spur women's labor-force participation, due to the fact that intra-household elderly care falls predominantly upon women. However, the share of women with older parents, who therefore are more likely to need intensive care, is close to zero below 35 years old and significantly increases only after 40 years old (Figure A14). If expanded access to retirement homes is one of the major drivers of increased female labor supply, we should observe larger treatment effects among older women.

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

$$y_{amt} \;=\; \alpha_m + \beta_b + \gamma_{rt} + \zeta_{at} + \sum_a \delta_0^a \text{Age}_a \times \text{Near bombed}_m \times \text{Post}_t \tag{3}$$

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

where the unit of observation is an age group a living in municipality m in year $t \in [1974, 2011]$.[45] Age_a is a set of dummies identifying individual ages or age bins. The variable ζ_{at} denotes age-year fixed effects, while all other variables are unchanged from equation (2). 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 LPT. They isolate the effect of nursery schools if they are the only expanded municipal service that differentially affects women of different ages across treated and control municipalities. The control group is represented by employed women between 50 and 54 years old.

Age-specific triple interactions confirm that the increase in female labor supply was larger among younger women (Figure 4 and Table A13). The number of women entering the labor market for the first time increased the most for women aged 25 to 28. Relative to bombing-distant municipalities and women between 50 and 54 years old, the number of new entrants into the labor market increased by 13 percent (from the pre-LPT mean) per post-LPT 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 35-year-olds. The number of women reentering the labor market after a hiatus increased by 21 percent to 24 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 LPT. This effect could suggest that expanded 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 A13, Panel D).[46]

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 younger women (Figure 4, Panel C). This decrease was between €315 and €648 per year (-2.2 to -4.6 percent) and was concentrated among women under

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[45]Variables measuring reentry into the labor market, instead of first entry, start in 1976 because the first years are necessary to detect a break in employment.

[46]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.

35.[47] In bombing-adjacent municipalities, younger women also became slightly more likely to work fewer days during the year (-3 percent; Figure 4, Panel D) and more likely to work outside their province of residence, although these last estimates are imprecise (+21 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).[48] 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 A14, 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 A14, Panel C).

More importantly, the Social Security data are the only dataset available at the municipal level which has yearly observations between 1974 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 can fix the age dimension and estimate treatment effects by calendar year (Figure A15). As discussed in Section 3.5, the results show the lack of differential pre-LPT 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 LPT. 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-LPT elections (Figure A16).[49] Although only suggestive, this finding is consistent with the main takeaway of Section 4.5: the level of political competition seems to be the most plausible mechanism to have induced local administrators to improve

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

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

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

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 LPT on main residences in 2007 (Section 4.7).

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.[50] Relative to the baseline 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 LPT, relative to men and older women (Table A15). Younger women became also more likely to hold positions with lower wages, more flexible hours, and higher commuting costs.[51]

4.7   Robustness Checks

In this section, we perform several robustness checks. First, we control for the main postwar economic intervention by including in Equation (2) the amount of aid received by a province through the Marshall Plan interacted with a post-reform dummy.[52] All the main treatment effects are robust to the inclusion of three different specifications of Marshall Plan aid (Figure A17).

Second, we estimate the baseline equation (2) on a matched subsample of bombing-adjacent and bombing-distant municipalities using population, area size, and region fixed

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[50]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.

[51]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 A15, Panel C).

[52]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.

effects (Table A5, column 3). These regressions show treatment effects that are close in magnitude and precision to the baseline estimates (Figure A18).

Third, the main findings are fully robust to several changes to the initial matching process between bombed and nonbombed locations (Figure A19). For example, including twenty-four variables in the propensity score matching, instead of the eight listed in Section 3.3, does not modify the results. Similarly, stratifying cities by region before matching them on observables lead to the same treatment effects.

Fourth, the treatment effects retain their statistical significance if standard errors are clustered at the province level, instead of at the level of bombed municipalities (Figure A20, Panel A). Similarly, the results hold if we estimate standard errors that are robust to both spatial and serial correlation (Conley, 1999; Figure A20, Panel B).

Fifth, we estimate regressions in which the dependent variables are expressed as shares of local residents (Figure A20, Panel C). The share of women in the workforce, and not only their total number, increased disproportionately in bombing-adjacent municipalities after the LPT.

Sixth, our baseline specification already includes many nonlinear trends. Moreover, the results are robust to controlling for even more nonlinear trends correlated with provinces, geographical characteristics, and characteristics of the real-estate market (Figure A20, Panels D to F).

Seventh, 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; Figure A20, Panel G).[53]

Eight, we estimate placebo treatments by assigning bombing-adjacent status at random (Figure A20, Panel H). The resulting coefficients are statistically insignificant and small in magnitude.

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 (LPT) 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 LPT base.

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[53]Although the effect of women's labor-force participation becomes slightly less precise, the result on the gender gap in participation and employment remains precisely estimated.

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 up to 20 percent, reducing the preexisting gender employment gap. 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 LPT. Our findings speak to the importance of high electoral competition among political candidates.

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)
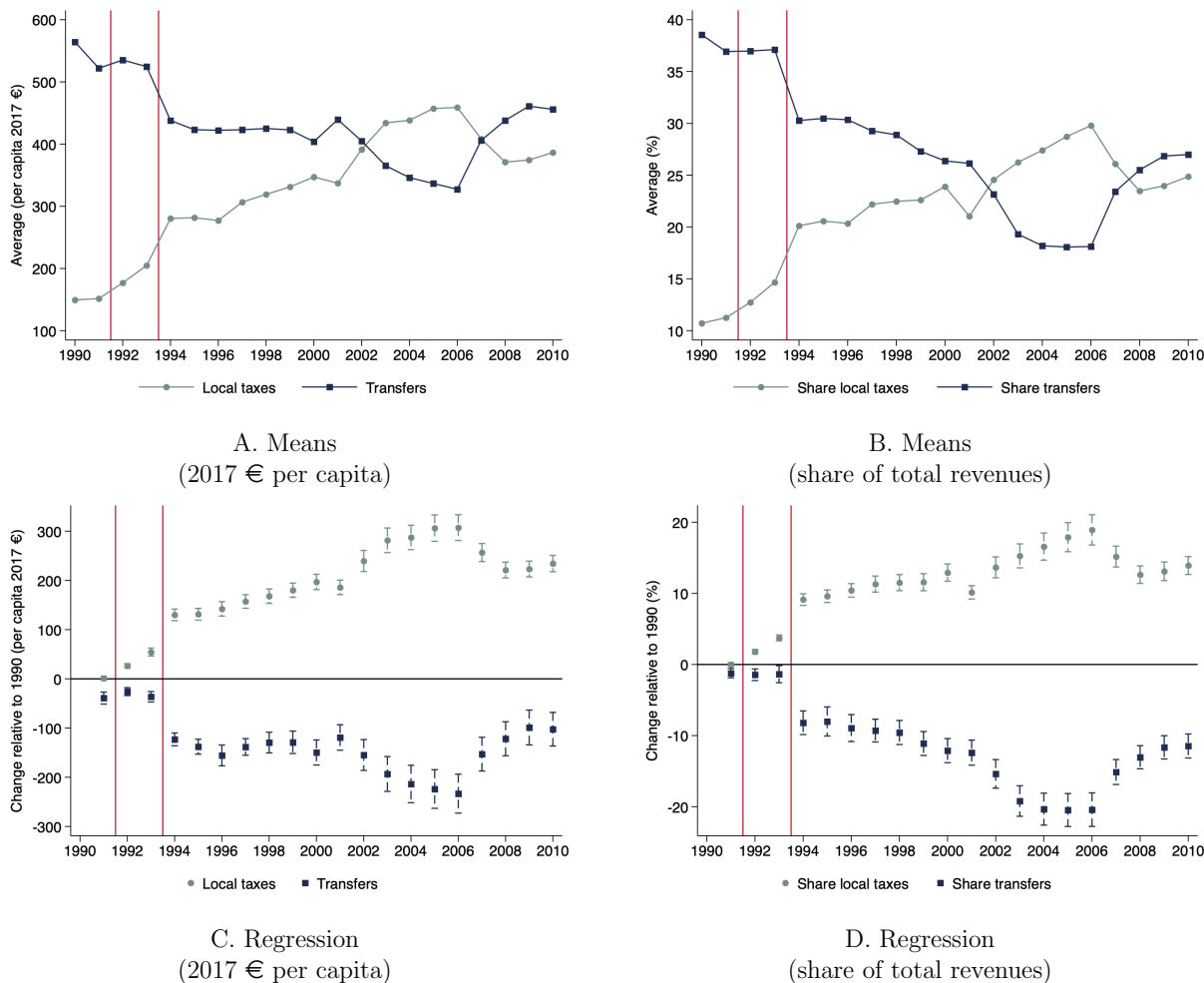
Notes: These graphs show the change in the composition of revenues of Italian municipalities. Panels A and B show the average revenues from local taxes and from transfers issued by higher levels of government (provinces, regions, central government), either as 2017 € per resident (panel A) or as a share 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	**Bombed**
(2000,12000]	**Around bombed**
(500,2000]	**Matched to bombed**
(.1,500]	**Around matched**
[0,.1]	**Other municipalities**
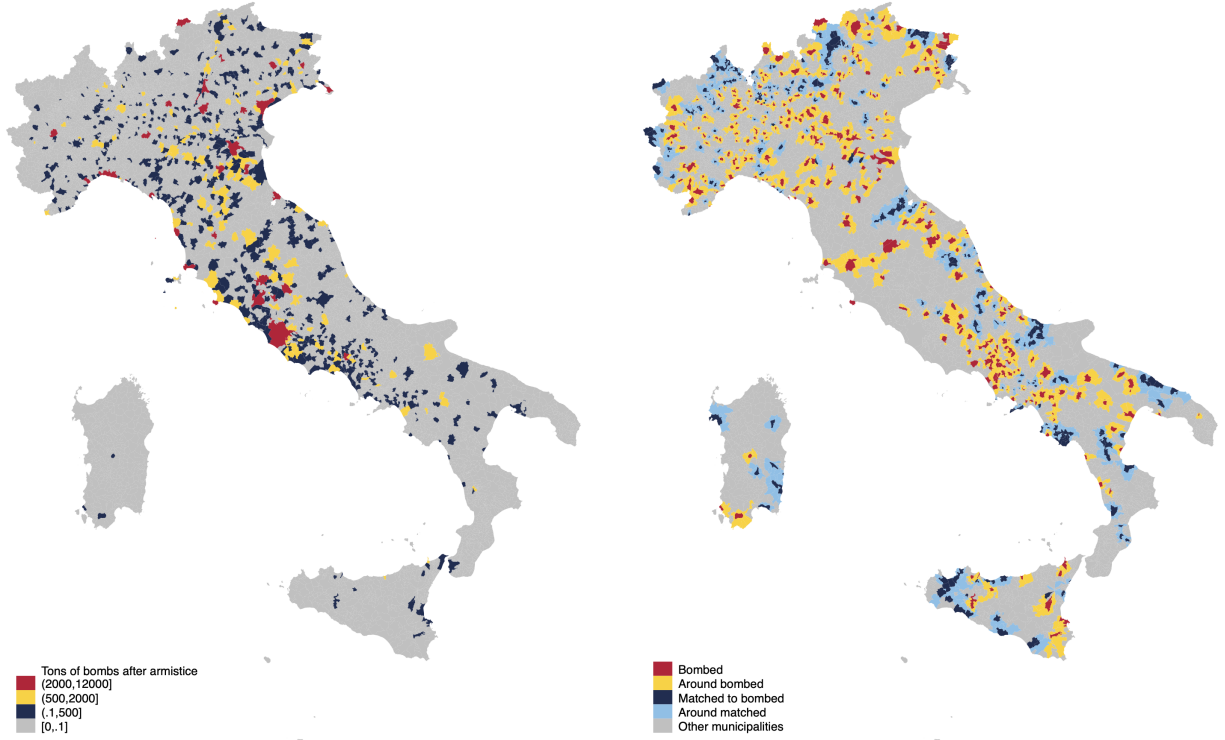
A. Distribution of WWII bombings   B. Municipalities in the sample

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. Women in labor force

B. Employed women

C. Men in labor force

D. Employed men

E. Gender gap in labor force

F. Gender gap in employment

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 omitted year is 1991. The regressions also include city fixed effects, region-year trends, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors are clustered at the city level. The vertical bars measure 90 percent CIs. Source: 8mila Census, ISTAT, available online at http://ottomilacensus.istat.it/.

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, age-year fixed effects, region-year fixed effects, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors are clustered at the city level. The vertical bars measure 95 percent CIs. Source: Istituto Nazionale della Previdenza Sociale (INPS).

Table 1: Summary Statistics

| | All years | | | | $t \leqq 1992$ | $t > 1992$ |
| | 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	6803.05	40861.85	56,092	1951-2011	6643.83	7195.87
Women in labor force	918.42	6077.42	56,092	1951-2011	776.83	1267.75
Employed women	793.57	5286.25	56,092	1951-2011	674.05	1088.43
Men in labor force	1823.58	10791.41	56,092	1951-2011	1828.36	1811.79
Employed men	1671.02	9804.48	56,092	1951-2011	1684.52	1637.71
Gender gap in labor force (M-W)	905.17	5485.53	56,092	1951-2011	1051.54	544.04
Gender gap in employment (M-W)	877.46	5198.78	56,092	1951-2011	1010.47	549.28
Foreign residents	235.76	2389.66	24,254	1991-2011	44.08	331.44
Pupils in nursery schools	27.47	235.47	24,254	1991-2011	19.60	31.40
Share below 3	2.66	0.96	32313	1981-2011	2.91	2.42
Agricultural workers	10.83	51.07	40344	1971-2011	11.37	10.03
Manufacturing workers	615.86	3516.15	40344	1971-2011	666.64	540.01
Panel D: Social Security data						
New entry in labor market	0.52	1.89	2,967,243	1974-2011	0.50	0.54
Reentry in labor market	0.54	1.21	2,433,138	1976-2011	0.35	0.59
Reentry in same firm	0.14	0.46	2,433,138	1976-2011	0.10	0.16
Median wage	13,180.84	8,463.31	2,967,243	1974-2011	13,341.32	13,036.72
Median days worked	225.31	90.87	2,967,243	1974-2011	224.21	226.40
Working outside province	2.11	5.45	2,967,243	1974-2011	1.33	2.43
Log median wage	9.29	0.78	2,967,243	1974-2011	9.34	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: 8mila Census, Istituto Nazionale di Statistica, available online at http://ottomilacensus.istat.it/. 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

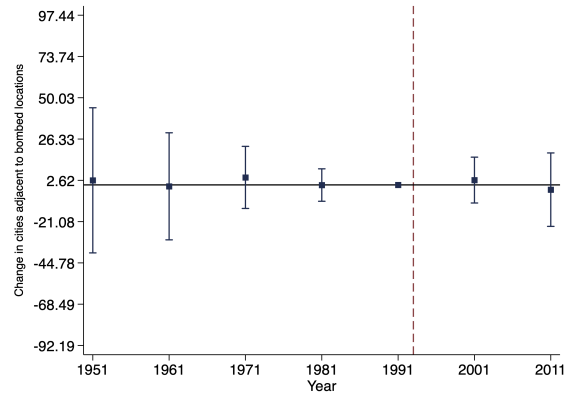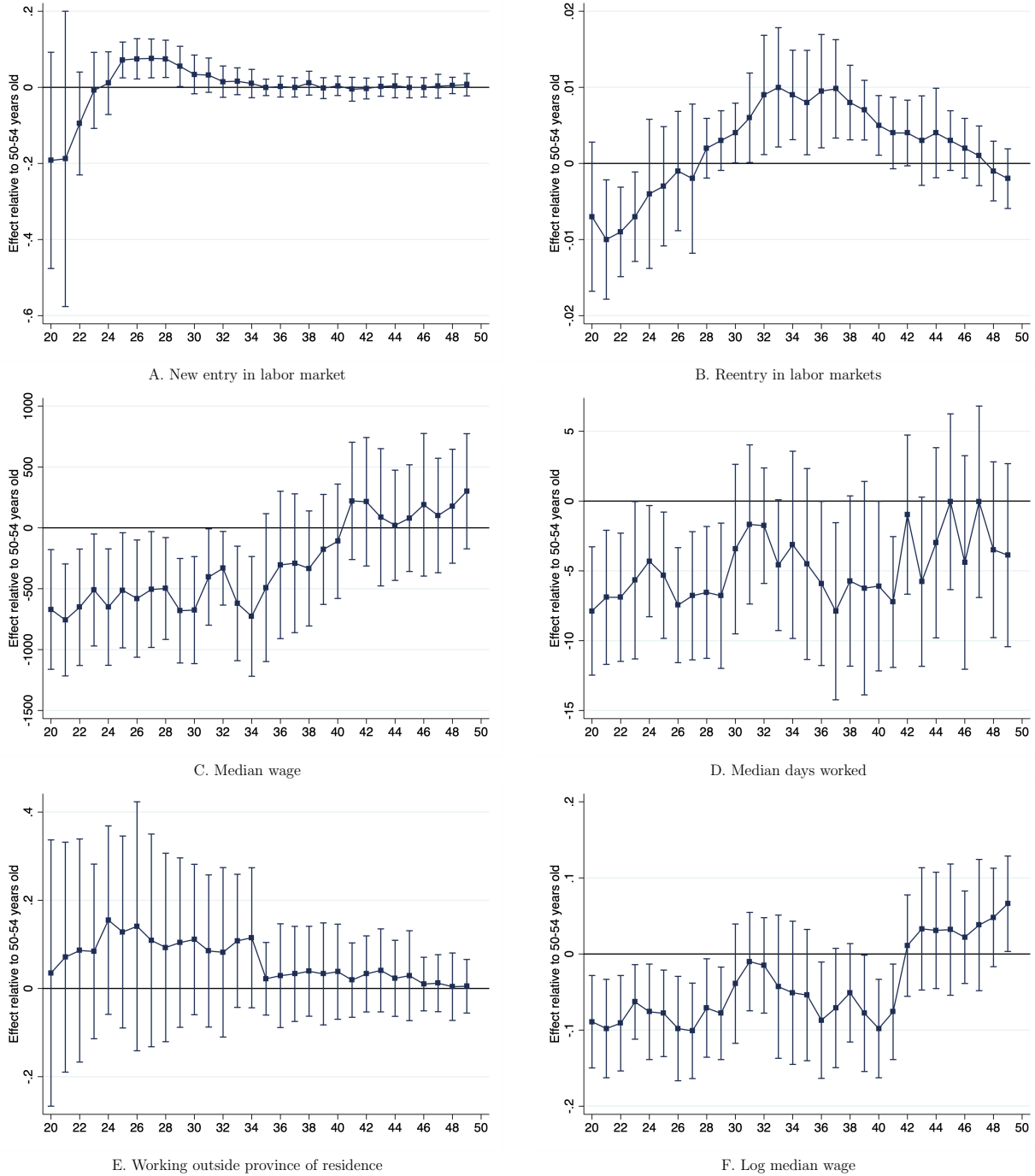|	Revenues from local taxes	Revenues from gov. transfers	Revenues per capita	Spending per capita	Women in labor force	Employed women	Men in labor force	Employed men	Gender gap in labor force	Gender gap in employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
				Panel A: Linear pre-LPT trends						
Near bombed x Trend	0.203	-0.262	-30.040	-24.530	0.060	0.065	-0.184	-0.041	-0.244	-0.105
	(1.144)	(2.423)	(25.356)	(25.145)	(0.469)	(0.416)	(0.734)	(0.668)	(0.496)	(0.477)
Observations	7,007	7,007	7,007	7,007	11,874	11,874	11,874	11,874	11,874	11,874
Dep. var.—mean	158.25	527.85	1675.00	1673.60	448.60	394.54	1049.55	974.73	600.95	580.19
Dep. var.—std. dev.	109.13	218.19	1196.34	1200.25	692.81	597.59	1476.77	1325.43	921.54	852.02
				Panel B: Nonlinear pre-LPT trends						
Near bombed x 1951					-1.055	-1.450	8.675	2.331	9.731	3.780
					(17.777)	(15.492)	(27.957)	(25.263)	(19.637)	(18.575)
Near bombed x 1961					-2.292	-2.872	4.999	-0.951	7.292	1.921
					(15.925)	(14.272)	(22.075)	(18.659)	(15.103)	(13.746)
Near bombed x 1971					2.751	1.646	9.918	4.077	7.167	2.431
					(11.129)	(8.860)	(13.835)	(10.831)	(11.277)	(10.501)
Near bombed x 1981					1.636	0.710	4.050	-0.374	2.414	-1.083
					(5.393)	(4.119)	(8.084)	(5.672)	(6.440)	(5.266)
Near bombed x 1990	-0.489	0.614	61.630	41.927						
	(2.286)	(4.858)	(50.681)	(52.222)						
Near bombed x 1991	1.695	-1.794	-5.483	-10.050						
	(2.566)	(6.828)	(49.972)	(54.871)						
Observations	7,007	7,007	7,007	6,988	11,874	11,874	11,874	11,874	11,874	11,874
Dep. var.—mean	158.25	527.85	1675.00	1673.60	448.60	394.54	1049.55	974.73	600.95	580.19
Dep. var.—std. dev.	109.13	218.19	1196.34	1200.25	692.81	597.59	1476.77	1325.43	921.54	852.02
F statistic	0.38	0.06	1.10	0.56	0.22	0.20	0.39	0.26	0.14	0.08
P value	0.69	0.94	0.33	0.57	0.93	0.94	0.81	0.90	0.97	0.99

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 linear pre-reform trends either between 1990 and 1992 using data from balance sheets of Italian municipalities (columns 1 to 4) or between 1951 and 1991 using data from the population censuses (columns 5 to 10). Panel B estimates nonlinear pre-reform trends. The F-statistic at the bottom tests for the joint significance of the nonlinear trends. The omitted year is 1992 in columns 1 to 4 and 1991 in columns 5 to 10. The regressions also include city fixed effects, region-year fixed effects, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors clustered at the 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								
Rev. from local taxes	12.321***	47,157	0.892	14.122***	47,028	0.903	158.54	108.92
	(4.481)			(4.745)				
Rev. from gov. transfers	-19.357***	47,156	0.843	-16.759***	47,027	0.854	528.31	218.83
	(5.655)			(5.966)				
Revenues per capita	-64.355**	47,153	0.608	-26.235	47,024	0.630	1677.24	1197.95
	(31.944)			(35.098)				
Spending per capita	-60.612*	47,140	0.607	-24.035	47,012	0.629	1674.14	1203.15
	(32.496)			(35.998)				
Deficit per capita	1.994	47,120	0.106	4.160	46,992	0.131	-4.24	148.30
	(4.601)			(6.155)				
Pupils in nursery schools	2.475***	7,277	0.879	2.774***	7,259	0.900	10.43	21.23
	(0.746)			(0.775)				
Residents below 3	3.415	9,589	0.972	3.256	9,565	0.974	124.64	243.34
	(2.416)			(3.550)				
Share below 3	0.063**	9,589	0.735	0.083***	9,565	0.753	2.84	1.03
	(0.026)			(0.029)				
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
	(0.012)			(0.013)				
Spending for local services (%)	1.195***	28,401	0.266	0.835**	28,319	0.327	54.8	16.25
	(0.337)			(0.365)				
Rev. for admin. tasks per employee	257.568**	28,560	0.063	292.717**	28,478	0.123	2244.73	3756.45
	(121.449)			(136.087)				
Has program for local develop.	0.074***	28,430	0.163	0.050***	28,347	0.233	0.61	0.49
	(0.016)			(0.018)				
Has nursery schools	0.054***	28,430	0.222	0.042***	28,347	0.296	0.63	0.48
	(0.014)			(0.016)				
Spending for nursery schools (%)	0.178***	28,248	0.283	0.045	28,165	0.387	1.01	2.03
	(0.065)			(0.065)				
Public nursery schools	0.052***	17,326	0.391	0.005	17,194	0.504	0.26	0.61
	(0.015)			(0.016)				
Pupils in private nursery schools	0.015	2,403	0.771	0.159	2,397	0.804	11.88	21.18
	(0.403)			(0.414)				

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, either region-year or province-year fixed effects, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. 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 city level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Online Appendix - Not For Publication

A   Additional Figures and Tables

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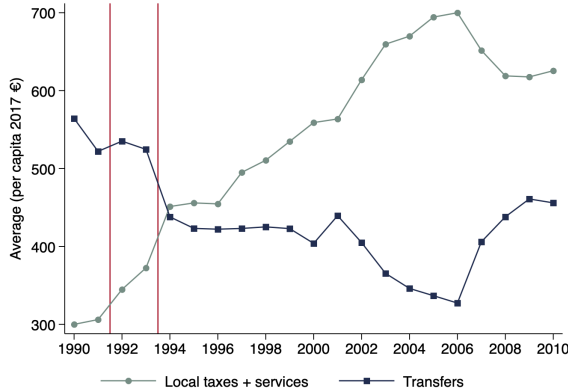
A. Revenues from local taxes and services
(2017 € per capita)
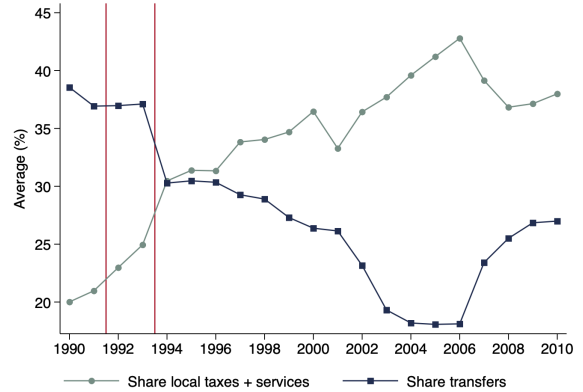
B. Revenues from local taxes and services
(share of total revenues)
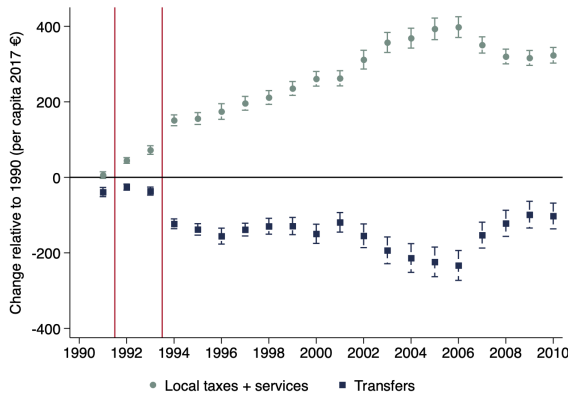
C. Revenues from local taxes and services
(2017 € per capita)
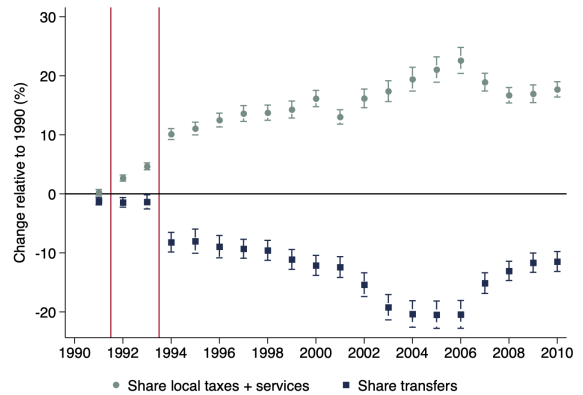
D. Revenues from local taxes and services
(share of total revenues)

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
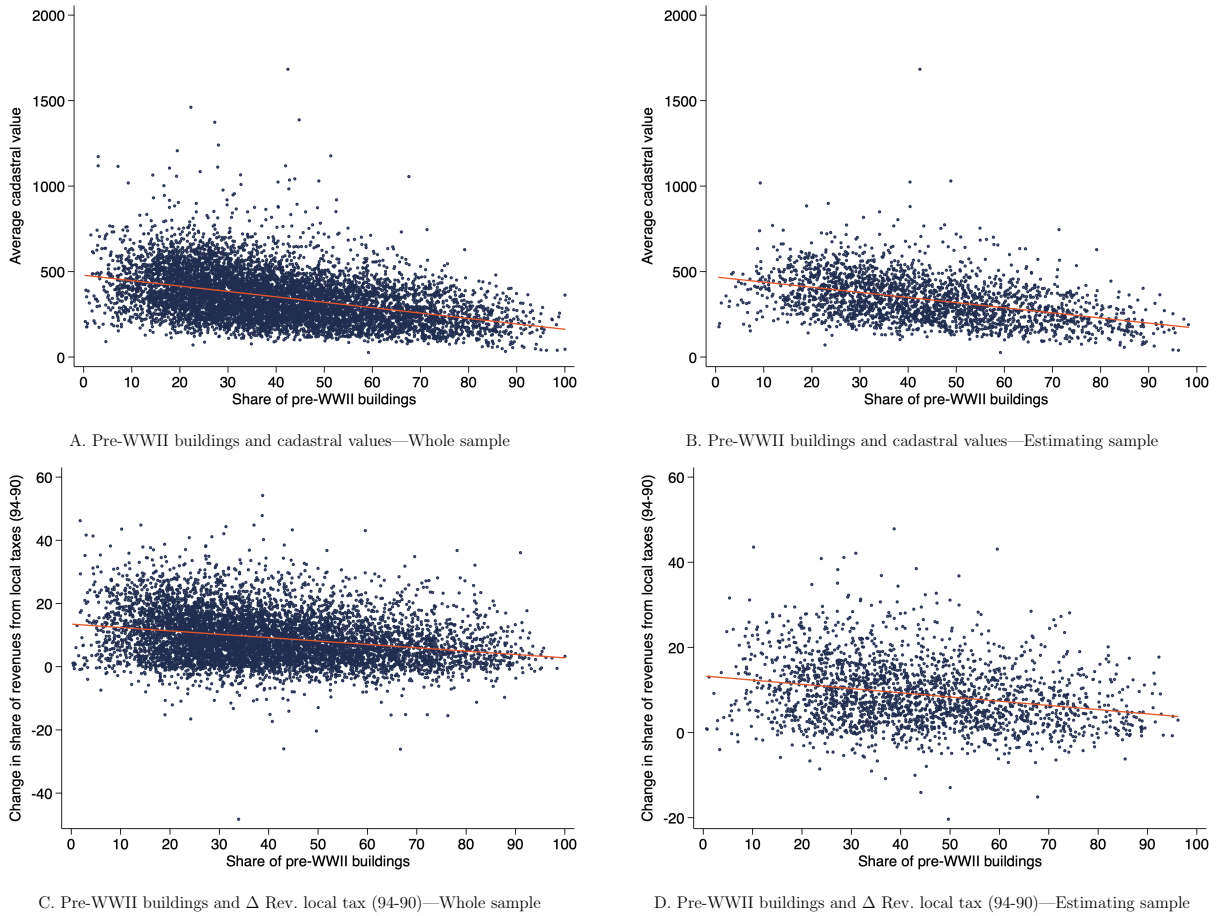
Notes: These graphs show the correlations between the share of pre-WWII buildings and the average cadastral value of buildings (panels A and B) or the difference in the share of revenues from local taxes between 1990 and 1994 (panels C and D). Panel B and D show only the municipalities in the main estimating 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: Effects of Allied Bombings on Female Labor-Force Participation

A. OLS, women in labor force

B. OLS, employed women

C. OLS, pupils in nursery school

D. IV, women in labor force

E. IV, employed women

F. IV, pupils in nursery school
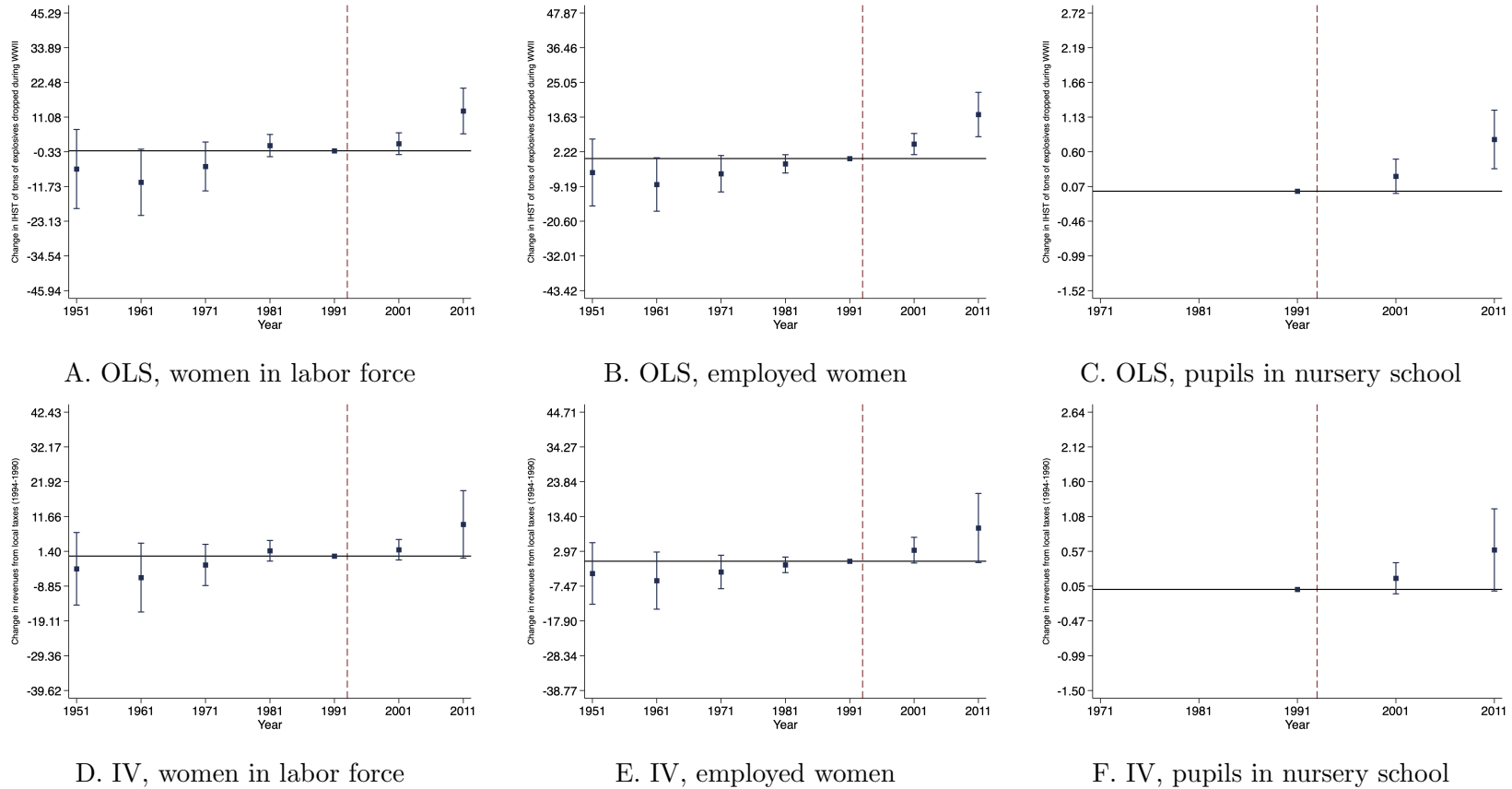
Notes: In panels A to C, several municipal outcomes are regressed on the inverse hyperbolic sine transformation (IHST) of the tons of post-armistice Allied bombings. In panels D to F, the same variables are regressed on the change in per-capita revenues from local taxes between 1990 and 1994, which is instrumented with the IHST of the tons of post-armistice Allied bombings. Both variables are interacted with year fixed effects and the omitted year is 1991. All specifications include municipal fixed effects, region-year trends, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors are clustered at the city level. The vertical bars measure 90 percent CIs. 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/; 8mila Census, ISTAT, http://ottomilacensus.istat.it/.

Figure A4: 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
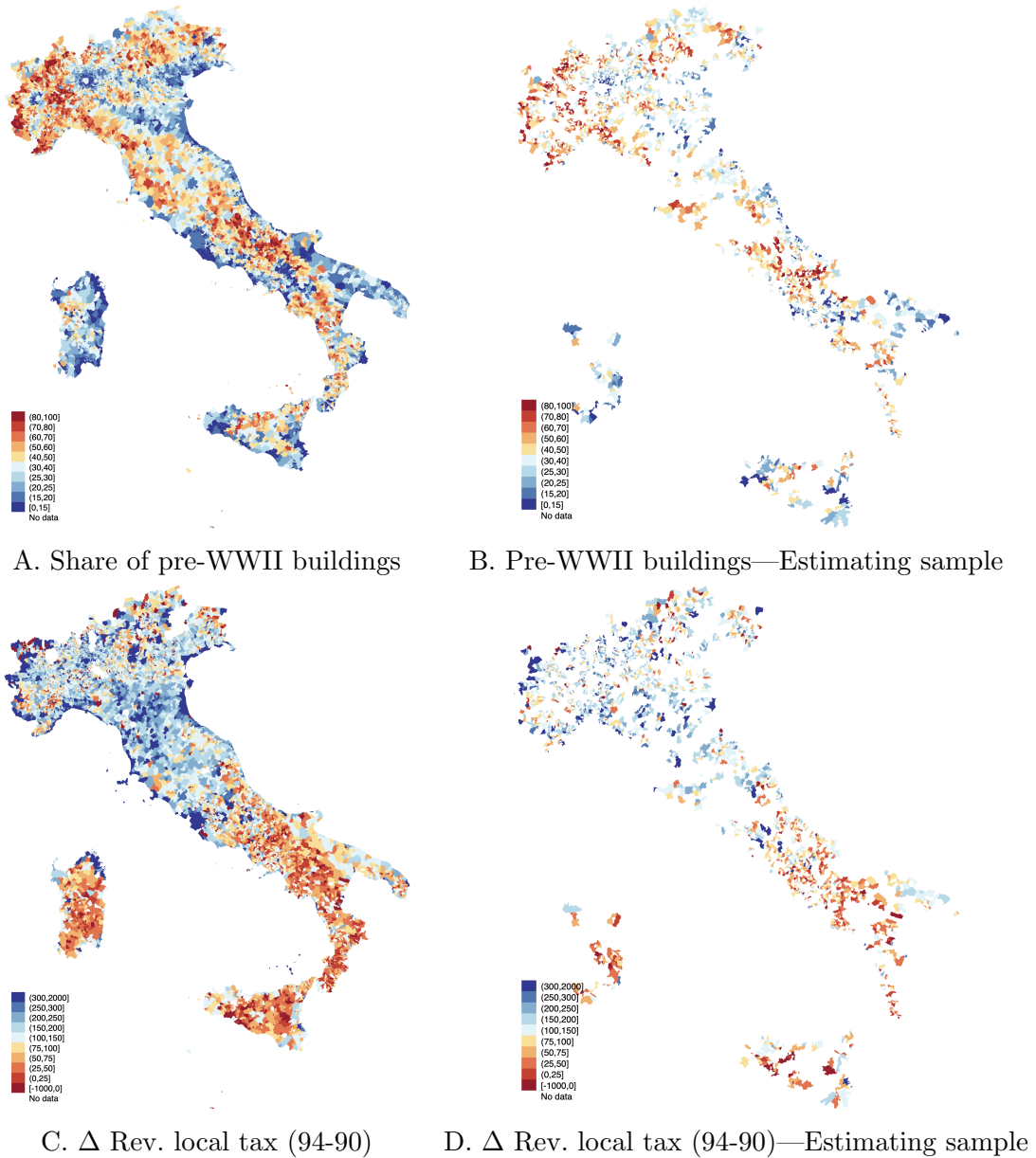
Notes: These graphs show the geographical distribution of the share of pre-WWII buildings and the change in per-capita revenues from local taxes between 1990 and 1994. Panel B and D show only the municipalities in the main estimating sample. Source: Italian Minister of the Interior, https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4; Atlante Statistico dei Comuni, http://asc.istat.it/asc_BL/.

Figure A5: Pre-Reform Trends, Main Outcomes from Census
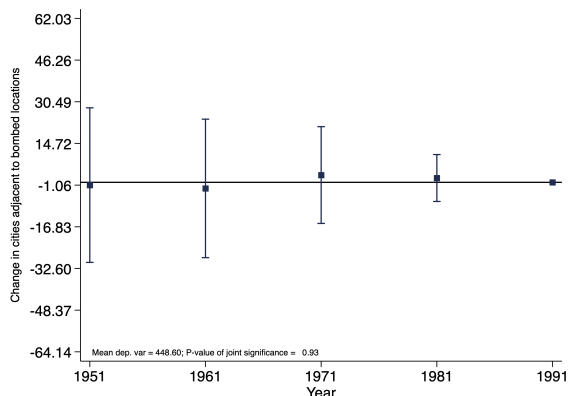
A. Women in labor force
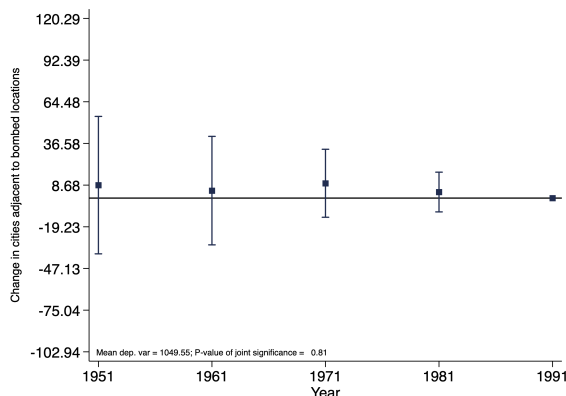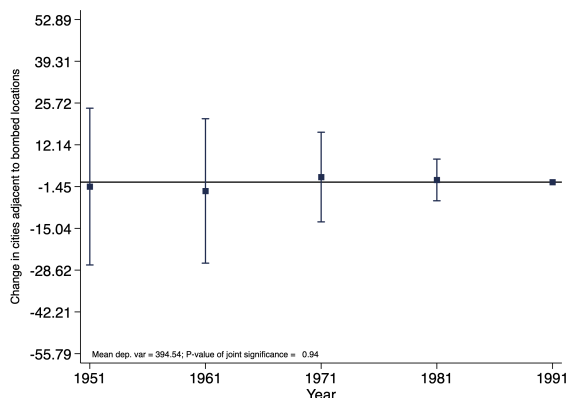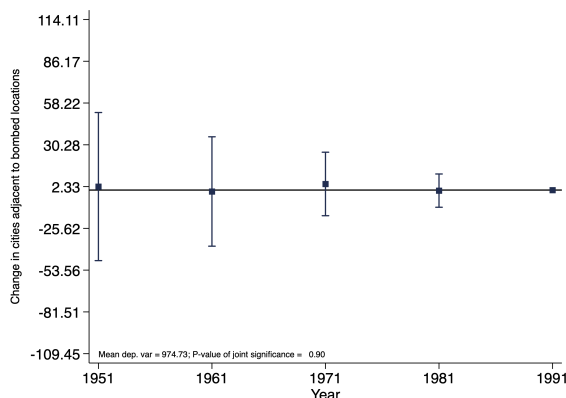
B. Employed women

C. Men in labor force

D. Employed men

E. Gender gap in labor force
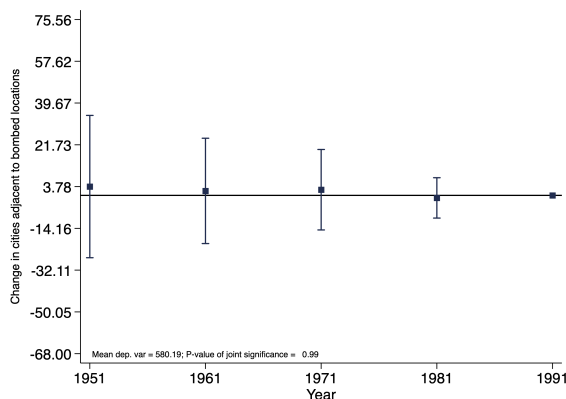
F. Gender gap in employment

Notes: These graphs show the interaction of Near bombed$_m$ with year fixed effects. The omitted year is 1991. All specifications include municipal fixed effects, region-year trends, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors are clustered at the city level. The vertical bars measure 90 percent CIs. Source: Istituto Nazionale della Previdenza Sociale (INPS).

Figure A6: 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 A8 provides more evidence on the remaining age bins. The regressions also include the pairwise interactions between the main variables, city fixed effects, age-year fixed effects, region-year fixed effects, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors are clustered at the city level. The vertical bars measure 95 percent CIs. Source: Istituto Nazionale della Previdenza Sociale (INPS).

Figure A7: Event Studies, Municipal Balance Sheets and Services
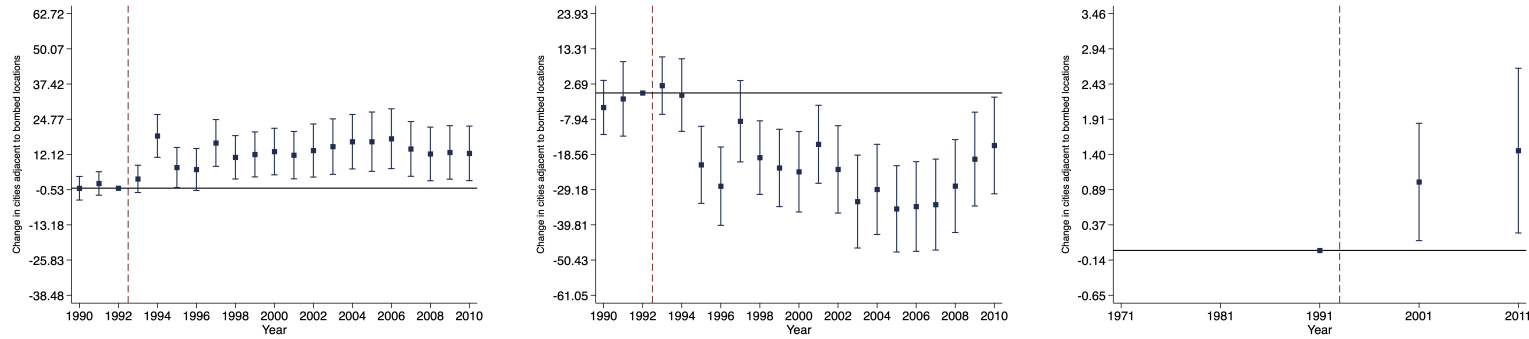
A. Revenues from local taxes   B. Revenues from gov. transfers   C. Pupils in nursery school

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, region-year trends, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors are clustered at the city level. The vertical bars measure 90 percent CIs. 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 A8: Women in the Workforce, Heterogeneity Based on Share of Young Parents
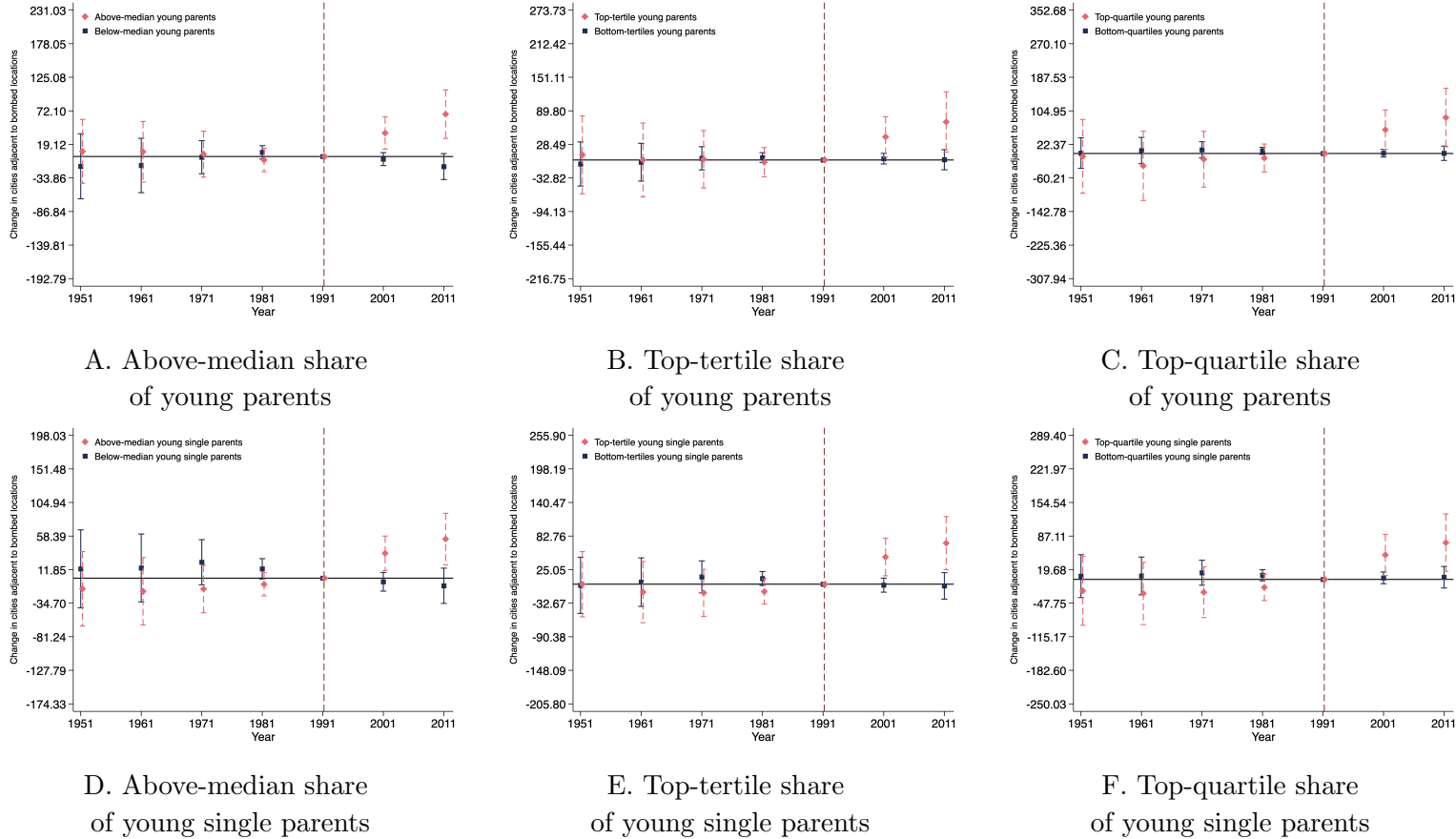
A. Above-median share
of young parents

B. Top-tertile share
of young parents

C. Top-quartile share
of young parents

D. Above-median share
of young single parents

E. Top-tertile share
of young single parents

F. Top-quartile share
of young single parents

Notes: These graphs show the post-LPT change in cities adjacent to municipalities bombed by Allied tactical air attacks during WWII, further distinguishing between cities with a high share of young parents (who therefore are more likely to have pre-kindergarten children) and cities with low share of young parents. Specifically, we use the share of residents who are below 35 years old and have children (Panels A to C) or the share of residents who are below 35 years old, are single, and have children (Panels D to F), computed relative to the total number of residents who are below 35 years old. The regressions also include city fixed effects, the dummy for a high share of young parents interacted with year fixed effects, region-year trends, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors are clustered at the city level. The vertical bars measure 90 percent CIs.

Figure A9: Effects of Fiscal Decentralization on Labor Markets, More Outcomes
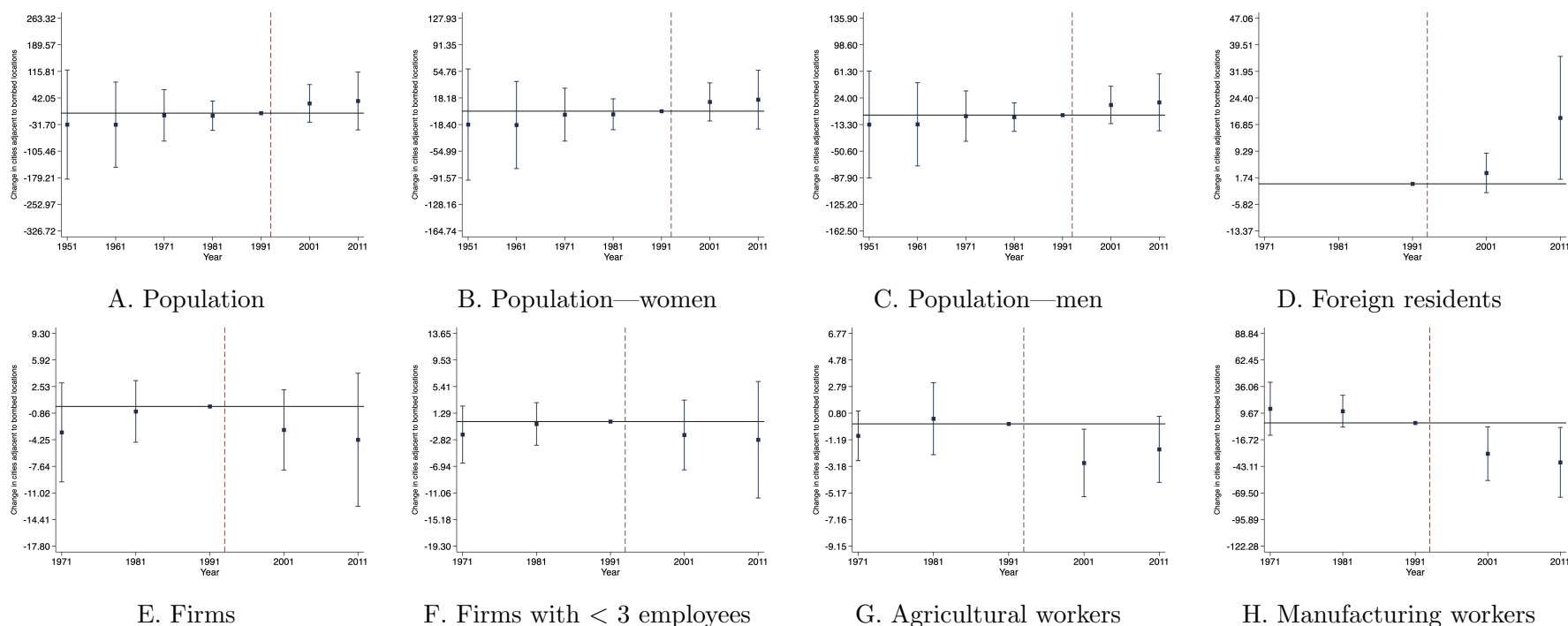
A. Population

B. Population—women

C. Population—men

D. Foreign residents

E. Firms

F. Firms with < 3 employees

G. Agricultural workers

H. 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 omitted year is 1991. The regressions also include city fixed effects, region-year trends, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors are clustered at the city level. The vertical bars measure 90 percent CIs.

Figure A10: Yearly Effects of Fiscal Decentralization, Instrumental Variables
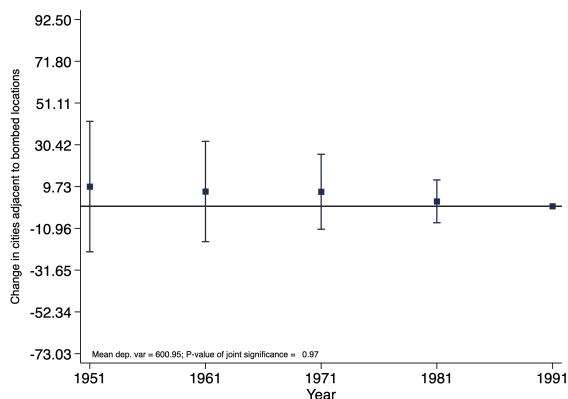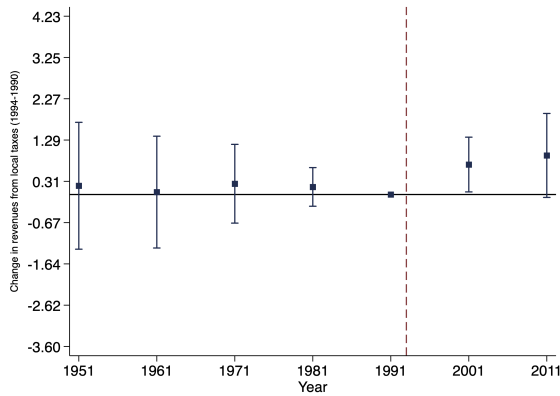

A. Women in labor force
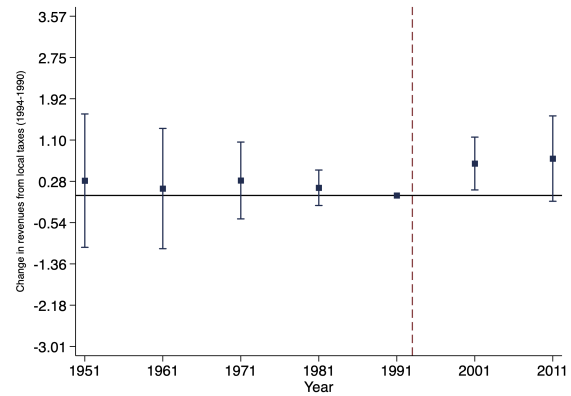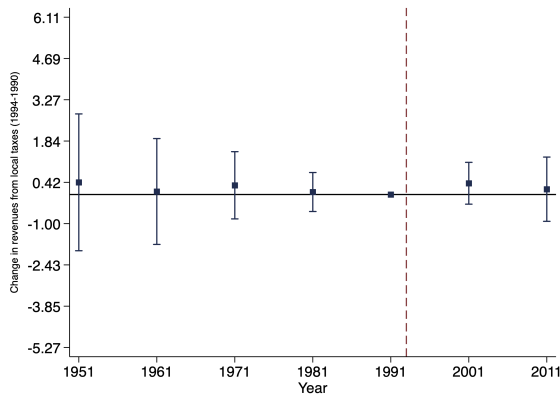
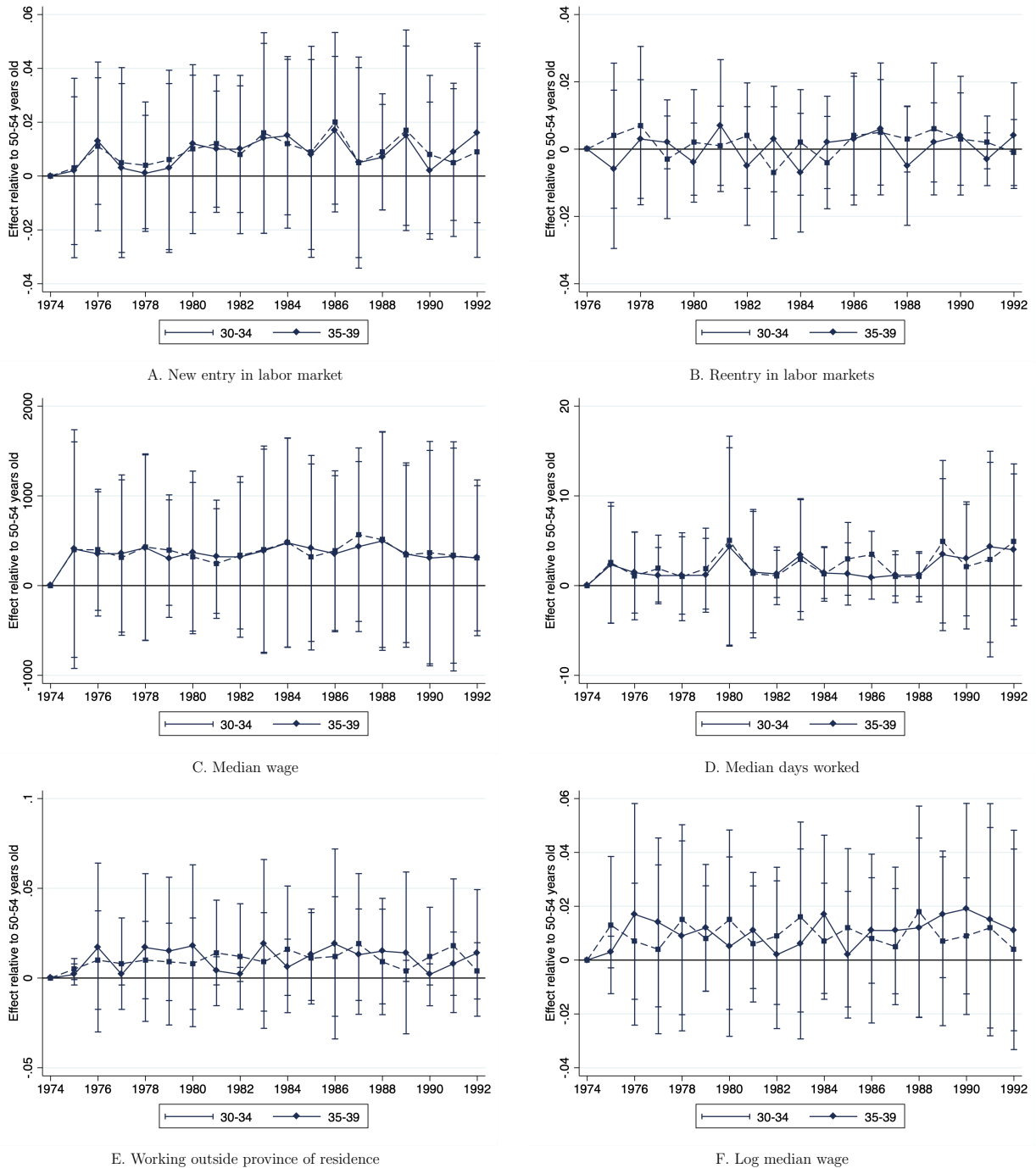B. Employed women
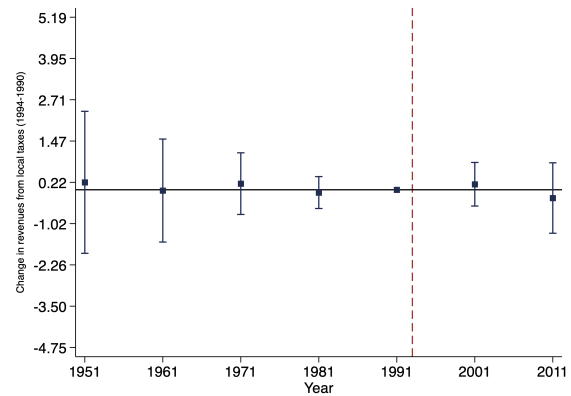
C. Men in labor force

D. Employed men
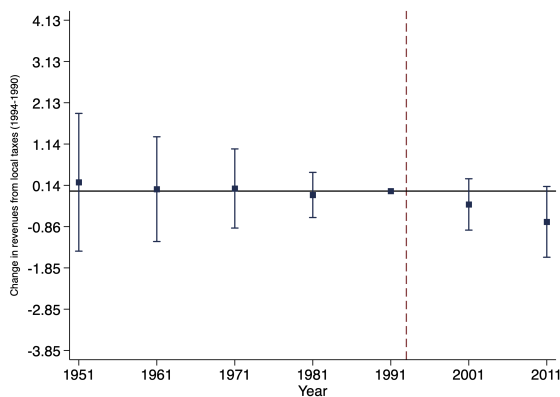
E. Gender gap in labor force
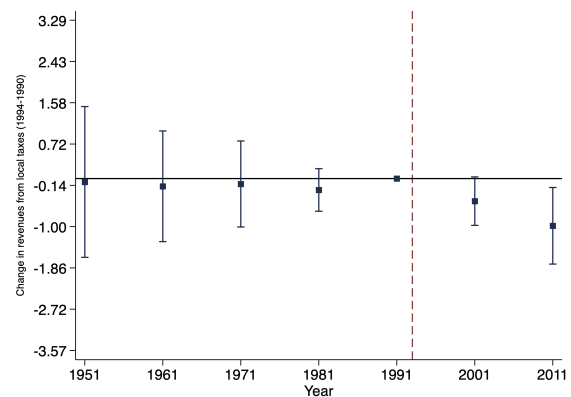
F. Gender gap in employment

Notes: The coefficients show the effect of an increase (€1) in the per-capita 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 omitted year is 1991. The regressions also include city fixed effects, region-year trends, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors are clustered at the city level. The vertical bars measure 90 percent CIs.

A10

Figure A11: Heterogeneity Based on Share of Owner-Occupied Buildings
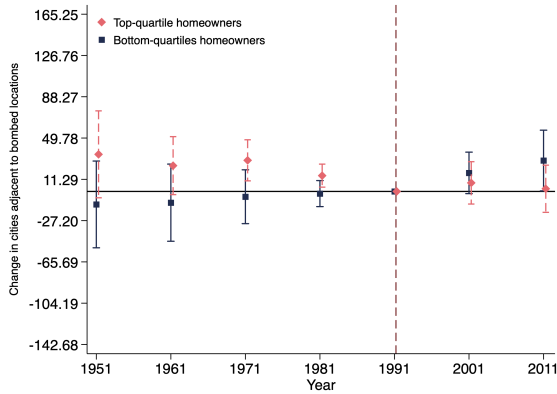
A. Women in labor force

B. Employed women

C. Men in labor force

D. Employed men

E. Gender gap in labor force
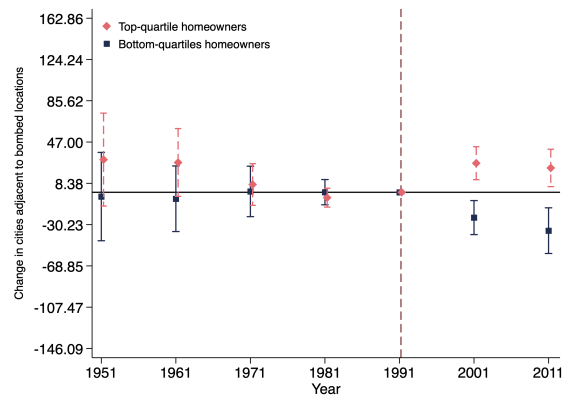
F. Gender gap in employment

Notes: These graphs show the post-LPT change in cities adjacent to municipalities bombed by Allied tactical air attacks during WWII, further distinguishing between cities in the top quartile of the share of owner-occupied buildings in 1991 and cities in the bottom three quartiles. The omitted year is 1991. The regressions also include city fixed effects, top-quartile-year fixed effects, region-year trends, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors are clustered at the city level. The vertical bars measure 90 percent CIs. Source: 8mila Census, ISTAT, available online at http://ottomilacensus.istat.it/.

A11

Figure A12: Women in the Workforce, Heterogeneity Based on Political and Municipal Competition

A. Women's participation—
Political competition

B. Women's participation—
Municipal competition

C. Women's participation—
Local preferences

D. Women's employment—
Political competition

E. Women's employment—
Municipal competition

F. Women's employment—
Local preferences

Notes: Panels A and D show heterogeneity effects in women's labor-force participation and employment based on the mean number of mayoral terms after 1993. Panels B and E show heterogeneity effects based on the number of adjacent municipalities. Panels C and F show heterogeneity effects based on the share of income earners with yearly taxable income below €15,000. Although shown in different panels for the sake of clarity, these three heterogeneity variables are simultaneously interacted with "Near bombed" and year fixed effects in the same specification. The regressions also include city fixed effects, the new heterogeneity variables interacted with year fixed effects, region-year trends, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors are clustered at the city level. The vertical bars measure 90 percent CIs.

Figure A13: Probability of Having a Child Below Three Years Old, SHIW

A. 1977-1993

B. 1977-2010
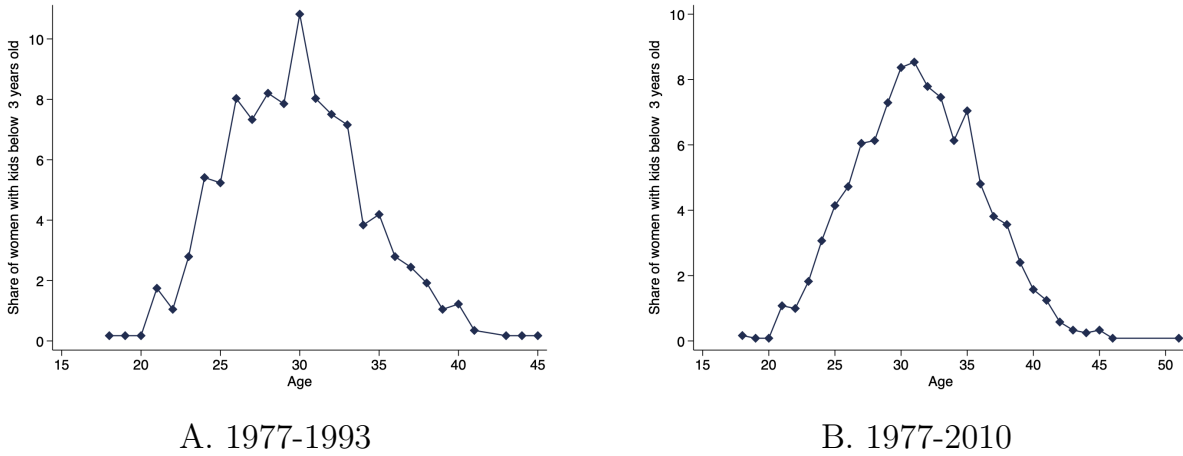
Notes: These graphs show the probability of having a child below three years old for women working in the private sector (qualp3==1 and settp9!=8 in the SHIW data). The data comes from sequential waves of the Bank of Italy's Survey of Household and Income Wealth, a representative survey of the Italian population. Panel A stops before the full implementation of the LPT, while panel B shows data from all the waves until 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 A14: Probability of Having a Parent $\geq$ 80 Years Old

A. Father

B. At Least One Parent
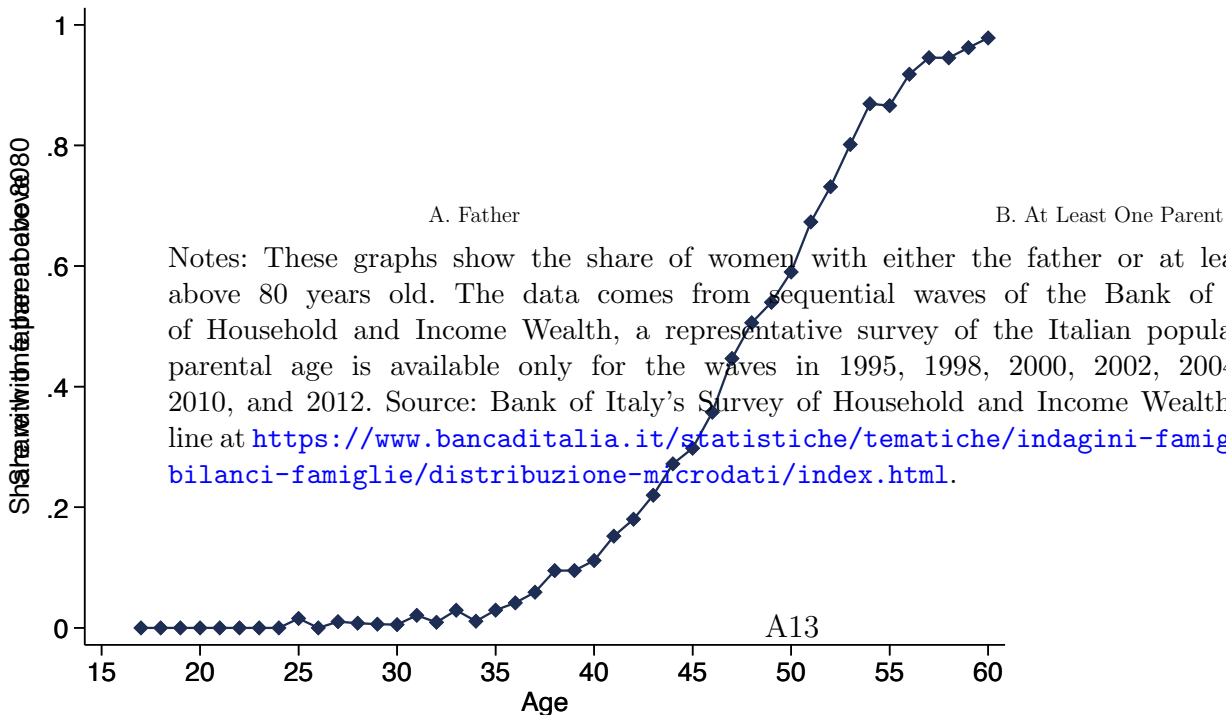
Notes: These graphs show the share of women with either the father or at least one parent above 80 years old. The data comes from sequential waves of the Bank of Italy's Survey of Household and Income Wealth, a representative survey of the Italian population. Data on parental age is available only for the waves in 1995, 1998, 2000, 2002, 2004, 2006, 2008, 2010, and 2012. 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.

A13

Figure A15: 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
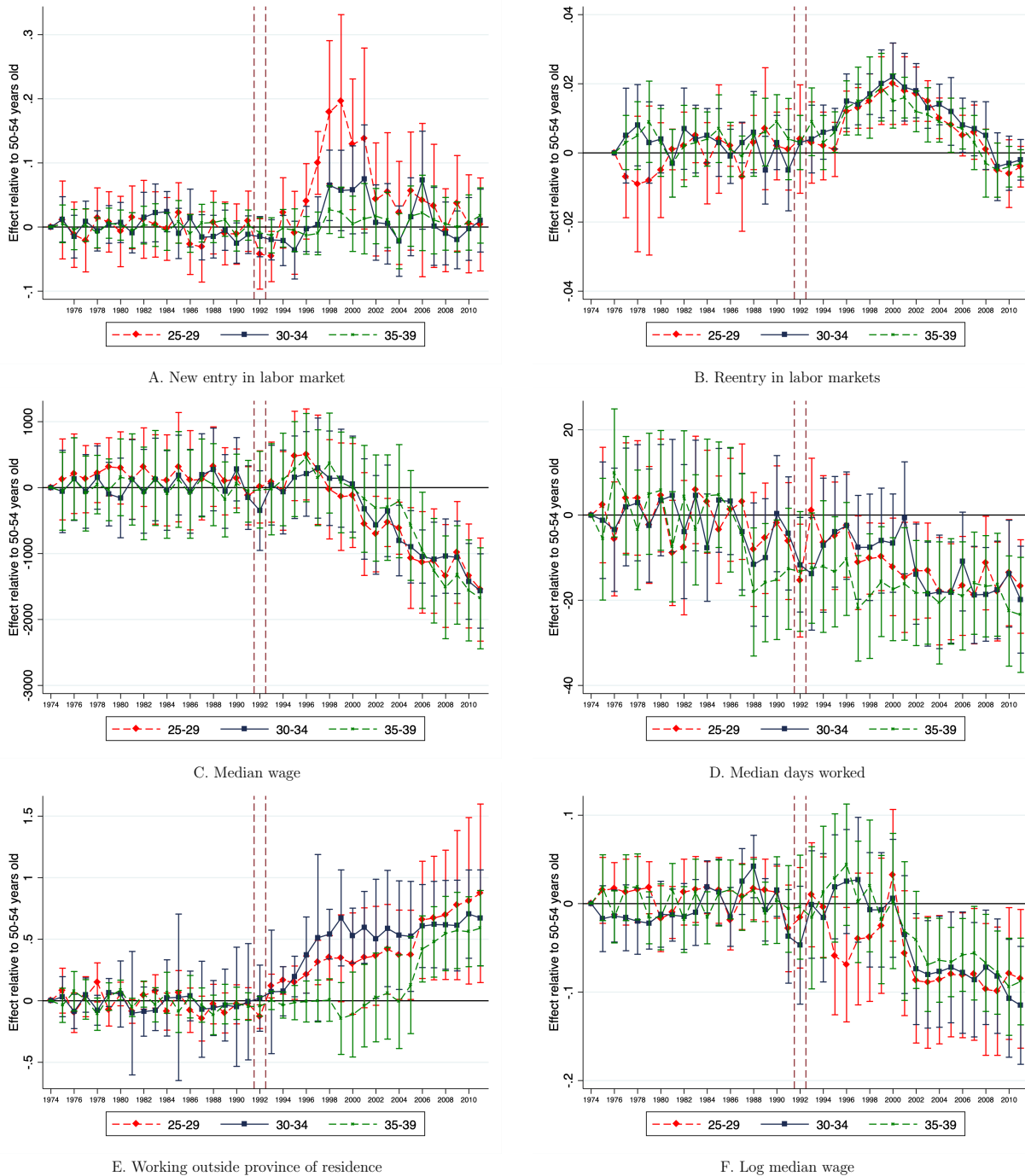
Notes: These graphs show triple interactions of age bins, a dummy equal to 1 for near-bombed locations, and year dummies. The sample includes only women. The control group is composed of municipalities adjacent to cities matched to bombed locations. For sake of clarity, the graphs shows the coefficients for only three age bins (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, age-year fixed effects, region-year fixed effects, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors are clustered at the city level. The vertical bars measure 95 percent CIs. Source: Istituto Nazionale della Previdenza Sociale (INPS).

Figure A16: Number of Municipal Elections by Date
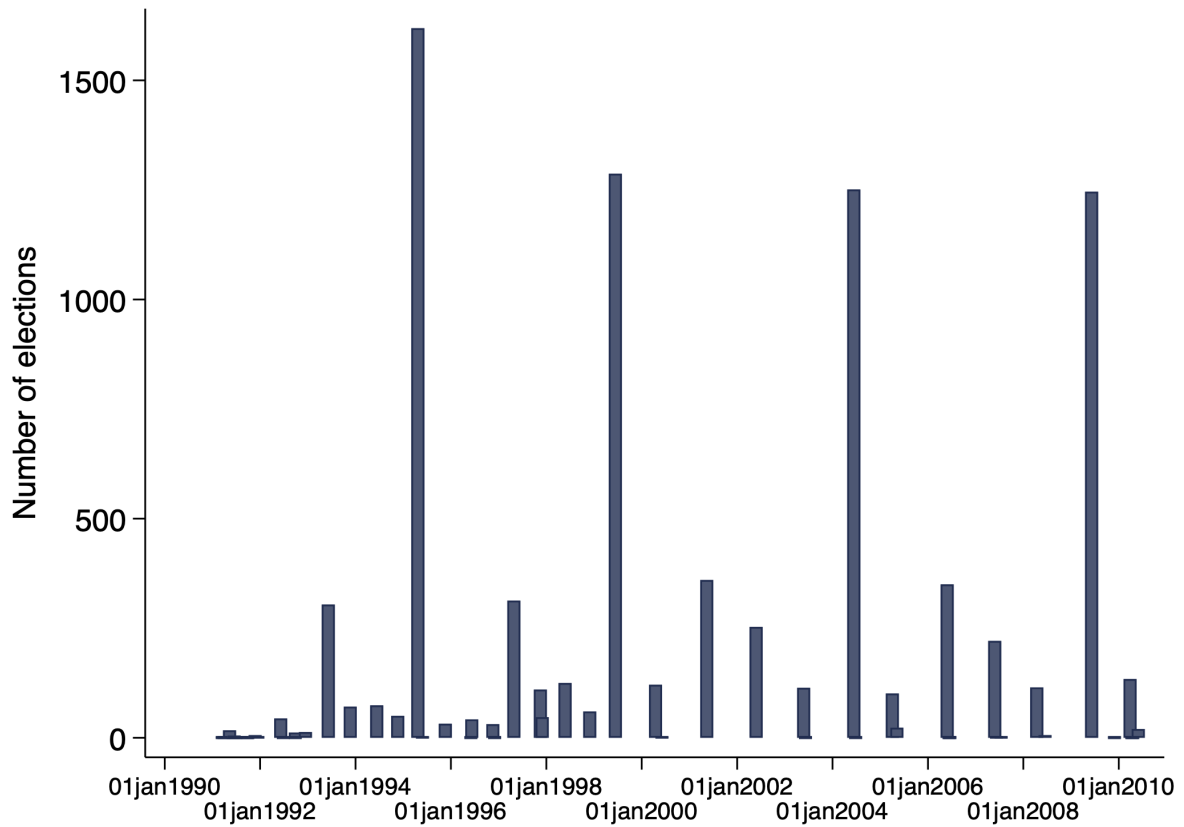
Notes: This graph shows the number of municipal elections by date in the estimating sample (near-bombed and near-others municipalities). Data before 1993 is likely incomplete. Source: Italian Minister of the Interior, available online at https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4.

Figure A17: Women in the Workforce, Controlling for Marshall Plan

A. Women's participation—
Total MP aid

B. Women's participation—
Reconstruction of buildings

C. Women's participation—
Reconstruction of private buildings

D. Women's employment—
Total MP aid

E. Women's employment—
Reconstruction of buildings

F. Women's employment—
Reconstruction of private buildings
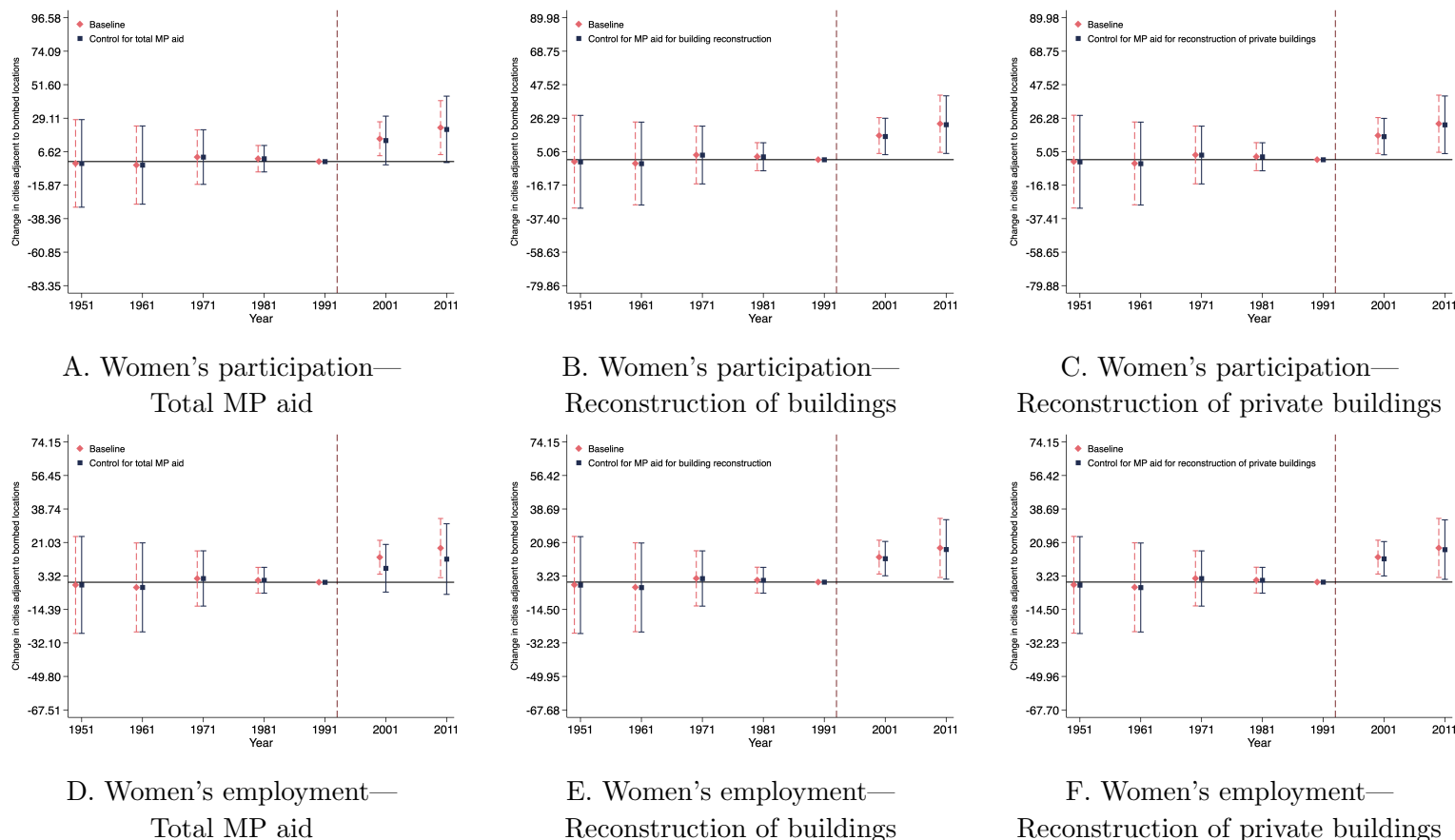
Notes: In Panels A and D, the regressions include the total amount of aid received by a province through the Marshall Plan. Aid is aggregated at the province level because none of the municipalities in the sample (near-bombed and near-others) directly received grants. In Panels B and E, the regressions include the amount of aid received by a province through the Marshall Plan to reconstruct public and private buildings. In Panels C and F, the regressions include the amount of aid received by a province through the Marshall Plan to reconstruct only private buildings. The regressions also include city fixed effects, region-year trends, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors are clustered at the city level. The vertical bars measure 90 percent CIs.

Figure A18: Effects of Fiscal Decentralization on Labor Markets, Matching Layer 1
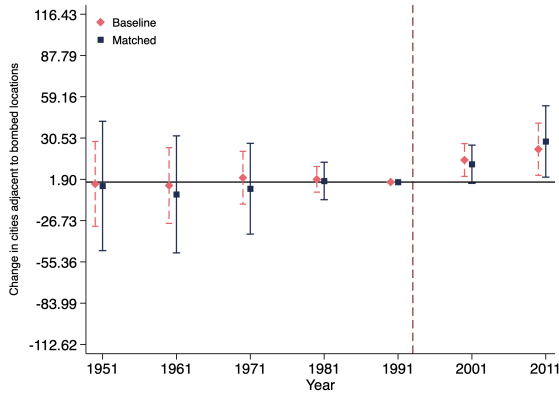
A. Women in labor force
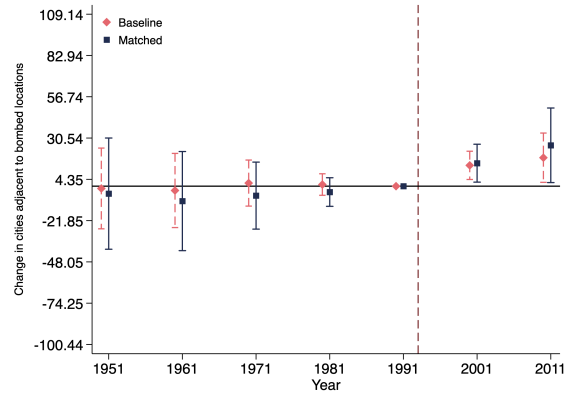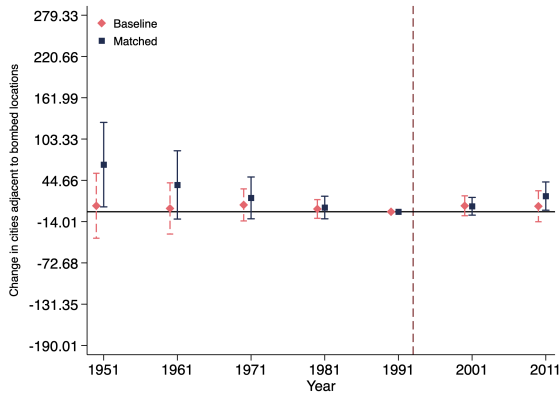
B. Employed women

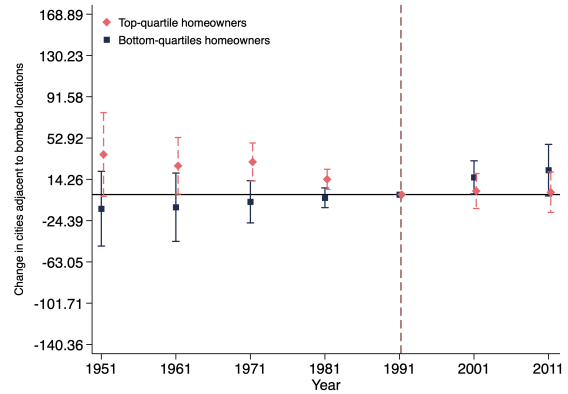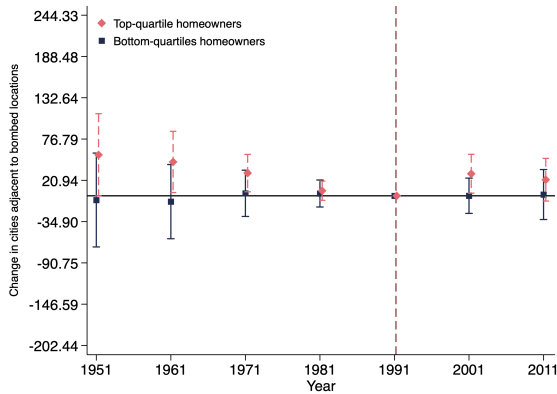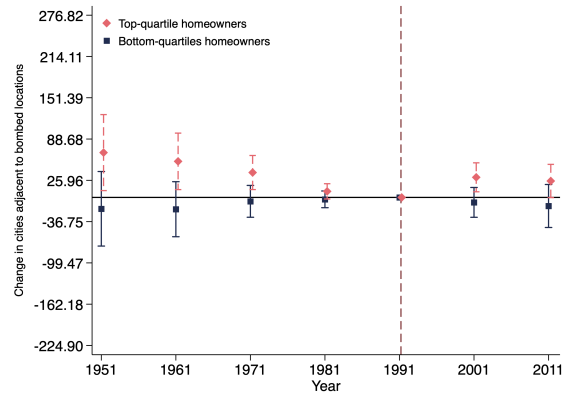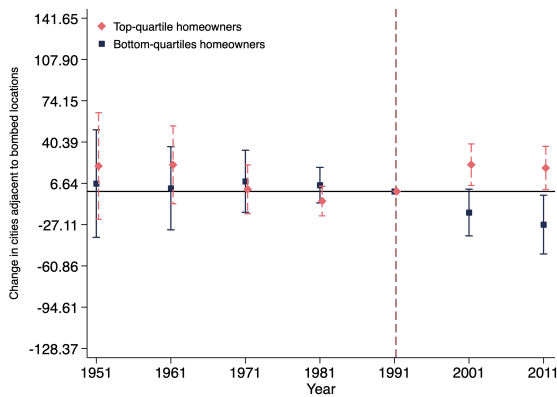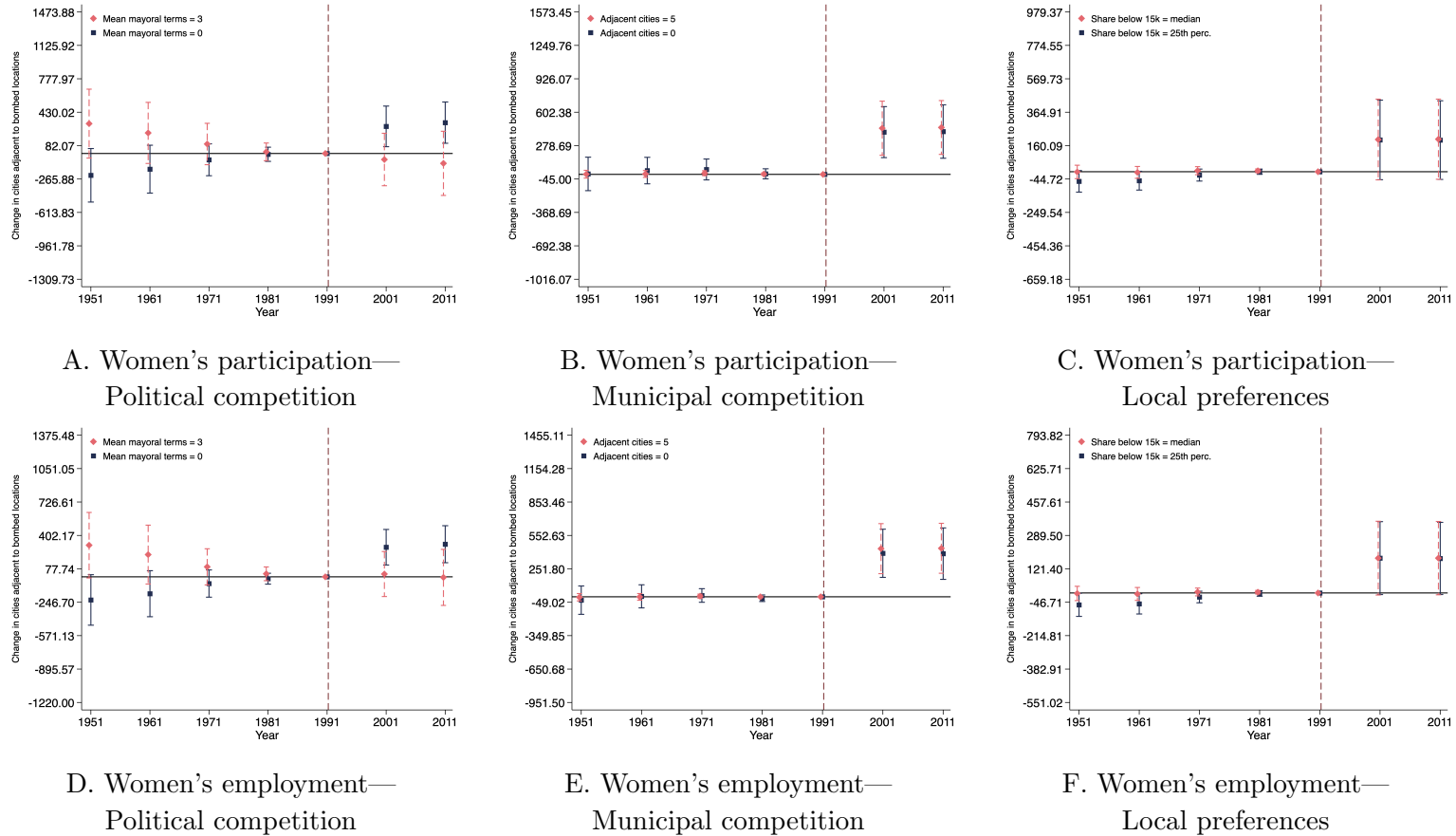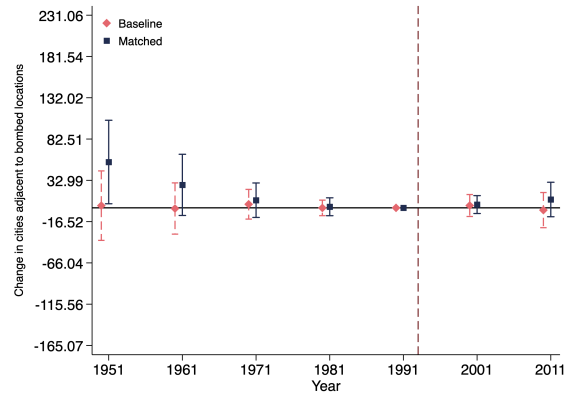C. Men in labor force

D. Employed men
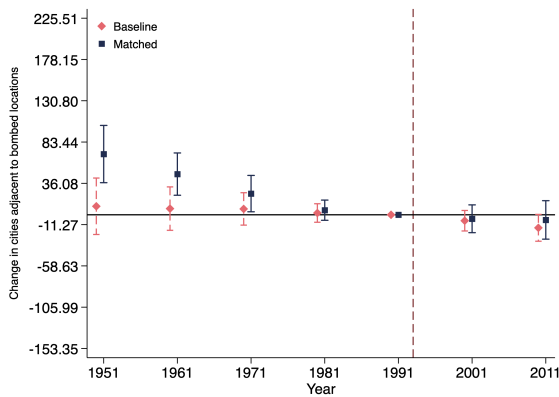
E. Gender gap in labor force
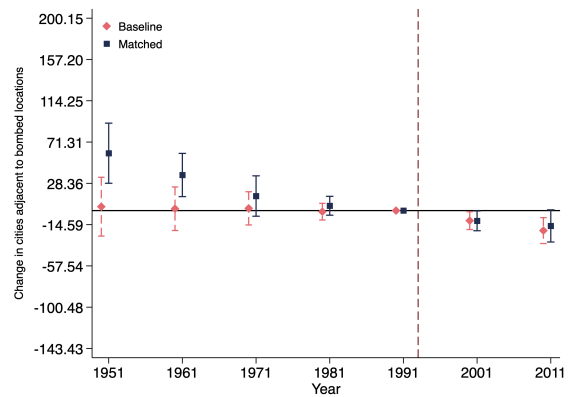
F. Gender gap in employment

Notes: 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, region-year trends, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors are clustered at the city level. The vertical bars measure 90 percent CIs.

Figure A19: Robustness of Matching Algorithm

A. Women's participation
Stratified by macro regions

B. Women's participation
Stratified by regions

C. Women's participation
24 variables

D. Women's participation
33 variables

E. Women's employment
Stratified by macro regions

F. Women's employment
Stratified by regions

G. Women's employment
24 variables

H. Women's employment
33 variables
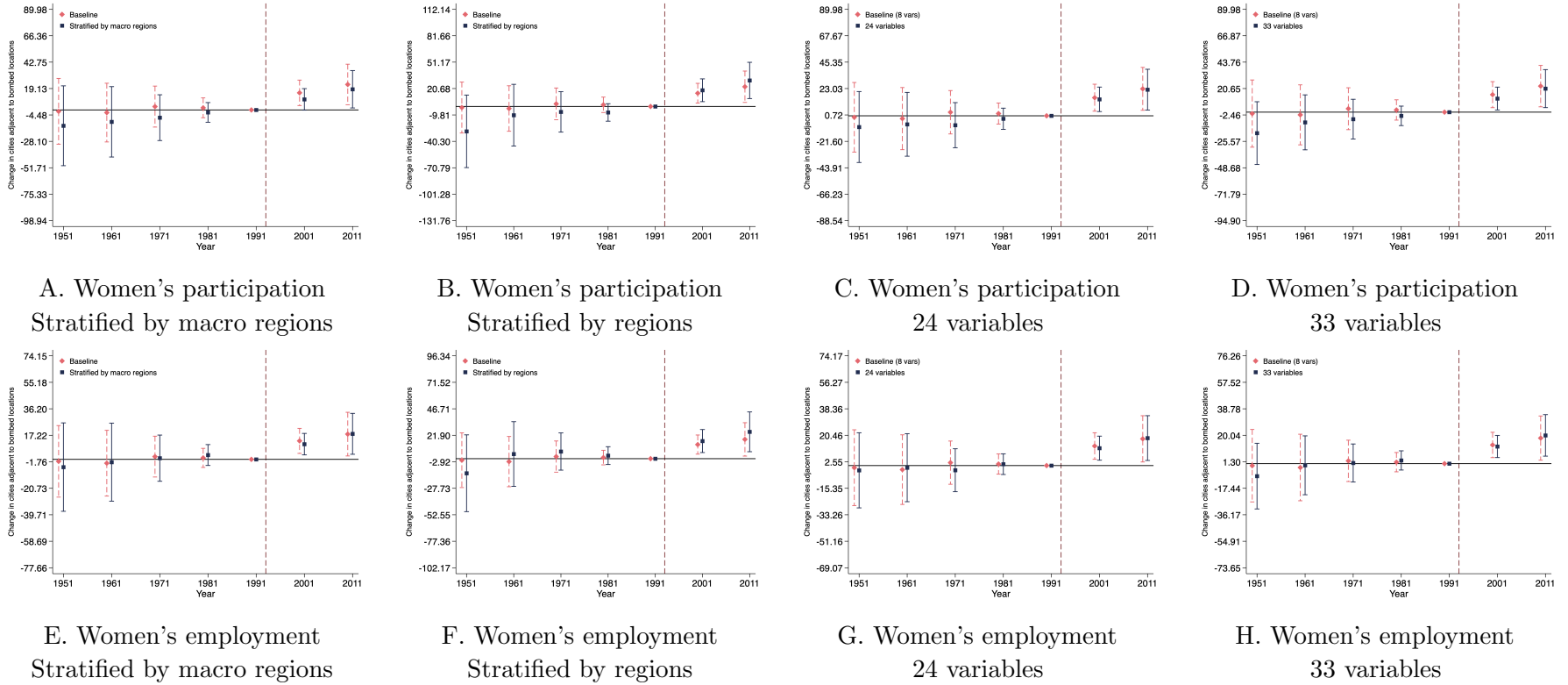
Notes: These graphs show the robustness of the initial matching algorithm between bombed and nonbombed locations. "Stratified by macro regions:" locations are first grouped by five macro regions and then matched on observables (same 8 vars used in baseline). "Stratified by regions:" locations are first grouped by twenty regions and then matched on observables (same 8 vars used in baseline). "24 variables:" 24 variables used for matching, instead of 8. "33 variables:" 33 variables used for matching, instead of 8. Full list of variables and propensity scores in Table A4. Standard errors are clustered at the city level. The vertical bars measure 90 percent CIs.

Figure A20: Women's Labor-Force Participation, Various Robustness Checks

A. Cluster at
province level

B. Spatial HAC
standard errors

C. Share of
women in labor force

D. Province-year
fixed effects

E. More geographic
trends

F. More RE
trends

G. No special-admin.
regions

H. Placebo
effects
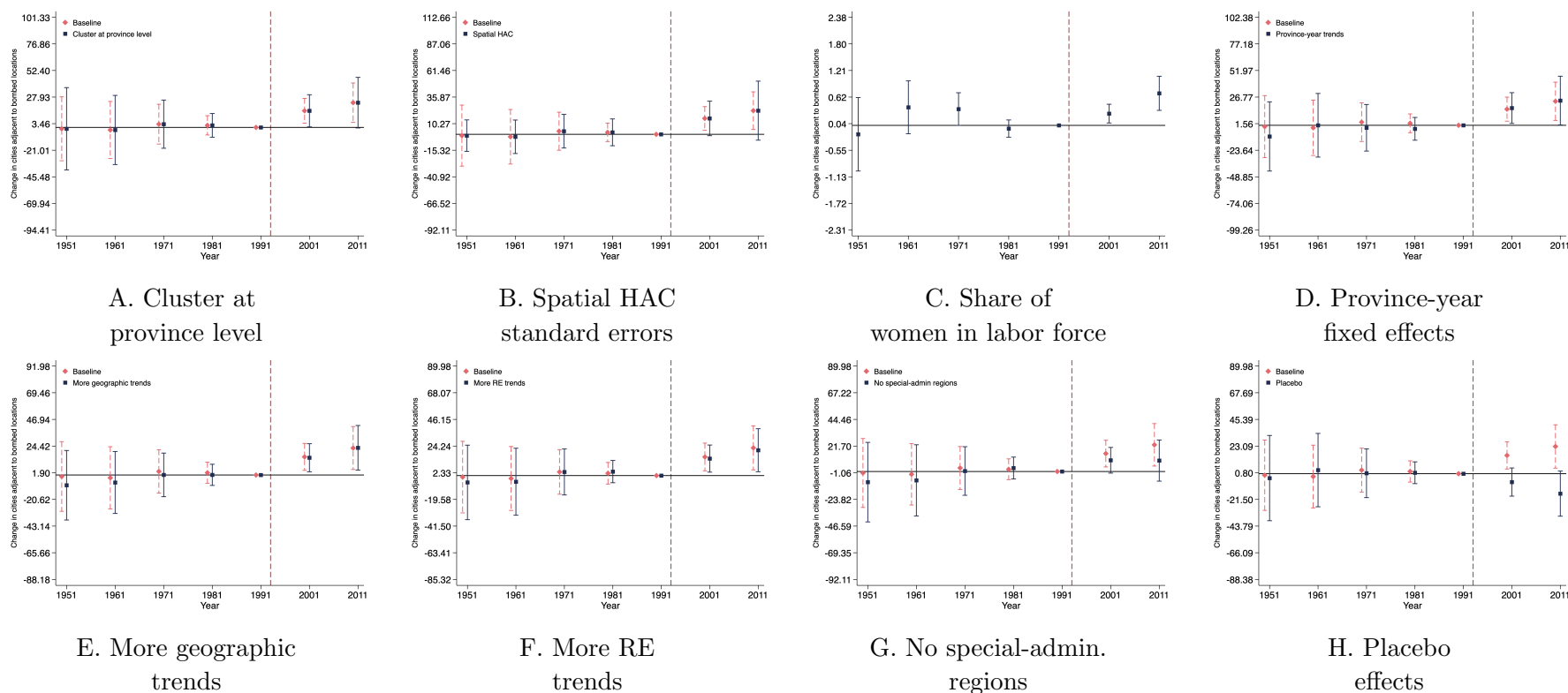
Notes: The dependent variable is women's labor-force participation. "Cluster at province level:" stand. errors clustered at the province level, rather than at the city level. "Spatial HAC:" standard errors corrected for spatial correlation among municipalities that are within 1,000km of each other and for autocorrelation for up to 20 years. "Share of women in labor force:" dep.var. is share of women in labor force, not absolute number. "Province-year FE:" region-year trends are replaced with province-year trends. "More geographic trends:" regressions include geographical variables (a dummy for mountain municipalities and a dummy for coastal cities) interacted with year fixed effects. "More RE trends:" 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. "No special-admin regions:" it excludes from the sample five special-administration regions (Valle d'Aosta, Trentino, Friuli, Sicilia, and Sardegna) that enjoy more autonomy. "Placebo:" the treatment variable "Near bombed" is assigned at random. The vertical bars measure 90 percent CIs.

A19

Table A1: Additional Summary Statistics from Balance Sheets

| | All years | | | |
| | Mean | Std. Dev. | Obs. | Availability |
	(1)	(2)	(3)	(4)
Panel A: 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
Panel B: 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
Panel C: Other variables				
Spend. on nursery schools (%)	1.15	2.10	95642	1998-2010
Rev. from nursery schools (%)	1.55	4.50	92504	1998-2010

Notes: This table shows additional summary statistics 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.

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 per-capita revenues from local taxes between 1990 and 1994							
Share of pre-WWII buildings	-1.067***	-1.094***	-1.371***	-1.242***	-0.991***	-1.065***	-0.865***
	(0.157)	(0.155)	(0.173)	(0.171)	(0.155)	(0.160)	(0.150)
Controls	Region	Province	(2) +	(3) +	(4) +	(5) +	(6) +
	FE	FE	building size	building qual.	geography	demography	economy
Observations	7,400	7,400	7,393	7,393	7,393	7,390	7,390
R^2	0.167	0.228	0.266	0.275	0.315	0.327	0.354
Dep. var.—mean	129.71	129.71	129.75	129.75	129.75	129.69	129.69
Dep. var.—std. dev.	123.29	123.29	123.29	123.29	123.29	123.22	123.22
Pre-WWII buildings—mean	39.97	39.97	39.99	39.99	39.99	39.99	39.99
Pre-WWII buildings—std. dev.	19.03	19.03	19.02	19.02	19.02	19.02	19.02
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 per-capita 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: Correlation between Allied Bombings and Effect of the Policy

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Dependent variables are share of prewar buildings (1-3) and change in per-capita revenues from local taxes (4-6)						
IHST post-armistice bombs	-1.539***	-1.449***	-1.312***	6.694***	4.252***	3.747***
	(0.260)	(0.203)	(0.187)	(1.557)	(1.121)	(1.124)
Controls	No	Region FE	Province FE	No	Region FE	Province FE
Dep. var.	Share of prewar buildings	Share of prewar buildings	Share of prewar buildings	Change in revenues	Change in revenues	Change in revenues
Observations	7,958	7,958	7,958	7,355	7,355	7,355
Dep. var.—mean	40.52	40.52	40.52	129.19	129.19	129.19
Dep. var.—std. dev.	19.03	19.03	19.03	123.61	123.61	123.61
Panel B: Dependent variable is number of women in labor force—1951-2011						
IHST post-armistice bombs x Post	11.641*	16.066***				
	(6.426)	(6.149)				
Δ Rev. local tax (94-90) x Post			17.355***	25.101***	7.724	12.463
			(2.458)	(4.872)	(6.234)	(9.680)
Specification	OLS	OLS	IV	IV	IV	IV
Controls	Region-Year + other trends	Province-Year + other trends	City + Year FE	Region-Year	Region-Year + other trends	Province-Year + other trends
Observations	54,705	54,705	51,056	51,056	50,589	50,589
Dep. var.—mean	523.61	523.61	535.30	535.30	535.30	535.30
Dep. var.—std. dev.	852.70	852.70	871.24	871.24	871.24	871.24
Panel C: Dependent variable is number of women with jobs—1951-2011						
IHST post-armistice bombs x Post	13.581**	15.694***				
	(5.688)	(5.467)				
Δ Rev. local tax (94-90) x Post			15.430***	21.788***	9.318	12.524
			(2.149)	(4.233)	(6.519)	(9.408)
Specification	OLS	OLS	IV	IV	IV	IV
Controls	Region-Year + other trends	Province-Year + other trends	City + Year FE	Region-Year	Region-Year + other trends	Province-Year + other trends
Observations	54,705	54,705	51,056	51,056	50,589	50,589
Dep. var.—mean	454.92	454.92	465.34	465.34	465.34	465.34
Dep. var.—std. dev.	729.82	729.82	746.78	746.78	746.78	746.78
Panel D: Pupils in nursery school—1991-2011						
IHST post-armistice bombs x Post	0.509***	0.464**				
	(0.196)	(0.185)				
Δ Rev. local tax (94-90) x Post			0.418***	0.537***	0.377	0.392
			(0.059)	(0.108)	(0.244)	(0.291)
Specification	OLS	OLS	IV	IV	IV	IV
Controls	Region-Year + other trends	Province-Year + other trends	City + Year FE	Region-Year	Region-Year + other trends	Province-Year + other trends
Observations	23,646	23,646	22,065	22,065	21,861	21,861
Dep. var.—mean	12.46	12.46	12.86	12.86	12.86	12.86
Dep. var.—std. dev.	26.92	26.92	27.52	27.52	27.52	27.52

Notes: In panel A, the share of prewar buildings in 1991 (columns 1-3) and the change in per-capita revenues from local taxes between 1990 and 1994 (columns 4-6) are regressed on the inverse hyperbolic sine transformation (IHST) of post-armistice Allied bombings. In the other panels, several municipal outcomes are regressed either on the IHST of the tons of post-armistice Allied bombings (columns 1 and 2) or on the change in per-capita revenues from local taxes between 1990 and 1994 instrumented with the IHST of the tons of post-armistice Allied bombings (columns 3-6). In these panels, both variables are interacted with a Post dummy that is equal to 1 after 1993. All specifications include municipal fixed effects. "Other trends" include year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Among municipalities with bombings, the 25th percentile of the tons of bombings is equal to 39 tons, while the median is equal to 114 tons. The 25th percentile of the change in revenues is equal to €62, while the median is equal to €113. Standard errors clustered at the province level in panel A and at the city level in the other panels, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

	Bombed after armistice (1)	Bombed after armistice (2)	Bombed after armistice (3)	Bombed after armistice (4)	Bombed after armistice (5)	Bombed after armistice (6)	Bombed after armistice (7)	Bombed after armistice (8)
Population (per 1000 residents)	0.01337	0.41490	0.10086	-0.05590	-0.19711	0.34122**	0.06332	-0.00050
	(0.04016)	(0.57610)	(0.12695)	(0.20323)	(0.14614)	(0.15183)	(0.05699)	(0.06090)
Population^2 (per 1000 residents)	-0.00073***	-0.00198	-0.00002	-0.00083	-0.00096***	-0.00112***	-0.00048**	-0.00040**
	(0.00015)	(0.00223)	(0.00072)	(0.00087)	(0.00034)	(0.00033)	(0.00019)	(0.00019)
Area (km^2)	0.01867***	0.02341	0.01635	0.01731***	0.03063***	0.02348***	0.01878***	0.02176***
	(0.00249)	(0.02418)	(0.01173)	(0.00638)	(0.00853)	(0.00271)	(0.00271)	(0.00282)
Area^2	-0.00007***	-0.00002	-0.00009	-0.00004	-0.00017**	-0.00014***	-0.00007***	-0.00008***
	(0.00001)	(0.00016)	(0.00006)	(0.00003)	(0.00007)	(0.00004)	(0.00001)	(0.00001)
Population density	-0.00021**	-0.00048	0.00038	0.00040	-0.00048**	-0.00046***	-0.00024***	-0.00013
	(0.00009)	(0.00128)	(0.00067)	(0.00071)	(0.00024)	(0.00017)	(0.00009)	(0.00009)
Number of buildings	0.00020*	-0.00072	-0.00024	0.00025	0.00089**	-0.00077*	0.00001	0.00018
	(0.00011)	(0.00176)	(0.00042)	(0.00059)	(0.00040)	(0.00046)	(0.00018)	(0.00019)
Share owner-occupied properties	-0.03673***	-0.10702**	-0.02510*	-0.07667***	-0.02279**	-0.03623***	-0.03472***	-0.03416***
	(0.00452)	(0.04290)	(0.01463)	(0.01514)	(0.00650)	(0.01115)	(0.00504)	(0.00532)
Share of population < 3 years old	-0.14224**	0.08819	-0.29518	-0.13009	-0.31578***	0.14879	-0.09830	-0.11385
	(0.05818)	(0.29196)	(0.19384)	(0.14495)	(0.09216)	(0.12324)	(0.07244)	(0.07607)
Coastal city							-0.04223	-0.02966
							(0.15765)	(0.16010)
Rural city							0.00511	0.01615
							(0.11460)	(0.11798)
Share women							0.04242	0.04230
							(0.02604)	(0.02767)
Foreign residents							-0.00200*	-0.00207*
							(0.00113)	(0.00115)
Share over 65							0.00046	0.02216
							(0.01036)	(0.01399)
Births							-0.01200*	-0.00816
							(0.00615)	(0.00637)
Pupils in nursery school							-0.00324	-0.00429
							(0.00318)	(0.00326)
Residents in labor force							0.00007	0.00006
							(0.00010)	(0.00010)
Firms							0.00031	0.00011
							(0.00036)	(0.00036)
Employees							-0.00002	0.00003
							(0.00006)	(0.00007)
Share of agricultural firms							0.01697	0.00457
							(0.02247)	(0.02296)
Share of manufacturing firms							-0.02346***	-0.02398***
							(0.00787)	(0.00816)
Share of retail firms							0.00805	0.00822
							(0.00775)	(0.00805)
Share of agricultural workers							-0.02321	-0.02248
							(0.02132)	(0.02129)
Share of manufacturing workers							0.01154***	0.00725*
							(0.00407)	(0.00423)
Share of retail workers							0.00263	-0.00080
							(0.00756)	(0.00791)
Vote share for DC								-0.79047
								(0.52371)
Vote share for extreme left								1.41139
								(1.52504)
Vote share for extreme right								1.74578
								(1.50919)
Winner DC (dummy)								0.16362
								(0.29661)
Winner center-left (dummy)								0.19859
								(0.30175)
Winner Lega Nord (dummy)								-0.09863
								(0.30146)
Youth outside labor force								0.01079
								(0.01116)
m^2 per resident								-0.03108
								(0.01978)
Avg. size of buildings								0.03420***
								(0.00698)
Specification	Baseline	Stratified by macro regions	Stratified by macro regions	Stratified by macro regions	Stratified by macro regions	Stratified by macro regions	More controls	More controls
Municipalities	All	Center	Islands	Northeast	Northwest	South	All	All
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,926	236	550	299	1,095	746	2,902	2,843

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)." One matching algorithm is not reported in this table due to lack of space. In addition to stratifying the observations by five macro regions (columns 2 to 6), we stratify them by twenty regions. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table A5: Differences in Observable Characteristics Measured in 1991

	Bombed	Near bombed vs. Near others	Matched adjacent	Mean	Std. dev.		Bombed	Near bombed vs. Near others	Matched adjacent	Mean	Std. dev.
	(1)	(2)	(3)	(4)	(5)		(1)	(2)	(3)	(4)	(5)
Panel A: Age of buildings and change in fiscal federalism											
Share of pre-WWII buildings	-5.030***	-2.670***	-2.332**	39.48	18.27	Δ Rev. local tax (94-90)	4.157	23.994***	25.754***	135.47	126.81
	(1.360)	(0.974)	(1.065)				(11.689)	(6.263)	(6.570)		
Panel B: Economic, demographic, geographical, and ideological characteristics of Italian municipalities											
Population in urban areas (%)	-1.403	-0.450	0.322	17.26	17.76	Coastal city	-0.051*	-0.019	-0.016	0.12	0.32
	(1.572)	(1.152)	(1.063)				(0.027)	(0.013)	(0.014)		
Dist. to econ. center (km)	-5.271*	-2.450	-1.838	38.10	22.91	Aging index	-2.011	-7.522	-8.705	144.30	125.28
	(3.026)	(1.515)	(1.513)				(11.090)	(7.336)	(8.126)		
Share below 6	0.233**	-0.027	-0.061	5.54	1.70	Share above 75	-0.302	-0.286	-0.153	8.37	3.57
	(0.108)	(0.084)	(0.091)				(0.268)	(0.191)	(0.201)		
Women in labor force	-0.565	11.504	5.246	1020.84	1524.11	Men in labor force	-14.554	4.314	1.110	1881.58	2894.92
	(47.445)	(12.508)	(12.977)				(24.615)	(6.170)	(7.380)		
Women with jobs	6.776	28.415	18.348	783.84	1181.45	Men with jobs	10.848	41.300*	30.279	1576.50	2306.98
	(59.882)	(19.357)	(19.135)				(60.633)	(20.887)	(20.673)		
Gender gap in labor force	-13.989	-7.190	-4.136	860.74	1492.29	Gender gap in employment	4.072	12.885*	11.931	792.66	1251.64
	(38.415)	(11.779)	(12.487)				(25.893)	(7.734)	(8.603)		
Firms with ≤ 2 employees	6.155	7.112	9.654*	288.00	382.15	Firms with ≥ 200 employees	0.042	0.007	0.003	0.07	0.36
	(11.866)	(5.738)	(5.367)				(0.028)	(0.009)	(0.011)		
Agricultural workers	-2.258	1.018	2.015*	11.54	36.19	Manufacturing workers	11.447	47.049	29.576	648.30	1185.20
	(2.557)	(0.990)	(1.176)				(93.055)	(38.533)	(36.083)		
Retail workers	-7.503	-3.275	-3.199	366.95	600.15	Real-estate workers	-7.589	0.919	0.479	85.21	137.41
	(19.803)	(9.732)	(10.098)				(11.678)	(3.495)	(3.619)		
Foreign residents	-4.628	-2.058	-3.452*	33.59	66.62	Share women	0.108	0.022	0.046	51.03	1.67
	(4.250)	(1.518)	(1.982)				(0.142)	(0.079)	(0.087)		
Pupils in nursery school	-0.039	-0.452	-0.883**	18.66	37.49	Residents with uni. degree	-0.731	0.183	-10.947	1270.76	2157.68
	(1.496)	(0.406)	(0.391)				(52.951)	(15.563)	(17.238)		
Household (HH) size	0.055**	0.025	0.022	2.74	0.38	m² per resident	0.497	0.406	0.471	35.24	5.38
	(0.023)	(0.017)	(0.017)				(0.388)	(0.257)	(0.304)		
Residents per building	-0.498	-0.210	-0.363	62.73	9.69	Inadequate housing (%)	0.307	0.142	0.127	0.20	1.93
	(0.612)	(0.457)	(0.519)				(0.201)	(0.087)	(0.078)		
HH in econ. distress (%)	-0.073	-0.079	-0.101	2.63	3.05	HH in need of caregiving (%)	-0.067	-0.041	0.045	2.13	1.07
	(0.140)	(0.099)	(0.111)				(0.073)	(0.066)	(0.063)		
Vote share for DC	-1.006	-0.387	-0.323	34.82	14.49	Vote share for extreme left	0.381*	0.172	0.077	4.60	3.34
	(1.363)	(0.660)	(0.873)				(0.217)	(0.173)	(0.205)		
Vote share for extreme right	0.254	0.098	0.038	4.13	3.07	Winner DC (dummy)	0.001	0.006	0.010	0.71	0.45
	(0.276)	(0.140)	(0.154)				(0.045)	(0.024)	(0.025)		
Winner center-left (dummy)	0.013	0.011	0.009	0.13	0.34	Winner Lega Nord (dummy)	-0.032	-0.023	-0.026	0.12	0.33
	(0.030)	(0.018)	(0.020)				(0.026)	(0.016)	(0.017)		
Panel C: Balance sheets of Italian municipalities											
Rev. local taxes (per cap.)	-5.923	14.704***	15.926**	163.98	124.57	Current spending (per cap.)	-76.690***	14.552	19.248	771.82	386.59
	(7.934)	(5.427)	(7.451)				(27.431)	(16.437)	(20.621)		
Rev. gov. transfers (per cap.)	-62.437***	-6.536	-4.974	511.76	225.80	Capital spending (per cap.)	-226.679***	21.835	55.606	597.58	804.55
	(16.566)	(12.012)	(13.905)				(69.586)	(38.170)	(39.280)		
Rev. local services (per cap.)	-10.188	7.746	15.794	175.97	209.28	Loan payments (per cap.)	1.051	-6.826	-5.675	93.51	117.96
	(17.703)	(7.458)	(9.552)				(11.626)	(4.750)	(6.311)		
Capital revenues (per cap.)	-136.131**	29.185	45.760	393.48	654.87	Total spending (per cap.)	-320.391***	45.972	86.683	1648.84	1188.24
	(55.982)	(28.148)	(29.585)				(91.731)	(62.458)	(66.808)		
Rev. loan servicing (per cap.)	-40.052*	-14.480	-9.262	205.74	276.74	Deficit (per cap.)	17.460	1.232	2.974	-9.44	128.05
	(23.249)	(13.891)	(16.433)				(11.618)	(3.109)	(3.716)		
Total revenues (per cap.)	-305.745***	51.047	92.594	1652.51	1190.91	Revenues over spending	0.001	0.001	0.003	0.99	0.05
	(89.769)	(62.011)	(66.193)				(0.004)	(0.002)	(0.002)		

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 per-capita revenues from local taxes between 1990 and 1994). In panel B, the dependent variables primarily come from the population census of 1991, the industrial census of 1991, and the national elections in 1992. In panel C, the dependent variables come from balance sheets of Italian municipalities. Regressions also include population, a dummy for rural cities, minimum altitude, and region fixed effects, as well as bombed-city fixed effects (columns 2 and 3). Standard errors clustered at the province level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Consequences of Allied Bombings in Bombing-Adjacent Municipalities

	(1)	(2)	(3)	(4)	(5)
Panel A: Dependent variable is share of prewar buildings in bombing-adjacent municipalities					
IHST post-armistice bombs	-0.794***	-0.605***	-0.605***	-0.799***	-0.447***
	(0.143)	(0.131)	(0.206)	(0.190)	(0.151)
Population in 1991					-0.001***
					(0.000)
Minimum altitude					0.007*
					(0.004)
Rural city					12.635***
					(1.517)
Controls	No	Region FE	Region FE	Province FE	Region FE
Cluster	Robust	Robust	Province	Province	Province
Observations	2,423	2,423	2,423	2,417	2,399
Dep. var.—mean	43.35	43.35	43.35	43.35	43.35
Dep. var.—std. dev.	19.19	19.19	19.19	19.19	19.19
Panel B: Dependent variable is change in per-capita revenues from local taxes (90-94) in bombing-adjacent municipalities					
IHST post-armistice bombs	4.580***	3.065***	3.065**	3.273***	4.198***
	(0.962)	(0.956)	(1.179)	(1.122)	(1.146)
Population in 1991					0.001**
					(0.000)
Minimum altitude					0.136***
					(0.023)
Rural city					-24.008***
					(8.612)
Controls	No	Region FE	Region FE	Province FE	Region FE
Cluster	Robust	Robust	Province	Province	Province
Observations	2,245	2,245	2,245	2,238	2,223
Dep. var.—mean	132.33	132.33	132.33	132.33	132.33
Dep. var.—std. dev.	120.59	120.59	120.59	120.59	120.59

Notes: In panel A, the share of prewar buildings in 1991 in *bombing-adjacent municipalities* is regressed on the inverse hyperbolic sine transformation (IHST) of post-armistice Allied bombings that hit *the adjacent targeted location*. In panel B, the change in per-capita revenues from local taxes between 1990 and 1994 in *bombing-adjacent municipalities* is regressed on the IHST of post-armistice Allied bombings that hit *the adjacent targeted location*. Among targeted locations, the 25th percentile of the tons of bombings is equal to 39 tons, while the median is equal to 114 tons. Standard errors clustered at the province level in panel A and at the city level in the other panels, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A7: Trends Before the LPT Introduction, More Variables from Census

	Population density (1)	Gender gap in education (2)	Population in urban areas (3)	Illiterate residents (4)	Residents with univ. degree (5)	Residents per building (6)	Population (7)	Youth outside labor force (8)	Agricultural workers (9)	Manufacturing workers (10)
					Panel A: Linear pre-LPT trends					
Near bombed x Trend	-0.126	0.229*	0.005	0.161	0.843	0.045	0.886	0.012	0.046	-0.686
	(0.291)	(0.136)	(0.017)	(0.538)	(0.532)	(0.037)	(2.406)	(0.013)	(0.057)	(0.797)
Observations	11,874	11,773	11,874	11,874	11,874	11,872	11,874	7,176	7,176	7,176
Dep. var.—mean	204.80	150.88	26.01	245.68	261.60	92.47	3737.20	15.53	7.69	394.08
Dep. var.—std. dev.	486.01	89.55	23.43	611.75	645.10	46.03	5394.41	7.23	37.32	915.43
					Panel B: Nonlinear pre-LPT trends					
Near bombed x 1951	6.510	-10.844*	-0.177	-6.118	-34.627	-1.574	-32.216			
	(10.903)	(6.040)	(0.640)	(21.511)	(21.454)	(0.988)	(91.636)			
Near bombed x 1961	2.255	-2.858	-0.187	-4.965	-33.750	-1.522	-32.577			
	(9.924)	(4.059)	(0.578)	(13.699)	(20.518)	(2.708)	(71.698)			
Near bombed x 1971	5.501	-0.085	-0.238	-2.721	-29.334*	-0.705*	-7.267	-0.247	-0.926	13.723
	(5.547)	(2.905)	(0.485)	(8.672)	(17.434)	(0.405)	(43.527)	(0.268)	(1.138)	(15.939)
Near bombed x 1981	2.627	-1.299	-0.042	-1.137	-19.105	-0.199	-8.484	-0.083	0.341	10.270
	(3.594)	(1.739)	(0.420)	(3.192)	(11.614)	(0.215)	(24.961)	(0.204)	(1.654)	(9.545)
Observations	11,874	11,773	11,874	11,874	11,874	11,872	11,874	7,176	7,176	7,176
Dep. var.—mean	204.80	150.88	26.01	245.68	261.60	92.47	3737.20	15.53	7.69	394.08
Dep. var.—std. dev.	486.01	89.55	23.43	611.75	645.10	46.03	5394.41	7.23	37.32	915.43
F statistic	1.45	0.93	0.07	0.05	0.74	0.89	0.21	0.43	1.15	0.60
P value	0.22	0.45	0.99	0.99	0.56	0.47	0.93	0.65	0.32	0.55

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 linear pre-reform trends between 1951 and 1991 using data from the population censuses. Panel B estimates nonlinear pre-reform trends. The F-statistic at the bottom tests for the joint significance of the nonlinear trends. The omitted year is 1991. The gender gap in education is the ratio of men with HS diploma over women with HS diploma, multiplied by 100. Youth outside labor force is the share of the population between 15 years old and 29 years old who is not working nor studying. The regressions also include city fixed effects, region-year fixed effects, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors clustered at the city level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table A8: Trends Before the LPT Introduction, Variables from 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	-0.004		0.001		-54.335		0.055	
	(0.012)		(0.006)		(60.010)		(0.091)	
Near bombed x 25-29 x Trend	0.005		0.017		-78.372		0.057	
	(0.005)		(0.016)		(66.623)		(0.084)	
Near bombed x 30-34 x Trend	0.004		0.012		-66.336		0.027	
	(0.003)		(0.015)		(74.253)		(0.051)	
Near bombed x 35-39 x Trend	-0.002		0.004		-37.662		0.193	
	(0.003)		(0.005)		(69.301)		(0.213)	
Near bombed x 40-44 x Trend	0.001		0.003		-120.064		0.276	
	(0.003)		(0.004)		(73.640)		(0.293)	
Near bombed x 45-49 x Trend	-0.005		0.006		-9.620		0.148	
	(0.003)		(0.009)		(77.999)		(0.084)	
Nonlinear trends—Partial F-test		0.70		0.70		0.87		0.96
Observations	765,872	765,872	723,678	723,678	765,872	765,872	765,872	765,872
R^2	0.428	0.428	0.321	0.321	0.269	0.269	0.219	0.219
Dep. var.—mean	0.45	0.45	0.35	0.35	14,198.22	14,198.22	3.457	3.457
Dep. var.—std. dev.	1.89	1.89	0.93	0.93	6,826.85	6,826.85	1.234	1.234

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. The table shows estimates of pre-reform linear ("Trend") and nonlinear trends using Social Security data on female employees of privately owned firms. The pre-reform years span from 1974 to 1992 for all variables, but "Reentry in labor market" (1976-1992). 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 individual pre-reform year dummies. The regressions also include the pairwise interactions between the main variables, city fixed effects, age-year fixed effects, region-year fixed effects, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors clustered at the city level in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Source: Istituto Nazionale della Previdenza Sociale (INPS).

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
	(0.351)			(0.396)				
Judicial system	-0.003	28,248	0.080	-0.009	28,165	0.129	0.06	0.25
	(0.010)			(0.011)				
Police	0.355***	28,248	0.209	0.432***	28,165	0.271	4.26	2.91
	(0.099)			(0.109)				
Education	0.342*	28,246	0.198	0.563***	28,163	0.312	10.23	5.06
	(0.178)			(0.194)				
Culture	0.119*	28,248	0.219	0.135**	28,165	0.286	1.89	1.93
	(0.062)			(0.067)				
Sports	0.196***	28,248	0.150	0.188***	28,165	0.218	1.54	1.45
	(0.049)			(0.056)				
Tourism	-0.062	28,248	0.096	-0.100*	28,165	0.153	0.66	1.27
	(0.045)			(0.052)				
Transport system	-0.534***	28,247	0.212	-0.563***	28,164	0.303	9.54	4.58
	(0.160)			(0.177)				
Public health	0.063	28,246	0.250	0.487**	28,163	0.395	18.75	7.4
	(0.225)			(0.237)				
Welfare	0.922***	28,248	0.321	0.809***	28,165	0.386	9.19	7.37
	(0.236)			(0.260)				
Local econ. development	0.094***	28,248	0.098	0.045	28,165	0.169	0.45	0.86
	(0.028)			(0.032)				

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 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: 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
	(0.026)			(0.028)				
LPT rate for homeowners (‰)	-0.062**	28,526	0.200	-0.097***	28,444	0.273	5.15	0.74
	(0.024)			(0.026)				
Max LPT rate (dummy)	-0.026**	28,536	0.137	-0.028**	28,454	0.202	0.13	0.33
	(0.012)			(0.014)				
Max LPT rate for homeowners (dummy)	-0.006	28,526	0.013	-0.009	28,444	0.048	0.02	0.12
	(0.005)			(0.005)				
Tax benefits for homeowners	-0.001	28,579	0.174	0.030*	28,497	0.244	0.31	0.46
	(0.016)			(0.018)				
Share of issued building permits	-1.369**	16,159	0.094	-1.879***	16,098	0.148	82.75	19.89
	(0.550)			(0.615)				

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 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 A11: Public Services, Heterogeneity Based on Political Competition, Municipal Competition, and Local Preferences

	Has fiscal infraction		Spending for welfare (%)		Foreign residents		Pupils in nursery schools		Share below 3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Near bombed x Post	-0.077	-0.093	4.018***	4.555***	108.661*	283.091***	-0.435	4.382	0.332**	0.601**
	(0.077)	(0.080)	(1.447)	(1.478)	(58.317)	(109.548)	(5.090)	(5.346)	(0.168)	(0.235)
Near bombed x Post x Runoff	-0.121**		1.939**		50.963		3.762*		0.002	
	(0.049)		(0.983)		(34.615)		(2.197)		(0.019)	
Near bombed x Post x Mayoral term		0.009		-0.341		-110.205**		-2.551		-0.216*
		(0.019)		(0.276)		(48.572)		(3.062)		(0.131)
Near bombed x Post x Adjacent cities	-0.005	-0.006	-0.254**	-0.232**	14.053**	22.447***	0.964	1.281	-0.014	-0.015
	(0.006)	(0.006)	(0.112)	(0.112)	(5.729)	(7.136)	(0.750)	(0.788)	(0.012)	(0.012)
Near bombed x Post x Below €15,000	0.001	0.002*	-0.024	-0.026	-2.299***	-3.358***	-0.070	-0.109**	-0.003	-0.002
	(0.001)	(0.001)	(0.017)	(0.017)	(0.661)	(0.910)	(0.048)	(0.052)	(0.002)	(0.002)
Available only after LPT	✓	✓	✓	✓						
Observations	17,128	17,137	26,854	26,868	6,848	6,848	6,848	6,848	9,123	9,123
R^2	0.194	0.193	0.325	0.325	0.715	0.684	0.891	0.889	0.726	0.726
Dep. var.—mean	0.51	0.51	9.39	9.40	18.90	18.90	9.93	9.93	2.80	2.80
Dep. var.—std. dev.	0.50	0.50	7.37	7.37	41.81	41.81	22.10	22.10	1.01	1.01

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. "Runoff" is a dummy equal to 1 when the closest election had a runoff. "Mayoral term" is the number of terms served by the current mayor. When the dependent variables come from the decennial censuses, "Runoff" is the total number of runoff elections after 1993 and "Mayoral term" is the average number of terms served by mayors 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. Some variables are available only after LPT. In this case, the main regressors do not include the variable "Post" in the interactions. When the dependent variable is available only after LPT, the regressions include region-year fixed effects, the heterogeneity variables in isolation, population, area of the municipality, a dummy for coastal cities, and a dummy for urban cities. Otherwise, the regressions include city fixed effects, the new heterogeneity variables interacted with year fixed effects, region-year trends, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors clustered at the city level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table A12: Effects on Political Participation

| | Effects of €1 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	0.0005***	0.0006**	6,058	0.34	0.47
	(0.0002)	(0.0002)			
Voted	0.0004***	0.0004***	6,058	0.75	0.43
	(0.0001)	(0.0001)			
Shown badges	0.0002*	0.0002*	6,058	0.08	0.26
	(0.0001)	(0.0001)			
Signed petition	0.0003**	0.0004***	6,058	0.16	0.37
	(0.0001)	(0.0001)			
Local preference	0.0007	0.0010**	1,162	0.28	0.45
	(0.0005)	(0.0004)			
Discuss politics often	0.0011***	0.0013***	1,162	0.50	0.50
	(0.0003)	(0.0004)			
Voting important	0.0006***	0.0007**	1,162	0.81	0.39
	(0.0002)	(0.0002)			
Participation important	0.0001	0.0001	1,162	0.32	0.47
	(0.0002)	(0.0002)			
Mean Δ Rev. local tax (94-90)	128.09	116.85			
Std. dev. Δ Rev. local tax (94-90)	52.47	49.54			

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 per-capita 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). The 25th percentile of the change in revenues is equal to €70, while the median is equal to €124. Standard errors clustered at the region level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

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

	Near bombed x Post x 20-24	Near bombed x Post x 25-29	Near bombed x Post x 30-34	Near bombed x Post x 35-39	Near bombed x Post x 40-44	Near bombed x Post x 45-49	Mean outcome	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Entry and reentry into the labor market								
New entry into the labor market	-0.095	0.060***	0.040***	0.003	-0.008	0.007	0.45	1.89
	(0.087)	(0.019)	(0.012)	(0.012)	(0.015)	(0.009)		
Reentry into the labor market	-0.073**	0.026	0.093***	0.082***	0.042*	0.022	0.39	0.98
	(0.028)	(0.026)	(0.032)	(0.029)	(0.022)	(0.014)		
Entry into a new firm	-0.314*	0.299***	0.367***	0.262***	0.115**	0.096**	2.39	5.78
	(0.174)	(0.104)	(0.106)	(0.082)	(0.056)	(0.041)		
Reentry into the same firm	-0.002	-0.001	0.019***	0.021***	-0.003	-0.003	0.13	0.43
	(0.002)	(0.002)	(0.006)	(0.007)	(0.002)	(0.002)		
Panel B: Characteristics of labor contracts								
Median wage	-647.523***	-555.762**	-549.956**	-315.301	82.953	165.886	14,109.20	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	-0.141***	-0.090	-0.125*	-0.070	-0.032	-0.054	8.47	2.21
	(0.038)	(0.066)	(0.068)	(0.073)	(0.067)	(0.058)		
Median days worked	-7.698***	-6.209***	-4.234***	-1.689***	1.731	3.804*	230.28	89.29
	(2.342)	(2.198)	(1.298)	(0.538)	(2.147)	(1.961)		
Working outside province of res.	0.199	0.405***	0.373***	0.224**	0.151**	0.097*	1.95	4.69
	(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***	0.222***	0.229***	0.158***	0.026	0.88	1.84
	(0.113)	(0.036)	(0.041)	(0.052)	(0.039)	(0.040)		
Panel D: Highest completed education (1987-2011)								
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 variables, city fixed effects, age-year fixed effects, region-year fixed effects, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors clustered at the city level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Source: Istituto Nazionale della Previdenza Sociale (INPS).

Table A14: Robustness checks, Employees of Privately Owned Firms

	New entry in labor market (1)	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	-0.095	-0.073**	-647.523**	-7.532**	0.199	-0.060**
	(0.088)	(0.032)	(262.109)	(3.159)	(0.416)	(0.026)
Near bombed x Post x 25-29	0.060***	0.026	-555.762**	-6.191**	0.405**	-0.047**
	(0.015)	(0.023)	(222.210)	(2.685)	(0.169)	(0.022)
Near bombed x Post x 30-34	0.040***	0.093***	-549.956**	-4.622*	0.373**	-0.044**
	(0.010)	(0.033)	(232.850)	(2.503)	(0.152)	(0.022)
Near bombed x Post x 35-39	0.003	0.082**	-315.301	-1.368	0.224*	-0.027
	(0.011)	(0.031)	(250.072)	(2.977)	(0.134)	(0.026)
Near bombed x Post x 40-44	-0.008	0.042	82.953	1.731	0.151*	0.016
	(0.02)	(0.026)	(223.908)	(2.774)	(0.080)	(0.022)
Near bombed x Post x 45-49	0.007	0.022	165.886	3.804	0.071	0.025
	(0.010)	(0.014)	(230.417)	(2.613)	(0.045)	(0.021)
Observations	2,398,512	2,356,318	2,398,512	2,398,512	2,398,512	2,398,512
	Panel B: Controls for city-year fixed effects					
Near bombed x Post x 20-24	-0.096	-0.067***	-599.304**	-7.897***	0.180	-0.053**
	(0.073)	(0.025)	(243.029)	(2.176)	(0.256)	(0.023)
Near bombed x Post x 25-29	0.063***	0.032	-507.602**	-5.218***	0.379***	-0.039*
	(0.013)	(0.029)	(226.134)	(1.509)	(0.141)	(0.020)
Near bombed x Post x 30-34	0.039***	0.100***	-504.257**	-4.589***	0.350**	-0.037*
	(0.017)	(0.037)	(222.406)	(1.298)	(0.164)	(0.021)
Near bombed x Post x 35-39	0.002	0.088***	-266.296	-1.543***	0.205	-0.021
	(0.016)	(0.033)	(250.235)	(1.340)	(0.137)	(0.024)
Near bombed x Post x 40-44	0.001	0.047*	114.472	2.319	0.139	0.021
	(0.010)	(0.024)	(234.327)	(2.128)	(0.085)	(0.020)
Near bombed x Post x 45-49	0.002	0.023	200.146	4.325**	0.080	0.029
	(0.009)	(0.015)	(201.271)	(1.943)	(0.060)	(0.018)
Observations	2,398,512	2,356,318	2,398,512	2,398,512	2,398,512	2,398,512
	Panel C: Placebo effects					
Near bombed x Post x 45-49	0.004	0.007	-18.761	-5.028	0.218	-0.037
	(0.008)	(0.023)	(395.865)	(4.333)	(0.135)	(0.039)
Near bombed x Post x 50-54	0.002	-0.015	-176.973	-7.925*	0.144	-0.062
	(0.007)	(0.017)	(388.864)	(4.317)	(0.123)	(0.039)
Near bombed x Post x 55-59	0.003	-0.019	-120.489	-4.145	0.064	-0.032
	(0.007)	(0.014)	(331.457)	(4.201)	(0.081)	(0.037)
Observations	958,947	916.974	958,947	958,947	958,947	958,947

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 A15: Quadruple Interactions, Employees of Privately Owned Firms

	Near bombed x Post x 20-24 x Female	Near bombed x Post x 25-29 x Female	Near bombed x Post x 30-34 x Female	Near bombed x Post x 35-39 x Female	Near bombed x Post x 40-44 x Female	Near bombed x Post x 45-49 x Female	Mean outcome	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Entry and reentry into the labor market								
New entry into the labor market	-0.023	-0.025	-0.045***	-0.041***	-0.035***	-0.010	0.45	1.89
	(0.047)	(0.028)	(0.015)	(0.010)	(0.012)	(0.008)		
Reentry into the labor market	0.777***	0.343***	0.297***	0.253***	0.248***	0.275***	0.55	1.89
	(0.066)	(0.077)	(0.078)	(0.071)	(0.060)	(0.052)		
Entry into a new firm	1.949***	2.048***	1.447***	1.090***	1.112***	1.190***	2.39	5.78
	(0.238)	(0.235)	(0.226)	(0.217)	(0.213)	(0.181)		
Reentry into the same firm	0.222***	0.108***	0.117***	0.097***	0.104***	0.112***	0.22	0.55
	(0.016)	(0.017)	(0.020)	(0.019)	(0.019)	(0.019)		
Panel B: Characteristics of labor contracts								
Median wage	-126.790	-100.240	-268.722	-62.175	124.081	72.747	14,198.22	6,826.85
	(251.578)	(243.661)	(266.358)	(309.529)	(294.435)	(245.725)		
Log median wage	-0.030	-0.037*	-0.042*	-0.035	0.001	0.012	9.78	0.89
	(0.023)	(0.021)	(0.024)	(0.028)	(0.023)	(0.021)		
Median hourly wage	0.005	0.040	-0.039	0.063	0.006	-0.028	9.51	2.48
	(0.075)	(0.075)	(0.080)	(0.086)	(0.087)	(0.072)		
Median days worked	-4.647	-4.152	-3.979	-2.476	0.465	2.863	236.89	86.71
	(3.057)	(2.684)	(2.979)	(3.191)	(2.765)	(2.478)		
Working outside province of res.	0.213	0.094	-0.211	-0.253***	-0.229***	-0.139***	1.93	4.67
	(0.170)	(0.126)	(0.129)	(0.054)	(0.033)	(0.037)		
Panel C: Characteristics of labor contracts for entrants or reentrants								
Median wage	-480.048**	-419.443**	-302.314	-431.331*	18.451	8.995	8,777.98	7,034.56
	(204.763)	(206.904)	(220.151)	(236.553)	(251.211)	(248.928)		
Log median wage	-0.097***	-0.095***	-0.062*	-0.107***	-0.056	0.004	9.09	1.02
	(0.032)	(0.032)	(0.033)	(0.037)	(0.037)	(0.037)		
Median hourly wage	-0.087***	0.059	0.083	0.195	0.222	0.119	9.31	3.5
	(0.031)	(0.123)	(0.130)	(0.134)	(0.140)	(0.037)		
Median days worked	-7.136***	-7.858***	-4.990*	-8.638***	-4.664*	-0.503	121.6	79.07
	(2.484)	(2.447)	(2.628)	(2.722)	(2.553)	(2.751)		
Working outside province of res.	0.046	-0.067	-0.155*	-0.164**	-0.149**	-0.031	1.29	2.55
	(0.099)	(0.086)	(0.085)	(0.082)	(0.068)	(0.060)		
Panel D: Highest completed education (1987-2011)								
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, city fixed effects, age-year fixed effects, gender-age fixed effects, gender-year fixed effects, region-year fixed effects, as well as year dummies interacted with dummies for deciles of population in 1991, dummies for deciles of minimum altitude, and a dummy for rural municipalities. Standard errors clustered at the city level in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Source: Istituto Nazionale della Previdenza Sociale (INPS).

B Fiscal Decentralization, Public Services, and Labor Markets

The literature on fiscal decentralization is vast and dates back several decades (Musgrave, 1959; Oates, 1972).[54]

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). 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 (Hatfield and Kosec, 2013).[55] 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 (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 (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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[54]Ahmad and Brosio (2006) provide a comprehensive description of relevant contributions and recent developments in this field.

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