

THE EFFECTS OF FISCAL DECENTRALISATION ON PUBLICLY PROVIDED SERVICES AND LABOUR MARKETS*

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This paper studies how fiscal decentralisation affects labour supply. It explores a 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 decentralisation that stems from differences in the average age of buildings caused by World War II bombings. Decentralisation expanded municipal services, such as nursery schools, especially in areas with greater political competition. The paper then investigates how the reform affected labour markets. Decentralisation increased female labour supply—probably through expanded availability of nursery schools—thereby reducing the gender gap in employment.

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 non-central areas for more independence (Dillinger, 1994; Arzaghi and Vernon Henderson, 2005; Ahmad and Brosio, 2006). Despite its growing importance, the effects of fiscal decentralisation on the provision of publicly provided services remain unclear. On the one hand, fiscal decentralisation 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, decentralisation 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 decentralisation 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 decentralisation from the effects of other trends. Second, fiscal reforms tend to be large-scale

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The data and codes for this paper are available on the Journal repository. They were checked for their ability to reproduce the results presented in the paper. The authors were granted an exemption to publish parts of their data because access to these data is restricted. However, the authors provided the Journal with temporary access to the data, which enabled the Journal to run their codes. The codes for the parts subject to exemption are also available on the Journal repository. The restricted access data and these codes were also checked for their ability to reproduce the results presented in the paper. The replication package for this paper is available at the following address: <https://doi.org/10.5281/zenodo.7668245>.

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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 decentralisation and government spending, fiscal decentralisation 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 labour supply and demand with far-reaching repercussions on local economies. As a result, a comprehensive analysis of fiscal decentralisation requires a combination of different kinds of data, ranging from balance sheets of local governments to labour-market outcomes.

This paper provides new empirical evidence on the effects of fiscal decentralisation on the provision of local public services and on the functioning of labour markets. It studies a 1993 reform that increased the fiscal decentralisation 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 pre-existing 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% 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 World War II (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 locations that are farer from targets, 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 decentralisation after the reform. Our analysis, then, measures the differences in several outcomes between target-adjacent and target-distant municipalities, as a result of the introduction of the LPT.

The second contribution of this paper is to track the effect of fiscal decentralisation on both the provision of public services and labour-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

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.

ways. First, local administrators shrank the size of the government, reducing both spending and revenues. They also rebalanced their spending in favour of revenue-generating and customer-facing services, reducing the budget for their internal administrative processes. These changes were not mandated by the reform and should be considered endogenous responses.² Despite the lower budget, there is no evidence of a reduction in the quality of public services. Second, access to public nursery schools, one of the most important municipal services, increased. We find that municipalities that raised more revenues through the LPT dedicated a larger share of their budget to nursery schools (+18%) and had more public nursery schools (+20%). In the same cities, the number of pupils in nursery schools increased by an additional 24% 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 labour market. In these locations, women's labour-force participation increased by up to 20%, leading to a reduction in the pre-existing gender gap in employment. Although these findings are highly suggestive of the impact of nursery schools on female labour 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 subsidised 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 labour supply increased the most among women under 35. Second, the data suggest that fiscal decentralisation 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 labour supply. The entry and reentry of women into labour markets started increasing in 1995. This two-year lag relative to the introduction of the LPT is to be expected if the increase in labour 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 analyse the motivations that might have induced administrators to expand the provision of local services after the LPT. Prior work has proposed several mechanisms through which fiscal decentralisation can improve the delivery of local services. One of the main hypotheses is that fiscal decentralisation 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 decentralisation can lead to better public services by raising the level of competition for new residents between municipalities (Seabright, 1996; Hatfield and Kosec, 2013).³ Finally, local politicians are likely to have better information regarding local preferences towards public services than the central government does (Hayek, 1945). 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 decentralisation is associated with

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

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

higher levels of political participation. Other factors, such as heightened competition between municipalities or heterogeneous preferences for publicly provided services, have a smaller and less precise explanatory power.

This paper contributes to two main strands of the literature. First, there is an extensive theoretical and empirical literature on the effects of fiscal decentralisation 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 labour markets (see Huther and Shah, 1998 for an overview). In most of these studies, the quantitative analysis relies on cross-country correlations.⁴ 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 decentralisation on both publicly provided services and labour markets within the same empirical setting.⁵ The results of this comprehensive analysis indicate that local fiscal institutions can have significant effects on individual labour supply.

Second, this paper emphasises the importance of subsidised 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 labour supply depends on the existing market for childcare. In countries with near-universal access to affordable childcare, expansion of subsidised preschool (usually schools for children between three months and six 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 labour supply (Gelbach, 2002; Baker *et al.*, 2008; Carta and Rizzica, 2018). This paper complements these findings by showing the importance of fiscal institutions in shaping the provision of public childcare.

1. Institutional Details and Data

1.1. *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% and 0.7%. The average rate was 0.57% between 1993 and 2010 (Table 1, panel A).

The reform had the explicit goal of transitioning the public sector toward fiscal decentralisation. 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

⁴ As a notable exception, Hatfield and Kosec (2013) studied 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 decentralisation.

⁵ Among all possible forms of fiscal decentralisation, this paper focuses on a reform that increased the revenue responsibilities of local governments, while keeping their (already decentralised) 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).

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

⁷ We convert all monetary values to 2017 €.

Table 1. *Summary Statistics.*

	All years				$t \leq 1992$	$t > 1992$
	Mean (1)	SD (2)	Obs. (3)	Availability (4)	Mean (5)	Mean (6)
<i>Panel A: local property tax</i>						
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
<i>Panel B: balance sheets of Italian municipalities</i>						
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.)	1,793.65	1,253.30	158,640	1990–2010	1,672.68	1,815.43
Total spending (per cap.)	1,808.21	1,261.42	158,606	1990–2010	1,670.90	1,833.36
<i>Panel C: census data</i>						
Population	6,803.05	40,861.85	56,092	1951–2011	6,643.83	7,195.87
Women in labour force	918.42	6,077.42	56,092	1951–2011	776.83	1,267.75
Employed women	793.57	5,286.25	56,092	1951–2011	674.05	1,088.43
Men in labour force	1,823.58	10,791.41	56,092	1951–2011	1,828.36	1,811.79
Employed men	1,671.02	9,804.48	56,092	1951–2011	1,684.52	1,637.71
Gender gap in labour force (M-W)	905.17	5,485.53	56,092	1951–2011	1,051.54	544.04
Gender gap in employment (M-W)	877.46	5,198.78	56,092	1951–2011	1,010.47	549.28
Foreign residents	235.76	2,389.66	24,254	1991–2011	44.08	331.44
Pupils in nursery schools	28.14	235.40	24,254	1991–2011	20.41	32.00
Share below 3	2.66	0.96	32,313	1981–2011	2.91	2.42
Agricultural workers	10.83	51.07	40,344	1971–2011	11.37	10.03
Manufacturing workers	615.86	3,516.15	40,344	1971–2011	666.64	540.01
<i>Panel D: Social Security data</i>						
New entry in labour market	0.52	1.89	2,967,243	1974–2011	0.50	0.54
Reentry in labour 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 non-agricultural privately owned Italian firms. *Source:* Istituto Nazionale della Previdenza Sociale, VisitINPS program.

ordinance on the new LPT, the tax rate was automatically set at 0.4% 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). Starting in 2002, they further increased due to the introduction of a municipal surcharge on the personal income tax,

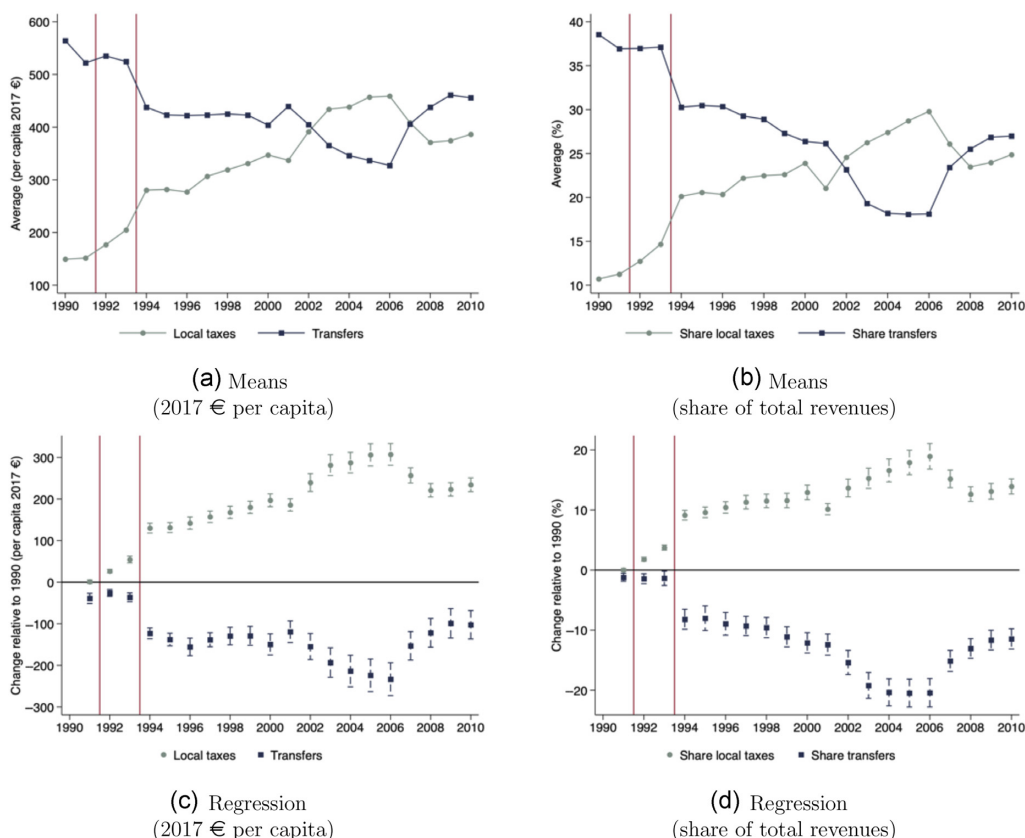


Fig. 1. *Revenues per Capita from Local Taxes and Government Transfers.*

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>.

while they decreased in 2007—albeit remaining substantially above the 1990 levels—due to the cancellation for homeowners of the LPT on their main residence.⁸

Because of the one-to-one replacement between revenues of the new local tax and inter-governmental transfers, the reform did not mechanically change the overall size of the municipal budgets, but only its composition.⁹ Over the same period, transfers from other levels of govern-

⁸ Our results start before 2002, linking the estimated effects to the first policy change. The other major decentralisation policy, the reform of the Title V of the constitution (fully implemented by law 42/2009), focused mainly on regions, instead of municipalities. To control for it, all our specifications include either region-year or province-year fixed effects.

⁹ However, we test whether fiscal decentralisation induced local administrator to endogenously vary spending and revenues.

ment decreased from €564 per resident in 1990 to €438 per resident in 1994.¹⁰ When combined with revenues from municipal services, payments from local taxes became the main source of local revenues by 1994 (30% of all revenues; see Online Appendix 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% of total revenues from local taxes (Table 1, panel B).¹¹

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 United States 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. We further discuss the determinants of cadastral values in Section 2, 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.¹² 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% of the average household disposable income in 1991 (Banca D'Italia, 1993).¹³ In short, the implementation of the LPT was salient to residents due to both its amount and its separate payment method.¹⁴

1.2. 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.¹⁵

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

¹⁰ Total revenues decreased by only 4.6%.

¹¹ Although the balance sheets become more detailed after 1998, we have key information of spending and revenues starting from 1990.

¹² 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.

¹³ Other contemporaneous local taxes and fees, such as a PIT surcharge and a waste disposal fee, were equal to at most 0.6%, on average, of disposable income (Baldini *et al.*, 2005).

¹⁴ The 1948 Italian Constitution created a highly centralised government. Therefore, the saliency of the new LPT also stemmed from the fact that it was one of the first major reforms in the history of the Italian republic in which revenue-generating authority was decentralised to local governments.

¹⁵ In the rest of this manuscript, we 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.

their residents (Online Appendix Table A1). The average city spent 40% 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% on public health, which includes sanitation, waste and water management, public housing and city planning; 9% on local transportation and 4% on a municipal police force.

Two other important areas of spending are education and welfare with 10% each.¹⁶ 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.

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 (2001 and 2011) observations. We use these data to study how participation in the labour market changed after the introduction of the LPT.

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 non-agricultural firms with at least one salaried worker. We use this dataset to study the effect of fiscal decentralisation on different types of employees within a municipality. To this end, the observations are aggregated by age, gender, municipality of residence and year.¹⁷ In this dataset, we observe workers entering the labour market for the first time or reentering it after a break in employment.¹⁸

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 (THOR; available at <https://web.archive.org/web/20170117120517/http://www.au.af.mil/au/afri/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 (2023), 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(a)). In Section 2.2, we further discuss how this variation can be used to isolate the effect of fiscal decentralisation after 1993.

2. Empirical Strategy

2.1. Cadastral Values, Age of Buildings and Allied Bombings

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 labour-

¹⁶ Other minor areas of spending are culture (2%, for theatres, museums, libraries), sports (1.6%), activities in support of local economic development (0.5%) and auxiliary services for the judicial system (0.1%).

¹⁷ This aggregation has two purposes. First, it aggregates the data at the same level of variation used by the empirical analysis in Section 3.6. 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.

¹⁸ 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.

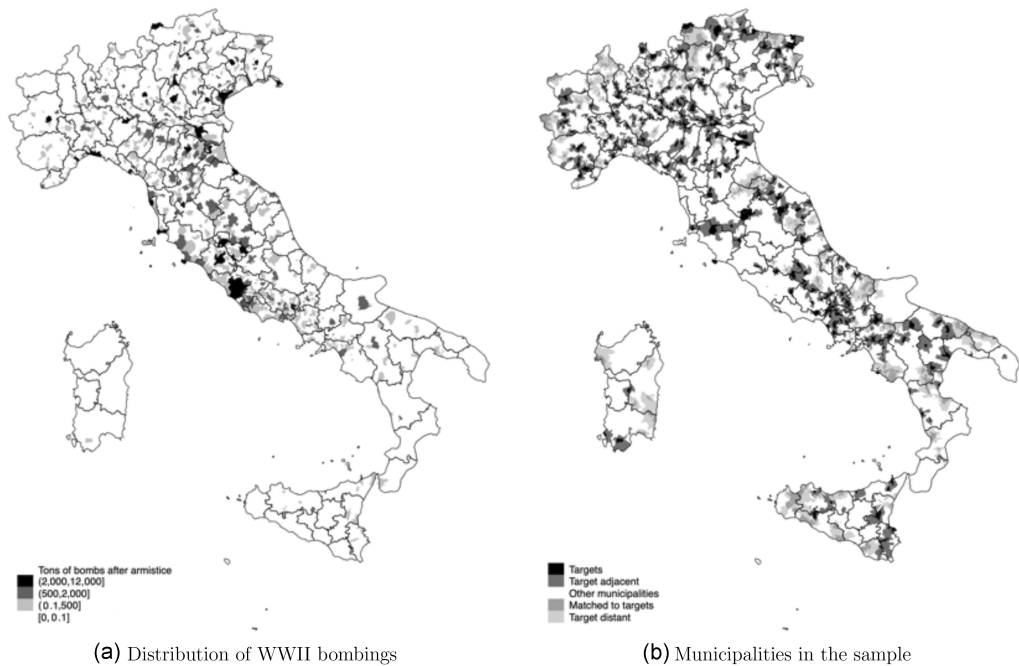


Fig. 2. *Distribution of WWII Bombings across Italian Municipalities.*

Notes: Panel (a) shows the distribution of Allied bombings during WWII that were executed after the Armistice of Cassibile between Italy and the Allied forces (September 3, 1944). Panel (b) shows the municipalities in the main estimating sample. Target cities are matched to other non-adjacent Italian municipalities using propensity-score matching. Specifically, we employ a nearest-neighbour propensity-score algorithm, impose a common support between treatment and control, and use 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. Then, the analysis compares cities around the target municipalities (target adjacent) to cities around the matched municipalities (target distant).

Source: USAF Theater History of Operations Reports (THOR) Database, available at <https://web.archive.org/web/20170117120517/http://www.au.af.mil/au/afri/thor/#.WH4IgC-P13Q>.

market outcomes changed across cities in which the share of revenues from local taxes increased differently after 1993 could be problematic. In fact, municipalities in which revenues from local taxes increased more are likely to have had a larger tax base, more expensive buildings, richer residents and, consequently, a more developed local economy. 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: $\text{LPT paid} = \text{cadastral value} \times \text{multiplier} \times \text{tax rate}$. 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% and 0.7%. As a result, any cross-municipality variation in the tax rate would be endogenous and possibly a symptom of other underlying differences.¹⁹

In the Italian context, the cadastral values are negatively correlated with the age of buildings. Specifically, a 13% 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 pre-war buildings, decreases the average cadastral value by €47, or 13% from the mean (Online Appendix 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 decentralisation after 1993. A 13% increase in the share of pre-WWII buildings, in fact, decreases the post-LPT per-capita revenues from local taxes by €14, or 11% from the mean (Online Appendix Table A2, panel B, column 1). 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 (Online Appendix Table A2, panel C).

The finding that cadastral values are negatively correlated with building age is not surprising. In fact, the cadastral values are assigned to buildings at the time of construction (Agenzia delle Entrate, 2013). After this initial assessment, they are never reevaluated on a case-by-case basis, unless a property undergoes a major renovation that affects its overall size or number of rooms. As the prices of real estate increased by a factor of 3.5 between 1950 and 2012 (real values; Cannari *et al.*, 2016), the cadastral values of older buildings remained essentially untouched over the decades. Additionally, compared with post-war buildings, pre-war 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

These initial correlations indicate that cross-municipal variation in the age of buildings leads to differences in cadastral values and, therefore, in the exposure to fiscal decentralisation. However, variation in the age of buildings can also reflect differences in other municipal characteristics, such as per-capita income, that can be correlated with labour-market outcomes. In the rest of this section, we propose to leverage only cross-municipal variation in the age of buildings that stems from Allied bombings during WWII, which have the potential to be uncorrelated with labour-market trends in 1991 (Figure 2(a)).

We use municipal-level data on Allied bombings to show two initial results. First, more bombings significantly reduced the share of pre-war buildings in 1991, therefore reducing the average age of the municipal stock of real estate. We regress the share of pre-WWII buildings in 1991 on the inverse hyperbolic sine transformation (IHST) of the tons of Allied bombings dropped on each municipality after the armistice of Cassibile (Online Appendix Table A3, panel A).²¹ The estimated correlation is statistically and economically significant. A three-fold increase in the tons of post-armistice bombings, which is equivalent to moving from the 25th percentile

¹⁹ In Section 3.3, we further analyse the endogenous response in the tax rates.

²⁰ Buildings that are at least 50 years old can be considered historic (*decreto legislativo* 42/2004).

²¹ Bianchi and Giorcelli (2023) showed that province-level bombings dropped after the armistice between Italy and the Allied forces (September 3, 1943) were not correlated with many pre-war provincial characteristics: industrial

to the median or from the median to the 75th percentile in the distribution of bombings, is associated with 4-percentage-point fewer pre-war buildings, a 10% increase from the mean (column 2). Second, more bombings are correlated with higher cadastral values and, therefore, higher exposure to fiscal decentralisation after 1993. To show this result, we regress the change in per-capita revenues from local taxes between 1990 and 1994 on the IHST of the tons of post-armistice bombings. We find that a three-fold increase in bombings is associated with a €13 increase in per-capita revenues from local taxes, a 10% increase from the mean (Online Appendix Table A3, panel B).

2.2. Leveraging Variation in Allied Bombings

To study the effects of fiscal decentralisation on publicly provided services and labour-market outcomes, we estimate the difference-in-differences specification

$$y_{mt} = \alpha_m + \gamma_{rt} + \sum_t \delta_t \text{Exp. bombs}_m \times \text{Year}_t + \varepsilon_{mt}, \quad (1)$$

where y_{mt} represents different outcomes of interest in municipality m and year t , while Exp. bombs_m is one of several variables that measure exposure to 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. Moreover, the γ_{rt} are fixed effects for either region r or province p interacted with year dummies.²² When the dependent variable has several pre-LPT observations, such as the census variables that become available in 1951, γ_{rt} also includes non-linear trends that are correlated with observable characteristics: year fixed effects are interacted with dummies for different percentiles (either tertiles, quartiles, quintiles or deciles) of population in 1991, dummies for deciles of minimum altitude and a dummy for rural municipalities. When the dependent variable is available only after the LPT implementation, (1) 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. However, this specification gains controls for city-level characteristics, such as population, altitude and a dummy for rural cities. Standard errors are clustered at the municipality level.

After providing a general description of the specification, we discuss two consecutive refinements of the estimating sample and the treatment variable. First, we start from the simplest application of (1). In this case, the estimating sample includes *all* Italian municipalities, while Exp. bombs_m measures the IHST of the tons of post-armistice Allied bombings in municipality m . Using the main labour-market outcomes as dependent variables, that is, the number of women either active in the labour force or employed in municipality m , the estimation of (1) on pre-LPT years shows signs of non-parallel pre-reform trends (Online Appendix Figure A2, panels A and B). Specifically, we observe a differential increase in the number of women active in the labour market that starts in 1961.

This initial specification has two main issues. Out of all 8,092 Italian municipalities in 1991, only 671 were directly targeted by Allied bombings after the Armistice of Cassibile. In other

and agricultural outcomes, population, innovation levels, geographical characteristics and electoral outcomes (Table 4; Bianchi and Giorcelli, 2023).

²² These fixed effects control for other shocks, such as other policies, natural disasters or macroeconomic events, that may have differentially affected a region or province over time. As discussed in Section 1, one example of such a shock is the reform of the Title V of the Italian constitution, which gave more fiscal responsibilities to regions.

words, this specification creates a control group that is much larger than the treatment group. Because of its larger size, the control group is therefore bound to be more heterogeneous, and possibly more negatively selected, than the treatment group. Moreover, due to imprecision in bombings, the officially reported tons of bombs dropped by Allied forces on each municipality may not reflect the actual tons of bombs that hit their targets.

Second, to overcome these two weaknesses of the initial specification, we balance the treatment and control groups by matching each city targeted by Allied bombings to a counterfactual non-targeted city. To perform the matching, we employ a nearest-neighbour propensity-score algorithm, impose a common support between treatment and control, and use 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 (Online Appendix Table A4, column 1). However, in Section 3.7, we show that the results are highly robust to alternative implementations of the matching algorithm. The result of this process is a balanced estimating sample with 314 municipalities targeted by bombings and 314 matched municipalities. Moreover, to reduce the risk of measurement error, a dummy equal to 1 for the 314 municipalities officially targeted by bombings replaces the tons of post-armistice Allied bombs as the treatment variable.

The data confirm that this second revision of (1) is an improvement over the first specification. Many observable characteristics are generally balanced between targeted and matched locations (Table 2, panel B, column (1)). Out of the 34 variables from the population and industry censuses, only two variables are statistically different at the 5% level. However, variables from municipal balance sheets are less balanced (Table 2, panel C, column (1)). Moreover, the estimation of (1) on pre-LPT years still shows some signs of non-parallel pre-reform trends (Online Appendix Figure A2, panels C and D). Although the pre-LPT coefficients are not individually or jointly significant, they indicate that the number of women active in the labour market was on a decreasing trend in targeted municipalities since 1951.

These last findings suggest that forming the treatment group around municipalities that were officially targeted by Allied bombings is not ideal. In fact, the targeted municipalities may have had strategic or economic importance during the war, which led German troops to station in or move through these locations.²³ Since treated and control locations had matching observables in 1991 by construction, control municipalities had to grow more quickly in the post-war decades to make up for the possible initial benefits held by target locations.

2.3. Baseline Specification

Based on the final results in Section 2.2, we propose to consider only bombed municipalities that were not officially targeted by Allied forces, which implies that they were not likely to possess any war-related strategic or economic advantage. We can achieve this goal because the WWII bombings were highly inaccurate. Especially in the case of nighttime bombings, which were preferred due to the lower probability of being spotted by anti-aircraft artillery units, area bombings were often the only viable option (Kirby and Capey, 1997). Therefore, many non-

²³ Although Bianchi and Giorcelli (2023) did not find evidence of these strategic advantages at the *province level*, they may have been more relevant at the municipality level.

Table 2. Differences in Observable Characteristics Measured in 1991.

	Near targets vs. near others			Matched adjacent (3)	Mean (4)	SD (5)		Near targets vs. near others			Mean (4)	SD (5)
	Targeted (1)	(2)	(3)					Targeted (1)	(2)	(3)		
Panel A: age of buildings and change in fiscal federalism												
Share of pre-WWII buildings	-5.030*** (1.360)	-2.670*** (0.974)	-2.332** (1.065)	39.48	18.27		Δ Rev. local tax (94-90)	4.157 (11.689)	23.994*** (6.263)	25.754*** (6.570)	135.47	126.81
Panel B: economic, demographic, geographical and ideological characteristics of Italian municipalities												
Population in urban areas (%)	-1.403 (1.572)	-0.450 (1.152)	0.322 (1.063)	17.26	17.76		Coastal city	-0.051* (0.027)	-0.019 (0.013)	-0.016 (0.014)	0.12	0.32
Dist. to econ. centre (km)	-5.271* (3.026)	-2.450 (1.515)	-1.838 (1.513)	38.10	22.91		Aging index	-2.011 (11.090)	-7.522 (7.336)	-8.705 (8.126)	144.30	125.28
Share below 6	0.233*** (0.108)	-0.027 (0.084)	-0.061 (0.091)	5.54	1.70		Share above 75	-0.302 (0.268)	-0.286 (0.191)	-0.153 (0.201)	8.37	3.57
Women in labour force	-0.565 (47.445)	11.504 (12.508)	5.246 (12.977)	1020.84	1524.11		Men in labour force	-14.554 (24.615)	4.314 (6.170)	1.110 (7.380)	1881.58	2894.92
Women with jobs	6.776 (59.882)	28.415 (19.357)	18.348 (19.135)	783.84	1181.45		Men with jobs	10.848 (60.633)	41.300* (20.887)	30.279 (20.673)	1576.50	2306.98
Gender gap in labour force	-13.989 (38.415)	-7.190 (11.779)	-4.136 (12.487)	860.74	1492.29		Gender gap in employment	4.072 (25.893)	12.885* (7.734)	11.931 (8.603)	792.66	1251.64
Firms with ≤ 2 employees	6.155 (11.866)	7.112 (5.738)	9.654* (5.367)	288.00	382.15		Firms with ≥ 200 employees	0.042 (0.028)	0.007 (0.009)	0.003 (0.011)	0.07	0.36
Agricultural workers	-2.258 (2.557)	1.018 (0.990)	2.015* (1.176)	11.54	36.19		Manufacturing workers	11.447 (93.055)	47.049 (38.533)	29.576 (36.083)	648.30	1185.20
Retail workers	-7.503 (19.803)	-3.275 (9.732)	-3.199 (10.098)	366.95	600.15		Real-estate workers	-7.589 (11.678)	0.919 (3.495)	0.479 (3.619)	85.21	137.41
Foreign residents	-4.628 (4.250)	-2.058 (1.518)	-3.452* (1.982)	33.59	66.62		Share women	0.108 (0.142)	0.046 (0.079)	0.045 (0.087)	51.03	1.67
Pupils in nursery school	-0.257 (1.486)	-0.590 (0.407)	-1.037** (0.396)	18.66	37.49		Residents with uni. degree	-0.731 (52.951)	0.183 (15.563)	-10.947 (17.238)	1270.76	2157.68
Household (HH) size	0.055** (0.023)	0.025 (0.017)	0.022 (0.017)	2.74	0.38		m ² per resident	0.497 (0.388)	0.406 (0.257)	0.471 (0.304)	35.24	5.38
Residents per building	-0.498 (0.612)	-0.210 (0.457)	-0.363 (0.519)	62.73	9.69		Inadequate housing (%)	0.307 (0.201)	0.142 (0.087)	0.127 (0.078)	0.20	1.93
HHH in econ. distress (%)	-0.073 (0.140)	-0.079 (0.099)	-0.101 (0.111)	2.63	3.05		HH in need of caregiving (%)	-0.067 (0.073)	-0.041 (0.066)	0.045 (0.063)	2.13	1.07
Vote share for DC	-1.006 (1.363)	-0.387 (0.660)	-0.323 (0.873)	34.82	14.49		Vote share for extreme left	0.381* (0.217)	0.172 (0.173)	0.077 (0.205)	4.60	3.34
Vote share for extreme right	0.254 (0.276)	0.098 (0.140)	0.038 (0.154)	4.13	3.07		Winner DC (dummy)	0.001 (0.045)	0.006 (0.024)	0.010 (0.025)	0.71	0.45
Winner centre-left (dummy)	0.013 (0.030)	0.011 (0.018)	0.009 (0.020)	0.13	0.34		Winner Lega Nord (dummy)	-0.032 (0.026)	-0.023 (0.016)	-0.026 (0.017)	0.12	0.33

Table 2. Continued

	Near targets			Near targets			Matched adjacent	Mean	SD	Targeted	vs. near others	Matched adjacent	Mean	SD
	Targeted	vs. near others		Targeted	vs. near others									
	(1)	(2)	(3)	(4)	(5)		(1)	(2)	(3)	(4)	(5)		(4)	(5)
Panel C: balance sheets of Italian municipalities														
Rev. local taxes (per cap.)	-5.923 (7.934)	14.704*** (5.427)	15.926** (7.451)	163.98	124.57	Current spending (per cap.)	-76.690*** (27.431)	14.552 (16.437)	19.248 (20.621)	771.82	386.59			
Rev. gov. transfers (per cap.)	-62.437*** (16.566)	-6.536 (12.012)	-4.974 (13.905)	511.76	225.80	Capital spending (per cap.)	-226.679*** (69.586)	21.835 (38.170)	55.606 (39.280)	597.58	804.55			
Rev. local services (per cap.)	-10.188 (17.703)	7.746 (7.458)	15.794 (9.552)	175.97	209.28	Loan payments (per cap.)	1.051 (11.626)	-6.826 (4.750)	-5.675 (6.311)	93.51	117.96			
Capital revenues (per cap.)	-136.131** (55.982)	29.185 (28.148)	45.760 (29.585)	393.48	654.87	Total spending (per cap.)	-320.391*** (91.731)	45.972 (62.458)	86.683 (66.808)	1648.84	1188.24			
Rev. loan servicing (per cap.)	-40.052* (23.249)	-14.480 (13.891)	-9.262 (16.433)	205.74	276.74	Deficit (per cap.)	17.460 (11.618)	1.232 (3.109)	2.974 (3.716)	-9.44	128.05			
Total revenues (per cap.)	-305.745*** (89.769)	51.047 (62.011)	92.594 (66.193)	1652.51	1190.91	Revenues over spending	0.001 (0.004)	0.001 (0.002)	0.003 (0.002)	0.99	0.05			

Notes: This table shows differences in the levels of observable characteristics measured in 1991. All monetary values are expressed in 2017 €. Column (1) compares municipalities targeted by Allied bombings after the Armistice of Cassibile to other matched non-targeted 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 three and region fixed effects). Column (2) compares municipalities around targeted cities (near target) to cities around municipalities matched to targeted locations (near others). Column (3) further matches near-target cities to near-other 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 are given in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

targeted municipalities that are adjacent to targeted locations were actually bombed, even though they were not included in the official list of targets.²⁴

Our preferred specification compares all 1,384 municipalities adjacent to the 314 locations officially targeted by Allied bombings after the armistice (from now on, *target-adjacent municipalities*) to all 1,058 municipalities adjacent to the 314 locations matched to target municipalities (*target-distant municipalities*; Figure 2(b)).²⁵ The main treatment variable becomes a dummy *Near targets_m* equal to 1 for target-adjacent municipalities:

$$y_{mt} = \alpha_m + \gamma_{rt} + \sum_t \delta_t \text{Near targets}_m \times \text{Year}_t + \varepsilon_{mt}. \quad (2)$$

Next, we show three important results about Equation (2): (i) parallel pre-reform trends between treatment and control, (ii) balanced observable characteristics just before the introduction of the LPT and (iii) evidence of war-related destruction in target-adjacent municipalities, in spite of no bombings officially reported by Allied forces.

First, the main assumption behind the baseline specification is that outcomes would have followed the same path in target-adjacent and target-distant municipalities in the absence of the LPT. While this assumption cannot be tested directly, we document that all outcomes followed 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 targets_m* with a linear time trend (Table 3, panel A). For all dependent variables, the coefficients of this interaction are insignificant and small. For example, the number of women in the labour force increased by an additional 0.06 individuals in target-adjacent municipalities for each pre-reform year between 1951 and 1991. This estimate is equal to an economically and statistically insignificant 0.01% 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 labour force and the number of employed men.

Alternatively, we estimate non-linear pre-LPT trends by interacting the treatment variable with individual year dummies (Table 3, panel B; Figure 3).²⁶ The coefficients of the interaction between *Near targets_m* and the pre-reform dummies are small in magnitude and not statistically different from zero. These findings indicate that target-adjacent municipalities and target-distant municipalities experienced parallel non-linear trends in labour-market outcomes for several decades before the implementation of the LPT reform. For example, in the case of the number of women in the labour force, the individual coefficients denote differences between treated and control municipalities that are equal to at most 2.7 residents or 0.6% of the pre-reform mean (Figure 3(a)).²⁷

²⁴ 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, which is located just 11 km to the south (<https://bit.ly/2UALQeQ>). Venafrò 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% 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>).

²⁵ The five Italian macro-areas and 20 regions do not predict the location of target-adjacent municipalities, while the 110 smaller provinces jointly do. Therefore, we show that the results are robust to controlling for either region-year or province-year fixed effects.

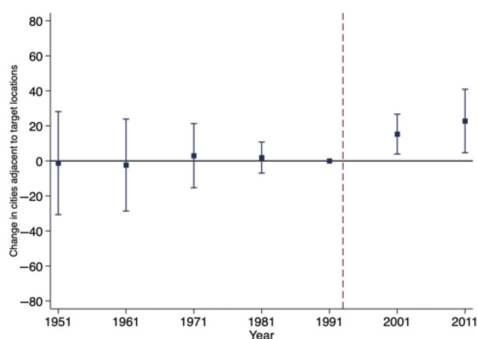
²⁶ In the case of the variables from the Italian census, we usually have five pre-reform observations starting in 1951 and ending in 1991. In the case of the balance-sheet data, we estimate non-linear pre-reform trends using three observations from 1990 to 1992. The omitted year is 1991 for the census variables and 1992 for the balance-sheet data.

²⁷ Online Appendix Table A5 shows small and insignificant pre-reform trends for more census variables.

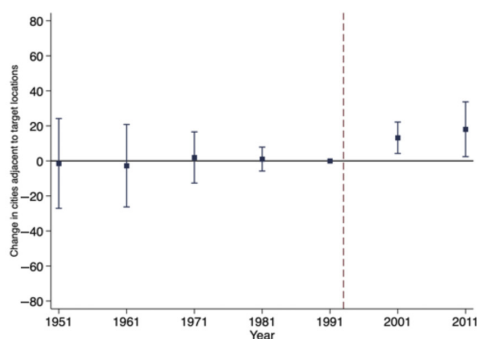
Table 3. Trends Before the LPT Introduction.

	Revenues from local taxes (1)	Revenues from gov. transfers (2)	Revenues per capita (3)	Spending per capita (4)	Women in labour force (5)	Employed women (6)	Men in labour force (7)	Employed men (8)	Gender gap in labour force (9)	Gender gap in employment (10)
<i>Panel A: linear pre-LPT trends</i>										
Near targets × trend	0.203 (1.144)	−0.262 (2.423)	−30.040 (25.356)	−24.530 (25.145)	0.060 (0.469)	0.065 (0.416)	−0.184 (0.734)	−0.041 (0.668)	−0.244 (0.496)	−0.105 (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	1,675.00	1,673.60	448.60	394.54	1,049.55	974.73	600.95	580.19
Dep. var.—SD	109.13	218.19	1,196.34	1,200.25	692.81	597.59	1,476.77	1,325.43	921.54	852.02
<i>Panel B: non-linear pre-LPT trends</i>										
Near targets × 1951					−1.055 (17.777)	−1.450 (15.492)	8.675 (27.957)	2.331 (25.263)	9.731 (19.637)	3.780 (18.575)
Near targets × 1961					−2.292 (15.925)	−2.872 (14.272)	4.999 (22.075)	−0.951 (18.659)	7.292 (15.103)	1.921 (13.746)
Near targets × 1971					2.751 (11.129)	1.646 (8.860)	9.918 (13.835)	4.077 (10.831)	7.167 (11.277)	2.431 (10.501)
Near targets × 1981					1.636 (5.393)	0.710 (4.119)	4.050 (8.084)	−0.374 (5.672)	2.414 (6.440)	−1.083 (5.266)
Near targets × 1990	−0.489 (2.286)	0.614 (4.858)	61.630 (50.681)	41.927 (52.222)						
Near targets × 1991	1.695 (2.566)	−1.794 (6.828)	−5.483 (49.972)	−10.050 (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	1,675.00	1,673.60	448.60	394.54	1,049.55	974.73	600.95	580.19
Dep. var.—SD	109.13	218.19	1,196.34	1,200.25	692.81	597.59	1,476.77	1,325.43	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

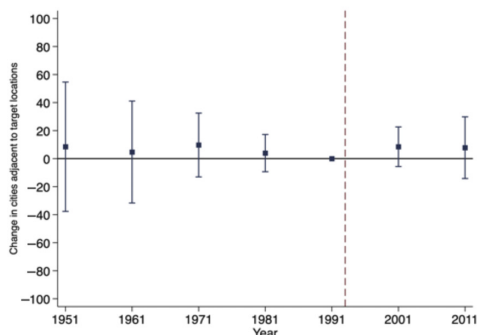
Notes: This table shows pre-reform trends in key city-level variables. Monetary values are expressed in 2017 €. ‘Near targets’ is 1 for municipalities adjacent to cities targeted by Allied tactical air attacks during WWII. The control group is composed of municipalities adjacent to cities matched to target 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 non-linear pre-reform trends. The F-statistic at the bottom tests for the joint significance of the non-linear 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 population in 1991, dummies for deciles of minimum altitude and a dummy for rural municipalities. Standard errors clustered at the city level are given in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.



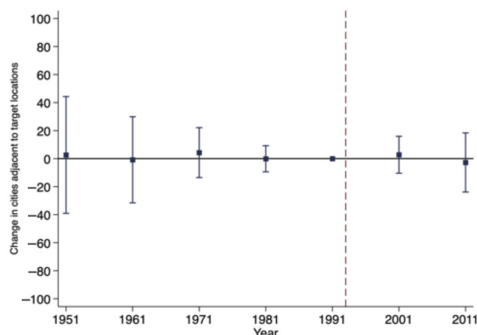
(a) Women in labour force



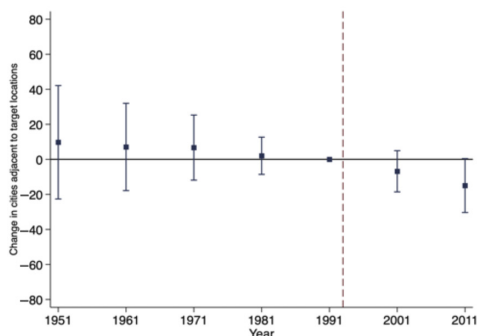
(b) Employed women



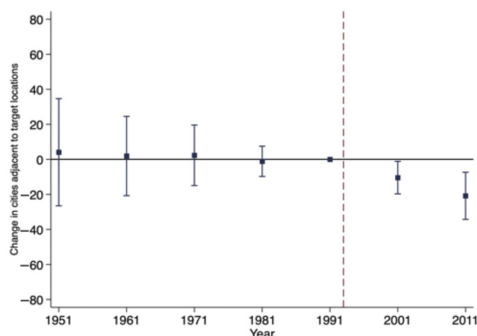
(c) Men in labour force



(d) Employed men



(e) Gender gap in labour force



(f) Gender gap in employment

Fig. 3. *Yearly Effects of Fiscal Decentralisation on Local Labour Markets.*

Notes: Panels (a) to (f) show the post-LPT change in cities adjacent to municipalities targeted by Allied tactical air attacks during WWII. The control group is composed of municipalities adjacent to cities matched to target 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% CIs.

Source: 8mila Census, ISTAT, available online at <http://ottomilacensus.istat.it/>.

Next, we repeat the same estimation using the INPS dataset, which has 18 yearly data points (1974–92) before the LPT introduction. As discussed in Section 1.2, 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 non-linear pre-reform trends between younger and older women, and between treated and control locations (Figure 4; Online Appendix Table A6). For both linear and non-linear trends, we fail to reject the hypothesis that treated and control municipalities were following parallel trends before the LPT. This result holds for all age groups.

Second, we show that just before the introduction of the LPT, target-adjacent and target-distant municipalities were similar in terms of observable characteristics, except for the share of pre-WWII buildings and the post-LPT degree of fiscal decentralisation. Out of 46 additional variables observed in 1991, only one is statistically different at the 5% level (Table 2, 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, target-adjacent and target-distant municipalities differed in the number of women in the labour force by 11.5 residents or 1.1% of the 1991 mean, in the number of men in the labour force by 4.3 residents or 0.2% of the mean and in the number of pupils in nursery school by 0.5 children or 2.4% of the mean. Moreover, these balancing tests range from labour-market outcomes to demographics, ideology, geography and municipal balance sheets, indicating that treated and control firms were similar in a wide variety of areas.

Third, there are several pieces of evidence indicating that target-adjacent municipalities 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. Relative to the control, target-adjacent locations have 2.7-percentage-point fewer pre-WWII buildings (Table 2, panel A, column (1)). They also experienced a €24 higher change in per-capita revenues from local taxes after the LPT, an 18% increase from the mean. Moreover, we show that the intensity of the Allied bombings *in the target locations* can predict the share of buildings destroyed during WWII *in the target-adjacent municipalities*. Specifically, a three-fold increase in the tons of explosives dropped on a targeted location increased the share of buildings in target-adjacent municipalities that were still damaged by the war in 1947 by up to 4% from the mean (Table 4, panel A), decreased the share of pre-WWII buildings in 1991 by up to 6% from the mean (Table 4, panel B), and increased the change in per-capita revenues from local taxes after 1993 in adjacent municipalities by €12.6 or 9.5% from the mean (Table 4, panel C).

3. Effects of Fiscal Decentralisation

3.1. *Effects on Municipal Spending*

3.1.1. *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 target-distant cities, target-adjacent municipalities experienced a disproportionate increase in the revenues from local taxes (on average, €12 after 1992; Online Appendix Figure A3, panel A) and a decrease in the revenues from government transfers (–€19; Online Appendix Figure A3, panel B). This effect is the first stage of the policy.

Our main findings suggest that fiscal decentralisation led to reduced waste and increased access to local services. First, the overall size of the municipal budget decreased disproportionately in target-adjacent cities. Spending per resident decreased by €61, while revenues per resident

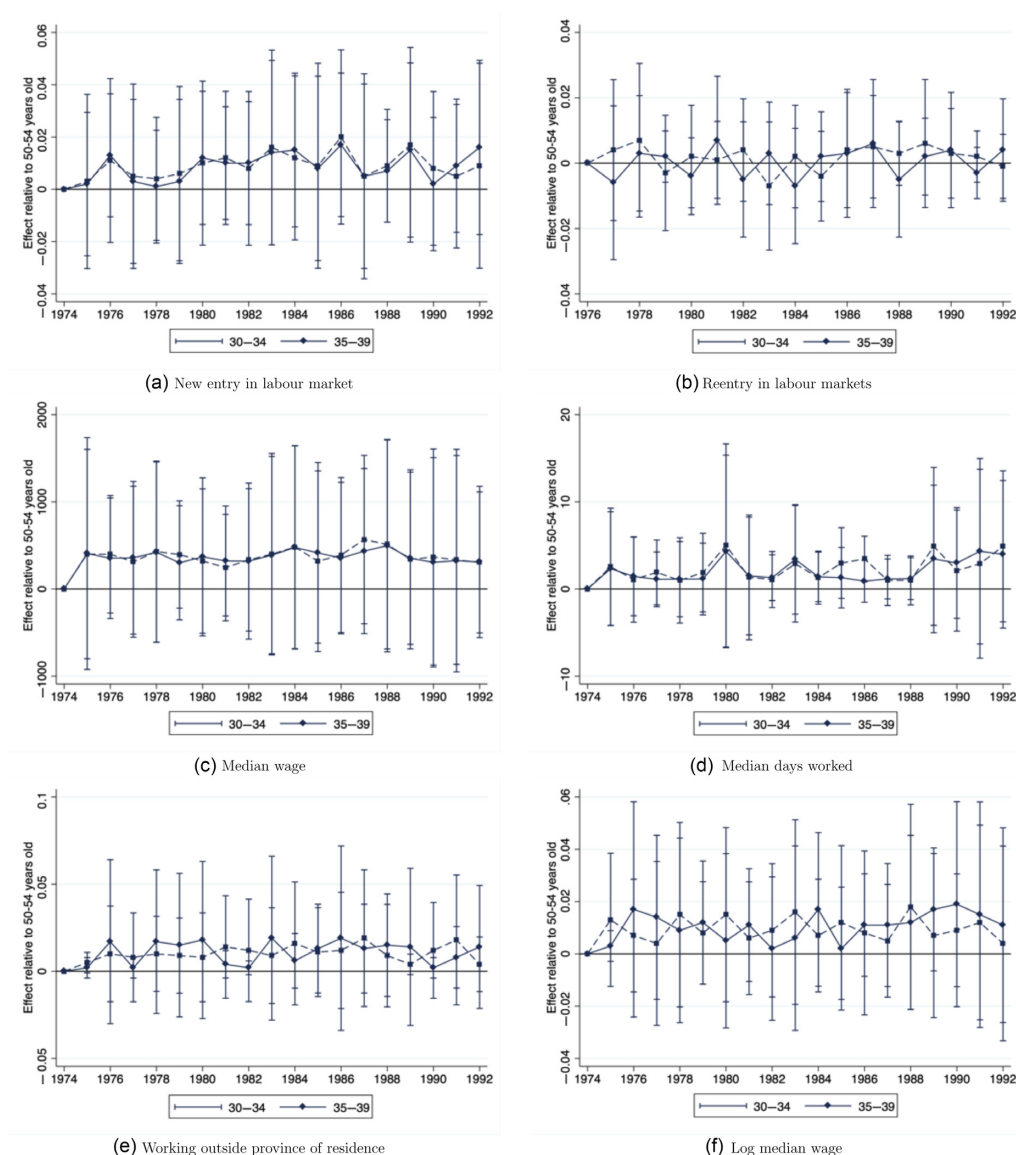


Fig. 4. Pre-Reform Trends with Main Specifications, Social Security Data.

Notes: Panels (a) to (f) show triple interactions of age, a dummy equal to 1 for near-target locations and pre-reform year dummies. The sample includes only women. The omitted age group is composed of 50–54-year-olds. For the sake of clarity, the graphs show the coefficients for only two age bins (30–34 years old and 35–39 years old). 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% CIs.

Source: Istituto Nazionale della Previdenza Sociale (INPS).

Table 4. *Consequences of Allied Bombings in Target-Adjacent Municipalities.*

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: dependent variable is the share of buildings still damaged by WWII in 1947</i>					
IHST post-armistice bombs	0.441*** (0.066)	0.368*** (0.059)	0.368*** (0.087)	0.342*** (0.076)	0.211*** (0.066)
Population in 1991					0.001*** (0.000)
Minimum altitude					−0.006*** (0.001)
Rural city					−1.388** (0.587)
Observations	2,342	2,342	2,342	2,336	2,321
Dep. var.—mean	33.82	33.82	33.82	33.82	33.82
Dep. var.—SD	8.60	8.60	8.60	8.60	8.60
<i>Panel B: dependent variable is the share of pre-war buildings in bombing-adjacent municipalities</i>					
IHST post-armistice bombs	−0.794*** (0.143)	−0.605*** (0.131)	−0.605*** (0.206)	−0.799*** (0.190)	−0.447*** (0.151)
Population in 1991					−0.001*** (0.000)
Minimum altitude					0.007* (0.004)
Rural city					12.635*** (1.517)
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.—SD	19.19	19.19	19.19	19.19	19.19
<i>Panel C: dependent variable is the change in per-capita revenues from local taxes (1990–4) in bombing-adjacent municipalities</i>					
IHST post-armistice bombs	4.580*** (0.962)	3.065*** (0.956)	3.065** (1.179)	3.273*** (1.122)	4.198*** (1.146)
Population in 1991					0.001** (0.000)
Minimum altitude					0.136*** (0.023)
Rural city					−24.008*** (8.612)
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.—SD	120.59	120.59	120.59	120.59	120.59
Controls	No	Region FE	Region FE	Province FE	Region FE
Cluster	Robust	Robust	Province	Province	Province

Notes: In all panels, the dependent variable measured in target-adjacent municipalities is regressed on the IHST of post-armistice Allied bombings that hit the *adjacent targeted location*. In panel A, the dependent variable is the share of buildings damages during WWII in target-adjacent municipalities from the 1947 Italy Country Study prepared by the U.S. Economic Cooperation Administration. In panel B, the dependent variable is the share of pre-war buildings in 1991 in target-adjacent municipalities. In panel C, the dependent variable is the change in per-capita revenues from local taxes between 1990 and 1994 in target-adjacent municipalities. Among targeted locations, the 25th percentile of the tons of bombings is equal to 39 tons, while the median is equal to 114 tons. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

decreased by €64 (Table 5, 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 target-adjacent municipalities, administrators prioritised spending for revenue-generating services. Welfare, education and police account for 24% of total spending and 39% of total revenues, while administrative tasks and transportation account for 50% of spending and only 14% of total revenues (Online Appendix Table A1).

Table 5. *Effects of Fiscal Decentralisation on Municipal Spending and Services*

	Region-year fixed effects			Province-year fixed effects			Mean outcome	SD
	Near targets	Obs.	R ²	Near targets	Obs.	R ²		
	× post (1)	(2)	(3)	× post (4)	(5)	(6)		
<i>Panel A: dependent variables from municipal balance sheets before and after the LPT</i>								
Rev. from local taxes	12.321*** (4.481)	47,157	0.892	14.122*** (4.745)	47,028	0.903	158.54	108.92
Rev. from gov. transfers	−19.357*** (5.655)	47,156	0.843	−16.759*** (5.966)	47,027	0.854	528.31	218.83
Revenues per capita	−64.355** (31.944)	47,153	0.608	−26.235 (35.098)	47,024	0.630	1677.24	1197.95
Spending per capita	−60.612* (32.496)	47,140	0.607	−24.035 (35.998)	47,012	0.629	1674.14	1203.15
Deficit per capita	1.994 (4.601)	47,120	0.106	4.160 (6.155)	46,992	0.131	−4.24	148.30
<i>Panel B: dependent variables from censuses before and after the LPT</i>								
Pupils in nursery schools	2.475*** (0.746)	7,277	0.879	2.774*** (0.775)	7,259	0.900	10.43	21.23
IHST pupils in nursery schools	0.135*** (0.029)	7,277	0.886	0.149*** (0.033)	7,259	0.896	2.28	1.05
Share below 3 in nursery schools	1.122** (0.479)	7,228	0.649	1.376** (0.568)	7,210	0.666	10.25	11.48
Share of population in nursery schools	0.039*** (0.008)	7,271	0.693	0.039*** (0.010)	7,253	0.712	0.30	0.26
Share below 3	0.057** (0.026)	9,687	0.723	0.067** (0.030)	9,663	0.745	2.84	1.03
Share between 4 and 5	−0.023 (0.021)	9,687	0.691	−0.061*** (0.024)	9,663	0.709	2.14	0.78
Share between 4 and 9	−0.064 (0.051)	9,687	0.840	−0.168*** (0.055)	9,663	0.857	7.81	2.50
Foreign residents	49.582*** (10.176)	7,271	0.662	50.568*** (9.868)	7,253	0.713	18.64	40.89
<i>Panel C: dependent variables are available only after the LPT</i>								
Has fiscal infraction	−0.006 (0.012)	17,954	0.192	−0.011 (0.013)	17,888	0.243	0.51	0.5
Spending for local services (%)	1.195*** (0.337)	28,401	0.266	0.835** (0.365)	28,319	0.327	54.8	16.25
Rev. for admin. tasks per employee	257.568** (121.449)	28,560	0.063	292.717** (136.087)	28,478	0.123	2244.73	3756.45
Has program for local develop.	0.074*** (0.016)	28,430	0.163	0.050*** (0.018)	28,347	0.233	0.61	0.49
Has nursery schools	0.054*** (0.014)	28,430	0.222	0.042*** (0.016)	28,347	0.296	0.63	0.48
Spending for nursery schools (%)	0.178*** (0.065)	28,248	0.283	0.045 (0.065)	28,165	0.387	1.01	2.03
Public nursery schools	0.052*** (0.015)	17,326	0.391	0.005 (0.016)	17,194	0.504	0.26	0.61
Pupils in private nursery schools	0.015 (0.403)	2,403	0.771	0.159 (0.414)	2,397	0.804	11.88	21.18

Notes: Monetary values are expressed in 2017 €. ‘Near targets’ is 1 for municipalities adjacent to cities targeted by Allied tactical air attacks during WWII. The control group is composed of municipalities adjacent to cities matched to target locations. Panel A uses dependent variables from municipal balance sheets that are available every year between 1990 and 2010. Panel B uses dependent variables from the censuses that are available in 1991, 2001 and 2011 (pupils in nursery schools) or in 1981, 1991, 2001 and 2011. Panel C uses dependent variables from balance sheets that are available only between 1998 and 2010 (the number of pupils in private nursery schools is available from the census only in 2011). In this case, the treatment variable is just ‘Near targets’, not its interaction with ‘post’. These regressions also include all controls described in Section 2. Standard errors clustered at the city level are given in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Spending on welfare, education and police was between 0.11σ and 0.15σ higher in target-adjacent cities after the LPT, while spending on administration and transportation was between 0.12σ and 0.14σ lower (Online Appendix Table A7). Overall, these findings are consistent with increased accountability of local administrators. Lower spending and the prioritisation 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 target-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 target-adjacent cities, revenues per administrative worker were €258 higher (Table 5, panel C).

Fourth, even though total spending decreased, target-adjacent municipalities devoted more resources to local services. The share of spending for local services was 1.2 percentage points higher in target-adjacent locations (Table 5, panel C).²⁸ Target-adjacent cities were 7.4 percentage points more likely to have programs for local economic development, a 12% increase from the mean. Similarly, they were 5.4 percentage points more likely to have at least one public nursery school, a 9% difference from the mean. These are two services that can have direct positive effects on the local labour 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.

3.1.2. *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 labour participation is an important goal in many developed countries.²⁹ 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 labour market was 40%. In comparison, female labour-force participation was equal to 52% among OECD countries and 51% in the European Union.³⁰ Among many possible solutions, the availability of affordable nursery schools has proved to be positively correlated with female labour 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 enrol in kindergarten until they start compulsory schooling at six years old. Public kindergarten, however, is managed by higher levels of government and so was not directly affected by the introduction of the LPT. Although nursery schools are subsidised by municipalities, families pay a monthly fee. The share of costs paid by families must be at least 50% and each municipality can autonomously decrease the level of subsidisation. Municipalities can also choose the fee structure: a flat payment or a tiered system based on household income or wealth. In 2018, a two-parent household with a gross annual income of €44,200 would pay on average €300 a month for a public nursery school (Cittadinanzattiva, 2018).

Demand for public nursery schools vastly exceeds available supply. In 2008, the total capacity of public nursery schools was equal to only 12% of the population below three years old (Cittadinanzattiva, 2018). For this reason, public nursery schools have long waiting lists. On average,

²⁸ This increase came at the expense of the other two main sources of spending: capital investments and debt repayments.

²⁹ 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).

³⁰ ILOSTAT database; data available online at <http://api.worldbank.org/v2/en/indicator/SL.TLF.CACT.FE.ZS?downloadformat=xml>.

27% of applicants (more than 52,000 children in 2008) are not admitted (Cittadinanzattiva, 2011). As a result, many households have to rely on private nursery schools. Out of all pupils enrolled in nursery schools in 2016, 48% attended private institutions, 39% public institutions and 13% private providers affiliated with municipalities.³¹ 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%.³²

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

We now show that target-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, target-adjacent municipalities invested more heavily in nursery schools. Compared with target-distant cities, they dedicated a larger share of their budget to nursery schools (+18% from mean), were 5.4 percentage points (+9% from mean) more likely to have at least one public nursery school and had 0.05 (+20% from mean) more public nursery schools (Table 5, panel C).³³

Second, higher provision translated into higher utilisation. In target-adjacent municipalities, enrolment in nursery schools increased by 2.5 children or 24% from the pre-LPT mean (Table 5, panel B). This finding is robust if we use the IHST of the enrolment in nursery schools (+0.14 log points). Moreover, there is evidence that the share of residents below three years old increased disproportionately after the LPT in target-adjacent locations (+0.06 percentage points or a 2% increase from the mean). This small increase may be the result of increased fertility or relocation of families with young children in response to the availability of better subsidised child care. However, the number of pupils in nursery schools increased in target-adjacent municipalities, even after accounting for the increase in the number of children below three years old. Specifically, the share of pupils in nursery schools increased by 1.12 percentage points (11% from the mean) when the denominator is the total number of children below three years old, and by 0.04 percentage points (13% from the mean) when the denominator is the total population.

Third, exposure to fiscal decentralisation did not have positive effects on pupils who did not benefit from municipal services. Specifically, the share of children above three years old, who attended kindergarten and elementary schools run by the central government, did not disproportionately increase in target-adjacent cities (Table 5, panel B). Moreover, 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 target-adjacent and target-distant municipalities (Table 5, panel C). This finding indicates

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

³² In the city of Milan, for example, public nursery schools cost between €0 a month for low-wealth households and €465 a month for high-wealth households (http://www.comune.milano.it/wps/portal/ist/it/servizi/educazione/Servizi_0-6_anni/Nidi_Micronidi/Quote_Contributive_+Nidi_+e_+Sezioni+Primavera). In the same municipality, private nursery schools cost between €460 and €800 a month (<https://www.milanolife.it/migliori-asili-nido-privati-milano/>).

³³ Most of these results are robust to adjusting *p*-values for multiple hypothesis testing (Online Appendix Table A8).

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.

3.2. *Effects on Local Labour Markets*

In this section, we study whether the changes in the provision of public services had effects on local labour markets. There are several services provided by municipalities that could have relevant consequences on both labour 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 labour demand. Investments in welfare programs, such as nursery schools, could also affect labour supply. The data indicate that participation in the labour market disproportionately increased in target-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 labour-market outcomes.

We establish five main findings. First, female labour-force participation significantly increased in target-adjacent municipalities. As we discussed in Section 2.3, target-adjacent and target-distant municipalities experienced similar trends in women's labour-force participation between 1951 and 1991 (Figure 3(a)). All pre-LPT coefficients are small and statistically insignificant. Then, in target-adjacent municipalities, female labour-force participation increased by 15 individuals (3.4%) in 2001 and by 23 individuals (5.1%) in 2011. Both coefficients are statistically different from zero. We find the same results if we measure only women's employment, rather than focusing on their labour-force participation (Figure 3(b)).

Second, there are not systematic differences in male labour-force participation between target-adjacent and target-distant municipalities. Relative to 1991 and target-distant locations, men's labour-force participation in target-adjacent municipalities increased by eight individuals (0.8%) in 1951, by five individuals (0.4%) in 1961, by 10 individuals (0.9%) in 1971, by four individuals (0.04%) in 1981 and by eight individuals (0.8%) in both 2001 and 2011 (Figure 3(c)). None of these coefficients is statistically different from zero. Focusing on male employment leads to the same conclusions (Figure 3(d)). The main consequence was a reduction in the pre-existing gender gap in labour-force participation (Figure 3(e)) and employment (Figure 3(f)).

Third, the labour-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 target-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 (Online Appendix Figure A4, panels A to C).³⁴ In target-adjacent cities with a share of young parents in the top quartile, women's labour-force participation increased by 58 individuals (12.9%) in 2001 and by 88 individuals (19.6%) 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 (Online Appendix Figure A4, panels D to F), rather than the share of all young parents.

Fourth, fiscal decentralisation did not induce large movements of people across municipalities. In target-adjacent cities, population increased by an additional 27 individuals in 2001 (0.7%) and by 34 individuals (0.9%) in 2011 after the LPT (Online Appendix Figure A5, panel A).

³⁴ To complete the triple-difference specification, we also include the interaction between the new dummy and year fixed effects.

Both coefficients are statistically insignificant. The results are similar if we focus exclusively on women (Online Appendix Figure A5, panel B) or men (Online Appendix Figure A5, panel C). Therefore, target-adjacent municipalities experienced an increase in both the total number and the share of women who were active in the labour market (Online Appendix Figure A6). For example, relative to 1991 and target-distant locations, the share of employed women in target-adjacent municipalities increased by 0.74 percentage points (2% from the mean) in 2001 and by 1.69 percentage points (4.7% from the mean) in 2011. However, even though total population did not change, we establish that the number of foreign residents disproportionately increased in target-adjacent municipalities after the LPT (Table 5, panel B). This finding indicates that better welfare services may have attracted more individuals in need of economic support.³⁵

Fifth, we do not find large and significant effects of fiscal decentralisation on the number of local firms. In target-adjacent cities, the total number of firms decreased by three units (1.5%) in 2001 and by four units (1.9%) in 2011 (Online Appendix Figure A5, panel D). This result does not change if we focus on firms with fewer than three employees (Online Appendix Figure A5, panel E), which represents the bulk of the Italian firm stock, or public-owned firms (Online Appendix Figure A5, panel F).³⁶ Overall, these findings suggest that the introduction of the LPT did not significantly change the overall demand for labour. 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 labour.

Finally, we can directly tie fiscal decentralisation to the labour markets by estimating instrumental variable regressions. In Equation (2), we replace Near targets_{*m*} with $\Delta\text{Rev. local tax (90–94)}_m$, which measures the change in per-capita revenues from local taxes between 1990 and 1994 in municipality *m*. Because $\Delta\text{Rev. local tax (90–94)}_m$ could be endogenous, as explained in Section 2.1, we instrument it with the baseline treatment Near targets_{*m*}. The overall pattern of results from these instrumental variable specifications is in line with the previous reduced-form estimates (Online Appendix Figure A7).

3.3. *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 decentralisation. The data support this assumption. Relative to target-distant cities, the per-capita revenues from local taxes in target-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 3.1).

In a frictionless environment, this result might not have been possible. In light of their cities' lower cadastral values, administrators of target-distant cities could have increased the LPT rates to equalise revenues from local taxes across municipalities. Tax rates, however, were bounded between 0.4% and 0.7%. Therefore, we should expect tax rates to be higher in target-distant municipalities, but this effect should not be sufficient to delete any difference in per-capita

³⁵ Data from the Survey of Households' Income and Wealth (SHIW), a survey by the Bank of Italy that is representative of the Italian population, indicate that foreign residents earn on average 27% less than domestic residents. This correlation is robust to controls for place of residence, age, marital status, household size, gender and even education.

³⁶ In target-adjacent cities, we also observe a larger reallocation of workers from agricultural (Online Appendix Figure A5, panel G) and manufacturing (Online Appendix Figure A5, 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 contrast, there are no changes in the number of public employees (Online Appendix Figure A5, panel I).

revenues from local taxes. As expected, compared to target-adjacent cities, target-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 (Online Appendix Table A9, column 6). These effects are statistically significant, but small in magnitude.

Moreover, compared to target-adjacent cities, target-distant municipalities were 2.8 percentage points more likely to reach the max LPT rate of 0.7%. This coefficient represents a sizeable 22% 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, target-distant municipalities were only 0.9 percentage points more likely to reach the max LPT rate. This finding indicates that, in addition to the fact that some municipalities were constrained by the upper bound to tax rates, many 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 target-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 decentralisation 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 decentralisation in cities with a lower share of owner-occupied main residences.³⁷ Therefore, we modify Equation (2) by adding a triple interaction between year fixed effects, the target-adjacent dummy and a new dummy equal to 1 for 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 labour-force participation and employment primarily stemmed from cities with a lower share of owner-occupied buildings (Online Appendix Figure A8).

3.4. *Magnitudes*

An ideal test for studying the effects of fiscal decentralisation 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 labour-force participation is the result of a negative income effect from higher taxation, rather than stemming

³⁷ The special LPT for main residences 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.

from improved local services. Although economists have not reached a consensus on the effect size of wealth on labour decisions, ‘some agreement exists among labour economists that large, permanent changes in real wages induce relatively modest differences in labour supply’ (Cesarini *et al.*, 2017; p. 3918). However, using Italian data, Giupponi (2019) found that individuals who lose a substantial portion of survivor benefits (on average, €2,000 a year or 32% of the survivors’ yearly labour 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% 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, if effective in increasing labour supply, such a small negative income effect should act mostly on the intensive margin (working hours). However, we find that the introduction of the LPT significantly increased women’s labour-force participation. Third, 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 3.2 indicated that the labour-market effects are concentrated in municipalities with higher shares of young parents, who are more likely to have pre-kindergarten children.³⁸

Next, we estimate the elasticities of local services and labour supply with respect to the degree of fiscal decentralisation. The per-capita revenues from local taxes increased by up to €19 more in target-adjacent municipalities, relative to target-distant locations. This difference is large in magnitude, 12% of the average share of revenues from local taxes in 1990, and precisely estimated. A 1% increase in the revenues from local taxes decreased total revenues and total spending by 0.3%, increased spending for local services by 0.2%, women’s participation and employment by 0.4%, the probability of having nursery schools by 0.7% and spending for nursery schools by 1.5%. The magnitude of these effects is consistent with the empirical evidence in the literature on public finance. For example, Gadenne (2017) estimated an elasticity of education spending to revenues from local taxes equal to 0.5, while Martinez (2018) estimated an elasticity of local spending from property tax revenues equal to 0.2.

Finally, we can directly estimate the elasticity of women’s labour-force participation to increased access to subsidised childcare. A 1% increase in the number of pupils enrolled in nursery schools increased women’s labour force participation by up to 0.21%. Moreover, considering an average cost differential between public and private nursery schools of €176, a 1% increase in the subsidies to public childcare increased women’s labour force participation by up to 0.14%.³⁹ In a similar Italian context, Carta and Rizzica (2018) estimated that a 1% increase in childcare subsidies increased women’s participation between 0.14% and 0.22%.

3.5. 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

³⁸ These findings are further corroborated by the analysis on Social Security data in Section 3.6.

³⁹ Carta and Rizzica (2018) estimated that the average monthly cost of nursery schools is €487 for private institutions and €311 for public institutions.

⁴⁰ Online Appendix B includes a longer discussion of the mechanisms studied by the theoretical and empirical literature on fiscal decentralisations.

of local politicians and the degree of competition between adjacent municipalities. Moreover, it could have transferred the responsibility to provide local services to administrators who are closer to the final users and, therefore, have better knowledge about their preferences.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 targets_{*i*} to estimate the change in labour outcomes between target-adjacent and target-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, target-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 (Online Appendix Table A10). These differences in the provision and utilisation of public services translated into a larger effect on women's labour-force participation and employment (Online Appendix Figure A9). 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 labour-market outcomes.

Finally, the fact that higher fiscal decentralisation 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 behaviour established in 2001. Specifically, we correlate several measures of active political participation with either the mean or median difference in the share of municipal revenues from local taxes between 1990 and 1994 in the respondents' region of residence, a measure of short-term exposure to fiscal decentralisation.⁴⁵ 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 decentralisation is correlated with higher levels of political participation (Online Appendix Table A11). For example, a €54 increase in mean revenues from local taxes (from the 25th percentile to the median) is associated with an 8% higher probability of being interested in politics and with a 3% higher probability of voting.

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.

⁴⁵ 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.

3.6. *Heterogeneities by Age and Gender*

In this section, we analyse the effect of fiscal decentralisation 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 labour 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 (Online Appendix Figure A10). 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 labour 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 subsidised retirement homes is another municipal service who may spur women's labour-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 (Online Appendix Figure A11). If expanded access to retirement homes is one of the major drivers of increased female labour 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:

$$y_{amt} = \alpha_m + \beta_b + \gamma_{rt} + \zeta_{at} + \sum_a \delta_0^a \text{Age}_a \times \text{Near targets}_m \times \text{Post}_t \\ + \sum_a \delta_1^a \text{Age}_a \times \text{Near targets}_m + \delta_2 \text{Near targets}_m \times \text{Post}_t + \varepsilon_{amt}. \quad (3)$$

Here the unit of observation is an age group a living in municipality m in year $t \in [1974, 2011]$;⁴⁶ 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 labour-market outcomes between younger and older women, between target-adjacent and target-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 labour supply was larger among younger women (Figure 5 and Online Appendix Table A12). The number of women entering the labour market for the first time increased the most for women aged 25 to 28. Relative to target-distant municipalities and women between 50 and 54 years old, the number of new entrants into the labour market increased by 13% (from the pre-LPT mean) per post-LPT year, city and age group (Figure 5(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

⁴⁶ Variables measuring reentry into the labour market, instead of first entry, start in 1976 because the first years are necessary to detect a break in employment.

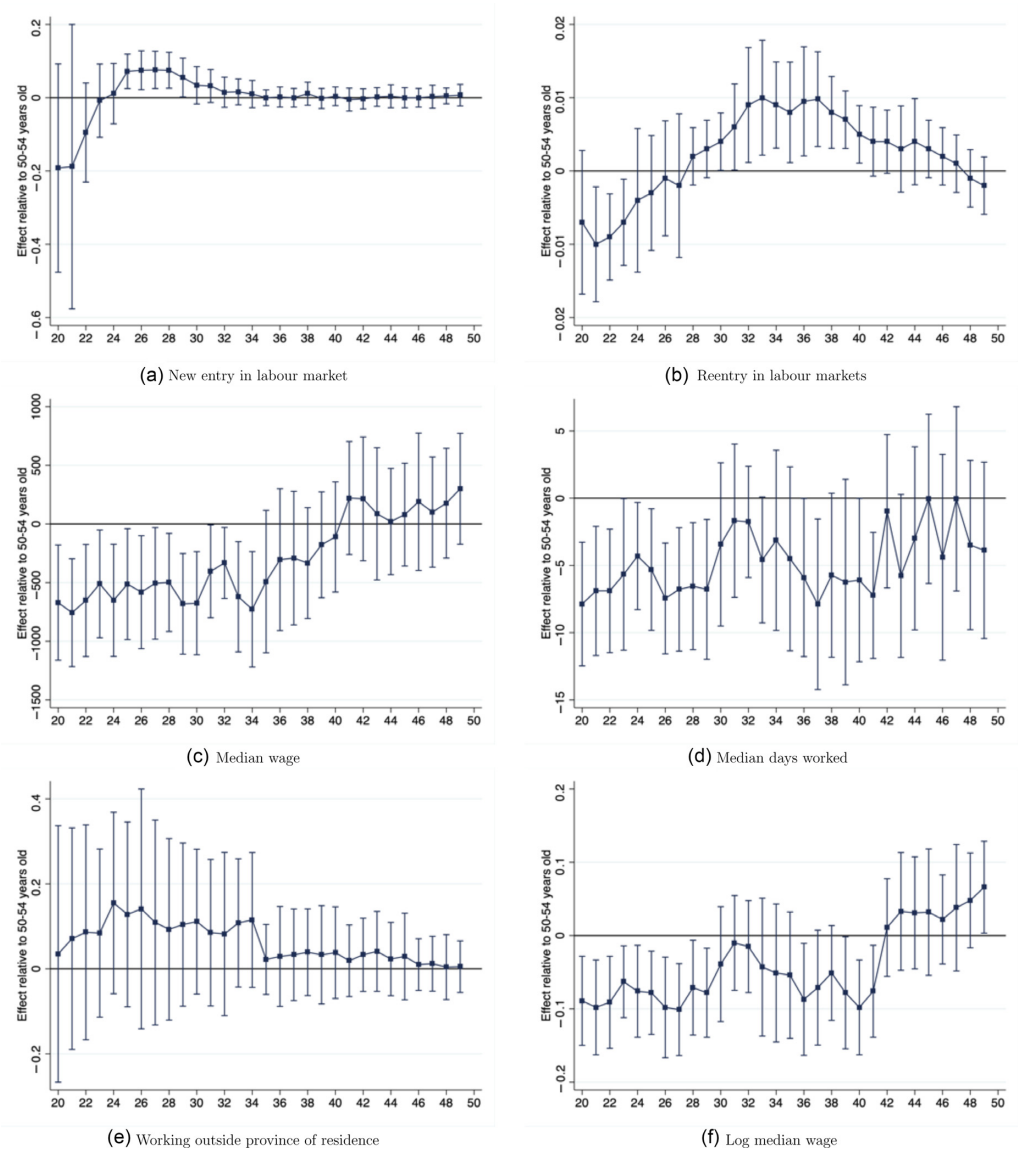


Fig. 5. Age Effects, Employees of Privately Owned Firms.

Notes: Panels (a) to (f) show triple interactions of age, a dummy equal to 1 for near-target locations and a post-1993 dummy. The sample includes only women. The control group is formed by municipalities adjacent to cities matched to target locations. The omitted age group is composed of 50–54-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% CIs.

Source: Istituto Nazionale della Previdenza Sociale (INPS).

labour market after a hiatus increased by 21% to 24% between ages 30 and 40 (Figure 5(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 labour supply of these women decreased disproportionately in target-adjacent municipalities after the LPT. This effect could suggest that expanded nursery schools might have allowed these women to pursue a post-secondary degree. The education information available in the INPS dataset indicates that the number of women with a university degree is 111% higher among 20–24-year-old women living in target-adjacent municipalities, relative to target-distant cities and women between 50 and 54 years old (Online Appendix Table A12, panel D).⁴⁷

After confirming that labour supply increased more for younger women, we analyse what types of jobs they were more likely to hold. We find that fiscal decentralisation slightly decreased median wages of younger women (Figure 5(c)). This decrease was between €315 and €648 per year (−2.2% to −4.6%) and was concentrated among women under 35.⁴⁸ In target-adjacent municipalities, younger women also became slightly more likely to work fewer days during the year (−3%; Figure 5(d)) and more likely to work outside their province of residence, although these last estimates are imprecise (+21%; Figure 5(e)). Overall, these results suggest that fiscal decentralisation decreased the reservation wage of younger women. The cost of participating in the labour force might have decreased because more women could enrol 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 labour supply or to slight modifications to (3).⁴⁹ Here, we want to briefly describe two follow-ups to the main results. First, estimating triple differences allows us to control non-parametrically for any change at the city-year level. We replace region-year fixed effects in (3) with city-year fixed effects. All the main findings are unaffected by this more demanding specification (Online Appendix Table A13, 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 (Online Appendix Table A13, panel C).

More importantly, the Social Security data are the only dataset available at the municipal level that has yearly observations between 1974 and 2011. Therefore, we can use them to study the dynamics of labour 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 (Online Appendix Figure A12). As discussed in Section 2.3, the results show the lack of differential pre-LPT trends between target-adjacent and target-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

⁴⁷ 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 labour contract (for example, joining a new firm or receiving a major promotion) after 2005. It is therefore more complete for younger employees.

⁴⁸ These findings are robust to using alternative wage measures, such as log median wage (Figure 5(f)) or median hourly wage (Online Appendix Table A12). Moreover, these results hold if we compute median wages using only new entrants or reentrants into the labour market (Online Appendix Table A12, panel C).

⁴⁹ Online Appendix Table A12 presents the estimated coefficients from all the regressions using INPS data. Online Appendix Table A13 presents robustness checks.

reasons. First, around the same period, most municipalities in the sample held the first post-LPT elections (Online Appendix Figure A13).⁵⁰ Although only suggestive, this finding is consistent with the main takeaway of Section 3.5: the level of political competition seems to be the most plausible mechanism to have induced local administrators to improve municipal services, such as public nursery schools. Second, we should not expect any change in the labour 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 stabilised or decreased. This non-increasing trend during the last years of the sample is consistent with the fact that cross-municipality differences in fiscal decentralisation may have decreased after the implementation of the PIT surcharge in 2002, and the cancellation of the LPT on main residences in 2007 (Section 3.7).

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 target-adjacent locations and a post-reform dummy.⁵¹ 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 labour supply in target-adjacent municipalities experienced a larger increase in younger women after the LPT, relative to men and older women (Online Appendix Table A14). Younger women also became more likely to hold positions with lower wages, more flexible hours and higher commuting costs.⁵²

3.7. Robustness Checks

In this section, we perform several robustness checks. First, we control for the main post-war 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.⁵³ All the main treatment effects are robust to the inclusion of three different specifications of Marshall Plan aid (Online Appendix Figure A14).

Second, we estimate the baseline Equation (2) on a matched subsample of target-adjacent and target-distant municipalities using population, area size and region fixed effects (Table 2, column (3)). These regressions show treatment effects that are close in magnitude and precision to the baseline estimates (Online Appendix Figure A15).

Third, the main findings are fully robust to several changes to the initial matching process between bombed and non-bombed locations (Online Appendix Figure A16). For example, including 24 variables in the propensity score matching, instead of the eight listed in Section 2.2,

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

⁵¹ The full specification also includes all triple and double interactions of the gender dummy with the other treatment variables.

⁵² These effects on the characteristics of the labour contracts are more precisely estimated when they are computed only on new entrants or reentrants into the labour market (Online Appendix Table A14, panel C).

⁵³ We cannot use the amount of aid at the city level because none of the target-adjacent or target-distant municipalities directly received grants. This fact alone suggests that post-war reconstruction is unlikely related to our findings.

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 city level (Online Appendix Figure A17, panel A). Similarly, the results hold if we estimate standard errors that are robust to both spatial and serial correlation (Conley, 1999; Online Appendix Figure A17, panel B).

Fifth, our baseline specification already includes many non-linear trends. Moreover, the results are robust to controlling for even more non-linear trends correlated with provinces, geographical characteristics, characteristics of the real-estate market and demography (Online Appendix Figure A17, panels D to H).

4. Conclusions

This paper studies how fiscal decentralisation affects the provision of public services and local labour markets. It exploits a 1993 Italian reform that introduced an 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.

In municipalities with higher exposure to fiscal decentralisation, local politicians increased the quantity of publicly provided services. One of the most important services, subsidised nursery schools for children between six months old and three years old, experienced a 24% enrolment increase. In municipalities with higher exposure to fiscal decentralisation, female employment increased by up to 20%, reducing the pre-existing gender employment gap. These findings are indicative of the importance of subsidised public childcare on the decision of women to participate in the labour 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 labour 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 labour-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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Additional Supporting Information may be found in the online version of this article:

Online Appendix Replication Package

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