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The Impact of Place-based Policies on Interpersonal Income Inequality•

Giuseppe Albanese,[*] Guglielmo Barone,[**] Guido de Blasio[***]

Abstract

This paper assesses the causal impact of the EU cohesion policy, aimed at reducing the regional divide within the EU, on interpersonal income inequality in receiving areas. We leverage a severe contraction of financing, which took place in an Italian region in 2007, and adopt a difference-in-discontinuity empirical design to show that the Gini index (of income) at the municipality level goes down because of the end of the policy. The improvement is due to the move of top earners toward the center of the distribution. The reduction in the Gini indicator is confirmed even if we resort to a region-level analysis. Our results suggest that, from a policy perspective, reducing spatial inequality might come with the cost of worsening inequality across individuals.

Keywords: regional divide, place-based policies, interpersonal income inequality.
JEL Classification: R10, D63.

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1. Introduction

Income inequality is of rising interest to economists, especially in light of its strong growth since the 1980s (Atkinson and Piketty, 2007; Piketty, 2014; Milanovic, 2016). At the same time, another dimension of inequality – that referred to regional differences within states – has also been the subject of renewed attention, both from the academic and the policy perspective. For example, Rosés and Wolf (2018) suggest that the Gini index of regional GDP in European regions is increasing since the 1980s; the IMF (2019) documents that disparities in economic activity across subnational regions in the average advanced economy have been gradually going upward during the last 30 years.

Place-based policies (PBPs) are the usual policy tool, widespread all over the world, to address inequality between regions. While their effects on local economic growth have been widely explored (e. g. Becker et al., 2010; Busso et al., 2013; Kline and Moretti, 2014a; Overman and von Ehrlich, 2020), much less is known, however, about their impact on interpersonal income inequality within recipient regions, which should arguably be an important piece of information to assess the desirability of the policy: it is important to examine whether addressing inequality between regions comes at the expense of personal distribution of income within targeted areas. Suppose that two different PBPs similarly increase growth relative to well-developed areas, thus reducing inequality between the areas. However, the former also results in more equal income distribution within the targeted area, as money mainly flows to lower-income individuals, while the latter worsens income distribution, because those who are better off reap greater benefits from the policy. Consistent with the ultimate goal of PBPs, which is redistribution, the first program should be preferred.

One dollar spent in areas lagging behind does not necessarily translate into a more egalitarian income distribution within targeted places (Bartik, 2020). There are three main channels through which PBPs might increase income inequality in the treated areas. Firstly, in a standard spatial equilibrium framework, mobile workers relocate to the area that receives the aid, causing an increase in the price of housing. With high elasticity of local labor supply and low elasticity of housing supply, local landowners are going to keep most of the gains (Moretti, 2010; Kline and Moretti, 2014b). If landlords also have a higher pre-policy income, the PBP would increase inequality. Secondly, suppose that the PBP involves, as is often the case, financial incentives to firms to increase employment. If the program is not effective, local unemployment will not improve, while the firm owners, typically belonging to the top end of the income distribution, will receive a financial premium for hiring the same number of people they would have hired anyway. Thirdly, considerations borrowed from political economics would suggest that the distribution of the benefits would also be affected according to the mechanism in place for rent extraction (e.g. Bjørnskov, 2010; Brollo et al., 2013). For instance, in the case of the EU cohesion policy most of the transfers are intermediated by local political entities. If politicians, together with the “corrupt” elite in their inner circle, divert the funds, then it seems unlikely that the poor will gain from the PBP.

This paper sheds some light on the effect of the EU structural funds, the largest European PBP, on income distribution in the recipient area, by leveraging the quasi-natural experiment provided by the exit in 2007 of one Italian region (Molise) from the pool of heavily subsidized regions (those belonging to the so-called Objective 1 program), without any compensation in the financing from the central government to local governments (aimed at offsetting the retrenchment of the EU aids). We prove our results by using evidence drawn from three different empirical strategies (and three corresponding different datasets).

First, we study the evolution over the 2005-2011 period of the Gini index (of income) at the municipality level, computed on the basis of data on individual income taken from tax records, contrasting treated municipalities in Molise with those belonging to the neighboring regions of Apulia and Campania that did not experience any contraction in funding. This comparison is made at the spatial border separating the two EU aid regimes, an approach known as difference-in-discontinuity ("diff-in-disc" henceforth, Grembi et al., 2016) design. This method can yield very credible local estimates because it minimizes the risk that results are driven by unobserved heterogeneity: intuitively, we compare trends for treated and control units at the geographic border so minimizing the risk that some confounders at the municipality-year level might bias the estimates. This is the core of our analysis. Results related to the municipality-level analysis show that the 2007 shrinkage of the EU intervention caused a small but significant decrease in the Gini indicator, equal to 1.9% of its average (computed on the treated units before the treatment) and 17.9% of its standard deviation. Then, we show a number of results supporting the validity of our research design: before the contraction in EU funds, spatial units near to both sides of the border display very similar trends; moreover, treated and control municipalities are ex-ante very similar with regards to some observable characteristics, which capture the exposure to time-varying shocks concurrent to the loss of the Objective 1 status. We also document that our core result on the improvement of income distribution is robust to a wide set of sensitivity checks.

The next step is deepening our understanding of the decrease in the Gini index. We do that by using the same diff-in-disc methodology to provide two further pieces of evidence: (*i*) the total number of taxpayers remains stable, so that we can exclude that the fall in the Gini index is due to low-income earners moving to the no-tax area; (*ii*) there is a negative impact of the treatment on the share of top-income earners (those whose income is larger than 75k euros, roughly corresponding to the 96th percentile). Overall, our evidence suggests that when the program was fully in place it benefited the well-off residents of the targeted place more. The result on the negative effect of the policy change on the upper tail of the income distribution is confirmed by quantile regressions run on individual-level data drawn from the Survey on Household Income and Wealth (SHIW), a representative biennial survey of the Italian population conducted by the Bank of Italy. We also calibrate a well-known welfare function (Atkinson, 1970) to show that the effect of the policy change on citizens' welfare is ambiguous, given that the improvement in the income distribution and the fall in the average income per taxpayer are two opposing forces that tend to cancel each other out.

Our second bunch of evidence is motivated by the fact that the PBP under scrutiny targets European NUTS-2 areas (in Italy: regions). Hence, a straightforward complementary exercise looks at income inequality at the same (regional) level of aggregation. Namely, we compare the Gini indicator in Molise after the funds associated with the Objective 1 program lapsed with that which would have been observed had the program continued. The counterfactual pattern is estimated with the synthetic control method proposed by Abadie et al. (2010) for comparative case studies, with other Italian untreated Southern regions as potential donors. We can not retrieve the regional-level measure from the municipality-level ones because the Gini coefficient is not decomposable (Bourguignon, 1979). Hence, we compute it on the basis of the European Union Statistics on Income and Living Conditions (EU-SILC) survey, the most important source of information for comparative statistics on income distribution and social exclusion at the European level. It turns out that our result on the decrease in income inequality extends to the regional-level analysis: estimates from the synthetic control method indicate that, on average, after 2006 the Gini coefficient decreased by 7%-8% of its average value. The negative impact of the policy change on inequality is confirmed if adopt a difference-in-differences estimation strategy using data on the Gini Index from the Luxembourg Income Study (LIS), available for several European regions.

Finally, we provide some evidence on the transmission channels, showing that the housing market plays a role in explaining our results. In particular, we show that the house price drop, experienced as a consequence of the treatment, affected disproportionally individuals who were better off.

Thus far, very few papers have discussed the income-distribution impact of PBPs. Glaeser and Gottlieb (2008) suggest that PBPs are only imperfect ways to help disadvantaged households. More formally, Gaubert et al. (2021) show that improving the economic conditions of underprivileged people in targeted areas is a necessary condition for PBPs to be welfare-enhancing. On the empirical side, Reynolds and Rohlin (2015) study the US federal Empowerment Zone program and find that benefits did not accrue to the lower-income residents of the treated areas. If any, they document an increase in the share of residents at the extremes of income distribution in treated units compared to controls. Freedman (2013) reaches an opposite conclusion, suggesting that the Empowerment Zone program in Texas had a positive effect on lower income households. Picarelli (2016) finds that the export processing zones program in Nicaragua benefited the upper-end the most. We add to this literature by using highly credible identification strategies to bring new empirical evidence on the most important PBP in place within the European Union. Moreover, to the best of our knowledge, this is also the first paper focusing on a well-celebrated measure of income inequality, instead of offering indirect evidence on different percentiles of the income distribution.[1]

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[1] Another (unpublished) study (Lang et al., 2022), subsequent to ours, addresses a very similar research question and finds similar results for a sample of European regions. More in general, our paper is also related to the literature on counterfactual evaluation of the EU cohesion policy. Notable contributions on the effect of this policy on growth include, among others, Becker et at. (2010), Becker et at. (2013), Becker et al (2018), Pellegrini et al. (2012), Giua (2017), Barone et al. (2016); the main take-home message is that the policy has been effective in spurring growth with two relevant caveats: (i) such positive effect is heterogeneous across regions and crucially depends on the quality of local institutions, and (ii) such positive effect tends to fade aways when the policy ends. Other studies have focused on the political consequences of the EU Cohesion Policy, pointing to mixed results on the role of the policy in contrasting populism (Crescenzi et al., 2020; Albanese et al., 2022).

The paper is structured as follows. Section 2 illustrates the policy experiment. Sections 3 and 4 present the municipality-level and the regional-level analyses, respectively. Section 5 proposes an interpretation of the results, also based on the theoretical and empirical literature on the effects of PBPs. Section 6 concludes.

2. The policy experiment

The European cohesion policy represents the largest program of redistribution of resources across member states in the world. As stated in the EU Treaties, the aim of the policy is to promote harmonious development by pursuing the goal of economic, social, and territorial cohesion. In this setting, the Union takes actions intended to reduce disparities between and, interestingly for our exercise, *within* regions, through a wide range of support for businesses and activities in areas such as research, environment, transport, employment, social inclusion, education, and institutional capacity-building. The program is financed mainly via the so-called Structural Funds: they include the European Regional Development Fund (ERDF) and the European Social Fund (ESF).

The policy has been in operation, in its current form, starting from the reform of the Structural Funds in 1988. Since then, it has been organized through investment priorities (the so-called “Objectives”) and multi-year financing cycles. Financial resources have increased from cycle to cycle, also reflecting the Union’s enlargement. Currently, they absorb more than one-fourth of the EU budget. In all the cycles, and in particular, in those more relevant for our analysis

(2000-2006 and 2007-2013), Objective 1 (renamed Objective Convergence in the 2007-2013 cycle) has represented the core of the European PBPs: it aims at supporting the economic development of NUTS-2 regions whose per capita GDP is less than 75% of the EU average. At the European level, about three-fourths of Structural Funds in the periods 2000-2006 and 2007-2013 were devoted to Objective 1. The rest of the Structural Funds went mostly to Objective 2 (replaced by Objective Competitiveness in the 2007-2013 cycle), which aimed to revitalize all areas facing structural difficulties, whether industrial, rural, urban, or dependent on fisheries.

In Italy, the country we focus on, the bulk of the cohesion policy has mainly addressed the southern regions: Abruzzi, Apulia, Basilicata, Calabria, Campania, Molise, Sardinia, and Sicily (the so-called Mezzogiorno). In the first two cycles (1989-1993 and 1994-1999) all the Mezzogiorno regions were assigned to Objective 1.[2] Subsequently, the coverage under Objective 1 has become less homogenous, with regions having lost their status, also due to the enlargement of the European Union to countries with a lower GDP per capita.

The case of Molise is particularly interesting (and well-suited for our identification strategy; see Section 3). During the 2000-2006 cycle, Apulia, Basilicata, Calabria, Campania, Sardinia, and Sicily were fully included in Objective 1, while Molise, whose per capita GDP exceeded the 75% threshold, was assigned to Objective 1 only under the so-called “phasing-out” status (in the EU jargon), that prescribed the exclusion of Molise from Objective 1 at the end of 2006.[3] In particular, the

[2] Abruzzi exited the Objective 1 in 1996.
[3] https://ec.europa.eu/regional_policy/sources/graph/poster2014/sf_elig_1989_2020.pdf.

future exit of Molise was programmed in 1999 by taking into account the last available data on relative GDP per capita, referred to the period 1994-96. This means that the exit decision depends on the relative economic performance until 1996, more than ten years before the exit, but not on that referred to the following years. As a matter of fact, the growth of GDP per capita in Molise for the period 1986-1996 is higher with respect to control regions, while the corresponding figures in the subsequent decade are pretty similar (Figure 1): before the treatment, treated and control units are on a similar growth path.[4]

[figure 1 here]

With the loss of the Objective 1 status, Molise automatically became an Objective 2 region, so facing a large drop in European funding. Importantly for our analysis, almost all the other Objective 1 regions continued to receive Objective 1 support also in the 2007-2013 cycle. An exception is Sardinia, which also lost its Objective 1 status during the 2007-13 cycle; being an island, however, we can not study this region through our core empirical strategy of Section 3 so we simply exclude it from the sample. By exiting from Objective 1, Molise faced a large drop in EU financial support: according to the European Commission, the expenditure (in per capita terms) more than halved (from 137 to 66 euros at constant 2010 prices; see Figure 2), while the funds for the unaffected regions remained pretty similar or even increased, except for Basilicata.[5] Additional evidence reported in the

[4] Figure A1 in the Online Appendix shows the same comparison relative to each control regions.
[5] Data are taken from the Historic dataset on Structural Funds by Member state, which provides historic long-term regionalized (NUTS-2) annual EU expenditure data for different EU funds (https://ec.europa.eu/regional_policy/it/policy/evaluations/data-for-research/). The drop in Basilicata is due to the phasing-out status. We address below the robustness of our results from the exclusion of Basilicata in the region-level analysis. When discussing the existence of an actual

Online Appendix (Figures A2-A3) shows that the drop in EU funding after 2006 was relevant both for the ERDF (mainly targeted towards infrastructure and innovation) and ESF (targeted towards employment, education, and social inclusion). Finally, to show that the different intensity of the treatment is in place also at the geographical border separating Molise from Campania and Puglia, we use the OpenCoesione dataset (available only from the cycle 2007-2013 on) to run an RDD model like that used in Section 3.1 that exploits the geographical border separating Molise from other neighboring regions that continued to receive Objective 1 funds after 2006 (Apulia and Campania): we find a sizable difference in the 2007-2013 average expenditure from EU programs (measured in 2010 euros per capita) at the border, equals to -131.3 euros (s.d. 29.7).

[figure 2 here]

Needless to say, the causal effects we will estimate in both exercises have to be interpreted as the difference between being in Objective 1 versus receiving the less generous treatment implied by Objective 2.

3. Municipality-level analysis

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decline in funds, it is also important to check that other public resources from the national level ones were smooth around 2007. Hence, we collect data from the Territorial public accounts maintained by the National Agency for Territorial Cohesion (https://www.contipubbliciterritoriali.it/CPTDE/CPTDE_Home.html). It turns out that in Molise after 2006 funds from the national government exhibit a smooth path, similar to those observed in the other regions located in the Mezzogiorno (results are available upon request). This evidence reassures about the fact that the central government did not compensate the loss of the Objective 1 status with its own resources.

3.1. Identification strategy

We want to estimate the causal impact of the end of the Objective 1 program on interpersonal income inequality in treated municipalities relative to untreated ones. The treatment consists of losing the Objective 1 financial aid. To increase comparability between treated and control units, we focus on municipalities near the geographical border separating Molise from other neighboring regions that continued to receive Objective 1 funds after 2006 (Apulia and Campania). Our diff-in-disc approach compares the pre-post change in the Gini index in municipalities located in Molise near the border with the corresponding figure referred to municipalities located in Apulia or Campania, near the same border (Figure 3a). In our preferred specification, we limit the sample to municipalities whose distance from the border is not greater than the optimal bandwidth retrieved using the procedure suggested by Imbens and Kalyanaraman (2012).[6] The diff-in-disc approach has some advantages relative to both the standard diff-in-diff strategy and the regression discontinuity design (RDD). With respect to the former, the assumption of a parallel trend is more credible; at the same time, this method does not suffer from the compound treatment problem, which can plague the RDD approach on administrative borders, because idiosyncratic time-invariant omitted variables changing at the threshold are differentiated away.

[figure 3 here]

In our framework, the estimating equation is:

[6] The optimal bandwidth is computed on the base of the corresponding RDD model in which the dependent variable is the change in the average Gini after the policy change (see Table 3).

$$gini_{i,t} = \delta_0 + \delta_1 f(D_i) + S_i\big(\gamma_0 + \gamma_1 f(D_i)\big) +$$

$$T_t\big[\alpha_0 + \alpha_1 f(D_i) + S_i\big(\beta_0 + \beta_1 f(D_i)\big)\big] + \varphi_b + \varepsilon_{it} \quad (1)$$

where $gini_{i,t}$ is the Gini coefficient of income in municipality *i* in year *t* (*t* = 2005-2011). $f(D_i)$ is a linear polynomial in the distance D_i of municipalities *i* from the border; distance is measured in travel time (with positive values for municipalities located in Molise and negative otherwise). We chose to measure distance in minutes because of the substantial land roughness that makes physical distance a less precise measure.[7] We also allow the coefficients of the linear polynomial to vary on both sides of the cutoff. S_i is the treatment variable: a dummy for municipalities located in Molise; T_t is a dummy variable for the post period (all years from 2007 on); φ_b are 8 border fixed effects, which ensure that each border municipality is compared to the nearest one on the opposite side. Finally, ε_{it} is the usual error term. The coefficient of interest is β_0, whose estimate is the diff-in-disc causal effect of stopping the Objective 1 treatment on income inequality.

3.2. Data

Confidential data on the Gini index of individual income at the (aggregate) municipality-year level, based on tax records, are made available by the Italian Ministry of Economy and Finance for the years from 2005 to 2011. Income is taxable income (i.e., gross income minus deductions) and includes all sources of income, such as labor (including pensions), business, and capital. From the same

[7] The median altitude on municipalities in our baseline sample is 372 meters, with a huge variation between 42 meters (10th percentile) to 749 meters (90th percentile). In unreported evidence, available upon request, we find that our core results are confirmed when we measure distance in km.

source, we also know some additional variables: the average income, the total number of taxpayers, and the share of taxpayers whose income is larger than 75,000 euros per year (roughly corresponding to the 96th percentile), as a proxy of the weight of the right tail of the income distribution. Data on the distance are taken from the Italian National Statistical Office (Istat). Table 1 presents the descriptive statistics, distinguishing between treated and control municipalities and before and after the treatment. Municipality-level data (Panel A) suggest that both in the treated and in the control units the Gini index increased after the treatment by about the same amount. On the other hand, region-level data (Panels B1 and B2) suggests that in the post period, Molise shows a decrease in the Gini index, while control regions exhibit a flat pattern, suggesting that the end of the EU funding might have reduced inequality. In what follows, we provide rigorous econometric evidence for this intuition.

[table 1 here]

3.3. Results

Baseline results and robustness tests. Table 2 reports our main diff-in-disc results. The dependent variable is the Gini coefficient observed for a window of two years before and five years after the policy change (which occurs in 2007). In our preferred specification (Column 1), we use the optimal bandwidth of 68 minutes around the border that separates Molise from Apulia and Campania, obtained following Imbens and Kalyanaraman (2012). The sample includes 561 municipalities. Our findings suggest that the contraction of EU financing caused a small but highly significant decrease in local income inequality, equal to -0.007, equal to 1.9% of its average (computed on the treated units before the treatment) and 17.9% of its standard deviation. To further put in perspective our point estimate, mind that between 2007 (before the Lehman Brothers collapse) and 2013 (after the sovereign debt crisis) the Gini index for the Italian economy increased by 0.014 (Oecd, 2015). More recently, Carta and De Philippis (2021) estimate that Covid-19 would have risen the Italian Gini index by 0.04, had social insurance benefits not been available.

[table 2 here]

In the next columns, we show that our main result is robust even if we perturb several empirical choices. We start by narrowing by 1/3 the bandwidth, and obtain a slightly higher fall (-0.011, significant at 1%; Column 2). In Column 3, we drop all the observations closer (10 minutes) to the two sides of the border. This exercise checks to what extent our findings are driven by cross-boundary spillovers, which turn out not to play a relevant role. The estimated coefficient is slightly larger in Column 4, in which we instead give more weight to observations from cities where the share of daily commuting flows (taken from Census data) is lower. Column 5 provides a correction for tax evasion, which might plague our outcome since it is derived from fiscal archives. Marino and Zizza (2012), by comparing survey results with official tax records, derive a tax evasion rate by gender, age, geographical area, and job type (employee, self-employed, etc.). We map these rates into municipalities according to their average composition in terms of the same variables from 2001 and, then, weighted the observations using 1 – (*imputed tax evasion rate*) as weight. Again, this robustness check does not alter our main findings. In Column 6, we take into account another potential confounder: in the last part of 2002, a large earthquake hit a number of municipalities located in Molise and Puglia, and over the medium-long term reconstruction expenditure might shape income distribution. Then, we add a dummy variable for hit municipalities interacted with a time trend: results are unaffected, suggesting that, at most, the intensity of earthquake consequences is smooth at the border. In the last column, we control for municipality-year varying local tax rates (surcharge on income tax as well as real estate tax) that could impact labor supply and wealth allocation, and then, on local income and its distribution: results are unaffected.

Table 3 tests the robustness of our result if we change the econometric specification. For each municipality in the sample in Table 2, Column 1, we average the Gini index in the pre- (2005-2006) and in the post- (2007-2011) period and compute the pre-post difference, which is the dependent variable in a parametric RDD estimation (e.g. Lemieux and Mulligan, 2008). Column 1 shows that the estimated impact is, if any, slightly larger. Results do not change if we recur to a quadratic polynomial of distance (Column 2) or use a local linear estimator based on the bias correction approach introduced by Calonico et al. (2014; Column 3). Figure 4 presents a graphical representation of the RDD results.

[table 3 and figure 4 here]

Our last exercise, shown in Table 4, is a placebo test. We pretend that the treatment border is the one separating Campania and Apulia (two regions with no policy change), while Molise is excluded from the sample. Reassuringly, we can not detect any significant effect using either the diff-in-disc approach (Column 1), or the RDD estimator (Column 2).

[table 4 here]

Diagnostics on the identification strategy. The results shown above rest on two assumptions: (*i*) other covariates potentially correlated with post treatment trends are balanced around the cutoff, and (*ii*) treated and control units at the threshold are not on a different path before the exit of Molise from Objective 1. As to (*i*), we pinpoint two relevant potential confounders. First, after 2008 the deep financial crisis led to a freezing of international trade that, in turn, hit disproportionally municipalities whose local economies relied more on manufacturing industries (as a proxy of specialization in tradable industries). It follows that it is important to check that the share of manufacturing is balanced around the cutoff. Similarly, in 2011-2012 Italy faced an acute sovereign debt crisis that called for stricter fiscal discipline. Again, it is important to check that treated and control units near the threshold display the same dependence on the non-market (that is: public) service sector. In Table 5 we use a spatial regression discontinuity setting at the municipality level to test some balancing requisites. We focus on municipalities within the optimal bandwidth of 68 minutes to the cutoff (the same used in our preferred specification) and use a linear polynomial with different coefficients on both sides of the border. All variables are measured in 2001, before the shrinkage in financing. Reassuringly, Row 1 shows that the share of industrial value added is balanced around the cutoff. Analogously, all sectoral shares are well balanced too (Rows 2-4), suggesting that the impact of the economic crises has been similar for treated and control municipalities. In the next rows w,e show that balancing conditions hold also for other variables potentially correlated with future trends. Row 5 shows that the unemployment rate is ex-ante similar. We also document that the shares of graduates and foreign people, which might capture different ex-ante exposure to skilled-bias shocks, are very similar on the two sides (Rows 6-7). Row 8 looks at pre-treatment trends in the Gini indicator (assumption (*ii*) above) by using the 2005-06 change in the Gini coefficient as the dependent variable: we are unable to document a failure of the parallel trend assumption. The same goes for the other outcomes, all based on the same tax data, which we are going to analyze below (Rows 9-11).

[table 5 here]

Figure 5 further supports both the validity of the local parallel trend assumption and the jump down in the Gini indicator in the post treatment period. It reports coefficients stemming from an event study analysis: before 2007 the Gini coefficient in municipalities located in Molise near the cutoff is stable over time; from 2007 onward, the negative difference between treated and control municipalities widens and becomes significant.

[figure 5 here]

Interpretation. Where does the drop in the Gini index come from? Exiting the program might have moved some low-income earners from above to below the no tax area; for example, many jobs might have been available because of the EU financing, and these jobs disappeared with the shrinking of the funds. In such a case, our Gini coefficient based on fiscal data would signal a drop in inequality, due to the fact that some low-income taxpayers are not observed any longer. In Table 6, Column 1 of Panel A, we use our preferred specification to re-estimate equation (1) with the total number of taxpayers (normalized with population) as dependent variable: the estimated change in the Gini coefficient can hardly be attributed to a change in the underlying number of taxpayers. If any, we find a positive coefficient, which is however not significant at the usual levels.[8] In Column 2, Panel A, we estimate the impact of the contraction of funds on the share of taxpayers with a very high income (higher than 75k euros; 4% of the sample). Our results suggest that the treatment negatively affects the income of the wealthiest. The estimated effect is sizeable, around one-third and one-fifth of the average and of the standard deviation of the dependent variable, respectively. Both results shed some light on the mechanism: when Objective 1 funds end, the occupation rate does not change while the share of top income goes down, consistently with the idea that some happy few benefited relatively more from cohesion funds. The last column in Panel A shows that the average income per taxpayer goes down (-1.8%), consistently with the decrease in the share of top incomes. All these results are confirmed when we use the optimal bandwidth computed for each outcome separately, instead of using our baseline bandwidth (see Panel B). All in all, Table 6 shows that the drop in the Gini index comes from

[8] This result does not depend on a significant change in population, the variable we use to normalize the number of taxpayers: when using ln (population) as dependent variable we estimate a parameter equal to 0.013 (0.009).

the decrease in the share of high-income taxpayers, while their total number is unaffected.

[table 6 here]

Ideally, a deeper insight into the effect of the treatment on income distribution would require more granular tax data that, unfortunately, we do not have. Nevertheless, we resort to a different data source to shed some further light on the impact of exiting the Objective 1 program on different points of the income distribution. Namely, we use individual-level data drawn from the SHIW and leverage a sample of about 7,500 individuals living in Molise, Apulia, or Campania in the period 2002-2012 (six waves) to run quantile regressions aimed at testing the impact of the policy change on different percentiles of the income distribution. Results are reported in Table 7. In all columns, we control for personal characteristics (age and its square, gender, citizenship, civil status, number of family members, education, and employment status), as well as regional and wave fixed effects. We detect a negative impact for all percentiles, whose magnitude (in absolute terms) is increasing as we move to the upper percentile. As to percentiles from 10th to 75th the negative effect is not negligible, ranging from -1.9% to -5.6% but it lacks statistical significance. At the 90th percentile, the size is much larger (-13.4%) and the coefficient is highly significant. Overall, this exercise points to a negative effect on the upper tail of income distribution and is consistent with our previous results.

[table 7 here]

We conclude this subsection by discussing whether the policy change has improved overall welfare. In fact, in our core empirical exercises, we find that while the income distribution improves, at the same time the average income goes down. How do these two contrasting forces shape citizens' welfare? In the Appendix, we borrow from Atkinson (1970) to measure the change in a widely used welfare function that depends positively on income and negatively on inequality (see also Antràs et al, 2017, for an application). The relative weight of inequality in the welfare function is modeled by means of an aversion-to-inequality parameter ρ. We calibrate the welfare function using our core results on the drop in the Gini index (-0.007, see Table 2, Column 1) and in the average income per capita (-1.8%, see Table 6, Column 3). Our findings indicate that the effect on welfare is ambiguous and depends both on ρ and on the functional form of the income distribution (Lognormal or Pareto). In particular, to detect a positive welfare effect, assuming that income is lognormally (Pareto) distributed, we need that ρ to be greater than 1.1 (1.8). Existing estimates for the Italian economy suggest that in 2008 $\rho = 1.6$ (Maciej Kot and Paradowski, 2022) so our test is not conclusive.

4. Region-level analysis

In the last Section, we have shown that exiting the Objective 1 program has a negative and significant causal effect on the Gini index computed at the municipality level. We now refine our previous analysis by examining what happens at the *regional* income distribution, which is a complementary perspective grounded on the fact that EU funds target explicitly NUTS-2 regions (and not the municipalities).

4.1. Identification strategy and data

We adopt the synthetic control method proposed by Abadie et al. (2010) for comparative case studies. The donor pool includes the following other southern Italian regions for which the intervention was not interrupted: Apulia, Basilicata, Calabria, Campania, and Sicily (Figure 3b). The treatment year is 2007. As for the choice of the control variables, we include the sectorial composition of value added (industry and non-market services) to control for different exposures to the collapse of trade (occurred in 2009) and to the sovereign debt crisis (2011), the share of graduates capturing ex-ante exposure to other unobserved time-varying shocks, and the EU funds per capita; all these controls are measured in 2006. We also control for all lagged values of the Gini coefficients (2000, 2003, 2006). All these data are provided by Istat (except for the Gini coefficient in 2000, see below).

As stated in the Introduction, regional-level Gini coefficients can not be safely obtained by aggregating municipality-level data because the indicator is not decomposable (Bourguignon, 1979). Hence, regional data on the Gini indicator (computed for households), available from Istat, are based on the EU-SILC survey and start from 2003 to 2017. Within the EU, EU-SILC is the most respected survey that collects comparable cross-sectional and longitudinal multidimensional microdata on income, poverty, social exclusion, and living conditions. As such, it is the reference source for comparative statistics on income distribution at the European level. To our aim, the period available for the pre-treatment estimation is rather short (4 years: 2003-2006). To circumvent such an inconvenience, we leverage the availability of data on the Gini index (computed for households) at the NUTS-2 level from the LIS (see Savoia, 2019). Data at our disposal include an unbalanced panel sample on European regions that continuously benefit from the Objective 1 program.[9] In order to make the pre-treatment estimation period longer, we regress the Gini index from Istat (available from 2003 to 2017) on the Gini index from LIS (available from 1986 to 2014 with jumps) on the Italian sample. The two samples overlap for Molise, Campania, Apulia, Basilicata, Calabria, and Sicily for years 2004, 2008, 2010, and 2014. We then use estimated coefficients to impute the Gini index from Istat in 2000. Finally, we impute missing values for 2001 and 2002 by linear interpolation.

4.2. Results

Figure 6 compares the dynamics of the Gini index of Molise for the period from 2000 to 2017 with that of its synthetic counterpart. In Panel A, data for 2000-2002 are imputed as explained above, while in Panel B there is no imputation. The main selected donors are Calabria and Basilicata in both panels. The usual balancing properties for each synthetic control estimation are reported in Table 8. The synthetic control traces rather very well the Gini index of our region of interest in the pre-treatment period, especially in the no-imputation case. From 2007 onward, the two lines diverge in both Panels, so signaling that exiting the Objective 1 program is conducive to a reduction in the Gini index at the regional

[9] See Table A1 in the Online Appendix.

level. In Panel A, in the post-treatment period, the difference between the Gini index in Molise and its synthetic counterpart is -0.024, while the same difference was much smaller before the treatment (-0.003); the same figures for Panel B are -0.021 and -0.004, respectively. The magnitude of the effect we estimate at the regional level is non-negligible: in Panel A, it equals 8.3% of the average Gini coefficient in Molise before the treatment (7.4% in Panel B). While this figure is larger than that estimated in the municipality-level analysis (1.9%), the two estimates are not strictly comparable because of (i) the non-decomposability of the Gini coefficient, and (ii) Gini coefficients for municipalities are based on individual income while Gini coefficients at the regional level are based on households' income.

[table 8 and figure 6 here]

The causal effect of the end of the Objective 1 program on the Gini indicator at the regional level is robust to the usual checks by interactively excluding one donor at a time from the donor pool (leave-on-out test; see Figure 7), except for Basilicata when the test is not conclusive because of the lack of a satisfying pre/treatment fit.

[figure 7 here]

To further corroborate the credibility of our result, we also conduct a placebo study by virtually reassigning the treatment to regions unaffected by it (see Abadie et al., 2010). In Figure 8, Panel A1, the decrease in the Gini index is the largest relative to those referred to in the placebo exercises. Another standard way to assess the validity of our placebo test is to look at the ratio of post/pre-treatment root mean squared prediction error (RMSPE), i.e. the average of the squared difference between the Gini index of a region and its synthetic counterpart before and after the treatment. A sizeable post-treatment RMSPE is not indicative per se of a significant effect of the intervention if the synthetic control does not closely reproduce the outcome of interest before the intervention. In all exercises, Molise stands out as the region with the highest RMSPE ratio (Panel A2). The same conclusions can be drawn even in the no-imputation case (Panels B1 and B2).

[figure 8 here]

Our last piece of evidence on the effect of the EU cohesion policy on income inequality at the regional level addresses the potential drawback of considering a relatively low number of regions in the donor pool. Our choice of focusing on other Italian regions as potential donors is due to the fact that regions belonging to the same country are most similar to Molise in terms of unobserved characteristics. Nevertheless, we enlarge the donor pool by leveraging the unbalanced panel of European regions for which the Gini index based on LIS data is available (while analogous data on the EU-Silc survey are not). These are regions that benefit from the Objective 1 regime either before or after the policy change in Molise. Unfortunately, the panel is very unbalanced so we can not replicate the synthetic control analysis and resort to a difference-in-differences approach. That is, we regress the Gini index on a dummy equal to one for Molise from 2007 onwards and various combinations of fixed effects. Results are reported in Table 9, Panel

A. In Column 1 there are no additional controls, and we detect a significant and negative impact. In Columns 2-5 we progressively saturate the specification with region- and year-fixed effects, as well as with country-specific trends. In our preferred specification (Column 5), we estimate a drop equal to 11.3% of the average Gini in the pre-treatment period, not far from of what we find in the synthetic control analysis (about 7%-8%, see above). The analysis with the sample of European control units gives us the degrees of freedom we need not to include Basilicata among control. We do that in Panel B and the results are nicely confirmed (the estimated effect is 8.6% of the average Gini before 2007).

[table 9 here]

5. Discussion

Transmission channels. We have shown that, using Italian data, exiting the Objective 1 program has a negative causal effect on income inequality, as measured by the Gini coefficient. Where does this result come from? We propose three speculative explanations, which are not exhaustive or mutually exclusive. They follow the discussion outlined in the Introduction.

Standard theoretical models of PBPs, routed in a spatial equilibrium framework, predict that local landowners are likely to benefit from spatially targeted programs (Kline and Moretti, 2014b). Moreover, the increase in real estate values should be higher where the elasticity of supply of houses is lower, and the latter is a feature widely documented in the Italian case (Accetturo et al., 2021; Ciani et al., 2019). To check whether our data support this prediction, we use municipality-year-level data on house prices (measured in euros per square meter), drawn from the Italian Revenue Agency's real estate archive, and estimate the diff-in-disc specification of equation (1) using log house price as dependent variable (Table 10).[10] In Column 1 we start by using the baseline bandwidth for sake of comparison with our core results in Table 2. We find a sizable drop in (log) housing values at the border, equal to -15.7%.[11] In Column 2 we use the optimal bandwidth: the decrease is always highly significant and equals -11.5%. In Columns 3-4, we split the sample according to the median of a proxy of rigidity of housing supply. Rigid municipalities are those in which the rate of owner-occupied housing is higher than the median.[12] As expected, the effect is fully driven by municipalities with supply constraints, which records a very large drop, while no effect is in place in municipalities with more elastic supply.[13] Does this piece of evidence speak to our previous results on the negative effect of the policy change on higher percentiles of the income distribution (see Tables 6 and 7)? The answer is affirmative. In fact, SHIW data shows that before 2007 the value

[10] Data are available at https://wwwt.agenziaentrate.gov.it/servizi/Consultazione/ricerca.htm.

[11] Because of the lower reliability of house prices for small-sized municipalities, we weight regression by city's population. In absence of this correction, the coefficient estimated would be equal to -0.336 (s.d. 0.030).

[12] The rate of owner-occupied housing is a reasonable proxy for rigidity, at least in the short run, because it captures the intensive margin along which the supply can accommodate positive demand shocks. Results (available upon request) are qualitatively similar when we proxy supply rigidity with land slope, which captures the fact that, on the extensive margin, steeper terrains make more difficult residential development.

[13] The negative effect on the housing price is not at odds with the no effect on population shown above. In fact, Ciani et al (2019) show that, in Italy, rigid local wages, low mobility of population and limited housing supply elasticity make local prices the variable that mostly adjust to absorb local shocks.

of housing wealth was increasing in income. This implies that the house price drop affects disproportionally individuals who were better off.[14]

[table 10 here]

Another potential channel is related to the effectiveness of the program when in place and involves, as in our case, financial incentives to firms to increase employment. To the extent that the program turns out not to be effective (i.e., firms in the treated area do not demand more labor than they would have done without the incentive), firm owners will receive a financial premium for hiring the same number of people they would have hired anyway. In the Italian case, the literature on PBPs consistently shows that these kinds of policies are characterized by an extremely low degree of effectiveness, regardless of the specific characteristics of the intervention (see Accetturo et al., 2020, for a discussion). According to the (lack of) effectiveness channel, the contraction of European funding may have had the effect of reducing unproductive public transfers to local entrepreneurs, who presumably are in the top right tail of income distribution.

A third line of reasoning suggests that the program can generate rent extraction mechanisms. Interestingly, Accetturo et al. (2014) show that the receipt of EU structural funds causes a deterioration of the endowments of trust and cooperation in the subsidized regions. Barone and Narciso (2015) document that

[14] Using data from the SHIW (waves 2002, 2004, 2006) on individuals living in Molise we find that those in the 0-10th percentile of the income distribution the average housing asset value is 35,000 euros; in the 10th-25th percentile the average is 67,000; in the 25th-50th percentile the average is 108,000; in the 50th-75th percentile the average is 177,000; in the 75th-90th percentile the average is 250,000; in the 90th-100th percentile the average is 200,000.

mafia-ridden municipalities receive a disproportionally higher amount of cohesion funds. De Angelis et al. (2020) find that the disbursement of EU funds in Italy is systematically associated with an increase in the number of white-collar crimes in the recipient municipality. Ultimately, it is possible that the improvement in the distribution of income that follows the contraction of European funds has to be put in relation to the decrease in the rent-seeking generated by the European cohesion policy.

External validity. We now discuss the external validity of our results. For example, we know that the impact of EU funds is shaped by the quality of institutions (Becker et al., 2013). It would not be surprising if also the impact on income distribution depended on the same variable: for example, poor institutions may favor rent extraction by an elite thus increasing inequality. In this respect, the sample we focus on displays lower human capital and a lower quality of government compared to other treated European regions: Molise is at the 23rd and 20th percentile, respectively, of human capital (measured as the share of the workforce with at least upper-secondary education) and quality of institutions (proxied by the QoG index developed at the University of Gothenburg), when compared to Objective 1 regions in the 2007-2013 programming period. Consequently, our results should more easily generalize to policies addressing low-quality institutions place.

Timing of the treatment. A final remark regards the timing of the treatment. In the 2000-2006 period, Molise was a convergence region but, concerning the 1994-1999 programming period, it had joined the “phasing out” regime, which probably implied a reduction in EU funds. It follows that we can not exclude the fact that in 2000 Molise received previous treatment in terms of fund withdrawal. However, this is not a concern for our estimation strategy. In fact, the event study plot in Figure 5 shows that two-three years after the treatment the effect is there and is rather stable. This means that if the 2000 shock had shaped the Gini index, in 2002-2003 this effect would have been stabilized, and, at the same time, our estimation sample in the municipality-level analysis starts in 2005, well after the unfolding of the effect of the potential previous shock. Not surprisingly, balancing properties reported in Table 5 show that, in 2005-2006, the Gini index in treated and control municipalities share the same path (if any, we estimate a positive but not significant coefficient). As to the region-level analysis, the argument is conceptually similar and, more importantly, if Gini in Molise before 2007 was shaped by the 2000 shock, then we should fail to find a satisfying synthetic control and this is not the case.

The exact identification of the treatment year is also made difficult by the fact that the financial endowments received from the EU must be spent within two years from the assignment (so-called "*N*+2 rule"). This implies that, in principle, the actual treatment might have started in 2008 or in 2009 (instead of 2007). We think that this is unlikely to be the case: for example, the event study in Figure 5 shows that some drop in the Gini index is already in place in 2007. In any case, we can not rule out alternative (and subsequent) treatment dates for sure. Reassuringly, in such a case the effect we estimate in the municipality-level analysis would be a lower bound of the true effect.

6. Conclusions

Our analysis answers the crucial question of the impact of spatially-targeted programs on income distribution in recipient areas. Using different samples and econometric techniques, we show reduced form evidence suggesting that the withdrawal of the EU Cohesion policy lowers the Gini index (of income) in previously treated areas relative to proper control areas. From a policy perspective, this new piece of evidence should be taken into account when dealing with income inequality between regions *vs* between individuals within regions. We point to three transmission channels: the housing market, the poor effectiveness of the program, rent extraction by local elites.

However, lower income inequality comes hand in hand with the decline in average income. We show that a welfare analysis, aimed at managing the trade-off between income and its distribution, is not conclusive. Moreover, the retraction of funds did not result in any benefit to those at the lower end of the income distribution.

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Tables and figures

Table 1. Descriptive statistics

Area	Full sample			Treated			Controls		
Periods	All	Before	After	All	Before	After	All	Before	After
Panel A: Municipality-level analysis									
Gini index on income	0.397	0.389	0.400	0.386	0.378	0.390	0.400	0.392	0.403
	(0.030)	(0.031)	(0.029)	(0.037)	(0.039)	(0.036)	(0.026)	(0.026)	(0.025)
Share of taxp. > 75k euros	0.041	0.037	0.042	0.025	0.024	0.026	0.046	0.041	0.047
	(0.038)	(0.038)	(0.038)	(0.041)	(0.040)	(0.042)	(0.036)	(0.036)	(0.036)
# taxpayers over 100 inhab.	0.638	0.640	0.638	0.763	0.775	0.758	0.599	0.597	0.599
	(0.114)	(0.121)	(0.111)	(0.084)	(0.094)	(0.080)	(0.091)	(0.094)	(0.089)
Avg. inc. (000 2010 euros)	13.804	13.698	13.846	12.245	12.150	12.283	14.303	14.194	14.346
	(2.659)	(2.725)	(2.632)	(2.435)	(2.520)	(2.401)	(2.533)	(2.600)	(2.505)
Observations	3,927	1,122	2,805	952	272	680	2,975	850	2,125
Panel B1: Region-level analysis (Italy)									
Gini index (EU-SILC – Istat)	0.301	0.303	0.300	0.279	0.284	0.277	0.305	0.307	0.304
	(0.023)	(0.025)	(0.022)	(0.014)	(0.008)	(0.016)	(0.022)	(0.026)	(0.020)
Observations	90	24	66	15	4	11	75	20	55
Imputed Gini index (EU-SILC/LIS)	0.303	0.307	0.300	0.282	0.291	0.277	0.307	0.310	0.304
	(0.025)	(0.029)	(0.022)	(0.016)	(0.012)	(0.016)	(0.025)	(0.031)	(0.020)
Observations	108	42	66	18	7	11	90	35	55
Panel B2: Region level analysis (Europe)									
Gini index (LIS)	0.305	0.307	0.303	0.309	0.335	0.292	0.304	0.305	0.304
	(0.037)	(0.045)	(0.032)	(0.030)	(0.032)	(0.013)	(0.037)	(0.046)	(0.032)
Observations	128	53	75	11	8	3	117	45	72

Notes. Average of outcomes by sample (standard deviation in parentheses). In Panel A, the full sample is made of 561 municipalities observed in the period 2005-2011. In Panel B1, the sample is made of 6 regions (Apulia, Basilicata, Calabria, Campania, Molise, and Sicily) observed in the 2000-2017 period (2003-2017 for the EU-SILC sample). In Panel B2 the sample is an unbalanced panel of 21 EU regions for the period 1986-2014 (see Table A1 in the Online Appendix for details).

Table 2. Municipality-level analysis (DiD estimates)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Ex. the Ob. 1 pr.	-0.007*** (0.003)	-0.011*** (0.003)	-0.008** (0.003)	-0.013*** (0.003)	-0.007*** (0.003)	-0.008*** (0.003)	-0.007** (0.003)
Bandwidth	67.6 min	45.1 min	67.6 min	67.6 min	67.6 min	67.6 min	67.6 min
Border Cities (10 min)	Y	Y	N	Y	Y	Y	Y
Mobility correction	N	N	N	Y	N	N	N
Tax evasion correction	N	N	N	N	Y	N	N
Earthquake correction	N	N	N	N	N	Y	N
Local taxes correction	N	N	N	N	N	N	Y
Observations	3,927	3,087	3,696	3,927	3,927	3,927	3,927

Notes. The estimates are obtained using a difference-in-discontinuity design. The dependent variable is the Gini Index of income. The period of observation is 2005-2011. Standard errors clustered at city level are in parentheses. All specifications include a linear polynomial of distance (separate regressions on both sides of the border) and 8 border FE. *** p<0.01, ** p<0.05, * p<0.1.

Table 3. Municipality-level analysis (RDD estimates)

	(1)	(2)	(3)
Molise	-0.011***	-0.010**	-0.011**
	(0.003)	(0.005)	(0.005)
Bandwidth	67.6 min	67.6 min	67.6 min
Forcing var	Parametric (Linear)	Parametric (Quadratic)	Non-Parametric (Local linear)
Observations	561	561	561

Notes. The estimates are obtained using an RDD design. The dependent variable is the pre-post change in the Gini Index of income; the regressor of interest is a dummy equal to one (zero) for municipalities belonging to Molise (Apulia and Campania). Column 1 (2) includes a linear (quadratic) polynomial of distance (separate regressions on both sides of the border); Column (3) uses a local linear regression with the bias correction approach by Calonico et al. (2014). All specifications include 8 border FE. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Municipality-level analysis: Placebo test

	(1)	(2)
Fake exit (=Apulia)	0.006	0.005
	(0.004)	(0.005)
Estimator	DiD	RDD
Bandwidth	64.5 min	64.5 min
Observations	2,296	328

Notes. Column 1: The dependent variable is the Gini Index of income. The estimates are obtained using a difference-in-discontinuity design; standard errors clustered at the city level are in parentheses. Column 2: the dependent variable is the pre-post change in Gini Index. The estimates are obtained using a regression discontinuity design; robust standard errors are in parentheses. The period of observation is 2005-2011. All specifications include a linear polynomial of distance (separate regressions on both sides of the border) and 8 border FE. *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Municipality-level analysis: Balancing checks

	coeff.	s.e.	bw	obs
Share industry	0.013	(0.040)	67,6 min	561
Share constr.	-0.007	(0.025)	67,6 min	561
Share mkt services	0.003	(0.031)	67,6 min	561
Share non-mkt services	-0.004	(0.023)	67,6 min	561
Unemployment rate	-0.123	(1.256)	67,6 min	561
Human capital	2.090	(2.091)	67,6 min	561
Share of foreign people	0.004	(0.004)	67,6 min	561
ΔGini (before treat.)	0.002	(0.003)	67,6 min	561
ΔShare of taxp. > 75k euros (before treat.)	0.002	(0.005)	67,6 min	561
Δ# taxpayers over pop. (before treat.)	0.000	(0.005)	67,6 min	561
Δ Ln (real) income per taxp. (before treat.)	0.004	(0.006)	67,6 min	561

Notes. The estimates are obtained using an RDD design. Molise is a dummy equal to one (zero) for municipalities belonging to Molise (Apulia and Campania). All specifications include a linear polynomial of distance (separate regressions on both sides of the border) and 8 border FE. Robust standard errors. *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Municipality-level analysis: Mechanism

	# taxpayers over pop.	Share of taxpayers > 75k	Ln (real) income per taxpayer
		Panel A: Baseline bandwidth	
Ex. the Ob. 1 pr.	0.011	-0.008**	-0.018**
	(0.007)	(0.004)	(0.009)
Bandwidth	67.6 min	67.6 min	67.6 min
Observations	3,927	3,927	3,927
		Panel B: Optimal bandwidth	
Ex. the Ob. 1 pr.	0.012	-0.007*	-0.023**
	(0.010)	(0.004)	(0.009)
Bandwidth	35.8 min	73.0 min	54.3 min
Observations	1,666	4,088	3,374

Notes. The estimates are obtained using a difference-in-discontinuity design. The regressor of interest is exiting the Objective 1 program. The period of observation is 2005-2011. All specifications include a linear polynomial of distance (separate regressions on both sides of the border) and 8 border FE. Standard errors clustered at city level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Individual-level analysis: SHIW

	P10	P25	P50	P75	P90
Ex. the Ob. 1 pr.	-0.019	-0.033	-0.034	-0.056	-0.134***
	(0.058)	(0.047)	(0.037)	(0.039)	(0.052)
Observations	7,398	7,398	7,398	7,398	7,398

Notes. The estimates are obtained using quantile regressions. The dependent variable is ln (real) income. The period of observation is 2002-2012. All specifications include age and its square, gender, citizenship, civil status, number of family members, education, and employment status. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Predictors in the pre-treatment period

Panel A: Imputation			
	Molise	Synthetic Molise	All potential donors
Gini index 2000	0.312	0.299	0.317
Gini index 2003	0.280	0.289	0.309
Gini index 2006	0.275	0.278	0.297
Share of industry value added in 2006	0.140	0.100	0.113
Share of non-mkt services value added in 2006	0.213	0.214	0.211
Share of graduates in 2006	0.069	0.069	0.067
Average funds per capita in 2000-2006 (euro)	137.440	198.607	155.614
Panel B: No Imputation			
	Molise	Synthetic Molise	All potential donors
Gini index 2000	.	.	.
Gini index 2003	0.280	0.281	0.309
Gini index 2006	0.275	0.278	0.297
Share of industry value added in 2006	0.140	0.116	0.113
Share of non-mkt services value added in 2006	0.213	0.209	0.211
Share of graduates in 2006	0.069	0.068	0.067
Average funds per capita in 2000-2006 (euro)	136.887	220.126	155.614

Notes. The third Column reports the average values for Apulia, Basilicata, Calabria, Campania, and Sicily.

Table 9: Region level analysis using European regions

	(1)	(2)	(3)	(4)	(5)
			Panel A: Full sample		
Ex. the Ob. 1 pr.	-0.029***	-0.034***	-0.033***	-0.034***	-0.033***
	(0.007)	(0.006)	(0.008)	(0.008)	(0.010)
Region FE	N	Y	N	Y	Y
Year FE	N	N	Y	Y	Y
Country-trend	N	N	N	N	Y
Observations	128	128	128	128	128
			Panel B: Excluding Basilicata		
Ex. the Ob. 1 pr.	-0.025***	-0.031***	-0.029***	-0.029***	-0.025***
	(0.006)	(0.006)	(0.007)	(0.007)	(0.009)
Region FE	N	Y	N	Y	Y
Year FE	N	N	Y	Y	Y
Country-trend	N	N	N	N	Y
Observations	117	117	117	117	117

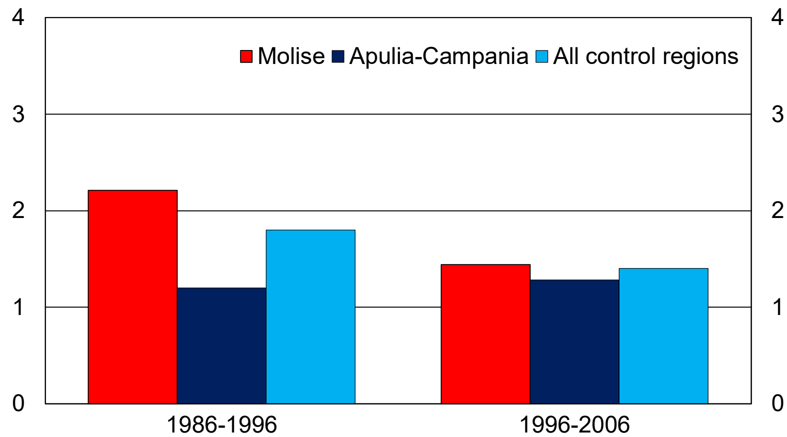
Notes. The dependent variable is the Gini Index of income. The estimates are obtained using a difference-in-differences design. The list of regions included in the sample is reported in Table A1 in the Online Appendix. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 10. Municipality-level analysis: House prices

	Full sample	Full sample	Rigid supply sample	Elastic supply sample
Ex. the Ob. 1 pr.	-0.157***	-0.115**	-0.268***	-0.009
	(0.056)	(0.052)	(0.049)	(0.066)
Bandwidth	Baseline (67.6 min)	Optimal (86.3 min)	Optimal (86.3 min)	Optimal (86.3 min)
Observations	3,927	4,396	2,191	2,205

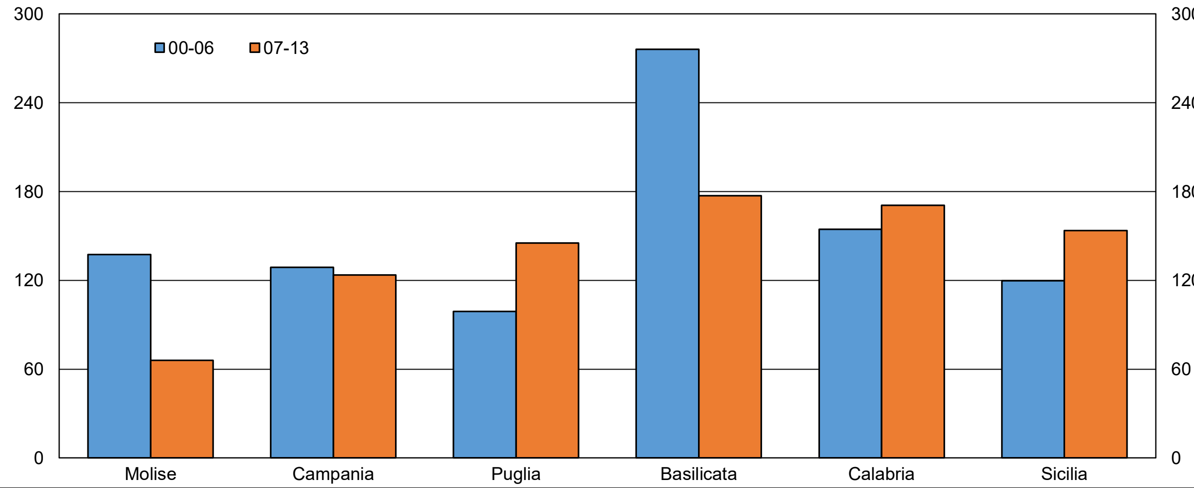
Notes. The estimates are obtained using a difference-in-discontinuity design. The dependent variable is ln house prices. The period of observation is 2005-2011. Elastic (rigid) supply sample includes municipalities with a rate of owner-occupied housing lower (higher) than the median. Standard errors clustered at city level are in parentheses. Regressions are weighted by population. All specifications include a linear polynomial of distance (separate regressions on both sides of the border) and 8 border FE. *** p<0.01, ** p<0.05, * p<0.1.

Figure 1. GDP per capita growth rate



Notes. The panel shows the growth rate of GDP per capita (euros at constant 2010 prices) (source: own elaborations on Ardeco data).

Figure 2. Spending from EU cohesion policy



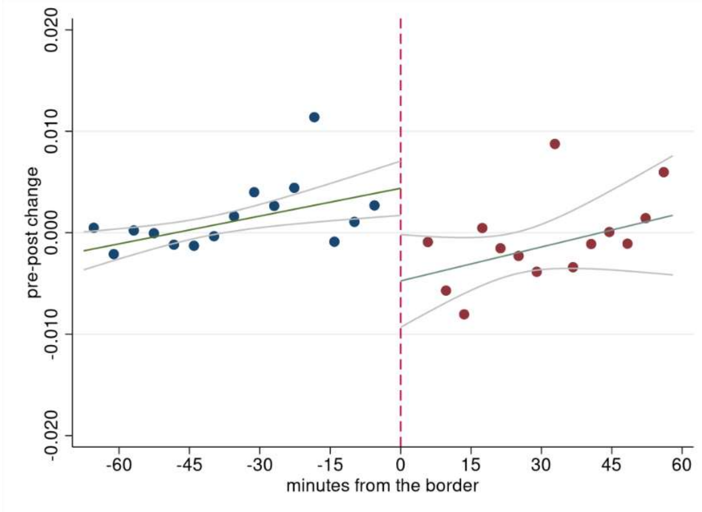
Notes. The figure shows the expenditure (euros per capita at constant 2010 prices) from EU cohesion policy (source: own elaborations on EU Commission data).

Figure 3. Policy discontinuity

(a) Sample "Municipalities"	(b) Sample "Regions"
	

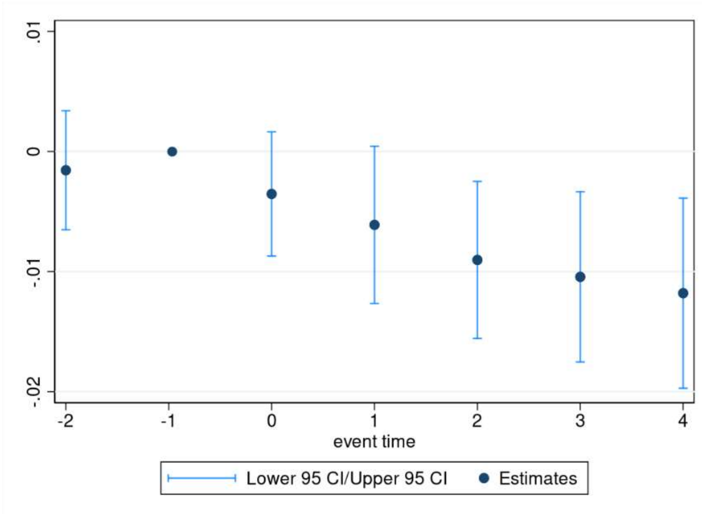
Notes. Panel (a) shows the area including the sample of municipalities used in Section 3: the black line indicates the border separating Molise from Apulia and Campania; the area in blue (yellow) covers the treated (control) municipalities located at a distance less than 67.6 minutes from the border. Panel (b) shows the sample of regions used in Section 4.

Figure 4. Municipality level analysis: RDD estimates



Notes. The dependent variable is the pre-post change in the Gini Index of income. The regressor of interest is a dummy equal to one (zero) for municipalities belonging to Molise (Apulia and Campania). RDD estimates computed with the Imbens-Kalyanaraman (2012) optimal bandwidth. The specification includes a linear polynomial of distance (separate regressions on both sides of the border) and 8 border FE. The period of observation is 2005-2011.

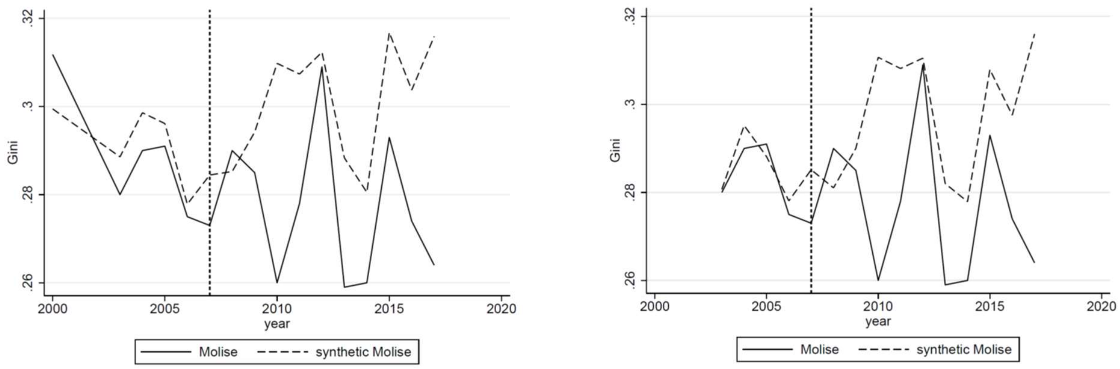
Figure 5. Municipality level analysis: Event study



Notes. The dependent variable is the Gini Index of income. The period of observation is 2005-2011. The bandwidth is computed using the Imbens-Kalyanaraman (2012) optimal rule. The specification includes a linear polynomial of distance (separate regressions on both sides of the border) and 8 border FE.

Figure 6. Synthetic control results

Panel A: Imputation | Panel B: No imputation

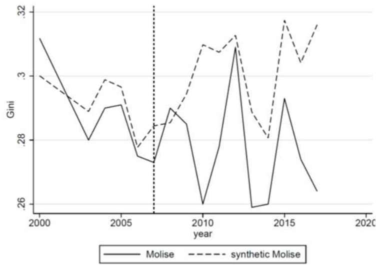


Notes. Panel A: RMSPE = 0.0072; donors: Apulia (0.005), Basilicata (0.365), Calabria (0.630). Panel B: RMSPE = 0.0034; donors: Basilicata (0.540), Calabria (0.460).

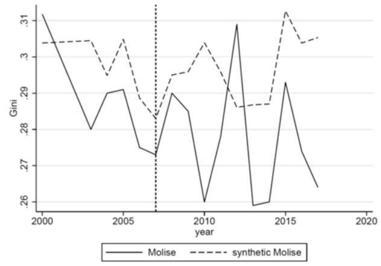
Figure 7. Leave-one-out tests

Panel A: Imputation

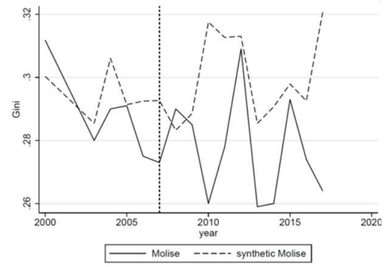
Panel A1: Excluding Apulia



Panel A2: Excluding Basilicata

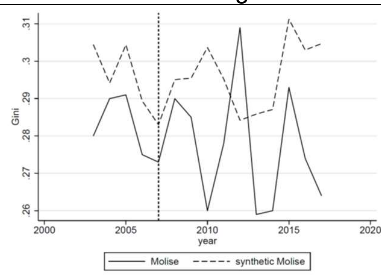


Panel A3: Excluding Calabria

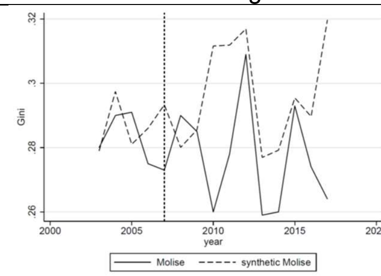


Panel B: No imputation

Panel B1: Excluding Basilicata



Panel B2: Excluding Calabria

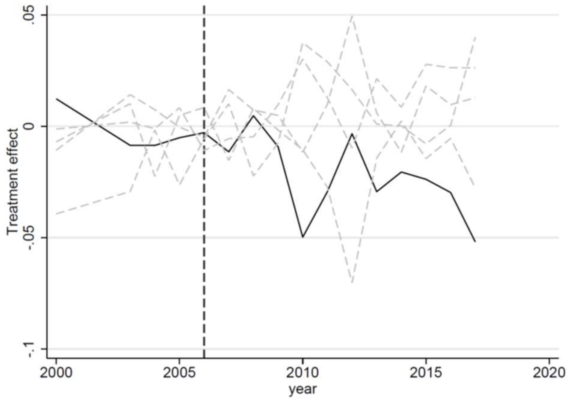


Notes. Panel A1: RMSPE = 0.0072; donors: Basilicata (0.357), Calabria (0.643). Panel A2: RMSPE = 0.0134; donors: Puglia (0.507), Calabria (0.493). Panel A3: RMSPE = 0.0104; donors: Campania (0.005) Basilicata (0.670), Sicily (0.325). Panel B1: RMSPE = 0.0158; donors: Apulia (0.538), Calabria (0.462). Panel B2: RMSPE = 0.0083; donors: Campania (0.234), Basilicata (0.766).

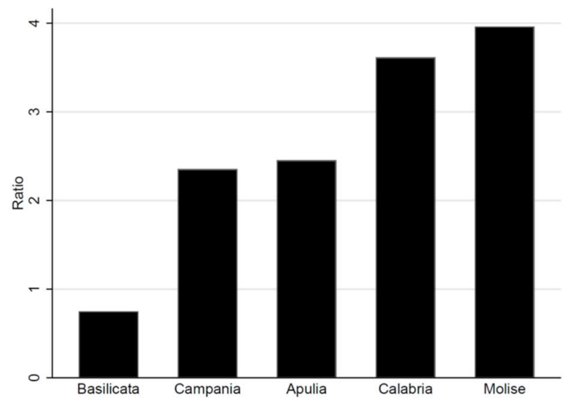
Figure 8. Placebos on the treated region

Panel A: Imputation

Panel A1: Molise vs placebos

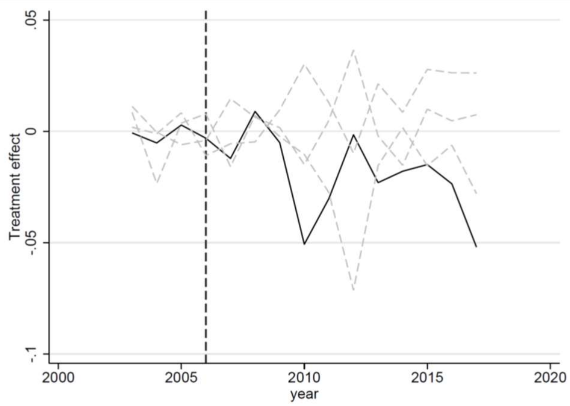


Panel A2: Ratio of post RMSPE vs pre RMSPE

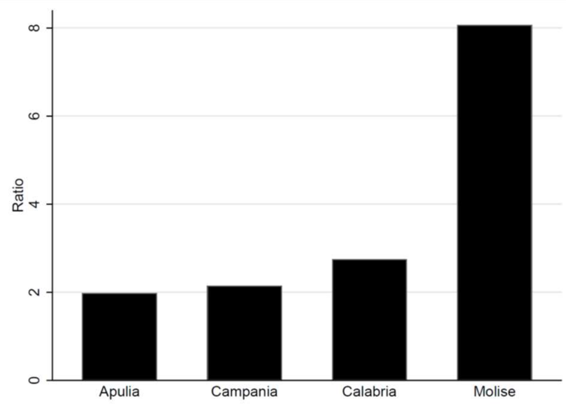


Panel B: No imputation

Panel B1: Molise vs placebos



Panel B2: Ratio of post RMSPE vs pre RMSPE



Notes. In panel A, Sicily is excluded because of the missed convergence of the optimization algorithm. In panel B, Basilicata and Sicily are excluded because of the missed convergence of the optimization algorithms.

The Impact of Place-based Policies on Interpersonal Income Inequality
Online Appendix

Welfare analysis

Following Atkinson (1970), we consider the following social welfare function W:

$$W = \Delta \times R \tag{A1}$$

where R is the aggregate income and Δ is the welfarist inequality correction. In particular:

$$\Delta = \frac{[E(r)^{1-\rho}]^{\frac{1}{1-\rho}}}{E(r)}$$

where r is individual-level income, and $\rho \geq 0$ is the degree of inequality aversion. If $\rho = 0$ (no inequality aversion), then $\Delta = 1$ and the social welfare coincides with aggregate income; if $\rho > 0$, then $\Delta < 1$ so that inequality penalizes welfare. Furthermore, Δ tends to be lower the higher the level of inequality in the distribution of income. Under the assumption that the distribution of income is lognormal or Pareto, Δ can be related to the Gini coefficient as follows.

Lognormal distribution case. Assume that market incomes are distributed lognormally with a mean parameter μ and a variance parameter σ^2. Then the welfarist corrections is equal to:

$$\Delta = exp\left\{-\rho\frac{\sigma^2}{2}\right\}.$$

The Gini coefficient associated with a lognormal distribution is given by $G = 2\Phi\left(\frac{\sigma}{\sqrt{2}}\right) - 1$; solving for σ, one obtains $\sigma = \Phi^{-1}\left(\frac{G+1}{2}\right)\sqrt{2}$ so that the welfarist corrections, expressed as a function of the Gini index is equal to:

$$\Delta = exp\left\{-\rho\left[\Phi^{-1}\left(\frac{G+1}{2}\right)\right]^2\right\}. \tag{A2}$$

Pareto distribution case. Assume that market incomes are distributed according to a Pareto distribution with shape parameter α. Then the welfarist correction is equal to:

$$\Delta = \frac{\alpha - 1}{\alpha}\left[\frac{\alpha}{\alpha - (1-\rho)}\right]^{\frac{1}{1-\rho}}.$$

The Gini coefficient associated with a Pareto distribution is given by $G = \frac{1}{2\alpha-1}$; solving for α, one obtains $\alpha = \frac{G+1}{2G}$ so that the welfarist corrections, expressed as a function of the Gini index is equal to:

$$\Delta = \frac{\alpha-1}{\alpha}\left[\frac{\alpha}{\alpha-(1-\rho)}\right]^{\frac{1}{1-\rho}} = \frac{1-G}{1+G}\left[\frac{1+G}{1+G-2G(1-\rho)}\right]^{\frac{1}{1-\rho}}. \qquad \text{(A3)}$$

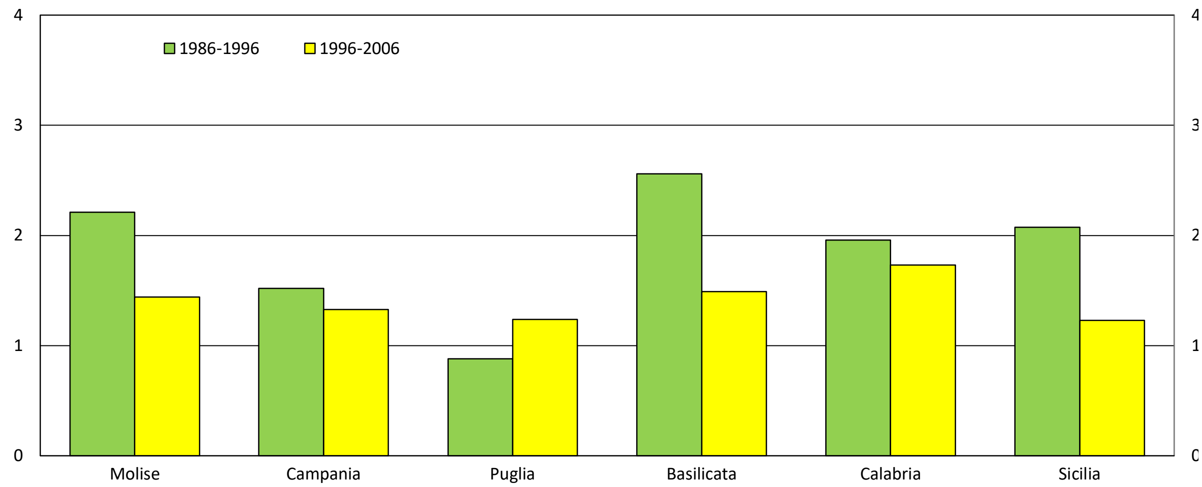
In order to compute W before and after the policy change, we use equations (A1) and (A2) for the lognormal case or (A1) and (A3) for the Pareto case. The Gini index before the treatment is set equal to 0.378 (see Table 1, Column 5), while after the treatment it is set to 0.371 (= 0.378 – 0.007, that is the effect estimated in Table 2, Column 1). This way, we can compute Δ before and after the treatment. To compute R we use the average income per taxpayer since the total number of taxpayers does not change in the post-treatment period (see Table 6, Column 1); its pre-treatment value is set to 12,150 (see Table 1, Column 5), while the post-treatment one equals 12,016 (resulting from the 1.8% drop from Table 6, Column 3). Figure A4 in this Online Appendix shows how W changes with ρ.

Additional Tables and Figures

Table A1. Regions and years in the LIS dataset

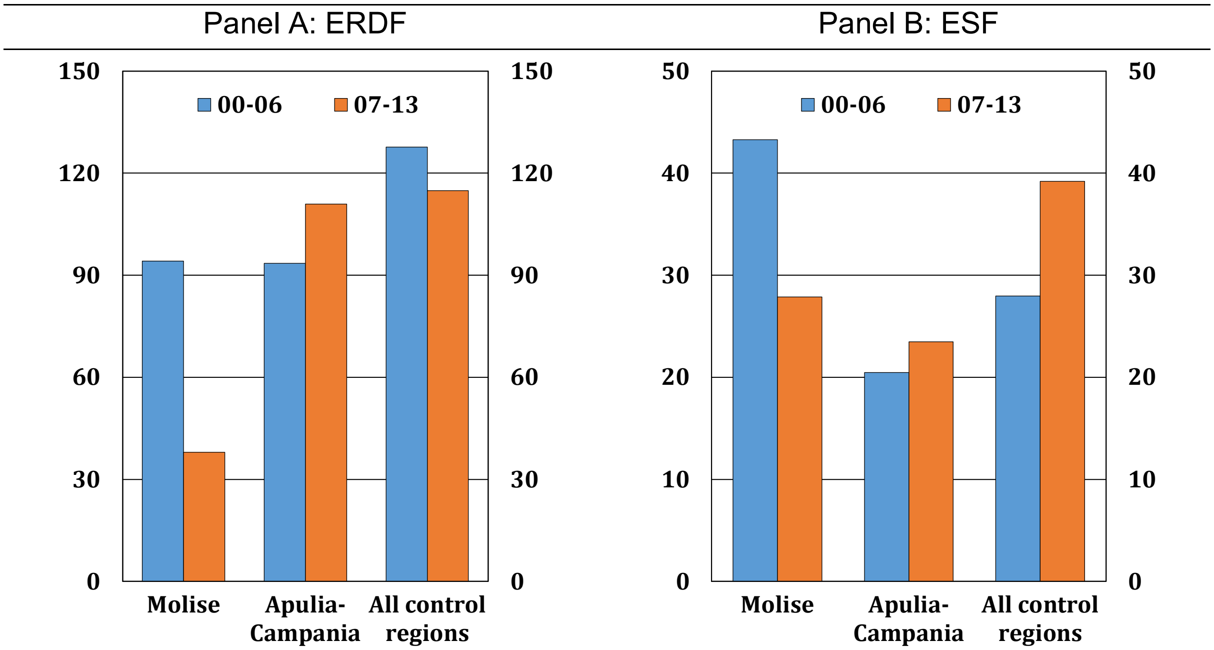
Country	Region	Available years
Germany	Brandenburg	1994, 2000, 2004, 2007, 2010, 2013
Germany	Mecklenburg-Vorpommern	1994, 2000, 2004, 2007, 2010, 2013
Germany	Thuringen	1994, 2000, 2004, 2007, 2010, 2013
Greece	Voreio Aigaio	2004, 2007, 2010
Greece	Kriti	2004, 2007, 2010
Greece	Thraki and Anatoliki Macedonia	2004, 2007, 2010
Greece	Ipeiros	2004, 2007, 2010
Greece	Thessalia	2004, 2007, 2010
Greece	Ionia Nisia	2004, 2007, 2010
Greece	Dytiki Ellada	2004, 2007, 2010
Greece	Peloponnisos	2004, 2007, 2010
Spain	Galicia	1990, 2004, 2007, 2010, 2013
Spain	Castilla-La Mancha	1990, 2004, 2007, 2010, 2013
Spain	Extremadura	1990, 2004, 2007, 2010, 2013
Spain	Andalucía	1990, 2004, 2007, 2010, 2013
Italy	Molise	1987, 1989, 1991, 1993, 1995, 1998, 2000, 2004, 2008, 2010, 2014
Italy	Campania	1986, 1987, 1989, 1991, 1993, 1995, 1998, 2000, 2004, 2008, 2010, 2014
Italy	Apulia	1986, 1987, 1989, 1991, 1993, 1995, 1998, 2000, 2004, 2008, 2010, 2014
Italy	Basilicata	1986, 1987, 1989, 1991, 1993, 1995, 1998, 2000, 2004, 2008, 2010, 2014
Italy	Calabria	1986, 1987, 1989, 1991, 1993, 1995, 1998, 2000, 2004, 2008, 2010, 2014
Italy	Sicily	1986, 1987, 1989, 1991, 1993, 1995, 1998, 2000, 2004, 2008, 2010, 2014

Figure A1. GDP per capita growth rate (regional level)



Notes. The panel shows the growth rate of GDP per capita (euros at constant 2010 prices) (source: own elaborations on Ardeco data).

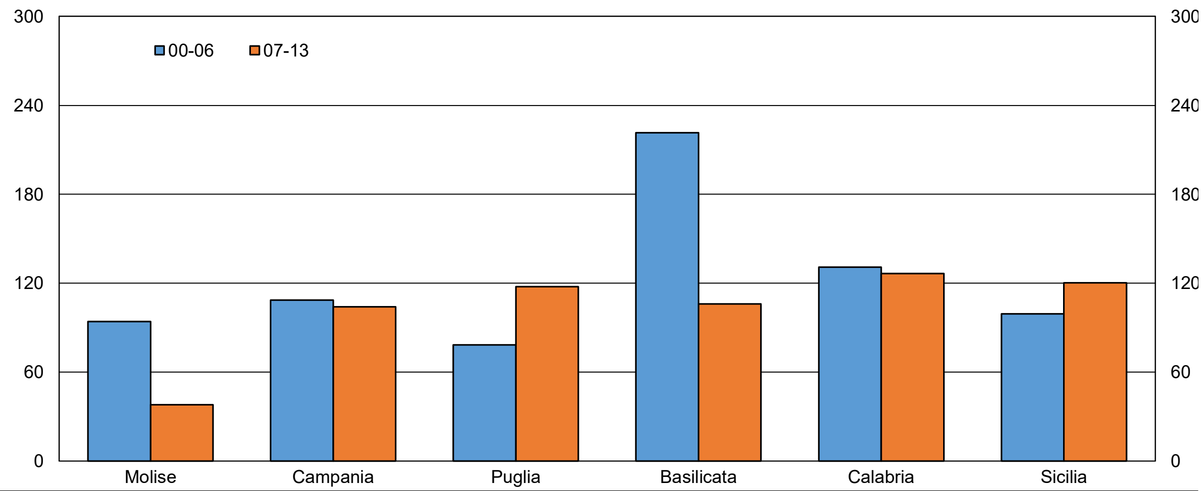
Figure A2. Spending from EU cohesion policy by fund



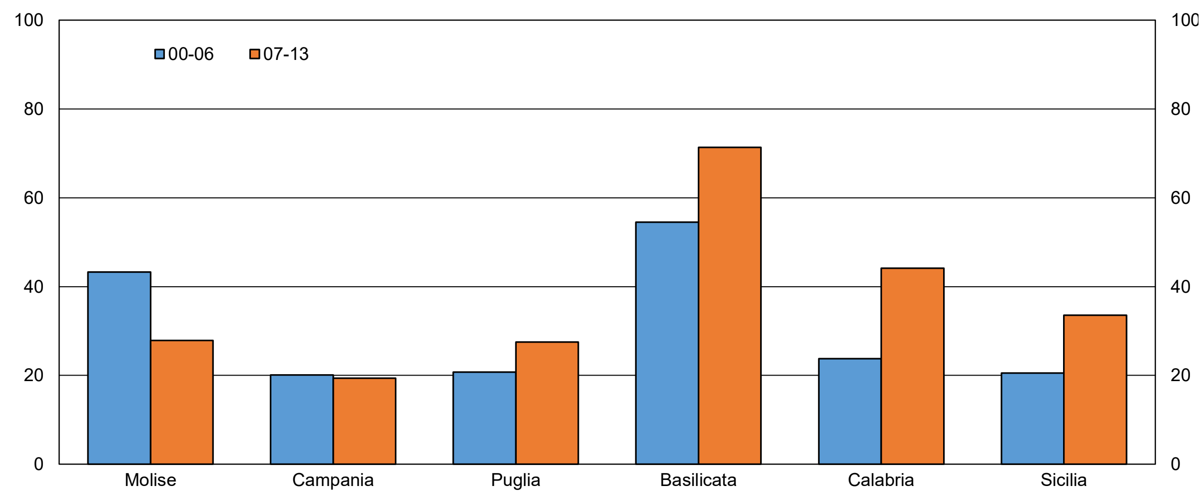
Notes. The figure shows the expenditure (euros per capita at constant 2010 prices) from EU cohesion policy (source: own elaborations on EU Commission data).

Figure A3. Spending from EU cohesion policy by fund (regional level)

Panel A: ERDF

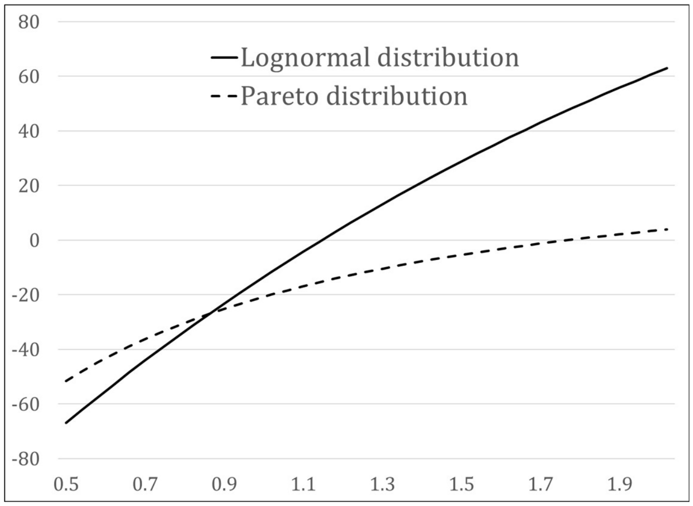


Panel B: ESF



Notes. The figure shows the expenditure (euros per capita at constant 2010 prices) from EU cohesion policy (source: own elaborations on EU Commission data).

Figure A4. Welfare analysis



Notes. The figure shows the change in the welfare function as a function of inequality aversion.