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When the Mafia Comes to Town

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Abstract
This paper investigates the effect of diffusion of organized crime on local economies by examining a legal institution that operated in Italy between 1956 and 1988. The law allowed Public Authorities to force mafiosos to resettle to another town. Using variation in the number of resettled mafia members across destination provinces in a differences-in-differences setting, I find no conclusive evidence on the effect of the policy on crime or homicides, while there is a very robust positive impact on employment in the construction sector. Results are consistent with mafia exploiting these new locations mainly for money laundering.

Keywords: Organized crime, law making, shadow economy.

JEL codes: K42, O17.

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1 Introduction

Organized crime in Italy developed first and foremost in some of the Southern regions. However, this phenomenon has spread and now affects also Northern and Central Italy and the other Southern Italian regions (IPAC (1994),Bandiera (2003)Ciconte (1998), Ciconte (2010), Sciarrone (1998), Varese (2011)). This paper focuses on "non-traditional" areas, i.e. those areas where organized crime was not originally present. There are several studies on the causes and the effects of organized crime on economic outcomes. However, to the best of my knowledge, the literature lacks a convincing identification, and merely focuses on traditional mafia regions. A focus on "non-traditional" areas is especially interesting because these areas typically have stronger institutions and better economic conditions. This allows one to isolate the causal effect of organized crime and to mitigate reverse causality issues related to the endogenous formation of organized crime in Southern Italy. Furthermore, the role of organized crime may be very different given the differing institutional features of the two regions, for instance, the mafia may prioritize institutional control in the traditional areas, whereas prioritize monetary gain in the non-traditional areas.

This paper aims to determine the causal effect of the arrival of organized crime to "non-traditional" areas on social and economic outcomes and discuss the possible implications for the inefficiencies generated and how they affect the market system. The presence of mafias in "non-traditional" areas has long been denied or ignored. There is now a growing concern in Italy about the impact of the diffusion of mafias in these areas and their infiltration in the legal economy. "SOS businesses", an organization sponsored by Confesercenti, a major business association in Italy, has estimated the activities of Italian mafia to amount to 7% of Italian GDP in 2007. Another sign of the relevance of organized crime in the economy is that 1516 firms have been seized by Public Authorities until 2010 because they were discovered to be mafia-related. I use a natural experiment to estimate the effect of the arrival of members of organized crime on local economies by using a policy that operated in Italy between 1956 and 1988. The law allowed public authorities to forcibly relocate mafia members to another town in Italy for 3 to 5 years. Using variation in the number of resettled mafia members across provinces of destination I estimate the impact on the incidence of crime and homicides, and employment by industrial sector. I find no evidence of an effect on total crime rates nor homicides rates, while there is a very robust positive effect on employment in the construction sector:

one more resettled mafioso per 100,000 inhabitants corresponds to an about 3% increase in employment. Anecdotal evidence suggests that the mafia preferred to limit the use of violence in non-traditional areas, so as not to attract the attention of legal enforcers, and instead invest there the proceeds of the illegal activities conducted elsewhere. The results in this paper are consistent with such evidence.

This paper is related to several strands of the literature. There is a recent literature in Italy on the effect of organized crime on local economies. Pinotti (2015) studies the effect of the formation of Sacra Corona Unita, SCU (an independent mafia organization) in Apulia on the GDP. Using a synthetic control method he estimates that the rise of organized crime is related to a 15% drop in GDP per capita over 30 years. The author discusses several caveats to a causal interpretation of this result. The main concern with this estimate is that the successful formation of a mafia organization in Apulia might be endogenous to changes in GDP per capita or that Apulia was hit by a negative shock that happened to be contemporaneous to the formation of SCU. Pinotti (2011) documents the existence of abnormal upswings in the homicide rate in mafia-affected regions during electoral periods. Coniglio *et al.* (2010) analyze the effect of organized crime on human capital accumulation and migration using a municipality-level panel data analysis that focuses on Calabria. They find that human capital accumulation is negatively affected by organized crime. Gennaioli *et al.* (2011) document a positive impact of public spending on organized crime using the case of the 1997 earthquake as a source of exogenous variation in public spending. Barone and Narciso (2012) instrument organized crime with rainfall in the 19th century and land productivity shifters and find that organized crime increases the amount of public funds to enterprises in Sicily. The most related paper to this study is Buonanno and Pazzona (2014), which studies the impact of migration flows from Southern to Northern provinces in Italy and the number of resettled mafia members on crime and concludes that the interaction between these two factors has contributed to the diffusion of organized crime. This paper differs from Buonanno and Pazzona (2014) in several ways: I use a different identification strategy that exploits the variation in the number of resettled mafia members in a differences-in-differences setting, I focus on a different period and I estimate the impact on both crime and economic outcomes.

Also related to this study is the literature on the efficiency costs of corruption and the economics of extortion. Bertrand *et al.* (2007) present experimental evidence on corruption in India and show that

corruption, intended as the ability to buy drivers' licenses without taking the test, "greases the wheel" but can also generate efficiency costs. Olken and Barron (2009) test industrial organization models of corruption and show that market structure affects the level of illegal payments. Similarly, the presence of different mafia organizations can affect the way organized crime operates, for instance the presence of different organizations can affect the level of payments for protection (Gambetta (1993)).

This paper is also related to the sociology literature on the origins of organized crime and its mechanisms of diffusion. Gambetta (1993) claims that the Sicilian Mafia is the business of private protection. It sells protection in areas where institutions are weak or enforces illegal contracts, since they cannot be enforced by legal institutions. For example mafia could play a big role in enforcing cartels in markets where cartels are not easily enforceable. Mafia would constitute a third party enforcer that can credibly inflict punishment in case of deviations. Bandiera (2003) provides an empirical test of Gambetta's theory. Along the same lines Varese (2011) offers an interesting perspective on the role of organized crime in the economy and some of the reasons why mafias succeeded in conquering new territories - the presence of significant sectors of the economy unprotected by the state can generate a demand for criminal protection, especially protection against competition, and a demand for services of dispute settlement.

The rest of the paper proceeds as follows. Section 2 presents the strategy used in the paper and the institutional details of the policy of forced resettlement. Section 3 describes the data. Section 4 describes the empirical strategy and the results for crime rates and homicides. Section 5 describes the empirical strategy and the results for employment by industrial sector and discusses caveats and possible channels driving the result. The last section concludes.

2 How to tackle this question: the policy of "Forced Resettlement"

Identifying the causal effects of organized crime on economic outcomes is particularly challenging. First, organized crime is hard to measure. The most often used measure for organized crime in the literature combines reported crimes and mafia-related homicides, but a problem with this approach is that under-

reporting of crimes might be systematically more severe in areas where organized crime is more powerful. Furthermore crime reports are the equilibrium result of the interaction between enforcers and criminals. Under-reporting is less of a concern for homicides, but the time series pattern might be driven by mafia wars, which are exceptional events arguably related to periods of crisis in the organization. Second, there are endogeneity concerns: the diffusion of organized crime is potentially endogenous to local economic outcomes. Consider an attempt to analyzing the effect of organized crime on competition in a given local market. On one hand, organized crime can be attracted by high rents in markets with low competition. On the other hand, Gambetta (1993) suggests that organized crime is the business of private protection. Thus highly competitive markets might demand mafias' services in order to enforce cartels. In general both economically depressed and flourishing areas can attract organized crime. The former by providing a fertile breeding ground for criminal activities, the latter by providing attractive business opportunities to invest the proceeds of such criminal activities and/or money laundering.

This paper tackles this question by using as a natural experiment a legal institution that operated in Italy between 1956 and 1988: the "Soggiorno Obbligato" (Forced Resettlement from now on). The national law allowed public authorities to force relocation of a mafioso to a town chosen by the authorities themselves. The widespread use of this law has been considered one of the most important causes for the diffusion of organized crime in non-traditional areas. The law was inspired by the idea that mafiosos would quit their criminal activities once they were out of their criminal network. The institution attempted to isolate powerful ringleaders from other members in the Mafia by forcing them to relocate to another town, under the assumption that organized crime is a byproduct of Southern Italian society and culture and could not develop in other areas.

However, it is a common opinion now that forced relocation was not enough to prevent the concerned individuals from communicating with their original networks and instead helped them to expand their networks in new areas and to discover new business opportunities. The Italian Parliamentary Antimafia Commission wrote in 1994: *"Forced resettlement, largely used without careful choices and without appropriate guarantees of control, has practically dispersed in many areas in Italy several individuals belonging to the mafia and has* ***implanted them in areas that would have probably been otherwise immune****. [...] people*

gradually implanted themselves in the area, brought their families there, created a favorable environment for their activities. It was a process that ***polluted the entire national territory****"* (IPAC (1994)). Also very interesting is the testimony of Gaspare Mutolo, a mafioso turned state witness, when asked about forced resettlement: *"The policy of forced resettlement has been a good thing, since it allowed us to contact other people, to discover new places, new cities"* (reported in Varese (2006)).

2.1 Institutional details of the policy

The law was passed in 1956 and it established that "... In case of serious danger, the person can be forced to relocate to a well-defined town" for three to five years. The law was very vague about how the destination place needed to be chosen and who was in charge of the choice. More details were contained in a subsequent legal provision, law 575/1965, that established a measure of forced resettlement specific to mafia members and added that "... Under exceptional public menace or danger for the concerned person, the Questore (Police Commissioner) or the National Director of Antimafia Prosecutions or the state prosecutor can ask the court to order forced relocation to a town, decided by the Questore, and with appropriate territorial and safety characteristics...". In 1982 the law was modified and some restrictions were established on the characteristics of the destination place: " ... forced relocation must be disposed in a town with no more than 5,000 inhabitants, far away from big metropolitan areas, so as to ensure an effective control ...". Forced resettlement was abrogated with the laws L. 327/1988 and L. 256/1993.

The documents of the Italian Parliamentary Antimafia Commission (IPAC from now on) contain some additional details about the implementation of forced resettlement. The IPAC examined the implementation of the law in 1976 and interrogated the President of the Palermo Court in order to collect information about how the destination town was chosen. The President of Palermo Court claimed that the Italian Department of the Interior compiled a list of towns where mafiosos could be relocated. The local Court then decided the destination from the list, preferring towns where the concerned person could be more easily controlled. He also said that the list changed over time. Unfortunately the document does not contain this list, nor any information about how the list was compiled. A useful piece of information is contained in the testimony of the Head of Italian Police in 1963. He said "we used to relocate people in Ustica, but then we had to stop

because the population opposed to it. We did not manage to find an island where to send them, thus we sent them in several towns in Italy and we have a list of towns where we can send them" (IPAC (1976-1982)) .

3 The data

3.1 The data on forced resettlement, treatment definition and variation

This paper uses data on the total number of people resettled to each province in the period 1961-1972 (IPAC (1976-1982)).

I define the treatment as the number of people resettled to a province p per 100,000 inhabitants in the period 1961-1972. I normalize the treatment with respect to the population to have a measure of relative exposure to the arrival of mafiosos for provinces of different sizes. Measurement error can be a concern, for example because a good measure of treatment intensity should contain information on the importance of the resettled mafioso in his organization. There are several concerns with using this policy as a source of exogenous variation in the diffusion of organized crime. There might be corruption in the form of a mafioso being able to be resettled where he prefers. Other sources of selection might generate from the opposition of destination towns to the arrival of mafia members. Furthermore forced resettlement might be applied together with other security measures that limit the freedom of the resettled mafioso, thus reducing his ability to develop new networks in the place of destination.However I am relying on the assumption that the higher the number of people resettled to a province, the higher the probability that resettled mafiosos can establish connections in the province of destination. This assumption does not hold if leaders are sent to areas where no other members of the organization were ever sent, but still have a higher ability to build networks. However it is reasonable to assume that important members of the mafia would be applied additional control measures and thus would be less effective in building new connections in the destination towns. The choice of normalizing by the average population in the period 1961-1972 is not optimal if more populated areas offer more opportunities to the mafia to expand their networks. Population density might be key in this analysis and it is not taken into account in the definition of the treatment. An alternative choice can be to normalize the number of people resettled to province p by the area of the province. Given that there is no

flawless choice for the normalization of the treatment I here use population as the most immediate one.

Figure 1 plots the distribution of the number of people resettled per 100,000 inhabitants (the treatment variable). The mean of the treatment variable is 7.22 and the standard deviation is 4.6. The total number of resettled people sent to province p in the period 1961-1972 has mean 30 and standard deviation 17. Italy has now 110 provinces, but the data is reorganized to reproduce the provincial boundaries in 1954, with 92 provinces. Excluding provinces belonging to Calabria, Campania, Sicily and Sardinia the number of provinces is 72. The total number of people resettled between 1961 and 1972 is 2360, excluding people resettled to provinces in Calabria, Campania, Sicily and Sardinia, the number of people resettled is 2154. The picture on the left in figure 2 shows a map of the intensity of the treatment across Italian provinces (at current borders), while the picture on the right shows the variation in the number of people resettled to each province without normalizing by the population. A comparison between the two figures shows that the normalization matters a lot for the identification of high intensity areas, since less populated provinces were assigned a higher per capita number of mafiosos.

3.2 The data-set on crime

The crime data-set contains the total number of crimes and the number of homicides by province and year for the period 1956-1975.

The series for total number of crimes covers all crimes reported by Police and Carabinieri from 1956 to 1975 (ISTAT (1956-1976)). The main source for this data-set is the Yearbooks of Judiciary Statistics, which has no distinction by type of crime. For the period 1958-1967 the available source (ISTAT (1958-1974)) reports crimes at the province level per broadly defined type of crime. Starting in 1968 the data set contains crimes reported by all Public Security Authorities and for which the penal action has started. This source distinguishes across types of crime in detail but the two sources are fairly different and not comparable. I thus use only data on the total number of crimes and only combine the data from the two available sources in order to form a panel for the number of homicides for the period 1956-1975, arguing that the difference in sources generates less of a concern for homicides relative to other types of crimes.

3.3 The data set on employment by sector

The data on employment by sector is extracted from the Census of Industry and Services. It covers the period 1951-2001 with decennial frequency, plus data for 1996 when there was a midway Census. The employment data distinguishes eight broad sectors: construction, manufacturing, mining, energy, transportation, commerce, credit and other services. The data collection method has improved over time. Until 1991 the gathering of data is based on the assumption of ignorance: the list of units to be included in the Census is compiled through the Census itself and the data collector executes a "door to door" analysis without knowing what he will find in the section he got assigned. After having collected the questionnaires, a match is performed with existing information to make sure no unit was ignored in the process.

Starting with the intermediate Census in 1996 and in 2001 the collection method changes dramatically. The National Institute of Statistics has now statistical archives about all the firms and the questionnaires are performed so as to update these archives. This change in the data collection process should allow a better coverage of individual firms and freelancers. However there are no reasons to believe that the variation in the effect of such changes is correlated with the treatment variable, while any general effect will be partialled out with the inclusion of year fixed effects.

4 Results

4.1 Econometric model and results for local crime rates

The empirical strategy is a differences in differences with the intensity of the treatment defined as the number of people resettled to province p per 100,000 inhabitants in the period 1961-1972.

The identifying assumption is that absent the treatment, any difference in trends between provinces would be independent of the treatment status.

There are several concerns with using this policy as a source of exogenous variation in the diffusion of organized crime. There might be corruption in the form of a mafioso being able to be resettled where he prefers or selection might arise from the opposition of destination towns to the arrival of mafia members. Furthermore, forced resettlement might be applied together with other security measures that limit the

freedom of the resettled mafioso, thus reducing his ability to develop new networks in the place of destination. However, as long as selection is unrelated to the trends the identifying assumption is satisfied.

The estimating equation is

$$log(crimerate)_{prt} = \alpha_p + \lambda_{rt} + \delta logpop_{prt} + \beta N_{pr} Post_t + \epsilon_{prt} \quad (1)$$

where $log(crimerate)_{prt}$ is the natural logarithm of the total number of reported crimes per-capita in province p, region r in year t, α_p is province fixed effect, *logpop* is the log of the population, N_p is the total number of mafiosos resettled to province p in the period 1961-1972 per 100,000 inhabitants,λ_{rt} are region-specific year effects and *Post* is a dummy for $year \geq 1973$.

All the models are estimated using yearly data from 1956 to 1961 and 1973 to 1975.

Panel A of table 1 shows the results for all provinces, excluding provinces from Campania, Calabria, Sicily and Sardinia (the "traditional" areas). When controlling only for province and year fixed effects, the coefficients on the treatment are negative and non-significant. However, controlling for region-specific year effects, the coefficient on the interaction term becomes positive, but still non-significant. In model (3) I control for the log of the population. Once I control for population I am looking at the effect on the numerator only, and this is again very small and negative but not significant. Controlling for region-specific time effects flips the sign of the coefficients from negative to positive, but again they are not significant. Overall there is no evidence of an effect of the treatment on crime rates.

Panel B of table 1 shows results for the sub sample of provinces in Lombardy, Veneto, Emilia Romagna, Liguria, Tuscany, Piedmont and Friuli (Northern Italy excluding Valle d'Aosta and Trentino Alto Adige and including Tuscany). The estimates are positive and only marginally significant when I don't control for neither region-specific trends nor population. The coefficients are instead very small and not significant when allowing for differential trends across regions. I also estimated models with interactions between the intensity variable and each year in the post-treatment. The results for the entire sample are very similar but for Northern Italy provinces the treatment effect in 1973 and 1974 is marginally significant when I control for population but not if region-specific time effects are included.

Overall the null cannot be rejected, but this might be due to lack of power, rather than a zero effect.

The available crime data does not report any distinction by type of crime, so the pattern of more mafia-related crimes, such as kidnappings for ransom, extortion, drug trafficking etc. might be hidden into the small positive point estimate on total crime rates, and the lack of precision of the estimates might be caused by the noise that is added when considering all types of crimes.

The results in table 1 do not change in a significant way when I include province specific linear trends.

In order to look for pre-trends or changes in trends I estimate the following model:

$$log(crimerate)_{pt} = \alpha_p + \lambda_{rt} + \delta logpop_{pt} + \sum_{\tau=1958}^{1960} \beta_\tau N_p D\tau_t + \sum_{\tau=1962}^{1975} \beta_\tau N_p D\tau_t + \epsilon_{pt} \quad (2)$$

where all the variables are defined as above and $D\tau_t$ is a dummy for $year = \tau$.

The graphs in figure 1 report the coefficients from equation 2. When controlling only for province and time fixed effects the coefficients show a slightly decreasing pattern, especially following 1968. This decreasing pattern might suggest that differences in trends are systematically related to the treatment status. This is consistent with high treatment provinces being on flatter trends than low treatment provinces. However, when I control for region specific trends this decreasing pattern disappears. There is no evidence of pre trends as the coefficients until 1965 are very close to zero. From 1966 the point estimates are bigger. However the estimates are imprecise so I do not have conclusive evidence on whether there is any effect. Figure 4 shows the pattern of coefficients for the sub-sample of provinces from Northern regions and Tuscany. When restricting the sample the point estimates are higher, however they are very noisy. Again I do not have enough power to conclude that there is no effect.

4.2 Results for homicides

The analysis for the incidence of homicides is conducted using Poisson regressions with the population as an exposure variable[1].

As described in the previous section the data on homicides has a break in 1968, where another source is available. Year fixed effects take care of this break to the extent that it affected all provinces in the same way. Figure 5, however, suggests that this might not be the case. The graph on the left plots the coefficients

[1]The Poisson specification is preferred to the log specification because there are province-year cells with zero homicides

from a specification that controls only for province and year fixed effects. There is a significant drop in the coefficient from 1967 to 1968. The graph on the right plots coefficients from a specification that also includes region-specific time effects. As shown in the figure, the drop in the coefficient from 1967 to 1968 is now smaller, and there is more overlap in the confidence intervals. Given this issue, it is hard to interpret the estimates. The negative and significant effects shown in the graph can be a result of reducing the level of the coefficients from 1968 on. One thing that is worth noticing is that until 1967 the pattern of coefficients does not show evidence of selection.

Figure 6 shows the estimated coefficients for the restricted sample: Lombardy, Piedmont, Liguria, Veneto, Friuli Venezia Giulia, Emilia Romagna and Tuscany. Again the coefficients are very hard to interpret because of the change in data source in 1968, but there is an increasing pattern after 1968. One possible interpretation is that the change in data source introduces a downward bias[2] that hides a positive effect of the policy on the incidence of homicides.

5 The effect on employment in different sectors

As discussed in the introduction, mafias' activities both in the legal and the illegal sector can have an important role in the economy. However the effect of mafias' activities can have different impacts across sectors. I measure the effect of resettled mafia members on the economy using employment in different sectors.

The empirical strategy closely follows the previous sections: differences in differences using variation across provinces of destination in the number of mafia members resettled per 100,000 inhabitants. The identifying assumption is that absent the treatment, differences in trends across provinces would be unrelated to the treatment status. The treatment assignment has to be independent on the trends or on any other unobserved characteristics that drives the trends. I will discuss caveats to the identification in the following subsection.

[2]High treatment areas are less affected by the change in data source.

The main estimating equation is

$$log(emp)_{pjt} = \alpha_{pj} + \lambda_{tj} + \beta_{post,j} N_p \mathbf{1}(year \geq 1971)_t + \epsilon_{pjt} \tag{3}$$

The dependent variable is the natural logarithm of employment in province p and sector j. A level specification would constrain the outcomes to grow by the same absolute amount over each of the ten years interval, which would be inappropriate given the considerable variation in size across provinces. I estimate equation 3 for each sector using data from 1951 to 2001, at decennial frequency. As discussed above the data collection process changed dramatically in 1996, however if the impact of this change is homogeneous across provinces the results are unaffected. As a robustness check I show results also for the sub-sample of years up to 1991.

In order to analyze the possible existence of pre trends I estimate the following equation using only data for 1951 and 1961

$$log(emp)_{pjt} = \alpha_{pj} + \lambda_{tj} + \beta_{pre,\,j} N_p \mathbf{1}(year = 1961)_t + \epsilon_{pjt} \tag{4}$$

The coefficient β_{pre} shows whether the percentage growth in employment in different sectors is systematically different between treatment and control before the treatment is received. This is not a test of the identifying assumption, given that differences in trends related to the treatment status might arise at the same time as the treatment itself. However having no evidence of pre trends is reassuring.

Table 4 shows the estimates of equation 3 and equation 4 for Construction, Manufacturing, Mining and Energy. The first row in each column shows the coefficient β_{post} for different specifications, while the second row shows the coefficients β_{pre}. In the first column I control for province and year fixed effects. The second column controls also for region-specific time effects. The third column controls for province and year fixed effects and the log of the population and the last column controls for province and year fixed effects, region-specific time effects and log of the population. Each panel shows the estimates for different sectors. The estimates of β_{post} for construction, manufacturing and energy are positive and significant while the estimates of β_{pre} are small and not significant. Table 3 shows the estimates of equation 3 and equation 4 for

transportation, commerce, credit and other services. The first row in each column shows the coefficient β_{post} for different specifications while the second row shows the coefficients β_{pre}. In the first column I control for province and year fixed effects. The second column controls also for region-specific time effects. The third column controls for province and year fixed effects and the log of the population. The last column controls for province and year fixed effects, region-specific time effects and log of the population. Each panel shows the estimates for different sectors. The estimates of β_{post} are very small in magnitude and not significant when I do not control for the log of the population while the estimates of β_{pre} are negative and significant for other services when I do not control for the log of the population and small and not significant otherwise and for all the other sectors. Overall there is not much evidence of an effect on transportation, commerce, credit and other services.

As an additional robustness check I estimate equation 3 including also province-specific linear trends. The results are shown in table 4. Overall the result for the construction industry is fairly robust while the results for other sectors change across specifications.

As discussed in the data section the data collection process changed dramatically in 1996. Thus I also estimate equation 3 with and without additional controls using only data up to 1991. The results are reported in tables A.1 and A.2 in the Appendix. The results without controlling for province-specific linear trends are very similar to the results in table 5 and table 3. When including province-specific linear trends the point estimates for construction are slightly smaller and not significant (the p-values vary across specifications from .055 to .09), but such specification is highly demanding, thus lack of precision is expected.

In order to look at the effect at different points in time I estimate the following equation for each sector

$$log(emp)_{pjt} = \alpha_{pj} + \lambda_{tj} + \beta_{1951,\,j} N_p \mathbf{1}(t = 1951) + \sum_{\tau=1971}^{2001} \beta_{\tau,\,j} N_p \mathbf{1}(t = \tau)_t + \epsilon_{pjt} \tag{5}$$

Equation 6 includes the interaction between the treatment N_p and time dummies for all the available years excluding 1961. Thus the coefficients β_τ are differences in differences coefficients using 1961 as the baseline year and the coefficient β_{1951} checks for the presence of pre-trends. Figure 7 shows the point estimates and 95% confidence intervals for the β coefficients in equation 6. The point estimate for the pre-trend is close to zero while the pattern of coefficients in the post period is hump-shaped. The estimate is

positive and significant in all census years following 1961, and it is increasing until 1981 after which it starts decreasing. The magnitude in 1971 suggests an average positive impact on employment in 1971 of 2% for one more mafia member (per 100,000 inhabitants). However, the magnitude is difficult to interpret given that I cannot rescale the coefficients by the first stage.

The next subsections discuss caveats to identification and possible channels for the interpretation of the positive impact of resettled mafia members on employment in the construction industry.

5.1 Potential confounding factors

In the period between the end of the Second World War and the late 60s, in particular between 1958 and 1963, Italy experienced an unprecedented economic boom and went from being a poor, mainly rural nation to an industrial power. This growth came along with inter-regional migration of the population, from rural areas in Southern Italy to industrial areas in Northern Italy. There were also movements within regions, from rural areas to the cities. The needs of a changing society created a huge demand for transport and energy infrastructures. The real estate market also experienced a major boom under the pressure of increasing demand for housing around the largest cities. As a consequence there was massive property speculation.

Even though the economic boom and the construction boom affected the whole country, there might be a concern that high treatment is systematically related to higher growth in the construction industry. If highly rural areas were receiving a higher per-capita number of resettled mafia members. These areas might be experiencing a higher percentage growth in the construction industry because there is a higher movement from rural areas to central cities or simply because there is more room for expansion of the construction sector. However, given that there is no evidence of differential trends related to the treatment in the period 1951-1961, for this mechanism to generate spurious correlation it must be the case that it took place starting only after 1961. Given lack of more specific controls, I test this possibility by controlling for the log of employment in the construction sector in 1951. Notice that, while including the log employment in 1961 causes OLS estimates to be inconsistent[3], under the strong assumption of no serial correlation OLS gives

[3]The logarithm of employment in 1961 is a function of the error term in 1961, and so is the error term in equation 6.

consistent estimates for equation 6:

$$log(emp)_{pt} - log(emp)_{p1961} = \alpha_t + \beta_t N_p + \delta_t log(emp)_{pj,1951} + \epsilon_{pt} \;\; for\; t \in \{1971, 1981\} \tag{6}$$

Table 5 shows the estimate for equation 6. For $t = 1971$ the point estimate is very small and not significantly different from zero, however for $t = 1981$ there is still a positive and significant (at the 5%) effect in the same order of magnitude as the estimates shown in the previous section. The coefficient δ_t is negative and significant, suggesting that there might be mean reversion. The drop in the point estimate for β_{1971} in equation 6 might be due to heterogeneous treatment effects and misspecification. Moreover, the fact that the point estimate for β_{1981} is still significant and that a graphical analysis reveals that the pattern of the coefficients is qualitatively similar to the pattern shown in figure 7 mitigates the concern that the result might be driven by mean reversion.

5.2 Possible channels

Construction is considered one of the sectors mostly affected by mafias (Saviano (2007), Sciarrone (1998), Varese (2011)). Of the 1516 firms seized by Public Authorities until 2011, the share of construction firms is 27.11%. The broader sector of retail and wholesale, housing and vehicles repair accounts for 27.84% of the total. 10.03% is the share of hotels and restaurants, 8.97% for real estate firms. Public authorities seized 10,438 real estates, 2,639 of which were in "non-traditional" areas (ANBSC (2011)). The construction sector allows one to launder huge amounts of money relatively easily. Real estate firms and monopolistic position in the production of concrete and cement lend support to the investments in the construction sector by assuring control over a network that goes from input and production to sale.

A first step towards understanding the channels behind the positive estimate obtained above is to distinguish whether the increase in employment registered in the data corresponds to a real increase in economic activity in the sector or not. Part of the positive effect estimated might be driven by fake hires for the scope of money laundering. A possible way to tackle this question is to compare the occupations declared in the census of the population to the occupations declared in the census of the industry. However, my prior is that fake hires is not the driving mechanism behind the estimated positive impact.

If the increase in employment in the construction sector is instead driven by real expansion of the sector, there are several possible channels. First, mafia members might be simply trying to invest the proceeds of its illegal businesses in the construction industry. This per se does not necessarily generate inefficiencies. Suppose, for example, that there is a strictly increasing return to investment in the construction industry and that all that mafia does is to invest money in the industry without otherwise affecting the working of the market. Along these lines mafias might even be slacking liquidity constraints that would otherwise limit the expansion of the construction sector. Even though interest rates were low during the Italian economic boom, access to credit might still be limited by collateral constraints or other types of frictions. Then mafia might behave as a liquidity provider. Anecdotal evidence suggests that mafias do not limit themselves to simply invest the proceeds of their illegal activities and that they can potentially influence competition and/or generate corruption. However the question of whether this generates inefficiencies is still open. If corruption of officials in order to obtain building permits is just eliminating frictions, then there is no efficiency cost and the undesirability comes only from the disutility of corruption and the strengthening of the mafia. If instead the mafia bribes officials in order to obtain building permits in areas where the social cost of construction is higher than the benefit, corruption has an efficiency cost.

Anecdotal evidence from traditional areas gives some support for this channel. Salvatore Lima, mayor of Palermo between 1958 and 1963 (killed by the mafia in 1992) is considered responsible for what is known as the Sack of Palermo, a dramatic urbanization of the territory that changed the looks of the city. He was also accused of having favored mafia related firms. Tano Badalamenti, a mafia member resettled in the early 70s, was found guilty of bribing officials in order to have the airport built near his hometown so as to take part to the works with his construction firms.

Another way mafias might cause an expansion in the construction sector is by diverting public funds from other socially more beneficial investments into construction of infrastructures and buildings. This would amount to the mafia generating its own demand in the construction sector, not only directly via the diversion of public funds but also via a multiplier mechanism: building infrastructures in previously rural areas increases the value of the land and makes it valuable to build in those areas. Mafia might also invest more in the construction sector because of lower marginal costs due to the use of illegal practices.

There is also growing concern about the fact that mafia related firms use poor cement and do not respect earthquake-proof regulations in construction (IPAC (1994)).

6 Conclusions

This paper studies the causal effect of the diffusion of organized crime in so-called "non-traditional" areas in Italy on both social and economic outcomes. There are several challenges that need to be faced when dealing with this question. First, measuring organized crime is a difficult task. Second, organized crime is endogenous to economic and social outcomes. Moreover the direction of the bias is unclear. If interested in analyzing the effect of organized crime on competition in a given local market, one needs to take into account that organized crime can be attracted by high rents in markets with low competition. On the other hand, highly competitive markets might demand mafias' services in order to enforce cartels. In general both economically depressed and flourishing areas can attract organized crime. The former by providing a fertile breeding ground for criminal activities, the latter by providing attractive business opportunities to invest the proceeds of such criminal activities and/or money laundering.

In order to overcome these issues I use a legal institution in force in Italy between 1956 and 1988. A law passed in 1956 allowed public authorities to order forced resettlement of mafia members to different towns. Using variation across destination provinces in the number of mafia members resettled to each province in the period 1961-1972 in a differences in differences setting, I estimate the impact of the exposure to mafia members on local crime rates, incidence of homicides and employment in different industrial sectors in the provinces of destination.

I do not find conclusive evidence for crime rates or homicides. The point estimates for crime rates are not significant, but I do not have enough power to draw clear conclusions. The lack of precision in the estimates might be due to the fact that there is no distinction across types of crime. Mafias are known to positively affect the incidence of some types of crime, like kidnappings for ransom, extortion, drug trafficking etc. However they might have a negative or zero impact on other types of crime, given that they might induce pet criminals to reduce their activity or to join the organization and focus on other more profitable activities. Unfortunately the data on homicides has a break in 1968 that might affect provinces in different

ways depending on their treatment status. This makes the estimated coefficients very hard to interpret, so it not possible to determine the impact of the arrival of mafia members to these areas on homicides without using better data.

Motivated by the importance that is attributed to the activities of organized crime in the legal economy and especially in the construction industry, I estimate the effect of the exposure to mafia members in 1961-1972 on employment in different industrial and services sectors. I find evidence of a positive impact on several sectors. However, the positive impact on employment in the construction sector appears to be the most robust. This result is evidence of mafias' activities in the local economies and especially in the construction sector. Several channels that might be at play and the implications in terms of economic efficiency can be very different.

Future work will attempt to disentangle such channels and shed light on the consequences in terms of economic efficiency.

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Table 1

The Impact of Resettled Mafia Members on Crime Rates

	(1) logcrimerate	(2) logcrimerate	(3) logcrimerate	(4) logcrimerate
	Panel A			
	EXCLUDING CAMPANIA, CALABRIA, SICILY AND SARDINIA			
Post*Treatment	-0.011	0.006	-0.008	0.006
	(0.008)	(0.009)	(0.009)	(0.009)
N	648	648	648	648
N_clust	72	72	72	72
	Panel B			
	RESTRICTED SAMPLE: NORTHERN ITALY AND TUSCANY			
Post*Treatment	0.014	0.006	0.016*	0.004
	(0.009)	(0.011)	(0.009)	(0.010)
N	432	432	432	432
N_clust	48	48	48	48
Controls :				
Province and Year FEs	Yes	Yes	Yes	Yes
Region-specific year effects	No	Yes	No	Yes
Log(population)	No	No	Yes	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Column 1 controls for province and year fixed effects, column 2 also controls for region specific time effects, column 3 controls for the log of the population and province and year effects and the last column includes all the previous controls. Standard errors are clustered at the province level. Panel A refers to the full sample exluding provinces from traditional areas. Panel B shows the estimates for the restricted sample of Northern provinces and Tuscany.

Table 2

The Impact of Resettled Mafia Members on Employment by Sector
Construction, Manufacturing, Mining and Energy

	(1) logemp	(2) logemp	(3) logemp	(4) logemp
		Panel A: Construction		
Post*Treatment	0.027**	0.028**	0.035***	0.038***
	(0.008)	(0.009)	(0.008)	(0.010)
Pre*Treatment	-0.003	0.012	-0.004	0.012
	(0.011)	(0.009)	(0.013)	(0.012)
		Panel B: Manufacturing		
Post*Treatment	0.031**	0.020*	0.043***	0.032**
	(0.009)	(0.009)	(0.010)	(0.010)
Pre*Treatment	-0.003	-0.005	-0.001	0.001
	(0.005)	(0.005)	(0.005)	(0.006)
		Panel C: Mining		
Post*Treatment	0.020	-0.032	0.030	-0.027
	(0.019)	(0.018)	(0.019)	(0.020)
Pre*Treatment	0.008	-0.034*	0.010	-0.033
	(0.016)	(0.015)	(0.017)	(0.018)
		Panel D: Energy		
Post*Treatment	0.028***	0.024*	0.031**	0.025*
	(0.008)	(0.011)	(0.009)	(0.012)
Pre*Treatment	0.010	0.006	0.015	0.012
	(0.009)	(0.006)	(0.011)	(0.007)
Controls:				
Log(population)	No	No	Yes	Yes
Region-specific time effects	No	Yes	No	Yes
Province & time FEs	Yes	Yes	Yes	Yes
N_clust	72	72	72	72

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Post*Treatment is β_{post} and Pre*Treatment is β_{pre}. Column 1 controls for province and year fixed effects, column 2 also controls for region-specific time effects, column 3 controls for the log of the population and province and year effects and the last column includes all the previous controls. The standard errors, reported in parentheses, are clustered at the province level.

Table 3

The Impact of Resettled Mafia Members on Employment by Sector
Transportations, Commerce, Credit and Other Services

	(1) logemp	(2) logemp	(3) logemp	(4) logemp
		Panel A: Transportations		
Post*Treatment	0.003	0.002	0.015*	0.019**
	(0.006)	(0.007)	(0.006)	(0.007)
Pre*Treatment	-0.001	-0.013	0.005	-0.003
	(0.005)	(0.006)	(0.004)	(0.005)
		Panel B: Commerce		
Post*Treatment	0.002	-0.004	0.015**	0.015***
	(0.006)	(0.005)	(0.005)	(0.004)
Pre*Treatment	0.001	-0.005*	0.004	0.002
	(0.003)	(0.002)	(0.003)	(0.002)
		Panel C: Credit		
Post*Treatment	0.011	0.004	0.021***	0.020*
	(0.006)	(0.008)	(0.006)	(0.008)
Pre*Treatment	0.001	0.001	0.003	0.003
	(0.003)	(0.003)	(0.003)	(0.004)
		Panel D: Other Services		
Post*Treatment	-0.011	-0.012	0.004	0.011
	(0.008)	(0.007)	(0.009)	(0.008)
Pre*Treatment	-0.014**	-0.018***	-0.006	-0.005
	(0.005)	(0.004)	(0.005)	(0.004)
Controls:				
Log(population)	No	No	Yes	Yes
Region-specific time effects	No	Yes	No	Yes
Province and time FEs	Yes	Yes	Yes	Yes
N_clust	72	72	72	72

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Column 1 controls for province and year fixed effects, column 2 also controls for region-specific time effects, column 3 controls for the log of the population and province and year effects and the last column includes all the previous controls. Post*Treatment is $beta_{post}$ and Pre*Treatment is β_{pre}. The standard errors, reported in parentheses, are clustered at the province level.

Table 4

The Impact of Resettled Mafia Members on Employment by Sector Controlling for Province-Specific Linear Trends

	(1) logemp	(2) logemp	(3) logemp	(4) logemp
		Panel A: Construction		
Post*Treatment	0.025**	0.027**	0.020*	0.019*
	(0.008)	(0.010)	(0.008)	(0.009)
		Panel B: Manufacturing		
Post*Treatment	-0.003	-0.003	0.005	0.007
	(0.006)	(0.008)	(0.005)	(0.008)
		Panel C: Mining		
Post*Treatment	0.011	-0.001	0.015	0.014
	(0.016)	(0.024)	(0.017)	(0.026)
		Panel D: Energy		
Post*Treatment	0.014	0.003	0.014	0.001
	(0.010)	(0.016)	(0.011)	(0.018)
		Panel E: Transportations		
Post*Treatment	0.001	0.007	0.005	0.013
	(0.006)	(0.010)	(0.004)	(0.007)
		Panel F: Commerce		
Post*Treatment	-0.001	-0.005	0.003	0.003
	(0.003)	(0.003)	(0.003)	(0.003)
		Panel G: Credit		
Post*Treatment	0.005	-0.001	0.011**	0.006
	(0.004)	(0.006)	(0.004)	(0.005)
		Panel H: Other Services		
Post*Treatment	-0.011*	-0.011	0.002	0.006
	(0.004)	(0.006)	(0.004)	(0.005)
Controls:				
Province & time FEs	Yes	Yes	Yes	Yes
Province-specific linear trends	Yes	Yes	Yes	Yes
Log(population)	No	No	Yes	Yes
Region-specific time effects	No	Yes	No	Yes
N	504	504	504	504
N_clust	72	72	72	72

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

All columns include province-specific linear trends. The standard errors, reported in parentheses, are clustered at the province level.

Table 5

Robustness Check

	(1) $log(emp)_{1971} - log(emp)_{1961}$	(2) $log(emp)_{1981} - log(emp)_{1961}$
Treatment	0.004	0.023*
	(0.009)	(0.009)
logemp51	-0.187**	-0.150**
	(0.057)	(0.049)
_cons	1.743**	1.451**
	(0.537)	(0.447)
N	72	72

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Robust standard errors in parentheses.

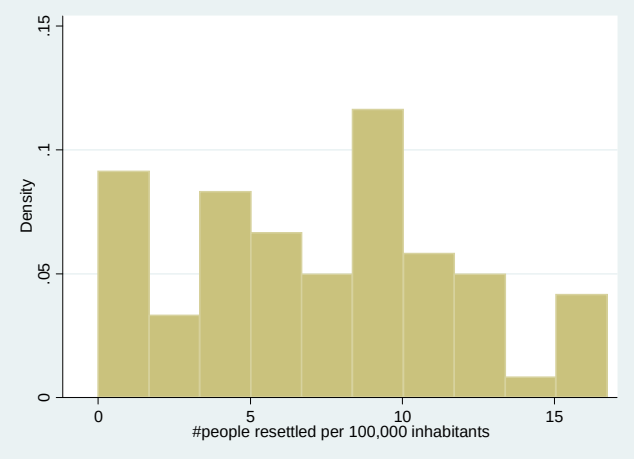

Figure 1: Histogram of the number of people resettled per 100,000 inhabitants.

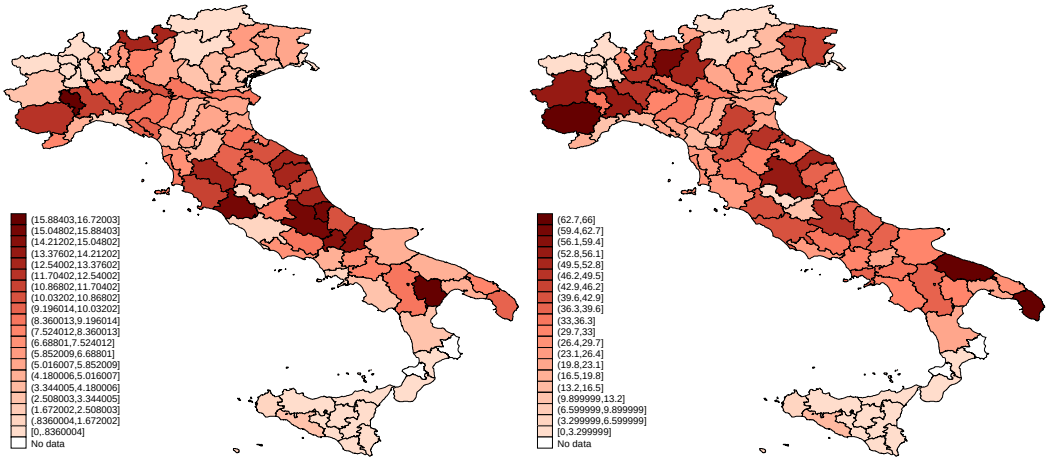

Figure 2: Map of the intensity of the treatment.
On the left provinces with higher per capita number of resettled mafiosos are filled with darker colors. On the right provinces with higher total number of resettled mafiosos are filled with darker colors. Each graph is obtained by dividing the range of the variable in 20 equally sized bins.

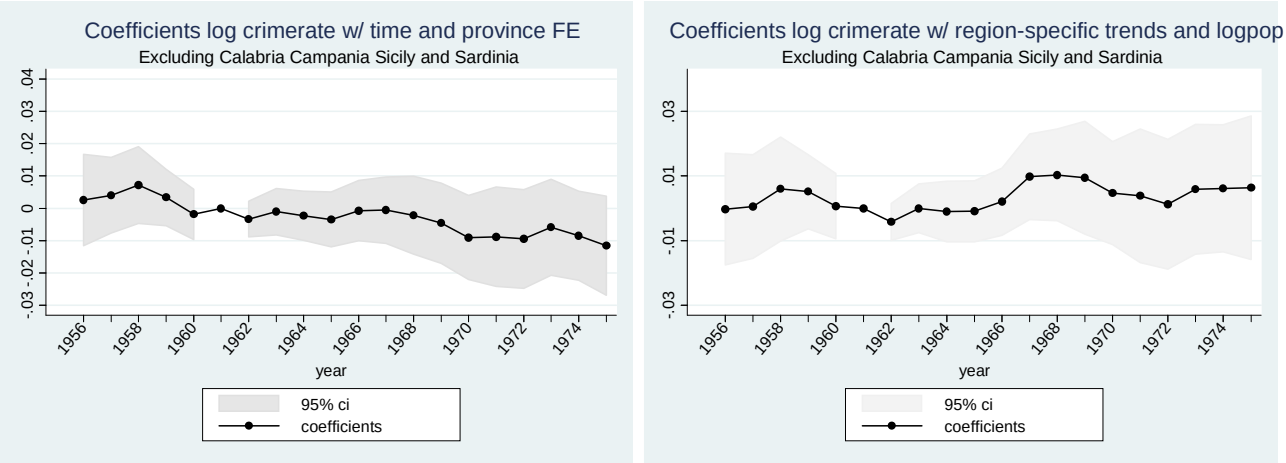

Figure 3: Estimates of β_τ from equation 2.
The plots show estimates of the coefficients β_τ for different specifications. The specification in the plot on the left controls for province and year fixed effects. The estimates reported on the right plot on the right are based on models that include province and year fixed effects, region-specific time effects and the log of the population.

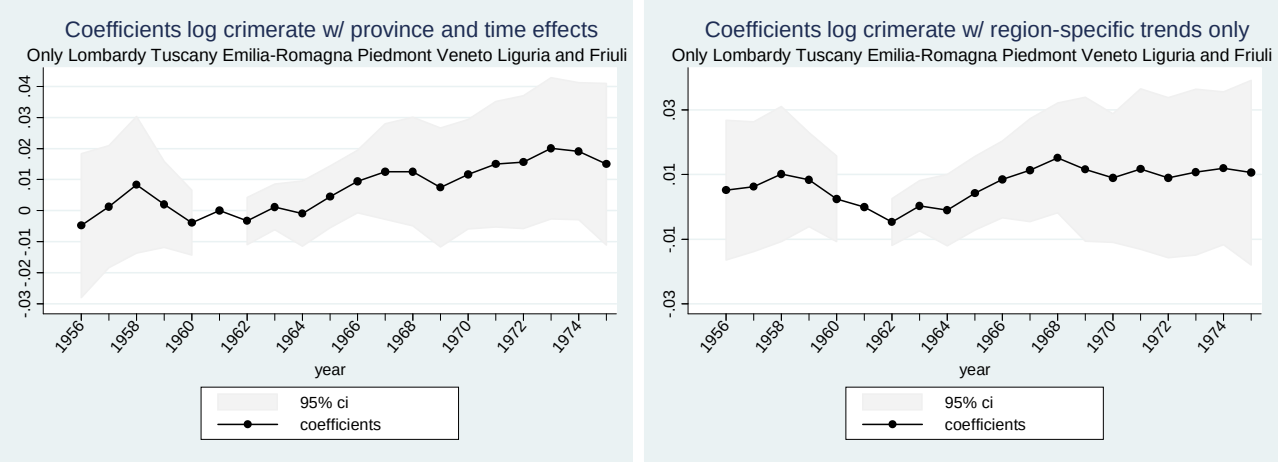

Figure 4: Estimates of β_τ from equation 2 for the restricted sample of Northern regions and Tuscany.
The plots show estimates of the coefficients β_τ for different specifications. The specification in the plot on the left controls for province and year fixed effects. The plot on the right shows coefficients from a specification that includes province and year fixed effects together with region specific time effects.

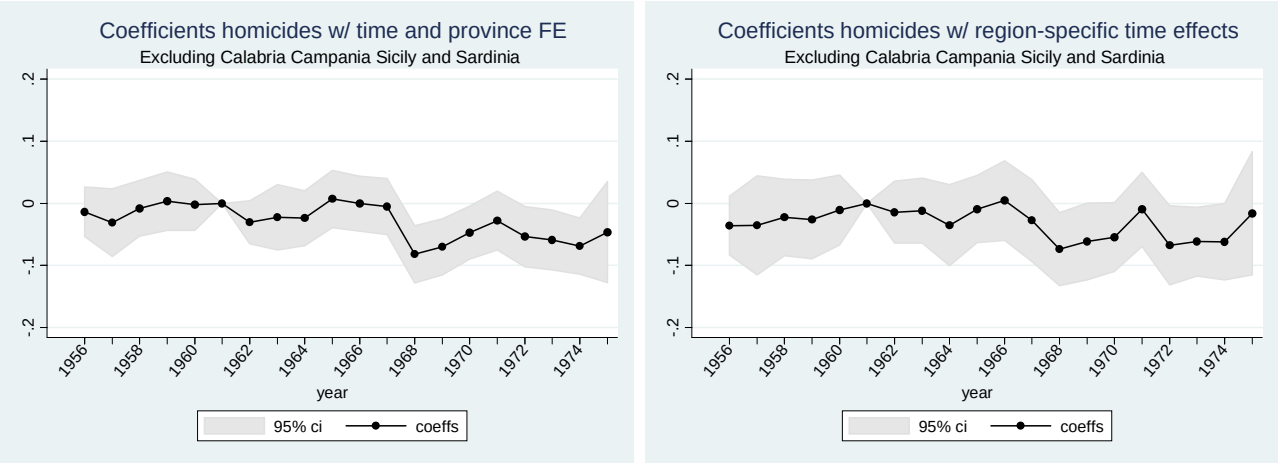

Figure 5: Coefficients for homicides. Full sample. The plot on the left shows the estimates from a specification that includes only time and province fixed effects. The plot on the right includes also region-specific time effects. The gray area in the plot corresponds to 95% confidence intervals.

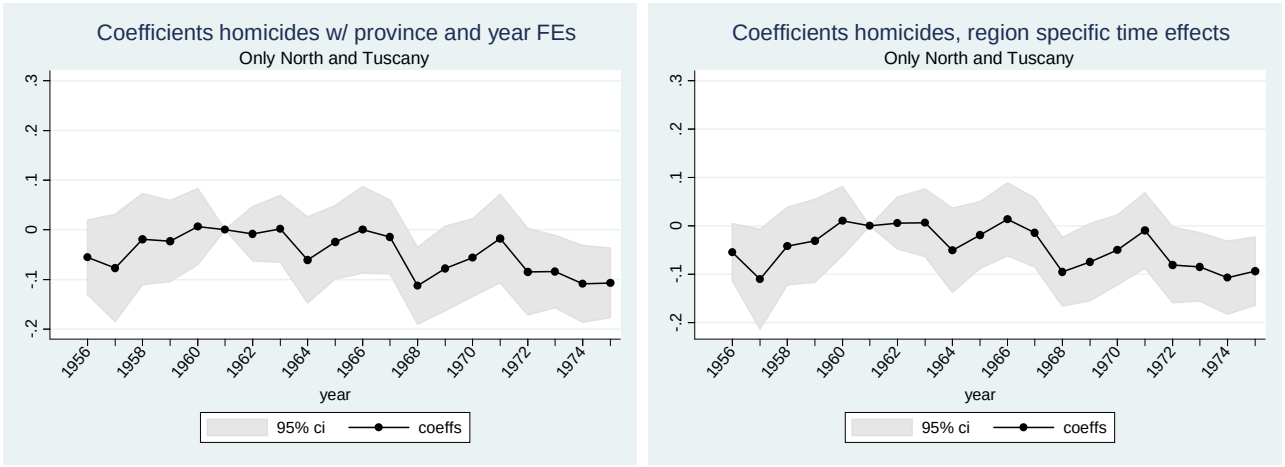

Figure 6: Results for homicides. Restricted sample: Lombardy, Piedmont, Liguria, Veneto, Emilia Romagna, Friuli Venezia Giulia and Tuscany. The plot on the left shows the estimates from a specification that includes only time and province fixed effects. The plot on the right includes also region-specific time effects. The gray area in the plot corresponds to 95% confidence intervals.

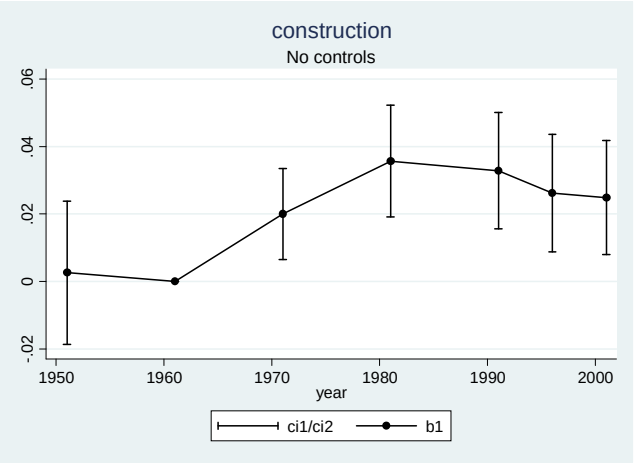

Figure 7: Estimates of equation 6. β_τ are differences in differences coefficients using 1961 as the baseline year and the coefficient β_{1951} checks for the presence of pre-trends. The vertical capped lines are 95% confidence intervals.

Appendix - Additional Robustness Checks

As discussed in the data section the data collection process changed dramatically in 1996. Thus I also estimate equation 3 with and without additional controls using only data up to 1991. The results are reported in table in the Appendix. The results without controlling for province-specific linear trends are very similar to the results in table 5 and table 3. However, when I include province specific linear trends the point estimates for construction are slightly smaller and not significant (the p-values vary across specifications from .055 to .09).

Table A.1

The Impact of Resettled Mafia Members on Employment by Sector
Sample Restricted to the Years 1951-1991

	(1) logemp	(2) logemp	(3) logemp	(4) logemp
		Panel A: Construction		
Post*Treatment	0.028***	0.030**	0.032***	0.035***
	(0.007)	(0.009)	(0.009)	(0.010)
		Panel B: Manufacturing		
Post*Treatment	0.024**	0.015	0.033***	0.025**
	(0.008)	(0.009)	(0.009)	(0.009)
		Panel C: Mining		
Post*Treatment	0.018	-0.030	0.026	-0.024
	(0.018)	(0.019)	(0.019)	(0.021)
		Panel D: Energy		
Post*Treatment	0.026**	0.020	0.029**	0.021
	(0.008)	(0.011)	(0.009)	(0.014)
		Panel E: Transportations		
Post*Treatment	0.006	0.008	0.013*	0.021*
	(0.006)	(0.008)	(0.005)	(0.008)
		Panel F: Commerce		
Post*Treatment	0.002	-0.005	0.013**	0.012**
	(0.005)	(0.004)	(0.005)	(0.004)
		Panel G: Credit		
Post*Treatment	0.010	0.003	0.019***	0.016*
	(0.006)	(0.007)	(0.005)	(0.007)
		Panel H: Other Services		
Post*Treatment	-0.012	-0.013	0.001	0.009
	(0.007)	(0.006)	(0.008)	(0.007)
Controls:				
Province & time FEs	Yes	Yes	Yes	Yes
Log(population)	No	No	Yes	Yes
Region-specific time effects	No	Yes	No	Yes
N	360	360	360	360
N_clust	72	72	72	72

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Differences in differences estimates on the restricted sample 1951-1991. The standard errors, reported in parentheses, are clustered at the province level.

Table A.2

The Impact of Resettled Mafia Members on Employment by Sector
Sample Restricted to the Years 1951-1991 and Province-Specific Linear Trends

	(1) logemp	(2) logemp	(3) logemp	(4) logemp
		Panel A: Construction		
Post*Treatment	0.014	0.017	0.013	0.014
	(0.009)	(0.010)	(0.009)	(0.010)
		Panel B: Manufacturing		
Post*Treatment	-0.002	-0.002	0.005	0.006
	(0.005)	(0.006)	(0.005)	(0.007)
		Panel C: Mining		
Post*Treatment	0.010	0.020	0.011	0.032
	(0.018)	(0.023)	(0.019)	(0.024)
		Panel D: Energy		
Post*Treatment	0.005	0.001	0.006	0.003
	(0.011)	(0.016)	(0.012)	(0.019)
		Panel E: Transportations		
Post*Treatment	-0.013	-0.014	-0.001	0.004
	(0.009)	(0.016)	(0.005)	(0.009)
		Panel F: Commerce		
Post*Treatment	-0.0001	-0.003	0.003	0.004
	(0.003)	(0.003)	(0.003)	(0.002)
		Panel G: Credit		
Post*Treatment	0.002	0.000	0.007	0.005
	(0.00354)	(0.005)	(0.004)	(0.005)
		Panel H: Other Services		
Post*Treatment	-0.006	-0.008	0.007	0.007
	(0.00505)	(0.007)	(0.004)	(0.005)
Controls:				
Province & time FEs	Yes	Yes	Yes	Yes
Province-specific linear trends	Yes	Yes	Yes	Yes
Log(population)	No	No	Yes	Yes
Region-specific time effects	No	Yes	No	Yes
N	350	350	350	350
N_clust	70	70	70	70

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Differences in differences estimates on the restricted sample 1951-1991 including province-specific linear trends. The standard errors, reported in parentheses, are clustered at the province level.