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When the mafia comes to town☆

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ABSTRACT

This paper investigates the diffusion of organized crime in new areas by examining a legal practice in effect in Italy between 1956 and 1988, namely the power for the authorities to force *mafiosi* to relocate to another town. Using the variation in the number of relocated mafia members according to destination provinces in a difference-in-differences setting, I find no conclusive evidence on crime and a very robust positive impact on employment in the construction industry. I show that the effect is driven primarily by provinces that had a low ex-ante level of financial development, suggesting that mafias take advantage of investment opportunities in the construction sector that are left unexploited due to liquidity constraints.

1. Introduction

Organized crime in Italy developed initially in the Southern regions of Sicily, Campania and Calabria. The presence of mafias in Northern and Central Italy and the other Southern Italian regions has long been denied or ignored, but there is now growing concern in Italy over the impact of the diffusion of mafias also outside their "traditional" territories (IPAC, 1994; Ciconte, 1998; Ciconte, 2010; Sciarrone, 1998; Varese, 2011): 29% of the 1708 mafia-linked firms seized by public authorities through 2011 were located outside those areas of origin.

The focus on non-traditional areas is of interest for at least two reasons. First, these areas typically have stronger institutions and better economic conditions, which mitigates problems of reverse causality relating to the endogenous formation of organized crime. Second, the role of organized crime may be very different in the two areas: for instance, a mafia may prioritize institutional control in its traditional areas and monetary gain elsewhere.

I estimate the effect of the arrival of members of crime organizations on the local economy by using a legislative provision in effect in Italy from 1956 to 1988. The law allowed the public authorities to forcibly relocate mafia members to another town in Italy. Using the variation in the number of relocated mafia members according to destination province, I estimate the impact of such relocation on the incidence of crime and homicides, and employment by industrial sector. I find no evidence of an effect on total crime rates or homicide rates, but there is a very robust positive effect on employment in the construction sector: one extra relocated *mafioso* per 100,000 inhabitants corresponds to an increase of about 3% in employment in construction. Anecdotal evidence suggests that the mafia preferred to limit recourse to violence in these non-traditional areas, so as not to draw the attention of the law enforcement bodies, and instead to invest the proceeds of the illegal activities conducted elsewhere.

Furthermore, construction is one of the sectors most heavily affected by mafia infiltration: 28% of the 1708 firms traceable to

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organized crime seized by public authorities are construction firms. The construction sector is ideal for laundering large amounts of money relatively easily. Real estate firms and monopolistic positions in the production of concrete and cement make investments in the construction sector appealing by assuring control over a network that goes from input and production to sale (Saviano, 2007; Sciarrone, 1998; Varese, 2011). Furthermore, a recent literature relates the mafia's interest in construction to politics (Daniele and Geys, 2015; Alesina et al., 2016; Buonanno et al., 2016). For instance, De Luca and De Feo (2017) study the impact of organized crime on electoral competition and show that the mafia obtained economic advantages in the construction sector in exchange for its electoral support.

This paper relates to several strands of the literature. A series of recent papers have inquired into the impact of organized crime on local economies. Pinotti (2015) studies the effect of the formation of the *Sacra Corona Unita*, SCU (an independent mafia organization) in the Puglia region on regional GDP. Using a synthetic control method, he estimates that the rise of organized crime is correlated with a 15% drop in per capita GDP over 30 years. Cook et al., 2013 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 the accumulation of human capital and on migration, with municipal-level panel data analysis focusing on the region of Calabria. They find that human capital accumulation is negatively affected by organized crime. Daniele and Marani (2011) show a negative and significant correlation between organized crime and foreign direct investment, suggesting that the presence of organized crime may deter economic growth. Gennaioli et al. (2011) document a positive impact of public spending on organized crime, taking the 1997 earthquake as a source of exogenous variation in public spending. Barone and Narciso (2013) instrument organized crime with rainfall in the 19th century and land productivity shifts and find that organized crime increases the amount of public funds going to enterprises in Sicily. The paper most closely related to the present study is Buonanno and Pazzona (2014). It looks at the impact on crime rates of migration flows from Southern to Northern provinces in Italy and the number of relocated mafia members; the conclusion is that the interaction between these two factors has indeed contributed to the diffusion of organized crime. My paper differs from Buonanno and Pazzona (2014) in several respects: in the identification strategy, which exploits the variation in the number of relocated mafia members in a difference-in-differences setting; in the period studied; and in estimating the impact not only on crime but also on 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 for India, showing that corruption, defined as the ability to buy drivers' licenses without taking the test, "greases the wheels" but can also generate efficiency costs. Olken and Barron (2009) test industrial organization models of corruption and show that the level of illegal payments is affected by market structure. Similarly, the presence of different mafia organizations can affect the way in which organized crime operates – for instance, the presence of different organizations can affect the level of protection payments (Gambetta, 1993) and the level of wealth-destroying violence (Rogers et al., 2013).

This paper also relates to the sociological literature on the origins of organized crime and the mechanisms of its diffusion. Gambetta (1993) argues that the essential business of the Sicilian Mafia is protection. It sells protection in areas where institutions are weak, or it enforces illegal contracts, since they cannot be enforced by legal institutions. For example, mafias could play a major role in enforcing cartels in markets where they are not otherwise easily enforceable. In this case the mafia would constitute a third party enforcer that can credibly inflict punishment for deviance. 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 research strategy and sets out the institutional details of the forced relocation program. Section 3 describes the data. Section 4 describes the empirical strategy and the results for crime rates and homicides; Section 5 for employment by industrial sector, with a discussion of caveats and possible alternative mechanisms for the result. Section 6 concludes.

2. The empirical strategy

Identifying the causal impact of organized crime on economic outcomes is particularly challenging. First, organized crime itself is hard to measure. The most common gauge in the literature combines reported crimes and mafia-related homicides, but a possible shortcoming is that crime might be systematically under-reported in areas where crime organizations are more powerful. Furthermore, crime reports are the equilibrium outcome of the interaction between enforcers and criminals. Under-reporting is less of a concern for homicides, but the pattern over time might be driven by mafia wars, which are exceptional events arguably related to periods of crisis in the organization. More generally, Buonanno et al. (2017) show that homicide follows very different patterns from other crimes, raising serious doubts about using it to proxy for crime in general. Second, there are issues of endogeneity: the diffusion of organized crime is potentially endogenous to local economic outcomes. Consider an attempt to analyze the effect of organized crime on competition in a given local market: the criminal organization may be attracted by high rents in markets with low competition, but Gambetta (1993) suggests, on the other hand, that as organized crime is in the business of private protection, highly competitive markets too might demand mafia services, as a means of cartel enforcement. In principle, economically depressed and flourishing areas alike could attract organized crime: the former by providing a fertile breeding ground for criminal activities, the latter by providing attractive opportunities for the investment of criminal proceeds and/or money laundering.

This paper approaches this question by taking advantage of a legal institution in effect in Italy between 1956 and 1988, namely "*soggiorno obbligato*" (forced relocation). The law, which empowered public authorities to force the relocation of a *mafioso* to a

town of their choice, has been widely considered to be one of the principal factors in the spread of organized crime beyond its traditional areas. The law was inspired by the idea that the *mafioso* would quit his criminal activities once he was removed from the broader criminal network. The idea was to isolate powerful ringleaders from other members in the Mafia by forcing them to relocate, on the assumption that organized crime is a byproduct of Southern Italian society and culture and could not develop in other areas.

However, the consensus view now is that forced relocation was not enough to prevent individual criminals from communicating with their original networks and instead helped them to expand those networks into new areas and to discover new business opportunities. The Italian Parliamentary Antimafia Commission wrote in 1994: "*In practice forced relocation, applied largely without careful choices and without appropriate guarantees of control, has dispersed a number of individuals belonging to mafias into many areas of Italy and has* ***implanted them in areas that would otherwise have probably been immune*** *…. [I]ndividuals gradually implanted themselves in the [new] 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* who turned state's witness, when asked about forced relocation: "*The policy of forced relocation 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 program

The relocation law, passed in 1956, established that "… In case of serious danger, the person can be forced to relocate to a specified town" for three to five years. The law was very vague about how the destination had to be chosen and who was to be responsible for the choice. More details were set out in a subsequent legal provision, Law 575/1965, which established a measure of forced relocation specific to known mafia members not necessarily convicted of any specific crime and added that "… Under exceptional public menace or danger for the concerned person, the *Questore* (Police Commissioner) or the National Antimafia Prosecutor or the state prosecutor can ask the court to order forced relocation to a town, decided by the *Questore*, that has appropriate territorial and safety characteristics …". In 1982 the law was amended, setting some restrictions on the characteristics of the destination town: "…forced relocation must be ordered in a town with no more than 5000 inhabitants, far away from metropolitan areas, so as to ensure effective control …". Forced relocation was abrogated, finally, by Laws 327/1988 and 256/1993.

The documents of the Italian Parliamentary Antimafia Commission (IPAC) contain additional details on the implementation of forced relocation. The Commission conducted an inquiry into the law's application in 1976, interrogating the President of the Court of Palermo to learn how the destination town was chosen. The President testified that the Italian Department of the Interior compiled a list of towns where *mafiosi* could be relocated and the local court picked the destination from that list, generally electing localities where the person could be more easily controlled. He also said that the list changed over time. Unfortunately, the document does not give the actual list, or any information on how it was compiled. One indication comes from the testimony of the head of the Italian Police in 1963: "We used to relocate people to the island of Ustica, but we had to stop because the local community opposed it. Failing to find another island to send them to, we sent them to several towns in Italy; we have a list of [eligible] towns" (IPAC, 1976–1982).

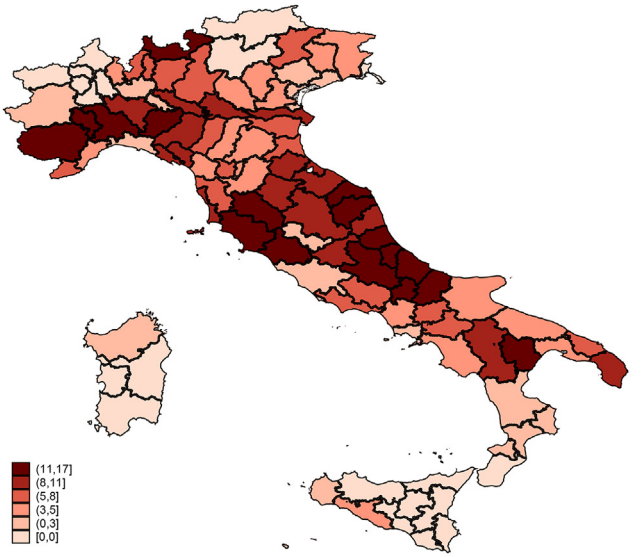
3. The data

3.1. Forced relocation, treatment definition and variation

This paper uses data on the total[1] number of individuals relocated to each province in the period 1961–1972 (IPAC, 1976–1982).[2] I define "treatment" as the number of *mafiosi* relocated 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 *mafiosi* for provinces of different sizes. Fig. 1 shows the geographical distribution of the treatment across the Italian provinces. Measurement error is a significant concern, in that a good measure of treatment intensity should factor in the relative importance of the relocated *mafioso* within his organization. There might also be corruption in the form of a *mafioso* being able to get relocated where he prefers. Other sources of selection might derive from the opposition of destination towns to the arrival of mafia members. Furthermore, forced relocation might be applied together with other security measures that limit the freedom of the relocated *mafioso*, thus reducing his ability to develop new networks locally. Here, however, my hypothesis is that the higher the per capita number of *mafiosi* relocated to a province, the greater the probability that they can establish connections there and also the greater the probability of their number including an important member of the organization. The choice of normalizing by the average population in the period 1961–1972 is not optimal if the more populous areas offer more opportunities for the mafia to expand. But as no flawless choice for the normalization of the treatment is available, population is adopted as the most immediate.

[1] Unfortunately, data on the yearly flow of relocated individuals by province is not available.

[2] The data on "forced relocation" are drawn from the "Relazione finale della Commissione Parlamentare d'inchiesta sul fenomeno della mafia in Sicilia" (IPAC, 1976). At page 289 of the aforementioned document a table reports the total number of people relocated between 1962 and 1972 by province of destination. One of the appendices to the main document (Volume IV tome XXII) reports a list of relocated people transmitted to the Committee by the Italian Ministry of the Interior on February 13th, 1974. It is unclear whether such list refers to the flow of people relocated in 1973 or to the stock of people relocated in 1973 and in previous years for which the measure of forced relocation is still in effect by the end of 1973. I thus decided not to add this information in order to avoid possible double-counting, i.e. counting twice a person relocated before 1973. All results are robust to the inclusion of this additional data source.



Notes: Provinces with higher per capita number of relocated *mafiosi* are filled with darker colors. The map is obtained by dividing the range of the variable in 6 equally sized bins.

Fig. 1. Map of the intensity of the treatment.

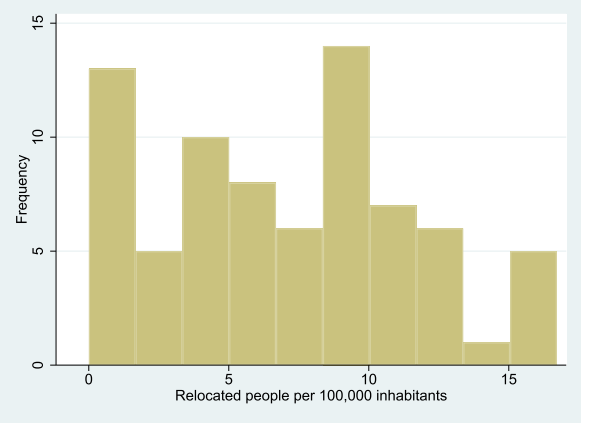


Fig. 2. Histogram of the number of people relocated per 100,000 inhabitants.

Fig. 2 plots the distribution of the number of *mafiosi* relocated per 100,000 inhabitants. The mean of this treatment variable is 7 and the standard deviation 4.6. The total number of *mafiosi* relocated in a province p in the period 1961–1972 has a mean of about 29 and a standard deviation of 17.4 (Table 1, Panel A). Italy has 110 provinces today, but the data is reorganized to reproduce the provincial boundaries as of 1954, with 92 provinces. I exclude provinces in the regions of Calabria, Campania and Sicily, lowering the final number of provinces considered to 75. The total number of individuals relocated between 1961 and 1972 is 2360; excluding those relocated in the traditional territories of organized crime, i.e., Calabria, Campania and Sicily, the number is 2165.

As mentioned above, there can be reasons to believe that the number of relocated people may be correlated to the characteristics of the destination provinces. Table 2 presents an informal test of this hypothesis: I estimate a regression of the treatment variable, i.e., the number of *mafiosi* relocated per 100,000 inhabitants, on population, incidence of crimes, homicides and migrations per 100,000 inhabitants in 1956, and onto the number of people employed in each sector per 100,000 inhabitants and per capita value added in 1951. Column (1) shows that the treatment variable is negatively correlated with crime, suggesting that destination provinces for forced relocation were chosen among those with lower incidence of crime. Forced relocation is also positively correlated with migration and negatively correlated with employment in manufacturing and commerce. However, column (2) shows that within-region variation in forced relocation is not correlated with crime rates, migration flows nor with employment in commerce. The negative correlation with employment in manufacturing is however robust to the inclusion of region fixed effects. I will thus include region-specific time effects and province-specific linear trends to account for different trends in the evolution of the outcomes across

Table 1
Summary statistics.

	Mean	SD	Min	Max	N
Panel A: *Forced Relocation data*					
Forced relocation	28.9	17.4	0	66	75
Forced relocation rate	7	4.6	0	16.7	75
Panel B: *Crime data*					
Crime rate	1991.7	1527.5	243.3	13,394.5	3073
Homicide rate	0.9	1.1	0	13.9	3073
Panel C: *Employment data*					
Construction	12,894.7	12,952.7	1082	99,437	525
Manufacturing	62,076.7	91,202.4	4593	822,950	525
Mining	797.8	1418.7	7	22,948	525
Energy	1590	2195.1	155	16,218	525
Transportations	10,117.9	16,153.5	825	141,598	525
Commerce	32,028.7	40,191.2	2769	328,015	525
Credit	4574.8	10,062.6	130	90,802	525
Other services	9853.206	18,708.94	342	216,163	525
Panel D: *Province characteristic*					
Population	565,486.1	594,671.7	97,558	3,998,560	3073
Immigration rate	5.9	0.8	4.1	8	2848
Bank branches rate in 1951	39.2	12.8	12.3	67.7	75
Value added per capita in 1951	0.7	0.6	0.1	3.8	75

Notes: rates are computed as the incidence per 100,000 inhabitants. Forced relocation data are drawn from IPAC (1976–1982) and refer to the period 1962–1972. Crime data, drawn from ISTAT, refer to the periods 1956–1975 and 1983–2003. Employment data are drawn from the Census of Industry and Services (ISTAT) and refer to the period 1951–2001 with decennial frequency, with the addition of 1996. The Institute for Research on Population and Social Policies (IRPPS) is the source for migration flows data. I use migration flows from 1956 to 1975 and from 1983 to 2000. The number of bank branches per 100,000 inhabitants is drawn from the Census of Industry and Services (ISTAT). Value added in 1951 is drawn from ISTAT.

provinces in the analysis that follows.

3.2. The dataset on crime

The crime dataset reports the total number of crimes and the number of homicides by province and year from 1956 to 1975 and from 1983 to 2003. The series for total crimes covers all the "Crimes reported by Public Security or Carabinieri" (ISTAT, 1956–1976). Unfortunately in my data, I cannot distinguish between types of crime: although a break-down of crimes by broad type is available in the Judicial Statistical Yearbooks for the period 1956–1967, the same is not true starting in 1968. Indeed, starting in 1968, the tables on "Crimes reported by Public Security or Carabinieri" by province only report the total number of crimes. At the same time another data source becomes available: "Crimes reported by all law enforcement authorities for which penal action has been undertaken". This data source distinguishes detailed types of crime, but it is not comparable with the one available until 1967. In order to avoid confounding effects, I limit my analysis to the total number of crimes, for which I am able to build a consistent panel dataset, and to the number of homicides, for which I combine the two sources, on the assumption that the difference between the two sources is less of a concern for homicides than other crimes. Panel B in Table 1 reports summary statistics for the number of crimes and homicides per 100,000 inhabitants.

3.3. The dataset on employment by sector

The data on employment by sector is extracted from the Census of Industry and Services, carried out every ten years from 1951 to 2001, plus a mid-term census in 1996. The employment data distinguishes eight broad sectors: construction, manufacturing, mining, energy, transportation, commerce, credit, and other services. The method of data collection has improved over time. Until 1991 it was based on the assumption of ignorance: the list of units to be included in the Census was compiled through the Census itself and the data collector executed a "door to door" analysis without knowing what he would find in the section he was assigned to. After collecting the questionnaires, a match was performed with existing information to make sure no unit was ignored in the process.

Starting with the mid-term Census in 1996 and in 2001 the collection method changes dramatically. The National Institute of Statistics (ISTAT) now has statistical archives on all firms, and the questionnaires are designed to update these archives. This transformation of data collection should allow better coverage of one-person firms and freelance workers. The impact of the change is unlikely to be related to the intensity of the treatment, and any common effect will be eliminated by including year fixed effects.

Panel C in Table 1 reports summary statistics for the number of people employed in each sector over the entire sample period.

Table 2
Forced relocation and province characteristics.

	(1)	(2)
	Dependent variable: Relocated *mafiosi* per 100,000 inhabitants	
Crimes	−0.010***	−0.004
	(0.003)	(0.003)
Homicides	−0.102	−0.186
	(0.595)	(0.420)
Population	−0.000	−0.000
	(0.000)	(0.000)
Immigration	0.005***	0.000
	(0.002)	(0.003)
Value added in 1951	−0.177	2.079
	(1.338)	(2.097)
Construction	0.001	−0.001
	(0.001)	(0.001)
Manufacturing	−0.000**	−0.001***
	(0.000)	(0.000)
Mining	−0.000	0.000
	(0.001)	(0.001)
Energy	−0.008	−0.006
	(0.008)	(0.006)
Credit	0.005	0.005
	(0.007)	(0.007)
Other services	0.005	−0.002
	(0.004)	(0.005)
Transportations	−0.001	0.001
	(0.001)	(0.001)
Commerce	−0.001*	−0.001
	(0.001)	(0.001)
Constant	16.954***	19.044***
	(1.813)	(2.552)
Region fixed effects	No	Yes
Observations	75	75

Notes: The dependent variable is the number of relocated people per 100,000 inhabitants. Value added is per capita. All other variables (apart from population) are in per 100,000 inhabitants. Population, crime and homicides are measured in 1956, while employment in each sector and value added are measured in 1951. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

4. The results

4.1. The econometric model and the results for local crime rates

The empirical strategy is difference-in-differences, with treatment intensity defined as the number of individuals relocated to a 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 taking the relocation program as an exogenous source of variation in the diffusion of organized crime. There could be corruption – a *mafioso* getting relocated where he himself prefers – or selection bias due to the opposition of the destination communities to the arrival of mafia members. Furthermore, forced relocation might be applied together with other security measures that limit the freedom of the relocated *mafioso*, restricting his ability to develop new networks locally. But this strategy allows me to control for any unobserved difference across provinces, so as long as selection is unrelated to the trends the identifying assumption is satisfied. Furthermore, I estimate specifications with region-specific time effects to account for any regional shock that may affect the outcomes and with province-specific linear trends, to account for differences in trends across provinces.

The equation estimated is

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

where $\log(crimerate)_{prt}$ is the natural logarithm of the total number of reported crimes per 100,000 inhabitants in province p, region r and year t; α_p are province fixed effects; N_{pr} is the total number of *mafiosi* relocated to province p in the period 1961–1972 per 100,000 inhabitants; λ_{rt} are region-specific year effects; and $Post_t$ is a dummy for $year \geq 1973$.

All the models are estimated using yearly data from 1956 to 1961 (pre-period) and 1973 to 1975 (post-period). This choice of pre- and post-period is based on two factors: i) before the 1960s forced relocation was rarely applied (IPAC, 1976), and ii) the available data refers to individuals relocated between 1961 and 1972.

Panel A of Table 3 shows the results for all provinces, excluding those in the regions of Campania, Calabria and Sicily (the "traditional" territories of organized crime). When controlling only for province and year fixed effects, the coefficient of the treatment is negative and non-significant. However, controlling for region-specific year effects, the coefficient of the interaction term becomes positive, though still not significant. Model (3) controls for the log of population, the log of migrants per 100,000 inhabitants and,

Table 3
The impact of relocated mafia members on crime rates.

	Dependent variable: log of crime rate				
	(1)	(2)	(3)	(4)	(5)
Panel A: Excluding Calabria, Campania and Sicily					
Post × Treatment	−0.001	0.006	0.007	0.005	−0.013
	(0.010)	(0.010)	(0.012)	(0.009)	(0.018)
log(population)			0.620	−0.048	−0.156
			(0.405)	(0.407)	(0.698)
log(immigration)			0.073	0.002	0.121***
			(0.063)	(0.080)	(0.044)
Observations	675	675	675	675	675
No. of provinces	75	75	75	75	75
Panel B: Northern Italy					
Post × Treatment	0.009	0.007	0.011	0.005	−0.024
	(0.011)	(0.013)	(0.012)	(0.012)	(0.029)
log(population)			0.160	−0.188	1.668
			(0.446)	(0.646)	(1.092)
log(immigration)			0.043	−0.030	0.066
			(0.083)	(0.091)	(0.053)
Observations	360	360	360	360	360
No. of provinces	40	40	40	40	40
Panel C: Excluding provinces bordering with Campania and Calabria					
Post × Treatment	0.006	0.004	0.015	0.004	−0.017
	(0.010)	(0.010)	(0.012)	(0.009)	(0.020)
log(population)			0.659	−0.021	0.193
			(0.465)	(0.444)	(0.888)
log(immigration)			0.086	−0.002	0.109**
			(0.066)	(0.081)	(0.044)
Observations	621	621	621	621	621
No. of provinces	69	69	69	69	69
Controls:					
Province and Year FEs	Yes	Yes	Yes	Yes	Yes
Region-specific year effects	No	Yes	No	Yes	No
Province-specific trends	No	No	No	No	Yes
VA in 1951 × Year FEs	No	Yes	Yes	Yes	Yes

Notes: The dependent variable is the natural logarithm of the total number of crimes per 100,000 inhabitants. log(*population*) is the natural logarithm of the population and log(*immigration*) is the natural logarithm of the number of migrants per 100,000 inhabitants. The reported coefficient (*Post* × *Treatment*) is the interaction term between a dummy that is equal to one after 1972 and zero otherwise and the number of relocated mafia members per 100,000 inhabitants. Panel A refers to the full sample excluding provinces from traditional areas. Panel B shows the estimates for the restricted sample of Northern provinces. Panel C refers to the full sample excluding provinces from Campania, Calabria and Sicily and provinces bordering with Campania and Calabria. Standard errors are clustered at the province level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

in order to make sure that the estimates are not driven by pre-existing differences in economic activity, I control for the interaction between value added per capita in 1951 and time dummies; the coefficient of interest is again very small and positive but not significant. Controlling for province-specific linear trends switches the sign of the coefficient from positive to negative, but again it is not significant. Overall the evidence on crime rates is not conclusive: the point estimates are small and not significant, and the standard errors are large.

Panel B of Table 3 shows the results for the sub-sample of provinces in Lombardy, Veneto, Emilia Romagna, Liguria, Piedmont, Valle d'Aosta, Trentino Alto Adige and Friuli (a grouping that corresponds to Northern Italy). The estimates are in line with those of Panel A. In Panel C I exclude the provinces directly bordering with mafia prone regions (Potenza, Matera, Foggia, Campobasso, Latina and Frosinone), as mafia activities there may be due to spillovers from traditional territories rather than forced relocation: results are unchanged.

The estimates reported in Table 3 may be imprecise due to the high volatility of crime rates. In order to reduce noise I estimate a cross-sectional model using the difference between average log crime rates after the treatment (1972–1975) and average log crime before the treatment (1956–1961) as dependent variable: results, reported in Table 4 are virtually unchanged.

Overall, the null hypothesis cannot be rejected, but this might be due to lack of power. The available data on crime does not distinguish by type of crime, so a possible pattern of more mafia-style crimes, such as kidnapping for ransom, extortion, drug trafficking etc., could be concealed within the small positive point estimate on total crime rates, and the lack of precision of the estimates might be due to the additional noise generated by considering all types of crimes.

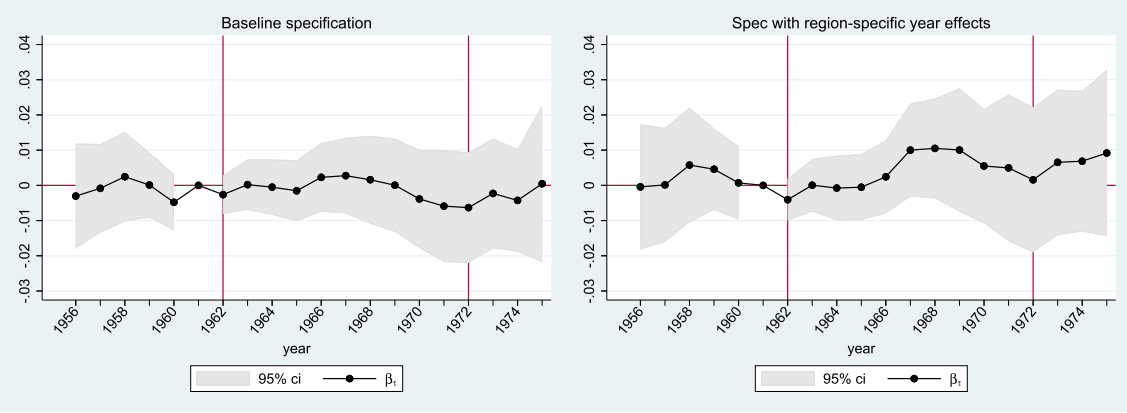
In order to look for pre-trends or changes in trends, the following model can be estimated:

$$\log(crimerate)_{prt} = \alpha_p + \lambda_{rt} + \sum_{\tau=1956}^{1960} \beta_\tau N_{pr} 1(t=\tau)_t + \sum_{\tau=1962}^{1975} \beta_\tau N_{pr} 1(t=\tau)_t + \epsilon_{prt} \quad (2)$$

Table 4
Changes in average crime rates.

	Dependent variable: Post-pre change in average log crime rate			
	(1)	(2)	(3)	(4)
Treatment	−0.001	0.006	0.006	0.004
	(0.009)	(0.009)	(0.012)	(0.009)
Δlog(population)			0.500	−0.131
			(0.425)	(0.429)
Δlog(immigration)			0.001	−0.036
			(0.121)	(0.129)
VA in 1951			0.085	−0.006
			(0.065)	(0.101)
Region fixed effects	No	Yes	No	Yes
Observations	75	75	75	75

Notes: The dependent variable is the difference between the average natural logarithm of the total number of crimes per 100,000 inhabitants in the period 1973–1975 and the average in the period 1956–1961. Δlog(*population*) is the difference in average log population in the two periods and Δlog(*immigration*) is the difference in average log migrants per 100,000 inhabitants in the two periods. The reported coefficient (*Treatment*) is the number of relocated mafia members per 100,000 inhabitants. Panel A refers to the full sample excluding provinces from traditional areas. Panel B shows the estimates for the restricted sample of Northern provinces. Panel C refers to the full sample excluding provinces from Campania, Calabria and Sicily and provinces bordering with Campania and Calabria. Standard errors are clustered at the province level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.



Notes: Estimates of β_τ from equation (2). The plots show estimates of the coefficients β_τ, obtained by regressing the log of provincial crime rate onto province and year fixed effects and a full set of year dummies, interacted with the number of relocated mafia members per 100,000 inhabitants. The estimates reported on the right are based on models that include region-specific time effects together with province effects.

Fig. 3. The impact of relocated mafia members on crime rates.

where all the variables are defined as above and $1(t = \tau)_t$ is an indicator for $year = \tau$.

The graphs in Fig. 3 report the coefficients from Eq. (2). There is no evidence of pre-trends, as until 1965 the coefficients are very close to zero. From 1966 on (year in which the mafia-specific forced relocation law becomes effective), the point estimates are larger, especially when I include region-specific time effects. However, the estimates are imprecise, so we lack conclusive evidence on whether there is any effect.

So far the analysis focused on short-term effects of forced relocation on crime rates. It is, however, possible that forced relocation had long-run effects on crime rates. In Table 5, I show the estimates obtained from a version of Eq. (1) where the number of forced relocations per 100,000 inhabitants is interacted with four indicator variables: an indicator for the period 1973–1975 (as in Table 3), one for 1983–1989, one for 1990–1996 and another one for the period 1997–2003. The evidence about long-run effects is mixed and non-conclusive, with the sign of the coefficient changing across specifications and time periods.

4.2. The results for homicides

The analysis for the incidence of homicides uses Poisson regressions with the population as an exposure variable. [3] As noted in the previous section, the data on homicides has a break in 1968, when another source becomes available. Year fixed effects would

[3] The Poisson specification is preferred to the log specification because some province-year cells have zero homicides.

Table 5
The long-run impact of relocated mafia members on crime rates.

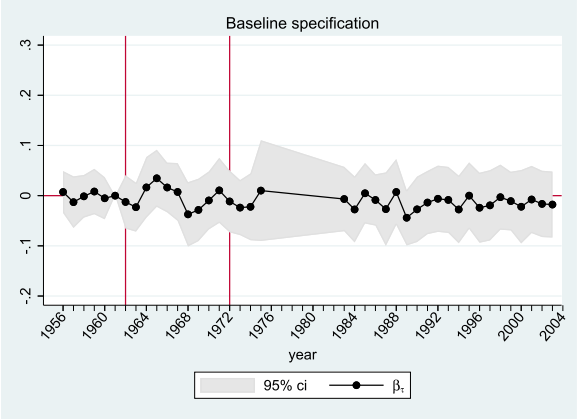
	Dependent variable: log of crime rate				
	(1)	(2)	(3)	(4)	(5)
1973–1975 × Treatment	−0.001	0.006	0.008	0.017	0.013
	(0.009)	(0.009)	(0.011)	(0.011)	(0.011)
1983–1989 × Treatment	−0.012	−0.012	−0.007	0.003	−0.001
	(0.009)	(0.009)	(0.011)	(0.009)	(0.014)
1990–1996 × Treatment	−0.019*	−0.010	−0.015	0.006	−0.008
	(0.010)	(0.010)	(0.011)	(0.008)	(0.015)
1997–2003 × Treatment	−0.009	0.003	−0.006	0.014**	0.000
	(0.010)	(0.006)	(0.012)	(0.007)	(0.016)
log(population)			0.545*	0.768***	0.927*
			(0.291)	(0.236)	(0.492)
log(immigration)			0.098**	0.001	0.080*
			(0.045)	(0.042)	(0.047)
Controls:					
Province and Year FEs	Yes	Yes	Yes	Yes	Yes
Region-specific year effects	No	Yes	No	Yes	No
Province-specific trends	No	No	No	No	Yes
VA in 1951 × Year FEs	No	Yes	Yes	Yes	Yes
Observations	2250	2250	2025	2025	2025
No. of provinces	75	75	75	75	75

Notes: The dependent variable is the natural logarithm of the total number of crimes per 100,000 inhabitants. log(*population*) is the natural logarithm of the population and log(*immigration*) is the natural logarithm of the number of migrants per 100,000 inhabitants. 1972–1975 × *Treatment* is the interaction term between a dummy that is equal to one between 1973 and 1975 and zero otherwise and the number of relocated mafia members per 100,000 inhabitants. The other interaction variables are defined in the same way. Standard errors are clustered at the province level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

solve this problem insofar as the change in the data source affected all provinces in the same way, but Fig. 4 suggests that this might not be the case. The graph plots the coefficients from a specification that controls only for province and year fixed effects. There is a large (although not significant) drop in the coefficient from 1967 to 1968. Given this problem, it is hard to interpret the estimates. The non-significant effects shown in the graph could be due to the reduction in the level of the coefficients from 1968 onward. One thing that is worth noticing is that until 1960 the pattern of coefficients shows no evidence of selection on trends.

5. The effect on employment in different sectors

As the introduction observes, mafia activities – both illegal and in legitimate businesses – can have an important economic role. However, the effect may well differ according to sector. I measure the effect of relocated mafia members on the economy using



Notes: Full sample. The gray area in the plot corresponds to 95% confidence intervals. The plot shows estimates of the coefficients β_τ, obtained by estimating Poisson regressions of the number of homicides onto province and year fixed effects and a full set of year dummies, excluding a dummy for 1961, interacted with the number of relocated mafia members per 100,000 inhabitants.

Fig. 4. Mafia relocation and homicides.

Table 6
The impact of relocated mafia members on employment by sector construction, manufacturing, mining and energy.

	Dependent variable: log of employment			
	(1)	(2)	(3)	(4)
Panel A: Construction				
Post × Treatment	0.020**	0.029***	0.029***	0.037***
	(0.009)	(0.010)	(0.009)	(0.011)
Pre × Treatment	−0.004	0.013	−0.006	0.011
	(0.014)	(0.013)	(0.018)	(0.019)
Panel B: Manufacturing				
Post × Treatment	0.027***	0.019*	0.038***	0.032***
	(0.010)	(0.010)	(0.010)	(0.011)
Pre × Treatment	−0.001	−0.005	0.000	0.002
	(0.006)	(0.007)	(0.007)	(0.009)
Panel C: Mining				
Post × Treatment	0.021	−0.030	0.029	−0.029
	(0.020)	(0.020)	(0.020)	(0.022)
Pre × Treatment	0.011	−0.032	0.012	−0.033
	(0.023)	(0.023)	(0.024)	(0.026)
Panel D: Energy				
Post × Treatment	0.021**	0.023*	0.026**	0.023*
	(0.009)	(0.012)	(0.010)	(0.013)
Pre × Treatment	0.008	0.006	0.014	0.012
	(0.012)	(0.010)	(0.016)	(0.011)
Controls:				
Log(population)	No	No	Yes	Yes
Region-specific time effects	No	Yes	No	Yes
Province & time FEs	Yes	Yes	Yes	Yes
Observations (1951–2001)	525	525	525	525
Observations (1951–1961)	150	150	150	150
No. of provinces	75	75	75	75

Notes: *Post* × *Treatment* is and estimate of β_{post}, the coefficient on the interaction term between a dummy equal to one from 1971 onwards and zero otherwise and the number of resettled mafia members per 100,000 inhabitants. *Pre* × *Treatment* is an estimate of β_{pre}, the coefficient on the interaction term between a dummy equal to one in 1961 and zero otherwise and the number of relocated mafia members per 100,000 inhabitants, obtained using only data from 1951 to 1961. 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. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

employment in different industries.

The main equation estimated is

$$\log(employment)_{prjt} = \alpha_{pj} + \lambda_{rjt} + \beta_{post,j} N_{pr} Post_t + \epsilon_{prjt} \tag{3}$$

The dependent variable is the natural logarithm of employment in province p, region r, sector j and year t; $Post_t$ is a dummy for $year \geq 1971$. [4] I estimate Eq. (3) for each sector using data every ten years from 1951 to 2001. As discussed above, the data collection process changed dramatically in 1996, but 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

$$\log(employment)_{prjt} = \alpha_{pj} + \lambda_{rtj} + \beta_{pre,j} N_{pr} \mathbf{1}(t = 1961)_t + \epsilon_{prjt} \tag{4}$$

The coefficients $\beta_{pre,\ j}$ show whether the rate of growth in employment in different sectors differs systematically between the treatment and control groups prior to the treatment. While this is not a direct test of the identifying assumption, given that differences in trends related to treatment status could arise at the same time as the treatment itself, the lack of any evidence of pre-trends is reassuring.

Table 6 shows the estimates of Eqs. (3) and (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}. The first column controls for province and year fixed effects, the second column 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,

[4] Identification in a levels specification would require the outcomes to grow by the same absolute amount over each ten-year interval, but this assumption seems unreasonable given the considerable variation in size across provinces.

Table 7
The impact of relocated mafia members on employment by sector transportations, commerce, credit and other services.

	Dependent variable: log of employment			
	(1)	(2)	(3)	(4)
Panel A: Transportations				
Post × Treatment	0.003	0.001	0.015**	0.018**
	(0.006)	(0.008)	(0.007)	(0.008)
Pre × Treatment	−0.001	−0.013	0.006	−0.002
	(0.006)	(0.010)	(0.005)	(0.007)
Panel B: Commerce				
Post × Treatment	0.001	−0.004	0.012**	0.015***
	(0.006)	(0.006)	(0.006)	(0.005)
Pre × Treatment	0.000	−0.005	0.005	0.002
	(0.004)	(0.004)	(0.004)	(0.003)
Panel C: Credit				
Post × Treatment	0.008	0.004	0.017**	0.019**
	(0.007)	(0.009)	(0.007)	(0.008)
Pre × Treatment	−0.001	0.001	0.001	0.002
	(0.004)	(0.005)	(0.005)	(0.006)
Panel D: Other Services				
Post × Treatment	−0.010	−0.012	0.003	0.010
	(0.008)	(0.008)	(0.009)	(0.009)
Pre × Treatment	−0.013**	−0.017***	−0.005	−0.005
	(0.006)	(0.006)	(0.007)	(0.006)
Controls:				
Log(population)	No	No	Yes	Yes
Region-specific time effects	No	Yes	No	Yes
Province and time FEs	Yes	Yes	Yes	Yes
Observations (1951–2001)	525	525	525	525
Observations (1951–1961)	150	150	150	150
No. of provinces	75	75	75	75

Notes: *Post* × *Treatment* is and estimate of β_{post}, the coefficient on the interaction term between a dummy equal to one from 1971 onwards and zero otherwise and the number of relocated mafia members per 100,000 inhabitants. *Pre* × *Treatment* is an estimate of β_{pre}, the coefficient on the interaction term between a dummy equal to one in 1961 and zero otherwise and the number of relocated mafia members per 100,000 inhabitants, obtained using only data from 1951 to 1961. 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. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

manufacturing and energy are positive and significant, while those of β_{pre} are small and not significant.

Table 7 shows the estimates of Eqs. (3) and (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}. The first column controls for province and year fixed effects, the second also for region-specific time effects; the third inserts 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. Omitting the control for log of population, the estimates of β_{post} are very small in magnitude and not significant, while the estimates of β_{pre} are negative and significant for "other services" and small and not significant otherwise and for all the other sectors.

To account for differences in trends across destination provinces, I estimate Eq. (3) including also province-specific linear trends. The results are shown in Table 8. Overall, the result for the construction industry is fairly robust while the results for other sectors change across specifications. As noted above, the data collection process changed drastically in 1996. Accordingly, I also estimate Eq. (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. Omitting controls for province-specific linear trends, the results are very similar to those reported in Tables 6 and 7. Including province-specific linear trends, the point estimates for construction are slightly smaller and not significant, but this specification is highly demanding, so lack of precision is to be expected.

To gauge the effect at different points in time, the following equation has been estimated for each sector:

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

Eq. (5) includes the interaction between the treatment N_p and time dummies for all the available years excluding 1961. Thus the coefficients β_τ are difference-in-differences coefficients using 1961 as the baseline year, and the coefficient β_{1951} checks for the presence of pre-trends. Fig. 5 shows the point estimates and 95% confidence intervals for the β coefficients in Eq. (5). 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; it is increasing until 1981 and decreasing thereafter. The magnitude in

Table 8
The impact of relocated mafia members on employment by sector controlling for province-specific linear trends.

	Dependent variable: log of employment			
	(1)	(2)	(3)	(4)
Panel A: Construction				
Post × Treatment	0.020**	0.027**	0.014*	0.020*
	(0.009)	(0.011)	(0.008)	(0.010)
Panel B: Manufacturing				
Post × Treatment	−0.003	−0.003	−0.000	0.007
	(0.006)	(0.009)	(0.006)	(0.009)
Panel C: Mining				
Post × Treatment	0.014	−0.004	0.016	0.009
	(0.017)	(0.027)	(0.016)	(0.029)
Panel D: Energy				
Post × Treatment	0.011	0.001	0.010	−0.001
	(0.011)	(0.018)	(0.011)	(0.020)
Panel E: Transportation				
Post × Treatment	0.000	0.007	0.005	0.009
	(0.006)	(0.011)	(0.007)	(0.009)
Panel F: Commerce				
Post × Treatment	−0.002	−0.005	−0.000	0.003
	(0.003)	(0.003)	(0.004)	(0.003)
Panel G: Credit				
Post × Treatment	0.001	−0.001	0.002	0.005
	(0.005)	(0.007)	(0.006)	(0.006)
Panel H: Other Services				
Post × Treatment	−0.009*	−0.011*	−0.003	0.004
	(0.004)	(0.007)	(0.004)	(0.006)
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
Observations	525	525	525	525
No. of provinces	75	75	75	75

Notes: *Post* × *Treatment* indicates the estimated coefficient on the interaction term between a dummy equal to one from 1971 onwards and zero otherwise and the number of relocated mafia members per 100,000 inhabitants. All columns include province-specific linear trends. In addition, 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. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

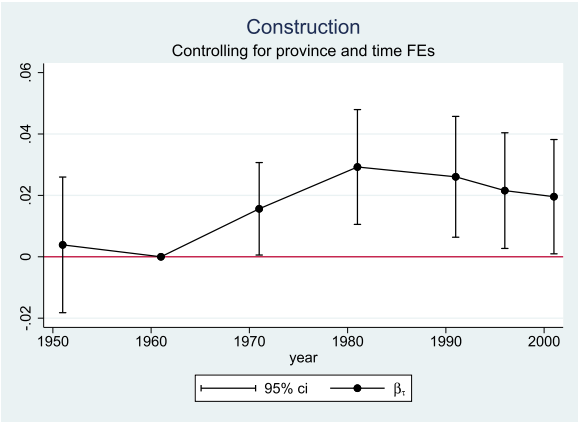
1971 suggests an average positive impact on employment in 1971 of about 2% for each additional mafia member (per 100,000 inhabitants). However, the magnitude is difficult to interpret given that I cannot rescale the coefficients by the first stage.

The next subsections discuss caveats to the identification strategy and possible mechanisms for the positive impact of relocated 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 1960s, in particular from 1958 to 1963, Italy experienced an unprecedented economic boom and was transformed from a poor, mainly rural nation into an industrial power. This growth came along with massive inter-regional migration, from rural areas in Southern Italy to industrial areas in the North. There was also movement within regions, from the countryside to the cities. Under the pressure of increasing demand for housing in the big cities, the real estate market experienced a major boom. As a consequence, there was massive property speculation.

Even though the economic boom and the construction boom affected the whole country, a possible concern is that high treatment could be systematically related to higher growth in the construction industry. Suppose that provinces with large rural areas were receiving more relocated mafia members per capita: these provinces might be experiencing higher growth in the building industry because there was greater movement from rural areas to central cities or simply because there was more room for expansion of construction. However, given that there is no evidence of differential trends related to the treatment in the period 1951–1961, for this mechanism to generate a spurious correlation it would have had to begin 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



Notes: Estimates of equation (5). β_τ are difference-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.

Fig. 5. The impact of relocated mafia members on employment in the construction industry.

Table 9
Robustness check I.

	(1) $\Delta\log(\text{employment})_{71,61}$	(2) $\Delta\log(\text{employment})_{81,61}$
Treatment	0.003	0.017*
	(0.008)	(0.009)
$\log(\text{employment})_{51}$	−0.185***	−0.178***
	(0.050)	(0.046)
Constant	1.742***	1.744***
	(0.470)	(0.417)
Observations	75	75

Notes: The dependent variable in column (1) is the change in log employment in construction between 1971 and 1961. The dependent variable in column (2) is the change in log employment in construction between 1981 and 1961. Treatment is the number of relocated mafia members per 100,000 inhabitants and $\log(employment)_{51}$ is the logarithm of employment in the construction sector in 1951. Robust standard errors in parentheses. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

of employment in 1961 causes OLS estimates to be inconsistent [5]; under the strong assumption of no serial correlation OLS gives consistent estimates for Eq. (6):

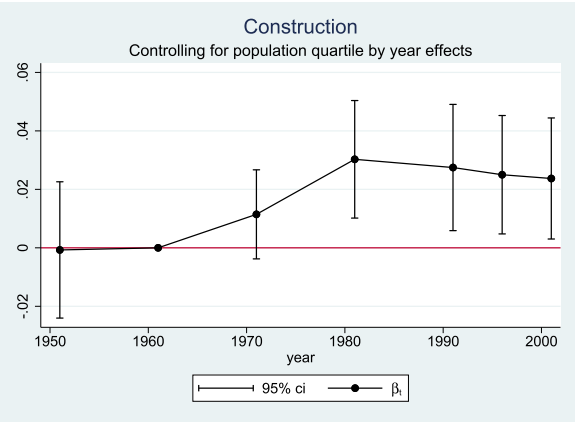
$$\Delta \log(employment)_{pt,61} = \alpha_t + \beta_t N_p + \delta_t \log(employment)_{pj,51} + \epsilon_{pt} \text{ for } t \in \{1971, 1981\} \quad (6)$$

Table 9 shows the estimate for Eq. (6). For $t = 1971$ the point estimate is very small and not significantly different from zero, but for $t = 1981$ there is still a positive effect, significant at the 5% level, of the same order of magnitude as the estimates shown in the previous section. The coefficient δ_t is negative and significant, suggesting the possibility of mean reversion. The drop in the point estimate for β_{1971} in Eq. (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 Fig. 5 mitigates the mean-reversion concern.

In order to make sure that the result on employment in construction is not driven by differential trends across provinces of different sizes, as an additional robustness check, Eq. (5) is estimated with a full set of year by population quartile effects. The results, shown in Fig. 6, are very similar to those shown in Fig. 5.

Another possible concern is that provinces hit by earthquakes during the sample period may bias the estimates on construction: in such provinces indeed the construction sector is likely to evolve on different trends. To the extent that, conditional on region fixed effects, earthquakes are exogenous, they should be orthogonal to within-region variation in forced relocation. That being true the specifications with region-specific time effects should take care of this concern. As a further robustness check, Panel B of Table 10 shows that the estimated effects of forced relocation on employment in the construction sector (reported in Panel A of Table 10) are robust to the exclusion of provinces hit by major earthquakes in the period 1943–1980 (Ascoli Piceno, Friuli Venezia Giulia, Perugia and Terni). In Panel C of Table 10, I exclude provinces bordering Campania and Calabria, so as to avoid spillovers from mafia prone regions to affect the results.

[5] The logarithm of employment in 1961 is a function of the error term in 1961, and so is the error term in Eq. (6).



Notes: Estimates of equation (5) augmented with population quartile by year effects. β_τ are difference-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.

Fig. 6. Robustness check II.

Table 10
Robustness check III.

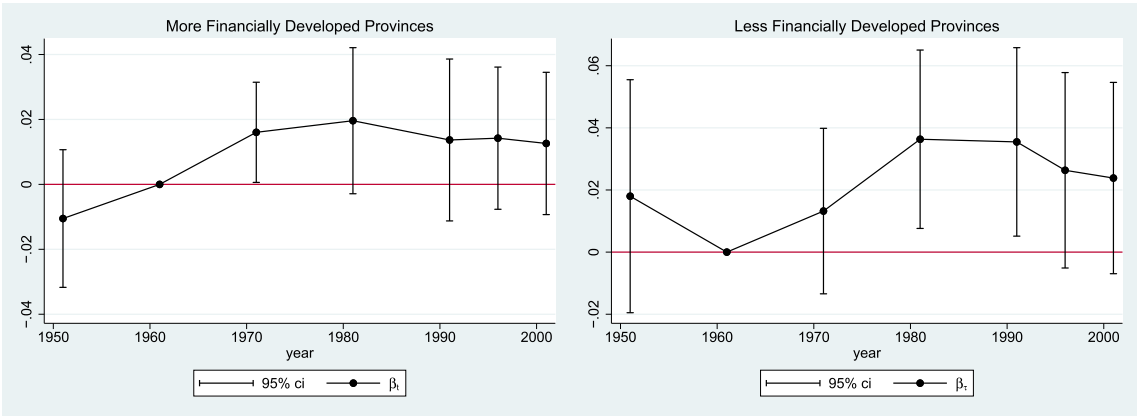
	Dependent variable: log of employment in Construction				
	(1)	(2)	(3)	(4)	(5)
Panel A: Full sample					
Post × Treatment	0.020**	0.029***	0.029***	0.037***	0.020*
	(0.009)	(0.010)	(0.009)	(0.011)	(0.010)
Observations	525	525	525	525	525
No. of provinces	75	75	75	75	75
Panel B: Excluding areas hit by major earthquakes					
Post × Treatment	0.015*	0.024**	0.023**	0.031***	0.020*
	(0.009)	(0.010)	(0.009)	(0.011)	(0.011)
Observations	483	483	483	483	483
No. of provinces	69	69	69	69	69
Panel C: Excluding provinces bordering with Campania and Calabria					
Post × Treatment	0.019*	0.027**	0.027**	0.036***	0.014
	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)
Observations	483	483	483	483	483
No. of provinces	69	69	69	69	69
Controls:					
Province & time FEs	Yes	Yes	Yes	Yes	Yes
Log(population)	No	No	Yes	Yes	Yes
Region-specific time effects	No	Yes	No	Yes	Yes
Province-specific linear trends	No	No	No	No	Yes

Notes: The dependent variable is the log employment in construction. Standard errors are clustered at the province level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

5.2. Mechanisms

Construction is universally considered to be one of the economic sectors most affected by organized crime (Saviano, 2007; Sciarrone, 1998; Varese, 2011): 27.11% of the 1516 firms seized by law enforcement bodies in Italy through 2011 were in the construction industry. The broader sector of retail and wholesale trade, housing and vehicle repair accounts for 27.84% of the total. The share of hotels and restaurants was 10.03%, that of real estate firms 8.97%. The authorities seized 10,438 properties, 2639 of which were in "non-traditional" areas (ANBSC, 2012). The construction sector can launder large amounts of money relatively easily. Real estate firms and monopolistic positions in the production of concrete and cement make investments in the construction sector profitable by assuring control over a network running from input and production to final sale.

There are several possible channels that can explain the positive effect of mafia relocation on the construction sector. First, *mafiosi* could be investing the proceeds of illegal businesses in construction, which per se does not necessarily generate inefficiencies: for example, there might be a strictly increasing return to investment in the construction industry and the mafia might invest in the industry without otherwise affecting the working of the market, or the investment might even serve to relax liquidity constraints that



Notes: The plot on the left shows the estimates for the sub-sample of more financially developed provinces, whereas the plot on the right shows the estimates for the subsample of less financially developed provinces. Provinces are split in more and less financially developed based on their per capita number of banks' branches in 1951, respectively at or above and below the median. β_τ are difference-in-differences coefficients using 1961 as the baseline year. The vertical capped lines are 95% confidence intervals.

Fig. 7. The impact of relocated mafia members and financial development.

would otherwise limit the growth of the sector. In order to test this hypothesis as a possible channel behind the results reported above, I explore heterogeneity in the impact of forced relocation on employment in the construction sector across different levels of ex-ante financial development. I estimate equation (5) separately for provinces where the number of bank branches per capita is below and above the median in 1951.[6,7] Even though interest rates were low through the boom, access to credit might still have been limited by collateral constraints or other frictions. In such a situation, the mafia could have been acting as liquidity provider. Consistently with this hypothesis, I find that the impact of forced relocation is greater in provinces with low level of financial development. The difference between the two coefficients is not statistically significant, but Fig. 7, which shows the dynamic impact of forced relocation for the two groups of provinces, highlights significant differences in the pre-trends between the two groups: among the provinces that were more highly developed financially, those that received a higher per capita number of relocated *mafiosi* appeared to experience higher employment growth in the construction sector even before the treatment, whereas among the less financially developed provinces the opposite holds. This evidence favors the interpretation that less financially developed provinces are more strongly affected by forced relocation.

Anecdotal evidence suggests that organized crime does not simply invest the proceeds of its illegal activities and that it can potentially influence competition and/or generate corruption. However, whether or not this generates inefficiencies is still an open question. If corruption in order to obtain building permits is just a way of eliminating frictions, then there is no efficiency cost and the undesirability comes only from the disutility of corruption and the strengthening of the mafia itself. But 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, then corruption does indeed have an efficiency cost.

Anecdotal evidence from the traditional mafia territories lends support to the presence of this channel. Salvatore Lima, Mayor of Palermo from 1958 to 1963 (killed by the Mafia in 1992) is considered to have been responsible for what is known as the Sack of Palermo, a dramatic urbanization of the territory that changed the face of the city. He was also accused of having favored Mafia-linked firms. Tano Badalamenti, a Mafia chieftain relocated in the early 1970s, was convicted of bribing officials to have the airport built near his hometown so that his construction firms could share in the building work.

Another way in which mafias could cause construction growth is by diverting public funds from other, socially more beneficial investments to infrastructures and buildings. This would amount to the mafia generating its own demand in construction, not only directly via the diversion of public funds but also via a multiplier mechanism: the presence of infrastructures in previously rural areas increases the value of the land and makes it profitable to build there. The mafias might also invest more in construction because of lower marginal costs thanks to illegal practices. There is also growing concern about the fact that mafia-linked firms use low-quality cement and do not respect earthquake-proof regulations (IPAC, 1994).

6. Conclusions

This paper studies the social and economic effects of the diffusion of organized crime in so-called "non-traditional" territories in Italy. There are several challenges in dealing with this issue. First, it is difficult devising a measure of organized crime. Second,

[6] The data on banks' branches in 1951 is obtained from the Census of Industry and Services.

[7] Notice that there is no significant difference in the treatment variable between the two groups.

organized crime is endogenous to economic and social outcomes. Further, the direction of the potential bias is unclear. If you want to analyze the effect of organized crime on competition in a given local market, you need 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 too might demand mafia-style services to enforce cartels. In principle, both economically depressed and flourishing areas can attract organized crime, the former as a fertile breeding ground for criminal activities, the latter thanks to attractive opportunities to invest the proceeds of crime and the possibility of money laundering.

To resolve these issues, I use the provision for forced relocation of *mafiosi* that was in effect in Italy from 1956 to 1988. Using the provincial variation in the number of relocated mafia members in the period 1961–1972 in a difference-in-differences setting, I have estimated the impact of this exposure on provincial crime rates, and employment in different industrial sectors.

No conclusive evidence concerning overall crime rates emerges. The point estimates for crime rates are not significant, but there is not enough power to draw clear conclusions. The lack of precision in the estimates could reflect the lack of distinction according to type of crime. Mafias are known to foster some particular types of crime, such as kidnapping, extortion, drug trafficking, etc. But they could have a zero or even a negative impact on other types of crime, say by obliging some criminals to reduce their activity or persuading them to join the criminal organization and redirect their efforts to other, more profitable crimes. 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 is impossible to determine the impact of the arrival of mafia members in these territories on homicides unless better data becomes available.

Motivated by the importance that analysts ascrive 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 service sectors. There is evidence of a positive impact on several sectors, the most robust being that on employment in construction. This result is evidence of the importance of the activities of organized crime in local economies, and in the construction sector in particular. The channels at play and the implications for economic efficiency can differ radically. I have shown that the effect is driven chiefly by provinces with low ex-ante levels of financial development, which suggests that the mafia seizes construction investment opportunities that are otherwise left unexploited because of liquidity constraints. Nonetheless there can be several sources of inefficiencies related to the entry of mafia-linked firms in the construction sector: among other, reduction in competition, diversion of public funds toward construction of useless infrastructures, and building more than socially optimal.

Appendix. Additional robustness checks

As the data section notes, the data collection process changed dramatically in 1996. Accordingly, it is worth estimating Eq. (3) with and without additional controls using only data up to 1991. The results without controlling for province-specific linear trends are very similar to the results in Tables 6 and 7 However, when province-specific linear trends are included the point estimates for construction are slightly smaller and not significant.

Table A.1
The impact of relocated mafia members on employment by sector sample restricted to the years 1951–1991.

	Dependent variable: log of employment			
	(1)	(2)	(3)	(4)
Panel A: Construction				
Post × Treatment	0.022**	0.030***	0.026**	0.034***
	(0.009)	(0.010)	(0.010)	(0.011)
Panel B: Manufacturing				
Post × Treatment	0.020**	0.014	0.029***	0.025**
	(0.009)	(0.010)	(0.010)	(0.011)
Panel C: Mining				
Post × Treatment	0.019	−0.029	0.026	−0.027
	(0.019)	(0.021)	(0.020)	(0.024)
Panel D: Energy				
Post × Treatment	0.020**	0.019	0.023**	0.019
	(0.009)	(0.013)	(0.011)	(0.015)
Panel E: Transportation				
Post × Treatment	0.007	0.006	0.013**	0.019*
	(0.006)	(0.010)	(0.006)	(0.009)
Post × Treatment	0.007	0.003	0.016**	0.016**
	(0.006)	(0.008)	(0.006)	(0.008)

(*continued on next page*)

Table A.1 *(continued)*

	Dependent variable: log of employment			
	(1)	(2)	(3)	(4)
Panel H: Other Services				
Post × Treatment	−0.011	−0.012*	0.002	0.008
	(0.007)	(0.007)	(0.008)	(0.008)
Controls:				
Province & amp; time FEs	Yes	Yes	Yes	Yes
Log(population)	No	No	Yes	Yes
Region-specific time effects	No	Yes	No	Yes
Observations	375	375	375	375
No. of provinces	75	75	75	75

Notes: Difference-in-differences estimates on the restricted sample 1951–1991. The standard errors, reported in parentheses, are clustered at the province level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.2
The impact of relocated mafia members on employment by sector sample restricted to the years 1951–1991 and province-specific linear trends.

	Dependent variable: log of employment			
	(1)	(2)	(3)	(4)
Panel A: Construction				
Post × Treatment	0.013	0.017	0.012	0.014
	(0.010)	(0.012)	(0.011)	(0.012)
Panel B: Manufacturing				
Post × Treatment	−0.003	−0.002	0.004	0.006
	(0.006)	(0.008)	(0.006)	(0.008)
Panel C: Mining				
Post × Treatment	0.013	0.016	0.014	0.028
	(0.019)	(0.028)	(0.020)	(0.030)
Panel D: Energy				
Post × Treatment	0.005	−0.000	0.006	0.001
	(0.011)	(0.019)	(0.012)	(0.022)
Panel E: Transportation				
Post × Treatment	−0.016*	−0.010	−0.006	0.008
	(0.010)	(0.019)	(0.008)	(0.011)
Panel F: Commerce				
Post × Treatment	−0.001	−0.003	0.001	0.003
	(0.003)	(0.003)	(0.003)	(0.003)
Panel G: Credit				
Post × Treatment	0.001	−0.000	0.005	0.005
	(0.004)	(0.006)	(0.004)	(0.006)
Panel H: Other Services				
Post × Treatment	−0.004	−0.008	0.008*	0.007
	(0.005)	(0.009)	(0.004)	(0.006)
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
Observations	375	375	375	375
No. of provinces	75	75	75	75

Notes: Difference-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. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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