

# When Migrants Mobilize against Labor Exploitation: Evidence from the Italian Farmlands

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*Migrant labor exploitation is widespread in developed countries, which host growing populations of undocumented migrants. While denouncing by migrants is essential to prosecute exploitative employers, an undocumented community actively hiding from the state is unlikely to whistleblow. I consider an intervention providing migrant farmworkers in Italy information and incentives to report on their racketeers. I leverage the staggered rollout of the intervention to study its effects in a difference-in-differences framework. The intervention empowered migrants to whistleblow, increased the prosecution of criminal organizations responsible for racketeering migrants, and raised awareness among natives, who became more favorable toward immigration and parties supporting it. These findings highlight the conditions under which undocumented migrants can take political action for their socioeconomic advancement. Unlike other integration policies which have been shown to backlash, highlighting migrants' vulnerability to exploitation might foster solidarity and more liberal immigration attitudes among natives.*

## INTRODUCTION

As conflict, poverty, and climate change continue to put people on the move, the question of how to integrate an expanding population of migrants in advanced economies has become of primary importance. While there is a debate about the determinants of successful legal migrant integration (Dancygier and Laitin 2014), a sizeable and growing fraction of immigrants remains undocumented and vulnerable to exploitative labor practices. According to the International Labour Organization,<sup>1</sup> 27 million people worldwide are victims of forced labor, most of whom are employed in Asia and Europe.

Migrant labor exploitation is not only problematic from a global human rights perspective, but it is also concerning for the economy and the rule of law of destination countries. Illegal labor can change the structure of labor markets, giving a competitive edge to firms willing to hire illegal workers for lower wages. It implies a loss of fiscal revenue for the state. Finally, when migrants are smuggled and controlled by criminal intermediaries, this phenomenon also reinforces criminal organizations. However, policy initiatives against labor exploitation have remained limited, in part because cracking down on illegal labor is difficult if migrants do not report on their racketeers.

In this article, I study the effects of an intervention designed to fight labor exploitation by explicitly targeting informal migrants and seeking to empower them to report on their exploitative employers. The intervention took place in Italy, one of the main hotspots of migrant arrival in Europe and a setting where illegal labor is often controlled by criminal organizations (Parliamentary Commission 2017, 52).

Starting in 2007, one of the main Italian unions of agricultural workers launched a campaign of in-person canvassing to provide migrants employed in agriculture with information about their rights, legal assistance to denounce their racketeers, and incentives to do so. Under Italian law, as in other countries, undocumented migrants can obtain a humanitarian residence permit if they are recognized as victims of exploitation. I use the staggered roll-out of this intervention to study its effects on (i) news reporting about labor racketeering, a measure of whether the intervention empowered migrants to denounce their exploitation, (ii) prosecution of criminal groups involved in labor racketeering, and (iii) public opinion.

Theoretically, it is unclear whether empowering a marginalized community that is trying to hide from the state can be successful. Undocumented workers who speak up against labor exploitation might risk unemployment, retaliation by criminal groups, and deportation. On the other hand, remaining in exploitative labor conditions is also costly, and migrants might decide to act if the risks they would incur when reporting were mitigated. I argue that a combination of information highlighting the mismatch between what migrants experience and what they could attain, and incentives to report tied to the hope of obtaining legal status can empower undocumented migrants to take the costly action necessary to denounce their

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<sup>1</sup> For more details, see <https://www.ilo.org/topics-and-sectors/forced-labour-modern-slavery-and-trafficking-persons>.

exploitative employers and improve their socioeconomic condition.

I leverage the staggered roll-out of the intervention across municipalities to study its effects in a difference-in-differences (DiD) framework. I provide evidence consistent with the validity of the DiD assumptions by showing that, first, treatment timing is uncorrelated with reports on the severity of exploitation produced by the union itself, a finding confirmed by the union leader in interviews I conducted; second, trends in outcomes before treatment are parallel; third, results are consistent across the full sample, a matched sample based on propensity score matching, a synthetic DiD approach, and using alternative estimators accounting for treatment effects heterogeneity bias.

One challenge with studying migrant labor exploitation relates to data limitations. The phenomenon is illegal, hard to detect, and its detection might correlate with the capacity of racketeers to hide their activity. I circumvent these problems by scraping news related to labor racketeering from seven news outlets of varying political orientation and by validating this measure with province-level data on unannounced audits to agricultural firms by the Labor Inspectorate. The measure is robust against reporting biases inherent in news-based indicators. Analyses also account for time-invariant differences in news coverage within locations, addressing factors such as population size, xenophobia, and political activity that may influence news coverage.

I present four main findings. The first directly relates to migrants' political activation: the intervention increased reporting about labor racketeering in the news. The increase is driven by articles discussing cases of migrants whistleblowing and mobilizing in public protests. Evidence indicates that the migrants themselves, rather than the union, are driving these effects. First, news did not cover the intervention itself, and mentions of unions are balanced across the treated and control group. Second, in places where unions limited themselves to collecting information on exploitation without conducting interventions with the migrants, there was no effect on labor racketeering news. This indicates that the campaign was effective at empowering migrants to whistleblow their condition of exploitation.

The second finding relates to the role of the state: the intervention led to a substantial increase in the number of properties seized from organized crime. This finding ties the role of migrants in denouncing exploitation to the role of criminal groups in controlling the system of labor exploitation. When workers denounce their employers, the state has leverage to identify and prosecute criminals and can crack down on their properties.

Third, the intervention increased state and civil society mobilization against labor racketeering: not only do news items reveal the emergence of civil society initiatives in treated municipalities, but public administrations also redistribute mafia-seized properties for public use at higher rates. The increase is not driven by the higher rates at which goods are seized, and is driven by assignment of previously mafia-owned properties to become agricultural cooperatives destined to social use, which are the main method to offer

nonexploitative working conditions to migrants employed in agriculture.

Finally, I consider the effects of this intervention on public opinion and on voting behavior of natives leveraging both panel surveys and election results at the municipal level. I document that the same respondent became 9% less likely to express sentiments of distrust toward migrants after the intervention took place in their municipality. Consistently, I show that the intervention increased the vote share for pro-integration far-left parties by 2 percentage points among survey respondents, and by 1 percentage point considering voting in the entire municipality. This gain is driven by a drop in votes for the main center-left party, which in the years of the study had adopted strong actions against the influx of new migrants, including pushing asylum seekers back to Libya before they could enter Italian waters (The Guardian 2017).

This study makes three theoretical contributions. First, while understanding the determinants of migrant integration is increasingly important, most of our knowledge is based on legal migrants, refugees, and asylum seekers (Dancygier and Laitin 2014). This article instead focuses mainly on undocumented migrants. The successful political and economic integration of undocumented migrants is likely determined by different factors than those applying to legal migrants. For economic integration, learning the language and high education increase legal migrants' chances of finding employment, but they might be insufficient for migrants who cannot access labor markets legally. Factors important for migrants' political integration, such as socioeconomic resources and networks, might not contribute to the activation of a population lacking political franchise and risking deportation if they make themselves visible to the state. Studies of undocumented migration are rare and often limited to the United States, where scholars have studied the effect of sanctuary policies on undocumented migrants' health outcomes (Hainmueller et al. 2017; Swartz et al. 2019) and crime rates (Hausman 2020).<sup>2</sup> A small set of studies has focused on undocumented migrants' activation using ethnographic methods to investigate migrants' mobilization initiatives (Cremaschi 2020; Delgado 1993; Omizzolo 2019). Most questions related to undocumented migrant workers remain unaddressed. This article starts filling this gap by studying the conditions under which undocumented migrants can gain voice and adopt the costly actions necessary to emancipate from exploitative regimes.

Second, this study considers a case in which migrants are active political agents of change. Previous studies have either considered the impact of interventions and events, such as migrants arrival or economic shocks, on the opinions of natives (Adida, Lo, and Platas 2018; Dancygier and Donnelly 2013; Emeriau 2024; Zhou, Grossman, and Ge 2023) or have considered how

<sup>2</sup> Related studies examine the determinants (Emeriau 2023) and effects of being granted legal status (Bahar, Ibáñez, and Rozo 2021; Comino, Mastrobuoni, and Nicolò 2020; Fasani 2018; Pinotti 2017).

migrants are impacted by policies granting or revoking rights, such as the right to access the country, citizenship, and clothing regulations (Abdelgadir and Fouka 2020; Hainmueller, Hangartner, and Pietrantonio, 2017; Masterson and Yassenov 2021).

Here, I consider a case in which, rather than receiving a concession from the state, migrants take costly actions to denounce racketeers to the authorities, seek state protection, and apply for the permission to work legally. This distinction likely matters for public opinion: while interventions favoring migrants' integration tend to backlash (Zonszein and Grossman 2022), an intervention highlighting migrants' vulnerability might increase the sympathy of natives toward migrants and trigger more acceptance of immigration, as shown in Bansak, Hainmueller, and Hangartner (2016), Bonilla and Mo (2018), and Alrababa'h et al. (2021). In this respect, this study represents a novel contribution to the literature on the determinants of support for migrant integration by showing that, not only natives' self-reported attitudes and willingness to accept migrants, but also their voting behavior is responsive to migrants' vulnerability.

Finally, this article contributes to the study of the unintended effects of immigration policies, particularly studies considering spillovers from immigration to organized crime. Previous work has documented how large migration influxes combined with restrictive immigration regimes and tight labor markets can generate profit opportunities for criminal organizations (Dipoppa 2024a; Luca and Proietti 2022). In line with the idea that criminal groups gain strength by exploiting migrants, this article shows that when migrants are empowered and defy exploitation, organized crime is undermined.

## THEORETICAL FRAMEWORK

### Migrant Political and Economic Integration

Political and economic participation are two fundamental dimensions of migrants' successful integration in host societies (Harder et al. 2018). Previous studies identified factors important for migrants' political integration, ranging from socioeconomic resources and socialization (Cho 1999), networks with co-ethnics (Bratsberg et al. 2021), and political opportunities created by party organization (Dancygier et al. 2015; Pons and Liegey 2019). Networks play an important role also for economic integration, together with language training and job matching technologies (Bansak et al. 2018).

This knowledge was built focusing on legal migrants and refugees, who eventually gain rights to participate in the labor market and politics. Less is known about undocumented migrants, whose networks, opportunities, and socioeconomic resources may not suffice for political and economic integration. In fact, a consistent result in this literature is that obtaining legal status significantly boosts migrants' political participation (Ferwerda, Finseraas, and Bergh 2020; Hainmueller,

Hangartner, and Pietrantonio 2015), and employment (Hainmueller and Hangartner 2019), and even deferring deportations improves undocumented workers' labor market participation (Amuedo-Dorantes and Antman 2022; Borjas and Cassidy 2019). Evidence underscoring the role of legal status for integration suggests that our knowledge on regular migrants' integration might not travel to those who are undocumented.

A handful of ethnographic studies have focused specifically on the political organization of undocumented migrants. Of particular relevance to this study is the work by Omizzolo (2019), who documented the extreme exploitation faced by Punjabi agricultural workers in the Agropontino plain in Italy, became an activist for their rights and participated in coordinating their efforts to mobilize to call attention on their situation.<sup>3</sup> Similarly pertinent to this case is the ethnography by Delgado (1993), examining a successful union campaign to organize undocumented Mexican workers in Los Angeles in 1986. Delgado argues that workers did not fear deportation: their previous lack of activism was due to insufficient union efforts to involve them. However, the conditions during Delgado's study period, marked by rare, non-salient, and ineffective deportations resulting in swift returns across the border, may differ significantly from the current context, in which immigration is a salient political topic, deportations are enforced, and return entails expensive and dangerous journeys.<sup>4</sup>

The conditions for the political mobilization of undocumented migrants, who lack franchise and face the threat of deportation, remain still unexplored. However, these migrants, who are employed in low-skill sectors, endure a large wage penalty, and are frequently victims of exploitation, have often strong reasons to mobilize. For a large fraction of the migrant population, therefore, the question of integration is rather a question of how to emancipate from exploitative working regimes. Under what conditions do undocumented migrants become empowered and manage to set themselves free from labor exploitation?

### The Determinants of Undocumented Migrants' Empowerment

Undocumented migrants face distinctive risks and challenges when considering whistleblowing and engaging in political action that citizens and legal migrants do not experience. First, the heightened visibility coming with political activism entails the risk of being identified and deported, with consequences that might outweigh the benefits of mobilization. Second, successful political

<sup>3</sup> While not focused on migrant politicization, Cremaschi (2020) is also relevant as it explores the survival strategies of undocumented agricultural workers in Italy.

<sup>4</sup> Other ethnographic studies consider the activism of refugees, who differ from undocumented migrants as they do not face deportation threats (Monforte and Dufour 2011), and Hinton's (2015) ethnography of the activism of DACA students, whose deportation was similarly lawfully deferred.

action hinges on garnering consensus among civil society, which can prove challenging if some natives deem undocumented migrants unworthy of making demands to the state. Third, whistleblowing and protesting can lead to job loss, especially since informal workers lack contracts safeguarding their right to strike without retaliation. Losing employment can be particularly costly for migrants, often devoid of the local support networks that can assist others during unemployment. Fourth, this same lack of networks can also mean that workers possess less information about their rights, and fewer resources to solve collective action problems, two fundamental conditions for reporting and mobilization. Finally, when smuggled and controlled by criminal groups, migrants face the risk of retaliation from organized crime, both against themselves and against their networks back home.

In contrast, legal migrants and native workers enjoy more favorable conditions when engaging in activism. For instance, the struggle of Mexican and Filipino agricultural workers in the US, led by Cesar Chavez, involved legal migrants and U.S. citizens with regular work contracts. They faced no risk of deportation, enjoyed support from local networks of friends and family in case of income loss, accessed resources from a local network offering information and organizational skills, and received backing from civil society, which actively participated in their boycott efforts (Garcia 2016).

Undoubtedly, the costs of denunciation and political activation for undocumented migrants are significant. However, the risks of inaction and remaining in exploitative working regimes are also elevated. Undocumented migrants face high likelihood of work-related and transportation accidents, exposure to diseases while working, and the constant threat of organized crime retaliation. Given these dangerous conditions, it is possible that some may opt for confronting the elevated costs of political activation and whistleblowing, rather than enduring exploitation. This choice becomes especially viable when considering that migrants have incentives to denounce their condition: several states grant migrants a temporary residence permit when they are found to be victim of exploitation;<sup>5</sup> the hope of attaining a legal (albeit temporary) status might be sufficient to tip the scales in favor of taking action.

I propose that migrants working in exploitative regimes, upon realizing the extent to which their rights are violated and recognizing the disparity between their current condition and the one they could achieve outside of exploitation, may be motivated to take on the risks associated with exiting labor exploitation and exposing their exploiters. This hypothesis aligns with the concept, dating back to Tocqueville (1856), that individuals experiencing a mismatch between their perceived status and the status they could potentially attain may experience dissatisfaction leading them to

mobilize.<sup>6</sup> A similar dynamic might drive the mobilization of undocumented migrant workers: the shared experience of learning about their rights, recognizing the discrepancy between their current situation and the status they could achieve, and being presented with a tangible incentive to report their exploiters could trigger political activation within this marginalized community.

Undocumented migrant activation to exit labor exploitation might manifest in various forms: reporting exploitation to authorities, a political act asserting rights through state institutions, or collective mobilization with other migrant victims. Group action aims to draw public attention to migrant labor exploitation, facilitating institutional mobilization and potential pathways for change.

## The Impact of Migrants' Empowerment

If migrants endeavor to liberate themselves from labor exploitation, what will be the repercussions on state capacity and politics? Past studies have explored the consequences of granting or revoking rights to migrants (Abdelgadir and Fouka 2020; Hainmueller, Hangartner, and Pietrantuono 2017). However, I examine a scenario where migrants are not passive recipients but rather active protagonists in their pursuit of social mobility. Achieving political voice is likely to produce different effects compared to just receiving rights, particularly on two outcomes: state capacity to curb crime, and natives' public opinion on immigration.

As for crime, studies show that regularization lowers crimes by allowing migrants to accept legal over illegal jobs (Hausman 2020; Pinotti 2017). Beyond diminishing incentives for crime, exiting exploitation could also deprive criminals of a lucrative source of profits obtained through exploitation. Moreover, if migrants are willing to report on their exploiters, the state stands to gain valuable information about criminals, enabling their prosecution. Hence, I hypothesize that denouncing labor racketeering could lead to increased prosecution of organized crime members responsible for exploiting migrants.

Regarding public opinion on immigration, recent studies suggest that support for the integration of migrants is higher when they are perceived as vulnerable (Alrababa'h et al. 2021; Bansak, Hainmueller, and Hangartner 2016). Notably, Bonilla and Mo (2018) show that emphasizing migrants' susceptibility to human trafficking influences public opinion toward more pro-immigrant survey responses. Based on these insights, I hypothesize that a political act emphasizing migrants' victimization through exploitation may shift public perceptions toward more pro-immigration stances and, sometimes, alter their partisan preferences.

<sup>5</sup> For example, nine E.U. states and the US (EU Parliament 2014, 41).

<sup>6</sup> This theory has been tested with reference to relative deprivation (Healy, Kosec, and Mo 2017), human trafficking (Mo 2018), and women empowerment (Kosec et al. 2021).

## CONTEXT AND QUALITATIVE EVIDENCE

### Migrant Labor Racketeering

A universally agreed-upon legal definition of labor exploitation is still lacking (Global Migration Group 2013). The ILO proposed a legal definition of forced labor as “work or service exacted under the threat of penalty.”<sup>7</sup> The term encompasses slavery and other coercive practices, such as debt bondage or retention of identity documents. In this article, I focus on a practice used to accomplish labor exploitation: labor racketeering. This is a form of illegal recruitment and control of the labor force that relies on intermediaries to hire and control workers. Often intermediaries are members of criminal organizations, which contribute to smuggle migrant workforce and control their behavior once they are working, preventing them from reporting their exploitation to the police (UNDOC 2018, 52). This practice is common in seasonal and unskilled sectors, such as agriculture, constructions, and the service industry, and it is present across countries (Augère-Granier 2021; Open Society 2020).

Labor racketeering is widespread in Italy, one of the first hot-spots of immigration into Europe and where criminal organizations are a long-standing presence. All four main Italian criminal organizations are involved in labor racketeering: mafias profit from this business by collaborating with foreign criminal organizations, as documented for the Sicilian mafia (Ministro dell’Interno 2021, 54), the ‘Ndrangheta, the Sacra Corona Unita (Parliamentary Commission 2022, 167 and 205), and the Camorra (La Repubblica 2021). This practice is used across the south, center, and north of Italy.<sup>8</sup> Reports by the Placido Rizzotto Observatory provide a recent picture of this phenomenon with reference to the agricultural sector: about 400,000 workers are involved in labor racketeering in agriculture—about one-third of the agricultural workforce. Of these, 49% are undocumented migrants and 40% in a situation of severe exploitation. In gross amounts, workers’ daily pay can reach 50 Euro, but this sum is forcefully curtailed: workers pay racketeers for transportation, food and water, such that their net wage is 20–30 Euros—about half the pay of legal workers. While Italians can also be employed through racketeering, the majority of workers in conditions of exploitation are migrants. Migrants are paid less than average—about 1 Euro per hour—they work longer hours, 62% of them live in ghettos without access to the most basic services, including water and sanitation,<sup>9</sup> and 76% develop diseases they did not have at the start of employment. This phenomenon has attracted periodic attention

from media and institutions, usually in correspondence with with woeful news stories.<sup>10</sup> Mobilized migrants denouncing their condition and seeking change have faced threats, violence, and even homicide.<sup>11</sup>

### The Intervention

In 1914, farmworkers from Bari were working in Cernigola, a town in Apulia a few kilometers to the north, accepting lower pays than local farmworkers, resulting in conflicts and occasional violence (Perrotta 2014). Union leader Giuseppe Di Vittorio, a historical figure of Italian unionism, resolved the conflict by uniting local and foreign farmworkers to demand a uniform salary from landowners. In 2007, following Di Vittorio’s legacy, the union of agricultural workers (Federazione Lavoratori Agro-Industria, or FLAI) organized a campaign to help workers exploited through racketeering.

The campaign had three key components: first, it provided workers with information about their rights. Second, it offered legal assistance to those who chose to report their exploitation to the authorities. Third, they offered an information impacting their incentives to denounce their employers: Italy, like other countries, offers a working permit to victims of exploitation, and the union assisted them in applying for this permit if their exploitation situation was assessed judicially. This permit allows non-E.U. citizens to legally work in Italy for 6 months, renewable for 12, and can be converted into a longer employment permit after expiration (Art 18, D.Lgs. July 25 1998, N. 286).

The first two components are direct solutions to the most basic obstacles to whistleblowing faced by migrants: the lack of awareness about their rights, unfamiliarity with bureaucratic procedures, and lack of legal resources necessary to navigate a trial successfully. The presence of the union is also likely to facilitate mobilization initiatives, by creating awareness and reflection on migrants’ collective condition. The third component, instead, is likely to address the deeper fundamental challenges to whistleblowing: legal status removes the threat of deportation, and opens the doors to legal employment, resolving issues of job loss and risks associated with criminal intermediaries in illegal labor. Taken together, the three components of the intervention hold the potential to fundamentally change migrants’ decision-making regarding mobilization and reporting about labor exploitation.

The campaign was based on a standard plan delivered by the national union, which provided

<sup>7</sup> ILO 1930, No. 29; see: [https://normlex.ilo.org/dyn/normlex/en/f?p=NORMLEXPUB:12100:0::NO::P12100\\_ILO\\_CODE:C029](https://normlex.ilo.org/dyn/normlex/en/f?p=NORMLEXPUB:12100:0::NO::P12100_ILO_CODE:C029)

<sup>8</sup> For example, in Trentino (Parliamentary Commission 2022, 267), Veneto (Parliamentary Commission 2022, 303), Lazio (Fanizza and Omizzolo 2019), and Toscana (La Repubblica 2021).

<sup>9</sup> Of the migrants living in informal settlements, 98% do not have access to the health services (Medici Senza Frontiere 2016).

<sup>10</sup> For example, news discussed the death caused by exhaustion for working excessively long hours of a migrant in Nardò (Il Fatto Quotidiano 2015) and the case of 12 migrants who died while being transported to the fields in an unsecured vehicle full beyond capacity (Forte and Giovannini 2018).

<sup>11</sup> It was the case for the agricultural workers and activists Soumaila Sacko (see <https://www.frontlinedefenders.org/en/profile/soumaila-sacko>) and Siddique Adnan (ANSA 2020). Leogrande (2016) highlights several instances of violent retaliation by racketeers against migrants.

indications and resources to territorial units on how to implement the intervention. An ex post survey confirmed that local units followed these standard procedures (FLAI CGIL 2016, Section 7). The unions used camper vans with a median of 4.5 unionists to reach migrants in the fields on a weekly basis, employing linguistic mediators for communication. The intervention offered the same information, assistance, and materials to migrants.

### *Timing of the Intervention*

The union-led intervention, initially promoted at the national level, was voluntarily adopted by local territorial units. It began in Apulia, where Di Vittorio originated, and expanded to other locations under the name “Street-Union.” Over 8 years, the intervention reached 49 municipalities across Italy (Figure A.1b in the Supplementary Material).<sup>12</sup> The decision to adopt the campaign was based on need (the existence of exploitation in that area) and will (the presence of union members willing to lead the intervention).<sup>13</sup> While data on union membership are not available below the regional level, the union itself collected data on the level of exploitation in different areas. I digitize these data, and test whether there is a relation between exploitation and the order of treatment adoption. Table A.1 in the Supplementary Material reveals an insignificant correlation between the order of treatment and levels of exploitation. This suggests that the highest or lowest exploitation areas were not specifically targeted first or later, even when adding region fixed effects (additional details in Section A.1 of the Supplementary Material). High-exploitation areas were also not more likely to receive treatment in absolute terms—many high-exploitation areas remain untreated. While other unobserved factors could explain why certain locations are treated earlier or later, robustness checks show that findings are not driven by the early-treated locations having specific characteristics different from the other municipalities treated later. After 2016, the union shifted focus toward advocating for institutional change, resulting in Law N.199/2016, which extends punishment to business owners and allocates more resources to support victims of exploitation.

### *Motives of the Participants*

Arguably, the union had economic incentives for embarking in this campaign, to reduce the downward pressure on salaries caused by informal labor. They, however, also faced considerable costs: the possibility of organized crime retaliation. Interviews with unionists suggest that some of them were indeed targets of threats. How could the campaign continue when

unionists were victims of attacks? I asked this question to one of the leaders, who replied that they received protection from “not being alone”: being embedded in networks of politically active individuals, with the resources to demand protection from the police and the capacity to increase their visibility and mobilize public opinion, helped unionists escape organized crime threats. Migrants in contact with union members might similarly be less “invisible.”<sup>14</sup>

A second obstacle is that the intervention could not address the root cause of the phenomenon of labor racketeering—the existence of a vulnerable, undocumented population lacking alternative job opportunities and facing deportation risks if they reported exploitation. The union intervention could partially address this obstacle by providing migrants with information about alternative job opportunities and about the humanitarian permit. While information and connections can be helpful, it is still migrants who had to take the costly action of denouncing their racketeers and the risk of unemployment, retaliation, and deportation in case the humanitarian permit did not go through.

## DATA

### **Media Coverage of Labor Racketeering**

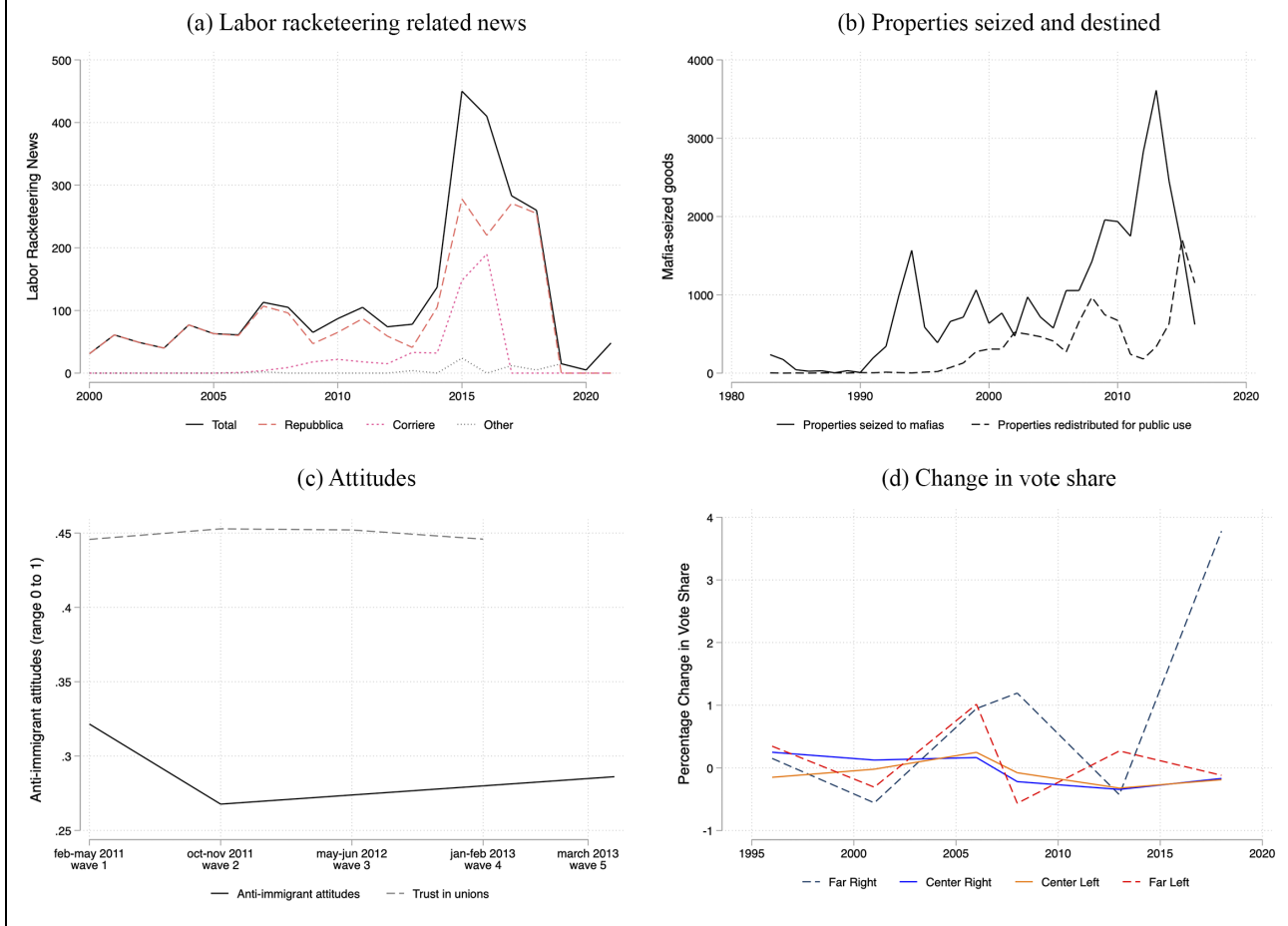
In Italy, there is no data on labor racketeering and its denunciation provided by the police at levels below the regional. I overcome the lack of data by scraping news items including the word labor racketeering (“caporalato”) from the seven main Italian national newspapers of different political slant<sup>15</sup> and validating this measure with province-level indicators of racketeering from government audits. The sample also includes the local editions of *Repubblica* and *Corriere*. While I do not scrape local-only newspapers, the local media landscape varies considerably across areas and in online archive availability. This non-homogenous sample might introduce nonrandom noise in news coverage by location. The news-based measure takes value different from zero when (i) labor racketeering takes place, and (ii) is uncovered, either through migrants denouncing their exploitation or through independent investigations. Newspapers can also cover the story if, after (i) and (ii), civil society initiatives against labor racketeering take place. This measure should thus be conceptualized not as an indicator of labor racketeering presence only, but as a measure of presence and prosecution of this phenomenon. Another aspect to notice to correctly conceptualize this measure is that newspapers tend to report news when extreme cases take place, such as severe exploitation, migrants’ killings, or revolts. As such, although the term “caporalato” might also refer to Italians employed through labor

<sup>12</sup> New locations might have been targeted after 2016, when the report stops. For this reason, I interrupt all analyses in this year.

<sup>13</sup> Interview with Jean-René Billongo, Placido Rizzotto Observatory, June 4, 2020.

<sup>14</sup> Interview with Marco Omizzolo, March 27, 2020.

<sup>15</sup> *Corriere della Sera*, *La Repubblica*, *La Stampa*, *Il Sole24Ore*, *Il Fatto Quotidiano*, *Libero*, and *il Manifesto*.

**FIGURE 1. Trends in Outcomes**

intermediation, the phenomenon I capture largely refers to extremely exploited migrants.

Figure 1a plots the temporal pattern in reporting about labor racketeering, indicating a similar trajectory across newspapers in time. For all newspapers, coverage of labor racketeering increases in 2014–15. In these years, Italy received two large waves of migrants, those escaping the aftermath of the Arab Spring (2011–12) and refugees from Syria (2014–15). The increase in news related to labor racketeering in those years is consistent with an increase in the phenomenon itself, driven by larger availability of migrant workforce. The presence of labor racketeering news is not systematically related to municipality size, a common concern with news-based measures (Figure A.1a in the Supplementary Material). It is unlikely that changes in local politics (the election of certain mayor) will affect national newspapers coverage, but I test for this possibility in Table A.3 in the Supplementary Material, showing that larger or smaller changes in voting do not affect reporting on labor racketeering. All fixed characteristics of the municipalities which might impact migrant labor exploitation and its reporting (e.g., the level of xenophobia, trust in institutions and state capacity, ...) are partialled out by municipality fixed effects.<sup>16</sup>

### Labor Racketeering Data Validation

I validate my news-based measure of labor racketeering by comparing it with information on audits conducted in agricultural businesses throughout Italy by the Labor Inspectorate, the institution inspecting irregularities in the workplace under the Ministry of Interior. I obtained province-level data from the Labor Inspectorate on the number of agricultural firms found hiring workers informally. I normalize this and the news-based measure by the province population.

There is a strong correlation between news-based and audit-based measures of irregular labor in agriculture: the correlation is 0.55 and the distributions map each other well spatially (Figure A.2 in the Supplementary Material). In Table A.2 in the Supplementary Material, I regress the audit-based on the news-based measure controlling for year fixed effects (these

<sup>16</sup> Two other metrics useful to judge the intervention's success would have been the number of humanitarian permits released and whether threats and violence against migrants took place in treated locations. Unfortunately, both these data are unavailable: the first is not released by the Ministry of Interior, the second is unlikely to be observed by either the police or the media, as those episodes remain largely under track.

account, e.g., for changes in government which might have triggered a different approach to investigations by the Inspectorate), region fixed effects, and a set of controls.<sup>17</sup> Results indicate a strongly significant correlation even in the most restrictive model, indicating that one additional news in one thousand residents translates into four more agricultural firms with irregularly hired workers over one thousand residents. In the results section, I also regress province-level treatment on the audit-based measure, confirming the results obtained with the news-based measure.

### Goods Seized from Mafias and Destined to Public Use

Since the approval of Law n. 646/1982, Italy has had a judicial tool to seize goods, properties, and firms owned by criminal organizations. Since 1996 (L. 109/96), the law mandates that seized mafia assets be used for social purposes. These assets can either become part of the state patrimony (often used as offices) or be assigned to local administrations for redistribution to cooperatives, NGOs, and associations. Information on seized and destined goods is publicly available through the National Agency for Seized Goods (ANBSC 2024). The data include the year of confiscation, which occurs shortly after the property is seized, marking the first judicial step to remove it from its owner. Seizure and confiscation happen quickly for mafia-owned goods once sufficient evidence suggests illegal activity or money laundering. As a result, there is a short time lag between the start of investigations and the seizure and confiscation. The number of seized and redistributed goods has been increasing over time (Figure 1b).

### Anti-Immigrant Attitudes

I use the panel survey conducted by Italian National Election Studies (2014) (ITANES) between 2011 and 2013, which included questions on voting behavior and political attitudes, including on immigration. The survey targeted 2,332 respondents over several waves, totaling 5,816 observations. Of these, 415 observations are in treated municipalities, belonging to 83 unique respondents interviewed over two or three waves. These individuals were in 22 of the treated municipalities (45% of the treated sample) and were asked questions before and after the union's intervention. This allows me to assess whether the same person changed their mind on immigration by comparing their responses before and after the intervention, and to those untouched by the intervention. I use information on attitudes toward migrants, voting intentions, and trust in unions. The average anti-immigrant attitude and trust in unions are plotted in Figure 1c.

<sup>17</sup> I control for potential confounders, such as foreign population, the number of union workers, and the time-varying number of audits conducted by the Labor Inspectorate in that year-province.

### National Elections

National elections results come from the Ministero dell'Interno (2024). I group party formations into four categories consistent over time: far-left, center-left, center-right, and far-right. Ideologically, extreme parties are categorized as such, regardless of coalition with moderate parties, while parties always running in coalition are placed in the respective centrist category.<sup>18</sup> To account for turnout changes affecting vote share, I calculate vote share as votes divided by total eligible voters, as every person in Italy is automatically registered to vote. Figure 1d shows the evolution of vote share changes for the four party groups.

### Municipalities Targeted by the Intervention

Information on municipalities reached by the intervention comes from FLAI CGIL (2016) on labor exploitation in agriculture by the union in 2016. Between 2007 and 2016, union members built and maintained relationships with immigrants in 49 municipalities across eight regions in Italy. The implementation was staggered over time (see Figure 2). Given the ongoing link between the union and migrant workers, I consider municipalities as treated in each subsequent year, though results remain robust to changes in this definition.

### EMPIRICAL STRATEGY

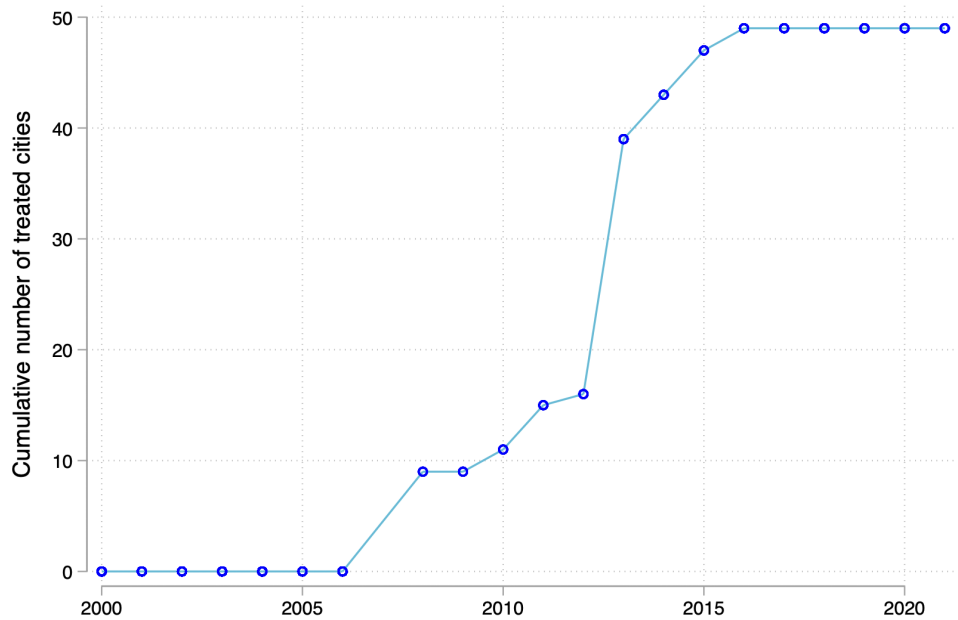
To identify the effects of the intervention, I rely on a DiD strategy comparing municipalities which were and were not targeted by the campaign, before and after it took place. For municipality  $i$  and year  $t$ , I estimate the following two-ways fixed effects model:

$$Y_{it} = \alpha_i + \beta_t + \gamma \text{Treat}_{it} + \epsilon_{it}, \quad (1)$$

where  $\text{Treat}$  is a municipality-specific time-varying indicator equal to 1 in municipalities and years in which the unit is targeted by the intervention. Municipality and year fixed effects guarantee that any time-invariant characteristic of the location is partialled out from the effect. For example, fixed effects account for whether the location is an agricultural area, or it has a history of xenophobia. I cluster standard errors at the municipal level.

A possible threat to identification could derive from treatment assignment being nonrandom, and in particular correlated with the level of exploitation of treated areas. If this is the case, the outcomes I consider will be already on different trends before the intervention. I account for this concern in three ways. First, I show that trends in outcomes—labor racketeering news, goods seized and destined, and voting—were largely parallel before the start of treatment in each location (Section B

<sup>18</sup> The Five Star Movement is excluded from analysis since it participated only from 2013, preventing pre-trend examination. Table A.5 in the Supplementary Material lists the parties and their groupings.

**FIGURE 2. Number of Treated Municipalities over Time**

of the Supplementary Material). Second, using information from reports produced by the union on the level of exploitation in each location in Italy, I show that locations with high (low) levels of exploitation were not more likely to be treated earlier (or later): the intervention targeted a mix of medium and high-level areas in each roll-out phase of the campaign (Section A.1 of the Supplementary Material). Third, findings are not driven by early-treated municipalities having specific characteristics different from late-treated: results are robust to dropping the entire sample of treated observations in the first, second, third, and fourth year (Table D.1 in the Supplementary Material). Fourth, to reduce observable differences between municipalities that ever received treatment and those that did not, I compare treated units to a restricted set of most-similar control units identified with nearest neighborhood propensity score matching.<sup>19</sup> A balance table reveals that matching substantially minimizes differences across treated and control units, considering both the matching and the other variables (Table B.2 in the Supplementary Material).

## EFFECTS OF THE INTERVENTION

### Reporting on Labor Racketeering in the News

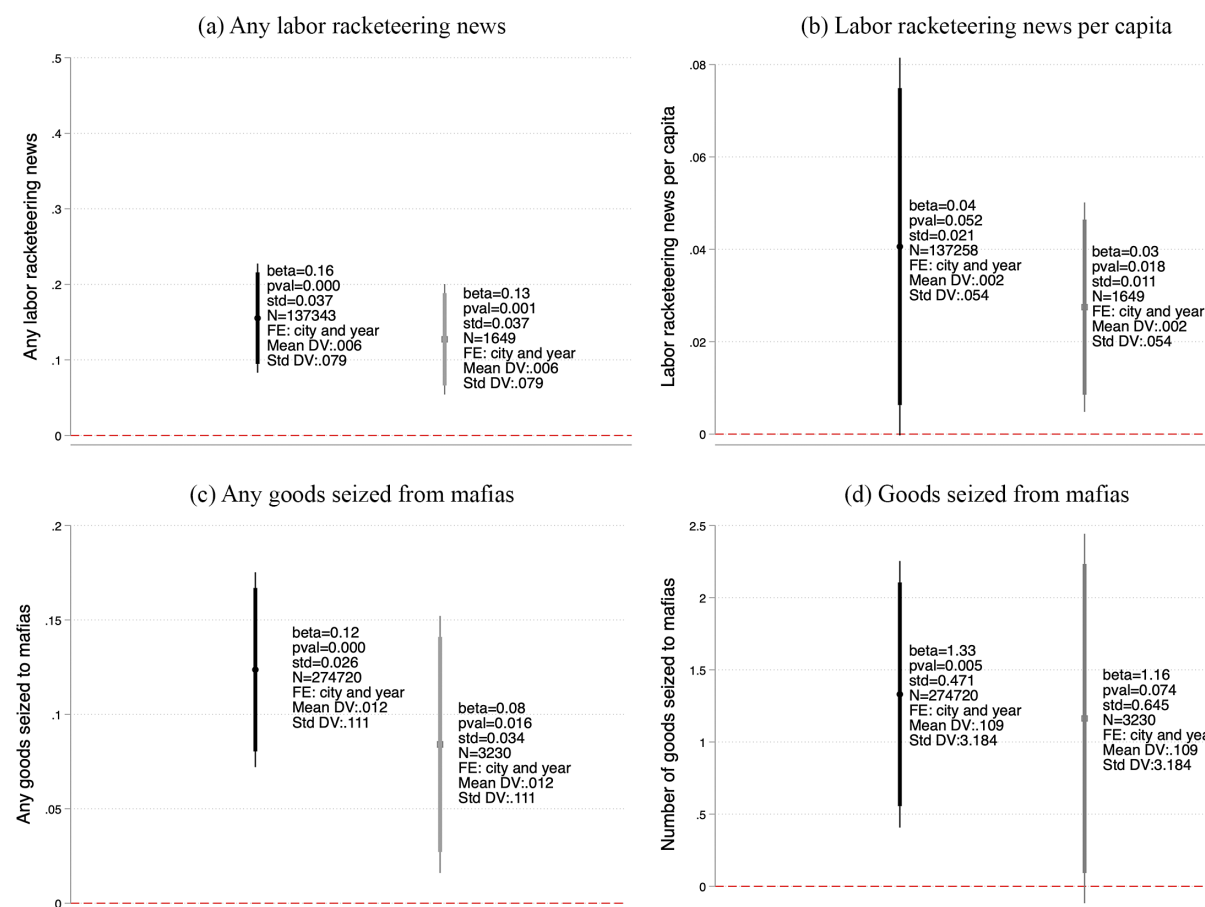
I start by asking whether the campaign was effective in increasing reporting on cases of labor exploitation in

newspapers. I observe a significant 15% increase in the likelihood that any news related to labor racketeering is reported in a municipality (Figure 3a), corresponding to an increase by 0.04 news in one thousand inhabitants—a 20-fold increase with respect to the mean of the dependent variable in the control group (Figure 3b, black). This effect is also present when considering the matched control sample (Figure 3b, gray). Results are robust to running estimates separately by the newspaper covering the news (Table D.4 in the Supplementary Material) and for the total number of news, rather than news per capita (column 1 of Table D.5 in the Supplementary Material). Since the distribution of the dependent variable is skewed toward zero, I also show robustness to a Poisson and negative binomial specifications (columns 2 and 3). Results are more precise when dropping the last two posttreatment periods, when larger but higher variance effects are observed (column 5). Results are robust to adding flexible controls for important confounders such as foreign population and male unemployment (columns 6 and 7).

### Content of the News

An important question is whether the increase in news is an automatic product of journalists reporting about the union's campaign, or if instead reporting reveals an increase in migrants whistleblowing and mobilizing at higher rates. To answer this question, I read and classify the content of news in treated municipalities after treatment and validate my classification with that of a research assistant (details are included in Section A.3 of the Supplementary Material). The majority of news (29%) discusses cases of migrants denouncing their racketeers or police operations against labor

<sup>19</sup> I match using the following pretreatment Census 2001 characteristics: share of population employed in agriculture and in unskilled labor, unemployment rate, population size and density, foreign population, illiteracy rates, and the regional number of FLAI union members in 2006.

**FIGURE 3. Effect on Labor Racketeering News and Goods Seized from Mafias**

*Note:* Results from DiD in Equation 1. Full (propensity score matched) sample coefficients in black (gray). Panel a considers any news related to labor racketeering, b considers the number of news per capita over one thousand residents, c considers any good seized from organized crime, and d considers the number of goods. All panels include confidence intervals at 95% and 90%, municipality and year fixed effects and standard errors are clustered at the municipal level. Results in tabular form in Table 1, APSR Dataverse files (Dipoppa 2024b).

exploitation. Second, there are demonstrations or initiatives by migrants or by the civil society to achieve change on labor exploitation (23%), third journalistic reports (22%), and finally policymaking initiatives or debates on how to fight this phenomenon (20%). A randomly selected sample of news in control locations discusses labor exploitation only in the context of more general journalistic reports about crime, without referencing protests and whistleblowing. The dynamic of migrants' mobilization and reporting in treated areas can be understood reading examples of news items:

The strike of North African workers continues for the sixth consecutive day. These migrants, who pick tomatoes in the fields, protest against low wages and illegal hiring practices, and promise to continue crossing their arms until their requests are met. The DDA (judicial investigative unit) in Lecce has opened an investigation on the exploitation of immigrants in the fields of Nardò. There are multiple reports by workers, including people received death threats from the racketeers. (Russi 2011)

The arrests were in the air for months. Last summer, the workers' fight in Masseria Boncuri, a shanty town home to migrants enduring the inferno of the farmlands, garnered significant attention. They bear 10-12 working hours under a scorching sun, paid 20-25 euros at best, below the poverty line. (Colluto 2012)

The unions' campaign did not receive direct media coverage, but unionists are often interviewed in the context of journalistic reports, and in this case they sometimes mention the campaign. Mentions of the union in the news (searching for "union," "Flai," or "Cgil") are balanced across the treated and control sample, indicating that the union did not play a role in differential media reporting on episodes happening in treated locations.

#### *Was It the Migrants or the Union?*

Another important question is whether the intervention was driven by migrants, who found the courage to

whistleblow, or directly by the union, who talked to the police themselves. Both theoretical and empirical evidence suggests that the effect was primarily driven by migrant behavior. First, both the union reports and interviews with union leaders indicate that the campaign did not involve talking to the police or the judiciary to suggest them to investigate. This is likely because prosecuting labor exploitation is challenging without workers willing to testify, and the police and judiciary in Italy are among the most overburdened bureaucracies. Encouragement to take on additional work may result in no action, while a formal denunciation of illegality compels them to act.

Second, in Section C of the Supplementary Material, I provide qualitative evidence that treatment effectively spurred migrant activation in several ways: (i) migrants in treated locations organized public demonstrations to bring attention to the issue; (ii) they denounced their employers, contributing to judicial efforts to curb exploitative labor practices; and (iii) some migrants dedicated themselves to long-term political activism, making it their career and engaging in advocacy for migrant rights.

Third, I offer a formal test that unions alone are unlikely to have caused the higher prosecution against labor racketeering. Imagine that contact with migrants does not influence prosecution, and instead all that matters is that unions identify where exploitation takes place. Union members could request prosecution directly in all locations where they have this information, without migrants' intervention. Using the same union reports I used for validation purposes, I observe the locations which the union identified as having medium or high levels of exploitation, but where they did not lead the same intervention involving contact with migrants. I use these locations to test if areas identified as high-exploitation but where the union did not intervene with migrants, after the period of investigation by the union, experience a similar increase in labor racketeering news. This is a placebo DiD using the same specification as the main result on labor racketeering news, but replacing the intervention with this placebo treatment. As shown in columns 2 and 3 of Table D.2 in the Supplementary Material, simply gathering information about exploitation without contact with migrants has a null effect on labor racketeering news.

Finally, I code news items about labor racketeering that also contain mentions of the word "protest." Those instances are the most likely to capture political activation to exit exploitative regimes. The DiD analysis indicates that those news items are significantly more likely to occur in treated cities after the intervention—a 16-fold increase over the mean of the dependent variable (column 4 of Table D.2 in the Supplementary Material).

Taken together, the content of news articles, the existing qualitative evidence, the null effect of placebo interventions not involving contact with migrants, and the effect of treatment on protests, provide evidence that the increase in reporting about labor racketeering is driven primarily by migrants' mobilization which triggered state intervention, but

also by the activism of local civil society and institutions to achieve change.

### *Magnitude of the Intervention and Impact of the Program*

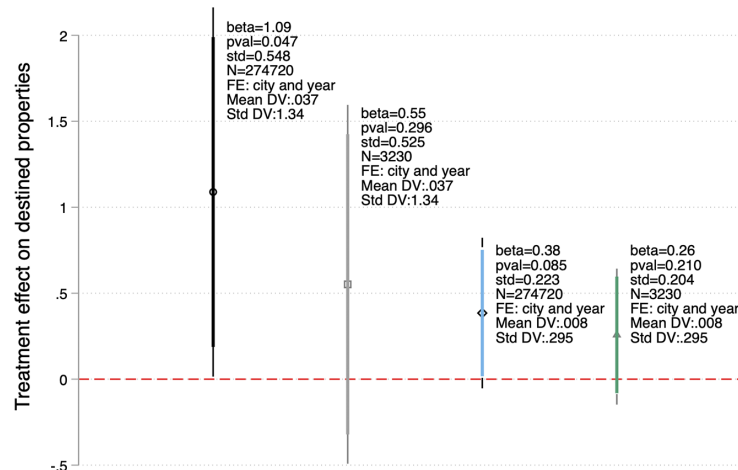
Are the effects on labor racketeering news reporting realistic, given the scope of the campaign? To address this question, I utilized data collected by the union on the number of migrants reached by their intervention. Over a 3-year period from 2012 to 2014, the union engaged with 21,442 migrant workers (FLAI CGIL 2016, 242), all in conditions of moderate to severe exploitation and thus in a condition to report exploitation to the state. I compute the average number of migrants reached per location-year ( $N = 167.6$ ), and impute this value to the missing location-years to estimate that a total of 47,761 migrants were likely reached by the campaign. How many of these migrants would respond to the intervention by whistleblowing or engaging in mobilization? As a benchmark, a 5-minute, in-person canvassing intervention on migrants persuaded 3.4% to turnout to vote (Pons and Liegey 2019). The union's intervention was a substantially more intensive interaction, but its objective was to modify a costlier behavior than voting. If we assume the same effectiveness, the intervention might have led 14,328 migrants to whistleblow. Hypothesizing a third of the effectiveness, the intervention could have activated 4,776 migrants. Both figures are substantial and could explain the presence of 0.7 additional news pieces per year, as in the main result.<sup>20</sup> This is particularly true when considering that political activation likely extended beyond the initially treated migrants to involve their peers, potentially leading to a larger number of activated individuals than those directly reached by the union.

Finally, I test if displacement of labor racketeering took place in neighboring municipalities. Evidence is weakly consistent with this possibility: municipalities bordering treated locations experience a (nonsignificant) drop in news related to labor racketeering. As migrants exit racketeering in treated areas, neighboring areas display either no change or a slightly stronger grip of racketeers and a consequent insignificant drop in reporting and prosecution (Table D.3 in the Supplementary Material). Treatment effects are instead similar excluding these cities from the analyses.

### **Organized Crime Prosecution**

The second dimension of interest is state capacity to prosecute racketeers: I ask if the intervention effectively increased state prosecution against organized crime, which is often the intermediary in the most severe cases of exploitation. The results suggest this is the case: treated municipalities experience a 12% increase in the likelihood of having any goods seized to organized crime in the years following the

<sup>20</sup> This effect might even be a low bound if the intervention leads some of the migrants to leave after the intervention.

**FIGURE 4. Effect on Properties Destined for Social Use**

Note: Results from DiD in Equation 1. Properties destined for any social use in solid lines, for agricultural use in dashed lines. Full (propensity score matched) sample coefficients in black (gray). Confidence intervals at 90% and 95%, municipality and year fixed effects are included. Standard errors are clustered at the municipal level. Results in tabular form in Table 2, APSR Dataverse files (Dipoppa 2024b).

intervention (Figure 3c). This corresponds to 1.16 to 1.3 more goods seized from organized crime—thirteen times more than the average in the control group considering the full sample (Figure 3d, black), and four times more considering the matched sample (gray coefficient). Seizing firms to mafias is a rarer event—for example, there are 44,462 seized properties in the database but only 5,365 seized firms. While less precise, there is also evidence that the number of firms seized from organized crime increased in treated municipalities (Table D.6 in the Supplementary Material).

### Properties Destined to Public Use

After properties are seized from organized crime, the investigations are concluded, and the goods are conclusively confiscated, they can be reassigned for public use. These goods can either be used as offices or be assigned to local administrations, who can give them for free to cooperatives and associations. While these properties represent freebies for local governments, distributing them is expensive, bureaucratically complex, and complicated by the fear that criminal groups might retaliate against those endowed with their former properties. Often, this means that properties seized from mafias are left unused (Cisterna 2012).

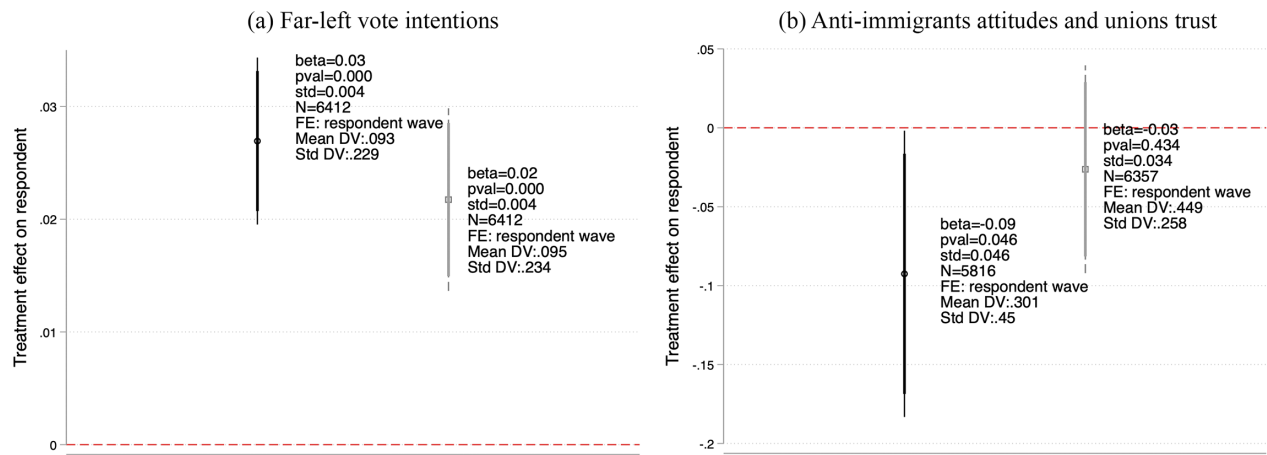
At the same time, financing cooperatives of agricultural workers is considered a best practice to curb exploitative practices in agriculture, since it creates legal employment options for agricultural workers (Guidelines on preventing labor exploitation in agriculture, Ministry of Labor 2021). I hypothesize that, if administrations face political pressure for adopting measures against exploitation, they might resort to goods seized from mafias by assigning them to cooperatives working in agriculture.

In line with this hypothesis, treated municipalities experience a significantly higher number of destined goods: 1.08 goods are destined to social use in treated municipalities, against an average in the control group of 0.037 (a 28-fold increase from the control group mean). This increase is largely driven by goods assigned to agricultural cooperatives: 0.26 more goods are destined to agricultural cooperatives in treated municipalities with respect to 0.008 in the control group (a 47-fold increase from the mean, Figure 4).<sup>21</sup> The effect is unlikely to be the automatic product of the increase in seized properties: while goods suspected to be mafia owned can be rapidly seized, for those goods to be reassigned there is a long multiple steps process, including concluding investigations, assigning the good to the ANBSC, then to local governments, making it compliant with regulations, and deciding its destination. The average time between the seizure and the final destination of a property is 2,023 days—5–6 years (Cisterna 2012). These findings are in line with Boeri, Di Cataldo, and Pietrostefani's (2024) conclusion that the destination of mafia goods can, in small part, compensate the territory for the damages it suffered from organized crime.

### Attitudes and Voting, Individual-Level Data

A campaign that generated higher media coverage of labor racketeering, prosecution of mafia members, and local governments mobilization, might have also impacted citizens' view on immigration and partisan preferences. In the last decade in Italy, both the center-right and center-left governments promoted restrictive

<sup>21</sup> Information on destination of the good and type of activity are not available for firms.

**FIGURE 5. Effect on Anti-Immigrant Attitudes and Far-Left Voting, Individual-Level Data**

Note: Results from DiD in Equation 2. Voting intentions for Rifondazione Comunista (Comunisti Italiani) in black (gray). Anti-immigrants attitudes (trust in unions) in black (gray). Confidence intervals at 90% and 95%, respondent and wave fixed effects are included and standard errors are clustered at the municipal level. Data from ITANES survey. Results in tabular form in Table 3, APSR Dataverse files (Dipoppa 2024b).

policies on immigration. In particular, the center-left government proposed and sealed a pact to intercept migrants trying to cross the Mediterranean and send them to Libya, an unprecedented initiative which violated international conventions requiring states to assess migrants' right to asylum. This decision was criticized by several institutions, including nonpartisan ones like the United Nations, which called the pact "inhumane" (The Guardian 2017). Given the stance of center-left governments, voters with more pro-immigration attitudes should be more likely to vote for parties that maintained a liberal approach toward immigration—namely, the far-left.

### Empirical Strategy

I consider the effects of the intervention on the public opinion of natives by capitalizing on the ITANES survey's inclusion of individuals in some treated locations (22 municipalities, 45% of the treated sample) before and after treatment. This analysis relies on a smaller sample (depending on the outcome, approximately six thousand observations), and it only includes the years 2011–13, a fraction of the treated period. It should thus be taken with caution. However, due to the fortuitous overlap of the panel survey rollout and of the union intervention, this sample allows to observe changes in the same respondents' immigration and voting preferences before and after treatment. This analysis is thus representative of the impact of treatment on individuals, rather than at the municipal level as in the rest of the analyses. For respondent  $r$  in wave  $w$ , I estimate

$$Y_{wt} = \rho_r + \omega_w + \lambda \text{Treat}_{rw} + v_{wt}, \quad (2)$$

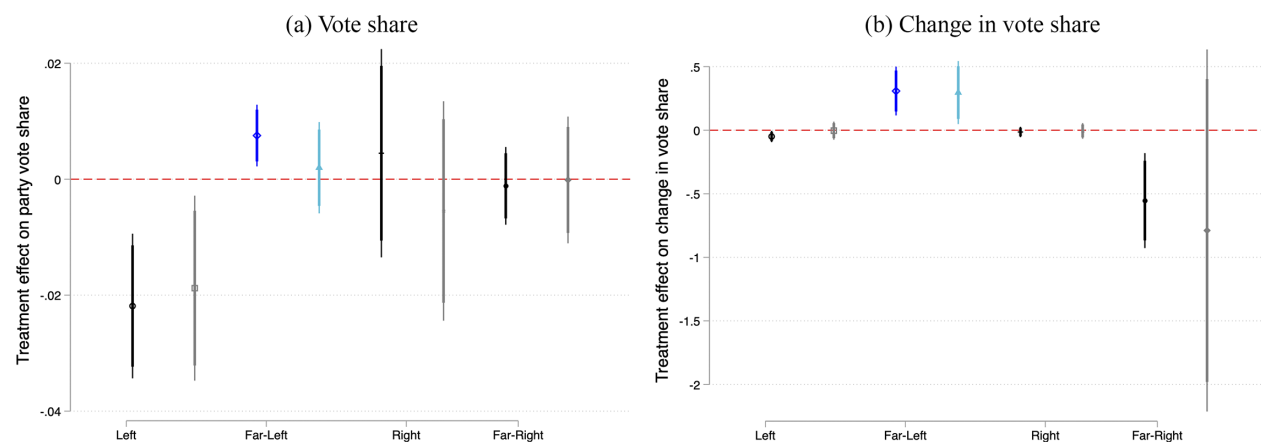
where  $\rho$  and  $\omega$  are respondents and wave fixed effects and  $\lambda$  is the coefficient of interest, capturing the change

in voting and anti-immigrants attitudes after the intervention.

Figure 5a shows that the same individual is 2–3% more likely to report voting for far-left pro-integration parties, and is 9% less likely to report anti-immigration sentiments (Figure 5b, solid line). A possible alternative explanation could be that the increase in far-left vote is not related to immigration but to the union's activity, which supports left-wing parties. However, if this was the case, we would also see a positive effect on center-left parties' vote share, which is not observed. Second, if the union was so influential as to sway voters, we should expect trust in this institution to increase. Instead, the effect on trust in unions is negative and null (Figure 5d, dashed line).

### Voting, Municipal-Level Data

I confirm the results on voting intentions using municipal-level data on voting in national elections. I present results for parties' vote share and change in vote share from the previous election year. Conceptually, the change in vote share better captures how large a variation in electoral performance occurred between elections, giving a more precise indication of the extent by which the intervention reshaped a municipality's political landscape. Results are presented in Figure 6. Starting from the coefficient of interest (far-left, in blue), there is a significant increase in far-left votes, the only political group which consistently supported pro-immigration policies. These parties experienced a positive change with respect to the previous elections that is much larger than the average change in the control group and corresponds to a 65%–79% increase from the previous election year. In levels, this effect corresponds to a 1% increase in far-left vote share. The positive change is visible in each of the two periods after treatment (Figure B.2 in the Supplementary

**FIGURE 6. Effect on Voting, Municipal-Level Data**

Note: Results from DiD in Equation 1. Full (propensity score matched) sample coefficients are displayed in black (gray). For the coefficient of interest, vote for the far-left, coefficients for the full (propensity score matched) sample are in blue (green). Confidence intervals at 90% and 95%, municipality and year FE are included and standard errors are clustered at the municipal level. Data on national elections 1994–2018 from the Ministry of Interior. Results in tabular form in Tables 5 and 6, APSR Dataverse files (Dipoppa 2024b).

Material). Considering the other parties, the center-left experienced a significant drop in vote share but an insignificant effect on the change in votes. This coalition lost votes posttreatment, consistently with the possibility that voters partly shifted from the center to the far-left due to immigration. The change from each previous election year is however not statistically significant. The center-right does not display consistent signs of change across specifications. I do not interpret results for the far-right parties—those are largely unstable across specifications, ranging from positive to negative, and there is no support for the parallel trends assumption for this outcome (Figure B.2 in the Supplementary Material). Instead, across robustness tests, the coefficients for the far-left party are consistently positive and significant. If the intervention is responsible for the change in vote share for the far-left, we should observe larger effects in municipalities that were treated closer to elections. In Table D.8 in the Supplementary Material, I test the same specification on the subsample of municipalities which were treated less than 1 year before elections: as expected, the change in votes is larger considering this subsample.

## Robustness

### Relaxing the Linearity Assumption

For the first three outcomes, the distribution of the dependent variable is skewed toward zero, as those tend to be rare events. I account for this skew by presenting results using a Poisson and negative binomial regression model for news (column 4 of Table D.5 in the Supplementary Material), goods seized to mafias (columns 1 and 2 of Table D.7 in the Supplementary Material), firms seized from mafias (Table D.6 in the Supplementary Material), and goods destined to social

use (columns 3 and 4 of Table D.7 in the Supplementary Material). Results are robust to this alternative specification.

### Relaxing the Treatment Homogeneity Assumption

Recent literature on dynamic DiD designs shows that these models exploit comparisons between early-treated and late-treated units which can bias the estimation of treatment effects if those are not constant across groups or times (Goodman-Bacon 2021). To account for treatment effects heterogeneity bias, I rerun event studies for all outcomes using four new estimators proposed in the recent DiD literature. Results in Figures D.1–D.4 in the Supplementary Material are consistent across specifications. Treatment effects display a gradual increase over time, with news and seized goods peaking a few years into the intervention. The pattern is consistent with the possibility that initially only a few migrants denounce exploitation, but as they succeed, others follow suit. In contrast, policymaking responses (goods destined) and voting show a more immediate reaction, aligning with a different process in which the intervention impacts public opinion.

### Relaxing the Parallel Trends Assumption for Voting Outcomes

The parallel trend assumption implicitly implies that it is possible to control for selection effects in which units enter treatment, as treatment post-periods are assumed to have evolved along the same lines as control pre-periods absent treatment. Using fixed effects and matching on observables alleviate this concern. Still, synthetic DiD approaches (Arkhangelsky et al. 2021) have proven particularly effective at compensating for

the potential lack of parallel trends by reweighting units to match their pre-exposure pre-trends. For each outcome, I present results in a synthetic DiD approach: results are consistent with the main analysis (Table D.11 in the Supplementary Material). Unlike other outcomes, voting is only observed in election years, reducing the coefficients available for assessing the common trends assumption. In an alternative specification, I compare *within* treated units: this strategy reduces the sample size and drops the last year of treatment, but it allows relaxing the assumption that trends between treated and control group are parallel, since it uniquely relies on within-treated units. The identifying assumption is no strategic selection into treatment timing. As discussed, the treatment decision was decentralized to the local labor union, and areas with the highest levels of exploitation were not systematically targeted first. Results in Table D.12 in the Supplementary Material show again a strongly positive and significant coefficient for far-left parties.

#### *Duration of Treatment*

Interviews with activists in the campaign indicated that, once a location was treated, unionists maintained a presence in the area.<sup>22</sup> In Figure D.5 in the Supplementary Material, I test the robustness of the findings to modifying the duration of treatment to less years, until only considering units as treated in the same year in which treatment started. Results are robust for all outcomes across all treatment duration.

#### *Size of Treated Sample*

Results are robust to using *p*-values from Fisher randomization inference (Table D.9 in the Supplementary Material). This test accounts for the fact that the number of treated observations is relatively small, but it also relies on the assumption that treatment is assigned randomly, which in this case might be excessively strong. I also test the robustness of results to using Conley–Taber confidence intervals. This method relaxes the asymptotic assumption that both the number of treated and untreated units is large and only relies on a large control group, as in this article. To construct the Conley–Taber confidence intervals, information from the untreated group is used to consistently estimate the distribution of the DiD point estimator. As Table D.10 in the Supplementary Material reports, CT confidence intervals are similar to those in the main specification, and for most outcomes this specification reduces their size.

#### *Simultaneous Interventions*

A classic concern in DiD designs is that simultaneous interventions might happen at the same time as treatment and drive results. Since this is a setting where different locations are treated in eight different points

in time, it seems unlikely that other groups independent from the union decided to treat the same set of locations in the same exact order chosen by the union. This might have happened by coincidence in some locations, but I show that removing any of the treated locations, results are identical: none of them in particular drives the findings (Figure D.6 in the Supplementary Material).

#### *Multiple Hypothesis Testing*

Since I estimate the effect of treatment on multiple outcomes, I account for multiple hypothesis testing. I compute sharpened *p*-values that factor in the potential rate of false rejections due to testing a large number of coefficients. Results reported in Table D.13 in the Supplementary Material confirm the significance of the estimates. Finally, I show that all results are robust to the inclusion of region times years flexible controls, accounting for region-specific time trends which might impact the outcomes of interest (Table D.14 in the Supplementary Material).

## CONCLUSIONS

This article studies the consequences of undocumented migrants empowerment to denounce and exit from labor exploitation. An intervention providing migrants with information and incentives to denounce their employers was effective in increasing news reporting about cases of exploitation, particularly news reporting about migrants denouncing their exploitative employers or migrants' mobilization initiatives. In line with racketeers often belonging to criminal organizations, higher whistleblowing of labor exploitation spilled over into higher rates of mafia prosecution, with a significant increase in seizure of mafia-owned properties. In this respect, this article points to an important policy implication: cracking down on migrants' exploitation directly undermines organized crime, by unveiling their activities and reducing their profits. One limitation of this study is that it cannot observe whether retaliation against undocumented migrants took place as, unfortunately, these data are unobserved.

I further document that intervention raised awareness about the vulnerable condition migrants were subject to among local residents and institutions, as revealed by news talking about initiatives against racketeering, and by local governments assign properties to agricultural cooperatives—a system to offer migrants legal employment.

Finally, this intervention did not produce a backlash against pro-immigration parties, as in other examples of policies favoring migrant integration. Instead, treated municipalities experience reduced anti-immigration attitudes and an increase in vote share for pro-immigration parties. This finding is in line with recent literature showing that highlighting migrants' fragility can lead natives to become more accepting of migrants (Bansak, Hainmueller, and Hangartner 2016). This article contributes to this literature by showing that not only attitudes toward migrants improve when

<sup>22</sup> Interview with Marco Omizzolo, March 27, 2020.

citizens are exposed to their condition of vulnerability, but that also voting behavior responds accordingly.

As labor exploitation becomes increasingly prevalent in developed countries (ILO 2022, 22), understanding under what conditions migrants can emancipate from exploitative regimes is of primary importance. This article sheds light on the determinants of migrants empowerment by showing that information and incentives to whistleblow can lead to the empowerment of marginalized undocumented communities.

This study presents promising results to address situations of severe exploitation, as it improved migrants' welfare at no political cost for the political side promoting it. In fact, the most recent government guidelines for fighting labor racketeering were developed in conjunction with the unions and incorporated several elements of this intervention.<sup>23</sup> Nonetheless, a few caveats are worth noticing. First, while news items do not mention any such instances, it is possible that the intervention might have produced retaliation by criminals against migrants. More carefully collected information than those received and reported by newspapers would be needed to assess whether retaliation took place. Second, while the intervention was effective at the scale at which it was led, scaling it up might produce different effects: systematic reporting by migrants could only be achieved if migrants who whistleblow are effectively rewarded with a humanitarian residence permit, a strategy which would require governments to grant temporary legal status to a larger number of migrants and which might produce different effects on public opinion. On the other hand, if reporting labor racketeering to the police became the equilibrium, exploiting migrants might become too costly and stop being a profitable business, with positive spillovers on legal competition, fiscal entries, and state capacity more in general. The question of whether scaling up this intervention would be overall beneficial for the state, for natives, and for migrants remains open. More broadly, this study underscores the importance to consider state actors at large when devising policy interventions. While governments might have (or believe to have) misaligned electoral incentives to lead pro-migrant interventions, other actors, such as unions, might have incentives to act and empower marginalized groups like undocumented migrants.

## SUPPLEMENTARY MATERIAL

To view supplementary material for this article, please visit <https://doi.org/10.1017/S0003055424001138>.

## DATA AVAILABILITY STATEMENT

Research documentation and data that support the findings of this study are openly available at the American Political Science Review Dataverse: <https://doi.org/10.7910/DVN/G126SC>.

<sup>23</sup> See: <https://www.lavoro.gov.it/temi-e-priorita/immigrazione/focus-on/Tavolo-caporalato/Documents/Piano-Triennale-post-CU.pdf>.

## ACKNOWLEDGEMENTS

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## CONFLICT OF INTEREST

The author declares no ethical issues or conflicts of interest in this research.

## ETHICAL STANDARDS

The author claims exceptions to the APSA's Principles and Guidance for Human Subject Research (2020) and provides reasoned justification in the appendix. Although drawing on research with human participants, the author claims exemption from organizational ethical review and provides reasoned justification in the appendix.

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# SUPPLEMENTARY INFORMATION

— For Online Publication —

## A Labor Racketeering Data Validation

### A.1 Order of treatment and levels of exploitation

In this section, I correlate the order in which locations treated and the levels of exploitation of each location recorded by the union in their report.<sup>1</sup> The union conducted an investigation on the levels of exploitation in agriculture in Italy by sending questionnaires to their local union representatives. Levels of exploitation are classified from 1 to 3 (low to high exploitation). While treatment is known at the municipal level, the report often includes information about broader locations, such as provinces. When this is the case, I attribute the value of the province to the treated location. For 10% of the treated sample, in the regions of Lazio, Abruzzo, and Basilicata, the union did not collect information and those observations are thus excluded from this analyses. Table A.1 presents a correlation analysis for locations in the treated sample (Columns 1-2) and for all locations on which there are data (Columns 3-4). There is no significant correlation between levels of exploitation and order of treatment when considering the treated sample, whether we consider the question in absolute terms or within regions (Columns 1 and 2). Considering the entire possible sample of locations the union could have chosen from (with the caveat that we can only observe areas on which they have collected data), there is a positive significant correlation for the entire country – meaning that the union prioritized low exploitation areas first – but not within regions (Columns 3-4). In line with the indications of the reports, the campaign targeted areas with presence of exploitation, but the order of treatment did not give priority to the highest (or lowest) exploitation zones.

### A.2 Validation of labor racketeering news-based measure

I obtained data on audits detecting labor-related irregularities in agricultural firms from the Labor Inspectorate (Ministry of Interior). Irregularities include hiring workers without a contract, using intermediaries to hire workers, minor labor, pregnant women heavy labor, excessive work hours, safety and health substandard conditions, and criminal offenses. Data are at the province level and start in 2012. To match this province-level database, I collapse my news-based measure at the province-year level, and I normalize both measure by province population in 2011. I do not normalize the audit-based measure by the number of inspections, as this variable is likely to be endogenous (but results are consistent using this measure and available upon request). I display the non-parametric correlation between the two measures in Figure A.2 and consider an OLS regression of one measure on the other including fixed effects and controls in Table A.2.

Given the limited sample size of news in treated areas, I read and classify each news item and seek validation by having a research assistant independently read and classify each news. This method improves precision and reduces the potential for subjective decisions by the researcher.

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<sup>1</sup>Primo Rapporto Agromafie, 2011, pages 92-224.

Table A.1: Correlation between order of treatment and levels of exploitation

|                 | (1)               | (2)               | (3)                 | (4)                |
|-----------------|-------------------|-------------------|---------------------|--------------------|
|                 | Within treated    | Within treated    | All locations       | All locations      |
| Order treatment | 0.0667<br>(0.109) | 0.0730<br>(0.148) | 0.0683*<br>(0.0377) | 0.0355<br>(0.0499) |
| Observations    | 18                | 18                | 48                  | 48                 |
| R-squared       | 0.032             | 0.197             | 0.062               | 0.235              |
| Region fe       | Yes               | Yes               | No                  | Yes                |

Note: Coefficients from the regression of the level of exploitation (range 1-3, where 3 is the highest level) on the order of treatment for treated units (Columns 1-2) and all locations (Columns 3-4). Region fixed effects are included in even columns and robust standard errors are in all regressions.

Figure A.1: News, population, and treated areas

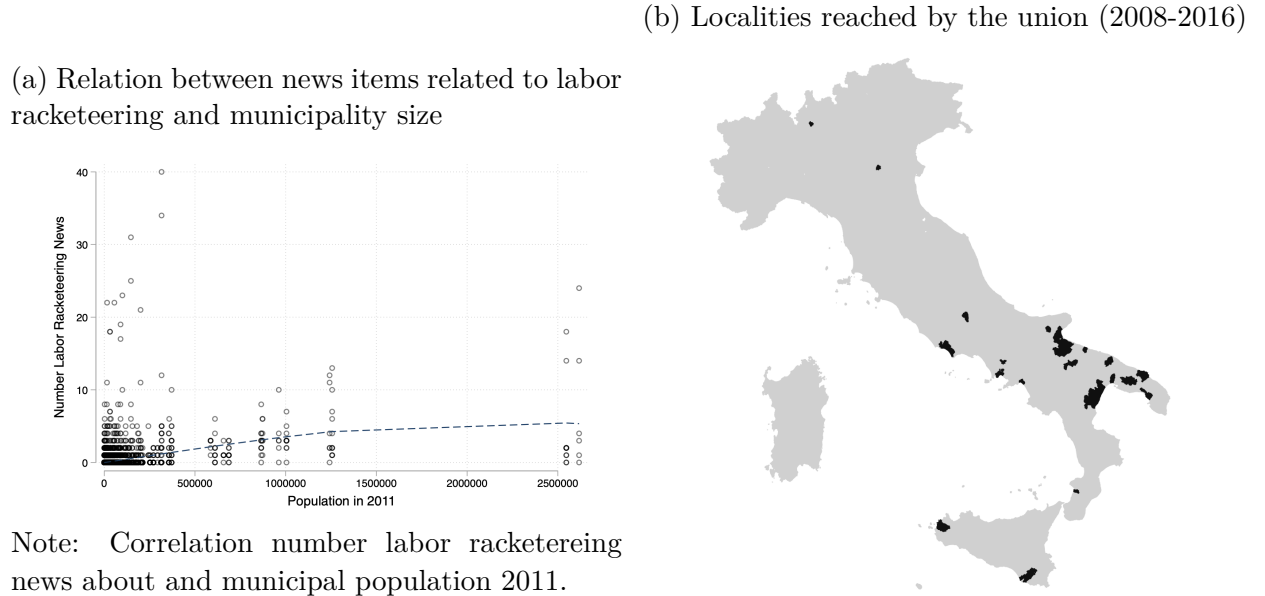
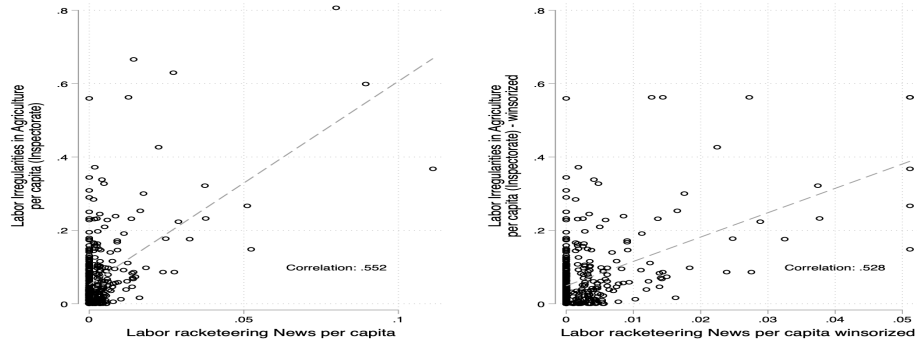


Figure A.2: Correlation between audit-based and news-based measure of labor racketeering



Note: Province-year level data on labor racketeering from 7 newspapers and from audits conducted by the Labor Inspectorate (2012-2016). Both measures are normalized by 2011 population and multiplied by 1000. Right panel: both measures are winsorized (90th percentile) to exclude outliers.

Table A.2: Correlation between audit-based and news-based measure of labor racketeering

|                         | (1)                                                      | (2)                                                      | (3)                                                      |
|-------------------------|----------------------------------------------------------|----------------------------------------------------------|----------------------------------------------------------|
|                         | Labor Irregularities<br>in Agriculture<br>(Inspectorate) | Labor Irregularities<br>in Agriculture<br>(Inspectorate) | Labor Irregularities<br>in Agriculture<br>(Inspectorate) |
| Labor Racketeering News | 5.347***<br>(0.420)                                      | 3.373***<br>(0.479)                                      | 3.397***<br>(0.481)                                      |
| Observations            | 430                                                      | 430                                                      | 430                                                      |
| R-squared               | 0.314                                                    | 0.461                                                    | 0.462                                                    |
| Year fe                 | Y                                                        | Y                                                        | Y                                                        |
| Region fe               | N                                                        | Y                                                        | Y                                                        |
| Controls                | N                                                        | N                                                        | Y                                                        |
| Mean DV Control         | .05                                                      | .05                                                      | .05                                                      |

Note: Province-year level data on labor racketeering from 7 newspapers (independent variable) and from audits conducted by the Labor Inspectorate (dependent variable), period 2012 to 2016. Both measures are normalized by 2011 population and multiplied by 1000. Controls, included in Column 3 only, include pre-2012 number of foreign born inhabitants, the number of union members and the number of inspections led by the Inspectorate. Year and region fixed effects are included and standard errors are clustered at the province level.

Table A.3: Null effect of change in voting on reporting on labor racketeering

|                          | (1)<br>News           | (2)<br>News            | (3)<br>News             | (4)<br>News            |
|--------------------------|-----------------------|------------------------|-------------------------|------------------------|
| Change Vote Center-left  | -0.00111<br>(0.00132) |                        |                         |                        |
| Change Vote Far-left     |                       | 1.31e-05<br>(0.000166) |                         |                        |
| Change Vote Center-right |                       |                        | -0.000514<br>(0.000732) |                        |
| Change Vote Far-right    |                       |                        |                         | 0.000647<br>(0.000742) |
| Observations             | 39,699                | 39,699                 | 39,699                  | 39,699                 |
| Municipality FE          | Yes                   | Yes                    | Yes                     | Yes                    |
| Year FE                  | Yes                   | Yes                    | Yes                     | Yes                    |

Note: Regression of province-level newspaper-based data of labor racketeering on change in vote share. Standard errors are clustered at the municipal level.

Table A.4: Summary Statistics

|                                         | Count  | Mean  | SD    | Min   | Max    |
|-----------------------------------------|--------|-------|-------|-------|--------|
| Any News Labor Racketeering             | 137258 | 0.01  | 0.08  | 0.00  | 1.00   |
| News Labor Racketeering                 | 137258 | 0.01  | 0.32  | 0.00  | 40.00  |
| News per capita                         | 137258 | 0.00  | 0.05  | 0.00  | 6.97   |
| Properties seized                       | 290880 | 0.11  | 3.11  | 0.00  | 983.00 |
| Properties to social use                | 290880 | 0.04  | 1.49  | 0.00  | 423.00 |
| Properties to agricultural cooperatives | 290880 | 0.01  | 0.35  | 0.00  | 60.00  |
| Vote Rifondazione                       | 4312   | 0.10  | 0.23  | 0.00  | 0.88   |
| Vote Comunisti                          | 4312   | 0.10  | 0.24  | 0.00  | 0.88   |
| Anti-Immigrant Feel                     | 4312   | 0.28  | 0.44  | 0.00  | 1.00   |
| Trust in unions                         | 4312   | 0.45  | 0.26  | 0.00  | 1.00   |
| Change Vote Center-right                | 47920  | -0.03 | 0.41  | -1.00 | 25.83  |
| Change Vote Far-right                   | 47920  | 0.84  | 3.14  | -1.00 | 134.62 |
| Change Vote Center-left                 | 47920  | -0.09 | 0.41  | -1.00 | 40.74  |
| Change Vote Far-left                    | 47920  | 0.11  | 1.47  | -1.00 | 79.85  |
| change Lega                             | 47920  | 3.93  | 22.44 | -1.00 | 653.09 |
| Vote Center-right                       | 47920  | 0.26  | 0.12  | 0.00  | 5.89   |
| Vote Far-right                          | 47920  | 0.10  | 0.10  | 0.00  | 2.56   |
| Vote Center-left                        | 47920  | 0.24  | 0.11  | 0.00  | 7.39   |
| Vote Far-left                           | 47920  | 0.05  | 0.04  | 0.00  | 1.74   |

Table A.5: Political parties in each group

| Far-Right                           | Center-Right                                  | Center-Left                             | Far-Left                        |
|-------------------------------------|-----------------------------------------------|-----------------------------------------|---------------------------------|
| Alleanza Lombarda                   | Nuovo Psi (2001, 2006)                        | Alleanza Democratica                    | Comunisti Italiani              |
| Alternativa Sociale                 | Abolizione Scorporo                           | Alleanza Democratica                    | Democrazia Atea                 |
| Alternativa Sociale                 | Alleanza Nazionale                            | Centro Democratico                      | La Rete                         |
| Azione Sociale                      | Ambienta-Lista                                | Civica Popolare Lorenzin                | La Sinistra L'Arcobaleno        |
| Blocco Nazionale<br>per le liberta' | Cantiere Popolare (2013)                      | Cristiano Sociali                       | Liberi E Uguali                 |
| Casapound                           | Centro Cristiano Democratico                  | Democratici Cristiani Uniti             |                                 |
| Destra Nazionale                    | Centro Democratico Cristiano                  | Democratici Di Sinistra                 | Nuova Sinistra                  |
| Die Freiheitlichen                  | Forza Italia                                  | Democratici Sinistra                    | della Valle D'Aosta             |
| Fiamma Tricolore                    | Fratelli D'Italia                             | Democrazia Cristiana (2006)             | Nuova Sinistra Unità            |
| Forza Nuova                         | Futuro E Libertà'                             | Federazione Laburista (1996)            | Smaller far-left parties        |
| Fronte Nazionale                    | Grande Sud (2013)                             | Girasole                                | Per Una Sinistra Rivoluzionaria |
| Grande Nord                         | Il Popolo Della Famiglia                      | Italia Dei Valori (2006, 2008)          | Potere Al Popolo                |
| Grande Sud                          | Intesa Popolare (2013)                        | Italia Europa Insieme                   | Proletaria                      |
| Italia Agli Italiani                | Liberal Democratici (2008)                    | Lega Autonomia Veneta                   | Rifondazione Comunista          |
| La Destra                           | Liberi Per Una Italia Equa                    | Lega Consumatori (2006)                 | Rivoluzione Civile              |
| Lega                                | Mir - Moderati In Rivoluzione                 | Lega Pensionati (2006)                  | Sardigna Nazione                |
| Lega Alpina Lumbarda                | Moderati In Rivoluzione (2013)                | Liberal Democratici Europei             | Sinistra Critica                |
| Lega Angela Bossi                   | Nuovo Psi (2001, 2006)                        | Liberali Per L'Italia                   | Sinistra Ecologia Libertà'      |
| Lega Nord                           | Partito Pensionati                            | Liga Fronte Veneto (2006)               | Verdi                           |
| Lega Per L'Autonomia                | Partito Repubblicano<br>Italiano (2001, 2006) | Lista Consumatori                       |                                 |
| Lega Sud                            | Partito Socialista (2001, 2006)               | Lista Dini                              |                                 |
| Movimento Per<br>L'Autonomia        | Pensionati Uniti                              | L'Ulivo                                 |                                 |
| Movimento Sociale                   | Popolo Della Libertà'                         | Margherita                              |                                 |
| Movimento Sociale Italiano          | Riformisti Italiani                           | Mastella (2006)                         |                                 |
| Mussolini                           | Sos Italia                                    | Paese Nuovo                             |                                 |
| Rifondazione Missina                | UDC E Democratici Di Centro                   | Partito Democratico                     |                                 |
| Terzo Polo                          | Unione Democratici Cristiani                  | Partito Pensionati (2006)               |                                 |
|                                     |                                               | Partito Popolare Italiano               |                                 |
|                                     |                                               | Partito Popolare Italiano (1996)        |                                 |
|                                     |                                               | Partito Repubblicano<br>Italiano (1996) |                                 |
|                                     |                                               | Partito Sardo D'Azione                  |                                 |
|                                     |                                               | Partito Socialista Italiano             |                                 |
|                                     |                                               | Patto Segni (1996)                      |                                 |
|                                     |                                               | Piu Europa                              |                                 |
|                                     |                                               | Prodi                                   |                                 |
|                                     |                                               | Repubblicani Europei                    |                                 |
|                                     |                                               | Rete - Movimento Democratico            |                                 |
|                                     |                                               | Rinnovamento                            |                                 |
|                                     |                                               | Rinnovamento Democratico                |                                 |
|                                     |                                               | Rosa Nel Pugno (2006)                   |                                 |
|                                     |                                               | Socialisti Italiani (1996)              |                                 |
|                                     |                                               | Udeur (2006)                            |                                 |
|                                     |                                               | Unione Democratica (1996)               |                                 |
|                                     |                                               | Südtiroler Volkspartei                  |                                 |

## B Identifying Assumptions

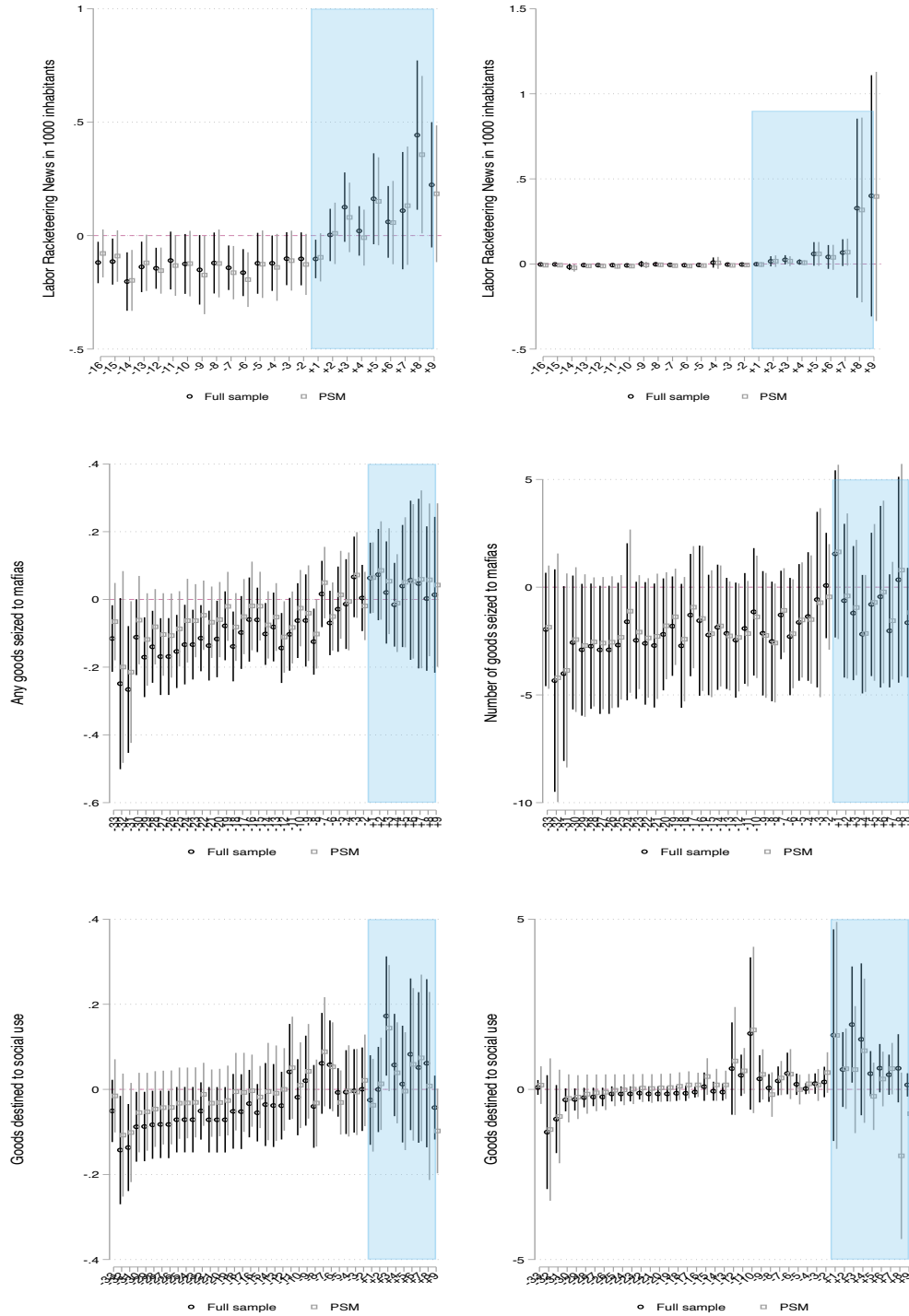
Figure B.1 and B.2 use an event-study specification to test the plausibility of the parallel trends assumption. I create dummies for each relative time period before and after treatment, and interact each dummy with the treatment variable (leads and lags specification). In line with the plausibility of the parallel trends assumption, these interactions are insignificant. Pre-trends are also plausibly parallel for change in voting, except for the change in vote for far-right parties, which exhibits an increasing trend even before treatment starts. This indicates that the results on far-right voting should not be interpreted. The coefficients for propensity-score matched units are plotted in gray and are generally consistent. With the matched sample, pre-trends are slightly improved, particularly for political outcomes. I formally test for the joint significance of the pre-trends in Table B.1. Each column includes the main DiD specification including, alongside the treatment dummy, a dummy equal to 1 in all the relative time periods before treatment. The lack of significance for the pre-periods coefficients contributes to supporting the plausibility of the parallel trends assumption. Additionally, pre-trends coefficients are considerably smaller than treatment coefficients, as shown by the relative magnitude of preperiod over postperiod coefficients at the bottom of the table. For properties seized from mafias only, the pre-trends coefficient is larger, but it is of the opposite sign than treatment effects, and insignificant.

Table B.1: Joint significance of the pre-trends

|                    | (1)<br>News Labor<br>racketeering | (2)<br>Seized<br>goods | (3)<br>Destined<br>goods | (4)<br>Vote Share<br>Left-Wing | (5)<br>Change Vote<br>Left-Wing |
|--------------------|-----------------------------------|------------------------|--------------------------|--------------------------------|---------------------------------|
| preperiod          | -0.00108<br>(0.00409)             | -2.026<br>(1.352)      | 0.0608<br>(0.102)        | 4.09e-05<br>(0.00221)          | 0.00601<br>(0.0799)             |
| Observations       | 137,258                           | 274,720                | 274,720                  | 47,397                         | 39,973                          |
| Municipality FE    | Yes                               | Yes                    | Yes                      | Yes                            | Yes                             |
| Year FE            | Yes                               | Yes                    | Yes                      | Yes                            | Yes                             |
| Relative magnitude | -0.027                            | -1.523                 | 0.056                    | 0.003                          | 0.008                           |

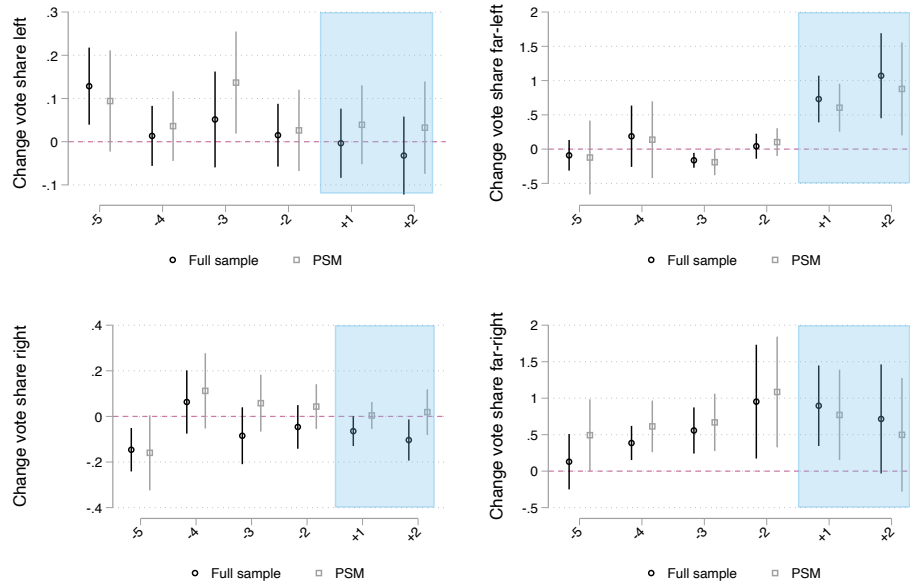
Note: Coefficients from the main DiD specification including a dummy equal to 1 for the relative time periods before treatment (period -1 is omitted and used as reference category). Relative magnitude at the bottom of the table refers to the size of the coefficient displayed in this table relative to the magnitude of the treatment coefficient for the respective outcome.

Figure B.1: Event Study: Labor racketeering news, Properties seized from mafias, and Mafia properties destined for social use



Note: Coefficients from the interaction of treatment indicator and a dummy for each year before and after the intervention. Dependent variables: Labor racketeering news (top), Properties seized from mafias (mid), and Mafia properties destined for social use (bottom). Treated periods are shaded in blue. Coefficients in gray refer to the propensity score matched sample. The omitted category is  $t - 1$ . Results in tabular form in Tables 6 and 7, APSR Dataverse files.

Figure B.2: Event Study: Change in Vote Share



Note: Coefficients from the interaction of treatment indicator and a dummy for each election year before and after the intervention. The omitted category is lag  $t - 1$ , the election year right before treatment starts. Treated periods are shaded. Coefficients in gray refer to the propensity score matched sample. Results in tabular form in Table 8, APSR Dataverse files.

Table B.2: Descriptive differences in full and matched sample

|                         | (1)<br>Control<br>Full    | (2)<br>Control<br>Matched  | (3)<br>Treated             | (4)<br>Std. Diff.<br>C Full vs T | (5)<br>Std. Diff.<br>C Match vs T |
|-------------------------|---------------------------|----------------------------|----------------------------|----------------------------------|-----------------------------------|
| Emp Agriculture         | 9.148<br>(8.545)          | 18.555<br>(13.684)         | 18.541<br>(11.961)         | 0.639                            | -0.001                            |
| Emp Unskilled           | 18.441<br>(7.424)         | 22.316<br>(8.970)          | 23.412<br>(8.913)          | 0.429                            | 0.087                             |
| Unemployed              | 10.123<br>(6.401)         | 18.112<br>(8.933)          | 18.410<br>(6.044)          | 0.941                            | 0.028                             |
| Foreign population      | 57.741<br>(42.135)        | 28.662<br>(28.654)         | 30.942<br>(27.528)         | -0.532                           | 0.057                             |
| Union members 2006      | 8.131<br>(3.709)          | 5.896<br>(2.933)           | 5.286<br>(2.915)           | -0.603                           | -0.148                            |
| Analphabetism           | 1.200<br>(1.438)          | 2.650<br>(2.066)           | 2.642<br>(1.467)           | 0.702                            | -0.003                            |
| Population density      | 294.798<br>(623.853)      | 589.532<br>(1,109.332)     | 628.996<br>(1,512.845)     | 0.204                            | 0.021                             |
| Population              | 7,093.142<br>(38,041.418) | 65,005.824<br>(366596.969) | 47,765.773<br>(139278.375) | 0.282                            | -0.044                            |
| City Surface            | 9.975<br>(13.441)         | 12.902<br>(19.322)         | 9.597<br>(16.049)          | -0.018                           | -0.132                            |
| Share males             | 97.143<br>(6.390)         | 97.260<br>(6.931)          | 95.416<br>(3.368)          | -0.239                           | -0.239                            |
| Elderly dependence      | 35.933<br>(12.301)        | 33.040<br>(10.765)         | 28.044<br>(7.531)          | -0.547                           | -0.380                            |
| Young dependence        | 20.375<br>(3.899)         | 20.685<br>(4.742)          | 22.563<br>(3.778)          | 0.403                            | 0.310                             |
| Index old people        | 195.349<br>(141.825)      | 178.549<br>(100.674)       | 132.542<br>(59.287)        | -0.409                           | -0.394                            |
| Share divorced          | 4.668<br>(2.027)          | 3.198<br>(1.744)           | 2.812<br>(1.238)           | -0.782                           | -0.181                            |
| Foreign pop minors      | 21.060<br>(8.153)         | 16.904<br>(7.861)          | 19.501<br>(5.704)          | -0.157                           | 0.267                             |
| Foreign italian couples | 2.531<br>(1.527)          | 1.673<br>(1.160)           | 1.224<br>(0.799)           | -0.758                           | -0.319                            |
| Foreign Employment      | 55.641<br>(10.827)        | 55.047<br>(12.689)         | 54.890<br>(10.801)         | -0.049                           | -0.009                            |
| Ratio foreign employed  | 83.340<br>(22.995)        | 72.057<br>(18.321)         | 69.809<br>(15.022)         | -0.493                           | -0.095                            |
| Ratio foreign unemp     | 73.217<br>(59.998)        | 124.890<br>(77.347)        | 136.743<br>(97.342)        | 0.556                            | 0.095                             |
| Foreign commuting       | 16.137<br>(8.356)         | 15.573<br>(8.927)          | 15.239<br>(5.816)          | -0.088                           | -0.031                            |
| Foreign in education    | 35.637<br>(20.816)        | 35.040<br>(26.668)         | 28.200<br>(11.850)         | -0.310                           | -0.234                            |
| Family size             | 2.361                     | 2.500                      | 2.668                      | 0.796                            | 0.397                             |

|                           |          |          |          |        |        |
|---------------------------|----------|----------|----------|--------|--------|
|                           | (0.269)  | (0.321)  | (0.278)  |        |        |
| Young living alone        | 7.331    | 6.410    | 4.967    | -0.431 | -0.286 |
|                           | (4.478)  | (3.938)  | (3.162)  |        |        |
| Single parent family      | 0.985    | 0.826    | 0.855    | -0.153 | 0.050  |
|                           | (0.794)  | (0.478)  | (0.309)  |        |        |
| Couples without kids      | 3.055    | 2.364    | 2.881    | -0.096 | 0.312  |
|                           | (1.535)  | (1.357)  | (0.953)  |        |        |
| Old living alone          | 29.536   | 27.823   | 26.723   | -0.334 | -0.161 |
|                           | (7.540)  | (5.676)  | (3.782)  |        |        |
| Property houses           | 76.884   | 73.262   | 71.582   | -0.564 | -0.163 |
|                           | (6.685)  | (7.938)  | (6.617)  |        |        |
| Urban housing             | 28.991   | 28.368   | 23.047   | -0.260 | -0.278 |
|                           | (20.204) | (15.826) | (10.731) |        |        |
| Sparse housing            | 35.824   | 33.788   | 37.949   | 0.059  | 0.122  |
|                           | (25.586) | (22.561) | (25.435) |        |        |
| Age house                 | 29.233   | 29.959   | 28.492   | -0.114 | -0.204 |
|                           | (4.623)  | (5.575)  | (4.546)  |        |        |
| Services in house         | 99.134   | 97.630   | 98.357   | -0.195 | 0.095  |
|                           | (3.006)  | (7.168)  | (2.614)  |        |        |
| Houses in good state      | 82.800   | 79.474   | 76.116   | -0.466 | -0.222 |
|                           | (11.122) | (12.122) | (9.063)  |        |        |
| Urban expansion           | 8.468    | 7.400    | 7.284    | -0.133 | -0.015 |
|                           | (6.572)  | (5.268)  | (6.005)  |        |        |
| Inhabitants per room      | 54.802   | 60.933   | 65.359   | 0.915  | 0.328  |
|                           | (7.323)  | (10.159) | (8.911)  |        |        |
| Education gender diff     | 102.379  | 101.825  | 103.162  | 0.051  | 0.074  |
|                           | (13.470) | (16.484) | (7.622)  |        |        |
| Adults studying           | 4.549    | 5.003    | 5.533    | 0.499  | 0.254  |
|                           | (1.454)  | (1.611)  | (1.330)  |        |        |
| Early exit education      | 16.604   | 21.675   | 21.069   | 0.363  | -0.041 |
|                           | (9.975)  | (12.740) | (7.184)  |        |        |
| Diploma or bachelor       | 49.277   | 43.768   | 45.084   | -0.312 | 0.086  |
|                           | (9.059)  | (11.565) | (9.948)  |        |        |
| Adults with diploma       | 18.626   | 17.753   | 17.664   | -0.096 | -0.009 |
|                           | (7.539)  | (7.956)  | (6.669)  |        |        |
| Education 15-19 years old | 98.224   | 97.662   | 97.266   | -0.305 | -0.136 |
|                           | (2.490)  | (2.196)  | (1.913)  |        |        |
| Education, middle school  | 37.866   | 38.857   | 35.373   | -0.284 | -0.357 |
|                           | (6.991)  | (8.189)  | (5.322)  |        |        |
| Not in empl nor educ      | 19.818   | 28.205   | 30.511   | 0.888  | 0.184  |
|                           | (8.540)  | (9.234)  | (8.481)  |        |        |
| Share empl over inactive  | 62.910   | 54.240   | 48.749   | -0.423 | -0.227 |
|                           | (31.570) | (21.364) | (11.235) |        |        |
| Unemployed male           | 8.293    | 15.132   | 14.958   | 0.863  | -0.019 |
|                           | (5.719)  | (7.782)  | (5.183)  |        |        |
| Unemployed female         | 12.890   | 23.185   | 24.213   | 0.956  | 0.071  |
|                           | (8.234)  | (11.772) | (8.505)  |        |        |

|                             |                    |                    |                    |        |        |
|-----------------------------|--------------------|--------------------|--------------------|--------|--------|
| Unemployed young            | 29.156<br>(15.433) | 42.113<br>(18.047) | 45.668<br>(10.799) | 0.877  | 0.169  |
| Employed Male               | 55.118<br>(7.716)  | 49.590<br>(7.564)  | 49.706<br>(5.071)  | -0.586 | 0.013  |
| Employed Female             | 35.581<br>(8.771)  | 27.461<br>(8.706)  | 26.248<br>(6.026)  | -0.877 | -0.115 |
| Employed                    | 45.135<br>(7.957)  | 38.277<br>(7.727)  | 37.604<br>(5.125)  | -0.796 | -0.073 |
| Employed in industry        | 31.302<br>(10.805) | 22.965<br>(8.966)  | 22.340<br>(6.218)  | -0.719 | -0.057 |
| Employed in services        | 40.774<br>(8.819)  | 41.916<br>(10.486) | 42.499<br>(10.142) | 0.128  | 0.040  |
| Employed in commerce        | 18.776<br>(5.138)  | 16.567<br>(4.650)  | 16.609<br>(2.489)  | -0.380 | 0.008  |
| Employed high skilled       | 25.829<br>(6.547)  | 25.721<br>(9.154)  | 29.355<br>(8.404)  | 0.331  | 0.292  |
| Employed med skilled        | 27.070<br>(7.753)  | 25.579<br>(9.689)  | 21.522<br>(5.030)  | -0.600 | -0.372 |
| Share pop commuting         | 59.777<br>(8.534)  | 52.972<br>(7.871)  | 52.027<br>(5.657)  | -0.757 | -0.098 |
| Improper housing conditions | 0.170<br>(0.669)   | 0.156<br>(0.253)   | 0.167<br>(0.211)   | -0.004 | 0.035  |
| Economic issues             | 2.001<br>(1.880)   | 4.283<br>(3.766)   | 4.346<br>(2.695)   | 0.714  | 0.014  |
| Overcrowded houses          | 1.010<br>(1.020)   | 1.903<br>(1.903)   | 1.984<br>(1.821)   | 0.467  | 0.031  |
| Observations                | 136,510            | 816                | 833                | -      | -      |

Note: Descriptive comparison of treated units vs control units in the full and matched sample.

## C Treatment and Migrants Activation: Qualitative Evidence

This section presents qualitative evidence of three types of migrant political activation from the union's intervention. The evidence comes from (i) union documents, (ii) newspaper articles on labor racketeering, and (iii) relevant legislation. I also conducted unstructured interviews with a union leader organizing the campaign and with the activist and researcher Marco Omizzolo to gain insights and clarifications on the intervention. First, multiple treated areas witnessed public mobilizations of migrants after the union intervention. Section 6 highlights two notable protests organized by migrants in 2011 and 2012, leading to judicial investigations into labor exploitation. Significant protests occurred in Agropontino, Gioia Tauro, and Caserta, with 2,000 and 1,000 agricultural workers rallying against exploitation (Il Manifesto, 2016, Il Manifesto, 2021, Global Project, 2011). Second, migrants began denouncing their employers following the union's intervention. While

newspapers do not typically cover individual reports, they do highlight large police operations stemming from these reports. For example, Section 6 mentions migrants reporting their exploiters after union intervention. In another instance, a large group of migrants in a treated area near Foggia denounced their racketeers, leading to the prosecution of seven agricultural companies (Foggia Today, 2023). Similarly, in Sermide, numerous denunciations were made by migrants following the union’s efforts (Radio Popolare, 2015). Lastly, for some migrants, the political capital gained through the union’s intervention extended beyond initial mobilization and led to long-term political activism. Yvan Sagnet, a migrant who harvested tomatoes in Nardò, a location treated by the FLAI campaign, played a crucial role in organizing sustained migrant mobilization. Sagnet continued his activism for migrant rights, becoming a Flai employee and founding an association dedicated to combating labor racketeering (Il Post, 2021). While not initially part of the Flai campaign, other examples exist of migrants who independently escaped exploitation and chose to engage in political activism to help others. Magdalena Jarczak, a worker in the Puglia countryside, managed to flee from labor exploitation and now assists other migrants as a unionist for Flai (Sky Tg24, 2017). Aboubakar Soumahoro played a significant role in organizing the mobilization of agricultural migrant workers and, in 2022, became a member of Parliament. (Infomigrants, 2022).

## D Statements

The data used for the results in this paper relies on publicly available data, with sources reported in the paper. This paper also cites interviews with two experts — individuals who led the intervention in the field – to learn about the roll out of the intervention. These interviews fall within the category of experts or third party consultations (e.g., interviews with social agency directors about their client intake procedures, or interviews with managers about their use of a particular program), classifying the project as non-human subject research. The interviews were conducted after the intervention had concluded, and the researcher had no involvement in the intervention as a consultant or participant at any stage. The interviews did not include information about the experts themselves, focusing instead on professional experiences related to the intervention. Consequently, these consultations fall outside the purview of the IRB according to the code of federal regulations. The researcher obtained informed consent by providing her name, affiliation, and contact information, explaining the general purpose of the research, and describing the objective of the interview— – gaining a better understanding of the intervention. This study did not benefit from any financial support.

## E Robustness and Additional Analyses

Table E.1: Treatment effect on labor racketeering news, dropping the first years of treatment

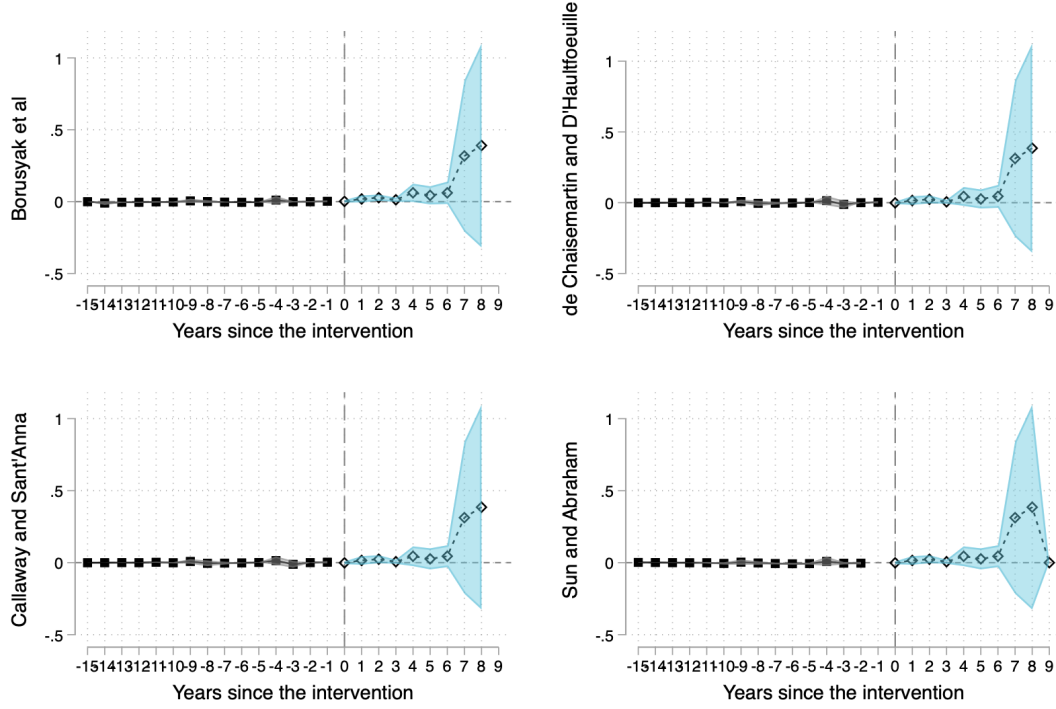
|                 | (1)<br>Drop<br>Treat 2008 | (2)<br>Drop<br>Treat 2010 | (3)<br>Drop<br>Treat 2011 | (4)<br>Drop<br>Treat 2012 |
|-----------------|---------------------------|---------------------------|---------------------------|---------------------------|
| Treated         | 0.0266**<br>(0.0119)      | 0.0237*<br>(0.0124)       | 0.0254*<br>(0.0139)       | 0.0256*<br>(0.0144)       |
| Observations    | 137,105                   | 137,071                   | 137,020                   | 137,003                   |
| Municipality FE | Yes                       | Yes                       | Yes                       | Yes                       |
| Year FE         | Yes                       | Yes                       | Yes                       | Yes                       |
| Mean DV         | .0017                     | .0017                     | .0017                     | .0017                     |

Table E.2: Effect on labor racketeering news is driven by migrants activation

|                      | (1)<br>News         | (2)<br>News          | (3)<br>News          | (4)<br>Protests       |
|----------------------|---------------------|----------------------|----------------------|-----------------------|
| Treated              | 0.0406*<br>(0.0209) |                      |                      | 0.00660*<br>(0.00396) |
| Placebo (dummy)      |                     | 0.00235<br>(0.00163) |                      |                       |
| Placebo (medium)     |                     |                      | 0.00222<br>(0.00189) |                       |
| Placebo (high)       |                     |                      | 0.00248<br>(0.00264) |                       |
| Observations         | 137,258             | 136,425              | 136,425              | 137,258               |
| Municipality-Year FE | Yes                 | Yes                  | Yes                  | Yes                   |
| Mean DV              | 0.00170             | 0.00170              | 0.00170              | 0.000400              |

Note: Main result (column 1). Placebo for locations where the union led investigations due to any (column 2) or medium-high levels of exploitation (column 3), but did not contact migrants. Effect of treatment on news on labor racketeering news including the word protest (“protesta, rivolta, corteo, dimostrazione”, column 4).

Figure E.1: Labor racketeering news, robustness to treatment effects heterogeneity bias



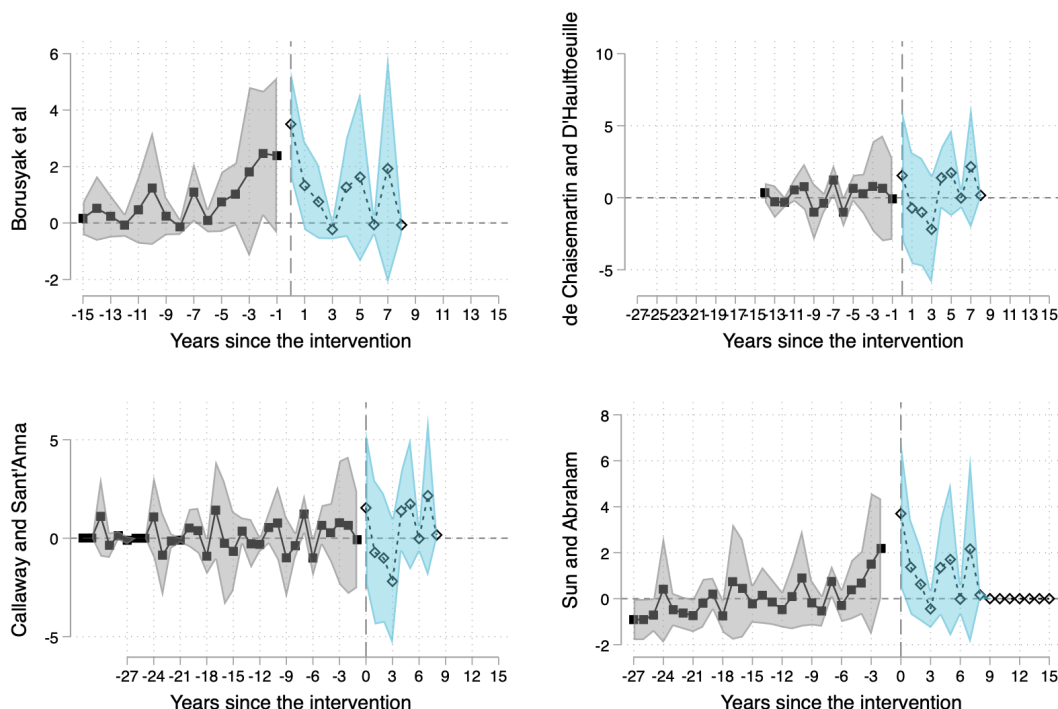
Note: Estimators proposed by De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021), Borusyak, Jaravel and Spiess (2024), and Sun and Abraham (2020). Each of these plots employs a distinct estimator to address the inherent issue in DiD designs with multiple treatment periods, where improper comparisons may arise between units treated later (acting as treatment) and units treated earlier (acting as controls). As treatment effects may not be homogeneous, these comparisons can lead to biased coefficients. The methods presented here adopt various approaches, including re-weighting observations and imputing as-if missing values, to address this potential source of bias. Results in tabular form are in Table 9, APSR Dataverse file.

Table E.3: Displacement: Effect on labor racketeering news excluding and including only municipalities neighboring treated areas

|                 | (1)                          | (2)                       | (3)                          | (4)                       |
|-----------------|------------------------------|---------------------------|------------------------------|---------------------------|
|                 | Without<br>neighbors<br>10km | Only<br>neighbors<br>10km | Without<br>neighbors<br>20km | Only<br>neighbors<br>20km |
| Treated         | 0.0422*<br>(0.0217)          | -0.0108<br>(0.0227)       | 0.0442*<br>(0.0234)          | -0.00157<br>(0.0122)      |
| Observations    | 136,659                      | 599                       | 135,758                      | 1,500                     |
| Municipality FE | Yes                          | Yes                       | Yes                          | Yes                       |
| Year FE         | Yes                          | Yes                       | Yes                          | Yes                       |
| Mean DV         | .002                         | .002                      | .002                         | .002                      |

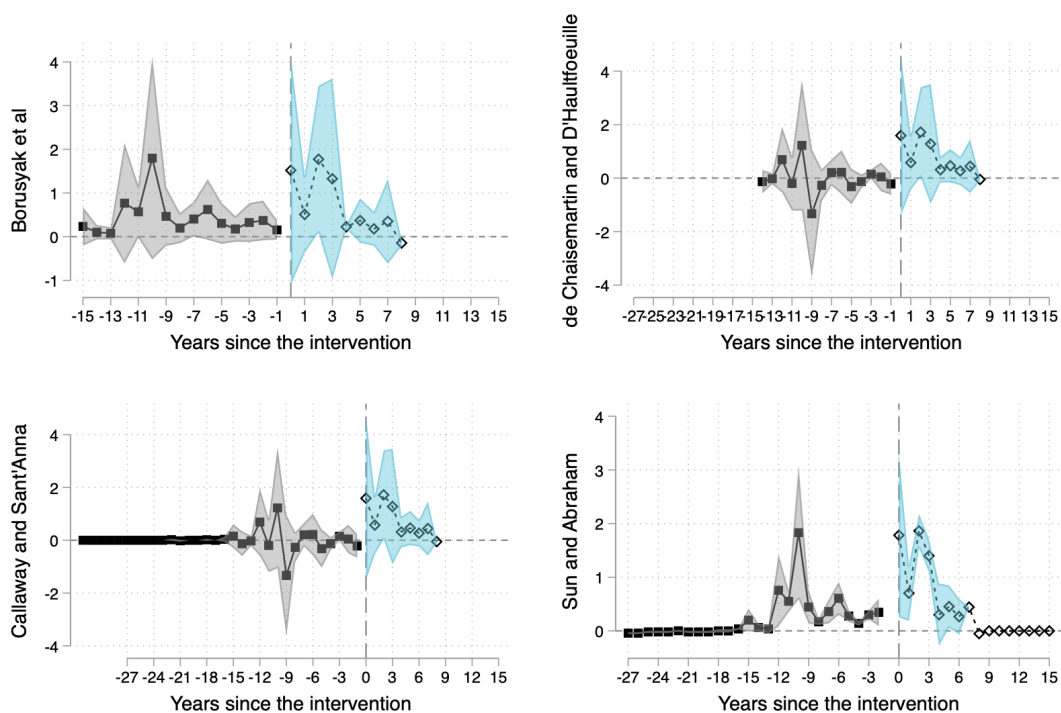
Note: Results from DiD in Equation 1 on labor racketeering news for the effect of the intervention. Columns 1 and 3 consider a control group which excludes all municipalities neighboring treated areas. Columns 2 and 4 include in the control group only municipalities within 10 or 20 kilometers from treated locations.

Figure E.2: Goods and properties seized from mafias, robustness to treatment effects heterogeneity bias



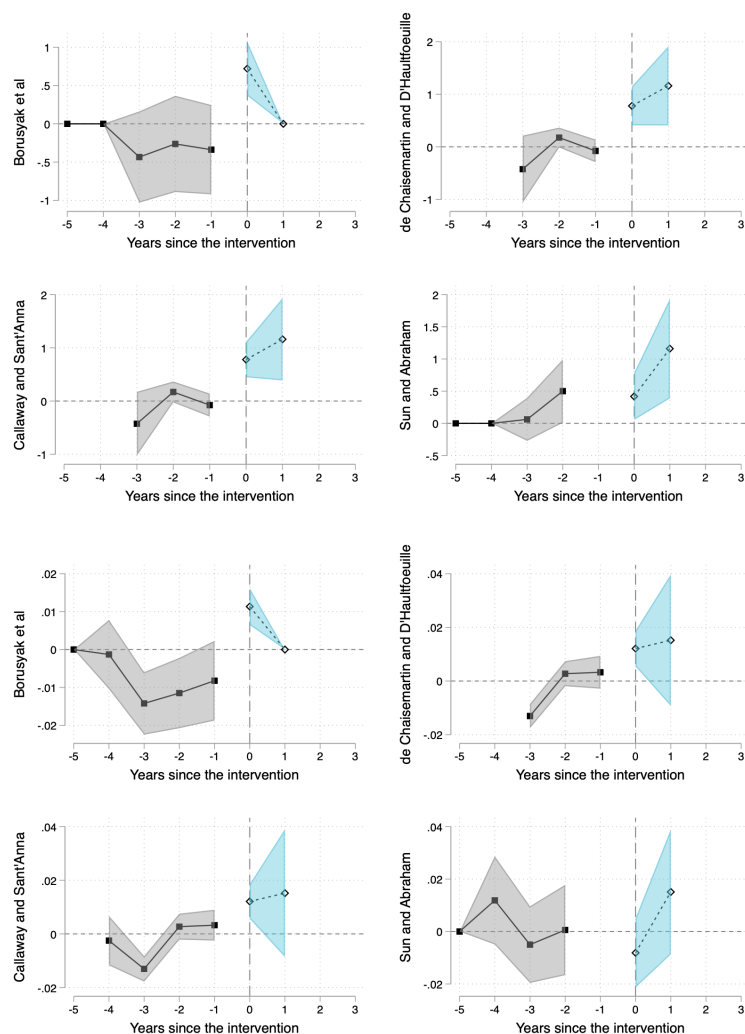
Note: Estimators proposed by De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021), Borusyak, Jaravel and Spiess (2024), and Sun and Abraham (2020). Results in tabular form are in Table 10, APSR Dataverse file.

Figure E.3: Properties destined to public use, robustness to treatment effects heterogeneity bias



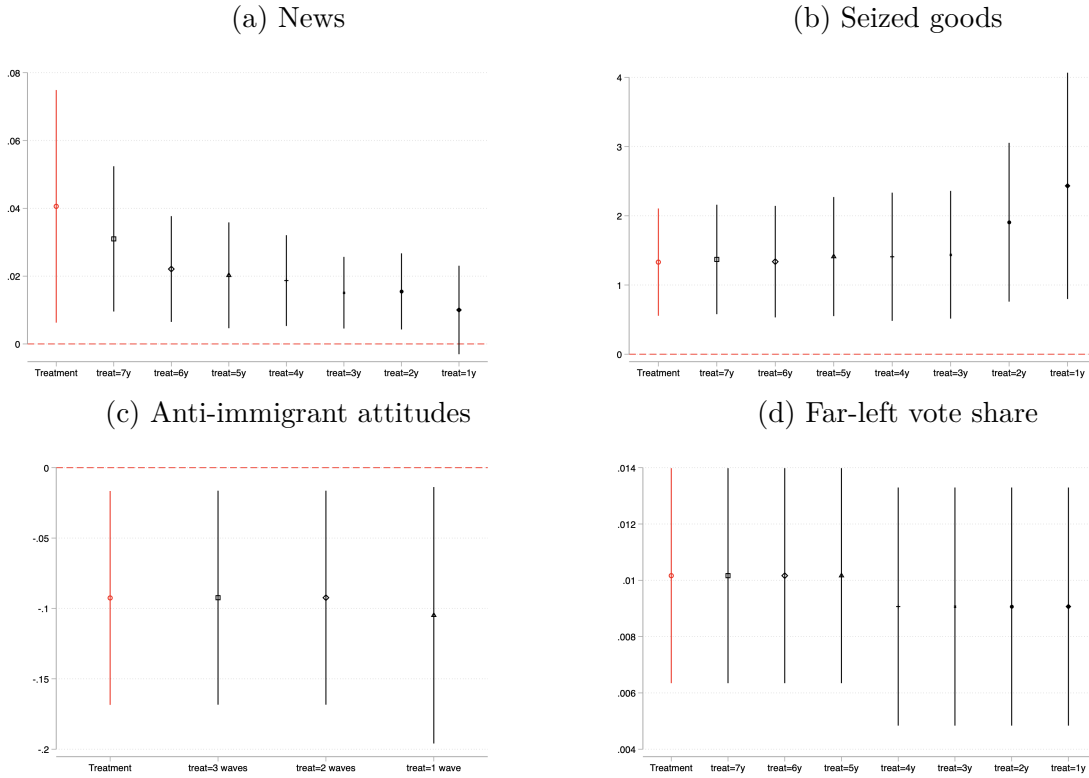
Note: Estimators proposed by De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021), Borusyak, Jaravel and Spiess (2024), and Sun and Abraham (2020).

Figure E.4: Voting (vote share and change), robustness to treatment effects heterogeneity bias



Note: Estimators proposed by De Chaisemartin and d'Haultfoeulle (2020), Callaway and Sant'Anna (2021), Borusyak, Jaravel and Spiess (2024), and Sun and Abraham (2020). Results in tabular form are in Table 11, APSR Dataverse file.

Figure E.5: Robustness to treatment duration



Note: The coefficient in red plots the treatment effect from the main specification in Equation 1. The following coefficients reduce the duration of treatment by one year at the time, until treatment is assumed to only last during the first year of the intervention. Findings are not dependent on the definition of the duration of treatment. Results in tabular form are in Tables 12 and 13, APSR Dataverse file.

Table E.4: Treatment effect on labor racketeering news, by newspaper

|                 | (1)<br>Full sample<br>Corriere | (2)<br>Matched sample<br>Corriere | (3)<br>Full sample<br>Repubblica | (4)<br>Matched sample<br>Repubblica |
|-----------------|--------------------------------|-----------------------------------|----------------------------------|-------------------------------------|
| treated         | 0.133***<br>(0.0334)           | 0.0935***<br>(0.0337)             | 0.108***<br>(0.0330)             | 0.0914***<br>(0.0335)               |
| Observations    | 137,343                        | 1,649                             | 137,343                          | 1,649                               |
| Municipality FE | Yes                            | Yes                               | Yes                              | Yes                                 |
| Year FE         | Yes                            | Yes                               | Yes                              | Yes                                 |
| Mean DV         | 0.00110                        | 0.00613                           | 0.00543                          | 0.0429                              |

Note: Results from a DiD capturing the change in labor racketeering news in treated municipalities after the intervention took place. The DV is the population share of news from Corriere (columns 1-2) and Repubblica (columns 3-4) in 1000 inhabitants. Municipality and year FE are included and standard errors are clustered at the municipal level.

Table E.5: Treatment effect on labor racketeering news, robustness

|                    | (1)<br>Total<br>news | (2)<br>Poisson     | (3)<br>Neg<br>Binomial | (4)<br>Stacked<br>by-event | (5)<br>Drop post<br>8 and 9 | (6)<br>Control<br>foreign pop | (7)<br>Control<br>unemployment |
|--------------------|----------------------|--------------------|------------------------|----------------------------|-----------------------------|-------------------------------|--------------------------------|
| Treated            | 0.735***<br>(0.263)  | 1.243**<br>(0.526) | 1.247*<br>(0.697)      | 0.0533*<br>(0.0323)        | 0.0221**<br>(0.00948)       | 0.0394*<br>(0.0209)           | 0.0382*<br>(0.0206)            |
| Observations       | 137,343              | 7,803              | 7,803                  | 1,231,871                  | 137,240                     | 137,258                       | 137,258                        |
| Municipality FE    | Yes                  | Yes                | Yes                    | Yes                        | Yes                         | Yes                           | Yes                            |
| Year FE            | Yes                  | Yes                | Yes                    | Yes                        | Yes                         | Yes                           | Yes                            |
| Cohort FE          | No                   | No                 | No                     | No                         | Yes                         | No                            | No                             |
| Mean DV            | .012                 | .006               | .006                   | .006                       | .002                        | .002                          | .002                           |
| Number of panelvar |                      | 459                | 459                    |                            |                             |                               |                                |

Note: Results from a DiD capturing treatment effect on news on labor racketeering in treated municipalities after the intervention. Municipality and year FE are included and standard errors are clustered at the municipal level. Column 1 includes the total number of news as dependent variable, Column 2 and 3 use a Poisson and Negative binomial model. Column 4 uses a stacked-by-event design to account for treatment effects heterogeneity (?), and includes cohort FE. Column 5 drops the last two post-periods, in which larger but more imprecise effects are observed. Column 6 includes flexible controls for foreign population and 7 for male unemployment.

Table E.6: Treatment effect on firms seized from mafias

|                    | (1)<br>Full         | (2)<br>Matched     | (3)<br>Neg Binomial |
|--------------------|---------------------|--------------------|---------------------|
| Treated            | 0.115**<br>(0.0517) | -0.0196<br>(0.118) | 0.818**<br>(0.344)  |
| Observations       | 274,720             | 3,230              | 1,224               |
| Municipality FE    | Yes                 | Yes                | Yes                 |
| Year FE            | Yes                 | Yes                | Yes                 |
| Number of panelvar |                     |                    | 36                  |

Note: DiD as in Equation 1 on firms seized from mafias. Results are reported on the full sample (column 1), matched sample (column 2), and using a negative binomial regression to account for the rarity of the seizure events (column 3).

Table E.7: Treatment effect on goods seized from mafias, Poisson and Negative Binomial

|                          | Seized            |                     | Destined           |                     |
|--------------------------|-------------------|---------------------|--------------------|---------------------|
|                          | (1)<br>Poisson    | (2)<br>Neg Binomial | (3)<br>Poisson     | (4)<br>Neg Binomial |
| Treated                  | 0.310+<br>(0.312) | 0.654***<br>(0.171) | 0.575**<br>(0.266) | 0.620***<br>(0.227) |
| Observations             | 52,836            | 52,836              | 29,546             | 29,546              |
| Number of municipalities | 1,554             | 1,554               | 869                | 869                 |
| Municipality FE          | Yes               | Yes                 | Yes                | Yes                 |
| Year FE                  | Yes               | Yes                 | Yes                | Yes                 |

Note: DiD as in Equation 1 on properties seized from mafias (columns 1-2) and destined to public use (columns 3-4).

Table E.8: Treatment effect on change in parties' vote share, treatment close to elections

|              | (1)<br>Full<br>Center-Left | (2)<br>Full<br>Far-Left | (3)<br>Full<br>Center-Right | (4)<br>Full<br>Far-Right' | (5)<br>Matched<br>Center-Left | (6)<br>Matched<br>Far-Left | (7)<br>Matched<br>Center-Right | (8)<br>Matched<br>Far-Right' |
|--------------|----------------------------|-------------------------|-----------------------------|---------------------------|-------------------------------|----------------------------|--------------------------------|------------------------------|
| Treated      | -0.0593**<br>(0.0256)      | 0.901***<br>(0.268)     | -0.0525**<br>(0.0253)       | 0.247<br>(0.336)          | 0.0223<br>(0.0699)            | 0.929**<br>(0.380)         | 0.0673*<br>(0.0399)            | 0.216<br>(0.381)             |
| Observations | 39,973                     | 39,973                  | 39,973                      | 39,973                    | 240                           | 240                        | 240                            | 240                          |
| City Year FE | Yes                        | Yes                     | Yes                         | Yes                       | Yes                           | Yes                        | Yes                            | Yes                          |
| Mean DV      | -0.0649                    | 0.150                   | -0.00559                    | 0.262                     | -0.0649                       | 0.150                      | -0.00559                       | 0.262                        |

Note: DiD as in Equation 1 on change in parties' vote share. Here, municipalities are considered treated only when the intervention happened less than one year before elections. Other treated observations are set as missing.

Table E.9: P-values from Fisher randomization inference

|              | (1)<br>News           | (2)<br>Goods seized  | (3)<br>Anti-Immigrant | (4)<br>Change Vote Far-left |
|--------------|-----------------------|----------------------|-----------------------|-----------------------------|
| Main         | 0.0406<br>(1.95)      | 1.331**<br>(2.82)    | -0.0925*<br>(-2.00)   | 0.308**<br>(3.15)           |
| RI p-values  | 0.00170***<br>(47.10) | 0.114***<br>(278.09) | 0.274***<br>(538.88)  | 0.106***<br>(542.61)        |
| Observations | 137258                | 274720               | 5816                  | 47835                       |

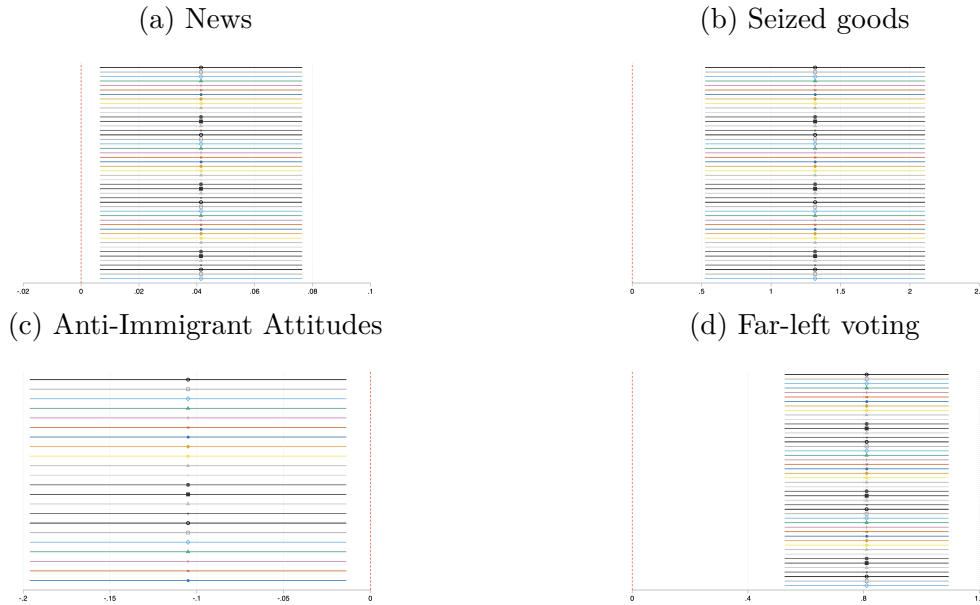
Note: P-values from main specification and from randomization inference with resampling replications n=1000.

Table E.10: Conley-Taber 2011 confidence intervals

|                           | Beta   | Low 95 | Upper 95 |
|---------------------------|--------|--------|----------|
| News                      |        |        |          |
| Standard                  | 0.041  | -0.000 | 0.081    |
| Conley-Taber              | 0.067  | 0.050  | 0.070    |
| Goods seized              |        |        |          |
| Standard                  | 1.331  | 0.407  | 2.254    |
| Conley-Taber              | 1.699  | -0.686 | 2.104    |
| Goods destined            |        |        |          |
| Standard                  | 1.089  | 0.015  | 2.162    |
| Conley-Taber              | 1.341  | 0.375  | 1.496    |
| Anti-Immigrant Attitudes  |        |        |          |
| Standard                  | -0.093 | -0.183 | -0.002   |
| Conley-Taber              | 0.138  | 0.747  | 0.747    |
| Change in far-left voting |        |        |          |
| Standard                  | 0.308  | 0.116  | 0.500    |
| Conley-Taber              | 0.287  | -1.937 | 2.187    |

Note: Coefficients and confidence intervals in the standard model and using the (Conley and Taber, 2011) correcting for a small number of policy changes.

Figure E.6: Dropping one observation at a time



Note: Each line is a coefficient from the main specification (Equation 1) dropping one treated unit at a time. Zero is marked as a red vertical line. Each coefficient is similar and does not lose significance, indicating that effects are not driven by any specific location. Results in tabular form are in Table 14, APSR Dataverse file.

Table E.11: Main results using Synthetic Difference-in-Differences

|          | (1)     | (2)        | (3)           |
|----------|---------|------------|---------------|
|          | News    | Imm Seized | Vote Far-Left |
| Treated  | 0.049*  | 1.258**    | 0.008***      |
|          | (0.027) | (0.560)    | (0.003)       |
| <i>N</i> | 137258  | 274720     | 50421         |

Note: Results from a Synthetic DiD (Arkhangelsky, Dmitry and Athey, Susan and Hirshberg, David A and Imbens, Guido W and Wager, Stefan, 2021) of treatment effect on each outcome. Municipality and year FE and standard errors clustered at the municipal level are included.

Table E.12: Change in voting, comparison within treated units

|                 | (1)<br>Change vote<br>Center-Left | (2)<br>Change vote<br>Far-Left | (3)<br>Change vote<br>Center-Right | (4)<br>Change vote<br>Far-Right |
|-----------------|-----------------------------------|--------------------------------|------------------------------------|---------------------------------|
| Treated         | 0.0634<br>(0.0461)                | 0.490***<br>(0.137)            | -0.110**<br>(0.0412)               | 0.843**<br>(0.387)              |
| Observations    | 288                               | 288                            | 288                                | 288                             |
| R-squared       | 0.542                             | 0.500                          | 0.550                              | 0.389                           |
| Municipality FE | Yes                               | Yes                            | Yes                                | Yes                             |
| Year FE         | Yes                               | Yes                            | Yes                                | Yes                             |

Note: Results from a DiD capturing treatment effect on change in vote share in municipalities treated with the union intervention. Municipality and year FE are included and standard errors are clustered at the municipal level. In this table, I compare only within treated units.

Table E.13: Multiple hypothesis testing correction

|                              | Standard | FDR Sharpened |
|------------------------------|----------|---------------|
| Anti-immigrant attitudes     | 3.74e-06 | .001          |
| Change in far-left vote      | .0455616 | .032          |
| Destined to public use       | .0468491 | .032          |
| Labor racketeering news      | .0516422 | .032          |
| Properties seized from mafia | .0047525 | .01           |
| Total                        | .0297618 | .0214         |

Note: Estimates using Anderson (2008) method for multiple hypothesis testing correction.

Table E.14: Controlling for region specific time trends

|                          | (1)<br>News about<br>racketeering | (2)<br>Goods seized<br>from mafia | (3)<br>Goods destined<br>to public use | (4)<br>Change vote<br>Far-Left |
|--------------------------|-----------------------------------|-----------------------------------|----------------------------------------|--------------------------------|
| Treated                  | 0.0372*<br>(0.0201)               | 1.170**<br>(0.492)                | 0.953*<br>(0.561)                      | 0.308***<br>(0.0980)           |
| Observations             | 137,258                           | 274,720                           | 274,720                                | 47,835                         |
| R-squared                | 0.162                             |                                   |                                        | 0.268                          |
| Municipality and Year FE | Yes                               | Yes                               | Yes                                    | Yes                            |
| Region times year FE     | Yes                               | Yes                               | Yes                                    | Yes                            |
| Mean DV                  | 0.00200                           | 0.109                             | 0.0370                                 | 0.106                          |

Note: Main DiD specification replicated on each of the main outcomes of interest using region times years fixed effects.

## SM References

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